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Turnout in Concurrent Elections: Evidence from Two Quasi-Experiments in Italy*

Enrico Cantoni, Ludovica Gazzè, and Jerome Schafer†

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Abstract

We study the effects of different types of concurrent elections using individual-level administrative and survey data from Italy. Exploiting different voting ages for the two Houses of Parliament in a voter-level Regression Discontinuity Design, we find no effect of Senate voting eligibility on voter turnout or information acquisition. We also estimate city-level Differences-in-Differences showing that concurrent high-salience municipal elections increase turnout in lower-salience provincial and European elections, but not vice-versa. These concurrency effects are concentrated in municipalities in the South of Italy, possibly due to weaker political parties and lower levels of social capital.

Keywords: turnout, concurrent elections, regression discontinuity design, Italy.

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1 Introduction

Low voter turnout is commonly considered a threat to the legitimacy of representative democracy (Lijphart, 1997). Low electoral participation, in fact, usually means unequal participation and unequal government responsiveness to citizens' needs (Avery, 2015; Franko, Kelly and Witko, 2016; Hajnal and Trounstine, 2005; Hill and Leighley, 1992). Yet, voter turnout in many established democracies has been decreasing over the last decades (Blais, 2010; Gaebler, Potrafke and Roesel, 2020), with little or no success by policy-makers to attenuate or reverse this trend.

Exploring the theoretical reasons for low and unequal participation, a growing literature highlights that many low-turnout countries, notably the U.S., display a high number and frequency of elections (e.g., Taylor et al., 2014). Frequent elections can reduce participation because more numerous elections increase information costs and require more time and effort from citizens (Leininger, Rudolph and Zittlau, 2016), potentially lowering voters' motivation to be civically engaged (Garmann, 2017). These negative effects tend to be particularly consequential in "second-order" elections at the regional and local level, especially so if they are held "off-cycle" (Lijphart, 1997). Yet, if election frequency increases the cost of participation, holding multiple elections concurrently seems a natural way to increase voter participation. Moreover, if different groups of voters care differentially about different elections, election timing may affect the composition of the electorate (Anzia, 2011; Halberstam and Montagnes, 2015; Leininger, Rudolph and Zittlau, 2016). Concurrent elections seem particularly promising to increase turnout among young voters, whose levels of participation are dismally low in many democracies (Hajnal and Trounstine, 2005; Kogan, Lavertu and Peskowitz, 2018).

In spite of the strong theoretical expectation that concurrent elections should raise levels of turnout, the prior empirical evidence is ambiguous. In a meta-analysis, Geys (2006) showed that about half of surveyed studies found a positive and statistically significant association between concurrency and turnout, whereas the other half found either null or negative effects. Recent work leverages Differences-in-Differences (DD) designs and provides a robust body of empirical evi-

dence showing that holding lower salience elections on the same day as higher salience elections may increase turnout in the U.S. (Anzia, 2011; Kogan, Lavertu and Peskowitz, 2018) and Europe (Fauvelle-Aymar and François, 2015; Garmann, 2016; Leininger, Rudolph and Zittlau, 2016). However, this literature also suggests that the magnitude and consequences of these effects depend on the political and social context. In particular, the electoral and policy implications of election timing may vary depending on the level of voter mobilization by political parties and interest groups (Anzia, 2011; Kogan, Lavertu and Peskowitz, 2018). Yet, we know little about the voter-level processes that affect turnout and the composition of the electorate in concurrent elections – also because the prior literature focuses primarily on aggregate-level shocks in election timing. Thus, the conditions under which concurrency increases turnout remain elusive.

We contribute by leveraging two quasi-experiments from Italy. First, we exploit a peculiarity of the Italian constitution; namely, the existence of two distinct voting ages for the two Houses of the Italian Parliament: 18 years for the Chamber of Deputies, and 25 years for the Senate. In other words, after turning 25, Italian voters are faced with two high-salience concurrent elections, while voters aged 18 to 25 can only vote for the Chamber of Deputies. We thus use a Regression Discontinuity Design (RDD) to analyze voter turnout and information acquisition around the Senate voting-age threshold. Because Italian electoral campaigns do not target individual voters,[1] our RDD analysis allows to estimate the turnout effect of the additional Senate ballot holding the overall salience of the election constant, meaning that treated (i.e., voters 25 or older) and control voters (i.e., voters younger than 25) were exposed to identical campaigns and media environments, among others. This natural quasi-experiment provides an opportunity to study the effects of individual-level variation in election concurrence.

Our RDD analysis on administrative voter-level data shows that Senate voting eligibility has no impact on turnout. Similarly, survey data we collected specifically for this study during the 2013 Italian general election reveal that voters across the 25-year cutoff report the same voting behavior and are, among others, equally likely to remember the names of Senate candidates running in their

[1] See Cantoni and Pons (N.d.); Novelli (2018) on the impersonal, unsophisticated campaign methods used today in Italy.

district. This suggests that Senate voting eligibility has no effect on information acquisition either.

The zero impact of Senate voting eligibility on turnout and knowledge about candidates is surprising. The 2006–2013 Italian electoral system made it harder for parties to muster a majority of seats in the Senate than in the Chamber of Deputies (see Section 2). This feature of the Italian electoral system was widely covered by Italian pundits ahead of elections, suggesting that the lack of effects is not driven by the Senate election being perceived as inconsequential. Overall, this finding provides a first piece of evidence that the effect of concurrency on turnout is highly dependent on the political context of an election, including the salience and level of party mobilization, which is held constant in our RDD analysis due to the lack of sophistication of Italian campaigns (e.g., no micro-targeting).

Second, we employ a municipality-level DD design based on quasi-random variation in the calendars of Italian municipal, provincial, regional, national, and European elections to analyze the effects of different combinations of concurrent elections on turnout and counts of valid ballots cast. Although elections of each type are usually held every 5 years, their calendars often shift over time due to early elections (e.g., caused by the death of a city's mayor or by snap national elections), thus creating plausibly exogenous within-city and within-Election-Day variation in the number and types of concurrent elections. This design allows to explore the heterogeneity of effects by the relative salience of concurring elections (e.g., a high-salience election concurring with a lower-salience one vs. a low-salience election concurring with a higher-salience one), where we proxy the salience of a certain election type (e.g., municipal elections) with average turnout absent concurrent elections (i.e., without concurring provincial, regional, and national elections).

Our city-level DD reveals that high-salience elections increase turnout and the number of valid ballots cast when they concur with lower-salience elections. The impact of concurrent high-salience elections is large in magnitude. For example, concurrent municipal elections increase turnout at provincial, European, and regional elections by 11.8, 9.9, and 7 percentage points, respectively, which translate to an increase in valid ballots casts of 9, 7.2, and 4.8 percentage points, respectively, despite also increasing invalid ballots cast by 2.7, 2.7, and 2.2 percentage points, re-

spectively. Conversely, turnout effects of concurrent low-salience elections are more nuanced and generally heterogeneous: they are positive and significant only in southern regions and are larger when differences in concurring elections are more marked (e.g., when low-salience provincial elections concur with European rather than with regional elections).

Several explanations help to reconcile the seemingly conflicting findings from our DD and RDD exercises. In Italian parliamentary elections, voters are not targeted separately by electoral campaigns for the House and Senate elections. This feature might lead voters to consider the two elections as if they were a single election, a conflation that reduces the marginal benefit voters derive from the additional Senate ballot.[2] By contrast, concurrent elections of different types induce voters who are primarily interested in the higher-salience election to also cast a valid ballot for the lower-salience one. This suggests that, when concurrent elections are perceived as different from one another and their concurrence "changes" the electoral environment (e.g., resulting in more intense campaigns and/or heightened media attention than if the same elections were held separately), Italian voters face relatively small costs of acquiring information for the lower-salience election. Finally, concurrency effects are concentrated in municipalities in the South of Italy, which suggests that these effects are stronger in regions where the ties between political parties and voters are weaker and levels of social capital are lower (Putnam, Leonardi and Nanetti, 1994).

Our contribution to the literature is two-fold. First, our voter-level RDD is unique in that it generates individual-level variation in the number of elections voters face (two for voters 25 or older, one for younger voters), while keeping the political context plausibly constant. Our null RDD finding points to heightened overall electoral salience as a key mechanism of the turnout effect of concurrent elections. Moreover, our unique survey data suggest that concurrent Upper and Lower House elections in Italy do not affect knowledge about candidates, likely because political campaigns do not differentially target likely voters – in contrast to concurrent Presidential and Senate elections in the U.S., where information asymmetries may lead to distortions in vote choice (Halberstam and Montagnes, 2015).

[2] At polling locations, Italian voters receive one paper ballot for each election (e.g., municipal, provincial, regional, Chamber of Deputies, Senate) they are eligible to vote for on that Election Day.

Second, while several authors have studied concurrent elections of different salience separately and in different contexts, we analyze concurrent elections in a unified setting and use our DD estimates to compare the effects of different combinations of high- and low-salience concurrent elections, which allows us to identify sources of heterogeneity. For example, using a subset of the data we use in our city-level analysis, Bracco and Revelli (2018) find that concurrent municipal elections increase voter turnout in less-salient provincial elections, while Revelli (2017) shows that turnout in municipal elections is higher when these contests concur with higher-salience national elections. Similarly, in their study of the electoral determinants of city performance in 93 large Italian municipalities, 2001–2010, Lo Prete and Revelli (2020) find that concurrent national parliamentary elections increase turnout in municipal elections, while concurrent European, regional, and provincial elections do not.[3] In the U.S., Fowler (2015) observes that the Democratic gubernatorial candidate's vote share is, on average, 6.4 percentage points higher in states with on-cycle gubernatorial elections. We show that turnout effects hinge crucially on the relative salience of the concurrent races. Moreover, we find that concurrency effects are concentrated in the South of Italy. Overall, we interpret our results as suggestive of the importance of the social and political context in mediating invidual responses to concurrent elections.

The paper proceeds as follows. Sections 2 and 3 discuss, respectively, the research setting and the data. Section 4 presents the results. Section 5 discusses theoretical implications in a unified framework. Section 6 concludes.

2 Research Setting

We rely on two sources of plausibly exogenous variation in the number and types of elections Italian voters face on Election Day. First, we exploit a peculiarity of the Italian legislature, namely the existence of different voting ages for the two Houses of Parliament. Second, we use quasi-random variation in the concurrence of municipal, provincial, and regional elections with local and

[3] Unlike our specifications, their regressions do not control for Election Day fixed effects, which allows them to estimate the effect of concurrent elections that happen on the same day across all municipalities (i.e., European and national parliamentary elections).

countrywide elections.

Italy features a perfectly bicameral legislature: the Parliament consists of two Houses that share the same powers and separately perform identical functions. All Members of Parliament (MPs) are elected on the same day and remain in power until the next election. Any MP can propose new bills, which, to become laws, must be approved in the same text by both Houses of Parliament. Either House can oust the executive passing a motion of no confidence, and a joint session of Parliament elects the President of the Republic.

The two Houses differ in a few minor features, including different sizes (i.e., 630 and 315 members for the Chamber of Deputies and the Senate, respectively[4]) and different minimum ages to become a member (i.e., 25 and 40 years to become a deputy or a senator, respectively). Historically, the two Houses have also been characterized by different electoral systems. Between 1993 and 2005, the Chamber and the Senate featured two slightly different "hybrid" electoral systems, each characterized by the coexistence of proportional and majoritarian components. From 2006 to 2013, members of both Houses were elected using a closed-list proportional system with majority premium (i.e., a guaranteed minimum number of seats allocated to the coalition of parties that received the largest number of votes).[5] The majority premium was awarded on a national basis for the Chamber of Deputies and region-by-region for the Senate.[6]

For this study, the most relevant difference between the two Houses of Parliament is the voting

[4]In addition to its 315 elected members, the Senate also has members with lifetime tenure, the so-called "senators for life." These include five senators appointed by the President of the Italian Republic "for outstanding merits in the social, scientific, artistic or literary field," plus former Presidents, who become senators for life *ex officio*. By contrast, the Chamber of Deputies has no "deputies for life." Henceforth, we talk interchangeably of the "Chamber of Deputies", the "House of Representatives", and the "lower House".

[5]In each multi-seat legislative district, parties ran with rosters of candidates. Because of the closed-list system, voters could not express preferences over party candidates running in their district. Moreover, candidates could run in multiple districts and they could choose, *after the election*, which district to be elected from. As such, it was extremely difficult for a voter to predict the candidates her vote would contribute to elect.

[6]In the Chamber, the coalition of parties receiving the largest number of votes nationwide was awarded a share of the available seats equal to the maximum between 54 percent and the sum of the vote shares of the parties in the coalition. In the Senate, each of the 20 regions in the country awarded a different majority premium to the coalition of parties receiving the largest vote share in that region. Relative to the Chamber of Representatives, the region-by-region premium made it more challenging for coalitions of parties to win a majority of seats in the Senate. Consequently, two of the three elections held with this system resulted in a hung Senate (2006 and 2013); that is, no coalition of parties had a clear majority of Senate seats. This suggests the lack of turnout effects of Senate voting eligibility (Section 4.1) is not driven by the Senate elections being inconsequential.

age, which the Constitution sets at 18 for the Chamber of Deputies and at 25 for the Senate. During parliamentary elections, every voter receives a ballot for the Chamber of Deputies, but only voters 25 or older also receive a ballot for the Senate.[7] In Section 4.1, we use an RDD on voter-level data to examine whether the discontinuous age-induced eligibility to vote for the Senate affects voter participation. Importantly, by comparing turnout and voter behavior across the age discontinuity within Parliamentary elections, the RDD holds the overall salience of the election constant, as Italian parties do no target campaigns to individuals of different ages. Specifically, the additional effort to acquire information to vote for the Senate might be small conditional on the decision to vote for the Chamber of Deputies, especially given the closed-list electoral system in force between 2006 and 2013, under which voters could not express preferences over party candidates and the same parties appeared on both the Chamber and Senate ballots. However, the marginal benefit of Senate voting eligibility might be small too if, for example, voters perceive the two elections to be about the same issue (e.g., the appointment of the executive branch of government that hinges on the cross-party distribution of seats in the two Houses of Parliament).

The second source of variation used in the paper comes from the three levels of administrative divisions in Italy: regions, provinces, and municipalities. The entire national territory is divided into 15 ordinary and 5 special regions.[8] The 20 regions are partitioned into 93 provinces, which, in turn, are divided into municipalities. Since the Italian Parliament amended the country's Constitution in 2001, the 15 ordinary regions have "residual" legislative powers; that is, they have exclusive legislative jurisdiction with regard to any matters not explicitly reserved by the Constitution to the national government. Italian regions also have important regulatory, administrative, and fiscal powers. By contrast, the powers of provinces are limited to minor aspects of zoning, the maintenance of primary and secondary school facilities, and the maintenance of provincial roads. Similarly to American cities and towns, Italian municipalities have broad powers over zoning regulations, pub-

[7] Italian elections rely on a traditional paper-ballot system. No registration is required to vote in Italy: individuals are automatically registered to vote at pre-designated polling locations based on their residence. At the end of the voting process, paper ballots are manually counted by election officials (Aldashev and Mastrobuoni, 2016). Except for Italians living abroad, there is no absentee or early voting in Italy.

[8] Special regions have larger autonomy from the central government and additional legislative jurisdiction than their ordinary counterparts.

lic safety, waste management, local taxes, and roads.[9] As shown in Figure 1, differences in powers across levels of administrative divisions are reflected in differences in turnout rates.

Since 1993, voters have been electing the executive and legislative branches of regional, provincial, and municipal governments, though, in 2011, provincial elections were abolished.[10] Like parliamentary and European elections, all local (i.e., sub-national) elections regularly follow a 5-year calendar.[11] In practice, though, several factors may shorten the term of local and national legislatures, thereby resulting in early elections. Early municipal and regional elections are called automatically in case of death, removal, resignation, or incapacitation of the mayor and governor, respectively, and the same principle applied to province presidents.[12] At the national level, the President of the Republic can call a snap election following the resignation of the prime minister or after ascertaining that the executive no longer has the support of the legislative (e.g., because one of the Houses of Parliament passed a motion of no confidence). These fluctuations in election calendars create within-city and within-Election-Day variation in the number and types of concurrent elections. While these early elections might be correlated with voters' sentiment affecting turnout, subsequent concurrent elections due to these shifts are likely exogenous. In Section 4.3, we exploit this variation using a DD design to estimate the effect of concurrent elections on turnout and valid ballots cast, and we show that our results are robust to dropping the first instances of these calendar shifts.

