



Do actions speak louder than words? Evidence on voter behavior from Madrid Río Park[☆]

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ABSTRACT

In this paper we estimate the electoral gains and the timing of the gains, from the promise to build and the subsequent construction of a large infrastructure project, Madrid Río Park, which was promised in the 2003 Madrid Mayoral election. We use as a case study the construction of the Madrid Río Park because its unique circumstances provide us with a quasi experiment that allows us to causally separate the prospective and retrospective behavior of voters regarding electoral pledges. We find that voters behave mostly retrospectively. The retrospective effects are sizable in magnitude: we find that after the Madrid Río Park was completed voters rewarded the incumbent with an additional three percentage points. Unexpectedly, but interestingly, we find only weak evidence of prospective behavior: voters do not strongly react to the promise to build the infrastructure.

1. Introduction

The concept of representation is key to representative democracies. On the one hand representation occurs via a mandate that voters give politicians to act according to what they promise to do when in office (mandate representation). On the other hand in representative democracies there is an accountability mechanism – recurring elections – that allows voters to punish or reward politicians for their actions while in office (accountability representation) (Naurin, 2011). The role of voters in the notion of mandate representation is mostly prospective and consists on reacting to promises they like or expectations about future behavior. In the notion of accountability representation, voters behave mostly retrospectively, once they have seen how politicians behave during the previous electoral term. Both notions of democratic representation are intertwined because in each election there is a punishment or reward to previous actions and a new mandate. This makes it very difficult to disentangle in practice which of the models of democratic representation fits better the behavior of voters or to what extent they are both at work (Fearon, 1999a; Mansbridge, 2003).

In this paper we are able to separate causally the prospective component of the reaction of voters to a promise from the retrospective reaction to politicians actions. We do so in the context of the quasi-experiment provided by the construction of Madrid Río Park, a highly

visible infrastructure project that took place in Madrid, Spain, and whose characteristics and development within the period of three different elections (2003, 2007 and 2011) provides an ideal setting to study the distinct reactions of voters.

The construction of Madrid Río Park was promised during the 2003 Madrid city electoral campaign by the incumbent party, the People's Party. The party pledged to underground a large part of the eight-lane highway that surrounded Madrid (the M-30) and proposed to create one of the largest green spaces in the city on the surface of the new tunnel. The pledge became one of the issues that dominated the campaign due to its impact on the main way of commuting within the city. Fulfilling the promise would require to complete what would be one of the largest infrastructure projects in the world at the time. The tunnel was completed in 2007, right before the following elections. The park, however, was not completed until a few months before the next election, in 2011. The construction of the infrastructure transformed the landscape of the area, particularly benefiting people living very close to the new park, a former highly undesirable area and now one of the attractive areas in the city.

The timing of the elections and the characteristics of the infrastructure provide a perfect setting to study the mechanisms by which the voters particularly benefited by the infrastructure have rewarded

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or punished the promise maker through the different stages of its development: at the time the promise was made, half-way through the completion of the infrastructure, and finally, when it was completed and open to the public. Consequently, we can investigate the mechanisms of prospective and retrospective voting in a causal manner.

Our empirical strategy follows a matched sample difference-in-differences approach. First, we identified the spatial units around the infrastructure, where voters benefited the most. We then obtained suitable controls by matching spatial units according to their voting, socio-demographic and geographic characteristics in the period previous to the promise of the infrastructure. Finally, we use a difference-in-differences estimation with fixed effects within this matched subsample to obtain estimates of the electoral rewards in each period controlling for unobserved heterogeneity and time varying characteristics of the units.

We find evidence of strong retrospective voting half-way through the completion of the infrastructure and after it was completed and open to the public. The effects are sizable in magnitude. We find retrospective rewards of 3 percentage points for the elections in 2007 and 2011. Also consistent with our expectations, and with our treatment being geographic, we find that the effects decrease with the distance to the infrastructure. We only find, however, prospective effects that are weak and statistically non-significant, despite the potential gains that voters most benefited by the infrastructure would obtain if the pledge was fulfilled.

The combination of weak prospective effects of the pledge and strong retrospective effects is consistent both with the existing literature that shows that voters do not think pledges will be generally fulfilled and high levels of pledge fulfillment by politicians (Thomson et al., 2017). Since voters exhibit some prospective reaction, but act mainly retrospectively, parties have a self-interested rationale for completing visible policies while in government. While there are several papers that have evaluated the retrospective component of the vote in general (Lewis-Beck and Paldam, 2000) and in the particular case of pledge fulfillment (Matthieß, 2020), to our knowledge our paper is the first one to empirically study the prospective electoral effect of pledges and to separate the prospective component of mandate representation from the retrospective component of accountability representation.

2. Literature review

This paper builds from and contributes to several strands of literature. In this section we briefly discuss each of them.

2.1. Democratic accountability and retrospective voting literature

First, this paper relates to the literature on the mechanisms behind democratic accountability and retrospective voting. The legitimacy of governments in modern democracies rests in the idea of democratic representation. One pillar of democratic representation is that governments will act on behalf of the campaign mandate from the majority they were elected from (Mansbridge (2003), Philp (2009)). In this kind of mandate representation politicians are held accountable by recurring elections. Voters retrospectively reward or punish politicians when they perform or not in accordance with the expectations of the public about the government they elected (McDonald et al., 2004; Grossback et al., 2005).

Empirical studies on retrospective voting have shown that voters respond to changes in their economic conditions, what is known as economic voting (MacKuen et al., 1992; Powell and Whitten, 1993; Lewis-Beck and Paldam, 2000; Hopkins and Pettingill, 2018). There is also evidence of retrospective voting in response to changes in school performance (Berry and Howell, 2007), crime rates (Arnold and Carnes, 2012), the quality of local roads (Burnett and Kogan, 2017), the response to natural disasters (Bechtel and Hainmueller,

2011; Ramos and Sanz, 2020), and to reward or punish policies in which the implementation is spatially localized (Stokes, 2016).

However, the voting mechanisms by which people keep politicians accountable are not only retrospective mechanisms. People can also vote with a prospective logic in mind, by selecting the candidates they believe will be better suited for governing and are likely to act on their promises. In fact, the original formulation of the economic theory of voting (Downs et al., 1957) rested mostly on prospective voting. For self-interested voters it seems convenient to be goal-oriented and support the party/candidate they expect will carry out the policies most promising for their well-being in the years ahead. Empirical research has identified many instances of forward-looking behavior at elections in the last three decades (Lockerbie, 1991; MacKuen et al., 1992; Alpino, 2018; Hsieh et al., 1998; Erikson et al., 2000). In our study, we hypothesize that it was in the interest of forward-looking voters living near the Manzanares River to support the party (PP) that promised to build the Madrid Río infrastructure from which they expected to benefit remarkably.

To sum, both prospective voting and retrospective voting are ways of contributing to democratic accountability (Fearon, 1999b). However, While there is evidence of the presence of both mechanisms in voting (MacKuen et al., 1992; Singer and Carlin, 2013), an important part of the voting literature argues that retrospective assessments of the performance of party/candidate are more influential at the polls than prospective ones (Woon, 2012) and, particularly, to incumbents seeking reelection (Campbell et al., 2010). This might be because past policies offer voters an informative signal about the incumbent's competency (Chen, 2013) while future policies inherently imply greater levels of uncertainty (Lacy and Christenson, 2017). Retrospective voting also provides voters with additional information about whether a party implemented policies they liked in the past, which may lead them to think that the same party will implement similar policies in the future (Dahlberg and Johansson, 2002). Numerous empirical studies on political decision-making in mass elections find that voters do recompense incumbents for the work done in government (Stein and Bickers, 1994; Norpoth, 1996; Chen, 2013; Woon, 2012). This is specially so in contexts where policy outcomes are clearly attributable to the initiatives of the incumbent (Powell and Whitten, 1993; Lanoue, 1994; Lay, 2009; Elinder et al., 2015).

