

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

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Abstract: We study the role of firms' political influence on the effectiveness of government spending. We build a unique database linking information on campaign contributions, state legislative elections, firm characteristics, and grant allocations of the 2009 Recovery Act. Using an instrumental variable approach, we document that, at the state level, a 10 percentage point increase in the share of spending given to politically connected firms lowers the job creation effect of stimulus by 30 percent. We also exploit ex-post close elections to obtain exogenous variation in political connections across firms, and we establish that connected firms are more likely to win stimulus grants, while those firms create fewer jobs after winning grants. A quantitative general equilibrium model shows that allocating more spending towards politically connected firms lowers the aggregate jobs multiplier as well and that this dampening effect is fully accounted for by connected firms charging higher markups.

JEL classification: D22, D72, E62, H57, R12

Key words: fiscal stimulus, public expenditure allocation, American Recovery and Reinvestment Act, political connections, campaign finance, regional economic activity

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1 Introduction

During economic downturns, aggressive fiscal stimulus measures are implemented to stabilize the economy. A substantial share of this stimulus is government purchases, through which funds are channeled directly to firms. The macroeconomics literature has been keenly interested in understanding the effectiveness of such government spending. Despite how valuable stimulus funds are to firms, the existing literature largely overlooks the influence businesses may exert over the disbursement of funds, and the subsequent impact on the effectiveness of spending. Incorporating these types of political economy considerations into macroeconomic studies has been called for decades (e.g., [Alesina et al. 1997](#); [Drazen 2000](#)).

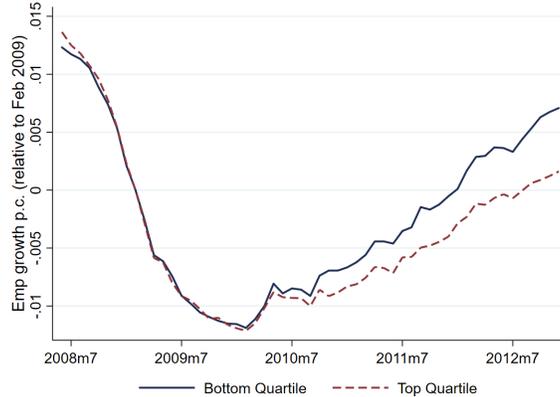
In this paper, we establish evidence that firms’ political influence distorts the allocation of stimulus spending and dampens the effectiveness of fiscal stimulus. To identify these effects, we build a unique database linking information on campaign contributions, state legislative elections, firm characteristics, and allocation of grants funded by the 2009 American Recovery and Reinvestment Act (ARRA). ARRA provides an appropriate setting for this study. Because rapid disbursement of funds was seen as crucial for achieving the primary goal of the fiscal stimulus in the midst of a recession—creating and saving jobs—federal agencies gave considerable discretion to state governments in selecting ARRA grants and firms that will produce goods and services for those grants. At the same time, with the average grant worth \$500,000, there was a great incentive for firms to leverage their existing connections with state politicians to influence the allocation of stimulus spending.

We start by evaluating how much politically connected spending impacted regional employment growth. As motivation, [Figure 1](#) shows that despite similar employment growth prior to the passage of ARRA, states that allocated a higher share of ARRA spending to politically connected firms grew, on average, 26 percent slower in subsequent years than states that allocated a lower share of spending to connected firms. To formalize our analysis, we build on the framework used in recent empirical macroeconomic literature that exploits cross-regional variation in government spending to estimate the jobs multiplier—defined as the number of jobs created per \$1 million spent.¹ We extend the framework by introducing the share of ARRA spending disbursed to politically connected firms as an additional explanatory variable.

We account for three main sources of endogeneity. First, local severity of the recession may increase ARRA spending and decrease the speed of recovery. Second, better managed states may be more successful in soliciting funds and may experience a swifter recovery. To

¹Studies that exploit cross-regional variation to estimate the effects of fiscal spending include [Chodorow-Reich et al. \(2012\)](#), [Wilson \(2012\)](#), [Nakamura and Steinsson \(2014\)](#), [Dupor and Mehkari \(2016\)](#), [Dupor and McCrory \(2018\)](#) and [Chodorow-Reich \(2019\)](#), [Corbi et al. \(2019\)](#).

Figure 1: Employment growth by politically connected spending share



Notes: Employment growth per capita between June 2008 and December 2012, relative to February 2009. The solid blue line depicts average growth across states in the bottom quartile of the share of ARRA spending through politically connected firms. The dashed red line depicts average growth across states in the top quartile of the share of ARRA spending through politically connected firms. Politically connected firms are defined as those that contributed to winning candidates in state legislative elections between 2006 and 2008.

account for such unobserved factors correlated with both ARRA spending and the speed of recovery, we follow the literature (e.g., [Wilson 2012](#); [Chodorow-Reich 2019](#)) and instrument for ARRA spending with the Department of Transportation (DOT) grants disbursed based on pre-existing formulas. Third, both firms’ ability and willingness to exert political influence and state-level employment growth may be correlated with common factors, such as corruption. We directly control for measures of political corruption, and we instrument for the share of politically connected spending with an indicator of whether the state prohibited corporate campaign contributions in 2002. We also control for other variables that could be correlated with our instruments and short-run state employment dynamics. Our identifying assumption is that, conditional on states’ economic and political environment at the onset of the recession, both instruments are unlikely to be correlated with unobserved factors that affected states’ speed of recovery.

We find that ARRA created or saved, on average, 27.2 jobs per million dollars spent, but raising the share of spending given to politically connected firms by 10 percentage points lowers this multiplier by 8.1 jobs, or equivalently, by 30 percent.² We conduct a battery of robustness tests: our baseline results hold after accounting for states’ industrial composition and firm age and size distributions, the geography of the housing bust, anticipation effects, as well as several alternative measures of political environment and worker influence.

We then provide direct, micro-level evidence that political influence impacted the allocation of stimulus spending and dampened employment growth by leveraging the firm-state level variation in our data. We identify the causal effect of firms’ political connections on

²10 percentage points is slightly larger than one standard deviation (8.7 percentage points).

the allocation of grants by exploiting ex-post close elections as a source of random variation. A key assumption is that winning by a very small margin is almost as good as random for the top two candidates (Lee, 2008; Akey, 2015). Using this quasi-random variation allows us to overcome the endogeneity of unobserved factors driving both firms’ connections to politicians and the probability of winning ARRA grants.

We find that “lucky” firms that supported more close election winners (treated) are 18 percent more likely to secure ARRA grants than “unlucky” firms that supported more close election losers (control). Our results are robust to controlling for various firm characteristics and placebo tests. We also explore potential mechanisms by analyzing heterogeneous treatment effects and find evidence of grants working partially through quid pro quo. Specifically, we find that the effects of connections are stronger for firms that are among the top five percent of contributors. We do not find evidence of stronger effects of connections for older or larger firms, or those supporting candidates from the majority party.

We further show that of politically connected spending dampens the job creation effect of winning stimulus grants at the establishment level. We implement a heterogeneous treatment, difference-in-difference approach that compares the establishment-level employment growth of grant winners versus non-winners for connected and non-connected firms. We find that establishments belonging to firms that gained more political connections through close elections exhibit significantly slower employment growth after winning a grant relative to their non-connected counterparts, and that this difference persists for at least six years. Our results are robust to various alternative specifications and a placebo test. Overall, empirically, we show that politically connected spending dampens regional- and business-level employment growth, and that the differences in the employment effect of fiscal stimulus between politically connected and non-politically connected firms arises from inefficiencies, rather than differences in productivity.

To assess whether our results also hold in the aggregate and to better understand the mechanism, we develop a quantitative general equilibrium model that allows for different channels through which politically connected spending could potentially create fewer jobs. We extend the multi-region framework of Nakamura and Steinsson (2014) to allow for two sectors—a politically connected sector and a non-connected sector—where firms in the connected sector are more productive (hence use fewer workers for the same level of production) and, at the same time, are able to charge higher markups. The model is calibrated to firm level data and to match the regional jobs multiplier estimates. The calibrated model points to large differences in productivity between connected and non-connected firms. Nevertheless, these differences can only explain 45 percent of the employment loss associated with the increase in the share of politically connected spending in partial equilibrium. In general equi-

librium, a 10 percentage points increase in the share of politically connected spending still lowers the aggregate jobs multiplier by 4.2 jobs, and this dampening effect is fully accounted for by politically connected firms charging higher markups.

In a nutshell, we show, empirically and quantitatively, that political influence matters both for the allocation of spending across firms and the success of fiscal stimulus at saving jobs. Disbursing stimulus through state authorities may facilitate swift implementation, but it opens the allocation process up to political influence and may come at the cost of lower job creation. Thus, when analyzing fiscal stimulus policies, it is important to take into consideration not just the size of the package and speed of disbursement, but also the political process by which funds are allocated across recipients.

Related Literature This paper bridges the literature 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 Wiwatantakantang, 2008; Akcigit et al., 2023), use lobbying (Kerr et al., 2014; Kang, 2016; Hassan et al., 2019) or campaign contributions (Faccio, 2004; Claessens et al., 2008; Cooper et al., 2010; Akey, 2015) to affect the design and implementation of public policy in their favor. The literature has documented that politically connected firms can increase their value through various channels, including tax benefits (Arayavechkit et al., 2018), less regulation (Fisman and Wang, 2015), more favorable terms for government loans (Khwaja and Mian, 2005), and government bailouts (Faccio et al., 2006).

More closely related to our analysis, the literature has studied how firms lever their political connections to capture government spending. There is more evidence for developing countries than for advanced economies.³ In the context of the United States, Duchin and Sosyura (2012) find that politically connected firms were more likely to receive Troubled Asset Relief Program (TARP) funds and that these firms subsequently had lower investment efficiency. Related to our work, Goldman et al. (2013) find that firms with a board of directors connected to the winning party in the 1994 federal elections received significantly more procurement contracts in the subsequent years. Brogaard et al. (2021) use sudden deaths and resignations of politicians to document that connected firms are able to initially bid lower prices and favorably renegotiate terms of procurement contracts.

At a more aggregate level, the literature has found mixed evidence on the importance of

³For developing countries see, for example, the studies for Brazil (Colonnelli and Prem, 2022), Czech Republic (Titl and Geys, 2019), India (Lehne et al., 2018), Lithuania (Baltrunaite, 2017), and Russia (Mironov and Zhuravskaya, 2016). For advanced economies see, for example, studies for Denmark (Amore and Benned- sen, 2013) and South Korea (Schoenherr, 2019).

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

These prior studies have primarily focused on federal-level campaign contributions and lobbying activities by large, publicly listed companies. We make two contributions to the literature. First, we build a novel database covering political activities of a nationally representative sample of U.S. firms that expands the scope of analysis beyond the large, publicly listed firms that are typically studied in the literature. Second, we establish a causal link between the political connections of firms to state politicians and the allocation of grants. This fact uncovers a novel sub-national mechanism through which political connection affects stimulus spending, as most prior studies focus on political connections or stimulus spending allocation at the federal level.⁴

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

Although prior studies recognize that firms are crucial for understanding the effects of

⁴Our work is complementary to [Boone et al. \(2014\)](#), who find that the U.S. congressional representation did not have an impact on the regional allocation of ARRA spending. Our work identifies the level of connection (firm-state politician) and allocation (firm-state) at which political factors are indeed important.

⁵Beyond the analysis of ARRA, [Nakamura and Steinsson \(2014\)](#) and [Dupor and Guerrero \(2017\)](#) exploit the geographic variation on military expenditure, and [Ramey and Zubairy \(2018\)](#) use quarterly time series data to 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; and [Corbi et al. \(2019\)](#) estimate the causal effect of public spending on local employment in Brazil using a regression discontinuity design. A more comprehensive review of the recent fiscal and employment multiplier literature can be found in [Chodorow-Reich \(2019\)](#).

fiscal stimulus, this literature had not studied how the allocation of government spending to politically connected versus non-politically connected firms impacts the macroeconomic effects of the policy. We contribute to this literature in two ways. First, we show that disbursing a higher share of stimulus to politically connected firms lowers the job creation effect of fiscal stimulus, both at the regional and establishment level. Second, we quantify the general equilibrium effect of politically connected spending on employment.

