The Political Economy of Discretionary Spending: Evidence from the American Recovery and Reinvestment Act*

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June 10, 2014

Abstract

We study the spatial allocation of expenditures in the American Recovery and Reinvestment Act (ARRA), one of the largest discretionary funding bills in the history of the United States. Contrary to both evidence from previous fiscal stimulus and standard theories of legislative politics, we do not find evidence of substantial political targeting. Party leaders did not receive more funds than rank-and-file legislators. Pivotal voters in the Senate and swing voters in the House also did not receive more money. While Democratic districts overall received more per resident than Republican districts, this differential

*We thank Bryan Hardy, Kyle Meng, Ruchita Tiwary, Sebastién Turban and Laurence Wilse-Samson for excellent research assistance and the Institute for New Economic Thinking for generous financial support. In addition, we thank Allan Drazen, Peter Orszag, David Romer, Brian Scholl, Erik Snowberg, John Wallis and Justin Wolfers for detailed comments. Any mistakes are our own.

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mostly disappears when we consider award per worker in the district or when we control for the district poverty rate. Democratic states also received modestly greater funds, but this is largely due to higher levels of funding going to places with more generous state welfare programs. At the same time, we find no relationship between the amount awarded and measures of the severity of the downturn in the local economy, while we do find more funds flowing to districts with higher levels of economic activity and a greater incidence of poverty. The results are consistent with the discretionary component of the ARRA being allocated through funding formulas or based on project characteristics other than countercyclical efficacy or political expediency, which stands in contrast to evidence from fiscal stimulus in the New Deal. One explanation suggests that legislative norms have reduced the scope of discretion—with attendant benefits and costs.

1 Introduction

The American Recovery and Reinvestment Act (ARRA) of 2009, otherwise known as the Obama stimulus bill, was one of the largest discretionary spending bills in US history. At the time of passage, it allocated a total of $787 billion, consisting of $212 billion in tax cuts, $267 billion in entitlement programs, and $308 billion in discretionary projects awarded through contracts, grants and loans. Passed during the the Great Recession, the stated aim of the ARRA was to appropriate funds "for job preservation and creation, infrastructure investment, energy efficiency and science, assistance to the unemployed, and State and local fiscal stabilization" (ARRA Preamble). In his remarks during signing the bill, President Obama emphasized the multiplicity of the mandate to create jobs and to do so by pursuing high quality projects: "What makes this recovery plan so important is not just that it will create or save three and a half million jobs over the next two years, including

nearly 60,000 in Colorado. It's that we are putting Americans to work doing the work that America needs done in critical areas that have been neglected for too long — work that will bring real and lasting change for generations to come."[1] In other words, the purpose of the act was to stimulate the economy, provide assistance to the unemployed and expand provision of high quality public goods. Despite this broad panoply of goals, the most important goal was to provide economic stimulus using fiscal policy. In the words of then minority leader John Boehner, "The president made clear when we started this process that this was about jobs. Jobs. Jobs. Jobs."[2] In fact, the bill became popularly known as the "Obama stimulus bill," emphasizing the recovery goals of the bill.

In this paper, we study the allocation of the funds in the ARRA, with an eye towards several objectives. First, given the size and significance of this bill, understanding how and where the money was spent is important in and of itself. Second, we use this bill to test theories from political economy in order to learn about the legislative process and the distribution of government spending in general. Finally, the ARRA provides an opportunity to examine the political economy of a particular type of government spending — namely, fiscal policy for the purpose of macroeconomic stabilization — and in doing so, we consider the implications for improving the design of such policies. Two prior papers have also looked at the distribution of funds in ARRA. Inman (2010) conducts a state-level analysis and finds that funding formulas explain most of the cross-state variation. We conduct a similar analysis and find similar results at the state level but we find that different funding formula variables matter at the district level. Gimpel, Lee and Thorpe (2013) conduct a county-level analysis and find no role for the unemployment rate

[1] Obama's remarks at stimulus signing, February 17, 2009. Available at http://www.nytimes.com/2009/02/17/world/americas/17iht-17textobama.20261060.html.

[2] Quoted in "Recovery Bill Gets Final Approval," *New York Times*, February 13, 2009. http://www.nytimes.com/2009/02/14/us/politics/14web-stim.html.

in the allocation of funds across counties. Our study is more comprehensive than either of these two and our analysis focuses on the district level which is not covered in either

There are a number of competing views about how funds would be distributed. One view, consistent with the administration's stated goal of providing stimulus, is that the geographic distribution of expenditures would reflect the economic evidence on fiscal multipliers. Recent evidence suggests that state and local multipliers are larger in areas with greater excess capacity (Dube, Kaplan and Zipperer, 2014; Nakamura and Steinsson 2013; Shoag 2010). Thus, we might expect funds to be targeted towards areas that were more heavily exposed to the economic downturn — for example areas with larger increases in unemployment. In addition, Johnson, Parker and Souleles (2006) document that individuals with low levels of income and low levels of liquidity respond more strongly to stimulus. We might therefore expect areas with higher levels of unemployment or higher rates of poverty to receive more money, due to the belief that targeting transfers towards these individuals would lead to higher multipliers and more job creation.

An alternative view of the stimulus bill is that it was filled with pork barrel projects. Less than two weeks before the signing of the bill, the future head of the House budget committee, Paul Ryan, referred to the bill as a "bloated porkfest" in an interview with the conservative news agency, Newsmax.[3] This view was widely reported in much of the mainstream press, including CNN[4] and the Washington Post[5]. The view of the stimulus bill as pork-laden is still common today. Edward Krosner, former editor of Newsweek, New York

[3] "'Porkfest' Will Crush Economy, Rep. Ryan Says," *Newsmax*, February 10, 2009. http://www.newsmax.com/RonaldKessler/Obama-stimulus-pork/2009/02/10/id/328187/.

[4] "What GOP Leaders Deem Wasteful in Senate Stimulus Bill," *CNN*, February 4, 2009. http://www.cnn.com/2009/POLITICS/02/02/gop.stimulus.worries/index.html.

[5] "Despite Pledges, Stimulus Has Some Pork," *Washington Post*, February 13, 2009. http://www.washingtonpost.com/wp-dyn/content/article/2009/02/12/AR2009021203502.html.

magazine, Esquire and the New York Daily News, wrote in a 2014 Wall Street Journal op-ed, "Mr. Obama repeated the rookie mistake he made with the stimulus bill, which became a bloated porkfest."[6]

The political economy literature provides further predictions about the pattern of spending. One theory is that politicians act in the interests of the party. Therefore, we might expect to see parties directing funds towards marginal districts in order to improve the electoral success of the party. However, there is a longstanding literature in political science suggesting that the United States, having a majoritarian political system, has weak political parties (Baron, 1991; Shor, 2006a; McCarty, Poole and Rosenthal, 2001). Indeed, using a regression discontinuity design, Albouy (2009) finds that states with a majority of congressional districts aligned with the party of the president receive a relatively small and statistically insignificant 2.6% more in intergovernmental transfers from the Federal Government. Shor (2006a; 2006b) finds similar results for the allocation of funds in state legislatures. In contrast, using less well-identified but more representative panel methods, Berry, Burden and Howell (2010) find that counties with a representative aligned with the party of the president do receive more funds.

Another theory posits that politicians are self-interested and maximize their own return without strong regard to either general welfare or party interests. In this case, districts with powerful politicians should receive more discretionary funds. Both Knight (2005) and Cox, Kousser and McCubbins (2010) provide empirical evidence that those with agenda-setting power are able to gain substantial rents. In contrast, Berry, Burden and Howell (2010) use a 23-year panel of county-level disbursements of federal government expenditures and find that committee leaders and members of important committees do not receive greater amounts of federal dollars per capita than other districts.

[6] "Obama the Management Failure," *Wall Street Journal*, January 8, 2014. http://online.wsj.com/news/articles/SB10001424052702303933104579302480571979884.

Similarly, political moderates who can credibly threaten to vote for either side may receive more funds for their districts. The most commonly used model in political economy, the probabilistic voting model (Lindbeck and Weibull, 1987), captures this intuition. It shows that policy and rents are apportioned to individuals in rough proportion to their probability of being swing voters. Rent accrual to ideological moderates has been verified in the Swedish context (Dahlberg and Johansson, 2002). In the context of the ARRA, we might expect senators and representatives with more moderate voting records to receive greater funds, as well as members who are seen as pivotal for this particular vote.

In our paper, we look at the distribution of funds across states and across congressional districts, and we examine how the allocation of funds was related to both the economic characteristics of the local area as well as the political characteristics of the area's congressional representatives. The geographic pattern of ARRA spending turns out to be generally inconsistent with most of the above predictions. When it comes to the economic determinants of stimulus, we find no significant correlation between the amount spent and the local unemployment rate, suggesting that the funds were not spatially targeted so as to maximize the fiscal multiplier. We do find, however, that congressional districts with a higher percentage of the population under the federal poverty line received a greater amount of funding. Finally, we also find that congressional districts with greater employment-per-resident (i.e., central urban and more economically-active areas) received substantially more funding. At the same time, these district-level findings for poverty and employment are not replicated at the state level; in fact, states with higher rates of poverty receive somewhat smaller amounts of funding, suggesting that the targeting to poor areas happens at the level of allocation within states and not across states. This is consistent with the budgeting process whereby a certain portion of funds allocated to a state are "set aside" for particular areas or purposes.

We divide our results on political targeting into whether individuals were targeted and whether groups (political parties) were targeted. For the former, we test whether members of Congress were able to secure more funds for their districts by exploiting their positions of power within Congress, for example through formal leadership positions or through their status as pivotal members. For the latter, we examine whether politicians influenced the allocation of funds in order to benefit one political party over the other.

Individual targeting does not appear to have played a role in the allocation of funds. We find no evidence that powerful members of Congress were able to secure more funding for their districts; this includes party leaders as well as Democratic and Republican committee leaders. In contrast to Dahlberg and Johansson (2002), we find that ideological moderates were actually *less* likely to receive ARRA funds. There is no evidence that pivotal legislators received substantially more funds. This is not surprising in the House where the bill was certain to pass due to the large majority held by the Democratic party at the time. However, it is somewhat surprising that it is also true in the Senate where the Democratic Party needed a minimum of two Republican votes to avoid a filibuster. It is important to note that our finding that pivotal members of the Senate did not receive more funds is not at odds with academic and popular accounts that pivotal members had large influence over the bill. McCarty, Poole and Rosenthal (2013) discuss press reports about the senators from Maine using their pivotal status to reduce the size of the bill by $200 billion and to adjust the Alternative Minimum Tax for inflation, while the Republican Arlen Specter from Pennsylvania used his clout to increase funding for the National Institutes of Health. We have no way to test for the impact of pivotal senators on the overall composition of the bill. However, we find no evidence that pivotal senators increased the amount of funds allocated to their home states. We interpret these results as showing that politics did not affect the geographical distribution of funds. However, this should not be construed as implying that politics or political

ideology of powerful legislators did not influence the bill.

While we see no evidence of targeting towards individual politicians, the evidence on group targeting (i.e., partisanship) is more mixed. In general, our evidence supports the weak party hypothesis. Importantly, neither of the two parties appear to have targeted extra funds towards marginal districts, i.e., those with close outcomes in the previous election. We find no significant impact of being a marginal Democrat versus a marginal Republican. This is an important result for two reasons. First, legislators working in the interests of the party might choose to target funds towards marginal districts because these are the districts that are most vulnerable to switching parties in the next election. Second, close Democratic districts and close Republican districts are likely to be similar along many other dimensions as well, and so this comparison offers the cleanest empirical test of whether districts were targeted based on their explicit party affiliation. Our results here echo those of Albouy (2009), although in contrast to the literature, we focus on a single piece of legislation; nonetheless, the legislation we analyze is sizable, with $308 billion in highly discretionary funds. Notably, Albouy (2009) finds his null results during a time of divided government when the president had to bargain with Congress in order to get legislation passed, while we find the same null effect during a period of strong unified government. We also look at the state-level allocations and consider whether swing states in the electoral college received more money, and we find that this is not the case; if anything, swing states receive less funding.

At the same time, we do find some evidence suggesting that strongly Democratic areas received slightly higher levels of funding, although we are unable to determine whether this reflects targeting for partisan political purposes or merely reflects the policy preferences of the politicians who wrote the bill. In our baseline sample that excludes state capitals, Democratic districts in the House received, on average, only $95 more per capita in discretionary funding than their Republican counterparts, despite the fact that Democrats

had large majorities in both chambers of Congress and held the presidency at the time of passage of the ARRA bill. Controlling for district characteristics that were relevant for some of the funding formulas, this differential falls to $34 per capita and is not statistically significant. In addition, we do not find any differential by party when we consider award per worker (as opposed to per resident) in the districts — with or without controls. We do find that strongly Democratic districts (with between 80 and 90 percent Democratic Party vote share) received more funds. However, this differential is at least in part driven by a small number of very dense urban districts with high levels of employment — and the differential is smaller when we consider the stimulus award per *worker* in the district. Thus, it may in part reflect dollars going where firms are located. Our results for the House of Representatives are echoed in the Senate: we find that states with more Democratic senators receive more money.

Overall, when we consider the variation in spending across congressional districts and states, we do not find much evidence of targeting based on either countercyclical or political considerations. It is possible that Democrats felt constrained in their ability to explicitly target districts based on partisan affiliation, yet they still engaged in a more subtle form of targeting, whereby funds were targeted based on characteristics such as employment and poverty *because* those characteristics tend to be associated with Democratic districts. Another possibility is that the policy preferences of Democratic legislators support the types of projects that tend to be located in Democratic constituencies. While we can rule out more explicit measures of partisan targeting, we are not able to identify or distinguish between these more subtle and limited forms. However, in general the evidence suggests that both economic and political targeting was limited.

The relative lack of political and countercyclical targeting in the most discretionary components of the ARRA contrasts with evidence from New Deal legislation. Fishback, Kantor and Wallis (2003) argue that grants under

the New Deal were targeted both to high unemployment areas and to swing district supporters of President Roosevelt in the prior presidential campaign. In the section where we interpret our results, we consider some possible explanations for this difference. In particular, we argue that since the 1930s, bills have increasingly relied on funding formulas. In the ARRA, we find that amount of money spent is highly correlated with employment and poverty, both of which are featured heavily in the formula language in the bill. Others have argued that these funding formulas were enacted in large part to facilitate quick disbursement of funds while limiting pork. However, even funding formulas can be altered to benefit certain districts over others, possibly allowing legislators to funnel large sums of money to their districts. Thus, we also argue that shifts in norms put additional constraints on today's bills to spread benefits relatively evenly across districts. This hypothesis is borne out in our results where we find that after taking out 7 districts with large projects, funding is spread relatively evenly across districts.

Some caution is warranted when drawing general conclusions from our results. The ARRA was passed by a new president who had spoken out against pork and in favor of government transparency. Moreover, the losing candidate in the recent presidential election had strongly advocated for abolishing earmarks (targeted geographical expenditures). Thus, from an early stage, the president had announced that he would not accept a bill with earmarks. Second, the president had run for office promising a post-partisan administration. Therefore, the White House was plausibly concerned about appearing overly partisan by spending money in a politically targeted way. Third, the bill was passed at the height of employment losses during the Great Recession, and thus there was a shared sense of urgency for the bill across the Democratic party. Finally, the bill was all but guaranteed passage in the House but needed a few Republicans to avoid a filibuster in the Senate.

These caveats notwithstanding, we feel our study does offer some general lessons. First we note that it is often the case that bills are marginal in

the Senate but assured to pass or fail in the House. This is due to the Senate's filibuster rule, the use of which has been increasingly common in recent years. Second, the urgency of the bill, though perhaps uncommon for a general spending bill, is probably not uncommon for a stimulus bill given that the onset of large recessions is usually quite rapid. Therefore, our study is likely to be quite relevant for understanding the political economy of fiscal stimulus. Third, our results are quite consistent with those from the burgeoning empirical political economy literature on distributive politics.

The remainder of the paper is structured as follows. In section 2, we summarize how decisions were made to allocate ARRA funds. In section 3, we describe the data that we use. Section 4 presents our results, and in section 5 we interpret our findings and discuss policy implications. Section 6 concludes.

2 How Congress Budgeted the ARRA

In this section, we describe the legislative process in general and highlight features particular to the passage of ARRA. Budget items are normally sent through finance committees as well as appropriations committees in both the House and the Senate. This was also true in the case of ARRA. Most of the details of the contracts, grants and loans (hereafter CGL) portion of the bill were decided in the 12 appropriations subcommittees in both the House and the Senate.

Since the Obama administration and leaders in the Republican Party both had argued strongly against expenditures which were explicitly targeted to particular districts (earmarks), no earmarks were incorporated into the bill. Before the bill was passed, each of the 12 appropriations subcommittees separately in both the House and the Senate came up with proposed budgets. These budgets were then reconciled across the House and the Senate and the resulting compromise was ultimately written into the bill. Since no Republican House members actually voted for the bill, Republicans in

the House of Representatives played little role in formulating the budget. In the Senate, all of the Democrats who were present voted in favor of the bill; they were joined by two independents who caucused with the Democrats, Joe Lieberman from Connecticut and Bernie Sanders from Vermont.[7] In addition, one Democratic Senator, Ben Nelson of Nebraska, publicly announced his ambivalence towards voting in favor of the bill in the weeks leading up to the vote. The Democrats and independents were joined by three Republican Senators — Susan Collins and Olympia Snowe from Maine, and Arlen Specter from Pennsylvania.[8] Thus, in the Senate, Republicans played a larger role than in the House, but the bill was still predominantly formulated by congressional staff on the Democratic side.

The bill was crafted with strong time constraints, which, combined with the desire to eliminate pork, led the appropriations committees to provide funding using pre-existing federal formulas. Some of the project money was allocated through competitive grants so that at least some projects were selected based solely on project quality. Thus, almost all of the CGL money was allocated using either funding formulas or competitive grants.

A hard commitment to pre-existing formulas would in theory reduce the rent-seeking behavior of politicians to simply bargaining for general increases in levels of funding through particular formulas, in lieu of particular projects for their districts or states. However, incentives to argue for marginal increases in formula funding are muted because in most formulas, an increase in funding of one dollar to a legislator's constituents would be accompanied by an increase in tens of dollars to other states andan hundreds of dollars to other districts. This point was made well in Grunwald (2012) who relates "The final spat on the Senate side did not pit the moderates against the leadership, but Specter against Ben Nelson, who wanted to tweak the

[7]Ted Kennedy did not vote due to illness and Al Franken was not allowed to sit for the Senate seat until the summer of 2009 due to litigation following a very close election.

