Stimulating the Vote: ARRA Road Spending and Vote Share

Emiliano Huet Vaughn*

Abstract

This paper estimates the impact of public good spending on voting behavior in the United States, using a quasi-experimental design and the distribution of American Recovery and Reinvestment Act (ARRA) road improvement projects across the state of New Jersey. I find an approximate 1.5 percentage point increase in the presidential vote share for the Democratic Party - largely responsible for ARRA's passage and widely perceived to be the more "tax-and-spend" friendly party - in areas close to highway and bridge improvement expenditures. I find no evidence of an effect on turnout. Results are consistent with two alternative mechanisms: one, a salience mechanism whereby spending and associated "funded-by" signage affect voter underlying political preferences; the other, a possible political multiplier effect through which stimulus spending improves local economic outcomes, making voters more willing to support incumbents. I present evidence at odds with the later explanation.

JEL Classification: D72, H41, H54
Keywords: public goods, voting, infrastructure, ARRA

*Department of Economics, Middlebury College, Warner Hall, Middlebury, VT 05753. (email: ehuetvaughn@middlebury.edu). This project greatly benefited from the research assistance of Nathan Weil, Tom Yu, and, especially, Leo Zabrocki. Share if this is a better love story than *Twilight.*

1 Introduction

This paper analyzes the effect of public good spending under the American Recovery and Reinvestment Act (ARRA) on voting behavior. I exploit a difference-in-difference design making use of the onset of ARRA and geographic variation in proximity to ARRA-created infrastructure projects.

The ARRA was passed by Congress and signed into law by President Obama in February of 2009 as a countercyclical stimulus measure in the midst of the Great Recession. Of the nearly $800 billion allocated, $105 billion was earmarked specifically for infrastructure investments, including $27.5 billion directed to highway and bridge improvement projects (Congressional Budget Office, 2016).

The act was particularly associated with the Democratic Party, being pushed as the chief budgeting priority of the nascent Obama administration, and, passed in the House of Representatives without a single Republican vote and in the Senate without a single Democrat voting against it. Such partisan split on a key expenditure bill only served to reinforce broadly-shared perceptions of the Democratic Party as the "tax and spend" party and the Republicans as the party preferring low tax rates and low levels of public expenditures (Pew Research Center, 2015).

Such associations invite the question of whether voters who most directly benefit from ARRA public goods expenditures are more likely to vote for the political party seen as responsible for them. I investigate this question here in the context of ARRA road and bridge projects in the state of New Jersey, finding that, indeed, Democratic presidential vote share rises significantly in municipalities near such projects.

Related questions have been explored by economists and political scientists in other contexts. Much of the existing work tests the effect of government transfers, specifically conditional cash transfer programs, rather than public goods spending, and, typically in developing countries. For instance, Manacorda et

al. (2011), using a regression discontinuity design, find that beneficiaries of an anti-poverty cash transfer program in Uruguay are more likely to report that they favor the incumbent. This positive relationship has since been confirmed with other conditional cash transfer programs in several other developing Latin American countries (e.g. Baez et al., 2012, in Colombia; Zucco, 2013, in Brazil; Galiani et al., 2016, in Honduras) and in Romania (Pop-Eleches and Pop-Eleches, 2012) as well (in contrast, see Blattman et al., 2014, for a null result from a Ugandan aid program).

In the case of public good spending, traditional pocketbook voting considerations (Kramer, 1971) are perhaps less pronounced than when transfers are more direct as in the case of the conditional cash transfer programs. In fact, Linos (2013), studying Honduran government programs, finds that conditional cash transfers positively affect incumbent mayor votes while public goods expenditures do not. Drazen and Eslava, (2010), on the other hand, document evidence from Colombia of a positive association between incumbent vote share and infrastructure spending.

Focusing on the United States, I present here novel findings on the impact of public good spending on vote shares using a simple quasi-experimental design. I estimate, via difference-in-difference specifications, an approximate 1.5 percentage point increase in the presidential vote share for the Democratic Party attributable to being near (within approximately 5 kilometers of) the origin of an ARRA road project. The identifying assumption required for the present research design is not random assignment of ARRA projects, but, rather that those areas near and far from ARRA road projects would have trended similarly in terms of voting outcomes in the absence of the ARRA projects. This assumption of "common trends" is supported by the pre-ARRA Democratic presidential vote share trends. Additionally, I show that the effect on presidential elections appears to be primarily a result of voters crossing party lines, or vote switching, rather than the result of additional voters turning out to the polls.

The finding is consistent with two alternative mechanisms. The first proposed

mechanism is inspired by the salience literature in public finance (Chetty et al., 2009; Goldin and Homonoff, 2013) and a novel feature of the ARRA-funded road projects. By federal regulations, all projects were required to include signage indicating that government tax dollars funded the road improvements. This regular reminder of the origin of a generally valued public good served to make the benefits of taxation more salient than otherwise. This may have caused voters in the surrounding areas, who presumably were exposed to the public good and the signs more frequently, to update their political preferences to a greater degree in support of a more liberal point on the tax-and-spend continuum, benefiting the Democratic Party.

Another possible mechanism explaining the results, what I term a local political multiplier effect, has little to do with underlying political preferences over the level of taxation and spending. If public good spending in fact stimulates the local economy (as designed) in the area of the road project, then this should make voters in these areas more likely to support the incumbent (in this case a Democrat). Indeed, existing work (Feyrer and Sacerdote, 2011; Chodorow-Reich et al., 2012; Wilson, 2012) has found evidence of significant fiscal multiplier effects from ARRA spending, and, Bagues and Esteve-Volart (2016) demonstrate that politicians receive more votes when economic conditions are good, using exogenous assignment of good economic conditions via regional lottery winners for identification. In the discussion of the results, I present evidence testing the implications of this political multiplier channel, finding little support for it, suggesting, by extension, that the salience channel may be responsible for the results (with associated implications for future policy design).

In the work that follows, I first present further background information regarding the ARRA program nationally and in the state of New Jersey and I then discuss data construction. I go on to present the basic results and examine threats to internal validity, before providing evidence relating to the mechanism at play. Finally, I conclude with a brief discussion of the implications of this work for future research and policy design.

2 Background and Data

2.1 ARRA Program Details

Though the ARRA constituted a federal expenditure program, monies for highway and bridge projects were transferred by the federal government to state Departments of Transportation (DOT) to allocate. The federal government encouraged states to quickly spend the funds, keeping in line with the goal of injecting emergency stimulus spending during the recession.

In New Jersey, the first bids on ARRA-funded road projects were made in April of 2009, two months after ARRA passage, while the last project issued took final bids in late 2010. Contracts were usually awarded a month or two after the bid date and construction began shortly thereafter, with construction on virtually all projects completed by the time the 2012 presidential election was held. The funded highway and bridge improvement projects consisted of a variety of work tasks including roadway reconstruction, resurfacing, pavement milling, bridge deck replacement and patching, and, safety improvements. State DOT civil servants determined the projects that were to receive funding on the basis of structural need and level of disrepair, congestion and safety concerns, and, per federal guidance, estimates of the number of construction jobs generated.

A unique feature of the ARRA-funded road projects is the aforementioned requirement that all projects include signage indicating the government funding. Per guidelines issued by President Obama, the NJ DOT directed that "all projects funded by the American Recovery and Reinvestment Act (ARRA) will bear a recovery emblem to make it easier for Americans to see which projects are funded by the ARRA. To meet this commitment, designers are to include the ARRA signs on all projects funded by the ARRA including projects under construction" (Figure 1 of online Appendix). These signs were standardized and placed at the endpoints of the road construction projects during construction, and, they remained after projects were completed. Figure 1 presents the sign template, and, Figure 2 of the online Appendix provides an example of

one of these "funded by" signs at a roadside.

In total, New Jersey spent $570 million on ARRA bridge and road improvement projects, with an average per successful bid cost of about $16 million. This represented one of the largest and most dramatic new federal investments in road infrastructure in decades in New Jersey, nearly equivalent in size to the total state expenditures by DOT for all awarded construction bids in fiscal year 2006, for example (totaling $638 million dollars).