The remainder of the paper is structured as follows. Section 2 describes the institutional features of ARRA and the data sources used in our analysis. Section 3 studies whether the distribution of ARRA resources across firms affects the state-level jobs multiplier. Section 4 studies how campaign contributions to state politicians determine the firm-level allocation of ARRA grants and the establishment-level employment effect of winning grants. Section 5 quantifies the general equilibrium effects of political connections on the jobs multiplier using a general equilibrium model. Section 6 concludes.

2 Institutional Context and Data

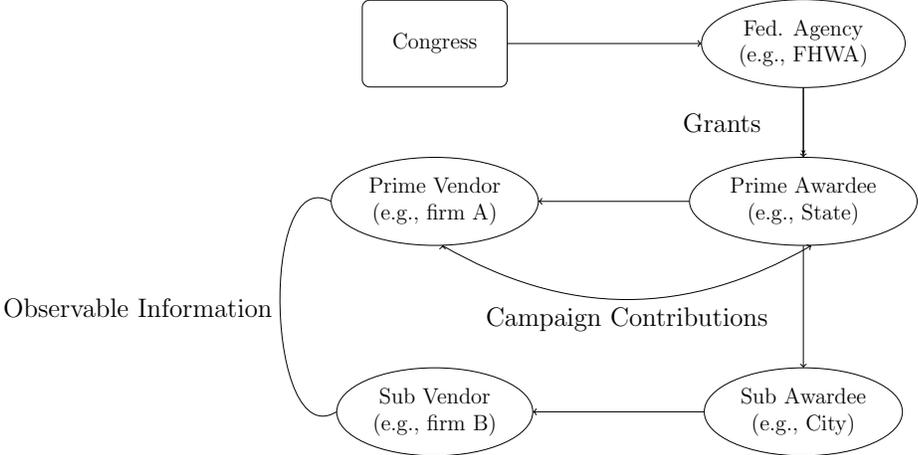
2.1 The American Recovery and Reinvestment Act

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. At roughly \$787 billion, it was, at the time, the largest fiscal stimulus package in United States history. The primary objective of ARRA was to create and save jobs. Stimulus funds were distributed in various forms, including tax relief (\$288 billion, or 36.6 percent), funding of entitlement programs (\$80 billion, or 10.2 percent) such as unemployment benefit extensions (Hagedorn et al., 2013; Chodorow-Reich et al., 2019), fiscal aid to state governments (\$144 billion, or 18.3 percent) (Chodorow-Reich et al., 2012), loans (\$20 billion, or 2.5 percent), and procurement contracts and grants (\$255 billion, or 32.4 percent).

Firms, which received 26 percent of total ARRA spending (\$205 billion), were awarded funds primarily through procurement contracts and grants. This study focuses on grants because this form of federal spending accounts for 84 percent of funds awarded to firms. Moreover, 75 percent of grants are channeled through subnational governments, which creates room for influence to be exerted over local politicians in the allocation process. For example, consider ARRA highway infrastructure investment projects. The Federal Highway Administration (FHWA) first appropriates ARRA funds to states, mostly through preexisting highway grant programs. State governments, who are 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 2 summarizes the fund distribution process.

Figure 2: Allocation of Grants and Contracts during ARRA



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

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

2.2 Data Sources

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

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

To measure political connections of firms to state legislators, we use campaign finance contribution data from the National Institute of Money in Politics (NIMP). NIMP is a non-profit 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.⁶ Because most ARRA grants were awarded in 2009 and 2010, we focus on standard elections for state legislative positions held between 2006 and 2008, with terms lasting until at least 2010. Terms for state legislators vary by state, with most lasting between two and four years. In our sample, there are about 5,000 elections in 2006 and 2008 and 500 elections in 2007. We obtain outcomes of these elections from the State Legislative Election Results Database compiled by Klarner et al. (2013).

2.3 Firms in State Politics and Federal Grants

Our resulting data set reveals three facts pertinent to our analysis. First, private-sector businesses actively engage in local elections via campaign contributions. On average, firms contribute a total of \$5,200 across five candidates per election cycle, or roughly \$1,000 per candidate. Firms account for at least 16 percent of all state campaign contribution cases

⁶Appendix B.1 provides additional details on the matching procedures.

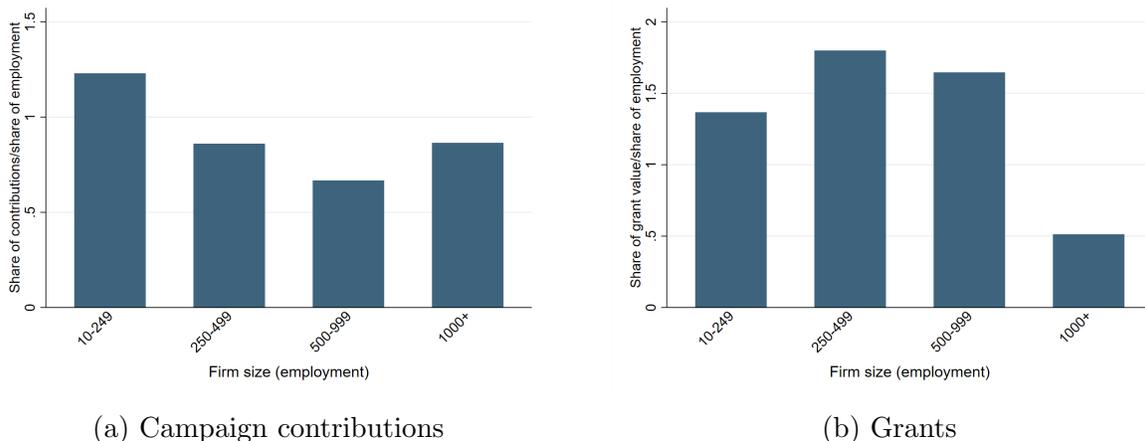
and 28 percent of their dollar amount.⁷ The remaining contributions are made by individuals, unions, and associations. The large share of firm campaign contributions may seem counterintuitive, as firms are perceived to primarily engage in political activities through business associations. However, business associations speak for industries and coalitions, not individual businesses. They are therefore more useful in influencing regulatory change than in helping specific 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 (SMEs) account for a disproportionate share of firm campaign contributions, even relative to their employment shares. Figure 3a shows the share of total campaign contribution amount accounted for by each firm size group divided by the share of total employment accounted for by that group. For example, combining the first two groups, firms with fewer than 500 employees account for 55 percent of campaign contributions and 46 percent of employment, leading to the ratio of 1.2. This finding is in contrast to the conventional belief that corporate political activities are mostly done by large firms. While this is true in the case of federal-level lobbying, which is associated with large fixed costs and entry barriers (Kerr et al., 2014), campaign contributions to local politicians appear to be much more accessible to SMEs. Our data therefore highlights both the importance of state-level political engagement by smaller businesses, and the advantage of using a nationally representative data set, such as NETS, over data that contains only publicly listed firms (e.g., Compustat).

Third, SMEs play an important role in Federal grant spending. Grant-winning firms were awarded, on average, 1.8 grants, and the average size of each grant was over \$500,000. To show this, Figure 3b depicts the ratio between grant value share of each firm size group and employment share of that group, similarly defined as in Figure 3a. While 46 percent of employment is in firms with less than 500 employees, 66 percent of ARRA grant spending to prime vendors went to these SMEs. The remainder of this paper investigates the connection between this political engagement, fiscal stimulus, and employment outcomes at the firm, state, and aggregate level.

⁷Recall that our analysis focuses on state legislative elections that occurred between 2006 and 2008. These elections pre-date the Citizens United Supreme Court case, which loosened restrictions on corporate campaign contributions.

Figure 3: Campaign contribution & grant shares relative to employment shares, by firm size



Notes: Left figure plots the dollar share of campaign finance contributions relative to the total employment share by firm size, and right figure plots the dollar share of ARRA grants awarded relative to employment share by firm size. 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 Employment: State Level

We start by showing that, controlling for total ARRA expenditure, states that allocated more ARRA grants to politically connected firms created fewer jobs. Our empirical approach exploits geographic variation in ARRA spending to firms and the share of that spending channeled through politically connected firms to identify the effects of both factors on local labor market outcomes. Given the importance of states in allocating ARRA grants, our regional analysis is conducted at the state level.

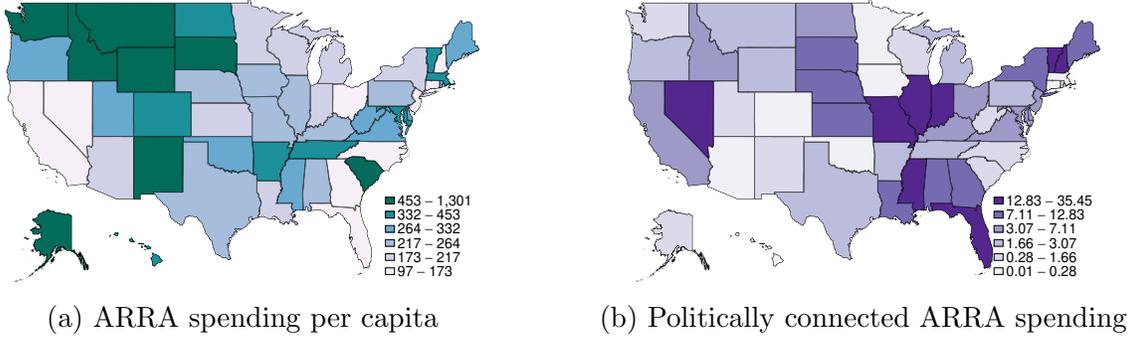
The existing empirical literature uses variation across states in ARRA spending per capita, depicted in [Figure 4a](#), to determine whether states that received more resources per capita created more jobs. Put simply, two states like Illinois and Pennsylvania, which each channeled around \$225 of ARRA stimulus per capita to firms, are expected to save a similar number of jobs in the canonical employment multiplier literature.⁸ This approach abstracts from the impact that the distribution of stimulus spending across firms may have on local employment outcomes. In contrast, we use variation in the fraction of ARRA allocated to politically connected firms, depicted in [Figure 4b](#), to determine whether the jobs multiplier differed in states with a higher fraction of politically connected spending.⁹ In particular, we examine whether the fact that Pennsylvania channeled less than 2.1 percent of ARRA

⁸We define ARRA spending as the resources allocated to firms via grants and contracts. Specifically, it is the total local amount reported in the recipient reports to prime and sub vendors of grants, and to prime- and sub-awardees of contracts.

⁹We define politically connected spending as ARRA grants allocated to firms (as prime vendors) that supported at least one winning candidate in state elections held between 2006 and 2008.

spending through politically connected firms, while Illinois channeled 22.7 percent, mattered for the state-level jobs multiplier.

Figure 4: Cross-state variation in ARRA spending



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.