[8]Arlen Specter was a Republican until May, 2009 when he switched to the Democratic Party.

Recovery Act's formula for distributing Medicaid funds to get rural states extra cash. Specter said: No way. Orszag did some calculations in his head, and informed Rahm that Nelson was hijacking the entire stimulus over $25 million."

It is not clear precisely how the commitment to use historical funding formulas was maintained. We suggest the possibility that legislative norms or norms among voters can sustain this commitment and that this allows for both contemporaneous improvements in avoiding wasteful bargaining or graft as well as increased reciprocity between legislators over time.

The formula money was disbursed by federal agencies and given to state governors. However, gubernatorial discretion was limited in terms of how it could be spent, where it could be spent and how quickly it should be spent. For example, highway money had to be used to build and repair highways and 50% of it had to be spent within 6 months, all of it within a year. The money that remained unspent was to be redistributed to other states by the Federal Department of Transportation. Other portions of the bill mandated that a certain percent of the funds be spent in rural or urban areas.

The competitive grant money was allocated to the Executive Branch agencies and was then allocated to recipients based upon merit using criteria described in the bill. The criteria used by the various departments differed by type of grant and were written by both legislators and staffers.

Despite the pervasive use of funding formulas and competitive grants, there was still room for substantial political influence over the distribution of expenditures. The Obama administration pushed certain projects of interest to the administration (including money for alternative energy, high speed rail, and local public transportation). Similarly, whereas pushing for district-specific projects was not allowed, members of Congress could have influenced how much money their district received by putting more money into programs that favor their district or state or by altering funding for-

mulas or program criteria. For example, members of Congress from rural areas might have tried to push the subcommittees to increase allotments to funding formulas based upon highway miles while representatives from urban districts might have pushed for increased funds through funding formulas for public transportation systems. Overall, despite the pervasive use of funding formulas and competitive bidding, there was indeed substantial scope for politicians to affect the geographical distribution of funds.

3 Data Description

3.1 American Recovery and Reinvestment Act of 2009

Our data on ARRA spending comes from the website www.recovery.gov, which was created to provide taxpayers with information on how the ARRA funds were spent, as mandated by the Act itself. Spending under the ARRA can be divided into three major categories: tax benefits; entitlements; and contracts, grants and loans. For the last category, Recovery.gov provides information on each individual recipient, including the recipient's address along with the primary zip code and congressional district where the activity was to be carried out. For the tax benefits and entitlements categories, only state-level statistics are available. Much of the analysis in this paper is therefore focused exclusively on the CGL funds. As of January 2014, the total estimated expenditure under the ARRA was $840 billion (an increase from the original estimate of $787 billion). Of this, CGL funds accounted for around $267 billion. We aggregate the amount of disbursement to the House district level and, for some specifications, to the state level.

We exclude from our baseline district-level analysis all congressional districts containing any part of a state capital. We do this because a large portion of the money sent to state capital districts was disbursed to the state governments and subsequently redistributed across the state. For example, educational grants are predominantly sent to state capitals and then distributed by governors and state legislatures throughout the state. This

type of spending is recorded in the ARRA data as going to the congressional district where the headquarters of the state governmental agency is located, and does not accurately reflect the distribution of spending across congressional districts. After dropping congressional districts containing any part of a state capital, we are left with 334 out of 435 congressional districts, implying that the average capital city is contained within two congressional districts. We discuss this issue in more detail below. Overall, $106 billion of the $267 billion in CGL funds remains after excluding state capitals from the sample.

For our state-level analysis, in some specifications we aggregate the CGL data to the state level. For other specifications, we use aggregate state-level data reported by the federal agencies and available through Recovery.gov. This agency-reported data includes the contracts, grants and loans as well as some of the entitlement money such as unemployment insurance and Medicaid funds.

3.2 Voting and Congress Data

We use vote returns from Congressional Quarterly for the November 2008 general election for the U.S. House of Representatives. The two-party Democratic vote share in each congressional district is computed as the number of votes for the Democratic candidate divided by the total votes for the Democratic and Republican candidates. The median Democratic vote share in our sample is 57.7%, reflecting the Democratic majority in the House at the time. On February 13, 2009 — the date of the vote on the ARRA conference report — there were 255 Democrats, 178 Republicans, and 2 vacant seats in the House of Representatives. We also obtain from Congressional Quarterly the average Democratic vote share for president at the state level.

Data on the tenure of each member of the House comes from the Office of the Clerk of the House of Representatives. We obtain information on tenure of senators from Roll Call, the newspaper published by Congressional Quarterly. We use data from the website Govtrack.us to find committee

assignments for the 111th Congress and each representative's vote on the ARRA. DW-Nominate scores for individual legislators were downloaded from voteview.com.

3.3 Other Data

We also use data for a number of demographic and economic characteristics at the state and district levels. Data on state-level Medicaid and county-level unemployment insurance expenditure is from the Bureau of Economic Analysis. District-level information on population, poverty, and land area comes from the U.S. Census Bureau. We use data on county-level unemployment rates from the Local Area Unemployment Statistics program at the Bureau of Labor Statistics. County-level information on home loans is from Home Mortgage Disclosure Act (HMDA) data made available by the Federal Financial Institutions Examination Council. District-level characteristics for unemployment rate, home loans, highway miles, and unemployment insurance are derived from the county-level information using geographic correspondences provided by the Missouri Census Data Center. For data on total employment, we use the 2008 LEHD Origin-Destination Employment Statistics (LODES) data available from the U.S. Census Bureau, and aggregate the census block-level data to the district level.[9]

4 Results

In this section, we present our main results. We begin by looking at how the money was distributed across different types of expenditures. In section 4.2, we discuss the correlation of expenditures with economic variables. In section 4.3, we test for targeting to the Democratic party. In section 4.4, we test for individual targeting of powerful political elites. In the final portion of the results section, we test for targeting of pivotal members of Congress

[9]LODES data is not available for Massachusetts, so we instead use the ZIP Code Business Patterns from the U.S. Census Bureau, and derive district-level employment using geographic correspondences from the Missouri Census Data Center.

as well as party defiers.

In each of the following sections, we separately discuss evidence from the Senate and the House of Representatives. On the one hand, the Senate seems a more natural place to look for the impact of politics on the allocation of funds because it was in the Senate that passage of the bill was uncertain and a few swing legislators had the potential to influence the outcome. It is also possible that Senate leaders who negotiated the bill were more able to obtain political rents for their districts in comparison with the House where there was little need for internal bargaining. A second advantage of looking at the Senate is that we can look at a larger fraction of the total ARRA expenditures. The supplemental expenditures for entitlement programs, such as money for Medicaid expansion and unemployment insurance, are not reported at the congressional district level. Additionally, a large portion of the CGL funds were formally allocated to state capitals but in practice spread across districts within the states; we thus have to omit these state-level program expenditures in our district-level analysis.

On the other hand, the fact that passage of the bill was assured in the House certainly does not rule out targeting of funds to party leaders and well-connected members of the House. The House still needed to be bargained with, and House leaders could have exploited their agenda-setting power. In addition, since the ARRA was passed only by Democrats in the House, it is possible that partisan effects could have been much stronger across House districts than across states. Another important difference between the House and the Senate is in the number of representatives. There are 435 House districts, 334 of which did not contain any portion of a state capital. Therefore, there are almost 7 times as many observations when looking at House districts as compared to the Senate. Finally, different from the Senate where there are two members per state, in the House, there is only one member per district. Thus, although we might expect stronger individual targeting in the Senate, we might expect stronger group targeting in the

House. In addition, we think empirical work is more credible when focused on the House due to the substantial increase in sample size. In this paper, we therefore look at the allocation of funds across states as well as across congressional districts.

4.1 What Was the Money Spent On?

In this section, we give an overview of what types of projects were funded with the ARRA money. In Appendix Figures A1 and A2, we display maps showing the amount of stimulus funds received, with darker areas indicating higher levels of funding per capita. Figure A1 shows per capita amounts at the state level, while Figures A2a and A2b show amounts at the congressional district level. Figure A2b omits districts containing portions of state capitals and therefore displays the distribution of funding we use in our main district-level analysis. There are no immediately obvious patterns from the maps.

The mean amount of stimulus funds received per resident in a district in the form of contracts, grants and loans was $469 per capita, or $900 per capita including state capitals. This reflects the exclusion of all CGL funds to state governments, particularly education funds which constituted a very large proportion of total funds. The mean amount of CGL funds received per resident of a state was $517 excluding state capitals and $1056 including state capitals. The discrepancy between states and districts reflects the fact that smaller states received a higher average amount of funds per resident. The mean receipt of funds per resident in the agency data is $1617. This is substantially higher than the CGL data because it includes money allocated to state-level programs such as unemployment insurance and Medicaid. We show descriptive statistics in Table 1. There is sizable variation in expenditure across congressional districts. The standard deviation of per capita expenditure is $543 per person excluding state capitals and $1786 per person including state capitals. We also report vote share information in Table 1. Since the House of Representatives was highly Democratic at the time, the mean Democratic vote share was 58%. The vote share was identical for

districts containing at least some portion of a state capital.

Focusing only on congressional districts outside of state capitals, we begin by showing a kernel density estimate of the per capita expenditure.[10] The distribution, shown in Figure 1a, is heavily skewed to the right. The lowest amount of money obtained by any district was Anthony Weiner's (Democratic) district in New York City, which received around $7 per person of CGL funding. The most received by any congressional district in our sample was around $3750 per person for Doc Hastings' (Republican) district containing the Tri-Cities and Yakima, Washington. Hastings' district received over 500 times more money per person than Weiner's district. In Figure 1b, we show a histogram and kernel density estimates of the Democratic vote share. We also show a kernel density of House two-party vote shares. There are two modes of the distribution of vote share, one Republican mode around 40% and another Democratic mode around 70%. There are also a few districts with uncontested Republican winners and almost 3 times as many Democratic ones.

Though the amount of variation across congressional districts was substantial, the amount of variation across states was much more muted. However, the large amount of variation across districts is largely driven by a small number of outliers. To show this, we decompose the sum of the squares of per capita funding into the mean and residual variation: $\sum_{i=1}^{I} {}_i y_i^2 = I\bar{y}^2 + \sum_{i=1}^{I} (y_i - \bar{y})^2$. We report the mean fraction of the sum of squares (or one minus the residual variation share of the sum of squares): $\frac{I\bar{y}^2}{\sum_{i=1}^{I} y_i^2} = 1 - \frac{\sum_{i=1}^{I} (y_i - \bar{y})^2}{\sum_{i=1}^{I} y_i^2}$. Overall, de-meaned cross-district variation accounts for 79.9% of the total sum of squares of payments per resident in the CGL data. Thus, the mean payment to a district accounts for a mere 20.1% of the total sum of squares.

[10] We use an Epanechnikov kernel with an optimal bandwidth minimizing mean squared integrable error relative to a fitted Gaussian distribution.

This shows that ARRA expenditures were not distributed equally. However, when we omit state capitals, the portion attributable to the mean rises to 42.8%.

We then identify 17 outliers, using a cutoff of $1300 per resident. Of these districts, 12 are Democratic districts and 5 are Republican districts. Out of the 10 districts receiving more than $2000 per resident, 5 are Republican and 5 are Democratic. If we take a simple regression model with per capita funds on the left hand side and only a constant term and Democratic dummy on the right hand side, adding in dummies for these 17 districts increases the R^2 from 0.007 to 0.808. Moreover, we find that the mean share of the sum of squares rises to 68.5%.

When we look at the distribution of funds on a per worker basis, we find 5 Republican districts and 2 Democratic ones which get more than $3500 per worker. In a simple regression with amount per worker on the left hand side and only a constant term and Democratic dummy on the right hand side, we obtain an R^2 of zero (to three significant digits). However, adding 7 dummies for the outlier districts increases the R^2 to 76.4%. Taking out these 7 outlier districts increases the mean share of the sum of squares from 41.6% to 67.7% which is very similar to the mean share in per resident terms after removing the 17 top outliers. The state-level mean shares using the CGL data are significantly higher. Including the funds going to state capitals, the mean share is 79.9%. Omitting state capital funds, it rises to 84.9%. Using the agency data, it is 93.6%. This result shows that state shares of per capita expenditure did not vary much. We display the distribution of expenditures per resident as well as per worker, including the outliers, in Figure 2.

The large amounts allocated to the outlier districts are due to sizable individual awards rather than a large number of awards. We show these results in Appendix Figure A3. The two top Republican recipients, Doc Hastings (WA) and Buck McKeon (CA), received 22% and 42% of their funds respectively in their top award. In both cases, these were large competitive

DOE grants or loans (water reclamation at the Hanford nuclear site and a solar energy loan, respectively). The two top Democratic recipients, Elijah Cummings (MD) and Barbara Lee (CA), received 29% and 11% of their funds from their top grant respectively. In the case of Elijah Cummings' district, the funds were Maryland Department of Education funds which would usually go to the state capital (Annapolis) but in this case went to Baltimore.[11] In the case of Barbara Lee's district, the large grant was to the California Department of Transportation for the construction of a local highway tunnel. More generally, the top 10 recipients all received over $2000 per recipient and in all cases, a majority of the money was contained in the top 5% of grants. The top 4 recipients all received over $3000 per resident and more than 80% of their funds were in the top 5% of awards. In general, as seen in Appendix Figure A3, there is a strong positive correlation between the amount a district received and the percent of funds received in the top 5% of awards. This suggests that outlier districts were outliers largely because they had a particular large project that they were awarded, often through a competitive grant.

The money in the ARRA bill was distributed through 207 different federal funding agencies. However, the top four of these account for 55% of the total CGL funds distributed. These four agencies are (in order): the Office of Elementary and Secondary Education ($64.7 billion), the Department of Energy ($38.3 billion), the Federal Highway Administration ($27.9 billion), and the Office of Special Education and Rehabilitation Services ($13.6 billion). The other top-10 granting agencies were the Department of Housing and Urban Development, the National Institutes of Health, the Federal Transit Administration, the Federal Railroad Administration, the Environmental Protection Agency and the Rural Utility Service. These 10 agencies distributed 74% of the $267 Billion in our data. The amounts for these top ten

[11] This is the only case we found where state funds went to a district that did not contain the state capital.

funding agencies are listed in Appendix Table A1. The top 5 granting agencies in the data excluding the state capitals were the Department of Energy, the Federal Highway Administration, the National Institute of Health, the Federal Transit Administration, and the Department of Housing and Urban Development (Appendix Table A2).

4.2 Economic Targeting

Given the stated countercyclical objective of the legislation, we now consider whether financial need or high excess capacity was predictive of how much funding a district received. In Appendix Figure A4, we non-parametrically regress the amount received per resident on our two measures of excess capacity. In both cases, we see that districts with higher unemployment received slightly less funding per district resident. These results reinforce similar findings by Gimpel, Lee and Thorpe (2013) and Inman (2010). In the district-level regressions in Table 2, we report coefficients on 7 different variables for 5 specifications each, reflecting results from 35 different regressions in all. A constant term and (in some specifications) additional covariates are included in these regressions, but only the coefficient on the variable of interest is displayed. The top three panels (rows) report the results from regressions using CGL funds per district *resident*; in the bottom two panels, we report regression results using CGL funds per *worker* in the district. We look at per worker specifications because we do find that, in contrast to the cross-state variation, more money went to highly urbanized areas that were centers of employment. Much of the formula money given to the states stipulated that a certain percentage of the funds be spent in urban areas, so it is not surprising that the large recipient districts were located in urban areas. At the same time, the districts that received the *least* amount of funding were also located in urban areas. Interestingly, the amount of funds received per resident is much more strongly correlated with employment in a district than with the percent of the district that is urban. Within large urban areas, there is substantial variation in employment levels across congressional districts.

Most people who work in Wyoming's congressional district also live in it. By contrast, a much lower percentage of people who work in the congressional districts in Manhattan also live there. Neighboring Brooklyn and Queens are much more places of consumption than production in comparison with both Manhattan and Wyoming. More CGL funds went to places like Manhattan with high levels of economic activity by firms or other recipient organizations than places like Wyoming which in turn received more CGL funds than urban consumption centers like Brooklyn and Queens. This, in part, explains the low amounts of funding received by Anthony Weiner's district. In panels A and D of Table 2, we show per resident and per worker results without any additional controls. In panel B, we control for 2008 employment, the poverty rate, highway miles and percent urban as well as 9 vote share bin dummies for the vote share of the congressional representative in the district. The results including all these covariates are generally similar to the unconditional results. In panel C, we also control for land area. In some specifications, this leads to different results. All of these covariates represent variables that were explicitly or effectively incorporated into funding formulas. Finally, in panel E, we report the per worker variant of panel C (omitting employment as a control).[12]

We find no statistically significant correlation in any of the specifications between amount per capita and either of our measures of unemployment or the per resident amount of unemployment insurance spent in the district. We also find no statistically significant correlation with percent urban in any of the specifications. We do find very strong positive correlations between employment in a district and amounts per resident. The estimates are very tightly estimated. Districts with 100,000 more workers on average get $200 more per resident. In all three per resident specifications, the t-statistics are above 5 and the point estimates vary by less than 0.06 across specifications. Moreover, employment is not only highly correlated with funding,

[12]The coefficients on our additional controls are reported in Appendix Table A3.

it is quantitatively important for explaining overall variation. Adding just employment increases the R^2 by almost 14 percentage points. No other variable in our district-level regressions has such large explanatory power. Also, adding in other controls does not change the marginal R^2 of adding in employment. The estimates are surprisingly invariant to controlling for percent urban. This is because urban areas have places with very low employment per resident ratios, such as residential districts Brooklyn, as well as places with very high employment per resident ratios, such as business districts in Manhattan, so that urbanness is not highly correlated with employment per resident. Employment is not correlated with amount received per worker in the district.

The second most important of our covariates is poverty. Poor districts received more money. Adding in poverty increases the R^2 by approximately 0.03. Again, this is not surprising given that some of the formula money set aside portions specifically for historically poor areas.[13] The results on poverty are less robust than those on employment. Adding in our other controls lowers the coefficient estimates slightly but also almost doubles the standard errors. Unconditionally, places with a one percentage point higher poverty rate received \$16 more in funds per resident and \$42 more per worker. Note that since between 1/3 and 1/2 of the residents in a district work,[14] the per worker coefficients are usually two to three times as high. Unconditionally, both coefficients are significant at the 1% level. With the full set of controls, the t-statistics in the per resident specification are just below significance at the 10% level with a t-statistic of 1.57 and the per worker coefficient is significant at between the 10% and 5% level with a t-statistic of 1.87.

