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Abstract

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 viewers' exposure to Berlusconi bias: idiosyncratic deadlines to switch to digital TV from 2008 to 2012. Digital TV increased the number of freeview channels tenfold. As a consequence, viewership of Berlusconi-controlled channels by digital users dropped by 19% from 2008 to 2010 elections. Although the control of most pre-digital outlets by Berlusconi was widely known, the switch caused a drop in his coalition vote share by 5.5 to 7.5 percentage points. The effect was stronger in towns with older and less educated voters. At least 30% of digital users had not filtered out the bias from 1994 to 2010. Moving to digital TV affected voting via turnout: previous Berlusconi supporters went to vote less than others, hence his vote share dropped. We discuss several Bayesian interpretations, and argue that they cannot fully explain these results. Coarse thinking, selective attention, and persuasion bias are broadly consistent with the evidence.

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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?[1]

To address these questions, we consider the long-lived bias towards Prime Minister Silvio Berlusconi in political information on Italian TV. For 10 years during the period 1994-2011, Berlusconi has controlled six out of seven national channels, due to his dual role as a media tycoon and Prime minister.[2] Durante and Knight (2012) document the existence of a bias in terms of time and quality of coverage of Berlusconi's party and opponents.

We exploit a quasi-random shock to the biased TV exposure of Italian viewers: idiosyncratic deadlines to forcely switch from analog to digital TV from 2008 to 2012. At the deadlines, old analog signals were switched off, and only digital signals kept on airing. Digital TV improved transmission efficiency, and increased the number of free national channels tenfold. Most digital channels are aired by new media companies, which have no ties to Berlusconi. After switching to digital TV, many Italian households changed their viewing habits: from June 2008 to June 2011, the share of viewers of Berlusconi-controlled channels dropped from 84% to 71%. Over the same period, viewers of new channels increased from 2% to 17%.[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. National legislation established dates for Italian regions based on similarity of infrastructures built in the 1950s, and to guarantee a homogeneous move for the North, Center and South of the country. We employ a spatial regression discontinuity strategy to estimate the causal

[1] Kamenica and Gentzkow (2011) and DellaVigna and Kaplan (2006) discuss Bayesian persuasion in the short run, and non-Bayesian persuasion in the long run.

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

[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.

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. We look at Piedmont, a region where Western towns switched to digital TV in Autumn 2009, while Eastern towns 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. Scaling the effect by 2005 Berlusconi supporters and nonvoters who watched new channels in 2010, we 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 towards opposition parties which our design does not capture.

We then investigate which demographics stopped supporting Berlusconi once on digital TV. We find that Berlusconi candidate vote share dropped the most in towns with older and less educated voters. On the contrary, towns with more (less) youngsters, or towns with higher (lower) social capital did not behave different from other towns. We show that switching to digital TV affected voting behavior via turnout: turnout dropped after the shock, especially in towns where voters were older and less educated. This suggests that many of his previous supporters did not vote in 2010. In treated towns with a ratio of elderly over total population one standard deviation above the mean, turnout dropped by one quarter of a standard deviation more, and Berlusconi candidate vote share dropped by one third of a standard deviation more than in other treated towns.

To our knowledge, this is the first paper which documents that persuasion by a biased sender can be sustained in the long run, even if all receivers are aware of the bias. We discuss plausible interpretations of these results. We argue that negative news omission, positive news omission, agenda setting, rational inattention, overlapping generations, changes in political advertising, changes in voters preferences, and lower political knowledge are unlikely to fully explain them. At the same time, coarse thinking with

[4]We provide external validity of results across geographic areas 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 needs not obtain more than 50% of valid votes to win an election.

limited memory, selective attention and persuasion bias seem broadly consistent with our evidence.

This paper falls within the literatures on persuasion in economics (DellaVigna and Gentzkow, 2010), and media and political outcomes (Prat and Stromberg, 2011). Besley and Prat (2006) and Anderson and MacLaren (2012) show that media bias may affect rational agents who do not know when information is omitted.[6] In the Internet Appendix, we show that our effect is not driven by positive or negative omitted information. Also, Kamenica and Gentzkow (2011) derive conditions for a biased signal by a sender to persuade 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 their choices once exposure drops. DeMarzo et al. (2001) model the effect of persuasion on boundedly-rational agents' opinion formation. They interpret persuasion bias as incomplete filtering of data. Building on them, DellaVigna and Kaplan (2006) model the effect of media bias on Bayesian and persuasion-biased agents. The latter systematically fail to take into account the full extent of estimated bias when updating their beliefs about signals. Our test is based on their intuition.

Empirical research on media and voting includes Gentzkow and Shapiro (2006) and Gentzkow and Shapiro (2010), who argue that media slant emerges from media outlets responding to audience preferences, which are biased towards likely-minded news. Gentzkow et al. (2011) document an effect of newspapers opening and closing on electoral turnout, but no effect of newspaper slant on candidate vote shares. In this paper, turnout drops more in towns where the effect of the shock to bias exposure is stronger.

As in DellaVigna and Kaplan (2007) and Enikolopov et al. (2011), we show that media bias affects voting behavior. We are different insofar as we look at lower exposure to a

[6] Larcinese et al. (2011) show that some US newspapers omit negative economic news when an ideologically close administration is in power.

long-lived, pervasive bias, instead of higher exposure to a new biased outlet.[7] This allows to test if persuasion can be sustained in the long run, even if the bias is systematic and every voter is aware of it. Since virtually all Italians were exposed to the bias before digital TV, we can estimate the ratio of voters who were systematically persuaded over time, and determine which demographics were most affected. 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 when Berlusconi becomes Prime Minister. The bias towards Berlusconi increases when he serves as Prime Minister. Some viewers sort into leftish outlets based on ideological preferences. Viewers with leftish ideological preferences sort, but they are not responsive to the bias in the first place. On the contrary, switching to digital TV involves all viewers, including those who support Berlusconi.

Digital TV viewers sort into all-entertainment channels, but not into news programs. Gentzkow (2006) and Prior (2005) show that once television and cable TV, respectively, become available to US viewers, some of them stop watching news programs and sort into entertainment programs. This reduces 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 must 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, we find that the effect was larger in towns with lower historical support. We interpret this as evidence of peer effects in voters' updating of beliefs about Berlusconi and his party, in line with evidence in Cialdini (1984) and with the model of Murphy and Shleifer (2004).