2.2 Data Construction

I identify ARRA-funded road projects and their location using the record of bid tabulations obtained from the New Jersey DOT (New Jersey Department of Transportation, 2016). For each project there is record of the project cost for the winning bid and details about the project locations, typically in the form of the name of the road or bridge under construction, the traversed county and municipality, and, mileage markers or landmarks (e.g. specific overpasses or exits) indicating the stop and start of the construction along the road or bridge. This information is used to identify specific latitude and longitude coordinates for these endpoints (where the "funded by" signs are placed) using Google Maps. These coordinates are then geo-plotted using QGIS mapping software. Figure 2 presents a map of these geo-plotted points inside the New Jersey municipal boundaries. As several approved ARRA construction bids contained work in discrete sites, the total number of unique geographic coordinates with ARRA road construction endpoints is 73. For each municipality, the distance to the public good is measured via QGIS as the distance between the municipality centroid and the closest "funded by" sign at an ARRA road project endpoint.

New Jersey presidential general election voting records are obtained from three sources. For the 2008 and 2012 elections, I use Dave Leip's Atlas of U.S. presidential Elections (Leip, 2016). 2004 election data comes from the New Jersey Department of State's election archive (New Jersey, 2016). 2000 election data comes from the Center for Congressional and presidential Studies at American

University's School of Public Affairs (Center for Congressional and presidential Studies, 2016). The data is available at the municipality level and I merge across years using each municipality's FIPS ID. For each year, the merged data contains each municipality's total votes in the presidential election, total votes for the Democratic Party candidate, total votes for the Republican Party candidate, and, total number of registered voters. With 565 municipalities followed over 4 election cycles, the panel, thus, contains 2,260 observations.

Municipality-level demographic data is collected from the American Community Survey (ACS) and the Decennial Census (United States Census Bureau, 2016a, 2016b). For 2008 and 2012, ACS 5-year estimates are used for the the population count, the share of African American population, the share of white population, the share of Hispanic population, and the unemployment rate.[1] ACS data does not exist for 2004 or 2000, but, for 2000, at least, there is largely comparable municipality-level demographic data in the 2000 Census which I use for the above demographic variables.[2]

Table 1 reports summary statistics. Column 1 presents summary statistics for the full sample, and, Column 2 and Column 3 present summary statistics for the municipalities considered treated and control municipalities, respectively, using the binary assignment to treatment described in the next section. There are some noticeable differences between treatment control municipalities, notably in the percentage of the population that is Hispanic, indicating the importance of controlling for background demographic characteristics in the analysis that follows.

[1]For 2012, the 5-year estimate centered at 2012 is used. As there is no 5-year estimate centered at 2008 (and since the ACS 1-year and 3-year estimates do not contain estimates for the vast majority of New jersey municipalities in any year), I use the ACS' 2009 5-year estimate for 2008.

[2]The Census Bureau reports some discrepancies between the data collection procedures used to obtain some of the demographic variables (e.g. in the estimation of employment numbers) in the the 2000 Census Long Form and the the ACS, and, consequently, in the results that follow I separately report regression results that include only ACS demographic data as well as results using both ACS and 2000 Census data (difference-in-difference estimates change very little). To my knowledge, there is no data for 2004 for my demographic variables collected at the municipality level.

3 Results

In the analysis of the results, I use both a continuous measure of municipality distance from the closest "funded by" sign at an ARRA road improvement project, and, alternatively, a binary assignment whereby municipalities are sorted into "treated" and "untreated" groups on the basis of whether the closest sign to the municipality geographic center is within a short distance (0.05 decimal degrees, or, approximately 5 kilometers). As is shown below, results are consistent across these alternative ways of categorizing municipalities as near to or far from the ARRA project termini, indicating a general result not dependent on the choice of 0.05 decimal degrees as a cutoff point in the binary assignment of treatment. For exposition purposes, however, I begin by presenting the results of the binary assignment into near and far municipalities.

3.1 Baseline Results

Figure 3 presents the resulting assignment of municipalities into treated (copper-colored) and untreated (teal-colored) groups on the basis of the above binary assignment. Using this assignment rule, 166 municipalities are counted as near to, or treated by, an ARRA public good project, and, 399 municipalities are counted as far from one, or untreated.

Figure 4 plots the time series of the Democratic share of the presidential vote in 21st century elections for New Jersey municipalities (scaled from 0 to 100), with averages for near and far municipalities plotted separately. The figure shows that prior to the ARRA road construction projects (prior to 2009) the time series of near municipalities' Democratic vote share closely tracked the times series of far municipalities' Democratic vote share. This suggests that the two time series would have continued to move in parallel in the absence of ARRA construction, giving support to the "common trends" assumption underlying the difference-in-difference identification pursued at present. The figure also shows an empirical departure from this parallel trend following the ARRA public goods infrastructure investments, with those municipalities near

an ARRA road sign increasing their Democratic vote share relative to others in 2012. Figure 5 presents the same data represented as annual differences between near and far municipalities' average Democratic presidential vote share.

This graphical evidence complements the results, presented in Table 2, of the following difference-in-differences (DD) regression:

$$DemVoteShare_{it} = \alpha_1 TREAT_i + \alpha_2 (TREAT_i \times POST_t) + \mathbf{YEAR_t}\psi + \mathbf{X_{it}}\beta \quad (1)$$

where $DemVoteShare_{it}$ is 100 times the fraction of total votes going to the Democratic presidential candidate in municipality i in year t, $TREAT_i$ denotes an indicator for whether municipality i eventually had an ARRA project sign within 0.05 decimal degrees of it's geographic center, $POST_t$ denotes an indicator for year t being 2012, $\mathbf{YEAR_t}$ denotes a vector of additional year fixed effects, and, $\mathbf{X_{it}}$ denotes a possibly empty vector of county-specific time trend (county-by-year) dummies or demographic controls. The coefficient α_2 represents the difference-in-difference estimator, the statistic of interest, showing the mean effect on the Democratic presidential vote share (in percentage points) of having an ARRA road project completed nearby (i.e. with a road project termini within 0.05 decimal degrees, or, approximately 5 kilometers, of a municipality center). Standard errors clustered by municipality are reported below each estimate.

Column 1 of Table 2 reports that having an ARRA road project constructed nearby is estimated to increase Democratic vote share by 1.654 percentage points. Column 2 includes county-specific time trends and reports a similar coefficient (1.791 percentage points), and, column 3 includes demographic controls for 2008 and 2012 data, finding, again, a similar coefficient (1.598 percentage points). All estimates are significant at the 1% level of significance. Column 1 of online Appendix Table 1 expands the demographic control data to 2000 (presented separately because of discrepancies in data construction

across years, see footnote 2),[3] and, column 2 of online Appendix Table 1 includes both the demographic controls and county-specific time trends. Estimates change very little (with the difference-in-difference point estimate close to 1.25) and remain significant at the 1% level of significance. Online Appendix Table 2 replicates Table 2 with an alternative two-way clustering of standard errors by county and municipal form of government (i.e. boroughs, townships, cities, and, towns/villages) and results for columns 1-3 remain significant at the 1% level of significance. Results are also robust to the interpolation of demographic controls for the missing 2004 data and then running the equivalent of equation (1)[4] for all years with the above demographic controls and county-specific time trends (unreported).

Equation (1) can be modified to allow a continuous measure of treatment intensity using the following regression

$$DemVoteShare_{it} = \alpha_1 DISTANCE_i + \alpha_2 (DISTANCE_i \times POST_t) + \mathbf{YEAR_t}\psi + \mathbf{X_i}\beta \quad (2)$$

where $DISTANCE_i$ denotes the distance from the center of municipality i to the site of the closest eventual ARRA project sign, measured in decimal degrees, and other variables are as before. Columns 4-6 in Table 2 replicate the regressions in Columns 1-3 using this continuous measure of treatment assignment instead. The associated difference-in-difference estimates are consistent across specification (1) and (2), with a greater distance from an ARRA road project being associated with lower Democratic vote share. In column 4, for example, the difference-in-difference estimate is -7.403, significant at the 1% level, indicating that a change in $DISTANCE_i$ of one standard deviation is associated with a 0.62 percentage point drop in Democratic presidential vote share. In columns 5 and 6, the same change is associated with a respective

[3] 2004 demographic data at the municipality level is not available.

