On the electoral effectiveness of pre-election policy promises

Matteo Alpino,[*] University of Oslo

This draft: March 20, 2017

Abstract

Can politicians gain votes by means of economic policy promises made during an election campaign? I empirically address this question by investigating voters' behavior in response to a specific electoral announcement: Silvio Berlusconi's promise to abolish the property tax on primary residences during the last days of the 2006 Italian election campaign. As homeowners would have been directly affected by the policy change, while renters would not have been, I am able to estimate the electoral effect of the announcement using a Difference-in-Differences approach in two independent datasets: an electoral survey and municipal-level electoral data. Both exercises suggest that Berlusconi's promise increased his vote share by a significant amount, which was large enough to be decisive.

1

Can politicians gain votes by means of economic policy promises made during the election campaign?

Candidates for political office make these promises very often; the latest example is Donald Trump, who pledged to enact several reforms during his race for the White House (e.g., to repeal Obamacare).[1] However, the extent to which these promises contributed to his unexpected victory remains unclear.

Traditional Downsian-type models assume pre-electoral promises to be binding and hence that they will be effective in convincing voters. However, some economists have criticized this assumption as unrealistic: Alesina (1988) shows that promises are time-inconsistent for policy-motivated politicians; Besley and Case (2003) argue that it is unreasonable to assume binding promises without explicitly specifying any commitment mechanism. Consistent with this line of criticism, some models assume that all pre-electoral policy promises are not credible (Barro, 1973; Besley and Coate, 1997; Osborne and Slivinski, 1996). In turn, these models seem at odds with the evidence that politicians keep most of their electoral pledges (Pétry and Collette, 2009; Barth, Finseraas, and Moene, 2015). Whether promises are credible is important because equilibrium policies are highly sensitive to this assumption. However, there is scarce empirical evidence on which stance is more appropriate. The aim of this paper is to contribute to the existing empirical literature on the issue by providing causal evidence that pre-electoral policy promises can have a large effect on the electoral outcome in high-stakes elections.

In order to identify the effect of electoral promises on voters, I exploit an episode with quasi-experimental features. During the 2006 electoral campaign for the Italian parliamentary election, incumbent Prime Minister

---

[1]See *Donald Trump's Contract with the American Voter* on www.donaldjtrump.com/contract/ for a list of promises.

Silvio Berlusconi promised the complete abolition of the property tax on all dwellings used as primary residences. He announced his promise live on national television during the last debate with his opponent, only six days before the election, and without having ever mentioned this reform before. Importantly, the proposed policy change was very specific; if enacted, it would have directly affected the tax bill of a highly identifiable group of voters, homeowners, leaving unchanged (at least in any direct way) the tax bill of the residual group (mainly renters). The basic idea behind my identification strategy exploits the last feature: I estimate a Difference-in-Differences (DID) model, assigning homeowners to the treatment group and the rest of the population (mainly renters) to the control group.

I apply the DID strategy to two different datasets. In section 2, I analyze an electoral survey in which respondents were interviewed twice, first a few months before the policy promise and again a few months after the election. Using both conventional and semiparametric DID, I estimate that homeowners are 6 percentage points (pp) more likely than renters to vote for Berlusconi's coalition due to the policy promise. In section 3, I analyze official electoral results at the municipal level in four consecutive elections. Using a DID design based on homeownership intensity, I estimate that in 2006, Berlusconi's vote share was differentially higher than in the previous three elections in municipalities with an higher fraction of homeowners, compared to municipalities with a lower fraction (0.7 pp higher per 1 standard deviation in homeownership intensity). Both results are robust to a battery of tests intended to strengthen the credibility of the main assumptions behind both exercises: parallel trends and the absence of contemporaneous group-level shocks.

The size of the effect is large: back-of-the envelope calculations based on the first DID suggest that the increase in vote share for Berlusconi's coalition was approximately 2 million votes; this figure roughly corresponds

3

to the difference between the final results and the support registered for Berlusconi's coalition in opinion polls before the policy promise (1.6 million). In other words, the effect is large enough to potentially tilt the outcome of a high-stakes election.

A recent paper by Elinder, Jordahl, and Poutvaara (2015) also estimates the effect of pre-electoral policy promises in high-stakes elections (Swedish national elections). Similar to the present paper, they rely on a DID strategy and find a positive effect. In contrast to the present paper, they focus on promises embedded in an official party manifesto; instead, I study a surprise promise announced on television shortly before the vote. Again in contrast to the present paper, they compare voters' choices across subsequent elections; instead, I have access to a survey where respondents were interviewed a few months before and after the promise. My tighter time window alleviates concerns related to contemporaneous group-level confounding shocks.[2]

The same research question motivates the recent branch of empirical work in political economy that seeks to determine whether voters affect or elect policies (Lee, Moretti, and Butler, 2004 and Pettersson-Lidbom, 2008). If promises are credible, elections force candidates to some degree of policy convergence; if not, voters can merely choose the candidate whose bliss point is closer to their own.

Finally, my paper is related to the studies that test whether voters reward incumbents after having received targeted transfers (Pop-Eleches and Pop-Eleches, 2012; Manacorda, Miguel, and Vigorito, 2011; Zucco, 2013; Labonne, 2013; Carozzi and Repetto, 2016).

---

[2]Other differences between the two papers include the policy itself (property tax vs. childcare benefits) and the ideological leaning of the proponent (conservative vs. social democrat). Both study national-level elections.

1   Background

Berlusconi served as prime minister between 2001 and 2006. In early 2006, he was predicted to fail to achieve re-election by 4.5 pp according to opinion polls.[3]

Six days before the election (April 3), public television hosted the final televised debate between Berlusconi and his opponent, Romano Prodi, the leader of the center-left coalition.[4] At the end of the debate, Berlusconi opened his final remarks with the following unanticipated announcement:

"The primary residence is sacrosanct to us, as it is the family. For this reason, we are going to abolish the property tax. Yes you got it right, we are going to abolish the property tax on all primary residences, including yours. This is a brave decision, but deeply sincere."[5]

The promised abolition would clearly apply only to primary residences, that is, to those dwellings that serve as main residence for the owner's household.[6] The announcement was completely unanticipated: the abolition of the property tax was not mentioned in the official platform of the center-right coalition.[7] Ultimately, Berlusconi did not manage to win the 2006 election, but he only lost by a handful of votes, much fewer than predicted by the opinion polls.[8] As a consequence, a center-left government was formed. Less than two years later, however, the new government was forced to resign, following the collapse of its slim parliamentary majority. In 2008, a new election was held, and Berlusconi's coalition won by

---

[3]Average since January 2006. Source: Wikipedia.

[4]In 2006 the Italian political landscape was dominated by two pre-election coalitions: center-right (headed by Berlusconi) and center-left (see online appendix A for details).

[5]Own translation of the Italian original, which is available in online appendix B.

[6]The economic watchdog *lavoce.info* estimated the tax cut to amount to approximately 3 billion euros.

[7]The original official platform, signed on February 24, is available upon request at the Online Historical Electoral Archive of the Ministry of the Interior.

[8]Berlusconi's coalition lost by approximately 25 thousand votes in the lower house and won by 400 thousand votes in the upper house.

a landslide. His new government immediately abolished the property tax on primary residences in his first legislative bill, thus keeping the promise made in 2006.[9] *Ex post* we can therefore regard Berlusconi's televised announcement as credible.
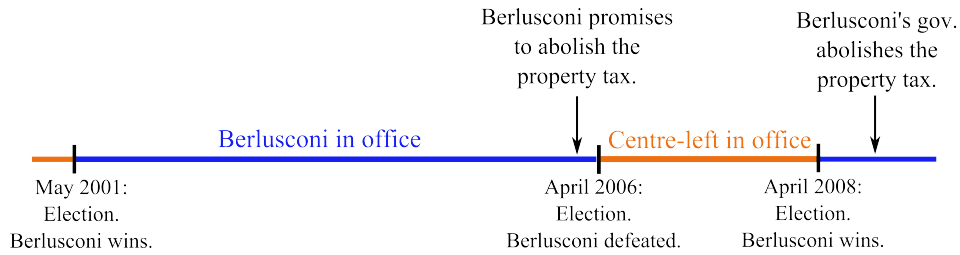
Figure 1: Timeline of political events

2 Evidence from the electoral survey

My goal is to estimate the voters' response to Berlusconi's promise to abolish the property tax on primary residences, using a DID strategy. In particular, I will test whether homeowners, who would have had their tax bills cut under the new reform, were more likely to vote for Berlusconi's coalition following his announcement; the renters serve as control group, as their tax bills would have been unaffected in any case.

Survey description The survey "Itanes 2006 Pre-Post" is a two-period panel electoral survey conducted around the 2006 elections by the Italian National Election Study (Itanes).[10] The respondents were first interviewed between February and March 2006, that is, before the election (April 9 and 10) and before the announcement (April 3), and then for a second time after the election, between April and June 2006 (see figure 2).

---

[9]The promise was also included in the 2008 platform. The abolition bill is legislative Decree 27 May 2008, n. 93.

[10]Itanes is an on-going project established in the 1990s in order to conduct pre- and post-election surveys. Itanes surveys have been already used in the economic literature by Durante and Knight (2012), and Durante, Pinotti, and Tesei (2015).

6

Importantly, the survey records whether each respondent lived in an owned dwelling (85 percent), or was instead renting at the time of the second interview.[11] Furthermore, the survey records vote intention at the time of the first interview (for both the lower and upper chambers) and asks about actual voting during the second interview (for both the lower and upper chambers). During the first interview, respondents were also asked about their voting decision in the previous national election held in 2001 (lower chamber only). Finally, respondents were asked detailed questions about their family and economic background.[12] The sample used in all the analysis below (with the exception of the multinomial logit regressions) excludes the respondents who refused to reply to the voting questions; in online appendix C, I discuss in detail the issues of attrition, reporting bias, and non-response, and I show that the incidence of non-response is identical among homeowners and renters.
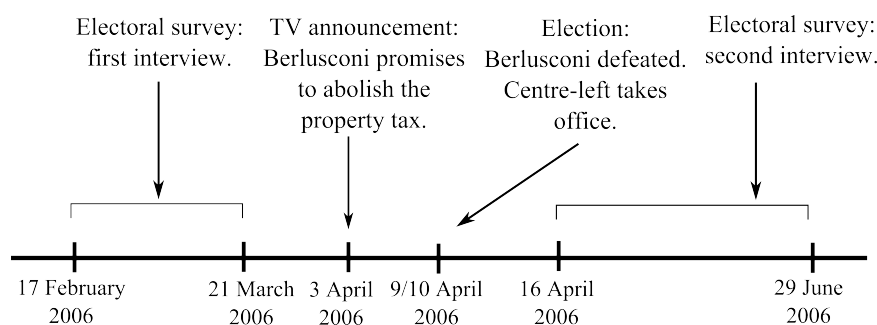
Figure 2: Timeline of events in 2006

Preliminaries   At the time of the first interview, any correlation between support for Berlusconi and homeownership cannot be driven by the yet-to-be-announced promise. Before turning to the main analysis, I mea-

---

[11]The survey question is as follows: "The dwelling where you live in is a) for rent; b) owned by you or by a family member of yours; c) refuse to reply." It was only asked during the second interview.

