Stimulating the Vote: ARRA Road Spending and Vote Share[†]

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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 projects in New Jersey. I find an approximate 1.5 percentage point increase in Democratic Party presidential vote share in areas close to highway and bridge expenditures. I consider two alternative mechanisms: one, a salience mechanism whereby spending and associated "funded-by" signage affect political preferences; the other, a possible political multiplier effect whereby stimulus spending improves local economic outcomes, generating incumbent votes. Evidence is inconsistent with the later explanation. (JEL D72, H41, H54, H76, R42)

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-differences 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 Barack 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 designated specifically for infrastructure investments, including $27.5 billion directed to highway and bridge improvement projects (Reichling 2012).

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 and Senate 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, Miguel, and Vigorito (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, Emeriau, and Fiala forthcoming, 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 with 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. Ortega and Penfold-Becerra (2008) finds a similar divergence in Venezuela. Drazen and Eslava (2010), on the other hand, documents evidence from Colombia of a positive association between incumbent vote share and infrastructure spending.

Regarding the case at hand, theoretically, there is good reason to question whether the positive voter response to cash transfers observed in developing countries implies a positive voter response to government public goods spending in a developed economy.[1] Beyond potential differences across levels of economic development, there is a fundamental difference, as noted by Persson and Tabellini (1999) and Lizzeri and Persico (2001), among others, between transfers of cash and spending on public goods in that the later cannot be as easily targeted to specific voters.[2] Relatedly, there is less ability to target the public good dollar to a voters' preferences. Direct cash transfers literally end up in the pocketbook of a voter, thus allowing them to spend a given government dollar on the highest marginal utility end. Government expenditures on public goods appear indirectly in (some) voters' consumption bundles but not their wallets, and, even if generally valued and broadly consumed, they may therefore be less likely to be credited by a reciprocal, or pocketbook, voter at

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[1] Positive voter responses are, of course, "positive" from the perspective of the party providing the government expenditure.

[2] Golden and Min (2013) make a related point, noting how the potential excludability of a given government transfer can more effectively discipline electoral reciprocity in voters than can non-excludable expenditures such as public goods. The abovementioned works by Ortega and Penfold-Becerra (2008) and Linos (2013) underscore this difference.

election time.[3] On the other hand, the non-rivalrous nature of public goods means a dollar of government public good spending, by giving a little to a lot, so to speak, could in principle lead to significant positive voter responses.

While a chief contribution of the present work is to try and resolve the fact of the matter, in the context of a once-in-a generation public expenditure program in the United States, estimating the size of any electoral dividend due to public good spending is an important corollary. In the US political landscape, federal election spending has now surpassed $6 billion.[4] Much of this is spent in the form of direct mailers, canvassing, phone calls, and advertisements, which recent work has shown to be largely ineffective at voter persuasion.[5] The present finding suggests that a focus on more foundational matters of governance and legislative priorities that deliver public goods to voters may yield considerably greater electoral reward for the responsible parties than traditional, general election spending to persuade marginal voters—for which the persuasive effect size has been found to be zero (Kalla and Broockman 2018).

By comparison, I estimate an approximate 1.5 percentage point increase in the presidential vote share for the Democratic Party attributable to being near (within approximately five kilometers of) the origin of an ARRA road project. This novel quasi-experimental finding about the impact of public good spending on vote shares relies on a difference-in-differences design. As such, the identifying assumption required for the present research 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. Moreover, the estimated effect sizes are robust to a variety of specifications and to the inclusion of a host of time-varying demographic controls that may be thought to predict electoral outcomes as well as county-specific time trends. Additionally, I show that the effect on presidential elections is more likely a result of voters crossing party lines, or vote switching, rather than the result of additional voters turning out to the polls. I also document similar, though more modest, effects of the public good spending on the Democratic vote share in US Senate elections.

The findings are consistent with two alternative mechanisms. The first proposed mechanism is inspired by the salience literature in public finance (Chetty, Looney, Kroft 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

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[3]The exception being those with preferences that place particularly high marginal utility on the public good consumption relative to other commodities that a direct transfer could buy.

[4]Green, Emma. 2017. "Most Campaign Outreach Has Zero Effect on Voters," Atlantic. September 30. https://www.theatlantic.com/politics/archive/2017/09/campaigns-direct-mail-zero-effect/541485/.

[5]See the meta-study by Kalla and Broockman (2018).

of a more liberal point on the tax-and-spend continuum, benefiting the Democratic Party. Under this interpretation, the findings provide new evidence of the importance that salience plays not only in individual responses to government taxation, but also in responses to government expenditures.

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, and, suggesting by extension, that the salience channel may be more likely to be responsible for the results (with associated implications for future policy design).

In the work that follows, I first present more details regarding the ARRA program nationally and in the state of New Jersey before discussing data construction. I go on to document 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.

I. Background and Data

A. 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 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.

FIGURE 1. ARRA "FUNDED BY" ROAD SIGN TEMPLATE

Source: US Department of Transportation via the New Jersey Department of Transportation

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, NJ 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).

