Telecracy: Testing for Channels of Persuasion

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Can biased information persuade in the long run? Political information on Italian TV has been biased towards Berlusconi's party since 1994. We exploit a shock to exposure to bias: idiosyncratic deadlines to switch to digital TV from 2008 to 2012. Digital TV increased the number of free channels tenfold. The switch caused a drop in Berlusconi's vote share by 5.5 to 7.5 percentage points. The effect was stronger for older and less educated voters. At least 30% of digital users had not filtered out the bias from 1994 to 2010. Selective attention and persuasion bias are consistent with our results.

I Introduction

There is growing evidence that exposure to biased information persuades decision-makers. This is true in several domains, such as political information in the media (DellaVigna and Kaplan (2007), Enikolopov et al. (2011)), financial analyst forecasts (Malmendier and Shanthikumar (2007)), and product advertisements (Meyers-Levy and Malaviya (1999)). But is persuasion sustainable in the long run? And if so, why do individuals not filter out systematic biases over time?

To address these questions, we consider the long-lived bias towards Prime Minister Silvio Berlusconi in political information on Italian TV. For 10 years from 1994 to 2011, Berlusconi has controlled six out of seven national channels, due to his dual role as a media tycoon and Prime minister.[1] Durante and Knight (2012) document the existence of a bias in terms of the time and quality of coverage of Berlusconi's party and his opponents. The recent stunning comeback by Berlusconi in 2013 elections after a massive appearance on TV is a vivid example of the effect of information bias on voting (see section II).

We exploit a quasi-random shock to the biased TV exposure of Italian viewers: idiosyncratic deadlines to forcibly switch from analog to digital TV from 2008 to 2012. At the deadlines, analog signals were switched off, and only digital signals kept on airing.

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[1] In other years, he directly controlled three channels, and influenced the others through the executives he had appointed when Prime Minister.

Digital TV improved transmission efficiency, and increased the number of free national channels tenfold. 51 out of 78 new channels are aired by new media companies, which have no ties to Berlusconi or to the governmental network.[2] After switching to digital TV, many Italian households changed their viewing habits: from October 2008 to June 2011, the average monthly share of viewers of Berlusconi-controlled channels dropped from 86% to 72%. Over the same period, the share of viewers of new digital channels increased from 3% to 18%.[3] Switch-off deadlines were spatially heterogenous, and largely idiosyncratic to the purposes of our analysis. European Union legislation imposed the move to digital TV by the end of 2012. National legislation established moving dates for Italian regions based on the similarity of infrastructures built in the 1950s, and to guarantee a homogeneous move for the north, center and south of the country. Moving to digital TV and the criteria to establish switch-off deadlines were not manipulable by current national or local politicians, nor by other local interest groups. We employ a spatial regression discontinuity strategy to estimate the causal effect of the shock to bias exposure on voting behavior at regional elections in March 2010, i.e. the first elections held during the switch off process. Our main analysis uses Piedmont, the only Italian region where western towns switched to digital TV in autumn 2009, while eastern towns switched in autumn 2010.[4] Berlusconi candidate vote share dropped by 5.5 to 7.5 percentage points after the shock compared to previous elections.[5] This effect is economically and statistically significant, and it is robust to several specification and placebo tests. We scale the effect by the 2005 Berlusconi supporters and nonvoters who watched new channels in 2010 to estimate that at least 30% of them changed their voting behavior after the switch off of analog TV. We interpret this as a lower bound for the ratio of voters who do not filter out biases in political information over time, since non-Berlusconi supporters may be subject to biases toward opposition parties that our design does not capture.

We then investigate which demographics stopped supporting Berlusconi once on digital

[2]Data on ownership of new digital channels is available from *e-Media Institute* and *DGTVi*.

[3]The total number of viewers over this period was about constant (95% of Italian households). Those who did not watch old channels or new digital TV were on satellite TV, which we discuss below. Our results are not driven by higher consumption of TV (intensive margin): in the Internet Appendix, we show that the viewing share of evening news programs, which only last 30 minutes, also decreased. Since viewers cannot watch two channels at the same time, they must have moved away from news programs on Berlusconi-controlled channels and sorted into new digital channels.

[4]We provide validity of results across regions and across elections.

[5]The average (unweighted) vote share of 2005 Berlusconi candidate in Piedmont towns was 54%, or 47% if weighing by the number of voters in each town. Since Italy has a multiparty system, a candidate may win elections with less than 50% of valid votes.

TV. We find that the drop in Berlusconi's coalition vote share was higher in towns with older and least educated voters. On the contrary, treated towns with more (less) youngsters, with higher (lower) social capital, income or unemployment did not behave differently from others. A major channel through which switching to digital TV affected voting was turnout, which dropped more in towns where voters were older after the shock. In treated towns with a ratio of elderly one standard deviation above the mean, turnout dropped by one quarter of a standard deviation more, and Berlusconi candidate vote share by one third of a standard deviation more than in other treated towns.

To our knowledge, this is the first paper that documents how persuasion by a biased sender can be sustained in the long run, even if all receivers are aware of the conflict of interest of the sender. We discuss a series of plausible interpretations of these results, and argue that selective attention and persuasion bias are broadly consistent with them.

This paper falls within the literature on persuasion in economics (DellaVigna and Gentzkow, 2010), and on media and political outcomes (Prat and Stromberg, 2011). Media bias may affect rational agents who do not know when information is omitted (Besley and Prat (2006) and Anderson and MacLaren (2012)). We show that, after the switch off, viewers do not sort into alternative sources of information. Kamenica and Gentzkow (2011) derive conditions under which a biased signal persuades a Bayesian receiver to take an action favorable to the sender. In our setting, voters are systematically persuaded over time. They consistently take actions they would have not taken without bias exposure, as revealed by choices once exposure drops. DeMarzo et al. (2001) model the effect of persuasion on bounded-rational agents. Building on them, DellaVigna and Kaplan (2006) show how media bias affects Bayesian and persuasion-biased agents. The latter systematically fail to take the full extent of bias into account when updating their beliefs. In section VI, we argue that, contrary to other interpretations, persuasion bias is broadly consistent with our evidence.

On the empirical side, Gentzkow and Shapiro (2006) and Gentzkow and Shapiro (2010) argue that media slant emerges from outlets responding to audience preferences. Gentzkow et al. (2011) document an effect of newspaper openings and closings on electoral turnout, but no effect of newspaper slant on candidates' vote shares. In this paper, turnout dropped more in towns where the effect of the shock to bias exposure was stronger.

As in DellaVigna and Kaplan (2007) and Enikolopov et al. (2011), we show that media

bias affects voting behavior. We are different because we look at lower exposure to a long-lived pervasive bias, instead of higher exposure to a new biased outlet. This allow testing if persuasion can be sustained in the long run, even if the bias is systematic and every voter knows that a major party leader controls most TV channels.[6] Hence, our test, contrary to previous literature, helps to disentangle plausible explanations for why receivers are persuaded over time, as discussed in section VI. Moreover, since virtually all Italians were exposed to the bias before digital TV, we can estimate how many Italians were systematically persuaded over time, and determine which demographics were affected the most. Finally, the effect we document has the potential to change election outcomes.

This paper also relates to Durante and Knight (2012). They look at viewers' response to change in partisanship of government-controlled media once Berlusconi becomes Prime Minister. The bias towards Berlusconi increases, and some viewers sort into leftish outlets based on ideological preferences. Leftish viewers sort, but they are not responsive to the bias in the first place. On the contrary, switching to digital TV involves all viewers, including Berlusconi supporters.

Digital TV viewers sort into all-entertainment channels, but not news programs. Gentzkow (2006) and Prior (2005) show that once television and cable TV, respectively, became available to US viewers, some of them moved from news programs to entertainment programs. This reduced their political knowledge and turnout at elections. The channel we document is similar, but our effect is not entirely explained by lower knowledge or interest in politics. For this to hold, Berlusconi supporters should be more likely than others to move to new channels. But then, we should observe a larger effect in towns where historical support for Berlusconi was higher. On the contrary, the effect was larger in towns with lower historical support.[7]

Durante et al. (2013) show that towns with early exposure to Berlusconi's network, based on Italy's morphology, voted more for his party from 1994 to 2006. They argue that light entertainment content has shaped beliefs over time, making voters more attracted by Berlusconi's party. Our quasi-experiment keeps TV content constant: new digital channels broadcast light entertainment shows[8], many of which were on Berlusconi's network in the

[6] In Enikolopov et al. (2011) availability of a non-governmental outlet after 1996, NTV, affected election results in 1999. NTV became state-controlled before 2003 elections. Hence, the length of exposure to independent media was lower than seven years.

[7] This is consistent with peer effects in voters' updating of beliefs, in line with Cialdini (1984) and with the model of Murphy and Shleifer (2004).

[8] See Figure I.

1980s or later. Hence, the effect we document cannot be driven by a change in ideological cues proposed by TV.[9]

This study provides a motive for increasing competition when information sources are biased, and the bias does not consist of news omission: higher competition reduces bias exposure, and individuals debias mechanically.[10]

In the rest of the paper, Section 2 describes the institutional setting, while Section 3 shows Italian households' reaction to digital TV. In Section 4, we discuss our identification strategy. Section 5 presents empirical results and robustness. In Section 6 we interpret the magnitude of the effect, and discuss plausible interpretations. In Section 7, we look at how many and which demographics were persuaded the most over time. Section 8 investigate the debiasing mechanism, and Section 9 presents conclusions.

II Institutional Setting

Television in Italy. Italian TV has been airing on an analog infrastructure since the 1950s. The analog system consisted of seven channels airing nationwide, plus several local channels. National channels belonged to three networks: (i) Rai Radiotelevisione Italiana, the government-owned network, with three channels: Rai Uno, Rai Due and Rai Tre; (ii) Mediaset Spa (previously Fininvest Spa), owned by Berlusconi and family, with three channels: Canale 5, Italia Uno and Rete Quattro; (iii) TeleMontecarlo, a minor channel acquired in 1999 by Telecom Italia Media Spa and renamed La7. Local channels aired at the town or regional level, covering local news and often not airing for 24 hours. Frequencies were directly assigned by the government, making TV one of the most concentrated and regulated industries in the country. Rai and Mediaset alone were still attracting more than 90% of Italian viewers in the first decade of the 2000s. Given the limited penetration of satellite TV, Italian TV has been a de facto duopoly for decades: public sector Rai and Berlusconi Mediaset covered the whole supply of TV services.

[9] Moreover, there is no geographic obstacle for TV signals to reach both sides of the border we look at. Consistently, we find no difference in Berlusconi's performance from 1995 to 2005 for towns in western and eastern Piedmont, which we should find if our effect was related to Durante et al. (2013).

[10] This is different from Hong and Kacperczyk (2010), who show that competition reduces the extent of a bias to which investors are always exposed. Our setting is also complementary to Mullainathan and Shleifer (2005), since Berlusconi's channels are ideologically biased, and do not slant news towards viewers' tastes.

Berlusconi has owned three out of the seven national channels since the 1980s. Moreover, he founded and has led a major political party since 1993, being elected Prime Minister three times: from 1994 to 1995, from 2001 to 2006 and from 2008 to 2011. In those years, he controlled the Rai network and picked the main executives and news programs directors. This raises concerns that a bias exists in favor of Berlusconi's party on Italian TV. Durante and Knight (2012) document such bias, which is stronger when Berlusconi is in power. Anecdotal evidence is also consistent with a bias.[11] Berlusconi's *incredible comeback*[12] in 2013 elections provides a vivid example of the effect of biased information on voting: according to all opinion polls, including those most favorable to him, Berlusconi was trailing behind the center-left coalition by about twenty percentage points as of October 2012. Many party officials asked him to resign. Under pressure, Berlusconi announced his retirement from active politics on October 24th 2012. But due to party fights, Berlusconi announced a "truth operation" in December 2012, consisting of appearing on all TVs willing to host him to explain Italians the truth about him and his previous government. From December 24th to January 14th 2013, Berlusconi has aired for more than 28 hours, while his main opponent for about 12 hours.[13] Over the same period, Berlusconi has been seen by 395 million viewers, while his main opponent by 184 millions. Elections were held in February 2013: Berlusconi and his main opponent obtained the same number of votes. Viewers have been exposed to a bias in TV information for almost 20 years. Despite the gradual diffusion of the internet, more than 85% of Italians were relying on TV as unique or major source of political information in 2009.[14]

Digital TV. Since 2008 a new technology has been put forward: terrestrial digital TV, which dramatically enhances transmission efficiency. Digital TV uses existing analog infrastructures, avoiding the high setup costs of cable and satellite TV. Receivers owns a *decoder*, i.e. a tool similar to a modem for internet connections.[15].

[11] For instance, in 2010 *Autorità Garante per le Comunicazioni*, an independent commission supervising communications, ordered most news programs to cut the coverage of Berlusconi in favor of opponents.

[12] See *The Economist*, Feb. 15th 2013.

