

Would you vote for me if I lower your taxes? *

PRELIMINARY DRAFT - PLEASE DO NOT CITE OR CIRCULATE

Matteo Alpino
(University of Oslo & ESOP, PhD Student)

Version: February 15, 2016

Abstract

Can politicians win votes by announcing to cut taxes during the electoral campaign? I tackle this question empirically by investigating voters' behaviour in response to a specific electoral announcement: Silvio Berlusconi's promise to abolish the property tax on main residences during the last days of campaign for the 2006 Italian general election. Under the identifying assumption that homeowners would have been directly affected by the policy change, while renters would have not, I estimate the Difference-in-Differences effect of the announcement in two independent datasets: a panel of municipalities, and an electoral survey. Both exercises suggest that Berlusconi's announcement increased his vote share by a significant and sizable amount. Additional results show that the announcement had the opposite effect on those voters who would have borne the cost of the reform.

*I especially thank my advisors Halvor Mehlum and Andreas Kotsadam. I am also grateful for comments to Alberto Alesina, Audinga Baltrunaite, Jacopo Bizzotto, Michela Carlana, Monique de Haan, James Fenske, Askill Halse, Rune Jansen Hagen, Giampaolo Lecce, Andreas Müller, Eivind Olsen, Andrea Papini, Paolo Piacquadio, Carlo Prato, Alessia Russo, Guido Tabellini, Gaute Torsvik, Santiago Perez Vincent and reading-group participants at University of Oslo and at Bocconi University, and seminar participants at the annual meeting of the Norwegian Association of Economists. I also thank the IFEL foundation for the provision of ICI data. I finally thank Bocconi University for their hospitality, as this paper was partly written while I was a visiting student at their Economics department. All mistakes are my own.

1 Introduction

Can politicians gain votes by mean of last minute fiscal announcements during the electoral campaign? In particular, can politicians gain votes by announcing lower taxes, without specifying how to finance the new reform?

Different political economics models would yield different answers to this question, depending on the assumptions on the politicians' preferences, and on the commitment technology at their disposal. According to retrospective voting theories, politicians are unable to commit *ex-ante* to any specific platform, and therefore electoral announcements are irrelevant (as standard for example in the political agency literature pioneered by [Barro \(1973\)](#)). On the contrary, in models where politicians have access to a commitment technology, a pre-electoral announcement would be able to attract all the (rational) voters whose net benefit of having the announcer in office and the proposed policy implemented are higher than the best alternative (for example in a Probabilistic voting model à la [Lindbeck and Weibull \(1987\)](#)). [Alesina \(1988\)](#) has explored explicitly the implications of pre-electoral announcements, showing that they matters even in absence of committment if they are credible, that is they are consistent with the preferences of their proponents. In absence of consesus from the theoretical literature, I see the research question posited at the beginning of this paper calling for an empirical answer.

Addressing this issue has important policy implications: if candidates are able to manipulate the electoral results by mean of fiscal announcements, they would be incentivized to deviate from the optimal fiscal policy, and their behaviour could generate a phenomeon similar to a political budget cycle (with a peak after the election, rather than before as in [Rogoff \(1990\)](#)).

In this paper, I tackle this question empirically by exploiting a natural experiment. In particular, I investigate voters' response to a specific policy announcement, that is the announced abolition of the property tax on main residence proposed by the incumbent Italian Prime Minister Silvio Berlusconi during the 2006 campaign for the national election.

This episode has several attractive features: first, it came as a complete surprise to voters, commentators and other politicians; second, it came only six days before the election, it was broadcasted on national public TV and extensively discussed in different media in the following days, suggesting that the vast majority of voters were aware of it ¹; third, it was specific, i.e. it

¹The reader may be tempted to believe that it is possible to compare informed vs. un-informed voters for identification using some exogenous instrument for the availability of the TV signal, as done, for example, by [Strmberg \(2004\)](#). This possibility is ruled out by the fact that the broadcasting channel, Rai Uno, is the main public channel and

specified which exact tax (property tax on the main residence) was to be abolished; fourth, if legislated, it would have affected directly the tax bill of a specific and well identifiable group of voters, the homeowners, leaving unchanged (at least in any direct way) the tax burden of the residual group (mainly renters).

The basic idea behind the identification strategy exploits this last feature: I implement a Difference-in-Differences design, by assigning homeowners to the *treatment* group, and the rest of the population (mainly renters) to the *control* group.

In practice, I investigate voters' response in two different and independent datasets: first, in an electoral survey, where the respondents are interviewed twice, once before the announcement, and once again after the election; second, in a panel of municipalities followed for four legislative elections (1994, 1996, 2001 and 2006).

First, using a classic Difference-in-Differences approach on individual-level survey data, I estimate that homeowners are 0.5 percentage points more likely to vote for Berlusconi's coalition after the announcement when compared to renters, while there was no significant difference between the two groups beforehand.

Second, I collect a panel dataset with municipality-level information on the vote share of Berlusconi's coalition, in all the four national-level legislative elections since he entered politics up until 2006 (the year of the announcement); I match this dataset with municipality-level data on the number of homeowner households from three Census waves (1991, 2001 and 2011). I then investigate how the change in Berlusconi's vote share in 2006, as compared to the three previous elections, varies across municipalities with different shares of homeowner households. This Difference-in-Differences estimation (that employs municipality- and election- Fixed Effects) suggests that municipalities with more homeowners display more votes for Berlusconi in 2006, compared to the past. In particular, when comparing two municipalities one standard deviation apart in terms of ownership rate (6.5%), the municipality with the higher share shows an electoral increase of 0.7 percentage points for Berlusconi's coalition, as compared to the previous ones.

Both results are corroborated by a battery of tests aimed at strengthen the credibility of the main assumptions behind both exercises: parallel trends, and absence of contemporaneous shocks. The evidence from both analysis points in the same direction: homeowners, i.e. those who would benefit from

its signal is very strong all over the Italian territory. Furthermore, both main Italian newspapers, *Corriere della Sera* and *La Repubblica*, broadcasted the event in streaming on their websites, and covered extensively the announcement in the next days.

the abolition of the property tax on main residences, are more likely to vote for the proponent of the tax reform following the announcement.. The result implies a causal and positive effect of the announcement on its proponent's vote share.

Finally, I add to my dataset the average tax level in each municipality-year, and the contribution of revenues from the property tax on main residences to the total revenues in each municipality-year. A specification similar to the one presented above, but restricted to the period 2001-2006 due to data availability, provides two additional results: first, municipalities with higher property taxes records more votes for Berlusconi's coalition in 2006; second, municipalities that rely more on the property tax as a source of revenues, records *less* votes for Berlusconi's coalition in 2006. The latter result is of particular interest, because it implies that voters internalize the (municipal) government budget constraint: voters in municipalities where the abolition of the property tax would imply a (bigger) drop in revenues, turn against the proposer of this policy, possibly understanding that the abolition would be necessarily financed via spending cuts, increase in taxation or deficit, or a mix of the three .

Overall, I find evidence that the policy proposal to abolish the property tax on main residences attracted voters toward his proponent. Furthermore, I find some evidence that the voters internalize the indirect effect of the abolition on the budget of the local government, and therefore punish the proponent if their net private cost of the reform outweighs its benefit.

The rest of the paper is structured as follows: Section 2 describes the episode under investigation, and provides details on the property tax on main residences in Italy; Section 3 describes the data, Section 4 explains the empirical strategy, and Section 5 reports the results. Finally, Section 6 concludes.

2 Institutional background

2.1 The episode

In April 2006, after a 5-year term in office, the incumbent Prime Minister of Italy Silvio Berlusconi was fated to fail re-election according to opinion polls. All the surveys estimated his centre-right coalition to be lagging behind the centre-left one by 2 to 6 percent, throughout the first months of the year (see Figure 1).

On the 3rd of April 2006, the public television hosted the final TV debate between Silvio Berlusconi and his opponent Romano Prodi, the leader of the

centre-left coalition. After one hour of debate, Berlusconi opened his final remarks with the following unanticipated announcement:

*“The main residence is sacrosanct to us, as it is the family. For this reason, we are going to abolish the ICI (i.e. the property tax). Yes, you got it right, we are going to abolish the property tax on all the main residences, including yours. This is a brave decision, but deeply sincere. (...)”*²

The announcement was covered extensively by different media outlets in the following days, and sparked a lively debate between members of the opposing coalitions: centre-left politicians labeled the proposal as irresponsible and populist, because it did not specify how was to be financed, while centre-right representatives argued that it would have been easy to finance the reform, via improved efficiency in the local administrations, and via a crack down on tax evasion ³.