We provide a motive for increasing competition in the TV industry when information sources are biased, and the bias does not consist of news omission. Higher competition reduces bias exposure, helping individuals to debias mechanically. This is different from

[7] Enikolopov et al. (2011) look at the effect of availability of a non-governmental outlet, NTV, from 1996 to 1999 and 2003 general elections. The acquisition of NTV by a state-controlled company after 1999 and before 2003 equalizes candidates coverage before 2003. Hence, length of exposure is at most 7 years.

Hong and Kacperczyk (2010), who show that competition reduces the extent of bias in financial analysts' reports, to which investors are always exposed[8].

In the rest of the paper, Section 2 describes the institutional setting, while Section 3 gives evidence on Italian households' reaction to the switch to digital TV. In Section 4, we discuss our identification strategy and the data we use. Section 5 presents the empirical results and tests their robustness. In Section 6 we interpret the magnitude of the effect we find, and discuss plausible explanations for it. In Section 7, we investigate which demographics mediate the long run effect of persuasion, and we estimate how many households have been systematically persuaded over time. Section 8 investigate the debiasing mechanism, and Section 9 concludes.

II Institutional Setting

Television in Italy. From the 1950s onwards, Italian TV has been airing on an analog infrastructure. 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.

[8]Our setting is complementary to Mullainathan and Shleifer (2005), since Italian TV channels are ideologically biased, and do not slant news towards viewers' taste.

From 2008 onwards, a new technology has been put forward: terrestrial digital TV, which dramatically enhances transmission efficiency. It uses existing analog infrastructures, avoiding high setup costs of cable and satellite TV. Receivers need to own a *decoder*, i.e. a tool similar to a modem for internet connections.[9] Digital TV allowed to air about ten times more freeview national channels than analog TV.

Berlusconi Bias. Berlusconi has owned three out of seven nationwide channels since the 1980s. Moreover, he has founded and led a major political party from 1994 onwards, being elected Prime minister three times since then: from 1994 to 1995, from 2001 to 2006 and from 2008 to 2011. In those years, he was controlling the Rai network and picking main executives and news programs directors. This raises concerns that a bias exists in favor of Berlusconi and his 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. In 2010, *Autorità Garante per le Comunicazioni*, an independent body supervising communications, ordered most news programs to cut the coverage time of Berlusconi's party in favor of opponents. Also, the major Italian news program (Tg1) was ordered to pay $100,000 for breach of the law on electoral campaigns. A Berlusconi bias has also been detected in TV shows.[10]. Viewers have been exposed to a Berlusconi 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 a source of political information in 2009.[11]

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: about sixty channels air nationwide as of Autumn 2012, and several new media companies have accessed the TV industry

[9]Decoders can be bought for as low as $30. Still, to ensure wealth constraints did not prevent households from going digital, the government supported purchases by low income households.

[10]The popular show *Forum* hosted a small entrepreneur from Abruzzo, hit by a devastating earthquake in 2008. She thanked Berlusconi's administration for their help, and attacked local politicians (from opposition parties) A few hours later, the lady admitted she was an actress, and she had followed a script provided by the show authors.

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

after 2008. In 2006, the (center-left) government regulated the transition from analog to digital TV, which was mandated by European Union legislation.[12] 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 law mentioned two criteria which could not be manipulated by current politicians: similarity of 1950s infrastructures and homogeneous move for North, Center and South of Italy.[13]

Viewers reaction to switch. Did viewing habits of Italians change after switching to digital TV? In Panel A of Figure I, the left axis reports the share of viewers of Berlusconi-controlled TV channels over the period June 2008 to June 2011,[14] and it is associated with the light-blue line. All six channels combined dropped from 84% of viewers in June 2008 to about 71% in June 2011. More than 95% of Italians regularly watch TV since the 1990s (see Istat). These drops cannot be driven by new TV users watching digital channels. The drop was substantial for all channels: the (unreported) share of viewers of Berlusconi network alone decreased from 40% to 32% over the same period. The right axis reports the share of viewers of new digital TV channels, and it is associated with the black 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, i.e. be exposed to unbiased (or otherwise biased) information. Second, whenever they watch new all-entertainment channels, they are not exposed to Berlusconi bias. Third, those who do not go digital by the deadline cannot access any signals: they are incapacitated to bias exposition. In the Internet Appendix, we show that less than 1 percentage point of digital viewers moved to news channels, and that an omission bias like the one documented by Larcinese et al. (2011) does not explain

[12] 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.

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

[14] This is the latest period when Berlusconi was Prime Minister.

our results. We also show that the incapacitation channel is unlikely to be relevant in our setting.

Panel B of Figure I plots the viewing share of new digital channels in March 2010 based on their content. The majority of households sorted into entertainment for kids or channels airing old movies and TV shows. Viewing shares in Panel B are daily averages for the time slot 6:30pm to 8:30pm, during which two major news programs on biased channels are aired, Tg1 and Tg5. In the Internet Appendix, we show that the viewing shares of these news programs have dropped by the same amount gained by new digital channels, therefore ruling out that these changes are explained by an increase in overall TV consumption after the switch. We also show that the move from news to entertainment channels was not paralleled by sorting into alternative information sources, such as newspapers or the internet.

Overall, evidence suggests that after the switch, habits changed in a way which should have not affected the voting behavior of viewers who had filtered out the Berlusconi bias in information over the years.

Switch off and election results across Regions. March 2010 Regional Elections were the first taking place during the switch off process. Elections were held in 13 out of 20 Italian regions.[15] Three regions had experienced a deadline to go digital before 2010 elections: Campania, Lazio and (Western) Piedmont. All other regions were still allowing for analog TV in March 2010. In the top graph of Figure II, we show the percentage of digital TV users in each region as of March 2010. Dark histograms refer to the three all-digital regions, light histograms to others.[16]

The bottom graph shows the percentage point change of Berlusconi party vote share between 2010 Regional Elections and 2009 EU Parliament elections, which are the closest to the first switch off wave of non-special statute regions (Autumn 2009). EU elections

[15]Five special-status Regions had elections in 2008. Abruzzo and Molise had early elections in 2006 and 2008.