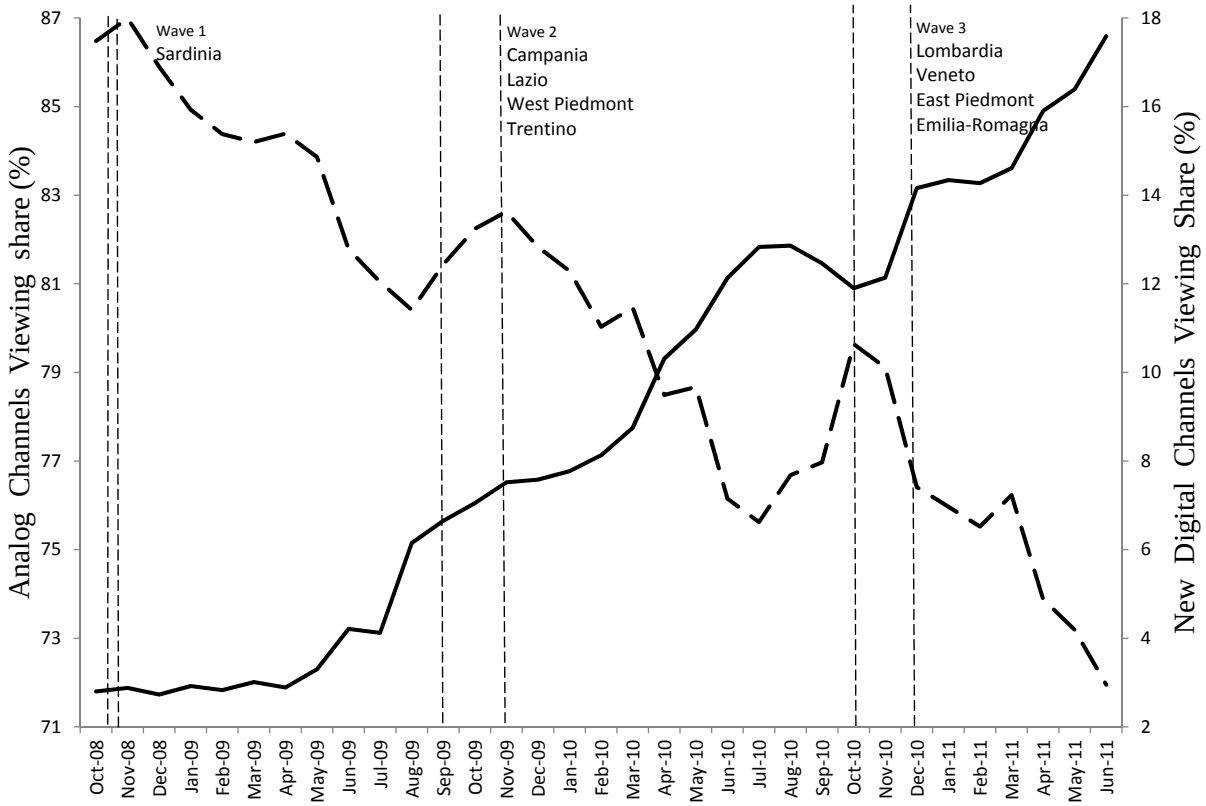
[13] See *La Stampa*, January 18th 2013, based on Auditel data elaborated by *Geca Italia*.

[14] See "8° Rapporto Censis/Ucsi sulla Comunicazione" (*www.censis.it*)

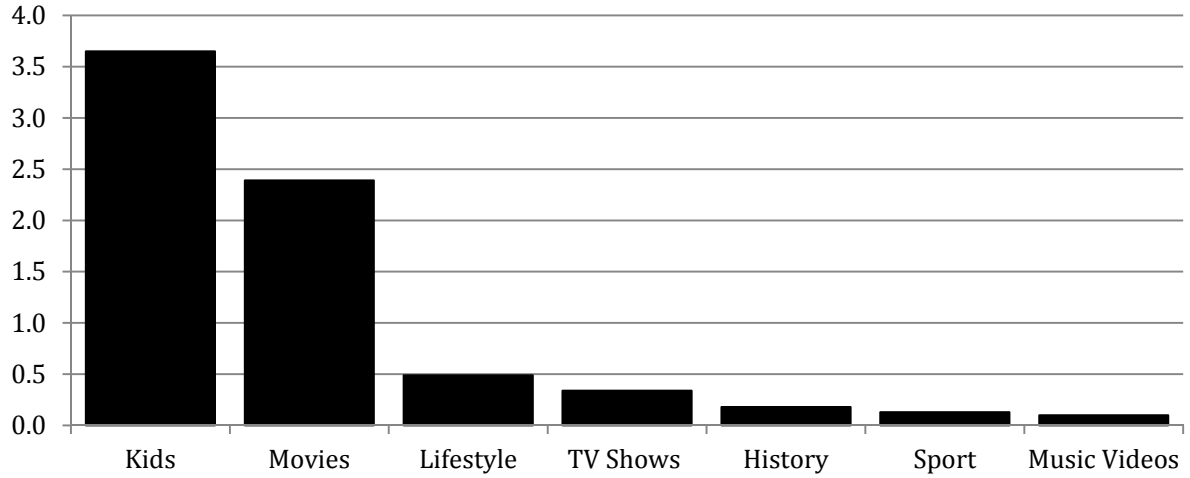
[15] Decoders could be bought for 50 Euros. Yet, to ensure anyone could go digital, the government established a 50-Euro voucher plan for households earning less than 10,000 Euros a year with one or more 65 year-old members. The central government repayed sellers upon proof of sale. These criteria could not be modified locally: the program was implemented at the national level.

Figure I: **Shock to Bias Exposure and Viewers Reaction**

A. Average daily viewing shares of Berlusconi-controlled channels (left) and new digital TV channels (right) around waves of deadlines to move to digital TV (6-8:30 pm)



B. Daily Viewing shares of new digital TV channels by content as of March 2010 (6-8:30 pm)



In Panel A, the left axis reports the average monthly viewing share of the seven national channels available both on analog and digital TV from October 2008 to June 2011 (dashed line). The right axis reports the share of new digital TV channels over the same period (solid line). Shares are the average percentage of all TV viewers who watch an analog or digital channel in the time slot 6-8:30pm, when evening news programs are aired on analog channels. "Wave 1", "Wave 2" and "Wave 3" refer to the first, second and third waves of deadlines to switch to digital TV. Areas of the coutry which moved to digital TV in each wave are enlisted. Panel B shows the average viewing shares of new digital TV channels by content for the daily time slot 6 to 8:30 pm in March 2010. Evidence of the drop in viewing shares of major news programs is in the Internet Appendix.

III Shock to Bias Exposure and Viewers Reaction

Switch to digital TV. Moving to digital TV from 2008 to 2012 has represented a major shock to the supply of TV channels in Italy: 78 new free channels are aired on digital TV at the national level, 51 of which have no ties to Berlusconi or to the governmental network [16]. In 2006, the center-left government regulated the transition from analog to digital TV, which was mandated by European Union legislation.[17] From 2008 onwards, households could switch to digital TV anytime. The act divided Italy in sixteen areas, each with an analog signal switch off date between October 2008 and July 2012. At that date, analog signals in the area were switched off, and only digital transmission was allowed. Households could have gone digital before the deadline, but were forced to do so by that day, or their TV would turn blank. Assigning switch off dates to areas, as well as the way areas were identified, was idiosyncratic to the purposes of our analysis: the criteria were the similarity of 1950s infrastructures and the homogeneous move for north, center and south of Italy.[18]. Crucially, the decision to move to digital TV, as well as the criteria for selecting deadlines and areas, could not be manipulated by national or local politicians, nor by other local interest groups.

Viewers reaction to switch. Did the switch change viewing habits of Italians? In panel A of Figure I, the left axis shows the share of viewers of Berlusconi-controlled TV channels over the period June 2008-June 2011, i.e. the latest period when Berlusconi was Prime Minister, which is associated with the dashed line. All six channels combined dropped from 84% of viewers in June 2008 to 71% in June 2011. The unreported share of viewers of the Berlusconi network alone decreased from 40% to 32% over the same period. More than 95% of Italians regularly watch TV since the 1990s (see Istat): these drops cannot be driven by new TV users attracted by digital TV. The right axis shows the share of viewers of new digital TV channels, which is associated with the solid line. This share increased from about 2% to more than 17% over the same period.

Digital TV may reduce the exposure to Berlusconi bias in three ways. First, viewers may access independent sources of news, being exposed to unbiased (or otherwise biased)

[16] Source: *e-Media Institute* and *DGTVi*. In particular, 4 channels are aired by Murdoch's News Corporation, 2 channels by Gruppo Espresso Editore, 2 channels by Cairo Editore, and 43 channels by others.

[17] See EU Directive 2007/65/EC, available at http://eur-lex.europa.eu/LexUriServ/LexUriServ.do?uri=OJ:L:2007:332:0027:01:EN:HTML.

[18] See http://www.agcom.it/default.aspx?message=viewdocument&Do cID=2708.

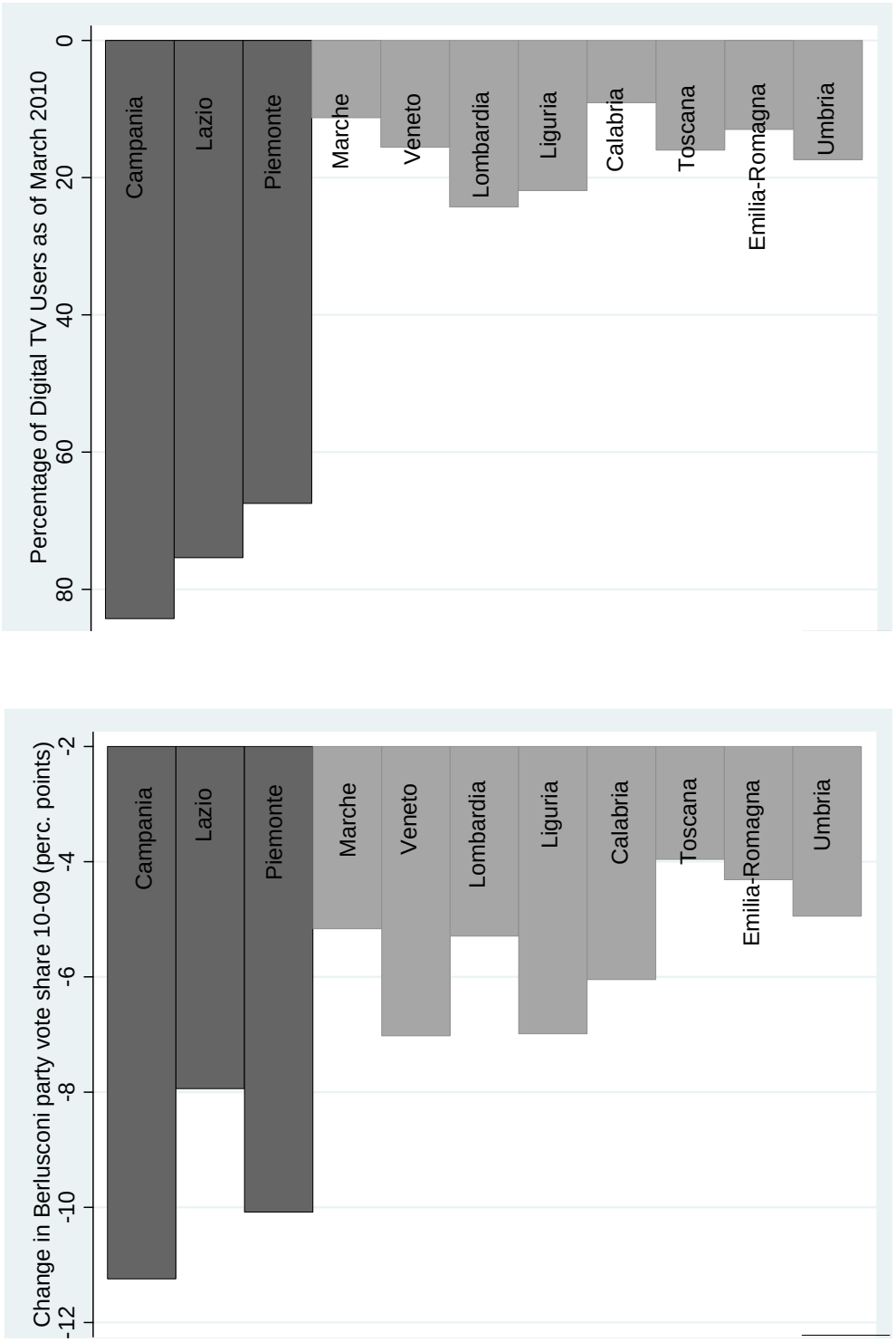
information, consistent with Mullainathan and Shleifer (2005). In the Internet Appendix, we show that the share of viewers watching digital news channels only increased from 0.2% in October 2008 to 0.8% in December 2010. Those watching all-entertainment channels soared from 1% to 11% over the same period. We also show that the move from news to entertainment channels was not paralleled by sorting into newspapers or the internet. Second, those who did not go digital by the deadline could not access any TV signals: they were incapacitated to bias exposure. The Internet Appendix shows that this channel is not relevant to our design: households watching TV at the 2009 switch date dropped temporarily, but were back at pre-switch levels in 10 weeks, i.e. before the electoral campaign started. Third, viewers may move away from news and talk shows on biased channels into all-entertainment digital channels. When doing so, their exposure to bias stops. This channel is consistent with viewing data. Panel B of Figure I plots the viewing share of new digital channels in March 2010 based on their content. Most digital users watch kid entertainment or old movies and TV shows. Viewing shares in panel B are daily averages for the slot 6:30pm to 8:30pm, when all evening news programs on biased channels are aired. In the Internet Appendix, we show that the viewing shares of the two major news programs, which last thirty minutes, have dropped by the same amount gained by new digital channels. Hence, results are not explained by an increase in the intensive margin of TV consumption, since viewers cannot watch two channels at a time. The change in habits documented above should have not affected the voting behavior of viewers who had filtered out the Berlusconi bias in information over time.