Furthermore, the prevalence of retrospective voting can be justified because it is a relatively simple decision rule that is also well suited to the preference of voters to hold parties/candidates accountable for their performance in power (Fiorina, 1978; Woon, 2012). All in all, it seems easier for voters to evaluate the record of the ruling party than to forecast the consequences of the policies that contending parties promise to enact if elected. Perhaps for this reason, election campaigns based on retrospective messages have been found by some papers to be more successful (Benoit, 2006; Philp, 2009).

Our paper contributes to this literature by providing an empirical analysis in which we analyze the prospective component of the vote and the retrospective component given the timing and characteristics of our case study.

2.2. Pledge literature

Second, this paper relates to the empirical literature that studies whether politicians and parties actually fulfill the promises they made in their election campaigns and how these promises affect vote. By tracking the promises included in campaign manifestos, this question has been studied in single-country studies (Artés, 2013; Bara, 2005; Costello and Thomson, 2008; Duval and Pétry, 2019; Kalogeropoulou, 1989; Kostadinova, 2013; Mansergh and Thomson, 2007; Matthieß, 2019; Moury, 2011; Moury and Fernandes, 2018; Naurin, 2014; Pétry and Duval, 2018; Pétry et al., 2018; Praprotnik, 2017; Royed, 1996; Thomson, 2001; Thomson and Costello, 2016), as well as in comparative multi-country studies (Matthieß, 2020; Thomson et al., 2017).

A common finding across the literature is that politicians' pledges are more likely to be held than to be broken, with fulfillment rates of more than 60 percent across countries, although the results vary between countries and elections (Thomson et al., 2017). This is also the case even for promises made outside the electoral campaign (Bevan et al., 2011; Kennedy et al., 2020). Overall this literature has challenged conventional wisdom of the public about these promises, which considers them as nothing more than empty words not to be trusted (Thomson, 2011; Naurin, 2011).

This literature has also studied the factors behind the citizen's evaluation of these promises (Naurin and Oscarsson, 2017; Thomson, 2011; Thomson and Brandenburg, 2019) and has found that while subjective factors have some importance on the citizens' evaluation (Marsh and Tilley, 2010), informed citizens still do correct evaluations regardless of other subjective factors (Duval and Pétry, 2018). Evidence from survey experiments shows that breaking a promise is evaluated more negatively than the corresponding positive evaluation for a fulfilled one, although it is unclear if partisanship moderates how this asymmetry in evaluation translates into voting behavior (Naurin et al., 2019).

While there is a clear theoretical link between pledges fulfillment and the mechanisms of prospective and retrospective voting, the observational evidence from comparative studies has not been able to disentangle the prospective effects of promises from the retrospective effects of their fulfillment. One reason that may explain the lack of research on this area is that it is difficult to causally separate these two components in observational studies in which many pledges are analyzed and voters may have both prospective and retrospective reasons to punish and reward politicians for each of the promises. To our knowledge one of the only papers that has tried to evaluate the retrospective component of pledge fulfillment is the country level comparative study of Matthieß (2020).

Our research complements this line of study by relating electoral pledges to both prospective and retrospective voting. Instead of country data we use a case study in which we focus on the timing of the voter's reaction to the promise and the completion of the infrastructure. Since we analyze the effect of an infrastructure whose main beneficiaries are clearly identifiable and we have geolocalized voting data at the census tract level, we can exploit the spatial variation in the response of voters over time to provide a causal evaluation of the prospective electoral impact of the pledge and the retrospective effect of the completion of an infrastructure consistent with its fulfillment.

2.3. Pork barrel and infrastructures literature

Thirdly, this paper relates to the literature on Pork Barrel and, particularly, to the literature that analyzes the use of infrastructure projects as an electoral tool. This literature has shown that incumbents can use these projects to obtain electoral benefits at the expense of larger social gains for the public (Cadot et al., 2006). Governments have incentives to deviate the use of public spending from general welfare concerns to benefit their strongholds (Cox and McCubbins, 1986), or to benefit marginal voters that might swing their vote (Lindbeck and Weibull, 1987; Dixit and Londregan, 1998).

Our case study is particularly related to De La Calle and Orriols (2010). They investigate the electoral effect of the expansion of the underground network in the city of Madrid by the regional government between 1995 and 2007. In their research, they do not find strong evidence of vote-seeking motivations for strongholds nor swing voters in the spatial expansion of the underground network, but they find that the incumbent benefited electorally from the infrastructure. Our research also studies a policy that took place in the city of Madrid, and we also find benefits for the incumbent from infrastructure projects, confirming their findings. However, our focus is on better understanding the prospective and retrospective mechanisms in the vote and their relation to pledge making, which has scarcely been analyzed before.

3. Case study: Madrid Rio Park

3.1. History

Since 1991, the City Council of Madrid has been governed by the conservative People's Party (PP). In the elections of 2003, this party ran a campaign with its former regional governor as a candidate for Mayor with an agenda of reforms promising to turn Madrid into a more cosmopolitan and modern city. Among these pledges, the People's Party promised to develop a highly ambitious infrastructure project in which significant parts of the M-30, the principal innermost ring road of the city, would be undergrounded. One of the most important features of the project would be the construction of a large green area across the banks of the Manzanares River by using the surface that would be freed-up by the underground tunnels.

The promise to build this infrastructure was one of the salient issues during the 2003 election campaign, since it was going to have a great impact in the busiest road used for car commuting within the city. While there is no empirical evidence on how salient the promise was, ample anecdotal evidence from news published in the main Spanish newspapers around the time of the electoral campaign suggest that the incumbent placed great importance to the promise to underground the M-30. In fact it was defined as the "star promise" of the campaign in several media outlets.¹

The People's Party pledged to finish the undergrounding of the highway in four years. The main parties in the opposition, the social democrat Spanish Socialist Workers' Party (PSOE) and the United Left (IU, communist) unanimously questioned the feasibility of the project, both in terms of the time needed to complete the reform as well as in terms of its overall economic viability and usefulness.²

At the time of the 2003 election, the social gains from the infrastructure were uncertain. While the proposed project could help to mitigate traffic jams and improve the connection between areas in the city, those benefits would only come in the future, after some years of critical traffic interruptions due to the magnitude of the reforms. Moreover, it was also possible that in the end the infrastructure could not compensate its cost.³

Although for the greater public there was uncertainty about its benefits, there were clear beneficiaries from the project if it was finally carried out: the residents in the neighborhoods surrounding the existing highway on both sides of the Manzanares River.

Due to their location, these neighborhoods suffered from high levels of noise and pollution, which made them not very desirable. The proposed infrastructure would imply a complete renewal of the area with clear potential benefits to its residents. Not only these reforms would eliminate the noise and pollution problems, but would also make the areas highly desirable to live, as the new park would include many

¹ To cite a few, EL PAÍS, one of the largest Spanish newspapers, defines the M-30 promise as the "star promise of the electoral campaign" (see "Casos autoriza a Gallardón a iniciar el proyecto de reforma de la M-30". 2003, July 31). ABC, another widely read newspaper, also defined the M-30 as the "star project" of the 2003 electoral campaign in an article published during the electoral campaign ("Gallardón: La reforma de la M-30 es el Metrosur de la próxima legislatura", ABC. 2003, May 14).