[4] Or the equivalent of equation (2), described below.

1.01 and 0.52 percentage point drop in Democratic vote share, estimates again being significant at the 1% level. As a robustness check, columns 3-4 in online Appendix Table 1 reproduce columns 1-2 in the same table using the continuous measure of treatment assignment instead, and, columns 4-6 of online Appendix Table 2 replicate columns 4-6 in Table 2 with the aforementioned alternative two-way cluster. In all cases, results remain highly significant and negative, with point estimates changing very little. Moreover, the general results reported from both specifications (1) and (2) also hold if introducing additional control variables for population size and form of municipal government, and, when interpolating the missing 2004 demographic variables to allow use of all years in the regressions that include demographic controls (unreported).

Table 3 presents difference-in-difference estimates focusing only on the two-party (Democratic and Republican) presidential vote share of Democrats, replicating the specifications in Table 2 with this alternative outcome. Markedly smaller effects using the two-party vote share measure would indicate much of the above documented movement to the Democratic candidate came about from substitution from third party votes (which on average represented about 2.4 percent of all ballots cast in my sample period) rather than from Republican votes. However, across all columns in Table 3 there is very little change from Table 2 in the coefficient of interest or its significance. This motivates the focus on the original measure of Democratic vote share presented in Table 2 (Democratic votes as a fraction of total votes casts) throughout the paper.

Table 4 explores the additional outcome of voter turnout, finding little evidence that ARRA spending affects it. All columns take the total presidential votes cast in a municipality divided by municipality population (using 2012 population for scaling) as the outcome. The specifications in columns 1-3 and 4-6 correspond to equations (1) and (2), respectively, with this alternative outcome replacing $DemVoteShare$. The coefficients on the statistics of interest are close to 0 and insignificant in all cases. In column 1 of Table 4, for instance, the confidence intervals rule out positive or negative effects of 1

percentage point. The insignificant result is unchanged if instead scaling votes by contemporaneous population which requires exclusion of a larger number (23) of municipality-year observations because of measurement inconsistencies.[5] Taken together, the results in Tables 1-3 suggest that vote switching, rather than mobilization of additional voters, is the primary means by which public goods spending affects Democratic vote share.

3.2 Internal Validity

A natural test for the internal validity of the above result is to perform a placebo test for an effect on Democratic vote share from an imagined shock to ARRA-treated municipalities in a year other than the year of ARRA occurrence. In Table 5, I present the results of such a test, with a placebo shock after the 2000 election. For easy comparison, columns 1 and 5 repeat columns 2 and 5 from Table 2, and, columns 3 and 7 repeat columns 2 and 5 from online Appendix Table 2 (which mirror Table 2 with an alternative two-way clustering). Columns 2, 4, 6, and 8 restrict the sample to 2000-2008 (to isolate a potential placebo effect distinct from the ARRA construction) and replace the post-2008 indicator in equation (1) and (3) with a post-2000 indicator equal to 1 if the observation is from after 2000 (these placebo specifications otherwise replicate columns 1, 3, 5, and 7 of the table, respectively). All columns include county-specific time trends. The results uniformly demonstrate small and insignificant effects of the placebo shock to ARRA-treated municipalities.

Such a result is not wholly unexpected in light of Figure 4. While the parallel pre-trends in it invite confidence that those areas near and far from ARRA

[5]In 19 municipalities there are one or more years in which votes cast exceed the year's population (resulting in 23 municipality-year observations). In some cases this may be due to the different timing of measurement of each of these variables (votes cast being measured on a November election day and population estimates made on the basis of samples taken throughout the year), but, as all but three of these instances occur in 2004, where population data comes from intercensal estimates (rather than the more intensively collected sources used for other years, namely, the Decennial Census itself or the American Community Survey) it is likely that many of these exceptions are the result of population interpolations. In any event, even when scaling by contemporaneous population the insignificant result on turnout remains whether using data from all years or excluding 2004 data.

road projects would have trended similarly in terms of voting outcomes in the absence of the ARRA projects, the identifying assumption could be violated by any other (non-ARRA) event between 2008 and 2012 that primarily affected the treatment group and not the control and had an independent impact on voting decisions. Events increasing the probability of voting for the Democrat pose the greatest threat to identification of the main result.

A key concern of this sort is the existence of a "Hurricane Sandy" effect. Hurricane Sandy was the second costliest hurricane in United States history, hitting much of the eastern seaboard just a little over a week before the 2012 election. It caused an estimated $36 billion dollars of damages to New Jersey, one of the states hardest hit by the hurricane, in addition to more than thirty deaths there (CNN, 2012). Many commentators spoke of the hurricane as providing an "October surprise" with the aftermath potentially benefiting President Obama in the event he handled the disaster capably as the chief executive (CBS News, 2012). The concern in terms of identification of an independent ARRA effect is that a natural disaster may incline voters to desire the protective hand of government more than otherwise, thus, voting for the Democratic Party for reasons unrelated to the specifics of ARRA spending. If the distribution of the hurricane across the state also lined up with sites of ARRA road projects, then, this may bias the above estimates.

To determine if a "Hurricane Sandy" effect poses a threat to identification, I take data from the New Jersey Department of Community Affairs (2016) on the total number of homes and rental units damaged by the hurricane within each municipality and scale this count by 2012 population. Table 6 reports the results of regressions that modify equations (1) and (2) to include this measure of hurricane damage by municipality. Specifically, the specifications include additional terms $Damages_i$ and $(Damages_i \times Post_t)$ terms, where $Damages_i$ indicate the per capita destruction to the housing stock caused by the hurricane in municipality i and $(Damages_i \times Post_t)$ is the interaction of this term with a post-2008 indicator.

In column 1 of Table 6, the difference-in-difference estimator measuring an

effect of ARRA projects on Democratic vote share remains almost identical to it's value in Table 2. This is true across all other columns in Table 6 and the corresponding columns of Table 2, suggesting that results are robust even when controlling for the spatial distribution of Hurricane Sandy destruction, and, that the results survive this potential threat to identification. Interestingly, the coefficient indicating the Sandy effect is not consistently significant across specifications and, in fact, alternates signs (though is mostly negative). This largely null result is consistent with the opinions of polling experts Harry Enten and Nate Silver of the website www.fivethirtyeight.com, who argue, contrary to the "October surprise" view, that polls before and after Sandy in the hardest hit states showed no clear movement toward Obama after the hurricane (Enten, 2012; Silver, 2016).

3.3 Mechanism

As previously mentioned, there are at least two competing (but possibly co-existing) mechanisms which may explain the observed results. One involves the transformation, whether conscious or not, of political preferences over the appropriate level of taxation for the purpose of public expenditures. In this case, ARRA projects make salient (more so than otherwise) the rewards of a more expansive government by providing individuals exposure to improved infrastructure projects, a generally popular category of government spending (Gallup, 2013), and signage reminding them daily that their nice, new roads are made possible by their tax dollars. In effect, they drive home the benefits that taxation affords the population for a given background level of taxation, potentially predisposing voters to support tax-and-spend friendly parties more.

The other mechanism may have little to do with fundamental political preferences. If ARRA spending has the intended stimulatory effect on the economy, then it is reasonable to expect that the areas where ARRA road projects are placed will enjoy improved economic conditions compared to others (whether from the work force being more proximate to the site of the road project, or, from worker on-the-job expenditures, and, associated multiplier effects). If in-

cumbents are more likely to be re-elected when economic conditions are good, then the increased vote share in 2012 for the Democratic incumbent (Barrack Obama) in areas near ARRA road projects may thus be explained by such a political multiplier effect.