Switch off and election results across Regions. The March 2010 regional elections were the first taking place during the switch off process. Elections were held in 13 out of 20 Italian regions.[19] Three of these regions went digital before 2010: Campania, Lazio and (Western) Piedmont. All other regions were still allowing analog TV in March 2010. In the top graph of Figure II, we show the percentage of digital TV users in March 2010 by region. Dark histograms are the three all-digital regions, light histograms others.[20] The bottom graph plots the change in Berlusconi party vote share between March

[19]Five special-status regions had elections in 2008. Abruzzo and Molise had early elections in 2006 and 2008. The Puglia and Basilicata regions are excluded. In both cases, a candidate supported by Berlusconi but dismissed by local party officials ran anyway, and won more than 8.5% of votes. It is unclear who Berlusconi supporters voted for.

[20]The ratio is below 100% in Campania and Lazio because we exclude satellite TV users. After the deadline, satellite users needed a decoder to access digital TV. They could have stayed on satellite TV only, which includes all-entertainment channels, hence it is similar to digital TV for our purposes.

Figure II: **Digital TV Users and Berlusconi Party Performance across Regions**



This picture compares the ratio of digital TV users and Berlusconi Party performance across Italian Regions where elections were held in March 2010. The top graph shows the percentage of digital TV users as of March 2010 for each region. The bottom graph shows the change in Berlusconi party vote share between 2010 Regional Elections and 2009 EU Parliament elections (in percentage points). Dark histograms refer to regions which had switched to digital TV before 2010 Regional elections (Campania, Lazio and Piedmont), but after 2009 EU Parliament elections.

2010 regional elections and June 2009 EU Parliament elections. EU elections allow to observe voting behavior as close as three months before autumn 2009 switch off deadlines. Moreover, in Italy EU Parliament elections are similar to Regional Elections in terms of turnout (65.1% in 2009 EU elections as opposed to 64.2% in 2010 Regional elections) and voting procedure, since in both cases voters are allowed up to one preference for a candidate in the party list.[21] The share of votes for the Lazio region has been corrected as described in the Internet Appendix, since the Berlusconi party was excluded in Rome for administrative reasons.

Berlusconi party vote share dropped the most in all-digital regions after the switch off wave of autumn 2009.[22] If the percent change of Berlusconi party vote share is used to account for heterogeneous ideologies across regions, the differences between all-digital and other regions are even more apparent. Hence, either three idiosyncratic shocks hit all-digital regions between June 2009 and March 2010 and caused a drop in Berlusconi vote shares in those regions, they were unrelated to digital TV usage, and they did not affect any other regions, or this evidence suggests a link between digital TV usage and Berlusconi electoral performance.

IV Identification Strategy and Data

Spatial RDD. Our identification strategy is based on a quasi-random experiment: idiosyncratic deadlines to switch from analog to digital TV in Italy around the March 2010 regional elections. Near the switch off date, the probability that households are on digital TV jumps to about one. As discussed in section III, voters who switch before elections are less exposed to Berlusconi bias than voters who switch after elections. Figure III describes the natural experiment. We look at Piedmont, the only region where some towns (West, black) switched to digital TV in the autumn 2009 [23], i.e. before elections. The other towns (East, white) went digital in the autumn 2010. Switch off dates were assigned at

[21] We cannot sensibly compare 2010 and 2005 Regional Election results across regions, since region Campania governor Bassolino was hit by a corruption scandal in Autumn 2007, which brought to his official incrimination for corruption, fraud and false testimony in February 2008. The leader of his own party asked for his resignation without success, after party's support in the Region had plummeted to record low levels.

[22] Berlusconi candidates won all three regions. In Veneto, like Piedmont, Berlusconi supported a candidate from the party Lega Nord. Candidates from Lega Nord may move votes to their own party. Comparing Veneto to other non-switch off regions gives a sense of this effect, which we address in section V.

[23] Analogic TV signals were gradually switched off in West Piedmont from September 24th to October 9th 2009

the province level, i.e. governmental partitions between region and towns. The timing of the switch to digital TV by Western Piedmont households is particularly suitable to the analysis. According to survey evidence from *Itanes* (Italian National Election Studies), 37% of Italian voters had decided which candidates to vote for no earlier than a few weeks before elections in 2008, and this figure has increased from 2000 onwards. Most undecided and non-ideological voters are exactly those who are likely to be most responsive to the information bias on TV.

Being a switch off town is a deterministic and discontinuous function of distance from a one-dimensional threshold, the border between Western and Eastern Piedmont. We exploit the spatial distribution of observations by estimating the effect of moving to digital TV on voting behavior in a regression discontinuity setting.[24] We estimate variations of the following baseline regression specification:

$$\Delta Berlusconi_{10-05ipb} = \alpha + \gamma switchoff_p + X'_{pre10ip}\delta + f(distance_i) + \Phi_b + \epsilon_{ipb} \qquad (1)$$

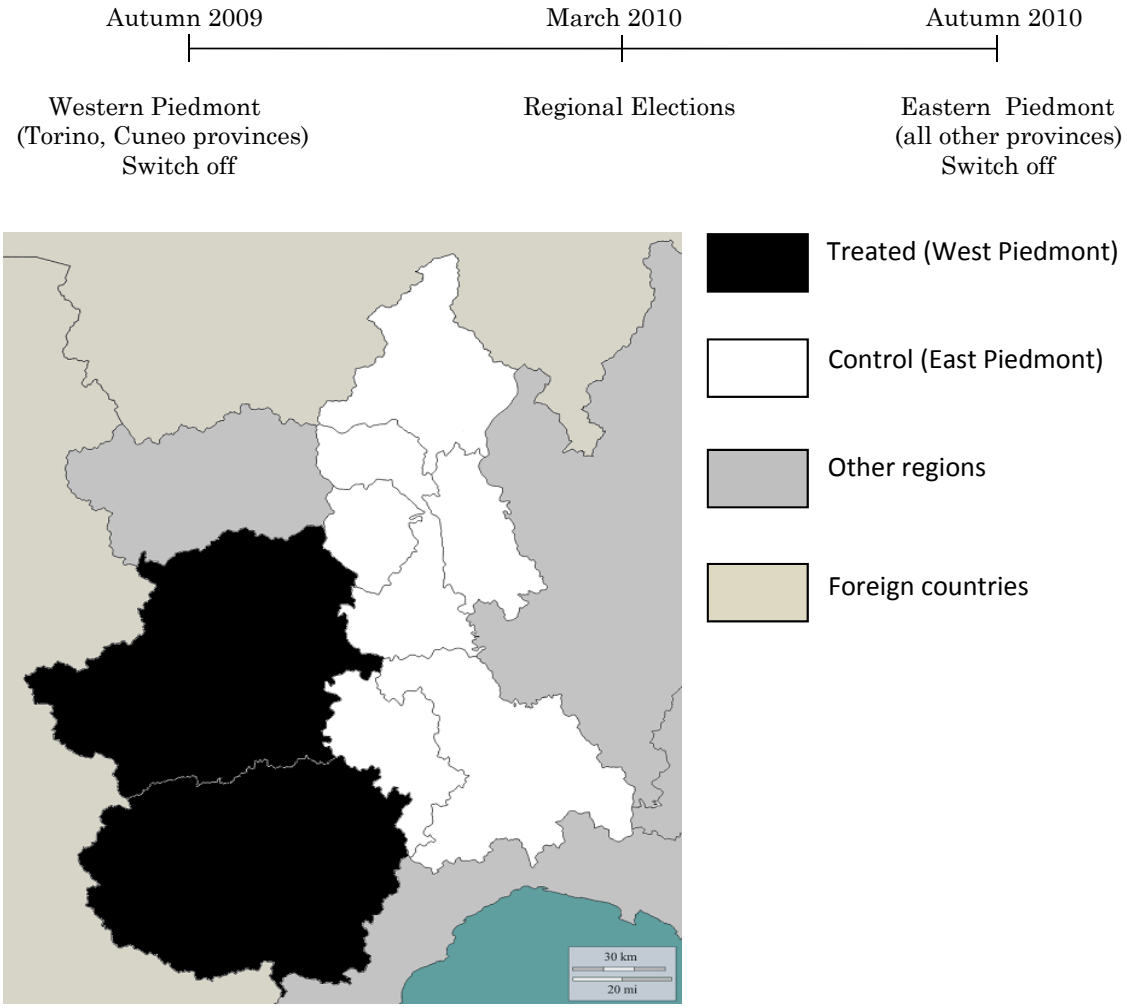
where $\Delta Berlusconi_{10-05ipb}$ is the change in Berlusconi candidate vote share between 2010 and 2005 regional elections in town i, province p along segment b of the treatment boundary, while $switchoff_p$ is an indicator which equals one if province p is in Western Piedmont, i.e. it is a treated province. $X_{pre10ip}$ is a set of town-level electoral and socio-demographic observables, expressed in differences between 2009 and 2005, or 2009 and 2001; $f(distance_i)$ is the regression discontinuity polynomial, which controls for smooth functions of the distance of town i from the border. Western towns are assigned a positive distance. Finally, Φ_b is a set of five border segment fixed effects, similar to Dell (2010), which help to average out unobserved characteristics common to towns at similar latitudes on each side of the border.[25] Identification is based on three assumptions: i) all observable and unobservable characteristics vary smoothly at the treatment border, except the treatment; ii) the estimated effects are driven by observations close to the border, where control towns are plausible counterfactuals for treated towns; iii) there is no sorting around the border. We examine the plausibility of i) in Table I, which reports summary statistics for town-level electoral and socio-demographic characteristics.[26] Each panel of Table I shows means of variables for treated (Switch) and control towns (No

[24] Close to the deadline, the probability of being on digital TV jumps to about 100% in the West.

[25] If one uses three or seven border segment f.e., the results are unaffected.

[26] In the Data Appendix, we show statistics for all covariates used in the analysis.

Figure III: **Natural Experiment: Switch to digital TV and 2010 Elections**



The graph below describes the natural experiment we exploit to identify a causal effect of forcely moving to digital TV on Berlusconi candidate electoral performance. In Autumn 2009, Western Piedmont provinces, Torino and Cuneo, switched to digital TV. They correspond to the black area in the picture. Elections were held in March 2010. The rest of Piedmont, i.e. Eastern provinces, switched to digital TV in Autumn 2010. Eastern provinces are white in the picture. Neighbor regions are dark gray, while foreign countries are light gray. [Map revised from D.Dalet, d-maps.com]

Standard errors for a paired t-test of the difference of means across groups are also reported.[27] The first panel shows statistics for the full sample, while others look at towns within 75Km, 50Km and 25Km around the border. Election outcomes include the change in Berlusconi candidates and main opponent vote shares across 2005-2000 and 2000-1995 regional elections. None of these changes are different across treated and control towns, neither for the full sample nor for towns close to the border. Socio-demographics include variables in differences and levels. Mean population in 2009 captures the size of towns before elections. It is not statistically different across treatment and control

[27]Standard errors are clustered at the province level (8 clusters). They are likely to be biased downwards. The bias goes *against* the null of no difference across conditions.

towns. However, magnitudes differ because western Piedmont includes Turin, which had more than 900,000 residents in 2009.[28] The share of employees in manufacturing and in services are similar across groups. The same holds for the change in foreign residents and income-adjusted recycling between 2009 and 2005. Both have been relevant topics in local Italian elections over the last decade.

Moving on to assumption ii), one would ideally rely on non-parametric regression discontinuity techniques using only observations very close to the border. Unfortunately, there are not enough towns around the border to do that. Hence, we identify a causal effect with a partially linear model. We include an indicator for switch off towns, i.e. a discontinuous function of distance from the border, and a smooth polynomial of the distance in the RHS of our regression model.

The third identifying assumption is that there is no sorting across the border. In our setup, moving from east to west would make no sense, since households in the East can access digital TV before their switch off date (40% of them do so). Sorting from west to east would be a problem if Berlusconi supporters in the West were willing to relocate to ensure a few more months of accessing analog TV only, whose channels are also available on digital TV, which is largely implausible.