B. *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 2015). 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 US presidential Elections (Dave Leip's Atlas of US Presidential Elections 2016). The 2004 election data comes from the New Jersey Department of State's election archive (New Jersey Department of State 2016). The 2000 election data comes from the Center for Congressional and Presidential Studies at American University's School of Public Affairs (Lubin and Voss 2001). 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 contains 2,260 observations. For the US Senate, on-cycle elections took place 6 times in the twenty-first century, yielding, 3,390 observations across all municipalities. Senate election data comes from the same sources as above for 2000, 2008, and, 2012, and with data for the mid-term elections coming from the Precinct-Level Election Data (Ansolabehere, Palmer, and Lee 2014) for 2002 and, again, the New Jersey Department of State's election archive (New Jersey Department of State 2016) for 2006 and 2014.

Municipality-level demographic data is collected from the American Community Survey (ACS), the Decennial Census, and the New Jersey Division of Taxation (United States Census Bureau 2016a; United States Census Bureau 2016b; New Jersey Division of Taxation 2017). For 2008 and beyond, ACS five-year estimates are used for the the population count, the share of African American population, the share of Hispanic population, and the unemployment rate.[6] ACS data at the municipality level does not exist for the other years used in the sample, 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.[7] Remaining years are interpolated

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[6]For 2012 and 2014, the five-year estimate centered at 2012 and 2014, respectively, is used. As there is no five-year estimate centered at 2008 (and since the ACS one-year and three-year estimates do not contain estimates for the vast majority of New Jersey municipalities in any year), I use the ACS' 2009 five-year estimate for 2008.

[7]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
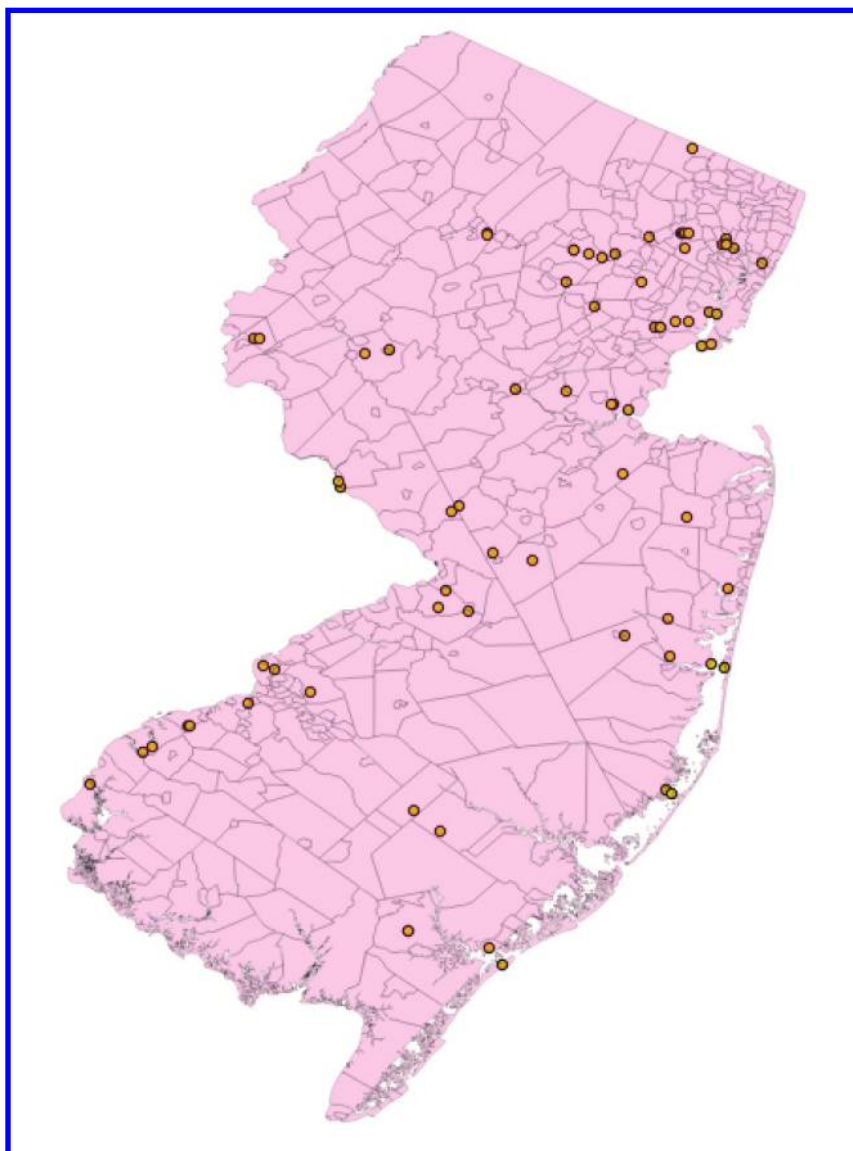
FIGURE 2. ARRA PROJECTS PLOTTED IN NEW JERSEY MUNICIPALITIES

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

for these variables, due to a lack of municipality-level data, while average residential sales price data is taken from the New Jersey Division of Taxation for all years.

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

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the ACS, and, consequently, in the online Appendix, I separately report regression results that include only ACS demographic data as well as results using both ACS and 2000 census data, but without the interpolations used in the main tables (difference-in-differences estimates change very little).