To simplify, imagine the probability of voting Democrat V is a function of three terms

$$V = f(TaxCost, Benefit, EconomicConditions)$$

where the $TaxCost$ represents the level of taxes the individual has to pay, with $\delta V/\delta TaxCost < 0$ indicating that, keeping the level of government benefits and economic conditions constant, increasing perceived tax rates will lower voter welfare and their chance of voting for the party advocating the higher tax rate (understood to be the Democratic Party in the current political context). $\delta V/\delta Benefit > 0$ indicates that, all other things equal, with an increase in the perceived $Benefit$ from taxation, voters are more likely to vote for the Democratic presidential candidate. Finally, $\delta V/\delta EconomicConditions$ represents the change in voter favor for an incumbent when economic conditions improve, with $\delta V/\delta EconomicConditions > 0$ in the case of the 2012 presidential election since the incumbent is a Democrat.

The ARRA program affects the voting decision in the following way

$$\frac{\delta V}{\delta ARRA} = (\frac{\delta V}{\delta Benefit})(\frac{\delta Benefit}{\delta ARRA}) + (\frac{\delta V}{\delta EconCond})(\frac{\delta EconCond}{\delta ARRA}) + C$$

where C is whatever affect there may be on perceived tax cost due to the ARRA program (it may be zero, to the extent individuals expect their previous year taxes to cover the costs of the spending, or negative, but, this is not material for the point at hand). For the ARRA program to increase the likelihood V of voting Democrat, in accordance with the results, either $(\frac{\delta V}{\delta EconCond})(\frac{\delta EconCond}{\delta ARRA})$

or $(\frac{\delta V}{\delta Benefit})(\frac{\delta Benefit}{\delta ARRA})$, or both, must be positive. The later term, if positive, is associated with the salience mechanism described, and, the former term, if positive, is associated with the political multiplier mechanism described. $\frac{\delta EconCond}{\delta ARRA}$ represents a pure "stimulus" effect and $\frac{\delta Benefit}{\delta ARRA}$ represents a pure "salience" effect.

Table 7 presents the results of regressions that seek to distinguish between the two competing mechanisms. If the political multiplier channel is pertinent in its own right there should be an increasing relationship between dollars spent on a particular road project and the Democratic vote share. This motivates equation (3)

$$\begin{aligned} DemVoteShare_{it} = \alpha_1 TREAT_i + \alpha_2(TREAT_i \times POST_t) + \\ \mu_1 AMOUNT_i + \mu_2(AMOUNT_i \times POST_t) + \\ \mathbf{YEAR_t}\psi + \mathbf{X_{it}}\beta \end{aligned} \tag{3}$$

which amends equation (1) with an additional $AMOUNT_i$ term denoting the dollars of total expenditure flowing to municipality i from the eventual ARRA project treating it (according to the binary assignment of treatment already described, with $AMOUNT_i$ being zero for untreated municipalities), and, the interaction term $(AMOUNT_i \times POST_t)$, with μ_2 an alternative difference-in-difference estimator representing the potential political multiplier channel.

The different results in Table 7 (and Table 3 of the online Appendix) come from versions of (3) using alternative ways of assigning the dollar amount per ARRA project to nearby municipalities. Uniformly, they indicate the dollar amount per project (independent from the existence of a project) has no statistically significant effect on Democratic vote share (i.e. the $(AMOUNT_i \times POST_t$ regressor is not significant at standard levels), casting doubt on the relevance of the political multiplier channel in this context.

Columns 1 and 2 of Table 7 (and of Table 3 of the online Appendix, which replicates Table 7 with the addition of county-specific time trends for each column)

report the difference-in-difference estimator μ_2 from equation (3) where each municipality i is assigned the full expenditure of the ARRA road project that treats it (and, again, 0 assigned to those municipalities not treated). In column 1, the specification is (3) without the $TREAT_i$ and $(TREAT_i \times POST_t)$ terms, while column 2 includes them. The general pattern of a significant α_2 term (consistent with the previous results) and small and insignificant μ_2 term is inconsistent with the political multiplier channel and lends weight to the salience mechanism. Columns 3 and 4 are identical to Columns 1 and 2 except that the full expenditure of each ARRA project, rather than assigned fully to each municipality i it treats, is divided evenly and then distributed to each municipality it treats. Columns 5 and 6 further refine the assignment of the dollar amount per ARRA project to nearby municipalities by distributing it to each municipality it treats in proportion to the municipality's share of total voters for all municipalities treated by the particular ARRA project. Columns 7 and 8 perform the same assignment of expenditure on the basis of total population, rather than total voters.

4 Conclusion

This work's main result of a significant and positive effect on Democratic presidential vote share attributable to ARRA investments in bridges and highways contributes to a growing literature studying the impact of government spending on political preferences. The work departs from existing work, much of which concentrates on developing countries and cash transfers, by studying electoral outcomes in the United States in the context of a large public goods expenditure program. The evidence presented suggests that the result is most likely explained by a salience mechanism, whereby "funded-by" signage accompanying the road expenditures serves to remind voters of the benefits afforded by their tax dollars, thus, changing their underlying political preferences. However, it should be noted that the possible political multiplier mechanism, whereby ARRA stimulus spending improves local economic outcomes, thus, making voters more willing to support incumbents, may be

expected to play a larger role in the event of a stimulus spending initiative larger than ARRA. In the present case, the median expenditure allocated to a treated municipality (via the expenditure assignment rule used in Columns 7 and 8 of Table 7) would only have been expected to generate around 56 jobs based on existing estimates (e.g. Wilson, 2012).

If indeed the salience mechanism described is primarily responsible for the increased Democratic vote share, this suggests governments wishing to win the support of voters should engage in efforts to more clearly label those expenditures that voters benefit from as "paid for by your tax dollars." This could include signage as in the present context or other frequently visible reminders that help to make the benefit side of taxation more obvious. Of course, with greater frequency of such salience efforts there may be a diminished salience for each marginal sign or similar effort, as a once novel reminder may become prosaic and ineffectual, as with marginal consumer advertising in saturated consumer advertising markets.

Of course, there are other natural limits to such a behavioral policy in public goods expenditures. In the ARRA context, the primary rationale given for inclusion of the signage was as part of a government transparency initiative aimed at informing citizens of how their tax dollars are put to use. If instead voters were to see the signage and associated expenditures as put forth in an attempt to instrumentally affect voting outcomes, there may well be a backlash. Relatedly, the spending analyzed in the current work comes from both a party on the liberal side of the tax and expenditure continuum and from a similarly liberal candidate in that party who uniquely played a leading role in bringing the spending into being. If a candidate were to promote similar infrastructure spending from within a party philosophically opposed to a large government budget, it is less clear how voters would interpret and credit the spending come election time.

References

Baez, Javier E., Adriana Camacho, Emily Conover, and Roman A. Zarate. 2012. "Conditional Cash Transfers, Political Participation and Voting Behavior," World Bank Working Paper Series 6215.

Bagues, Manuel and Esteve-Volart. 2016. "Politicians? Luck of the Draw: Evidence from the Spanish Christmas Lottery," *Journal of Political Economy*, 124(5): 1269-1294.

Blattman, Christopher, Mathilde Emeriau, and Nathan Fiala. 2014. "Does Foreign Aid Buy Domestic Votes? Experimental Evidence from a Ugandan Employment Program," Working Paper.

CBS News, *Hurricane Sandy: Election 2012's October Surprise?* (http://www.cbsnews.com/news/hurricane-sandy-election-2012s-october-surprise/, 2012).

Center for Congressional and Presidential Studies, *2000 Election Data Archive* (http://www.american.edu/spa/ccps/Data-Sets.cfm, 2016).

Chetty, Raj, Adam Looney, and Kory Kroft. 2009. "Salience and Taxation: Theory and Evidence," *American Economic Review*, 99(4):1145-1177.