Eventually, the centre-left coalition won the election by a very narrow margin (about 25.000 votes in the Lower House), and his candidate Romano Prodi managed to form a government supported by a slim majority. Less than two years later, Prodi was forced to resign and a snap elections was called in April 2008. Berlusconi’s coalition won by a landslide, and the new government abolished the property tax on main residences (excluding properties falling in categories A1-“luxury houses”, A8-“villas” and A9-“castles”) in his first legislative bill ⁴.

2.2 The property tax in Italy between 1993 and 2006

ICI, literally “Municipal Tax on Properties”, is the official name of the property tax mentioned throughout this paper. It was introduced in 1993⁵.

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

²Autonomous translation of the original speech in Italian: “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.”

³See for example the article “Lite sull’ Ici, il premier insiste. L’ Unione: disperato autogol” on *Corriere della Sera*, 2006, 5 April.

⁴Decreto legge 27 maggio 2008, n. 93.

⁵Decreto legislativo 11 luglio 1992, n.333 and decreto legislativo 30 dicembre 1992, n. 504

category C (commercial purposes, e.g. shops, cowsheds etc). Within the A category, the cadastral value varied across sub-categories⁶ and was an increasing function of the size (measured in number of rooms). Furthermore, 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 buildings used as main residences by the owner. Since 1997 municipal governments were also allowed to apply differential tax rates for buildings used as main residences⁷. Furthermore, the standard deduction was increased to 200,000 Italian Lira (about 100 Euro), but municipal governments were allowed to increase it up to 500,000 Italian Lira (about 250 Euro). Alternatively, they were allowed to reduce the tax rate on main residences by maximum 50% .

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

3 Data

3.1 The ITANES electoral survey

The ITANES (Italian National Election Studies) group conducted a two-period panel electoral survey around the 2006 elections. The first interview took place between February and March 2006, that is before the election (9-10 April) and before the announcement (3 April), while the second interview took place between mid-April and the end of July 2006. The survey is of particular interest because it records whether each respondent lived in an owned residence, or was, on the contrary, on rent at the time of the second interview. Furthermore, the survey records vote intention at the time of the first interview, and asks about actual voting in the polls during the second interview. During the first interview, respondents are also asked about their voting decision in the previous national election held in 2001. Finally, respondent are also asked detailed questions about their family and economic

⁶A1 - Luxury residences; A2 - Regular residences; A3 - Economic residences; A4 - Popular residences; A5 - Super-popular residences; A6 - Rural residences; A7 - Small villas; A8- Villas; A9 - Historical castles and palaces; A10 Private offices; A11 - Typical regional residences (e.g. cabins).

⁷Law 23 December 1996, nr. 662.

background.

The initial sample 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 said they were not sure about it. 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 since this question is asked exactly in the second round, but luckily the number of individuals dropping out at this stage is very low.

Table 1 reports summary statistics for all the variables used in the analysis, splitted by ownership status.

3.2 The municipality-level dataset

The electoral data are from the on-line historical archive of the Ministry of the Interior. I pool together the results of all the legislative elections (lower house, proportional tier) since Berlusconi entered into politics up until 2006 (1994, 1996, 2001 and 2006) at the municipality-level, and I calculate the share of votes for Berlusconi's coalition⁸ (as a percentage of total electorate). My sample consists of almost all⁹ the Italian municipalities (about 8000).

The Census records the housing condition of each households, and in particular records whether each household live: a) in an residence owned by one of its members, b) on rent, or c) under a different arrangement (residual category). 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 and for each of the following Census waves: 1991, 2001 and 2011. I do not have

⁸Berlusconi's coalition is defined as the electoral cartel supporting Berlusconi as candidate to *Presidente del Consiglio dei Ministri* (Prime Minister). Berlusconi's coalition is not stable over time: beside Forza Italia (Berlusconi's own party), it included Alleanza Nazionale and Lega Nord in 1994; Alleanza Nazionale, Centro Cristiano Democratico and Cristiani Democratici Uniti it in 1996; Alleanza Nazionale, Centro Cristiano Democratico and Lega Nord in 2001 and 2006.

⁹I exclude few observations: a) those municipalities that split or merge during the period under investigation, b) those municipalities whose electoral result is suspect (e.g. turnout greater than 100 %). The latter problem usually 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 on duty; c) all the municipalities in the region Valle d'Aosta (about 0.2% of total Italian population) because it was under a different electoral law in 2006.

this variable for each electoral year, therefore I adopt the following criterion in order to match this information with 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 linked to the 1996 election is the average between Census 1991 and 2001, and the variable linked to the 2006 election is the average between Census 2001 and 2011). 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 data.

Furthermore, I collect information on different policies of the municipal governments. In particular, I have yearly data on the property tax rate on main and non-main residences, and on deductions for each municipality for the period 1993-2006, from the Foundation for Local Economics and Finance (IFEL) powered by the National Association of Italian Municipal Governments (ANCI). I also collect annual data on the balance sheets of the municipal governments from the Ministry of the Interior; in particular I collect revenues from the property tax on main residences for each municipality (available only for year 2006), revenues from the whole property tax (both main and non-main residences lumped together, for the period 1998-2006) and total revenues (again for the period 1998-2006). Using these informations, I compute two variables: the average property tax on main residences in each municipality for year 2001 and 2006; and the ratio of revenues from the property tax on main residences over total revenues in each municipality for year 2001 and 2006¹⁰.

¹⁰As stated above, the revenues specific to the property tax on main residences is only available for 2006, while only the overall revenues from the property tax is available for year 2001. I calculate two different estimates of the revenues specific to the property tax on main residences for 2001 using two different methodologies: a) I extrapolate it from the total (main+non-main residences) property tax revenues assuming that the ratio of total property tax revenues over revenues from the tax on main residence is constant over time (in 2001 and in 2006); b), I extrapolate the implicit tax base using the formula $ICImainresidencerevenues_{2006,m} = [(taxbase_{2006,m} \times taxrate_{2006,m}) - deduction_{2006,m}] \times taxpayers_{2006,m}$, and assume $taxbase_{2006,m} = taxbase_{2001,m}$. . These two methods produce very similar results, therefore I will just rely on the first and more intuitive measure.

3.3 The Bank of Italy Survey on Household Income and Wealth (SHIW)

The SHIW is a survey conducted every two years by the Bank of Italy and aimed at collecting information on income, consumption and savings of Italian households. Particularly important to the purpose of this paper, the survey records the ownership status of the main residences of all the interviewed households. The 2006 edition covers 7768 households composed of 19551 individuals and 13009 income-earners. In 2006, 71.6% of the interviewed households owned their place of residence, 19.7% were on rent, 5.7% occupied it free of charge, 2.4% occupied it in usufruct and 0.5% occupied it under a redemption agreement. Table 2 reports selected summary statistics from the survey, contrasting homeowners with the rest of the population.

4 Empirical strategy

My goal is to estimate the voters' response to Silvio Berlusconi's announcement of abolition of the property tax on the main residence. In particular, I will test if voters who would have their tax bill cut under the new reform, are more likely to vote for Berlusconi's party or coalition after the announcement.

My empirical strategy relies on the fact that homeowners would enjoy a decrease in their total tax bill if the announcement was to be implemented, while renters would see their tax bill unaffected in any case.

In other words, I will assign homeowners to the *treatment* group, and renters to the *control* group, and employ a Difference-in-Differences type of strategy.

4.1 Who are the homeowners in Italy?

As anticipated in the introduction, this paper will compare voting behaviour of homeowners vs. renters, exploiting the fact that the abolition of the property tax would have directly affected the tax bill of the former group, but not of the latter. The decision to purchase or to rent the main place of residence is obviously not random from an individual point of view, and it is likely to be correlated with socio-economic characteristics.

This section investigates the correlates of homeownership in order to shed light on potential selection problems. To this end, I analyze the Survey on Household Income and Wealth (SHIV) conducted by the Bank of Italy. In particular, I predict ownership status using individual economic and demographic characteristics in a linear probability model. Let $O_{i,h}$ be a dummy

equal to 1 if household h , of which individual i is a member, owns his main place of residence. Furthermore, let X_h be a vector of household-level characteristics, and $W_{i,h}$ a vector of individual-level characteristics. I then estimate the following model:

$$O_{i,h} = \beta X_h + \gamma W_{i,h} + \epsilon_{i,h} \quad (1)$$

Vector X_h includes the self-reported market valuation (in millions of Euro) of the place of residence (either owned, rented or else); vector $W_{i,h}$ contains age (in decades), age squared, gender dummy, dummies for the highest education degree achieved (none, midvocational, highschool, college, with elementary being the excluded category), and dummies for the current employment status (blue-collar worker, housewife, selfemployed, retired, pensioner, student, unemployment, other, with white-collar occupation being the excluded category). I restrict the sample to individuals aged 18 or more on the 9th of April 2006, that is having the right to vote in the election, and I cluster the standard errors at the household level.