Standard errors. In a spatial RDD framework residuals may be correlated at the treatment-control level and spatially. We correct standard errors in three ways. First, we cluster them at the province level, i.e. the level at which quasi-experimental conditions are assigned. There are 8 provinces in Piedmont: standard errors are likely biased downwards. As a finite-sample correction, we multiply the error terms by $\sqrt{\frac{C}{C-1}}$, where C is the number of clusters, to estimate the variance-covariance matrix. We also use critical values of a t-student distributed variable with $C-1$ degrees of freedom to establish test statistics significance. If we use the rule of Donald and Lang (2007), significance of estimated coefficients does not change. We alternatively account for the small number of clusters using a wild bootstrap methodology at the cluster level. Cameron and Miller (2011) show that this method is superior to other standard asymptotic tests for as low as five cluster.

[28] This is about 260 times the average population of western towns excluding Turin. We provide specifications without Turin and neighboring towns in Table III.

Table I: **Summary Statistics**

	Full Sample			< 75 km			< 50 Km			< 25 Km		
	Treated	Control	p-value	Treated	Control	p-value	Treated	Control	p-value	Treated	Control	p-value
Election outcomes												
Δ Berlusconi 05-00	-0.029	-0.035	0.761	-0.029	-0.033	0.817	-0.029	-0.032	0.879	-0.034	-0.038	0.842
Δ Berlusconi 00-95	0.177	0.150	0.493	0.176	0.145	0.422	0.168	0.145	0.521	0.161	0.154	0.871
Δ Main comp. 05-00	0.106	0.088	0.152	0.106	0.087	0.156	0.106	0.086	0.138	0.105	0.090	0.349
Δ Main comp. 00-95	0.041	0.045	0.758	0.041	0.046	0.706	0.039	0.048	0.491	0.035	0.055	0.237
Berl. Hist. Support	0.486	0.499	0.697	0.484	0.497	0.723	0.491	0.495	0.918	0.490	0.489	0.971
Socio-demographics												
Population 09	5110	2432	0.194	5261	2489	0.185	5864	2443	0.157	6994	1933	0.173
Taxable Inc. p.c. 01	9388	9452	0.915	9408	9565	0.783	9534	9556	0.972	9600	9356	0.720
% manufacturing empl.	0.122	0.127	0.836	0.125	0.130	0.819	0.134	0.140	0.808	0.133	0.132	0.982
% services empl.	0.136	0.126	0.557	0.131	0.125	0.714	0.124	0.124	0.966	0.124	0.118	0.673
Δ unemployment 10-01	0.011	0.005	0.530	0.012	0.006	0.540	0.012	0.005	0.494	0.014	0.009	0.669
Δ % foreigners 09-05	0.871	0.785	0.532	0.872	0.781	0.524	0.898	0.812	0.599	0.973	0.747	0.294
Δ recycling 09-05	0.152	0.120	0.524	0.153	0.121	0.527	0.163	0.130	0.526	0.172	0.130	0.317
Observations	565	641	1206	546	615	1161	457	471	928	287	265	552

This table reports summary statistics for observables at the Piedmont town level before 2010 regional elections. Variables are grouped into Electoral controls and Socio-demographic controls. Details about variable definitions and more summary statistics can be found in the Appendix. Each Panel reports the mean of a variable for Treated (Switch) and Control (No Switch) towns. P-values for paired t-tests of the difference of the two means are reported for each variable. Standard errors are clustered at the province level. Panels report statistics for the Full Sample, and for towns within 75 Km, 50 Km and 25 Km from the border, respectively.

Both frameworks assume that errors for towns in different provinces are uncorrelated. Hence, we also use the procedure of Conley (1999), allowing for spatial dependence of unknown form.[29]

Weighting scheme. We use votes at the town level to compute electoral outcome variables. These data are plausibly more precise in large towns (DellaVigna and Kaplan, 2007). We estimate Equation 1 using both OLS and weighted least squares, where weights are average logarithm of total voters in 2010 and 2005 elections.[30]

Baseline covariates. Although a consistent estimate of the treatment effect does not require it, we add baseline covariates to reduce the sampling variability in the estimator (see Lee and Lemieux [2010]). All covariates' descriptions, summary statistics and balancing are in the Data Appendix. Electoral controls are changes in Berlusconi candidate vote share across elections before digital TV was introduced. Data on election results are from DWSIDE (DataWarehouse Sistema Integrato Dati Elettorali) by *Osservatorio Elettorale at Consiglio Regionale del Piemonte*. We add demographic controls at the town level from the 2001 Census (Istat), i.e. the latest available before the introduction of digital TV, and more up-to-date demographics from sources described in the Data Appendix.

V Estimation Results

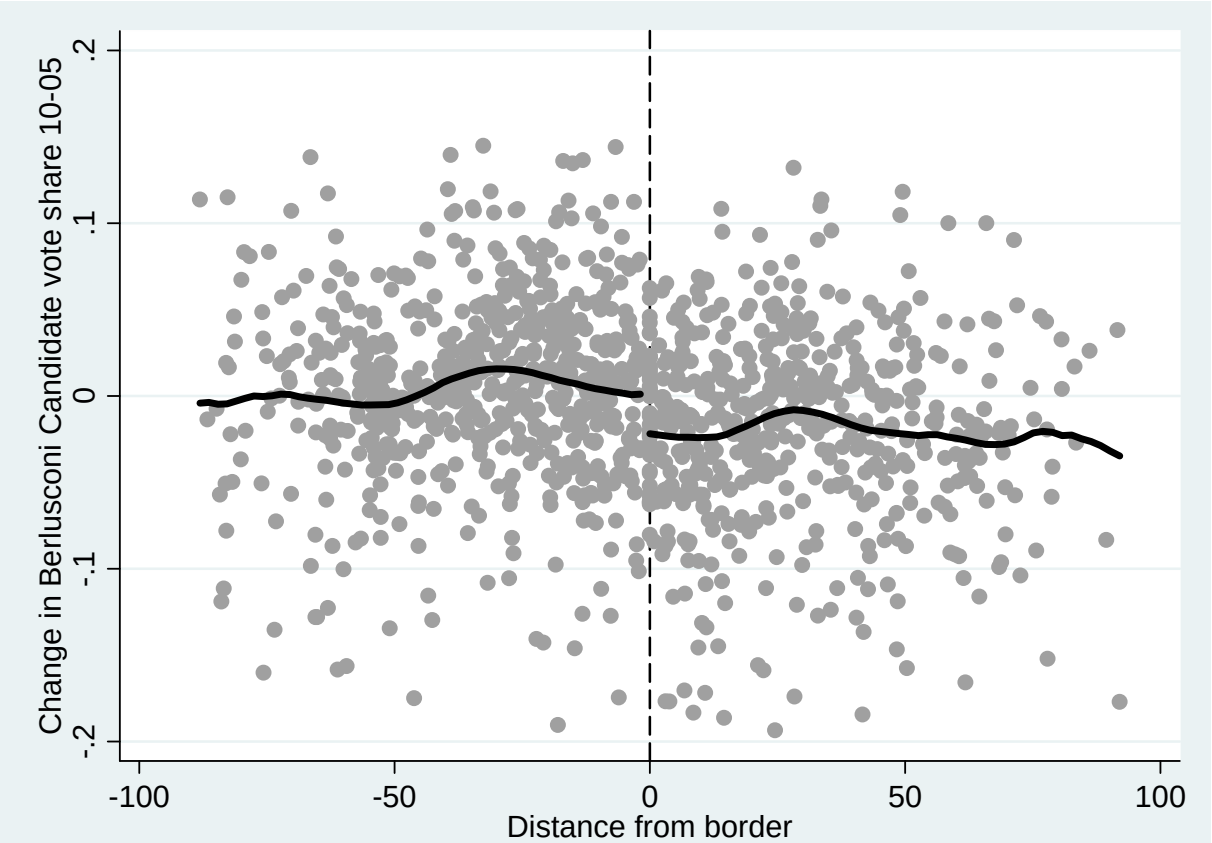
Baseline Specifications. Figure IV plots the change in Berlusconi candidate vote share in Piedmont towns between 2010 and 2005. The dashed line represents the border between Western and Eastern Piedmont. The vote share of Berlusconi candidate dropped in treated towns (positive distance) more than in control ones. Table II shows results for estimating Equation 1. In Panel A the RD polynomial is linear in distance. Columns (1) and (2) use the whole sample of Piedmont towns. In column (1), we estimate that the vote share of Berlusconi candidate in 2010 dropped by 4.7 percentage points more in the

[29] We use a bandwith of 0.25 degrees in longitude and latitude, i.e. approximately 30 Km in each dimension. This gives more conservative standard errors than other bandwiths. In particular, here it is more conservative than using one degree, as in Dell et al. (forthcoming) and Dell (2010), or 3 degrees as in Kline and Moretti (2011).

[30] Weighing by absolute number of voters would give Turin a weight close to 20%, the second-largest Western town of 1.3%, and the median Piedmont town by size of 0.06%.

West than in the East, compared to Berlusconi candidate share in 2005.[31] This effect is statistically significant when standard errors are clustered at the province level and when allowing for spatial correlation of unknown form. In column (2), more weight is given to towns with more voters, whose data are plausibly more precise. The coefficient associated with the treatment indicator, as well as computed standard errors, are very similar to those in column (1). The two specifications also have a similar R^2,

Figure IV: **Change in Berlusconi candidate performance around treatment border**



This picture plots the change of Berlusconi candidate vote share between 2010 and 2005 regional elections against the distance from the border of each Piedmont town. Distance is negative for control towns, positive for treated towns. Treated towns switched to digital TV before 2010 regional elections. Observations are trimmed at the 1-99 percentiles change in Berlusconi candidate vote share.

i.e. both models explain the same portion of variation in the dependent variable. Columns (3) to (8) only use observations close to the border. The coefficient of interest ranges between -4.8 and -5.5 percentage points for towns within 75Km and 50Km from the

[31]It would be incorrect to interpret this coefficient as the causal effect of moving to digital TV on Berlusconi candidate vote share, since 2/5 of Eastern households were accessing digital TV in March 2010. See section VI.

border. The drop in Berlusconi candidate vote share was about 6 percentage points higher in the West than in the East for towns within 25Km around the border. Results do not change if one approximates for smooth effects of distance using a third degree polynomial (see panel B). Coefficients on the indicator for treated towns range from -4.6 to -6.1 percentage points. All t-statistics are significant at the 5% level or lower. The effect is larger for towns 15 Km around the border (available upon request), with estimated coefficients ranging between -6 and -7 p.p., and standard errors between 1.2 to 1.7 p.p. We only have 6 clusters though, whose size is heavily unbalanced: two only contain 13 and 18 observations, while the largest one 119, i.e. about one third of the sample (334 observations). Reliable statistical inference is therefore hard.

Specification tests and Alternative Explanations. In Table III, we examine the robustness of results to various specifications of the empirical model and alternative explanations. Please refer to the Internet Appendix for additional results. Standard errors clustered at the province level and corrected as in Table II are reported below coefficients.[32] In panel A, we consider the complete spatial structure of observations using a cubic polynomial in longitude and latitude.[33] Magnitude of coefficients and statistical significance are similar to Table II.[34] In panel B, we propose an alternative correction of standard errors. Two towns are in the same cluster if they belong to the same province and to the same decile of the per capita regional income distribution. This increases the number of clusters: we do not need to account for the non-convergence of clusters to their asymptotic distribution. But it assumes that residuals for towns in a same province across different deciles of income per capita are uncorrelated. Since the RD polynomial is cubic coefficients and R^2 are those in Panel B of Table II. Standard errors are similar to Table II. Panel C allows for heterogeneous treatment effects adding interactions of Switch off with the cubic distance polynomial. The estimated effect is one to two percentage points lower than in previous specifications, unless we limit the analysis to towns within 25Km around the border. In panel D, we provide a difference-in-differences estimator, without exploiting the spatial dimension of the data. Some coefficients are

[32] Wild bootstrapped s.e. and spatial HAC s.e. give similar results as in Table II.

[33] Our border is a mono-dimensional discontinuity. Yet, distance is high for towns in the north east (negative) and in the south west (positive), since there is no border segment at their latitude. Modeling the longitude-latitude structure alleviates concerns that those towns have been wrongly accounted for in Table II.

[34] Longitude-latitude polynomials of degree one and two to address concerns of overfitting at the discontinuity give similar results.

smaller than those estimated in baseline specifications, but differences disappear in towns 25Km around the border. Statistical significance is unaffected. We also estimate the average treatment effect on the treated (ATT) using a nearest-neighbor matching estimator based on the propensity score.[35] Untabulated results are very similar to panel D: we estimate an ATT of -0.036 (s.e. 0.008). In the last three Panels of Table III, we test alternative explanations.