One possible explanation for the lack of targeting towards areas with high unemployment could be because it instead targeted shovel ready projects,

[13]For example, some of the money disbursed by the Department of Labor used definitions of poor areas defined in a bill in 1965.

[14]Note that this ratio is different from the employment to population ratio as standardly defined in that it includes the elderly and children.

and perhaps places with more shovel ready projects were places with lower unemployment (or lower increases in unemployment). The Obama administration said that "shovel-ready" projects would be made high priority and this was reflected in the bill. Much of the formula grant money had stipulations that money would have to be returned if not spent quickly enough. While it is difficult to measure shovel readiness using an *ex-ante* measure, we do have recipient-reported information on the pace at which the funds were disbursed and spent. To assess shovel readiness, we construct measures of what fraction of the funds in a district were disbursed to projects that were completed within one year. Using this measure, we show in Appendix Figure A5 that places which were allocated more money were on average somewhat slower in completing projects. This is possibly because districts which received more money received money for large infrastructure projects and these on average took longer. Nonetheless, Appendix Figure A5 shows uniformly that places which received more money did not complete their projects more quickly. And across all specifications in Table 2, the coefficient on percent completed within one year is negative and significant at the 5% level or less. Moreover, in unreported regressions, we note that controlling for percent of funds spent within one year does not impact the coefficients on unemployment. Though shovel readiness may have played a role in the selection of projects, we find no evidence that it influenced how money was allocated across congressional districts.

We now consider the state-level results on economic targeting displayed in Table 3. We show the coefficient estimates for the change in the unemployment rate between January 2009 and January 2007; in the specifications with controls, we include the ratio of state employment to state total population, the poverty rate, medicaid expenditures per capita, interstate highway miles, and the average Democratic vote share for the two Senators serving terms in 2009. The specification is rather sparse because of the limited degrees of freedom with only 50 observations. This obviously also limits our ability

to interpret our results. We show results for four different specifications: the agency data without controls, the agency data with controls, the CGL data including funds sent to state capitals, and the CGL data removing the funds that went to state capitals. We include controls in both of our CGL specifications. Even without controls, the change in the unemployment rate only adds 0.015 percentage points to the R^2. In contrast, adding our other five controls raises the R^2 to 0.436. In none of the specifications does the coefficient on change in unemployment come close to statistical significance at conventional levels. The results using the agency data are particularly surprising since those data include the federal provisions towards state unemployment insurance programs and aid to state Medicaid programs that was explicitly targeted to states with above-median unemployment rates.[15]

In contrast to the district-level findings, employment[16] and poverty comes in negative and statistically significant in most specifications. One exception is that state employment has a positive sign and is significant at the 10 percent level in the agency data specification without controls. The employment numbers are small. Increasing the workforce in a state by 10% of a state's population is correlated with a reduction in funding equal to slightly less than $9 per person in our specification with the largest coefficient. The coefficients on poverty are sizable, negative and statistically significant in all four specifications. Though poor areas and high employment areas were targeted within states through money set aside for poor areas and centers of employment, states with higher poverty rates and higher employment did not receive more money. Echoing Inman (2010), we find that the single most important explanatory variable in terms of adding to the explained share

[15]When we examine just the funding distributed by the Department of Labor, which was largely for unemployment insurance, we unsurprisingly find that areas with higher unemployment received more money.

[16]In contrast to the district level where we include employment, here we include the employment to population ratio in the state. Whereas population does not vary substantially across congressional districts, it does across states and we account for that by dividing employment by state population.

of total variation is pre-crisis Medicaid expenditures per capita, which was explicitly incorporated into a Medicaid funding formula.

4.3 Group Targeting: Partisanship

We next investigate whether members of Congress acted in the interests of their party. At the time of passage of the ARRA, Democrats had a strong majority in both the House and the Senate and they also held the presidency. Therefore, they were able to pass the legislation without any support from Republicans in the House, and they passed the bill in the Senate, overcoming a potential Republican filibuster, with the help of only three Republicans. Did, then, the Democrats get a large majority of the funds, as might be expected from simple and standard political economy models?

As De Rugy (2010) has shown, districts represented by Democrats received substantially larger sums of ARRA money than those represented by Republicans. In our database of contracts, grants and loans, districts represented by Democrats received 55% more than those represented by Republicans. In Table 4, we displays the results from regressing the district-level amount on a dummy for Democratic member of the House. From column (1), we can see that the mean Republican district received $684 in funds whereas the mean Democratic district received $1,057. However, as pointed out by Nate Silver (2010), the state capitals received funds that, while nominally allocated to the capital, were in turn allocated across the state.[17] Education funds were generally allocated in this manner. The top 17 recipient districts are all part of state capitals, and 26 of the top 30 are state capital districts as well. The probability of either of these events occurring by chance is below 1 in 10^{13}. However, the pattern of funds allocations is slightly more nuanced than Silver's account. He argues that state capitals tend to be heavily Democratic and therefore these transfers to state agencies are more likely to be counted as going to Democratic districts. In fact, on average, state capital

[17] Albouy (2009) also points out the state capitals problem as part of his justification for running state-level regressions.

districts have almost the same Democratic vote share as districts not located in state capitals. In Table 1, we show that the Democratic vote share for the House seat is 58% whether or not we include the 101 out of 435 districts which contain portions of a state capital.[18] While urban areas tend to vote more Democratic, there are many districts containing portions of state capitals and the surrounding suburban land or even surrounding rural land (e.g., Wyoming is itself one district). Republican districts containing state capitals are slightly larger and less dense. More importantly, state capital districts *in larger states* do tend to be more Democratic than average, while capital districts in smaller states tend to be more Republican. The state capitals in the largest states received disproportionately larger sums because most of the education and Medicaid spending for the entire state is given to the capital, and states with large populations received more total education and Medicaid funds. The districts of the top ten recipients are, in order, the capitals of California, New York, Illinois, Florida, Texas, Ohio, Michigan, Georgia, Pennsylvania, and Massachusetts. Only two of the top 17 districts, and four of the top 26 districts were represented by a Republican member of the House of Representatives.

Since we do not know how the funds nominally allocated to state capitals were actually dispersed within the states, we exclude all districts that include state capitals from our district-level analysis. This eliminates from our sample 13 states which do not have a congressional district without some part of a state capital. In our revised sample, the partisan gap is substantially lower, with Democratic districts receiving 23% more than Republican districts. The average Republican district receives $416 per capita as compared to $510 per capita in the average Democratic district. While this $95 differential is statistically significant (see column 2 of Table 4), it becomes $19 and statistically insignificant once percent living in poverty is introduced as a control. We

[18] Our set of capital districts differs slightly from Silver's. We identify 101 congressional districts containing some portion of a state capital city or its surrounding county. Silver defines 78 districts as containing all or part of a state's capital city.

then add additional controls. None of the controls change the coefficient on the Democrat dummy by much, with the exception of the poverty variable. Column 5 of Table 4 shows that, conditional on the four other funding formula controls, including employment but excluding poverty, the coefficient rises slightly to $109 and remains significant at a 5% level. However, re-introducing poverty reduces the coefficient back to $33 and makes it insignificant at conventional levels. In the per worker regressions, reported in columns 7 and 8, the Democrat dummy is insignificant and small with or without controls. In fact, conditional upon controls, the coefficient on the Democrat dummy is -$85. Ultimately, it is not clear whether Democrats funneled money to their districts through funding formulas that targeted high poverty areas and high employment areas or whether these areas got more because politicians were trying to create jobs in poor areas and centers of employment. Nonetheless, it seems unlikely that poor areas benefited from explicit targeting of Democratic areas. In fact, the poverty variable is slightly more strongly correlated with funds per resident in Republican areas than Democratic ones.

We now look more closely at the amount received by congressional districts as a function of the House two-party Democratic vote share. Figure 3a plots the CGL funds per resident against the Democratic vote share. Five Republican outliers who received substantially larger amounts stand out and roughly triple the number of Democrats also stand out. Figure 3c shows that the Democratic outliers are all in districts which are 100% or very close to 100% urban. Thus, it is not surprising that in Figure 3b, we see only two Democratic outliers though we still see the same five Republican outliers. In Figure 3d, we display the results of a nonparametric regression of the worker-per-resident ratio on vote share. Interestingly, we find a similar hump for districts with 80-90 percent Democratic vote share. This suggests that much of the hump in funding for Democrats in the 80-90 percent range is attributable to those districts being centers of employment.

In the scatter plots in Figure 3, we can see that Democrats receiving between 80 and 90 percent of the vote share seem to have received a higher amount of funds per capita. To assess this further, we non-parametrically regress per capita amount of CGL funds on the Democratic Party vote share and show the results in Figure 4a.[19] We also use a (semi-parametric) partial linear model to regress the per capita CGL funds on the Democratic Party vote share, controlling parametrically for our five funding formula controls: percent living in poverty, percent living in an urban area, land area, road miles, and employment. We estimate:[20]

$$A_i = f(v_i) + X\beta + \epsilon_t$$

where A_i the the per capita amount of ARRA funding received in district i, X is the set of demographic and economic controls and $f(v_i)$ is a non-parametric function of the two-party Democratic vote share.[21] The results are shown in Figure 4b.

Figures 4a confirms the evidence from the simple scatter plot that strongly Democratic districts with around 80 percent of the vote share receive well above the mean amount of CGL funds. Figure 4b shows that this relationship continues to hold after we account for covariates. Figure 4a also suggests that highly Republican districts get a modest amount more than the average recipient. However, this difference is not statistically significant and does

[19] We use the Stata command `lpoly,` using a "rule of thumb" plug-in bandwidth that minimizes the conditional weighted mean integrated square error. Standard errors are obtained by bootstrapping with 100 replications.

[20] Due to concerns that very urban districts got substantially more money and are also highly Democratic, we ran specifications controlling for a dummy variable which takes on a value of one for districts with 100% of their area in urban land. We also tried controlling for a dummy which takes on a value of one if a district has 90% or more of its land in urban areas. Our results are highly robust to these alternative specifications.

[21] We use the Yatchew method to difference out the parametric component X, and use local polynomial regression (Stata command `lpoly`) to estimate the $f(v_i)$ component non-parametrically; the bandwidth selection is based on the "rule of thumb" plug-in method. Standard errors are obtained by bootstrapping with 100 replications.

not survive the inclusion of covariates in Figure 4b. Figure 5 shows the same results with amount per worker as the left hand side variable. In Figure 5a, we see that, whereas Democrats in the 80-90 percent vote share range do not get as much more as in the per resident figure, they still do get more than Democrats in less safe districts. However, highly Republican districts also now get substantial amounts. The bootstrapped confidence intervals are much wider for the very Republican districts because they are driven by one outlier who received a substantial amount. Putting in our five controls also increases the amount received per capita for marginal Republican districts though not for marginal Democratic ones.

Interestingly, in Table 5, when we compare close Democrats to close Republicans, we find that these districts receive about the same amount. If anything the marginal Democratic districts obtained *less* CGL funds as the coefficient on the difference between marginal Democrats and the reference group of marginal Republicans is negative in four out five specifications. However, it is never statistically significant. Democratic and Republican districts with around 50% Democratic vote share are likely to be similar in terms of other characteristics, so comparing these districts offers another way to test whether the partisan representation of the district affects the spending allocation. By contrast, the point estimates for Democratic districts with between 80 and 90 percent vote share are large in all specifications and statistically significant at a five percent or lower level in four out of five specifications. The effects in per worker terms are smaller in magnitude, even after adjusting for the fact that there are 2-3 residents per worker in an average district. This is consistent with the number of Democratic outliers being much smaller in per worker terms than in per resident terms. These results lend support to the argument that the higher average level of spending in Democratic districts is driven more by district characteristics than by party affiliation *per se*. However, the substantially larger amount of CGL funds going to districts where Democrats received between 80 and 90 percent of

the vote share suggests that there may be a partisan gap in funds, or some other factor that may be different in these districts.

At the state level, in Table 6 we look at the correlation between ARRA receipts and the party of the state's governor, a dummy variable for states who supported Obama by a 2 percentage vote margin or less, a variable with the number of Democratic senators in the state, and the Democratic vote share in the previous Senate elections averaged over both senators in each state. We find no evidence that Democratic Governors received a greater amount of funds[22] nor do we find evidence that swing states which marginally supported Obama in the 2008 election benefited. If anything swing supporters of Obama (Florida, Indiana and North Carolina) got less than other states; in two of our four state-level specifications, the coefficients are statistically significant at a 5% level and of moderate to large size. The results are not robust as the coefficient size is much smaller when controlling for funding formula covariates and when we include the money which is reported as accruing to state capitals. However, swing state supporters of the president certainly did not benefit in ARRA.

Our partisanship results for the Senate are similar to our district-level results for the House, in the sense that highly Democratic areas receive slightly more funds on average. Using the agency data, the coefficient on the number of Democratic senators is \$186 per resident without controls and \$130 per resident with controls. The estimated coefficient is roughly 1/2 the size using the CGL data on the full sample of states (panel c), and it is virtually zero after stripping out the state capitals (panel d). We find a similar pattern of results when considering the average Democratic vote share of the two senators. The unconditional correlation in panel a suggests that a 10% increase in the Democratic vote share of both senators increases ARRA

[22]Democratic Governorship at the time of passage of ARRA has been used as an instrument for stimulus (Conley and Dupor, 2011). However, we find that the Democratic Governorship dummy has a low marginal R^2, is not statistically significant and oscillates in sign depending upon covariates.

funding by about $100 per capita. (Recall that the average level of funding in the agency-reported data is about $1600 per capita.) The difference between the agency data results and the CGL results partially reflects that the agency data has 60% more of the ARRA bill expenditures than the CGL data does. However, it also reflects the fact that states with more generous welfare programs received more funds, and these states tend to be more Democratic.[23] The fact that the Senate relied on Republicans in the ARRA vote whereas the House did not does not seem to have impacted the degree to which Democratic districts benefited relative to Democratic states. Finally, we show in Appendix Figure A6 a plot of state-level amount per resident on presidential vote share using both the agency data and the CGL data. There is a clear positive relationship in the agency data but no relationship in the CGL data.

Overall, we find that Democratic districts did get a modest amount more per resident than Republican districts. However, much of the differential is attributable to differential poverty rates and employment concentrations in Democratic areas at the district level and to differences in generosity of welfare programs at the state level. It is not clear whether Democratic districts and states received more on average than Republican districts because the bill targeted Democratic priorities such as poverty or whether those areas received more funds because they were Democratic.

4.4 Individual Targeting: Party Elites

Did legislators use their individual positions of power to their own benefit? We break down per capita ARRA funds by whether members had leadership positions in Congress, whether they were legislative swing voters and how long they had been members in Congress. Our findings are predominantly negative; that is, powerful politicians don't appear to have received more funds.

[23]Recall that unemployment insurance and Medicaid funds are included in the agency-reported data but not the CGL data.

The Democratic leadership in the House, including committee chairs and party leaders, did not receive more CGL funds than average. Out of 20 Democratic leaders in our sample, only 2 were in the top 10% of recipients. The top ARRA fund recipient in the Democratic Party leadership was the Speaker of the House, Nancy Pelosi, who represented San Francisco, California. She received the 11th largest amount of funds of the 334 representatives in our sample. The second largest recipient in the Democratic leadership was Patrick Murphy of Pennsylvania's District 8, which serves Levittown and surrounding areas. He received the 28th highest amount.

In Table 7, we regress amount per resident and per worker on dummies for the Democratic leadership and for the Republican leadership separately. (For Republican leaders, in addition to the party leadership, we include the ranking members of committees.) We do this with and without our baseline controls. We show that Democratic congressional leaders on average get the same or less than other members of Congress. The coefficient on the Democratic leadership is not statistically significant in any of the specifications. The coefficient is zero in the per resident specification without controls and negative in all others. In Appendix Table A4, we report the dummies separating out the Speaker of the House (Pelosi) on the Democratic side and the Minority Leader (Boehner) on the Republican side. Pelosi is an outlier and the speaker dummy is statistically significant at the one percent level in all specifications except the per worker specification with controls. In the per resident specification with controls, she gets almost $600 more per resident than other similar members. Conditional on the speaker dummy, the rest of the Democratic leadership gets significantly (at the 5% level) less when controlling for funding formula amounts, in both the per worker and the per resident specifications. They get $125 less in the per resident specification. On the other hand, returning to Table 7, we see that in all specifications, the Republican leadership gets more than average. With controls, the amounts are statistically significant at the 10% level. However, the minority leader on

the Republican side is also an outlier. Unconditionally, his district gets $335 less per resident and $151 less conditional upon controls. Despite the fact that Boehner's district is located at the 11th percentile in overall the distribution of funds, the Boehner results are significant themselves at the 1% level without controls, suggesting that these estimates are plagued by small sample sizes and skewed data. After accounting for Boehner, the rest of the Republican leadership gets $409 more on average conditional upon controls, and this is significant at the 10% level. However, again, these results are driven by two outliers, Buck McKeon and Doc Hastings, who on average, get $3319 more funds per resident. Taking out these two outliers, we do not find evidence that the rest of the Republican leadership received more than other congressional members (Appendix Table A4).

Because of the small numbers of party leaders and the skewness of the distribution of funding, we also carry out small sample tests to assess whether these individuals received a disproportionate amount of CGL funds. Intuitively, by comparing those in power during the ARRA bill to those in power in the following and prior sessions of Congress, we estimate the impact of being in power precisely when the ARRA bill passed. We matched the districts of current congressional leaders in the 111th Congress to the districts of their predecessors who held the same positions in the 110th Congress. There were 6 Republican leaders who we could match due to a number of exits of Republicans in the leadership of the 110th Congress. In 5 out of 6 cases, the district with a leader in power during the ARRA vote received more money. This would happen by random chance 4% (one-sided test) of the time. We also look at leadership changes between the 111th and 112th Congress. For Republicans, this raises our sample size to 10. We find that in 7 out of 10 cases, the district with a leader in power during the ARRA vote received more, which would happen by random chance with a probability of 8.9%. We run the same comparison for Democratic leaders as well. In the case of Democrats, there is no evidence that committee chairs received more money;

in only 5 out of 11 cases did the leader in power during the ARRA vote get more. These results are reported in Appendix Table A5.