Congressional Budget Office, *Estimated Impact of the American Recovery and Reinvestment Act on Employment and Economic Output from October 2011 Through December 2011* (http://www.cbo.gov/sites/default/files/cbofiles/attachments/02-22-ARRA.pdf, 2016)

CNN, *Hurricane Sandy Fast Facts* (http://edition.cnn.com/2013/07/13/world/americas/hurricane-sandy-fast-facts, 2012).

Chodorow-Reich, Gabriel, Laura Feiveson, Zachary Liscow, and William Gui Woolston. 2012. "Does State Fiscal Relief During Recessions Increase Employment? Evidence from the American Recovery and Reinvestment Act." *American Economic Journal: Economic Policy*, 4 (3): 118-145.

Drazen, Allan and Marcela Eslava. 2010. "Electoral Manipulation via Voter-

Friendly Spending: Theory and Evidence," *Journal of Development Economics*, 92(1):39-52.

Enten, Harrry. 2012. *Was It Hurricane Sandy That Won It for President Obama?* (https://www.theguardian.com/commentisfree/2012/dec/04/hurricane-sandy-won-president-obama).

Feyrer, James and Bruce Sacerdote. 2011. "Did the Stimulus Really Stimulate? Real Time Estimates of the Effects of the American Recovery and Reinvestment Act," NBER Working Paper No. 16759..

Galiani, Sebastian, Nadya Hajj, Pablo Ibarraran, Nandita Krishnaswamy, and Patrick J. McEwan. 2016. "Electoral Reciprocity in Programmatic Redistribution: Experimental Evidence," NBER Working Paper No. 22588.

Gallup, 2013. *Americans Widely Back Government Job Creation Proposals.*

Goldin, Jacob and Tatiana Homonoff. 2013. "Smoke Gets in Your Eyes: Cigarette Tax Salience and Regressivity," *American Economic Journal: Economic Policy*, 5(1): 302-336.

Kramer, Gerald H. 1971. "Short-Term Fluctuations in US Voting Behavior, 1896-1964." *American Political Science Review*, 65(1): 131-43.

Leip, David, *Dave Leip's Atlas of U.S. Presidential Elections* (http://www/uselectionatlas.org, 2016).

Linos, Elizabeth. 2013. "Do Conditional Cash TransferPrograms Shift Votes? Evidence from the Honduran PRAF," *Electoral Studies*, 32: 864-874.

Luttmer, Erzo F.P. and Monica Singhal. 2014. "Tax Morale," *Journal of Economic Perspectives*, 28(4): 149-168.

Manacorda, Marco, Edward Miguel, and Andrea Vigorito. 2011. "Government Transfers and Political Support," *American Economics Journal: Applied Economics*, 3(3): 1-28.

New Jersey Department of Community Affairs, *Total Number of Homes and*

Rental Units Damaged by Superstorm Sandy (http://www.njspotlight.com/stories/13/03/14/assessing-damage-from-superstorm-sandy/, 2016).

New Jersey Department of Transportation, *Division of Procurement Awarded Projects* (http://www.state.nj.us/transportation/business/procurement/ConstrServ/awards16.shtm, 2016).

New Jersey Department of State, *NJ Election Information and Results Archive* (http://www.njelections.org/election-information-archive-2004.html, 2016).

Pew Research Center, 2015. "Views of Political Parties and How They Manage Government."

Pop-Elches, Christian and Grigore Pop-Elches. 2012. "Targeted Government Spending and Political Preferences," *Quarterly Journal of Political Science*, 7(30): 285-320.

Silver, Nate. 2016. *How Much Do "October Surprises" Move the Polls?* (http://fivethirtyeight.com/features/how-much-do-october-surprises-move-the-polls/).

United States Census Bureau, *American Community Survey* (http://www.census.gov/programs-surveys/acs/, 2016a).

United States Census Bureau, *2000 Census* (http://www.census.gov/main/www/cen2000.html, 2016b).

Wilson, Daniel J. 2012. "Fiscal Spending Jobs Multipliers: Evidence from the 2009 American Recovery and Reinvestment Act," *American Economic Journal: Economic Policy*, 4 (3): 251-282.

Zucco Jr., Cesar. 2013. "When Pay Outs Pay Off: Conditional Cash-Transfers and Voting Behavior in Brazil 2002-2010," *American Journal of Political Science*, 47(3): 810-822.

Table 1: Summary Statistics

	(1) All municipalities	(2) Treated Municipalities	(3) Control Municipalities
Democratic Vote Share (percentage)	50.36	54.66	48.60
	(13.72)	(14.55)	(12.95)
Democratic Vote Share - Two Party (percentage)	51.51	55.75	49.75
	(13.73)	(14.66)	(12.93)
Turnout (as fraction of population)	0.4616	0.4394	0.4707
	(0.1094)	(0.1139)	(0.1061)
Distance from origin of nearest ARRA project (in decimal degrees)	0.1055	0.0310	0.1365
	(0.0840)	(0.0119)	(0.0816)
Fraction Assigned to Binary Treatment	0.2938	1	0
	(0.4556)	(0)	(0)
Unemployment Rate	5.997	6.159	5.930
	(3.645)	(3.654)	(3.640)
Percentage African American	7.400	8.592	6.904
	(12.34)	(14.17)	(11.45)
Percentage Hispanic	9.435	12.75	8.054
	(11.75)	(14.28)	(10.22)
Observations	2260	1596	662
	1695*	1197*	498*

Notes: Astericked observation counts refer to the observation numbers for unemployment rate, percentage African American, percentage Hispanic, as 2004 data is not available at the municipality level for these variables. Turnout summary statistics are based on the 2249 (1587 treated and 662 control) observations used in Table 5, which includes only those observations without obvious inconsistencies between turnout numbers and the population count data used for scaling (dropping those observations with turnout in excess of the 2012 population). Standard deviations are in parentheses.

Table 2: ARRA Public Good Spending and Voting

Dependent Variable: Democratic Vote Share (0 - 100)						
	Binary Treatment			Continuous Treatment		
	(1)	(2)	(3)	(4)	(5)	(6)
Treated x Post 2008	1.654***	1.791***	1.598***			
	(0.365)	(0.343)	(0.342)			
Distance x Post 2008				-7.403***	-12.01***	-6.137***
				(1.705)	(2.180)	(1.701)
County-Specific Time Trend		X			X	
Demographic Controls			X			X
Observations	2260	2260	1130	2260	2260	1130
adj. R^2	0.059	0.342	0.657	0.087	0.345	0.672

Notes: This table reports difference-in-differences estimates of the effect of ARRA road spending on presidential voting outcomes. All columns come from regressions of Democratic presidential vote share (from 0 to 100) on a measure of municipality proximity to the ARRA public good project, the interaction of this with an indicator for the year being after 2008, year fixed effects, and, possibly additional controls. Columns 1-3 present the coefficient on the interaction term using a binary measure of municipality proximity to the public good, whereby, municipalities are sorted into "treated" and "untreated" groups on the basis of whether the closest "ARRA-funded-by" sign (located at the terminus of each road construction project) is within a distance of 0.05 decimal degrees (approximately 5 kilometers) from the municipality geographic center. Columns 4-6 present the coefficient on the interaction term using a continuous measure of this distance (in decimal degrees). County-specific time trends include county-by-year interaction terms. Demographic controls include the share of African-American population, the share of Latino/Hispanic population, and the unemployment rate. Standard errors are clustered by municipality (565 clusters). * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 3: ARRA Public Good Spending and Voting - Two Party Vote Share

Dependent Variable: Two-Party Democratic Vote Share (0 - 100)						
	Binary Treatment			Continuous Treatment		
	(1)	(2)	(3)	(4)	(5)	(6)
Treated x Post 2008	1.778***	1.706***	1.602***			
	(0.354)	(0.338)	(0.343)			
Distance x Post 2008				-8.690***	-11.32***	-6.590***
				(1.632)	(2.151)	(1.698)
County-Specific Time Trend		X			X	
Demographic Controls			X			X
adj. R^2	0.054	0.324	0.651	0.081	0.328	0.665
N	2260	2260	1130	2260	2260	1130