Table II: **Effect of switch to digital TV on Berlusconi Candidate vote share**

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Full Sample		< 75 km		< 50 km		< 25 km	
	A. Distance from the border							
Switch off	-0.047	-0.045	-0.050	-0.048	-0.055	-0.054	-0.063	-0.059
Clust. province	*0.009***	*0.008***	*0.006***	*0.006***	*0.009***	*0.009***	*0.011***	*0.012***
Wild bootstrap	*0.019***	*0.017***	*0.016***	*0.015***	*0.018***	*0.017***	*0.027***	*0.025***
Spatial HAC	*0.008***	*0.008***	*0.008***	*0.008***	*0.010***	*0.010***	*0.012***	*0.012***
R^2	0.397	0.394	0.396	0.395	0.424	0.418	0.437	0.433
	B. Cubic polynomial, distance from the border							
Switch off	-0.061	-0.057	-0.056	-0.054	-0.050	-0.048	-0.051	-0.046
Clust. Province	*0.012***	*0.012***	*0.011***	*0.011***	*0.013***	*0.013***	*0.009***	*0.009***
Wild bootstrap	*0.020***	*0.020***	*0.018***	*0.017***	*0.016***	*0.015***	*0.021***	*0.019***
Spatial HAC	*0.010***	*0.010***	*0.010***	*0.011***	*0.011***	*0.012***	*0.013***	*0.012***
R^2	0.404	0.399	0.398	0.397	0.425	0.419	0.441	0.439
Electoral controls	yes	yes	yes	yes	yes	yes	yes	yes
Socio-dem. controls	yes	yes	yes	yes	yes	yes	yes	yes
Border segment f.e.	yes	yes	yes	yes	yes	yes	yes	yes
Weighted LS	no	yes	no	yes	no	yes	no	yes
Observations	1,206	1,206	1,161	1,161	928	928	552	552

This Table reports results for estimating the following spatial RDD model:

$$\Delta Berlusconi_{10-05ipb} = \alpha + \gamma switchoff_p + X'_{pre10ip}\delta + f(distance_i) + \Phi_b + \epsilon_{ipb}$$

Each observation is a town in Piedmont. In Panel A the RDD polynomial in the distance of a town from the border is linear. In Panel B, it is cubic. Switch off is a dummy variable which equals one for treated towns, zero otherwise. Columns report results for the Full Sample, and for towns within 75Km, 50Km and 25Km from the border. In even columns, observations are weighted by the average of the log of voters in 2010 and 2005 elections. For each specification, three sets of standard errors are reported. Clust. province s.e. are clustered at the province level (8 clusters), and corrected as suggested by Donald and Lang (2007). Wild bootstrap s.e. follow the procedure suggested by Cameron and Miller (2011). Columns (1) to (6) are based on 900 repetitions of bootstraps, while columns (7) and (8) on 100 repetitions. Spatial HAC s.e. allow for spatial dependence of unknown form following Conley (1999). Significance is as follows: *10%, **5%, ***1%.

[35] We predict the propensity score via a logit regression of the treatment on 2005 Berlusconi candidate share, unemployment rate in 2009, newsagents p.c. in 2009, recycling over income p.c. in 2009 and share of foreign residents in 2009. Results are similar if we modify first stage controls.

Towns around Turin have a peculiar manufacturing and urban structure. In panel E, we exclude them: results are similar to panel B of Table II. Berlusconi candidate in 2010 was a member of *Lega Nord*, a long-term ally in Northern Italy. Voters in the West may know or trust this party less than others. In panel F we add a dummy which equals one if there is a branch of Lega Nord in town, and its interaction with Switch off. If voters in the west voted less for Berlusconi candidate because they knew him or his party less, the effect should be lower in towns where Lega Nord campaigned more actively. Unreported coefficients on both dummies are economically and statistically insignificant, while the main result is unaltered. Unreported results also show that the effect does not change if we exclude votes for UDC, a junior ally of Berlusconi's in 2005 but not in 2010. In panel G we exclude towns at the border or within 5 Km from it, and results are similar to panel D.

Placebo Analysis. If the effect we document in Table II is due to the switch to digital TV, we should observe no effect of being a town in Western Piedmont on Berlusconi candidate performance in earlier elections. In Table IV, column (1) and (2) estimate Equation 1 using the change in Berlusconi candidate vote share between 2005 and 2000 as a dependent variable.[36] All households were on analog TV in 2005. Being a town in the West had no effect on the change in vote share of Berlusconi candidate.[37] In columns (3) and (4), we use the change in vote share of the Berlusconi party between 2009 and 2004 EU Parliament elections as a dependent variable.[38] Since EU Parliament elections were held in June 2009, they allow to look at voting behavior just three months before the west went digital, when less than 25% of Piedmont households were on digital TV, and nine months before 2010 regional elections.[39] We find no effect of being a Western town on the change in the Berlusconi party vote share between the 2009 and 2004 EU elections.

Following Imbens and Lemieux (2008), we propose a spatial placebo analysis using artificial borders. If our interpretation of the treatment is correct, we should find no

[36]Wild bootstrap s.e. are more conservative than clustered and spatial HAC s.e. .

[37]The same holds for change in vote shares between 2000 and 1995, or 2005 and 1995 as dependent variables.

[38]In EU Parliament elections, Italians choose a party and may name a candidate from the party list. Turnout in the two types of elections is similar: 65.1% of voting age Piedmontese showed up in 2009 EU elections, 64.2% in 2010 regional elections.

[39]Moreover, Piedmont GDP was at its bottom growth in June 2009, and started to recover afterwards. This placebo allows to address concerns that differential effects of the economic crisis across the border drive the results.

effect when estimating Equation 1 using an artificial border *within* Western Piedmont and looking at Western towns only. In columns (5) and (6), we set the artificial border at 50 Km from the true one. All towns to the west of it are assigned to an artificial treatment condition, and all towns to the east (and to the west of the true border) are assigned to an artificial control condition.[40] We find no effect of being an artificially treated town on the change of the Berlusconi candidate vote share.[41] Analogously in columns (7) and (8) we only consider towns in Eastern Piedmont, and set an artificial border at 50 Km from the real border. Again, we find no effect of being in the artificial treatment group.

External Validity. All results so far refer to Piedmont. We estimate the effect of digital TV in an inter-regional setting which allows us to exploit idiosyncratic deadlines to switch to digital TV. We compare towns in the province of Cuneo (Piedmont), which went digital before 2010 Elections, to towns in the provinces of Imperia and Savona, in the neighboring region Liguria, where voters could still access analog TV but had to choose among different Presidential candidates in 2010.[42] The border divides Northern (Piedmont) and Southern (Liguria) towns; northern towns are the treated group. Untabulated summary statistics show that towns in the two regions differ significantly under several dimensions. We therefore run a diff-in-diff estimation, whose specification is similar to Equation 1, but without controlling for distance from the border. The change in vote shares is more negative in Piedmont towns, both statistically and economically, ranging from -2.5 to -4.7 percentage points. We depict this effect in the Internet Appendix. Pooling the Liguria provinces and eastern Piedmont as a unique control group gives similar results. In the Internet Appendix, we show that results survive across elections. We analyze the link between digital TV usage and Berlusconi candidates performances in the 2011 and 2006 province elections. In all provinces where analog TV was still available in 2011, Berlusconi candidates did not lose votes compared to 2006. In all but one province that had switched to digital TV before the 2011 elections, Berlusconi candidates obtained lower vote shares than in 2006. The tests above exhaust all applications of our identification strategy. Towns within other regions moved to digital TV at the same time, hence they cannot provide more information

[40] Results do not change if we set the artificial border is set at the median distance from the true border in both directions.

[41] We only have two provinces here, so clustering at the province level is not sensible. The coefficient in column (6), panel B is not small in magnitude. However, the related statistic is insignificant.

[42] This also addresses the concern that the Berlusconi candidate in Piedmont in 2010, Roberto Cota, is a native of eastern Piedmont, while his main competitor is from the west.

Table III: **Specification Tests and Robustness**

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Full Sample		< 75 km		< 50 km		< 25 km	
A. Cubic polynomial, longitude and latitude								
Switch off	-0.060	-0.057	-0.056	-0.055	-0.049	-0.048	-0.063	-0.062
Clust. province	0.013***	0.012***	0.011***	0.011***	0.011***	0.011***	0.008***	0.008***
R^2	0.425	0.422	0.406	0.406	0.428	0.425	0.455	0.451
B. Clustering by province*average income decile								
Switch off	-0.061	-0.057	-0.056	-0.054	-0.050	-0.048	-0.051	-0.046
s.e.	0.011***	0.011***	0.011***	0.011***	0.013***	0.012***	0.016***	0.014***
N. of clusters	77	77	77	77	69	69	58	58
R^2	0.404	0.399	0.398	0.397	0.425	0.419	0.441	0.439
C. Heterogeneous Treatment Effects								
Switch off	-0.038	-0.035	-0.035	-0.030	-0.043	-0.038	-0.057	-0.052
Clust. province	0.010***	0.008***	0.010**	0.009**	0.014**	0.013**	0.020**	0.020*
R^2	0.409	0.406	0.403	0.404	0.429	0.425	0.444	0.444
D. OLS specifications								
Switch off	-0.034	-0.033	-0.032	-0.032	-0.033	-0.032	-0.053	-0.050
Clust. Province	0.009***	0.008***	0.008***	0.008***	0.008***	0.009***	0.006***	0.005***
R^2	0.392	0.389	0.387	0.386	0.413	0.406	0.435	0.431
E. Excluding Turin and neighboring towns								
Switch off	-0.061	-0.057	-0.056	-0.053	-0.049	-0.047	-0.051	-0.047
Clust. Province	0.012***	0.012***	0.011***	0.012***	0.013***	0.013***	0.009***	0.008***
R^2	0.403	0.399	0.398	0.397	0.425	0.419	0.440	0.437
Observations	1,194	1,194	1,149	1,149	916	916	544	544
F. Lega effect								
Switch off	-0.061	-0.057	-0.056	-0.054	-0.051	-0.049	-0.053	-0.050
Clust. Province	0.012***	0.012***	0.011***	0.011***	0.013***	0.013***	0.010***	0.009***
R^2	0.404	0.399	0.398	0.397	0.425	0.419	0.443	0.441
G. Excluding towns close to border (OLS specifications)								
Switch off	-0.030	-0.028	-0.029	-0.027	-0.030	-0.028	-0.056	-0.053
Clust. Province	0.010**	0.009**	0.010**	0.009**	0.011**	0.010**	0.013***	0.012***
R^2	0.408	0.407	0.405	0.406	0.439	0.432	0.465	0.466
Observations	1,120	1,120	1,075	1,075	842	842	466	466
Electoral controls	yes	yes	yes	yes	yes	yes	yes	yes
Socio-dem. controls	yes	yes	yes	yes	yes	yes	yes	yes
Border segment f.e.	yes	yes	yes	yes	yes	yes	yes	yes
Weighted LS	no	yes	no	yes	no	yes	no	yes
Observations	1,206	1,206	1,161	1,161	928	928	522	522

This Table reports results for estimating variations of the following spatial RDD model:

$$\Delta Berlusconi_{10-05ipb} = \alpha + \gamma switchoff_p + X'_{pre10ip}\delta + f(distance_i) + \Phi_b + \epsilon_{ipb}$$

Each observation is a town in Piedmont. Switch off is a dummy variable which equals one for treated towns, zero otherwise. Columns report results for the Full Sample, and for towns within 75Km, 50Km and 25Km from the border. In even columns, observations are weighted by the average of the log of voters in 2010 and 2005. Except for Panel B, standard errors are clustered at the province level (8 clusters), and corrected as in Donald and Lang (2007) for downward bias. Significance is as follows: *10%, **5%, ***1%.

than in Figure II. Recent 2013 elections do not help either: all Italian towns are on digital TV since Autumn 2012.[43].

[43]Moving eastern households do not help either. Migration from biased channels in the west may have continued after 2009, keeping our effect unchanged. But digital TV may have reached its maximal viewing share in the West by March 2010, i.e. our effect may have reversed in 2013.

Table IV: **Placebo Analysis**

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Δ Berlusconi 05-00		Δ EU Parl. 09-04		Placebo border W		Placebo border E	
A. Distance from the border								
Switch off	0.008	0.011	0.011	0.013	-0.016	-0.007	0.017	0.008
Cluster prov.	*0.011*	*0.010*	*0.008*	*0.014*				
Spatial HAC	*0.010*	*0.013*	*0.007*	*0.009*	*0.007*	*0.009*	*0.010**	*0.009*
.								
R^2	0.195	0.226	0.124	0.132	0.454	0.554	0.378	0.460
B. Cubic polynomial, distance from the border								
Switch off	0.005	0.009	0.012	0.017	-0.007	-0.024	0.002	-0.005
Cluster prov.	*0.014*	*0.017*	*0.016*	*0.018*				
Spatial HAC	*0.013*	*0.017*	*0.008*	*0.010**	*0.010*	*0.014**	*0.010*	*0.011*
.								
R^2	0.199	0.233	0.135	0.134	0.458	0.561	0.386	0.464
Electoral controls	yes	yes	yes	yes	yes	yes	yes	yes
Socio-dem. controls	yes	yes	yes	yes	yes	yes	yes	yes
Border segment f.e.	yes	yes	yes	yes	no	no	no	no
Half dist. border	no	yes	no	yes	no	yes	no	yes
Observations	1,206	928	1,206	928	565	259	641	350

This Table reports results for estimating variations of the following spatial RDD model:

$$\Delta Placebo\_var = \alpha + \gamma switchoff_p + X'_{pre10ip}\delta + f(distance_i) + \Phi_b + \epsilon_{ipb}$$

Each observation is a town in Piedmont. In columns (1) and (2), the dependent variable is the change in Berlusconi candidate vote share between 2005 and 2000. In columns (3) and (4), it is the change in Berlusconi party vote share between 2009 and 2004 European Parliament Elections. Columns (5) to (8) test for placebo effects of switching to digital TV on the 2010-2005 change in Berlusconi candidate vote share within treatment ((5) and (6)) and control ((7) and (8)) groups, as suggested in Imbens and Lemieux (2008). Switch off is a dummy variable which equals one for treated towns, zero otherwise. In all columns, observations are weighted by the average of the log of voters in 2010 and 2005. In even columns the analysis is limited to towns within 50Km from the border in both directions. Cluster prov. standard errors are clustered at the province level (8 clusters), and corrected as in Donald and Lang (2007) for downward bias. Spatial HAC s.e. allow for spatial dependence of unknown form following Conley (1999). Significance is as follows: *10%, **5%, ***1%.

