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Roberto Basile
Valerio Filoso

Does Local Politics Matter? Quasi-experimental Evidence from Italian Municipal Elections

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Indirizzo dell'Autore:
Valerio Filoso
Dipartimento di Giurisprudenza, via Mezzocannone 18, 80138 Napoli – Italy
tel. (39 +) 081 2534664
fax: (39 +) 081 2144333
Email: filoso@unina.it
web site: https://www.docenti.unina.it/valerio.filoso

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Classificazione JEL: H11, H7
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Does Local Politics Matter? Quasi-experimental Evidence from Italian Municipal Elections*

Roberto Basile
Department of Economics
Second University of Naples

Valerio Filoso
Department of Law
University of Naples

September 16, 2014

Abstract

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Contents

1 Introduction

Do political divides have an economic impact on citizens' welfare? The overheated political debate notwithstanding, it is far from clear whether this is really the case. According to the median voter theorem (Downs, 1957), when citizens' preferences are unimodal, rational politicians find profitable to move their electoral platforms towards the center of the political spectrum: over time, this results in increasing similarities between programs and negligible differences between implemented policies. As a consequence, in two-party or bipolar systems, left-wing and right-wing parties or coalitions would mitigate partisanship and implement homogeneous policies, thus producing similar outcomes.

The literature on the effects of local partisanship has focused either on final or on intermediate economic variables of interest influenced by local policies (Gerber and Hopkins, 2011; Ferreira and Gyourko, 2009; Pettersson-Lidbom, 2008), but has not tempted to estimate the net effect of the *whole bundle of benefits and costs* originated by municipalities. Municipalities deliver fiscal packages with local public goods and costs impacting on the welfare of their districts: some municipalities succeed on given margins, while others fall short on different ones. To fully assess the value of the *net* benefits provided by a municipality would require a complete set of observations on every aspect of economic life affected, an impossible task for any researcher. In this paper, we use housing prices to overcome this problem.

Basic microeconomic reasoning suggests that, while municipalities are relatively free to choose their preferred mix of costs and benefits, housing prices adjust in response to utility-maximizing behavior across locations and ultimately will reflect the value of the available endogenous amenities (Kuminoff et al., 2013). In a world where markets adjust without frictions, relocating to a new place is just a matter of indifference for the marginal citizen, with housing prices moving upward in cities whose municipalities increase the provision of net benefits (Oates, 1969; Tiebout, 1956). Given this framework, *we investigate whether the provision of net fiscal benefits, as reflected in housing prices, varies systematically with the political color of municipalities.*

The estimation of the effects of partisanship is challenging since a number of confounding variables are at work. For example, left-wing parties use to promise more income redistribution and public services (typically, schooling) financed by higher tax rates, aims which are easier to attain the higher the level of local income (Rehm, 2011; Allan and Scruggs, 2004). Since housing prices are positively related to incomes, we expect a positive association between the presence of a left-wing municipality and housing prices, but this would not be informative of the causal link we are searching for. Obtaining credible estimates of the effect of partisanship requires a deliberate identification strategy designed to overcome omitted variable bias.

The Italian local electoral system provides a quasi-experimental setting for estimation. In 1993, the Italian National Parliament passed a law which regulates municipal elections according to a majority principle. Cities with more than 15,000 inhabitants have a top-two runoff voting system: with no absolute majority at the first round, then a ballot election is held between the two highest voting mayoral candidates. Cities with less than 15,000 inhabitants have a single-winner voting system, with the highest voting mayoral candidate being elected. In both systems, either the party or the coalition backing up the winning mayoral candidate obtains a sufficient number of seats in the Municipal Council to become majority. The mayor and the corresponding Municipal Council remain in charge for five years. In sum, this is a *winning-party-takes-all* system.

This electoral law can be thought as a natural experiment in the electoral districts in which the majority at the second – but sometimes also at the first – turn is formed *without* a large margin. These districts experience a quasi-random assignment to a bundle of policies almost orthogonal to the prevailing political preferences. Since minor differences in political preferences may result in large differences in the type of policies implemented, this becomes an opportunity to employ a sharp Regression Discontinuity Design (RDD) (Imbens and Lemieux, 2008) to evaluate the *causal* effect of political parties on real estate prices at 3, 4, and 5 years after the election. To perform our estimations, we employ a novel collection of municipal election results linked to real estate prices for the years 2003–2011, for a total of 1,246 observations in which left-wing and right-wing parties confront in a given election. Our data is detailed enough to distinguish between residential or commercial usage and central or peripheral location. The RDD is implemented through a local linear estimator with three alternative bandwidths and a penalized regression spline estimator. The key finding is striking: *we find no evidence of a difference in housing price dynamics in cities ruled by left-wing and right-wing majorities.* The results are extremely robust when checked across different types of land use and suggest that political partisanship, at the local level, is not able to impact the overall level of citizen's welfare, as proxied by real estate price dynamics.

The paper is organized as follows: section 2 provides the theoretical background on the effect of mayoral partisanship; section 3 describes the dataset used in the estimation; section 4 defines our identification strategy and the necessary econometric background for estimation; section 5 discusses empirical results and elaborates on alternative explanations; section 6 concludes and provides lines of future research.

2 Theory

The median voter theorem and its discontents

Since the seminal work of Downs (1957) economists have maintained that centrist policies are the most likely outcome of political competition. Given an unimodal distribution of voters' political preferences, candidates who want to get elected need to maximize the probability that their political platform will be chosen by the largest number of people. This strategy can be attained by shaping electoral programs upon the preferences of the median voter. As a result, elected officials' policies should be centrist and robust to changes in the distribution of preferences not affecting the median voter. In this framework, partisanship can be understood as a disequilibrium phenomenon destined to be wiped out by competition among parties.

Empirical evidence (Kelley, 2014; Gerber and Lewis, 2004; Milanovic, 2000) and theoretical considerations (Acemoglu et al., 2013; Alesina, 1988) cast doubts on this neat result. Pivotal to the strong result of unconditional convergence toward the median voter's preferences is the assumption that politicians just maximize the probability of their elections and that they can commit credibly to a given policy. Nonetheless, politicians could either care about being (re)elected and about policy outcomes because after elections they are often in the position of benefiting their constituencies. Accordingly, in repeated electoral games politicians have an incentive to maximize their probability of being elected by announcing centrist policies but are tempted to hijack these policies after taking office. This makes median voter policies dynamically inconsistent. Rational forward-looking voters cannot systematically trust centrist electoral proclamations and this results into ex ante consistently divergent political platforms and persisting partisanship.

The effect of partisanship on local politics

Anecdotal observation suggests that, compared to national politics, local elections should be less influenced by partisanship and ideological confrontation, but the existing evidence points at a nontrivial role for partisanship also at the local level. For example, Hajnal and Trounstine (2014) find that the political divide between Democrats and Republicans in the U.S. is substantial in municipal elections and that political affiliation is a strong predictor of electoral behavior even when voters' heterogeneity in demographic factors is taken into account. Heath et al. (1999) show that local elections in UK – a country in which central governments have gradually eroded the powers of local authorities – partisanship remains a significant determinant of voting behavior. According to Meredith and Grissom (2010) there are three main channels through which partisanship may have an impact on local politics: (a) influencing the selection process of candidates (Adrian, 1952); (b) affecting the characteristics of the electorate: voters turnout is usually higher in partisan elections (Alford and Lee, 1968); (c) inducing decision biases: when party cues are unavailable, voters may decide upon gender, race, and incumbency (Bonneau and Cann, 2013; Schaffner et al., 2001; McDermott, 1997).

The evidence on the economic significance of partisanship points to a much lesser role. Ferreira and Gyourko (2009) estimate the impact of electing a democratic mayor on policy outcomes at the municipal level using a sample of nearly 2,000 direct mayoral elections in over 400 U.S. cities in the period 1950–2000. They use several outcome variables: budgetary variables (total revenues per capita, total taxes per capita, total expenditures per capita, total employment per capita), allocation of resources (percent spent on salaries and wages, police and fire department, parks and recreation) and crime indexes (murders, robberies, burglaries, larcenies). Using a RD estimator, they find no evidence of systematic differences between Republican and Democratic mayorships. These results are consistent with Alesina (1988), who claims that cities are more homogeneous in their political preferences than higher levels of government and that municipal competition may lead to decreased partisanship as the costs of switching to another municipality is relatively low.

Gerber and Hopkins (2011) hypothesize that mayoral partisanship will more likely affect policy outcomes in policy areas where there is less shared authority between local, state, and federal governments. Political science literature assumes that the two major parties have distinct electoral coalitions and governing philosophies that should lead to quite different policy outcomes, especially on issues of taxing and spending. On one hand, left-wing parties might pursue increased taxes and expanded services, whereas right-wing parties might pursue tax cuts and service reductions. On the other hand, if the various constraints on local policymakers are binding, the impact of mayoral partisanship might prove negligible. Mayor's partisanship is an important determinant of fiscal outcomes in some policy areas where local decision makers are less constrained by other levels of the U.S. federal system. Using an RD design with a sample of U.S. mayoral election in large cities from 1990 to 2006, Gerber and Hopkins (2011) find support for this proposition. Similarly, Leigh (2008) uses a panel data of U.S. states over the period 1941–2002 and finds that the differences between Democratic and Conservative governors are almost negligible.

Pettersson-Lidbom (2008) estimates the causal effect of party control on fiscal and economic outcomes employing a panel dataset of Swedish municipalities. Using RD design, the author finds differences between parties: Left-wing local governments spend and tax 2-3% more than right-wing counterparts; left-wing governments also have 7% lower unemployment rates due to a higher level of employment in the public sector.

Other related studies suggest that ideological differences may drive systematic differences in enacted policies. For example, Picazo-Tadeo et al. (2011) find that ideological differences between parties at the municipal level lead to different water management regimes in Andalusia (Spain), with right-wing majorities leading to more outsourcing. Also, Blom-Hansen et al. (2006) detect a financially significant difference between left-wing and right-wing municipalities in Denmark and Norway with regard to tax policy: according to their data, left-wing municipalities, especially in Denmark, appear to levy higher income and property taxes. Nonetheless, these works do not control for endogenous sorting and preexisting political preferences, so their results must be taken with a grain of salt.