Notes: This table reports equivalent difference-in-difference regressions as Table 2 but with the *two-party* Democratic vote share as the outcome. All columns come from regressions of two-party Democratic presidential vote share (from 0 to 100) on a measure of municipality proximity to the ARRA public good project, the interaction of this with an indicator for the year being after 2008, year fixed effects, and, possibly additional controls. Columns 1-3 present the coefficient on the interaction term using a binary measure of municipality proximity to the public good, whereby, municipalities are sorted into "treated" and "untreated" groups on the basis of whether the closest "ARRA-funded-by" sign (located at the terminus of each road construction project) is within a distance of 0.05 decimal degrees (approximately 5 kilometers) from the municipality geographic center. Columns 4-6 present the coefficient on the interaction term using a continuous measure of this distance (in decimal degrees). County-specific time trends include county-by-year interaction terms. Demographic controls include the share of African-American population, the share of Latino/Hispanic population, and the unemployment rate. Standard errors are clustered by municipality (565 clusters). * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 4: ARRA Public Good Spending and Voter Turnout

Dependent Variable: Presidential Voter Turnout (% of pop.)						
	Binary Treatment			Continuous Treatment		
	(1)	(2)	(3)	(4)	(5)	(6)
Treated x Post 2008	0.000418	-0.00248	0.00126			
	(0.00456)	(0.00443)	(0.00489)			
Distance x Post 2008				-0.0336	-0.00589	-0.00574
				(0.0230)	(0.0322)	(0.0227)
County-Specific Time Trend		X			X	
Demographic Controls			X			X
Observations	2249	2249	1126	2249	2249	1126
adj. R^2	0.088	0.228	0.486	0.084	0.228	0.486

Notes: This table reports difference-in-differences estimates of the effect of ARRA road spending on voter turnout. All columns come from regressions of voter turnout (total presidential votes cast in a municipality divided by municipality population) on a measure of municipality proximity to the ARRA public good project, the interaction of this with an indicator for the year being after 2008, year fixed effects, and, possibly additional controls. Columns 1-3 present the coefficient on the interaction term using a binary measure of municipality proximity to the public good, whereby, municipalities are sorted into "treated" and "untreated" groups on the basis of whether the closest "ARRA-funded-by" sign (located at the terminus of each road construction project) is within a distance of 0.05 decimal degrees (approximately 5 kilometers) from the municipality geographic center. Columns 4-6 present the coefficient on the interaction term using a continuous measure of this distance (in decimal degrees). County-specific time trends include county-by-year interaction terms. Demographic controls include the share of African-American population, the share of Latino/Hispanic population, and the unemployment rate. Results include only those observations without obvious inconsistencies in turnout and population data (dropping those observations with turnout in excess of 2012 population). Standard errors are clustered by municipality (563 clusters). * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 5: Placebo Test - ARRA Effect After Year 2000

Dependent Variable: Democratic Vote Share (0 - 100)								
	Binary Treatment				Continuous Treatment			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Treatment x Post 2008	1.791***		1.791***					
	(0.343)		(0.582)					
Treatment x Post 2000		-0.0554		-0.0554				
		(0.412)		(0.566)				
Distance x Post 2008					-12.01***		-12.01***	
					(2.180)		(4.077)	
Distance x Post 2000						1.709		1.709
						(2.684)		(3.732)
Observations	2260	1695	2260	1695	2260	1695	2260	1695
adj. R^2	0.342	0.346	0.342	0.346	0.345	0.349	0.345	0.349

Notes: This table reports results from a placebo test for an effect of ARRA road projects on voting outcomes. For easy comparison, columns 1 and 5 repeat columns 2 and 5 from Table 2, and, columns 3 and 7 repeat columns 2 and 5 from online Appendix Table 2. Columns 2, 4, 6, and 8 restrict the sample to 2000-2008 and replace the post-2008 indicator with a post-2000 indicator equal to 1 if the observation is from after 2000 (and otherwise replicate columns 1, 3, 5, and 7, respectively as detailed in Table 2 and online Appendix Table 2). * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 6: ARRA Public Good Spending and Voting With Hurricane Sandy

Dependent Variable: Democratic Vote Share (0 - 100)						
	Binary Treatment			Continuous Treatment		
	(1)	(2)	(3)	(4)	(5)	(6)
Treated x Post 2008	1.732***	1.810***	1.605***			
	(0.362)	(0.344)	(0.345)			
Distance x Post 2008				-7.556***	-12.05***	-6.237***
				(1.691)	(2.180)	(1.731)
Sandy Damages x Post 2008	-5.584***	-2.044*	-0.294	-5.203***	-1.740	0.249
	(1.629)	(1.216)	(1.514)	(1.445)	(1.196)	(1.435)
County-Specific Time Trend		X			X	
Demographic Controls			X			X
Observations	2260	2260	1130	2260	2260	1130
adj. R^2	0.075	0.344	0.662	0.102	0.347	0.677

Notes: This table reports difference-in-differences estimates of the effect of ARRA road spending on presidential voting outcomes controlling for possible confounding effects of Hurricane Sandy. The regressions reported correspond to the specifications in equivalent columns in Table 2 with the addition of 2 regressors : a term indicating the per capita destruction to the housing stock caused caused by Hurricane Sandy in a given municipality, and, the interaction of this term with the post-2008 indicator (reported above). For all other variables, see the note to Table 2. Standard errors are clustered by municipality (565 clusters). * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

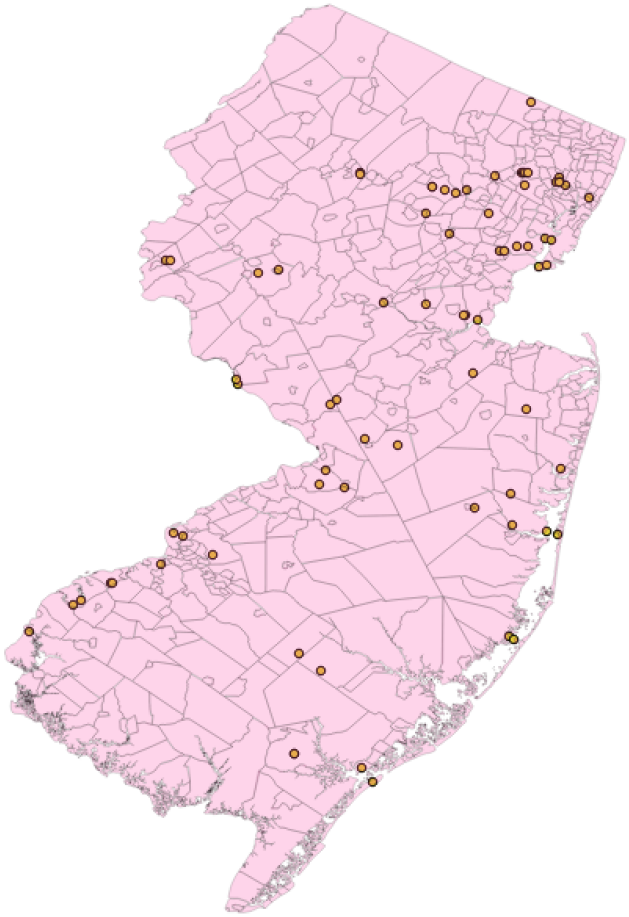
Table 7: ARRA Sign Salience vs. Local Multiplier

	Dependent Variable: Democratic Vote Share (0 - 100)							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Treated x Post 2008		1.699***		1.686***		1.711***		1.698***
		(0.389)		(0.375)		(0.374)		(0.374)
Dollars x Post 2008	0.00109	-0.000256						
(100,000)	(0.000979)	(0.000786)						
Dollars2 x Post 2008			0.00243	-0.000840				
(100,000)			(0.00313)	(0.00251)				
Dollars3 x Post 2008					0.00152	-0.00150		
(100,000)					(0.00249)	(0.00194)		
Dollars4 x Post 2008							0.00189	-0.00114
(100,000)							(0.00244)	(0.00201)
Observations	2260	2260	2260	2260	2260	2260	2260	2260
adj. R^2	0.025	0.058	0.019	0.058	0.020	0.058	0.021	0.058

