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Distributive Politics inside the City? The Political Economy of Spain's *Plan E*

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Abstract

We study the allocation of investment projects by municipal governments across groups of voters using data from a fiscal stimulus program carried out in Spain between 2009 and 2011. This program provided municipalities with a large endowment to spend in public investments and required the geocoding of each individual project. Combining these data with disaggregated election information at the census area level, we study whether politicians use expenditures to target their supporters or to raise turnout. Estimates from regression, matching and RDD methods show no evidence of local governments targeting areas of core support. Instead, investment goes disproportionately to low turnout areas, suggesting that politicians use funds to increase participation. We confirm this hypothesis by showing that, in the following elections, turnout is increased in areas that received more investment. Our results suggest that mobilization can be a strong force in shaping the allocation of resources across voter groups within cities.

Keywords: Political economy; Distributive Politics; Core voters; Turnout; Partisan alignment.

JEL classification: R53; H76; D72

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1. Introduction

Whether politicians can effectively buy electoral support via targeted policies is a question that has understandably received much attention both academically and in the public debate. Voters have been shown to reward incumbents for spending, be it in the form of a specific program targeted to individuals (like an anti-poverty cash transfer, see e.g. Manacorda, Miguel and Vigorito 2011; Pop-Eleches and Pop-Eleches 2012; Baez et al. 2012), or public infrastructure projects (such as a nation-wide road network as in Voigtlaender and Voth 2014). While this literature convincingly shows that voters respond to spending in the polls, it is generally silent on whether and how politicians allocate this spending across voter groups for electoral purposes.

The literature that studies alignment effects along party lines in the allocation of funds between central and local governments could potentially be informative on this matter. There is pervasive evidence that national level politicians favour local governments that are ruled by their own party in the allocation of resources. However, this alignment effect could be due to two different mechanisms. On the one hand, by favouring aligned municipalities, politicians may be indirectly trying to target their core supporters. Alternatively, they may be using these funds to help the local mayor secure re-election (as in the political agency model by Bracco et al. 2015). In the absence of data at the intra-municipal level, distinguishing between spending targeted to voters and to support the local mayor is challenging.

In this paper we use finely disaggregated data to study whether politicians allocate spending in space in response to the spatial distribution of voters. In particular, we ask if investment spending goes disproportionally to areas of strong support for the incumbent or if it is used as a mobilization device to increase turnout. For this purpose, we use geo-located data on municipal investment projects financed by *Plan E*, a 12 billion Euros stimulus program which transferred funds from the Spanish central government to municipalities between 2009 and 2010. This program provides an ideal setting to study distributive politics for several reasons. To begin with, municipal governments had substantial discretion in the use of funds with respect to both type and location of investment projects. Given the urgency to implement this fiscal stimulus, the national government quickly processed the applications for funding, approving in full over 99% of them (Montolio, 2016). Virtually all municipalities applied, and the amount they received was three times as large as their spending in infrastructures in an average year. Finally, all *Plan E* investment projects were geo-located by the municipal authorities. These characteristics of *Plan E* allow us to exploit within-municipal variation in spending to study distributional politics.

To our knowledge, we are the first to study distributive politics inside cities. What enables us to do this is the combination of finely disaggregated data on electoral outcomes and

investment projects.[1] In this context, we consistently find no evidence of partisan bias in the allocation of projects within municipalities. The bias that has been identified in the alignment literature is entirely absent within cities. In fact, political support, as measured by the vote share of the incumbent, does not affect the geographic allocation of spending. We find that investment goes disproportionately to areas of low turnout, suggesting that politicians use funds to increase participation. Using data on *ex-post* electoral outcomes, we provide evidence of an electoral response to local spending in terms of increased participation. Areas receiving a project see a 0.4 percentage points increase in turnout, conditional on past turnout levels. Taken together, these two results suggest that local investment is an effective instrument to mobilize voters.

The most important empirical challenge we face when conducting our analysis arises because the geographical distribution of voter preferences within the city is endogenous to economic, social and cultural factors. These factors may, in turn, also affect investment decisions (Brollo and Nannicini, 2012). This identification problem is also shared by much of the literature studying the electoral determinants of spending across *core* and *swing* voters (as in Levitt and Snyder, Jr. 1995 or Ansolabehere and Snyder 2006). We overcome this issue by first relying on intra-municipal variation in the incumbents' electoral support, and then by using as-good-as-random variation in the identity of the incumbent party in a close election regression discontinuity design.

Our analysis starts by asking whether local politicians target areas of strong electoral support.[2] To this end, we regress measures of investment at the census-area level – e.g. a dummy for receiving at least one project – on the vote share of the incumbent party, controlling for the shares of all major parties and municipal fixed effects. Including all vote shares as controls captures possible determinants of investment that are related to political preferences. Furthermore, they serve as proxies for unobserved socio-economic and cultural factors that also affect the demand for investment. Then, we follow a complementary approach by aggregating information at the municipal level. We define a measure of partisan bias as the difference in the vote share of the left-wing party in census areas that received one investment project and those that did not, averaged across each municipality. This measure is used as an outcome variable in a regression-discontinuity design (RDD) to identify whether incumbents who won close elections disproportionately invest in areas of core support. The regression-discontinuity design relies on comparing this measure of partisan bias in municipalities where the left-wing party barely won and where it barely lost.

Estimates from the regression analysis are all very close to zero and are precisely esti-

[1] Our finest unit of observation is the census area. Spain has over 35,000 census areas that have no electoral representation and are defined for merely statistical purposes. There are a total of 8,116 municipalities in Spain and roughly one in four has more than one census area.

[2] This hypothesis is closely related to the *core voters* hypothesis in the political economy literature (see, e.g. Cox and McCubbins 1986; Dixit and Londregan 1995).

mated. In the most demanding specification, with municipal fixed effect and the full set of controls, census areas with a 10% higher vote share of the incumbent have a 0.16 percentage points higher chance to receive an investment project, with a corresponding 95% confidence interval of [-0.75, 0.43] percentage points. Compared to the baseline probability of receiving a project of 40%, this effect appears extremely small. Estimates obtained using RDD are also not significantly different from zero, hence our analysis provides evidence that mayors do not use spending to favour areas of core support. This is in contrast to previous work that found a positive association between expenditures and the share of core voters (Levitt and Snyder, Jr., 1995; Ansolabehere and Snyder, 2006). Our identification strategy implicitly rests on the assumption that investment projects have a very localized effect on voters' utility, in the sense that only voters in the census area that receive a project are affected. To relax this assumption, we allow investment projects to have a less localized effect by creating "buffers" of radius 25, 50 and 100 meters around each of them. In this way, a project carried out close to a border of two census areas is counted as having taken place in both. Alternatively, we restrict our sample by concentrating only on those categories of projects that are most likely to have localized benefits. Results from these two additional specifications are in line with our baseline result and show that there is no effect of electoral support on investment decisions.

We then turn to the hypothesis that politicians target low participation areas with spending to persuade potential voters to turn out in the polls. Using again variation at the census area level, we find a negative association between spending and turnout. A 1% increase in the previous election's turnout decreases the probability of an area receiving a project by 0.14 percentage points. Similarly, a negative correlation is found when using the number of projects received or the fraction of investment received by the census area over the municipal total. The evidence overall supports the hypothesis that politicians use spending to mobilize the inactive electorate rather than benefiting their voters directly.

But what are the electoral benefits? As mentioned above, there is now a large body of evidence that individually targeted transfers – such as conditional cash transfer programs – are rewarded by a higher probability of turning out and supporting the incumbent. But much less is known about the electoral effects of local investments. Recently, Voigtlaender and Voth (2014) showed that a national highway construction plan helped raise support for the Nazi party in Germany. Despite the fact that sub-national governments carry out two-thirds of all public investment in developed countries (OECD, 2013), the potential electoral effects of local spending have been understudied. To investigate this, we use data on the subsequent municipal elections of 2011 and find that, conditional on previous electoral results, census areas that received an investment project do not increase their support for the incumbent party. However, we observe a response on political participation: conditional on initial levels, areas that receive *Plan E* projects experience an increase in turnout. Moreover, by exploring heterogeneous effects of receiving a project across turnout levels, we identify that this effect comes mainly from low turnout areas. One interpretation is that localized spending changes

voters' perception of the importance their vote can have in shaping distributive policies within the city.

We conclude the empirical analysis by providing additional robustness checks that strengthen the validity of our results. Among them, we show that the main results are not sensitive to specification by implementing estimators based on nearest-neighbour matching and trimming using the propensity score (Abadie and Imbens, 2006; Imbens, 2015).

This paper studies the distribution of public money within the city, hence it lies at the intersection between urban economics and political economy. An important strand of this literature asks if political factors can shape local policies. Ferreira and Gyourko (2009) and Pettersson-Lidbom (2008) study how parties differ in implementing policies in the US and Sweden, respectively, using a regression-discontinuity design. Along the same lines, Solé-Ollé and Viladecans-Marsal (2013) show that centre-right municipal governments in Spain have more expansive zoning policies. This literature treats municipalities as units of observation and therefore abstracts from variation within the city boundaries in both the intensity of policy intervention and the geographic distribution of electoral support. To the best of our knowledge, this paper is the first to investigate partisan differences in policies inside the city.

Our paper also addresses a frequent mismatch between empirical analyses of distributional politics and the theory invoked when interpreting the findings. As Cox (2009) points out, several studies document whether parties target swing or core *districts*, but are not informative about how resources are distributed across groups of *voters*.[3] Most of these papers analyse the allocation of government funds across municipalities, districts or states. For instance, Wright (1974) uses information on New Deal spending and electoral data for US states and finds that the democratic government in power disproportionally targets "swing states". More recently, Strömberg (2004) studies the allocation of the New Deal relief funds at the county level and finds that swing counties with relatively many radio listeners receive more funds, presumably because media presence increases the electoral impact of spending. Ansolabehere and Snyder (2006) use data on US state expenditures across counties and find evidence in favour of the core voters hypothesis but no evidence of swing voter targeting.[4] By studying allocations across geographical areas within municipalities, our paper avoids the problem highlighted by Cox (2009). Census areas are not districts, counties or municipalities and have no institutional entity of their own. This allows for a more direct mapping between the predictions of these models and the empirical analysis. Overall, our results lend little evidence in favour of traditional core voter models.

A growing literature shows evidence of an alignment effect in the allocation of national

[3] A similar point is made in the review by Golden and Min (2013): "The weakness [of these studies] is that results accord poorly with the individual-level theory that is usually held to be relevant."

[4] An important challenge faced by this literature is to identify core and swing areas. One way to tackle this issue is to use survey data to obtain an estimate of the distribution of voter preferences (Dahlberg and Johansson, 2002).

transfers to local governments. For example, Solé-Ollé and Sorribas-Navarro (2008) use a difference-in-differences approach to document that Spanish municipalities aligned with upper tier governments are favoured in the allocation of transfers. Using different research designs, this effect has been documented for several countries, such as Albania (Case, 2001), Italy (Bracco et al., 2015), Portugal (Migueis, 2013), and the United States (Levitt and Snyder, Jr., 1995). We distinguish ourselves from this literature because, in our context, there are no local administrative units or electoral districts between the allocating body and the spatial voter groups that constitute our unit of observation.

2. Institutional setting

2.1. Plan E

Plan E was announced in November 2008 by the Spanish centre-left national government of José Luis Rodríguez Zapatero.[5] It was a large stimulus plan aimed at boosting economic activity and fostering employment growth in the midst of the financial crisis. The plan was carried out in two parts, starting in 2009 with *FEIL*, which provided municipalities with roughly 8,000 million Euros, and following, in 2010, with the smaller *FEESL* program, accounting for over 4,000 millions Euros. There was an additional, yet much smaller plan affecting province level bodies called *CN* over this period. Funds from FEIL and FEESL made available to municipalities where determined by a strict per capita rule. In total, the *Plan E* transferred public funds to local government for about 0.8% of the 2009 Spanish GDP.

The actual investment and spending decisions were carried out by municipalities. Municipal governments would apply for funding of investment projects and these applications would be approved by the central government which would finance the spending. Over 99% of municipalities applied and received funding for investment projects, mostly for infrastructures, each of which could not exceed 5 million Euros (see Montolio 2016).[6] The near universal take up of the plan and anecdotal evidence from local politicians we have interviewed suggests the approval criteria were very lax and did not influence municipal decisions substantially.[7] The timing for the planning and execution of projects was very tight: after the Parliament approved the $FEIL$ package in the end of November 2008, municipalities had less than two months to present investment projects and were required to start the works at the latest in mid April.

[5]Formally, the name of the policy was *Plan Español para el Estímulo de la Economía y el Empleo* (Spanish Plan for Employment and Economic Stimulus).

[6]A total of 19 municipalities did not conduct *Plan E* projects. In all cases, these were part of a municipal association which itself allocated projects for municipal governments.

[7]A politician from the centre-right *Partido Popular*, talking about *Plan E* said: "It was an enormous grant, which many interpreted as a letter to the three kings". It is worth noting that the lax criteria of the national government in the approval of projects was motivated by its desire to initiate spending as fast as possible in the context of the economic stimulus program.