It is more difficult to analyze the impact of committee chair positions in the Senate. Thirty-two states have senators who were either committee chairs or ranking members at the time of the ARRA vote. We run a state-level regression of the amount of stimulus funds on a dummy for a state with a committee chair or ranking minority member and we find the coefficient to be negative in all four of our usual specifications. The coefficients, however, are not statistically significant in any specification. We also note that Inman (2010) finds in his regressions that the states with committee chairs or ranking members of the powerful fiscal-related Senate committees received a statistically insignificant lower amount than other states. Overall, we find no evidence that states with powerful senators or House members received more ARRA funds.

Legislative tenure can be thought of as a proxy for social connections or institutional knowledge of the functioning of Congress. For this reason, it is possible that higher tenure legislators are able to procure a greater amount of funds. Row 3 of Table 7 shows that tenure is positively correlated with amount received per resident and per worker. Both are significant at the 10% level. However, Figure 6b shows that many of the higher tenure (shaded more heavily) observations are in the 80-90 percent range. Conditional upon baseline controls, coefficients drop by a half to two-thirds. An additional (2-year) congressional session is associated with $2.5 more in funds per resident and is not statistically significant at conventional levels. We show, in Appendix Figure A7, non-parametric and semi-parametric regressions of amount received per capita on tenure. We also break them down by party. For Republicans, we can see that very high tenure individuals actually receive less, and for Democrats, the increases in funding with tenure are solely due to a few outliers with more than 50 years in the House.

We show similar results in the Senate in the second column of Table 9.

Here the results are even starker than in the House. All of the coefficients on years in the Senate are small and none are statistically significant at conventional levels. In the next column, we show that average congressional tenure[24] in the state is positively correlated with amount received when we use the agency data, likely reflecting longer tenure for states with more generous welfare programs. Conditional upon controls, the size of the coefficient drops from $15 per resident per year of tenure to below $10 per resident per resident per year. In the CGL data, we find no significant correlation and the coefficient sign turns negative with controls.

Overall, we find little evidence that socially connected legislators or legislators in positions of power were able to grab more funds.

4.5 Individual Targeting: Pivotal Members of Congress and Party Discipline

Legislators can also use their individual power to gain political rents by claiming to be undecided. Legislators in the middle of the ideological distribution can more credibly claim to be undecided and thus need persuading in terms of rents for their district. To assess this possibility, we look at how Congress member ideology correlates with per capita receipts of ARRA funding. We use the first dimension of McCarty, Poole and Rosenthal's DW-Nominate, which is the most commonly used measure of congressional ideology. In the modern era, more than 85% of legislative voting behavior can be explained by the first dimension alone.

We are interested in how ideology correlates with funding for two reasons. First, theories of congressional politics suggest that moderate politicians may be able to capture larger amounts of rents because they are swing voters on bills and thus able to demand compensation in order to vote in favor of a bill. Second, we want to make sure that returns to vote share

[24] We define average congressional tenure to be the average number of years of tenure in the House plus the average years of tenure in the Senate divided by two.

are not simply proxying returns to ideology. In Appendix Figure A8, we semi-parametrically regress ARRA receipts per capita at the district level on DW-Nominate, controlling for baseline funding formula controls in both panels A and B but including linear vote share controls in panel B. Liberal members do get more funds. This is also seen in the first column of Table 8. However, vote share controls reduce the estimated magnitude and take away the statistical significance of DW-Nominate. Moreover, the coefficient on DW-Nominate is small and is not statistically significant in the per worker regressions with or without controls. In the second column of Table 8, we replace DW-Nominate with its absolute value to capture ideological extremity (as opposed to partisanship). The results are very similar.

The DW-Nominate results are unsurprising because the ARRA was expected to pass with a wide margin in the House and thus no one was pivotal. By contrast, in the Senate, the administration had to negotiate to get three Republicans (Susan Collins of Maine, Olympia Snowe of Maine, and Arlen Specter of Pennsylvania) to break a potential filibuster. Moreover, one conservative Democrat, Ben Nelson of Nebraska, publicly aired his ambivalence towards voting in favor of the bill. In column 3 of Table 8, we put in a dummy for districts located in Pennsylvania and Nebraska. Unfortunately, we cannot include the two Maine districts in our analysis because both contain part of the state capital. The coefficient on the dummy ranges from -89 to +71 but is far from significant in all specifications.

More importantly, in our state-level analysis in Table 9, the estimates are consistently negative and sometimes statistically significant. We show 8 results total, four with a dummy for Maine, Pennsylvania and Nebraska and four with a dummy for only Pennsylvania and Nebraska.[25] Estimates range from -$41 to -$288 across CGL and agency data sets and two of the

[25]Note that the results in the specification taking out state capitals (panel d) are identical for the regression with Maine and the one without since both Maine districts contain part of the capital, Portland, and thus are not in that sample.

seven different results are significant at a 5% level or lower. In three out of three specifications, adding in Maine lowers the magnitude of the coefficients. However, the coefficients are negative in all specifications. We take this as strong evidence that pivotal states did not get more money. If anything, they got less.

Another measure of whether legislators act in the interests of their party is whether they punish party members who deviate from the party line. For ARRA, there were a few legislators who voted against the majority of their party: Republicans who abstained from voting in the House or even voted for it in the Senate, and Democrats who abstained or voted against the ARRA bill. No House Republicans voted for the bill; however, two refrained from voting. Additionally, seven Democrats voted against the bill, one voted present and one did not vote. Party defectors did get less on average than those voting with their parties, in both the Democratic and the Republican party. Coefficients are not significant at conventional levels and amount to -$86 pooled across parties or -$88 for Democrat defiers. These results are shown in the last 2 columns of Table 8. Putting in most baseline controls does not dramatically change the estimates. However, adding land area increases coefficient magnitudes. With the full set of baseline controls, defiers do get less and results are statistically significant in per worker or per resident specifications at the 5% level or less. For Democrats, the amounts are -$144. However, looking at Figure 6d, we see that with the exception of one outlier, Democratic defiers get exactly the expected amount conditional upon vote share. Dan Lipinski, a Democrat who represents the 3^{rd} congressional district in the suburbs of Chicago, was a strong outlier, and his district received only $75 per resident. Lipinski voted "present" on the bill, and his district received the 17th lowest amount per capita in Congress. In a median regression, which reduces the influence of outliers, the coefficient on voting against party is -$49 per capita and is not statistically significant at conventional levels (results not shown).

Our results in this section combined with our results from section 5.5 indicate that ARRA did not target individuals. This is true whether they were in powerful position within Congress, pivotal for the vote outcome or whether they defied their party.

5 Interpretations and implications for future policy

To summarize our findings, we find little to no evidence of positive targeting of funds towards powerful individuals. We also find limited targeting in favor of the party that held the majority in both chambers of Congress and held the presidency. Additionally, we find no targeting of funds towards districts that had large increases in unemployment. Nonetheless, we do find that districts with high levels of employment and with a high poverty rate received more funds, though we do not find the same correlations across states.

Our findings that party leaders, swing districts, and ideological moderates did not receive more funding, and that defiers did not suffer a clear penalty, may not be surprising to close political observers. This is especially so given that contracts, grants and loans were allocated based on either formulas or competitive grants. Funding formulas are intentionally coarse instruments for the purposes of limiting politically-motivated geographical targeting. At the same time, since formula money is allocated by state governors, it is possible to a limited degree for Congress to target particular districts in small states by mandating spending in poor areas, urban areas, rural areas, areas with large highway systems and in other similar ways. Similarly, while the competitive bidding process limits the ability to target specific areas, the choice of projects still allows a degree of discretion and thus influence. Moreover, while targeting individual districts would have been difficult, the bill could have easily used funding formulas and even competitive grants to tilt money strongly towards more Democratic states. The spatial outcome for

the ARRA stands in stark contrast to findings about the other large fiscal stimulus in U.S. history: the New Deal. Fishback, Kantor and Wallis (2003) document that although New Deal *loans* were not politically targeted or targeted to high unemployment areas, *grants* were targeted both to high unemployment areas and to swing-district supporters of Roosevelt in the prior presidential campaign. In this section, we discuss some possible explanations for the limited geographic targeting in the ARRA bill, and why this may have been different during the New Deal era.

One possible explanation for our results is that the spatial allocation reflected the desires of the administration and Congress. As its name suggests, the bill had two components — recovery and reinvestment. While some parts of the bill (Unemployment Insurance, Supplemental Nutrition Assistance Program, Medicaid) were focused on recovery (or at least relief), the CGL component was more focused on reinvestment. Certainly automatic stabilizers, for example, are well targeted to those with a high propensity to consume out of income and those in (or at risk of of falling into) poverty. Perhaps, then, the spatial allocation of CGL funds reflected the variation in local needs for public goods rather than jobs.

However, even though other parts of the bill were better targeted, we find it unlikely that the allocation of CGL funds was what the administration would have preferred in the absence of political constraints. In January of 2009, the unemployment rate in California was 9.7% and rising rapidly; the unemployment rate in Michigan was 11.4%; however, the unemployment rate in North Dakota was 3.9%. Many schools and highways needed to be rebuilt, and it would not have been particularly difficult to use the severity of the business cycle in an area as a factor in allocating funds. We think a more plausible argument is that there are certain tradeoffs that arise when greater targeting in fiscal policies is allowed, and the administration's relatively non-targeted approach reflected constraints and not preferences.

There are a number of costs that may arise from finer grained geographic

targeting. First, there may be increased delays in project selection due to bargaining between legislators, something which runs counter to rapid enactment in face of a major economic crisis. For example, when Congress tried to set aside a portion of the Medicaid money for high unemployment areas, the Senate opted for a lower percentage than the House and this caused delay in bargaining in the conference committee. Second, such discretion may lead to precisely the type of political opportunism that is both predicted by theories of legislative politics and at least partly confirmed by the New Deal experience. Such opportunism could have deligitimized a piece of legislation that was already quite controversial and under substantial scrutiny. For these reasons, political constraints — real or perceived — may have prevented the administration from engaging in a more targeted approach, instead favoring the use of funding formulas and competitive bidding.

In a majoritarian system where politicians gain re-election in part by providing funds to their districts, funding formulas limit the degree to which politicians are able to do so. Starting with the Federal-Aid Highway Act of 1916, Congress increasingly used formulas to allocate federal funds. Besides attempting to reduce graft, formulas were introduced to allocate funds streamlined bargaining by allowing Congress to decide on the level of funding and then delegate the dispersion of those funds. It allowed for Congress to retain control over what types of projects are funded while bypassing bargaining over the spatial distribution of the funding in which politicians may be particularly interested. Over time, the use of formulas to allocate funds has become more prevalent. In addition to allowing for timely passage of the legislation, the channeling of money through existing programs also provided a method to ensure that funds were spent quickly, which was a priority for this particular piece of legislation.

However, while a relatively non-targeted approach has merits in terms of reducing the possibility of graft, and possibly expediting the pace of legislation, it comes with costs as well. In particular, it reduces the ability to target

based on *economic* as well as political considerations. It is noteworthy that while the fiscal stimulus in the New Deal may have provided greater awards to swing districts, it also channeled more money to higher unemployment areas. The good news is that political constraints appear to have made fiscal policy more politically neutral; the bad news is that those constraints may have also reduced the countercyclical efficacy.

This has a number of implications for future stimulus bills. First, it means that the components of the policy which are better targeted — e.g., automatic stabilizers — are quite important and it may be useful to put greater weight on these. At the same time, besides automatic stabilizers, there are economically sensible reasons to increase public goods provision as part of a countercyclical policy, as discussed in DeLong and Summers (2013). The relevant hurdle rate for such projects effectively falls, and an optimal fiscal policy should likely include an expansion in the funding of such projects. However, it should be possible to include local area unemployment rates, or other transparent measures of excess capacity, as a factor in the funding allocation for contracts, grants and loans. Such an approach would combine the virtues of a more rules-based policy regime with some of the gains from a more targeted variant of stimulus policy. Had we found that the political process generated sufficient demand for added funding in harder hit areas, the need for such an explicit rule would not arise. However, that is not what we found, suggesting that the inclusion of local excess capacity measures in funding formulas is likely to have a substantial payoff. Moreover, we note that the government relied on *already existing* funding formulas to disperse the funds, which means that the time for updating the formulas — in order to better accommodate the objectives of countercyclical fiscal policy — is probably now, before the next crisis hits. Of course, this presupposes understanding how to create new legislative norms.

6 Conclusion

In this paper, we have looked at the spatial distribution of ARRA funds to assess how it was targeted economically and politically, and to use it as a window towards evaluating theories of political economy. We find that funds were distributed relatively equally across states. Moreover, dropping a small number of outlier districts who each received money for a particular large project, we find that funds were distributed relatively equally across congressional districts. We find no evidence that funds were funneled towards powerful or pivotal legislators nor do we find that parties punished members of Congress who defied their party. We do find that urban Democratic districts received a moderately higher amount of per capita funding. However, most of this gap between Republicans and Democrats disappears when we control for a small number of district or state characteristics that appeared in the formulas used to allocate funds. Formulas based upon welfare programs administered at the state level are very useful in explaining cross-state variation in funding; poverty rates and employment, which also are pervasively used in federal formulas, are strongly predictive of the cross-district variation.

A comparison of the targeting of discretionary funds under the ARRA with New Deal legislation is useful. Grants under the New Deal appear to have been targeted both to higher unemployment areas as well as to swing district supporters of President Roosevelt. The reduced political and cyclical targeting in the ARRA may reflect a more rules-based environment today compared to the 1930s and may also explain the lower amount of variation across space in federal transfers and loans. We hope that future stimulus policies will increase countercyclical targeting by explicitly using measures of local unemployment and excess capacity as part of the allocation of contracts, grants and loans. This was done in the allocation of Medicaid funds already in ARRA.

We see our findings and their contrast with the empirical literature on targeting in the Great Depression as evidence of a shift in social norms over

the allocation of federal funds. However, we would like to see more conclusive evidence on the role of norms. In general, we see our findings as a call for continued empirical research on the distribution of funds. If there has been a large shift in norms, have norms within the legislature shifted? Or is the norms shift within the public? Alternatively, has the presence of pork lessened over time due to greater coverage of politics in the media (Levitt and Snyder, 1995)? These are questions that empirical political economists will hopefully address.

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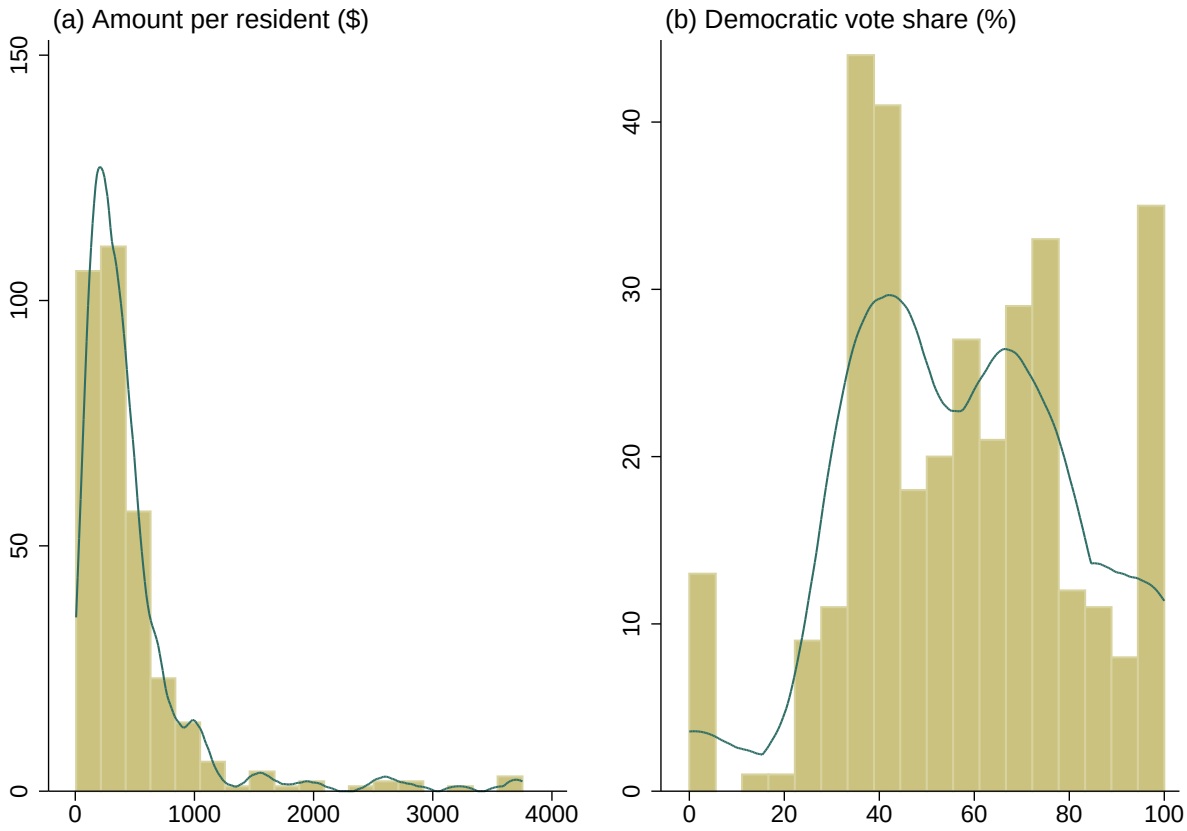
Figure 1. Histograms of District-Level Stimulus Amount and Democratic Vote Share



Notes: Sample includes the 334 districts not containing state capitals. Stimulus data includes only contract, grants, and loans reported on Recovery.gov. Democratic vote share is the two-party vote share: the percentage of Democratic votes out of total votes for the Democratic and Republican candidate in the 2008 congressional election.