Notes: This table reports difference-in-differences estimates of the effect of ARRA road spending on presidential voting outcomes, testing the effect of the project existing at all vs. the effect of increasing project expenditures. Columns 1, 3, 5, and 7 come from regressions of Democratic presidential vote share (from 0 to 100) on a measure of the dollars of total expenditure flowing to municipality i from the eventual ARRA project treating it (according to the binary assignment of treatment described in Table 2, with $AMOUNT_i$ being zero for untreated municipalities), and, the interaction of this term with an indicator for the year being after 2008, and, year fixed effects. Columns 2, 4, 6, 8 also add the binary measure of municipality proximity to the ARRA public good project (described in Table 2), and, the interaction of this term with the indicator for the year being after 2008. The specifications use alternative ways of assigning the dollar amount per ARRA project to nearby municipalities (as described in the text). Standard errors are clustered by municipality (565 clusters). * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

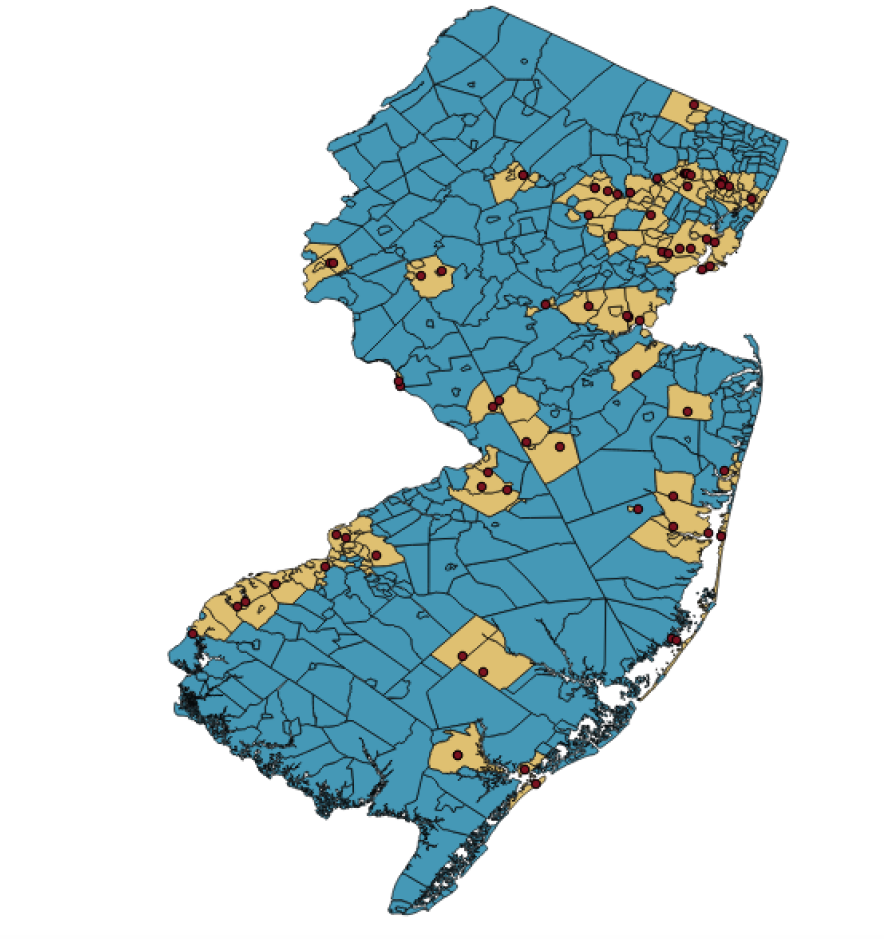
Figure 1: ARRA "Funded By" Road Sign Template

Figure 2: ARRA Projects Plotted in New Jersey Municipalities



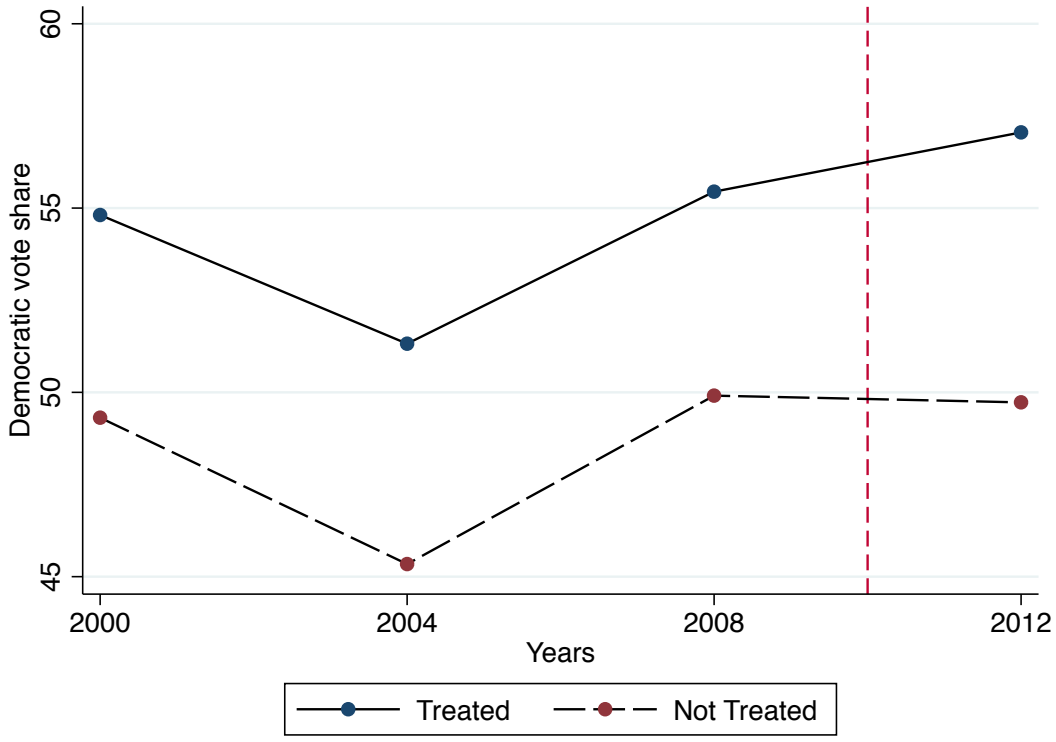
Notes: This figure plots the geographic coordinates of ARRA road project termini inside New Jersey municipality boundaries.