[16]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.

allow to isolate voting behavior just a few months before and after switch off deadlines.[17] The share of votes for region Lazio has been corrected as described in the Internet Appendix, since 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.[18] If we use the percent change of Berlusconi party vote share to account for heterogenous ideologies across regions, the difference between all-digital and other regions is even more apparent. Hence, either three idiosyncratic shocks 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 March 2010 Regional Elections. Near the switch off date, the probability that households are on digital TV jumps to about one. Voters who switch before elections are less exposed to Berlusconi bias than voters who switch after elections. This is supported by evidence in Figure I. Figure III describes the natural experiment. We look at Piedmont, the only region where some towns (West, black) switched to digital TV in Autumn 2009, i.e. before elections. The other towns (East, white) went digital in Autumn 2010. Switch off dates were assigned at the province level, i.e. governmental partitions between Region and towns. The timing of switch to digital TV by Western Piedmont households is particularly suitable to our analysis: according to survey evidence from *Itanes* (Italian National Election Studies), in 2008 37% of Italian voters have decided which candidates to vote for no earlier than a few

[17] Regions Puglia and Basilicata are excluded. In both cases, a candidate supported by Berlusconi but dismissed by local party officials ran anyway, and got more than 8.5% of votes. It is unclear who Berlusconi supporters voted for.

[18] Berlusconi candidates still won all three regions. In Veneto, as in Piedmont, a candidate from an allied party, Lega Nord, was supported by Berlusconi. Candidates from Lega Nord may move votes to their party. Comparing Veneto to other non-switch off regions gives a sense of the magnitude of this effect. We will address this point in section V.

weeks before elections, 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 political 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.[19] 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 in 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.[20]

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 strategic sorting around the border.

We examine the plausibility of assumption i) in Table I, which reports summary statistics for electoral and socio-demographic characteristics at the town level.[21] Each Panel of

[19] After the deadline, the probability of being on digital TV is around 40% in Eastern towns and jumps to about 100% in the West.

[20] If we use three or seven border segment f.e., results are unaffected.

[21] In the Appendix at the end of the paper, we report statistics for all covariates used in the analysis.

Table I shows means of variables for treated (Switch) and control towns (No Switch). Standard errors for a paired t-test of the difference of means across groups are also reported.[22] The first Panel shows statistics for the whole sample. Other Panels 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 seems 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 towns. However, magnitudes differ, because western Piedmont includes Turin, which had more than 900,000 residents in 2009.[23] The share of employees in manufacturing and those in services are also similar across groups. The same holds for the change in percentage of foreign residents, and in 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), we 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 do it. 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 (about 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. Treating standard errors correctly is particularly crucial in a spatial

[22]Standard errors are clustered at the province level (8 clusters). They are likely to be biased downwards. This goes against the null hypothesis of no difference in means across treatment and control, which we want to corroborate.

[23]This is about 260 times the average population of western towns excluding Turin. To ensure Turin does not drive our results, we provide specifications without Turin and neighboring towns in Table III.

RDD framework, since residuals may be not only correlated at the treatment-control level, but also 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, thus 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, when estimating the variance-covariance matrix. Also, we use critical values of a t-student distributed variable with $C-1$ degrees of freedom, instead of a standard normal, to establish test statistics significance. If using the rule of Donald and Lang (2007), significance of all estimated coefficients does not change. Following Cameron and Miller (2011), we alternatively account for the small number of clusters using a wild bootstrap methodology at the cluster level. These two frameworks assume that errors for towns in different provinces are uncorrelated. But towns close in space and in different provinces may have correlated errors. We thus provide a third set of standard errors based on the procedure of Conley (1999), which allows for spatial dependence of unknown form.[24]

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). Hence, we estimate Equation 1 using both OLS and weighted least squares, where weights are average logarithm of total voters in 2010 and 2005 elections.[25]

Baseline covariates. Although a consistent estimate of the treatment effect does not require that we add baseline covariates to our specification, we do so in order to reduce the sampling variability in our estimator (see Lee and Lemieux [2010]). All covariates' descriptions, summary statistics and balancing can be found in the Appendix at the end of the paper. Crucially, we use electoral controls, i.e. observations of the change in Berlusconi candidate vote share across elections *before* digital TV was introduced in Italy. Data on electoral results prior 2010 Piedmont elections are those collected in the database DWSIDE (DataWarehouse Sistema Integrato Dati Elettorali) by *Osservatorio*

[24]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 lower or higher bandwiths. In particular, here it is more conservative than using one degree, as in Dell et al. (forthcoming) and Dell (2010), and of 3 degrees (200 miles), as in Kline and Moretti (2011).

[25]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%.

Elettorale at Consiglio Regionale del Piemonte. In addition, we add several controls for demographics at the town level. We use 2001 Census data diffused by Istat, which are the latest available before the introduction of digital TV in 2008, and more up-to-date demographics from several sources, as described in the 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. In Table II, we report results for estimating Equation 1. Panel A uses linear distance from the border as the RD polynomial. 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 West than in the East, compared to Berlusconi candidate shares in 2005.[26] This effect is statistically significant when standard errors are corrected for clustering at the province level and for allowing spatial correlation of unknown form. In column (2), we give more weight to towns with more voters, whose voting 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 similar R^2, i.e. both models explain the same portion of variation in the dependent variable. In columns (3) to (8), we only use observations close to the border. When we consider towns within 75Km and 50Km around the border, the coefficient of interest ranges between -4.8 and -5.5 percentage points. If we only consider towns within 25Km around the border, the drop in Berlusconi candidate vote share was about 6 percentage points higher in the West than in the East. Results do not change if we approximate for smooth effects of distance using a third degree polynomial (see

[26]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.