Table 3 reports the result from estimating equation (1) via OLS. In column (1) to (4), I include different set of controls separately (value of residence, age, education and employment), in column (5) I include all of them at the same time, and finally the specification in column (6) also includes Fixed Effects at the regional-level ¹¹.

On average, homeowners live in more valuable residences than renters (a 100,000 Euro increase in the value of the residence increases by 50 percentage points the probability of being an homeowner); the probability of living in a owned residence increases with age (3 percentage point every 10 years), and with education (both college and highschool graduates are about 5 percentage points more likely to own, than people with only elementary school degree). Finally, employment status and occupation is also correlated with the probability of living in a owned residence; compared to white collar workers, the following categories are less likely to live in an owned residence: blue collars (-15 p.p.), housewives (-5 p.p), pensioners (-5 p.p.) and unemployed (-10 p.p); again compared to white collars, retirees (+2 p.p.) and students (+5 p.p.) are on the contrary more likely to live in a owned residence.

The overall picture emerging from the survey suggests that individuals living in a residence owned by themselves or by another household member are on average older, better educated and working in more lucrative occupations than renters. However, the very low R^2 (at most 10%) suggests that despite all these characteristics are correlated with the dependent variable, they can only explain a small part of its total variance. In other words, living in a owned residence is very common among Italian households, and it

¹¹Italy is composed by 20 regions (*Regioni*).

is absolutely not limited to a specific group (either demographic, geographic or economic) of the total population. Just to make an example, the share of population owning their residence is still well above one half (58 %) also among blue collars.

4.2 The electoral response to the announcement in the ITANES survey

This section outlines the empirical strategy specific to the electoral survey data, where the same respondents are interviewed twice, once before the announcement (and the election), and again after the election, as sketched in the timeline below.

17 February	•	1st interview: start
21 March	•	1st interview: end
3 April	•	Announcement
9-10 April	•	Election
1 May	•	2nd interview: start
5 July	•	2nd interview: end

The most immediate way to test my hypothesis is to run a cross-section OLS regression of voting behaviour in the 2006 election on a dummy for homeownership. Let V_i be a dummy equal to 1 if respondent i declares to have voted for Berlusconi's coalition in the 2006 election (during the second i.e. post-election interview), and 0 otherwise, and O_i a dummy equal to 1 if the respondent lives in a residence owned by himself or by another member of his household, and 0 otherwise. The equation to be estimated is then:

$$V_i = \beta O_i + \epsilon_i \quad (2)$$

However, the *treatment status*, that is being an homeowner, is not randomly assigned to respondents, and on the contrary is likely to be correlated with other personal characteristics, as highlighted in the previous section. These characteristics may in turn be correlated with the probability of voting for Berlusconi (or centre-right parties in general), and therefore bias my result. To account for this, I augment the specification (2) with the following controls: a) a dummy equal to one if the respondent declared to intend to vote for Berlusconi's coalition when interviewed the first time, that is *before* the announcement; b) a dummy equal to one if the respondent have voted for Berlusconi's coalition in 2001 (the previous election) ; c) a set of dummies and variables capturing age, occupation and education, that is all the variables found to be correlated with homeownership in the SHIV (see the

previous section). However, this exercise may still in principle suffer from omitted variable bias. To enhance the credibility of this exercise, I will run the same specification as in (2), but using as dependent variable I_i , that is a dummy equal to one if the respondent declared to intend to vote for Berlusconi's coalition *before* the announcement. If the announcement was the only reason why owners are more likely to vote for Berlusconi, I should find no correlation between homeownership and voting intention *before* the announcement. I will then estimate the following equation:

$$I_i = \gamma O_i + \epsilon_i \quad (3)$$

and test if γ is not significantly different from zero.

Finally, I will implement a proper Difference-in-Differences design. Let $W_{i,t}$ be a panel variable, that is a variables recorded in two periods, $t = \textit{before}$ i.e. before the announcement, and $t = \textit{after}$ i.e. after the election. The variable $W_{i,t}$ is equal to 1 when $t = \textit{before}$ if respondent i declared to intend to vote for Berlusconi's coalition during the first interview, and 0 otherwise; the variable $W_{i,t}$ is equal to 1 when $t = \textit{after}$ if respondent i reported to have actually voted for Berlusconi's coalition during the second interview, and 0 otherwise¹². I will then regress $W_{i,t}$ on ownership status¹³ interacted with a dummy for $t = \textit{after}$, plus time and individual Fixed effects:

$$W_{i,t} = \beta O_i \times \textit{After} + \alpha_i + \lambda_t + \epsilon_{i,t} \quad (4)$$

Due to the presence of individual fixed effects, identification in equation (4) will come only from "switchers", that is respondents whose value of the dependent variable changes between the first and the second period. In this set-up the coefficient of interest, β , captures the causal effect of the announcement on the *treated* respondents under two assumptions: the absence of any contemporaneous shocks, and the parallel trends assumption. The first assumption rules out the presence of contemporaneous shocks taking place between the time of the first and of the second interview, and affecting voting behaviour of homeowners, or of a group of voters correlated with homeowners. To account for this, I include in specification (4) some individual level controls likely to be correlated with homeownership: dummies for

¹²Note that this is not a natural panel variable (i.e. the same variable observed at different points in time for the same individuals), but rather two distinct variables: vote intention (recorded during the first interview), and actual voting (recorded during the second interview). I thank a commentator for pointing this out to me.

¹³Here I am assuming that a respondent who declared to be homeowner during the second interview was already an homeowner few months before at the time of the first interview, when he was not explicitly asked about it.

being unemployed, retired, student, housewife and blue collar. Controlling for the interaction of these variables with the *After* dummy makes sure that any result is not driven by a contemporaneous shock hitting a group correlated with homeowners. The parallel trend assumptions states that in the absence of the treatment the difference in the probability of voting Berlusconi between members of the two groups would have been the same in both periods. It is difficult to provide support for this assumption because there are only two time periods, *before* and *after*, and therefore I can not plot the pre-treatment trends or include group-specific trends in the regression ¹⁴.

I address this shortcoming by including an additional temporal observation to my panel dependent variables: I add the vote in the previous 2001 election, as reported in the first interview. The time dimension t takes now three value ($t = 2001, Before, After$) and therefore I can also include individual-level linear time trends to equation (4), in order to enhance the credibility of the parallel-trend assumption, which must always hold *conditional* on the included covariates. The new specification is:

$$W_{i,t} = \beta O_i \times After + \alpha_i + \lambda_t + \sum_i \alpha_i \times t + \epsilon_{i,t} \quad (5)$$

4.2.1 Non response in the survey

A common problem in electoral surveys is non response, since many individuals refuse to reveal their voting decision. Many researchers simply drop from the sample the individuals who did not respond, but this would lead to biased estimates if the *treatment* variable is correlated with the probability of not responding, as noted for example by Kendall et al. (2015). In my sample of 1377 individuals who participated in both interviews, 202 ($\simeq 14\%$) of them did not respond to the voting question ¹⁵.

Furthermore, 34 ($\simeq 2\%$) refused to reply to the question asking whether they live in a owned residence or not, that is the treatment question ¹⁶.

To investigate the severity of this problem in my specific application, I regress a dummy equal to 1 if the respondent revealed his vote, on three dummies: the first equal to 1 if the respondent is an homeowner, the second equal to 1 if the respondent is not, and third equal to 1 if he refused to reply to the housing question. Members of the latter group, that is no-repliers to the housing question, are 85 percentage points more likely to refuse to reply

¹⁴Same problem encountered by Card and Krueger (1994) in their seminal work

¹⁵Among these 202, 16 refused to reveal whether they voted or not, and 186 refused to reveal which party they voted for.

¹⁶Out of these 34, 3 did not vote at all, 14 voted the centre-left coalition, 4 voted the Berlusconi's coalition, and 13 refused to reply to the voting question.

to the voting question as well, compared to other individuals. However, and most importantly, homeowners are not significantly more or less likely to refuse to reply compared to renters (the P-value of the t-test for the equality of the two coefficients is equal to 0.97). In light of this stark result, I can safely drop non-repliers to both questions from my sample; from now onward I will always work with this sample of 1154 individuals, unless stated otherwise.