TABLE 1—SUMMARY STATISTICS

	All municipalities	Treated municipalities	Control municipalities
	(1)	(2)	(3)
Democratic vote share (percentage)	50.36 (13.72)	54.66 (14.55)	48.57 (12.95)
Democratic vote share–two party (percentage)	51.51 (13.73)	55.75 (14.66)	49.75 (12.93)
Turnout (as fraction of population)	0.4616 (0.1094)	0.4394 (0.1139)	0.4707 (0.1061)
Distance from origin of nearest ARRA project (in decimal degrees)	0.1055 (0.0840)	0.0310 (0.0119)	0.1365 (0.0816)
Fraction assigned to binary treatment	0.2938 (0.4556)	1 (0)	0 (0)
Unemployment rate	5.997 (3.645)	6.159 (3.654)	5.930 (3.640)
Percentage African American	7.400 (12.34)	8.592 (14.17)	6.904 (11.45)
Percentage Hispanic	9.435 (11.75)	12.75 (14.28)	8.054 (10.22)
Population density (1197/498) (pop. per square mile)	3,404 (5,876)	5,258 (7,499)	2,633 (4,847)
Average home sale price (in dollars)	340,322 (277,033)	311,836 (196,802)	352,173 (303,511)
Observations	2,260 1,695*	664 498*	1,596 1,197*

Notes: Observation counts with asterisks refer to the observation numbers for raw data on unemployment rate, percentage African American, and percentage Hispanic, as 2004 data is not available at the municipality level for these variables and is interpolated in that year in main regression results. Turnout summary statistics are based on the 2,249 (662 treated and 1,587 control) observations used in Table 4, 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.

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 and in population density, indicating the importance of controlling for background demographic characteristics in the analysis that follows (though in practice doing so changes results very little, suggesting the main finding is robust to any demographic differences).

II. Results

In this section, I review the outcomes of presidential elections first followed by the outcomes of US Senate elections. I then provide an analysis of the potential mechanisms at play. 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.

A. *Baseline Results*

Figure 3 presents the resulting assignment of municipalities into treated (light-shaded) and untreated (dark-shaded) 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 twenty-first 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 time series of far municipalities' Democratic vote share. While there are limits to inference due to the relatively short twenty-first century time window,[8] the available evidence 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-differences 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:

$$(1) \quad DemVoteShare_{it} = \alpha_1 TREAT_i + \alpha_2(TREAT_i \times POST_t) + \mathbf{YEAR}_t\psi + \mathbf{X}_{it}\beta,$$

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 its 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, including the share of African American population, the share of Latino/Hispanic population, the unemployment rate, population density, average home sale price, and municipal form of government. The coefficient α_2 represents

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[8] Municipality-level data in the pre-2000 years is far less accessible; hence, the twenty-first century focus.
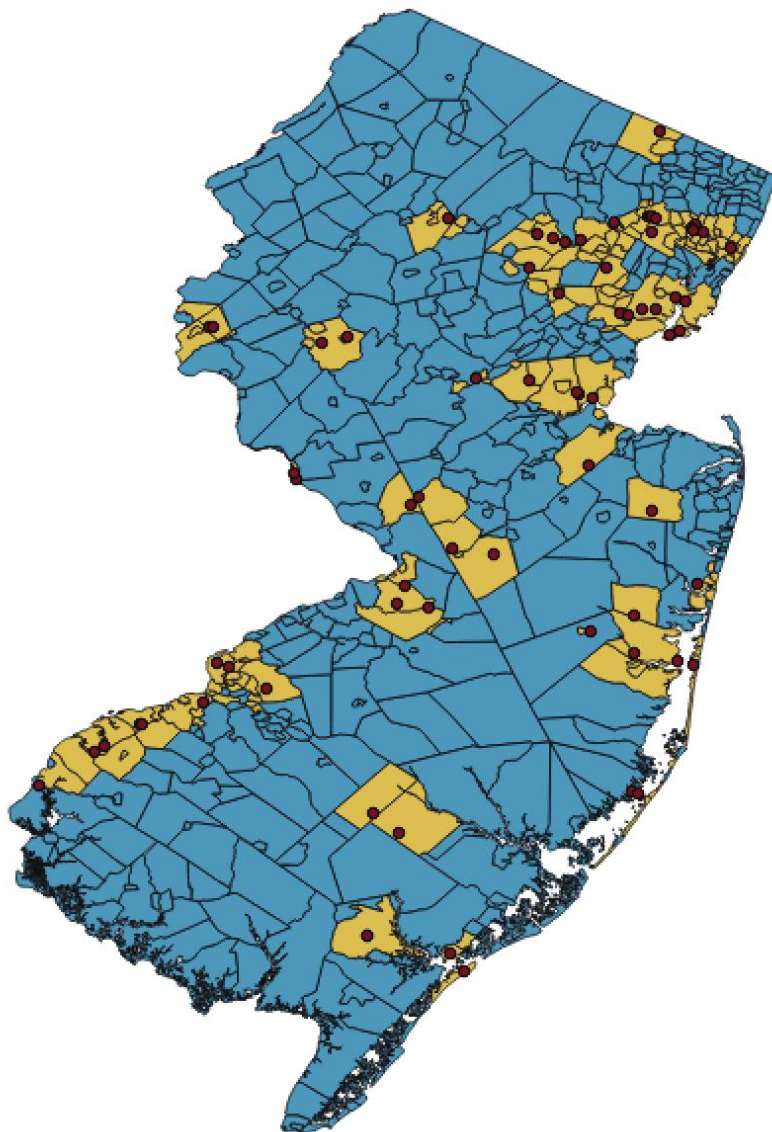
FIGURE 3. TREATMENT (LIGHT) AND CONTROL (DARK) MUNICIPALITIES