3.1 Empirical Model

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

$$G_{s,T} = \alpha + \beta_1 A_{s,T}^{pc} + \beta_2 S_{s,T} + \mathbf{X}_{s,0}\Gamma + \varepsilon_{s,T} \quad (1)$$

$$A_{s,T}^{pc} = \delta + \phi_1 IV_{s,0}^{Apc} + \phi_2 IV_{s,0}^S + \mathbf{X}_{s,0}\Theta + \nu_{s,T} \quad (2)$$

$$S_{s,T} = \delta + \phi_1 IV_{s,0}^{Apc} + \phi_2 IV_{s,0}^S + \mathbf{X}_{s,0}\Theta + \nu_{s,T} \quad (3)$$

Equation 1 specifies the second-stage regression, where $G_{s,T} = (E_{s,T} - E_{s,0})/P_{s,0}$ is the change in employment in state s between an initial period ($t = 0$) and an end period ($t = T$), scaled by population. $A_{s,T}^{pc}$ denotes the total ARRA grant and contract spending per capita distributed between $t = 0$ and $t = T$ to firms. $S_{s,T}$ is the share of total ARRA spending per capita given to prime vendor grant awardees that supported at least one winning candidate in state elections held between 2006 and 2008. $\mathbf{X}_{s,0}$ is a set of control variables, all of which are pre-determined in the initial period. Equations 2 and 3 denote two first stage regressions for $A_{s,T}^{pc}$ and $S_{s,T}$, respectively. These regressions incorporate two excluded instruments, $IV_{s,0}^{Apc}$ and $IV_{s,0}^S$, one each for ARRA spending per capita and share allocated to politically connected firms.

To measure the impact of fiscal policy, the literature estimates the marginal effect of ARRA spending on employment, or the jobs multiplier. Under the specification where ARRA only creates jobs through total spending, the jobs multiplier is the number of jobs saved per additional \$1 million spent, or simply β_1 . In our framework, employment is affected by both the additional spending and how that spending is allocated across firms. Specifically, the jobs multiplier is given by $\beta_1 + \beta_2 \left(\frac{\partial A_{s,T}^{pc,c} / \partial A_{s,T}^{pc} - S_{s,T}}{A_{s,T}^{pc}} \right)$, where the term in parentheses captures whether the allocation of additional ARRA spending $\left(\frac{\partial A_{s,T}^{pc,c}}{\partial A_{s,T}^{pc}} \right)$ differs from the existing allocation ($S_{s,T}$). If the allocation is unchanged, the jobs multiplier remains β_1 . If, the allocation changes—say $\left(\frac{\partial A_{s,T}^{pc,c}}{\partial A_{s,T}^{pc}} > S_{s,T} \right)$ —then the sign of β_2 determines whether increasing the share of politically connected spending increases or decreases the jobs multiplier.¹⁰

3.2 Instrumental Variables

Using Recovery Act Recipient Reports data, we calculate the amount of ARRA stimulus disbursed to firms within a state by December 2010, scaled by each state’s working age population in 2009 ($A_{s,T}^{pc}$). We sum the amount allocated to four types of recipients—grant prime vendors, grant sub vendors, contract prime vendors, and contract sub vendors, which adds up to \$71 billion. The total ARRA spending allocated to firms is about 26 percent of total ARRA spending paid out during this period.

Our analysis introduces a second endogenous variable that measures the share of ARRA stimulus disbursed to politically connected firms ($S_{s,T}$). We calculate $S_{s,T}$ as the sum of the amount allocated to grant prime vendors who supported at least one winning candidate during the state legislative elections held in 2006 through 2008 divided by total ARRA stimulus disbursed to firms within a state. We focus on political connections formed during elections held between 2006 and 2008, which determined the state officials who were in office when ARRA funds were disbursed to firms in 2009 and 2010.

We face three sources of endogeneity. First, there is a 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, our OLS results could be biased if the severity of current economic conditions impacted the degree to which firms were able to exert their political influence to obtain ARRA funds. Second, ARRA was in part allocated based on how severely states were impacted by the crisis. Third, states played a role in soliciting funds from the federal government, and states who were more successful in doing so may also have been better managed, and consequently may have had

¹⁰See Appendix A.1.1 for a more formal discussion of the jobs multiplier in our framework.

better economic performance.

We construct two instruments to address these endogeneity concerns.¹¹ The first instrument addresses the endogeneity of $S_{s,T}$ by capturing the potential of firms to attain political connections. In particular, we introduce an indicator denoting whether a state prohibited or permitted corporate campaign contributions in state elections as of 2002. The indicator is based on information from the Federal Elections Commission’s (FEC) Campaign Finance Law 2002 publication. Figure 5 shows that 21 states across the country prohibit corporate campaign contributions in state elections.¹² For example, while Pennsylvania prohibits them, Illinois permits them. The idea is that the formation of political connections via campaign contributions is less likely if the state prohibits them. Because we measure corporate campaign contribution restrictions in 2002, it is unlikely to be associated with either the state’s economic conditions during our analysis period or the firm’s ability to exert influence due to (or in spite of) these economic conditions.

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

3.3 Dependent and Control Variables

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

¹¹Our IV strategy can also address potential bias that arises from measurement errors in the explanatory variables. In particular, politically connected spending may contain measurement errors that stem from opacity in campaign contribution information.

¹²The campaign contribution restrictions we capture with our instrument pertain to corporations. Contributions by unincorporated businesses are treated differently.

ment Statistics (CES) data on total statewide, non-farm, seasonally adjusted employment, and working age population data is obtained from the United States Census Bureau.

We introduce eight control variables to our baseline specification because they are potentially correlated with employment growth, ARRA spending, and our instruments. All control variables are measured before the initial period. We share five control variables in common with [Wilson \(2012\)](#). To account for states' initial employment situation, we control for employment-to-population ratio in 2009 and lagged employment growth between December 2007 and February 2009. We account for the fact that the run-up in house prices is correlated with the depth of the subsequent crisis and possibly with formula factors used in the construction of our DOT instrument by controlling for the change in the house price index between 2003Q4 and 2007Q4. We also control for two sources of ARRA stimulus not channeled through firms. We measure the change between 2004 and 2006 in the three-year trailing average of personal income per capita because ARRA provided fiscal stimulus to states using a formula that explicitly factored in the change in average personal income per capita. Tax relief to state residents is controlled for by summing the state share of people eligible for the payroll tax cut multiplied by the total nation cost of the payroll tax cut and the state share of AMT payments in 2007 multiplied by the total nation cost of the AMT adjustment. Additionally, to account for region-specific employment trends, we also control for Census Division fixed effects.

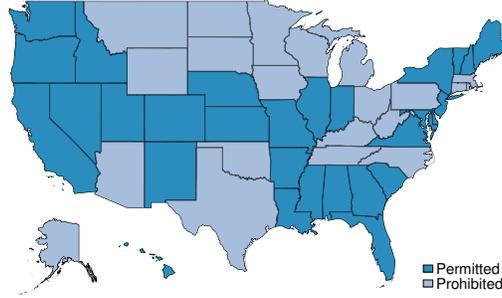
The last two variables account for potential omitted factors correlated with political influence and state level employment growth. We control for the fraction of employees in each state that are union members in 2008. The reasoning is that labor unions may exert their political influence to shape campaign finance laws, and the prevalence of labor unions may affect the degree of its labor market flexibility. We also control for state corruption, measured as an indicator variable that differentiates between states above/below the median in terms of the average number of officials convicted of corruption-related cases per capita between 1976 and 2002 ([Glaeser and Raven, 2006](#)). We do so because the degree of political corruption in a state could be related to its campaign finance regulations as well as the speed at which the state can recover from recessions. ¹³

3.4 Baseline Results

Columns (1) and (2) of [Table 1](#) report the first stage results for ARRA spending per capita and fraction of politically connected ARRA spending, respectively. We consider anticipated DOT spending per capita as the instrument for ARRA spending per capita and

¹³Summary statistics for the variables used in our baseline are shown in [Table B.1](#) in [Appendix B.2](#).

Figure 5: Corporate campaign contribution limits



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

the campaign contribution indicator as the instrument for fraction of politically connected spending. While anticipated DOT spending is positively correlated with both endogenous variables, the campaign contribution indicator is only positively associated with the endogenous variable it is instrumenting for, fraction of politically connected spending.

Our baseline second stage result, reported in column (1) of Table 2, shows that while ARRA saves jobs, increasing the share of politically connected spending dampens the jobs multiplier. When the marginal million in ARRA spending is allocated according to the cross sectional mean ($\frac{\partial A_{s,T}^{pc,c}}{\partial A_{s,T}^{pc}} = \bar{S}_T$), 27.2 jobs are saved for every additional \$1 million in ARRA spent. If instead, we allow the allocation of that same marginal million dollar to be biased towards connected firms by 10 percentage points ($\frac{\partial A_{s,T}^{pc,c}}{\partial A_{s,T}^{pc}} = \bar{S}_T + 0.1$), the job multiplier decreases to 19.1 jobs. Increasing the share of politically connected spending by 10 percentage points, which is slightly higher than one standard deviation of 8.7 percentage points, above the mean reduces the jobs saved per \$1 million in ARRA spent by 8.1 jobs, or by 30 percent.

In column (2) of Table 2, we show that excluding the corruption indicator and union membership has little impact on the coefficients of ARRA spending and share of politically connected spending, which provides support for the exogeneity of our campaign contribution IV.¹⁴ Further, the third to last row of the table reports the first-stage F-statistic. We check for possible weak instrument bias by comparing the first-stage F-statistic with critical values obtained by Stock and Yogo (2005). The F-statistics fall between the 10 percent and 15 percent significance level critical values.

¹⁴Tables A.1 and A.2 in Appendix A.1.2 report the coefficients for the full set of controls.

Table 1: Baseline: First stage results

	(1)	(2)
	ARRA spending pc	Frac. connected spending
DOT spending pc (ths)	1.664*** (0.544)	0.503** (0.191)
Corp contrib (indicator)	0.012 (0.032)	0.143*** (0.024)
Full controls	Yes	Yes
Division FE	Yes	Yes
Obs.	50	50
R-sq	0.74	0.69

Notes: The dependent variable in column (1) is ARRA funding allocated to firms and in column (2) is the share of the spending allocated as prime vendor grants to politically connected firms. The variables of interest are the excluded instruments in the second stage—anticipated DOT spending per capita and an indicator of whether a state permits corporate campaign contributions. The full set of controls include division fixed effects, prior employment growth, initial employment p.c., house price growth between 2003 and 2007, change in personal income before the crisis, expected tax benefits p.c., union membership, and corruption indicator. ***, **, and * indicate sig. at the 1%, 5%, and 10% sig. levels. Robust SEs.

Table 2: Baseline: Second stage results
Dependent variable: change in emp-pop ratio, Feb 09 - Dec 10

	(1)	(2)
	Baseline	Drop union & corr controls
ARRA spending pc	27.17** (11.03)	24.56** (11.19)
Frac connected spending	-0.0252** (0.0128)	-0.0220* (0.0113)
ARRA controls	Yes	Yes
Connected controls	Yes	No
Division FE	Yes	Yes
F-stat	5.420	6.096
Obs.	50	50
R-sq	0.39	0.40

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

3.5 Robustness

Our identification strategy relies on instrumenting the spatial distribution of ARRA spending and the degree of firms' political connections. Omitted factors that are correlated with the instruments and also with the outcome variable could challenge our identification.

Table A.3 in Appendix A.1.3 explores omitted factors that the existing literature has explored as potentially being correlated with state employment growth and the DOT instrument. We show that accounting for state industrial composition, change in house prices during the housing bust, and anticipation of the passage of ARRA stimulus do not qualitatively or quantitatively affect our results.

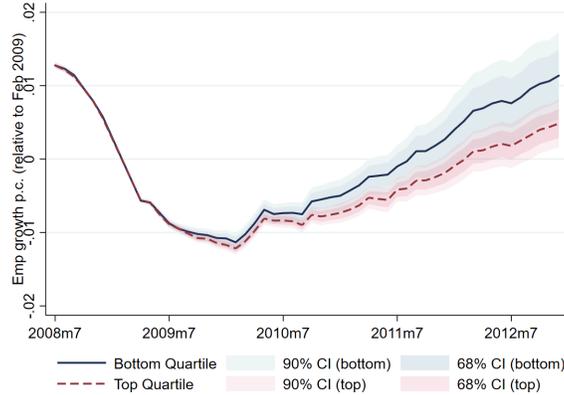
Table A.4 shows that introducing alternative proxies for labor market flexibility and political environment does not qualitatively or quantitatively alter our baseline results. Specifically, we show that our baseline results hold if we measure labor influence using an indicator of whether the state passed right to work legislation; measure political environment using state managerial capacity scores from the Maxwell School’s Government Performance Project; or base our campaign contribution instrument on 2008 state election laws.