[12]The questionnaires are available in English at www.itanes.org/en/questionnaires (2006 A Pre and 2006 B Post).

sure this correlation by estimating some cross-sectional regressions on the determinants of vote intention in the first interview. Let I_i be a dummy equal to 1 if respondent i intends to vote for Berlusconi at the time of the first interview. Additionally, let H_i be a dummy for homeownership and X_i a vector of controls (vote in 2001 and demographic, education and employment characteristics).[13] I then estimate the following equation

$$I_i = \beta_0 + \beta_1 H_i + \beta_2 X_i + \varepsilon_i, \tag{1}$$

by OLS and WLS.[14] As the questionnaire includes separate questions for vote intentions for the lower and upper houses, I run separate regressions for the two. Table 1 reports the estimated coefficients, both for unconditional (odd columns) and conditional regressions (even columns). The estimate

Intended to vote for Berlusconi, first interview.

	Lower House				Upper House			
	(1) OLS	(2) OLS	(3) WLS	(4) WLS	(5) OLS	(6) OLS	(7) WLS	(8) WLS
Homeowner	0.029	-0.001	0.009	-0.003	0.026	-0.006	0.015	0.006
	(0.038)	(0.029)	(0.044)	(0.033)	(0.042)	(0.029)	(0.047)	(0.033)
Controls	No	Yes	No	Yes	No	Yes	No	Yes
N	1102	1102	1102	1102	967	967	967	967
R^2	0.00	0.47	0.00	0.45	0.00	0.50	0.00	0.49

Linear Probability Model. Itanes 2006 Pre-Post survey data. The dependent variable is I_i. OLS and WLS coefficients of H_i are reported. Controls include demographic, education, and employment characteristics and dummies for voting choices in 2001 (see online appendix for details). Only voters older than 24 years are eligible to vote for the upper chamber. Robust standard errors in parentheses. Stars * / ** / *** denote significance at the 0.10 / 0.05 / 0.01 level.

Table 1: Cross-sectional linear regressions.

of β_1 is always very close to zero and never significantly different from zero across the columns of table 1. In other words, before the promise, home-

---

[13] $I_i = 0$ includes respondents willing to vote for a different party, not vote at all, and who are undecided. $H_i = 1$ includes voters who reside in a dwelling owned by themselves or by another household member. I assume that homeownership status did not change between the two interviews. See online appendix C for descriptive statistics.

[14] Solon, Haider, and Wooldridge (2015) explain that in this case weighting is necessary for consistency if the sampling procedure is endogenous, and recommend reporting both OLS and WLS results. I weight using the probability weights included in the survey.

owners were not more likely than renters to support Berlusconi's coalition.

Conventional DID I define $W_{i,t}$ as a dummy equal to one if i intends to vote for Berlusconi (first interview, $t = 0$), or effectively voted for Berlusconi (second interview, $t = 1$).[15] I then regress $W_{i,t}$ on ownership status[16] interacted with a dummy for the second interview $After_t$, plus time and individual fixed effects[17]:

$$W_{i,t} = \tau H_i \times After_t + \alpha_i + \lambda_t + \varepsilon_{i,t} \tag{2}$$

where the error is clustered at the respondent level. In this setup the coefficient of interest, τ, captures the average effect of the promise on the treated respondents (ATT) under two assumptions: the absence of any contemporaneous shocks to the control and/or the treatment group and the parallel trends assumption.

The first assumption would be violated if one of the candidates were to have announced another policy targeting homeowners (or a group of voters who are more, or less, likely to be homeowners) between the first and the second interview. For example, a hypothetical promise to increase retirement benefits would be confounded with the property tax promise, as retirees are more likely to be homeowners than are other voters. I am unaware of any specific announcement of this kind, but I include in specification (2) dummies for certain employment categories and gender, interacted with the $After_t$ dummy.[18] Controlling for these interactions ensures that τ does not capture a trend driven by any of these groups.

The parallel trends assumption states that the pre-promise difference

---

[15]Non-respondents to the voting and vote intention questions are dropped.

[16]I assume that homeownership status did not change between the two interviews.

[17]Note that the inclusion of the individual fixed effects is not necessary for identification because ownership is constant over time for each respondent: if I were to replace the fixed effects α_i with the control H_i, the OLS estimate of τ would be the same.

[18]See online appendix C for a list and summary statistics of the controls.

in outcome between homeowners and renters would have been the same in the 2006 election, had Berlusconi not pledged to abolish the property tax. Figure 3 plots the share of votes for Berlusconi's coalition[19] (unweighted
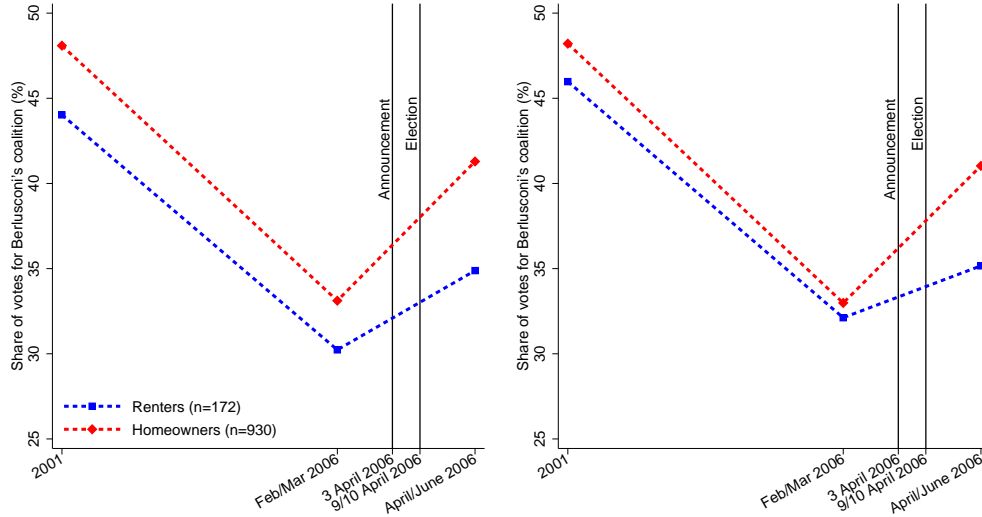
Figure 3: Parallel Trends - ITANES survey

data on the left, weighted on the right), separately for the treatment and control groups at three points in time: the 2001 election (as reported in the first interview), at time of the first interview (vote intention), and at the 2006 election (as reported in the second interview).[20] The plot is encouraging, but I also implement a placebo test: I estimate the DID specification (2) using as first period 2001, and as second period the first interview in 2006 (both before the promise). Panel A of table 2 reports the placebo estimates for the lower house, with or without interacted controls, estimated using both OLS and WLS. The placebo estimates are negative, very small in size, and never significantly different from zero. The parallel trends assumption is more plausible in light of this test, but I will relax it

---

[19]The average of $W_{i,t}$

[20]Lower house only, as vote in 2001 available only for the lower house. The graph comes with the additional assumption that anyone who was a renter (owner) in 2006 was already a renter (owner) in 2001.

when estimating the DID using the semiparametric procedure proposed by Abadie (2005).

Panel A - Placebo.

	Lower House			
	(1) OLS	(2) OLS	(3) WLS	(4) WLS
Homeowner × After	-0.011 (0.040)	-0.026 (0.039)	-0.004 (0.044)	-0.016 (0.043)
Controls × After	No	Yes	No	Yes
Time periods	2	2	2	2
Respondents	841	841	841	841

Panel B - Baseline, full sample.

	Lower House				Upper House			
	(1) OLS	(2) OLS	(3) WLS	(4) WLS	(5) OLS	(6) OLS	(7) WLS	(8) WLS
Homeowner × After	0.035 (0.029)	0.052* (0.030)	0.050 (0.032)	0.070** (0.033)	0.054* (0.032)	0.062* (0.033)	0.053 (0.038)	0.062 (0.038)
Controls × After	No	Yes	No	Yes	No	Yes	No	Yes
Time periods	2	2	2	2	2	2	2	2
Respondents	1102	1102	1102	1102	967	967	967	967

Panel C - Restricted sample: only heads of household and their partners.

	Lower House				Upper House			
	(1) OLS	(2) OLS	(3) WLS	(4) WLS	(5) OLS	(6) OLS	(7) WLS	(8) WLS
Homeowner × After	0.050 (0.033)	0.062* (0.034)	0.058 (0.039)	0.074* (0.040)	0.049 (0.035)	0.057 (0.036)	0.048 (0.042)	0.057 (0.042)
Controls × After	No	Yes	No	Yes	No	Yes	No	Yes
Time periods	2	2	2	2	2	2	2	2
Respondents	847	847	847	847	833	833	833	833

Linear Probability Model. Itanes 2006 Pre-Post survey data. Fixed effects at the respondent level are always included. Panel A reports placebo estimates: the outcome in the first period is vote for Berlusconi's coalition in 2001, and in the second is vote intention for Berlusconi's coalition in the first interview in 2006. In panels B and C, the dependent variable in the first period is vote intention for Berlusconi's coalition in the first interview, and actual vote for Berlusconi's coaliton in 2006 in the second period. After is a dummy for the second period. The estimates in panel A and B are on the full sample, while in panel C only heads of household and their partners are included. Controls include employment dummies and male dummy (see online appendix C). Only voters older than 24 years of age are eligible to vote for the upper chamber. Standard errors clustered at the respondent level in parentheses. Stars * / ** / *** denote significance at the 0.10 / 0.05 / 0.01 level.

Table 2: Conventional DID

Panel B of table 2 reports estimates of τ for both chambers, with or without interacted controls, estimated using both OLS and WLS. The DID estimate is approximately 6 pp, and it is slightly higher when the controls interacted with *After* are included. The estimates are quite stable in size across the columns of panel B, but not very precise, and occasionally

borderline insignificant at the 10% level. Clustering the standard errors at the municipality level, instead that at the respondent level, yields slightly tighter confidence intervals (see online appendix G).

In the analysis presented thus far, H_i defines as treated all those individuals who live in a owned dwelling; this definition is the only one possible with the information at hand.[21] However, it entails the disadvantage of also including in the treated group individuals who did not pay the property tax, despite living in a owned dwelling, e.g., young adults living with their parents. To address this concern, panel C of table 2 reports the DID estimates obtained on a sub-sample that includes only heads of households and their partners (both married and not): the magnitude of treatment effect is very similar.

