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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 using ARRA as a laboratory. Through an IV approach, we show that a 10 percentage points increase in the share of politically connected spending lowers the job creation effect of stimulus by 33 percent at the state level. We exploit ex-post close state-level elections to establish that firms that contributed to winning candidates create fewer jobs after winning grants. Using a quantitative general equilibrium model, we show that politically connected spending also lowers the aggregate jobs multiplier, and that the dampening effect is rationalized by connected firms charging higher markups.

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1 Introduction

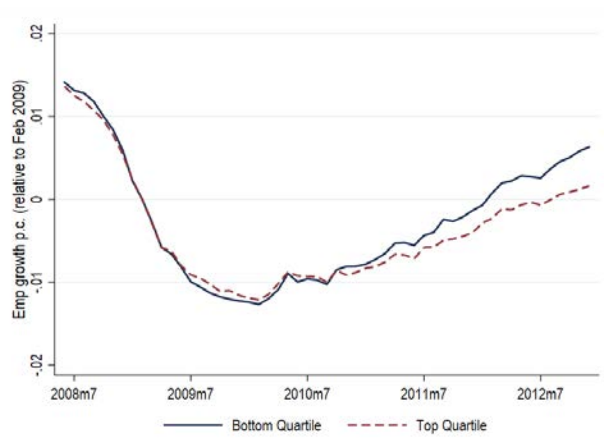
During economic downturns, aggressive fiscal stimulus measures are implemented to stabilize the economy. A substantial share of this stimulus is government purchases, through which funds are channeled directly to firms. The macroeconomics literature has been keenly interested in understanding the effectiveness of such government spending. Despite how valuable stimulus funds are to firms, the 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). We first use an instrumental variable approach to show that a 10 percentage points increase in the share of ARRA spending allocated to politically connected firms lowers the job creation effect of stimulus by 33 percent at the state level. We then exploit ex-post close elections to establish that firms that contributed to winning candidates in state-level elections were more likely to win ARRA grants, but subsequently created fewer jobs. Finally, we use a quantitative general equilibrium model to document that politically connected spending also lowers the aggregate jobs multiplier.

ARRA provides an appropriate setting for this study for several reasons. 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 disbursing ARRA grants and selecting firms to deliver the goods and services associated with these grants. At the same time, with the average grant worth $385,000, firms had an incentive 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. 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 persistently slower 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.We extend the framework by introducing the share of ARRA spending disbursed to politically connected firms as an additional explanatory variable.

Figure 1: Emp 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.

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, states with better managed governments may be more successful in soliciting funds and may experience a swifter recovery. To 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, 26.8 jobs per million dollars spent, but raising the share of spending given to politically connected firms by 10 percentage points lowers this multiplier by 8.7 jobs, or equivalently, by 33 percent. 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 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 are 23 percent more likely to secure ARRA grants than "unlucky" firms that supported more close election losers. 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 10 percent of contributors for their candidates. 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 politically connected spending dampens the job creation effect of winning stimulus grants at the establishment level. Specifically, we compare 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 our findings are consistent with the idea 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 two sectors differ in their productivity and their ability to charge higher markups under profit maximization. The model is calibrated to firm level data and to match the regional jobs multiplier estimates. In the calibrated model, connected firms have substantially higher productivity (hence use fewer workers for the same level of production) than non-connected firms. Nevertheless, the differences in productivity can only explain 28 percent of the employment loss associated with the increase in the share of politically connected spending in partial equilibrium. In general equilibrium, a 10 percentage points increase in the share of politically connected spending still lowers the aggregate jobs multiplier by 5.2 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 for the allocation of spending across firms and the effectiveness of fiscal stimulus. 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 how firms exert their political influence and the literature examining the local and aggregate effects of fiscal stimulus.

The literature studying how firms leverage their political connections to capture government spending primarily focuses on three dimensions of firm influence. The first dimension is how firms exert their influence over the design and implementation of public policy, by, for example, employing current or former politicians (Bunkanwanicha and Wiwattanakantang, 2008; Goldman et al., 2013; Akcigit et al., 2023), lobbying (Kerr et al., 2014; Kang, 2016; Hassan et al., 2019), or directly contributing to political campaigns (Faccio, 2004; Claessens et al., 2008; Cooper et al., 2010; Akey, 2015). Second is what firms advocate for, which includes tax benefits (Arayavechkit et al., 2018), less regulation (Fisman and Wang, 2015), more favorable terms for government loans (Khwaja and Mian, 2005), government contracts (Brogaard et al., 2021), and government bailouts (Faccio et al., 2006). The third dimension is how firms' influence affects the disbursement of government funds (Duchin and Sosyura, 2012; Boone et al., 2014; Leduc and Wilson, 2017).

The empirical literature studying the effectiveness of stimulus spending exploits geographic or temporal variation to estimate the impact of additional dollars spent on employment or output. Chodorow-Reich et al. (2012) exploit 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. 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.[1]

Beyond the analysis of ARRA, Cohen et al. (2011) use changes in congressional committee chairmanships as a source of exogenous variation in state-level federal expenditures and find that fiscal spending shocks dampen corporate investment activity. Ramey and Zubairy (2018) use quarterly time series data and Dupor and Guerrero (2017) exploits the geographic variation in military expenditure to study the cyclical properties of fiscal multipliers.[2] Nakamura and Steinsson (2014) also rely on geographic variation in military expenditure, but complement their empirical analysis with a structural model to quantify the general equilibrium effect of stimulus spending. In a similar vein, Farhi and Werning (2017a) and Farhi and Werning (2017b) study the aggregate effect of government policy in a model of currency unions.

Our contribution to the literature is twofold. First, we advance the literature by documenting that politically connected spending dampens the impact of stimulus spending on employment at the firm, state, and aggregate levels. By doing so, we contribute to the empirical literature by showing that political connections have an impact on real outcomes, even in general equilibrium, and to the macroeconomics literature by showing that *both* the level of spending and how it is allocated across recipients matters for the effectiveness of fiscal stimulus. Second, we complement the existing literature studying political connections by establishing a causal link between firms' connections to state politicians and the allocation of grants. In doing so, we document a novel sub-national mechanism through which political connections impact stimulus spending.

The remainder of the paper is structured as follows. Section 2 describes the institutional features of ARRA and the data sources used. 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

[1] A more comprehensive review of the recent fiscal and employment multiplier literature can be found in Chodorow-Reich (2019).

[2] 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.

employment effect of winning grants. Section 5 quantifies the aggregate effects of political connections on the jobs multiplier using a quantitative general equilibrium model. Section 6 concludes.

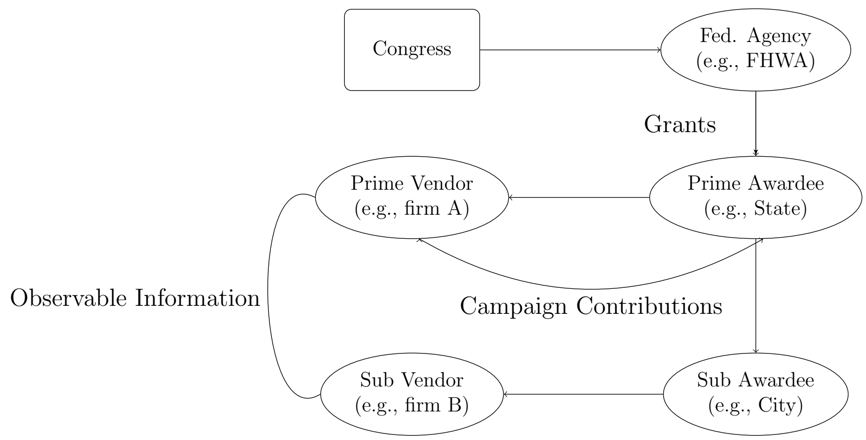
2 Institutional Context and Data

2.1 The American Recovery and Reinvestment Act

ARRA was an economic stimulus package 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, funding of entitlement programs, such as unemployment benefit extensions (Hagedorn et al., 2013; Chodorow-Reich et al., 2019), fiscal aid to state governments (Chodorow-Reich et al., 2012), loans, and procurement contracts and grants ($239 billion, or 30.4 percent of the total ARRA spending).

Firms 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 sub-national 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.[3] 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. 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.

[3]There are plenty of anecdotal examples of politically connected firms being awarded government contracts. For instance, in Tennessee a private firm has, for years, been awarded contracts to operate prisons in jurisdictions represented by state legislators that the firm regularly contributes to, despite the fact that an external audit has denounced the firm's performance. The campaign contributions of this firm can be found at CoreCivic (2020) and in more details at FollowTheMoney.org. For the external audits, see Tennessee Comptroller of the Treasury (2020), "Performance Audit Report."

2.2 Data Sources

We obtain information on firm characteristics from the National Establishment Time Series (NETS). NETS is a longitudinal data set of businesses in the U.S. that contains establishment-level information including number of employees, location, industry, and 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.[4]

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

To measure political connections of firms to state legislators, we use campaign finance contribution data from the National Institute of Money in Politics (NIMP). NIMP is a nonprofit organization that compiles public records on campaign finance at the federal and state level. We use probabilistic name and location matching to construct firm-level information on the amount of campaign contributions made by firms to politicians running for office in state legislative elections.[5] 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

[4]In all our analysis, we remove entities in the public sector as well as business associations and labor unions (NAICS 8139) as we focus on political connections formed in the interest of individual private-sector businesses.

[5]Appendix B.1 provides additional details on the matching procedures.

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).

3 State Level Analysis

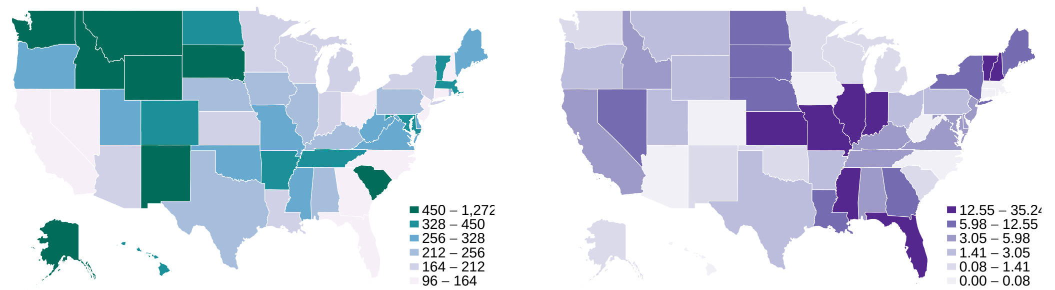
We start by showing that, controlling for total ARRA expenditure per capita, states that allocated more ARRA grants to politically connected firms created fewer jobs. Our empirical approach exploits geographic variation in ARRA spending to firms and the share of that spending channeled through politically connected firms to identify the effects of both factors on local employment. 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 3a, to determine whether states that received more resources per capita created more jobs. Put simply, two states like Illinois and Texas, which each channeled around $215 of ARRA stimulus per capita to firms, are expected to save a similar number of jobs in the canonical employment multiplier literature. 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 3b, to determine whether the jobs multiplier differed in states with a higher fraction of politically connected spending. In particular, we examine whether the fact that Texas channeled less than 2 percent of ARRA spending through politically connected firms, while Illinois channeled 21 percent, mattered for the state-level jobs multiplier.