Notes: This figure plots the geographic coordinates of ARRA road project termini inside New Jersey munici-pality boundaries and indicates the assignment of municipalities to treated (light) or control (dark) 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 dec-imal degrees, or approximately 5 kilometers—of the origin of an ARRA road improvement project).

the difference-in-differences 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.
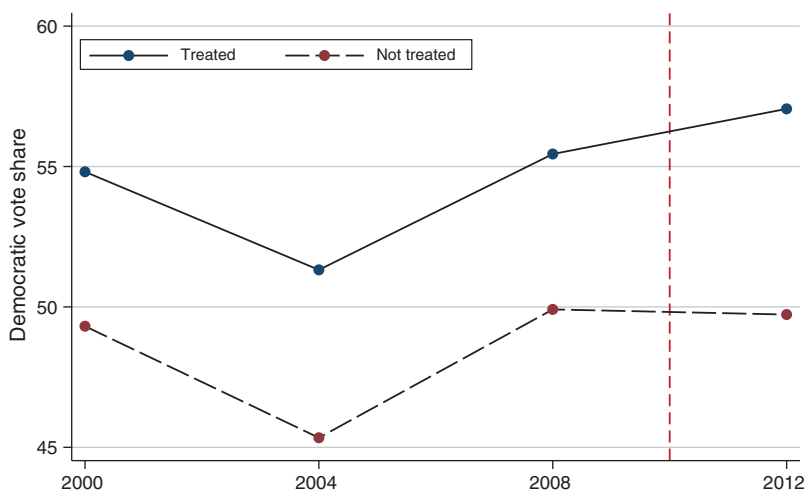
FIGURE 4. EFFECT OF ARRA SPENDING (DEMOCRATIC VOTE SHARE)

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).
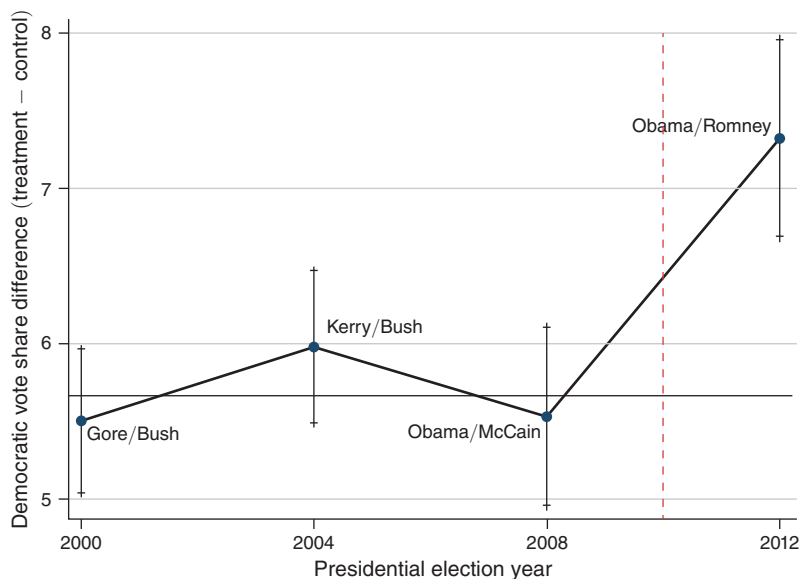
FIGURE 5. EFFECT OF ARRA SPENDING (DEMOCRATIC VOTE SHARE DIFFERENCE)

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 percent 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 observations in the year). The horizontal line represents the average pre-ARRA difference in Democratic vote share across treated and control.

Table 2—ARRA Public Good Spending and Voting

	Dependent variable: Democratic presidential vote share (0–100)							
	Binary treatment				Continuous treatment			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Treated × Post 2008	1.654	1.791	1.307	1.398				
	(0.365)	(0.343)	(0.354)	(0.354)				
Distance × Post 2008					−7.403	−12.01	−6.125	−9.930
					(1.705)	(2.180)	(1.763)	(2.337)
County-specific time trend		X		X		X		X
Demographic controls			X	X			X	X
Treated/Distance	5.671	2.973	1.971	0.419	−41.21	−25.32	−23.94	−6.077
	(1.220)	(1.047)	(0.743)	(0.672)	(6.786)	(6.674)	(4.376)	(4.604)
Observations	2,260	2,260	2,260	2,260	2,260	2,260	2,260	2,260
Adjusted R^2	0.059	0.342	0.652	0.758	0.087	0.345	0.668	0.759

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 1–4 present the coefficient on the interaction term Treated × Post 2008 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 5–8 present the coefficient on the interaction term Distance × Post 2008 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 the African American population, the share of the Latino/Hispanic population, the unemployment rate, population density, average home sale price, and municipal form of government. Standard errors are clustered by municipality (565 clusters).