The capitalization of fiscal variables

The key assumption of our model is that real estate prices are responsive to changes in the fiscal packages originated by local public policies; in the public finance literature this phenomenon is known as the *capitalization hypothesis*. In a world of frictionless spatial competition among local authorities (Tiebout, 1956), Oates (1969) explicitly introduced real estate price determination observing that housing prices should adjust to local fiscal differentials and found corroborating evidence for the United States.

Following Oates' contribution, the empirical literature has shown that capitalization is a pervasive phenomenon (Hilber, 2011; Fischel, 2001, 2009; Hamilton, 1976) which does not vanish in spatial equilibrium and persists in the long run (Stadelmann and Billon, 2014, 2012; Epple et al., 1978). The extent of capitalization varies in response to heterogeneity in preferences (Arnott and Stiglitz, 1979), mobility (Jud, 1984), and liquidity constraints (for example, local public services for the elderly are capitalized more strongly than services for young and mobile couples, as in Shan (2010)). Moreover, as full tests for capitalization are impossible to obtain, most empirical studies have focused on narrowly-defined public services, like schooling (Fack and Grenet, 2010; Gibbons and Machin, 2006, 2003), environmental regulation (Bui and Mayer, 2003), zoning restrictions (Glaeser et al., 2005b,a), or subsidies in agriculture (Clark et al., 1993).

The overwhelming evidence provided by the literature on capitalization suggests that housing prices convey important information on fiscal variables, though it is not generally possible to test whether the production of local services meets Samuelson's efficiency criterion (Yinger, 1982). These two statements are the backbones of the consensus view on capitalization. In what follows we do not question the issue of efficiency in the provision of public goods: quite obviously, it can vary across space, social groups, and types of services. We just assume that housing prices are systematically responsive to local differentials and that capitalization patterns are not systematically different across Italian municipalities.

Summing up, the best available evidence to date suggests that differences in the political composition of municipalities have a small or negligible effect on some given margins of citizens' welfare. However, a definitive complete test of the whole bunch of benefits and costs originated by municipalities is still lacking. This is precisely the point we intend to investigate in the next sections with our RD design.

3 Data

3.1 Sources

Our novel dataset is obtained by merging observations from three sources.

Prices This is a dataset on real estate prices released by the *Agenzia del Territorio* (the Italian public agency of territory). The prices are collected on a twice-a-year basis from various market sources and are not linked in any sense to the official prices used to calculate the real estate tax (ICI or IMU), which are known to systematically diverge from actual exchange prices. Different prices are observed by location (generic, central, peripheral), type of use (residential, commercial or industrial), and bounds (highest price, lowest price). Real estate prices for Sicily are not available.

Elections This is a dataset on municipal election results collected by the Italian Home Office. We label "Right-Wing" any coalition containing at least one of the major right-wing parties (*Forza Italia*, *Alleanza Nazionale*, *Polo delle Libertà*, *Il Popolo della Libertà)*. We label "Left-Wing" any coalition containing at least one of the major left-wing parties (*Partito Democratico*, *Democratici di Sinistra*). Dubious cases (unidentified party names) were dropped altogether.

Codes This is a dataset on the city names, codes, and residing population released by ISTAT (the Italian Office of Statistics). These data were used to obtain a safe merging between the two previous datasets.

3.2 Selection

The dataset used for estimation contains a collection of municipal election results linked to real estate prices for the years 2003–2011, for a total of 1,246 observations and an initial number of 13,784 elections held during that period: the gap between these two numbers is explained by the high degree of political fragmentation at the local level and by difficulties in the identifiability of many lists' denominations. Since we focus exclusively on the right-wing/left-wing voting alternative, we discard a number of cases:

1. In cities with less than 15,000 inhabitants, we drop observations for elections in which either the first or the second highest voting mayor is not backed up by a clearly left-wing or right-wing coalition or party.

2. In cities with more than 15,000 inhabitants, we drop observations for first-round elections or ballots in which either the first or the second highest voting mayor is not backed up by a clearly left-wing or right-wing coalition or party.

Tables 3 and 4 report the average regional levels of real estate prices by type of house (residential and non-residential), while fig. 1 shows the provincial distribution of the average real estate prices over the period 2003–2011. The traditional North-South dualism in real estate prices clearly emerges. Table 5 displays the regional distribution of municipalities with a right-wing party and a left-wing party majority. A higher frequency of left-wing parties in charge appears in North-Eastern and and in Central regions.

Table 6 reports the average annual growth rates of real estate prices following a new municipal election. Finally, two-sample t tests in table 8 clearly show that there is no statistical difference between the average growth rates of real estate prices in municipalities governed by right-wing and left-wing majorities.

3.3 The Italian system of decentralization

The relevant fiscal aspects of the Italian system of local authorities at the regional, provincial, and municipal level have been radically reformed by a constitutional law passed in 2001 and fully enacted in

2003. This change in legislation redefined the balance of power between the central state and the local authorities, reserving the traditional function of macroeconomic stabilization to the central government, while giving the regions the possibility to establish new taxes and municipalities the power of changing rates on local taxes, which basically reduce to a property tax, an additional income tax, and other minor taxes financing public services. On average, around 65% of all the revenues collected by municipalities are derived from local taxes, grants from other levels of government and tariffs, as it can be seen in the table 1.

The local council's areas of competence are diversified and far-reaching, impacting on several aspects of city life and, ultimately, on citizens' welfare and local amenities. The main areas of action are: 1. Administration, management, and control; 2. Local police; 3. Public education; 4. Culture; 5. Sport and recreation; 6. Tourism; 7. Transport; 8. Environmental management; 9. Social services; 10. Commerce. A more comprehensive list can be found in tab. 7.

4 Empirical identification strategy

The evidence reported in Table 4 cannot represent a proper test of the causal effect of political parties on the growth rate of local real estate prices. An accurate evaluation of the left-wing party policy effect must contend with problems of isolating the effect of local public policies from the confounding effect induced by other factors. To overcome this problem, we rely on a quasi-experimental design, the Regression Discontinuity (RD) design introduced by Thistlethwaite and Campbell (1960).

As discussed above, a large number of studies have already used a RD design to estimate electoral effects on various political and economic outcomes of interest. Generally speaking, the RD approach is a way of estimating *treatment* effects in a quasi-experimental setting where treatment assignment is a discontinuity function of an observed variable at a known threshold value (Lee and Lemieux, 2010; Imbens and Lemieux, 2008). Specifically, the RD design estimates local average impacts around the threshold at the point where treatment and comparison units are most similar. Thus, the RD design well fits the aim to identify a policy impact by separating the effect of other factors influencing the outcome under analysis.

In a sharp RD design, we can construct RD estimates by fitting the model

$$y_i = m(x_i) + w_i\tau + \epsilon_i \tag{1}$$

where the outcome variable y_i for each unit i is a smooth function of the assignment variable (x_i). In equation (1), τ is a measure of the discontinuity of the conditional expectation of the outcome as a function of assignment variable at the threshold value c, $w_i = 1\{x_i \geq c\}$ is the treatment and ϵ_i is an error term. The parameter τ is interpreted as evidence of a *causal* effect of the treatment, provided that all other factors affecting y_i are evolving smoothly with respect to x_i. A sufficient condition for identification of τ is to assume continuity of $m(x)$ at c and the existence of the limits $\lim_{x\uparrow c} E[w_i|x]$ and $\lim_{x\downarrow c} E[w_i| = x]$. In the case of a sharp design, $\lim_{x\uparrow c} E[w_i|x] = 0$ and $\lim_{x\downarrow c} E[w_i| = x] = 1$, so that

$$\tau_{SRD} = \lim_{x\downarrow c} E[y_i|x_i] - \lim_{x\uparrow c} E[y_i|x_i]. \tag{2}$$

In our case, for the municipality i, the *outcome* variable is

$$y_i = 100 \times \delta^{-1} \log \frac{P_{i,t_0+\delta}}{P_{i,t_0}}, \tag{3}$$

that is, the average annual growth rate of real estate prices computed over the time interval $(t_0, t_0 + \delta)$, where t_0 is a given municipal election date and $\delta \in \{3, 4, 5\}$ is the number of years after the election over which the average is calculated.

The *treatment* variable, w_i, is a dummy variable taking value zero for an election won by the right-wing party and value one for an election won by the left-wing party at time t_0. The *assignment* variable, x_i, is the difference between the fraction of votes awarded to the left-wing party minus the fraction of votes awarded to the right-wing party, briefly indicated as the *left-wing vote margin*. When x_i exceeds the cutoff of zero, the municipality is then governed by a left-wing coalition.

The presence of a *sharp* discontinuity in the formation of a majority allows us to implement a sharp RD design: average left-wing party effects are estimated by comparing the average annual growth rate of real estate prices of the group of cities with a value of x_i just above the threshold with the average annual growth rate of real estate prices of the group of cities with a value of x_i just below the threshold.

We claim that the municipalities with a vote share for the left-wing party just below the cutoff (zero and just below) will be very similar to municipalities with a vote share for the left party just above the cutoff (for example, those scoring 0.01), except that they are governed by a right-wing party. Thus, municipalities just below the threshold can be used as a comparison group for the municipalities just above to estimate the *counterfactual* (what would have happened to the group of cities controlled by the left-wing party if they were controlled by the right party).

Therefore, the RD design is a valuable tool for identifying electoral effects only when the relevant actors do not have precise control over election results, that is when the winners and the losers of close elections do not differ systematically. This is enough to assume that the treatment (i.e. the election of the left-wing party) is as good as randomly assigned around the cut-off. The local random assignment implies that the discontinuity gap at the cut-off identifies the treatment effect and that we do not need any control and any model to consistently detect the effects of left-wing parties on the outcome.

The assumption of no systematic sorting at the discontinuity could be violated, for example, if parties over-perform in close elections in their own strongholds due to electoral fraud. A number of studies contend for example that a precise control over election results by the relevant actors is possible in large elections, such as those for the U.S. House of Representatives (over the post-War period) where the incumbent party is more likely to win very close elections (e.g., Hainmueller et al. (2014); Caughey and Sekhon (2011); Grimmer et al. (2011); Snyder (2005)). In order to assess whether the evidence of systematic incumbent advantages in the U.S. House indicates a general problem with the use of RD to measures electoral effects, Eggers et al. (2013) examine whether similar problems occur in other electoral settings, including every partisan, single winner, plurality/majoritarian election setting where data could be collected and assembled. Thus, they study elections to the U.S. House in other time periods as well as statewide, state legislative, and mayoral races in the U.S.; they also study national and/or local elections in a variety of other countries. However, they do not find any other case exhibiting systematic incumbent advantages. Thus, they conclude that the assumptions behind the RD design are likely to be met in a wide variety of electoral settings and that RD design is a fundamentally sound and widely applicable approach to learning about the effect of election results on a variety of political and economic outcomes.