Figure 2. District-Level Outliers

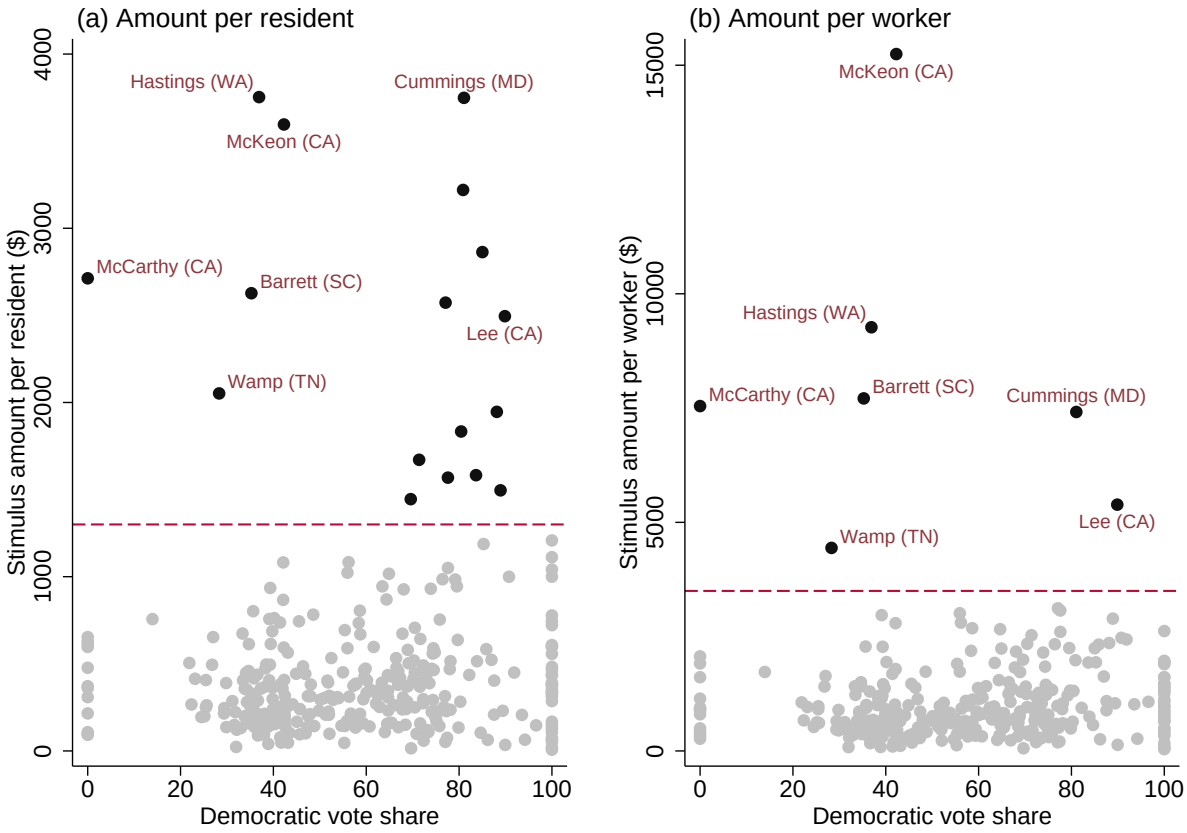
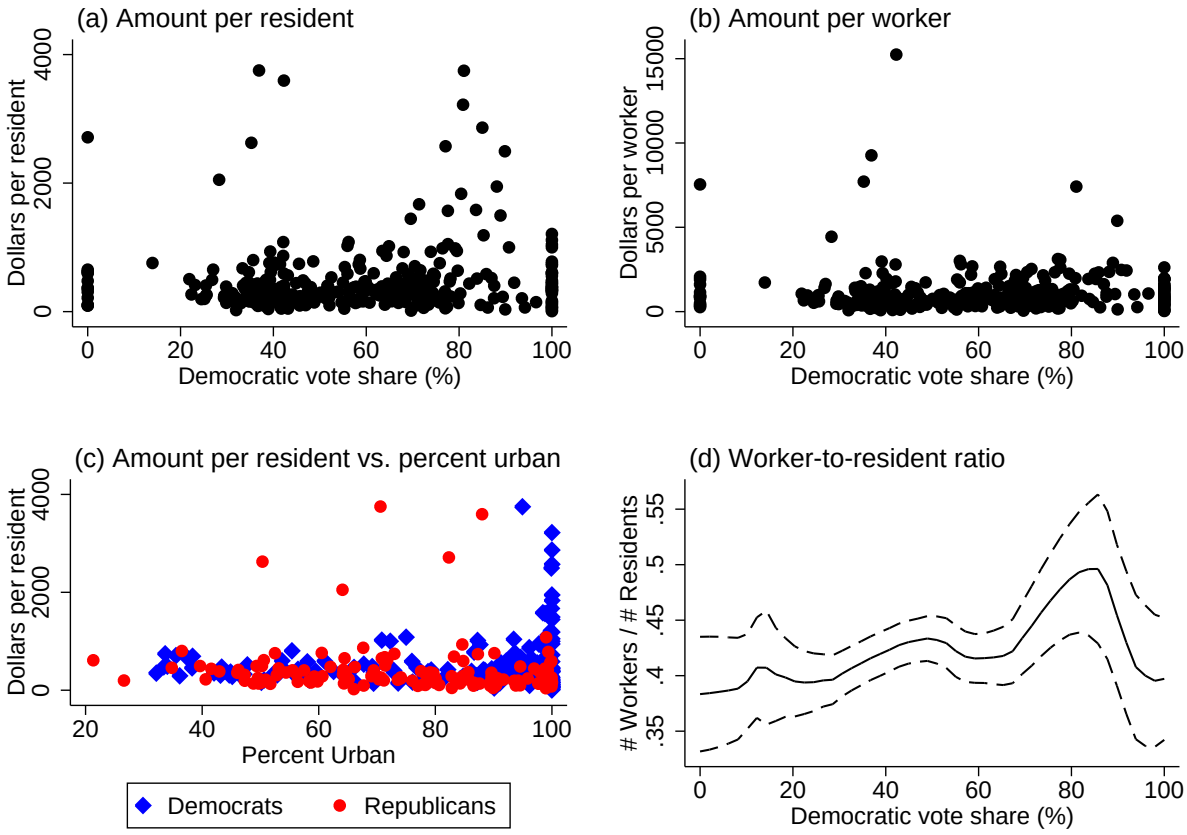
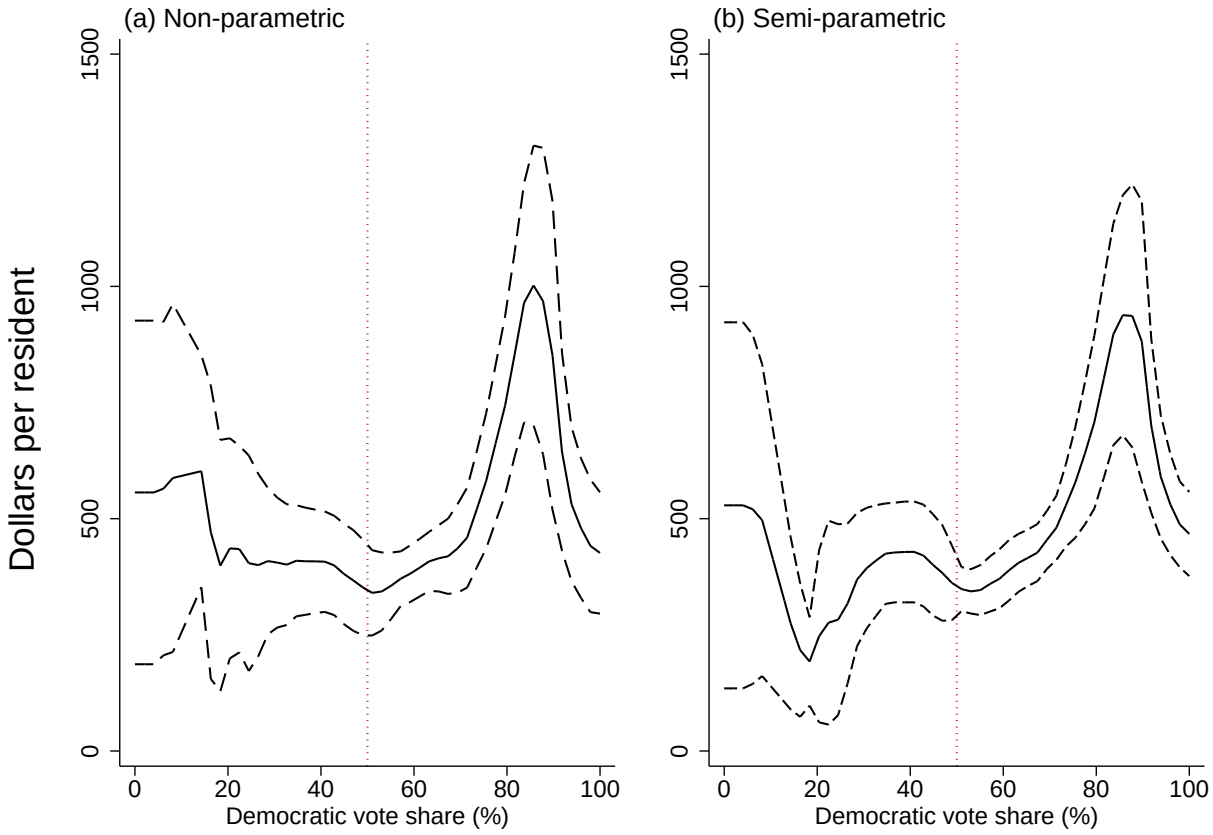


Figure 3: District-Level Stimulus Amount Per Resident and Amount Per Worker



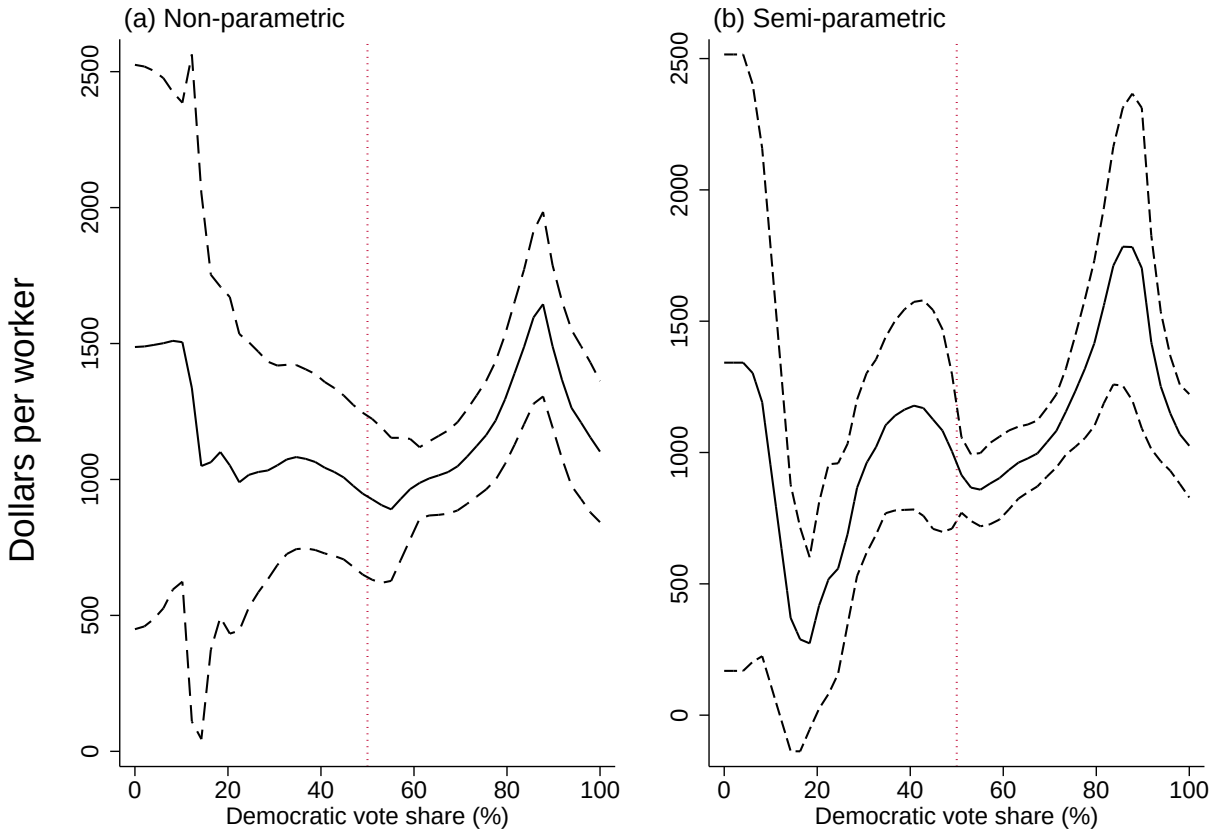
Notes: For the scatter plots, each dot represents one of the 334 districts not containing state capitals. "Amount per resident" refers to people living in the congressional district; "Amount per worker" refers to people employed within the congressional district, though they may reside elsewhere. Panel (d) displays the results of a non-parametric regression of the worker-to-resident ratio on the Democratic vote share.

Figure 4: District-Level Non-parametric and Semi-parametric Regressions of Amount Per Resident on Democratic Vote Share



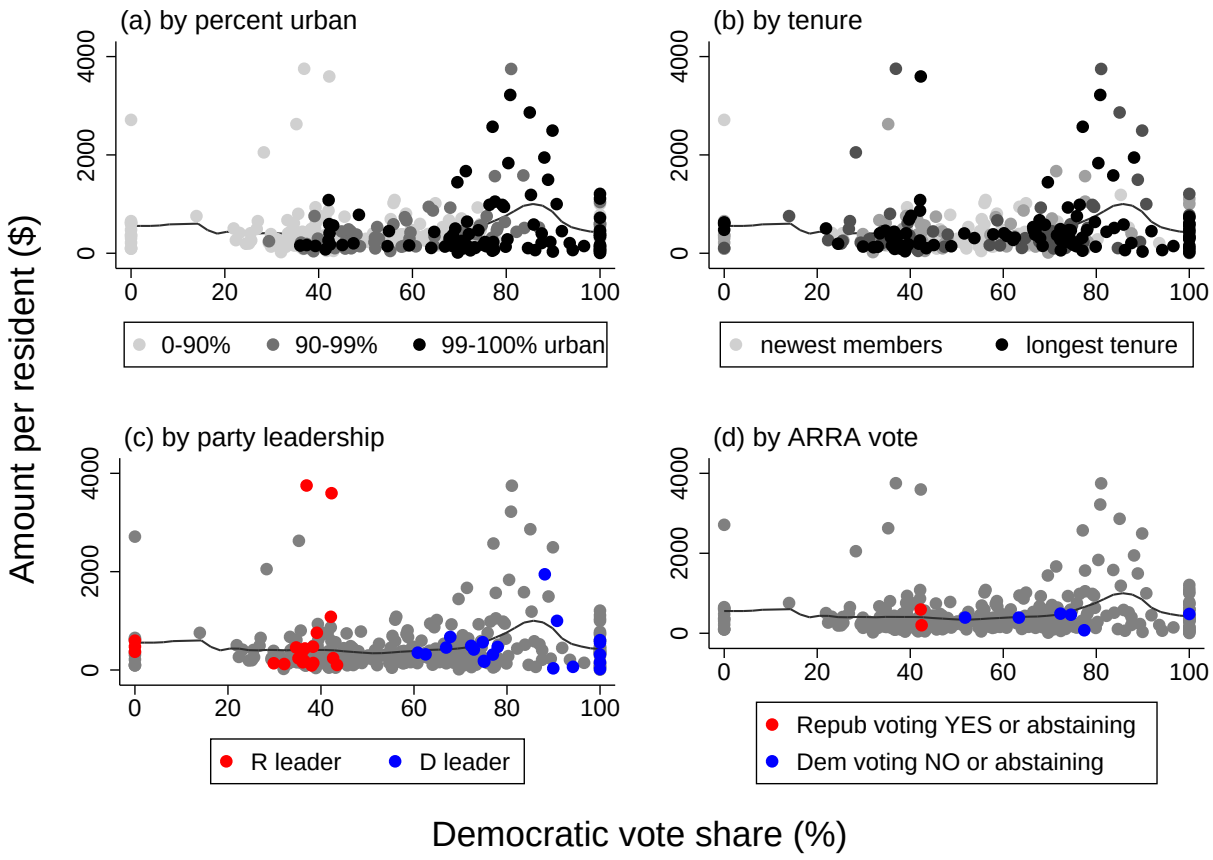
Notes: Panel (a) is a non-parametric regression (using kernel-weighted local polynomial smoothing) of stimulus amount per resident on Democratic vote share. Panel (b) reports the results of semi-parametric regression that includes (parametric) controls for employment, poverty rate, percent urban, land area, and road miles. Sample includes 334 congressional districts not located in state capitals.

Figure 5: District-Level Non-parametric and Semi-parametric Regressions of Amount Per *Worker* on Democratic Vote Share



Notes: Corresponds to Figure 3, except that the dependent variable in this figure is amount per worker, and employment is not included in the set of controls in Panel (b).

Figure 6: District-Level Stimulus Amount Per Resident



Notes: Each dot represents one of the 334 districts not containing state capitals. Percent urban is the percentage of the district population living in urban areas. Tenure refers to the number of terms served by the congressional representative. Leadership refers to party leaders and committee chairs (for Democrats) or ranking members (for Republicans) in the House of Representatives.

Table 1: Descriptive Statistics

Variable	With Capitals Mean	With Capitals Std. Dev.	Without Capitals Mean	Without Capitals Std. Dev.
State level				
CGL amount (millions)	5334	5705	2112	3133
CGL amount per capita	1056	450	517	262
Agency-reported amount (millions)	9444	10685		
Agency-reported amount per capita	1617	429		
Δ Unemployment Rate Jan07-Jan09	2.96	1.25		
Employment per resident (%)	45	4.11		
Poverty Rate (%)	12.7	2.94		
Medicaid per capita	976	326		
Interstate miles per capita	.00029	.00035		
State population (millions)	6.13	6.8		
Avg Senate Dem Vote Share (%)	52.4	13.9		
District level				
CGL amount (millions)	613	1213	316	370
CGL amount per capita	900	1786	469	543
Stimulus amount per worker	1941	3623	1101	1308
Poverty Rate (%)	13.4	5.56	13.6	5.83
Percent urban	79	19.8	79.9	20.3
Land Area (square miles)	.813	3.07	.54	1.01
Total highway miles	852	975	762	864
Employment (1000s of workers)	297	100	289	103
% spent in 1 year	32.2	9.9	31.2	9.19
House Democratic vote share	57.7	23.4	57.8	24.4
Tenure (number of 2-year terms)	6.24	4.55	6.53	4.65
DW-Nominate score	-.0336	.441	-.0329	.44
Total CGL amount	$267 billion		$106 billion	
Total agency-reported amount	$472 billion			

Notes: CGL amount refers to the funds received by the state or district in the form of contracts, grants and loans. Employment per resident is calculated as total state employment divided by state population times 100. Medicaid per capita is the per capita Medicaid expenditure in 2005. The district-level political variables (Tenure, Democratic vote share, DW-Nominate score) refer to the House member representing that district.