Semiparametric DID   The parallel trends assumption is implausible if the selection into treatment is affected by some pre-promise variables, the distribution of which differs between homeowners and renters. For example, this would be the case if Berlusconi promised the abolition of the property tax in response to a decline in support among a group of people (e.g., disaffected center-right voters) that has more homeowners than the average.[22] If the parallel trends assumption only holds conditional on a vector of pre-promise variables X_i, the ATT is equal to the sample analog of

$$\mathbb{E}\left[\frac{W_{i,1} - W_{i,0}}{\mathbb{P}(H_i = 1)} \times \frac{H_i - \pi(X_i)}{1 - \pi(X_i)}\right] \tag{3}$$

where $\pi(X_i) = \mathbb{P}(H_i = 1 | X_i)$ is the propensity score (Abadie, 2005).[23] Employing this semiparametric DID estimator allows me to relax the par-

---

[21]See footnote 11 for the exact survey question.

[22]A sort of "Ashenfelter's dip" (Ashenfelter, 1978, Ashenfelter and Card, 1985). Disaffected center-right voters who own a dwelling may have eventually decided to vote for Berlusconi anyway, with or without the promise.

[23]Common support of the propensity score is also necessary.

allel trends assumption. I include in X_i individual-level demographic, education and employment characteristics and dummies for having voted for Berlusconi's coalition, and the centre-left coalition in 2001. The estimated effects are very similar to those obtained using the conventional DID, for both the lower and upper chambers (columns (1) and (3) of table 3). Abadie (2005)'s estimator also allows me to calculate the ATT for subgroups of the population. Columns (2) and (4) report the effect for the baseline group and the additional effect for those individuals who had already voted for Berlusconi's coalition in 2001. The promise increased the probability to vote for Berlusconi's coalition by 10 pp for those who had already voted for his coalition in 2001 (significant at the 10% level); the effect on the rest of the population is 2 pp and not significantly different from zero.

	(1) Lower House	(2) Lower House	(3) Upper House	(4) Upper House
Berlusconi voters 2001		0.080		0.089
		(0.062)		(0.070)
Constant	0.048*	0.019	0.060*	0.025
	(0.028)	(0.030)	(0.032)	(0.035)
Observations	1,096	1,096	953	953
P-value test of the sum		0.067		0.060

ATT estimates and standard errors (in parentheses) from the semiparametric DID by Abadie (2005), calculated using the Stata command `absdid` developed by Houngbedji (2016). The propensity scores are estimated using a linear equation; the regressors X_i include dummies for having voted for Berlusconi's coalition, and for the centre-left coalition in 2001 and demographic, occupational and educational characteristics. I impose common support by trimming the observations that fall outside it (see the propensity score histograms in online appendix F). Stars * / ** / *** denote significance at the 0.10 / 0.05 / 0.01 level.

Table 3: Semiparametric DID

Suggestive evidence on voting flows The conventional DID shows that the support for Berlusconi's coalition grew differentially more among homeowners, compared to the rest of the electorate; furthermore, the semiparametric DID suggests that the effect is driven by those who had voted for Berlusconi in the previous election. However, it is unclear what exactly they intended to do before the promise. In fact, undecided voters, those

13

who intended to abstain, and those who intended to vote for a different party are are all grouped together in $W_{i,0} = 0$. To provide some suggestive answers to this question, I turn to a unordered multinomial logit model. I separately estimate the effect of homeownership before (in the first interview) and after (in the second interview) the announcement on the probability of multiple voting choices.[24] The multinomial logit regressions always include demographic, employment and education controls, plus dummies for previous voting choices and/or intentions.[25] I report the average partial effects of homeownership on the homeowners (APT) in table 4.[26] The effect of homeownership on the probability to vote for Berlusconi's coalition is very small and insignificant before the promise (panel A), while it becomes positive (5 to 6 pp) and significant thereafter (panel B). Moreover, the effect on the probability of abstaining is very small and insignificant before the promise (panel A), while it becomes negative (-3 pp) and significant thereafter (panel B). This suggests that roughly half of the electoral increase for Berlusconi's coalition is driven by voters who intended to abstain at the time of the first interview. The effect of homeownership on the probability of voting for the center-left or for another party is small and insignificant in both the first and the second interview, suggesting that the rest of the increase for Berlusconi's coalition was driven by undecided voters. Although the results in this section amount to correlations and not to causal effects, they are consistent with the DID. Coupled with the previous evidence, they suggest that the promise was more effective in convincing disaffected center-right voters, who were undecided or planned to abstain at the time of the first interview. As such, they are consistent with voting models based on the comparison of utility under different candidates, where

---

[24]The dependent variable in the first interview takes six options: a) abstain, b) center-left, c) Berlusconi's coalition, d) another party, e) refuse to reply, f) undecided. In the second interview, it takes only a), b), c), d) and e).

[25]See the note to table 4 and online appendix C.

[26]The APT can be interpreted as ATT because the treatment is binary.

Panel A - Vote intention, first interview.

	Lower House					
	Abstain	Centre-Left	SB's coal.	Other	No Reply	Undecided
Homeowner	-0.004	-0.037	-0.013	0.003	0.039	0.012
	(0.014)	(0.025)	(0.028)	(0.006)	(0.026)	(0.032)
Respondents	50	445	386	8	139	349

	Upper House					
	Abstain	Centre-Left	SB's coal.	Other	No Reply	Undecided
Homeowner	-0.007	-0.030	-0.012	-0.003	0.023	0.029
	(0.015)	(0.024)	(0.028)	(0.005)	(0.025)	(0.035)
Respondents	47	389	353	9	112	306

Panel B - Vote decision, second interview.

	Lower House				
	Abstain	Centre-Left	SB's coal.	Other	No Reply
Homeowner	-0.034**	-0.012	0.048*	0.000***	-0.002
	(0.013)	(0.026)	(0.026)	(0.000)	(0.024)
Respondents	96	616	462	1	202

	Upper House				
	Abstain	Centre-Left	SB's coal.	Other	No Reply
Homeowner	-0.030**	-0.030	0.062**	0.000***	-0.001
	(0.015)	(0.027)	(0.029)	(0.000)	(0.026)
Respondents	84	529	411	2	190

Multinomial Logit Model. Itanes 2006 Pre-Post survey data. Controls include dummies for voting choice in the 2001 election and demographic, educational and employment characteristics. The controls in panel B also include dummies for different vote intentions in the first interview (see online appendix C). The table reports the average partial effects of homeownership calculated on the subpopulation of the homeowners (APT). Each APT is the effect on the probability of the outcome specified on top of each column. APTs in each row are obtained from a different multinomial logit regression. Vote intention (dependent variable in panel A) has six possible outcomes; vote decision (dependent variable in panel B) has five possible outcomes. Only voters older than 24 years of age are eligible to vote for the upper chamber. The sample size of each regression is the sum of the row Respondents. Non-responders are included in the sample. The standard errors in parentheses are calculated using the linearization method. Stars * / ** / *** denote significance at the 0.10 / 0.05 / 0.01 level.

Table 4: Multinomial Logit

promises are more effective on voters who are almost indifferent between different alternatives.

3 Evidence from official electoral data

Survey data may in principle suffer from misreporting problems. I therefore complement the previous evidence with an analysis of official electoral data. However, the latter suffers from the disadvantage of being available only at the time of election and not at the individual level.

Data description The electoral data are from the online historical archive of the Ministry of the Interior. I pool the results of all parliamentary elections from when Berlusconi entered politics until 2006 (1994, 1996, 2001 and 2006) at the municipal level, and I calculate the share of votes for Berlusconi's coalition as a percentage of the total electorate. I restrict the dataset to the lower house only. My sample consists of the vast majority of Italian municipalities (approximately 8000). The Italian census (1991, 2001 and 2011) records whether each household a) lives in a dwelling owned by one of its members, b) rents, or c) lives under a different arrangement. I calculate the share of households living in a flat owned by one of its members as a percentage of total households in each municipality for each census year. I match election years to census years by the closest year available. The control variables are also from the same census waves. [27]

DID strategy The unit of observation is the municipality-election year, and the data feature a panel of approximately 8000 municipalities followed for four elections. I exploit variation across municipalities in the share of homeowners in the population to implement a DID estimation based on the intensity of homeownership at the municipality level. The (ideal) control group consists of all the municipalities in which the share of homeowners is exactly zero. The treatment intensity is then linearly increasing in the share of homeowners in each municipality: the more home-

---

[27]See online appendix D for details and discussion on the construction of the dataset.

owners there are in a municipality, the more voters are affected by the property tax announcement. The availability of electoral data both before the announcement (1994, 1996 and 2001) and after (2006) allows me to implement a DID by interacting a 2006 dummy with the share of home-owners. Fixed effects at the municipality level account for time-invariant differences across observational units. Let $Y_{m,t}$ and $H_{m,t}$ be the share of votes for Berlusconi's coalition and the share of homeowners, respectively, in municipality m and election t, and additionally let me define λ_t as the time fixed effects and μ_m as the municipality fixed effects. My baseline specification is then as follows:

$$Y_{m,t} = \theta H_{m,t} \times \lambda_{2006} + \zeta H_{m,t} + \lambda_t + \mu_m + \varepsilon_{m,t} \qquad (4)$$

where the coefficient of interest is θ.[28] Standard errors are clustered at the municipality level to account for serial correlation.[29] I weight regression (4) by the number of eligible voters in the municipality because the election of interest is at the national level. My strategy relies again on the absence of contemporaneous shocks, and on the parallel trends assumption. The first assumption is stronger here than in the previous section, as the period of potential confounding factors is 5 years long. I am unaware of any policy or shock specifically targeting homeowners during this period, but it may nevertheless be the case that one of the policies enacted during the 2001-06 term, or some other shock, affected a group of voters whose distribution across municipalities is correlated with homeownership intensity. I address this concern by augmenting the specification in (4) with some municipality-level controls interacted with the 2006 dummy.[30]

---

[28]Examples of recent papers that rely on a similar DID strategy are Mian and Sufi (2012) and Casaburi and Troiano (2015).

[29]See Bertrand, Duflo, and Mullainathan (2004). The online appendix also reports standard errors clustered at the province level (102 clusters).

[30]See table 5 for a list of controls and the online appendix for summary statistics.