A priori, the fundamental assumption that candidates do not perfectly control the electoral outcome seems also likely to be met in our case study. Albeit a few cases of electoral fraud can always take place at the municipality level, there is no anecdotal evidence of a systematic corrupt electoral manipulation. Moreover, in the next section, before implementing the sharp RD design, we will formally test its validity by assessing the continuity of the distribution of the forcing variable near the electoral threshold, as suggested by Lee (2008) and McCrary (2008).

5 Results

5.1 Evidence from a local linear estimation

As we are focusing solely on elections in which left-wing parties face right-wing parties, our forcing variable is the share of votes for the left-wing party minus the share of votes for the right-wing party and the cutoff value is zero: positive values for the forcing variable indicate a victory for the Left, negative values a victory for the Right. The corresponding treatment variable is zero for Right-wing-ruled and one for Left-wing-ruled municipalities. The outcome variable is the percent change in real estate prices measured after three, four, and five years after the elections. We also distinguish between residential and non-residential facilities and between urban and peripheral locations.

A valid assessment of the RD design requires a preliminary exploration of the distribution of the forcing variable to ascertain whether any suspect change takes place in the neighborhood of the discontinuity. To perform this check, we report in fig. 2 an histogram of the density of majoral elections by left-wing party margin of victory (the difference between the share of votes won by the left-wing and the right-wing coalitions), along with a corresponding estimated kernel density function. The visual inspection of the graph shows a reasonably smooth distribution and does not reveal any relevant jump (or endogenous sorting) at the discontinuity cutoff of zero. Moreover, we implement McCrary (2008)'s test (reported in fig. 4) and find no evidence of discontinuity of the assignment variable at zero (diff $=-.112$, se $=.155$).[1] This result supports our assumption of no systematic sorting (no manipulation) at the discontinuity. With these results in hand, we can safely proceed to our main estimation.

To implement the sharp RD design, we estimate two distinct local linear kernel regressions on both sides of the discontinuity (Hahn et al., 2008). These regression models explain the change in real estate prices as functions of the forcing variable; more specifically, the average treatment effect of a left-wing majority is computed as the difference between the intercepts of the two local linear regressions on the right and left sides of the cutoff point: $\widehat{\tau}_{SRD} = \widehat{\alpha}_r - \widehat{\alpha}_l$.

As it is well known, the choice of the kernel function has little impact on the correct specification of the local linear regression model, whereas the choice of the appropriate bandwidth to balance precision and bias can be crucial: smaller bandwidths tend to produce lower bias and higher variance, and *vice versa*. We take advantage of the contribution of Imbens and Kalyanaraman (2012) that presents a data-dependent method for choosing an asymptotically optimal bandwidth in the case of a RD design. Local linear regression models are estimated using a triangular kernel function and bandwidths larger (twice) and smaller (half) than the optimal bandwidth for testing the robustness of the results. Standard errors are bootstrapped. A geographical map of the municipalities included in the estimation is displayed in fig. 3.

A preliminary graphical representation of our findings is provided in figures 5, 6, and 7[2] for several types of real estate units after three, four, and five years from the election. In each subgraph we plot the relation between the outcome variable (the percent change in real estate prices) on the y-axis and the forcing variable (the share of votes for the left-wing party minus the share of votes for the right-wing party) on the x-axis, on either sides of the cutoff. The vertical line at zero sharply distinguishes between treated (i.e., those governed by a left-wing majority) and non-treated municipalities (i.e., those governed by a right-wing majority). The corresponding estimates are displayed in table 10 in which we collect all the main results.

[1] McCrary (2008) suggests testing the null hypothesis of continuity of the density of the forcing variable at the threshold, against the alternative of a jump in the density function at that point. Thus, the focus of McCrary (2008)'s test is on the difference $\tau_{f(x)} = \lim_{x\downarrow c} f_x(x) - \lim_{x\uparrow c} f_x(x)$.

[2] To avoid clutter, in this case we limit ourselves to the case of optimal bandwidths.

While the visual impression of the various graphs is somewhat scattered, with some candidates to significant treatment effects, the analytical assessment of the results leaves no room for ambiguity: *there is no evidence of a significant left-wing party effect on the dynamics of real estate prices*. The results remain the same across residential and non-residential facilities and across central and peripheral locations. The use of alternative bandwidths (optimal, double, and half) does not seem to make any difference. This is consistent with the graphical evidence reported above: in most of the subgraphs in figures 5, 6, and 7 the outcome variables have a flat profile for any margin of victory or defeat. This indicates that even in cities where left-wing party candidates won or lost by large margins, different policies were not being implemented (or they have not being capitalized in house prices).

A check of the robustness of the results from the local linear estimator relates to the *plausibility of the randomization hypothesis*: according to this fundamental assumption for the credibility of the RD design, municipalities close to the cutoff should be identical on average. While this assumption can never be fully tested because of possibly infinite confounders, we can instead check whether the distribution of some potentially relevant, observable, and exogenous variables is balanced between treated and nontreated units. The results of this check are reported in table 9 and show that coastal and provincial capital cities may not be perfectly balanced. As suggested by Lee and Lemieux (2010), we first regress our outcome variable on the exogenous covariates, then we repeat the previous RD estimation on the residualized variable, to partial out the effect of potential threats to the RD design. The results for this exercise are collected in table 11 and do not contradict the neat ones obtained previously. After this last check, we can safely conclude that the assumptions underpinning the validity of our quasi-experimental design are valid and that the result of no effect of political partisanship is robust to a number of specifications and estimation methods.

5.2 A penalized regression spline estimator

Rau (2011) proposes an alternative nonparametric method for estimating treatment effects in a RD design using penalized regression spline estimators and generalized cross-validation to choose the smoothing parameters. He also proposes to exploit the Bayesian interpretation of penalized regression to obtain the standard errors from the posterior variance-covariance matrix (see also Wood, 2006). In a Monte Carlo simulation study, Rau (2011) shows the Bayesian based confidence intervals perform quite well in terms of realized coverage probabilities and outperforms frequentist based confidence intervals for the local polynomial estimators.

In this section we use the penalized regression spline method to estimate the following semiparametric regression equation:

$$y_{it} = \alpha + w_{it}\tau + f(x_{it}) + f\left(x_{it}\right)w_{it} + \epsilon_{it} \tag{4}$$

where y_{it} is the value of the outcome (the real estate price growth rate) in the municipality i in the election year t; w_{it} is the dummy indicating whether in the election period t the elected major belongs to a left-wing coalition and τ is its associated parameter measuring the treatment effect; $f(x_{it})$ is an unknown smooth function capturing the conditional expectation of y given the forcing variable x_{it} (the margin of victory in election t in city i);[3] the interaction term $f(x_{it})w_{it}$ allows us to assess whether the smooth effect of x_{it} is different for the treated (i.e. when $w_{it} = 1$). The error term ϵ_{it} captures

[3] This flexible control for the vote share helps absorb variation coming from non-close election. Thus, the pure left-wing party effect, τ, is consistently estimated controlling for the margin of the victory in nonlinear form. Several studies use polynomial expansions (up to a cubic term) to approximate this nonlinear function (see, e.g., Ferreira and Gyourko (2014)). Although rather easy to implement, this solution remains arbitrary and might introduce severe multicollinearity.

all other observed and unobserved determinants of the real estate price growth rate and it is assumed to be identically and independently distributed. The method used to identify and estimate model 4 is briefly described in the Appendix.

The estimation results of model 4 are reported in columns 3-5 of table 12. Again, we replicate the estimate of this model eighteen times, that is for each combination of the type of house (residential vs. non-residential), the location (central, peripheral or any) and the time span used for the computation of the average annual growth rate of the price (3, 4 and 5 years). The estimated parameters τ clearly indicate again that the impact of having a left-wing majority on the real estate price dynamics is negligible and not statistically different from zero: the associated p-value is always higher than 0.10. Only in one case (residential houses of any type) the parameter τ approaches a statistical significance at 10% when the growth rate of house prices is computed after 5 years from the election: the -1.108 coefficient would imply that having a left-wing majority leads to this growth rate being one percent lower. This result is consistent with the one obtained using the local liner estimator.

In practice, eq. 4 may be inefficient because the error term may have important components that vary at the municipality level or at the year of election level. Therefore, more precise estimates of τ can be obtained by including fixed effects for the election years (γ_t) and fixed effects for the municipality (F_i).[4] Moreover, we use include in this augmented RD regression equation the smooth interaction between the latitude (*northing*) and the longitude (*easting*) of the centroids of the municipalities included in our sample, $h\left(no_i, e_i\right)$. This *geoadditive* term, also known as the *spatial trend surface*, allows us to isolate the effect of other spatial factors influencing the outcome under analysis:

$$y_{it} = \alpha + w_{it}\tau + f(x_{it}) + f\left(x_{it}\right) w_{it} + \gamma_t + F_i + h\left(no_i, e_i\right) + \epsilon_{it} \tag{5}$$

The estimation results of model 5, reported in columns 6-8 of table 12, clearly suggest the evidence discussed above are strongly robust to spatial and time controls.

Finally, that left-wing party effects on local house prices truly are close to nil is further confirmed by the very small effects found even when running the following model

$$y_{it} = \alpha + w_{it}\tau + x_{it}\beta_1 + x_{it}w_{it}\beta_2 + \epsilon_{it} \tag{6}$$

where the forcing variable enter linearly. These results are reported in columns 9-11 of table 12. Even though this model provides upwardly biased estimates of the true left-wing party effect, there is no evidence that having a major from a left-wing coalition is associated with statistically different outcomes in terms of real estate prices.