Table 2: District-Level Economic Targeting

Independent Var:	Unemp Rate	Change in Unemp	UI per capita	% spent in 1 year	Employment (thousands)	Poverty Rate	% Urban
Panel (a): Amount per resident							
No controls	-8.840	-32.68	-26.85	-10.50***	1.962***	16.14***	1.562
	(11.62)	(25.83)	(322.1)	(3.390)	(0.366)	(5.911)	(1.015)
R^2	0.001	0.007	0.000	0.032	0.139	0.030	0.003
Panel (b): Amount per resident							
All controls except	8.523	-2.242	484.6	-10.21***	1.970***	14.05*	0.533
land area	(14.47)	(31.37)	(475.1)	(3.683)	(0.321)	(8.172)	(2.004)
R^2	0.257	0.256	0.260	0.285	0.256	0.256	0.256
Panel (c): Amount per resident							
All controls	6.824	-6.252	505.1	-9.430**	2.019***	12.87	-1.579
	(15.24)	(33.04)	(484.4)	(3.557)	(0.315)	(8.219)	(1.434)
R^2	0.277	0.277	0.280	0.300	0.277	0.277	0.277
Panel (d): Amount per worker							
No controls	31.09	-32.07	42.14	-33.32***	-0.207	42.03***	-2.963
	(31.62)	(66.99)	(728.4)	(11.23)	(0.477)	(11.52)	(2.594)
R^2	0.002	0.001	0.000	0.055	0.000	0.035	0.002
Panel (e): Amount per worker							
All controls	22.41	-6.855	1061.7	-30.57***	0.052	30.76*	-2.544
	(35.70)	(74.86)	(1032.1)	(10.81)	(0.341)	(16.45)	(3.847)
R^2	0.102	0.101	0.104	0.144	0.101	0.101	0.101
Observations	334	334	334	334	334	334	334

Notes: The dependent variable is district-level stimulus receipts as contracts, grants and loans for 334 districts not in state capitals; panels (a)-(c) use stimulus amount per resident, and panels (d)-(e) use amount per worker in the district. UI per capita indicates year 2008 total district-level unemployment insurance receipts in thousands of dollars per capita. The district unemployment rate is the value for January 2009. The change in unemployment is the difference between the unemployment rates in January 2009 and January 2007. "All controls" include the 9 vote share dummies shown in Table 5, as well as employment, poverty rate, percent urban, land area, and road miles (though employment is not included in the per worker specifications). Robust standard errors in parentheses, adjusted for clustering at the state level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 3: State-Level Economic Targeting

Independent Variable:	Change in unemployment	State Employment	Percent Poverty
Panel (a): Amount per resident – Agency-reported			
No controls	-42.60	18.31*	-48.28*
	(40.38)	(10.49)	(25.20)
R^2	0.015	0.031	0.110
Observations	50	50	50
Panel (b): Amount per resident – Agency-reported			
With controls	-7.591	-30.79	-66.69*
	(65.92)	(20.68)	(37.15)
R^2	0.436	0.436	0.436
Observations	50	50	50
Panel (c): Amount per resident – Contracts, Grants, Loans			
With controls	-18.45	-41.32**	-49.55**
	(42.83)	(18.91)	(22.54)
R^2	0.683	0.681	0.681
Observations	50	50	50
Panel (d): Amount per resident – Contracts, Grants, Loans			
With controls	-88.73	-57.58**	-88.80*
(no capitals)	(56.59)	(25.96)	(46.75)
R^2	0.379	0.284	0.284
Observations	37	37	37

Notes: The dependent variable is stimulus amount per state resident, using the agency-reported data from Recovery.gov for panels (a) and (b), and the contracts, grants and loans data for panels (c) and (d). Panel (d) omits the funds allocated to state capital districts. Each cell reports the coefficient from a separate regression. The change in unemployment is the difference between the unemployment rates in January 2009 and January 2007. State Employment refers to the total employment in the state divided by state population, multiplied by 100. "Controls" include medicaid expenditures per capita, interstate highway miles, the poverty rate, employment per state resident, and the average of the two-party Democratic vote share in the prior election for each of the two senators. Robust standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 4: District-Level Group Targeting: Political Party

	Per Resident						Per Worker	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Democratic Representative	373.4***	94.96**	92.88*	19.00	109.3**	33.88	34.00	-85.12
	(122.6)	(45.44)	(51.08)	(61.58)	(50.07)	(64.45)	(158.4)	(181.4)
Employment (thousands)			1.953***	2.172***	2.097***	2.247***		
			(0.366)	(0.395)	(0.322)	(0.359)		
Percent Poverty				22.19***		20.49**		39.85**
				(7.437)		(7.859)		(14.80)
Land Area					108.0*	103.6*		327.6*
					(54.13)	(54.72)		(193.5)
Total Highway miles					-0.0171	-0.0399		-0.152
					(0.0498)	(0.0596)		(0.193)
Percent Urban					0.183	0.198		1.576
					(1.391)	(1.441)		(3.494)
Constant	683.6***	415.5***	-148.8	-472.1***	-260.2*	-521.1***	1085.1***	423.0
	(78.84)	(48.56)	(103.3)	(148.1)	(129.6)	(185.9)	(162.7)	(347.5)
Observations	433	332	332	332	332	332	332	332
R^2	0.011	0.007	0.145	0.195	0.176	0.217	0.000	0.071
Includes state capitals	X							

Notes: The dependent variable in columns (1)-(6) is district-level stimulus receipts as contracts, grants and loans, per resident; in column (7)-(8), the dependent variable is stimulus receipts per worker employed in the district. Democrat is a dummy variable for the party affiliation of the member of the House of Representatives. Column (1) includes data for all congressional districts; columns (2)-(8) include data for only those districts not located in state capitals. Two House seats were vacant at the time of the ARRA vote, and they are coded as missing party affiliation, so the number of observations is 2 less than in other tables. Robust standard errors in parentheses, adjusted for clustering at the state level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 5: District-Level Group Targeting: Vote Share Blocks

	Per Resident			Per Worker	
	(1)	(2)	(3)	(4)	(5)
Unopposed Republican (13)	161.5	242.1**	123.8	437.9	150.7
	(130.9)	(115.7)	(106.5)	(278.7)	(290.3)
Dem vote share 1-30 (16)	70.13	147.9	-95.42	77.35	-579.6
	(133.8)	(124.8)	(174.0)	(343.1)	(537.4)
Dem vote share 30-40 (63)	-6.304	60.67	26.38	-31.88	-117.2
	(100.3)	(105.8)	(91.28)	(321.4)	(287.7)
Dem vote share 50-60 (42)	-32.82	13.01	-56.16	-138.5	-330.1
	(66.14)	(89.64)	(87.81)	(285.7)	(298.4)
Dem vote share 60-70 (44)	12.87	86.87	15.65	-39.66	-228.8
	(67.85)	(75.33)	(71.92)	(263.1)	(254.6)
Dem vote share 70-80 (51)	105.4**	146.4***	100.1	34.42	-101.7
	(40.03)	(47.52)	(64.82)	(219.4)	(235.6)
Dem vote share 80-90 (20)	792.9***	685.3***	603.7***	837.3**	658.6
	(144.0)	(147.3)	(189.1)	(390.6)	(460.0)
Dem vote share 90-99 (5)	-21.47	261.3*	106.6	405.7	-100.5
	(188.0)	(135.5)	(206.7)	(395.0)	(551.4)
Unopposed Democrat (34)	47.45	125.2	51.49	-16.47	-215.5
	(103.5)	(115.0)	(139.3)	(353.5)	(374.0)
Employment (thousands)		1.741***	2.019***		
		(0.334)	(0.315)		
Observations	334	334	334	334	334
R^2	0.121	0.220	0.277	0.031	0.101
Additional controls			X		X

Notes: The dependent variable in columns (1)-(3) is stimulus receipts per district resident; the dependent variable in columns (4)-(5) is stimulus receipts *per worker* (i.e., per person employed in the district). The vote share blocks are dummy variables that equal 1 if the Democratic vote share for that representative falls in the specified range. The number of representatives in each group is indicated in parentheses. The omitted category is 40-50% Democratic vote share, and there are 46 representatives in this group. "Additional controls" include poverty rate, percent urban, land area, and road miles. Robust standard errors in parentheses, adjusted for clustering at the state level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 6: State-Level Group Targeting: Partisanship

Independent Variable:	Democratic Governor	Obama 50-52%	# Dem Senators	Senate Dem Vote Share
Panel (a): Amount per resident – Agency-reported				
No controls	43.78	-239.2***	185.8***	10.41***
	(120.7)	(76.71)	(62.13)	(3.739)
R^2	0.003	0.018	0.136	0.115
Observations	50	50	49	50
Panel (b): Amount per resident – Agency-reported				
With controls	14.86	-101.1	129.9***	8.765***
	(103.0)	(84.47)	(43.97)	(2.966)
R^2	0.367	0.370	0.424	0.436
Observations	50	50	49	50
Panel (c): Amount per resident – Contracts, Grants, Loans				
With controls	-134.9	-175.5*	69.82	6.902**
	(82.55)	(89.56)	(47.63)	(3.157)
R^2	0.664	0.651	0.655	0.681
Observations	50	50	49	50
Panel (d): Amount per resident – Contracts, Grants, Loans				
With controls	96.18	-233.2***	0.253	-0.232
(no capitals)	(70.38)	(80.80)	(49.94)	(2.857)
R^2	0.312	0.340	0.281	0.284
Observations	37	37	36	37

Notes: The dependent variable is stimulus amount per state resident, using the agency-reported data from Recovery.gov for panels (a) and (b), and the contracts, grants and loans data for panels (c) and (d). Panel (d) omits the funds allocated to state capital districts. Each cell reports the coefficient from a separate regression. Obama 50-52% is a dummy variable indicating whether 2008 presidential vote share fell in that range. # Dem Senators is a count of the number of Democratic or independent senators for that state, and takes a value of 0, 1, or 2; Minnesota is omitted from the specification since one Senate seat was vacant at the time. Senate Dem Vote Share refers to the average of the two-party Democratic vote share in the prior election for each of the two senators. "Controls" include medicaid expenditures per capita, interstate highway miles, the poverty rate, and employment per state resident. Robust standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 7: District-Level Individual Targeting: Party Elites

	Per Resident		Per Worker		Per Resident		Per Worker	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Democratic leader	0.155	-85.94	-18.35	-246.7				
	(76.52)	(67.47)	(155.6)	(156.0)				
Republican leader	232.0	372.5*	1027.6	1139.7*				
	(195.3)	(197.7)	(709.7)	(661.5)				
Number of terms					7.021*	2.430	14.06*	7.567
					(3.506)	(3.079)	(7.780)	(7.331)
Observations	334	334	334	334	332	332	332	332
R^2	0.010	0.301	0.035	0.140	0.004	0.276	0.002	0.101
Vote share controls		X		X		X		X
Additional controls		X		X		X		X

Notes: The dependent variable is district-level stimulus receipts as contracts, grants and loans for 334 districts not in state capitals, per resident or per worker. The leadership and number of terms variables refer to the member of the House of Representatives. Democratic leaders include committee chairs as well as the Speaker of the House (Pelosi); Republican leaders include ranking minority members of committees as well as the Minority Leader (Boehner); the Majority Leader and both Whips represented districts in state capitals, and so are not included in the sample. "Additional controls" include employment, poverty rate, percent urban, land area, and road miles. Robust standard errors in parentheses, adjusted for clustering at the state level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 8: District-Level Individual Targeting: Party Discipline and Pivotal Members

Independent Variable:	DW-Nominate	abs(DW-N)	PA, NE	Vote Against Party or Abstain	Democrats voting against
Panel (a): Amount per resident					
No controls	-145.4***	308.6*	-6.932	-86.49	-87.65
	(51.83)	(159.3)	(38.24)	(57.93)	(68.22)
R^2	0.014	0.008	0.000	0.001	0.000
Panel (b): Amount per resident					
All controls except	-108.7	6.933	40.70	-82.79	-66.96
land area	(153.9)	(161.6)	(44.25)	(49.96)	(60.11)
R^2	0.256	0.255	0.257	0.255	0.257
Panel (c): Amount per resident					
All controls	-119.5	14.43	70.76	-132.1***	-143.9**
	(158.2)	(166.3)	(52.22)	(46.71)	(57.75)
R^2	0.276	0.275	0.277	0.277	0.278
Panel (d): Amount per worker					
No controls	-76.02	385.6	-89.36	-230.5	-150.5
	(167.6)	(359.1)	(99.54)	(145.9)	(167.6)
R^2	0.001	0.002	0.000	0.001	0.000
Panel (e): Amount per worker					
All controls	-27.32	132.3	33.91	-323.7**	-334.8**
	(366.5)	(412.2)	(143.5)	(126.3)	(159.2)
R^2	0.101	0.101	0.101	0.102	0.102
Observations	332	332	334	332	334

Notes: The dependent variable is district-level stimulus receipts as contracts, grants and loans for 334 districts not in state capitals; panels (a)-(c) use stimulus amount per resident, and panels (d)-(e) use amount per worker in the district. Each cell reports the coefficient from a separate regression. The DW-Nominate and Voting Against Party columns refer to the member of the House of Representatives. abs(DW-N) is the absolute value of the DW-Nominate score. "PA, NE" is a dummy variable for districts located in those states. "Vote against party or abstain" includes 5 Democrats who voted against the bill, 1 Democrat who voted present, and 2 Republicans who did not vote. "Democrats voting against" include 5 Democrats who voted against the bill and 1 who voted present. "All controls" include the 9 vote share dummies shown in Table 5, as well as employment, poverty rate, percent urban, land area, and road miles (though employment is not included in the per worker specifications). Robust standard errors in parentheses, adjusted for clustering at the state level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

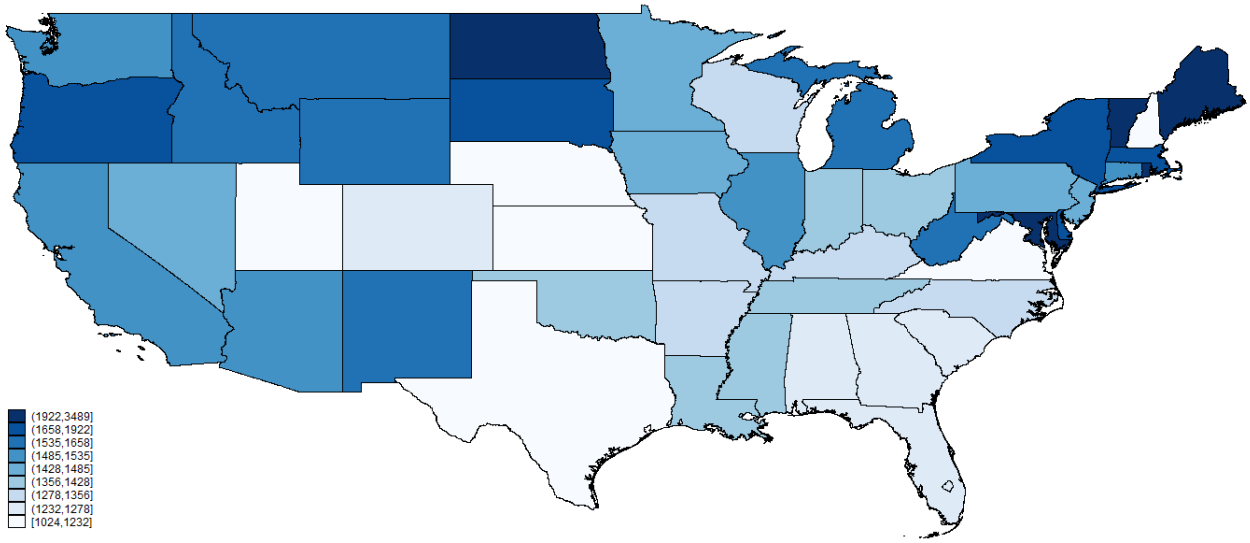
Table 9: State-Level Individual Targeting: Party Elites and Pivotal Members

Independent Variable:	Senate Chair or Ranking Member	Average Senate Tenure	Average Cong Tenure	NE, PA ME	NE, PA
Panel (a): Amount per resident – Agency-reported					
No controls	-44.37	3.964	15.91**	-56.43	-287.5**
	(150.9)	(5.035)	(7.712)	(211.8)	(134.4)
R^2	0.003	0.007	0.043	0.001	0.018
Observations	50	50	50	50	50
Panel (b): Amount per resident – Agency-reported					
With controls	-74.95	2.458	4.186	-101.7	-249.8***
	(99.00)	(3.614)	(5.501)	(131.30)	(71.32)
R^2	0.442	0.439	0.439	0.439	0.448
Observations	50	50	50	50	50
Panel (c): Amount per resident – Contracts, Grants, Loans					
With controls	-90.19	0.277	9.043	-69.75	-108.5
	(65.31)	(3.714)	(7.284)	(113.5)	(112.4)
R^2	0.689	0.681	0.693	0.682	0.683
Observations	50	50	50	50	50
Panel (d): Amount per resident – Contracts, Grants, Loans					
With controls	-133.4	-3.629	-4.796	-40.75	-40.75
	(84.54)	(2.910)	(4.130)	(61.78)	(61.78)
R^2	0.333	0.298	0.294	0.285	0.285
Observations	37	37	37	37	37

Notes: The dependent variable is stimulus amount per state resident, using the agency-reported data from Recovery.gov for panels (a) and (b), and the contracts, grants and loans data for panels (c) and (d). Panel (d) omits the funds allocated to state capital districts. "Senate Chair or Ranking Member" is a dummy variable indicating whether a senator from that state is the highest ranking Democratic or Republican member of a Senate committee. Average Senate Tenure is the average number of years served by the 2 senators from that state. Average Congressional Tenure is calculated by taking a simple average of Average Senate Tenure and Average House Tenure, where the latter indicates the average number of years served by members of the House in that state. "Controls" include medicaid expenditures per capita, interstate highway miles, the poverty rate, employment per state resident, and the average of the two-party Democratic vote share in the prior election for each of the two senators. Robust standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

A1 Online Appendix

Figure A1. State-Level Stimulus Amount Per Resident



Notes: Stimulus funds paid out as of February 2014, by state. Source is Federal agency-reported data from Recovery.gov. Total amount for 50 states is $460 billion, and includes spending on entitlements as well as contracts, grants, and loans.