To provide support for the parallel trends assumption, I plot the share of votes for Berlusconi's coalition over time for the bottom four deciles (in blue), and the top four deciles (in red) of the homeownership distribution in 2006. Two things stand out: first, the trend is roughly parallel across deciles, with the lines almost never crossing each other; second, up until the 2001 election, the red lines (most treated deciles) are always below the blue ones (less treated deciles), indicating lower support for Berlusconi in municipalities with high homeownership, but this pattern reverts in 2006.[31] Finally, I will also augment the specification in (4) with province-level or municipality-level linear trends.
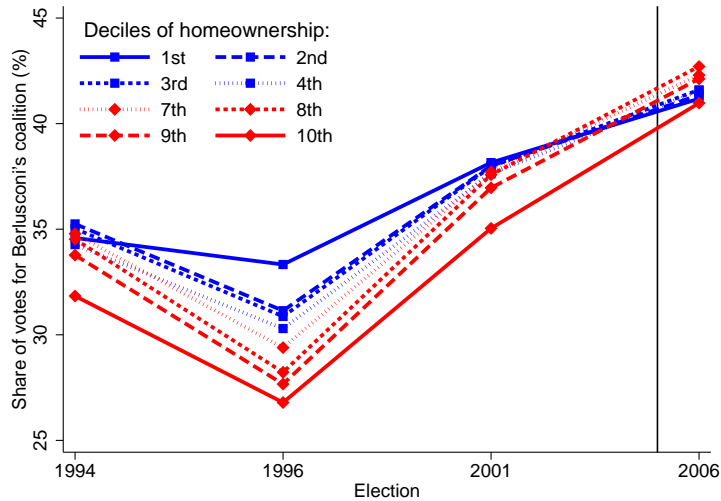
Figure 4: Parallel Trends - Municipal-level data

Results Estimates of θ from equation (4) are reported in table 5. The coefficient in the baseline specification (first column) is positive, significant and robust to the inclusion of province-level linear trends in column (2) and municipality-level linear trends in column (3). In column (4), I control for some municipality characteristics interacted with a dummy for the 2006 elections: the point estimate is not significantly different from the

---

[31]For a different parallel trends plot see online appendix D.

18

estimate in column (1) but again significantly different from zero. Finally, I simultaneously include controls and province-level trends, in column (5), and controls and municipal-level trends, in column (6): the point estimate is still positive and significant, but the size is smaller than in column (1) (18 pp vs. 11 pp). The most conservative specification, presented in column (6), implies that, when comparing two municipalities one standard deviation apart in terms of homeownership share (6.5%), the municipality with the higher share shows an electoral increase of 0.7 pp for Berlusconi's coalition in the 2006 elections relative to the previous elections. Overall the evidence presented here is consistent with the results reported in the previous section.

	Share of votes for Berlusconi's coalition					
	(1)	(2)	(3)	(4)	(5)	(6)
Homeowners (share) × 2006	0.18***	0.20***	0.13***	0.17***	0.13***	0.11***
	(0.02)	(0.02)	(0.04)	(0.02)	(0.02)	(0.02)
Municipality FE	Yes	Yes	Yes	Yes	Yes	Yes
Local Linear Trends	No	Province	Municipality	No	Province	Municipality
Controls × 2006	No	No	No	Yes	Yes	Yes
No. of periods	4	4	4	4	4	4
No. of municipalities	7988	7988	7988	7988	7988	7988

The dependent variable is the share of eligible voters who voted for Berlusconi's coalition. Each period is one election (1994, 1996, 2001, 2006). 2006 is a dummy for the 2006 election. Homeowners (share) is the share of households (out of total resident households) living in a residence owned by one of its members. The controls include: share of residents with highschool degree or higher degree; share of residents with no education; share of students; share of housewives; share of retirees. Each regression also includes each term of each interaction separately. The regressions are weighted by eligible voters in municipality. Standard errors clustered at the municipality level. Stars * / ** / *** denote significance at the 0.10 / 0.05 / 0.01 level.

Table 5: Municipal level DID

4   Interpretation and conclusion

Back-of-the-envelope calculations based on the DID estimates from section 2 suggest that the announcement increased the number of votes for Berlusconi's coalition by approximately 2 million votes; calculations based on

section 3 yield an higher figure, approximately 3.7 million.[32] Are these figures reasonable? To place them in proper perspective, I can compare them with the opinion polls and with the actual election outcome. In the months before the elections, opinion polls predicted a lead for the center-left coalition over Berlusconi's coalition of, on average, 4.5 pp, corresponding to 1.6 million votes.[33] In the actual election, however, the center-left won by fewer than 25 thousand votes in the lower house and even lost by more than 400 thousand votes in the upper house. The causal effect of the announcement estimated based on the electoral survey is thus of similar magnitude to the gap in favor of the center-left predicted by the pollsters, which then vanished in the actual election. In other words, the size of the causal effect estimated in this paper is consistent with the story, popular among Italian political commentators, stating that the promise to abolish the property tax was the decisive factor in closing the gap between the two coalitions in 2006.[34] More important for the scope of this paper, the size of the causal effect is of reasonable magnitude, thereby enhancing the credibility of the empirical results; moreover, the effect is large enough to potentially tilt the outcome of the election, and thus, it is economically and politically significant.

My quasi-experimental evidence on the effect of pre-electoral policy promises (the promised abolition of the property tax) on voting behavior shows that voters who would have experienced a net economic benefit from the promised fiscal reform (a lower tax bill) were more likely to vote in favor of its proponent. Accordingly, my results speak in favor of a set of assumptions that are common in many political economy models: office-motivated politicians are able to commit to policy platforms before the

---

[32]The two figures are not statistically different from each other. See online appendix E for details on their calculation.

[33]Source: Wikipedia.

[34]See, for example, the article "La metà d'Italia s'è turata il naso" in *La Repubblica*, 16 April 2006.

election, and voters take this into account in their voting decisions. I show not only that pre-electoral policy promises can have an electoral impact but also that this effect can be large enough to potentially reverse the outcome of a high-stakes election.

My test is admittedly conducted under rather favorable conditions: the promise was clear, specific, verifiable, had a predictable effect on voters' tax bills, and experienced widespread media coverage.[35] As such, it is not likely that my results generalize to all types of elections or policy promises, but their internal validity is strong.[36] What policy promises are most credible? Candidates for which offices have higher commitment capacity? Further research is needed to uncover the answers to these questions.

---

[35]Homeowners directly pay the property tax every year (it is not withheld from income), making it easy to calculate the reduction in taxes and allowing them to easily monitor the effective implementation of the abolition.

[36]Local elections may have lower media coverage than national elections. Candidates running for a seat in parliament may have less commitment capacity (compared to candidates for prime minister/president), as they cannot pass legislation alone. The implementation of certain tax cuts (e.g., to the marginal income tax rate) is more complicated to verify than the property tax analyzed here.

References

Abadie, Alberto (2005), "Semiparametric difference-in-differences estimators." *The Review of Economic Studies*, 72, 1–19, URL http://restud.oxfordjournals.org/content/72/1/1.abstract.

Alesina, Alberto (1988), "Credibility and policy convergence in a two-party system with rational voters." *The American Economic Review*, 78, pp. 796–805, URL http://www.jstor.org/stable/1811177.

Ashenfelter, Orley (1978), "Estimating the effect of training programs on earnings." *The Review of Economics and Statistics*, 60, 47–57, URL http://www.jstor.org/stable/1924332.

Ashenfelter, Orley and David Card (1985), "Using the longitudinal structure of earnings to estimate the effect of training programs." *The Review of Economics and Statistics*, 67, 648–660, URL http://www.jstor.org/stable/1924810.

Barro, RobertJ. (1973), "The control of politicians: An economic model." *Public Choice*, 14, 19–42, URL http://dx.doi.org/10.1007/BF01718440.

Barth, Erling, Henning Finseraas, and Karl O. Moene (2015), "Political reinforcement: How rising inequality curbs manifested welfare generosity." *American Journal of Political Science*, 59, 565–577, URL http://dx.doi.org/10.1111/ajps.12129.

Bertrand, Marianne, Esther Duflo, and Sendhil Mullainathan (2004), "How much should we trust differences-in-differences estimates?" *The Quarterly Journal of Economics*, 119, 249–275, URL http://qje.oxfordjournals.org/content/119/1/249.abstract.

Besley, Timothy and Anne Case (2003), "Political institutions and policy choices: Evidence from the united states." *Journal of Economic Literature*, 41, 7–73.

Besley, Timothy and Stephen Coate (1997), "An economic model of representative democracy." *The Quarterly Journal of Economics*, 112, 85–114, URL http://www.jstor.org/stable/2951277.

Carozzi, Felipe and Luca Repetto (2016), "Distributive politics inside the city? the political economy of spains plan e." Unpublished.

Casaburi, Lorenzo and Ugo Troiano (2015), "Ghost-house busters: the electoral response to a large anti tax evasion program*." *The Quarterly Journal of Economics*, URL http://qje.oxfordjournals.org/content/early/2015/10/28/qje.qjv041.abstract.

Durante, Ruben and Brian Knight (2012), "Partisan control, media bias, and viewer responses: Evidence from berlusconi's italy." *Journal of the European Economic Association*, 10, 451–481, URL http://dx.doi.org/10.1111/j.1542-4774.2011.01060.x.

Durante, Ruben, Paolo Pinotti, and Andrea Tesei (2015), "The political legacy of enterainment tv." Unpublished.

Elinder, Mikael, Henrik Jordahl, and Panu Poutvaara (2015), "Promises, policies and pocketbook voting." *European Economic Review*, 75, 177 – 194, URL http://www.sciencedirect.com/science/article/pii/S0014292115000112.

Houngbedji, K. (2016), "Abadie's semiparametric difference-in-differences estimator." *Stata Journal*, 16, 482–490(9), URL http://www.stata-journal.com/article.html?article=st0442.

Labonne, Julien (2013), "The local electoral impacts of conditional cash transfers: Evidence from a field experiment." *Journal of Development Economics*, 104, 73 – 88, URL http://www.sciencedirect.com/science/article/pii/S0304387813000692.

Lee, David S., Enrico Moretti, and Matthew J. Butler (2004), "Do voters affect or elect policies? evidence from the u. s. house." *The Quarterly Journal of Economics*, 119, 807–859, URL http://qje.oxfordjournals.org/content/119/3/807.abstract.

Manacorda, Marco, Edward Miguel, and Andrea Vigorito (2011), "Government transfers and political support." *American Economic Journal: Applied Economics*, 3, 1–28, URL http://www.aeaweb.org/articles?id=10.1257/app.3.3.1.

Mian, Atif and Amir Sufi (2012), "The effects of fiscal stimulus: Evidence from the 2009 cash for clunkers program*." *The Quarterly Journal of Economics*, URL http://qje.oxfordjournals.org/content/early/2012/07/26/qje.qjs024.abstract.

Osborne, Martin J. and Al Slivinski (1996), "A model of political competition with citizen-candidates." *The Quarterly Journal of Economics*, 111, 65–96, URL http://qje.oxfordjournals.org/content/111/1/65.abstract.

Pétry, François and Benoît Collette (2009), *Measuring How Political Parties Keep Their Promises: A Positive Perspective from Political Science*, 65–80. Springer New York, New York, NY, URL http://dx.doi.org/10.1007/978-0-387-89672-4_5.

Pettersson-Lidbom, Per (2008), "Do parties matter for economic outcomes? a regression-discontinuity approach." *Journal of the European Economic*

Association, 6, 1037–1056, URL http://dx.doi.org/10.1162/JEEA.2008.6.5.1037.