6 Conclusions

In this paper we have contributed to the study of the impact of political partisanship at the municipal level. Given that real estate market prices adjust to the value of the package of goods and bads provided by municipal administrations (the capitalization hypothesis), we have tested whether these prices also react to different political majorities. Our estimates have shown unambiguously that left-wing majorities, on average, do not perform better than right-wing majorities at the municipal level. This result is partially in line with those obtained in the literature which focused solely on specific indicators of local public policies, like unemployment or taxation. Moreover, contrary to previous literature, we

[4] Specifically, we include three dummy variables indicating whether the municipality is located in the coast, in the mountains and if it is a provincial capital, respectively.

are in the position of evaluating the impact of the whole bunch of policies enacted by local political authorities.

This lack of differentiation on the efficiency margin can be interpreted in several ways. The first one is the most straightforward: according to the median voter theorem, political parties tend to occupy the median position in the political spectrum and to enact very similar policies. In equilibrium, the outcome of local policies pursued by the Right and the Left cannot diverge systematically. An alternative explanation calls into question the role of redistributive policies: municipal policies could actually differ between Right and Left, but just on a purely distributive margin. On balance, these policies would favor some groups over others, but would not be capable of pushing the efficiency frontier of the local economy outwardly. An implication of this second scenario is that spatial mobility could interact with redistributive policies and exacerbate territorial unbalances. Nonetheless, we find that partisanship does not change the urban-suburban housing prices differentials: this suggests that alternative city councils do not differ systematically on how they target their policies, at least in the spatial dimension.

Another explanation for the lack of difference we found in the data is that city councils may face systematic constraints to enacting their agenda, given the legal framework they are forced to operate in. Though a national law was introduced in 1999 to achieve budget balance (the Domestic Stability Pact), the rules have changed significantly over the following years and currently very little is known about actual sanctions for its violations. Several ad hoc budget rebalancing operated by central government have also contributed to weaken municipal fiscal discipline (Cioffi et al., 2012). Moreover, we have convincing evidence that fiscal behavior of Italian municipalities significantly relates to political variables, like the number of political parties seated in the city council (Grembi et al., 2012). Though the most recent years have seen an increasing concern for fiscal stability of local governments due to the Italian macroeconomic imbalances, we have reasons to be skeptical about the decisive role of exogenous budget rules during our window of observation.

Our preferred explanation relates to the combined effect of the job and the real estate market. Most Italian internal migration is driven by unemployment and wages considerations (Di Cintio and Grassi, 2013; Piras, 2012; Basile and Causi, 2007). Moreover, Italy has high fixed legal costs related to house sales and an exceptionally low loan-to-value index (around 40%) (Maclennan et al., 1998): both factors conjure to make spatial mobility costly, with the latter making housing decisions more dependent on current rather than future income. If our conjecture is correct, municipalities enjoy a rent which politicians may use at their benefit to relax competition between districts and increase wasteful spending.

What about the economic costs of political partisanship? The data on the expenses of local political campaigns cannot be easily obtained since municipalities usually neither collect nor release data on this issue, but some back-of-the-envelope calculations can help provide a rough estimate of the values involved. One remarkable exception to the general lack of data is the municipality of Rome, Italy's capital city, which has published some selected figures on electoral expenses borne by the thirty three elected council men in 2013's elections (Menicucci, 2013): these sum up to €4.234.660, with per elected official expenses amounting to €128.323 (sd = €351,345). This is clearly an understatement of the real costs of partisanship given that the expenses incurred by non-elected candidates are not available. Since current population in Rome is currently 2,649,724 inhabitants, this produces an average cost of partisanship of roughly €1,6 per resident citizen. With the Italian population being around 60,920,000 people and assuming that partisanship expenses per capita remain roughly the same across Italy, it follows that the average cost of partisanship related to a full electoral turn amounts to €97,359,383. While it is reasonable to conjecture that these expenses surely benefit the political groups involved in the electoral competitions, our evidence suggests that the actual benefit for the citizens is much more questionable.

A final word on the limitations of our work. Italian political landscape is more patchy than our analysis suggests, since a large majority of municipalities are run by civic lists which are not affiliated to national parties: nonetheless, often mayoral candidates are the top candidate of apparently nonpartisan civic lists. This behavior is mostly driven by the desire to release the grip of national parties on local decisions. To study whether this link results in increased efficiency in municipal decisions will be the object of our future explorations.

Appendix

A Identification and estimation of the penalized regression model 4

The univariate smooth term $f(x)$ in equation (4) can be approximated by a linear combination of known basis functions b_q

$$f\left(x\right)=\sum_{q}\beta_{q}b_{q}(x) \tag{7}$$

where β_q are unknown parameters to be estimated. To avoid mis-specification bias, $q's$ must be made fairly large. But this may generate a danger of over-fitting. As it will be better clarified below, smoothness of the functions can be controlled by penalizing wiggly functions in the model fitting. Thus, a measure of 'wiggliness' $J \equiv \beta_q^{'}\mathbf{S}\beta_q$, where **S** is a positive semi-definite matrix, is associated with the smooth function. Typically, the wiggliness measure evaluates a function like the univariate spline penalty $\int f^{''}(x)^2 d(x)$ or its thin-plate spline generalization (Wood, 2003, 2006a).

In the case of the interaction term $f\left(x\right)w$, the basis functions $b_q(x)$ are pre-multiplied by a diagonal matrix containing the values of the interaction variable (w). To estimate (4) it is desirable to use the same degree of smoothness (that the the same smoothing parameter λ) for the two smooth terms.

The penalized spline base–learners can be extended to two or more dimensions to handle interactions by using thin–plate regression splines or tensor products (Wood, 2006a, Section 4.1.5). In the case of a tensor product, smooth bases are built up from products of 'marginal' bases functions. For example,

$$h(no,e)=\sum_{q_{no}}\sum_{q_e}\beta_{q_{no},q_e}b_{q_{no}}(no)b_{q_e}(e)$$

Corresponding wiggliness measures are derived from marginal penalties (Wood, 2006a).

Given the bases for the smooth term, equation (4) can be re-written in matrix terms as a large linear model

$$\begin{aligned} \mathbf{y} &= \Sigma_q\beta_q b_q(x)+\ldots+\boldsymbol{\varepsilon} \\ &= \mathbf{X}\boldsymbol{\beta}+\boldsymbol{\varepsilon} \end{aligned} \tag{8}$$

where matrix **X** includes all the basis functions, while $\boldsymbol{\beta}$ contains all the smooth coefficient vectors, β_q, the intercept coefficients and τ.

As mentioned above, the number of parameters for a smooth term in a semi-parametric model has to be large enough to avoid mis-specification bias, but not too large to escape overfitting. To solve this trade-off, we need to penalize lack of smoothness. Thus, parameters $\boldsymbol{\beta}$ in model (8) can be estimated by minimizing the penalized residual sum of squares

$$\widehat{\boldsymbol{\beta}} = \arg\min \|\mathbf{y} - \mathbf{X}\boldsymbol{\beta}\|^2 + \lambda\boldsymbol{\beta}'\mathbf{S}\boldsymbol{\beta} \tag{9}$$

where $\lambda \geq 0$ is a smoothing parameter which control the flexibility of the function estimate with large values enforcing smooth estimates and small values allowing for high flexibility. Employing a large number of basis functions yields a flexible representation of the nonparametric effect $f(\cdot)$ where the actual degree of smoothness can be adaptively chosen by varying λ.

Given the smoothing parameter, λ, resulting estimate is

$$\widehat{\boldsymbol{\beta}} = \left(\mathbf{X}'\mathbf{X} + \lambda\mathbf{S}\right)^{-1}\mathbf{X}'\mathbf{y} \tag{10}$$

The covariance matrix of $\widehat{\boldsymbol{\beta}}$ can be derived from that of $\mathbf{y}$

$$V_{\widehat{\boldsymbol{\beta}}} = \left(\mathbf{X}'\mathbf{X} + \lambda\mathbf{S}\right)^{-1}\mathbf{X}'\mathbf{X}\left(\mathbf{X}'\mathbf{X} + \lambda\mathbf{S}\right)^{-1}\sigma^2 \tag{11}$$

If we also assume normality, that is $\mathbf{y} \sim \mathcal{N}\left(0, I\sigma^2\right)$, then

$$\widehat{\boldsymbol{\beta}} \sim \mathcal{N}\left(E\left(\widehat{\boldsymbol{\beta}}\right), V_{\widehat{\boldsymbol{\beta}}}\right) \tag{12}$$

It must be recognized, however, that frequentist confidence intervals based on the naive use of $\widehat{\beta}$ and the corresponding covariance matrix perform quite poorly in terms of realized coverage probability (Wood, 2006b). Thus, in practice, in additive models based on penalized regression splines frequentist inference yields to reject the null hypothesis too often. To overcome this problem and following Wahba (1983), Silverman (1985) and Wood (2006a,b), a Bayesian approach to coefficient uncertainty estimation can be implemented. This strategy recognizes that, by imposing a particular penalty, we are effectively including some prior beliefs about the likely characteristics of the correct model. This can be translated into a Bayesian framework by specifying a prior distribution for the parameters β. Specifically, Wood (2006b) shows that using a Bayesian approach to uncertainty estimation results in a Bayesian posterior distribution of the parameters

$$\boldsymbol{\beta}|\mathbf{y} \sim \mathcal{N}\left(E\left(\widehat{\boldsymbol{\beta}}\right), \left(\mathbf{X}'\mathbf{X} + \lambda S\right)^{-1}\sigma^2\right) \tag{13}$$

This latter result can be used directly to calculate credible intervals for any parameter. Moreover, it turns out (Wahba, 1983; Wood, 2006b) that the credibility intervals derived via Bayesian theory are well behaved also from a frequentist point of view, i.e. their average coverage probability is very close to the nominal level $1 - \alpha$, where α is the significance level.