VI Interpretations

Magnitude of the effect. The probability of accessing digital TV in Western Piedmont was close to one in March 2010, but about 40% of households in the East were accessing it.[44] Ee divide estimated coefficients by the difference in the probability of being exposed to the treatment across conditions, i.e. 0.6.[45] To estimate a lower and an upper bound, we use coefficients in the WLS model using the full sample with linear distance polynomial (Table 2, panel A, column (2)) and for the plain diff-in-diff estimator (Table 3, panel D, column (2)), i.e. -4.5 and -3.3 percentage points, respectively. Moving to digital TV caused a drop of Berlusconi candidate vote share between 5.5 to 7.5 percentage points. Assuming a homogeneous effect had all Eastern viewers moved to digital TV before the elections, the Berlusconi candidate vote share would have dropped by an additional 1.4 to 1.9 percentage points. In fact, he won by a margin of 0.46 percentage points over his main opponent. Thus, the effect we document has the potential to change election results.

The magnitude of the effect is in line with Enikolopov et al. (2011). The availability of an independent media outlet that three quarters of the Russians accessed decreased votes for the government party by 8.9 percentage points. If one assumes a homogenous effect on all Russian voters had the network been available to everyone, the overall drop would rise to 12 percentage points, which includes voters who filter out the bias over time.

Interpretations. We now assess a series of plausible candidate interpretations for results in Table II.

Negative news omission. The bias towards Berlusconi may consist in omitting negative information about him or his party. This would drive the results if, after moving to digital TV, individuals sorted into new sources of information. In the Internet Appendix, we show that individuals who move away from evening news programs, clustered in the daily time slot 6-8:30pm, sorted into all-entertainment digital channels, as graphically emphasized by panel B of section III. We also show that after going digital, treated individuals did

[44] Auditel March 2010 bulletin states that 19% of Piedmontese households were on analog TV in March 2010. They were in the east, since analog signal was unavailable in the west. About one third of Piedmontese live in the east. Hence, about 40% of Eastern households had moved to digital TV before March 2010.

[45] Coefficients should be divided by the difference in the limit of this probability when distance approaches zero from both sides. We assume that digital TV is homogenously diffused in the east. We have no data to infer the ratio of households accessing digital TV at the province level. Untabulated statistics for plausible predictors of switching before the deadline, such as the ratio of elderly, the ratio of youngsters, the ratio of college educated inhabitants, and the historical support for Berlusconi party, do not differ substantially across Eastern provinces.

not sort into other sources of information.[46]

Agenda setting. Italian news programs last thirty minutes. Directors decide which information to emphasize more or less. For the U.S., Larcinese et al. (2011) show that left-leaning outlets do not emphasize unemployment figures when Democrats are in power. We are not aware of systematic evidence of such behavior in Italy. Yet, Berlusconi was Prime minister in 2010: directors of biased news programs may not have emphasized the rise in unemployment before elections. But then Berlusconi should have lost more votes in treated towns where unemployment increased the most before elections. This is because voters there had no need to hear about unemployment on TV to know it was rising. In the Internet Appendix, we show that this was not the case.[47]

Rational Inattention. TV may be the sole means reminding voters about upcoming elections. Once viewers stop watching the news, they may not want to pay the cost of learning the election date from non-TV sources. This could drive the results if Berlusconi supporters were more likely to sort into new digital channels than others. Under this interpretation, the drop in Berlusconi share should be lower where the cost of learning election dates from non-TV sources is lower. But the effect is not smaller in towns with more newsagents per capita,[48] with a local office of the Berlusconi candidate party in town, or where demographics that access the internet more often abound (see Table V).

Campaign Advertising on digital TV. Due to the large number of new channels on digital TV, the costs of TV advertising may drop. Opposition parties or local interest groups may run aggressive anti-Berlusconi advertisements on new digital channels more easily. But in Italy, political ads have been banned since 1999.

Overlapping generations. New generations may start to be exposed to the bias *and* to take part to collective decision-making over time. But voting in Italy is allowed to all

[46] As for newspapers, the average number of purchased and freely-distributed daily newspapers per hundred inhabitants decreased *more* in Western provinces than Eastern provinces in 2009 and 2010, as compared to 2008. This is also true for (unreported) average number of purchased and freely-distributed weekly publications per capita. Hence, our results cannot be explained by Western households sorting into newspapers more than Eastern households after the switch off. As for the internet, we show that Google SVI for searches for the two major Italian newspapers, and the most read Piedmont-based newspaper, do not differ across a western (Turin) and an eastern (Alessandria) provinces which had enough users to compute the index in 2010. The same is true for unreported indices for searches of other terms related to elections, such as the Italian for *elections* and *candidates*, the surnames of Piedmont regional election candidates, the surname of national political leaders and party names.

[47] Immigration and crime were also major political campaign topics. Change in the Berlusconi candidate vote share is uncorrelated with change in percentage of immigrants and absolute number of immigrants from 2005 to 2009.

[48] In Italy, newsagents display a summary of captions on the street. Looking at them would remind individuals that elections are upcoming.

citizens 18 years old or older. Anyone who voted in 2010 had been exposed to the bias for 16 years. Conversely, no one born after 1993, when Berlusconi created his party, voted in 2010.[49]

Change in preferences. New digital channels may have directly affected the political preferences of viewers. All-entertainment channels may air ideological cues that individuals were not exposed to on analog TV. Panel B of section III plots viewing shares of new digital TV channels by content. The two most relevant categories, entertainment for kids and movies/shows, replicate old material 24/7, most of which had aired on Berlusconi's network in the 1980s or later. Berlusconi's TV itself may have shaped viewers' political preferences towards his own ideology over the years (see Durante et al. (2013)). But since digital channels only have light entertainment content, our design disentangles the effect of information from that of ideological cues on TV. This interpretation is similar to the drop in interest for politics of viewers unexposed to news in Gentzkow (2006) and Prior (2005), which would explain our results if Berlusconi supporters sorted into digital TV more than others. But then, we should expect a larger effect in towns with higher historical support for Berlusconi. The effect we document is stronger in towns with *lower* historical support (see section VI).

Coarse Thinking. Following the intuition of Mullainathan et al. (2008), individuals over the years may have unconsciously associated good feelings from watching TV shows with Berlusconi, who was extensively covered in news programs. This is only consistent with our results if individuals were affected by limited memory. Otherwise, once exposed to the same shows as in the past they would recall the association with Berlusconi and would still support him.

Selective Attention. Schwartzstein (2012) proposes a model of selective attention to freely available information which can produce persistently biased beliefs consistent with our evidence: some voters may only attend to political information during the electoral campaign. Note that in the model, after an attention shock, agents need time to learn which sources of information to attend to. Hence, the debiasing process should be slow. Our evidence shows that voting behavior changed no later than five months after the shock.

Persuasion Bias. DellaVigna and Kaplan (2006) show how Bayesian and persuasion-biased agents form beliefs when exposed to systematically biased information. Over time,

[49]This explanation is also inconsistent with the larger effect detected in older towns (see Table V).

Bayesian agents are exposed to a high enough number of signals to filter out the bias in full. Persuasion-biased agents systematically fail to take the extent of bias into account: they will never filter it out in full. Our design can be interpreted as a test for persuasion bias, which was not possible in DellaVigna and Kaplan (2006) or (Enikolopov et al., 2011) due to the shorter exposure to biased information in their settings. The evidence is consistent with the existence of persuasion-biased agents: while exposed to the bias, these agents are systematically convinced to vote for Berlusconi. Once exposure drops, they are not persuaded to go and vote for him anymore. This process needs not be conscious: viewers need not realize they were exposed to a bias to change their behavior after a drop in exposure.

VII Who is systematically persuaded?

Demographics and information processing. We investigate which demographics are persuaded the most over time. Two possibly unrelated dimensions seem relevant: the extent of bias exposure before the shock, and potential cognitive biases. In the Internet Appendix, we show that Italians aged 60 or higher are more likely than younger groups to watch TV every day.[50] As for cognition, aging of the brain has been shown to worsen cognitive abilities.[51] We test the hypothesis that the elderly are less likely to filter out biases over time.[52] Piedmont towns are sorted by the ratio of individuals aged 64 or higher over the whole population. We look at the interaction between being in a treated town and in a town at the top of the elderly distribution. Table V shows that in treated towns whith the highest ratio of elderly, Berlusconi candidate vote share dropped by 2.3 percentage points more than in other treated towns.

As a placebo corroboration, we sort towns by the ratio of people aged 16 to 24 over total population. Young voters are not more exposed to TV than others (see Internet Appendix), and have no different cognitive abilities than non-elderly age groups. In columns (3) and (4) of Table V, we find no differential effect in towns with high or low

[50] We also show that individuals aged 64 or higher are *not* more likely than other voting age groups to read newspapers at least once a week, or to listen to the radio.

[51] see Craik and Salthouse (2008) for a review. Gabaix et al. (2009) study the implications of biases on elderly economic decision-making.

[52] Elderly might watch different light entertainment contents than other demographics, such as soap operas instead of movies. But our argument only requires that those on digital TV watch light entertainment contents similar to what they used to watch on analog TV.

ratios of young voters compared to other treated towns.[53]

To disentangle the extent of bias exposure from cognitive abilities, we look at education. High and low education people do not differ in terms of time of TV exposure.[54] But less educated individuals may have lower cognitive abilities. In columns (5) and (6) of Table V, we show that in towns with least educated individuals the effect of moving to digital TV was 1.8 to 2.1 percentage points larger than in other towns. This effect is less statistically robust than for the elderly, but the magnitudes are similar. The dummies for high percentage of elderly and low percentage of educated people are not highly correlated $(0.1512, p < 1\%)$. We look at the double interaction between the ratio of elderly and the ratio of college educated individuals in a town. One should expect the effect to be stronger in towns with the most elderly *and* least educated voters than in other treated towns. Consistently, untabulated results show that in those towns the negative effect was 5.7 percentage points larger than in corresponding control towns (s.e. 0.013), on top of a baseline effect of -2.5 percentage points in other treated towns.[55] We interpret the mediating role of age and education as evidence that cognitive abilities are important to explain why individuals can be systematically persuaded over time.

Since 2008 Berlusconi has been involved in sexual scandals. Support may drop if voters are sensitive to them. Some demographics, such as the elderly, might be more sensitive than others. To test if this channel explains our results, we look at social capital. The higher the social capital in a town, the more people are concerned with ethical behavior of politicians, and the less likely they would support Berlusconi after 2008. In columns (7) and (8) of Table V, we use the ratio of individuals employed in non-profit organizations to proxy for social capital at the town level. This measure is not available for all Piedmont towns. Towns at the top or bottom of the social capital distribution did not behave differently than others.

[53]Youngsters use the internet more than others. This affected elections outcomes in Piedmont, since towns with higher access to the internet also voted more for the candidate of a novel party, Movimento 5 Stelle, whose campaign was mainly run on the internet. The non-result for younger treated towns is thus inconsistent with an information interpretation based on internet access or with overlapping generations of voters.

[54]We do not have local data on differential contents accessed by viewers based on education. This is not relevant to our interpretation though, since we only need that an individual accesses similar contents before and after the switch.

[55]These findings are not at odds with Enikolopov et al. (2011), who document no demographic mediation of their effect. In the short run, all viewers may react to a bias. We should expect no significant mediating role of demographics. In the long run, though, we should find mediating effects of demographics if certain groups are more likely to be systematically persuaded than others.