Voter turnout in Italy is higher than in the U.S. and most European countries, but it has been experiencing a steady decline since the late 1970s (Figure 1). Participation is usually lower in provincial than in regional, European, and municipal elections, and it is highest during national

[9] However, while cities and towns in the U.S. have considerable control over the administration and organization of primary and secondary schools, in Italy most of these functions are delegated to the national government.

[10] The executive and legislative branches of the provincial government are now "indirectly" elected by the mayors and city councilors of the municipalities that constitute the provincial territory.

[11] Provinces and municipalities originally followed 4-year electoral calendars, which were extended to 5 years starting in 2000.

[12] After the death, removal, resignation, or incapacitation of a mayor, municipal elections are held at the earliest Election Day set by the Ministry of Interior. That is, the Ministry of Interior sets a single Election Day a year for all municipal elections to be held in the 15 ordinary regions (typically, a single day in March to June). If a mayor dies or steps down after that date, municipal elections take place in the following calendar year. In the meantime, her/his duties and responsibilities are passed to the deputy mayor or to an individual appointed by the central government, depending on the exact cause of the elected mayor's removal.

parliamentary elections. We thus refer to elections as ranked in the following decreasing order of salience: parliamentary, municipal, regional, European, and provincial.

3 Data

This project relies on administrative data as well as survey data we collected specifically for this study: for the Senate-voting-age RDD, we use administrative data on voter-level turnout and survey data on information acquisition and voting behavior; for the DD on concurrent elections, we use administrative city-level data on turnout and valid votes cast.

We pool two administrative sources of voter-level turnout data.[13] First, we use the *Osservatorio Prospex sull'Astensionismo Elettorale* (henceforth the *Prospex data*), which are administrative turnout data at the individual level collected and digitized by the Italian research foundation *Istituto Cattaneo*. The Prospex data are an unbalanced panel of approximately 140,000 voters from 100 Italian precincts, who were followed over regional and statewide elections held in 1994 through 2006. The sampling procedure used to construct the Prospex data ensures the sample is representative of the 1981 national population in terms of areas of residence and city size.[14] Beside turnout, the data contain basic voters' socio-demographic characteristics, including: date of birth, gender, municipality of residence, educational attainment, and occupation.[15] Second, we complement the Prospex data with administrative turnout information from the city of Bologna, in northern Italy (henceforth the *Bologna data*). The Bologna data contain similar information as the Prospex data, but cover different elections; that is, the 2004 and 2009 European elections, and the 2008 and 2013 parliamentary elections. For each of these elections, the sample consists of all Bologna voters who were between 22 and 28 years old.[16]

[13] In Italy, the only official sources of turnout data are attendance sheets that are signed by election officers in charge of identifying voters at polling stations. After the election, these sheets are transferred to a warehouse annexed to the local courthouse.

[14] See http://www.cattaneo.org/activity/rete-prospex/ (in Italian) for more information on the Prospex sample. Accessed: August 22, 2019.

[15] Until 2001, educational attainment and occupation were reported on official attendance sheets used for election administration, albeit with a large share of missing values.

[16] To construct the Bologna data, we digitized all Bologna's attendance sheets from the 2004, 2008, 2009, and 2013 elections. We then sent these data to the municipal statistical office, which matched them against administrative

Table 1 reports summary statistics for the long version of the Prospex panel dataset (column 1), for the Bologna data (column 2), and the pooled Prospex and Bologna data (column 3). All samples are restricted to parliamentary elections and only include voters aged 22–28 for ease of comparison. Average turnout in the Prospex data is 88 percent, which is in line with the high level of voter participation in Italy. Because the Bologna sample covers more recent, lower turnout elections (2008 and 2013), average turnout is slightly lower in column 2 (78 percent). Educational attainment and occupation are missing for more than 60 percent of voters in the Prospex data, and they are never observed in the Bologna data. Consistently with the young age of these voters, only 4 percent of individuals in the Bologna data are married. 8 percent of voters in the Prospex data live in the Emilia-Romagna region, whose capital is Bologna.[17]

To collect data on voter behavior and information, we administered an anonymous survey to 1,193 18-to-30-year-old voters outside 10 randomly selected polling stations in Bologna during the February 24–25, 2013, parliamentary elections. Among others, we asked voters if they recalled the names of House or Senate candidate(s) running in their district, which party they voted for, how much time they spent acquiring political information before the election, and their level of agreement with a series of statements on their turnout decision. The full text of the survey and summary statistics of surveyed voters are reported in Appendix A.1. Summary statistics of interviewed voters' characteristics are reported in table A2.

The Italian Ministry of Interior provided city-level counts of all ballots, invalid ballots (i.e., the sum of over- and under-votes), and valid ballots (i.e., total-minus-invalid ballots) cast in almost every municipal, provincial, regional, European, House of Representatives, and Senate election

demographic data at the individual level. After matching, the municipality of Bologna sent us a file including the turnout information and socio-demographic variables typically unreported in voter attendance sheets (e.g., marital status, immigration status, position within the household). To balance confidentiality with our need to implement the RDD around the 25-year-old discontinuity, the municipality of Bologna restricted the final sample to voters aged 22 to 28 in the four sampled elections.

[17] Appendix Table A1 shows that observable covariates in the pooled Prospex-Bologna data are balanced across voters on the two sides of the Senate voting eligibility threshold. Specifically, the table reports coefficients from RD regressions using voter characteristics (i.e., education, occupation, marital status, region of residence, and gender) as placebo outcomes. Out of 21 coefficients, only two are significant at the 5-percent level, and one is significant at the 10-percent level. Once accounting for multiple inference using Anderson (2008)'s procedure, all coefficients fall short of statistical significance.

held in Italy between 1993 and 2015, inclusive.[18] Summary statistics of city-level variables are reported in table A3.[19]

4 Results

4.1 Senate Voting Eligibility Does Not Increase Turnout

We first examine the turnout effect of Senate voting eligibility. The parameter of interest is the standard RD treatment effect at the Senate voting-age cutoff:

$$\tau \equiv \tau(age_i = 25) = \mathbb{E}\left[turnout_i(1) - turnout_i(0) | age_i = 25\right],$$

where $turnout_i(1)$ and $turnout_i(0)$ denote voter i's turnout when she can vote for both Houses of Parliament or only the Chamber of Representatives, respectively. As usual in RD designs, τ denotes a "local" treatment effect; that is, the average effect on individuals who are exactly 25 on Election Day.

Table 2 reports non-parametric estimates of τ, p-values, and confidence intervals based on the procedure developed by Calonico, Cattaneo and Titiunik (2014); Calonico, Cattaneo and Farrell (2017). Column 1 displays results from a non-parametric RDD regression that only controls for the running variable (i.e., age in days minus age in days of voters who are exactly 25 on Election Day). Column 2 further controls for full sets of dummies for educational attainment, gender, occupation, marital status, region of residence, and election year.[20]

[18] We distinguish between turnout and casting a valid ballot. With turnout, we mean the act of turning out on Election Day, independently of whether the voter casts a valid ballot, a blank ballot ("undervote"), or a spoiled vote ("overvote"). With valid ballots, we mean ballots that are correctly cast according to election rules and are thus included in the final vote tally.

[19] Four special regions with incomplete municipal turnout data (Aosta Valley, Friuli-Venezia Giulia, Sicily, and Trentino-Alto Adige) are excluded from all city-level samples and regressions. We similarly exclude a few observations with clear inconsistencies in the reported vote tallies (e.g., more ballots cast than registered voters). Because the Ministry of Interior city-level data on provincial elections are available only in 2004 through 2011, we collected pre-2004 and post-2011 calendars of provincial elections from *wikipedia.it* to construct the "concurrent-provincial-election treatment" for these years.

[20] Notice that the optimal RD bandwidth changes when we include covariates, thus resulting in columns 1 and 2 having different sample sizes. In both columns, we implement the bias-corrected RD estimator using Calonico, Cattaneo and Farrell (2017)'s *rdrobust* Stata routine. We use a local linear regression to construct the point estimator,

Eligibility to vote for the Senate has no effect on turnout. The estimated treatment effect is a small and insignificant $-.1$ percentage point (column 1). Robust 95-percent confidence intervals are tightly centered around zero, ranging from -2.1 to $+1.8$ percentage points. The inclusion of covariates in column 2 does not substantively affect the estimate ($-.7$ percentage point), though it reduces precision slightly.[21]

Figure 2 documents the zero turnout effect graphically. The red line denotes a linear fit of turnout on the running variable, estimated separately on each side of a ± 968-day neighborhood around the 25-year-old discontinuity, using a triangular kernel and the MSE-optimal bandwidth from Table 2, column 1. The point cloud represents average turnout by bins of the running variable, where the number of evenly-spaced bins is chosen following Calonico, Cattaneo and Titiunik (2015).[22] The plot shows no discontinuity in average turnout across the two sides of the 25-year-old discontinuity.

Despite the zero average effect, are there any voters who respond to Senate voting eligibility by turning out with higher (or lower) probability? Table 3 shows that the answer is negative. Specifically, the estimated turnout effect is centered around zero and insignificant even when we restrict attention to voters living in northern (column 2) or southern regions (column 3), men (column 4) or women (column 5), high-school and college graduates (column 6), and voters in more recent political elections (column 7). If information acquisition costs correlated negatively with education, we would expect a positive turnout effect on highly educated individuals (column 6). We interpret the lack of such effect as evidence against the hypothesis that information acquisition costs prevent Senate voting eligibility from increasing turnout.[23]

a local quadratic for the bias correction, a triangular kernel, one common MSE-optimal bandwidth selector, and HC1 heteroskedasticity-robust variance estimator.

[21] In Table A4 we implement a DD exercise parallel to that of Section 4.3 using the voter-level data from ITANES and Bologna. Specifically, we show that, in regional and parliamentary elections, the turnout effect of concurrent municipal elections is similar across voters aged 18–30 and voters of all ages. Though most of these estimates fall short of conventional levels of statistical significance, they suggest that the lack of turnout effects due to Senate voting eligibility is not driven by idiosyncrasies of voters close to the Senate-voting-age threshold.

[22] We construct the RD plot using Calonico, Cattaneo and Farrell (2017) Stata *rdplot* routine.

[23] In Table 1, we focused on voters between 22 and 28 years of age to ease comparison across the Bologna and Prospex data. However, RD regressions are not restricted to that age range; in practice, though, all but one optimal bandwidths reported in Table 3 are.

Further corroborating our null result, Appendix A.2 documents that the zero turnout effect is stable and robust across alternative RDD bandwidths (Figure A3) and replacing the actual 25-year-old cutoff with placebo age discontinuities (Figure A4). Similarly, we find a tight zero from placebo RDDs of voter turnout in non-parliamentary elections (Figure A5), in which the 25-year-old cutoff induces no discontinuity. For this placebo exercise, we use the 1995, 2000, and 2005 regional elections from the Prospex data, and the 2004 and 2009 European elections from the Bologna data.

The zero effect of Senate voting eligibility on turnout admits at least two possibly complementary explanations. First, given the high importance of parliamentary elections, voters may face a "salience ceiling." That is, voters may perceive the election to the Chamber of Deputies as so important, that there may be no room for the Senate election to further increase voters' political interest and turnout. This salience ceiling may be reinforced by voters conflating the House and Senate ballots into a single issue, namely the appointment of the executive, nullifying any additional benefit of casting two ballots instead of one.[24] Second, because Italian parties do no target campaigns to different voters, our RDD holds the overall electoral salience constant. Thus, our null finding suggests that concurrent elections increase turnout only when additional ballots bring additional salience (e.g., more intense campaigns). This explanation comports with the null effect of voting eligibility on knowledge about candidates that we report below.

4.2 Senate Voting Eligibility Does Not Increase Voter Information

Although we find no effect on turnout, it is possible that Senate voting eligibility affects the quantity and type of information voters gather before the election, their voting behavior, and their rationales for turning out on Election Day. To explore this possibility, we use the survey data we collected in Bologna during the 2013 parliamentary election.

Table 4 shows that Senate voting eligibility has no effect on survey outcomes. Rows corre-

[24] Section 4.2 provides additional evidence that Senate voting eligibility does not increase voting costs, which could counteract positive effects on turnout, and that voters perceive the Senate election as being (at least) as consequential as the House election.

spond to survey variables. For each outcome, columns 1 through 3 report summary statistics, while columns 4, 5, 7, and 8 report, respectively, the estimated RD effect, robust p-value, bandwidth, and sample size computed using Calonico, Cattaneo and Titiunik (2014) estimator. Column 6 reports False Discovery Rate (FDR) q-values robust to multiple inference based on Anderson (2008)'s procedure. Voters barely below and above the 25-year-old cutoff display, among others, statistically indistinguishable probabilities to support the Democratic Party or the 5-Star Movement (question Q4), same levels of agreement with statements on their turnout decisions (questions Q7 through Q13), and the same perceived importance of being eligible to vote for both Houses of Parliament as opposed to only the lower House (question Q22). Particularly, all respondents strongly agree with statements like "Voting is important even if one ballot is inconsequential" (question Q9) and "I am interested in politics also outside of election time" (question Q12), which suggests that the number of ballots they are eligible to cast during parliamentary elections is irrelevant for their decision to turn out.

Relative to their younger peers, voters 25 or older report spending marginally fewer hours acquiring political information (question Q18) and are less likely to remember the name of a candidate running for the House of Representatives in their district (question Q2). At the same time, however, we find no effect on the probability that voters correctly recall the name of a Senate candidate (question Q3), or on the probability that knowing House or Senate candidate(s) affected voting behavior (questions Q2 and Q3).[25] Overall, voters who can vote for both Houses of Parliament acquire the same amount and type of information and display the same voting behavior as their younger peers who cannot cast ballots for the Senate. This suggests that information costs are not responsible for the lack of impact of Senate voting eligibility on turnout.[26]

As survey participation was voluntary, it is possible that the probability of answering the survey changes discontinuously across the 25-year-old threshold.[27] To dismiss this concern, we use

[25]Moreover, after adjusting the p-values in Table 4 for multiple hypotheses testing, the negative effect of Senate voting eligibility on knowing the name of a candidate running for the House of Representatives becomes insignificant.

[26]RD graphs for selected survey outcomes are reported in Figures A6 and A7.

[27]For example, this could be because people whose 25th birthday falls exactly on Election Day may be unwilling to spend time answering a survey.

the test described by Cattaneo, Jansson and Ma (2018, 2016) to check if the density of the running variable is continuous at the treatment cutoff. Because there should be the same number of voters barely above and barely below the 25-year-old cutoff, in expectation, detecting a discontinuity would suggest that the probability of answering the survey changes abruptly at the threshold. The Cattaneo, Jansson and Ma (2016) test produces a p-value of .948, which reassures that the probability of answering the survey is indeed continuous across the treatment cutoff.

Another concern is that the *type* of survey respondents may change discontinuously at the cutoff. Assuaging this concern, Table A2 shows that observable voter characteristics are balanced across the two sides of the discontinuity. Thus, the zero impact of Senate voting eligibility on voter information and behavior is not driven by discontinuous changes in the population of survey respondents.