A total of 57,850 investment projects were carried out by municipal governments using *Plan E* funding between 2009 and 2011. The most common projects were those described as "rehabilitation and improvement of public spaces", which refers to refurbishment of parks, plazas and pedestrian walkways (see Figure B.7 in the appendix). The second most common type was "equipment and service infrastructure" which is a much more heterogeneous category encompassing street lighting, improvement of transport infrastructure, occasionally refurbishment of parks and sport facilities as well as water works. The average cost of each project was slightly above € 210,000, indicating small and middle-scale projects were common. *Plan E* endowments roughly tripled the pre-crisis amount of yearly municipal funds for municipal investments in Spain.

There are no rigorous analyses of the overall effectiveness of *Plan E* on the Spanish economy. A subsequent investigation by the Court of Auditors found that by 2011 only 4% of the employees who were hired specifically to work on *Plan E* projects were still working for the same firm after the program had ended. However, it is unclear whether this can be interpreted as indication of Plan E failing in its objective to increase demand and contain the economic contraction. *Plan E* data on spending at the municipal level has been recently used by Montolio (2016) to document short term effects of these funds on local level unemployment in Catalonian municipalities. It is important to emphasize that our paper does not evaluate *Plan E* in terms of its original objectives but rather uses the data generated by *Plan E* as an input to study distributive policies.

2.2. Municipalities and Local Elections

Spain had 8,116 municipalities in 2011. Municipalities are the lowest level of territorial administration of the Spanish state and have autonomy in managing their interests as recognized in the Spanish constitution. Their functions are partly dependent on size and encompass lighting, transport network upkeep, public parks, local services (e.g. sports facilities, public libraries), waste disposal, water and sewage services.[8] Municipal financing is based on municipal taxes (the largest of which are a property tax and a tax on firms) and transfers from the national and regional governments. Note that *Plan E* project financing was not part of these regular transfers.

The governing body is the municipal council and its members are directly elected by residents. Municipal elections are held every four years under a single-district, closed list, proportional electoral system.[9] Municipal council seats (from a minimum of three to a maximum of 57 in Madrid) are assigned following the D'Hondt rule. The single-district electoral rule is important for our analysis as it allows us to treat spatial units within the municipalities as

[8] See details in law number 7/1985 (2 of April 1985, *Ley reguladora de las bases del régimen local).*

[9] See Chapter IV of *Ley Orgánica del Régimen Electoral General.* Municipalities with populations under 250 inhabitants have an open list system with voters able to express multiple preferences for different candidates. These municipalities will not be used in our analysis.

voter groups rather than electoral districts. It also grounds the notion that all votes for a party contribute the same towards the goal of winning government (something that does not apply in multi-district constituencies). The municipal mayor is elected by the council under a majority rule and in general this majority is obtained through coalition building after elections. The council votes proposals by the mayor, who acts mainly as an the agenda-setter. Given the strong discipline enforced by parties in Spain and the impossibility of calling early elections, local governments are usually stable. Below, the *ruling party* refers to the party of the mayor.

For data collection and voting purposes, the National Statistical Institute (*INE*) divides the Spanish territory into roughly 35,000 electoral areas (also referred to as census areas) with no administrative powers. These areas are defined as a function of municipal boundaries and population. Census areas are the smallest spatial unit for which we can obtain electoral results from Ministry of Internal Affairs (*Ministerio del Interior*) and will constitute our main unit of analysis. Given that many municipalities are small, only 2,278 municipalities had more than one census area within their boundaries in 2007.

2.3. Political Parties in 2007 and 2011

The socialist party (*PSOE*) held the national government between 2004 and 2011 under two terms of President Zapatero. *Plan E* was formulated and executed under his presidency, in the context of the financial crisis, with increasing unemployment and a collapsing of construction sector. At the national level, the centre-right Popular Party (*PP*) was the main opposition party and would continue to take power from the socialists in 2012.

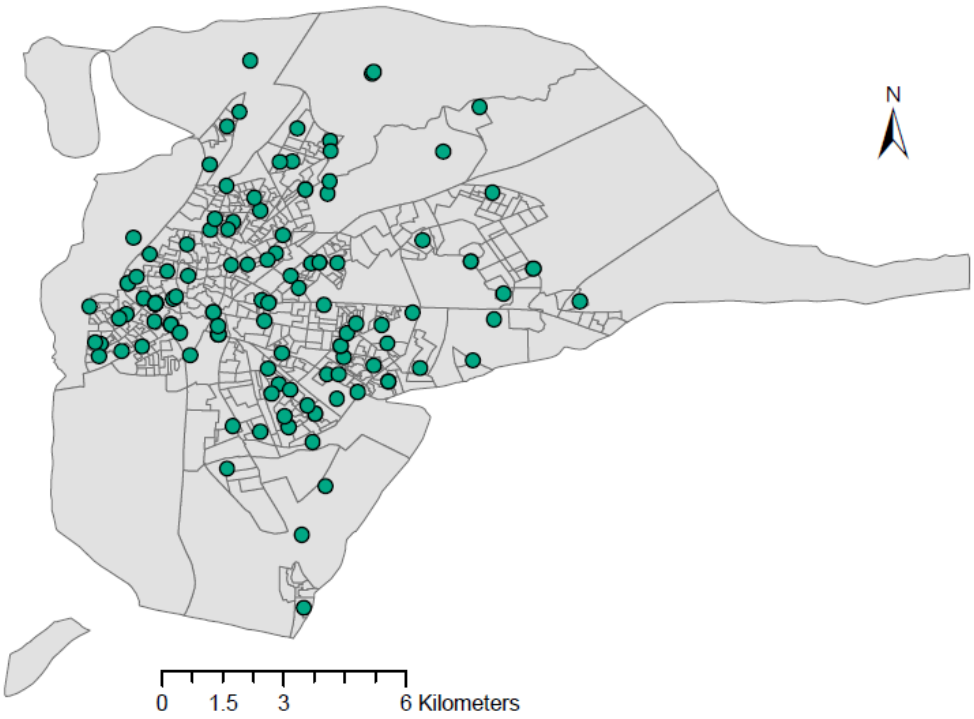
The municipal elections before and after *Plan E* took place in 2007 and 2011, respectively. In the 2007 election, the two main parties, Zapatero's *PSOE* and the centre-right *PP*, obtained comparable results. A total of 36% of municipalities were ruled by *PSOE* in 2007, while 39% were ruled by *PP*. In 2011, almost three years into the financial crisis, these figures changed to 27.5% and 46.6% respectively. In both terms, the third party with most appointed mayors was the nationalist Catalan party *Convergéncia i Unió* which ruled 5.2% and 6.3% of municipalities, respectively. A handful of smaller parties, either of national or regional scope, ruled most of the remaining municipalities.

3. Data and Descriptive statistics

In order to study the relationship between public spending and the geography of voter support we need disaggregated data on electoral outcomes and geo-located data on *Plan E* investment projects. Data on individual projects were obtained directly from the *Plan E* website, and include the coordinates of projects (as geo-located by the municipal authorities), a short description, a classification in terms of project types and the cost of each project. As an illustration of the spatial variation in the data, figure 1 shows the projects located in the municipality of Sevilla.

Figure 1

Plan E Projects: Sevilla



Notes: Points correspond to different *Plan E* projects located in the municipality of Sevilla in Andalucía. Polygons correspond to the different census areas comprising this municipality.

The raw data contain a total of 57,850 projects. Several of them corresponded to investment categories that clearly yield no differential geographical benefit to voters. For example, spending on technological upgrading of the public administration is usually assigned to the town hall but does not render benefits to people living next to the town hall. We identify and exclude a total of 6,574 projects which correspond to these categories.[10] In addition, for a subset of projects, the geo-location data on latitude and longitude is incorrect or missing. When possible, we located these projects manually using information from the short project description. In total, we were able to hand code 3,065 projects ourselves. Our final sample therefore contains a total of 38,353 projects (for details on these restrictions see table B.1 in the appendix).

Project types in this sample and their frequencies are displayed in table 1. We can see that the most common type of investments is related to rehabilitation of public space (an example of which is shown in figure B.7 in the appendix). Infrastructures related to basic and cultural services, with presumably localized benefits, are also frequent project types. We will further

[10]The categories in question are: technological upgrading of the public administration, electronic management, industrial rehabilitation, efficiency in the management of water sources, management and treatment of urban waste, repairs in water supply systems and repairs in sewage outlet systems.

explore the heterogeneity of the localized effects of different project types in section 6.

TABLE 1
DESCRIPTIVES - SUMMARY OF PROJECT TYPES

	N. of projects	Frequency
Rehabilitation of public space	7107	18.53
Basic services infrastructure	5924	15.45
Construction and improvement of social and cultural facilities	5819	15.17
Cultural and sport related buildings and equipment	3946	10.29
Energy efficiency and conservation	3813	9.94
Improvement in public spaces and road networks	2423	6.32
Social buildings and equipment	1718	4.48
Construction and upgrading of education centres	1385	3.61
Urban sustainability and pollution control	875	2.28
Promoting mobility and safety	853	2.22
Protection of historical and landscape heritage	767	2
Conservation of historical and municipal sites	569	1.48
Other	3154	8.22

Notes: Number and relative frequency for all the investment projects, by project type. Sample restricted to projects which have correct geocoding information. All municipalities.

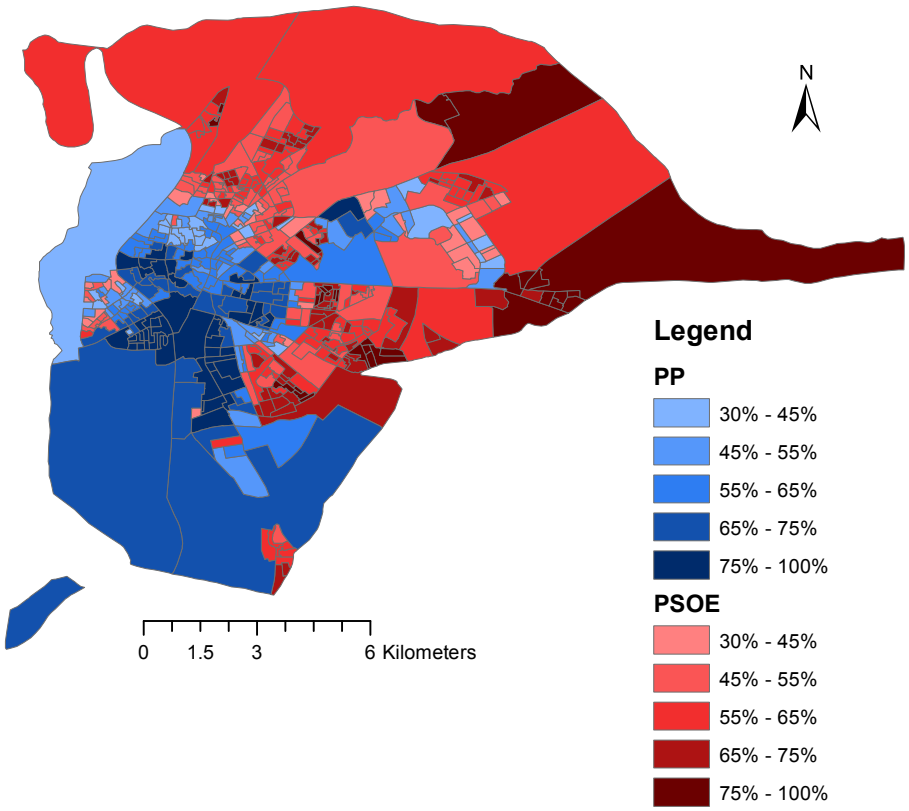
We combine information on *Plan E* investment projects with data on municipal and national elections. Data on electoral outcomes at the census area level are obtained from the Ministry of Internal Affairs, the body responsible for collecting and disseminating information on electoral results. We complement it with information on mayors and their political party of affiliation from the same source. Figure 2 plots results of the 2007 municipal elections for each of the 522 census areas of Sevilla. Red areas are those where left-wing *PSOE* obtained more than half of the votes while blue indicates area of *PP* majority. We can see that the support for both parties varies significantly across the city, with the city center being mostly a centre-right area. This within-city variation in electoral support will be instrumental to study the link between the geography of voter support and the allocation of *Plan E* projects in the following sections.

Furthermore, we integrate our dataset with information from the 2001 Population Census. Census data includes characteristics at the census areas level such as population, and density, together with the fractions of college graduates, unemployed, home-owners, foreigners and the number of elderlies and children. To control for possible factors affecting the local demand for investment, we also use information on the number of households that reported the presence of crime and a lack of green areas in the neighbourhood. Lastly, we also include the fraction of urban discontinuous terrain at the census area level (from Corine Land Cover).

We will limit our analysis to municipalities having at least two census areas in order to have variation in either party support or turnout within each municipality. This excludes small and very small towns, restricting our sample to 2,278 municipalities. We will further restrict our analysis to municipalities ruled in 2007 by one of the 9 national level parties with

Figure 2

2007 vote shares: Sevilla



Notes: Census areas shaded in blue are those in which *PP* (centre-right) was the most voted party in the 2007 municipal elections. Census areas shaded in red are those in which *PSOE* (centre-left) was the most voted party. Different shades indicate different vote shares as shown in the legend.

most mayors.[11] We impose this restriction in order to ensure we can correctly match the party names in the census area electoral data with those appearing in the data on mayors. We will show that our main results are robust to looking at municipalities ruled by PP or PSOE only (see section 6). Our final sample is composed of 2,047 municipalities.