² Both opposition parties were unanimous in their rejection of the project (see, for instance, "PSOE e IU creen que la reforma de la M-30 prometida por el PP la colapsará", El País. 2003, March 05). The infrastructure kept being a subject of debate during its construction after the 2003 election, as both opposition parties denounced that it was executed without the assessment of environmental impact required by the European Union (see "El PSOE pide al Ayuntamiento que realice una declaración de impacto ambiental en las obras de la M-30", El Mundo. 2003, April 04).

³ Recent cost benefit analyses of the infrastructure argue that the benefits greatly exceeded the costs. See Giannelli et al. (2018).

new amenities for recreation and outdoor leisure. Additionally, access to the city center would improve drastically.⁴

At the time of the 2007 election, the underground tunnels were practically completed and three weeks before the voting day they were inaugurated by the mayor in a ribbon-cutting event. In this election, the main pledge by the incumbent was the completion of Madrid Río Park, still heavily questioned by the opposition by its enormous economic cost, as the most expensive infrastructure project in the history of Spain. The incumbent won the election again and kept its new promise, completing the landscaping of the surface by the time of the next election. One month before the 2011 election, Madrid Río Park was inaugurated and open to the public.

The incumbent won the election for a third time in 2011. Madrid Río Park is nowadays is a landmark, being one of the largest green areas in the city. The surrounding neighborhoods are now considered very desirable places to live and visit. As a consequence, housing prices in the area have increased drastically over the last decade.⁵

3.2. Ideal setting

The development of Madrid Río Park from a promise to a real city landmark is close to an ideal case to study the electoral rewards to the incumbent obtained from the electoral pledge from the voters directly benefited by the infrastructure. Several characteristics contribute to making this case study a unique setting.

First, from the point of view of causal identification, the infrastructure is a clear geographic treatment, which was received by voters living close to the infrastructure and less so by everyone else. Therefore, we can use a quasi-experimental design comparing treated units with very similar controls.

Second, since the incumbent People's Party had governed the City Council since 1991 to 2011, there should be no doubts in the voters' attribution of responsibilities for the infrastructure to the incumbent. The incumbent was the only party that could claim credit for the infrastructure, since it was the one that promised it and carried it out until its completion, while the opposition parties heavily opposed it, considering it first an unfeasible project and then a waste of resources.⁶ This clarity in the government responsibility makes the reward or punishment by voters in the local elections more salient. The People's Party was also the incumbent in the Regional Government, which made attribution of responsibilities easier for voters. There could be some external effects of the infrastructure to other governments different than the local government such as the regional and the central governments. In addition to the same party being in power in the regional government, another reason why this should not be a problem for our identification is because the attribution of responsibility to the wrong administration (regional vs. local government) would lead to a bias towards zero in our estimates, as in that case, some voters would not reward the local government, but only the regional government. This case is unlikely, however, given that the mayor of the city was a very visible figure and that he personalized the credit claim of the

⁴ Several recent studies have analyzed the increases in quality of life due to the construction of Madrid Río. See de Andrés (2016), Giannelli et al. (2018), Abajo et al. (2020).

⁵ An analysis following prices of houses located right around the park advertised in idealista.com, one of the main Spanish real state websites, is presented in de Andrés (2016). One can now find many specialized blogs and news articles that refer to Madrid Río as one of the most demanded areas in Madrid in recent years. See, for example, "En las entrañas del barrio más demandado de Madrid para vivir (y trabajar)". El Confidencial. 2020, November 10.

⁶ The Socialist Party, the main opposition party, as a reaction to the promise to underground the M-30 also proposed a less ambitious, lower scale renovation of the area. See, for example, "Jiménez quiere que la M-30 llegue a convertirse en tres grandes avenidas". El País. 2003, May 10.

construction of the infrastructure through many ribbon cuttings over the years that were widely covered in the news. Madrid-Río was in fact seen as one of the main achievements of Mayor Alberto Ruiz-Gallardón in several biographic notes that were published in the news after he retired from politics.⁷

Third, and most importantly, the timing of the promise and the completion of an infrastructure consistent with its fulfillment allows to establish a clear distinction between the prospective and retrospective components of the vote in each election.

In the elections before 2003, the infrastructure was not even a project, nor it was an issue on the electoral discussion. Consequently, the voting in these elections could not have been affected by a non-existing project. We take these elections as our pre-treatment baseline.

The election of 2003 had the undergrounding of the highway was a salient electoral pledge. In this election the promise could only activate the prospective component of the vote for those living in the areas close to where the infrastructure would take place.

In the 2007 election, the undergrounding of the highway was completed. The incumbent had made a pledge of finishing the landscaping of the surface, turning it into Madrid Río Park. In this context the "treated" voters could have voted retrospectively, by punishing or rewarding some of the effects the 2003 promise already materialized (noise reduction or capital gains), as well as prospectively, by believing in the new promise and the benefits that it would imply (e.g. use of park facilities). Given that the undergrounding of the highway was already completed and the funding for the landscaping of the surface area had already been approved as part of the initial project, the retrospective component was likely more relevant for voters in this election.

Finally, right before the 2011 election the construction of Madrid Río Park was finished and there was no other infrastructure promise nor divisive issue that affected directly voters living near the park. Consequently, the vote in this election was mainly retrospective, since these voters had no other reason to benefit the incumbent compared to the rest of the voters but to reward the fulfillment of its promise to build Madrid Río Park.

This is close to an ideal setting provided that two conditions are met. The first one is that voters had enough information about the pledge in 2003. If voters only became informed of the pledge after the construction started, there could be other differentiating factors between 2003 and later years (e.g. saliency of the project) that could create differences in behavior unrelated to prospective or retrospective behavior, making the comparison against pre-treatment period less credible.

As previously mentioned, we have only indirect evidence of how salient the promise was in 2003. However, the detailed coverage of the promise in the media makes it very plausible that voters, and particularly those living around the river, knew about it. Voters who lived by the Manzanares River at the time of the 2003 local election campaign had strong incentives to be informed about the project to bury the heavy traffic near their homes and subsequently build a large green space. In the first place, because the project was going to have a strong impact on their well-being in the coming years. On the one hand, they would have to suffer the multiple inconveniences (noise, dust, traffic jams) derived from the undergrounding works in the vicinity of their homes. On the other hand, the projected infrastructure, once completed, had a great welfare-enhancing potential and those living in the surroundings of the Manzanares River were the most direct beneficiaries of the externalities generated by the project, such as an improved and healthier environment and higher real estate values. Secondly, because the entire infrastructure was the subject of a heated

⁷ See for example an article published in El Economista. "La herencia de Gallardón: Madrid, el ayuntamiento más endeudado de España... con las mejores infraestructuras". El Economista. 2008, January 18.

debate during the electoral campaign both among candidates and in the media, which, as mentioned before, referred to it as the “star promise” of the campaign. Thirdly, during the campaign there were specific events and information campaigns specifically tailored to residents of the neighborhood that were covered also in the news. All in all we take this evidence as suggestive that voters were generally aware of the main pros and cons of the promise in 2003.