4.3 Concurrent Elections Increase City-Level Turnout, Invalid Ballots, and Valid Ballots

We now examine the effect of concurrent elections on city-level outcomes in municipal, provincial, regional, European, House of Representatives, and Senate elections. To estimate these impacts, we run the following DD regression separately for each of the six election types mentioned above:

$$y_{i,t} = \beta^m T^m_{i,t} + \beta^p T^p_{p(i),t} + \beta^r T^r_{r(i),t} + \delta_i + \gamma_t + \varepsilon_{i,t}, \tag{1}$$

where $y_{i,t}$ denotes one out of three possible outcomes: voter turnout (i.e., votes cast as a share of a generic city i's voters), the share of invalid ballots (i.e., the sum of over- and under-votes divided by i's voter counts), or the share of valid ballots (i.e., turnout minus invalid ballots).[28] $T^m_{i,t}$, $T^p_{p(i),t}$, and $T^r_{r(i),t}$ are dummies for whether city i held concurrent municipal, provincial, or regional elections on Election Day t, respectively; δ_i and γ_t denote full sets of city and Election Day fixed effects. Importantly, we estimate two types of turnout effects: the effect of high-salience elections concurring with lower-salience ones *and* the effect of less-salient elections concurring

[28] The three outcomes share the same denominator to ease comparison of estimates from different models.

with higher-salience contests.[29]

Identification in equation 1 requires within-city and within-Election Day variation. That is, some cities must switch treatment status over time (i.e., they must be observed sometimes with, and sometimes without concurrent elections), and some election days must feature both treated and control cities.[30] These switches in treatment status happen as a result of early elections. While these early elections might be correlated with voters' sentiment affecting turnout, subsequent concurrent elections due to these shifts are likely exogenous. Below, we show that our results are robust to including or dropping these early elections from our sample. In these and all other city-level regressions, observations are weighted by counts of eligible voters. To account for potential serial correlation of regression residuals (Bertrand, Duflo and Mullainathan, 2004), standard errors are clustered by province. However, because provinces are nested within regions, this clustering scheme may yield standard errors that, in the case of β^r, are biased downwards. Thus, only for β^r, we report p-values from a post-estimation, wild-bootstrap procedure (Cameron, Gelbach and Miller, 2008; Webb, 2014) instead of province-clustered standard errors. These p-values are robust to serial correlation within regions and to the possibility that we have too few clusters (i.e., 16 regions).[31]

Table 5 reports the main city-level impact estimates. Panels A through C report estimated effects on turnout, invalid ballots, and valid ballots, respectively. Each column corresponds to a different election type/sample. Control outcome means are computed using cities-election dates without concurrent provincial, regional, or municipal elections.

Concurrent municipal elections have a sizable, positive effect on voter turnout (Panel A). This effect is larger in lower-salience, lower-turnout elections, but remains positive and significant even during parliamentary elections, which, on average, attract a larger share of the voting-age pop-

[29] In this section, we measure the salience of an election as average voter turnout absent concurrent elections. For example, provincial elections are less salient than regional elections because, without concurrent elections, turnout in the former is 59.2% vs. 66.9% in the latter (see Panel A in Table 5, columns 1 and 3).

[30] Because of the Election Day fixed effects, we cannot separately estimate the impact of concurrent elections that take place on the same day in every municipality (i.e., European and parliamentary elections).

[31] Moreover, Appendix Tables A11 A12 show that patterns of significant effects from city-level DD regressions are substantively unaffected by accounting for multiple hypotheses testing.

ulation than municipal elections.[32] By contrast, provincial elections significantly increase voter turnout only when they concur with European (+3.9 percentage points) or municipal elections (+.6 percentage points). Similarly, concurrent regional elections have a positive turnout effect during European (+11.8 percentage points) and municipal elections (+1.6 percentage points), though the paucity of clusters rules out clear-cut conclusions.

Positive effects on turnout are paralleled by positive effects on the number of invalid ballots cast (Panel B), a phenomenon that admits at least two explanations. On the one hand, the concurrence of multiple elections may confuse some voters, leading them to cast an invalid ballot by mistake. On the other hand, some voters may be interested in and intentionally cast a valid ballot only for the concurrent election.[33]

Panel C reports the net effect of higher turnout and more invalid ballots, namely effects on valid ballots. In most cases, the increase in invalid ballots does not completely offset the impact on turnout, so positive turnout effects translate to more valid ballots cast. However, this is not true for some lower-salience elections concurring with higher-salience ones (e.g., provincial elections concurring with municipal ones in column 4, and municipal elections concurring with House or Senate elections in columns 5 and 6), which feature zero effects on valid ballots cast despite positive and significant effects on turnout. In these cases, identical increases in turnout and invalid ballots may suggest that voters find the additional ballot worthless. Alternatively, voters interested primarily in the low-salience election may face steep information acquisition costs for the higher-salience election.

The asymmetry between the large impact of concurrent municipal elections on valid ballots cast in provincial elections and the null effect of concurrent provincial elections on municipal outcomes suggests that the dissimilarity between elections is not the most important factor affecting voting behavior. Voters in the low-stake provincial elections would likely vote in municipal elections even if these were held on a separate day. By contrast, voters in municipal elections are willing to cast

[32] Absent concurrent elections of other types, voter turnout in municipal elections is 70.8 percent (column 4), against 82.5 percent in parliamentary elections (column 5).

[33] A third possibility is that election officers put more effort into detecting invalid ballots when a low-salience election coincides with a high-salience one (Aldashev and Mastrobuoni, 2016).

the additional provincial ballot when the elections coincide, but would not go to the polls for the provincial election alone. One potential explanation for this asymmetry is ideological voting; that is, voting along party lines in different elections (thus reducing election-specific information acquisition costs). Unfortunately, it is difficult to explore patterns of disjoint voting in Italy, because most candidates in local elections run with local lists that are hard to classify according to the national party system. Furthermore, the decision of subnational candidates to align with national parties is endogenous to the political context (e.g., whether the ruling national party is popular or not; Daniele, Galletta and Geys, 2020). Consequently, it is difficult to predict ex ante how the turnout effects of election concurrency may affect which party wins second-order elections.

4.4 City-Level Placebo Tests

In the online appendix, we perform two tests of the "parallel-trends" assumption underlying regression 1. In Table A5, we implement a test of Granger causality (Granger, 1969). The idea is to see whether causes happen before consequences, and not vice versa. Specifically, we test whether, conditional on current treatment status, *future* concurrent elections have no impact on electoral outcomes *today*. It would be heartening to find that treatment status has no (partial) explanatory power on current outcomes, while finding otherwise may signal that treated and control municipalities are on different time trends.

Reassuringly, future treatment status has little to no partial predictive power on present electoral outcomes. Virtually all impact estimates in Table A5 are smaller in both magnitude and statistical significance than corresponding estimates from Table 5. This is particularly evident for concurrent municipal elections, which have sizable and significant effects on almost every outcome and in every sample of Table 5, but whose future realizations exhibit puny and mostly insignificant correlations with present electoral outcomes. A handful of impact estimates reach conventional levels of statistical significance. However, in all but one such cases, a test of joint insignificance fails to reject the null hypothesis of zero partial correlation between future concurrent elections and present outcomes.

In Table A6, we test whether our results are driven by the specific instances causing changes in municipal election calendars (i.e., by early municipal elections caused by the mayor's death or resignation). To do so, we exclude election dates coinciding with early municipal elections from all samples/columns. For every city, we identify early municipal elections as municipal elections that take place within four years or fewer since earlier elections of the same type.[34] All estimates are substantively identical to the ones in Table 5. That is, our results are not driven by voter turnout responses to the events that cause early municipal elections.

4.5 The Effects of Concurrent Elections Are Concentrated in Southern Municipalities

Due to its long history of political fragmentation, Italy is characterized by substantial geographic heterogeneity in economic development and social capital (Guiso, Sapienza and Zingales, 2004). Several studies find that the high degree of geographic variation in social capital translates to important differences in terms of voter behavior. For example, Putnam, Leonardi and Nanetti (1994) find that high levels of membership in local associations strengthen the ties between political parties and voters in the North of Italy, which contrasts with mostly clientelistic relationships in the South. Nannicini et al. (2013) show that higher levels of social capital correspond to better political accountability. Using a sample of municipal elections in Italy, De Benedetto and De Paola (2016) argue that the strength of the clientelistic relationship between incumbent mayoral candidates and voters explains the positive (negative) effect of incumbency on turnout that the authors observe in regions with low (high) levels of social capital. Table 6 shows that southern regions are characterized by lower turnout across all election types. Thus, understanding what policies could reduce disparities in political participation across regions may contribute to the study of democracies with (quasi-)federalist institutions like Italy.

We now examine whether concurrent elections have stronger effects in southern regions, which are characterized by lower levels of economic development and social capital than northern regions. For each election type in our sample, we estimate DD regression 5 separately for municipalities in

[34] Because municipal elections were held on four-year calendars until 1999, we use three years or fewer for municipal elections held between 1993 and 1999.

northern and southern regions. Table 6 reports impact estimates on turnout in northern (panel A) and southern regions (panel B).[35]

Turnout effects of concurrent elections are larger in southern than northern municipalities. This holds true across election types (columns) and for different types of concurrent elections (rows). Particularly when local elections concur with nationwide parliamentary or European ones, turnout effects in southern municipalities are an order of magnitude larger than in the Center-North. For instance, concurrent regional elections increase turnout in European elections by 24 and 3.5 percentage points in southern and northern municipalities, respectively. Similarly, coinciding provincial elections raise parliamentary-election turnout by 5.6 percentage points in southern municipalities and *reduce* it by $-.8$ percentage point in the Center-North. Interestingly, lower-salience elections in the South increase turnout even when they concur with higher-salience contests. For example, provincial, regional, and municipal elections increase turnout in House elections by 5.6, 11.5, and 6.6 percentage points, respectively.

Stronger turnout effects in the South (and, more broadly, in areas with lower-than-average turnout[36]) are consistent with the "ceiling effect" of salience being less binding in low-turnout areas. That is, adding concurrent elections in the North may be relatively ineffective at enhancing turnout, because most northern voters already vote in most elections, particularly in high-salience parliamentary ones. By contrast, the margins for increasing political interest (hence, voter turnout) in the South may be wider, leaving more space for concurrent elections to increase political participation. In this case, our heterogeneous analysis suggests that the turnout benefits from aggregating multiple elections on a single Election Day may be larger in areas characterized by relatively low levels of voter mobilization.[37]

[35] Because effects on invalid and valid ballots largely follow the patterns of turnout impact estimates, we report them in Appendix Tables A7 and A8.

[36] Of course, southern and norther regions differ across many dimensions, including average educational attainment and income. Indeed, heterogeneity of turnout impact estimates along these two dimensions (in Appendix Tables A9 and A10, respectively) is largely consistent with the one between northern and southern regions.

[37] An alternative explanation might point to the greater role of public sector employment in the South of Italy, though we could expect that this would increase voter mobilization in local elections and thus attentuate the effects of concurrent elections.

5 Theoretical Framework

Our empirical findings suggest that the literature should pay more attention to how individual voters respond to the effects of concurrency. To formalize this idea, here we outline a simple theoretical framework building on Aldrich, 1993's discussion of the rational choice model of voting. Our model posits that voters who turn out on Election Day face two types of voting costs and benefits. First, there are voting costs and benefits that are "fixed," in that they do not depend on the number of ballots voters can cast at the voting booth. The opportunity cost of reaching one's polling station is an example of a fixed voting cost, while adherence to social norms may represent an example of a fixed voting benefit. Second, there are ballot-specific voting costs (e.g., the cost of acquiring information about candidates running in a specific race) and benefits (e.g., the utility gain of marginally increasing the preferred candidate's victory probability in a specific race).

Formally, we model a generic voter i's (i) decision to turn out on Election Day and, conditional on turning out, (ii) to cast a valid (instead of a blank) ballot for the K separate elections held on that day. If voter i casts a set $M \neq \emptyset$, $|M| = M \leq K$ of valid ballots, she incurs a fixed cost F_i to go to the voting booth, as well as ballot-specific costs C_{ik}, $k \in M$ (e.g., endogenous information acquisition costs). By contrast, the ballot-specific cost of casting a blank ballot is zero (i.e., $C_{ik} = 0, \forall k \in M^C$). Under these assumptions, concurrent elections never decrease turnout, as the voter has the option to ignore the concurrent election and cast a blank ballot.

Similarly, voter i derives positive utility B_i from turning out, independently of the ballots she casts, and a ballot-specific expected benefit B_{ik} from casting a valid ballot in election k. B_i can be interpreted as the utility from conforming to social norms (Feddersen and Sandroni, 2006) or social pressure (Funk, 2010). Following Riker and Ordeshook (1968), the expected benefit is equal to the utility gain from casting the decisive vote in favor of one's preferred candidate in election k, $p_{ik}b_{ik}$, plus a utility term that is independent of the election outcome, D_{ik}. In this paper, we focus on the nature of ballot-specific costs and benefits and on their interactions. For simplicity, we assume pivotal probabilities (and, thus, $p_{ik}b_{ik}$) to be negligible (e.g., Coate, Conlin and Moro,

2008; Enos and Fowler, 2014; Feddersen, 2004). Blank ballots induce no ballot-specific benefits; that is $b_{ik} = 0, \forall k \in M^C$.

Voter i casts a set $M \neq \emptyset$ of ballots on Election Day if her indirect utility evaluated at M is weakly positive. That is:

$$V_i(M) = \delta_i D_i - \gamma_i C_i + B_i - F_i \geq 0, \tag{2}$$

where D_i and C_i are $M \times 1$ vectors denoting the benefits and costs of the M ballots cast by i, respectively, while δ and γ are $M \times M$ matrices of weakly positive weights (with main-diagonal terms equal to 1) that allow these costs and benefits to vary depending on the combination of concurring elections.

This flexible description of a voter's utility illustrates that the turnout effect of holding multiple elections concurrently depends on the nature of these elections. Specifically, we discuss four cases. Importantly, these cases are not mutually exclusive and the net effect of concurrent elections will depend on the relative magnitude of the individual benefits, costs, and weights.

- $\gamma_{jk} \leq 1$: subadditive voting costs. The concurrence of election j weakly decreases the marginal cost of voting in election k. This case could arise from economies of scope in acquiring information for elections j and k. For example, concurrent elections that feature the same parties or similar issues might increase turnout and the number of valid ballots more than elections with different focuses.
- $\gamma_{jk} > 1$: superadditive voting costs. The concurrence of election j increases the marginal cost of voting in election k. For example, voter fatigue may cause marginal voting costs to increase in the number of ballots voters cast on Election Day. With superadditive voting costs, concurrent elections might increase turnout less among individuals with lower cognitive ability.
- $\delta_{jk} < 1$: subadditive voting benefits. The concurrence of election j decreases the marginal benefit of voting in election k. For example, Gerber, Green and Larimer (2008); Funk (2010); Dellavigna et al. (2017) show that social pressure is a crucial driver of voter turnout. If turnout is just a response to social pressure, the presence of concurrent elections may gen-

erate limited additional benefits to voters, and turnout may not increase. However, social pressure might be stronger when multiple elections are held together. For example, an individual might be asked about her voting behavior in *each* election. In that case, theories of turnout that rely on social pressure might be consistent superaddivite voting benefits (i.e., $\delta_{jk} \geq 1$).

– $\delta_{jk} \geq 1$: superadditive voting benefits. The concurrence of election j weakly increases the marginal benefit of voting in election k. For example, voters could derive additional benefits from electing officials at different levels (e.g., municipal, regional, and national) if these officials jointly shape public policy. Alternatively, voters could derive extra utility from split-ticket voting (i.e., voting for a candidate from one party in one election and a candidate from another party in the other election; e.g., Chari, Jones and Marimon, 1997).

The magnitude of the ballot-specific benefits and costs determines the impact of adding a particular ballot to Election Day. The more salient an election—that is, the higher the net benefit—the more it can increase turnout in a low-salience election. Whether those additional ballots will be valid or invalid depends on the similarity of concurring elections and on the other factors described above.[38]

In summary, our stylized model posits that the interaction between ballot-specific voting costs and benefits drives the turnout effect of holding multiple elections concurrently. The higher the salience of an election, that is its specific benefit, the more it increases turnout when it concurs with a low-salience election. Yet, whether higher turnout translates to more valid votes (instead of blank ballots) depends on the similarity of the information acquisition process across concurrent elections and on the relative importance of ballot-specific benefits versus benefits that are independent of the

[38]Equation 2 models an individual voter's decision to cast a ballot. However, in the DD analysis in Section 4.3, we use city-level data. The concurrence of elections j and k raises city-level turnout in election k if, and only if, it increases the number of voters receiving positive indirect utility. With concurring election j, individuals with subadditive costs (resp. superadditive benefits) vote more numerously in election k, while individuals with superadditive costs (resp. subadditive benefits) prefer to abstain (i.e., cast a blank ballot) in election k. To derive meaningful predictions, we need these countervailing effects not to cancel out in the aggregate. To interpret our findings, we make the somewhat restrictive assumption that all voters share the same weighting matrices δ and γ. Importantly, this assumption still allows heterogeneity across voters in ballot-specific costs and benefits D_i and C_i, such as information acquisition costs, while imposing the same behavioral model on the whole population.

number of Election Day ballots (e.g., adherence to social norms).