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

Figure 3: Cross-state variation in ARRA spending



(a) ARRA spending per capita (b) Politically connected 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.

channeled through politically connected firms.:

$$G_{s,T} = \alpha + \beta_1 A^{pc}_{s,T} + \beta_2 S_{s,T} + \mathbf{X_{s,0}}\Gamma + \varepsilon_{s,T}, \tag{1}$$

$$A^{pc}_{s,T} = \delta + \phi_1 IV^{A^{pc}}_{s,0} + \phi_2 IV^{S}_{s,0} + \mathbf{X_{s,0}}\Theta + \nu_{s,T}, \tag{2}$$

$$S_{s,T} = \delta + \phi_1 IV^{A^{pc}}_{s,0} + \phi_2 IV^{S}_{s,0} + \mathbf{X_{s,0}}\Theta + \nu_{s,T}. \tag{3}$$

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. $A^{pc}_{s,T}$ denotes the total ARRA grant and contract spending (measured in \$, millions (mn)) 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 $A^{pc}_{s,T}$ and $S_{s,T}$, respectively. These regressions incorporate two excluded instruments, $IV^{A^{pc}}_{s,0}$ and $IV^{S}_{s,0}$, 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. The economic intuition for this approach is clear; we aim to compare two states with the same per capita fiscal stimulus where the only difference is that one state allocates a higher share of the resources towards politically connected firms. Specifically, in our setting, the jobs multiplier is given by $\beta_1 + \beta_2 \left(\frac{\partial A^{pc,c}_{s,T} / \partial A^{pc}_{s,T} - S_{s,T}}{A^{pc}_{s,T}} \right)$, where the term in parentheses captures whether the allocation of additional ARRA spending $\left(\frac{\partial A^{pc,c}_{s,T}}{\partial A^{pc}_{s,T}} \right)$ differs from the existing allocation ($S_{s,T}$). If the allocation is unchanged, the jobs multiplier remains β_1. If, the allocation changes—say $\left(\frac{\partial A^{pc,c}_{s,T}}{\partial A^{pc}_{s,T}} > S_{s,T} \right)$—then the sign of β_2 determines whether increasing the share of politically connected spending increases or decreases the jobs multiplier.[6]

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 (measured in \$, mn), scaled by each state's working age population in 2009 ($A^{pc}_{s,T}$) and deflated using seasonally adjusted CPI (base year 2008). We sum the amount allocated to four types of recipients—grant prime vendors, grant sub vendors, contract prime vendors, and contract sub vendors.

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, ARRA was in part allocated based on how severely states were impacted by the economic downturn. Sec-

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

ond, 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 better economic performance. Third, politically connected spending is endogenous. It is worth noting that 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.

We construct two instruments to address these endogeneity concerns.[7] The first instrument, used by Wilson (2012), Conley and Dupor (2013) and Chodorow-Reich (2019), addresses the endogeneity of $A^{pc}_{s,T}$ 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 real 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.[8] In our data, DOT funding accounts for 30.4 percent of all spending, and 60.4 percent of grants to prime vendors. Although the DOT instrument is derived from DOT spending, the instrument is highly correlated with per capita spending allocated to firms as in previous studies (the correlation is 0.74).

The second instrument addresses the endogeneity of $S_{s,T}$ by capturing the potential of firms to build political connections. We introduce an indicator denoting whether a state permitted direct 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 4 shows that 29 states across the country permitted corporate campaign contributions in

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

[8]The first three factors are measured in 2006 and the last in 2008.

state elections.[9] For example, while Texas prohibits them, Illinois permits them.[10] 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. Additionally, because we measure direct corporate campaign contributions during the 2006–2008 election cycles, and base our instrument on 2002 state campaign finance laws, our empirical approach will not be impacted by the 2010 Citizens United ruling regarding independent political expenditure.[11]

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.[12]

We introduce eight control variables to our baseline specification, the majority of which are motivated by previous studies. 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

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

[10]To be more specific, in the FEC's Campaign Finance Law 2002 publication, under the category of "corporation to candidate", the exact wording for Texas is "prohibited", for Illinois is "unlimited", and, as an additional example, for Idaho is "Limited to $5,000 each for a candidate in a primary or general election, or $1,000 each for other candidates per election."

[11]For a detailed discussion of why our empirical strategy is not affected by the Citizens United ruling, as well as supporting evidence from two robustness exercises, see Online Appendix B.3.

[12]Employment data are obtained from the Bureau of Labor Statistics' (BLS) Current Employment Statistics (CES) data on total statewide, non-farm, seasonally adjusted employment, and working age population data is obtained from the United States Census Bureau.

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. Because ARRA provided fiscal stimulus to states using a formula that explicitly factored in the change in average personal income per capita, we measure the change between 2004 and 2006 in the three-year trailing average of real 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. To account for region-specific employment trends, we also control for Census Division fixed effects.

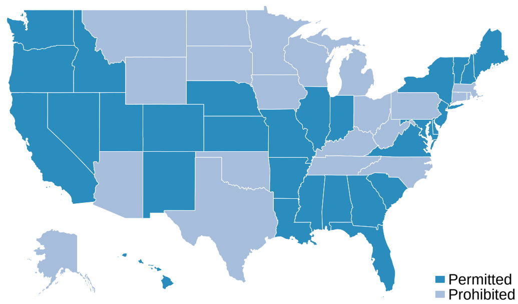
The last two variables account for potential omitted factors correlated with political influence and state level employment growth. We control for the fraction of employees in each state that are union members in 2008. The reasoning is that labor unions may exert their political influence to shape campaign 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 the corporate 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

[13]Summary statistics for the variables used are shown in Table B.2 in Appendix B.4.

Figure 4: Corporate campaign contribution limits



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

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 $\left(\frac{\partial A^{pc,c}_{s,T}}{\partial A^{pc}_{s,T}} = \bar{S}_T\right)$, 26.8 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 ($\frac{\partial A^{pc,c}_{s,T}}{\partial A^{pc}_{s,T}} = \bar{S}_T + 0.1$), the job multiplier decreases to 18.1 jobs. Thus, increasing the share of politically connected spending by 10 percentage points above the mean reduces the jobs saved per \$1 million in ARRA spent by 8.7 jobs, or by 33 percent.[14]

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.[15] 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.

[14] 10 percentage points is slightly higher than one standard deviation of 8.6 percentage points,

[15] Tables B.3 and B.4 in Appendix B.4.2 report the coefficients for the full set of controls.

Table 1: Baseline: First stage results

	(1) ARRA spending (mn pc)	(2) Frac connected ARRA
DOT IV (ths pc)	1.690***	0.469**
	(0.530)	(0.195)
Corp contrib (dummy)	0.013	0.136***
	(0.031)	(0.025)
Full controls	Yes	Yes
Division FE	Yes	Yes
Obs.	50	50
R-sq	0.74	0.68

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 indictor. ***, **, 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) Baseline	(2) Drop union & corr controls
ARRA spending (mn pc)	26.79**	24.37**
	(10.67)	(11.13)
Frac. connected spending	-0.0265**	-0.0223*
	(0.0134)	(0.0116)
ARRA controls	Yes	Yes
Connected controls	Yes	No
Division FE	Yes	Yes
F-stat	5.914	6.191
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.1 in Appendix A.1.2 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.2 shows that introducing alternative proxies for labor market flexibility and political environment does not 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.[16]

Finally, Table A.3 shows that accounting for possible correlation between employment growth, contribution limits, and state and firm characteristics (age, and size), does not alter our baseline results. 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 may also experience a different pace of recovery.

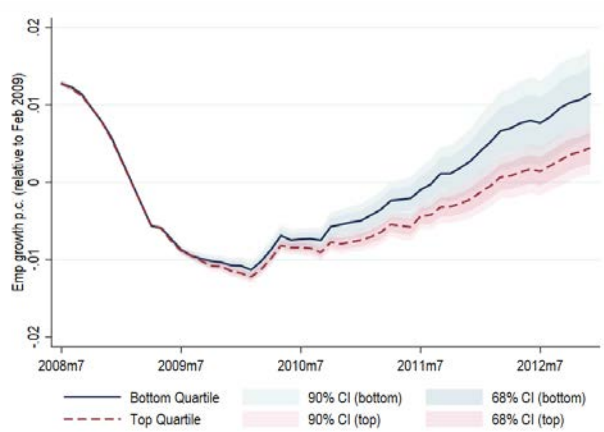
3.6 Differential Evolution of Employment Growth

We also 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

[16] Campaign finance laws are persistent. Relative to 2002, only Colorado is codified differently in 2008.

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 5 shows that in December 2010, employment growth of the bottom quartile is 0.26 percentage points higher than the top quartile. By December 2012, employment growth of the bottom quartile is 0.70 percentage points higher than the top.

Figure 5: Employment Growth by Degree of Political Connectedeness



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 Firm Level Analysis

To establish 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 document that gaining political connections increases the firms' probability of winning stimulus grants, and that 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).

We focus on state legislative elections held between 2006 and 2008, with terms lasting until at least 2010. Our close election sample encompasses 600 elections across 48 states, or 10 percent of all elections held during this period (see Figure B.3 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 B.4. Among firms participating in close elections, 66.3 percent contributed to only one candidate.[17] Supporting the top two candidates in the same election is relatively rare, occurring in only 4.1 percent of firm-election pairs.[18]

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

[17] Table B.5 shows the distribution of the no. of candidates firms support in close elections.

[18] Simultaneously 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.

level. Therefore, we aggregate firms' political connections to the firm-state level.

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

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

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, $Connected_{is}$, that takes a value of one if $Frac(Win)_{is}$ 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, $Connected_{is}$ is 1 if that candidate won the election and zero otherwise. If the firm supported two candidates in close elections, $Connected_{is}$ is 1 if one or both of the candidates won their election and zero if neither did.