Finally, table A.5 shows that accounting for possible correlation between employment growth, contribution limits, state and firm characteristics (age, size, and productivity), does not alter our baseline results. In particular, we explore the possibility that features of the political environment, such as the lower and upper houses of the state legislature being controlled by different parties and being a swing state, may affect both campaign finance laws and the pace of economic recovery. We examine if corporate and individual campaign contributions are substitutes in states with strict corporate campaign contribution limits by controlling for the strictness of states’ individual campaign contribution limits. We account for the possibility that older and larger firms may have more resources to advocate for looser campaign finance laws, and at the same time may experience a different pace of recovery.

3.5.1 Differential Evolution of Employment Growth

We now examine whether the share of politically connected spending has a persistent effect on employment growth. We first estimate our baseline IV specification redefining the dependent variable as the change in employment between February 2009 and each month from July 2008 until December 2012. We then evaluate the predicted employment growth at each point in time for the bottom and top quartiles of the share of politically connected spending, evaluating all other variables at their means. In addition to predicted employment growth, we also report the 90 percent and 68 percent confidence intervals. Figure 6 shows that in December 2010, employment growth of the bottom quartile is 0.24 percentage points higher than the top quartile. By December 2012, employment growth of the bottom quartile is 0.65 percentage points higher than the top quartile.

Figure 6: Employment Growth by Degree of Political Connectedness



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

4 Political Connections and Employment: Firm Level Evidence

To establish more direct, micro-level evidence consistent with our previous finding and to shed light on the underlying mechanism, we turn to firm-level data. In this section, we show that gaining political connections increases the firms’ probability of winning stimulus grants, but politically connected firms create fewer jobs after receiving grants compared to their non-connected counterparts.

4.1 Political Connections and Grant Allocation

We first show that gaining political connections to state legislators has a positive impact on firms’ probability of winning grants. By identifying this causal effect, we verify that the type of political connection we measure is operative and relevant in the context of fiscal stimulus. To achieve this, we need to overcome potential endogeneity concerns: firms’ political connections are heavily influenced by their characteristics such as size and industry, as well as unobserved factors such as superior access to political information, all of which could also be strongly correlated with attainment of government grants. Therefore, we use plausibly exogenous variation in political connections by exploiting close elections for state legislatures. Our assumption is that the outcome of a close election is difficult to predict and largely determined by random factors (Lee, 2008). We follow the literature in defining a close election as one won by a five 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).

As mentioned in the previous section, we focus on state legislative elections held between 2006 and 2008, with terms lasting until at least 2010. Our close election sample encompasses 629 elections across 48 states, or 10 percent of all elections held during this period (see Figure A.1 for the distribution of margin of victory). On average, there were nearly 13 close elections in a state and they were not concentrated in any particular region or in swing states, as shown in Figure A.2. Among firms participating in close elections, nearly 67 percent contributed to only one candidate.¹⁵ Supporting both top candidates in the same election is rare, occurring in only 1.1 percent of firm-election pairs.¹⁶

Because a firm can secure political connections to more than one legislator in a state, firms’ political connections gained through elections vary at the firm-state-politician level. Meanwhile, the outcome variable of interest, that indicates whether a firm receives an ARRA grant in a state, is defined at the firm-state level. Therefore, we aggregate firms’ political connections to the firm-state level. Such aggregation turns out to be also useful for employment growth analysis in Section 4.2.

Specifically, we construct $Frac(Win)_{i,s}$ as the number of close election winners supported by firm i in state s , divided by the number of close election candidates supported by firm i in state s . That is,

$$Frac(Win)_{i,s} = \frac{\sum_j (Supported_{i,s,j} \times Win_{s,j})}{\sum_j Supported_{i,s,j}} \quad (4)$$

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 dummy variable, $Connected_{i,s}$, that takes a value of one if $Frac(Win)_{i,s}$ is greater than or equal to 0.5. Our objective is to compare the grant outcomes of firms that randomly gained large political connections in state s with those of less-connected firms in the same state. For example, if a firm supported one candidate in a close election, $Connected_{i,s}$ is 1 if that candidate won the election and zero otherwise. If the firm supported two candidates in close elections, $Connected_{i,s}$ is 1 if one or both of the candidates won their election and zero if neither did.

¹⁵Table B.2 shows the full distribution of the number of candidates firms support in close elections.

¹⁶Simultaneously supporting competing candidates, so-called “hedging”, is rarely observed in other election settings as well (for example, see Akcigit et al., 2023). In the results reported here we drop hedging cases, but our results are also robust to their inclusion.

We compare the two groups of firms by running the following regression:

$$Y_{i,s} = \beta_0 + \beta_1 \text{Connected}_{i,s} + \gamma' X_{i,s} + \epsilon_{i,s} \quad (5)$$

$Y_{i,s}$ indicates whether firm i receives a grant in state s and zero otherwise and $X_{i,s}$ is a vector of control variables. In our baseline specification, we control for the number of candidates firm i supported in close elections in state s ($NumCandCE_{i,s}$). If we were to compare a firm that supported 20 candidates, for example, with one that supported only two, we would expect that the former firm would gain more connections on average. Because unobserved factors which drove the firm to support more candidates may be correlated with grant outcomes, it is important to control for $NumCandCE_{i,s}$. We include state fixed effects so that we compare a firm with strong connections in state A to a firm with weak connections in state A, not in state B. Because the amount of ARRA spending received and the level of engagement in political activities systematically differ across industries, we control for the industry of the firms. Under our identifying assumption, given these controls, $Connected_{i,s}$ is uncorrelated with the error term.

As supporting evidence for this assumption, we show in Table 3 that there are no statistically significant differences between connected and non-connected firms in a state in their observable characteristics. We examine firm size, firm age and the total number of candidates that a firm supported in the state. The latter variable captures the overall engagement of the firm in state politics. We also examine whether the firm’s headquarter is located in the state because local firms may have better information on the state’s political environment. Finally, we consider firm credit scores because financially distressed firms may have stronger incentive to make connections with politicians (Adelino and Dinc, 2014).

Table 3: Balance of Characteristics

	(1)	(2)	(3)	(4)	(5)
	Firm Size	Firm Age	Total Num. Cand.	Headquarter	Credit Scores
Connected	0.011 (0.072)	-0.115 (0.158)	0.020 (0.036)	0.002 (0.008)	-0.116 (0.230)
NAICS4 FE & State FE	Yes	Yes	Yes	Yes	Yes
NumCandCE FE	Yes	Yes	Yes	Yes	Yes
Obs.	9965	9965	9965	9965	7378
R-sq	0.65	0.34	0.76	0.40	0.22

Notes: *Connected* indicates whether 50% or more of candidates a firm supported in close elections won the election in a state and *NumCandCE* refers to the number of candidates firms supported in close elections in a state. *Firm size* is measured by log employment, *Total Num. Cand.* is the log of total number of candidates a firm supported in a state, *Headquarter* indicates whether a firm’s headquarter is located in a given state, and credit scores are measured by Paydex scores. Standard errors are clustered two ways, by state and industry, and the results are robust to not clustering the standard errors.

Table 4 shows that gaining political connections has a positive and statistically significant effect on the probability of winning the grant. Our baseline specification (col. 1) indicates that a stronger political connection increases the chances of winning a grant by 0.56 percentage points. To interpret the estimated effect, it is important to note that grant allocation is heavily concentrated in a small share of firms.¹⁷ Among the control group, the mean probability of winning a grant is 3.2 percent, implying that the estimated marginal treatment effect of stronger political connections is a 18 percent increase in the probability of winning a grant.

In the remaining four columns, we analyze heterogeneous treatment effects by interacting *Connected* with some characteristics of the firm, denoted as *HetVar*, to better understand the mechanism. In Column (2), we ask whether the connection has a stronger effect if a firm is the main donor—defined as being one of the top 5 percent of contributors—for the majority of the candidates it supported. We find that main donors receive even larger benefits from being connected, as can be seen from the interaction term, and being a main donor by itself does not increase the chances of winning a grant if the firm is not connected. These results are consistent with grants at least partly working through quid pro quo. In Column (3), we test whether the effects are stronger if a firm has mostly supported candidates in the majority party in a given state, but we do not find such evidence. Therefore, it appears that it is sufficient to make connections to individual politicians, regardless of their party affiliations, to receive the benefits. In Columns (4) and (5), we also interact *Connected* with whether a firm is large (more than 5,000 employees) or with its age. We do not find any evidence that political connections have stronger effects for large or older firms, though such firms are generally more likely win grants even without political connections.

We also conduct several robustness checks, the results of which are reported in Table A.7 in the appendix. First, we run a placebo regression to show that being connected to legislators in a given state has no impact on receiving grants in *other* states, as state legislators can only exert influence over grant allocation in their own states. Second, we also show that being treated in a given state does not have a significant impact on receiving grants in the same state as a sub vendor, because sub vendors are chosen by local governments (e.g., cities or counties) and thus state legislators are likely to play only a limited role, if any, in the allocation of grants to sub vendors. These results support our identifying assumption. Third, we show that our main result is robust to using a tighter margin of victory in defining close elections. Lastly, we show that our main result is robust to using $Frac(Win)$ defined in Equation (4) instead of *Connected*.

¹⁷The mean probability of winning a grant in our sample is 3.7 percent. Cox et al. (2020) documents similar evidence that federal procurement contracts are concentrated in a small fraction of firms.

Table 4: The Effect of Political Connections on Winning a Grant

	(1)	(2)	(3)	(4)	(5)
	Win	Win	Win	Win	Win
Connected	0.561** (0.241)	0.584** (0.267)	0.583** (0.199)	0.540** (0.250)	0.618** (0.197)
Connected × HetVar		2.683** (1.153)	-0.401 (0.319)	-0.331 (1.839)	0.057 (0.035)
HetVar		-1.175 (0.794)	0.617 (0.539)	9.102*** (2.212)	0.178*** (0.052)
Constant	3.185*** (0.141)	3.178*** (0.151)	3.165*** (0.116)	2.032*** (0.270)	3.188*** (0.115)
NAICS4 x State FE	Yes	Yes	Yes	Yes	Yes
NumCandCE FE	Yes	Yes	Yes	Yes	Yes
HetVar	None	Main Donor	Majority Party	Large Firm	Firm Age
Obs.	9965	9965	9965	9965	9965
R-sq	0.34	0.34	0.34	0.35	0.34

Notes: Unit of analysis is firm × state. The dependent variable, *Win*, indicates whether a firm received a grant in a state as a prime vendor, multiplied by 100 for ease of interpretation. *Connected* indicates whether 50% or more of candidates a firm supported in close elections won the election in a state and *NumCandCE* refers to the number of candidates firms supported in close elections in a state. Additional controls include firm size and whether a firm’s headquarter is located in a given state. ***, **, and * indicate sig. at the 1%, 5%, and 10% sig. levels. Standard errors are clustered two ways, by state and industry.