Panel B). Here, 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 to alternative explanations. Please refer to the Internet Appendix for additional results. In each Panel, we use the whole sample of Piedmont towns (columns (1) and (2)), as well as towns within 75Km, 50Km and 25Km around the border (columns (3) to (8)). Standard errors clustered at the province level and corrected as in Table 2 are reported below coefficients.[27] In Panel A, we consider the complete spatial structure of observations using a cubic polynomial in longitude and latitude.[28] Magnitude of coefficients and statistical significance are similar to those in Table II.[29] 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, reported below coefficients: we do not need to account for the non-convergence of clusters to their asymptotic distribution. As a downside, we assume that residuals for towns in the same province but belonging to different deciles of income per capita are uncorrelated. The RD polynomial is cubic in distance from the border, thus coefficients and R^2 are those in Panel B of Table II. Standard errors are similar to those in Table II. In Panel C, we allow for heterogeneous treatment effects adding interactions of the dummy Switch off with the cubic distance polynomial. The estimated effect is one to two percentage points

[27] Wild bootstrapped s.e. and spatial HAC s.e. give similar results as in Table 2.

[28] This specification was introduced by Dell (2010). In her case, the boundary is a multidimensional discontinuity in longitude-latitude space. Our border is a mono-dimensional discontinuity. Yet, distance is high for towns in the North East (negative) and in the South West (positive), without a border segment at their latitude. Modeling the longitude-latitude structure of towns alleviates concerns that those towns have been wrongly accounted for in the monodimensional specification.

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

lower than in previous specifications, unless we limit the analysis to towns within 25Km around the border. In Panel D, we provide results for a difference-in-difference estimator, without exploiting the spatial dimension of the data. As in Panel C, some coefficients are smaller than those estimated in baseline specifications, but differences disappear in towns 25Km around the border. Statistical significance is unaffected. Finally, we estimate the average treatment effect on the treated (ATT) using a nearest-neighbor matching estimator based on the propensity score.[30] (Untabulated) results are very similar to those in Panel D: we estimate an ATT of -0.036 (s.e. 0.008). Overall, results are robust to alternative specifications of Equation 1.

In the last three Panels of Table III, we test alternative explanations. First, towns in the area around Turin have a peculiar manufacturing and urban structure. There might be latent unobservables so that Berlusconi candidate performed bad there in 2010. The effect we attribute to Western Piedmont may be a local effect. In Panel E, we exclude Turin and all neighboring towns. Magnitudes and statistical significance are similar to those in Panel B of Table II. A second concern is that Berlusconi candidate in 2010 was not a member of his party (PDL), but of *Lega Nord*, a long-term ally in Northern Italy. Voters in the West may know and trust this party less than voters in the East. In Panel F we add a dummy which equals one if there is a local branch of Lega Nord in town, and its interaction with the treatment condition. If voters in the West voted less for Berlusconi candidate because they knew him or his party less, the effect should be less negative in towns where Lega Nord campaigned more actively. (Unreported) coefficients on both dummies are economically and statistically insignificant, and the main result is unaltered. Eventually, we check if the result is driven by towns at the border, which might explain why our estimates do not change when restricting the sample. In Panel G, we exclude towns at the border or within 5 Km from it, and run a diff-in-diff estimation.[31] Results are similar to Panel D, both in terms of magnitude and statistical significance.

[30] We predict the propensity score running a logit regression of the treatment on 2005 Berlusconi candidate share, unemployment rate in 2009, number of newsagents p.c. in 2009, recycling over income p.c. in 2009 and share of foreign residents in 2009. Results are similar if we include or exclude variables from the first stage.

[31] Since we artificially create a discontinuity in the distance dimension, we do not add a distance polynomial.

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 dependent variable.[32] In 2005, all households were still on analog TV. Being a town in the West has no effect on the change in vote share of Berlusconi candidate.[33]

Note that 2010 was a year of economic crisis. This might correlate with latent factors in the West which reduced Berlusconi candidate vote share there. In columns (3) and (4), we use the change in vote share of Berlusconi party between 2009 and 2004 EU Parliament elections as dependent variable.[34] The economic crisis started to hit Piedmont in 2008. GDP growth was at its bottom in the second quarter of 2009, and it started to recover afterwards. EU elections were held in June 2009,[35] when less than 25% of Piedmont households were accessing digital TV. Hence, 2009 EU elections happened when the economic crisis had hit Piedmont already, but access to digital TV was still limited. We find no effect of being a Western town on the change in vote share of Berlusconi party between 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 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, all towns to the east (and to the west of the true border) to an artificial control condition.[36] We find no effect of being an artificial treatment town on the change of Berlusconi candidate vote share.[37] Analogously, in columns (7) and (8) we

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

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

[34]In EU Parliament elections, Italians choose a party and may name a candidate from the party list.

[35]Some effects of crises are delayed, e.g. there is hysteresis in the labor market. In baseline specifications, controls include the change in unemployment between 2010 and 2001.

[36]In Imbens and Lemieux (2008) the artificial border is set at the median distance from the true border in both directions. Results do not change if we do so.

[37]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. The related statistic is insignificant though.

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 Validation. All results so far refer to Piedmont. One might wonder if the effect holds more generally.[38] We estimate the effect of digital TV in an inter-regional setting. 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 neighboring region Liguria, where voters could still access analog TV and had to choose among different candidates in 2010. The border divides Northern (Piedmont) and Southern (Liguria) towns, and northern towns are the treated group. Untabulated summary statistics show that, as expected, 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. Untabulated results confirm that the change in vote shares is significantly lower in Piedmont towns, both statistically and economically, ranging from -2.5 to -4.7 percentage points across specifications.

In the Internet Appendix, we show that results survive across elections. We analyze the link between digital TV usage and Berlusconi candidates performances in 2011 vs. 2006 Province Elections, for the provinces where elections where held in 2011. In all provinces where analog TV was still available in 2011, Berlusconi candidates did not lose votes compared to 2006. In all but one provinces which had switched to digital TV before 2011 elections, Berlusconi candidate obtained a lower vote share than in 2006.

VI Interpretations

Magnitude of the effect. The probability of accessing digital TV in Western Piedmont as of March 2010 was close to one, but about 40% of households in the East were also accessing digital TV. To back out the overall effect of switching to digital TV on election outcomes, we divide estimated coefficients by the difference in the probability of being

[38] Also, Berlusconi candidate in 2010, Roberto Cota, is a native of Eastern Piedmont, while his main competitor is originally from the West. The effect we attribute to the West could be a negative trend of Berlusconi party which was moderated in the East by choosing a local candidate.

exposed to the treatment across conditions, i.e. 0.6.[39] To estimate a lower and an upper bound, we use coefficients in the weighted least squares 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. Our results imply that moving to digital TV causes a drop of Berlusconi candidate vote share of 5.5 to 7.5 percentage points. Note that one fifth of Piedmontese, all in the East, were not accessing digital TV before 2010 elections. Taking a partial equilibrium view, and assuming the effect would have been homogenous had these viewers moved to digital TV before 2010 elections, Berlusconi candidate vote share would have dropped by an additional 1.4 to 1.9 percentage points. Berlusconi candidate won 2010 elections by a margin of 0.46 percentage points over his main opponent. Thus, the effect we document has the potential to change the results of an election.