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

$$Y_{i,s} = \beta_0 + \beta_1 Connected_{i,s} + \gamma' X_{i,s} + \epsilon_{i,s}, \tag{5}$$

where $Y_{i,s}$ 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 a firm 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	0.001	-0.019	0.019	0.014	-0.215
	(0.060)	(0.018)	(0.034)	(0.011)	(0.212)
NAICS4 FE & State FE	Yes	Yes	Yes	Yes	Yes
NumCandCE FE	Yes	Yes	Yes	Yes	Yes
Obs.	8033	8033	8033	8033	6346
R-sq	0.68	0.35	0.77	0.48	0.23

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 (Column 1) indicates that a stronger political connection increases the chances of winning a grant by 0.83 percentage points. To interpret the estimated effect, it is important to note that grant allocation is heavily concentrated in a small share of firms.[19] Among the control group, the mean probability of winning a grant is 3.6 percent, implying that the estimated marginal treatment

[19]The mean probability of winning a grant in our sample is 4.3 percent. Cox et al. (2020) documents similar evidence that federal procurement contracts are concentrated in a small fraction of firms.

effect of stronger political connections is a 23 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 10 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 firm size (measured by log employment) 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.5 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*.

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

	(1) Win	(2) Win	(3) Win	(4) Win	(5) Win	(6) Win
Connected	0.828***	0.895***	0.860***	0.704*	0.961***	1.053***
	(0.298)	(0.319)	(0.257)	(0.378)	(0.191)	(0.271)
Connected × HetVar		3.980***	-0.790	1.230	1.131	0.289
		(1.248)	(0.523)	(1.827)	(0.725)	(0.194)
HetVar		-0.255	0.530	7.425***	2.918***	1.383***
		(1.030)	(0.612)	(1.643)	(0.821)	(0.318)
Constant	3.609***	3.585***	3.580***	2.409***	3.580***	3.722***
	(0.175)	(0.183)	(0.243)	(0.243)	(0.109)	(0.193)
NAICS4 x State FE	Yes	Yes	Yes	Yes	Yes	Yes
NumCandCE FE	Yes	Yes	Yes	Yes	Yes	Yes
HetVar	None	Main Donor	Majority Party	Large Firm	Firm Age	Ln Emp
Obs.	8033	8033	8033	8033	8033	8033
R-sq	0.36	0.36	0.36	0.37	0.36	0.38

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 estimate 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:

$$Y_{e,f,s,j,t} = \sum_{k=2005}^{2014} \delta_k I_{(k=t)} \times Grant_{f,s} + \sum_{k=2005}^{2014} \gamma_k I_{(k=t)} \times Grant_{f,s} \times Connected_{f,s} + \sum_{k=2005}^{2014} \lambda_k I_{(k=t)} \times Connected_{f,s} + \alpha_e + \eta_{j,t} + \phi_{s,t} + \epsilon_{e,f,s,j,t}. \quad (6)$$

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. $Grant_{f,s}$ is 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. $\eta_{j,t}$ and $\phi_{s,t}$ represent industry by year fixed effects and state by year fixed effects, respectively.[20]

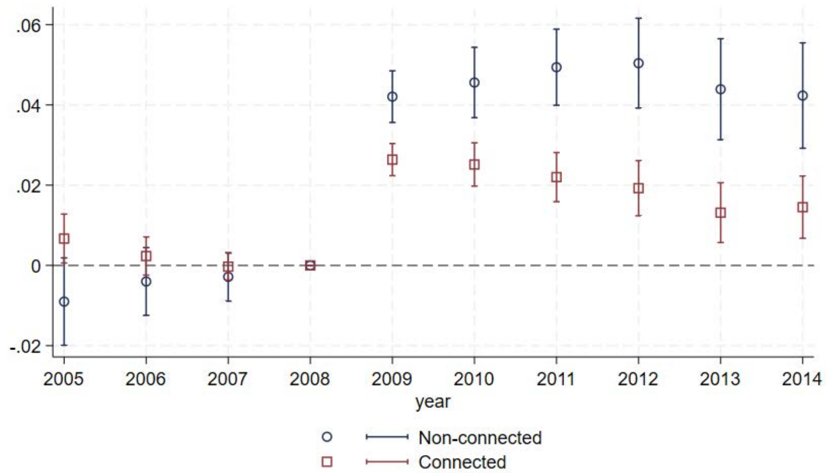
Figure 6 displays the coefficients of interest, δ_k and $\delta_k + \gamma_k$, which show the difference in employment between grant winners and non winners among non-connected and connected firms, respectively. The results indicate that establishments in non-connected firms increase their employment by 4.2 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 2.6 percent. 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.

To verify whether it is indeed the political connections that create the employment growth differences, we conduct a placebo test. Our baseline regression tests whether having a 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.[21] Figure 7 show no significant differences in employment growth. If our

[20] Crane and Decker (2019) document that employment dynamics in NETS are subject to large measurement errors, mostly due to the prevalent imputation of employment records among small establishments. Though we conduct our baseline analysis at the establishment level, we minimize the impact of outliers by trimming the observations at 1% and removing establishments that exhibit spurious jumps in employment (Díez et al., 2021). We have also verified that the results are robust to using only nonimputed observations as shown in Figure B.5.

[21] We estimate a version of regression equation (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 establish-

Figure 6: Establishment-level Employment after winning an ARRA Grant



Notes: Unit of analysis is establishment × year. The figure displays the effects of a firm winning a grant in state s to its establishments in that state when the firm did not gain political connections (blue circles) and when the firm did gain political connections (red squares) in that state. Standard errors are clustered at the establishment level and the error bands indicate 95% confidence intervals.

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 6.[22]

5 A General Equilibrium Model

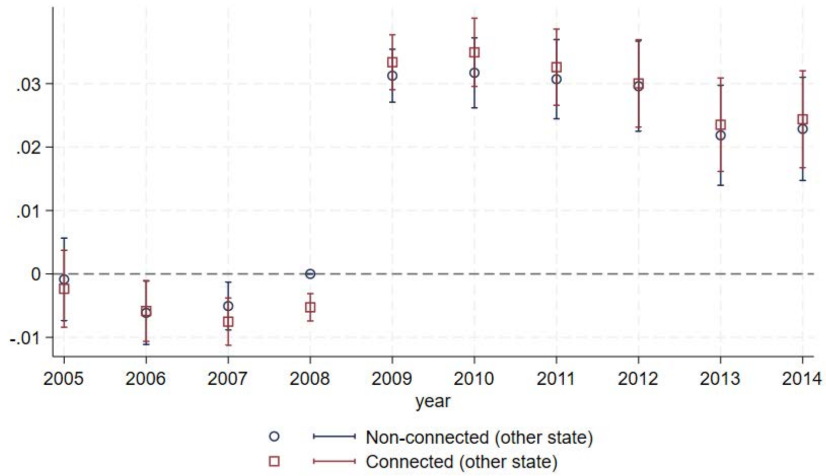
Our empirical analysis shows that politically connected firms create fewer jobs after winning government grants, which weakens the local employment effect of fiscal stimulus. To better understand the channels through which this occurs, and to analyze whether the state- and firm-level results continue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

ment'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.

[22]We find that this result is also robust to using only nonimputed observations as shown in Figure B.6 in the Online Appendix.

Figure 7: Employment Growth: Placebo



Notes: Unit of analysis is establishment × year. The figure displays the effects of a firm winning a grant in state s to its establishments in that state when the firm did not gain political connections (blue circles) and when the firm did gain political connections (red squares) in some state $z \neq s$. 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. Standard errors are clustered at the establishment level and the error bands indicate 95% confidence intervals.

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 channel 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 channel, rooted in inefficiency, is that connections to politicians give firms leverage to charge higher markups to the government.[23] 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.[24]

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.

5.1 Model Description

5.1.1 Households

The representative consumer at home seeks to maximize their utility:

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

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.[25]

[23] Using heterogeneous markups to model the second channel is consistent with our empirical results, as well as those from Brogaard et al. (2021) and Fan and Zhou (2023). 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.

[24] While both channels may be active in reality, our empirical results in Section 4 are consistent with the markup-based explanation. We find in Section 4.2 that politically connected firms create fewer jobs after winning grants, even after extracting a component of political connections that is not correlated with firm characteristics by using quasi-random variation in close elections. In Table 4 we document 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. We allow for differential productivity in the model because the model is designed to be more general than the close elections environment we study empirically.

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

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

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

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 H and F goods.

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

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

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

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

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:

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

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} 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.[26]

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 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,

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

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

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:

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

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:

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

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 $\hat{y}_t^{ag}=n(P_HY_H)/(PY)\hat{y}_{H,t}+(1-n)(P_HY_H)/(PY))\hat{y}_{F,t}$. Finally, it may respond directly to the aggregate government spending $\hat{g}_t^{ag}=n\hat{g}_{H,t}+(1-n)\hat{g}_{F,t}$.[27]

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

[27]The results also hold under a constant nominal or real interest rate monetary policy rule.

z in sector s is:

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

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:

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

Firm z must satisfy demand for its product. The demand for firm z's product comes from three sources: home and foreign consumers and the government.

Firm z is therefore subject to the following constraint:

$$\frac{1}{n}\frac{1}{\mu_s}\left[nC_{H,s,t} + (1-n)C^*_{H,s,t} + nG_{H,s,t}\right]\left(\frac{p_{H,s,t}(z)}{P_{H,s,t}}\right)^{-\theta_s} \leq A_{H,s}L_{H,s,t}(z)^a. \tag{17}$$

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

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

where $MC_{H,s,t}(z)$ denotes the firm's nominal marginal cost (the Lagrange multiplier in 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.7 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 1.29 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	5.51	employment share of M	20.74%	20.81%
$\phi_{r,M}$	preference M	0.35	basket price ratio	1.00	1.00
χ	labor disutility	1.40	employment to pop.	60.60%	60.60%
θ_m	elasticity m	4.03	average elasticity	4.0	4.0
θ_M	elasticity M	1.59	β_2 of employment reg.	-0.0265	-0.0265
P_C	price level	4881	β_1 of employment reg.	26.79	26.80
ϕ_m	mean m sector fiscal shock	-2.01	mean ARRA ($, mn) pc	0.0003	0.0003
σ_m	stdev. m sector fiscal shock	0.45	stdev. ARRA ($, mn) pc	0.0002	0.0004
ϕ_M	mean M sector fiscal shock	-5.69	mean share ARRA connected	6.52%	6.65%
σ_M	stdev. M sector fiscal shock	2.76	stdev. share ARRA connected	8.57%	8.55%

Notes: The firm and employment shares of M are calculated using NETS data. Following Barnatchez et al. (2017), we exclude firms with less than 10 employees from calculation as this group if over-represented in NETS. The employment-to-population ratio is based on BLS estimates for January 2009. β_1 and β_2 reported in this table are obtained from our baseline estimation of regression (1) in Section 3. The mean and standard deviation of ARRA per millions and share ARRA connected are calculated across the states used in the empirical analysis in Section 3. Table A.6 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 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 in price levels in the steady state, we target a relative price of 1. Note that, although only 1.29 percent of firms are politically connected, they account for 20.74 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 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 ($A^{pc}_{s,T}$), 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

$$G_{s,T} = \alpha + \beta_1 A^{pc}_{s,T} + \beta_2 S_{s,T} + \varepsilon_{s,T}, \tag{19}$$

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). 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.[28] 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 1.7, 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 also explained by connected firms charging higher markups.