Table 2 includes some descriptive statistics for our sample. As Panel A shows, census areas have an average surface area of about 8 squared kilometers, and about 1,100 eligible voters. Given that they are designed to contain comparable numbers of voters, there is substantial variation in their physical size, matching the variation in densities, from large cities with small census areas to sparsely populated and extended countryside villages with large ones. Panel B indicates that 40% of census areas received at least one project, with a corresponding average

[11]These are *PP*, *PSOE*, *CIU*, *IU*, *CC*, *ERC*, *PNV*, *PAR* and *BNG*. By national level parties we mean parties that also run in national elections.

investment per capita of 215 Euros. In the last panel of table 2 we also report some average figures from the 2001 Population Census variables that will be used as controls in our main specification.

TABLE 2
DESCRIPTIVES - CENSUS AREA LEVEL DATA

	Mean	Std. dev.	Min	Max
A. General information				
Surface (2007, km2)	8.41	34.69	0.004	1125.112
Density (2007, 1000 inh./km2)	19.86	21.55	0.001	349.804
Population (2007)	1,423	563.75	294	12,859
Eligible voters (2007)	1,100	441.03	226	10,881
Turnout (2007)	0.61	0.12	0.085	0.922
Turnout (2011)	0.62	0.10	0.157	1.000
B. Plan E projects				
Indicator for receiving 1+ projects	0.40	0.49	0.00	1.00
N. of projects received	0.91	1.72	0.00	49.00
Investment in projects (Euros per capita)	214.76	713.16	0.00	33420.26
C. Population Census information (2001)				
Higher education	0.12	0.10	0.000	0.556
Home owners	0.84	0.12	0.004	1.000
Foreigners	0.04	0.05	0.000	0.811
Households with 1+ unemployed	0.16	0.07	0.003	0.730
Households reporting not enough green areas	0.38	0.24	0.000	0.993
Households reporting crime is high	0.24	0.19	0.000	0.977
People 0-16 yrs.	0.15	0.05	0.031	0.394
People 16-64 yrs.	0.67	0.05	0.280	0.927
People 65+ yrs.	0.17	0.08	0.006	0.654
Observations		28,083		

Notes: Panel A reports national averages for some characteristics of interest for the 28,083 census areas in the sample (2,047 municipalities). Turnout figures refer to the 2007 and 2011 municipal elections, respectively. Panel B shows descriptives for the *Plan E* investment program, and panel C shows data from the 2001 Population Census. Figures represent the national average of the fraction of people, in given census area, with a particular characteristic at the time of the Census. In some categories – explicitly indicated – the unit of observation is the household and not the individual.

4. Distributive Policies

In this section we start by testing whether incumbent politicians target their core supporters in the allocation of public works. To this end, we use within-city variation in the location of projects with both OLS and a regression-discontinuity design. Then, we turn to the alternative mobilization hypothesis, according to which politicians target areas of low

turnout.[12]

4.1. Targeting Supporters

We want to test whether politicians use *Plan E* funds to target their supporters. At a first glance, the correlation between some measure of the incumbent's electoral support and investment could be interpreted as the relevant statistic to answer this question. However, giving this correlation a causal interpretation is problematic because of the likely omitted variable problem, which would arise in the presence of unobservable determinants of investment that are correlated with electoral support. For instance, if lower income areas both tend to vote left and to need more investment, a positive correlation between the incumbent's vote share and investment in areas ruled by left wing mayors could exist even if there is no tactical targeting of supporters.

We try to solve this identification problem in two ways. To start, we run a within-municipality regression of investment on the vote share of the incumbent – the variable we use to measure incumbent support – at the census area level, controlling for the vote share of all the largest parties. These vote shares serve as proxies for unobserved determinants of investment that are correlated with the support for these parties. As a second, complementary strategy, we aggregate data at the municipal level to implement a regression-discontinuity design (RDD). Since the ideal randomized experiment in which the location of voters is randomly assigned is unfeasible, we resort to using close elections to "randomize" the identity of the ruling party (see, e.g. Lee 2008; Imbens and Lemieux 2008). Given the distribution of support for parties within the city, this randomization allows us to know whether a given party favoured its areas of core support in the allocation of *Plan E* funds. In our RDD design, we study whether municipal governments of municipalities in which the left-wing party *PSOE* barely won tend to invest more in areas of left-wing core support than in municipalities where this party barely lost.

4.1.1. Within-City Regression Analysis

We start by using disaggregated data at the census-area level directly. To this end, we estimate the following model by OLS:

$$y_{cm} = \alpha_m + \beta VoteShareInc_{cm} + \sum_{p=1}^{P} \delta_p VoteShare_{p,cm} + \gamma' X_{cm} + \epsilon_{cm} \quad (1)$$

where y_{cm} is some measure of investment in census area c of municipality m and $VoteShareInc$ is the vote share of the incumbent's party, defined as the ruling party at the

[12] We have also attempted a test of the swing voter hypothesis in the spirit of Ansolabehere and Snyder (2006) and Wright (1974). We used the historical standard deviation of the incumbents' vote share as a proxy for the number of swing voters. It is unclear whether this variable appropriately measures swing voter presence. With this caveat in mind, our estimation results (not reported) lend no evidence in support of this hypothesis.

time of *Plan E* inception in late 2008. β is the coefficient of interest. A positive β implies that areas with relatively large support for the incumbent receive, on average, more investment. We also include a municipality fixed effect α_m to capture unobserved differences between municipalities. Additionally, we control for the vote shares of all the main parties – defined in section 3 – and for a set of census area characteristics, X_{cm}, which includes a quadratic in population and a series of variables from the 2001 Census.[13] Finally, the fraction of urban discontinuous terrain (from Corine Land Cover), distance form the urban centroid, surface (and its square) and the density of the census area are included to control for geographical characteristics.

Given that we introduce the vote shares of all major parties as controls (among which there is always the incumbent's party), identification of β comes from comparing how much voters of a given party are rewarded with investment when this party is in power and when it is not. Vote shares also serve as proxies for unobserved determinants of transfers that are correlated with the electoral preferences of voters. For instance, left-wing areas may receive more funds just because they also are areas with lower incomes. The identifying assumption, as usual, is that, conditional on all controls and municipal effects, the vote share of the incumbent is mean independent of the unobserved term ϵ_{cm}.

In table 3 we report estimates using three different measures of investment as dependent variables: a dummy for receiving at least one project, a variable that counts the number of projects received and, finally, the ratio of *Plan E* spending in a given census area over the municipal total. In addition to the municipal fixed effects, in column 1 we only include our vector of controls X. In column 2, instead, we only include the vote shares of the main parties as controls, whereas in column 3 we have both sets of controls. Results show that all coefficients are negative but very small in magnitude. Taking column 3 as our preferred specification, we see that an increase in the vote share of the incumbent by 10% is associated with a decrease in the probability of receiving a project of 0.16 percentage points. This coefficient is very small in magnitude and statistically insignificant. However, given the standard error of 0.03, and the corresponding 95% confidence interval of [-0.75,0.43] percentage points, this result is still informative as the evidence points strongly towards an effect that is very close to zero. Similar results are found when using alternative measures of investment as dependent variables, suggesting that, overall, this analysis lends little support to the hypothesis that local governments disproportionately target their supporters with investment.

[13] Specifically, in order to capture some of the differences across census areas, we add the number of unemployed, foreign residents, home owners, college educated, elderly, and children. To control for possible factors affecting the local demand for investment, we also control for the number of households that complained about the presence of crime and the lack of green areas in the area.

Table 3

Effect of incumbent's vote share on the propensity to invest

	(1) Project 1/0	(2) Project 1/0	(3) Project 1/0
Vote Share Inc. (2007)	-0.039	-0.024	-0.016
	(0.03)	(0.03)	(0.03)
Controls	FE + geo ctrls.	FE+ elect. ctrls.	FE+ full ctrls.
R^2	0.33	0.28	0.33
Observations	27892	27903	27892
	N. projects	N. projects	N. projects
Vote Share Inc. (2007)	-0.154	-0.059	-0.059
	(0.13)	(0.12)	(0.13)
Controls	FE + geo ctrls.	FE+ elect. ctrls.	FE+ full ctrls.
R^2	0.38	0.34	0.38
Observations	27892	27903	27892
	Inv. share	Inv. share	Inv. share
Vote Share Inc. (2007)	-0.005	-0.006	-0.002
	(0.01)	(0.01)	(0.01)
Controls	FE + geo ctrls.	FE+ elect. ctrls.	FE+ full ctrls.
R^2	0.45	0.44	0.45
Observations	27892	27903	27892

Notes: Municipality fixed effects are included in all columns. S.e. are clustered at the municipal level. As dependent variable we use, respectively: in the first panel, a dummy equal to one if the census area received at least one investment project; in the second, the number of investment projects; and, finally, in the third, the fraction of the *Plan E* municipal investment that goes to the census area. Electoral controls include the vote shares of all 9 major parties (see section 3).

4.1.2. Close elections regression-discontinuity design

Because OLS estimates may suffer from omitted variable bias, we also implement a close elections regression-discontinuity design (RDD). Specifically, we use the fact that elections decided by a narrow margin provide as-good-as-random variation in the identity of the ruling party in the municipality (see e.g. Lee 2008). The first step consists in aggregating the census area information into a measure of "supporter bias" at the municipal level. We consider two alternatives. The first measure we construct is meant to capture the *extensive* margin of investment, that is, whether areas with many supporters are more likely to receive a project on average. Let c index census areas and m municipalities, and N_m be the number of areas in municipality m. For each municipality, we first calculate the aggregate vote share of *PSOE*, the party that is ruling in the majority of municipalities in our sample, in areas that received

and did not receive a project as follows:

$$VoteShare_m^P = \frac{\sum_{c=1}^{N_m} VotesPSOE_{cm} \times P_{cm}}{\sum_{c=1}^{N_m} Votes_{cm} \times P_{cm}}$$
$$VoteShare_m^{NP} = \frac{\sum_{c=1}^{N_m} VotesPSOE_{cm} \times (1 - P_{cm})}{\sum_{c=1}^{N_m} Votes_{cm} \times (1 - P_{cm})},$$

where $VotesPSOE_{cm}$ is the number of votes obtained by $PSOE$ in census area c of municipality m, and P_{cm} is an indicator taking value 1 if the census area received at least one investment project.[14] Our extensive margin measure of core-voters bias in the allocation of investment projects is then constructed as the difference in the vote share of $PSOE$ in areas that received a project and in areas that did not:

$$ExtCoreBias_m = VoteShare_m^P - VoteShare_m^{NP} \tag{2}$$

This measure is straightforward to interpret. For example, a value of 0.05 indicates that the vote share of $PSOE$ was 5 percentage points larger in areas that received at least one project than in areas that received none.[15] Notice that this measure is defined for *all* municipalities, including those where the left-wing party is not in power. Therefore, even if parties favour their supporters in the allocation of projects, we should not expect any asymmetry in the unconditional distribution of the bias measure as right-wing governments favouring their voters would appear with negative values. In fact, this distribution, shown in figure B.5 in the appendix, is centred around zero and displays substantial variation across municipalities.

We then consider a second measure of partisan bias, called $IntCoreBias_m$, that captures both the extensive and the *intensive* margins, that is, the decision of how much to spend. To this end, we combine data on spending per project to information on project locations. Our measure is defined as the municipal level correlation coefficient between the census area vote share of the left-wing party and the fraction of total *Plan E* funding allocated to that census area. A high value of this measure in a municipalities ruled by the left means that left-wing incumbents tend to concentrate investment in areas where they have relatively many voters.

These two measures are used as outcome variables in a close election RDD to test whether left-wing incumbents favour their voters in the allocation of projects.[16] We will use the $PSOE$ victory margin over the second party (or the loss margin with respect to the most voted party

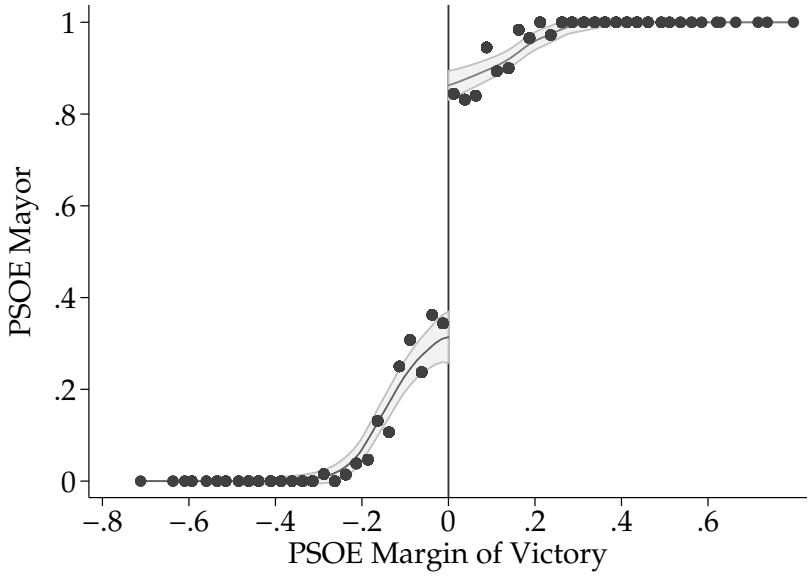
[14] One possibility would be to weigh each area by its population, to give more importance to areas with more voters. However, this has no effect on the results, perhaps because census areas are designed to have roughly comparable population. In fact, 99% of all census areas have a population between 500 and 2000.