The magnitude of the effect is in line with Enikolopov et al. (2011): availability of an independent media outlet which three quarters of Russians accessed decreased votes for the government party by 8.9 percentage points. If we assumed a homogenous effect on all Russian voters had the network been available to everyone, the overall drop would rise to 12 percentage points. This likely includes a short-run effect on Bayesian voters, in line with (Kamenica and Gentzkow, 2011).

Bayesian interpretations. We now discuss plausible Bayesian interpretations for the 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 our results if, after moving to digital TV, individuals sorted into sources of information they were not accessing before. They may get to know facts about Berlusconi or his party they had not been aware of previously. In the Internet Appendix, we show that individuals who move to new digital TV sort into non-news programs, as graphically emphasized by Panel B of section III.

[39] 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.

We also show that after moving to digital TV, treated individuals do not sort into other sources of information, such as newspapers or news websites on the internet.

Positive news omission. There might be positive information about Mr Berlusconi or his candidate after the switch off. Those on digital TV would be less exposed to it. But then, one should observe higher support for Berlusconi in the whole country, since most Italian households had not switched to digital TV as of March 2010. In the Internet Appendix, we show that trust in Berlusconi measured at the national level declined from February 2010 onwards.

Agenda setting. Italian news programs typically last thirty minutes. Directors decide which information to emphasize more or less. For the US, Larcinese et al. (2011) show that left-leaning outlets do not emphasize unemployment figures when Democrats are in power. Berlusconi was Prime minister in 2010: directors of biased news programs may have not emphasized the rise in unemployment before elections. But then, Berlusconi should lose votes in Western and Eastern towns where unemployment increased the most before elections, since voters there would not need to hear about unemployment on TV to know it is rising. In the Internet Appendix, we show that this is not the case.

Rational Inattention. TV may be the sole means reminding voters about upcoming elections. Once viewers stop watching news channels, they may not want to pay the cost of learning when elections are held from non-TV sources (including asking peers or family members). This could drive our 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,[40] with a local office of Berlusconi candidate party in town, or where demographics who 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 advertisement on new digital channels more easily. But in Italy, political ads have been banned since 1999.

[40]In Italy, newsagents display a summary of captions of newspapers on the street. Simply looking at them while walking around would remind individuals that elections are upcoming.

Overlapping generations. Observed long-run persuasion could be driven by new generations who start to be exposed to the bias *and* to take part to collective decision-making continuously. Voting in Italy is allowed to all citizens (not immigrants) who are 18 years old. Anyone who was allowed to vote in 2010 had been exposed to the bias for 16 years. Conversely, no one born after 1994, when Berlusconi created his party, voted in 2010.[41]

Change in preferences. Moving to new digital channels may have directly affected 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, only include channels replicating old material 24/7: kid channels propose old cartoons and shows which aired on Italian TV in the '80s, '90s and early 2000s, especially on Berlusconi's network. The same is true for movies and TV shows. Even if shows carried ideological cues, both viewers on digital and analog TV would have been exposed to them over the years. As a side point, Berlusconi's TV itself may have shaped viewers' political preferences towards his own ideology over the years. Voters may therefore support him without a need for a persistent bias in his favor on TV. If this is true, a fraction of Berlusconi supporters who do not change their voting behavior after switching to digital TV would have also been affected by Berlusconi bias. In this case, the effect we detect would be a lower bound for the overall effect of Berlusconi bias on Italians' voting behavior over time.

Non-Bayesian interpretations. Moving on to non-Bayesian explanations, a few of them may be broadly consistent with our evidence.

Coarse Thinking. Following the intuition of Mullainathan et al. (2008), over the years individuals may have unconsciously associated good feelings from watching TV shows to Berlusconi, who was extensively covered in news programs. This explanation is consistent with results if individuals had limited memory. If they had not, once exposed to the same shows as in the past, they would recall the association with Berlusconi and still be willing to go vote for him. This interpretation is similar to the drop in interest for politics of

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

viewers unexposed to news proposed by (Gentzkow, 2006) and Prior (2005), which requires that Berlusconi supporters are more likely to sort into digital TV than others. Hence, we should expect a larger effect in towns with higher historical support for Berlusconi. But the effect we document is stronger in towns with *lower* historical support (see section VI).

Selective Attention. Schwartzstein (2012) proposes a model of selective attention to freely available information which may produce persistently biased beliefs. After an attention shock, agents need time to learn which sources of information to attend to in this setting. If we interpret moving to digital TV as a shock to information voters attend to, selective attention implies that the debiasing process is slow. Not that our evidence implies that voting behavior changed no more than five months after the shock.

Persuasion Bias. DellaVigna and Kaplan (2006) show how a Bayesian and a persuasion-biased agents form beliefs when exposed to systematically biased information. Over time, a Bayesian agent is exposed to a high enough number of signals to filter out the bias in full. A persuasion-biased agent systematically fails to take the extent of bias into account: she will never filter it out in full. While exposed to the bias, she is systematically convinced to vote for Berlusconi. Once exposure drops, she is not persuaded to go and vote for him anymore. This process needs not be conscious: a viewer needs not realize she was exposed to a bias to change her behavior.

VII Who is systematically persuaded?

Demographics and information processing. We investigate which demographic groups are persuaded the most over time.