[28]One exception is a higher standard deviation on the ARRA expenditure.

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 8.7 jobs. 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 $\beta_2 = -0.0073$, which implies a reduction in jobs multiplier only by 2.4 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

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

Using model simulated data to estimate regression (20), we recover $\beta_{1,GE} = 71.93$ and $\beta_{2,GE} = -0.0157$. Accounting for reallocation across regions increases the baseline effect 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 expenditure costs the aggregate economy 5.2 jobs.

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.[29] 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

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

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.

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 $\beta_{2,GE} = -0.0004$, which implies that the markup channel explains virtually all of the decrease in the aggregate jobs multiplier. This result stems form 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 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 microfound the inefficiency channel to better assess the costs and benefits of political distortions, and to conduct further counterfactual analysis.[30] 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 classic questions related to fiscal spending.[31]

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[30]For 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.

[31]For 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 $A^{pc}_{s,T}$, while accounting for the fact that $S_{i,T} = A^{pc,c}_{i,T}/A^{pc}_{i,T}$, where $A^{pc,c}_{i,T}$ denotes politically connected ARRA spending per capita:

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

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

A.1.2 Robustness

Table A.1 explores omitted factors that the existing literature has explored as potentially being correlated with state employment growth and our DOT instrument. Table A.2 tests the sensitivity of the baseline to our control for labor market flexibility, state capacity, and to accounting for individual campaign contribution limits. Table A.3 tests for additional confounding factors. These include controls for a split state legislature, being a swing state, strictness of individual contribution limits, average firm age and size.

Table A.1: Robustness for ARRA per capita IV

Dependent variable: change in emp-pop ratio

	(1) Baseline	(2) Manu share	(3) Bartik	(4) Housing bust	(5) Anticipation
ARRA spending (mn pc)	26.79**	26.40**	29.36***	29.92**	25.07**
	(10.67)	(10.49)	(10.48)	(12.76)	(10.87)
Frac. connected spending	-0.0265**	-0.0335**	-0.0208*	-0.0303*	-0.0238*
	(0.0134)	(0.0158)	(0.0109)	(0.0160)	(0.0137)
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	5.914	5.815	5.542	4.788	5.764
Obs.	50	50	50	50	50
R-sq	0.39	0.39	0.51	0.40	0.57

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.2: Robustness 1 for Frac Connected Spending IV

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

	(1) Baseline	(2) RTW	(3) Admin capacity	(4) Contrib Limit in 2008
ARRA spending (mn pc)	26.79**	23.99**	31.26**	26.78**
	(10.67)	(9.902)	(12.70)	(10.66)
Frac. connected spending	-0.0265**	-0.0238*	-0.0289*	-0.0257*
	(0.0134)	(0.0130)	(0.0158)	(0.0138)
Division FE	Yes	Yes	Yes	Yes
F-stat	5.9135392	6.1140313	4.7970437	5.8581127
Obs.	50.000	50.000	50.000	50.000
R-sq	.3949696862	.4060297126	.3709173666	.3990347768

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.3: 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 legislature	Swing state	Indiv Contrib Limits	Avg Firm age	Avg Firm size
ARRA spending (mn pc)	26.79**	24.13*	23.64**	32.11***	26.79**	25.07***
	(10.67)	(12.84)	(10.83)	(12.40)	(10.67)	(9.492)
Frac. connected spending	-0.0265**	-0.0253*	-0.0267**	-0.0334**	-0.0265**	-0.0273**
	(0.0134)	(0.0137)	(0.0129)	(0.0154)	(0.0134)	(0.0134)
Full controls	Yes	Yes	Yes	Yes	Yes	Yes
F-stat	5.914	5.484	5.555	5.607	5.914	7.282
Obs.	50	50	50	50	50	50
R-sq	0.39	0.43	0.43	0.37	0.39	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 Firm Level Analysis

A.2.1 Political Connections and Grant Allocation

Table A.4 shows the baseline in Column (1). Results from estimating Equation (5) with firm size controls are reported in Column (2). Column (3) shows estimates when close elections are defined using a 3% margin of victory.

Table A.5 shows results from placebo tests for whether being connected in state s has any effect on winning ARRA grants as prime vendors in state $z \neq s$ (col 1), or as sub vendors in state s (col 2). Column (3) shows the results from estimating Equation (5) using 3% margin of victory as a tighter definition of close elections. Column (4) shows the results from using $Frac(win)$ instead of $Connected$.

Table A.4: The Effect of Political Connections on Winning a Grant; Full Table

	(1) Win	(2) Win	(3) Win
Connected	0.828***	0.806**	0.901**
	(0.298)	(0.302)	(0.341)
Headquarter		3.095**	2.586
		(1.214)	(1.675)
Constant	3.609***	1.365	2.442**
	(0.175)	(0.857)	(1.166)
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.	8033	8033	5390
R-sq	0.36	0.38	0.41

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. *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.5: Robustness: Effect of Political Connections on Grant Allocation

	(1) Grant PV Other	(2) Grant SV	(3) Win	(4) Win
Connected	-0.326	0.225	0.743**	
	(0.564)	(0.300)	(0.341)	
Frac(Win)				0.858**
				(0.361)
Constant	10.237***	3.229***	4.335***	3.633***
	(0.331)	(0.176)	(0.195)	(0.195)
NAICS4 x State FE	Yes	Yes	Yes	Yes
NumCandCE FE	Yes	Yes	Yes	Yes
Margin of Victory	5%	5%	3%	5%
Obs.	8033	8033	5390	8033
R-sq	0.46	0.38	0.39	0.36

Notes: Unit of analysis is firm × 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. $Frac(Win)$ 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. SEs are clustered two ways, by state and industry.

A.3 Externally Calibrated Model Parameters

Table A.6: 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.

B Online Appendix

B.1 Data Construction: Merging Details

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

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

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

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

    - Names are standardized to improve match quality. This procedure involves capitalization, elimination of special characters, standardization of company type (e.g., COMPANY changed to CO), and standardization of common words (e.g., variations of the word PRODUCT to

PROD). The first and longest words of the name are saved as separate variables to be used later in merging.

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

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

1. When available, the first match pass is based on the Dunsnumber. This step is only possible when matching NETS to the Recipient Report data. While we give preference to matches made based on dunsnumber, we reconsider all records with valid name and address information for the name and address matching steps described below.

2. The second match pass links records where the company name matches exactly. After this pass, we once again reconsider all records for the subsequent name and address matching passes.

3. The third match pass links records that match exactly on the 5-digit zip code, exactly on the longest or first word of the company name, and have similar full company names based on the Levenshtein distance and Jaro-Winkler score.

4. The fourth match pass links records that match exactly on the 3-digit zip code, exactly on the longest or first word of the company name, and have similar full company names based on the Levenshtein distance and Jaro-Winkler score.

5. The fifth match pass links records that match exactly on the state code, exactly on the longest or first word of the company name, and have similar full company names.

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

1. Records that match on Dunsnumber are always given preference.

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

3. For the remaining multiple matches, we disambiguate them using the name Jaro-Winkler score, bigram string distance for name, overlap in years of business activity and campaign contribution / ARRA contracts, and firm size.[32]

4. We break ties for the remaining multiple matches randomly. The share of matches subject to the random tie-breaking is less than five percent.

5. One reason why there are multiple matches is because businesses in NETS often appear in the data multiple times with different Dunsnumbers and even with different headquarter Dunsnumbers, but with the same name and address. This occurs, for example, when businesses apply for credit rating in multiple occasions and the NETS fails to recognize that those entities are in fact the same business. To address this issue, we collect firms in NETS with the same name and address in the same year but different headquarter

[32]Because microenterprises are known to have high imputation rates, when there are duplicate matches, we prefer firms that have five or more employees.

Dunsnumbers, and choose the firm identifier that has the most legitimate information (e.g., nonmissing credit rating, nomissing employment) as the identifier used for matching.

Merging Evaluation: Our merging procedure identifies 56 percent of prime vendors from the Recipient Report data in NETS. These firms account for 67 percent of records and 86 percent of the grant dollar value. Our merging procedure identifies 34 percent of contributors from NIMP in NETS. These firms account for 43 percent of contribution records and 44 percent of their value. It is worth noting that while we drop individual, party, and non-contribution records from NIMP data before matching, non-business entities remain in our data. For example, 52 percent of the unmatched records are associated with labor unions, business associations, and political committees. These entities explain the vast majority of the unmatched records.

B.2 Firms in State Politics and Federal Grants

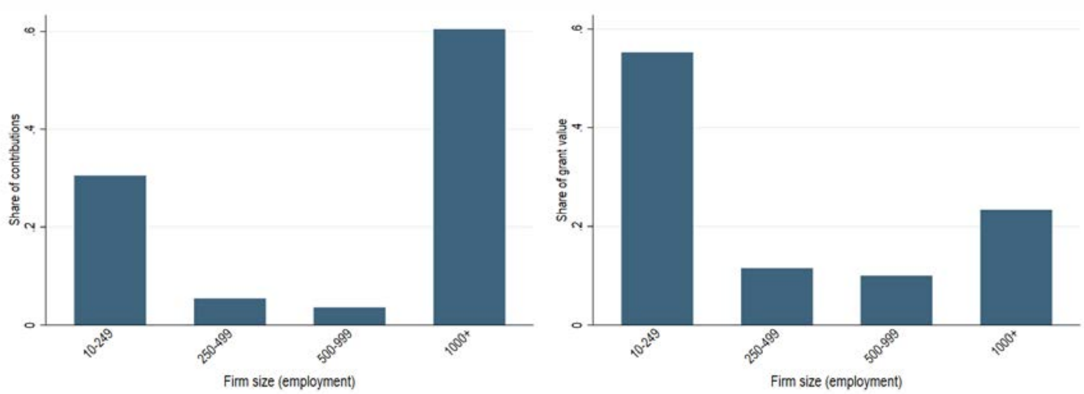
Our data, which combines information on firm characteristics from NETS, allocation of grants from the ARRA Recipient Report, and campaign contributions from NIMP, reveals three facts. First, private-sector businesses actively engage in local elections via campaign contributions. On average, firms contribute a total of \$5,000 across five candidates per election cycle, or roughly \$1,000 per candidate. Firms account for at least 13 percent of all state campaign contribution cases and 23 percent of their dollar amount.[33] 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.