A second potential threat that could contaminate the analysis is the fact that Madrid mayoral elections were held on the same day as the regional elections in all the years. Regional elections do attract a lot of attention which could blur voter's views on which party or administration is responsible for the promise and construction of the infrastructure. We believe, however, that this is not a problem for our identification, based on several reasons. First, because the People's Party was in power in both administrations since years before the promise was made and until after the project was completed. Additionally, as described in a previous section, the mayor of the city was a very visible figure that personalized the credit claim of the promise and the construction of the infrastructure.⁸ Lastly, there is evidence suggestive of voters attributing responsibility differently even between same party politicians of different levels of government: the local PP candidate, Alberto Ruiz-Gallardón won an undisputed absolute majority while the regional PP candidate, Esperanza Aguirre, obtained 42.927 votes less within the same city districts in the regional elections and lost the absolute majority.⁹ In our opinion this is evidence consistent with the fact that many voters were able to distinguish the context of both elections and punish or reward candidates differently.

4. Empirical approach

The previous characteristics serve as a basis for our empirical approach. Our goal is to estimate the effect of Madrid Rio Park in the incumbent's vote share for the units directly benefited by its construction at the different stages of the project. We take advantage of the geographical nature of the treatment to construct our treatment and control groups, and we use the timing of the events to separate prospective and retrospective components of vote.

Our empirical design follows a two-step approach. In the first step, we use a matching procedure in order to find suitable units for comparison. In this step we use the observable information included in our dataset, to pre-process the data in order to make comparisons more reliable (Stokes, 2016). In the second step, we account for unobservable differences between treatment and control units by running a difference-in-differences regression on the matched sub-sample.

4.1. Data

Our main data consists on highly disaggregated information at the census tract level for the entire city of Madrid. For every tract unit, we have gathered information from the Ministry of Internal Affairs on the electoral outcomes in each election within our period of study (from 1991 to 2011). We have also gathered data at the census tract level from the Spanish Statistical Office (INE). This data includes

⁸ We have collected more than a dozen of articles and interviews published in the main Spanish newspapers in which the Mayor of the city explains the promise and takes credit for it either in the interview or through ribbon cutting events. Consistent with our interpretation, after Alberto Ruiz-Gallardón retired from politics, many media outlets reported Madrid-Rio as one of the main achievements of his long political career. See, for example, “La herencia de Gallardón: Madrid, el ayuntamiento más endeudado de España... con las mejores infraestructuras.” *El Economista*. 2008, January 18.

⁹ The PP obtained 831,337 (out of a total of 1,656,279 valid votes in the regional elections while as much as 874,264 (out of a total of 1,676,837 valid votes) in the local elections. Source: Madrid Directorate for Economic Policy (Dirección General de Política Económica de la Comunidad de Madrid).

several socio-demographic variables such as age, gender, educational attainment and income for each election year.

Census tract units are required to maintain a similar population size by law. As population in a given geographical space may increase or decrease over time, the boundaries of the census tract might change from one election year to another. In order to follow the same geographical units over time, we take the boundaries of the 1991 census tract as a baseline. We then use Geographical Information Software (GIS) to map the precincts and population from the years after 1991 into the 1991 boundaries. For this process, we assume that population is distributed uniformly along the parts of the surface of the tract in which there are residential buildings in each year. This mapping allows us to calculate voting outcomes and socio-demographic variables for homogeneous geographic units.

The result of this procedure is a balanced panel dataset of electoral, geographic and socio-demographic information in which our units of observation are census tracts units measured according to the 1991 boundaries (see Figs. 1 and 2).

4.2. Treatment definition

To identify the effect of Madrid Rio Park on voting behavior, we exploit the variation provided by the spatial distribution of the units around Madrid Rio Park.

Although the project was a city project intended to improve the city's traffic and landscaping overall, the units closer to the park were disproportionately benefited compared to the rest. The reason is that amenities of the park will normally be less frequently used and the capital gains from residential and commercial property will also be smaller (Grahn and Stigsdotter, 2003; Rosen and Fullerton, 1977; Luttik, 2000). Therefore, the tracts closer to the park are our treatment units. Since we have geolocalized data, we use the distance from the units to the closest point in Madrid Rio Park through the street network as the assignment variable. Distance through the street network is a closer approximation to the walking distance that the voters will consider in their evaluations of their proximity and accessibility to the park than the euclidean distance, thus providing a reasonable basis to compute the area of influence of the infrastructure.

In order to calculate which units within Madrid are sufficiently close to be considered as “treated” we rely on previous literature which suggests that around 1000 meters is the distance people might be willing to walk to reach a close destination (Zhou and Kim, 2013; Dygryn et al., 2010; Troped et al., 2010). Consequently, the units further from that distance could be considered as part of the control group. For units closer than 1000 m, the treatment will be particularly intense (higher enjoyment of the amenity and higher housing appreciation) the closer a unit is to the park. In order to compute the upper bound of the effect of the pledge on voters we use 200 meters as the distance that defines the treatment. In particular, we compare units within 200 meters from the park with units that are further than 1000 m. We exclude the units in-between these two limits, as for those units we also expect an effect of the infrastructure, although weaker. Since this definition of the upper bound treatment involves a discretionary element, as an additional robustness we present results using 400 and 600 meters as the threshold to define the treatment group.

4.3. Matching

In order to make treatment and control units more comparable, we pre-process our data using a propensity-score-matching process. In particular, we use a one-to-two greedy matching. We pair each treated unit to two control units based on the logit propensity scores computed using as independent variables in the logit model the vote shares for the three principal contenders in the pre-pledge elections, geographical location variables and socio-demographic characteristics such as gender, age and educational attainment. We use two matches

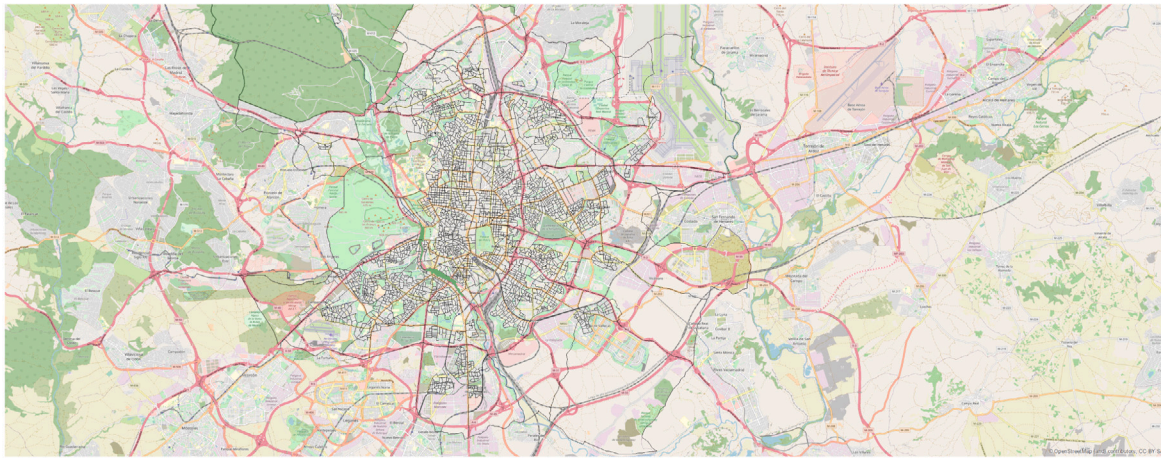


Fig. 1. Municipality of Madrid.