[15] As a robustness check, we have also considered an alternative measure, defined as the ratio $VoteShare_m^T / VoteShare_m^{NT}$ instead of the difference. Results are similar and not reported.

[16] An alternative approach would be to implement the RD design without aggregating, followed by clustering standard errors at the municipal level in estimation. Results from this specification – and their interpretation – are analogous and are available upon request.

Figure 3

First Stage Discontinuity



Notes: The vertical axis measures the probability of having a *PSOE* mayor and horizontal axis measures the difference in vote shares obtained by *PSOE* in the 2007 municipal elections relative to the runner-up party if *PSOE* won the election (positive values) or the most voted party if *PSOE* lost the election (negative values). Solid lines represent fitted values from a local polynomial smooth regression estimated with an Epanechnikov kernel. Dashed lines correspond to 95% confidence intervals.

in case of defeat) as the running variable. Given that, under the Spanish electoral system, mayors are elected by the municipal council and not directly by voters, this is a fuzzy regression-discontinuity design (Imbens and Lemieux, 2008). The corresponding first-stage is as follows:

$$PSOE_m = \pi_0 + \pi_1 \mathbf{1}(VoteMarginPSOE_m > 0) + f(VoteMarginPSOE_m) + \gamma' X_m + u_m, \tag{3}$$

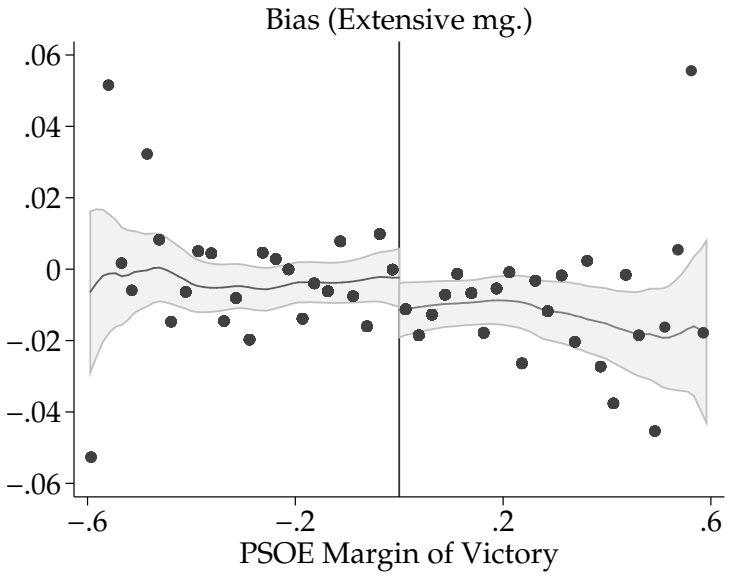
where $PSOE_m$ is a dummy taking value 1 if $PSOE$ is in power in the municipality by the time *Plan E* was carried out, $VoteMarginPSOE_m > 0$ is a dummy taking value 1 if *PSOE* was the most voted party in the 2007 municipal elections and $f(VoteMargin_m)$ is a polynomial in the vote margin. X_m is a vector of controls including the number of census areas, population, and the average census area density and surface. We will use a linear control function in our paper but results using second or third degree polynomials are analogous for all the bandwidths we considered. Figure 3 shows that there is indeed a large discontinuity in the probability of a *PSOE* government around the threshold. First-stage regressions using different bandwidths are provided in the appendix's table B.5 and confirm our instrument is strong in all cases.

Before moving to the second stage, we show a reduced form graph in figure 4, plotting our extensive bias measure against the $PSOE$ margin of victory using local polynomial smooth regressions on either side of the thresholds to fit the data. Local means calculated in 2.5% bins of the winning margin are presented as black dots. We can observe that there a is small

negative discontinuity in the bias measure once *PSOE* wins the election, suggesting that left-wing mayors do not systematically favour areas of core support. If anything, the sign of the jump suggests the opposite.

FIGURE 4

REDUCED FORM GRAPH FOR THE INTENSIVE MARGIN MEASURE



Notes: Vertical axis plots our extensive margin measure of core-support bias, $ExtCoreBias_m$, equal to the difference in $PSOE$ vote share between areas that received and not received projects. The horizontal axis measures the difference in vote shares obtained by *PSOE* in the 2007 municipal elections relative to the runner-up party if *PSOE* won the election (positive values) or the winning party if *PSOE* lost the election (negative values). Solid lines represent fitted values from a local polynomial smooth regression estimated with an Epanechnikov kernel independently on both sides of the threshold. Dots represent averages within intervals of 2.5% of the vote margin. Dashed lines correspond to 95% confidence intervals.

The second stage of the fuzzy RD design is given by:

$$Bias_m = \alpha + f(VoteMargin_m) + \delta PSOE_m + \gamma' X_m + \epsilon_m, \tag{4}$$

where the outcome variable $Bias_m$ can be either $ExtCoreBias_m$ or $IntCoreBias_m$ and vector X_m include controls as defined above. Results for IV estimates of δ for different bandwidths around the threshold value and for both measures are reported in Table 4.

Table 4

Effect of $PSOE$ victory on spending bias measures - Intensive and extensive margin

	bw=0.5	bw=0.25	bw=0.1	bw=0.05	bw=CCT
A. Supporter Bias - Extensive Margin					
PSOE mayor	-0.010	-0.020	-0.041	-0.019	-0.036
	(0.010)	(0.018)	(0.029)	(0.031)	(0.023)
Bandwidth	0.50	0.25	0.10	0.05	0.22
Observations	1304	886	394	199	791
	bw=0.5	bw=0.25	bw=0.1	bw=0.05	bw=CCT
B. Supporter Bias - Intensive Margin					
PSOE mayor	0.044	-0.199	-0.221	0.097	-0.316
	(0.150)	(0.256)	(0.394)	(0.483)	(0.301)
Bandwidth	0.50	0.25	0.10	0.05	0.18
Observations	1929	1320	589	300	1012

Notes: Robust s.e in parentheses. Controls included in all specifications. In the rightmost column the bandwidth is chosen automatically using the method by Calonico, Cattaneo and Titiunik (2014). The number of observations in panel A is lower for all bandwidths because municipalities in which either all or none of the census areas received a project are excluded from estimation.

Panel A presents estimates for the outcome variable $ExtCoreBias_m$. We observe that for different bandwidth values the coefficient on *PSOE* mayor is negative, as suggested by the graphical analysis, but it is not statistically significant. In all cases, the coefficient is also small; a value of -0.02 indicates that, when a municipality has a *PSOE* mayor, the areas receiving projects have, on average, a 2 percentage points *lower PSOE* vote share than those not receiving projects. Alternative specifications using different bandwidths or estimating the model using the data-driven bandwidth selector method by Calonico, Cattaneo and Titiunik (2014) lead to similar results.

Panel B presents estimates for the alternative outcome variable $IntCoreBias_m$, which incorporates both the intensive and extensive margins of investment. For ease of interpretation, the dependent variable is standardized to have mean zero and standard deviation one. The estimates continue to be negative and not significant, with the coefficient on the specification with the tightest bandwidth taking a value of 0.097, indicating that municipalities with *PSOE* mayors experience an increase in the correlation between *PSOE* vote shares and cost shares of roughly 0.1 of a standard deviation. Not only is this effect statistically insignificant, it is also fairly small. Estimates for other bandwidths are somewhat larger, although still not statistically significant. Taken together, these results complement the regression estimates and again provide no evidence of a supporter bias.

Both the OLS and RDD results are in contrast to the predictions of core voters models

such as Cox and McCubbins (1986), with the empirical evidence in their favour (see, e.g. Ansolabehere and Snyder 2006, Levitt and Snyder, Jr. 1995), and with papers finding a positive alignment effect across different levels of government. Given the previous literature, and the size and discretionary nature of *Plan E*, it is somewhat surprising to find no evidence of supporter bias in the data. However, as Cox (2009) and Golden and Min (2013) point out, using aggregated data may lead to misleading results. Given that these data are usually only available at the district or municipal level, a positive correlation between incumbent support and spending should not be interpreted as evidence in favour of the core voter hypothesis. Rather, it could support the quite different view that politicians target core *districts or municipalities*. Our results could be informative in this respect because, by using disaggregated infra-municipal data on the location of projects and voters, we are able to directly test for core supporter bias. The result that politicians do not target their supporters also sheds some light on the motives behind the alignment effect found in the literature, that is, the fact that national level politicians favour local administrations where their own party is in office. Our results suggest that this effect is more closely related to the incentives national politicians may have to favour local party members, such as following party guidelines, than to their intention to target their local supporters directly to gain votes.

4.2. Mobilization

We also explore whether politicians target investment to areas of low turnout in order to mobilize potential voters (Cox, 2009). By targeting low participation areas with investment projects, parties can induce unmobilized supporters to show up at the polls. This strategy will have an effect if localized investment leads voters to change their beliefs about both the importance of their constituency in the politicians' agenda and, more generally, the potential effect of their vote on distributive politics.[17] We begin by studying how turnout affects investment decisions estimating the following model:

$$y_{cm} = \alpha_m + \beta Turnout_{cm} + \sum_{p=1}^{P} \delta_p VoteShare_{p,cm} + \gamma' X_{cm} + \epsilon_{cm} \tag{5}$$

where, as before, y is either a dummy for having received at least one project, the number of projects received, or the share of municipal investment that is destined to census area c in municipality m. The coefficient of $Turnout$ – the turnout in the 2007 municipal election – is of interest as it measures the marginal effect of turnout on spending. As table 5 shows, we observe a strong negative correlation between turnout and investment. This shows local governments target areas with low electoral participation with additional investment projects.

[17] These campaigns may have undesirable effects if they mobilize people who support the opponent. Modern electoral campaigns consider both persuasion and mobilization as a joint objective and are increasingly effective (Nickerson and Rogers, 2014).

Some care is needed, however, when giving those results a causal interpretation, as there may be omitted variables, not controlled for by the fixed effect, that are correlated with turnout. As an example, if areas with lower income tend to have low turnout and require more investments, turnout may capture part of this effect and estimates would be negatively biased.

TABLE 5

THE EFFECT OF TURNOUT ON THE PROPENSITY TO INVEST

	(1) Project 1/0	(2) Project 1/0	(3) Project 1/0
Turnout 2007	-0.136***	-0.223***	-0.141***
	(0.04)	(0.05)	(0.05)
Controls	FE + geo ctrls.	FE+ elect. ctrls.	FE+ full ctrls.
R^2	0.33	0.28	0.33
Observations	27892	27903	27892
	N. projects	N. projects	N. projects
Turnout 2007	-0.848***	-1.048***	-0.870***
	(0.26)	(0.23)	(0.25)
Controls	FE + geo ctrls.	FE+ elect. ctrls.	FE+ full ctrls.
R^2	0.38	0.34	0.38
Observations	27892	27903	27892
	Inv. share	Inv. share	Inv. share
Turnout 2007	-0.104***	-0.111***	-0.107***
	(0.02)	(0.02)	(0.02)
Controls	FE + geo ctrls.	FE+ elect. ctrls.	FE+ full ctrls.
R^2	0.45	0.44	0.45
Observations	27892	27903	27892

Notes: Municipality fixed effects are included in all columns. S.e. are clustered at the municipal level. As dependent variable we use, respectively: in the first panel, a dummy equal to one if the census area received at least one investment project; in the second, the number of investment projects; and, finally, in the third, the fraction of the *Plan E* municipal investment that goes to the census area.

Keeping this in mind, results suggest that politicians tend to target areas of low turnout. As the most restrictive specification of column 3 shows, a 1 percentage point increase in turnout is associated with a 0.14 percentage points decrease in the probability of receiving a project. This effect is not negligible: a standard deviation increase in turnout (about 12 percentage points) corresponds to a decrease in the probability of receiving a project of approximately 1.7 percentage points.

5. Electoral Effects of Public Expenditures

After having considered the electoral determinants of investment, a natural question is whether the strategic distribution of investment yields any electoral rewards for the incum-

bent party in the following municipal elections. Specifically, we will study whether areas that received at least one investment project reward the incumbent, either with votes or with an increased turnout.