4.2 Differential Employment Effects

We lever the exogenous variation in political connections obtained from close elections to show that politically connected firms create fewer jobs after winning an ARRA grant compared to their non-connected counterparts. To do so, we run an event study regression to trace the evolution of employment around when a firm wins an ARRA grant from a state. We also introduce interaction terms to examine the heterogeneous treatment effects with respect to whether a firm is politically connected in a state. Specifically, we estimate the following regression equation:

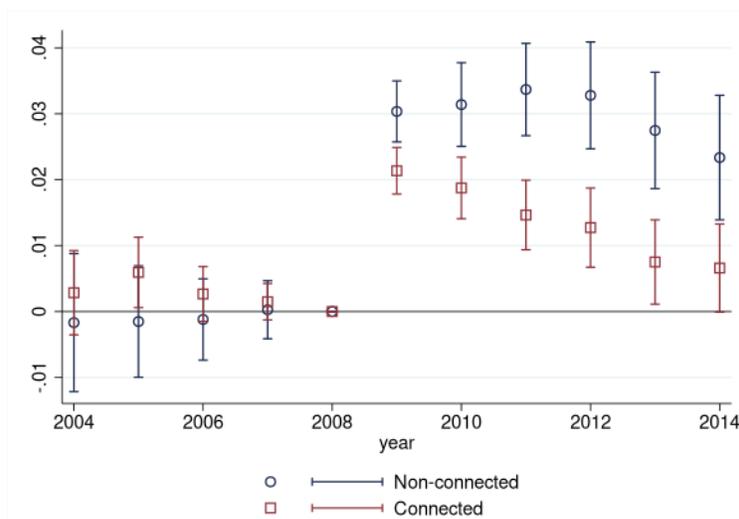
$$\begin{aligned}
 Y_{e,f,s,j,t} = & \sum_{k=2004}^{2014} \delta_k I_{(k=t)} \times Grant_{f,s} + \sum_{k=2004}^{2014} \gamma_k I_{(k=t)} \times Grant_{f,s} \times Connected_{f,s} \\
 & + \sum_{k=2004}^{2014} \lambda_k I_{(k=t)} \times Connected_{f,s} + \alpha_e + \eta_{j,t} + \epsilon_{e,f,s,j,t}.
 \end{aligned} \tag{6}$$

The outcome variable $Y_{e,f,s,j,t}$ is log employment, where e is establishment, f is the firm that owns the establishment, s is state, j is industry and t is year. $Grant_{f,s}$ indicates whether firm f won an ARRA grant in state s , and $Connected_{f,s}$ indicates whether firm f is politically connected in state s , as defined in the Section 4.1. $I_{(k=t)}$ is a dummy that takes the

value of one if $k = t$, where 2008 is set as the base year in the estimation. α_e is establishment fixed effect, which controls for any time-invariant characteristics of establishments and firms, which include the fixed effects used in Table 4 (i.e., industry, state and the number of candidates supported in close elections). $\eta_{j,t}$ indicates industry by year fixed effect.¹⁸

Figure 7 displays the coefficients of interest, δ_k and $\delta_k + \gamma_k$, which show the difference in employment between grant winners and non winners among non-connected and connected firms, respectively. The results indicate that establishments in non-connected firms increase their employment by 3 percent in the first year after winning grants relative to non-connected firms that did not win grants, while their connected counterparts increase their employment only by 2 percent. Also, the difference in employment growth persists for at least six years. Note that we do not find any non-parallel trends in employment prior to 2009, lending support to the difference-in-difference specification. In addition, we do not find any statistically significant difference in establishment size in 2008 or the average grant value between the connected and non-connected firms in the sample.

Figure 7: Establishment-level Employment after winning an ARRA Grant



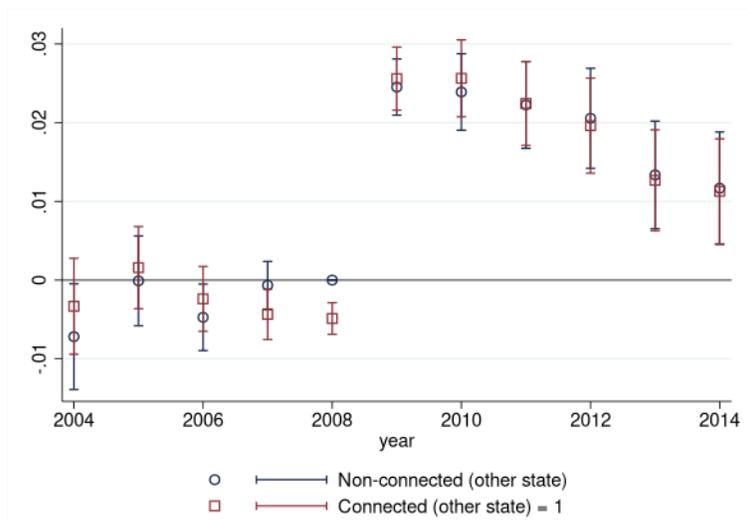
Notes: Unit of analysis is establishment \times year. Displays the employment growth of establishments in state s that belong to firms that won grants in state s and did not gain political connections (blue line) and of establishments in a state the belong to firms that won grants in that state and did gain political connections (red line).

To verify whether it is indeed the political connections that create the employment growth differences, we conduct a placebo test. While our baseline regression tests whether having a

¹⁸Crane and Decker (2020) document that employment dynamics in NETS are subject to large measurement errors, mostly due to the prevalent imputation of employment records among small establishments. Because imputations occur at the establishment level and a firm can have multiple establishments, we conduct our analysis at the establishment level, rather than firm level. We further minimize the impact of outliers by trimming the observations at 1% and removing establishments that exhibit spurious jumps in employment (Díez et al., 2021).

political connection in state s weakens the employment effect of winning a grant in state s , the placebo regression tests whether having a political connection in state $z \neq s$ weakens the employment effect of winning a grant in state s .¹⁹ Figure 8 show no significant differences in employment growth. If our baseline results were not driven by political connections, but rather by unobserved firm characteristics associated with both political engagement and growth such as productivity, we would expect to find differential evolution of employment as exhibited in Figure 7.

Figure 8: Employment Growth: Placebo



Notes: Unit of analysis is establishment \times year. Displays the employment growth of establishments in state s that belong to firms that won grants in state s and did not gain political connections in states $z \neq s$ (blue line) and of establishments in state s the belong to firms that won grants in state s and *did* gain political connections in states $z \neq s$ (red line). To accommodate firms owning establishments and participating in close elections in many states, we create copies of each establishment (operating in state s) record, one for each of the other states ($z \neq s$) in which the firm participated in close elections and owns establishments. Observations are weighted so that each establishment has a weight of one in the estimation.

5 A General Equilibrium Model

Our empirical analysis shows that politically connected firms create fewer jobs after winning government grants, which weakens the local employment effect of fiscal stimulus. To better understand the channels through which this occurs and to analyze whether the state- and firm-level results continue to hold in the aggregate, we build a quantitative general

¹⁹In practice, we estimate a version of regression 6, where for each establishment e , owned by firm f , operating in state s at time t , we change the value of $Connected_{f,s}$ to $Connected_{f,z}$, where $z \neq s$ represents a state different than state s in which firm f also participated in close elections and owns establishments. To accommodate cases in which firms own establishment and participate in close elections in many states, we create multiple copies of an establishment's (operating in state s) record, one for each of the other states in which the firm owns establishments and participated in close elections. Regressions are weighted so that each focal establishment has a weight of one in the estimation.

equilibrium model.

We introduce a two sector extension of the New Keynesian model of [Nakamura and Steinsson \(2014\)](#). The model consists of two regions that belong to a monetary and fiscal union. We refer to the regions as home (H) and foreign (F), and a fraction n of the population lives in region H while a fraction $1 - n$ are located in region F . Regions are indexed by r , with $r = \{H, F\}$. We extend the model by considering two sectors in each region, M and m , and sectors are indexed by s , with $s = \{M, m\}$. In each region, there is a continuum of firms indexed by $z \in [0, 1]$ and each firm belongs to one of the two sectors. The measure of each sector is denoted by μ_M and $\mu_m = 1 - \mu_M$, respectively.

Sector M —the politically connected sector—and sector m —the non-connected sector—differ along two dimensions that represent the two channels through which politically connected spending could affect the jobs multiplier. A benign explanation is that connected firms are more productive, and thus better able to afford the costs associated with building political connections ([Kerr et al., 2014](#)). Under this scenario, connected firms can produce goods and services specified in grants with fewer workers. The model embeds this mechanism by allowing firms in sector M to be more productive. An alternative explanation, rooted in inefficiency, is that connections to politicians give firms leverage to charge higher markups to the government.²⁰ Under this hypothesis, politically connected firms extract a higher profit share and employ fewer workers. The model embeds this mechanism by allowing firms in sector M to charge higher markups.²¹

We assume that (region \times sector) government spending is exogenous and stochastic. This assumption is consistent with our empirical design in which firms formed political connections prior to the passage of ARRA. Therefore, political connections are exogenous to fiscal policy. Similarly, our model takes the size of the politically connected sector (μ_M) as exogenous.

²⁰Using heterogeneous markups to model the second channel is consistent with our empirical results, as well as those from [Brogaard et al. \(2021\)](#). If political connections increase the probability of winning government contracts for any price, then effectively, the demand faced by these firms becomes less elastic and they can charge higher markups. Indeed, [Fan and Zhou \(2023\)](#) show that gaining political connections increases firms' markups.

²¹While both channels may be active in reality, it is important to note that our empirical results in Section 4 are consistent with the markup-based explanation rather than the productivity-based explanation. In our firm level analysis, we extract a component of political connections that is not correlated with firm characteristics by using quasi-random variation in close elections. We still find in Section 4.2 that politically connected firms create fewer jobs after winning grants. We also find in Table 4 that the effect of *Connected* on grant allocation is larger when firms are important donors, but not when they are older or larger, both of which are characteristics positively associated with higher productivity. It is still important to allow for differential productivity in the model because the model is designed to be more general than the close elections environment we study empirically.

5.1 Model Description

5.1.1 Households

The representative consumer at home seeks to maximize their utility given by:

$$E_0 \sum_{t=0}^{\infty} \beta^t \left[\frac{C_t^{1-\sigma^{-1}}}{1-\sigma^{-1}} - \chi \frac{L_{H,t}^{1+\nu^{-1}}}{1+\nu^{-1}} \right] \quad (7)$$

where β denotes the household's subjective discount factor, C_t denotes household consumption (per capita) of a composite good, $L_{H,t}$ is the quantity of labor supplied, σ is the intertemporal elasticity of substitution, ν is the Frisch-elasticity of labor supply, and χ is the disutility of supplying labor. We assume a region-wide labor market with perfect wage equalization across sectors.²²

The composite (per capita) consumption good in expression 7 is an index given by:

$$C_t = \left[\phi_H^{\frac{1}{\eta_r}} C_{H,t}^{\frac{\eta_r-1}{\eta_r}} + \phi_F^{\frac{1}{\eta_r}} C_{F,t}^{\frac{\eta_r-1}{\eta_r}} \right]^{\frac{\eta_r}{\eta_r-1}} \quad (8)$$

where $C_{H,t}$ and $C_{F,t}$ denote the (per capita) consumption of composites of home and foreign produced goods, respectively. The parameter $\eta_r > 0$ denotes the elasticity of substitution between home and foreign goods, and ϕ_H and $\phi_F = 1 - \phi_H$ are preference parameters that determine the household's relative preference for home and foreign goods.

Variables $C_{H,t}$ and $C_{F,t}$, are given by

$$C_{r,t} = \left[\phi_{r,M}^{\frac{1}{\eta_s}} C_{r,M,t}^{\frac{\eta_s-1}{\eta_s}} + \phi_{r,m}^{\frac{1}{\eta_s}} C_{r,m,t}^{\frac{\eta_s-1}{\eta_s}} \right]^{\frac{\eta_s}{\eta_s-1}}, \quad (9)$$

where $C_{H,M,t}$ and $C_{H,m,t}$ ($C_{F,M,t}$ and $C_{F,m,t}$) denote the (per capita) consumption of composites of home (foreign) produced goods by politically connected and non-connected firms, respectively. The parameter $\eta_s > 0$ denotes the elasticity of substitution between high and low markup baskets. The parameters $\phi_{H,M}$ and $\phi_{H,m} = 1 - \phi_{H,M}$ ($\phi_{F,m}$ and $\phi_{F,m} = 1 - \phi_{F,m}$) are preference parameters that determine the household's relative preference for home (foreign) goods produced by politically connected and non-connected firms, respectively.