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, finding a slightly smaller coefficient (1.307 percentage points). Column 4 includes both, and, like the other estimates, reports a coefficient close to 1.5 percentage points (1.398). All estimates are significant at the 1 percent level of significance. Notably, the result is quite stable to controls for shifts in Democrat-supporting demographic groups and measures of the financial crisis proxied through unemployment and housing price variables.

Online Appendix Table 1 uses a more restrictive criteria for inclusion of control variable data and excludes demographic interpolations (see Section IIB for discussion of data sources and discrepancies in data construction across years). Column 1 of the table uses only the years for which demographic controls can be pulled from the same data sources across years. In columns 2 and 3, all years for which demographic data at the municipality level can be used without interpolation (regardless of data source) are used. As can be seen in online Appendix Table 1, estimates change very little from Table 2 (with the difference-in-differences point estimate again close to 1.5) and remain significant at the 1 percent level. 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). The corresponding results in columns 1–4 remain significant at the 1 percent level of significance and of similar magnitude to the previous

estimates. As a further robustness check, I use propensity score matching methods[9] to construct a subsample of untreated municipalities whose pre-ARRA profile of observable characteristics (county and the demographic controls mentioned above) more closely matches that of the eventually treated municipalities. I then perform difference-in-differences specifications, as in equation (1), using this subsample of municipalities and the eventually treated ones. The results, presented in online Appendix Table 3, once again demonstrate significant ARRA effects consistent with the other estimates (though slightly smaller in size). As can be seen in the table, the estimates are robust to a variety of implementation choices in the propensity score matching method.[10]

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

$$(2) \qquad DemVoteShare_{it} = \alpha_1 DISTANCE_i + \alpha_2 (DISTANCE_i \times POST_t)$$

$$+ \mathbf{YEAR}_t \psi + \mathbf{X}_i \beta,$$

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 5–8 in Table 2 replicate the regressions in columns 1–4 using this continuous measure of treatment assignment instead. The associated difference-in-differences 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 5, for example, the difference-in-differences estimate is -7.403 and significant at the 1 percent level, indicating that a change in $DISTANCE_i$ of 1 standard deviation is associated with a 0.62 percentage point drop in Democratic presidential vote share. In columns 6, 7, and 8, the same change is associated with a respective 1.01, 0.52, and 0.83 percentage point drop in Democratic vote share, estimates again being significant at the 1 percent level. As a robustness check, columns 4–6 in online Appendix Table 1 reproduce columns 1–3 in the same table using the continuous measure of treatment assignment instead, and columns 5–8 of online Appendix Table 2 replicate columns 5–8 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.

Table 3 presents difference-in-differences 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

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[9] See Rosenbaum and Rubin (1983) for an introduction to propensity score matching, and Leuven and Sianesi's Stata suite psmatch2, which implements the matching procedure https://ideas.repec.org/c/boc/bocode/s432001.html

[10] Namely, estimates are robust to the decision to match on observables using values from only the last year prior to ARRA (2008), the first year of the data (2000), or the full pre-ARRA year-by-year profile of observable characteristics in the matching procedure. Online Appendix Table 3 uses a control group derived from logit specifications of treatment status on the observables in the propensity score matching procedure, but results are virtually identical if instead identifying propensity score via probit regressions.

TABLE 3—ARRA PUBLIC GOOD SPENDING AND VOTING—TWO PARTY VOTE SHARE

	Dependent variable: Two-Party Democratic presidential vote share (0–100)							
	Binary treatment				Continuous treatment			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Treated × Post 2008	1.778	1.706	1.414	1.303				
	(0.354)	(0.338)	(0.344)	(0.353)				
Distance × Post 2008					−8.690	−11.32	−7.374	−9.196
					(1.632)	(2.151)	(1.721)	(2.324)
County-specific time trend		X		X		X		X
Demographic controls			X	X			X	X
Treated/Distance	5.552	3.094	1.815	0.477	−40.01	−26.68	−22.61	−7.084
	(1.231)	(1.067)	(0.756)	(0.693)	(6.647)	(6.833)	(4.242)	(4.741)
Observations	2,260	2,260	2,260	2,260	2,260	2,260	2,260	2,260
Adjusted R^2	0.054	0.324	0.652	0.748	0.081	0.328	0.666	0.748

Notes: This table reports equivalent difference-in-differences 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 1–4 present the coefficient on the interaction term Treated × Post 2008 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 5–8 present the coefficient on the interaction term Distance × Post 2008 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 the African American population, the share of the Latino/Hispanic population, the unemployment rate, population density, average home sale price, and municipal form of government. Standard errors are clustered by municipality (565 clusters).

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.

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 1–4 and 5–8 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 at the 5 percent level throughout the table. 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.[11] Taken together, the results in Tables 2– 4 suggest that vote switching, rather than mobilization of additional voters, is a more

[11] 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.