Figure A2a. District-Level Amount Per Resident, With State Capitals

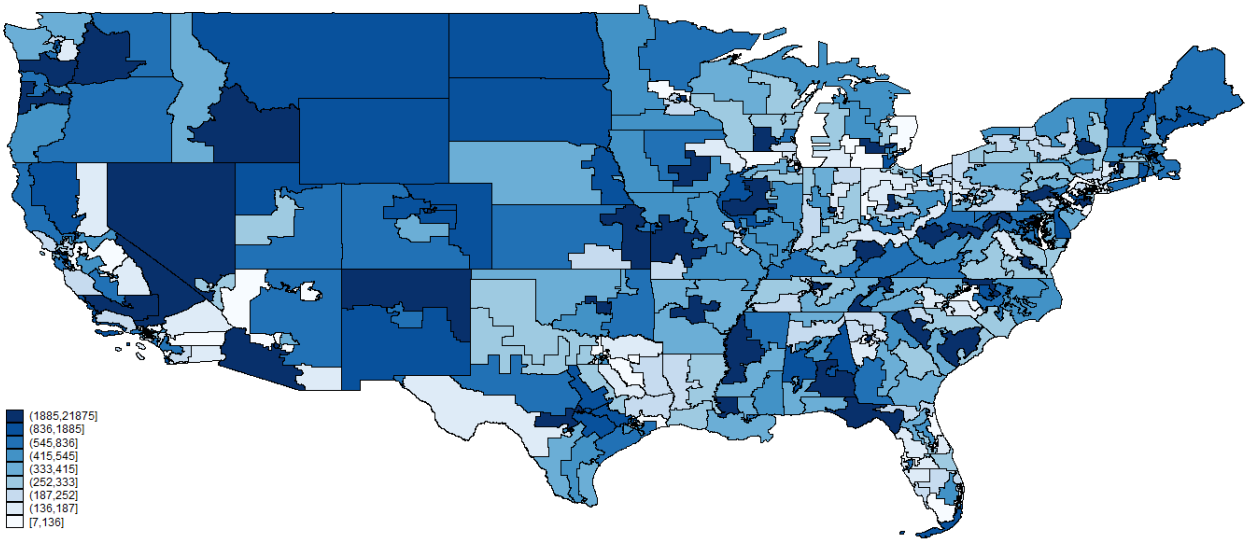


Figure A2b. District-Level Amount Per Resident, Without State Capitals

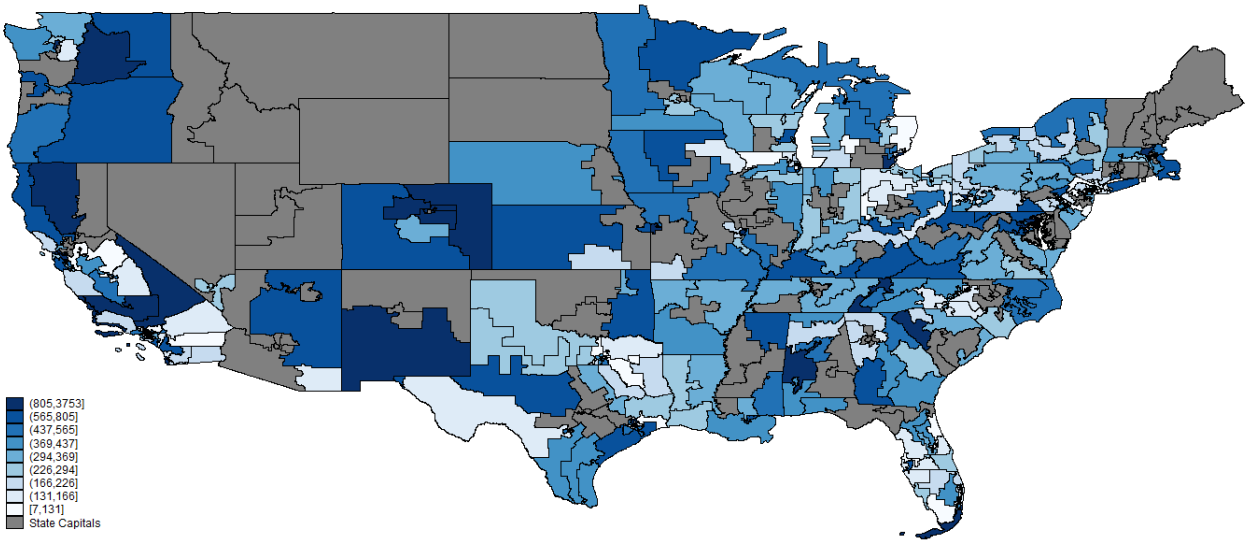


Figure A3. Proportion of Total District Amount Accounted for by Largest 5% of Awards in that District

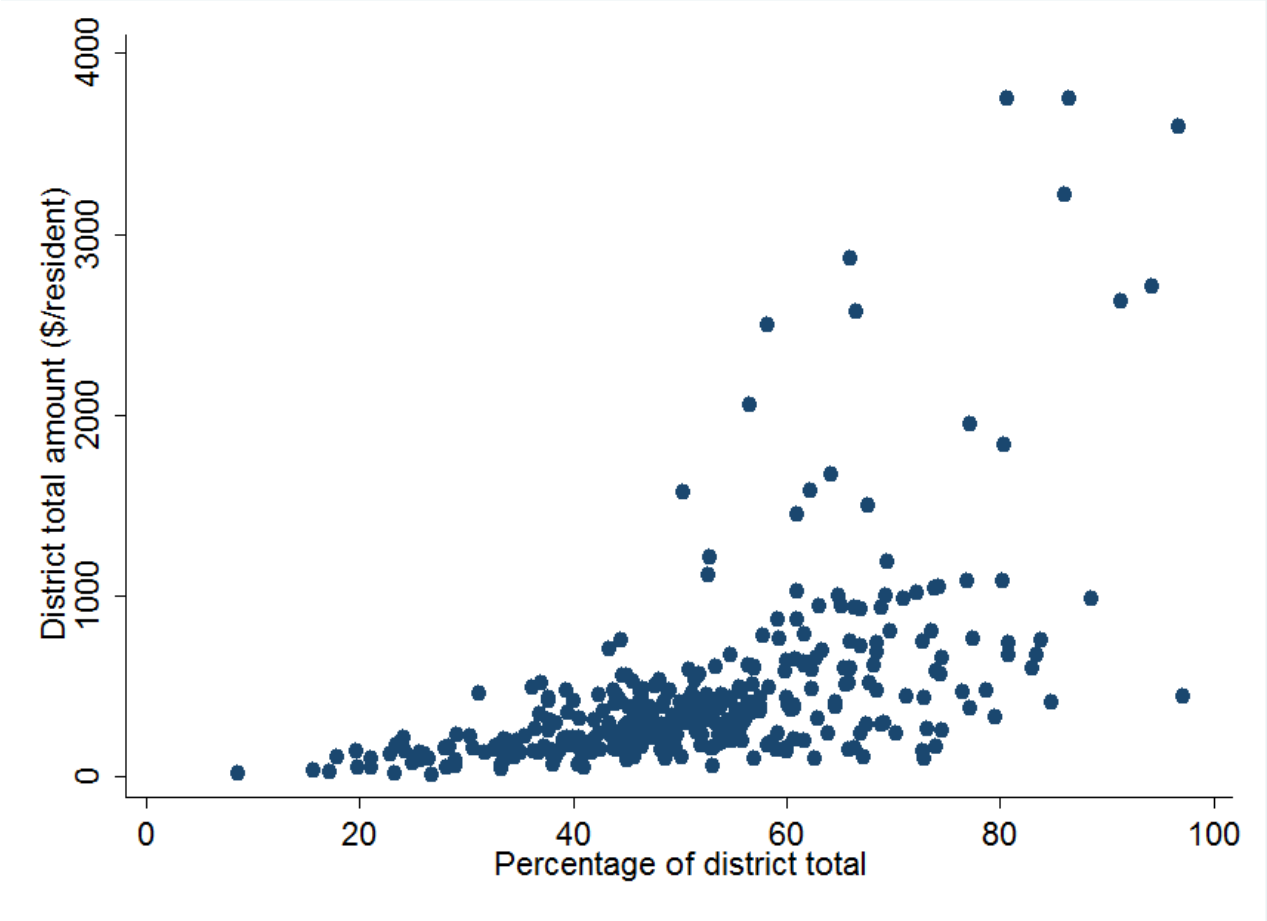


Figure A4. District-level: Amount Per Resident vs. Unemployment Rate and Excess Capacity