The sectoral baskets, $C_{r,s,t}$, are given by

$$C_{r,s,t} = \left[\left(\frac{1}{\mu_s} \right)^{\frac{1}{\theta_s}} \int_0^{\mu_s} c_{r,s,t}(z)^{\frac{\theta_s-1}{\theta_s}} dz \right]^{\frac{\theta_s}{\theta_s-1}}, \quad (10)$$

²²For more details see [Woodford \(2003\)](#) and [Carvalho et al. \(2021\)](#).

where $c_{H,M,t}(z)$ and $c_{F,M,t}(z)$ ($c_{H,m,t}(z)$ and $c_{F,m,t}(z)$) denote (per capita) consumption of variety z of type M (m), of connected (non-connected) home and foreign produced goods, respectively. There is a continuum of measure one of varieties in each region. The parameters $\theta_M > 1$ and $\theta_m > 1$ denote the elasticity of substitution between different varieties for each sector (connected and non-connected). This differential elasticity captures the inefficient channel through which political connections lead to lower employment growth.

Goods markets are completely integrated across regions. Home and foreign households face the same prices for each of the differentiated goods produced in the economy. We denote these prices by $p_{H,M,t}(z)$ and $p_{H,m,t}(z)$ for home produced goods by connected and non-connected firms, respectively, and $p_{F,M,t}(z)$ and $p_{F,m,t}(z)$ for foreign produced goods by connected and non-connected firms, respectively. All prices are denominated in a common currency called dollars.

Households have access to complete financial markets. There are no impediments to trade in financial securities across regions. The representative home household faces a flow budget constraint (per capita) given by:

$$P_t^C C_t + E_t [M_{t,t+1} B_{t+1}] \leq B_t + W_{H,t} L_{H,t} + \int_0^1 \Xi_{H,t}(z) dz - T_t, \quad (11)$$

where P_t^C is a price index that gives the minimum price of a unit of the consumption good C_t , B_{t+1} is a random variable that denotes the state contingent payoff of the portfolio of financial securities held by households at the beginning of period $t + 1$ (financial markets are complete and perfect, full insurance), $W_{H,t}$ denotes the nominal wage in region H and period t , $\Xi_{H,t}(z)$ is the profit of home firm z from region H in period t , and T_t denotes lump sum taxes. To rule out Ponzi schemes, household debt cannot exceed the present value of future income in any state of the world.²³

5.1.2 The Government

The economy has a government that conducts fiscal and monetary policy. We assume that the deviation of government spending from its steady state value as a fraction of per capita output in the steady state, $\hat{g}_{r,s,t} = \frac{G_{r,s,t} - G_{r,s}}{Y_{r,s}}$, follows an exogenous process, where $G_{r,s,t}$ denotes government spending per capita in sector s in region r in time t . To ensure that the fraction of government spending allocated to each sector is well-defined, we assume

²³The problem of the household in the second region is largely analogous. The relevant price indices for each consumption basket are given by $P_{r,s,t} = \left[\frac{1}{\mu_s} \int_0^{\mu_s} p_{r,s,t}(z)^{1-\theta_s} dz \right]^{\frac{1}{1-\theta_s}}$, $P_{r,t}^C = \left[\phi_{r,M} (P_{r,M,t})^{1-\eta_s} + \phi_{r,m} (P_{r,m,t})^{1-\eta_s} \right]^{\frac{1}{1-\eta_s}}$, and $P_t^C = \left[\phi_H (P_{H,t}^C)^{1-\eta_r} + \phi_F (P_{F,t}^C)^{1-\eta_r} \right]^{\frac{1}{1-\eta_r}}$.

that the log of $\hat{g}_{r,s,t}$ follows an AR (1) process so that $\hat{g}_{r,s,t}$ is positive in our simulations. That is,

$$\log(\hat{g}_{r,s,t}) = \rho_{r,s} \log(\hat{g}_{r,s,t-1}) + \epsilon_{r,s,t}, \quad \epsilon_{r,s,t} \sim N(\phi_s, \sigma_s^2). \quad (12)$$

Note that total government spending in sector s in the home region is $nG_{H,s,t}$. Within a sector-region, the government is assumed to consume the same basket and faces the same prices as the household.

The government levies lump-sum taxes to pay for its purchases of goods. Our assumption of perfect financial markets implies that any risk associated with variation in lump-sum taxes and transfers across the two regions is undone through risk-sharing. Moreover, the model features Ricardian equivalence. The government's budget is:

$$n(P_{H,M,t}G_{H,M,t} + P_{H,m,t}G_{H,m,t}) + (1-n)(P_{F,M,t}G_{F,M,t} + P_{F,m,t}G_{F,m,t}) = T_t. \quad (13)$$

The government operates a common monetary policy for the two regions. This policy consists of the following augmented Taylor-rule for the economy-wide nominal interest rate:

$$\hat{r}_t^n = \rho_r \hat{r}_{t-1}^n + (1 - \rho_i) (\phi_\pi \hat{\pi}_t^{ag} + \phi_y \hat{y}_t^{ag} + \phi_g \hat{g}_t^{ag}), \quad (14)$$

where hatted variables denote percentage deviations from steady state. The nominal interest rate is denoted by \hat{r}_t^n . It responds to variation in the weighted average of consumer price inflation in the two regions $\hat{\pi}_t^{ag} = n\hat{\pi}_t^C + (1-n)\hat{\pi}_t^{C*}$, where $\hat{\pi}_t^C$ is consumer price inflation in the home region and $\hat{\pi}_t^{C*}$ is overall price inflation in the foreign region. It also responds to variation in the weighted average of output in the two regions $\hat{y}_t^{ag} = n(P_H Y_H)/(PY)\hat{y}_{H,t} + (1-n)(P_H Y_H)/(PY)\hat{y}_{F,t}$. Finally, it may respond directly to the aggregate government spending $\hat{g}_t^{ag} = n\hat{g}_{H,t} + (1-n)\hat{g}_{F,t}$.²⁴

5.1.3 Firms

There is a continuum of firms indexed by $z \in [0, 1]$ in the home region. Each firm belongs to one of two sectors—politically connected (M) or non-connected (m). Firm z in sector s specializes in the production of differentiated good z , the output of which we denote $y_{H,s,t}(z)$. The per capita production function of firm z in sector s is:

$$\frac{y_{H,s,t}(z)}{n} = A_{H,s} L_{H,s,t}(z)^a \quad (15)$$

²⁴The quantitative results also hold under a constant nominal or real interest rate monetary policy rule.

where $L_{H,s,t}(z)$ denotes the (per capita) hours of labor that firm z in sector s in the home region employs, and $A_{H,s}$ is a sector-specific productivity parameter. It is through heterogeneity in productivity across sectors that the model captures the benign channel through which politically connected spending may lower the jobs multiplier.

Firm z in sector s acts to maximize its value:

$$E_t \sum_{j=0}^{\infty} M_{t,t+j} \left[p_{H,s,t+j}(z) \frac{y_{H,s,t+j}(z)}{n} - W_{H,s,t+j}(z) L_{H,s,t+j}(z) \right] \quad (16)$$

Firm z must satisfy demand for its product. The demand for firm z 's product comes from three sources: home consumers, foreign consumers and the government. Firm z is therefore subject to the following constraint:

$$\frac{1}{n} \frac{1}{\mu_s} \left[n C_{H,s,t} + (1-n) C_{H,s,t}^* + n G_{H,s,t} \right] \left(\frac{p_{H,s,t}(z)}{P_{H,s,t}} \right)^{-\theta_s} \leq A_{H,s} L_{H,s,t}(z)^a, \quad (17)$$

Firms take regional wages as given and face pricing frictions à la [Calvo \(1983\)](#). Therefore, each period, firm z can re-optimize its price with probability $1-\alpha$, or keep its price unchanged with probability α . Optimal price setting when firm z is allowed to change its price is

$$p_{H,s,t}(z) = \frac{\theta_s}{\theta_s - 1} E_t \sum_{j=0}^{\infty} \frac{\alpha^j M_{t,t+j} \frac{y_{H,s,t+j}(z)}{n}}{E_t \sum_{k=0}^{\infty} \alpha^k M_{t,t+k} \frac{y_{H,s,t+k}(z)}{n}} MC_{H,s,t+j}(z), \quad (18)$$

where $MC_{H,s,t}(z)$ denotes the firm's nominal marginal cost (the Lagrange multiplier on equation (17) in the firm's constrained optimization problem) and $M_{t,t+1}$ is the stochastic discount factor of the household. Intuitively, when firm z can change its price, the firm sets it equal to a constant markup over a weighted average of current and future marginal costs. Heterogeneous markups across sectors allow the model to capture the inefficient channel through which politically connected spending impacts employment growth.

5.2 Quantitative Analysis

The model is solved by first order perturbations around its steady state using [Gensys \(Sims, 2002\)](#). Online Appendix [B.4](#) shows the full system of linear equations that characterizes the model. Our calibration closely follows [Nakamura and Steinsson \(2014\)](#). To match the average population of a U.S. state, n is set to 2 percent. To focus on relative productivity, we normalize $A_{r,m}$ to 1 in both regions. We assume the same elasticity of substitution within and across regions ($\eta_r = \eta_s = 2$), taking the calibrated value from [Nakamura and Steinsson \(2014\)](#). Consistent with the fraction of politically connected firms in the data, the share of

firms in the connected sector (μ_M) is set to 1.17 percent. We set persistence of government spending process ($\rho_{r,s}$) to be 0.9 for all sector-regions. The remaining parameters are specific to our model, and we calibrate them to match salient moments of the data.

Table 5: Internally Calibrated Parameters

Parameter	Description	Value	Targeted Moment	Data	Model
A_M	productivity M	5.04	employment share of M	28.27%	28.27%
$\phi_{r,M}$	preference M	38.45%	basket price ratio	1.00	1.00
χ	labor disutility	1.50	employment to pop.	60.60%	60.60%
θ_m	elasticity m	4.02	average elasticity	4.0	4.0
θ_M	elasticity M	1.90	β_2 of employment reg.	-0.0252	-0.0252
P_C	price level	4.94×10^3	β_1 of employment reg.	27.30	27.30
ϕ_m	mean m sector fiscal shock	-5.71	mean ARRA per million	0.0003	0.0003
σ_m	stdev. m sector fiscal shock	2.77	stdev. ARRA per million	0.0002	0.0005
ϕ_M	mean M sector fiscal shock	-1.88	mean share ARRA connected	6.80%	6.85%
σ_M	stdev. M sector fiscal shock	0.48	stdev. share ARRA connected	8.72%	8.72%

Notes: β_1 and β_2 reported in this table are obtained from estimating regression (1) after deflating regression variables including ARRA spending variables. BLS seasonally adjusted CPI (base year 2008) was used. Table A.8 in Appendix A.3 shows all externally calibrated parameters.

Table 5 shows the result of the calibration. Although all parameters jointly determine the set of moments, there are some strong relationships between parameters and targets. The relative productivity between sectors (A_M) and the consumer preference for the politically connected sector ($\phi_{r,M}$) jointly determine the relative basket price between sectors and the employment share of the connected sector. To avoid differences from price levels in the steady state, we target a relative price ratio of 1. Note that, although only 1.17 percent of firms are politically connected, they account for 28.27 percent of employment. Thus, the calibration points to politically connected firms facing larger demand and being, on average, more productive than non-connected firms. The level of disutility of labor (χ) targets the employment to population ratio for people 16 or older at the beginning of 2009.