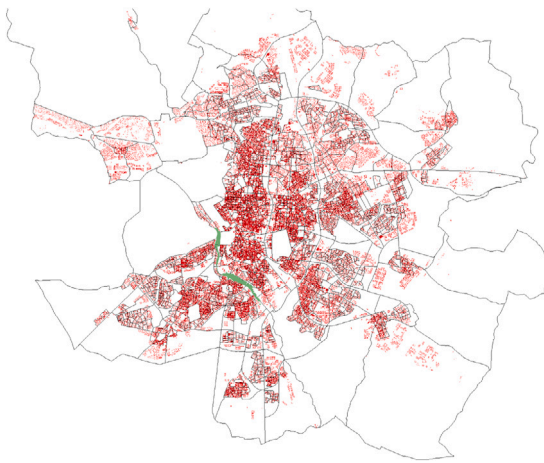


Fig. 2. Municipality of Madrid at the tract census level. Madrid Rio Park in green. Residential use in red. (For interpretation of the references to color in this figure legend, the reader is referred to the web version of this article.)

because we found estimates to be more stable across specifications when using two neighbors instead of just one. Our results, however, are robust to using either just one match or more than two.

This procedure provides a matched subsample in which the included observable characteristics of the treatment and control groups are balanced in the pre-treatment period. The rationale for using a matching procedure is provided in Fig. 3 that represents the incumbent's vote share in both the full sample and the matched subsample for both the treatment and the control groups. The Figure shows that the support for the incumbent in the treatment and control groups is very similar in the matched subsample while not as much if one uses the whole sample. This is important given the limited amount of controls and time periods that we have available in our database. Of course, the matching process only allows us to balance the sample in the observable differences across the treated and control groups in the pre-treatment period. If there are unobservable differences between the treatment and control units, a matching might not be enough to identify the treatment effect of the promise. In addition, as we are only matching observables in the pre-treatment period, there might be changes in those variables in the post-treatment period that should be taken into account.

4.4. Difference-in-differences

Since the set of observable variables in our dataset is not exhaustive and there may be other unobserved variables related to our outcome,

we use a difference-in-differences strategy as our preferred empirical design. Our empirical model can be summarized with the following equation:

$$Y_{i,t} = \alpha + \beta_{2003}D_{i,2003} + \beta_{2007}D_{i,2007} + \beta_{2011}D_{i,2011} + \delta X_{i,t} + \eta_i + \lambda_t + \epsilon_{i,t}$$

$Y_{i,t}$ is the incumbent's vote in a tract i on a election year t . The key variables of the model are the interaction terms between the treatment variables and the treatment periods, which are denoted by $D_{i,t}$. The model also includes an additional set of socio-demographic controls denoted by X .

In our empirical design, socio-demographic variables serve two functions. First, they are useful to assess the pre-treatment balance between our units before the estimation, as shown in Table 5, in the Appendix. Second, when included in the models, they increase precision of the estimates and reduce any potential biases in our coefficients of interest due to the changes in the outcome variable that come from changes in those covariates.

Our specification includes census tract fixed effects, η_i , and time fixed effects, λ_t . Those two sets of dummies capture time-invariant factors that are idiosyncratic to each tract and that could be correlated with our outcome in addition to the average pre-treatment differences between our treated and control groups. Because we are including census tract and time fixed effects, we do not need to include the standard treatment and after variables, as they are subsumed into the fixed effects. We cluster the errors by census tract in order to control for both spatial autocorrelation and heteroskedasticity.

Our coefficients of interest are the β coefficients, which are the difference-in-differences estimates for average treatment on the treated units (ATT). These estimates measure the average difference between the incumbent vote share in the matched units with and without the treatment for each election in 2003, 2007 and 2011. Note that in this specification, as we are not including pre-treatment years interactions, we are comparing each post-treatment year against the average pre-treatment period, which avoid results to depend on which pre-treatment year we use as reference.¹⁰

The key identification assumption for this model is the usual assumption of parallel trends. This assumption means that the incumbent's vote share would have followed the same trend in both treated and untreated tracts had the pledge to build Madrid Rio never existed. Although we cannot test this assumption directly, we can support its validity by comparing the evolution of the incumbent's vote share for

¹⁰ In the appendix we report an event type specification in which each pre-treatment year is also interacted with the treatment. In this regression results are qualitatively the same, although significance varies depending on which pre-treatment year is excluded from the regression.

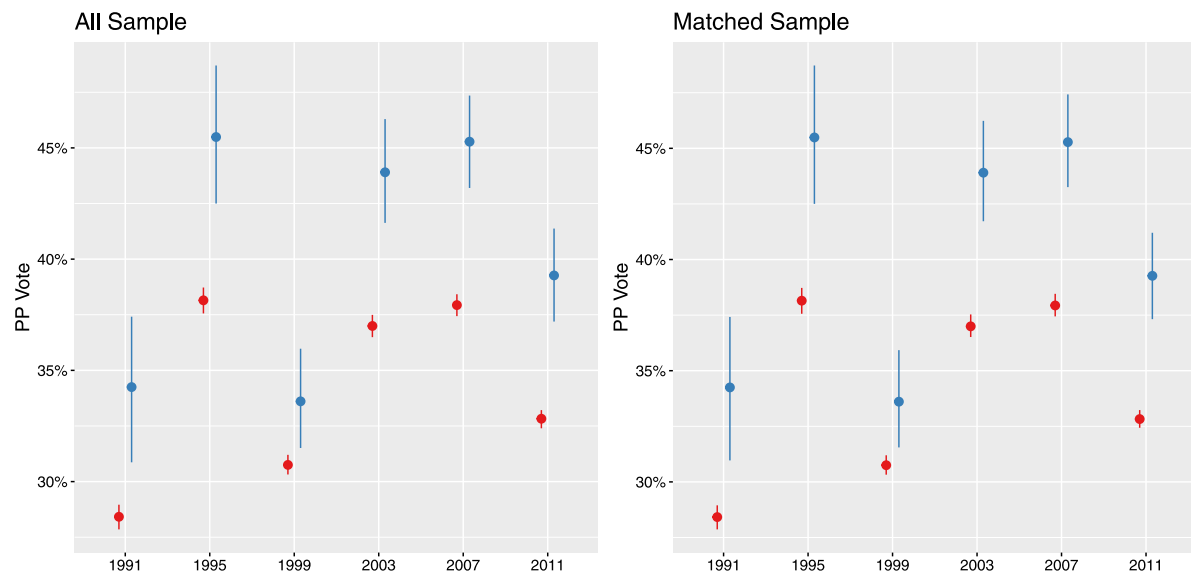


Fig. 3. Matched sample. Mean vote share for the incumbent. Control (red), treatment (blue). (For interpretation of the references to color in this figure legend, the reader is referred to the web version of this article.)

the treated and control units in the pre-treatment period. A graphical representation of these trends in the matched sample is provided in Fig. 3.

We can provide more formal evidence of its fulfillment in the pre-treatment period by regressing the incumbent's vote share on the interactions between the treated and year dummies, and the control and year dummies. Since this regression obtains the slopes of the pre-treatment trends, we can check whether they are parallel by testing if they are statistically different. The results of these regressions are shown in Table 7. As we can see both in the figure and the table, the identifying assumption necessary for the difference-in-differences estimates is satisfied in our tests for the pre-treatment period.