4.3 Empirical strategy for municipality-level data

This section outlines the empirical strategy designed for the dataset of election results of a panel of municipalities: it is a modification of the Difference-in-Differences approach applied to the electoral survey, where the homeowners have been assigned to the *treatment* group, and the rest of the population to the *control* group. The unit of observation is the municipality, and the data feature a panel of about 8000 municipalities followed for four elections. I exploit variation across municipalities in the share of homeowners in the population to implement a Difference-in-Differences estimator based on intensity of homeownership at the municipality-level. The (ideal) control group is made by all the municipalities where the share of homeowners is exactly zero, and therefore no residents would be directly affected by the abolition of the property tax on the main residence. The treatment intensity is then linearly increasing in the share of homeowners in each municipality: the more homeowners in a city, the more voters are affected by the property tax announcement. Availability of electoral data both before the announcement (1994, 1996 and 2001) and after (2006) allows to implement a Difference-in-Differences by interacting a 2006-dummy with the share of homeowners. Let $Y_{m,t}$ and $O_{m,t}$ be respectively the share of votes for Berlusconi's coalition and the share of owners in municipality m and election t , and additionally let us define λ_t the time Fixed Effects, and μ_m the municipality Fixed Effects. My baseline specification is then:

$$Y_{m,t} = \beta O_{m,t} \times \lambda_{2006} + \gamma O_{m,t} + \lambda_t + \mu_m + \epsilon_{m,t} \quad (6)$$

where the coefficient of interest is β ¹⁷. I cluster the standard errors at the province level (102 clusters).

My strategy rests on two assumptions, standard to any Difference-in-Differences application: first, the absence of contemporaneous shocks, and second, the parallel trends assumption. The first assumption states that

¹⁷Examples of recent papers relying on a similar Difference-in-Differences strategy that uses as treatment variable the share of treated observations within a geographical or administrative units are [Mian and Sufi \(2012\)](#) and [Troiano and Casaburi \(\)](#)

no contemporaneous (i.e. hitting between 2001 and 2006) shock is affecting the voting behaviour of the homeowners, or of a group of voters whose cross-municipality distribution is correlated with the cross-municipality distribution of homeowners. I am not aware of any policy or shock targeted specifically to homeowners, but it may still be the case that one of the policies enacted during the 2001-06 term, or some other electoral announcements, or some other external shocks affected a group of voters whose distribution is correlated with homeownership intensity. I address this concern by augmenting the specification in (6) with municipality-level controls interacted with the 2006 dummy: these controls include variables found to be correlated with homeownership in the SHIV survey. The second assumption states that the outcome variable was following a time trend which was parallel across groups, prior to the announcement; in other words, it states that in the absence of the announcement there would have been no larger difference in the outcome variable between groups, compared to the previous period. Despite this assumption is fundamentally un-testable, I provide evidence that strengthen its credibility by following both most common procedures in the applied literature : a) I show a placebo test featuring a “fake” announcement in 2001; b) I augment the specification in (6) with municipality-level linear trends ¹⁸.

4.3.1 Heterogeneity analysis and the municipal government budget constraint

The approach layed down so far focuses solely on comparing tax payers (homeowners) to non tax payers. However, there are at least two more dimensions that could generate heterogeneity in the electoral response to the property tax announcement.

First, the magnitude of the property tax varies across municipalities and over time, both because the tax rate is set by each municipal government, and because the tax base is based on the cadastral value of the residences (see section 2.2). It is therefore reasonable to expect that, when comparing two municipalities with the same share of homeowners, voters in the municipality with the higher tax should be more appealed by the announcement. I can test this effect simply by augmenting specification (6) with the average tax level in each municipality, and with its interaction with the 2006 dummy; unfortunately I must reduce the sample to only two periods (2001 and 2006) due to data availability.

Second, theory predicts that rational forward-looking voters internalize the government budget constraint, that is they are aware that a cut in tax-

¹⁸Note that these trends are estimated using mainly pre-announcement time periods, so they escape Wolfers (2006)’s concerns.

ation must necessarily be financed by either a rise in other taxes or in the deficit, or by a decrease in government spending ¹⁹. As explained above, the revenues from the property tax accrue to the municipal governments, and are in fact their main source of financing. However, the importance of the property tax on main residences as a source of revenues varies across municipalities, when measured as a share of total revenues. This implies that the reform would cause a bigger drop in revenues in some municipalities, and a lower drop in others. Rational voters resident in municipalities that rely heavily on the property tax on main residence as a source of revenues should foresee that under the new reform their municipalities would be necessarily forced to either raise other taxes, cut spending on public goods, or increase the budget deficit. This may imply that in these municipalities even homeowners have a net cost (rather than a benefit) from the reform, and therefore that may have the incentive to vote *against* its proponent, Berlusconi. In order to test whether voters internalize the local government budget constraint, I augment specification (6) with the revenues from the property tax on main residence, and its interaction with the 2006 dummy. Again, as for the tax level, I must reduce the sample to only two periods (2001 and 2006) due to data availability.

5 Evidence

5.1 Evidence from the electoral survey

I start by comparing voting decision between homeowners and non-homeowners in the cross section of respondents, that is I estimate equation (1) via OLS. Results are reported in Table 4: column (1) reports the most parsimonious specification with only the dummy for homeownership as regressor; in column (2) I control for voting intention in the first interview, and for voting in 2001; in (3) I control for age, gender, marital status, and number of kids; in (4) I control for a set of education dummies; in (5) for a set of employment/occupation dummies; in (6) I control for all the above variables at the same time.

Homeowners are approximately 6 percentage points more likely to vote for Berlusconi's coalition in 2006; the point estimate is remarkably stable, and always significant at conventional levels across the columns of Table 4.

Next, I move to the placebo test. I run the same specifications as above, but using as dependent variable a dummy for vote intention *before* the announcement. In other words I estimate equation (3) via OLS. This amounts

¹⁹This dates back to the Ricardian Equivalence and to Barro (1974).

to a placebo test: if the announcement is the only reason why homeowners are more likely to vote for Berlusconi, then I should expect to find no effect when the dependent variable is recorded *before* the announcement. Results are reported in table 5. Homeowners are not more likely to vote for Berlusconi compared with renters, when interviewed few months *before* the announcement. The point estimate is never higher than 1 percentage point, and the corresponding robust standard errors are always about three times larger. This result strengthens the likelihood that the increase in support for Berlusconi among homeowners in the election is due to the property tax announcement.

Next, I move to the Difference-in-Differences with individual-level Fixed Effects. First, I consider only two time periods, that is I run specification (4). Table 6 presents the results: column (1) corresponds to the baseline specification, while in column (2) to (8) I add different controls interacted with a dummy for $t = \textit{after}$. The controls include variables likely to be correlated with homeownership: dummies for being unemployed, retired, student, housewife and blue collar. Controlling for these variables interacted with the *After* dummy makes sure that any result is not driven by a contemporaneous shock hitting a group correlated with homeowners. In column (8) I include all these controls at the same time.

The coefficient on the interaction of interest, $O_{i,t} \times \textit{After}$, is positive but marginally insignificant in the first column of Table 6. The high standard errors could be the result of the limited variance of the homeownership variable, combined with the small sample size. Column(2) to (8) seem to confirm this hypothesis; when controlling for other factors (which account for part of the unexplained variation of the dependent variable in column (1)), the coefficient of interest remains stable in terms of size, but precision increases. This is especially evident in the last column. Overall, the size of the effect, around 5 percentage points, is consistent with the cross-sectional evidence.

Next, I replicate the Difference-in-Differences including a third time period, that is including in the dependent variable the voting choice in the previous 2001 election. The individual-level controls are measured in 2006, therefore I restrict my attention to those variables that are not very likely to have changed compared to 2001²⁰. The first two columns of Table 7 report results from specification (3) on the three-period panel: the point estimate of the coefficient of interest is very similar to what estimated using only two periods. In columns (3) and (4) I add individual-level linear time trends, to test if the Difference-in-Differences estimate in (1) and (2) is driven by pre-

²⁰For example, people being on retirement in 2006 may well be still working in 2001, therefore I drop the control *Retired*.

existing trends, or in other words, to test if the parallel-trend assumption is violated. The point estimate of the coefficient of interest remains stable also when individual-level time trends are included, providing strong support for the parallel trends assumption, and therefore enhancing the credibility of the Difference-in-Differences approach. However the standard errors in columns (3) and (4) are very high, but this is not surprising given that specification (4) is very demanding (it fits a separate linear trend for each respondent).