The degree of substitution between varieties in each sector (θ_M, θ_m) is jointly calibrated to match two moments. First, we target an average elasticity of 4 in the economy. Second, we compute a jobs multiplier regression with simulated data and match the coefficients from Equation (1). Specifically, we feed in government spending shocks to the home region 10,000 times, and generate government spending per capita ($A_{s,T}^{pc}$), share of spending to the politically connected sector ($S_{s,T}$), and employment growth over two years ($G_{s,T}$). This simulated data resembles cross-sectional data of 10,000 states. We then estimate the regression

$$G_{s,T} = \alpha + \beta_1 A_{s,T}^{pc} + \beta_2 S_{s,T} + \varepsilon_{s,T}, \quad (19)$$

and target the value of β_2 in Equation (19) to the estimated value of β_2 in Equation (1). The scale of the economy is determined by the steady state price of the consumption basket

(P_C), directly affecting the value of β_1 in Equation (19).²⁵ We choose the value of P_C so that β_1 in Equation (19) matches its counterpart in Equation (1). The processes governing the log-normal innovations for government expenditure in each sector are used to target the mean and standard deviation of the two independent variables in Equation (1).

The model successfully captures the empirical moments.²⁶ The calibration strategy selects large differences in the productivity of the two sectors. In the model, the log difference in total factor productivity between politically connected firms and non-connected firms is 1.67, which points to the benign channel being active. Nevertheless, the difference between the calibrated elasticity of substitution of each sector shows that the inefficiency channel is also active, and that the dampening effect of politically connected fiscal spending on employment is partly explained by connected firms charging higher markups.

Evaluating Equation (19), the model and data predict that, holding regional fiscal stimulus constant, a 10 percentage points increase in the share of fiscal expenditure allocated to politically connected firms decreases the local jobs multiplier by 8.2 jobs.²⁷ The benign story of higher productivity in the politically connected sector cannot fully explain this difference: Imposing $\theta_M = \theta_m = 4$ —hence eliminating differences in markups—delivers a value of $\beta_2 = -0.0112$, which implies a reduction in jobs multiplier only by 3.7 jobs.

The model is calibrated to match state-level regressions. As emphasized by Nakamura and Steinsson (2014), these regressions difference out general equilibrium effects that reallocate expenditure and production across states. Therefore, we use the model to calculate the aggregate employment effect of political connections. Specifically, we feed in government spending shocks to both home and foreign regions 10,000 times, and generate data that contains government spending per capita (A_T^{pc}), share of spending to the politically connected sector (S_T), and employment growth over two years (G_T) for the aggregate economy. This data resembles cross-sectional data of 10,000 U.S. economies. We then estimate the regression

$$G_T = \alpha + \beta_{1,GE} A_T^{pc} + \beta_{2,GE} S_T + \varepsilon_T \quad (20)$$

Using model simulated data to estimate regression (20), we recover $\beta_{1,GE} = 67.6$ and $\beta_{2,GE} = -0.0116$. Accounting for reallocation across regions increases the baseline effect

²⁵In the simulation, we deflate nominal government spending with the price deflator defined using P_t^C to clean the effects of government spending from changes in aggregate prices. Accordingly, we re-estimate Equation (1) using the CPI deflator (base year 2008) to match the results in calibration. However, the estimates are practically identical: only β_1 changes from 27.12 to 27.30 and β_2 stays unchanged.

²⁶One exception is a higher standard deviation on the ARRA expenditure. Nevertheless, the model moment exhibits the same order of magnitude as the data moment.

²⁷This number is a touch higher than 8.1 jobs reported in Table 2 because we use real government spending for this exercise.

of fiscal stimulus and dampens the negative effect of politically connected spending on the jobs multiplier. A 10 percentage points increase in the share of politically connected fiscal expenditure costs the aggregate economy 4.5 jobs, which is roughly half its counterpart in partial equilibrium.

The baseline effect of stimulus spending is larger in general equilibrium than in partial equilibrium because the increase in local wages arising from fiscal stimulus in one state further triggers an increase in demand for goods produced in other regions.²⁸ To understand why the negative effect of political connections is muted in general equilibrium, consider two scenarios: one where the government spends 100 dollars only on the politically connected sector versus a second one where it spends the same 100 dollars only on the non-connected sector. In partial equilibrium, a given amount of increase in real government spending pushes up local prices by more when it is spent on the connected sector, compared to when it is spent on the non-connected sector, leading to a larger decline in local private consumption in the first scenario. In general equilibrium, the first scenario entails more reallocation of expenditure across states than in the second: a larger increase in local prices in the first scenario triggers a higher increase in demand for foreign goods, boosting aggregate consumption by more and muting the negative effect of connected spending.

In order to understand the role of markups in general equilibrium, we impose once again $\theta_M = \theta_m = 4$. This general equilibrium experiment delivers a value of $\beta_{2,GE} = -0.00016$, which implies that the markup channel explains virtually all of the decrease in the aggregate jobs multiplier. This result stems from the fact that reallocation is even stronger in the absence of markups, as the higher productivity of sector M implies a lower relative price when compared to sector m .

Summarizing, our general equilibrium model shows that politically connected spending dampens the effect of fiscal stimulus even when accounting for general equilibrium forces. Moreover, differences in productivity between politically connected and non-connected firms only play a role in partial equilibrium.

6 Conclusion

When faced with economic downturns, governments mobilize hundreds of billions of dollars in stimulus. State and local officials are often given substantial discretion in channeling government resources directly to firms in the interest of quickly disbursing funds. These conditions create incentives for firms to exert political influence over the disbursement of

²⁸Monetary policy does not fully counteract this effect in our calibration, and thus, the general equilibrium effect is larger than its partial equilibrium counterpart.

stimulus spending. Yet, little is known about the impact of this political influence on the actual allocation of government spending and its effectiveness.

To tackle these questions, we use ARRA as a laboratory to establish causal evidence that firms with stronger political connections are more likely to secure stimulus grants, and that an increase in the share of stimulus spending given to politically connected firms dampens the job creation effect of the stimulus. Using different levels of data and identification strategies, as well as a quantitative general equilibrium model, we show evidence for this dampening effect at firm, state, and aggregate levels. Overall, our findings indicate that it is important to take into account the political process by which funds are allocated to firms when analyzing the effectiveness of fiscal stimulus.

We conclude by discussing two avenues for future research. First, while we find evidence that inefficiencies are an important channel through which politically connected spending lowers the jobs multiplier, we model this inefficiency as politically connected firms charging higher markups. In future research, it is important to further micro found the inefficiency channel to better assess the costs and benefits of political distortions, and to conduct further counterfactual analysis.²⁹ Second, our findings open the door for politically connected fiscal spending to affect outcomes in other contexts. If our channel is indeed active in other contexts, this approach has potential for providing new insights to classical questions on fiscal spending.³⁰

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²⁹For example, strong ties between firms and local politicians can create “home bias” for local regions in procurement ([García-Santana and Santamaría, 2023](#)), which could make government demand price inelastic.

³⁰For example, this channel can potentially shed light on why fiscal adjustments based on spending cuts are less costly compared to tax reductions ([Alesina et al., 2015, 2019](#)), as the former reduces allocative inefficiencies entailed by spending.

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A Appendix

A.1 Political Connections and Employment: State Level

A.1.1 The Jobs Multiplier

The jobs multiplier can be derived by taking the partial derivative of Equation 1 with respect to $A_{s,T}^{pc}$, while accounting for the fact that $S_{i,T} = A_{i,T}^{pc,c}/A_{i,T}^{pc}$, where $A_{i,T}^{pc,c}$ denotes politically connected ARRA spending per capita:

$$\frac{\partial G_{s,T}}{\partial A_{s,T}^{pc}} = \left(\beta_1 - \beta_2 \frac{A_{s,T}^{pc,c}}{(A_{s,T}^{pc})^2} \right) + \left(\frac{\beta_2}{A_{s,T}^{pc}} \right) \frac{\partial A_{s,T}^{pc,c}}{\partial A_{s,T}^{pc}} = \beta_1 + \beta_2 \left(\frac{\frac{\partial A_{s,T}^{pc,c}}{\partial A_{s,T}^{pc}} - S_{s,T}}{A_{s,T}^{pc}} \right) \quad (21)$$

Equation 21 allows for the allocation of the marginal ARRA spending $\left(\frac{\partial A_{s,T}^{pc,c}}{\partial A_{s,T}^{pc}} \right)$ to differ from the existing allocation ($S_{s,T}$). Note that if the allocation remains the same $\left(\frac{\partial A_{s,T}^{pc,c}}{\partial A_{s,T}^{pc}} = S_{s,T} \right)$, the jobs multiplier is simply equal to β_1 . However, this framework allows us to calculate the marginal effect of ARRA spending when the allocation of these resources vary. In particular, if $\left(\frac{\partial A_{s,T}^{pc,c}}{\partial A_{s,T}^{pc}} > S_{s,T} \right)$, and $\beta_2 < 0$, then an extra \$1 million will create less than β_1 jobs due to differences in job creation by politically connected and non-politically connected firms.

A.1.2 Baseline Results

First stage: Table A.1 reports full baseline first stage regression.

Second stage: Table A.2 reports four versions of the second-stage IV specification. Col. 1 includes only the two endogenous variables of interest, ARRA spending per capita and fraction of politically connected spending. Col. 2 includes all controls used in Wilson (2012). Col. 3 adds division FEs and corresponds to column 2 in Table 2. Col. 4 is our baseline and corresponds to col. 1 in Table 2.

Table A.1: Baseline: First stage results

	(1)	(2)
	ARRA spending pc	Frac connected spending
DOT spending pc (ths)	1.664*** (0.544)	0.503** (0.191)
Corp contrib (indicator)	0.012 (0.032)	0.143*** (0.024)
Emp growth (07-09)	2.865* (1.527)	-0.463 (0.661)
Emp pc (09)	-0.712 (0.846)	-0.135 (0.378)
Change in PI moving avg	-87.940* (46.033)	54.656** (23.238)
HPI growth (03-07)	-0.094 (0.332)	-0.074 (0.131)
Tax benefits (mn pc)	71.849 (332.413)	-334.377** (162.517)
Corruption (dummy)	-0.001 (0.043)	-0.027 (0.022)
Union membership (08)	-0.079 (0.619)	0.072 (0.284)
Constant	0.445 (0.399)	0.201 (0.169)
Division FE	Yes	Yes
Obs.	50	50
R-sq	0.74	0.69

Notes: Dependent variables: in col. 1 is ARRA funding allocated to firms and in col. 2 is the share of the spending allocated as prime vendor grants to politically connected firms. The variables of interest are the excluded instruments in the second stage—anticipated DOT spending per capita and an indicator of whether a state permits corporate campaign contributions. Controls include division FE, prior employment growth, initial employment p.c., house price growth in 2003–2007, change in personal income before the crisis, expected tax benefits p.c., union membership, and corruption indicator. ***, **, and * indicate sig. at the 1%, 5%, and 10% sig. levels. Robust SEs.

A.1.3 Robustness

Table A.3 explores omitted factors that the existing literature has explored as potentially being correlated with state emp. growth and our DOT instrument. Table A.4 tests the sensitivity of the baseline to our control for labor market flexibility (col. 2), state capacity (col. 3), and to accounting for individual campaign contribution limits (col. 4). Table A.5 tests for additional confounding factors. These include controls for a split state legislature (col. 2), being a swing state (col. 3), strictness of individual contribution limits (col. 4), average firm age (col. 5) and size (col. 6).