5. Results

5.1. Difference-in-differences estimates

The results of our estimations are provided in Table 1. This table shows the difference-in-differences estimates of the electoral rewards obtained by the incumbent from voters directly benefited by the construction of Madrid Rio Park in different elections as compared to the control sample of very similar geographical units that are outside the area of influence of Madrid Rio Park. In this table, we have a different difference-in-differences coefficient for each of the years after 2003. We keep a separate year interaction in the after period so that we can analyze the prospective and retrospective components of the vote, which, as explained, were different in each of the years. In our preferred estimation of Table 1 we are comparing the support for the incumbent in each of the after years against the average pre-treatment support. We do this so that the significance of the results is less sensitive to which pre-treatment year is used as benchmark for comparison (e.g. excluded from the regression). In Table 2 in the appendix we show that results are qualitatively the same when using different specifications in which each of the pre-treatment years is interacted with the treatment.

Columns 1 and 2 show the results of our preferred definition for the treatment, in which we consider as treated those tracts within a 200 meters distance from Madrid Rio Park. The difference-in-differences estimates in Column 1 show statistically significant results for the difference-in-differences coefficient corresponding to the 2007 and

2011, but not for 2003. Recall that in 2007 the construction of the underground tunnel was finished, but not the park, which was still a pledge. Consequently, we can interpret the statistical significance of this coefficient as partial evidence from a prospective effect of the pledge for the park, and a retrospective effect from the construction of the underground tunnel. Taken together, this interaction coefficient shows that voters directly benefited by the infrastructure increased their support for the incumbent in 3.133 percentage points compared to the control group. Similarly, the interaction coefficient is also significant and large in magnitude for 2011. In that year, the fulfillment of the promise to build Madrid Rio Park was rewarded with a 3.465 percentage points vote increase. Note, however that the interaction corresponding to the 2003, although quantitatively sizable, is smaller in magnitude (1.6 percentage points in our preferred specification of column 1) and not statistically significant, showing a weak response of voters to the promise itself. That is, our results imply that the voters have acted more retrospectively than prospectively, rewarding the incumbent once the promise to underground the highway and finish Madrid Rio Park was enacted.

In Column 2, the model includes socio-demographic covariates in order to control for post-treatment bias in the results. We find no changes in the statistical significance of the results and coefficients are of similar size, with an electoral reward of 2.970 percentage points in the 2003 election and a electoral reward of 3.275 percentage points in the 2011 election. Thus, the conclusions are unchanged, showing the robustness of the results to potential post-treatment bias.

We consider our preferred definition for the treatment (200 m) as providing a higher bound for the results due to the closeness to Madrid Rio Park of the tract units included in the treatment group according to that definition. In Columns 3 to 6, we show the results using as a maximum distance to define the treatment 400 and 600 m. The inclusion in the treatment group of units further from the infrastructure should result in a weaker effect found in the estimates if the effect is genuine and depends on the distance to the infrastructure. Columns 3 and 4 show results that are consistent with this reasoning. In Column 3 we could not find a significant effect for the elections in 2003 and 2007. We find, however, a statistically significant effect in the 2011 election, with an electoral reward for the incumbent of 1.886 percentage points, which is still sizable, although, as expected, smaller than when we

Table 1
Difference-in-differences estimates.

Dependent variable	200 m		400 m		600 m	
Incumbent's vote share	(1)	(2)	(3)	(4)	(5)	(6)
DD 2003	1.604 (0.990)	1.524 (0.946)	0.280 (0.784)	0.398 (0.760)	1.012 (0.852)	1.108 (0.840)
DD 2007	3.133** (1.370)	2.970** (1.315)	1.505 (1.265)	1.728 (1.226)	1.279 (1.011)	1.538 (0.983)
DD 2011	3.465** (1.380)	3.275** (1.282)	1.886** (0.883)	2.254*** (0.815)	1.703** (0.743)	2.022*** (0.696)
Female population (%)		−0.222 (0.295)		−0.253 (0.220)		0.0698 (0.190)
Over 65 population (%)		0.0494 (0.123)		0.170* (0.0906)		0.168*** (0.0618)
Population with Univ. Studies (%)		−0.203* (0.105)		−0.0904 (0.0922)		−0.0878* (0.0527)
Tract fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Time fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Intercept	34.18*** (0.575)	48.36*** (16.92)	31.80*** (0.317)	43.77*** (10.95)	30.31*** (0.316)	24.96** (9.866)
Observations	252	252	504	504	834	834
R-squared	0.702	0.713	0.621	0.633	0.543	0.561
Number of tracts	42	42	84	84	139	139

Clustered standard errors in parentheses.

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table 2
Year by year estimates.

Dependent variable	200 m		400 m		600 m	
Incumbent's vote share	(1)	(2)	(3)	(4)	(5)	(6)
DD 1995 vs. 1991	0.216 (0.910)	0.276 (0.886)	0.138 (0.612)	0.0272 (0.574)	−0.111 (0.496)	0.0134 (0.506)
DD 1999 vs. 1995	0.0495 (2.010)	−0.289 (2.044)	0.306 (1.365)	0.367 (1.379)	0.383 (1.089)	0.114 (1.091)
DD 2003 vs. 1999	1.499 (1.790)	1.012 (1.788)	0.0301 (1.305)	0.219 (1.207)	0.793 (1.141)	1.336 (1.109)
DD 2007 vs. 2003	1.529* (0.765)	1.450* (0.726)	1.225 (0.947)	1.310 (1.035)	0.267 (0.732)	0.483 (0.804)
DD 2011 vs. 2007	0.332 (0.930)	0.307 (0.867)	0.381 (0.930)	0.409 (1.083)	0.423 (0.755)	0.184 (0.928)
Tract fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Controls	No	Yes	No	Yes	No	Yes
Observations	84	84	168	168	278	278
R-squared	0.935	0.943	0.935	0.936	0.930	0.935
Number of tracts	42	42	84	84	139	139

Clustered standard errors in parentheses.

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

use a narrower definition of the treatment. Since in the election of 2011 there is no incentive for treated units to vote prospectively, this result provides further evidence that voters directly benefited by Madrid Rio have acted retrospectively, rewarding the construction of an infrastructure. Column 4, in which we include socio-demographic controls in the model, leads to the same conclusion, showing a 2.254 percentage point increase in the incumbent vote share in 2011.

In Columns 5 and 6 we use 600 as the threshold to define the treatment. Similarly to the previous models, we find weak and non-significant evidence of prospective voting in our estimates. In addition, in the case of the 2007 and 2011 coefficients we find a further reduction in their magnitude that is consistent with the weakening of the possible effects as we include units further from the infrastructure in the model. Again, in this specification the only significant coefficient is the one corresponding to the 2011 election. In this case the estimated electoral reward for the incumbent is 1.704 percentage points in the model without socio-demographic controls and 2.022 percentage points when covariates are included in the model.