Overall, the quasi-experimental evidence from survey data presented in this section, suggests that the announced abolition of the property tax was effective in attracting homeowners' votes toward Berlusconi, its proponent. The estimated effect of the announcement on the homeowners' probability of voting Berlusconi is 5 percentage points. Several robustness checks suggest that the two fundamental assumptions behind the Difference-in-Differences (absence of contemporaneous shocks, and parallel trends) hold in the present application.

5.2 Evidence from the municipality-level data

I present the main results of this section in table 9. Column (1) reports the estimated coefficient of the interaction of interest (i.e. share of homeowners interacted with the 2006 dummy) from the baseline specification (6). The remaining columns progressively add different controls, meant to investigate the solidity of the two main assumptions: columns (2) and (3) deal with the parallel trend assumption, while (4) with the contemporaneous shock assumption. The last two columns address these two issues contemporaneously. Municipality Fixed Effects are always included.

The coefficient in column (1) is positive and significant, and very robust to the inclusion of Province-level linear trends (2), or Municipal-level linear trends (3). In column (4), I control for a list of municipality characteristics likely to be correlated with homeownership interacted with a dummy for the 2006 elections: the point estimate is not significantly different from the estimate in column (1) (at 10 %), but again significantly different from zero; this test suggests that the main result is not driven by a contemporaneous shock to a group of voters, which are also more (or less) likely to be homeowners: highly educated, students, retirees and housewives. Finally, I include controls and province-level trends at the same time, in column (5), and controls and municipal-level trends at the same time, in column (6): the point estimate is still positive and significant, but the size is slightly smaller than in column (1) (18 percentage points vs. 11 percentage points). I also conduct a placebo test where I exclude the 2006 election from the sample, and pretend the announcement occurred in 2001. The results are reported

in Table 10, where each column corresponds to the same specification as Table 9: in most specification the estimated coefficient is small and insignificant, but the point estimate jumps up and becomes significant in column (4) (0.07) and especially column (6) (0.27). This test does not completely rule out the hypothesis that support for Berlusconi among homeowners was already peaking in 2001, therefore I will always consider as my most robust estimates those controlling for municipal-level linear trends, i.e. column (3) and (4) in Table 9.

The election under investigation is for a national-level political office, therefore I am interested in estimating the average effect representative at the national level. Italian municipalities are very heterogeneous in size, and if the result presented above happen to be driven by very small municipalities, it may have a negligible impact on the aggregate national level electoral outcome. To address this concern, I repeat the above analysis by weighting the regressions by different measures of municipality size: eligible voters in 2006, the mean of votes cast in the 1994, 1996 and 2001 elections, population size in 1991, and population size in 2001 ²¹. The results, presented in Table 11, are very similar to the estimated coefficients in the un-weighted regressions for all the specifications (1) - (6), both in terms of size and significance. The only exception occurs when using population size in 1991, which delivers estimated coefficients two to three times higher than the un-weighted regression, and always very significant.

The most conservative specification is reported in column (6) of Table 11: the coefficient of interest on the interaction between the share of homeowner and a 2006 dummy using most weighting schemes is 0.11 . This estimate 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 percentage points for Berlusconi's coalition in the 2006 elections, as compared to the previous ones. This effect may sound small, but it is in fact very large due to the very high homeownership rate in Italy: a back-of-the-envelope calculation suggests that the electoral announcement mobilized about 3.5 millions votes toward Berlusconi's coalition. This figure is huge, especially when considering that the centre-left coalition collected only about 25.000 votes more than Berlusconi's coalition in 2006; in other words, my estimate suggests that without the property tax announcement, Berlusconi would have incurred in a major defeat in 2006. At this point a word of caution is due: what I estimate in this section is the *direct* effect of the announcement on the homeowners compared to the rest

²¹Weighting regressions when studying national-level electoral outcome using sub-national data is a strategy implement by, for example, [DellaVigna and Kaplan \(2007\)](#).

of the population; this implies that in this section I am not able to identify any other effect stemming from the announcement which affects all voters (both homeowners and not) in the same way; furthermore, in this section I am not able to disentangle other effects stemming from the announcement which affects heterogeneously different groups in the populations, for example residents in high-tax municipalities vs. residents in low-tax municipalities, or residents in high-reliance on property tax municipalities, vs. others : this will be investigated in the next section. Some of these effects may go in the opposite direction with respect to the effect estimated in this section, so that the *net* electoral gain from the announcement could be much smaller than 3.5 million votes ²².

Overall the evidence presented here is consistent with the results reported in the previous section, and suggests that homeowner voters are more likely to vote for Berlusconi's coalition following the property tax announcement.

5.3 Additional dimensions: tax level and the municipal government budget constraint

In this section I investigate other potential effects of the announcement by exploiting two additional sources of cross-municipalities heterogeneity: a) heterogeneity in the level of the property tax on main residences paid (in Euro), and b) heterogeneity in the importance of revenues from the property tax on main residence as a share of total municipal revenues. Due to data availability, the analysis focuses only on two periods (2001 vs. 2006), which prevents me from implementing convincing robustness checks.

Table 12 presents the results; in the first column I replicate the baseline specification (6) on the two-period sample for sake of comparison: the point estimate is still positive and significant, but slightly smaller. In column (2) I include the average level of property tax on main residences paid by a taxpayer (in thousands of Euro) interacted with the 2006 dummy to test if municipalities where the tax bill to be abolished is higher shows more support for Berlusconi, but the point estimate is exactly zero. Instead in column (3) I include the interaction between the share of total municipal

²²A similar caveat regarding the interpretation of the coefficient in an analogous setting is discussed by Karol and Miguel (2007). They estimate the electoral effect of the Iraq war on the result of the US Presidential election exploiting cross-counties variation in casualties: using this strategy they are able to identify the electoral effect of one additional casualty (in the same way I am able to identify the effect of one additional homeowner), but they can not identify any effect affecting voters in all the counties in the same direction, for example a rally around the incumbent in time of war effect, or the general support for how foreign policy was conducted.

revenues stemming from the property tax on main residence and the 2006 dummy: the coefficient is negative and significant at 1%. Next, in column (4) I include both the new interactions at the same time: the coefficient on the tax level interaction becomes positive and significant, while the coefficient on the revenues remains negative and significant, but doubles in size. The change in the size of the estimated coefficients between columns (2)-(3) and column (4) is not surprising, as the two new variables are very likely to be correlated among each other, because they are both functions of the tax base and of the tax rate. It is therefore important to include both of them to avoid to incur in a classic omitted variable bias. The results in (1)-(4) are robust when controlling for the usual battery of control variables (columns (5)-(8) of the same table).

Before turning to the interpretation, I re-run the same specifications weighting the regressions by population size in 2001 (Table 13), and by eligible voters in 2006 (Table 14): the results are qualitatively similar. I adopt as baseline estimate for the two new coefficients the most conservative specification, i.e. column (8) of Table 14. The interpretation of this new evidence is as follows: a) when comparing two municipalities 1000 Euro apart in terms of average property tax on main residence paid, the municipality with the higher tax shows an electoral increase of 2 percentage points for Berlusconi's coalition in the 2006 elections, as compared to the previous one; this effect is small at the national level, considering that the average tax level in 2006 was 175 Euro (standard deviation 264); b) when comparing two municipalities one standard deviation apart in terms of importance of revenues from the property tax on main residence (4.4%), the municipality that relies the most on this tax shows an electoral *decrease* of 0.6 percentage points for Berlusconi's coalition in the 2006 elections, as compared to the previous one. This is also a small effect at the national level, considering that on average the property tax on main residences contributed to 4% of the total revenues.

Overall the first result of this sub-section is consistent with the previous evidence: the announcement convince some taxpayers of the property tax to vote for Berlusconi, and the effect is slightly stronger the higher the tax.

The second result is more interesting and novel: the drop in Berlusconi's support among residents in municipalities that rely heavily on the property tax on main residence as a source of revenues, suggests that voters internalize the municipal government budget constraint, i.e. they foresee that the abolition of the tax would cause a drop in revenues, that must be necessarily compensated either by higher local taxes, lower level of local public goods, or higher deficits.

6 Conclusion

This paper presents quasi-experimental evidence on the effectiveness of electoral announcements promising lower taxes as a tool for attracting votes. My results suggest that politicians are able to attract some of those voters who would have a net benefit from the promised fiscal reform in terms of lower taxes; they also suggest that voters who would have to bear the indirect costs of the reform (via the government budget constraint) are, on the contrary, more likely to turn against its proponent.

The evidence presented here is relevant for the literature aimed at investigating the determinants of voting choices; in particular, it is consistent with the idea of voters as rational agents maximizing their private economic utility.