Pop-Eleches, Cristian and Grigore Pop-Eleches (2012), "Targeted government spending and political preferences." *Quarterly Journal of Political Science*, 7, 285–320, URL http://dx.doi.org/10.1561/100.00011017.

Solon, Gary, Steven J. Haider, and Jeffrey M. Wooldridge (2015), "What are we weighting for?" *The Journal of Human Resources*, 50, 301–316.

Zucco, Cesar (2013), "When payouts pay off: Conditional cash transfers and voting behavior in brazil 200210." *American Journal of Political Science*, 57, 810–822, URL http://dx.doi.org/10.1111/ajps.12026.

ONLINE APPENDIX NOT FOR PUBLICATION

Appendix A   The Italian Political System

Italy is a parliamentary democracy; voters elect representatives in two chambers. Italian citizens 18 year old or older are eligible to elect representatives to the lower house only; citizens 25 year old or older are eligible to elect representatives to both. The Prime Minister is appointed by the President of the Republic, who usually choses for the job the leader of the winning party or coalition. The Prime Minister forms a government which must have a majority in both houses of the parliament.

Between 1994 and 2010, the Italian political system has been dominated by the competition between two main coalitions: the centre-right led by Berlusconi, and the centre-left; taken together, they included most of the existing political parties. Berlusconi, formerly a business man, entered politics shortly before the 1994 election, which he won; he served as prime minister only for less than one year because his government lost parliamentary support due to the defection of one coalition party. Berlusconi then lost the 1996 election, which was followed by five years of centre-left government. He won instead the 2001 election, managing to serve as prime minister for the whole next five-year term. Between 1994 and 2001, the electoral law (Law 4 August 1993, n. 276 and 277) prescribed a hybrid system, partly proportional and partly majoritarian.[37]

Few months before the 2006 election, Berlusconi's parliamentary major-

---

[37]In particular, in the lower house 75 percent of the seats were assigned in first-past-the-post single-member districts, and 25 percent according to proportional representation (with separate ballot papers); in the upper house 75 percent of the seats were assigned in first-past-the-post single-member districts, while the rest were assigned to lower ranked candidates in the single-member districts, in proportion to the vote share of the party they were affiliated with.

ity legislated a new electoral law (Law 21 December 2005, n. 270). The new law introduced proportional representation for both chambers, with a majority bonus system in favor of the winning coalition.[38] In order to be eligible for the majority bonus, single parties must form a pre-election coalition by officially indicating a joint leader and a joint platform. Both the name of the leader, and the platform must be submitted to the Ministry of the Interior. Coalition leaders were *de facto* candidates to become Prime Minister. All the large, medium and small parties joined either one of the two main coalitions well before the election, leaving outside only a dozen of micro parties who run independently and whose joint share eventually amounted to less than 0.5%. All in all, the Italian political system was effectively bipolar in 2006, and therefore I always consider Berlusconi's coalition as a unique entity, and never analyze the results of its single member parties.

In 2006 Berlusconi's coalition lost by 25 thousands votes in the lower house, and even won by 400 thousands vote in the upper house. Nevertheless, this resulted in a centre-left majority in both houses (quite large in the lower house, only two seats more in the upper house). Romano Prodi, the head of the centre-left coalition, managed thus to form a government.

In this paper, Berlusconi's coalition is defined as the electoral cartel supporting Berlusconi as candidate to Prime Minister. It was named *Casa della Liberta'* (House of Freedoms) in 2006. Berlusconi's coalition is not stable over time: *Forza Italia* (Berlusconi's own party), *Alleanza Nazionale* and the Christian Democrats (under different denominations) have been always part of the coalition between 1994 and 2006. *Lega Nord* instead was part of the coalition only in 1994, 2001 and 2006, but run as an inde-

---

[38]In particular the law established a majority bonus to the winning coalition at the *national* level (excluding votes in the region Valle d'Aosta) for the lower house, and majority bonuses to the coalition winning in every single region (excluding Valle d'Aosta and Trentino-Alto Adige) for the upper house. Italy is divided in twenty regions, the highest sub-national administrative level, roughly comparable to US states.

pendent party in 1996, following its withdraw of parliamentary support to Berlusconi's government in late 1994.

Appendix B   The property tax

ICI, literally "Municipal Tax on Properties", is the official name of the property tax mentioned throughout this paper. It was introduced in 1993.[39]

The tax base consisted of buildings, building plots and farmlands, and was based on the cadastral value of the property (defined as estimated return from renting). Restricting the focus on buildings, the cadastral value depended on their destination of use: category A (private residence), category B (public buildings, e.g. schools, barracks, storage points etc) and category C (commercial purposes, e.g. shops, cowsheds etc). Within the A category, the cadastral value varied across sub-categories[40] and was an increasing function of size (measured in number of rooms). Furthemore, in bigger municipalities the cadastral value depended on the exact geographical location of each building, with the municipality surface split in up to three zones.

The tax rate to be applied to the cadastral value was set by each municipal government between 4% and 7%. A standard deduction of 180,000 Italian Lira (about 90 Euro) was applied to the taxes levied on those dwellings used as main residence by the owner. Since 1997 municipal governments were also allowed to apply differential tax rates for dwellings used as main residence.[41] Furthermore, the standard deduction was increased to 200,000

---

[39]Legislative Decree 11 July 1992, n.333 and Legislative Decree 30 December 1992, n. 504

[40]A1 - Luxury Regions; A2 - Regular Regions; A3 - Economic Regions; A4 - Popular Regions; A5 - Super-popular Regions; A6 - Rural Regions; A7 - Small villas; A8- Villas; A9 - Historical castles and palaces; A10 Private offices; A11 - Typical regional Regions (e.g. cabins).

[41]Law 23 December 1996, nr. 662.

Italian Lira (about 100 Euro), but municipal governments were allowed to increased it up to 500,000 Italian Lira (about 250 Euro). Alternatively, they were allowed to reduce the tax rate on main residences by at most 50%.

All the revenues from the tax accrued to the municipalities, which under the Italian law are governed by a mayor, and by a city council, both elected every five years.

Berlusconi's announcement The original announcement by Berlusconi on April 3, 2006 is as follows: *"Per noi la casa, la prima casa, è sacra come è sacra la famiglia. Per questo aboliremo l'ICI (i.e. the property tax). Avete capito bene: aboliremo l'ICI su tutte le prime case, e quindi anche sulla vostra. E' una decisione coraggiosa, ma profondamente sentita."*

Appendix C   Itanes survey

C.1   Controls and descriptive statistics

Controls in table 1 include: dummy for having voted for the Berlusconi's coalition in 2001 (lower house only), and dummy for having voted for the centre-left coalition in 2001 (lower house only). Demographic variables: age, male dummy, dummy for married, dummy for single (omitted category is divorced). Education variables: college dummy, high school dummy, mid-vocational school dummy, no education dummy. Employment dummies: blue collar, unemployed, housewife, student, retired, white collar, self-employed dummy, worker on temporary contract, specialist (includes medical doctors, lawyers, notaries, architects, etc.), and employer with at least four employees. Geographic residence dummies: north and south.

Interacted controls in table 2 include dummies for males, college graduate, blue collar, unemployed, housewife, student, retired, white collar, self-employed, worker on temporary contract, specialist (medical doctors, lawyers, notaries, architects, etc.), and employer with at least four employees.

Regressors used to estimate the propensity scores for the semiparametric DID in table 3 include: dummy for having voted for the Berlusconi's coalition in 2001 (lower house only), and dummy for having voted for the centre-left coalition in 2001 (lower house only). They also include: Demographic variables: age, male dummy, dummy for married, dummy for single (omitted category is divorced). Education variables: college dummy, high school dummy, mid-vocational school dummy, no education dummy. Employment dummies: blue collar, unemployed, housewife, student, retired, white collar, self-employed, worker on temporary contract, specialist (medical doctors, lawyers, notaries, architects, etc.), and employer with at least four employees. Geographic residence dummies: north and south.

Controls included in the multinomial logit regressions in panel A of table 4 are the same as in table 1, plus a dummy for no reply to the housing question. Controls included in the multinomial logit regressions in panel B of table 4 are the same as in panel A plus: dummy for vote intention for Berlusconi's coalition in the first interview, dummy for vote intention for centre-left coalition in the first interview, dummy for being undecided when asked about vote intention in the first interview.

Table A6:   Differences in means (weighted samples)

	Renter	Homeowner	Δ	s.d.
Age	44.72	48.47	-3.76	1.60
Married	0.66	0.64	0.02	0.06
Single	0.37	0.35	0.01	0.04
Male	0.45	0.49	-0.04	0.05
College degree	0.06	0.10	-0.04	0.02
Highschool degree	0.31	0.37	-0.06	0.04
Midschool/vocational degree	0.46	0.33	0.12	0.05
No education	0.02	0.03	-0.01	0.01
Blue collar	0.34	0.16	0.18	0.04
Unemployed	0.06	0.06	0.01	0.02
Housewife	0.17	0.14	0.03	0.03
Student	0.07	0.06	0.00	0.02
Retired	0.17	0.26	-0.09	0.04
Self-employed	0.19	0.18	0.01	0.04
White collar	0.17	0.22	-0.06	0.03
Temporary contract	0.10	0.05	0.05	0.03
Specialist (doctor, lawyer, notary, etc.)	0.03	0.03	-0.01	0.02
Employer (at least four employees)	0.05	0.03	0.02	0.02
North	0.37	0.44	-0.07	0.05
South	0.33	0.35	-0.01	0.04
N	172	930	.	.
Freq.	0.16	0.84	.	.

	Renters	Homeowners	Δ
Age	44.63	47.40	-2.76**
Married	0.65	0.64	0.00
Single	0.37	0.35	0.02
Male	0.48	0.52	-0.04
College degree	0.07	0.10	-0.03
Highschool degree	0.31	0.38	-0.06
Midschool/vocational degree	0.44	0.35	0.09**
No education	0.02	0.03	-0.00
Blue collar	0.31	0.17	0.14***
Unemployed	0.07	0.06	0.01
Housewife	0.17	0.14	0.03
Student	0.05	0.07	-0.02
Retired	0.16	0.24	-0.08**
Self-employed	0.19	0.20	-0.01
White collar	0.17	0.22	-0.05
Temporary contract	0.10	0.05	0.05**
Specialist (doctor, lawyer, notary, etc.)	0.02	0.04	-0.02
Employer (at least four employees)	0.04	0.03	0.01
North	0.37	0.41	-0.04
South	0.37	0.39	-0.02
N	172	930	

$^{*}\ p < 0.10,\ ^{**}\ p < 0.05,\ ^{***}\ p < 0.01$

Table A7: Differences in means (unweighted samples)

Table A8:   Differences in means (weighted), lower house

	Renters	Homeowners	Δ	s.d.
Vote for B in 2006	0.35	0.41	-0.06	0.04
Vote for B in 2001	0.46	0.48	-0.02	0.05
Vote intention for B in 2006	0.32	0.33	-0.01	0.04
N	172.00	930.00	.	.
Freq.	0.16	0.84	.	.