This framework helps to rationalize some of our findings. In the voter-level RDD, the zero effects of Senate voting eligibility on voter turnout and information suggest that voters perceive the House and Senate elections to be about a single issue (i.e., the identity of the cabinet that will be eventually appointed by the President of the Republic). This conflation likely nullifies the marginal benefit of casting the Senate ballot, conditional on already voting for the House of Representatives. In the city-level DD, high-salience (e.g., municipal) elections increase turnout and valid ballots cast in lower-salience (e.g., provincial) elections, thus suggesting that marginal information acquisition costs are relatively small. Small marginal costs are consistent with Italians voting ideologically along party lines across ballots. In our model, this would translate to a high information acquisition cost for the first ballot, to learn about parties' platforms or ideologies, and a negligible marginal cost for additional ballots.

6 Conclusion

We study the effects of different types of concurrent elections on political participation using Italian administrative and survey data. We exploit plausibly exogenous variation in the number and type of choices Italian voters face on Election Day. Individual-level RDDs show that becoming eligible to vote for the Senate at age 25 has no impact on voter turnout or knowledge about candidates. We also estimate city-level DD models showing that concurrent high-salience elections increase turnout in lower-salience "second-order" elections, but not vice-versa. Moreover, concurrency effects are concentrated in municipalities in the South of Italy, a region with relatively weak political parties and low levels of social capital.

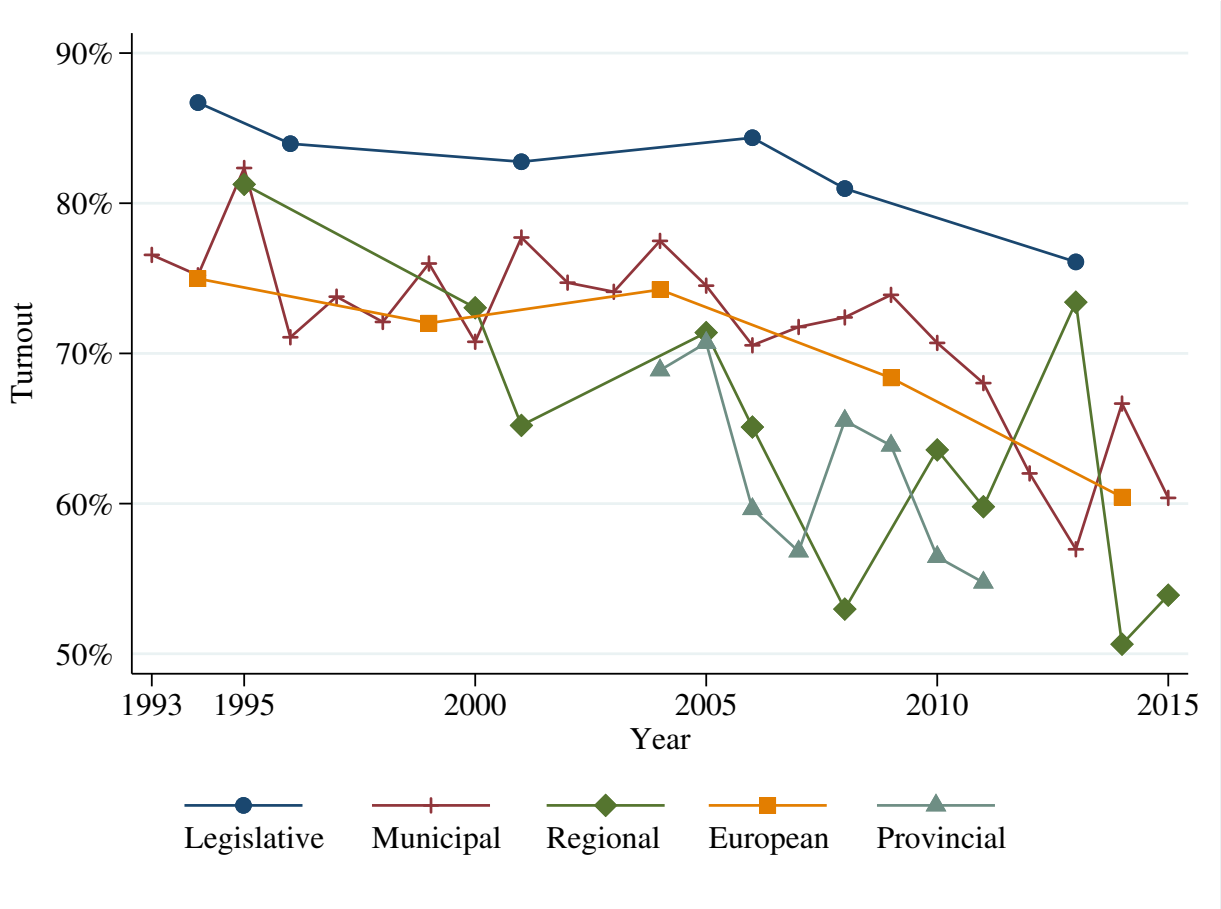
Our precise null RDD findings provide individual-level evidence that the mobilizing effect of concurrency are highly dependent on the political context. Because the salience of elections to the Lower House of the Italian Parliament is already high, eligibility to vote in another high salience election may not raise turnout if electoral campaigns do not micro-target their efforts to mobilize voters – as evidenced by the equal levels of information about candidates to the Upper

House among voters just below the age threshold. These findings comport with recent work in the American context showing that the effects of election timing on the composition of the electorate depend on voter mobilization efforts by political parties and interest groups (Anzia, 2011; Kogan, Lavertu and Peskowitz, 2018).

Our city-level DD findings indicate that the effects of concurrency on turnout tend to be stronger in contexts where the level of party mobilization is lower, like the South of Italy. This contributes to explaining the sources of heterogeneity in the effects of holding multiple elections concurrently (Geys, 2006). Interestingly, we also find that concurrency (slightly) increases the number of invalid ballots cast, thus suggesting a trade-off between the mobilizing effects of holding multiple elections concurrently and the risk of "choice fatigue" (Augenblick and Nicholson, 2016; Garmann, 2017).

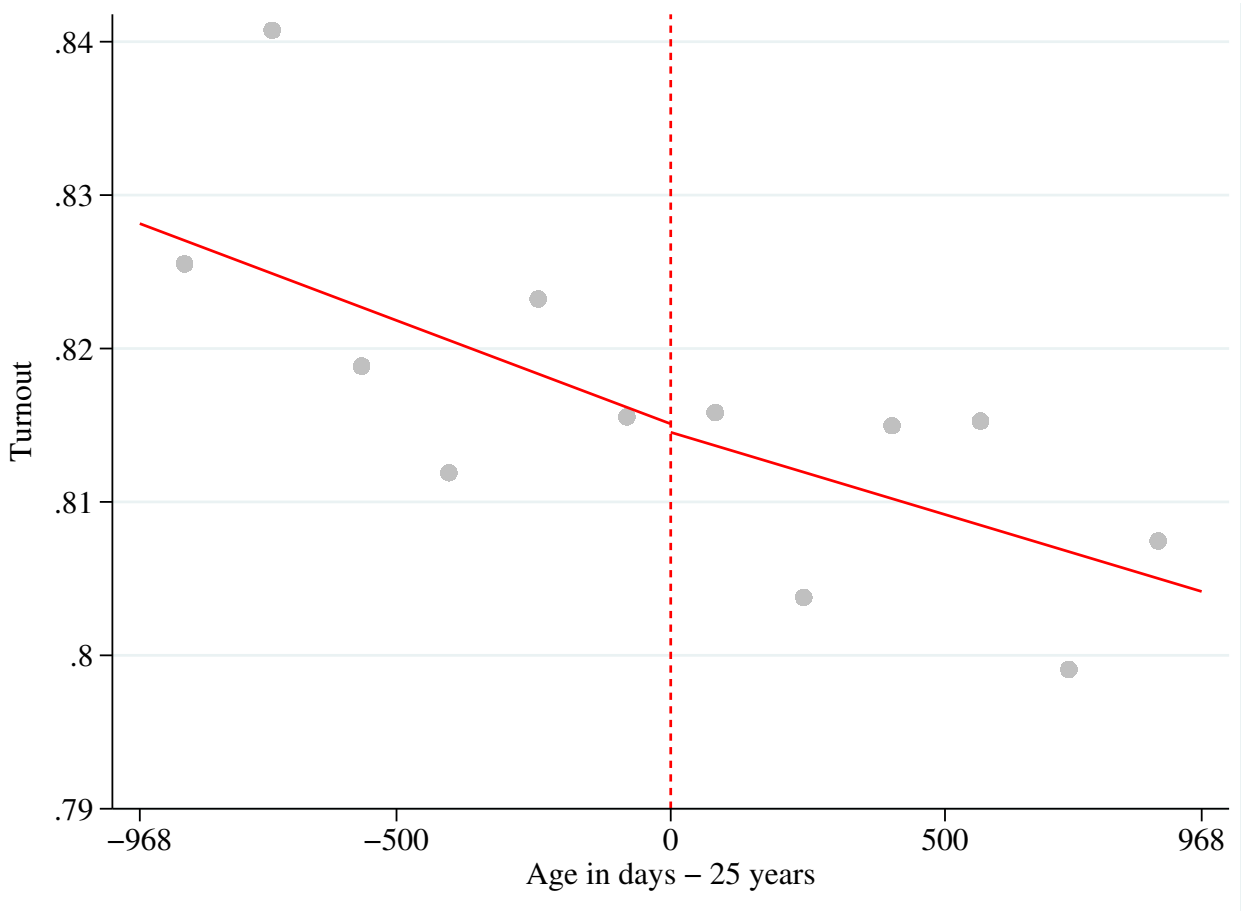
In all, our results suggest that, although concurrent elections are no panacea for stopping the turnout hemorrhage observed in many democracies, they can certainly be a partial remedy in some circumstances. For example, organizing low-salience regional elections on the same day as high-salience municipal elections generally seems like a good idea, given that low levels of participation in regional elections may undermine the legitimacy of (quasi-)federalist institutions. However, our findings also suggest that the turnout effects of holding second-order elections on-cycle are often constrained by a "salience ceiling", which depends on the political and social context, as well as by the prevalence of ideological voting, which might lower information acquisition costs. Finally, our study is mostly silent about the consequences of concurrency for who wins elections and resulting policy outcomes (Kogan, Lavertu and Peskowitz, 2018; Lo Prete and Revelli, 2020), though our RDD results indicate that expanding Senate voting eligibility to 18-24 year-olds would be unlikely to raise the relatively low levels of participation among this age-group.

Figure 1: Countrywide Turnout in Italy by Election Type and Year



Notes: The figure plots average turnout by election type and year using official tallies from the Ministry of the Interior.

Figure 2: Voter-Level Turnout Around 25-Year Discontinuity



Notes: The red line denotes a linear fit of voter-level turnout on the age-based running variable, estimated separately on each side of a ±968-day neighborhood around the 25-year-old Senate voting-eligibility discontinuity, using a triangular kernel and the MSE-optimal bandwidth from Table 2, column 1. The point cloud represents average turnout by bins of the running variable, where the number of equally-spaced bins is chosen following Calonico, Cattaneo and Titiunik (2015).

Table 1: Summary Statistics of Voter-Level Samples

Sample:	Prospex	Bologna	Pooled
	(1)	(2)	(3)
Age	25.1	25.2	25.2
	(1.7)	(1.7)	(1.7)
Voted	.875	.777	.815
Election year:			
1994	.187	.000	.073
1996	.252	.000	.099
2001	.376	.000	.147
2006	.185	.000	.072
2008	.000	.512	.312
2013	.000	.488	.298
Education (1996):			
Missing	.625	1.000	.853
Illiterate	.003	.000	.001
Elementary school	.022	.000	.009
Middle school	.260	.000	.101
High school	.085	.000	.033
College	.005	.000	.002
Occupation (1996):			
Missing or unemployed	.681	1.000	.875
Agricultural worker	.006	.000	.002
Blue-collar worker	.050	.000	.019
Artisan	.023	.000	.009
Retail worker	.010	.000	.004
White-collar worker (low level	.015	.000	.006
White-collar worker (mid level	.031	.000	.012
White-collar worker (high level	.004	.000	.002
Homemaker or Retiree	.026	.000	.010
Student	.154	.000	.060
Marital status:			
Missing	1.000	.002	.392
Single, divorced or widowed	.000	.959	.584
Married	.000	.040	.024
Lives in Emilia-Romagna	.075	1.000	.639
Female	.486	.485	.486
N	20,081	31,323	51,404

Notes: The table reports means and standard deviations (in parentheses, only for non-binary variables) of voter characteristics. Samples in columns 1 and 2 are restricted to the Prospex and Bologna data, respectively. Summary statistics in column 3 are from the pooled Prospex and Bologna sample. All samples are restricted to parliamentary elections (i.e., 1994, 1996, 2001, 2006, 2008, and 2013) and to voters aged 22 to 28.

Table 2: Turnout Effect of Senate Voting Eligibility

	Voter-Level Turnout	
	(1)	(2)
1(age ≥ 25)	-.001	-.006
Robust p-value	.975	.390
Robust $CI_{95\%}$	[-.020, .019]	[-.032, .012]
Outcome mean (age < 25)	.822	.818
Covariates	No	Yes
Bandwidth (days)	1,937	1,403
N	45,227	32,624

Notes: The table reports the estimated turnout effect of Senate voting eligibility (i.e., of a voter being 25 or older on the day of a parliamentary election) in the pooled Prospex-Bologna sample. All estimates are from local linear regressions based on Calonico et al. (2014) optimal bandwidth selector with HC1 heteroskedasticity-robust variance estimator. Column 2 controls for full sets of educational attainment, gender, occupation, marital status, region, and year dummies.

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

Table 3: Turnout Effect of Senate Voting Eligibility: Heterogeneity

	Voter-Level Turnout						
Sample:	Full Sample	North	South	Men	Women	HS Grad or	'06, '08, and '13
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
1(age ≥ 25)	-.001	-.007	.023	.013	-.012	.008	-.005
Robust p-value	.975	.386	.359	.298	.485	.909	.540
Robust $CI_{95\%}$	[-.020, .019]	[-.033, .013]	[-.027, .073]	[-.013, .044]	[-.032, .015]	[-.048, .053]	[-.034, .018]
Outcome mean (age < 25)	.822	.822	.822	.822	.824	.822	.823
Bandwidth (days)	1,937	1,675	1,663	1,858	2,326	1,653	2,031
N	45,227	33,579	5,443	22,262	26,525	3,110	32,426

Notes: Each column reports the estimated turnout effect of Senate voting eligibility (i.e., of a voter being 25 or older on the day of a parliamentary election) in a specific sample. Column 1 uses the entire pooled Prospex-Bologna sample and is identical to Column 1 of Table 2. Samples of columns 2 and 3 are restricted to voters living in northern and southern regions (Sardinia and Siciliy, inclusive), respectively. Samples of columns 4 and 5 are restricted to male and female voters, respectively. The sample of column 6 is restricted to voters with at least a high-school diploma. The sample of column 7 is restricted to the 2006, 2008, and 2013 parliamentary elections. All estimates are from local linear regressions based on Calonico et al. (2014) optimal bandwidth selector with HC1 heteroskedasticity-robust variance estimator.