My results are also in contrast with retrospective voting theories, that assume politicians to be unable to commit to pre-electoral agendas, and electoral announcements to be not contractable, and therefore irrelevant: if voters can not trust the announcement, they would just disregard it, and base their decision simply on expectation on future politicians' behaviour once in office. One way to reconcile my result with the realistic assumptions behind the retrospective voting theories is the following: one single, specific, salient, visible, and easy-to-monitor announcement, such the one studied in this paper ²³, may act as commitment device for the politician. It is true that the politician is not bound in any formal way to implement his electoral promise, but failing to deliver it would severely undermine its credibility.

An alternative explanation goes back to the analysis of credibility by [Alesina \(1988\)](#): abolishing the property tax of main residence can be seen by voters as a reform well in line with the policy preferences of a centre-right politician like Silvio Berlusconi, and therefore they can consider it a credible promise, even in the absence of formal commitment.

The investigation of the exact mechanism behind my result, as well as its policy implications, are left as open questions for future research.

²³The property tax in Italy is actively paid by the tax payer via a bank or postal transfer, therefore monitoring the implementation of the promise would have been mechanic, and cost-free. To make the opposite case, a decrease in the marginal tax rate of the income tax would have been on the contrary much harder and more costly to monitor by, for example, employed workers who receive their wage net of taxes.

References

- 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>.
- Barro, Robert J. (1974), “Are government bonds net wealth?” *Journal of Political Economy*, 82, pp. 1095–1117, URL <http://www.jstor.org/stable/1830663>.
- Barro, Robert J. (1973), “The control of politicians: An economic model.” *Public Choice*, 14, 19–42, URL <http://dx.doi.org/10.1007/BF01718440>.
- Card, David and Alan B. Krueger (1994), “Minimum wages and employment: A case study of the fast-food industry in new jersey and pennsylvania.” *The American Economic Review*, 84, pp. 772–793, URL <http://www.jstor.org/stable/2118030>.
- DellaVigna, Stefano and Ethan Kaplan (2007), “The fox news effect: Media bias and voting.” *The Quarterly Journal of Economics*, 122, 1187–1234, URL <http://qje.oxfordjournals.org/content/122/3/1187.abstract>.
- Karol, David and Edward Miguel (2007), “The electoral cost of war: Iraq casualties and the 2004 u.s. presidential election.” *The Journal of Politics*, 69, 633–648, URL http://journals.cambridge.org/article_S0022381600005855.
- Kendall, Chad, Tommaso Nannicini, and Francesco Trebbi (2015), “How do voters respond to information? evidence from a randomized campaign.” *American Economic Review*, 105, 322–53, URL <http://www.aeaweb.org/articles.php?doi=10.1257/aer.20131063>.
- Lindbeck, Assar and Jrgen W. Weibull (1987), “Balanced-budget redistribution as the outcome of political competition.” *Public Choice*, 52, 273–297, URL <http://dx.doi.org/10.1007/BF00116710>.
- 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>.

- Rogoff, Kenneth (1990), “Equilibrium political budget cycles.” *The American Economic Review*, 80, pp. 21–36, URL <http://www.jstor.org/stable/2006731>.
- Strmberg, David (2004), “Radio’s impact on public spending.” *The Quarterly Journal of Economics*, 119, 189–221, URL <http://qje.oxfordjournals.org/content/119/1/189.abstract>.
- Wolfers, Justin (2006), “Did unilateral divorce laws raise divorce rates? a reconciliation and new results.” *American Economic Review*, 96, 1802–1820, URL <http://www.aeaweb.org/articles.php?doi=10.1257/aer.96.5.1802>.

Appendix A Figures

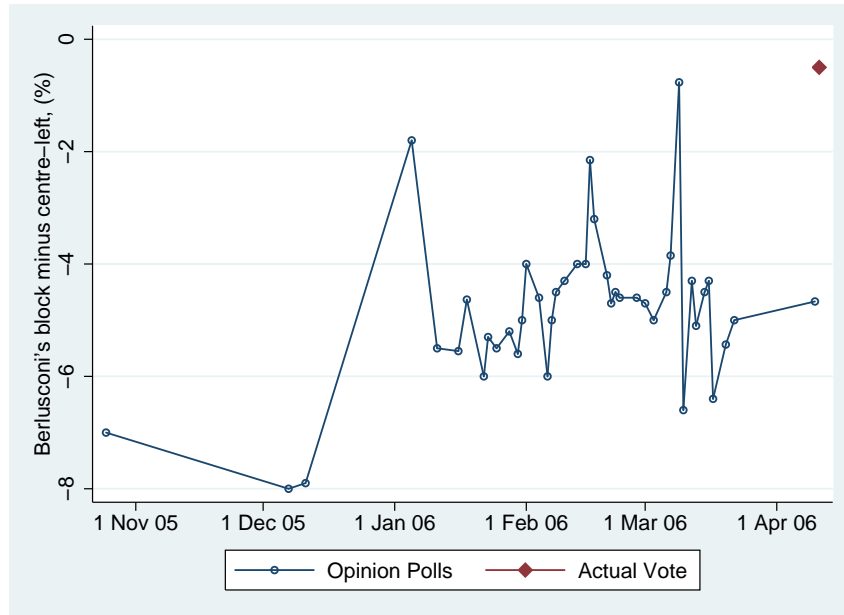


Figure 1: Vote difference between Berlusconi's block and centre-left block

Appendix B Tables

<i>N</i> <i>Size</i>	Renters		Owners	
	176 15%		978 85%	
	Mean	S.D.	Mean	S.D.
Voted Centre-Right in 2006 (d)	.34	.47	.40	.49
Vote intention for Centre-Right (d)	.30	.46	.31	.46
Voted Centre-Right in 2001 (d)	.33	.47	.35	.47
Age	44	16	47	16
Married (d)	.59	.49	.62	.48
Single (d)	.34	.47	.33	.47
Male (d)	.47	.5	.52	.49
Parent (d)	.64	.48	.65	.47
Number of children	1.28	1.21	1.32	1.22
College (d)	.06	.25	.10	.30
Highschool (d)	.31	.46	.37	.48
Midvocation (d)	.43	.49	.34	.47
Elementary (d)	.15	.36	.14	.35
No degree (d)	.02	.14	.02	.15
Blue collar (d)	.21	.40	.11	.31
Unemployed (d)	.06	.24	.03	.18
Housewife (d)	.17	.37	.14	.34
Student (d)	.05	.22	.06	.25
Retired (d)	.15	.36	.23	.42
White collar (d)	.14	.35	.16	.37

Notes: variables followed by (d) are dummies. Source: ITANES 2006.

Table 1: ITANES: summary statistics

<i>N</i> <i>Size</i>	Renters		Owners	
	Mean	S.D.	Mean	S.D.
	5,544		14,007	
	28%		72%	
Value of the residence	153	115	243	198
Age	41	22	46	22
College (d)	.04	.21	.09	.29
Highschool (d)	.19	.39	.25	.43
Midvocational (d)	.4	.49	.32	.46
No degree (d)	.14	.34	.10	.30
Blue collar (d)	.20	.40	.11	.31
Housewife (d)	.13	.33	.10	.30
Selfemployed (d)	.06	.24	.07	.264
Retired (d)	.13	.34	.23	.42
Pensioner (d)	.06	.24	.05	.23
Student (d)	.14	.35	.15	.36
Unemployed (d)	.07	.26	.04	.21

Notes: variables followed by (d) are dummies.