	Renters	Homeowners	Δ
Vote for B in 2006	0.35	0.41	-0.06
Vote for B in 2001	0.44	0.48	-0.04
Vote intention for B in 2006	0.30	0.33	-0.03
N	172	930	

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

Table A9: Differences in means (unweighted), lower house.

Table A10:   Differences in means (weighted), upper house

	Renters	Homeowners	Δ	s.d.
Vote for B in 2006	0.35	0.42	-0.07	0.05
Vote intention for B in 2006	0.33	0.34	-0.01	0.05
N	149	818	.	.
Freq.	0.15	0.85	.	.

	Renters	Homeowners	Δ
Vote for B in 2006	0.34	0.42	-0.08*
Vote intention for B in 2006	0.32	0.34	-0.03
N	149	818	

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

Table A11: Differences in means (unweighted), upper house.

Homeowners	Other (2nd int)	Berlusconi (2nd int)	Total
Other (1st int)	525	97	622
Berlusconi (1st int)	21	287	308
Total	546	384	930

Renters	Other (2nd int)	Berlusconi (2nd int)	Total
Other (1st int)	105	15	120
Berlusconi (1st int)	7	45	52
Total	112	60	172

Table A12: Cross tabulation of vote and vote intention - Lower House

Homeowners	Other (2nd int)	Berlusconi (2nd int)	Total
Other (1st int)	452	87	539
Berlusconi (1st int)	21	258	279
Total	473	345	818

Renters	Other (2nd int)	Berlusconi (2nd int)	Total
Other (1st int)	90	12	102
Berlusconi (1st int)	8	39	47
Total	98	51	149

Table A13: Cross tabulation of vote and vote intention - Upper House

C.2 Attrition

The sample of the first interview included 2005 individuals. Right after the first interview 1307 participants declared themselves available for the follow-up after the election, 431 not available, and 267 not sure yet. This initial attrition is not worrisome because the decision to participate in the second round was taken before the announcement, and therefore is exogenous. The second interview includes 1137 of those who declared themselves available beforehand (86%) and 240 of those who were not sure (90%) for a total of 1377 individuals. Unfortunately, I can not investigate whether this attrition is correlated with homeownership because the housing question is asked only in the second round. However, any attrition due to homeowners convinced by the promise, but ashamed to admit so, would not be a problem for the DID identification; in fact this kind of attrition would only

lead to an underestimate of the true effect, not an overestimate.

C.3   Non response in the survey

A common problem in electoral surveys is non response, because many individuals refuse to reveal their voting decision. Dropping the individuals who did not respond from the sample would lead to biased estimates if the *treatment* variable is correlated with the probability of not responding.

The ITANES sample makes no exception in this respect: approximately 15% of the respondents refused to disclose their voting choice during the second interview, and about 2% refused to disclose their housing status (see Table A14 for details). Both questions are asked after the promise.

Table A14:   Respondents and non response

	All	Age≥25
Respondents in both interviews	1377	1216
Refused to disclose his/her vote in 2nd interview	202	190
Refused to disclose his/her vote intention in 1st interview	88	70
Refused to disclose his/her housing status	34	32
Respondents used in DID	1102	967

To investigate the severity of this problem in my specific application, I regress a dummy equal to one if the respondent revealed his vote, on two dummies: the first equal to one if the respondent is an homeowner and the second equal to one if the respondent is not, so that the residual category is made by those who did not respond to the housing question:

$$NoReplyVote_i = \theta_0 + \theta_1 Renter_1 + \theta_2 Homeowner_i + \varepsilon_i \qquad (5)$$

The coefficients estimated via OLS, Probit and Logit model are reported in table A15. Non-repliers to the housing question, are 76 pp more likely to refuse to reply to the voting question as well, compared to other individuals

(first test in table A15, $H_0 : \theta_1 = \theta_2$). However, and most importantly, homeowners are not significantly more or less likely to refuse to reply to the voting question compared to renters (second test in table A15, $H_0 : \theta_1 = 0; \theta_2 = 0$). In light of this stark result, I safely drop non-repliers to both questions from the sample used in the main DID analysis.

I also drop those who do not reply to the vote intention question in the first interview.

On the contrary, the sample used in the multinomial logit analysis always include all the respondents, and no reply is considered as one of the possible choices (see table 4 for the number of respondents in each category).

	Lower House			Upper House		
	(1) OLS	(2) Logit	(3) Probit	(4) OLS	(5) Logit	(6) Probit
Renter	-0.241***	-1.324***	-0.774***	-0.255***	-1.349***	-0.796***
	(0.087)	(0.406)	(0.244)	(0.091)	(0.416)	(0.251)
Homeowner	-0.242***	-1.331***	-0.778***	-0.257***	-1.361***	-0.802***
	(0.084)	(0.363)	(0.223)	(0.088)	(0.371)	(0.229)
$H_0 : \theta_1 - \theta_2 = 0$, p-value	0.97	0.97	0.97	0.96	0.96	0.96
$H_0 : \theta_1 = 0; \theta_2 = 0$, p-value	0.02	0.00	0.00	0.01	0.00	0.00
N	1377	1377	1377	1216	1216	1216

Notes: estimated coefficients from equation (5). Robust standard errors in parenthesis. Stars * / ** / *** denote significance at the 0.10 / 0.05 / 0.01 level.

Table A15: Homeownership and non reply

C.4 Reporting bias

Respondents may of course decide to misreport their voting intention, or voting choice, or both. This problem is a threat to identification only if the decision to misreport is caused by the property tax promise. Vote intention is recorded before the promise, so it is not a cause of serious concern. However, vote is recorded in the second interview. Anecdotal evidence suggests that Berlusconi voters do not like to disclose their vote, due to social stigma; furthermore, some voters may not like to disclose to have voted for Berlusconi after his defeat, because some people do not like

to side with a loser. However, both arguments do not point to a differential reporting bias between homeowners and renters, and so they are not a cause of concern. One may finally image that some voters who previously stated not to intend to vote for Berlusconi, but eventually did due to the property tax promise, are ashamed to admit so, revealing to have voted on egoistic ground. However, this mechanism would work against finding any effect; so if this concern is grounded, my analysis estimates a lower bound.

Appendix D Official electoral data

Dataset   The electoral system of the upper house was based on single-member districts between 1994 and 2001, making it difficult to compare the electoral results with the proportional system in 2006. The lower house had instead a proportional tier (25% of the seats) between 1994 and 2001, directly comparable to the electoral law in place in 2006. For this reason, I restict my analysis to the electoral results of the lower house, and I calculate the vote share for Berlusconi's coalition in each municipality in the proportional tier.

I exclude few municipalities: a) the municipalities that split or merge during the period under investigation, b) the municipalities whose electoral result is problematic (e.g. turnout greater than 100%; this problem occurs due to the presence of non-residents allowed to vote in a municipality different from their place of residence, like members of the armed forces or police on duty); c) all the municipalities in the autonomous region Valle d'Aosta (about 0.2% of total Italian population, 1 seat in the lower house), where a different electoral law applied, and so the two main coalitions were not running under the same denomination as in the rest of the country.

Homeownership intensity at the municipal level is not available for each electoral year, but only in Census years (1991, 2001 and 2011). Thus I adopt the following criterion in order to match this homeownership intensity to the electoral data: a) for electoral years overlapping with a Census year, I simply use the variable from the corresponding Census (this is the case with year 2001); b) for electoral years not overlapping with a Census year, I use the closest Census wave (Census 1991 is linked to the 1994 election); c) for electoral years not overlapping with a Census year, and equidistant from the preceding and next Census wave, I use a simple average of the two (the variable matched to the 1996 election is the average

between Census 1991 and 2001, and the variable matched to the 2006 election is the average between Census 2001 and 2011).

The results are similar if instead election-years are matched to the closest preceding Census (see table A20). From the same Census waves, I also collect the following control variables: as a share of residents older than 6 years: a) residents with a high school degree or higher, b) residents without any school degree; and as a share of residents older than 14 years: c) students, d) housewives, and e) retirees. I adopt the same criterion as above to match Census variables to election variables.

Additional parallel trends plot   A different way to provide support for the parallel trends assumption is the following: I plot the regression lines $E[Y_{m,t}|H_{m,t}]$, estimated by nonparametric local constant regressions, separately for each election year.[42] Parallel regression lines in the pre-promise period necessarily imply parallel pre-promise time trends between homeowners and renters, as illustrated in figure A5. Indeed, figure A6 shows that the relationship between $Y_{m,t}$ and $H_{m,t}$ is roughly parallel across the years 1994, 1996 and 2001; the lines are approximately flat for low values of $H_{m,t}$ and clearly decreasing for higher values.[43] Conversely, the regression line for 2006 is flat everywhere, if not slightly increasing for higher values of $H_{m,t}$.

---

[42]Epanchikov kernel with a bandwidth equal to 0.25 pp.
[43]With the exception of a level drop in the 1994 line around $H_{m,t}$=0.73.

	Year				Overall
	1994	1996	2001	2006	
Votes for Berlusconi's	0.343	0.296	0.375	0.417	0.358
coalition	(0.138)	(0.0822)	(0.107)	(0.112)	(0.120)
Home	0.769	0.771	0.773	0.769	0.771
	(0.0914)	(0.0782)	(0.0715)	(0.0655)	(0.0773)
Housewives	0.141	0.137	0.132	0.117	0.132
	(0.0473)	(0.0422)	(0.0456)	(0.0381)	(0.0444)
Students	0.0622	0.0638	0.0655	0.0660	0.0644
	(0.0169)	(0.0164)	(0.0208)	(0.0185)	(0.0183)
Retired	0.202	0.224	0.246	0.264	0.234
	(0.0855)	(0.0746)	(0.0790)	(0.0681)	(0.0806)
High school graduates	0.501	0.387	0.274	0.317	0.370
or higher degree	(6.834)	(3.421)	(0.0642)	(0.0636)	(3.836)
Without education degree	0.291	0.205	0.120	0.105	0.181
	(1.439)	(0.720)	(0.0617)	(0.0484)	(0.812)
N	7997	7993	7837	7931	

Notes: the table reports means (main figures) and standard deviations (in parenthesis) for the variables listed in the first column, in different election years. **Votes for Berlusconi's coalition** is calculated as a share of elegible voters; **Homeowners (share)** is the share of households (out of resident households) living in an estate owned by one of its members; **Housewives**, **students** and **retired** are calculated as a share of population older than 14 years old; **High school graduates or with higher degree**, and **Without education degree** are calculated as a share of population older than 6 years old.