So far everything is conditional on λ, the smoothing parameters controlling the trade-off between fidelity to the data and smoothness of the fitted spline. The optimal smoothing parameter can be selected minimizing the generalized cross validation (GCV) score:

$$GCV(\lambda) = \frac{N \parallel \mathbf{y} - \mathbf{X}\widehat{\beta} \parallel^2}{\left[N - tr(\mathbf{A})\right]^2} \tag{14}$$

where $\mathbf{A} = \mathbf{X}(\mathbf{X}'\mathbf{X} + \sum \lambda\mathbf{S})^{-1}\mathbf{X}'$ is the hat matrix for the model being fitted and its trace, $tr(A)$, gives the effective degrees of freedom edf (i.e. the number of identifiable parameters in the model). The edf are a general measure for the complexity of a function estimates that allows to compare the smoothness even for different types of effects (e.g. nonparametric versus parametric effects). If λ=0, then edf is equal to the size of the β vector minus the number of constraints. Positive values of λ lead to an effective reduction of the number of parameters. If λ is high, we have very few edf.

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B Tables and graphs

B.1 Descriptive statistics

Table 1
Municipal revenues, 2011

Type	Amount	%
Local taxes	33,393,246	39.8
Capital alienations	13,661,668	16.3
Tariffs for services	12,507,188	14.9
Grants from other levels of government	11,564,046	13.8
Services on behalf of third parties	6,603,167	7.9
Debt openings	6,122,464	7.3
Total	83,851,779	100.0

Source: ISTAT (Italian Statistical Office), retrieved on May 22nd, 2014 from `http://www.istat.it/it/archivio/72865`. All figures are in thousands of euros.

Table 2
Municipal expenses, 2011

Type	Amount	%
Administration, management, and control	16,321,599	30.2
Environmental management	11,107,970	20.5
Social services	8,623,086	16.0
Transport	5,725,561	10.6
Public education	5,172,048	9.6
Local police	3,119,392	5.8
Culture	1,666,227	3.1
Sport and recreation	757,089	1.4
Productive services	475,930	0.9
Commerce	454,595	0.8
Tourism	323,107	0.6
Justice	309,029	0.6
Total	54,055,633	100.0

Source: ISTAT (Italian Statistical Office), retrieved on May 22nd, 2014 from `http://www.istat.it/it/archivio/91984`. All figures are in thousands of euros.

Table 3
Average real estate prices: Residential

	Year						
	2005	**2006**	**2007**	**2008**	**2009**	**2010**	**Total**
Region							
Abruzzo	964	854	1,062	1,152	1,134	1,171	981
Basilicata		811	802	693	719	557	731
Calabria	511	587	797		647	696	588
Campania	985	1,024	1,470	1,489	1,260	1,728	1,225
Emilia-Romagna	1,315	1,378	1,682	1,294	1,451	1,171	1,432
Lazio	1,221	1,262	1,655	1,933	1,684	1,697	1,507
Liguria	2,414	2,004	2,522	4,644	2,232	2,301	2,474
Lombardia	1,430	1,495	1,477	1,520	1,358	1,326	1,411
Marche	1,332	1,339	1,429	1,680	1,334	1,569	1,399
Molise		1,135					1,135
Piemonte	1,071	1,138	1,225	1,293	1,269	1,230	1,202
Puglia	662	833	894	976	1,067	954	873
Sardegna	931	941	1,657	960		1,150	1,146
Toscana	2,993	1,455	1,822	2,281	1,767	2,272	1,784
Umbria	709	1,041	1,025		1,072		1,043
Veneto	959	1,129	1,399	1,465	1,158	2,596	1,261
Total	1,031	1,098	1,393	1,511	1,443	1,368	1,302
Geographical partition							
North West	1,483	1,351	1,530	1,763	1,359	1,418	1,436
North East	1,048	1,253	1,447	1,427	1,413	1,527	1,374
Center	1,398	1,312	1,634	1,991	1,629	1,831	1,584
South	781	862	1,117	1,168	1,059	1,174	972
Islands	931	941	1,657	960		1,150	1,146
Total	1,031	1,098	1,393	1,511	1,443	1,368	1,302

Table 4
Average real estate prices: Non-residential

	Year						
	2005	**2006**	**2007**	**2008**	**2009**	**2010**	**Total**
Region							
Abruzzo	1,141	953	1,224	1,457	1,295	1,464	1,137
Basilicata		821	935	713	677	552	748
Calabria	644	713	1,137		750	889	735
Campania	1,167	1,181	1,634	1,648	1,447	1,938	1,397
Emilia-Romagna	1,451	1,529	1,847	1,413	1,469	1,308	1,477
Lazio	1,429	1,406	1,739	2,227	1,846	1,823	1,671
Liguria	2,432	1,660	2,097	3,184	2,132	2,059	2,145
Lombardia	1,428	1,592	1,525	1,555	1,373	1,436	1,453
Marche	1,417	1,357	1,525	1,762	1,367	1,651	1,449
Molise		1,642					1,642
Piemonte	1,134	1,086	1,286	1,180	1,247	1,261	1,194
Puglia	838	957	1,003	1,172	1,263	1,124	1,028
Sardegna	1,062	1,008	1,411	1,047		1,288	1,180
Toscana	1,616	1,337	1,675	2,136	1,563	1,935	1,584
Umbria	802	1,312	1,036		1,203		1,182
Veneto	1,037	1,290	1,472	1,535	1,212	3,640	1,372
Total	1,139	1,188	1,440	1,615	1,424	1,492	1,356
Geographical partition							
North West	1,498	1,330	1,518	1,635	1,361	1,483	1,432
North East	1,140	1,409	1,535	1,508	1,435	1,891	1,441
Center	1,376	1,374	1,614	2,118	1,517	1,822	1,553
South	951	995	1,265	1,356	1,202	1,353	1,127
Islands	1,062	1,008	1,411	1,047		1,288	1,180
Total	1,139	1,188	1,440	1,615	1,424	1,492	1,356

Table 5
Frequencies for the parties in charge

	Observations			Percentages		
	Right	Left	Total	Right	Left	Total
Region						
Abruzzo	13	26	39	33.3	66.7	100.0
Basilicata	6	13	19	31.6	68.4	100.0
Calabria	23	24	47	48.9	51.1	100.0
Campania	67	71	138	48.6	51.4	100.0
Emilia-Romagna	12	136	148	8.1	91.9	100.0
Lazio	38	49	87	43.7	56.3	100.0
Liguria	5	15	20	25.0	75.0	100.0
Lombardia	86	80	166	51.8	48.2	100.0
Marche	13	28	41	31.7	68.3	100.0
Molise	0	2	2	0.0	100.0	100.0
Piemonte	32	57	89	36.0	64.0	100.0
Puglia	71	73	144	49.3	50.7	100.0
Sardegna	9	19	28	32.1	67.9	100.0
Toscana	15	158	173	8.7	91.3	100.0
Umbria	8	25	33	24.2	75.8	100.0
Veneto	38	35	73	52.1	47.9	100.0
Total	436	811	1,247	35.0	65.0	100.0
Geographical partition						
North West	123	152	275	44.7	55.3	100.0
North East	50	171	221	22.6	77.4	100.0
Center	74	260	334	22.2	77.8	100.0
South	180	209	389	46.3	53.7	100.0
Islands	9	19	28	32.1	67.9	100.0
Total	436	811	1,247	35.0	65.0	100.0

Figure 1

Average real estate prices by provinces, 2003-2011

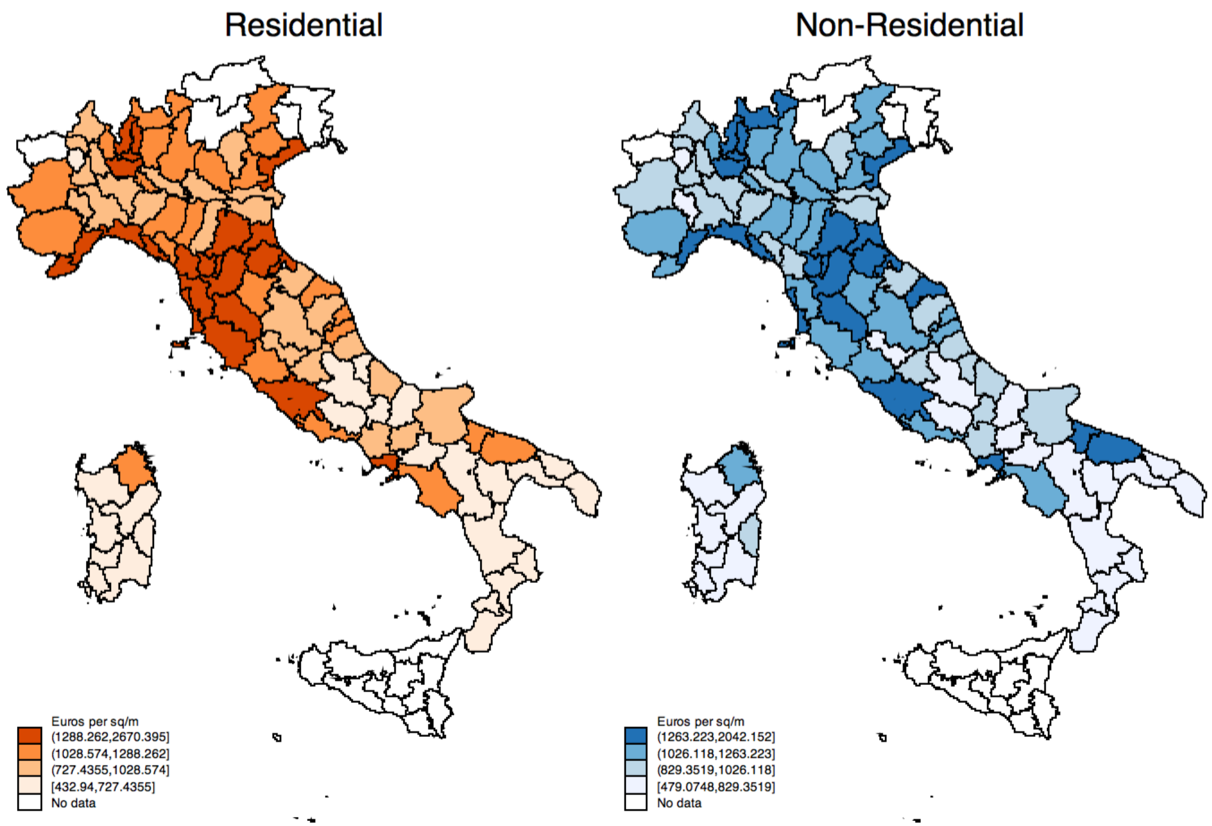

FIGURE 2
Left-to-right difference in the share of votes
Histogram and kernel density