Table A.2: Baseline: Second stage results
Dependent variable: change in emp-pop ratio, Feb 09 - Dec 10

	(1)	(2)	(3)	(4)
	No controls	Wilson Controls	Wilson & Division FE	Baseline
ARRA spending pc	20.41 (12.62)	14.77** (6.261)	24.56** (11.19)	27.17** (11.03)
% connected	-0.0366* (0.0219)	-0.0261* (0.0138)	-0.0220* (0.0113)	-0.0252** (0.0128)
Emp growth (07-09)		0.118*** (0.0323)	0.00436 (0.0594)	0.00360 (0.0664)
Emp pc (09)		0.00853 (0.0383)	0.0333 (0.0394)	0.0268 (0.0383)
Change in PI moving avg		-4.402** (2.162)	-2.444 (2.216)	-2.395 (2.200)
HPI growth (03-07)		0.00212 (0.00855)	0.0116 (0.0119)	0.0107 (0.0110)
Tax benefits (mn pc)		1.893 (6.367)	-5.285 (8.719)	-0.364 (10.56)
Corruption (dummy)				-0.000692 (0.00149)
Union membership (08)				-0.0373 (0.0260)
Constant	-0.0107*** (0.00275)	-0.00508 (0.0150)	-0.0209 (0.0200)	-0.0162 (0.0199)
Division FE	No	No	Yes	Yes
F-stat	8.914	8.356	6.096	5.420
Obs.	50	50	50	50
R-sq	-0.19	0.33	0.40	0.39

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

Table A.3: Robustness for ARRA per capita IV
Dependent variable: change in emp-pop ratio

	(1)	(2)	(3)	(4)	(5)
	Baseline	Manu share	Bartik	Housing bust	Anticipation
ARRA spending pc	27.17** (11.03)	26.77** (10.81)	29.68*** (10.87)	30.51** (13.33)	25.56** (11.32)
% connected	-0.0252** (0.0128)	-0.0321** (0.0154)	-0.0199* (0.0104)	-0.0291* (0.0153)	-0.0228* (0.0131)
Standard controls	Yes	Yes	Yes	Yes	Yes
Manufacturing share	No	Yes	No	No	No
Exp. emp change (Bartik)	No	No	Yes	No	No
HPI growth (07-09)	No	No	No	Yes	No
F-stat	5.420	5.340	5.112	4.311	5.243
Obs.	50	50	50	50	50
R-sq	0.39	0.39	0.50	0.39	0.56

Notes: Dependent variable: Δ in emp. between Feb. 2009 (Dec. 2008 in col. 5) and Dec. 2010 relative to working age pop. in 2009 (2008 in col. 5). Key variables: ARRA spending p.c. and the share allocated through politically connected firms. IVs: anticipated DOT spending and an indicator of whether a state permits corporate campaign contributions. Standard controls: division FE, prior emp. growth, initial emp. p.c., house price growth in 2003–2007, change in personal income before the crisis, expected tax benefits p.c., corruption dummy, and union membership. Additional controls: the share of state employment in the manufacturing sector (col. 2); expected change in employment (Bartik) (col. 3), and change in house prices in 2007Q4–2009Q1 (col. 4). Col. 5 tests whether our results hold if the initial period is changed from Feb. 2009 to Dec. 2010. ***, **, and * indicate sig. at the 1%, 5%, and 10% sig. levels. The F-stat test statistic is reported. Robust SEs.

Table A.4: Robustness 1 for Frac Connected Spending IV
Dependent variable: change in emp-pop ratio, Feb 09 - Dec 10

	(1)	(2)	(3)	(4)
	Baseline	RTW	Admin capacity	Contrib Limit in 2008
ARRA spending pc	27.16** (11.04)	24.57** (10.20)	31.40** (13.15)	27.17** (11.03)
% connected	-0.0248* (0.0133)	-0.0241* (0.0137)	-0.0297* (0.0165)	-0.0252** (0.0128)
Division FE	Yes	Yes	Yes	Yes
F-stat	No	No	Yes	
Obs.	5.355	5.589	4.537	5.420
R-sq	50	50	50	50
r2	0.39	0.40	0.35	0.39

Notes: Dependent variable: Δ in emp. b/w Feb. 2009 and Dec. 2010, relative to working age pop. in 2009. Key variables: ARRA spending p.c. and % allocated through politically connected firms. IVs: anticipated DOT spending and an indicator of whether a state permits corporate campaign contributions in 2002 (2008 in col 5). Standard controls (unless otherwise notes, cols. 2 and 3): division FE, prior emp. growth, initial emp. p.c., house price growth in 2003–2007, change in personal income before the crisis, expected tax benefits p.c., corruption dummy, and union membership. Col. 2 indicator for right to work states instead of union membership. Col. 3 governmental administrative capacity instead of corruption. Col. 4 measures the corporate campaign contribution limit based on 2008 election laws rather than 2002. ***, **, and * indicate sig. at the 1%, 5%, and 10% sig. levels. The F-stat test statistic is reported. Robust SEs.

Table A.5: Robustness 2 for Frac Connected Spending IV
Dependent variable: change in emp-pop ratio, Feb 09 - Dec 10

	(1)	(2)	(3)	(4)	(5)	(6)
	Baseline	Split Leg.	Swing State	Indiv Contrib	Avg Firm age	Avg Firm size
ARRA scaled	27.16** (11.04)	24.18* (12.90)	23.71** (10.85)	32.82** (13.12)	27.02** (10.76)	23.44*** (8.780)
% connected	-0.0248* (0.0133)	-0.0244* (0.0133)	-0.0248* (0.0128)	-0.0334** (0.0160)	-0.0233* (0.0135)	-0.0306* (0.0171)
Full controls	Yes	Yes	Yes	Yes	Yes	Yes
scale	2009 pop	2009 pop	2009 pop	2009 pop	2009 pop	2009 pop
F-stat	5.355	5.398	5.404	5.038	6.206	6.409
Obs.	50	50	50	50	50	50
R-sq	0.39	0.43	0.43	0.34	0.40	0.41

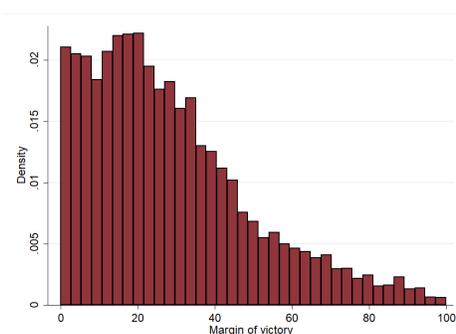
Notes: The dependent variable is the Δ in emp. b/w Feb. 2009 and Dec. 2010 relative to working age pop. in 2009. Key variables: ARRA spending scaled by working age population in 2009; and the share allocated through politically connected firms. IVs: anticipated DOT spending and an indicator of whether a state permits corporate campaign contributions. Standard controls: division FE, prior emp. growth, initial emp. p.c., house price growth in 2003–2007, change in personal income before the crisis, expected tax benefits p.c., corruption dummy, and union membership. Col. 2 controls for same party control of lower and upper houses of state legislature. Col. 3 controls for swing states. Col. 4 controls for the strictness of individual campaign contribution limits. Col. 5 controls for the avg. age of firms. Co. 6 controls for avg. firm employment. ***, **, and * indicate sig. at the 1%, 5%, and 10% sig. levels. The F-stat test statistic is reported. Robust SEs.

A.2 Political Connections and Employment: Firm Level

A.2.1 Summary Statistics

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

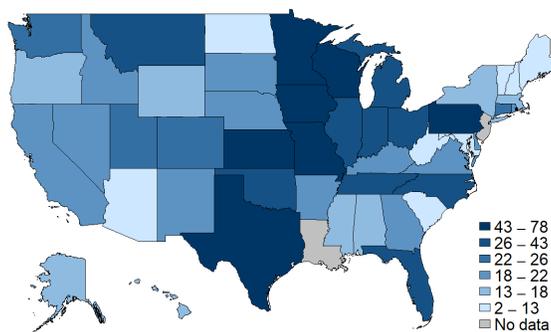
Figure A.1: Margin of Victory in State Legislative Elections (2006-2008)



Notes: Sources are ICPSR State Legislative Election Returns Database. Depicted is 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.

Figure A.2 shows the number of candidates in close elections across the United States. There is ample variation across states in the number of candidates, and close elections are not concentrated in swing states or a specific region.

Figure A.2: Number of candidates associated with close elections



Notes: Sources are NETS, ICPSR State Legislative Election Returns Database, Authors' own calculation. Figure plots the distribution of candidates who were running for office in close elections during the 2006, 2007, and 2008 election cycles.

A.2.2 Political Connections and Grant Allocation

Table A.6: The Effect of Political Connections on Winning a Grant; Full Table

	(1)	(2)	(3)
	Win	Win	Win
Connected	0.561** (0.241)	0.486*** (0.146)	0.802** (0.376)
Instate		2.343*** (0.740)	2.375** (0.993)
Constant	3.185*** (0.141)	1.594*** (0.452)	1.963*** (0.717)
NAICS4 x State FE	Yes	Yes	Yes
NumCandCE FE	Yes	Yes	Yes
Emp Category FE	No	Yes	Yes
Margin of Victory	5%	5%	3%
Obs.	9965	9965	6712
R-sq	0.34	0.36	0.38

Notes: Unit of analysis is firm \times state. The dependent variable, *Win*, indicates whether a firm received a grant in a state as a prime vendor, multiplied by 100 for ease of interpretation. *Instate* indicates whether a firm's headquarter is located in a given state. We also control for four-digit NAICS by state fixed effects, and fixed effects for the number of candidates a firm supported in close elections and its size category measured by the number of employees. When employment is included as log employment instead, its coefficient is positive and statistically significant in all columns (not reported). ***, **, and * indicate sig. at the 1%, 5%, and 10% sig. levels. Standard errors are clustered two ways, by state and industry.

Table A.7: Robustness Tests for the Effect of Political Connections on Grant Allocation

	(1)	(2)	(3)	(4)
	Grant PV Other	Grant SV	Win	Win
Connected	-0.088 (0.524)	0.148 (0.348)	0.947** (0.446)	
Frac(Win)				0.678*** (0.249)
Constant	8.411*** (0.305)	2.814*** (0.203)	3.481*** (0.234)	3.150*** (0.133)
NAICS4 x State FE	Yes	Yes	Yes	Yes
NumCandCE FE	Yes	Yes	Yes	Yes
Margin of Victory	5%	5%	3%	5%
Obs.	9965	9965	6712	9965
R-sq	0.42	0.36	0.35	0.34

Notes: Unit of analysis is firm \times state. *Grant PV Other* indicates whether a firm received a grant in any states other than the focal state as a prime vendor. *Grant SV* indicates whether a firm received a grant in the focal state as a sub vendor. *Win* indicates whether a firm won a grant as a prime vendor in a state. All three variables are multiplied by 100 for ease of interpretation. *Frac(Win)* is the share of election winners among the candidates a firm supported in close elections in a state, and *Connected* indicates whether *Frac(Win)* is greater than or equal to 50%. We control for four-digit NAICS by state fixed effects and fixed effects for the number of candidates a firm supported in close elections. ***, **, and * indicate sig. at the 1%, 5%, and 10% sig. levels. Standard errors are clustered two ways, by state and industry.

A.3 Externally Calibrated Model Parameters

Table A.8: Externally Calibrated Model Parameters

Parameter	Value	Definition	Source/Quantification
σ	1	Intertemporal elasticity of substitution	Nakamura and Steinsson (2014)
ν	1	Frish-elasticity of labor supply	Nakamura and Steinsson (2014)
β	0.99	Subjective discount factor	Nakamura and Steinsson (2014)
a	2/3	Curvature of production function	Nakamura and Steinsson (2014)
α	0.75	Calvo parameter	Nakamura and Steinsson (2014)
ϕ_H	0.69	Degree of home bias in consumption in the home region	Nakamura and Steinsson (2014)
ϕ_π	1.5	Inflation response in Taylor rule	Nakamura and Steinsson (2014)
ϕ_y	0.5	Output response in Taylor rule	Nakamura and Steinsson (2014)
ϕ_g	0	Direct response of monetary policy to fiscal shock	Nakamura and Steinsson (2014)
ρ_r	0.8	Lagged dependence in Taylor rule	Nakamura and Steinsson (2014)
$Corr_G$	0	Correlation of government spending shocks	Nakamura and Steinsson (2014)
$\omega_{r,s}$	0.2	Government spending to output ratios for each region-sector	Nakamura and Steinsson (2014)
ϕ_F^*	0.99	Degree of home bias in consumption in the foreign region	Nakamura and Steinsson (2014) calculation: $\phi_F^* = 1 - \phi_H^* = \phi_F * n / (1 - n)$

Notes: Value for all externally calibrated parameters that are not described on the main text.