[33] Recall 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.

Second, small- and medium-sized enterprises (SMEs) account for a little over one-third of firm campaign contributions. Figure B.1a shows the share of total corporate campaign contribution amount accounted for by each firm size group. Combining the first two groups, firms with fewer than 500 employees account for nearly 36 percent of campaign contributions. This finding highlights that corporate political activities are not only done by large firms. While large firms generally dominate 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, 2.6 grants, and the average size of each grant was over $385,000. To show this, Figure B.1b depicts the share of grant value accounted for by each firm size group. Specifically, 67 percent of ARRA grant spending to prime vendors went to SMEs.

Figure B.1: Campaign contribution & grant shares, by firm size



(a) Campaign contributions (b) Grants

Notes: Left figure plots the dollar share of campaign finance contributions by firm size, and right figure plots the dollar share of ARRA grants awarded by firm size. Firm size is measured by number of employees in 2008 (NETS). Corporate campaign contributions are measured using data on campaign contributions by firms in state legislative elections between 2006 and 2008 (NIMP). 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.

B.3 Addressing Citizens United

In January of 2010, the United States Supreme Court announced its ruling in *Citizens United vs. Federal Election Commission (FEC)* that would significantly alter campaign finance laws. This decision overturned a twenty-year precedent, previously prohibiting corporations and unions from engaging in independent political expenditures. It effectively lifted restrictions, allowing these entities to participate in independent political expenditures with minimal limitations. Given this significant change to campaign finance laws, there may be a concern that the ruling might undermine the validity of our jobs multiplier estimation.

In this section, we argue that the ruling does not undermine our empirical strategy in estimating the jobs multiplier, nor does it affect our findings. First, we differentiate between the campaign finance laws that we leverage in our analysis and the ones that were affected by the *Citizens United* decision and discuss why the ruling does not materially impact corporate campaign contributions or associated laws, which we focus on in our analysis. Second, we compare the timing of our analysis to the timing of Citizens United. Specifically, we discuss the construction of our endogenous and instrumental variables and argue that they predate the Citizens United ruling, and thus, would not be affected by it. Finally, we conduct robustness checks to reinforce our argument.

The Citizens United ruling impacted independent political expenditure laws, but our paper exclusively focuses on direct corporate campaign contributions to measure politically connected spending. Independent political expenditure is defined by the FEC to be "expenditure for a communication that expressly advocates the election or defeat of a clearly identified candidate and which is not made in coordination with any candidate or their campaign or political party."[34] The Citizens United ruling allowed for corporations and unions to spend money on political advertising. They could do so by spending money independently or through contributions to various types of advocacy groups, like super-PACs, but it did not change the regulations for how corporations contribute directly to campaigns (Klumpp et al., 2016).

[34] See the Federal Election Commission website: https://www.fec.gov/help-candidates-and-committees/candidate-taking-receipts/understanding-independent-expenditures/ (accessed on March 19, 2024).

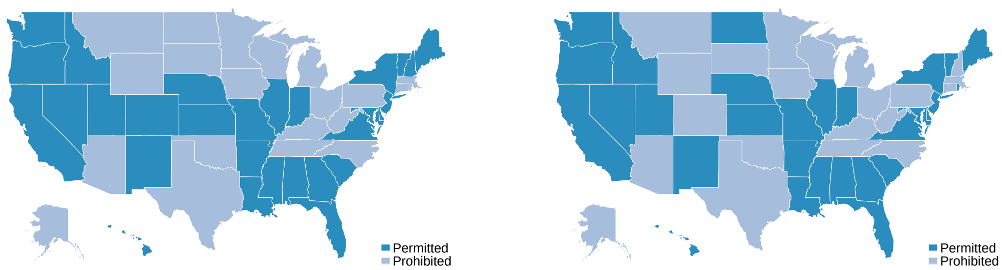
We focus on corporate campaign contributions when constructing our endogenous variable of interest—fraction of politically connected spending. We define politically connected spending as the value of prime vendor ARRA grants allocated to firms that directly contributed to winning candidates in sate legislative elections. To measure politically connected spending, we use campaign contribution data for 2006-2008 from the National Institute of Money in Politics (NIMP). Other data, including independent political expenditure, is available for download, but was not used in our construction of the amount of politically connected spending. We drop contributions made by non-individual contributors and those in the broad sectors party and non-contributions. We then use probabilistic name and address matching of corporate contributors to firms in NETS, as well as firms in NETS to Recovery Act data on grants and contracts. By doing so, we can identify at the firm level which firms made campaign contributions to winning candidates in state legislative elections, and whether or not these firms ultimately won prime vendor ARRA grants. Our focus on direct corporate campaign contributions should alleviate concerns that our estimates would be affected by changes to independent expenditure as a result of the Citizens United ruling.

Although we precisely target direct corporate campaign contributions, there may still be concern about correlation between the campaign laws we use to construct our instrument and the laws on independent political expenditure. To rectify this concern, we discuss the construction of our instrumental variable and then compare it to independent political expenditure laws.

We define our instrument using 2002 state election laws for whether a state prohibits or permits corporate campaign contributions. In the construction, we codify Illinois, which allows for unlimited corporate campaign contributions, and Arkansas, which limits contributions to $1,000 per candidate per election, as permitting corporate campaign contributions. We consider only states that explicitly do not allow any corporate campaign contributions, like Pennsylvania, as prohibiting them.

Figure B.2a compares compares the map of states that prohibited versus permitted corporate campaign contributions in 2002 to the map of states that prohibit versus permit independent political expenditure in the years leading up to Citizens United (Spencer and Wood, 2014). The figure makes clear that there is not a perfect correlation between states that prohibited corporate campaign contribution and those that prohibited independent political expenditure prior to the Citizens United ruling. Specifically, four states—New Hampshire, Colorado, North Dakota, and Rhode Island—have different laws for the two categories preceding the Citizens United ruling.

Figure B.2: Corporate Campaign Contributions and Independent Political Expenditure



(a) Corporate campaign contribution limits (b) Independent political expenditure

Notes: Left figure shows whether states permit or prohibit political contributions by corporations. Right figure shows whether states permit or prohibit independent political expenditures. Note that there are four states that have different rules when it comes to independent political expenditure vs. corporate campaign contribution limits (prohibit vs. permit): CO, ND, RI, NH, and RI.

An additional reason that our analysis should not be affected by the Citizens United ruling is that the timing of the ruling and its impact does not overlap with our analysis period. The endogenous variable is defined as the share of ARRA spending channeled through politically connected firms. We consider prime vendor ARRA grants allocated to firms between February 2009 and December 2010. These firms are identified as being politically connected if, in the 2006, 2007, or 2008 elections, they contributed to the campaign of a winning candidate for the state legislature, where the seat would be held until at least the end of 2010. Therefore, these grants could only be influenced by state politicians that are already in power in 2009 or 2010, whereas the Citizens United ruling could only affect politicians that are elected in 2010 elections and then enter the state legislature in early 2011.

Given our focus on direct corporate campaign contributions made during the 2006–2008 election cycles, we align our instrumental variable with this timing by using 2002 state election laws (with robustness done using 2008 state election laws). Therefore, the instrument, like the endogenous variable, predates the 2010 Citizens United ruling. In fact, there is evidence that the Citizens United ruling did not impact spending until the 2012 election cycle, which also is after the end of our analysis period.[35]

To further verify that the *Citizens United* ruling does not affect our research design, we perform robustness additional robustness analysis. Table B.1 shows two robustness exercises and compares them to our baseline results, which are reported in Column (1). In Column (2) we instrument for the share of politically connected spending with an indicator of whether independent political expenditure is permitted or prohibited in the state in the years prior to Citizens United. We find that, in contrast to our baseline estimates, that the coefficient on the share of politically connected spending is close to zero and insignificant. We interpret these results as showing that our baseline results are capturing direct corporate campaign contributions, rather than independent political expenditure.

In order to address concerns about the timing of the Citizens United ruling and its impact, we test whether our results hold if we only consider ARRA spending in 2009. These results are depicted in Column (3) of Table B.1. It removes any funding that may have been allocated in 2010 after the Supreme Court announced the Citizens United ruling. Our baseline the results hold both in terms of magnitude and significance. This supports our claim that the timing of the Citizens United should not impact our analysis.

Overall, Table B.1 solidifies our claim that the Citizens United ruling does not materially affect our empirical strategy in estimating the jobs multiplier, or the associated results.

B.4 State Level Analysis

B.4.1 Summary Statistics

Table B.2 reports the summary statistics for all the variables used in our analysis.

[35] For example, see Evers-Hillstrom et al. (2020).

Table B.1: Robustness related to Citizens United

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

	(1)	(2)	(3)
	Baseline	Indep Poli Expend	2009 ARRA only
ARRA spending (mn pc)	26.79**	26.46**	34.68**
	(10.67)	(10.32)	(14.68)
Frac. connected spending	-0.0265**	-0.00578	-0.0251**
	(0.0134)	(0.0177)	(0.0120)
Division FE	Yes	Yes	Yes
ARRA years	2009-2010	2009-2010	2009
F-stat	5.914	6.544	4.887
Obs.	50	50	50
R-sq	0.39	0.45	0.32

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. In columns (1) and (2) ARRA spending in calculated based on Feb. 2009 through Dec 2010. spending, while in column (3) only spending between Feb. 2009 and Dec 2009 is considered. Columns (1) and (3) include two IVs: anticipated DOT spending and an indicator of whether a state permits corporate campaign contributions. Column (2) also includes the two IVs: anticipated DOT spending and an indicator of whether a state permits or prohibits independent political expenditure. 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. ***, **, and * indicate sig. at the 1%, 5%, and 10% sig. levels. The F-stat test statistic is reported. Robust SEs.