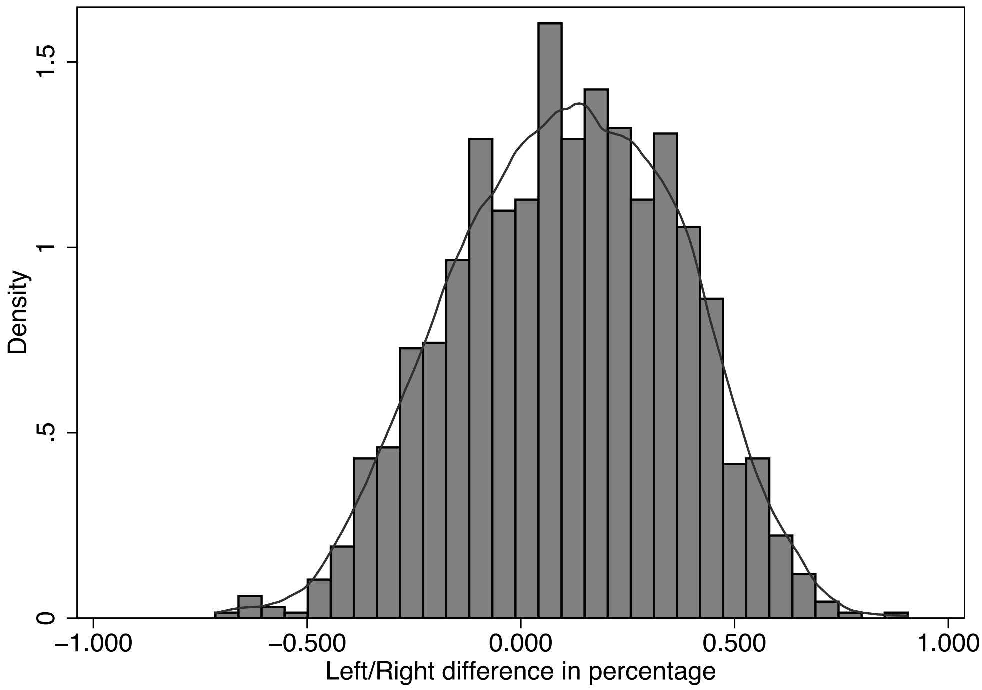

Table 6
The average annual growth rate of real estates prices following a new municipal election

	Descriptive statistics					
Residential	Mean	Md	σ	min	max	n
Central location						
After three years	1.799	0.568	4.737	-22.630	22.363	452
After four years	1.790	0.567	4.473	-16.120	18.037	371
After five years	1.999	0.789	4.048	-4.567	15.432	248
Peripheral location						
After three years	1.298	0.627	4.119	-22.846	20.649	410
After four years	1.319	0.510	3.825	-15.955	15.919	332
After five years	1.569	0.628	3.381	-4.889	14.388	213
Any location						
After three years	1.825	0.902	4.569	-22.733	18.237	452
After four years	1.854	0.890	4.361	-16.042	16.186	371
After five years	2.055	1.204	3.937	-4.461	15.432	248
Non-residential						
Central location						
After three years	0.893	0.155	3.886	-21.975	15.183	451
After four years	0.849	-0.049	3.678	-16.219	14.201	370
After five years	1.040	0.251	3.294	-4.789	11.987	247
Peripheral location						
After three years	0.889	-0.186	4.126	-23.223	29.030	369
After four years	0.751	-0.178	3.679	-16.093	24.917	299
After five years	0.929	0.094	3.143	-4.591	19.859	189
Any location						
After three years	1.044	0.262	3.866	-22.433	18.782	452
After four years	1.013	0.088	3.686	-16.163	16.360	371
After five years	1.169	0.360	3.329	-4.708	12.985	248

Table 7

The main areas of municipal jurisdiction

Economic development and productive activities	Land, environment and infrastructure	Services to individuals and communities
Retail commerce regulation, licenses	Urban planning	Health protection
Industry plants licensing	Environmental protection	Social services
Job seeker register	Aqueducts construction and maintainance	Education and school services
Tourism	Transport and road traffic	Local police
Agriculture	Civil protection	Assets and cultural activities
Organization of professional training courses	Cadastre	Sport plants licensing
	Construction licensing	Conservation of cultural heritage
	Waste collection and disposal	
	Public roads construction and maintainance	
	Public works	
	Traffic regulation and planning	
	Water management and sewers	

B.2 Estimation results

FIGURE 3
Municipalities included in the estimation

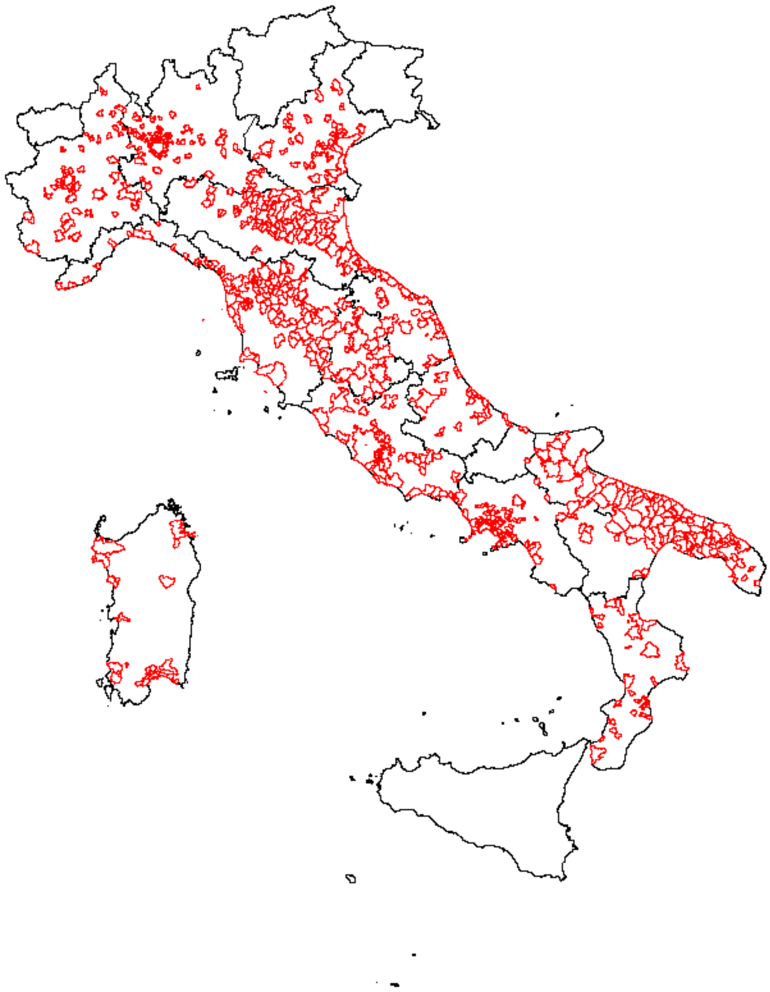

Table 8
Student's t test for the difference between groups
Variable: Average annual growth rate of local real estate prices

After 3 years

Type	**Location**	$\bar{X}_L$	σ_L	$\bar{X}_R$	σ_R	Δ	p-value
Residential	Any	1.423	4.251	2.171	4.807	-0.748	0.960
Residential	Central	1.383	4.327	2.156	5.045	-0.773	0.960
Residential	Peripheral	0.933	3.794	1.616	4.367	-0.683	0.955
Non-Residential	Any	0.767	3.641	1.282	4.041	-0.514	0.922
Non-Residential	Central	0.630	3.795	1.120	3.957	-0.489	0.909
Non-Residential	Peripheral	0.461	3.405	1.276	4.657	-0.815	0.973

After 4 years

Type	**Location**	$\bar{X}_L$	σ_L	$\bar{X}_R$	σ_R	Δ	p-value
Residential	Any	1.585	4.375	2.050	4.351	-0.465	0.844
Residential	Central	1.473	4.403	2.022	4.520	-0.549	0.879
Residential	Peripheral	1.066	3.894	1.506	3.773	-0.439	0.848
Non-Residential	Any	0.783	3.787	1.181	3.610	-0.398	0.846
Non-Residential	Central	0.669	3.834	0.982	3.562	-0.313	0.788
Non-Residential	Peripheral	0.222	3.439	1.158	3.814	-0.936	0.987

After 5 years

Type	**Location**	$\bar{X}_L$	σ_L	$\bar{X}_R$	σ_R	Δ	p-value
Residential	Any	2.142	4.078	2.006	3.866	0.136	0.399
Residential	Central	1.989	4.125	2.005	4.017	-0.016	0.512
Residential	Peripheral	1.621	3.398	1.541	3.384	0.081	0.434
Non-Residential	Any	1.222	3.485	1.139	3.248	0.083	0.427
Non-Residential	Central	1.132	3.395	0.987	3.245	0.146	0.371
Non-Residential	Peripheral	0.665	2.892	1.077	3.278	-0.412	0.814

TABLE 9
Randomization check

	Bandwidth								
	Optimal			Half			Double		
Variable	τ	$s.e.$	p-value	τ	$s.e.$	p-value	τ	$s.e.$	p-value
Alpine	0.013	0.123	0.917	0.101	0.176	0.566	0.054	0.089	0.542
Coastal	0.084	0.049	0.084	0.082	0.067	0.223	0.073	0.042	0.083
Provincial capital	0.064	0.041	0.118	0.057	0.054	0.292	0.062	0.035	0.073
Log of altitude	-0.008	0.253	0.975	0.230	0.356	0.518	-0.145	0.185	0.433
Log of total surface	0.100	0.202	0.623	0.266	0.286	0.353	0.082	0.147	0.576
Log of population, 2001	0.146	0.167	0.382	0.092	0.201	0.647	0.208	0.134	0.121
Log of population, 2008	0.169	0.167	0.311	0.099	0.200	0.620	0.229	0.134	0.086
Log of population, 2009	0.172	0.167	0.304	0.101	0.201	0.616	0.232	0.134	0.083
Log of population, 2010	0.172	0.167	0.306	0.099	0.201	0.621	0.233	0.134	0.083