Table 2: SHIV: summary statistics

	Houseowner					
	(1)	(2)	(3)	(4)	(5)	(6)
Value of residence	0.55*** (0.05)				0.47*** (0.05)	0.48*** (0.05)
Age		0.01** (0.01)			0.03*** (0.01)	0.03*** (0.01)
Age ²		0.00 (0.00)			-0.00 (0.00)	-0.00 (0.00)
College			0.11*** (0.01)		0.07*** (0.02)	0.06*** (0.02)
HighSchool			0.05*** (0.01)		0.05*** (0.01)	0.05*** (0.01)
MidVocational			-0.05*** (0.01)		0.01 (0.01)	0.01 (0.01)
NoEducation			-0.06*** (0.01)		-0.01 (0.02)	-0.02 (0.02)
BlueCollar				-0.14*** (0.01)	-0.12*** (0.01)	-0.13*** (0.01)
Housewife				-0.05*** (0.01)	-0.06*** (0.01)	-0.06*** (0.01)
Selfemployed				0.03** (0.02)	-0.03 (0.02)	-0.03* (0.02)
Retired				0.09*** (0.01)	0.03** (0.01)	0.02* (0.01)
Pensioner				-0.02 (0.02)	-0.05** (0.02)	-0.05*** (0.02)
Student				0.01 (0.01)	0.05*** (0.01)	0.05*** (0.01)
Unemployed				-0.10*** (0.02)	-0.08*** (0.02)	-0.07*** (0.02)
<i>N</i>	19,551	19,551	19,551	19,551	19,551	19,551
Region FE						X
<i>R</i> ²	0.05	0.01	0.01	0.03	0.08	0.10

Notes: The dependent variable is a dummy equal to one if the respondent lives in a residence owned by himself or by another member of his household. Value of residence is the self-reported market valuation (in hundredthousands of Euro) of the place of residence (either owned, rented or else); age and age squared are measured in decades; the education dummies refer to the highest degree attained, and elementary education is the excluded category; the excluded category from the set of employment dummies is white collar occupation (regressions in columns (4), (5) and (6) also include a dummy for the residual employment categories, but the coefficient is not reported). Standard errors are clustered at the household level. Significance stars are at conventional levels.

Table 3: Correlates of homeownership in the SHIV

	Voted for Berlusconi in 2006					
	(1)	(2)	(3)	(4)	(5)	(6)
Houseowner	0.07*	0.05**	0.07*	0.07*	0.07*	0.06**
	(0.04)	(0.03)	(0.04)	(0.04)	(0.04)	(0.03)
Past voting controls		X				X
Demographic controls			X			X
Education controls				X		X
Employment controls					X	X
N	1,154	1,154	1,154	1,154	1,154	1,154
R^2	0.00	0.61	0.02	0.01	0.01	0.62

Notes: The dependent variable is a dummy equal to one if the respondent voted for Berlusconi's coalition in 2006. **Houseowner** is a dummy equal to one if the respondent lives in a residence owned by himself or by another member of his household. **Past voting controls** include a dummy equal to one if the respondent intended to vote for Berlusconi's coalition prior to the announcement, a dummy equal to one if the respondent intended to vote for the centre-left coalition prior to the announcement, a dummy equal to one if the respondent voted for Berlusconi's coalition in 2001, and a dummy equal to one if the respondent voted for the centre-left coalition in 2001. **Demographic controls** include age, age squared, a currently married dummy, a single dummy, a gender dummy, a parent dummy, and number of children. **Education controls** include dummies for highest educational degree achieved: college degree, high-school, mid-vocational, elementary and none. **Employment controls** include dummies for the following categories: blue collars, unemployed, housewives, students and retired. Robust standard errors in parentheses, significance stars at conventional levels.

Table 4: Cross-sectional OLS

	Vote intention for B. before the announcement					
	(1)	(2)	(3)	(4)	(5)	(6)
Houseowner	0.01 (0.04)	0.00 (0.03)	0.00 (0.04)	0.02 (0.04)	0.01 (0.04)	-0.01 (0.03)
Past voting controls		X				X
Demographic controls			X			X
Education controls				X		X
Employment controls					X	X
N	1154	1154	1154	1154	1154	1154
R^2	0.00	0.44	0.01	0.00	0.00	0.45

Notes: The dependent variable is a dummy equal to one if the respondent intended to vote for Berlusconi *before* the announcement, that is during the first interview. **Houseowner** is a dummy equal to one if the respondent lives in a residence owned by himself or by another member of his household. **Past voting controls** include a dummy equal to one if the respondent voted for Berlusconi's coalition in 2001, and a dummy equal to one if the respondent voted for the centre-left coalition in 2001. **Demographic controls** include age, age squared, a currently married dummy, a single dummy, a gender dummy, a parent dummy, and number of children. **Education controls** include dummies for highest educational degree achieved: college degree, highschool, mid-vocational, elementary and none. **Employment controls** include dummies for the following categories: blue collars, unemployed, housewives, students and retired. Robust standard errors in parentheses, significance stars at conventional levels.

Table 5: Placebo

	Voted for Berlusconi's coalition							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Houseowner \times <i>After</i>	0.05 (0.03)	0.05 (0.03)	0.05 (0.03)	0.05 (0.03)	0.05* (0.03)	0.05 (0.03)	0.05 (0.03)	0.06* (0.03)
Student \times <i>After</i>		-0.02 (0.03)						-0.05* (0.03)
Unemployed \times <i>After</i>			-0.04 (0.06)					-0.07 (0.06)
Housewife \times <i>After</i>				0.01 (0.03)				-0.02 (0.03)
Retired \times <i>After</i>					-0.10*** (0.02)			-0.12*** (0.03)
Blue collar \times <i>After</i>						0.01 (0.02)		0.02 (0.02)
Temporary \times <i>After</i>							0.02 (0.04)	0.00 (0.04)
Individual FE	X	X	X	X	X	X	X	X
Time FE	X	X	X	X	X	X	X	X
T	2	2	2	2	2	2	2	2
$N \times T$	2308	2308	2308	2308	2308	2308	2308	2308
R^2	0.06	0.06	0.06	0.06	0.07	0.06	0.06	0.07

Notes: The dependent variable is a dummy equal to one in the first period if the respondent intended to vote for Berlusconi during the first interview, and equal to one in the second period if the respondent eventually voted for Berlusconi in the 2006 election. *After* is a dummy for the second period. **Houseowner** is a dummy equal to one if the respondent lives in a residence owned by himself or by another member of his household. Each regression also includes each term of the interaction separately, but coefficient are not reported. Robust standard errors in parentheses, significance stars at conventional levels.

Table 6: Difference-in-Differences with two periods

	Voted for Berlusconi's coalition			
	(1)	(2)	(3)	(4)
Houseowner \times <i>After</i>	0.05 (0.03)	0.05 (0.03)	0.04 (0.06)	0.05 (0.06)
Student \times <i>After</i>		0.12*** (0.04)		-0.30*** (0.08)
Housewife \times <i>After</i>		0.01 (0.03)		0.02 (0.06)
Blue Collar \times <i>After</i>		0.00 (0.02)		0.01 (0.05)
Individual FE	X	X	X	X
Individual Linear Time Trends			X	X
Time FE	X	X	X	X
T	3	3	3	3
$N \times T$	3462	3462	3462	3462
R^2	0.02	0.03	0.59	0.60

Notes: The dependent variable is a dummy equal to one in the first period if the respondent voted for Berlusconi's coalition in the 2001 election, equal to one in the second period if he intended to vote for Berlusconi in the next election during the first interview (March/April 2006), and equal to one in the third period if he eventually voted for Berlusconi in the 2006 election. *After* is a dummy for the third period. **Houseowner** is a dummy equal to one if the respondent lives in a residence owned by himself or by another member of his household. Each regression also includes each term of the interaction separately, but coefficient are not reported. Robust standard errors in parentheses, significance stars at conventional levels.

Table 7: Difference-in-Differences with three periods

	Year				Overall
	1994	1996	2001	2006	
Votes for Berlusconi's coalition	0.343 (0.138)	0.296 (0.0822)	0.375 (0.107)	0.417 (0.112)	0.358 (0.120)
Houseowners	0.769 (0.0914)	0.771 (0.0782)	0.773 (0.0715)	0.769 (0.0655)	0.771 (0.0773)
Housewives	0.141 (0.0473)	0.137 (0.0422)	0.132 (0.0456)	0.117 (0.0381)	0.132 (0.0444)
Students	0.0622 (0.0169)	0.0638 (0.0164)	0.0655 (0.0208)	0.0660 (0.0185)	0.0644 (0.0183)
Retired	0.202 (0.0855)	0.224 (0.0746)	0.246 (0.0790)	0.264 (0.0681)	0.234 (0.0806)
High school graduates or higher degree	0.501 (6.834)	0.387 (3.421)	0.274 (0.0642)	0.317 (0.0636)	0.370 (3.836)
Without education degree	0.291 (1.439)	0.205 (0.720)	0.120 (0.0617)	0.105 (0.0484)	0.181 (0.812)
Average property tax on main residences			154.5 (248.4)	175.3 (263.9)	165.0 (265.5)
Revenues from property tax on main residences			0.0389 (0.0417)	0.0425 (0.0445)	0.0407 (0.0432)
<i>N</i>	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 eligible voters; **Houseowners (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; **Average property tax on main residences** are the revenues from the property tax on main residences divided by the number of houseowner households; **Revenues from the property tax on main residences** is calculated as a share of total municipal revenues.