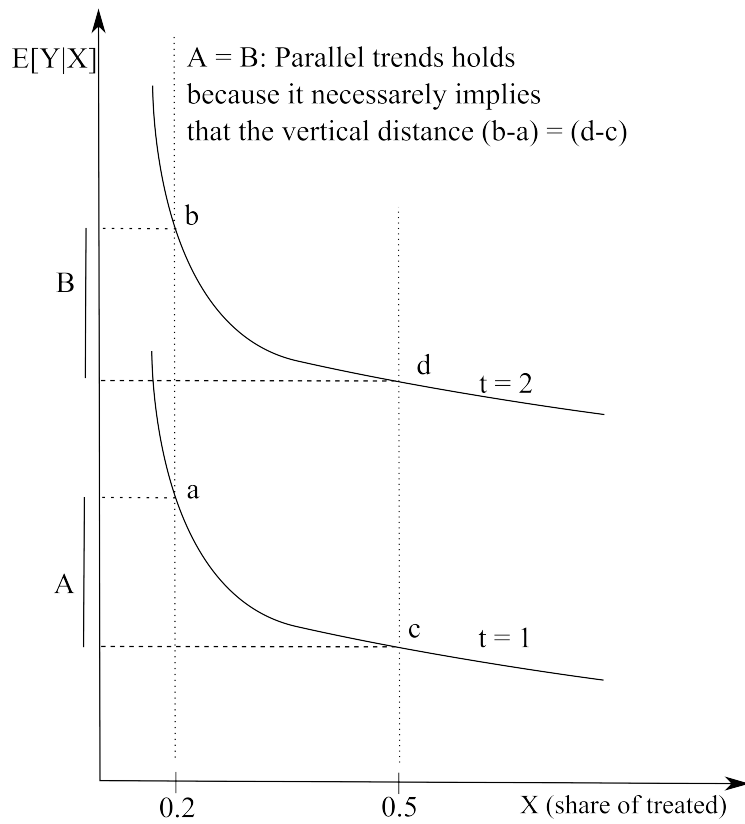Table A16: Summary statistics of the municipality-level data
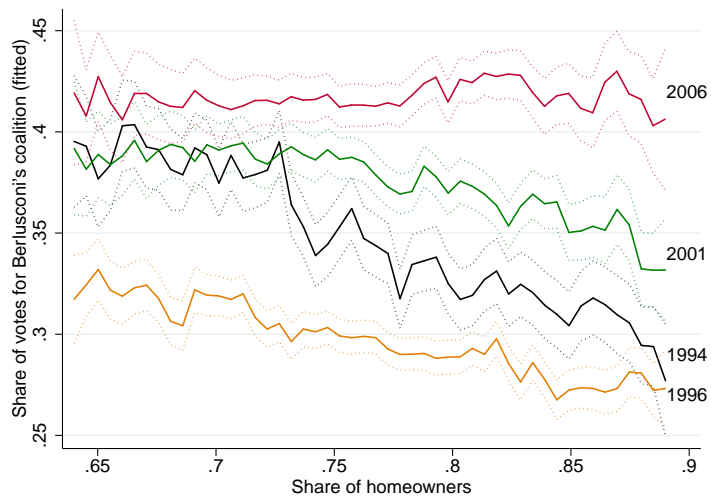
Figure A5: Example

Figure A6: Local constant fitted regression lines

Appendix E   Back-of-the-envelope calculation

The most conservative DID specification from table 2 in section 2 (ITANES survey) yields an effect around 6.5 pp; I multiply this figure by the percentage size of the treatment group (homeowners) in the overall Italian population (71% according to the Survey on Household Income and Wealth from the Bank of Italy) to yield 4.6 pp. The Difference-in-Difference regressions of section 2 are estimated on the full sample of citizens having the right to vote, including those who did not turn out at the 2006 election. Therefore, by multiplying the previous figure by the total size of the electorate (46.99 millions in the lower house and 42.2 in the upper one) it is possible to obtain the estimate of the electoral effect of the announcement in terms of absolute votes: around 2 millions (2.1 in the lower house, and 1.9 in the upper house).

The DID coefficient in the most conservative specification of section 3 is 11 pp. I multiply this figure by the size of the treatment group in each municipality (% of homeowners), and then by the size of the electorate in each municipality (individual having the right to vote, i.e. the denominator of the outcome variable in all the regressions), to have an estimate of the effect in absolute votes in each municipality. Summing these estimates across all the municipalities yield an aggregate effect of about 3.7 millions.

The average of opinion polls conducted between November 2005 and March 2006 assigned to the centre-left coalition 51.51 percent of the votes, and to Berlusconi's coalition 46.98 percent of the votes. These figures must be multiplied by the number of effective valid votes in 2006, i.e. 38.15 millions in the lower house and 34.15 in the upper, to obtain a rough estimate of the gap between the two coalitions in absolute numbers: 1.7 million votes against Berlusconi's block in the lower house, and 1.5 in the upper one. One may argue that the actual turnout, and so the number

of valid votes, is endogenous to Berlusconi's announcement, and therefore too high to be used in calculating the gap in the opinion polls. However, if we assume that the turnout would have been lower by -say- 3 million valid votes, the opinion polls still imply an absolute gap of 1.6 millions in the lower house, and 1.4 in the upper house.

In the actual election, the centre-left won by less than 25 thousand votes (49.8% vs. 49.7%) in the lower house, and lost by more than 400 thousand votes in the upper house (43.8% vs. 45%) (actual data).

Appendix F   Additional figures
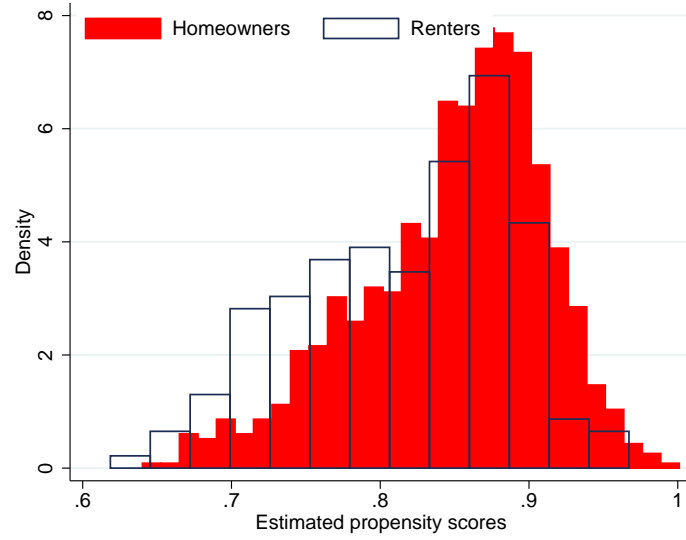

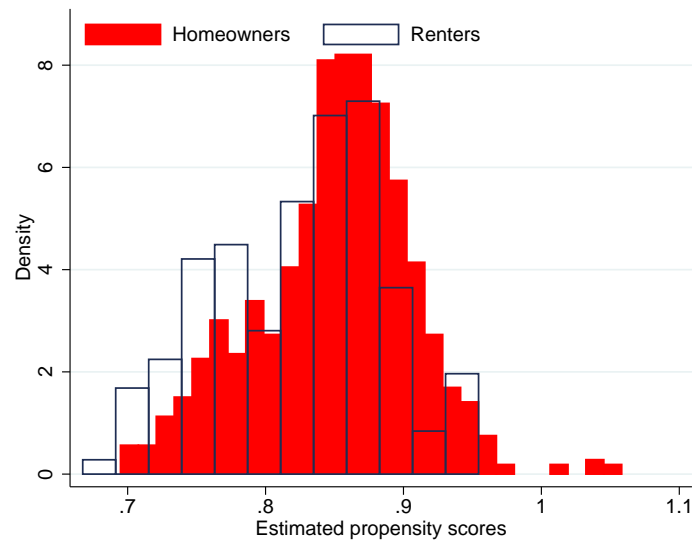Figure A7: Distribution of propensity score - Itanes survey, lower house.

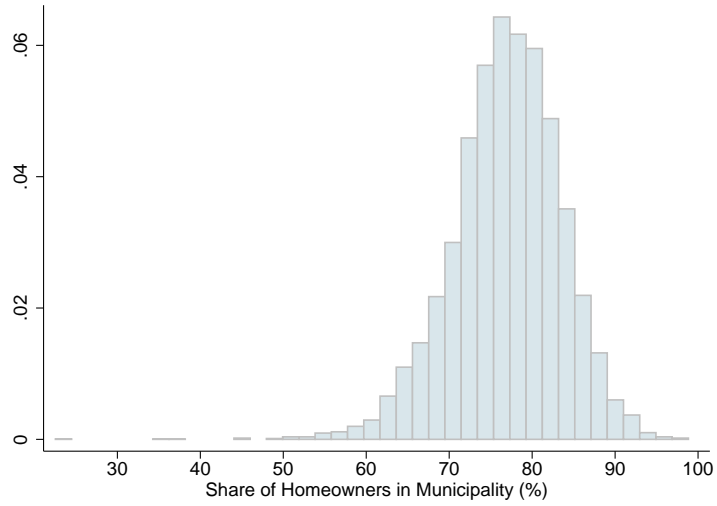Figure A8: Distribution of propensity score - Itanes survey, upper house.