Figure 4
McCrary's test on the discontinuity of the assignment variable

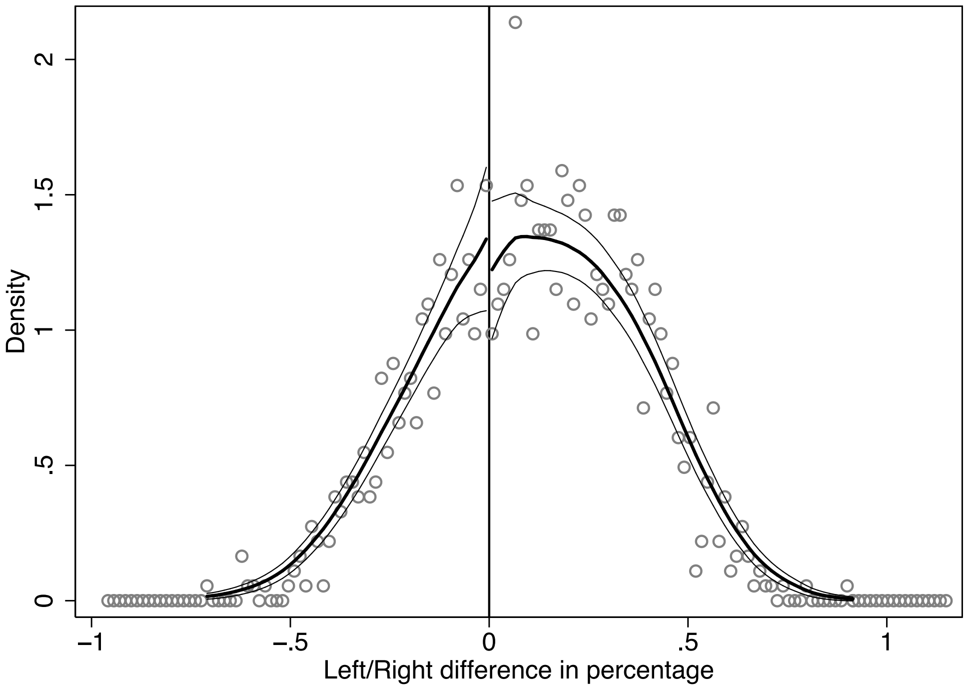

TABLE 10
Regression discontinuity estimates
Local linear regressions, varying bandwidth

Dependent variables		Bandwidth											
		Optimal				Half				Double			
Type	Location	τ	*s.e.*	p-value	n	τ	*s.e.*	p-value	n	τ	*s.e.*	p-value	n
After 3 years													
Residential	Any	-0.483	(1.308)	0.712	452	-0.558	(1.844)	0.762	452	-0.115	(0.924)	0.901	452
Residential	Central	-0.236	(1.290)	0.855	452	-0.138	(1.808)	0.939	452	0.161	(0.919)	0.861	452
Residential	Peripheral	0.620	(1.126)	0.582	410	1.063	(1.713)	0.535	410	0.263	(0.796)	0.741	410
Non-residential	Any	0.102	(0.958)	0.915	452	0.259	(1.338)	0.846	452	0.229	(0.690)	0.740	452
Non-residential	Central	0.250	(0.944)	0.791	451	0.909	(1.341)	0.498	451	0.305	(0.682)	0.655	451
Non-residential	Peripheral	0.741	(1.027)	0.471	369	0.508	(1.392)	0.715	369	0.520	(0.752)	0.490	369
After 4 years													
Residential	Any	-1.086	(1.614)	0.501	371	-1.424	(2.105)	0.499	371	-0.826	(1.149)	0.472	371
Residential	Central	-0.736	(1.544)	0.634	371	-0.652	(1.971)	0.741	371	-0.399	(1.095)	0.716	371
Residential	Peripheral	0.157	(1.063)	0.882	332	0.922	(1.489)	0.536	332	-0.173	(0.839)	0.837	332
Non-residential	Any	-0.170	(1.232)	0.890	371	-0.281	(1.521)	0.853	371	0.077	(0.881)	0.930	371
Non-residential	Central	-0.188	(1.234)	0.879	370	0.019	(1.575)	0.991	370	-0.032	(0.874)	0.971	370
Non-residential	Peripheral	0.794	(1.021)	0.437	299	0.913	(1.152)	0.428	299	0.590	(0.801)	0.461	299
After 5 years													
Residential	Any	-1.387	(1.473)	0.346	248	-1.210	(1.989)	0.543	248	-0.973	(1.081)	0.368	248
Residential	Central	-1.429	(1.484)	0.335	248	-1.147	(1.993)	0.565	248	-0.809	(1.091)	0.458	248
Residential	Peripheral	1.241	(1.383)	0.370	213	1.767	(2.130)	0.407	213	0.391	(0.969)	0.686	213
Non-residential	Any	-0.008	(1.220)	0.995	248	-0.042	(1.606)	0.979	248	0.117	(0.906)	0.897	248
Non-residential	Central	0.014	(1.166)	0.990	247	-0.034	(1.579)	0.983	247	0.132	(0.869)	0.879	247
Non-residential	Peripheral	1.331	(0.929)	0.152	189	1.400	(1.114)	0.209	189	0.959	(0.731)	0.189	189

Table 11
Regression discontinuity estimates on residualized outcome variable
Local linear regressions, varying bandwidth

Dependent variables		Bandwidth											
		Optimal				Half				Double			
Type	Location	τ	*s.e.*	p-value	n	τ	*s.e.*	p-value	n	τ	*s.e.*	p-value	n
After 3 years													
Residential	Any	-0.311	(1.247)	0.803	452	-0.327	(1.747)	0.852	452	-0.042	(0.890)	0.963	452
Residential	Central	-0.039	(1.334)	0.977	452	0.387	(1.823)	0.832	452	0.287	(0.944)	0.761	452
Residential	Peripheral	0.561	(1.024)	0.584	410	1.259	(1.526)	0.409	410	0.159	(0.749)	0.832	410
Non-residential	Any	0.298	(1.026)	0.771	452	0.682	(1.389)	0.623	452	0.419	(0.727)	0.564	452
Non-residential	Central	0.480	(0.837)	0.567	451	0.856	(1.194)	0.474	451	0.224	(0.645)	0.728	451
Non-residential	Peripheral	0.888	(0.940)	0.345	369	1.039	(1.279)	0.416	369	0.412	(0.715)	0.565	369
After 4 years													
Residential	Any	-1.082	(1.616)	0.503	371	-1.087	(2.110)	0.607	371	-0.834	(1.150)	0.468	371
Residential	Central	-0.715	(1.621)	0.659	371	-0.184	(2.076)	0.929	371	-0.471	(1.152)	0.683	371
Residential	Peripheral	0.056	(1.043)	0.957	332	0.914	(1.451)	0.529	332	-0.297	(0.823)	0.718	332
Non-residential	Any	-0.139	(1.284)	0.914	371	-0.013	(1.565)	0.993	371	0.116	(0.918)	0.900	371
Non-residential	Central	-0.133	(1.245)	0.915	370	0.140	(1.570)	0.929	370	0.002	(0.882)	0.998	370
Non-residential	Peripheral	0.884	(0.939)	0.346	299	1.254	(1.096)	0.252	299	0.535	(0.768)	0.486	299
After 5 years													
Residential	Any	-1.309	(1.480)	0.376	248	-0.855	(2.011)	0.671	248	-0.939	(1.076)	0.383	248
Residential	Central	-1.338	(1.496)	0.371	248	-0.811	(2.023)	0.689	248	-0.757	(1.090)	0.488	248
Residential	Peripheral	0.827	(1.113)	0.458	213	2.259	(1.604)	0.159	213	-0.030	(0.830)	0.972	213
Non-residential	Any	0.041	(1.205)	0.973	248	0.197	(1.577)	0.901	248	0.132	(0.896)	0.883	248
Non-residential	Central	0.096	(1.153)	0.934	247	0.100	(1.537)	0.948	247	0.171	(0.863)	0.843	247
Non-residential	Peripheral	1.574	(0.798)	0.049	189	2.456	(0.841)	0.003	189	1.039	(0.666)	0.119	189

Figure 5
Regression discontinuity
Optimal bandwidth
The effect of partisanship after three years

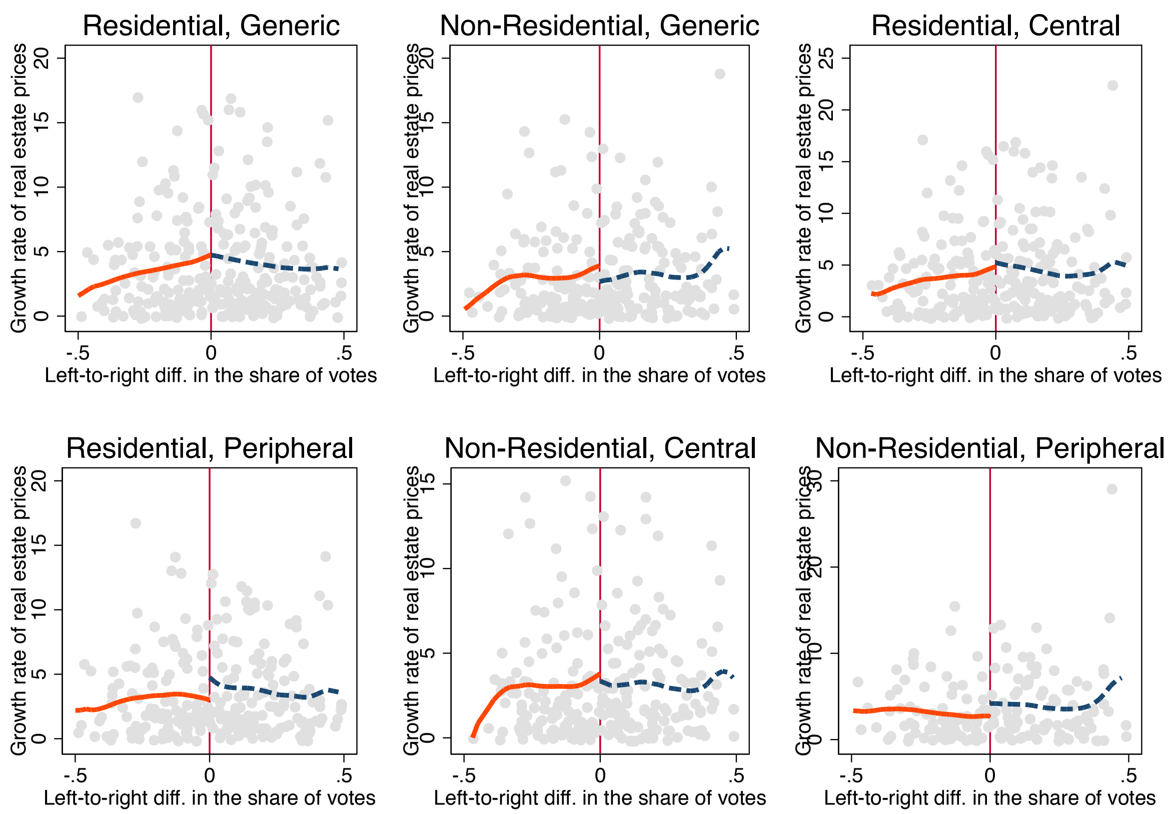

Figure 6
Regression discontinuity
Optimal bandwidth
The effect of partisanship after four years