5.1. Effects on the Incumbent's Vote Share

In order to estimate the effect of *Plan E* investment on the vote share obtained in 2011 by the 2007 incumbent party we consider the model:

$$VoteShare_{cm}^{2011} = \alpha_m + \beta y_{cm} + \lambda VoteShare_{cm}^{2007} + \sum_{p=1}^{P} \delta_p VoteShare_{p,cm} + \gamma' X_{cm} + \epsilon_{cm} \quad (6)$$

where y_{cm} again measures investment in census area c of municipality m. We include the same controls as those present in section 4 plus the vote share obtained by the incumbent in 2007 and municipal fixed effects when indicated. The coefficient of interest is β and can be interpreted as the effect of investment on the 2011 election's vote share of the party that was in power in 2007, conditional on the vote share they received in 2007. In this way, we are measuring the increase in the electoral support for the incumbent's party due to investment.[18]

Table 6 shows the results from estimating equation 6. The effect of investment projects on the incumbent's vote share in the next election is, in general, positive, but extremely small and statistically insignificant in all specifications. To put things into perspective, the third coefficient in the first row of the table indicates that receiving a project decreases the vote share of the incumbent by less than 0.01 percentage points. Varying the selection of controls and fixed effects, or the measure of *Plan E* investment, does not affect the main message: there appears to be no effect of investment projects on incumbents' vote shares within a census area. One possible explanation for this is that our way to measure investment's effect on voters is inadequate. This would happen if, for instance, the benefits of receiving an investment project were diffuse to voters other than those living in the immediate proximities. In section 6 we will consider alternative specifications to ensure that results are robust to more general ways to define the investment variables.

5.2. Effects on Turnout

Finally, we turn to study the effect of investment on mobilization. In section 4.2, we showed that projects where allocated to areas of low turnout within municipalities. However, turnout is potentially correlated with many other factors (e.g. income) which in turn

[18] A related way to estimate β would be to regress the difference in vote shares ($VoteShare^{2011} - VoteShare^{2007}$) on y_{cm} and controls. This procedure is similar in spirit to the first difference estimator used in panel data, but unnecessarily imposes the restriction $\lambda = 1$. We have also considered an alternative panel specification with census area fixed effects which leads to similar results.

Table 6

Effect of investment projects on next elections' results

	(1) Vote sh. in 2011	(2) Vote sh. in 2011	(3) Vote sh. in 2011
Project 1/0	0.00005	-0.00041	-0.00004
	(0.0007)	(0.0008)	(0.0007)
Controls	FE + geo ctrls.	FE+ elect. ctrls.	FE+ full ctrls.
R^2	0.94	0.94	0.94
Observations	27880	27891	27880
	Vote sh. in 2011	Vote sh. in 2011	Vote sh. in 2011
N. projects	0.00024	0.00013	0.00024
	(0.0002)	(0.0002)	(0.0002)
Controls	FE + geo ctrls.	FE+ elect. ctrls.	FE+ full ctrls.
R^2	0.94	0.94	0.94
Observations	27880	27891	27880
	Vote sh. in 2011	Vote sh. in 2011	Vote sh. in 2011
Inv. share	0.00132	-0.00009	0.00120
	(0.0019)	(0.0019)	(0.0019)
Controls	FE + geo ctrls.	FE+ elect. ctrls.	FE+ full ctrls.
R^2	0.94	0.94	0.94
Observations	27880	27891	27880

Notes: Municipality fixed effects are included in all columns. S.e. are clustered at the municipal level. As dependent variable we use, respectively: in the first panel, a dummy equal to one if the census area received at least one investment project; in the second, the number of investment projects; and, finally, in the third, the fraction of the *Plan E* municipal investment that goes to the census area.

could affect demand for public investment. Given this, one must be careful when interpreting those results as evidence that politicians use investment to mobilize voters. That being said, the mobilization hypothesis has one additional testable implication we can take to the data. If mayors use the investment projects to foster mobilization, and this strategy were effective, we should observe that, conditional on initial turnout, areas receiving projects exhibit higher turnout in the following election. To study the effect of investment on 2011 turnout, we estimate

$$Turnout_{cm}^{2011} = \alpha_m + \beta y_{cm} + \lambda Turnout_{cm}^{2007} + \sum_{p=1}^{P} \delta_p VoteShare_{p,cm} + \gamma' X_{cm} + \epsilon_{cm}, \quad (7)$$

where $Turnout_{cm}^{2011}$ and $Turnout_{cm}^{2007}$ are measures of turnout in 2011 and 2007 for census area c and municipality m, and y_{cm}, $VoteShare_{p,cm}$, and X_{cm} are defined as above. Results in table 7 show that areas that received at least one project have 0.39 percentage points addi-

tional turnout in 2011 with respect to 2007. The effect is small but consistently positive across specifications and for different measures of y_{cm}, and it is statistically significant at the 1% level in all cases.

TABLE 7

EFFECT OF INVESTMENT PROJECTS ON 2011 TURNOUT

	(1) Turnout 2011	(2) Turnout 2011	(3) Turnout 2011
Project 1/0	0.0040***	0.0023***	0.0039***
	(0.0007)	(0.0007)	(0.0007)
Controls	FE + geo ctrls.	FE+ elect. ctrls.	FE+ full ctrls.
R^2	0.88	0.86	0.89
Observations	27880	27891	27880
	Turnout 2011	Turnout 2011	Turnout 2011
N. projects	0.0013***	0.0008***	0.0012***
	(0.0002)	(0.0002)	(0.0002)
Controls	FE + geo ctrls.	FE+ elect. ctrls.	FE+ full ctrls.
R^2	0.88	0.86	0.89
Observations	27880	27891	27880
	Turnout 2011	Turnout 2011	Turnout 2011
Inv. share	0.0072***	0.0064***	0.0067***
	(0.0020)	(0.0021)	(0.0020)
Controls	FE + geo ctrls.	FE+ elect. ctrls.	FE+ full ctrls.
R^2	0.88	0.86	0.89
Observations	27880	27891	27880

Notes: Municipality fixed effects are included in all columns. S.e. are clustered at the municipal level. As dependent variable we use, respectively: in the first panel, a dummy equal to one if the census area received at least one investment project; in the second, the number of investment projects; and, finally, in the third, the fraction of the *Plan E* municipal investment that goes to the census area.

However, finding that the effect of turnout is small on average does not mean that it is so for all census areas. To explore one dimension in which this heterogeneity might be relevant, we introduce in the model interactions between our *Plan E* project measures (dummy, number of projects and cost share) and turnout in 2007. Table 8 presents the estimates obtained once these interactions are included. For ease of interpretation, we have demeaned the variable $Turnout_{cm}^{2007}$, so that we can interpret the coefficient on y_{cm} as the marginal effect evaluated at the sample average of $Turnout_{cm}^{2007}$. The negative coefficient of the interaction term indicates that the effect of investment is greater in areas of low turnout, showing that the largest electoral response of spending is found in areas of low participation. Specifically, looking at column 1, if turnout is 20 percentage points below the mean, the average effect of a project is to increase turnout in the following election by $.0037 - .02 * (-.2) = 0.008$, that

is, 0.8 percentage points. When looking at the amount of investment, however, the effect is stronger. An increase of the share of investment going to a particular census area by 20 percentage points above the mean is associated with a 5 percentage points increase in turnout in the following election. These results suggest that voters respond to localized policies by increasing their involvement in local matters. However, they appear to do so through electoral participation rather than by rewarding the politician responsible for the project.

But why would politicians try to mobilize voters? A possible explanation is that they believe they will be able to mobilize voters who disproportionately vote for their party. To test this hypothesis we augment model 6 with turnout in 2007 and its interaction with the investment variable y_{cm}. In this way we aim to capture whether investment affects the incumbent's vote share differentially depending on turnout. In all resulting estimates (unreported), the coefficient of the interaction term is small and statistically insignificant, thus providing no evidence in favour of this mechanism.

TABLE 8

EFFECT OF INVESTMENT PROJECTS ON 2011 TURNOUT - INTERACTIONS

	Turnout 2011	Turnout 2011	Turnout 2011
Project 1/0	0.0037***		
	(0.001)		
Proj.1/0 × Turnout 2007	-0.0203		
	(0.015)		
N. projects		0.0013***	
		(0.000)	
N.Proj. × Turnout 2007		-0.0062**	
		(0.003)	
Inv. share			0.0189***
			(0.004)
Inv. share × Turnout 2007			-0.1671***
			(0.031)
Turnout 2007	0.3034***	0.3018***	0.3084***
	(0.026)	(0.023)	(0.024)
Controls	FE+ full ctrls.	FE+ full ctrls.	FE+ full ctrls.
R^2	0.89	0.89	0.89
Observations	27880	27880	27880

Notes: Municipality fixed effects and controls are included in all columns. S.e. are clustered at the municipal level. The dependent variable is 2011 turnout in all specifications. The variable $Turnout_{cm}^{2007}$ is demeaned, so that that the baseline coefficient of each of the investment variables can be interpreted as the marginal effect evaluated at the sample average of $Turnout_{cm}^{2007}$.

hence providing evidence of the use and efcacy of municipal policies as electoral tools to increase participation

Our findings on mobilization effects serve two purposes. First, they inform the literature on turnout buying by providing evidence of the use and efficacy of municipal policies as electoral tools to increase participation (Nichter 2008; Chen 2013), something that had not been

explored before. Secondly, they show that investment projects do lead to an active response by voters even if not through favouring the incumbent party. Also, by showing a response in the polls, this evidence indirectly supports our claim that projects indeed have localized effects.

6. Additional Specifications and Robustness checks

6.1. Projects with Localized Benefits

As shown in section 4, our different estimates show incumbents do not target their supporters in the allocation of *Plan E* projects. However, this result implicitly rests on the assumption that the benefits of receiving a *Plan E* project are limited to the census area that receives it, hence ruling out spillovers to neighbouring areas. This is the direct consequence of how we constructed our investment variables. However, it seems reasonable to believe that at least some kinds of investments – such as gymnasiums or cultural centers – could provide local services that are enjoyed by a larger constituency. Other investments, like a new road, might even benefit other municipalities.

To ensure that our results are robust to other assumptions on how dispersed these benefits are, we perform our analysis again by restricting our attention to types of project for which benefits can be considered to be "broadly", "narrowly", or "very narrowly" localized. To classify projects into these categories we use the project type descriptions included in the original *Plan E* data source.[19] Although these definitions are somewhat arbitrary, some guidance on the selection of projects with localized benefits is offered by the literature of political budget cycles, which suggests that spending in parks and roads is very prone to strategic manipulation because of its visibility among voters (see for example Drazen and Eslava 2010 and Repetto 2016).

In table B.6 of the appendix we report the effect of the incumbent's vote share on, respectively, the probability of receiving a project, the number of projects and the cost share of projects received in each census area (the equivalent to our baseline table 3). Estimates are provided restricting the sample to broadly, narrowly and very narrowly localized project types as defined above. Results are consistent across specifications and confirm that politicians do not target their supporters in the allocation of *Plan E* funding. Table B.7 turns to study the effect of these types of projects on the next election's turnout and again confirms the results we presented in our main analysis. Projects with localized benefits increase turnout and this

[19] In our definition of very narrowly localized benefits we restrict our attention to projects relative to i) rehabilitation of public space and ii) improvement in public spaces and road networks. In our definition of narrowly localized benefits we additionally incorporate iii) conservation of historical and municipal sites and iv) protection of historical and landscape heritage. Finally, in our definition of broadly localized benefits we add those relative to v) basic service infrastructures, vi) cultural and sports related buildings and equipment, vii) promoting mobility and safety, viii) urban sustainability and pollution control, ix) construction and upgrading of education centres and x) construction and improvement of social and cultural facilities.

effect does not depend on the measure of projects used and is fairly robust to the restriction applied to project types. Estimates from the same analysis applied to equations 5 and 6 are also in line with the baseline results and are omitted.

Another possibility to mitigate concerns on the dependence of our results on the way the investment variables are defined is offered by the use of buffers. A buffer is simply an area – in our case a circle – constructed, in the map, around each investment project point. This procedure incorporates into our analysis the fact that the localized effects of public works in all likelihood extend beyond the boundaries of the census area in which each project is located. By increasing the radius of these buffers, we are assuming that the effects of investment are more and more disperse, as more census areas will be affected. Appendix's tables B.10 and B.11 show that, when using buffers of radius 25 or 50 meters, results are qualitatively very similar to our baseline results. We have also used 100 meter buffers and obtained qualitatively analogous estimates (not reported).

Taken together, these results confirm that politicians distribute investment in a way that is completely unrelated to the location of their supporters. However, this does not mean that the geographical distribution of resources follows no criteria. For example, by inspecting table B.3, which shows the complete results from our baseline equation 1, we see that there is a strong positive correlation between population and the probability of receiving a project.[20] This suggests that politicians might be targeting more populated areas, where potentially the demand for public investment is higher and projects have more exposure.

6.2. Using matching as an alternative to regression

Census areas that receive investment projects may differ in several aspects from those that do not. To begin with, areas in smaller cities are mechanically more likely to be "treated" with a project for the mere fact that these cities contain few census areas. As a consequence, treated areas will disproportionately be in small, scarcely populated municipalities. When the distribution of covariates in the treated and control groups differs substantially, regression models tend to rely too much on extrapolation and give biased results if the linearity assumption is not satisfied. To address this problem, in this section we draw from the literature on treatment effects to re-estimate some of the models used in the previous sections using different methods which are robust to covariate imbalances. In particular, we reconsider models 6 and 7, where we considered the electoral effects of expenditures. In order to have a binary treatment, we use the dummy for receiving at least one project as the treatment variable.