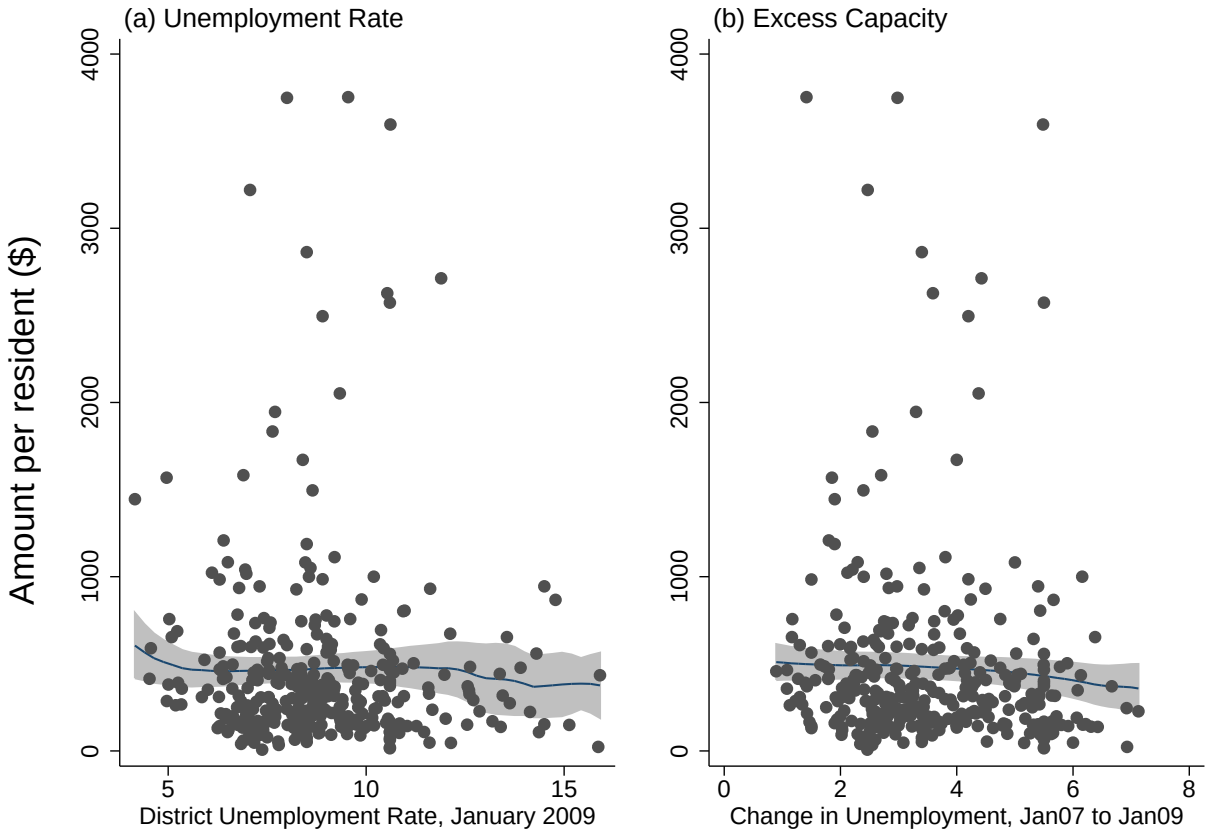


Figure A5. District-level: Shovel readiness

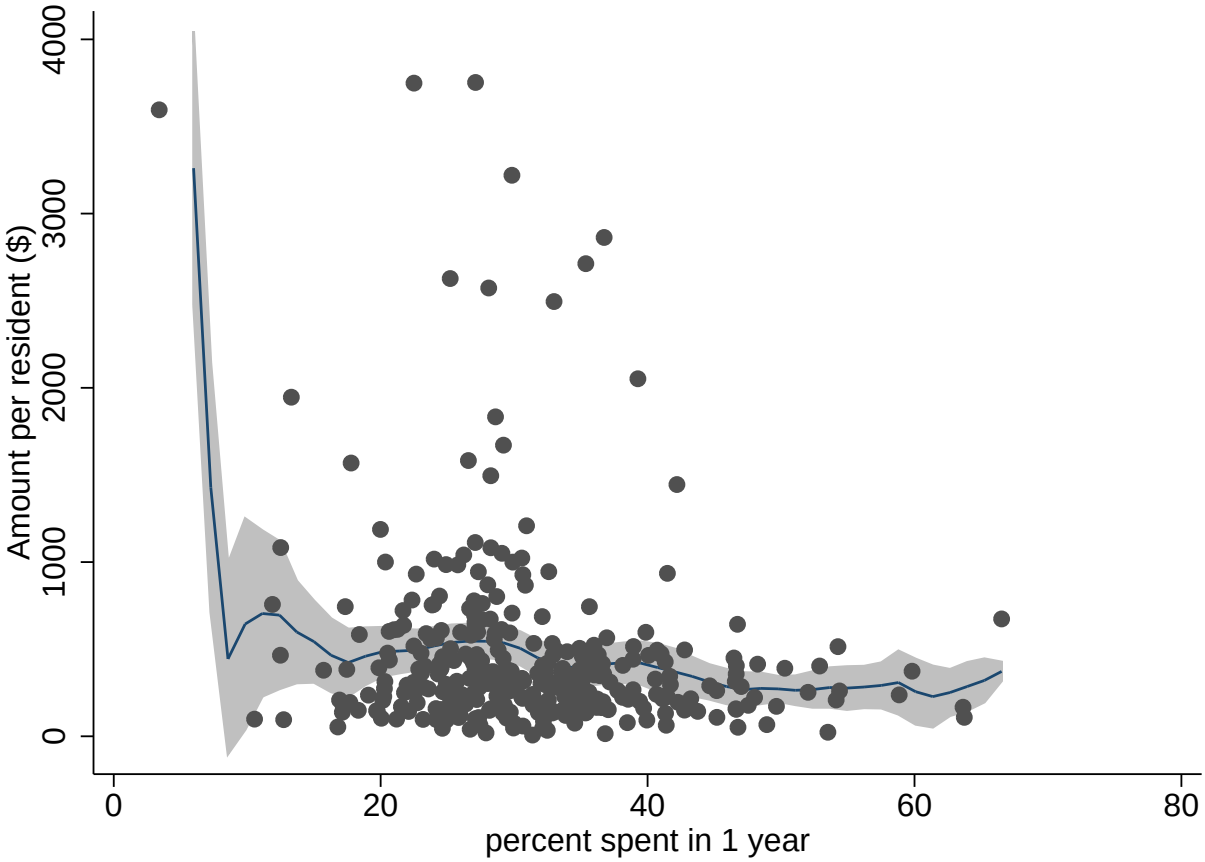


Figure A6: State-Level Amount Per Resident vs. Presidential Vote Share

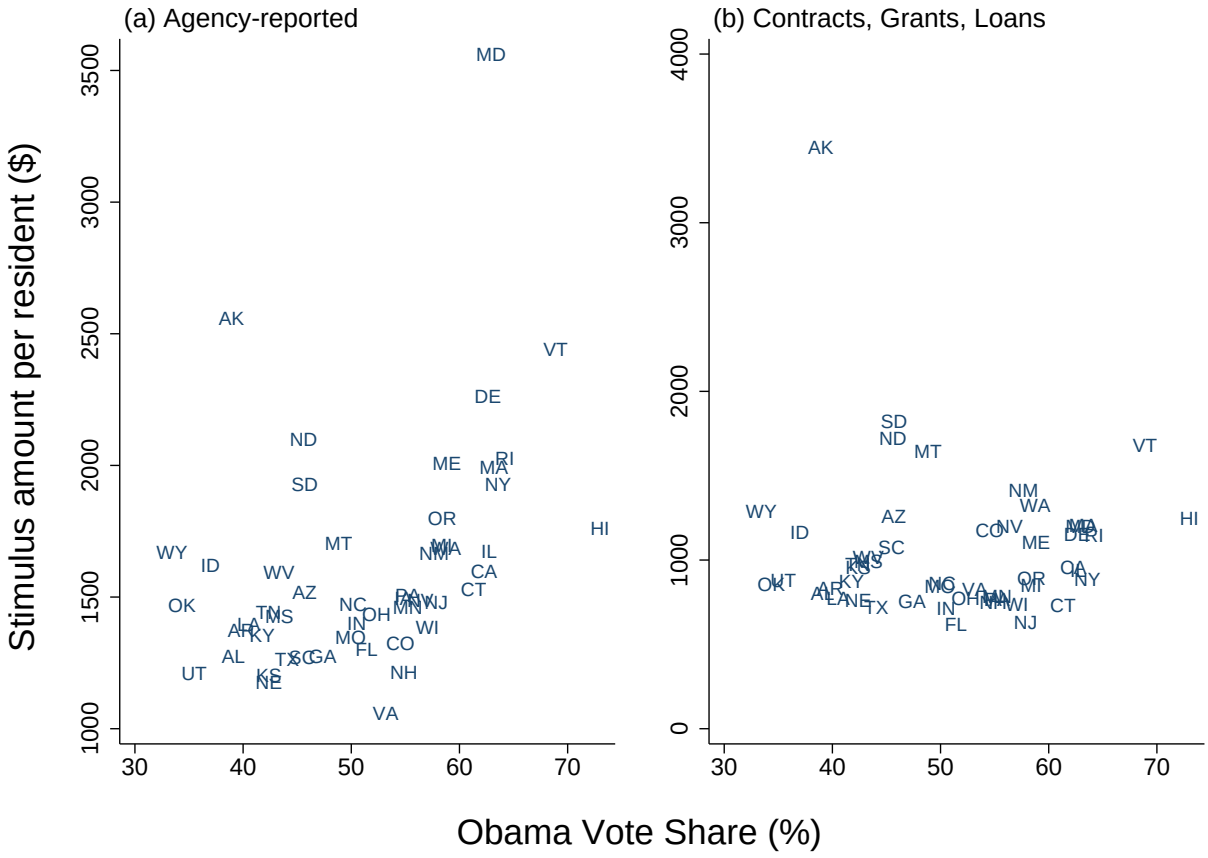
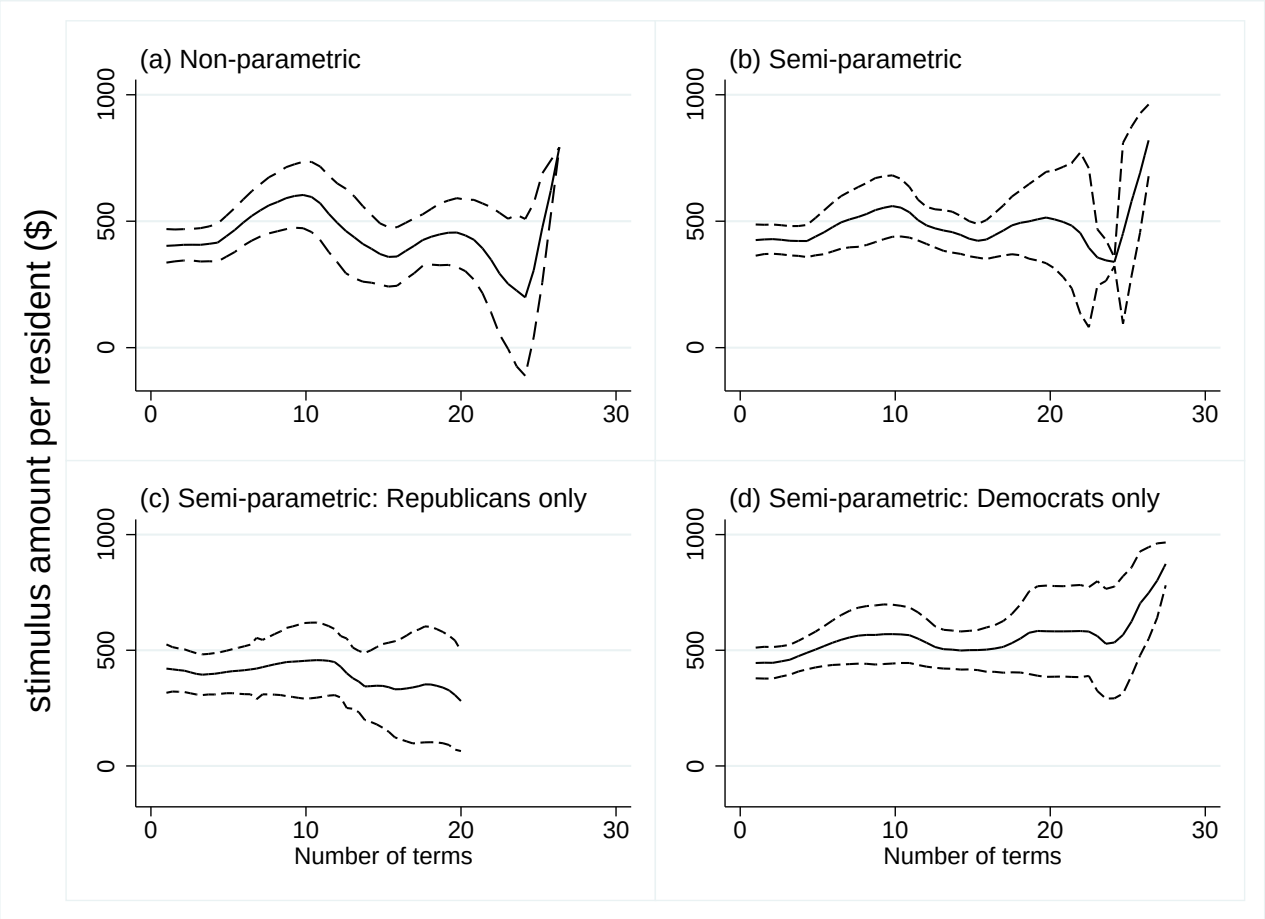
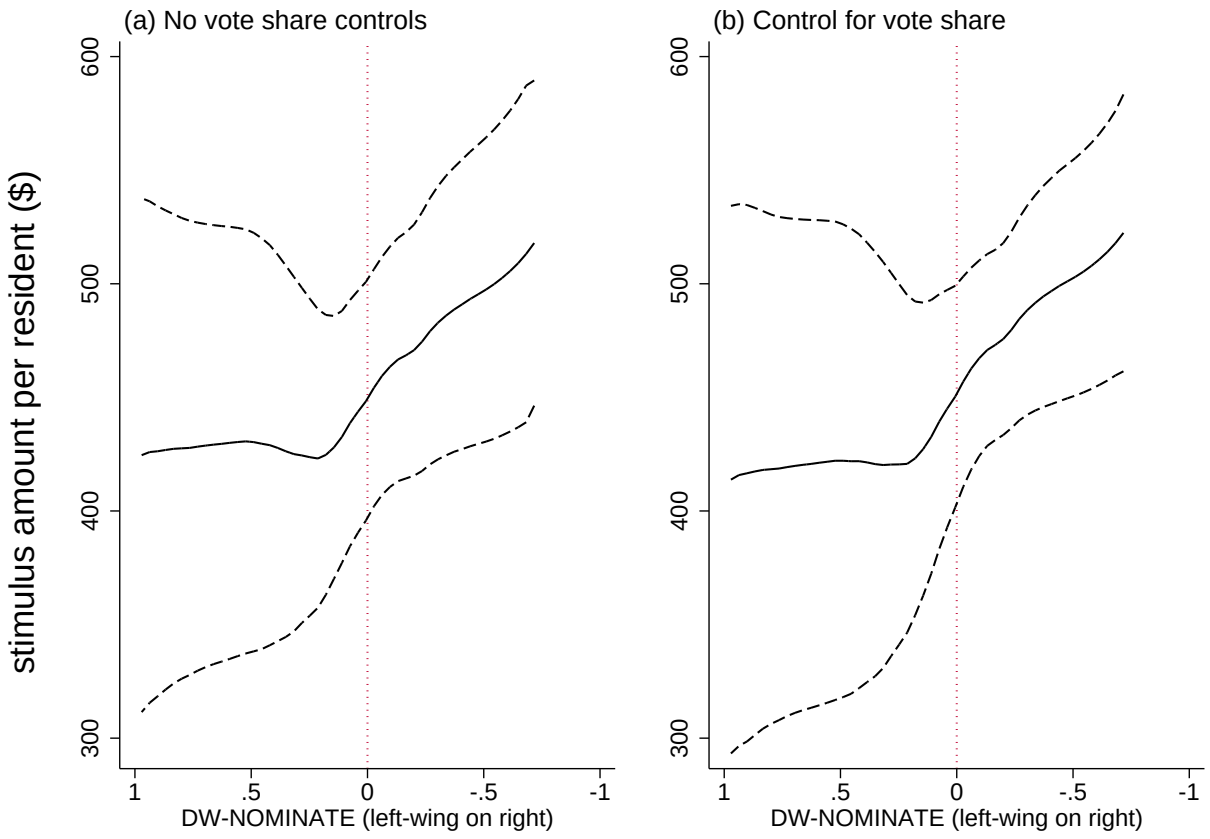


Figure A7. District-level: Amount per resident vs. Congressional tenure.



Notes: Semi-parametric controls include total employment, poverty rate, percent urban, land area, and road miles.

Figure A8: Semi-parametric regression of amount per resident on DW-NOMINATE score. Controlling for Democratic vote share.



Notes: Semi-parametric regression of stimulus amount per resident on DW-NOMINATE score for 334 congressional districts. The set of semi-parametric controls includes total employment, poverty rate, percent urban, land area, and road miles. Panel (b) adds the Democratic vote share as an additional parametric control. The horizontal axis is reversed so that left-wing representatives appear on the right (to correspond more closely to the previous figures).

Table A1. Top Funding Agencies, Includes Money Going to State Capitals

Rank w/ caps	Funding Agency	Rank no caps	Total Amount (million $) w/ caps	no caps	% in caps	Number of awards w/ caps	Median amount ($) w/ caps	Max award size ($) w/ caps
1	Office of Elementary and Secondary Education	9	64,678	1,922	97%	756	809,162	4,880,000,000
2	Department of Energy	1	38,352	20,952	45%	4316	649,350	1,360,000,000
3	Federal Highway Administration	2	27,892	19,196	31%	13879	569,170	261,000,000
4	Office of Special Education and Rehabilitative Services	44	13,585	309	98%	735	316,278	1,230,000,000
5	Department of Housing and Urban Development	5	10,869	5,831	46%	6411	317,515	326,000,000
6	National Institutes of Health	3	10,462	7,384	29%	16891	257,773	157,000,000
7	Federal Transit Administration	4	9,277	6,692	28%	974	2,000,000	423,000,000
8	Federal Railroad Administration	21	7,515	866	88%	86	8,315,000	2,550,000,000
9	Environmental Protection Agency	28	7,354	699	90%	903	612,000	433,000,000
10	Rural Utilities Service	6	6,741	4,811	29%	2148	1,295,500	83,100,000
11	Administration for Children and Families	14	5,015	1,568	69%	2992	261,244	220,000,000
12	U.S. Army Corps of Engineers - civil program financing only	7	4,638	3,456	25%	4583	132,302	62,300,000
13	Department of Labor	31	4,248	529	88%	707	946,034	489,000,000
14	Public Buildings Service	8	4,242	2,696	36%	1487	167,267	148,000,000
15	National Telecommunication and Information Administration	11	4,218	1,850	56%	294	5,660,544	155,000,000
16	Department of Justice	10	4,086	1,909	53%	4728	157,102	136,000,000
17	Department of Education	46	2,898	290	90%	1929	58,017	1,120,000,000
18	National Science Foundation	13	2,690	1,599	41%	4980	330,935	148,000,000
19	Health Resources and Services Administration	12	2,468	1,603	35%	3680	333,469	89,700,000
20	Department of Health and Human Services	16	2,357	1,125	52%	899	367,801	62,500,000
21	Federal Financing Bank	15	2,037	1,202	41%	3	692,000,000	1,200,000,000
22	Department of the Army	20	1,943	904	53%	1973	359,613	32,700,000
23	Department of the Air Force	23	1,541	806	48%	1708	314,774	51,700,000
24	Rural Housing Service	19	1,538	934	39%	1753	95,000	54,000,000
25	Department of Defense (except military departments)	26	1,529	753	51%	283	680,144	531,000,000

Table A2. Top Funding Agencies, Excludes Money Going to State Capitals

Rank no caps	Funding Agency	Rank w/ caps	Total Amount (million $) no caps	w/ caps	% in caps	Number of awards no caps	Median amount ($) no caps	Max award size ($) no caps
1	Department of Energy	2	20,952	38,352	45%	2668	675,381	1,190,000,000
2	Federal Highway Administration	3	19,196	27,892	31%	9057	595,605	261,000,000
3	National Institutes of Health	6	7,384	10,462	29%	12061	253,194	157,000,000
4	Federal Transit Administration	7	6,692	9,277	28%	681	1,770,192	423,000,000
5	Department of Housing and Urban Development	5	5,831	10,869	46%	4753	297,457	144,000,000
6	Rural Utilities Service	10	4,811	6,741	29%	1441	1,402,000	83,100,000
7	U.S. Army Corps of Engineers - civil program financing only	12	3,456	4,638	25%	3156	156,606	62,300,000
8	Public Buildings Service	14	2,696	4,242	36%	967	190,648	127,000,000
9	Office of Elementary and Secondary Education	1	1,922	64,678	97%	183	287,500	720,000,000
10	Department of Justice	16	1,909	4,086	53%	3050	143,288	50,200,000
11	National Telecommunication and Information Administration	15	1,850	4,218	56%	125	6,162,554	155,000,000
12	Health Resources and Services Administration	19	1,603	2,468	35%	2471	341,595	12,000,000
13	National Science Foundation	18	1,599	2,690	41%	3191	328,505	18,500,000
14	Administration for Children and Families	11	1,568	5,015	69%	1895	269,838	27,700,000
15	Federal Financing Bank	21	1,202	2,037	41%	1	1,200,000,000	1,200,000,000
16	Department of Health and Human Services	20	1,125	2,357	52%	359	457,844	62,500,000
17	Department of Veterans Affairs	26	1,000	1,501	33%	1275	280,417	29,600,000
18	National Aeronautics and Space Administration	30	999	1,104	10%	375	500,000	166,000,000
19	Rural Housing Service	24	934	1,538	39%	1305	89,081	44,700,000
20	Department of the Army	22	904	1,943	53%	907	382,000	32,700,000
21	Federal Railroad Administration	8	866	7,515	88%	26	5,002,500	400,000,000
22	Federal Aviation Administration	27	829	1,287	36%	267	1,811,658	30,300,000
23	Department of the Air Force	23	806	1,541	48%	960	285,235	23,700,000
24	Department of the Navy	28	801	1,261	36%	255	1,513,770	64,800,000
25	Office of Science	32	762	961	21%	287	600,000	65,000,000

Table A3. District-Level Stimulus Amount: Other Controls

	Per Resident				Per Worker			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Percent Poverty	16.14***				42.03***			
	(5.911)				(11.52)			
Percent Urban		1.562				-2.963		
		(1.015)				(2.594)		
Land Area			52.88**				257.3**	
			(25.24)				(99.41)	
Total Highway miles				0.00556				0.154**
				(0.0257)				(0.0677)
Constant	249.1***	344.3***	440.6***	464.9***	528.4***	1338.1***	962.4***	983.6***
	(74.59)	(77.96)	(31.69)	(41.63)	(143.6)	(211.2)	(69.30)	(103.4)
Observations	334	334	334	334	334	334	334	334
R^2	0.030	0.003	0.010	0.000	0.035	0.002	0.040	0.010

Notes: The table shows coefficients on the "additional controls" that were included in several of the regressions discussed in the paper. The dependent variable is district-level stimulus amount per resident or stimulus amount per worker received as contracts, grants and loans. Robust standard errors in parentheses, adjusted for clustering at the state level. * p < 0.10, ** p < 0.05, *** p < 0.01.

Table A4: District-Level Regressions: Stimulus Amount vs. Party Elites (House of Representatives)

	Per Resident		Per Worker		Per Resident		Per Worker	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Committee chair (D)	-80.32	-124.7**	-92.50	-302.7**				
	(70.38)	(57.31)	(173.0)	(148.3)				
Ranking member (R)	259.7	399.3*	1117.4	1222.3*				
	(204.2)	(204.8)	(739.1)	(682.1)				
Pelosi	1489.0***	585.9***	1315.9***	656.2				
	(31.30)	(168.8)	(68.81)	(398.1)				
Boehner	-335.4***	-150.7**	-753.5***	-424.9*				
	(31.30)	(63.12)	(68.81)	(213.8)				
Democratic leader					0.155	-84.14	-18.35	-240.9
					(76.64)	(65.37)	(155.8)	(149.4)
Republican leader					-99.93*	37.55	-104.3	11.39
					(53.21)	(43.75)	(91.83)	(113.4)
McKeon, Hastings					3318.9***	3284.6***	11318.9***	11033.3***
					(67.25)	(78.50)	(2110.4)	(2142.5)
Observations	332	332	332	332	334	334	334	334
R^2	0.037	0.307	0.044	0.146	0.212	0.492	0.440	0.511
Vote share controls		X		X		X		X
Additional controls		X		X		X		X

Notes: The dependent variable is district-level stimulus amount per resident or stimulus amount per worker received as contracts, grants and loans for 334 districts not in state capitals. Democratic leaders include committee chairs as well as the Speaker of the House (Pelosi); Republican leaders include ranking minority members of committees as well as the Minority Leader (Boehner); the Majority Leader and both Whips represented districts in state capitals, and so are not included in the sample. "Additional controls" include employment, poverty rate, percent urban, land area, and road miles. Robust standard errors in parentheses, adjusted for clustering at the state level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A5. District-Level Small Sample Committee Tests: House of Representatives

	Compare district of 111th committee leaders to districts represented by committee leaders in...		
	110th Congress	112th Congress	110th or 112th
Democrats			
No. of cases	3	8	11
No. of times 111th leader receives more	1	4	5
Percentage	0.33	0.50	0.45
p-value:			
1-sided test	0.50	0.69	0.50
2-sided test	1.00	1.00	1.00
Republicans			
No. of cases	6	4	10
No. of times 111th leader receives more	5	2	7
Percentage	0.83	0.50	0.70
p-value:			
1-sided test	0.04	0.76	0.09
2-sided test	0.08	1.00	0.18

Notes: This table shows the results of small sample tests ("Fisher exact tests") for whether committee leaders receive more stimulus funds. "Leaders" here includes both chairpersons and ranking members. We compare the districts whose representatives are leaders during the 111th Congress (which passed the ARRA) to districts whose representatives are leaders *of the same committee* during the previous or following Congress. This test only works for those committees with a change in leadership.

Table A6: State-Level Regressions: Stimulus Amount vs. Party Elites (Senate)

	(1) Agency	(2) Agency	(3) CGL	(4) CGL
Democratic leader	21.21	-27.20	-52.69	-156.3
	(154.8)	(112.3)	(86.42)	(97.84)
Republican leader	-109.9	-146.7	-146.5*	-110.7
	(166.6)	(113.2)	(80.28)	(89.86)
Observations	50	50	50	37
R^2	0.018	0.386	0.658	0.336
Includes state capitals	X	X	X	
Additional controls		X	X	X

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: The dependent variable is stimulus amount per state resident, using the agency-reported data from Recovery.gov for columns (1) and (2), and the contracts, grants and loans data for columns (3) and (4). Column (4) omits the funds allocated to state capital districts. Democratic leader is an indicator for whether a senator from that state chairs a committee or holds a Senate leadership position; Republican leader is an indicator for whether a senator from that state is the ranking Republican member of a committee or holds a Senate leadership position. Robust standard errors in parentheses. * p < 0.10, ** p < 0.05, *** p < 0.01.

Table A7: District-Level Regressions: Voting Against Party and Party of Governor

	Per Resident							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Democratic Governor	52.96		52.56	56.01	88.46		85.92	88.68
	(75.73)		(75.75)	(77.40)	(71.85)		(72.10)	(74.00)
Democrat voting against		-87.65	-84.13	10.58		-143.9**	-121.0*	-61.92
		(68.22)	(73.95)	(69.28)		(57.75)	(70.96)	(102.1)
(Dem Vote No)*(Dem Gov)				-189.0				-114.6
				(129.8)				(104.2)
Observations	334	334	334	334	334	334	334	334
R^2	0.002	0.000	0.003	0.003	0.282	0.278	0.283	0.283
Vote share controls					X	X	X	X
Additional controls					X	X	X	X

Notes: Dependent variable is district-level stimulus amount per resident. "Democrat voting against" include 5 Democrats who voted against the bill and 1 who voted present. Vote share controls indicate the 9 vote share dummies shown in Table 5. "Additional controls" include employment, poverty rate, percent urban, land area, and road miles. Robust standard errors in parentheses, adjusted for clustering at the state level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.