** p < 0.01, * p < 0.05, ~ p < 0.10

Table 4: Survey Data: Voting Behavior and Information Acquisition

	Sample Mean (1)	Sample St. Dev. (2)	N (3)	RDD Estimate (4)	Robust p-value (5)	FDR q-value (6)	Bwidth (Days) (7)	RDD N (8)
Correctly names House candidate(s)? (Q2)	.210	.408	1,193	-.383	.007	.142	880	214
Correctly names Senate candidate(s)? (Q3)	.083	.276	1,193	.010	.860	1.000	1,292	318
Knowing House candidate(s) affected voting behavior? (Q2)	.102	.303	1,193	-.106	.219	1.000	1,394	340
Knowing Senate candidate(s) affected voting behavior? (Q3)	.029	.169	1,193	-.079	.229	1.000	1,288	317
Voted Democratic Party for House? (Q4)	.194	.396	1,193	-.075	.748	1.000	1,009	243
Voted 5-Star Movement for House? (Q4)	.143	.351	1,193	.101	.256	1.000	1,102	270
Electoral system affected voting behavior? (Q6)	.122	.327	1,193	.036	.861	1.000	1,741	427
Disagree (= 1) or Agree (= 5):								
Possibility of a tie affected turnout (Q7)	2.27	1.49	1,186	-.17	.758	1.000	1,224	296
Convenience of voting affected turnout (Q8)	1.91	1.34	1,184	.05	.814	1.000	1,386	335
Voting is important even if one ballot is inconsequential (Q9)	4.80	.65	1,186	.01	.780	1.000	1,289	316
Also voted to be seen by friends, acquaintances (Q10)	1.21	.69	1,184	.03	.862	1.000	1,453	351
Voted because like-minded people could not turn out (Q11)	1.81	1.22	1,184	.16	.491	1.000	1,818	445
Interested in politics outside election time (Q12)	3.51	1.25	1,186	-.54	.132	1.000	1,094	270
Had to give up something to vote (e.g., travel, leisure) (Q13)	1.46	1.06	1,184	.12	.843	1.000	1,320	322
Minutes spent to vote? (Q17)	15.7	57.1	1,180	-5.7	.353	1.000	865	208
Minutes willing to spend to vote? (Q18)	246	373	1,167	18	.635	1.000	1,246	303
Hours/week spent acquiring political info? (Q19)	5.02	5.18	1,164	-2.60	.073	1.000	1,184	284
Importance of voting for Senate & House vs. House only (1 = less important, 5 more than twice as important) (Q22)	3.42	1.23	1,155	-.35	.176	1.000	1,319	317

Notes: Each row reports summary statistics (columns 1-3) and RDD results (columns 4-8) of a different voter variable from Bologna's survey data. RDD estimates (column 4), p-values (column 5), and bandwidths (column 7) are from local linear regressions based on Calonico et al. (2014) optimal bandwidth selector with HC1 heteroskedasticity-robust variance estimator. Column 6 reports False Discovery Rate (FDR) q-values robust to multiple hypotheses testing based on Anderson (2008)'s procedure.

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

Table 5: Effect of Concurrent Elections on Turnout, Invalid Ballots, and Valid Ballots

Election type:	Provincial	European	Regional	Municipal	House	Senate
	(1)	(2)	(3)	(4)	(5)	(6)
			A. City-Level Turnout			
1(concurrent provincial election)	-	.039 **	.020	.006 ~	-.004	-.004
	-	(.010)	(.014)	(.004)	(.015)	(.014)
1(concurrent regional election)	-.011	.118	-	.016	.002	.005
	{.949}	{.522}	-	{.738}	{.985}	{.959}
1(concurrent municipal election)	.118 **	.099 **	.070 **	-	.031 **	.030 **
	(.007)	(.007)	(.009)	-	(.008)	(.009)
Joint test p-value	.000	.000	.000	.716	.001	.004
Control outcome mean	.591	.639	.669	.708	.825	.824
N	18,342	35,343	32,542	38,079	42,423	42,432
			B. (Under- and Over-Votes)/Voters			
1(concurrent provincial election)	-	.018 **	.001	.005 **	.001	.002
	-	(.003)	(.006)	(.001)	(.003)	(.002)
1(concurrent regional election)	.007	.033	-	.004	.014	.014
	{.691}	{.537}	-	{.515}	{.340}	{.396}
1(concurrent municipal election)	.027 **	.027 **	.022 **	-	.029 **	.025 **
	(.002)	(.003)	(.004)	-	(.004)	(.004)
Joint test p-value	.000	.000	.000	.037	.000	.000
Control outcome mean	.013	.032	.045	.028	.041	.042
N	18,342	35,343	32,542	38,079	42,423	42,432
			C. Valid Votes/Voters			
1(concurrent provincial election)	-	.021 **	.019	.001	-.005	-.005
	-	(.008)	(.013)	(.004)	(.013)	(.013)
1(concurrent regional election)	-.018	.086	-	.011	-.012	-.010
	{.908}	{.521}	-	{.781}	{.881}	{.910}
1(concurrent municipal election)	.090 **	.072 **	.048 **	-	.002	.005
	(.007)	(.005)	(.008)	-	(.005)	(.006)
Joint test p-value	.001	.000	.000	.822	.949	.784
Control outcome mean	.578	.607	.623	.680	.785	.782
N	18,342	35,343	32,542	38,079	42,423	42,432

Notes: The table reports estimated effects of concurrent provincial, regional, and municipal elections on turnout (Panel A), invalid votes (Panel B), and valid votes (Panel C). Each column represents a different election sample. All regressions are weighted by voter counts and control for election round (i.e., first round or runoff; only relevant for provincial and municipal elections), day of the election, and city fixed effects. Standard errors clustered at the province level are reported in parentheses. P-values robust to clustering at the region level, reported in braces, are computed with a 10,000-replication wild bootstrap using Webb (2014) weights. P-values of joint insignificance are robust to clustering at the province level. ** $p < 0.01$, * $p < 0.05$, ~ $p < 0.10$

Table 6: Effect of Concurrent Elections on Turnout: North vs. South

	City-Level Turnout					
Election type:	Provincial	European	Regional	Municipal	House	Senate
	(1)	(2)	(3)	(4)	(5)	(6)
			A. Northern and Central Regions			
1(concurrent provincial election)	-	.010 **	-.001	.004	-.008 ~	-.008 ~
	-	(.004)	(.008)	(.003)	(.005)	(.004)
1(concurrent regional election)	-	.035	-	.016	-.009	-.006
	-	{.862}	-	{.788}	{.916}	{.944}
1(concurrent municipal election)	.116 **	.083 **	.043 **	-	.009 **	.007 *
	(.008)	(.006)	(.004)	-	(.003)	(.003)
Control outcome mean	.595	.693	.689	.708	.861	.859
N	13,042	24,510	24,020	25,991	29,430	29,437
			B. Southern Regions			
1(concurrent provincial election)	-	.087 **	.035 **	.003	.056 **	.057 *
	-	(.012)	(.005)	(.005)	(.022)	(.023)
1(concurrent regional election)	-	.240	-	.009	.115	.116
	-	{.956}	-	{.868}	{.128}	{.154}
1(concurrent municipal election)	.113 **	.173 **	.104 **	-	.066 **	.067 **
	(.015)	(.012)	(.011)	-	(.004)	(.005)
Control outcome mean	.583	.521	.610	.707	.752	.748
N	5,300	10,833	8,522	12,088	12,993	12,995

Notes: The table reports estimated effects of concurrent provincial, regional, and municipal elections on turnout in Northern (Panel A) and Southern regions (Panel B). Each column represents a different election sample. All regressions are weighted by voter counts and control for election round (i.e., first round or runoff; only relevant for provincial and municipal elections), day of the election, and city fixed effects. Standard errors clustered at the province level are reported in parentheses. P-values robust to clustering at the region level, reported in braces, are computed with a 10,000-replication wild bootstrap using Webb (2014) weights.

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

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Turnout in Concurrent Elections: Evidence from Two Quasi-Experiments in Italy

Online Appendix

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A Online Appendix

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A.1 Voter-Level Survey

Figure A1: Voter-Level Survey (Original Version, in Italian)

0)	SESSO?	a) UOMO b) DONNA
1)	QUAL E' LA TUA DATA DI NASCITA (GG/MM/AA)?	|__|__|/|__|__|/|__|__|
2)	CONOSCI IL NOME DI UNO O PIU' CANDIDATI ALLA CAMERA DEI DEPUTATI NELLA TUA CIRCOSCRIZIONE ELETTORALE?	a) SI' b) NO ➔ passa alla domanda 3
2bis)	SE SI', CHI?	_____ _____ _____
2ter)	SE SI', IL FATTO CHE QUESTE PERSONE FOSSERO CANDIDATE ALLA CAMERA HA INFLUITO SULLA TUA DECISIONE AD **ANDARE A VOTARE** (A PRESCINDERE DA CHI TU ABBIA VOTATO) O CI SARESTI ANDATO IN OGNI CASO?	a) SI' b) NO
3)	CONOSCI IL NOME DI UNO O PIU' CANDIDATI AL SENATO DELLA REPUBBLICA NELLA TUA CIRCOSCRIZIONE ELETTORALE?	a) SI' b) NO ➔ passa alla domanda 4
3bis)	SE SI', CHI?	_____ _____ _____
3ter)	SE SI', IL FATTO CHE QUESTE PERSONE FOSSERO CANDIDATE AL SENATO HA INFLUITO SULLA TUA DECISIONE AD **ANDARE A VOTARE** (A PRESCINDERE DA CHI TU ABBIA VOTATO) O CI SARESTI ANDATO IN OGNI CASO?	a) SI' b) NO
4)	PER QUALE PARTITO HAI VOTATO ALLA CAMERA DEI DEPUTATI? OPPURE: HAI VOTATO SCHEDA BIANCA? SCHEDA NULLA? NON HAI RITIRATO LA SCHEDA? *(-999 se rifiuta di rispondere)*	_____
5)	PER QUALE PARTITO HAI VOTATO AL SENATO DELLA REPUBBLICA? OPPURE: HAI VOTATO SCHEDA BIANCA? SCHEDA NULLA? NON HAI RITIRATO LA SCHEDA? (*scrivere -888 se N/A perché minore di 25 anni, -999 se rifiuta di rispondere)*	_____
6)	IL MECCANISMO DI DISTRIBUZIONE DEI SEGGI PREVISTO DALLA LEGGE CALDEROLI HA INFLUENZATO IL TUO COMPORTAMENTO DI VOTO?	a) SI' b) NO ➔ passa alla domanda 7
6bis)	SE SI', IN CHE MODO?	_____ _____ _____

Per ciascuna delle seguenti affermazioni, indica quanto sei d'accordo in una scala da 1 ("Per niente d'accordo") a 5 ("Del tutto d'accordo")	Scala di importanza				
	Per niente d'accordo	Non molto d'accordo	Né in disaccordo né d'accordo	Abbastanza d'accordo	Del tutto d'accordo
	1	2	3	4	5
7) Per decidere se andare a votare o astenerti, hai considerato anche la probabilità di un pareggio tra due o più partiti					
8) Per decidere se andare a votare o astenerti, la facilità e il breve tempo con cui è possibile votare hanno giocato un ruolo rilevante					
9) Anche se un singolo voto non fa la differenza, è comunque importante partecipare al processo democratico e andare a votare					
10) Sei andata/o a votare anziché astenerti anche perché volevi che persone che ti conoscono vedessero che andavi a votare					
11) Sei andata/o a votare perché pensi che altre persone che avrebbero votato come te non hanno potuto per varie ragioni					
12) La politica ti interessa anche al di fuori della campagna elettorale					
13) Per votare hai dovuto rinunciare a qualcosa (viaggio, riposo, svago)					

14)	RICOPRI O HAI RICOPERTO RUOLI "ISTITUZIONALI" (RAPPRESENTANTE DI CLASSE O D'ISTITUTO A SCUOLA, DI CONDOMINIO, O ALTRO)?	a) SI' b) NO
15)	HAI VOTATO ANCHE ALLE SCORSE ELEZIONI COMUNALI (15-16 MAGGIO 2011)?	a) SI' b) NO
16)	HAI VOTATO ANCHE ALLE ELEZIONI POLITICHE DEL 2008?	a) SI' c) N/A se allora non 18enne b) NO
17)	QUANTO TEMPO HAI DEDICATO AL VOTO (PER ESEMPIO, ANDARE AL SEGGIO, FARE LA FILA, ECC.)? NON CONTARE IL TEMPO DEDICATO AD ATTIVITA' CHE AVRESTI FATTO ANCHE SE NON CI FOSSERO STATE LE ELEZIONI	_____ Minuti
18)	QUANTO TEMPO AL MASSIMO SARESTI STATO DISPOSTO A IMPEGNARE PER VOTARE (PER ESEMPIO, PER ANDARE AL SEGGIO, FARE LA FILA, ECC.)?	_____ Minuti
19)	QUANTO TEMPO IN UNA SETTIMANA TIPO DEDICHI A INFORMARTI DI POLITICA (GIORNALI, WEB, TV, RADIO, ECC.)?	_____ Ore
20)	QUAL E' IL PIU' ALTO GRADO DI ISTRUZIONE CHE HAI FINORA **CONSEGUITO**?	a) Licenza media o meno (incluso 3 anni di scuola professionale) b) Diploma di scuola superiore (5 anni) c) Laurea triennale d) > Laurea triennale
21)	QUAL E' LA TUA FASCIA DI REDDITO ANNUO LORDO (ESCLUDENDO IL REDDITO PERCEPITO DAI TUOI GENITORI, PARTNER, ECC)?	a) 10 000 € o meno (incluso no reddito) b) (10 000 €, 20 000 €] c) (20 000 €, 30 000 €] d) (30 000 €, 50 000 €] e) Più di 50 000 €
22)	QUANTO PENSI CHE SIA PIU' IMPORTANTE POTER VOTARE SIA ALLA CAMERA SIA AL SENATO RISPETTO A POTER VOTARE SOLO ALLA CAMERA?	a) Meno importante b) Più importante ma meno di due volte più importante c) Due volte più importante d) Più di due volte più importante

Figure A2: Voter-Level Survey (English Version)

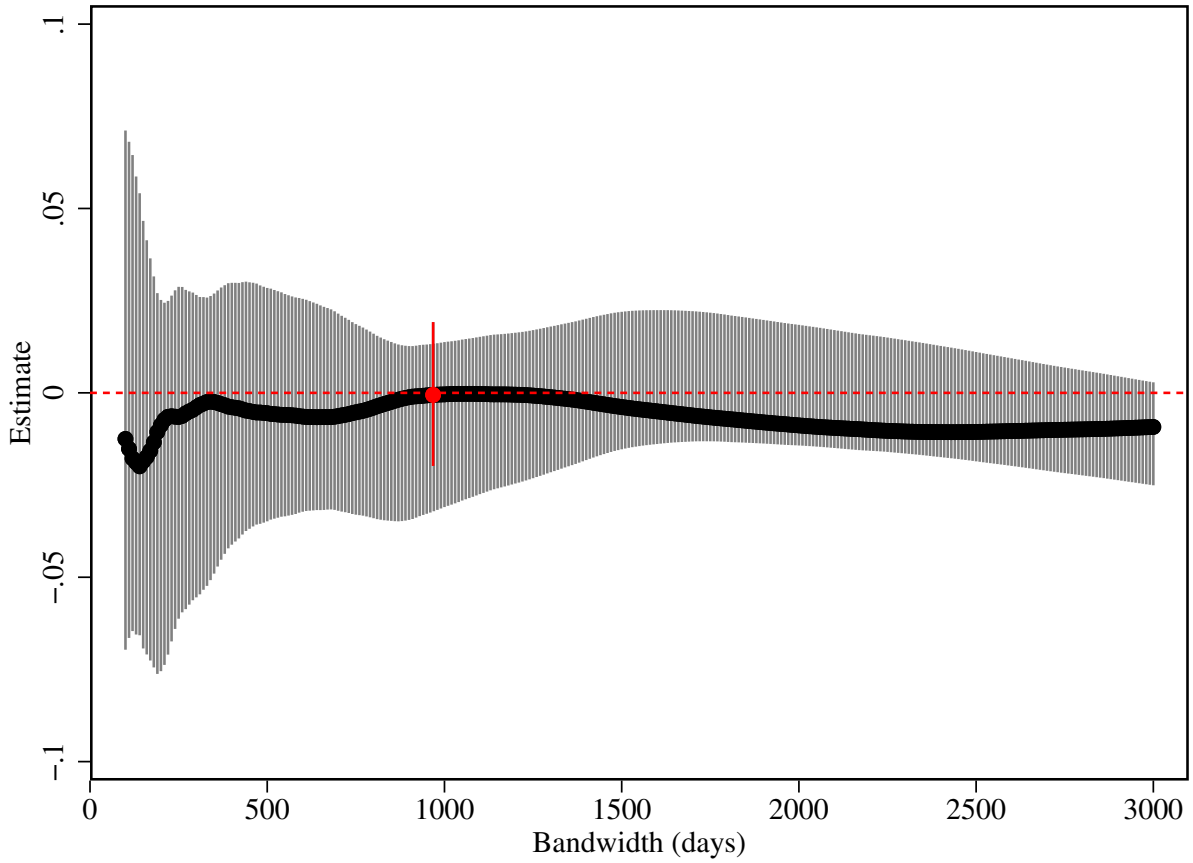
0)	GENDER?	a) MALE b) FEMALE
1)	WHAT IS YOUR BIRTHDATE (DAY/MONTH/YEAR)?	|__|__|/|__|__|/|__|__|
2)	DO YOU KNOW THE NAME OF ONE OR MORE INDIVIDUALS RUNNING FOR THE HOUSE OF REPRESENTATIVES IN YOUR DISTRICT?	a) YES b) NO ➔ go to question 3
2bis)	IF SO, WHAT IS/ARE THEIR NAME(S)?	_____ _____ _____
2ter)	IF SO, DID THE FACT THAT THESE PEOPLE WERE RUNNING FOR THE HOUSE OF REPRESESENTATIVES HAVE ANY INFLUENCE ON YOUR DECISION TO **GO TO THE POLLS** (REGARDLESS OF WHICH PARTY YOU CAST YOUR VOTE FOR) OR YOU WOULD HAVE VOTED ANYWAY?	a) YES a) NO
3)	DO YOU KNOW THE NAME OF ONE OR MORE INDIVIDUALS RUNNING FOR THE SENATE IN YOUR DISTRICT?	a) YES b) NO ➔ go to question 4
3bis)	IF SO, WHAT IS/ARE THEIR NAME(S)?	_____ _____ _____
3ter)	IF SO, DID THE FACT THAT THESE PEOPLE WERE RUNNING FOR THE SENATE HAVE ANY INFLUENCE ON YOUR DECISION TO **GO TO THE POLLS** (REGARDLESS OF WHICH PARTY YOU CAST YOUR VOTE FOR) OR YOU WOULD HAVE VOTED ANYWAY?	a) YES b) NO
4)	WHICH PARTY DID YOU VOTE FOR AT THE HOUSE OF REPRESENTATIVES?	_____
5)	WHICH PARTY DID YOU VOTE FOR AT THE SENATE? *(write -888 if N/A because younger than 25)*	_____
6)	DID THE ELECTORAL RULE ("Calderoli Law", ie the so-called *porcellum*) THAT "TRANSLATES" BALLOTS INTO SEATS INFLUENCE YOUR VOTING BEHAVIOR FOR AT LEAST ONE OF THE TWO HOUSES OF THE PARLIAMENT?	a) YES b) NO ➔ go to question 7
6bis)	IF SO, HOW?	_____ _____ _____