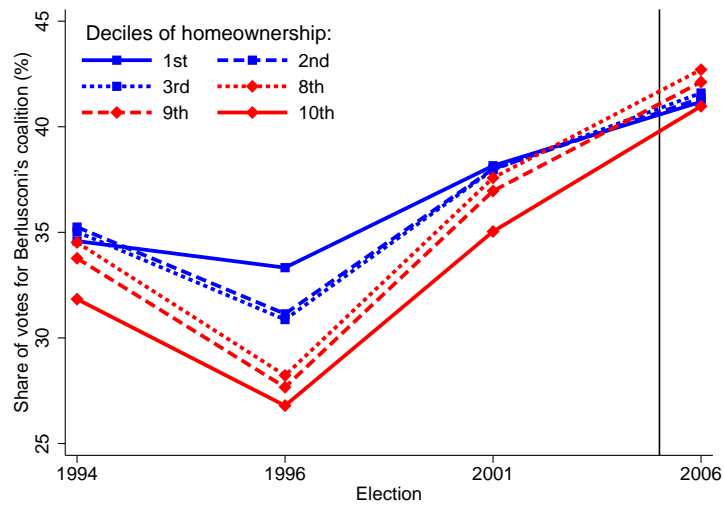Figure A9: Distribution of Homeownership in 2006

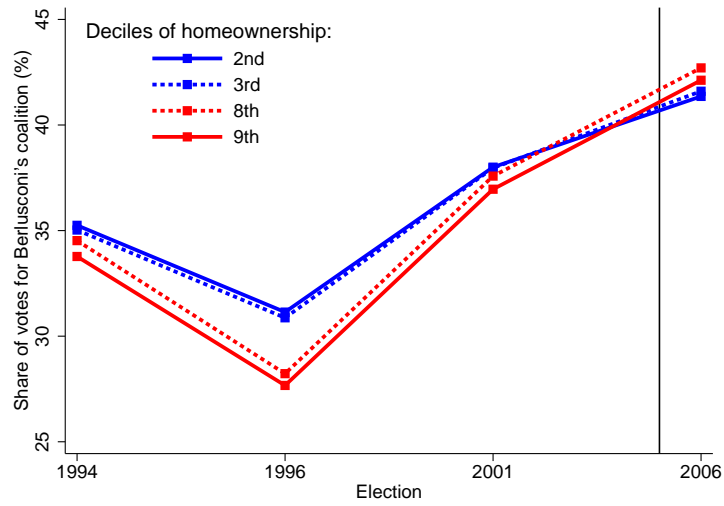

Figure A10: Parallel Trends - Municipal-level data

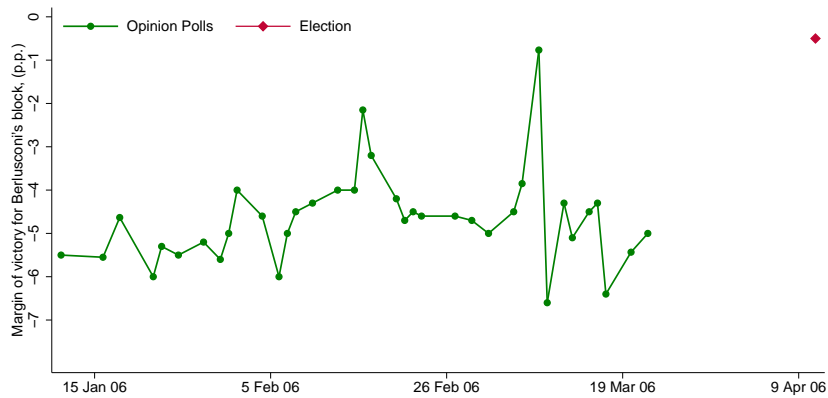Figure A11: Parallel Trends - Municipal-level data

Figure A12: Opinion polls, various pollsters, daily average. Source: Wikipedia.

Appendix G Additional tables

G.1 Additional Itanes linear regressions

Panel A - Placebo.

	Lower House			
	(1) OLS	(2) OLS	(3) WLS	(4) WLS
Homeowner × After	-0.011	-0.026	-0.004	-0.016
	(0.042)	(0.040)	(0.045)	(0.043)
Controls × After	No	Yes	No	Yes
Time periods	2	2	2	2
Respondents	841	841	841	841

Panel B - Baseline, full sample.

	Lower House				Upper House			
	(1) OLS	(2) OLS	(3) WLS	(4) WLS	(5) OLS	(6) OLS	(7) WLS	(8) WLS
Homeowner × After	0.035	0.052**	0.050*	0.070**	0.054	0.062*	0.053	0.062*
	(0.024)	(0.025)	(0.029)	(0.029)	(0.033)	(0.033)	(0.037)	(0.037)
Controls × After	No	Yes	No	Yes	No	Yes	No	Yes
Time periods	2	2	2	2	2	2	2	2
Respondents	1102	1102	1102	1102	967	967	967	967

Panel C - Restricted sample: only heads of household and their partners.

	Lower House				Upper House			
	(1) OLS	(2) OLS	(3) WLS	(4) WLS	(5) OLS	(6) OLS	(7) WLS	(8) WLS
Homeowner × After	0.050*	0.062*	0.058	0.074**	0.049	0.057	0.048	0.057
	(0.030)	(0.032)	(0.037)	(0.037)	(0.036)	(0.037)	(0.042)	(0.041)
Controls × After	No	Yes	No	Yes	No	Yes	No	Yes
Time periods	2	2	2	2	2	2	2	2
Respondents	847	847	847	847	833	833	833	833

Linear Probability Model. Itanes 2006 Pre-Post survey data. Fixed effects at the respondent level are always included. Panel A reports placebo estimates: the outcome in the first period is vote in 2001, and the outcome in the second period is vote intention in the first interview in 2006. In panels B and C, the dependent variable in the first period is vote intention in the first interview, and actual vote in 2006 in the second period. After is a dummy for the second period. The estimates in panel A and B are on the full sample, while in panel C only heads of household and their partners are included. Controls include employment dummies (see the online appendix). Only voters older than 24 years of age are eligible to vote for the upper chamber. Standard errors clustered at the municipality level in parentheses. Stars * / ** / *** denote significance at the 0.10 / 0.05 / 0.01 level.

Table A17: Conventional DID - Standard errors clustered at the municipality level

G.2 Multinomial Logit on ITANES survey

Panel A - Vote intention, first interview.

| | Lower House | | | | | |
	Abstain	Centre-Left	SB's coal.	Other	No Reply	Undecided
Homeowner	0.003	-0.037	-0.007	0.003	0.032	0.008
	(0.017)	(0.027)	(0.030)	(0.004)	(0.029)	(0.035)
Respondents	50	445	386	8	139	349

| | Upper House | | | | | |
	Abstain	Centre-Left	SB's coal.	Other	No Reply	Undecided
Homeowner	0.001	-0.038	0.007	-0.005	0.013	0.022
	(0.017)	(0.027)	(0.030)	(0.005)	(0.027)	(0.037)
Respondents	47	389	353	9	112	306

Panel B - Vote decision, second interview.

| | Lower House | | | | |
	Abstain	Centre-Left	SB's coal.	Other	No Reply
Homeowner	-0.045***	-0.004	0.071**	0.000***	-0.023
	(0.015)	(0.027)	(0.028)	(0.000)	(0.026)
Respondents	96	616	462	1	202

| | Upper House | | | | |
	Abstain	Centre-Left	SB's coal.	Other	No Reply
Homeowner	-0.034*	-0.023	0.072**	0.000***	-0.015
	(0.017)	(0.028)	(0.032)	(0.000)	(0.028)
Respondents	84	529	411	2	190

Multinomial Logit Model, weighted by the probability of being included in the sample. Itanes 2006 Pre-Post survey data. Controls include dummies for voting choice in the 2001 election, and demographic, education and employment characteristics. Controls in panel B also include dummies for different vote intentions in the first interview (see online appendix). The table reports the average partial effects of homeownership calculated on the subpopulation of the homeowners (APT). Each APT is the effect on the probability of the outcome specified on top of each column. APTs in each row are obtained from a different multinomial logit regression. Vote intention (dependent variable in Panel A) has six possible outcomes, vote decision (dependent variable in Panel B) has five possible outcomes. Only voters older than 24 years old are eligible to vote for the upper chamber. The sample size of each regression is the sum of the row Respondents. Non repliers are included in the sample. The standard errors in parentheses are calculated using the linearization method. Stars * / ** / *** denote significance at the 0.10 / 0.05 / 0.01 level.

Table A18: Average marginal effects of homeownership, multinomial logit, weighted.

G.3 DID with real election data

	Share of votes for Berlusconi's coalition					
	(1)	(2)	(3)	(4)	(5)	(6)
Homeowners (share) × 2006	0.18***	0.20***	0.13	0.17***	0.13***	0.11*
	(0.03)	(0.06)	(0.10)	(0.05)	(0.05)	(0.06)
Municipality FE	Yes	Yes	Yes	Yes	Yes	Yes
Local Linear Trends	No	Province	Municipality	No	Province	Municipality
Controls × 2006	No	No	No	Yes	Yes	Yes
No. of periods	4	4	4	4	4	4
No. of municipalities	7988	7988	7988	7988	7988	7988

The dependent variable is the share of residents (out of eligible voters) who voted for one of the parties belonging to Berlusconi's coalition. Each period is one election (1994, 1996, 2001, 2006). 2006 is a dummy for the 2006 election. Homeowners (share) is the share of households (out of total resident households) living in a residence owned by one of its members. The controls include: share of residents with highschool degree or higher degree; share of residents with no education; share of students; share of housewives; share of retirees. Each regression also includes each term of each interaction separately. The regressions are weighted by eligible voters in municipality. Standard errors clustered at the province-level in parentheses. Stars * / ** / *** denote significance at the 0.10 / 0.05 / 0.01 level.

Table A19: Municipal level DID with s.e. clustered at the province level

	Share of votes for Berlusconi's coalition					
	(1)	(2)	(3)	(4)	(5)	(6)
Homeowners (share) × 2006	0.19***	0.19***	0.06	0.14***	0.16***	0.12***
	(0.02)	(0.02)	(0.04)	(0.02)	(0.02)	(0.02)
Municipality FE	Yes	Yes	Yes	Yes	Yes	Yes
Local Linear Trends	No	Province	Municipality	No	Province	Municipality
Controls × 2006	No	No	No	Yes	Yes	Yes
No. of periods	4	4	4	4	4	4
No. of municipalities	7988	7988	7988	7988	7988	7988

The dependent variable is the share of residents (out of eligible voters) who voted for one of the parties belonging to Berlusconi's coalition. Each period is one election (1994, 1996, 2001, 2006). 2006 is a dummy for the 2006 election. Homeowners (share) is the share of households (out of total resident households) living in a residence owned by one of its members. The controls include: share of residents with highschool degree or higher degree; share of residents with no education; share of students; share of housewives; share of retirees. Each regression also includes each term of each interaction separately. The regressions are weighted by eligible voters in municipality. Standard errors clustered at the province-level in parentheses. Stars * / ** / *** denote significance at the 0.10 / 0.05 / 0.01 level.

Table A20: Municipal level DID - Alternative Census match

	Turnout					
	(1)	(2)	(3)	(4)	(5)	(6)
Homeowners (share) × 2006	0.09**	0.14***	0.14**	0.08**	0.10***	0.10**
	(0.04)	(0.05)	(0.06)	(0.04)	(0.04)	(0.05)
Municipality FE	Yes	Yes	Yes	Yes	Yes	Yes
Local Linear Trends	No	Province	Municipality	No	Province	Municipality
Controls × 2006	No	No	No	Yes	Yes	Yes
No. of periods	4	4	4	4	4	4
No. of municipalities	7988	7988	7988	7988	7988	7988

The dependent variable is turnout. Each period is one election (1994, 1996, 2001, 2006). 2006 is a dummy for the 2006 election. Homeowners (share) is the share of households (out of total resident households) living in a residence owned by one of its members. The controls include: share of residents with highschool degree or higher degree; share of residents with no education; share of students; share of housewives; share of retirees. Each regression also includes each term of each interaction separately. The regressions are weighted by eligible voters in municipality. Standard errors clustered at the municipality level. Stars * / ** / *** denote significance at the 0.10 / 0.05 / 0.01 level.

Table A21: Municipal level DID with s.e. clustered at the municipality level