To start with, we estimate the propensity score - defined as the probability of receiving an investment project conditional on a set of covariates - using a logit model. Specifically,

[20] The effect is marginally decreasing as the coefficient of the squared population is negative and significant, but continues to be positive even in the 99^{th} percentile of the census area population distribution ($0.164 - 2 \times 0.012 \times 3.004 = 0.0929$). The raw correlation between population and the number of projects received in a census area is 0.16.

for model 6, we calculate the propensity score using a subset of the controls in equations 6 and 7: population, surface, density, the 2001 Census demographic indicators, the vote shares of all major parties, plus the vote share of the incumbent in 2007.[21] One way to measure the imbalance between the covariates in the two groups is to calculate, for each covariate k, the normalized difference, defined as

$$\Delta_k = \frac{\bar{X}_{T,k} - \bar{X}_{C,k}}{\sqrt{(S^2_{T,k} - S^2_{C,k})/2}},$$

where the numerator is the difference between the sample means of treated and control units while the terms in the denominators are the corresponding sample variances (Imbens and Wooldridge, 2009).

In table 9 we see that in the original sample those differences exceed, in a few cases, the recommended value of 0.25 (Imbens and Wooldridge, 2009), suggesting imperfect overlap. For example, areas that receive a project are substantially larger and less densely populated, on average, than those that do not. After trimming the sample by dropping observations with extreme values of the propensity score, the balance improves for some variables (see column 2).[22] Results improve by trimming the sample further, as shown in the rightmost column of table 9. This result motivates us to use the interval $[0.3, 0.7]$, rather than the optimally calculated boundaries, as our trimmed sample in the following. The table of normalized differences for model 7 is very similar and hence omitted.

In column 2 of table 10 we see that the OLS estimate of the effect of a project on the next election's vote share is reduced when trimming the sample by keeping only census areas with propensity score between 0.3 and 0.7, and is still insignificantly different from zero. The same message is confirmed by using propensity score matching on the trimmed sample (column 3), nearest neighbour matching (Abadie and Imbens, 2006) using the full sample (column 4) or requiring neighbours to be from the same province (column 5) or municipality (column 6, sample is restricted to large municipalities with at least 50 census areas).[23]

For what concerns the effects on turnout, we see from table 11 that the positive effect we found in section 5 is robust. Using the trimmed sample or variations of nearest-neighbour matching yield coefficients that are still significant albeit somewhat smaller. The results in this subsection suggest that, overall, the OLS estimates we obtained in our main specifications are not excessively sensitive to the choice of a linear functional form.

[21] The use of only a subset of covariates is necessary in order to avoid the curse of dimensionality problem in the subsequent analysis using nearest neighbour matching.

[22] The optimal boundaries for trimming are obtained using Crump et al. (2009)'s procedure.

[23] All estimations are performed using Stata 14's command *teffects*. The nearest neighbour specifications use the bias adjustment correction, based on all the covariates used for matching, suggested by Abadie and Imbens (2006). Notice that the number of observations varies because when the algorithm fails to find an appropriate match for a given census area, this is dropped from estimation.

Table 9

Normalized differences for each covariates - vote shares equation

	Original sample	.11<e(X)<.89	.3<e(X)<.7
Population (2007)	0.349	0.275	0.054
Surface (c.area)	0.338	0.371	0.254
Density, 1000 inh/km2 (c.area)	-0.918	-0.802	-0.418
Vote share of PP (in c.area)	-0.197	-0.183	-0.046
Vote share of PSOE (in c.area)	0.069	0.071	0.010
Vote share of CIU (in c.area)	-0.014	0.017	0.044
Vote share of IU (in c.area)	-0.005	0.003	-0.013
Vote share of PNV (in c.area)	-0.065	-0.049	-0.041
Vote share of BNG (in c.area)	0.098	0.075	0.036
Vote share of CC (in c.area)	0.048	0.027	0.019
Vote share of ERC (in c.area)	0.033	0.059	0.043
Vote share of PAR (in c.area)	0.067	0.042	-0.006
Fraction of HH reporting high crime	-0.485	-0.409	-0.208
Fraction of HH with 1+ unemployed	0.029	0.014	-0.033
Fraction of HH declaring not enough green	-0.013	-0.005	-0.015
Fraction of home owners	0.061	0.035	0.003
Fraction of foreigners	-0.132	-0.069	-0.004
Fraction of people 0-16 yrs.	0.322	0.209	0.000
Fraction of people 65+ yrs.	-0.044	0.001	0.105
Fraction of ref. persons with higher educ.	-0.373	-0.321	-0.126
Vote share incumbent (2007)	-0.011	0.001	-0.019
Observations	28083	24187	14830

Notes: For each element of the covariates vector X, the normalized difference is defined as the difference between the average in the treatment and control group, rescaled by the square root of the average difference of the sample variances:

$$\frac{\bar{X}_{T,k} - \bar{X}_{C,k}}{\sqrt{(S^2_{T,k} - S^2_{C,k})/2}}$$

In the second column one can notice the improvement in balance when using the trimmed sample restricted to observations with propensity score $e(X)$ between 0.103 and 0.897 (see Crump et al. 2009 for details on how to calculate these bounds). In the third column the trimmed sample is manually set to $0.3 < e(X) < 0.7$.

6.3. Other robustness checks

In table B.8 we estimate models 1, 5, 6 and 7 by restricting the sample to province capitals. All qualitative results are preserved. As an additional robustness check we restrict our sample to municipalities ruled by either a *PSOE* or a *PP* mayor. Table B.9 is in line with our baseline estimates, suggesting that our results are not driven by municipalities ruled by smaller or occasionally "fringe" parties. Finally, we consider an alternative to specification 1 in which we use the incumbent's vote share averaged over the 2008, 2007, 2004 and 2003 national and municipal elections. Estimates are reported in table B.2 in the appendix show our main results are not sensitive to the way we measure political support.

Table 10

Electoral effects of investment on 2011 vote shares - alternative estimation procedures

	(1) OLS	(2) OLS (trim)	(3) PS (trim)	(4) NN	(5) NN-prov.	(6) NN-munic.
Project 1/0	0.0028	0.00100	-0.0020	0.0018	0.00038	0.000042
	(0.0025)	(0.0026)	(0.0024)	(0.0013)	(0.0011)	(0.0013)
Vote share inc.(2007)	0.76***	0.74***				
	(0.0270)	(0.0262)				
R^2	0.63	0.58				
Observations	27880	14830	14830	27880	27880	14735

Notes: The dependent variable is the vote share of the 2007 municipal incumbent in the subsequent 2011 election. Column 1 replicates the first column of table 6 as reference. The second column uses the same specification but using a trimmed sample for which the propensity score is between 0.3 and 0.7. Column 3 reports results for a propensity-score matching model using the same trimmed sample. Columns 4 to 6 all use the nearest neighbour matching method of Abadie and Imbens (2006) with one neighbour, respectively, using the full sample (column 4), requiring matches to be from the same province (5) or the same municipality (6). Standard errors are clustered at the municipality level in columns 1-2 and heteroskedasticity-robust in the others.

Table 11

Electoral effects of investment on 2011 turnout - alternative estimation procedures

	(1) OLS	(2) OLS (trim)	(3) PS (trim)	(4) NN	(5) NN-prov.	(6) NN-munic.
Project 1/0	0.012***	0.0099***	0.0100***	0.0050***	0.0043***	0.0018*
	(0.0012)	(0.0012)	(0.0014)	(0.0007)	(0.0007)	(0.0010)
Turnout 2007	0.56***	0.59***				
	(0.0252)	(0.0146)				
R^2	0.74	0.73				
Observations	27880	14656	14656	27880	27880	14735

Notes: The dependent variable is the turnout in the 2011 municipal election. Column 1 replicates the first column of table 6 as reference. The second column uses the same specification but using a trimmed sample for which the propensity score is between 0.3 and 0.7. Column 3 reports results for a propensity-score matching model using the same trimmed sample. Columns 4 to 6 all use the nearest neighbour matching method of Abadie and Imbens (2006) with one neighbour, respectively, using the full sample (column 4), requiring matches to be from the same province (5) or the same municipality (6). Standard errors are clustered at the municipality level in columns 1-2 and heteroskedasticity-robust in the others.

7. Conclusions

In 2009 and 2010, the Spanish government carried out a large scale stimulus program that transferred €12 billion to municipalities to increase infrastructure spending and foster employment. This policy provides an ideal setting to analyse how local politicians distribute resources to voters within cities. In the first place, the allocation of funds to municipalities was made on a per-capita basis, mitigating concerns about endogenous take up. Secondly,

municipalities enjoyed substantial discretion in deciding how and where to allocate investment. And finally, investment projects were geo-located, enabling us to assign each of them to finely disaggregated electoral areas.

We find that the spatial allocation of spending within municipalities is not affected by the geography of political support, as shown by estimates from census-area level regressions, RDD and matching methods. However, we show that investment goes disproportionately to areas of low turnout, suggesting that politicians use funds to increase participation. Data on subsequent electoral results confirm this hypothesis, by providing evidence that voters respond to local spending by increasing turnout. This increase in participation is found to be especially significant in areas with low initial turnout.

Our results contribute to the still very incomplete understanding of how electoral conditions and incentives shape the allocation of investments within the city. Despite the importance of local investment on national spending, the lack of data at a sufficiently disaggregated level has hampered the study of these issues. The previous evidence of tactical distribution of resources between different levels of government does not hold when looking at the distribution of these resources to groups of voters. The channel through which distributive policies affect electoral outcomes operates by changing voters' participation decision and not their attitude towards the incumbent. We interpret this as a signal that agents will become more sensitive to the local level political agenda when observing the actual policies taking place. This suggests that localized investment can change the perception of voters about how much they can influence politicians' allocations, thus generating a reaction in the polls. Our paper makes a first attempt at uncovering how distributive politics operate at the very local level. Further understanding of the determinants of local level investment decisions and their electoral effects remains an open topic for future research.

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Online Appendix - not for publication

Appendix A. Data sources

Municipality codes and names; population

To have a reliable list of municipality names and official municipality and province codes, we use the *INE* list of all Spanish municipalities for the years 2001-2011.

URL: http://www.ine.es/daco/daco42/codmun/codmun11/11codmunmapa.htm

Population data at the municipal level is available from 1996 onwards at the *Padrón continuo municipal de habitantes*:

URL: http://www.ine.es/dynt3/inebase/es/index.html?padre=517&dh=1

Census area maps (shapefiles)

To be able to connect electoral data (available at the census area level) and the investment projects (geolocated with latitude and longitude), we need map shapefiles with the borders of each census area. Since boundaries usually change over time, we need to have updated maps for each municipal election year (2007 and 2011) before and after the *Plan E* investment program. The 2011 shapefile with all census areas is freely available at the following url:

URL: http://www.ine.es/censos2011_datos/cen11_datos_resultados_seccen.htm

We also purchased the map for 2007 directly from *INE*, which we use as a reference in all the empirical analysis. In order to be able to assign the data from the 2001 Census to the 2007 census areas, we also bought the shapefile map for 2001 from the same source.

***Plan E* Investment data**

The Plan-E data on investment projects with geographical coordinates and amount were downloaded by hand from:

URL: http://www.seap.minhap.gob.es/fondosinversionlocal/utilidades/geolocalizacion-de-proyectos.html

and saved as a .csv directly. Each investment project comes with, among other variables, the geographical coordinates. Those coordinates are then used, together with the 2007 shapefile, in ArcGIS to overlay the investment project data (as a "point layer") with the census area polygons as described below.

Electoral data

The electoral data are at the "Mesa" level (= polling stations, within-municipality) for several municipal and national elections. Data are then aggregated at the census area level in order to have the same level of aggregation of the shapefiles and the Census data. Each census area usually contains just a handful of polling stations, so the amount of aggregation is minimal.

URL: http://www.infoelectoral.interior.es/min/areaDescarga.html?method=search

There, we downloaded electoral results for the 1999, 2003, 2007 and 2011 municipal election, plus the 2000, 2004 and 2008 national elections, both at the disaggregated "mesa" level and at the municipal level.

Those data lack information on the identity of the mayor in each year and on the corresponding party in power, so we obtain such information using data from

URL: http://www.seap.minhap.gob.es/en/web/areas/politica_local/sistema_de_informacion_local_-SIL-/datos_legislaturas_1979_2015.html

Census area data We downloaded some variables, used as controls in the empirical analysis, from the 2001 Population census directly from the *INE* website:

URL: http://www.ine.es/censo/es/inicio.jsp

We obtain population data at the census area level for 2007 and 2011 from:

URL: http://www.ine.es/jaxi/menu.do?type=pcaxis&path=/t20/e245/&file=inebase&L=0

Finally, we obtain information from Corine Land cover on the fraction of urban discontinuous terrain.