For each of the following statements, please tell how much you agree with it on a scale ranging from 1 ("Totally disagree") to 4 ("Completely agree")	Importance				
	Totally disagree	Somewhat disagree	Neither disagree nor agree	Somewhat agree	Totally agree
	1	2	3	4	5
7) To choose whether to vote or to abstain, you also considered the probability of a tie between two or more parties					
8) To choose whether to vote or to abstain, the easiness and the short time required to cast your ballot played a relevant role					
9) Even though a single vote does not make the difference, it is important to participate in the democratic process and to go to the polls					
10) You went to cast your ballot because you wanted that other people you know could see you going to the voting station					
11) You went to cast your ballot because other people who think as you do could not go to the polls for a variety of reasons					
12) You are interested in politics also when there are no electoral campaigns					
13) You had to give up something in order to vote (e.g. travel, rest, leisure)					

14)	HAVE YOU EVER HAD "INSTITUTIONAL DUTIES" (E.G. CLASS OR SCHOOL REPRESENTATIVE, ETC.)?	a) YES b) NO
15)	DID YOU VOTE AT THE PREVIOUS MUNICIPAL ELECTIONS TOO (MAY 15-16, 2011)?	a) YES b) NO
16)	DID YOU VOTE AT THE 2008 POLITICAL ELECTIONS TOO?	a) YES c) N/A if then below 18 b) NO
17)	HOW MUCH TIME DID YOU DEDICATE TO VOTING ACTIVITIES (E.G. GOING TO AND QUEUEING AT THE VOTING STATION, ETC.)? DO NOT COUNT THE TIME DEDICATED TO ACTIVITIES THAT YOU WOULD HAVE CARRIED OUT EVEN WITHOUT ELECTIONS	_____ Minutes
18)	HOW MUCH TIME WOULD YOU HAVE BEEN WILLING TO DEDICATE, AT MOST, TO VOTING ACTIVITIES (E.G. GOING TO AND QUEUEING AT THE VOTING STATION, ETC.)?	_____ Minutes
19)	HOW MANY HOURS IN A TYPICAL WEEK DO YOU DEDICATE TO GATHERING INFORMATION ABOUT POLITICS (ON NEWSPAPERS, TV, INTERNET, RADIO, ETC.)?	_____ Hours
20)	WHAT IS THE HIGHEST SCHOOL DEGREE YOU HAVE **OBTAINED** SO FAR?	a) Middle school degree or less b) High school degree (5 years) c) Bachelor's Degree (3 years) d) > Bachelor's Degree (3 years)
21)	WHAT IS YOUR YEARLY GROSS INCOME BRACKET (EXCLUDING THE INCOME PERCEIVED BY YOUR PARENTS, PARTNER, ETC.)?	a) 10 000 € or less (including no income) b) (10 000 €, 20 000 €] c) (20 000 €, 30 000 €] d) (30 000 €, 50 000 €] e) More than 50 000 €
22)	HOW "RELATIVELY MORE IMPORTANT" DO YOU THINK IT IS TO BE ELIGIBLE TO CAST A BALLOT **BOTH** AT THE LOWER **AND** AT THE UPPER HOUSE OF PARLIAMENT AS OPPOSED TO AT THE LOWER HOUSE ONLY?	a) Less important b) More important, but less than twice as important c) Twice as important d) More than twice as important

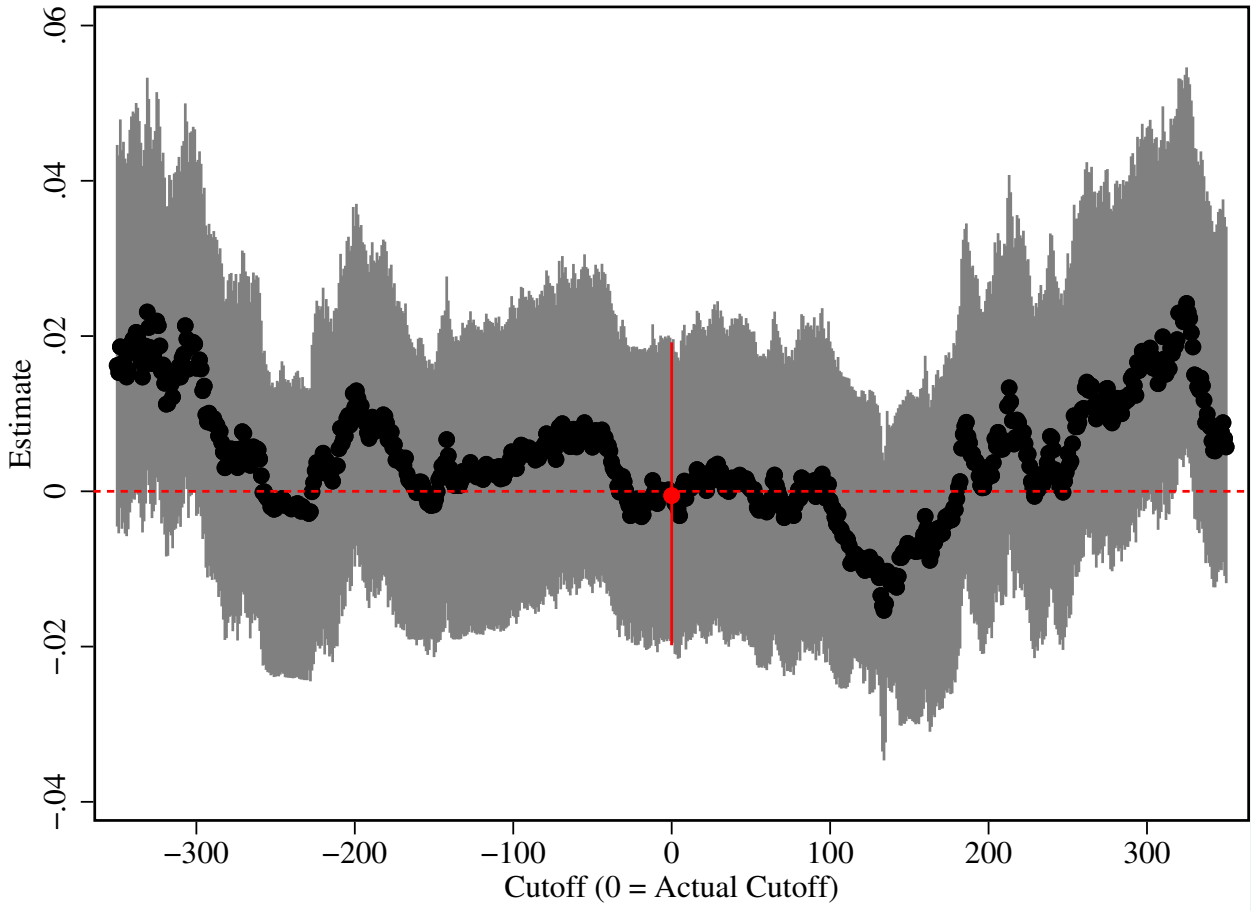
A.2 Turnout RD Robustness Checks

Figure A3: Voter-Level RD Estimates: Placebo Checks Varying Estimation Bandwidth



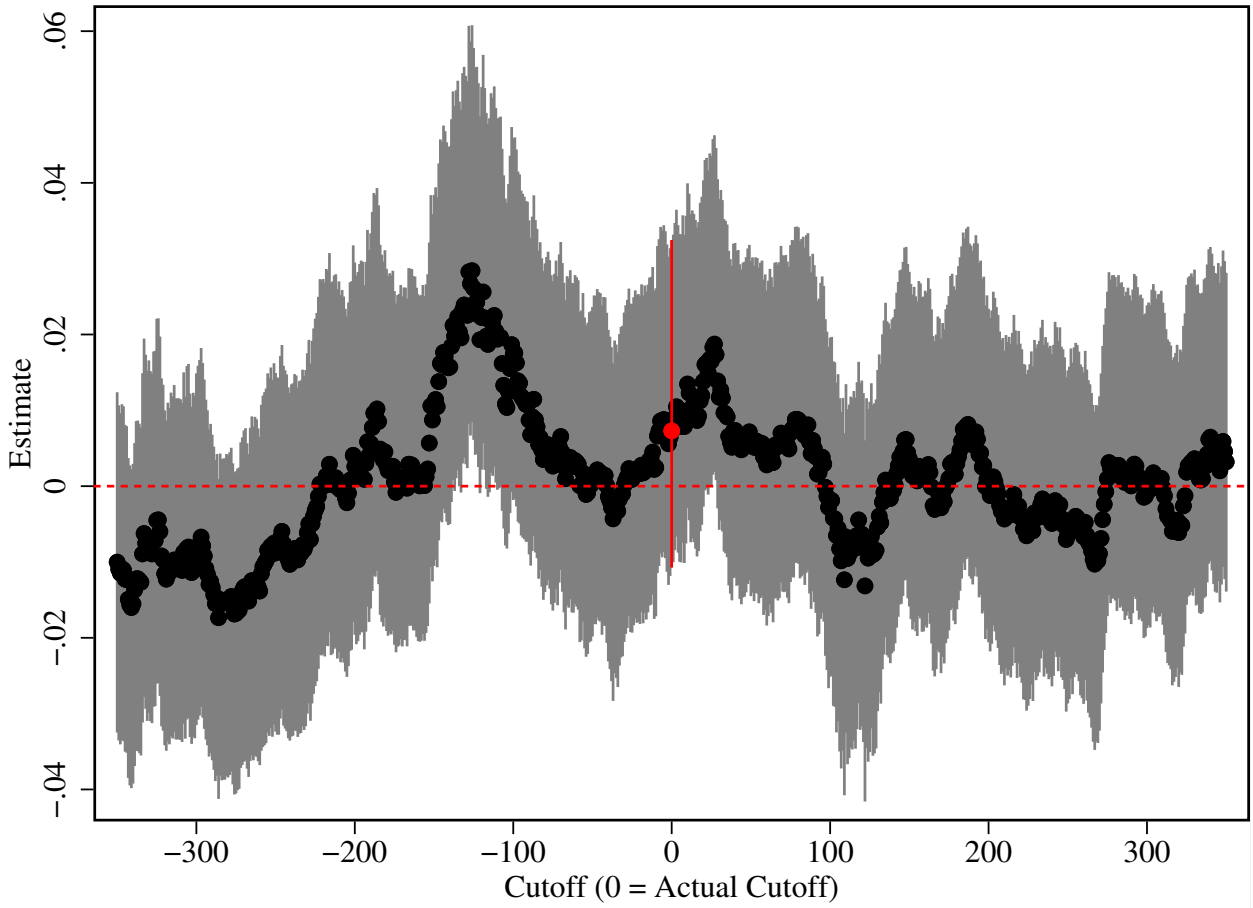
Notes: Each black point in the figure represents an RD estimate of the turnout effect of Senate voting eligibility in Italian parliamentary elections based on a different bandwidth (constrained to be the same for bias and treatment effect estimation) around the 25-year old discontinuity. Vertical gray bars denote corresponding 95-percent confidence intervals. The red dot and the red line denote, respectively, the main RD estimate and 95-percent confidence interval based on Calonico, Cattaneo and Titiunik (2014) procedure (Table 2, column 1).

Figure A4: Voter-Level RD Estimates: Placebo Checks Using Alternative Cutoffs



Notes: The figure plots RD estimates of the turnout effect of Senate voting eligibility in Italian parliamentary elections and corresponding 95-percent confidence intervals based on different placebo discontinuities. The x-axis denotes the cutoff value used for estimation relative to the actual Senate voting-eligibility cutoff.

Figure A5: Voter-Level RD Estimates: Placebo Checks Using Alternative Cutoffs in Non-Parliamentary Elections



Notes: The figure plots placebo RD estimates and 95-percent confidence intervals of the turnout effect of being exactly 25 on the day of a non-parliamentary election. The x-axis denotes the cutoff value used for estimation relative to the 25-year-old cutoff.