Two dimensions seem most relevant: potential cognitive biases, and the extent of bias exposure before the shock. In the Internet Appendix, we show that individuals aged 60 or higher are more likely than other demographics to watch TV every day. At the same time, aging of the brain has been shown to worsen cognitive abilities.[42] We test the hypothesis that elderly are less likely to filter out biases over time. We sort Piedmont towns by the ratio of elderly (individuals aged 64 or higher) over the whole population. We create

[42]For an exhaustive review, see Craik and Salthouse (2008).

a dummy, High, which equals one if a town is above the top tercile of the distribution of ratio of elderly. Low is a dummy which equals one if a town is below the bottom tercile. In columns (1) and (2) of Table V, we augment the RHS of Equation 1 with the dummies, their interactions with treatment condition, and the ratio of elderly over total population. The interaction between high ratio of elderly and treatment condition is negative and statistically significant, both when using the whole sample of towns or those within 50Km around the border. In treated towns where the ratio of elderly is high, Berlusconi candidate vote share dropped by 2.3 percentage points more than in others.

As a placebo corroboration, we sort Piedmont 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 should have similar cognitive abilities to other non-elderly. In columns (3) and (4), we augment the RHS of Equation 1 adding High and Low dummies for the top and bottom tercile of the ratio of youngsters distribution, as well as their interactions with the treatment condition and the level of youngsters ratio. We find no different effect in towns with high or low ratios of young voters compared to other treated towns.[43]

To disentangle the effect of extent of exposure from cognitive abilities, we look at education. High and low education people are similarly exposed to TV [Istat]. But less educated individuals may have lower cognitive abilities. In columns (5) and (6) of Table V, we sort towns by the ratio of college educated people over total population, and define analogous dummies and interactions as above. In towns with less educated individuals, the effect of moving to digital TV is 1.8 to 2.1 percentage points larger than in other towns. This effect is less statistically robust than for elderly, but 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 top and bottom thirds for ratio of elderly and top and bottom thirds for ratio of college educated individuals in a town. We should expect the effect to be stronger in towns with most elderly *and* least educated voters than in other treated towns. (Untabulated) results show that in the former towns Berlusconi candidate vote share was 5.7 percentage

[43] Youngsters use the internet more than others. This affected elections outcomes in Piedmont (see Internet Appendix). A higher effect in younger towns would have been consistent with an information interpretation.

points lower than corresponding towns in the control area (s.e. 0.013), on top of a baseline effect on -2.5 percentage points in other treated towns.[44]

We interpret the mediating role of elderly and less educated voters on the effect of a drop to bias exposure on voting behavior as evidence that cognitive abilities are important to explain why individuals can be systematically persuaded over time.

From 2008 onwards, Berlusconi has been known for sexual scandals. Voters support may drop if sensitive to these topics, and some demographics may be more sensitive than others. To test this channel, we look at social capital: the higher the social capital in a town, the more people are possibly concerned with ethical and moral conduct of politicians, the less likely they would support Berlusconi after 2008. It is hard to find a good proxy for social capital at the town level. In columns (7) and (8) of Table V, we use the ratio of individuals employed in non-profit organizations, and sort towns based on it.[45] This measure is not available for all Piedmont towns. We add dummies for the top and bottom third, and their interactions with the treatment. Towns at the top or bottom of the social capital distribution did not contribute differently from others.

Finally, we look at historical support for Berlusconi. If Berlusconi supporters were more likely to sort into new digital channels, we should detect a larger effect in towns with high historical support. Historical support is the average vote share of Berlusconi party in 1995, 2000 and 2005 Piedmont regional elections. We add dummies for the top and bottom thirds of the distribution of historical support, and interactions, to the baseline specification (columns (9) and (10). Towns with high historical support behave like others. The effect is 1.4 percentage points larger in towns where historical support was low. Statistical significance is not detected for towns closer to the border. Our interpretation is as follows: 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

[44]These findings are not at odds with Enikolopov et al. (2011), who document a short-run effect of availability of NTV in Russia on voting behavior. In the short run, all voters may be affected by a bias, hence an econometrician should not expect to find any significant mediating role of demographics. In our setting, we should find mediating effects of demographics if certain groups are more likely to be systematically persuaded than others.

[45]Guiso et al. (2008) use the number of non-profit organizations as a proxy. (Non)results are robust to using a dummy equal to one if a blood donation station exists in a town (Guiso et al., 2004), or the change in recycling over per capita income from 2005 to 2009.

added to lower bias exposure in changing beliefs about Berlusconi party of the former.

How many agents are systematically persuaded? 95% of Italians were exposed to 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.[46] The dissuasion rate can be interpreted as a lower bound for the fraction of voters who are systematically persuaded.[47]

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

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

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.[48] The denominator accounts for change 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

[46] 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.

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

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

DellaVigna and Kaplan (2007), despite a different correction term, since we do not exclude third party voters and we have opposite effects on 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 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 our 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 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 back out the ratio of viewers who can access digital TV and decide to watch new channels, we compute the number of households who watched new digital channels in Italy in March 2010, and divide it by the number of households who had access to digital TV in March 2010, to get the audience of new digital TV channels among digital users: $\alpha = \dfrac{0.093 \times 56,388}{31,646} = 0.1657$. Now, 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 over 5 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.[49] This implies that at least 30% of voters were systematically persuaded by biased information over the years. As expected, the magnitude is lower

[49]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.

than the negative persuasion rate of Enikolopov et al. (2011),[50] who find that being exposed to NTV convinces 66% of viewers not to vote for the government party, Unity.

VIII Understanding the debiasing mechanism

In the Internet Appendix, we show that the vote share of Berlusconi candidate's main opponent has slightly increased after the shock. Third party shares were unaffected. A scenario where all 2005 Berlusconi supporters voted in 2010, and some of them chose other parties, is inconsistent with this evidence. Therefore, we look at the effect of the shock on turnout to understand how lower exposure to bias affected voting.

Effect on turnout. In Table VI, turnout is the change in log of total voters in 2010 and 2005.[51] In all specifications, we control for a third-degree distance polynomial. We estimate Equation 1 with change in turnout as dependent variable. We add the change in 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.[52] Column (1) shows that turnout has not changed differently in switch off towns compared to other towns.[53] In column (2), we add interactions of the treatment with dummies which equal one if a town is above the top tercile or below the bottom tercile for ratio of elderly over total population. From Table V, we know that the shock to bias exposure had the strongest effect in towns with more elderly. Column (2) shows that turnout has dropped significantly more in those towns. Columns (3) to (5) use towns 75Km, 50Km and 25Km around the border. Exception made for column (4), the drop in turnout is significantly higher in treated towns with most elderly than other treated towns. The difference between change in turnout for towns in the middle and high old ratio groups is more negative for treated than control towns. The mean ratio of elderly in oldest treated

[50]Their bias consists of negative propaganda against the government party. In our case, treated voters are less exposed to a positive bias.