URL: http://www.eea.europa.eu/data-and-maps/data/clc-2006-vector-data-version-6

Data restrictions

Our final dataset is based on the 2007 census areas for which we have information on the geographical boundaries (and coordinates). These areas are those that appear in the 2007 shapefile from *INE*. Of the 35,323 census areas in this dataset, we drop 5,833 belonging to municipalities with only one census area. Then, we drop municipalities where the mayor does not belong to any of the 9 largest parties, as defined in section 3 in the main text. By doing this, we lose 1,405 additional areas. Finally, we drop the municipality of “"Mañón”, because there is a conflict between the map – which has it divided in two census areas – and the electoral data, where it appears as having just one.

Regarding the projects data, the original data has 57,850 of them but 15,682 of them are incorrectly geo-located. We drop 6,574 projects that have no localized benefits, such as those related to modernization of the electronic equipment of municipal buildings or to sewage works. With the help of a research assistant, we went through all the remaining projects with incorrect geo-coding and we were able to hand-code roughly one-fifth of them. ArcGIS is used to assign all the 38,353 correctly geo-localized projects (which are points with geographical coordinates) to census areas (areas with polygon boundaries).

After this step, we have a dataset at the census area level. We replace all the *Plan E* variables (such as the dummy for receiving at least one project) with zeros if a given census area did not receive any project. In the special case in which we observe, in our sample, that the whole municipality carried out no investment projects at all, we replace all variables as missing instead. This is the case for 28 municipalities.

Construction of the final dataset

The basis of our dataset are the 35,323 census areas in which Spain was divided as of 2007, as they appear in the INE shapefile. To assign investment projects – which come with latitude

and longitude – to census areas we need information on the exact boundaries of each area, obtained from a shapefile for all 2007 census areas. Using ArcGIS 10.3.1, we overlay the point layer (that is, the dataset of geolocated project points) to the map of census areas. ArcGIS then calculates how many project points fall into each census areas, and the total cost. Finally, it saves the resulting dataset as a comma-separated values file that can be read by STATA.

The electoral data for 2007 are then directly merged using a unique census area identifier (labelled *CUSEC* in the raw data) to the main dataset. A slight complication arises when one tries to merge information for other years (such as, for instance, the 2001 Population Census data or electoral data for other years) to the 2007 census area dataset, because the boundaries of the census areas change over time. To be able to merge data from other years with the 2007 dataset, we create, for each year in which a map shapefile is available (2001 and 2011), a dataset that links the census areas boundaries to the 2007 ones. These two datasets allow us to directly link data for 2001 and 2011 to the 2007 census areas.

As an illustration on how census areas are linked across different years, consider the case in which the 2001 census area A is divided in two areas in 2007, B with surface 9/10 of the original one, and C with surface 1/10. Imagine that we want to have the variable "number of foreigners", only available for 2001, for all the 2007 census areas. Assume, for the sake of the example, that the number of foreigners living in area A was 100 in 2001. To assign this number to the new 2007 boundaries, we simply assume that those people are uniformly located in space. Hence, we assign 90 of them to area B and the remaining 10 to area C.[24] This simple procedure allows us to obtain a single cross-section for the 2007 census areas with several variables from other years, with the advantage of having kept the geographical boundaries constant.

Appendix B. Additional results

Appendix B.1. Projects Descriptives

Table B.1 presents descriptives for all projects and projects in our sample, respectively. The difference between two samples corresponds to projects for which geo-coding is not available and projects which should not have a geographically precise impact on municipal residents (for instance, improvements to the town hall internet connection, etc.). In our sample we also drop all projects of the *CN* investment program, because it was a plan meant to distribute resources to provincial rather than municipal authorities. Likewise, we exclude projects executed by associations of municipalities as we cannot attribute to these associations a single incumbent party. We have a total of 38,353 correctly geocoded projects roughly equally divided between the two *Plan E* programs (*FEIL* and *FEESL*). Our sample of correctly geocoded projects covers 7,210 municipalities (90% of the total). The average project costs 244 thousand

[24] For the years in which no shapefile is available, we first merge to the closest year available and then apply the described procedure.

euros, with projects being slightly more costly in FEIL than FEESL. Comparing panels A and B of Table B.1 we can note that projects that are not in our sample are slightly cheaper on average.

TABLE B.1
DESCRIPTIVES STATISTICS ON INVESTMENT PROJECTS

	All	FEIL	FEESL	CN
A. All projects				
Cost in million euros	12308	7933	4232	143
Participating municipalities	8097	8058	8067	713
Average cost of a project (thousand EUR)	213	260	168	69
N. of projects	57850	30566	25214	2070
B. Only correctly geocoded projects				
Cost in million euros	9376	6270	3106	
Participating municipalities	7210	6879	8023	
Average cost of a project (thousand EUR)	244	292	184	
N. of projects	38353	21460	16893	

Appendix B.2. Additional OLS results

In this short appendix we provide some figures and tables that complement the results in the paper. Table B.2 replicates results showing the partial correlation between mayor vote shares and projects as discussed in section 4. Instead of using the incumbent party's 2007 vote share we use the mean vote share obtained in the last four elections, obtaining results analogous to the baseline estimates in table 3. Table B.3 is the complete version of the first panel of table 3, where the dependent variable is a dummy for receiving one or more projects.

Table B.2

Effect of incumbent's vote share on the propensity to invest - using means

	(1) Project 1/0	(2) Project 1/0	(3) Project 1/0
Vote Share Inc.(mean)	-0.038	-0.026	-0.017
	(0.04)	(0.03)	(0.03)
Controls	FE + geo ctrls.	FE+ elect. ctrls.	FE+ full ctrls.
R^2	0.33	0.28	0.33
Observations	27892	27903	27892
	N. projects	N. projects	N. projects
Vote Share Inc.(mean)	-0.203	-0.107	-0.100
	(0.15)	(0.13)	(0.14)
Controls	FE + geo ctrls.	FE+ elect. ctrls.	FE+ full ctrls.
R^2	0.38	0.34	0.38
Observations	27892	27903	27892
	Inv. share	Inv. share	Inv. share
Vote Share Inc.(mean)	-0.006	-0.009	-0.004
	(0.01)	(0.01)	(0.01)
Controls	FE + geo ctrls.	FE+ elect. ctrls.	FE+ full ctrls.
R^2	0.45	0.44	0.45
Observations	27892	27903	27892

Notes: Municipality fixed effects are included in column 2 and 3. S.e. are clustered at the municipal level. The variable Vote Share Inc. is the average vote share of the incumbent party in 2008 taken over the previous four elections (both municipal and national: 2008, 2007, 2004 and 2003).

Table B.3

Effect of incumbent's vote share on the propensity to invest - full table

	(1) Project 1/0	(2) Project 1/0	(3) Project 1/0
Vote Share Inc. (2007)	-0.039	-0.024	-0.016
	(0.03)	(0.03)	(0.03)
Population (2007)	0.170***		0.171***
	(0.01)		(0.01)
Population sq. (2007)	-0.013***		-0.013***
	(0.00)		(0.00)
Fraction of HH reporting high crime	0.061**		0.064**
	(0.03)		(0.03)
Fraction of HH with 1+ unemployed	-0.115		-0.122*
	(0.08)		(0.07)
Fraction of HH declaring not enough green	-0.015		-0.016
	(0.02)		(0.02)
Fraction of home owners	-0.165***		-0.160***
	(0.03)		(0.03)
Fraction of foreigners	-0.031		-0.032
	(0.11)		(0.11)
Fraction of people 0-16 yrs.	-0.466***		-0.481***
	(0.11)		(0.12)
Fraction of people 65+ yrs.	0.005		0.010
	(0.09)		(0.09)
Fraction of ref. persons with higher educ.	-0.045		-0.016
	(0.06)		(0.07)
Fraction of Urban Discontinuous Terrain	-0.023**		-0.023*
	(0.01)		(0.01)
Distance to closest Urban Centre	-0.000		-0.000
	(0.00)		(0.00)
Surface (c.area)	0.001***		0.001***
	(0.00)		(0.00)
Surface squared (c.area)	-0.000***		-0.000***
	(0.00)		(0.00)
Density, 1000 inh/km2 (c.area)	-0.005***		-0.005***
	(0.00)		(0.00)
Vote share of PP (in c.area)		-0.256***	-0.168**
		(0.09)	(0.08)
Vote share of PSOE (in c.area)		-0.341***	-0.135
		(0.08)	(0.08)
Vote share of CIU (in c.area)		-0.086	-0.135
		(0.18)	(0.14)
Vote share of IU (in c.area)		-0.094	-0.125
		(0.13)	(0.11)
Vote share of PNV (in c.area)		-0.103	0.025
		(0.15)	(0.14)
Vote share of BNG (in c.area)		0.114	0.020
		(0.22)	(0.23)
Vote share of CC (in c.area)		-0.269	-0.123
		(0.20)	(0.20)
Vote share of ERC (in c.area)		0.107	0.225
		(0.19)	(0.18)
Vote share of PAR (in c.area)		-0.346	-0.706
		(0.62)	(0.67)
Controls	FE + geo ctrls.	FE+ elect. ctrls.	FE+ full ctrls.
R^2	0.33	0.28	0.33
Observations	27892	27903	27892

Notes: Municipality fixed effects are included in all columns. S.e. are clustered at the municipal level. Population is in thousand inhabitants. The dependent variable is a dummy equal to one if the census are received at least one project.

Appendix B.3. Additional RDD Results

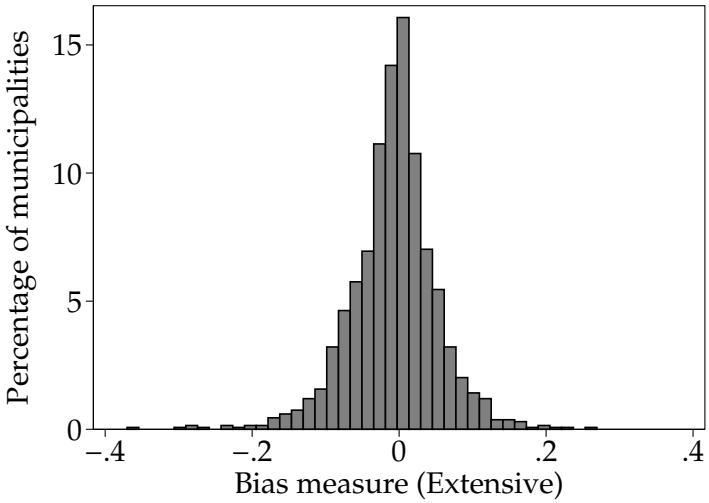
Figure B.5 displays the distribution of the extensive margin bias measures used as a dependent variable in our RDD estimates in section 4. A municipality with a bias equal to 0.01 is one in which the vote share of *PSOE* in census areas that received at least one project is 1% greater, on average, than the vote shares in untreated areas. The distribution is roughly symmetric around zero, suggesting that there is no disproportionate favouring of areas with many *PSOE* (left-wing) voters in the whole sample.

In figure B.6 and in table B.4 we report balancing checks for the controls variables used in the RDD model in the paper. All variables appear to be balanced around the threshold.

Table B.5 presents first stage regressions for the IV estimates using close election RD provided in section 4.1.2 (see equation 3). In all columns the dependent variable is a dummy taking value 1 if the municipality is ruled by a *PP* mayor in the onset of Plan E. We can see that for all bandwidths the variable $\mathbf{1}(Vote\ Margin > 0)$ has a positive and strongly significant coefficient. F-statistics provided in the table indicate that the instrument is strong for all selected bandwidths.