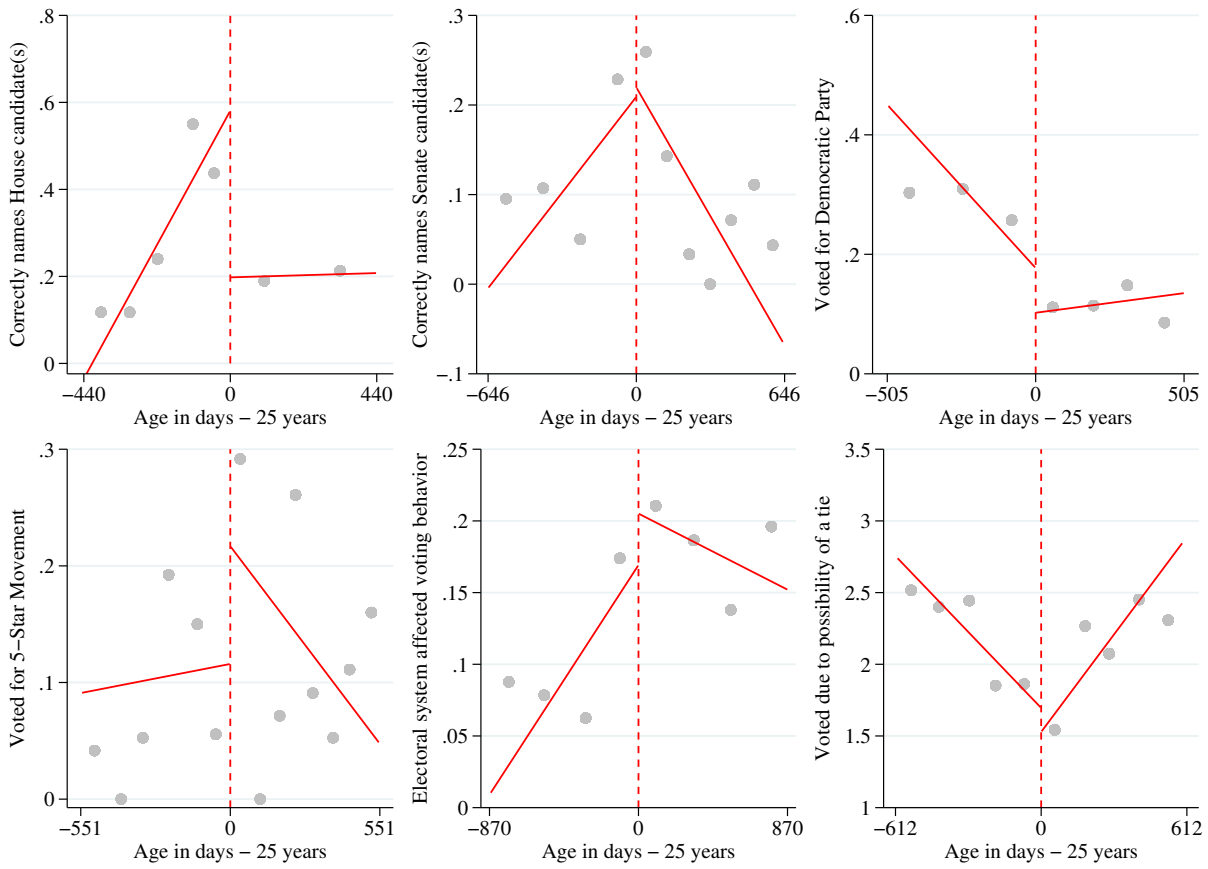
Table A1: Voter-Level RD Estimates: Placebo Effects Using Voter Covariates as Outcomes

	Sample Mean (1)	Sample St. Dev. (2)	N (3)	RDD Estimate (4)	Robust p-value (5)	FDR q-value (6)	Bwidth (Days) (7)	RDD N (8)
Education (1996):								
Missing education	.628	.483	241,305	-.0121	.198	1.000	1,051	24,384
Illiterate	.016	.126	241,305	.0014	.157	1.000	1,038	24,069
Elementary school	.147	.354	241,305	.0042	.068	.759	1,416	32,957
Middle school	.133	.340	241,305	.0095	.247	1.000	1,126	26,166
High school	.061	.239	241,305	-.0049	.386	1.000	1,274	29,531
College	.015	.123	241,305	.0017	.026	.681	1,540	35,777
Occupation (1996):								
Missing occupation or unemployed	.643	.479	241,305	-.0121	.169	1.000	1,118	25,915
Agricultural worker	.014	.117	241,305	-.0006	.721	1.000	1,930	45,018
Blue-collar worker	.052	.222	241,305	-.0003	.669	1.000	1,344	31,295
Artisan	.026	.158	241,305	.0002	.995	1.000	1,777	41,415
Retail worker	.018	.133	241,305	.0002	.681	1.000	1,727	40,216
White-collar worker (low level)	.019	.136	241,305	.0010	.496	1.000	1,855	43,246
White-collar worker (mid level)	.041	.198	241,305	-.0002	.764	1.000	1,416	32,957
White-collar worker (high level)	.018	.134	241,305	.0001	.541	1.000	1,751	40,785
Homemaker or Retiree	.134	.341	241,305	-.0010	.835	1.000	1,774	41,356
Student	.036	.186	241,305	.0140	.039	.681	957	22,099
Marital status:								
Missing marital status	.850	.357	241,305	.0038	.767	1.000	903	20,856
Single, divorced	.144	.352	241,305	-.0057	.716	1.000	917	21,169
Married	.006	.076	241,305	.0017	.430	1.000	1,519	35,284
Lives in Emilia-Romagna	.228	.420	241,305	-.0006	.875	1.000	932	21,594
Female	.519	.500	241,305	-.0111	.269	1.000	1,759	40,983

Notes: Each row reports summary statistics (columns 1-3) and RDD results (columns 4-8) of a different voter variable from the pooled Prospex-Bologna sample. RDD estimates (column 4), p-values (column 5), and bandwidths (column 7) are from local linear regressions based on Calonico et al. (2014) optimal bandwidth selector with HC1 heteroskedasticity-robust variance estimator. Column 6 reports False Discovery Rate (FDR) q-values robust to multiple hypotheses testing based on Anderson (2008)'s procedure.
** $p < 0.01$, * $p < 0.05$, ~ $p < 0.10$

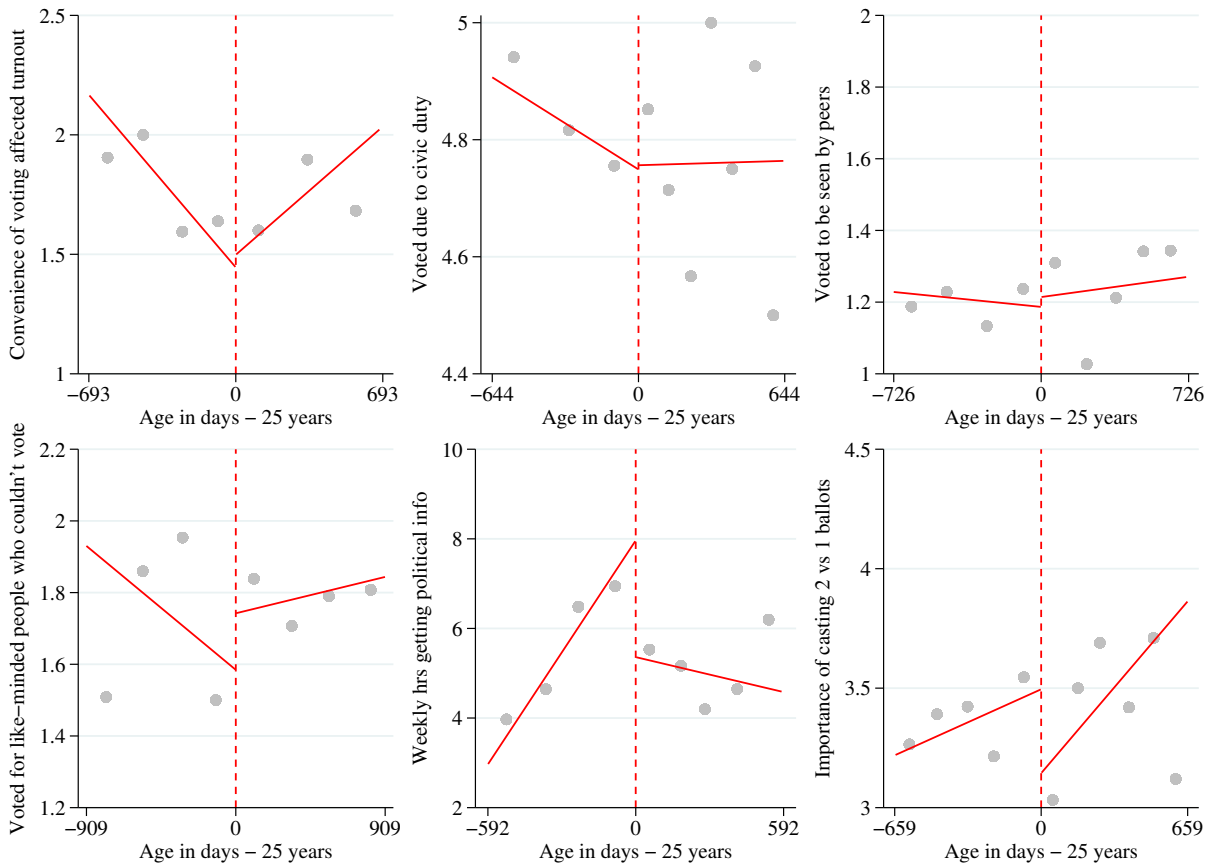
A.3 Survey RD Graphs and Summary Statistics

Figure A6: Voter-Level Information and Political Behavior around 25-Year Discontinuity (Survey Outcomes) – Part 1



Notes: In each plot, the solid red line represents the linear fit of a distinct survey outcome on the age-based running variable. Estimation bandwidths are chosen following Calonico, Cattaneo and Titiunik (2014) and correspond to those reported in Table A2, column 7. Point clouds represent mean outcomes by bins of the running variable, where the number of equally-spaced bins is chosen following Calonico, Cattaneo and Titiunik (2015).

Figure A7: Voter-Level Information and Political Behavior around 25-Year Discontinuity (Survey Outcomes) – Part 2



Notes: In each plot, the solid red line represents the linear fit of a distinct survey outcome on the age-based running variable. Estimation bandwidths are chosen following Calonico, Cattaneo and Titiunik (2014) and correspond to those reported in Table A2, column 7. Point clouds represent mean outcomes by bins of the running variable, where the number of equally-spaced bins is chosen following Calonico, Cattaneo and Titiunik (2015).

Table A2: Survey Data: Baseline Characteristics

	Sample Mean (1)	Sample St. Dev. (2)	N (3)	RDD Estimate (4)	Robust p-value (5)	FDR q-value (6)	Bwidth (Days) (7)	RDD N (8)
Female	.452	.498	1,185	.127	.351	.779	1,327	324
Ever had "institutional" roles? (Q14)	.376	.484	1,193	.033	.775	.779	1,522	367
Voted in the past municipal election (5/15/2011)? (Q15)	.865	.342	999	.105	.163	.678	1,179	284
Voted in the 2008 parliamentary election? (Q16)	.917	.276	702	.003	.954	.788	816	191
Educational attainment: (Q20)								
Middle-school degree or less	.145	.352	1,188	-.063	.234	.678	1,590	384
High-school degree	.496	.500	1,188	-.155	.151	.678	1,557	375
Bachelor's degree	.198	.399	1,188	.290	.018	.305	1,135	278
More than bachelor's degree	.162	.368	1,188	-.043	.532	.779	1,278	315
Income: (Q21)								
€10k or less (including no income)	.684	.465	1,057	.142	.131	.678	1,489	327
(€10k, €20k]	.190	.393	1,057	-.124	.168	.678	1,351	302
(€20k, €30k]	.091	.287	1,057	.012	.635	.779	1,369	305
(€30k, €50k]	.027	.163	1,057	-.030	.236	.678	943	209
€50k or more	.008	.087	1,057	-.024	.291	.775	1,939	441

Notes: Each row reports summary statistics (columns 1-3) and RDD balancing exercises (columns 4-8) of a different voter characteristics from Bologna's survey data. RDD estimates (column 4), p-values (column 5), and bandwidths (column 7) are from local linear regressions based on Calonico et al. (2014) optimal bandwidth selector with HC1 heteroskedasticity-robust variance estimator. Column 6 reports False Discovery Rate (FDR) q-values robust to multiple hypotheses testing based on Anderson (2008)'s procedure.

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

A.4 City-Level Summary Statistics and DD Robustness Checks

To test for Granger causality, we estimate models of the following form:

$$y_{i,t} = \alpha^m T^m_{i,\tau} + \alpha^p T^p_{p(i),\tau} + \alpha^r T^r_{r(i),\tau} + \beta^m T^m_{i,t} + \beta^p T^p_{p(i),t} + \beta^r T^r_{r(i),t} + \delta_i + \gamma_t + \varepsilon_{i,t}, \tag{3}$$

where $\tau > t$ is the date of the next city i's election. More precisely, if regression 3 is run using, say, the sample of municipal elections, t denotes the date of a municipal election held in city i, and $\tau > t$ is the date of the following municipal election in the same city.

Equation 3 augments regression 1 with indicator variables for future concurrent elections. The α's are the coefficients of interest, which measure the current electoral effect of future coincident elections. Following the structure of Table 5, Table A5 reports the estimated α's.

Table A3: City-Level Summary Statistics

Election type:	Provincial	European	Regional	Municipal	House	Senate
	(1)	(2)	(3)	(4)	(5)	(6)
			A. Covariates and Treatments			
1(runoff)	.265	0	0	.250	0	0
1(concurrent provincial election)	1	.422	.189	.274	.030	.030
1(concurrent regional election)	.020	.036	1	.111	.052	.052
1(concurrent municipal election)	.348	.340	.161	1	.055	.056
			B. Outcomes			
City-Level Turnout	.651	.700	.702	.729	.823	.822
	(.135)	(.118)	(.115)	(.106)	(.077)	(.077)
(Under- and Over-Votes)/Voters	.021	.051	.054	.032	.042	.042
	(.019)	(.034)	(.034)	(.021)	(.027)	(.033)
Valid Votes/Voters	.630	.649	.648	.697	.782	.780
	(.124)	(.108)	(.109)	(.099)	(.083)	(.084)
N cities observed both w/ and						
…a provincial election	0	5,366	5,880	4,733	1,048	1,048
…a regional election	127	1,583	0	5,148	2,057	2,057
…a municipal election	2,710	4,985	5,079	0	1,628	1,630
N	18,342	35,343	32,542	38,079	42,423	42,432

Notes: The table reports summary statistics of city-level data received from the Italian Ministry of Interior. Variables in Panels A and B refer to, respectively, treatments and outcomes for city-level regressions. Each column represents a different election type/sample. Within each each column/sample, city-election dates are the units of observation. Standard deviations of non-dichotomous variables are reported in parentheses. All summary statistics are weighted by the number of voters in a given city-election date.

Table A4: Turnout Effect of Concurrent Municipal Elections in the Voter-Level ITANES and Bologna Data

Election type:	Regional	House
	(1)	(2)
	A. All Voters	
1(concurrent municipal election)	.039 ~	.012
	(.021)	(.014)
Outcome mean	.779	.866
N	174,495	195,703
	B. Voters Aged 18-30	
1(concurrent municipal election)	.029	-.002
	(.025)	(.013)
Outcome mean	.786	.853
N	51,139	51,760

Notes: The table reports estimated effects of concurrent municipal elections using voter-level turnout data from ITANES and the municipality of Bologna. Panel A includes all voters excluding residents of the four special regions which are excluded due to incomplete election calendars in these regions), while Panel B focuses on the subsample of voters aged 18-30. All regressions control for election round day of the election and city fixed effects. Standard errors clustered at the city level are reported in parentheses.
** $p < 0.01$, * $p < 0.05$, ~ $p < 0.10$

Table A5: City-Level Placebo Exercises: Granger Causality

Election type:	Provincial	European	Regional	Municipal	House	Senate
	(1)	(2)	(3)	(4)	(5)	(6)
			A. City-Level Turnout			
1(concurrent provincial election$_\tau$)	-	-.031 **	-.037 ~	-.005	.003	.006
	-	(.007)	(.019)	(.008)	(.011)	(.010)
1(concurrent regional election$_\tau$)	-	-.023	-	-.003	-.010	-.010
	-	{.890}	-	{.795}	{.855}	{.849}
1(concurrent municipal election$_\tau$)	-.013	.004	-.006	-	.0005	-.004 ~
	(.009)	(.004)	(.004)	-	(.0031)	(.002)
Joint test p-value	.335	.293	.826	.875	.959	.321
Control outcome mean	.631	.686	.693	.729	.838	.836
N	11,270	28,228	25,822	30,966	35,331	35,340
			B. (Under- and Over-Votes)/Voters			
1(concurrent provincial election$_\tau$)	-	.003	-.017 **	.0001	.003	.002
	-	(.002)	(.003)	(.0010)	(.005)	(.004)
1(concurrent regional election$_\tau$)	-	-.005	-	.00001	.004	.004
	-	{.989}	-	{.999}	{.843}	{.823}
1(concurrent municipal election$_\tau$)	.003 ~	.002	-.002	-	.002	.003
	(.002)	(.001)	(.002)	-	(.002)	(.002)
Joint test p-value	.172	.731	.607	.999	.806	.867
Control outcome mean	.014	.037	.038	.029	.043	.044
N	11,270	28,228	25,822	30,966	35,331	35,340
			C. Valid Votes/Voters			
1(concurrent provincial election$_\tau$)	-	-.033 **	-.020	-.005	.001	.005
	-	(.009)	(.017)	(.007)	(.015)	(.013)
1(concurrent regional election$_\tau$)	-	-.017	-	-.003	-.014	-.014
	-	{.743}	-	{.684}	{.647}	{.585}
1(concurrent municipal election$_\tau$)	-.016 ~	.003	-.004	-	-.001	-.007 **
	(.009)	(.003)	(.004)	-	(.002)	(.003)
Joint test p-value	.188	.130	.872	.808	.673	.026
Control outcome mean	.617	.649	.656	.699	.795	.792
N	11,270	28,228	25,822	30,966	35,331	35,340

Notes: The table reports results from placebo regressions of turnout (Panel A), invalid votes (Panel B), and valid votes (Panel C) on future concurrent provincial, regional, and municipal elections. Each column represents a different election sample. All regressions are weighted by voter counts and control for current and future election round (i.e., first round or runoff; only relevant for provincial and municipal elections), day of the election, city fixed effects, as well as dummies for concurrent provincial, regional and municipal elections. Standard errors clustered at the province level are reported in parentheses. P-values robust to clustering at the region level, reported in braces, are computed with a 10,000-replication wild bootstrap using Webb (2014) weights. P-values of joint insignificance are robust to clustering at the province level.