TABLE 4—ARRA PUBLIC GOOD SPENDING AND VOTER TURNOUT

	Dependent variable: Presidential voter turnout (percent of pop.)							
	Binary treatment				Continuous treatment			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Treated × Post 2008	0.000418	−0.00248	0.00680	0.00350				
	(0.00456)	(0.00443)	(0.00531)	(0.00520)				
Distance × Post 2008					−0.0336	−0.00589	−0.0522	−0.0293
					(0.0230)	(0.0322)	(0.0281)	(0.0372)
County-specific time trend		X		X		X		X
Demographic controls			X	X			X	X
Treated/Distance	−0.0314	−0.0285	−0.00889	−0.00786	0.159	0.221	0.0752	0.0967
	(0.0101)	(0.00934)	(0.00720)	(0.00693)	(0.0571)	(0.0680)	(0.0437)	(0.0535)
Observations	2,249	2,249	2,249	2,249	2,249	2,249	2,249	2,249
Adjusted R^2	0.088	0.228	0.497	0.561	0.084	0.228	0.498	0.562

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 1–4 present the coefficient on the interaction term Treated × Post 2008 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 5–8 present the coefficient on the interaction term Distance × Post 2008 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 the African American population, the share of the Latino/Hispanic population, the unemployment rate, population density, average home sale price, and municipal form of government. 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).

likely means by which public goods spending affects Democratic vote share (though it is possible that a fall in Republican turnout and an offsetting rise in Democrat turnout, leaving total turnout unaffected, could explain the findings without any previous voters actually switching the party for which they vote).

B. *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 6 from Table 2, and columns 3 and 7 repeat columns 2 and 6 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 equations (1) and (2) 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. Further placebo test specifications (unreported) replicating the other columns of Tables 2 and online Appendix Table 2 including the demographic controls, similarly

TABLE 5—PLACEBO TEST—ARRA EFFECT AFTER YEAR 2000

	Dependent variable: Democratic presidential vote share (0–100)							
	Binary treatment				Continuous treatment			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Treated × Post 2008	1.791 (0.343)		1.791 (0.582)					
Treated × Post 2000		−0.0554 (0.412)		−0.0554 (0.566)				
Distance × Post 2008					−12.01 (2.180)		−12.01 (4.077)	
Distance × Post 2000						1.709 (2.684)		1.709 (3.732)
Observations	2,260	1,695	2,260	1,695	2,260	1,695	2,260	1,695
Adjusted 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 6 from Table 2, and columns 3 and 7 repeat columns 2 and 6 from online Appendix Table 2 (using an alternative two-way cluster). 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).

do not yield a significant positive coefficient on the placebo term for the binary treatment placebo specifications, or a significant negative coefficient on the placebo term for the continuous treatment placebo specifications.

Such a result is not wholly unexpected in light of Figure 4. While the parallel pretrends in it invite confidence 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, 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. While this kind of threat is common to difference-in-differences designs and cannot be disproven, generally speaking, specific threats to identification can be assessed.

A key concern of this sort is the existence of a "Hurricane Sandy" effect. At the time, 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 of damages in New Jersey, one of the states hardest hit by the hurricane, in addition to more than 30 deaths there.[12] 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.[13] 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

[12] CNN. 2013. "Hurricane Sandy Fast Facts." July 13. http://edition.cnn.com/2013/07/13/world/americas/hurricane-sandy-fast-facts.

[13] Boerma, Lindsey. 2012. "Hurricane Sandy: Election 2012's October Surprise?" CBS News. October 28. http://www.cbsnews.com/news/hurricane-sandy-election-2012s-october-surprise/.

Table 6—ARRA Public Good Spending and Voting with Hurricane Sandy

	Dependent variable: Democratic presidential vote share (0–100)							
	Binary treatment				Continuous treatment			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Treated × Post 2008	1.732	1.810	1.361	1.410				
	(0.362)	(0.344)	(0.355)	(0.356)				
Distance × Post 2008					−7.556	−12.05	−6.384	−9.972
					(1.691)	(2.180)	(1.767)	(2.337)
Sandy damages × Post 2008	−5.584	−2.044	−2.872	−0.970	−5.203	−1.740	−2.722	−0.730
	(1.629)	(1.216)	(1.463)	(1.224)	(1.445)	(1.196)	(1.366)	(1.189)
County-specific time trend		X		X		X		X
Demographic controls			X	X			X	X
Treated/Distance	5.914	3.043	2.110	0.456	−41.69	−25.47	−24.30	−6.169
	(1.210)	(1.050)	(0.740)	(0.674)	(6.808)	(6.665)	(4.362)	(4.589)
Observations	2,260	2,260	2,260	2,260	2,260	2,260	2,260	2,260
Adjusted R^2	0.075	0.344	0.656	0.759	0.102	0.347	0.672	0.759

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 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).

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 (O'Dea 2013) on the total number of homes and rental units damaged by the hurricane within each municipality and scale this count by the 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-differences estimator measuring an effect of ARRA projects on Democratic vote share remains almost identical to its 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 neither positive nor consistently significant across specifications. This militates against the "October surprise" view of the hurricane serving as a boon to President Obama (a conventional wisdom challenged at the time by polling experts Harry Enten and Nate Silver of the website www.fivethirtyeight.com, who noted that polls before and after Sandy in the hardest hit states showed no clear movement toward Obama after the hurricane.)[14]

[14] Enten, Harry. 2012. "Was It Hurricane Sandy That Won It for President Obama?" The Guardian. December 4. https://www.theguardian.com/commentisfree/2012/dec/04/hurricane-sandy-won-president-obama.