Table V: **Interaction Effects**

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	Elderly		Youngsters		Education		Social Capital		Hist. support	
Switch off	-0.049	-0.042	-0.056	-0.046	-0.047	-0.036	-0.047	-0.037	-0.053	-0.043
Clust. Province	*0.011***	*0.011***	*0.009***	*0.013***	*0.015***	*0.013***	*0.011***	*0.012***	*0.012***	*0.009****
Wild Bootstrap	*0.016***	*0.013***	*0.019***	*0.024**	*0.019***	*0.018**	*0.018***	*0.020**	*0.021***	*0.024**
Switch off*Top 3	-0.023	-0.022	0.004	-0.001	-0.006	-0.009	-0.007	-0.006	0.004	0.005
Clust. Province	*0.005***	*0.006***	*0.005*	*0.004*	*0.007*	*0.007*	*0.006*	*0.006*	*0.006*	*0.009*
Wild Bootstrap	*0.008***	*0.007***	*0.005*	*0.008*	*0.008*	*0.009*	*0.007*	*0.008*	*0.005*	*0.011*
Switch off*Bottom 3	0.002	0.004	-0.008	-0.009	-0.018	-0.021	-0.010	-0.012	-0.014	-0.010
Clust. Province	*0.004*	*0.008*	*0.012*	*0.011*	*0.008**	*0.009**	*0.006*	*0.007*	*0.003****	*0.006*
Wild Bootstrap	*0.006*	*0.006*	*0.013*	*0.014*	*0.009***	*0.013*	*0.007*	*0.008*	*0.006***	*0.006*
T3, B3, ratio	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes
Electoral controls	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes
Socio-dem. controls	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes
Border segment f.e.	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes
< 50 km	no	yes	no	yes	no	yes	no	yes	no	yes
Observations	1,206	928	1,206	928	1,206	928	1,178	905	1,206	928
R^2	0.413	0.428	0.404	0.421	0.405	0.429	0.395	0.419	0.397	0.422

This Table reports results for estimating variations of the following spatial RDD model:

$$\Delta Berlusconi_{10-05ipb} = \alpha + \gamma switchoff_p + \gamma_1 switchoff_p \times Topthird + \gamma_2 switchoff_p \times Bottomthird + X'_{pre10ip}\delta + f(distance_i) + \Phi_b + \epsilon_{ipb}$$

Each observation is a town in Piedmont. Switch off is a dummy variable which equals one for treated towns, zero otherwise. Columns (1) and (2) focus on interactions of treatment effect with the ratio of elderly in a town, i.e. the ratio of individuals aged 61 or higher over the total population. Switchoff*Top 3 is one if a town is in the treatment group and above the top tercile of towns sorted by ratio of elderly, zero otherwise. Switchoff*Low is one if a town is in the treatment group and below the lowest tercile of towns sorted by ratio of elderly, zero otherwise. Columns (3) and (4) repeat the same exercise for the ratio of youngsters, i.e. individuals aged between 16 and 24. Columns (5) and (6) repeat it for the ratio of individuals who hold a graduate degree over total population. Columns (7) and (8) repeat it for the ratio of individuals who are employed in not-for-profit companies over total population. Columns (9) and (10) repeat it for the average of Berlusconi candidate vote share in 2005, 2000 and 1995 regional elections. Odd columns report results for the full sample. In even columns the analysis is limited to towns within 50Km from the border. In all columns, observations are weighted by the average of the log of voters in 2010 and 2005. Cluster prov. standard errors are clustered at the province level (8 clusters), and corrected as in Donald and Lang (2007) for downward bias. Wild bootstrap s.e. follow the procedure suggested by Cameron and Miller (2011). Significance is as follows: *10%, **5%, ***1%.

(Non)results are robust to using alternative proxies for social capital: the number of non-profit organizations in a town (Guiso et al., 2008), a dummy equal to one if a blood donation station exists in town (Guiso et al., 2004), and the change in recycling over per capita income from 2005 to 2009.

Historical support. Finally, we look at the historical support for Berlusconi. If Berlusconi supporters were more likely to sort into new digital channels, one should detect a larger effect in towns with high historical support. Historical support is the average vote share of the Berlusconi party in 1995, 2000 and 2005 Piedmont regional elections. Towns with high historical support behaved like the others. The effect was 1.4 percentage points larger in towns where historical support was low.[56] Our interpretation is that in towns where support for Berlusconi was low, 2005 supporters were more likely to interact with non-2005 supporters after the shock. Hearing from them may have added to lower bias exposure in changing beliefs about the Berlusconi party.

In the Internet Appendix, we run a set of placebo interactions tests: we estimate specifications as in Table V using seven alternative dependent variables.[57] Towns at the top or bottom of the distribution based on any of them did not behave differently than others.

How many agents are systematically persuaded? 95% of Italians were exposed to the Berlusconi bias before the introduction of digital TV. How many of them were systematically persuaded over time? We compute the *dissuasion rate*, i.e. the share of people who, after moving to new digital channels, were dissuaded from voting for Berlusconi candidate in 2010.[58] The dissuasion rate can be interpreted as a lower bound for the fraction of voters who are systematically persuaded.[59]

The 2010 vote share of Berlusconi candidate, b_j, with $j = S, \bar{S}$ for treated and untreated towns, respectively, is defined as follows:

$$b_j = \frac{b - (1 - p - o)d_j f}{t_{05} - b d_j f + (1 - t_{05}) d_j f}$$

[56] Statistical significance is not detected for towns closer to the border.

[57] We look at income p.c., unemployment rate, TV subscriptions per household (intensity of TV usage), number of farms per capita (rural areas), number of chemist shops per capita (level of services), percentage of kids below age 6 who attend kindergarten (working mothers), and average tax rate.

[58] Similar to the persuasion rate in DellaVigna and Gentzkow (2010), this rate estimates the ratio of people less exposed to a bias who are persuaded to change their behavior.

[59] It is a lower bound because non-Berlusconi voters may be persuaded by other biases we do not capture in our analysis.

where b, p and o are the 2005 vote share of Berlusconi candidate, main opponent and other parties, respectively; d_j is the share of voters who went digital before 2010 elections; t_{05} is the 2005 turnout, and f is the dissuasion rate. The numerator includes Berlusconi voters in 2005 minus those who were not voting against him in 2005, who went digital and were dissuaded from voting for him in 2010.[60] The denominator accounts for changes in turnout: 2005 turnout t_{05} is corrected by subtracting those who voted for Berlusconi in 2005 but were dissuaded to vote for him in 2010, and by adding 2005 nonvoters who voted against him after lower bias exposure. We solve for the difference in Berlusconi candidate vote share between switch off and no-switch off towns, $b_S - b_{\bar{S}}$ and isolate the dissuasion rate:

$$f = \frac{b_S - b_{\bar{S}}}{(d_{\bar{S}} - d_S)(b + nv)} \times \frac{(b + nv)t_S t_{\bar{S}}}{b(1-b) + nv(1-nv)}$$

where $nv = 1 - t_{05}$ is the ratio of non-voters in 2005. The interpretation of f carries on from DellaVigna and Kaplan (2007), despite a different correction term, since we do not exclude third party voters and we have opposite effects on the turnout of 2005 Berlusconi supporters and nonvoters. The first term scales the change in Berlusconi candidate vote share across treatment and control areas by the difference in the ratio of individuals exposed to digital TV times the population at risk of dissuasion, i.e. 2005 Berlusconi supporters and 2005 nonvoters. The second term corrects for changes in the turnout after the move to digital TV. We estimate f using results from section V and Auditel audience data. For $b_S - b_{\bar{S}}$, we use a high and low bound from the estimations of $\hat{\gamma}$ in Table II, panel A, column (2), which refers to the linear distance polynomial when observations are weighted, and in Table III, panel D, which refers to the difference-in-difference specification: $b_S - b_{\bar{S}} \in (-0.045, -0.033)$; t_S and $t_{\bar{S}}$ are the average turnout in switch off and non-switch off provinces (0.658 and 0.621, respectively); b and nv equal 0.4710 and 0.2863, respectively. We compute national audience for new digital channels by summing up average daily audience rates for each channel in March 2010, which is 0.093. This is an average including areas which were not fully digital in 2010. To get the audience of new digital TV channels among digital users, we compute the number of households who watched new digital channels in Italy in March 2010, and divide it by the number of

[60] For 2005 nonvoters, dissuasion should be interpreted as voting against Berlusconi in 2010.

households who had access to digital TV in March 2010: $\alpha = \frac{0.093 \times 56,388}{31,646} = 0.1657$. Since we do not observe audience data at the regional level, we assume that the share of digital users who watch new digital channels in Piedmont is the same as the national share. The whole switch off area population has access to digital TV before 2010 elections, which is two thirds of Piedmont population, while only 2/5 of eastern households do. Hence, $d_{\bar{S}} - d_S = (2/5 - 1)a = -0.0994$. Eventually, we estimate that the dissuasion rate is $f \in (29.9\%, 40.8\%)$.

At least 30% of those less exposed to Berlusconi bias were dissuaded from voting for his candidate in 2010.[61] This implies that at least 30% of voters were systematically persuaded by biased information over the years. As expected, the magnitude is lower than the negative persuasion rate of Enikolopov et al. (2011), who find that being exposed to NTV persuades 66% of viewers not to vote for the government party.

VIII Understanding the debiasing mechanism

Neither the center-left candidate nor third parties have systematically attracted Berlusconi candidates's votes after the shock (see Internet Appendix). We therefore look at the effect going digital on turnout to understand how lower exposure to bias affected voting.

Effect on turnout. In Table VI, turnout is the change in the log of total voters in 2010 and 2005.[62] In all specifications, we control for a third-degree distance polynomial. We estimate Equation 1 using change in turnout as a dependent variable. We add the change in the log of voting population from 2005 to 2010, the ratio of youngsters and a dummy equal to one for towns in the top third of the distribution of social capital as controls.[63] Turnout has not changed differently in switch off towns compared to other towns (column (1)).[64] From Table V, we know that the shock to bias exposure had the strongest effect in towns with more elderly. Column (2) shows that the turnout has also dropped significantly more in those towns than in others. With exception made for column

[61] This measure assumes that non-Berlusconi voters in 2005, $p+o$, are as likely to sort into new channels as Berlusconi voters and non-voters. Otherwise, the estimated rate is a lower bound of the true rate. Durante and Knight (2012) show that non-Berlusconi voters sort into a leftish outlet (RaiTre) once Berlusconi raises to power. We have no data on digital TV audience by political affiliation.

[62] This definition follows DellaVigna and Kaplan (2007). Results are similar if one uses the ratio of actual voters over voting age population.

[63] Younger towns supported a new party campaigning on the internet more (Internet Appendix). Turnout has likely increased there.

[64] The average 2010-2005 change in turnout in Piedmont was -9.9 percent (s.d. 7.7 percent).

(4), the drop in turnout was significantly higher in treated towns with the most elderly than in other treated towns. The mean ratio of the elderly in the oldest treated towns (0.47) is about one standard deviation higher than the mean in all treated towns (0.38, s.d. 0.08). Being in the former group is associated with a 2.3 percentage point larger drop in the Berlusconi candidate vote share (mean 2.5, s.d. 6.8), i.e. about one third of a standard deviation. Also, the drop in turnout is 2.1 percentage point larger than in other treated towns (mean 8.7, s.d 7.7), i.e. about one fourth of a standard deviation larger.

Debiasing mechanism. In light of the results so far, we propose the following debiasing mechanism: a) after moving to digital TV, individuals are less exposed to Berlusconi bias; b) Berlusconi supporters are less motivated to show up at elections: turnout drops, especially in towns with more elderly and less educated voters; c) Berlusconi candidate's vote share drops, especially in towns with more elderly, less educated voters.[65] Evidence in line with points b) and c) is given by Table VI and Table V, respectively. We provide additional evidence in the Internet Appendix, where we also show that this mechanism is consistent with voting in all other Italian regions. In the Internet Appendix we also investigate the relationship between historical support and the effect of digital TV, which is consistent across all Italian regions.

IX Conclusions

The bias towards Berlusconi in political information on Italian TV has systematically persuaded voters for 16 years. A quasi-random drop to bias exposure reduced Berlusconi candidate vote share by 5.5 to 7.5 percentage points in 2010. At least 30% of those watching new channels have been persuaded over time. Elderly and least educated voters were affected the most. The effects we document have the potential to change election results, are valid across geographic areas and elections. We assess a series of possible interpretations, and argue that selective attention and persuasion bias are broadly consistent with the evidence. Our results imply that policies should be implemented to help individuals, and the most vulnerable demographics in particular, filter out biases in information over time. Examples include voters and political information, investors and analyst forecasts, or patients and treatment recommendations. In our setting, persuasion

[65]This mechanism needs not be conscious. Voters support Berlusconi as long as they are exposed to the bias. Once exposure drops, they are not motivated to support him anymore.

survived despite awareness that Berlusconi controlled most TV channels. Mandated disclosure of conflicts of interest is therefore not a sufficient provision. Protection of systematically persuaded agents should add to the rights of free speech and minority representation in motivating antitrust provisions [66].

The results also speak to Italy, a member of the *Group of Eight*. People have wondered if media control by Berlusconi has favored his maintainance of power for years.