An interesting result suggested by the results in Table 2, and particularly in the specifications of Columns 1 and 2, is that there seems

to be an accumulation effect in the response of voters. The differential behavior of voters benefited by Madrid-Rio seems to appear for the first time, although weakly, in 2003, when only prospective mechanisms can explain the result, increases even more in 2007, the first time that the retrospective mechanism is in place, and then such differential reaction is maintained through 2011 when the prospective mechanism is no longer in place and only retrospective motives can explain differential results. To confirm this interpretation of the results, in Table 2 we run the same model as in column 1 in the previous table, but using only each pair of consecutive years in each of the regressions. This table shows four things. First, we can see that there is no significant increase or decrease in voter support year to year in the pre-treatment period (consistent with the fulfillment of the parallel trend assumption). Second, the change in the magnitude of the coefficient starts in 2003, with an increase of 1.5 points compared to the close to zero increases of previous years. Third, in 2007 there is an additional increase of 1.5 points compared to 2003 (making the overall effect compared to the pre-treatment period of approximately 3 percentage points, as in column 1 of Table 2). Lastly, in 2011 the differential increase compared

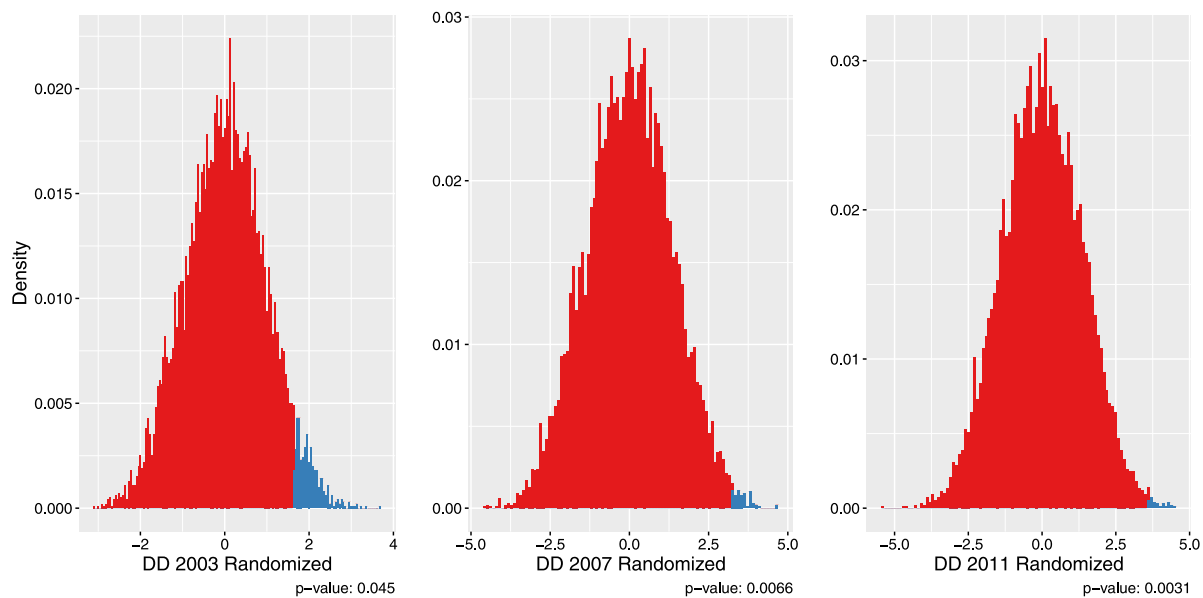


Fig. 4. Randomization placebo.

to 2007 is relatively small (Table 2), but the overall effect compared to 1999 is even bigger than in 2007 and is clearly significant across different specifications (see again column 1 of Table 1).

Considered together, these results provide strong evidence that voters have acted retrospectively and weak evidence that the prospective mechanism is also at work. Voters most benefited by the construction of Madrid Rio park rewarded the incumbent from the announcement of the promise (weakly) and more clearly once the infrastructure was a tangible reality, both in 2007 and 2011.

Note that our estimates are potentially conservative considering that it is likely that voters in the control group have also benefited from a reduction in commuting times due to the construction of the highway tunnels as part of the infrastructure, and from having the park as an additional leisure option to go to from time to time. If these benefits lead to an electoral reward by the voters in the control group as well, it would make our estimates biased toward zero and it would be more unlikely to find a significant effect of the treatment, as found in our estimates.

6. Robustness checks

In this section we show the results of two falsification tests aimed at providing support to the causal interpretation of our results.

6.1. Randomization placebo test

A first check of the robustness of the results is provided by a placebo test in which we randomize the dichotomous assignment to the treatment. That is, we assign tracts randomly to the treatment and control group and estimate the model. We repeat the random assignment 10,000 times and then we estimate the model for each of those 10,000 random assignments. If our results are truly causal, we should not expect any significant effect, on average, in the placebo regressions. More technically, since the assignment to the treatment is random in this exercise, by design, we should expect a normalized distribution of the coefficient estimates centered around zero. Moreover, the size coefficients found in our true estimation should be located on the tails of the distribution if the effect we found is genuine. To test for the statistical significance of our results we follow a procedure inspired in Abadie and Dermisi (2008) and compute the one-side p-values for the total distribution of coefficients with respect to our original estimation.

The results of this randomization placebo test are shown in Fig. 4. The results of the randomization placebo test for our main estimation are perfectly consistent with our main effects being causal. First, we find a normal distribution of coefficients from the randomized treatment regressions. Second, the distribution is centered around zero. And, third, the true coefficient is located in the tails of the distribution, and the fraction of coefficients with a size bigger than our true estimates is below the required threshold for standard levels of confidence. Overall this implies that there is small probability of obtaining our results by random chance, thus providing evidence that our results are the consequence of a genuine effect of the infrastructure on the incumbent's vote share.

6.2. Pre-treatment placebo test

A second check of the robustness of our results is provided by a pre-treatment placebo test in which we use our main estimation but we include only observations for the pre-treatment period and consider the first year in our data as a placebo pre-treatment period and the two following years as post-treatment periods. Since none of the years used in this estimation were affected by the real treatment – because Madrid Rio Park was not even a proposal before 2003 – there should be no differences between the treated and control groups and our difference-in-differences strategy should not find any significant results. This test could be considered as additional evidence consistent with the fulfillment of the parallel trends assumption in the pre-treatment period.

The results of this placebo test is shown in Table 3. The difference-in-differences estimates for the treatment effect are not statistically significant in neither of the specifications. The point estimates are also small in magnitude. As in our main estimates, we also run this test using alternative specifications in which we increase the distance for which we consider units as part of the treatment group and the results do not change. Overall this table shows that there are no distinguishable differences between our placebo treatment and control groups, providing support to the credibility of our key findings and identification assumption.

7. Alternative hypothesis, mechanisms and extensions

In this section we discuss mechanisms and alternative hypothesis that could explain our results.

Table 3
Pre-treatment Placebo. Difference-in-differences estimates.

Dependent variable	200 m		400 m		600 m	
Incumbent's vote share	(1)	(2)	(3)	(4)	(5)	(6)
DD 1995	0.216 (0.914)	0.389 (0.917)	0.138 (0.613)	−0.0116 (0.620)	−0.111 (0.496)	−0.332 (0.543)
DD 1999	0.266 (2.105)	0.325 (2.093)	0.444 (1.389)	0.321 (1.395)	0.273 (1.077)	0.0998 (1.094)
Female population (%)		−0.304 (0.650)		−0.507 (0.401)		−0.0527 (0.280)
Over 65 population (%)		−0.119 (0.237)		−0.0322 (0.130)		−0.0590 (0.103)
Population with Univ. Studies (%)		0.136 (0.158)		−0.0717 (0.103)		−0.0655 (0.0605)
Tract fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Time fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Intercept	34.18*** (0.424)	49.98 (35.09)	31.80*** (0.273)	60.29*** (21.03)	30.31*** (0.214)	35.01** (14.33)
Observations	252	252	504	504	834	834
R-squared	0.702	0.713	0.621	0.633	0.543	0.561
Number of tracts	42	42	84	84	139	139

Clustered standard errors in parentheses.

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table 4
Alternative outcomes.