C. *Additional Electoral Outcomes*

While President Obama led the push for ARRA, spending much of his early term political capital on its passage, congressional Democrats also played an essential role in bringing ARRA into being. It is thus natural to wonder if they also received electoral support from ARRA beneficiaries. To complement the findings in presidential elections, I explore the consequences of ARRA public goods expenditures on legislative branch elections using Democratic vote share in US Senate elections. The effects are generally consistent, though more modest in size and significance, suggesting on net that down ticket candidates also experienced some sort of electoral gains attributable to ARRA infrastructure spending.

Table 7 reports results that modify equations (1) and (2) by changing the outcome to the Democratic share of the two-party vote in US *Senate* elections.[15] Results in panel A correspond to the binary treatment assignment used in equation (1) and results in panel B correspond to the continuous treatment assignment used in equation (2). Columns 1–2 restrict the sample of Senate elections to the 2000–2012 time period for easy comparability with the presidential election results. In these columns, we see results consistent with the presidential findings: significant, positive coefficients on the ARRA difference-in-differences estimate for the binary treatment specifications and significant and negative coefficients on the difference-in-differences estimate for the continuous treatment specifications. While consistent, there is some difference in the size of the point estimates. For example, in column 1 of panel A, having an ARRA road project constructed within approximately 5 kilometers of a municipality center is estimated to increase municipal Democratic senatorial two-party vote share by 0.921 percentage points, whereas, the equivalent specification with the presidential vote outcome (column 4 of Table 3) is 1.778 percentage points. For the other estimates in these columns, the ratio of Senate coefficients to their corresponding presidential coefficients is larger, but still less than 1.

In the remaining columns of Table 7, the 2014 election is also included in the sample. In columns 3–4, the same regressions run in columns 1–2 are run again on the full sample. Results remain qualitatively similar, but, in column 4 of panel A, the ARRA difference-in-differences estimate is no longer statistically significant (while in panel B, and in both panels of column 3, it is). These regressions in effect present an average effect for all post-treatment years, while the remaining columns (5–6) allow for heterogeneity in the effect sizes based on proximity of the election to the time of road expenditures. Looking at column 6 of panel A, the statistically insignificant coefficient found in column 4 of the same panel can be seen to result from averaging across two coefficients: one that is statistically significant (and larger) indicating an immediate impact in the first election following the public good spending, and one that is not statistically significant for the next election that follows. This significant and larger effect in the first post-ARRA election and attenuation of significance and effect

Silver, Nate. 2016. "How Much Do "October Surprises" Move the Polls?" Fivethirtyeight. October 30. http://fivethirtyeight.com/features/how-much-do-october-surprises-move-the-polls/.

[15] Unlike in presidential elections, third party votes in the Senate elections are minimal, making for little divergence between Democratic vote share and Democratic two-party vote share. For data availability reasons, I focus on the later.

TABLE 7—ARRA PUBLIC GOOD SPENDING AND VOTING (SENATE)

	Dependent variable: Two-Party Democratic Senate vote share (0–100)					
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A. Binary treatment						
Treated × Post 2008	0.921	0.871	1.212	0.594		
	(0.431)	(0.427)	(0.388)	(0.402)		
Treated × 2012					0.921	0.906
					(0.431)	(0.422)
Treated × 2014					1.504	0.281
					(0.456)	(0.514)
Treated	6.410	0.754	6.410	0.750	6.410	0.750
	(1.321)	(0.729)	(1.321)	(0.730)	(1.322)	(0.731)
Adjusted R^2	0.070	0.757	0.067	0.754	0.067	0.754
Panel B. Continuous treatment						
Distance × Post 2008	−7.507	−8.921	−11.87	−6.508		
	(2.026)	(2.951)	(2.089)	(2.704)		
Distance × 2012					−7.507	−9.001
					(2.027)	(2.911)
Distance × 2014					−16.22	−4.015
					(2.703)	(3.761)
Distance	−41.19	−6.711	−41.19	−6.364	−41.19	−6.364
	(6.768)	(4.894)	(6.768)	(4.871)	(6.769)	(4.872)
Adjusted R^2	0.089	0.758	0.091	0.754	0.091	0.754
County-specific time trend		X		X		X
Demographic controls		X		X		X
Observations	2,825	2,825	3,390	3,390	3,390	3,390

Notes: This table reports difference-in-differences estimates of the effect of ARRA road spending on votes for US Senate candidates. All columns come from regressions of Democratic Senate candidate two-party 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. Panel A presents the coefficient on an 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. Panel B presents the coefficient on the interaction term using a continuous measure of this distance (in decimal degrees). Columns 1–2 present the sample using elections between 2000 and 2012 for comparability with the presidential election results. Columns 3–6 include the 2014 election as well. Columns 5–6 break down the post-2008 average treatment effect into separate effects in 2012 and 2014. County-specific time trends include county-by-year interaction terms. Demographic controls include the share of the African American population, the share of the Latino/Hispanic population, the unemployment rate, population density, average home sale price, and municipal form of government. Standard errors are clustered by municipality (565 clusters).

size afterward is also visible in the continuous treatment specification in column 6 of panel B (though in this case the average effect across both years is still significant in the corresponding specification in column 4 of the panel).[16] As with the presidential election results, the results in Table 7 do not change when controlling for Hurricane Sandy (a threat to internal validity that is less acute in Senate elections a priori given the stronger role of the executive branch in immediate response to natural disasters), and the Senate election results pass equivalent placebo tests.