Table 8: Summary statistics of the municipality-level data

	Votes for Berlusconi's coalition (share)					
	(1)	(2)	(3)	(4)	(5)	(6)
Houseowners (share) \times 2006	0.18*** (0.04)	0.18*** (0.04)	0.20** (0.09)	0.16*** (0.04)	0.11*** (0.04)	0.12* (0.06)
Municipality FE	X	X	X	X	X	X
Province-level linear trends		X			X	
Municipal-level linear trends			X			X
Controls \times 2006				X	X	X
T	4	4	4	4	4	4
$N \times T$	31953	31953	31953	31953	31953	31953
R^2	0.37	0.42	0.24	0.39	0.44	0.28

Notes: The dependent variable is the share of residents (out of eligible voters) who voted for one of the parties belonging to Berlusconi's coalition. **2006** is a dummy for the 2006 election. **Houseowners (share)** is the share of households (out of resident households) living in an estate 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, but coefficients are not reported. Standard errors are clustered at the province-level (102 clusters). Significance stars at conventional levels.

Table 9: Baseline municipal-level Difference-in-Differences

	Votes for Berlusconi's coalition (share)					
	(1)	(2)	(3)	(4)	(5)	(6)
Houseowners (share) \times 2001	0.04 (0.03)	0.04 (0.05)	0.04 (0.14)	0.07** (0.03)	0.02 (0.05)	0.27* (0.15)
Municipality FE	X	X	X	X	X	X
Province-level linear trends		X			X	
Municipal-level linear trends			X			X
Controls \times 2001				X	X	X
T	3	3	3	3	3	3
$N \times T$	23959	23959	23959	23959	23959	23959
R^2	0.22	0.34	0.24	0.26	0.53	0.59

Notes: The dependent variable is the share of residents (out of eligible voters) who voted for one of the parties belonging to Berlusconi's coalition. **2001** is a dummy for the 2001 election. **Houseowners (share)** is the share of households (out of resident households) living in an estate 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, but coefficients are not reported. Standard errors are clustered at the province-level (102 clusters). Significance stars at conventional levels.

Table 10: Municipal-level Diff-in-Diff: Placebo test in 2001

Weights	(1)	(2)	(3)	(4)	(5)	(6)
None	0.18*** (0.04)	0.18*** (0.04)	0.20** (0.09)	0.16*** (0.04)	0.11*** (0.04)	0.12* (0.06)
Votes cast: mean '94, '96, '01	0.18*** (0.02)	0.19*** (0.05)	0.13 (0.10)	0.17*** (0.04)	0.12*** (0.04)	0.11* (0.06)
Elegible voters in 2006	0.18*** (0.02)	0.20*** (0.05)	0.14 (0.10)	0.17*** (0.04)	0.13*** (0.04)	0.11* (0.06)
Population in 1991	0.28*** (0.07)	0.29*** (0.06)	0.28** (0.11)	0.26*** (0.05)	0.20*** (0.04)	0.20*** (0.06)
Population in 2001	0.18*** (0.02)	0.20*** (0.05)	0.14 (0.10)	0.17*** (0.04)	0.13*** (0.04)	0.11* (0.06)
Municipality FE	X	X	X	X	X	X
Province-level linear trends		X			X	
Municipal-level linear trends			X			X
Controls \times 2006				X	X	X
T	4	4	4	4	4	4
$N \times T$	31953	31953	31953	31953	31953	31953

Notes: Each row reports the coefficient of interest $\hat{\beta}$ (on the interaction between the share of homeowners and a dummy for 2006) using a different variable as weight. In each regression 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 column reports result for a given specification. 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, but coefficients are not reported. Standard errors are clustered at the province-level (102 clusters). Significance stars at conventional levels. The regression weighting variable is reported in the first column.

Table 11: Municipal-level Diff-in-Diff weighted by different municipality sizes

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Houseowners (share) \times 2006	0.13*** (0.04)	0.13*** (0.04)	0.13*** (0.04)	0.14*** (0.04)	0.10*** (0.04)	0.10*** (0.04)	0.10*** (0.04)	0.11*** (0.04)
Property tax on main residence: average tax \times 2006		0.00 (0.00)		0.02*** (0.01)		0.00 (0.00)		0.02*** (0.01)
Property tax on main residence: revenues \times 2006			-0.10*** (0.03)	-0.20*** (0.05)			-0.08** (0.03)	-0.18*** (0.05)
Municipality FE	X	X	X	X	X	X	X	X
Controls \times 2006					X	X	X	X
T	2	2	2	2	2	2	2	2
$N \times T$	15963	15771	15768	15768	15963	15771	15768	15768
R^2	0.38	0.38	0.39	0.39	0.40	0.40	0.40	0.40

Notes: The dependent variable is the share of residents (out of eligible voters) who voted for one of the parties belonging to Berlusconi's coalition. **2006** is a dummy for the 2006 election. **Houseowners (share)** is the share of households (out of resident households) living in an estate owned by one of its members. **Property tax on main residence: average tax** is the average tax paid by each houseowner household in each municipality (in thousands of Euro), calculated as revenues from the property tax on main residence divided by houseowner households. **Property tax on main residence: revenues** is the revenues from the property tax on main residence as a share of total municipal revenues. **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, but coefficients are not reported. Standard errors are clustered at the province-level (102 clusters). Significance stars at conventional levels.

Table 12: Municipal-level Diff-in-Diff: additional channels

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Houseowners (share) \times 2006	0.12*** (0.03)	0.12*** (0.03)	0.12*** (0.03)	0.13*** (0.04)	0.12*** (0.03)	0.12*** (0.03)	0.12*** (0.03)	0.14*** (0.03)
Property tax on main residence: average tax \times 2006		-0.01* (0.01)		0.01 (0.01)		-0.00 (0.00)		0.02** (0.01)
Property tax on main residence: revenues \times 2006			-0.08*** (0.02)	-0.14** (0.05)			-0.06*** (0.02)	-0.14*** (0.04)
Municipality FE	X	X	X	X	X	X	X	X
Controls \times 2006					X	X	X	X
T	2	2	2	2	2	2	2	2
$N \times T$	15963	15771	15768	15768	15963	15771	15768	15768
R^2	0.35	0.35	0.36	0.36	0.38	0.38	0.39	0.39

Notes: The dependent variable is the share of residents (out of eligible voters) who voted for one of the parties belonging to Berlusconi's coalition. **2006** is a dummy for the 2006 election. **Houseowners (share)** is the share of households (out of resident households) living in an estate owned by one of its members. **Property tax on main residence: average tax** is the average tax paid by each houseowner household in each municipality (in thousands of Euro), calculated as revenues from the property tax on main residence divided by houseowner households. **Property tax on main residence: revenues** is the revenues from the property tax on main residence as a share of total municipal revenues. **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, but coefficients are not reported. Standard errors are clustered at the province-level (102 clusters). Significance stars at conventional levels. Regressions are weighted by population size in 2001.

Table 13: Municipal-level Diff-in-Diff: additional channels. Weighted by population in 2001

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Houseowners (share) \times 2006	0.12*** (0.03)	0.11*** (0.03)	0.12*** (0.03)	0.13*** (0.03)	0.12*** (0.03)	0.12*** (0.03)	0.12*** (0.03)	0.14*** (0.03)
Property tax on main residence: average tax \times 2006		-0.01** (0.01)		0.01 (0.01)		-0.00 (0.00)		0.02** (0.01)
Property tax on main residence: revenues \times 2006			-0.08*** (0.02)	-0.14** (0.05)			-0.07*** (0.02)	-0.14*** (0.04)
Municipality FE	X	X	X	X	X	X	X	X
Controls \times 2006					X	X	X	X
T	2	2	2	2	2	2	2	2
$N \times T$	15963	15771	15768	15768	15963	15771	15768	15768
R^2	0.35	0.35	0.36	0.36	0.38	0.38	0.39	0.39

Notes: The dependent variable is the share of residents (out of eligible voters) who voted for one of the parties belonging to Berlusconi's coalition. **2006** is a dummy for the 2006 election. **Houseowners (share)** is the share of households (out of resident households) living in an estate owned by one of its members. **Property tax on main residence: average tax** is the average tax paid by each houseowner household in each municipality (in thousands of Euro), calculated as revenues from the property tax on main residence divided by houseowner households. **Property tax on main residence: revenues** is the revenues from the property tax on main residence as a share of total municipal revenues. **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, but coefficients are not reported. Standard errors are clustered at the province-level (102 clusters). Significance stars at conventional levels. Regressions are weighted by eligible voters in 2006.

Table 14: Municipal-level Diff-in-Diff: additional channels. Weighted by eligible voters in 2006