Dependent variable	(1) Turnout	(2) Turnout	(3) PSOE	(4) PSOE	(5) IU	(6) IU
DD 2003	−0.541 (0.709)	−0.510 (0.718)	−0.974 (0.805)	−0.909 (0.816)	−0.537 (0.404)	−0.560 (0.375)
DD 2007	1.056 (1.157)	1.110 (1.160)	−1.534* (0.825)	−1.389* (0.798)	−0.462 (0.352)	−0.515 (0.317)
DD 2011	0.286 (0.860)	0.401 (0.835)	−0.962 (0.893)	−0.751 (0.857)	0.0428 (0.494)	−0.0549 (0.419)
Female population (%)		−0.164 (0.203)		0.0177 (0.193)		0.0792 (0.0852)
Over 65 population (%)		0.0855 (0.0643)		0.119 (0.0802)		−0.107*** (0.0296)
Population with Univ. Studies (%)		0.0128 (0.0604)		0.0738 (0.0725)		0.0118 (0.0325)
Tract fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Time fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Constant	61.84*** (0.485)	68.94*** (11.33)	16.33*** (0.409)	12.29 (11.02)	5.535*** (0.133)	2.876 (4.643)
Observations	252	252	252	252	252	252
R-squared	0.830	0.832	0.662	0.670	0.792	0.812
Number of tracts	42	42	42	42	42	42

Clustered standard errors in parentheses.

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

7.1. Persuasion or mobilization

Our benchmark results have provided evidence of an increase in support for the incumbent in the area of influence of the infrastructure starting (weakly) in 2003 and becoming quite strong in 2007 and 2011. In this subsection we study whether the increase of support to the incumbent is explained by a persuasion effect, a mobilization effect, or both. If people that would otherwise not vote show up to the polls due to the infrastructure, the increase in support would be due to the higher mobilization of previous non-voters. If the infrastructure is able to change the vote of people that would have voted for other political parties had the infrastructure not been promised or constructed, the increase in support would be due to a persuasion effect.

While survey evidence could be preferable for a test of mobilization and persuasion effects, we can explore whether such effects are present using a similar model as our benchmark regressions of Table 1, in which we use as dependent variables turnout or votes shares of opposition parties, instead of the incumbent vote share, depending on whether we want to check for mobilization or persuasion.

The results of these tests is provided in Table 4. In Columns 1 and 2 we use turnout to check for mobilization effects. In Columns 3 to 6 we use the vote for the two main left-wing parties (PSOE and IU) to check for persuasion effects.

The difference-in-differences coefficients for 2003 point to persuasion explaining the (weak) prospective effects detected on this year. While non significant, the sign of the relevant coefficients are negative, which implies lower turnout around the areas of influence of the park, which is not consistent with mobilization effects. The coefficients of columns 3 to 6 are also non significant, but their negative sign and non-negligible magnitude is consistent persuasion effects.

The coefficients for the 2007 and 2011, which capture retrospective effects, show that both persuasion and mobilization explain the increase in support for the incumbent. The signs of the 2007 and 2011 coefficients in column 1 and 2 are now positive and of relevant magnitude, showing some mobilization effects, while the coefficients of columns 3 to 6 are negative, pointing to some persuasion effects.



Fig. 5. Socio-demographic variables in the post-treatment period. Control (red), treatment (blue). (For interpretation of the references to color in this figure legend, the reader is referred to the web version of this article.)

While the results of Table 4 should be taken with caution, they suggest that all in all, the promise and building of the infrastructure seems to have both mobilized some voters and persuaded others.

7.2. Demographic change and new voters

The construction of Madrid Río Park constitutes an improvement in the general attractiveness of the area, since it transformed the area from a peripheral district close to the highway noise and pollution, to a comfortable living district close to a green space. This increase in the attractiveness of the area may have resulted in a change of the demographics of the people living in the area, possibly with a demography close to the incumbent's electorate, thus explaining the increase in their vote.

We can test this hypothesis by studying the evolution of the observable socio-demographic characteristics in the matched sample for the treated and control groups. Since we matched the units with their observable characteristics in the pre-treatment period, we could only expect substantial differences in the post-treatment period if the construction of Madrid Río Park has altered the demography of the treated units by attracting new inhabitants.

A graphical representation of the socio-demographic variables in the post-treatment period is shown in Fig. 5. We can observe that the confidence intervals from the treated and control groups overlap in every year, which implies that they are not significantly different in any socio-demographic characteristic. In addition, we observe the same trends in both groups. Thus, we do not have evidence of a demographic change during the period of study in the treated tracts compared to the control tracts. This is not surprising given that the last election cycle of our study (2007 to 2011) coincided with the economic crisis that started in 2008 and, consequently, with the burst of the housing bubble. During those years, particularly in the latter ones, the housing market was in shock and there were very few transactions in the market, which helps our identification strategy. This is not to say that there was no demographic change in the area as results of the infrastructure, but that such a change likely took place once the economic crisis was over, outside our period of study.

7.3. Other infrastructures

Another alternative hypothesis that could explain our results is that during the elections within our period of study, other visible

infrastructure projects were constructed, such as the reform of the Madrid Underground transportation system (De La Calle and Orriols, 2010). Since these projects could have been rewarded by the voters in our study, they could affect our results. While this claim is reasonable, note that it would imply that the effects found here are, in fact, conservatively estimated. This is because there were no other relevant infrastructures that particularly affected the areas around Madrid Río. This means that other potential construction projects would have potentially benefited units in the control group more than units in the treatment group, thus reducing the size of the effects found in our results.

8. Conclusions

In this paper we have shown evidence that voters behave retrospectively with respect to Madrid Río Park. We also found weak evidence showing that a prospective mechanism might also be at work.

This result adds upon the evidence provided by the previous literature. While it is documented that a promise is more likely to be held than to be broken, voters do not trust politicians and they do not believe in their promises. We find that voters react more strongly to what politicians actually deliver than to what is promised, which in this case was the completion of Madrid Río Park.

Our results are also interesting to the literature on the electoral effects of public policies. The construction of large infrastructure projects is usually politically controversial because the promise to perform such projects immediately becomes a salient political issue. From the political point of view, such large projects are thought as prominent factors in the electoral success of politicians that lead the project. However, there is little empirical evidence on the electoral effect of such projects. In addition, theoretical models disagree on whether the effect of the infrastructure on the voting behavior of those most benefited by the infrastructure occur at the time the promise to perform the project is made or after it has been completed. We have been able to isolate the effects of the new infrastructure on the voting behavior of those most likely to benefit from the project, and to contribute to a better understanding of retrospective and prospective effects. We have only found weak evidence of prospective behavior, but we are able to detect clear evidence of retrospective behavior in which voters reward politicians only after the infrastructure has been completed.

This results find only weak support to models that predict that rational voters should vote also prospectively. However the result is

consistent with accountability models of the democracy in which a prominent role of voters' in the political process consists on rewarding and punishing incumbents' performance in office. In this regard a contribution of our paper is to inform our interpretation of current accountability models.

Finally, we conclude by noting that in order to provide results that have a causal interpretation we have used a case study with strong internal validity, sacrificing some external validity. While internal validity is a strength of our approach, the lack of external validity is a limitation. Testing the generalizability of our results is an avenue for future research.

Data availability

Data will be made available on request.

Appendix A. Supplementary data

Additional tables and references can be found online in the Supplementary material at <https://doi.org/10.1016/j.electstud.2022.102489>.

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