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

Table A6: City-Level Robustness Checks: Excluding Anomalies in Municipal Election Calendars

Election type:	Provincial	European	Regional	Municipal	House	Senate
	(1)	(2)	(3)	(4)	(5)	(6)
			A. City-Level Turnout			
1(concurrent provincial election)	-	.039 **	.021	.002	.012	.012
	-	(.010)	(.015)	(.004)	(.012)	(.012)
1(concurrent regional election)	-.014	.123	-	.016	-.001	.002
	(.943)	{.519}	-	{.746}	{.990}	{.985}
1(concurrent municipal election)	.113 **	.091 **	.067 **	-	.054 **	.053 **
	(.007)	(.006)	(.009)	-	(.010)	(.011)
Joint test p-value	.002	.000	.000	.772	.000	.000
Control outcome mean	.592	.639	.669	.704	.825	.824
N	18,098	35,038	32,104	33,632	41,141	41,148
			B. (Under- and Over-Votes)/Voters			
1(concurrent provincial election)	-	.018 **	.001	.005 **	.008 **	.006 **
	-	(.003)	(.006)	(.001)	(.002)	(.002)
1(concurrent regional election)	.006	.033	-	.004	.013	.013
	(.748)	{.532}	-	{.586}	{.323}	{.387}
1(concurrent municipal election)	.025 **	.025 **	.021 **	-	.020 **	.021 **
	(.002)	(.003)	(.004)	-	(.003)	(.003)
Joint test p-value	.000	.000	.000	.076	.000	.000
Control outcome mean	.013	.032	.045	.027	.041	.042
N	18,098	35,038	32,104	33,632	41,141	41,148
			C. Valid Votes/Voters			
1(concurrent provincial election)	-	.021 **	.021	-.003	.004	.006
	-	(.008)	(.013)	(.004)	(.012)	(.011)
1(concurrent regional election)	-.021	.090	-	.012	-.014	-.012
	(.911)	{.516}	-	{.778}	{.866}	{.887}
1(concurrent municipal election)	.088 **	.067 **	.046 **	-	.034 **	.032 **
	(.007)	(.005)	(.009)	-	(.009)	(.008)
Joint test p-value	.006	.000	.003	.804	.001	.002
Control outcome mean	.579	.607	.623	.676	.785	.782
N	18,098	35,038	32,104	33,632	41,141	41,148

Notes: The table reports the same estimates as Table 5. Differently from Table 5, however, all samples exclude election dates coinciding with irregularities in municipal election calendars (i.e., municipal elections that, due to a mayor's death or removal, occur earlier than 5 years from the prior municipal election).

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

Table A7: Effect of Concurrent Elections on Invalid Ballots: North vs. South

	(Under- and Over-Votes)/Voters					
Election type:	Provincial	European	Regional	Municipal	House	Senate
	(1)	(2)	(3)	(4)	(5)	(6)
			A. Northern and Central Regions			
1(concurrent provincial election)	-	.010 **	-.010 *	.005 **	.002	.002 ~
	-	(.002)	(.004)	(.001)	(.002)	(.001)
1(concurrent regional election)	-	.003	-	.004	.007	.010
	-	{.943}	-	{.669}	{.498}	{.469}
1(concurrent municipal election)	.023 **	.017 **	.017 **	-	.014 **	.012 **
	(.002)	(.001)	(.003)	-	(.002)	(.003)
Control outcome mean	.011	.030	.047	.028	.035	.037
N	13,042	24,510	24,020	25,991	29,430	29,437
			B. Southern Regions			
1(concurrent provincial election)	-	.030 **	.015 **	.004 **	.019 **	.018 **
	-	(.004)	(.005)	(.001)	(.004)	(.005)
1(concurrent regional election)	-	.066	-	.004	.028	.024
	-	{.95}	-	{.598}	{.330}	{.400}
1(concurrent municipal election)	.038 **	.056 **	.046 **	-	.050 **	.046 **
	(.003)	(.004)	(.005)	-	(.004)	(.003)
Control outcome mean	.015	.036	.040	.026	.052	.052
N	5,300	10,833	8,522	12,088	12,993	12,995

Notes: The table reports estimated effects of concurrent provincial, regional, and municipal elections on invalid votes in Northern (Panel A) and Southern regions (Panel B). Each column represents a different election sample. All regressions are weighted by voter counts and control for election round (i.e., first round or runoff; only relevant for provincial and municipal elections), day of the election, and city fixed effects. Standard errors clustered at the province level are reported in parentheses. P-values robust to clustering at the region level, reported in braces, are computed with a 10,000-replication wild bootstrap using Webb (2014) weights.

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

Table A8: Effect of Concurrent Elections on Valid Ballots: North vs. South

	Valid Votes/Voters					
Election type:	Provincial	European	Regional	Municipal	House	Senate
	(1)	(2)	(3)	(4)	(5)	(6)
			A. Northern and Central Regions			
1(concurrent provincial election)	-	.0002	.009	-.002	-.011 *	-.010 *
	-	(.0024)	(.008)	(.003)	(.004)	(.005)
1(concurrent regional election)	-	.032	-	.012	-.016	-.016
	-	{.844}	-	{.812}	{.839}	{.836}
1(concurrent municipal election)	.093 **	.065 **	.026 **	-	-.005 *	-.004
	(.007)	(.005)	(.006)	-	(.002)	(.004)
Control outcome mean	.584	.663	.642	.680	.827	.822
N	13,042	24,510	24,020	25,991	29,430	29,437
			B. Southern Regions			
1(concurrent provincial election)	-	.057 **	.020 **	-.001	.037 *	.039 *
	-	(.012)	(.007)	(.004)	(.018)	(.019)
1(concurrent regional election)	-	.174	-	.005	.087	.092
	-	{.96}	-	{.908}	{.25}	{.28}
1(concurrent municipal election)	.075 **	.117 **	.057 **	-	.016 **	.021 **
	(.016)	(.009)	(.007)	-	(.004)	(.004)
Control outcome mean	.567	.485	.570	.681	.700	.696
N	5,300	10,833	8,522	12,088	12,993	12,995

Notes: The table reports estimated effects of concurrent provincial, regional, and municipal elections on valid votes in Northern (Panel A) and Southern regions (Panel B). Each column represents a different election sample. All regressions are weighted by voter counts and control for election round (i.e., first round or runoff; only relevant for provincial and municipal elections), day of the election, and city fixed effects. Standard errors clustered at the province level are reported in parentheses. P-values robust to clustering at the region level, reported in braces, are computed with a 10,000-replication wild bootstrap using Webb (2014) weights.

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

Table A9: Effect of Concurrent Elections on Turnout: Municipalities with Low vs. High Shares of College Graduates

	City-Level Turnout					
Election type:	Provincial	European	Regional	Municipal	House	Senate
	(1)	(2)	(3)	(4)	(5)	(6)
		A. Cities w/ Low Share of College Graduates				
1(concurrent provincial election)	-	.045 **	.038 **	.003	.022	.023
	-	(.012)	(.015)	(.005)	(.014)	(.014)
1(concurrent regional election)	-	.144	-	.013	-.001	-.000
	-	{.565}	-	{.873}	{.998}	{.999}
1(concurrent municipal election)	.107 **	.106 **	.095 **	-	.056 **	.054 **
	(.008)	(.009)	(.011)	-	(.010)	(.009)
Control outcome mean	.572	.621	.659	.739	.814	.811
N	9,125	17,458	15,726	18,080	20,974	20,968
		B. Cities w/ High Share of College Graduates				
1(concurrent provincial election)	-	.037 **	.013	.006	-.006	-.006
	-	(.010)	(.013)	(.004)	(.013)	(.012)
1(concurrent regional election)	-	.106	-	.014	.002	.005
	-	{.51}	-	{.747}	{.98}	{.95}
1(concurrent municipal election)	.114 **	.096 **	.063 **	-	.026 **	.025 **
	(.008)	(.008)	(.009)	-	(.007)	(.008)
Control outcome mean	.596	.643	.671	.702	.829	.828
N	9,217	17,885	16,816	19,999	21,449	21,464

Notes: The table reports estimated effects of concurrent provincial, regional, and municipal elections on turnout in municipalities with share of college graduates lower than (Panel A) or higher than the median city (Panel B). Each column represents a different election sample. All regressions are weighted by voter counts and control for election round (i.e., first round or runoff; only relevant for provincial and municipal elections), day of the election, and city fixed effects. Standard errors clustered at the province level are reported in parentheses. P-values robust to clustering at the region level, reported in braces, are computed with a 10,000-replication wild bootstrap using Webb (2014) weights.
** $p < 0.01$, * $p < 0.05$, ~ $p < 0.10$

Table A10: Effect of Concurrent Elections on Turnout: Municipalities with Low vs. High Average Income

	City-Level Turnout					
Election type:	Provincial	European	Regional	Municipal	House	Senate
	(1)	(2)	(3)	(4)	(5)	(6)
			A. Cities w/ Low Average Income			
1(concurrent provincial election)	-	.070 **	.039 **	.003	.043 *	.046 **
	-	(.011)	(.008)	(.005)	(.018)	(.018)
1(concurrent regional election)	-	.214	-	.001	.026	.026
	-	{.972}	-	{.975}	{.706}	{.702}
1(concurrent municipal election)	.106 **	.139 **	.101 **	-	.066 **	.065 **
	(.009)	(.010)	(.008)	-	(.005)	(.006)
Control outcome mean	.587	.562	.626	.733	.776	.772
N	9,081	17,726	15,390	18,702	21,290	21,286
			B. Cities w/ High Average Income			
1(concurrent provincial election)	-	.025 **	.004	.004	-.009	-.009
	-	(.008)	(.012)	(.004)	(.006)	(.006)
1(concurrent regional election)	-	.081	-	.014	-.002	.001
	-	{.52}	-	{.779}	{.98}	{.99}
1(concurrent municipal election)	.122 **	.085 **	.057 **	-	.019 **	.019 **
	(.009)	(.006)	(.009)	-	(.005)	(.007)
Control outcome mean	.592	.663	.681	.701	.844	.843
N	9,261	17,617	17,152	19,377	21,133	21,146

Notes: The table reports estimated effects of concurrent provincial, regional, and municipal elections on turnout in municipalities with average income lower than (Panel A) or higher than in the median city (Panel B). Each column represents a different election sample. All regressions are weighted by voter counts and control for election round (i.e., first round or runoff; only relevant for provincial and municipal elections), day of the election, and city fixed effects. Standard errors clustered at the province level are reported in parentheses. P-values robust to clustering at the region level, reported in braces, are computed with a 10,000-replication wild bootstrap using Webb (2014) weights.
** $p < 0.01$, * $p < 0.05$, ~ $p < 0.10$

A.5 DD P-Values Robust To Multiple Inference

Table A11: Effect of Concurrent Elections on Turnout, Invalid Ballots, and Valid Ballots - Multiple Inference

Election type:	Provincial	European	Regional	Municipal	House	Senate
	(1)	(2)	(3)	(4)	(5)	(6)
			A. City-Level Turnout			
1(concurrent provincial election)	-	.039	.020	.006	-.004	-.004
	-	[.000]	[.161]	[.101]	[.774]	[.795]
	-	<.001>	<.292>	<.188>	<1.000>	<1.000>
1(concurrent regional election)	-.011	.118	-	.016	.002	.005
	{.949}	{.522}	-	{.738}	{.985}	{.959}
	<1.000>	<.921>	-	<1.000>	<1.000>	<1.000>
1(concurrent municipal election)	.118	.099	.070	-	.031	.030
	[.000]	[.000]	[.000]	-	[.000]	[.001]
	<.001>	<.001>	<.001>	-	<.001>	<.003>
Control outcome mean	.591	.639	.669	.708	.825	.824
N	18,342	35,343	32,542	38,079	42,423	42,432
			B. (Under- and Over-Votes)/Voters			
1(concurrent provincial election)	-	.018	.001	.005	.001	.002
	-	[.000]	[.898]	[.000]	[.854]	[.462]
	-	<.001>	<1.000>	<.001>	<1.000>	<.921>
1(concurrent regional election)	.007	.033	-	.004	.014	.014
	{.691}	{.537}	-	{.515}	{.340}	{.396}
	<1.000>	<.921>	-	<.921>	<.679>	<.817>
1(concurrent municipal election)	.027	.027	.022	-	.029	.025
	[.000]	[.000]	[.000]	-	[.000]	[.000]
	<.001>	<.001>	<.001>	-	<.001>	<.001>
Control outcome mean	.013	.032	.045	.028	.041	.042
N	18,342	35,343	32,542	38,079	42,423	42,432
			C. Valid Votes/Voters			
1(concurrent provincial election)	-	.021	.019	.001	-.005	-.005
	-	[.006]	[.130]	[.731]	[.715]	[.680]
	-	<.011>	<.238>	<1.000>	<1.000>	<1.000>
1(concurrent regional election)	-.018	.086	-	.011	-.012	-.010
	{.908}	{.521}	-	{.781}	{.881}	{.910}
	<1.000>	<.921>	-	<1.000>	<1.000>	<1.000>
1(concurrent municipal election)	.090	.072	.048	-	.002	.005
	[.000]	[.000]	[.000]	-	[.755]	[.441]
	<.001>	<.001>	<.001>	-	<1.000>	<.919>
Control outcome mean	.578	.607	.623	.680	.785	.782
N	18,342	35,343	32,542	38,079	42,423	42,432

Notes: The table reports the same point estimates as Table 5. Province-clustered asymptotic p-values are reported in brackets. Wild bootstrap p-values robust to clustering at the region level are reported in braces. False Discovery Rate (FDR) q-values robust to multiple hypotheses testing are reported in angle brackets and are based on Anderson (2008)'s procedure.

Table A12: Effect of Concurrent Elections on Turnout: North vs. South - Multiple Inference

	City-Level Turnout					
Election type:	Provincial	European	Regional	Municipal	House	Senate
	(1)	(2)	(3)	(4)	(5)	(6)
			A. Northern and Central Regions			
1(concurrent provincial election)	-	.010	-.001	.004	-.008	-.008
	-	[.010]	[.877]	[.202]	[.105]	[.081]
	-	<.014>	<.395>	<.152>	<.087>	<.071>
1(concurrent regional election)	-	.035	-	.016	-.009	-.006
	-	{.862}	-	{.788}	{.916}	{.944}
	-	<.395>	-	<.395>	<.395>	<.395>
1(concurrent municipal election)	.116	.083	.043	-	.009	.007
	[.000]	[.000]	[.000]	-	[.001]	[.031]
	<.001>	<.001>	<.001>	-	<.002>	<.033>
Control outcome mean	.595	.693	.689	.708	.861	.859
N	13,042	24,510	24,020	25,991	29,430	29,437
			B. Southern Regions			
1(concurrent provincial election)	-	.087	.035	.003	.056	.057
	-	[.000]	[.000]	[.526]	[.014]	[.019]
	-	<.001>	<.001>	<.295>	<.019>	<.023>
1(concurrent regional election)	-	.240	-	.009	.115	.116
	-	{.956}	-	{.868}	{.128}	{.154}
	-	<.395>	-	<.395>	<.102>	<.119>
1(concurrent municipal election)	.113	.173	.104	-	.066	.067
	[.000]	[.000]	[.000]	-	[.000]	[.000]
	<.001>	<.001>	<.001>	-	<.001>	<.001>
Control outcome mean	.583	.521	.610	.707	.752	.748
N	5,300	10,833	8,522	12,088	12,993	12,995

Notes: The table reports the same point estimates as Table 6. Province-clustered asymptotic p-values are reported in brackets. Wild bootstrap p-values robust to clustering at the region level are reported in braces. False Discovery Rate (FDR) q-values robust to multiple hypotheses testing are reported in angle brackets and are based on Anderson (2008)'s procedure.