Table B.2: Summary Statistics

	Mean	SD	Min	Max	N
A) Dependent variable					
Emp growth pc (Feb 09 - Dec 10)	-0.007	0.007	-0.025	0.027	50
B) Explanatory variables					
ARRA spending ($, ths.) pc	0.304	0.191	0.096	1.272	50
Frac. connected spending	0.065	0.086	0.000	0.352	50
Emp growth (07-09)	-0.051	0.024	-0.119	-0.006	50
Emp pc (09)	0.447	0.040	0.377	0.551	50
HPI growth (03-07)	0.218	0.118	-0.113	0.422	50
Change in PI moving avg	0.001	0.000	-0.000	0.003	50
Tax benefits ($, ths) pc	0.587	0.114	0.451	0.954	50
Union membership (08)	0.114	0.058	0.035	0.249	50
Corruption (dummy)	0.500	0.505	0.000	1.000	50
C) Instrumental variables					
DOT IV ($, ths.) pc	0.163	0.071	0.114	0.454	50
Corp contrib (dummy)	0.580	0.499	0.000	1.000	50

B.4.2 Baseline Results

First stage: Table B.3 reports full baseline first stage regression.

Second stage: Table B.4 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 B.3: Baseline: First stage results

	(1) ARRA spending (mn pc)	(2) Frac connected ARRA
DOT IV (ths pc)	1.690***	0.469**
	(0.530)	(0.195)
Corp contrib (dummy)	0.013	0.136***
	(0.031)	(0.025)
Emp growth (07-09)	2.615*	-0.141
	(1.460)	(0.559)
Emp pc (09)	-0.627	-0.172
	(0.843)	(0.348)
Change in PI moving avg	-85.571*	52.125**
	(47.794)	(23.008)
HPI growth (03-07)	-0.098	-0.049
	(0.330)	(0.125)
Tax benefits (mn pc)	51.064	-281.272*
	(312.707)	(147.120)
Corruption (dummy)	-0.010	-0.027
	(0.041)	(0.022)
Union membership (08)	-0.003	-0.145
	(0.606)	(0.232)
Constant	0.385	0.243
	(0.388)	(0.152)
Full controls	Yes	Yes
Division FE	Yes	Yes
Obs.	50	50
R-sq	0.74	0.68

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.

Table B.4: Baseline: Second stage results

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

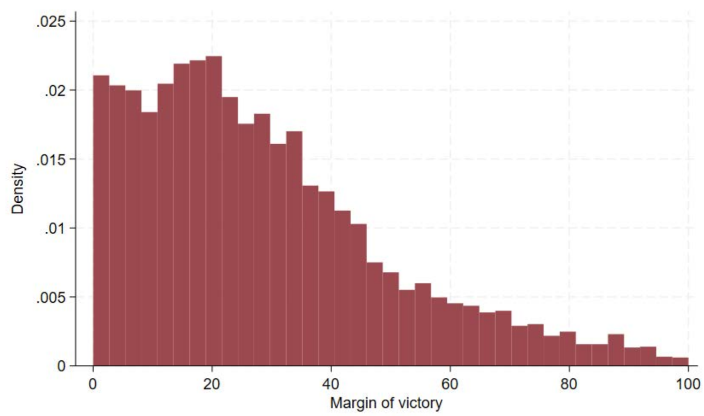
	(1) No controls	(2) Wilson Controls	(3) Wilson & Division FE	(4) Baseline
ARRA spending (mn pc)	21.32	14.65**	24.37**	26.79**
	(13.27)	(6.342)	(11.13)	(10.67)
Frac. connected spending	-0.0366*	-0.0264*	-0.0223*	-0.0265**
	(0.0222)	(0.0141)	(0.0116)	(0.0134)
Emp growth (07-09)		0.127***	0.0179	0.0194
		(0.0348)	(0.0542)	(0.0599)
Emp pc (09)		0.00780	0.0297	0.0226
		(0.0379)	(0.0383)	(0.0363)
Change in PI moving avg		-4.686**	-2.700	-2.722
		(2.265)	(2.312)	(2.260)
HPI growth (03-07)		0.00350	0.0125	0.0114
		(0.00874)	(0.0122)	(0.0110)
Tax benefits (mn pc)		1.213	-5.329	0.528
		(6.170)	(8.349)	(10.10)
Corruption (dummy)				-0.000458
				(0.00149)
Union membership (08)				-0.0451*
				(0.0273)
Constant	-0.0109***	-0.00431	-0.0186	-0.0130
	(0.00285)	(0.0148)	(0.0190)	(0.0183)
ARRA controls	No	Yes	Yes	Yes
Connected controls	No	No	No	Yes
Division FE	No	No	Yes	Yes
F-stat	8.916	8.767	6.191	5.914
Obs.	50	50	50	50
R-sq	-0.22	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.

B.5 Political Connections and Employment: Firm Level

Figure B.3 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.

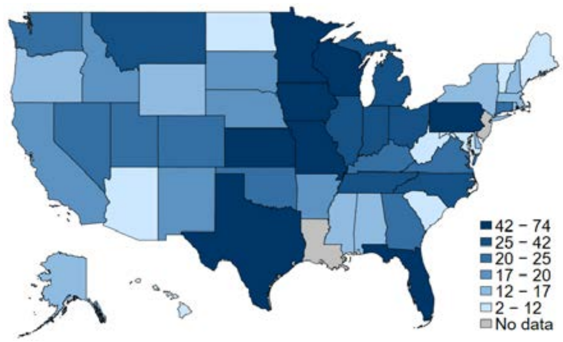
Figure B.3: Margin of Victory in State Legislative Elections (2006-2008)



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 B.4 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 B.4: 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.

Table B.5 reports the distribution of the number of candidates firms support in close elections in each state.

Table B.5: Number of Candidates a Firm Supports in Close Elections in a State

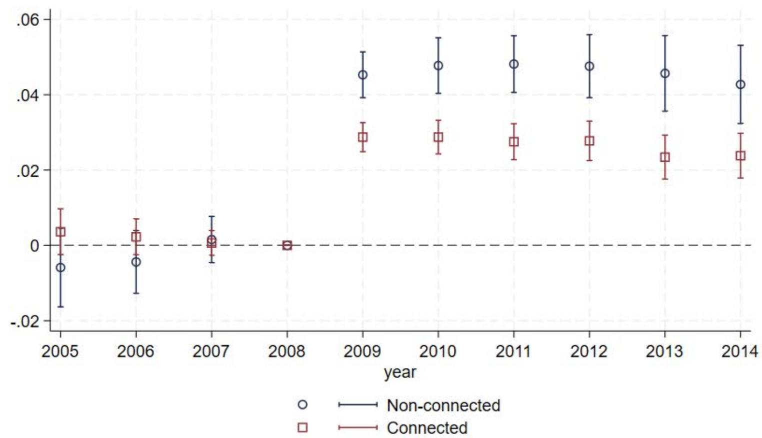
	(1) Frequency	Percent
1	7194	66.3
2	1171	10.8
3	627	5.8
4	408	3.8
5	286	2.6
6+	1160	10.7
Total	10846	100.0

B.6 Robustness: Differential Employment Effects

Figure B.5 shows that the results from estimating Equation (6) while only using nonimputed observations at the establishment-year level. Because nonimputed observations are not necessarily representative of the underlying dataset, we estimate a propensity score using state, two digit NAICS industry, year, establishment age, and establishment size (log employment) as explanatory variables.

Then we weight each observation by the inverse of the propensity scores. We find that the results are similar to the baseline results shown in Figure 6.

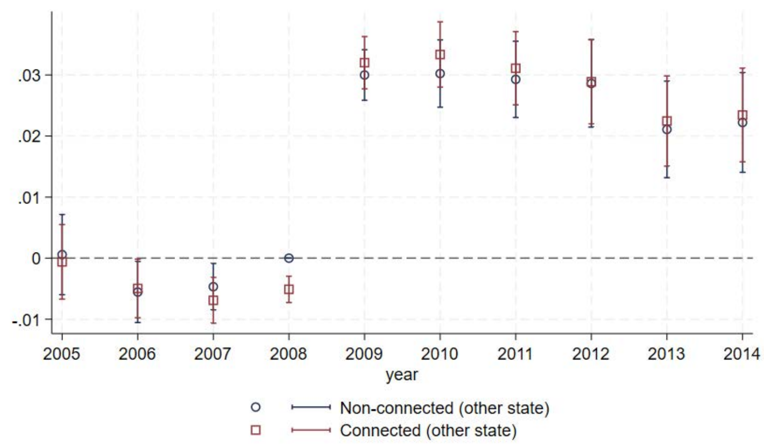
Figure B.5: Establishment-level Employment after winning an ARRA Grant: Only Nonimputed Observations



Notes: Unit of analysis is establishment × year. The figure displays the effects of a firm winning a grant in state s to its establishments in that state when the firm did not gain political connections (blue circles) and when the firm did gain political connections (red squares) in that state. Only nonimputed observations are used. We weight each observation by the inverse propensity score probabilities of being nonimputed to make the sample representative of the full sample. Standard errors are clustered at the establishment level and the error bands indicate 95% confidence intervals.

Figure B.6 shows that the results shown in Figure 7 are robust to using only nonimputed observations.

Figure B.6: Employment Growth: Placebo, Only Nonimputed Observations



Notes: Unit of analysis is establishment × year. The figure displays the effects of a firm winning a grant in state s to its establishments in that state when the firm did not gain political connections (blue circles) and when the firm did gain political connections (red squares) in some state $z \neq s$. 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. Standard errors are clustered at the establishment level and the error bands indicate 95% confidence intervals.