[51]Results are similar if we use the ratio of voters over voting age population.

[52]In the Internet Appendix, we show that youngest towns voted more for a new party which ran an aggressive internet campaign. Turnout has likely increased there. Voter turnout has also been used as a proxy for social capital (Guiso et al., 2004)

[53]Turnout has dropped everywhere. The average 2010-2005 change in turnout in Piedmont was -9.9 percent (s.d. 7.7 percent).

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 p.p. larger drop in 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 percent 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 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; c) turnout drops, especially in towns with more elderly and less educated voters; d) Berlusconi vote share drops, especially in towns with more elderly, less educated voters.[54] Figure V depicts this mechanism. The top two graphs plot the 2010-2005 change in turnout at Regional elections against the ratio of elderly. Each point is a town in Piedmont. Gray points are towns where the *absolute* number of votes for Berlusconi candidate in 2010 was higher than in 2005. In black towns, it was lower than in 2005. The top left graph refers to the control group, the top right to the treated one. In both groups, turnout decrases more the more the elderly. But for high ratio of elderly, turnout decreases more in treated than in control towns. Moreover, Berlusconi candidate was losing absolute votes in virtually all the former towns, and was gaining votes in some corresponding towns in the control group. Old towns in the treated area are those where turnout decrased the most, and where Berlusconi lost more absolute votes, i.e. supporters. The two bottom graphs plot the change in turnout between 2010 and 2005 over the ratio of college educated individuals in each town. The bottom left graph refers to control, the right one to treated towns. Grey and black points are interpreted as above. In towns with few college-educated voters, there is a mass of black points in the switch off area which is not observed in control towns. In the Internet Appendix, we show that this mechanism is consistent with voting behavior in all other Italian regions. The relationships between historical support and effect of shock to bias exposure, and digital TV usage and effect of shock are also shown to be consistent across all Italian regions.

[54]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.

IX Conclusions

We show that 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 voters watching new channels have been persuaded over time. Elderly and less educated voters were affected the most. After the drop to exposure, Berlusconi supporters went to vote less, and his vote share dropped. 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. The effects we document and the debiasing mechanism are economically and statistically significant, have the potential to change results of elections, and are valid across geographic areas and across elections. We discuss a series of Bayesian interpretations, and argue that they cannot fully explain our results. Among non-Bayesian interpretations, coarse thinking with limited memory, selective attention and persuasion bias are broadly consistent with them.

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 are voters exposed to biased political information, investors exposed to biased analysts forecasts, or patients exposed to biased treatment recommendations. In our setting, persuasion survived over time even if all Italians were aware that Berlusconi controlled most TV channels. Mandating disclosure of conflicts of interest is therefore not a sufficient provision. In the case of TV, protection of systematically persuaded agents should add to the rights of free speech and minority representation in motivating antitrust provisions. Apart from Italy, this is at odds with recent developments in several countries, such as Hungary (see letter Neelie Kroes), France (see complaints High-level group EU), Mexico and Thailand, as well as with the creation of large media conglomerates, such as Murdoch's News Corporation.

Our results also shed light on why tycoons with strong ideological preferences invest large financial resources in media conglomerates: if broadcasting biased information persuades

the audience over the years, investing is particularly worthwhile. Last but not least, results speak to the case of Italy, a member of the *Group of Eight* and the 8^{th} largest economy in the world. For years, people have wondered if media ownership concentration in Berlusconi's hands has favored his acquisition and maintainance of power. This is relevant to economic stability worldwide, as the recent EU debt crisis made salient. Results imply that TV has been crucial to create and maintain political consensus towards Berlusconi over the years.

Finally, implications of these results 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 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 excessively intrusive of free will and determination of individuals? This paper has barely scratched the surface of issues that await to be dissected by researchers in many disciplines.

Appendix

In this section, we define variables which we labeled *Electoral controls* and *Socio-demographic controls* in the paper. All variables are observed at the town level unless otherwise specified. Since our identification is based on a regression discontinuity design, we provide evidence that observable factors other than the treatment condition vary smoothly across the boundary in Table VII.

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

- *Precints*: number of electoral precints in a town. This variable proxies for the size of towns. We prefer this to total population which would correlate with other variables in the RHS.
- 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. In European Parliament elections, voters choose a party list and are allowed to assign a preference to one candidate on the list.
- *Share Berlusconi Prov. pre10*: share of Berlusconi candidate vote share in the closest Province Presidential elections before 2010. Rules for Province elections are similar to Regional elections: voters pick a candidate to the office of Province President. They can also express a preference for one candidate in a party list to enter the province Parliament.
- Δ *csx 05-00*: change in center-left candidate (main opponent) vote share between 2005 and 2000 Regional Elections.

- Δ *csx 00-95*: change in center-left candidate (main opponent) vote share between 2000 and 1995 Regional Elections.

Socio-demographic controls are taken from the 2001 Census run by *Istituto Nazionale di Statistica* (Istat) or other sources, which we specify below. They include:

- Δ *unemployment 10-01*: change in unemployment rate between 2010 and 2001. (Istat)

- Δ *unemployment 09-05*: change in unemployment rate between 2009 and 2005 at the province level. This measure is added to capture different trends in unemployment in the West and the East. For instance, the change in unemployment from 2001 to 2010 might be the same for two towns in the West and the East, but unemployment in the East may have risen at the beginning of the decade, while in the West closer to 2010 Regional elections. (Istat)

- Δ *perc. foreign 09-05*: change in the percentage of foreign residents between 2009 and 2005. This variable controls for immigration patterns, which have been a major topic in all Italian electoral campaigns over the last decade. (*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 2009 and 2005. This variable is added to capture the effect of first immigrants arriving to small towns: although the percentage of these immigrants with respect to local population might be low, the fact the immigrants joined the community per se makes immigration a salient issue to locals. (*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 (in liters) assigned to Piedmont farms at the province level. Milk production quotas have been an important issue for local elections in Piedmont over the last years.