FIGURE B.5

DISTRIBUTIONS OF THE EXTENSIVE BIAS MEASURE



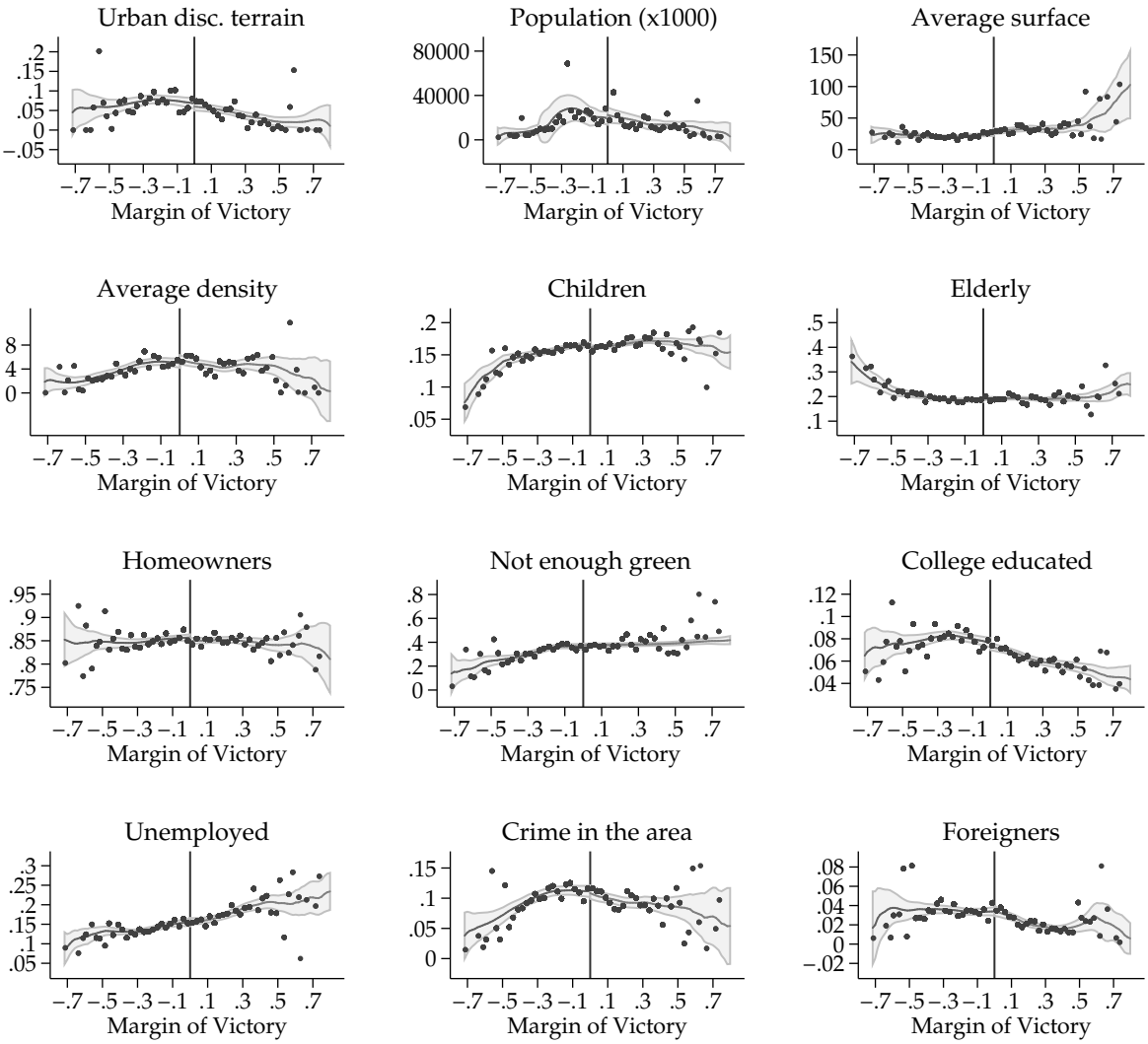
Histogram of the bias measure defined in equation 2. High values correspond to municipalities in which projects where disproportionately allocated to areas of strong support for the left-wing party PSOE.

Appendix B.4. Additional Robustness Checks

Tables B.6 and B.7 display the estimates for different definitions of projects types with localized benefits, as discussed in section 6. In table B.6 we focus on testing whether the incumbent targets areas of strong electoral support within the municipality using our specification with municipal fixed effects. Consistently with the findings elsewhere in the paper, we find that local governments do not favour their voters in the allocation of *Plan E* projects. Alternatively, in B.7 we present results for the effect of localized projects on turnout. Again, consistently with other results in our paper, we find statistically significant and relatively small effects of projects on turnout.

Figure B.6

Balance of covariates checks



RDD balancing checks for each covariate used in the RDD estimation. Dots represent averages within intervals of 2.5% of the vote margin. Solid lines represent fitted values from a local polynomial smooth regression estimated with an Epanechnikov kernel. Shaded areas correspond to 95% confidence intervals.

Table B.4

Balance of covariates checks - regressions

	Population	Surface	Density	Elderly	Children
PSOE vote share >= 0.5	3.454	1.712	0.050	0.009	-0.008
	(12.111)	(5.990)	(1.250)	(0.011)	(0.005)
R^2	0.00	0.00	0.01	0.00	0.00
Observations	598	598	598	598	598
	Homeown.	No green	Unemp.	Crime	Foreigners
PSOE vote share >= 0.5	-0.014	0.000	0.001	0.018	0.002
	(0.012)	(0.029)	(0.014)	(0.018)	(0.010)
R^2	0.01	0.00	0.00	0.00	0.00
Observations	598	598	598	598	598

Notes: Robust s.e in parentheses. Coefficients are the estimated jump in each covariate at the threshold of winning margin equal to zero, using a local linear regression with bandwidth equal to 10% at each side. Population is in thousands, surface in km^2, density in thousands of inhabitants/km^2, and the rest of the variables are fractions of total municipal population. Results using different bandwidth are analogous.

Table B.5

First stage for the RDD analysis of core voters bias

	bw=0.5	bw=0.25	bw=0.1	bw=0.05
$\mathbf{1}(Vote\ Margin\_m > 0)$	0.537***	0.414***	0.426***	0.510***
	(0.030)	(0.042)	(0.070)	(0.098)
F-statistic	321	96	37	27
R^2	0.69	0.56	0.35	0.29
Observations	1956	1339	598	305

Notes: In all columns the dependent variable is a dummy taking value 1 if the municipality is ruled by a *PSOE* mayor in the onset of *Plan E*. From left to right, we restrict the sample to elections in which the absolute value of the vote margin was less than 50, 25, 10 and 5% respectively. Robust s.e. in parenthesis. The $F-test$ for null of no jump in the probability of the left-wing party obtaining the mayor office at the discontinuity is reported for different bandwidth choices.

Table B.8 presents a summary of our results focusing exclusively on big cities where we include in our sample all province capitals. Column 1 shows that the partial correlation between incumbent's vote share and the allocation of projects is negative and not significant. Column 2 shows politicians disproportionately allocate projects to low turnout areas and column 4 shows that conditional on initial turnout, areas that received projects have higher turnout in subsequent elections.

Table B.9 presents a summary of our results focusing exclusively on cities ruled by the two main Spanish parties, the center-right *PP* and the center-left PSOE. Column 1 shows that the partial correlation between incumbent's vote share and the allocation of projects is still negative and not significant. Column 2 shows politicians disproportionately allocate projects to low turnout areas and column 4 shows that conditional on initial turnout, areas

Table B.6

The effect of turnout on the propensity to invest - Projects with Localized Benefits

	Broad	Narrow	Very narrow
	Project 1/0	Project 1/0	Project 1/0
Turnout (2007)	-0.120***	-0.136***	-0.094**
	(0.04)	(0.04)	(0.04)
Controls	FE+ full ctrls.	FE+ full ctrls.	FE+ full ctrls.
R^2	0.31	0.25	0.25
Observations	27756	25159	24274
	N. Projects	N. Projects	N. Projects
Turnout (2007)	-0.608***	-0.302***	-0.238***
	(0.20)	(0.10)	(0.09)
Controls	FE+ full ctrls.	FE+ full ctrls.	FE+ full ctrls.
R^2	0.36	0.32	0.32
Observations	27756	25159	24274
	Cost share	Cost share	Cost share
Turnout (2007)	-0.043***	-0.060***	-0.047**
	(0.01)	(0.02)	(0.02)
Controls	FE+ full ctrls.	FE+ full ctrls.	FE+ full ctrls.
R^2	0.39	0.29	0.28
Observations	27756	25159	24274

Notes: The dependent variable is a dummy equal one if the census area received at least one project. Sample restricted to different project types in each column, with projects with "broadly", "narrowly" and "very narrowly" localized effects in columns 1, 2 and 3 respectively. Observations from municipalities having no projects of a particular type are excluded, in each case, from the estimation sample. Standard errors clustered at the municipal level. Municipal fixed effects and full set of controls included in all specifications.

that received projects have higher turnout in subsequent elections.

Finally, tables B.10 and B.11 summarizes results using 25 meter and 50 meter buffers along projects to measure the census areas that where benefited by Plan E. We can observe that all qualitative results remain unchanged, with only minor changes in the point estimates. We have also produced a similar table using 100 meter bandwidths (not shown) and results remain unchanged. If anything, the coefficients that are significantly different from zero are larger in absolute value when using this alternative buffer. These estimates show that our main results are robust to different assumptions about who benefited from *Plan E* projects and who did not.

Table B.7

Projects with Localized Benefits - *Ex-Post* Effects on Turnout

	Broad	Narrow	Very narrow
	Turnout (2011)	Turnout (2011)	Turnout (2011)
Project 1/0	0.0037***	0.0035***	0.0032***
	(0.00)	(0.00)	(0.00)
Turnout 2007	0.2960***	0.3134***	0.3149***
	(0.02)	(0.03)	(0.03)
Controls	FE+ full ctrls.	FE+ full ctrls.	FE+ full ctrls.
R^2	0.89	0.88	0.88
Observations	27744	25151	24268
	Turnout (2011)	Turnout (2011)	Turnout (2011)
N. Projects	0.0014***	0.0019***	0.0019***
	(0.00)	(0.00)	(0.00)
Turnout 2007	0.2963***	0.3135***	0.3150***
	(0.02)	(0.03)	(0.03)
Controls	FE+ full ctrls.	FE+ full ctrls.	FE+ full ctrls.
R^2	0.89	0.88	0.88
Observations	27744	25151	24268
	Turnout (2011)	Turnout (2011)	Turnout (2011)
Cost share	0.0027	0.0061***	0.0056***
	(0.00)	(0.00)	(0.00)
Turnout 2007	0.2956***	0.3133***	0.3148***
	(0.02)	(0.03)	(0.03)
Controls	FE+ full ctrls.	FE+ full ctrls.	FE+ full ctrls.
R^2	0.89	0.88	0.88
Observations	27744	25151	24268

Notes: The dependent variable is the 2011 turnout (at the census area level) in all specifications. Sample restricted to different project types in each column, with projects with "broadly", "narrowly" and "very narrowly" localized effects in columns 1, 2 and 3 respectively. Observations from municipalities having no projects of a particular type are excluded, in each case, from the estimation sample. Standard errors clustered at the municipal level. Municipal fixed effects and full set of controls included in all specifications.

Table B.8

Robustness checks - big cities

	Project 1/0	Project 1/0	Vote sh. 2011	Turnout 2011
Vote sh. 2007	-0.0064		0.7256***	
	(0.060)		(0.017)	
Turnout 2007		-0.1094*		0.3045***
		(0.064)		(0.014)
Project 1/0			-0.0001	0.0045***
			(0.001)	(0.001)
	FE+ full ctrls.	FE+ full ctrls.	FE+ full ctrls.	FE+ full ctrls.
R^2	0.31	0.31	0.93	0.91
Observations	16602	16602	16593	16593

Notes: Sample restricted to province capitals. Municipality fixed effects and controls are included in all columns. S.e. are clustered at the municipal level.

Table B.9

Robustness checks - *PP* and *PSOE* mayors

	Project 1/0	Project 1/0	Vote sh. 2011	Turnout 2011
Vote sh. 2007	-0.0236		0.7623***	
	(0.039)		(0.015)	
Turnout 2007		-0.1514***		0.2974***
		(0.050)		(0.024)
Project 1/0			-0.0001	0.0041***
			(0.001)	(0.001)
	FE+ full ctrls.	FE+ full ctrls.	FE+ full ctrls.	FE+ full ctrls.
R^2	0.33	0.33	0.94	0.89
Observations	24848	24848	24837	24837

Notes: Sample restricted to municipalities ruled by either *PP* or *PSOE*. Municipality fixed effects and controls are included in all columns. S.e. are clustered at the municipal level.

Table B.10

Robustness checks - Buffers (25 meters)

	Proj.1/0 (25m)	Proj.1/0 (25m)	Vote sh. 2011	Turnout 2011
Vote sh. 2007	-0.0088		0.7407***	
	(0.043)		(0.018)	
Turnout 2007		-0.1253**		0.2944***
		(0.050)		(0.020)
Proj.1/0 (25m)			0.0005	0.0032***
			(0.001)	(0.001)
	FE+ full ctrls.	FE+ full ctrls.	FE+ full ctrls.	FE+ full ctrls.
R^2	0.30	0.31	0.94	0.89
Observations	27892	27892	27880	27880

Notes: In this table we use the presence of a project within 25 meters (rather than the point location of the project) to construct the project dummy. Municipal fixed effects and controls are included in all columns. S.e. are clustered at the municipal level.

Table B.11

Robustness checks - Buffers (50 meters)

	Proj.1/0 (50m)	Proj.1/0 (50m)	Vote sh. 2011	Turnout 2011
Vote sh. 2007	0.0059		0.7407***	
	(0.050)		(0.018)	
Turnout 2007		-0.1069**		0.2944***
		(0.047)		(0.020)
Proj.1/0 (50m)			0.0001	0.0028***
			(0.001)	(0.001)
	FE+ full ctrls.	FE+ full ctrls.	FE+ full ctrls.	FE+ full ctrls.
R^2	0.29	0.29	0.94	0.89
Observations	27897	27897	27885	27885

Notes: In this table we use the presence of a project within 50 meters (rather than the point location of the project) to construct the project dummy. Municipal fixed effects and controls are included in all columns. S.e. are clustered at the municipal level.

Figure B.7
Examples of *Plan E* Investments

Restoration and accessibility improvement in nearby streets in the urban spaces around Eresma, Toro and Tormes streets in the municipality of Leganés, south of Madrid. The total cost of this project was € 3,200,000, financed by *Plan E* through FEIL.

Environmental adaptation and improvement in the low areas of Barrio del Villablanca, in the municipality of Almería, in Southern Spain. The total cost of this project was € 4,864,380 financed by *Plan E* through FEESL.

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