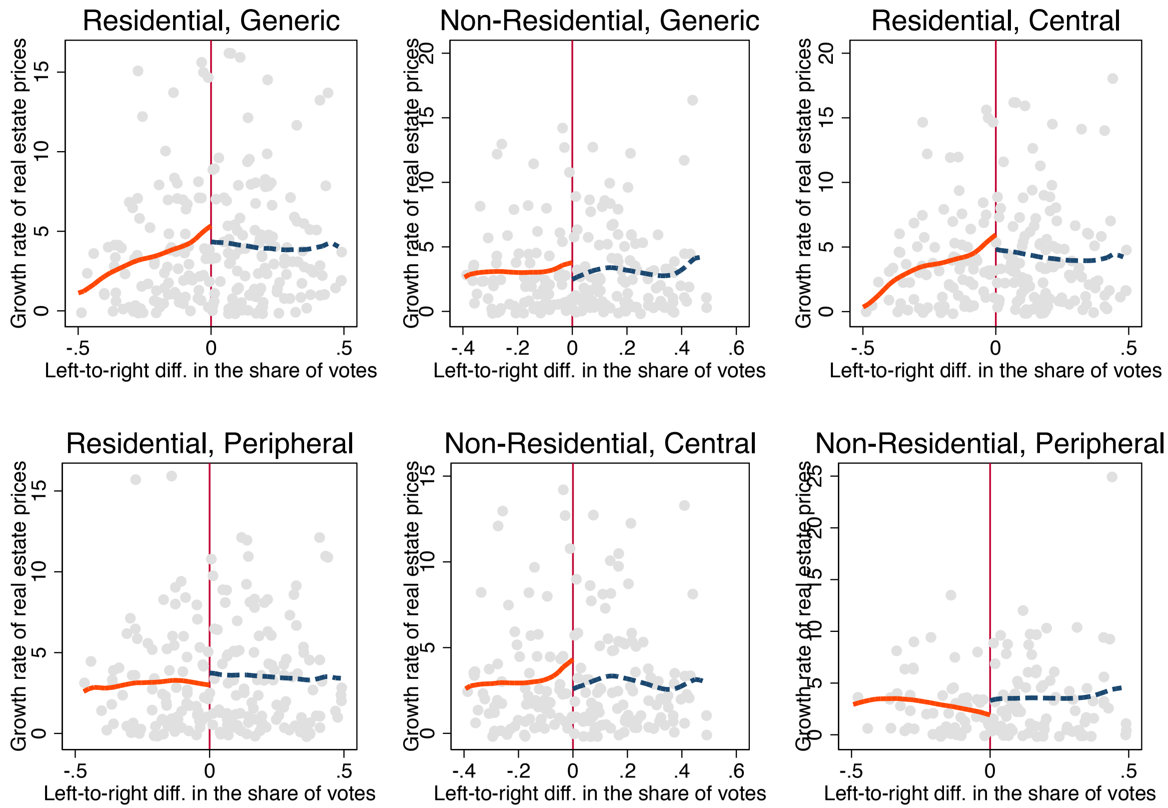

FIGURE 7
Regression discontinuity
Optimal bandwidth
The effect of partisanship after five years

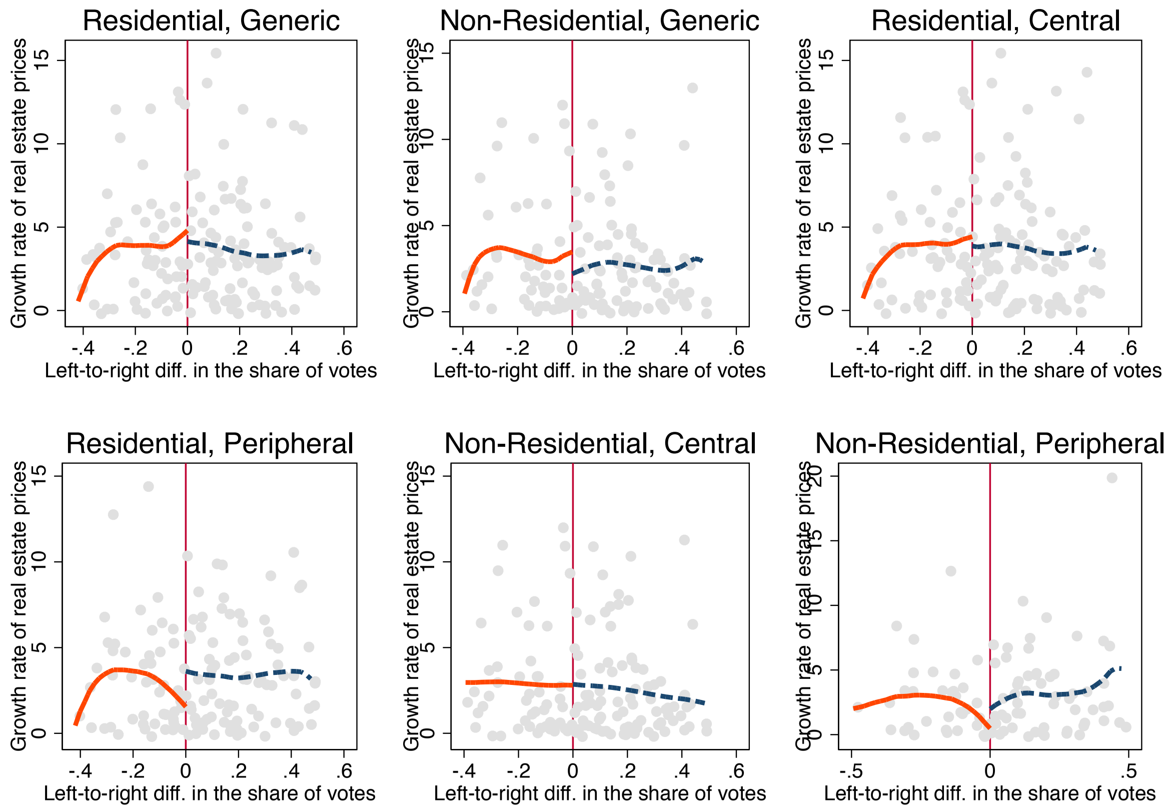

TABLE 12
RD estimates
Penalized regression spline estimator

		Eq. 4			Eq. 5			Eq. 6			
Type	Location	τ	*s.e.*	p-value	τ	*s.e.*	p-value	τ	*s.e.*	p-value	n
After 3 years											
Resid.	Any	-0.067	(0.609)	0.912	-0.012	(0.583)	0.983	-0.067	(0.729)	0.926	427
Resid.	Central	0.056	(0.642)	0.930	0.166	(0.557)	0.766	0.083	(0.768)	0.914	427
Resid.	Periph.	0.064	(0.585)	0.913	0.149	(0.492)	0.762	0.072	(0.701)	0.919	391
Non-Res.	Any	0.036	(0.517)	0.944	0.052	(0.434)	0.905	0.048	(0.619)	0.938	427
Non-Res.	Central	0.109	(0.524)	0.836	0.186	(1.035)	0.858	0.150	(0.628)	0.812	426
Non-Res.	Periph.	0.137	(0.608)	0.822	0.085	(0.530)	0.873	0.151	(0.729)	0.836	351
After 4 years											
Resid.	Any	-0.552	(0.691)	0.425	-0.415	(0.598)	0.489	-0.613	(0.817)	0.454	349
Resid.	Central	-0.385	(0.712)	0.589	-0.232	(0.633)	0.714	-0.410	(0.841)	0.627	349
Resid.	Periph.	-0.265	(0.672)	0.694	-0.137	(0.564)	0.809	-0.333	(0.793)	0.675	316
Non-Res.	Any	-0.163	(0.584)	0.781	0.084	(1.300)	0.948	-0.146	(0.690)	0.832	349
Non-Res.	Central	-0.254	(0.581)	0.662	0.185	(1.670)	0.912	-0.226	(0.687)	0.742	348
Non-Res.	Periph.	0.339	(0.669)	0.613	0.341	(0.584)	0.559	0.358	(0.794)	0.653	284
After 5 years											
Resid.	Any	-1.092	(0.788)	0.168	-1.053	(0.678)	0.122	-1.180	(0.901)	0.192	228
Resid.	Central	-0.920	(0.817)	0.261	-0.839	(0.723)	0.247	-0.964	(0.934)	0.303	228
Resid.	Periph.	-0.420	(0.911)	0.645	-0.417	(0.661)	0.529	-0.730	(0.895)	0.415	199
Non-Res.	Any	-0.208	(0.670)	0.757	-0.178	(0.576)	0.757	-0.132	(0.766)	0.863	228
Non-Res.	Central	-0.178	(0.661)	0.788	-0.203	(0.896)	0.821	-0.030	(0.757)	0.968	227
Non-Res.	Periph.	0.295	(0.785)	0.708	0.287	(0.667)	0.668	0.231	(0.893)	0.796	176

Notes: Eq. 5 includes fixed effects for the years of election, three dummy variables indicating whether the municipality is located in the coast, in the mountains and if it is a provincial capital, and the smooth interaction between the latitude (*northing*) and the longitude (*easting*) of the centroids of the municipalities included in our sample, $h\,(no_i, e_i)$.

Discussion Papers
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