Broadly speaking, the relatively robust effect of ARRA road spending on Democratic two-party vote share in the proximate Senate election (with some

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[16] We do not see this attenuation pattern in column 5, showing that in this case the inclusion of the demographic controls and county-specific time trends does affect estimates (for the 2014 year effect) significantly.

indication of subsequent attenuation) is consistent with the presidential election results. However, the mostly smaller effect size observed relative to the presidential elections suggests the possibility of somewhat different political dynamics at play in the two elections. Such differences in size may be explained by greater emphasis on local concerns in congressional elections vis á vis presidential races, or by the more diffuse responsibility for ARRA attributed to any one senator (who provides only 1 vote out of 100 in one house of the legislative branch) as compared to the sole credit given to the president for the actions of the executive branch,[17] though, certainly other possibilities exist. While these differences are of interest and worth investigating in future work, the remainder of the paper focuses on the explanation of the general finding of a positive ARRA difference-in-differences estimate, seeking to distinguish alternative explanatory mechanisms.

D. *Mechanism*

As previously mentioned, there are at least two competing (but possibly coexisting) mechanisms that 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 (Jones 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 incumbents are more likely to be reelected when economic conditions are good (Bagues and Esteve-Volart 2016), then the increased vote share in 2012 for the Democratic incumbent (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

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[17] Consistent with this later explanation, Google searches for Obama + ARRA yield twice the number of results as Senate + ARRA.

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). Note that $\delta V / \delta Benefit > 0$ indicates that, all other things equal, with an increase in the perceived *Benefit* from taxation, voters are more likely to vote for the Democratic 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 elections where the incumbent was a Democrat.

The ARRA program affects the voting decision in the following way:

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

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 $\left(\frac{\delta V}{\delta EconCond}\right)\left(\frac{\delta EconCond}{\delta ARRA}\right)$ or $\left(\frac{\delta V}{\delta Benefit}\right)\left(\frac{\delta Benefit}{\delta ARRA}\right)$, 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. The term $\frac{\delta EconCond}{\delta ARRA}$ represents a pure "stimulus" effect and $\frac{\delta Benefit}{\delta ARRA}$ represents a pure "salience" effect.

Table 8 presents the results of regressions that seek to distinguish between the two competing mechanisms using the presidential election results. 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),

$$(3) \quad DemVoteShare_{it} = \alpha_1 TREAT_i + \alpha_2(TREAT_i \times POST_t) + \mu_1 AMOUNT_i$$

$$+ \mu_2(AMOUNT_i \times POST_t) + \mathbf{YEAR}_t\psi + \mathbf{X}_{it}\beta,$$

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-differences estimator representing the potential political multiplier channel.

The different results in Table 8 (and Table 4 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

TABLE 8—ARRA SIGN SALIENCE VERSUS LOCAL MULTIPLIER

	Dependent variable: Democratic Presidential vote share (0–100)							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Treated × Post 2008		1.699 (0.390)		1.685 (0.375)		1.710 (0.374)		1.696 (0.374)
Dollars × Post 2008 (100,000)	0.00109 (0.000979)	−0.000256 (0.000787)						
Dollars2 × Post 2008 (100,000)			0.00244 (0.00314)	−0.000815 (0.00252)				
Dollars3 × Post 2008 (100,000)					0.00153 (0.00250)	−0.00147 (0.00195)		
Dollars4 × Post 2008 (100,000)							0.00190 (0.00245)	−0.00112 (0.00202)
Observations	2,260	2,260	2,260	2,260	2,260	2,260	2,260	2,260
Adjusted 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 versus 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, and 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).

significant at standard levels), casting doubt on the relevance of the political multiplier channel in this context.

Columns 1 and 2 of Table 8 (and of Table 4 of the online Appendix, which replicates Table 8 with the addition of county-specific time trends for each column) report the difference-in-differences 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, positive α_2 term (consistent with the previous results) and a 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. Extensions of Table 8 (and Table 4 of the online Appendix) that either mirror the specifications in the other columns of Table 2 (and Table 2 of the online Appendix) or utilize yet another alternative way of assigning the dollar amount per ARRA project to nearby municipalities,[18] further support the

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[18] Essentially, the alternative treats bridges and roads differently by distributing the initial ARRA road project expenditure amount evenly across the project termini in road work, while counting bridge sites as the recipient of

salience mechanism (again yielding consistently significant and positive α_2 terms, with coefficient size in line with the previous estimates) while similarly failing to provide any support for the political multiplier mechanism (again yielding small μ_2 terms that in no case demonstrate a significantly positive coefficient).

III. Conclusion

This work's main result of a significant and positive effect on Democratic 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 whose benefits are less direct and likely less apparent to pocketbook voters than money in the bank. The evidence presented suggests that the ARRA vote share result is most likely explained by a salience mechanism that helps to overcome this limitation, 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 in favor of the "tax and spend" party. However, it should be reiterated that while the evidence is consistent with this salience mechanism, it is not definitive, and other possible mechanisms exist. In particular, the political multiplier mechanism explored, 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 8) 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.

Additionally, 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

the full bid amount, and, then, performs regressions using this amount and transformations of it that are equivalent to those in Table 8 and Table 4 of the online Appendix.

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.

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