B.7 A Generalized Equilibrium Model: Linearized System of Equations

1. Home consumption Euler equation

$$\hat{c}_t = E_t \hat{c}_{t+1} - \sigma \left(\hat{r}_t^n - E_t \hat{\pi}_{t+1}^C \right) \tag{22}$$

2. Backus-Smith condition

$$\hat{c}_t^* - \hat{c}_t = \sigma \hat{q}_t^C \tag{23}$$

3. Home Phillips curve, politically connected sector

$$\hat{\pi}_{H,M,t} = \beta E_t \hat{\pi}_{H,M,t+1} + \kappa \zeta_{H,M,a} \left[(\psi_a + \psi_{H,M}) \hat{y}_{H,M,t} + \psi_{H,m} \hat{y}_{H,m,t} - \hat{p}_{H,M,t} + \sigma^{-1} \hat{c}_t \right] \tag{24}$$

4. Home Phillips curve, non-connected sector

$$\hat{\pi}_{H,m,t} = \beta E_t \hat{\pi}_{H,m,t+1} + \kappa \zeta_{m,a} \left[(\psi_a + \psi_{H,m}) \hat{y}_{H,m,t} + \psi_{H,M} \hat{y}_{H,M,t} - \hat{p}_{H,m,t} + \sigma^{-1} \hat{c}_t \right] \tag{25}$$

5. Foreign Phillips curve, politically connected sector

$$\hat{\pi}_{F,M,t} = \beta E_t \hat{\pi}_{F,M,t+1} + \kappa \zeta_{M,a} \left[\hat{q}_t^C + (\psi_a + \psi_{F,M}) \hat{y}_{F,M,t} + \psi_{F,m} \hat{y}_{F,m,t} - \hat{p}_{F,M,t} + \sigma^{-1} \hat{c}_t^* \right] \tag{26}$$

6. Foreign Phillips curve, non-connected sector

$$\hat{\pi}_{F,m,t} = \beta E_t \hat{\pi}_{F,m,t+1} + \kappa \zeta_{m,a} \left[\hat{q}_t^C + (\psi_a + \psi_{F,m}) \hat{y}_{F,m,t} + \psi_{F,M} \hat{y}_{F,M,t} - \hat{p}_{F,m,t} + \sigma^{-1} \hat{c}_t^* \right] \tag{27}$$

7. Home CPI inflation

$$\hat{\pi}_t^C = \phi_H \phi_{H,M} \left(\frac{P_H^C}{P^C}\right)^{(\eta_s-\eta_r)} \left(\frac{P_{H,M}}{P^C}\right)^{1-\eta_s} \hat{\pi}_{H,M,t} + \phi_H \phi_{H,m} \left(\frac{P_H^C}{P^C}\right)^{(\eta_s-\eta_r)} \left(\frac{P_{H,m}}{P^C}\right)^{1-\eta_s} \hat{\pi}_{H,m,t}$$
$$+ \phi_F \phi_{F,M} \left(\frac{P_F^C}{P^C}\right)^{(\eta_s-\eta_r)} \left(\frac{P_{F,M}}{P^C}\right)^{1-\eta_s} \hat{\pi}_{F,M,t} + \phi_F \phi_{F,m} \left(\frac{P_F^C}{P^C}\right)^{(\eta_s-\eta_r)} \left(\frac{P_{F,m}}{P^C}\right)^{1-\eta_s} \hat{\pi}_{F,m,t} \tag{28}$$

8. Foreign CPI inflation

$$\hat{\pi}_t^{C*} = \phi_H^* \left(\frac{P_H^{C*}}{P^{C*}}\right)^{1-\eta_r} \hat{\pi}_{H,t}^{C*} + \phi_F^* \left(\frac{P_F^{C*}}{P^{C*}}\right)^{1-\eta_r} \hat{\pi}_{F,t}^{C*} \tag{29}$$

$$= \left(\frac{P_H^{C*}}{P^C}\right)^{(\eta_s-\eta_r)} \left(\frac{P^C}{P^{C*}}\right)^{1-\eta_r} \left[\phi_H^* \phi_{H,M}^* \left(\frac{P_{H,M}}{P^{C*}}\right)^{1-\eta_s} \hat{\pi}_{H,M,t} + \phi_H^* \phi_{H,m}^* \left(\frac{P_{H,m}}{P^{C*}}\right)^{1-\eta_s} \hat{\pi}_{H,m,t}\right]$$
$$+ \left(\frac{P_F^{C*}}{P^C}\right)^{(\eta_s-\eta_r)} \left(\frac{P^C}{P^{C*}}\right)^{1-\eta_r} \left[\phi_F^* \phi_{F,M}^* \left(\frac{P_{F,M}}{P^{C*}}\right)^{1-\eta_s} \hat{\pi}_{F,M,t} + \phi_F^* \phi_{F,m}^* \left(\frac{P_{F,m}}{P^{C*}}\right)^{1-\eta_s} \hat{\pi}_{F,m,t}\right] \tag{30}$$

9. Home resource constraint (per capita), politically connected sector

$$\hat{y}_{H,M,t} = \frac{C_{H,M}}{Y_{H,M}} \left[\hat{c}_t - (\eta_r - \eta_s)\, \hat{p}_{H,t}^C - \eta_s \hat{p}_{H,M,t}\right]$$
$$+ \frac{(1-n)}{n} \frac{C_{H,M}^*}{Y_{H,M}} \left[\hat{c}_t^* - (\eta_r - \eta_s)\, \hat{p}_{H,t}^{C*} + \eta_r \hat{q}_t^c - \eta_s \hat{p}_{H,M,t}\right] + \hat{g}_{H,M,t} \tag{31}$$

10. Home resource constraint (per capita), non-connected sector

$$\hat{y}_{H,m,t} = \frac{C_{H,m}}{Y_{H,m}} \left[\hat{c}_t - (\eta_r - \eta_s)\, \hat{p}_{H,t}^C - \eta_s \hat{p}_{H,m,t}\right]$$
$$+ \frac{(1-n)}{n} \frac{C_{H,m}^*}{Y_{H,m}} \left[\hat{c}_t^* - (\eta_r - \eta_s)\, \hat{p}_{H,t}^{C*} + \eta_r \hat{q}_t^c - \eta_s \hat{p}_{H,m,t}\right] + \hat{g}_{H,m,t} \tag{32}$$

11. Foreign resource constraint (per capita), politically connected sector

$$\hat{y}_{F,M,t} = \frac{nC_{F,M}}{(1-n)Y_{F,M}} \left[\hat{c}_t - (\eta_r - \eta_s)\,\hat{p}^{C}_{F,t} - \eta_s \hat{p}_{F,M,t}\right] + \frac{C^{*}_{F,M}}{Y_{F,M}} \left[\hat{c}^{*}_t - (\eta_r - \eta_s)\,\hat{p}^{C*}_{F,t} + \eta_r \hat{q}^{c}_t - \eta_s \hat{p}_{F,M,t}\right] + \hat{g}_{F,M,t} \tag{33}$$

12. Foreign resource constraint (per capita), non-connected sector

$$\hat{y}_{F,m,t} = \frac{nC_{F,m}}{(1-n)Y_{F,m}} \left[\hat{c}_t - (\eta_r - \eta_s)\,\hat{p}^{C}_{F,t} - \eta_s \hat{p}_{F,m,t}\right] + \frac{C^{*}_{F,m}}{Y_{F,m}} \left[\hat{c}^{*}_t - (\eta_r - \eta_s)\,\hat{p}^{C*}_{F,t} + \eta_r \hat{q}^{c}_t - \eta_s \hat{p}_{F,m,t}\right] + \hat{g}_{F,m,t} \tag{34}$$

13. Real per capita home output

$$\hat{y}_{H,t} = \frac{P_{H,M}Y_{H,M}}{P_H Y_H}\hat{y}_{H,M,t} + \frac{P_{H,m}Y_{H,m}}{P_H Y_H}\hat{y}_{H,m,t} \tag{35}$$

with $P_H = 1$.

14. Real per capita foreign output

$$\hat{y}_{F,t} = \frac{P_{F,M}Y_{F,M}}{P_F Y_F}\hat{y}_{F,M,t} + \frac{P_{F,m}Y_{F,m}}{P_F Y_F}\hat{y}_{F,m,t} \tag{36}$$

with $P_F = 1$.

15. Real aggregate output

$$\hat{y}_t = \frac{nP_H Y_H}{PY}\hat{y}_{H,t} + \frac{(1-n)P_F Y_F}{PY}\hat{y}_{F,t} \tag{37}$$

with $P_H = P_F = P = 1$.

16. Home relative price, politically connected sector

$$\hat{p}_{H,M,t+1} - \hat{p}_{H,M,t} = \hat{\pi}_{H,M,t+1} - \hat{\pi}^{C}_{t+1} \tag{38}$$

17. Home relative price, non-connected sector

$$\hat{p}_{H,m,t} = \frac{1}{\phi^C_{H,m}\left(\frac{P_{H,m}}{P^C_H}\right)^{1-\eta_s}}\left[\hat{p}^C_{H,t+1} - \phi^C_{H,M}\left(\frac{P_{H,M}}{P^C_H}\right)^{1-\eta_s}\hat{p}_{H,M,t+1}\right] \quad (39)$$

18. Foreign relative price, politically connected sector

$$\hat{p}_{F,M,t+1} - \hat{p}_{F,M,t} = \hat{\pi}_{F,M,t+1} - \hat{\pi}^C_{t+1} \quad (40)$$

19. Foreign relative price, non-connected sector

$$\hat{p}_{F,m,t} = \frac{1}{\phi^C_{F,m}\left(\frac{P_{F,m}}{P^C_F}\right)^{1-\eta_s}}\left[\hat{p}^C_{F,t+1} - \phi^C_{F,M}\left(\frac{P_{F,M}}{P^C_F}\right)^{1-\eta_s}\hat{p}_{F,M,t+1}\right] \quad (41)$$

20. Home consumer price, domestic goods

$$\hat{p}^C_{H,t+1} - \hat{p}^C_{H,t} = \hat{\pi}^C_{H,t+1} - \hat{\pi}^C_{t+1} \quad (42)$$

21. Home consumer price, foreign goods

$$\hat{p}^C_{F,t} = -\frac{\phi_H}{\phi_F}\left(\frac{P^C_H}{P^C_F}\right)^{1-\eta_r}\hat{p}^C_{H,t} \quad (43)$$

22. Foreign consumer price, domestic goods

$$\hat{p}^{C*}_{H,t} = \phi^*_{H,M}\left(\frac{P_{H,M}}{P^{C*}_H}\right)^{1-\eta_s}\hat{p}_{H,M,t} + \phi^*_{H,m}\left(\frac{P_{H,m}}{P^{C*}_H}\right)^{1-\eta_s}\hat{p}_{H,m,t} \quad (44)$$

23. Foreign consumer price, foreign goods

$$\hat{p}^{C*}_{F,t} = \phi^*_{F,M}\left(\frac{P_{F,M}}{P^{C*}_F}\right)^{1-\eta_s}\hat{p}_{F,M,t} + \phi^*_{F,m}\left(\frac{P_{F,m}}{P^{C*}_F}\right)^{1-\eta_s}\hat{p}_{F,m,t} \quad (45)$$

24. Home GDP deflator

$$\hat{p}_{H,t} = \frac{Y_{H,M}P_{H,M}}{P_H Y_H}\hat{p}_{H,M,t} + \frac{Y_{H,m}P_{H,m}}{P_H Y_H}\hat{p}_{H,m,t} \tag{46}$$

where $\hat{p}_{H,t} = (P_{H,t}/P_t^C - P_H/P^C)/(P_H/P^C)$ and $P_H = 1$.