Figure 3: Treatment (Copper) and Control (Teal) Municipalities



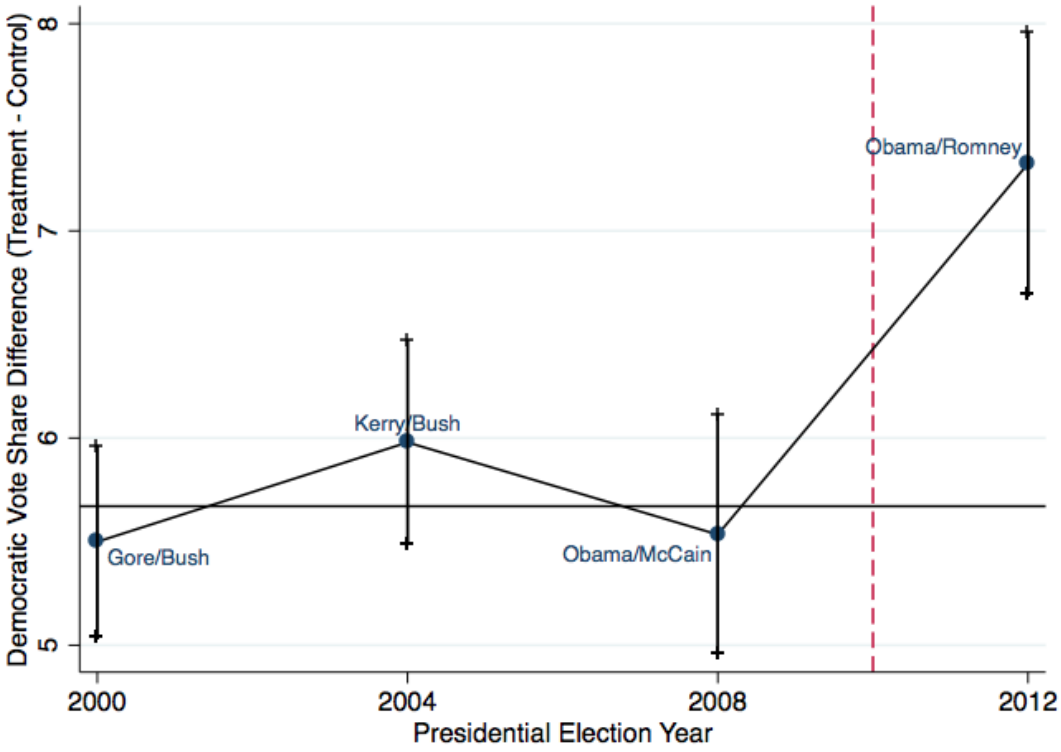
Notes: This figure plots the geographic coordinates of ARRA road project termini inside New Jersey municipality boundaries, and, indicates the assignment of municipalities to treated (copper) or control (teal) status on the basis of the binary assignment of treatment used in the text (whereby municipalities are sorted into "treated" and "untreated" groups on the basis of whether the municipality geographic center is within a short distance - 0.05 decimal degrees, or, approximately 5 kilometers - of the origin of an ARRA road improvement project).

Figure 4: Effect of ARRA Spending (Democratic Vote Share)



Notes: This figure plots the time series of the Democratic presidential vote share for municipalities that are "treated" and "untreated" according to the binary assignment of treatment used in the text (whereby municipalities are sorted into "treated" and "untreated" groups on the basis of whether the municipality geographic center is within a short distance - 0.05 decimal degrees, or, approximately 5 kilometers - of the origin of an ARRA road improvement project).

Figure 5: Effect of ARRA Spending (Democratic Vote Share Difference)



Notes: This figure contains the time series of Democratic presidential vote share differences across treatment and control municipalities (using the binary assignment mechanism described in the text). Averages are empirical averages for the year with 95% confidence intervals constructed using standard errors derived from the empirical distribution (i.e. taking the square root of the difference between the square of the treated municipalities' year-specific standard deviation and the square of the control municipalities' year-specific standard deviation, and, dividing it by the squre root of the total number of observation in the year). The horizontal line represents the average pre-ARRA difference in Democratic vote share across treated and control.

Appendix

For Online Publication

Table 1 of online Appendix: ARRA Public Good Spending and Voting

Dependent Variable: Democratic Vote Share (0 - 100)				
	Binary Treatment		Continuous Treatment	
	(1)	(2)	(3)	(4)
Treated x Post 2008	1.244***	1.231***		
	(0.340)	(0.352)		
Distance x Post 2008			-6.868***	-7.844***
			(1.783)	(2.357)
Demographic Controls	X	X	X	X
County-Specific Time Trend		X		X
Observations	1695	1695	1695	1695
adj. R^2	0.628	0.733	0.645	0.734

Notes: This table reports additional difference-in-differences estimates of the effect of ARRA road spending on presidential voting outcomes. All columns come from regressions of Democratic presidential vote share (from 0 to 100) on a measure of municipality proximity to the ARRA public good project, the interaction of this with an indicator for the year being after 2008, year fixed effects, and, possibly additional controls. Columns 1 and 3 replicate the specifications in Columns 3 and 6 of Table 2 adding year 2000 data including demographic control information from an alternative source (see text). Columns 2 and 4 also include county-by-year interaction terms. In columns 1 and 2 the coefficient on the interaction term using a binary measure of municipality proximity to the public good, whereby, municipalities are sorted into "treated" and "untreated" groups on the basis of whether the closest "ARRA-funded-by" sign (located at the terminus of each road construction project) is within a distance of 0.05 decimal degrees (approximately 5 kilometers) from the municipality geographic center. Columns 3 and 4 present the coefficient on the interaction term using a continuous measure of this distance (in decimal degrees). Standard errors are clustered by municipality (565 clusters). * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 2 of online Appendix: ARRA Public Good Spending and Voting

Dependent Variable: Democratic Vote Share (0 - 100)						
	Binary Treatment			Continuous Treatment		
	(1)	(2)	(3)	(4)	(5)	(6)
Treated x Post 2008	1.654***	1.791***	1.598***			
	(0.550)	(0.582)	(0.593)			
Distance x Post 2008				-7.403***	-12.01***	-6.137**
				(2.690)	(4.077)	(2.535)
County-Specific Time Trend		X			X	
Demographic Controls			X			X
Observations	2260	2260	1130	2260	2260	1130
adj. R^2	0.059	0.342	0.657	0.087	0.345	0.672

Notes: This table reproduces the regressions of Table 2 with an alternative two-way clustering of standard errors by county and municipal form of government (67 clusters). It reports difference-in-differences estimates of the effect of ARRA road spending on presidential voting outcomes. All columns come from regressions of Democratic presidential vote share (from 0 to 100) on a measure of municipality proximity to the ARRA public good project, the interaction of this with an indicator for the year being after 2008, year fixed effects, and, possibly additional controls. Columns 1-3 present the coefficient on the interaction term using a binary measure of municipality proximity to the public good, whereby, municipalities are sorted into "treated" and "untreated" groups on the basis of whether the closest "ARRA-funded-by" sign (located at the terminus of each road construction project) is within a distance of 0.05 decimal degrees (approximately 5 kilometers) from the municipality geographic center. Columns 4-6 present the coefficient on the interaction term using a continuous measure of this distance (in decimal degrees). County-specific time trends include county-by-year interaction terms. Demographic controls include the share of African-American population, the share of Latino/Hispanic population, and the unemployment rate. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 3 of online Appendix: ARRA Sign Salience vs. Local Multiplier

	Dependent Variable: Democratic Vote Share (0 - 100)							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Treated x Post 2008		1.866***		1.881***		1.903***		1.888***
		(0.367)		(0.356)		(0.354)		(0.354)
Dollars x Post 2008	0.00107	-0.000405						
(100,000)	(0.000815)	(0.000696)						
Dollars2 x Post 2008			0.00175	-0.00208				
(100,000)			(0.00222)	(0.00195)				
Dollars3 x Post 2008					0.000867	-0.00260		
(100,000)					(0.00170)	(0.00166)		
Dollars4 x Post 2008							0.00122	-0.00225
(100,000)							(0.00171)	(0.00182)
Observations	2260	2260	2260	2260	2260	2260	2260	2260
adj. R^2	0.330	0.343	0.330	0.343	0.330	0.342	0.330	0.342

Notes: This table reports the results from specifications identical to those described in Table 7 but with all columns here additionally including county-specific time trends. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Figure 1 of online Appendix: ARRA Road Sign Instructions

New Jersey Department of Transportation
QUALITY IMPROVEMENT ADVISORY
CAPITAL PROGRAM SUPPORT
Director: Walter McGrosky Telephone: (609) 530-5367

QIA No. QIA 049

Approved: W. McGrosky
Date: May 15, 2009

Subject: American Recovery and Reinvestment Act Sign (Construction Identification Sign)

Process Affected:

Scope [X] Design Right of Way Utilities Environmental Historic [X] Construction

Bureaus Affected: In-house Design, Project Management, Traffic Engineering, Construction, Design Consultants

Procedure(s) Affected:

Nature of Issue(s): On March 3, 2009 President Obama made the commitment that all projects funded by the American Recovery and Reinvestment Act (ARRA) will bear a recovery emblem to make it easier for Americans to see which projects are funded by the ARRA. To meet this commitment, designers are to include the ARRA signs on all projects funded by the ARRA including projects under construction.

Figure 2 of online Appendix: Example of ARRA "Funded By" Road Sign