Table VI: **Effect of Switch to digital TV on Turnout**

	(1)	(2)	(3)	(4)	(5)
	Full Sample		< 75 Km	< 50 Km	< 25 Km
Switch off	0.017	0.030	0.026	0.017	0.016
Clust. province	*0.013*	*0.013**	*0.013**	*0.014*	*0.014*
Switch off*H. Old		-0.021	-0.023	-0.013	-0.036
Clust. province		*0.005****	*0.005****	*0.009*	*0.018**
Switch off*L. Old		-0.008	-0.008	-0.011	-0.013
Clust. province		*0.007*	*0.007*	*0.009*	*0.010*
Ratio of Youngsters	0.843	0.440	0.508	0.722	0.612
Clust. province	*0.171****	*0.215**	*0.240**	*0.323**	*0.471*
Δ Voting pop. 10-05	0.201	0.206	0.201	0.219	0.212
Clust. province	*0.043****	*0.040****	*0.021****	*0.023****	*0.035****
H, L, ratio in levels	no	yes	yes	yes	yes
Electoral controls	yes	yes	yes	yes	yes
Socio-dem. controls	yes	yes	yes	yes	yes
Border segment f.e.	yes	yes	yes	yes	yes
Observations	1,206	1,206	1,161	928	552
R^2	0.240	0.252	0.264	0.281	0.341

This Table reports results for estimating variations of the following spatial RDD model:

$$\Delta Turnout_{10-05ipb} = \alpha + \gamma switchoff_p + \gamma_1 switchoff_p \times H.Old + \gamma_2 switchoff_p \times L.Old + X'_{pre10ip}\delta + f(distance_i) + \Phi_b + \epsilon_{ipb}$$

Each observation is a town in Piedmont. In all columns, the dependent variable is the change in the log of voters between 2010 and 2005 Piedmont Regional Elections. Switch off is a dummy variable which equals one for treated towns, zero otherwise. Switchoff*H. Old is one if a town is in the treatment group and above the top tercile of towns sorted by ratio of elderly, zero otherwise. Switchoff*L. Old is one if a town is in the treatment group and below the bottom tercile of towns sorted by ratio of elderly, zero otherwise. The RDD polynomial is cubic in the distance from the border. S.e. are clustered at the province level (8 clusters), and corrected for downward bias as in Donald and Lang (2007). Significance is as follows: *10%, **5%, ***1%.

This is relevant to economic stability worldwide, as the recent EU debt crisis made salient. Results imply that controlling TV has been crucial to create and maintain political consensus towards Berlusconi over time.

Finally, the results have implications which go beyond the scope of economic research. If long-run persuasion is due to cognitive biases, is it fair to exploit it? Or may this conduct

[66] This is also at odds with recent developments in countries like Hungary (letter Neelie Kroes), France (complaints High-level group EU), Mexico and Thailand, and with the creation of large media conglomerates like Murdoch's News Corporation.

even constitute grounds for criminal allegations? Are elections a satisfactory source of power legitimization in countries where information is systematically biased? Which interventions to protect vulnerable groups are legitimate, and which are excessively intrusive on free will? This paper has barely scratched the surface of the issues that await to be investigated by researchers in many disciplines.

Data Appendix

In this section, we define variables labeled *Electoral controls* and *Socio-demographic controls* in the paper. All variables are observed at the town level unless otherwise specified.

Electoral controls are computed from data published by *Osservatorio Regionale at Consiglio Regionale del Piemonte*, and include:

- *Precints*: number of electoral precints in a town.
- Δ *Berlusconi 05-00*: change in Berlusconi candidate vote share between 2005 and 2000 Regional Elections.
- Δ *Berlusconi 00-95*: change in Berlusconi candidate vote share between 2000 and 1995 Regional Elections.
- Δ *Berlusconi EU 09-04*: change in Berlusconi party list vote share between 2009 and 2004 European Parliament Elections.
- *Share Berlusconi Prov. pre10*: share of Berlusconi candidate vote share in the closest Province Presidential elections before 2010.
- Δ *csx 05-00*: change in center-left candidate vote share between 2005 and 2000 Regional Elections.
- Δ *csx 00-95*: change in center-left candidate vote share between 2000 and 1995 Regional Elections.

Socio-demographic controls come from the 2001 Census by *Istituto Nazionale di Statistica* (Istat) unless otherwise specified. They include:

- Δ *unemployment 10-01*: change in unemployment rate between 2010 and 2001.
- Δ *unemployment 09-05*: change in unemployment rate between 2009 and 2005 at the province level.
- Δ *perc. foreign 09-05*: change in the percentage of foreign residents between 2009 and 2005. (*Rete Unitaria della Pubblica Amministrazione in Piemonte (RUPAR)*, available at http://www.ruparpiemonte.it/infostat/index.jsp)
- Δ *abs. foreign 09-05*: change in the absolute number of foreign residents between 2005 and 2009. (*Rete Unitaria della Pubblica Amministrazione in Piemonte (RUPAR)*, available at $http://www.ruparpiemonte.it/infostat/index.jsp$)
- Δ *milk prod quotas 10-08*: change in EU milk production quotas (liters) assigned to Piedmont farms at the province level.
- Δ *recycling inc 09-05*: change in percentage recycling over average taxable income. (*Sistema Piemonte, http* : $//www.sistemapiemonte.it/webruc/raccoltaRifiutiReportAction.do?btnAggiorna = aggiornaComuniDaComune$

- *Events environment 09-05*: number of interventions to address major pollution events. (*Anagrafe Regionale Siti Contaminati*, available at http://www.regione.piemonte.it/ambiente/bonifiche/home.htm)
- *Newsagents pop 09*: number of newsagents per 1000 inhabitants. (*Regione Piemonte, Osservatorio Commercio*, available at *http* : *//www.regione.piemonte.it/commercio/ossCommercio.htm*)
- *Tabacchi pop 09*: number of liquor stores (*Tabacchi*) per 1000 inhabitants. (*Regione Piemonte, Osservatorio Commercio*, available at *http* : *//www.regione.piemonte.it/commercio/ossCommercio.htm*)
- *Arci*: dummy equal to one if *Arci* clubs exist in town, i.e. leftish meeting points for elderly and youngsters. (*Arci Piemonte*, available at *http* : *//www.arcipiemonte.it/affiliati_piemonte*)
- *Acli*: dummy equal to one if *Acli* clubs exist in town, i.e. catholic meeting points for elderly and youngsters. (*Acli*, available at $http : //www.acli.it/index.php?option = com_google\&view = advanced\&id = 5\&Itemid = 141$)
- *Avis*: dummy equal to one if a blood donation station exists in a town. (*AVIS*, available at $http : //www.avis.it/usr_view.php/ID = 1403$)
- *Density 01*: inhabitants per squared Km from 2001 Census.
- *Male 01*: share of men over all inhabitants from 2001 Census.
- *Hsize 01*: average number of components per household from 2001 Census.
- *Manufacturing 01*: number of employees in manufacturing from 2001 Cesus.
- *Services 01*: number of employees in services from 2001 Cesus.
- *Tourism 01*: percentage of days hotel rooms are occupied over the whole year from 2001 Cesus.
- *Banking 01*: number of checking accounts per 100 inhabitants from 2001 Census.
- *Cars 01*: number of cars per 100 inhabitants from 2001 Census.
- *Students 01*: number of high school students from 2001 Census.
- *Health care efficiency 01*: number of days $\times$ patients needed to recover over one year from 2001 Census.
- *Chemists 01*: average number of inhabitants per each chemist shop from 2001 Census.
- *Disp. income 01*: average disposable income per inhabitant from 2001 Census.
- *Farms 01*: number of farms from 2001 Census.
- *Large HH 01*: number of households with 5 or more components from 2001 Census.
- *Retired 01*: number of inhabitants from 65 to 79 years old from 2001 Census.
- *Very Old 01*: number of inhabitants older than 80 years old from 2001 Census.

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Table VII: **Summary Statistics for all controls at the town level**

In this Table, we provide summary statistics for all variables which enter our main specifications as controls measured at the town level. Each Panel reports the mean of a variable for Treated (Switch) and Control (No Switch) towns. P-values for paired t-tests of the difference of the two means are reported for each variable. Standard errors are clustered at the province level. Panels report statistics for the full sample, as well as restricted samples to 75 Km, 50 Km and 25 Km around the treatment border. S.e. are bold if the associated t-statistic is significant at the 5% level or lower.

	Full Sample			≤ 75 km			≤ 50 Km			≤ 25 Km		
	Treated	Control	p-value	Treated	Control	p-value	Treated	Control	p-value	Treated	Control	p-value
Election outcomes												
Δ Berlusconi 05-00	-0.029	-0.035	0.761	-0.029	-0.033	0.817	-0.029	-0.032	0.879	-0.034	-0.038	0.842
Δ Berlusconi 00-95	0.177	0.150	0.493	0.176	0.145	0.422	0.168	0.145	0.521	0.161	0.154	0.871
Δ Berlusconi EU 09-04	0.006	0.002	0.686	0.009	0.002	0.563	0.011	0.001	0.421	0.015	-0.003	0.168
Berlusconi Prov. Pre 10	0.502	0.527	0.652	0.500	0.522	0.699	0.507	0.517	0.883	0.508	0.483	0.661
Δ Main comp. 05-00	0.106	0.088	0.152	0.106	0.087	0.156	0.106	0.086	0.138	0.105	0.090	0.349
Δ Main comp. 00-95	0.041	0.045	0.758	0.041	0.046	0.706	0.039	0.048	0.491	0.035	0.055	0.237
Precints	5.335	2.841	0.190	5.473	2.875	0.180	6.020	2.817	0.157	7.146	2.321	0.183
Socio-demographics												
Δ unemployment 10-01	0.011	0.005	0.530	0.012	0.006	0.540	0.012	0.005	0.494	0.014	0.009	0.669
Δ % foreigners 09-05	0.871	0.785	0.532	0.872	0.781	0.524	0.898	0.812	0.599	0.973	0.747	0.294
Δ abs. foreigners 09-05	176.6	71.50	0.158	181.9	74.00	0.151	206.0	71.59	0.119	269.8	54.26	0.137
Δ recycling 09-05	0.152	0.120	0.524	0.153	0.121	0.527	0.163	0.130	0.526	0.172	0.130	0.317
Events environment 09-05	0.727	0.757	0.943	0.738	0.783	0.911	0.849	0.713	0.742	0.969	0.513	0.429
Newsagents pop 09	1.244	1.088	0.090	1.138	1.096	0.681	1.045	1.111	0.614	0.963	1.111	0.387
Tabacchi pop 09	1.471	1.427	0.883	1.305	1.407	0.507	1.242	1.463	0.225	1.157	1.367	0.209
Arci	0.145	0.200	0.356	0.148	0.204	0.419	0.149	0.170	0.749	0.136	0.125	0.901
Acli	0.237	0.098	0.317	0.238	0.099	0.317	0.223	0.098	0.327	0.202	0.079	0.262
Avis	0.285	0.201	0.342	0.289	0.210	0.360	0.287	0.221	0.475	0.244	0.170	0.404
Observations	565	641	1206	546	615	1161	457	471	928	287	265	552

Table VIII: **Summary Statistics for all controls at the town level - continued**

In this Table, we provide summary statistics for all variables which enter our main specifications as controls measured at the town level. Each Panel reports the mean of a variable for Treated (Switch) and Control (No Switch) towns. P-values for paired t-tests of the difference of the two means are reported for each variable. Standard errors are clustered at the province level. Panels report statistics for the full sample, as well as restricted samples to 75 Km, 50 Km and 25 Km around the treatment border. P-values are bold if lower than 5%.

	Full Sample			< 75 km			< 50 Km			< 25 Km		
	Treated	Control	p-value	Treated	Control	p-value	Treated	Control	p-value	Treated	Control	p-value
Density 01	187.4	119.5	0.391	193.4	123.2	0.384	217.9	115.5	0.248	237.4	120.1	0.285
Male 01	0.498	0.489	0.097	0.497	0.489	0.132	0.495	0.489	0.219	0.494	0.489	0.413
Hsize 01	2.239	2.212	0.535	2.250	2.214	0.400	2.295	2.212	**0.033**	2.297	2.214	0.058
Manufacturing 01	676.5	359.3	0.223	698.4	371.9	0.216	783.6	379.7	0.185	871.9	282.3	0.161
Services 01	858.6	397.4	0.121	881.2	406.9	0.127	995.9	408.9	0.147	1198	311.2	0.158
Tourism 01	0.071	0.061	0.529	0.069	0.058	0.459	0.066	0.048	0.195	0.054	0.042	0.375
Banking 01	44.74	36.91	0.240	44.72	37.56	0.337	45.58	38.25	0.315	41.03	36.92	0.435
Cars 01	58.82	59.66	0.513	58.88	59.85	0.432	59.52	60.28	0.562	59.47	60.30	0.645
Students 01	180.5	77.55	0.174	185.9	80.66	0.170	207.3	83.90	0.177	251.1	47.74	0.171
Health care eff. 01	5351	2624	0.237	5537	2577	0.210	6065	2934	0.277	7175	1272	0.187
Chemists 01	1522	1066	0.195	1559	1075	0.171	1687	1033	0.106	1543	953.9	0.229
Disp. Income 01	13897	14239	0.587	13842	14337	0.431	13940	14489	0.464	13856	14059	0.817
Farms 01	114.5	87.79	0.517	117.6	90.67	0.524	117.6	102.5	0.720	111.6	126.1	0.755
Large HH 01	71.56	33.36	0.085	73.71	34.11	0.078	81.25	33.17	0.069	91.95	28.22	0.129
Retired 01	754.0	401.9	0.228	776.5	411.9	0.219	862.0	415.7	0.193	1044	336.1	0.193
Very Old 01	219.9	136.2	0.265	225.3	139.6	0.274	247.8	140.7	0.230	294.9	115.9	0.216
Observations	565	641	1206	546	615	1161	457	471	928	287	265	552