25. Foreign GDP deflator

$$\hat{p}_{F,t} = \frac{Y_{F,M}P_{F,M}}{P_F Y_F}\hat{p}_{F,M,t} + \frac{Y_{F,m}P_{F,m}}{P_F Y_F}\hat{p}_{F,m,t} \tag{47}$$

where $\hat{p}_{F,t} = (P_{F,t}/P_t^C - P_F/P^C)/(P_F/P^C)$ and $P_F = 1$.

26. Aggregate GDP deflator

$$\hat{p}_t = \frac{nP_H Y_H}{PY}\hat{p}_{H,t} + \frac{(1-n)P_F Y_F}{PY}\hat{p}_{F,t} \tag{48}$$

where $\hat{p}_t = (P_t/P_t^C - P/P^C)/(P/P^C)$ and $P_H = P_F = P = 1$.

27. Real exchange rate

$$\begin{aligned}\hat{q}_t^c = {} & \left(\frac{P_H^{C*}}{P^C}\right)^{(\eta_s-\eta_r)}\left(\frac{P^C}{P^{C*}}\right)^{1-\eta_r}\left[\phi_H^{C*}\phi_{H,M}^{C*}\left(\frac{P_{H,M}}{P^{C*}}\right)^{1-\eta_s}\hat{p}_{H,M,t} + \phi_H^{C*}\phi_{H,m}^{C*}\left(\frac{P_{H,m}}{P^{C*}}\right)^{1-\eta_s}\hat{p}_{H,m,t}\right] \\ & + \left(\frac{P_F^{C*}}{P^C}\right)^{(\eta_s-\eta_r)}\left(\frac{P^C}{P^{C*}}\right)^{1-\eta_r}\left[\phi_F^{C*}\phi_{F,M}^{C*}\left(\frac{P_{F,M}}{P^{C*}}\right)^{1-\eta_s}\hat{p}_{F,M,t} + \phi_F^{C*}\phi_{F,m}^{C*}\left(\frac{P_{F,m}}{P^{C*}}\right)^{1-\eta_s}\hat{p}_{F,m,t}\right]\end{aligned} \tag{49}$$

28. Monetary policy

$$\hat{r}_t^n = \rho_r \hat{r}_{t-1}^n + (1-\rho_i)\left(\phi_\pi \hat{\pi}_t^{ag} + \phi_y \hat{y}_t^{ag} + \phi_g \hat{g}_t^{ag}\right) \tag{50}$$

29. Home government spending, politically connected sector

$$\hat{g}_{H,M,t} = \rho_G \hat{g}_{H,M,t-1} + \epsilon_{H,M,t} \tag{51}$$

30. Home government spending, non-connected sector

$$\hat{g}_{H,m,t} = \rho_G \hat{g}_{H,m,t-1} + \epsilon_{H,m,t} \tag{52}$$

31. Foreign government spending, politically connected sector

$$\hat{g}_{F,M,t} = \rho_G \hat{g}_{F,M,t-1} + \epsilon_{F,M,t} \tag{53}$$

32. Foreign government spending, non-connected sector

$$\hat{g}_{F,m,t} = \rho_G \hat{g}_{F,m,t-1} + \epsilon_{F,m,t} \tag{54}$$

33. Expectational error (η) equations

$$\hat{c}_t - E_t \hat{c}_{t+1} = \Pi_1 \eta_{1,t} \tag{55}$$

$$\hat{c}_t^* - E_t \hat{c}_{t+1}^* = \Pi_2 \eta_{1,t} \tag{56}$$

$$\hat{\pi}_{H,M,t} - E_t \hat{\pi}_{H,M,t+1} = \Pi_3 \eta_{1,t} \tag{57}$$

$$\hat{\pi}_{H,m,t} - E_t \hat{\pi}_{H,m,t+1} = \Pi_4 \eta_{1,t} \tag{58}$$

$$\hat{\pi}_{F,M,t} - E_t \hat{\pi}_{F,M,t+1} = \Pi_5 \eta_{1,t} \tag{59}$$

$$\hat{\pi}_{F,m,t} - E_t \hat{\pi}_{F,m,t+1} = \Pi_6 \eta_{1,t} \tag{60}$$

34. Home labor, politically connected per capita

$$\hat{y}_{H,M,t} = a\hat{L}_{M,t} \tag{61}$$

35. Home labor, non-connected per capita

$$\hat{y}_{H,m,t} = a\hat{L}_{m,t} \tag{62}$$

36. Foreign labor, politically connected per capita

$$\hat{y}_{F,M,t} = a\hat{L}_{F,M,t} \tag{63}$$

37. Foreign labor, non-connected per capita

$$\hat{y}_{F,m,t} = a\hat{L}_{F,m,t} \tag{64}$$

38. Home labor per capita

$$\hat{L}_{H,t} = \frac{L_{H,M}}{L_H}\hat{L}_{H,M,t} + \frac{L_{H,m}}{L_H}\hat{L}_{H,m,t} \tag{65}$$

39. Foreign labor per capita

$$\hat{L}_{F,t} = \frac{L_{F,M}}{L_F}\hat{L}_{F,M,t} + \frac{L_{F,m}}{L_F}\hat{L}_{F,m,t} \tag{66}$$

40. Aggregate labor

$$\hat{L}_t = \frac{nL_H}{L}\hat{L}_{H,t} + \frac{(1-n)L_F}{L}\hat{L}_{F,t} \tag{67}$$

with $\hat{L}_t = (L_t - L)/L$.

41. Home production real wage

$$\nu^{-1}\hat{L}_{H,t} + \sigma^{-1}\hat{c}_t = \hat{w}_{H,t} + \hat{p}^C_{H,t} \tag{68}$$

where $\hat{w}_{H,t} = (W_{H,t}/P^C_{H,t} - W_H/P^C_H)/(W_H/P^C_H)$.

42. Foreign production real wage

$$\nu^{-1}\hat{L}_{F,t} + \sigma^{-1}\hat{c}^*_t = \hat{w}_{F,t}(x) + \hat{p}^C_{F,t} - \hat{q}^C_t \tag{69}$$

where $\hat{w}_{F,t} = (W_{F,t}/P^C_{F,t} - W_F/P^C_F)/(W_F/P^C_F)$.

43. Home CPI price level

$$\hat{P}^C_t - \hat{P}^C_{t-1} = \hat{\pi}^C_t \tag{70}$$

where $\hat{P}^C_t = (P^C_t - P^C)/P^C$.

44. Foreign CPI price level

$$\hat{P}_t^{C*} - \hat{P}_{t-1}^{C*} = \hat{\pi}_t^{C*} \tag{71}$$

where $\hat{P}_t^{C*} = (P_t^{C*} - P^{C*})/P^{C*}$.

45. Government budget constraint

$$\begin{aligned}&\frac{nP_H Y_H}{PY}\frac{PY}{P^C C}\left[\frac{P_{H,M}Y_{H,M}}{P_H Y_H}\left(\hat{g}_{H,M,t} + \omega_{H,M}\hat{p}_{H,M,t}\right) + \frac{P_{H,m}Y_{H,m}}{P_H Y_H}\left(\hat{g}_{H,m,t} + \omega_{H,m}\hat{p}_{H,m,t}\right)\right] \\ &+\frac{(1-n)P_F Y_F}{PY}\frac{PY}{P^C C}\left[\frac{P_{F,M}Y_{F,M}}{P_F Y_F}\left(\hat{g}_{F,M,t} + \omega_{F,M}\hat{p}_{F,M,t}\right) + \frac{P_{F,m}Y_{F,m}}{P_F Y_F}\left(\hat{g}_{F,m,t} + \omega_{F,m}\hat{p}_{F,m,t}\right)\right] = T\hat{T}_t\end{aligned} \tag{72}$$

B.8 Calibration

Table B.6: Calibrated Parameters

Targeted Moment	Data	Sources	Description
avg. population share of a U.S. state	2%	BEA	average of (state population/U.S. population), 2008
share of politically connected firms	1.29%	NETS, NIMP	# politically connected firms (NETS, NIMP)/# firms in the U.S. (NET), 2008 (exclude firms with emp < 10)
employment share of connected firms	20.74%	NETS, NIMP, BDS	emp of politically connected firms (NETS, NIMP)/emp of U.S. firms (NETS), 2008 (exclude firms with emp < 10)
employment-to-population	60.6%	BLS	employment-to-population (16 and over), seasonally adjusted, Jan 2009.
β_1 of multiplier regression	26.79	NETS, NIMP, Recovery Act Recipient Report, BLS, FHFA, FEC, NCSL, Census	β_1 of regression (1), variables (including ARRA spending) are deflated using seasonally adjusted CPI (base year 2008)
β_2 of multiplier regression	-0.0265	NETS, NIMP, Recovery Act Recipient Report, BLS, FHFA, FEC, NCSL, Census	β_2 of of regression (1), variables (including ARRA spending) are deflated using seasonally adjusted CPI (base year 2008)
mean of ARRA per capita	0.0003	NETS, Recovery Act Recipient Report, BLS	mean of $A^{pc}_{s,T}$, deflated by seasonally adjusted CPI (base year 2008)
stdev. of ARRA per capita	0.0002	NETS, Recovery Act Recipient Report, BLS	standard deviation of $A^{pc}_{s,T}$, deflated by seasonally adjusted CPI (base year 2008)
mean of politically connected ARRA share	6.52%	NETS, NIMP, Recovery Act Recipient Report	mean of $S_{s,T}$
stdev of politically connected ARRA share	8.57%	NETS, NIMP, Recovery Act Recipient Report	standard deviation of $S_{s,T}$

Notes: Data are obtained from the National Establishment Time Series (NETS), National Institute of Money in Politics (NIMP), Recovery Act Recipient Report, U.S. Census Bureau Business Dynamics Statistics (BDS), Bureau of Economic Analysis (BEA), Bureau of Labor Statistics (BLS), Federal Housing Finance Agency (FHFA), Federal Election Commission (NCSL), National Conferene of State Legislatures (NCSL).

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