- Δ *recycling inc 09-05*: change in percentage recycling over average taxable income, as a proxy for social capital at the town level. (*Sistema Piemonte, available at http* : $//www.sistemapiemonte.it/webruc/raccoltaRifiutiReportAction.do?btnAggiorna = aggiornaComuniDaComune$

- *Events environment 09-05*: number of interventions by the Regional Environmental Agency to address heavy pollution events, as a proxy of environmental quality at the town level (*Anagrafe Regionale Siti Contaminati*, available at http://www.regione.piemonte.it/ambiente/bonifiche/home.htm)

- *Newsagents pop 09*: number of newsagents per 1000 inhabitants, as a proxy for availability of alternative information sources. (*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. Many of these stores also sell newspapers. (*Regione Piemonte, Osservatorio Commercio*, available at $http://www.regione.piemonte.it/commercio/ossCommercio.htm$)

- *Arci*: dummy equal to one is an *Arci* club exist in town, i.e. leftish meeting points for elderly and youngsters. These clubs often represent the only meeting point in small towns. (*Associazione Ricreativa e Culturale Italiana Piemonte*, available at $http://www.arcipiemonte.it/affiliati\_piemonte$)

- *Acli*: dummy equal to one is an *Acli* club exist in town, i.e. catholic meeting points for elderly and youngsters. These clubs often represent the only meeting point in small towns. (*Associazioni Cristiane Lavoratori Italiani*, 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 town. (*Associazione Volontari Italiani Sangue*, available at $http : //www.avis.it/usr_view.php/ID = 1403$)

- *Density 01*: number of inhabitants per squared Km from 2001 Census. (Istat)
- *Male 01*: share of males over all inhabitants from 2001 Census. (Istat)
- *Hsize 01*: average number of components per household from 2001 Census. (Istat)
- *Manufacturing 01*: number of employees in manufacturing from 2001 Cesus. (Istat)
- *Services 01*: number of employees in services from 2001 Cesus. (Istat)
- *Tourism 01*: percentage of days hotel rooms are occupied over the whole year from 2001 Cesus. (Istat)
- *Banking 01*: number of checking accounts per 100 inhabitants from 2001 Census.
- *Cars 01*: number of cars per 100 inhabitants from 2001 Census. (Istat)
- *Students 01*: number of high school students from 2001 Census. (Istat)
- *Health care efficiency 01*: number of days $\times$ patients needed to recovery over one year from 2001 Census. (Istat)
- *Chemists 01*: average number of inhabitants per each chemist shop from 2001 Census. (Istat)
- *Disp. income 01*: average disposable income per inhabitant from 2001 Census. (Istat)
- *Farms 01*: number of farms from 2001 Census. (Istat)
- *Large HH 01*: number of households with 5 or more components from 2001 Census. (Istat)
- *Retired 01*: number of inhabitants from 65 to 79 years old from 2001 Census. (Istat)
- *Very Old 01*: number of inhabitants older than 80 years old from 2001 Census. (Istat)

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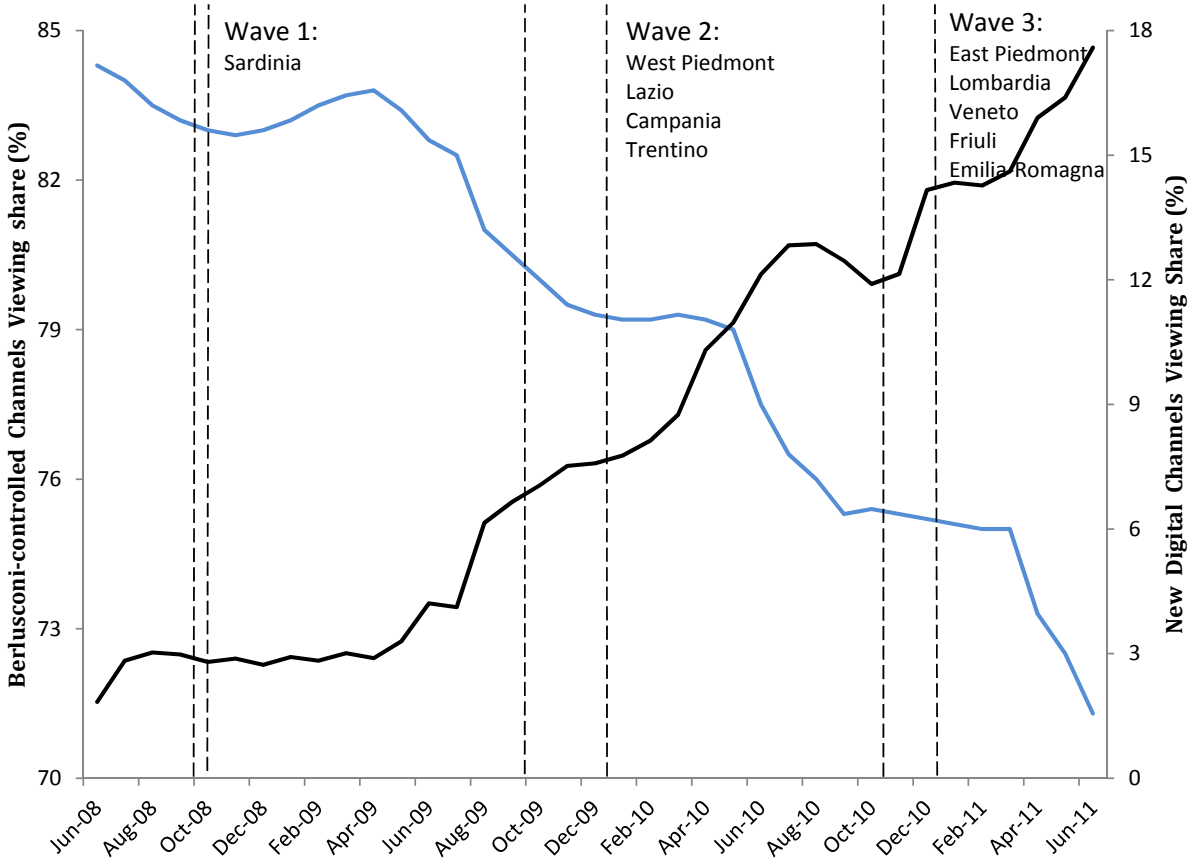
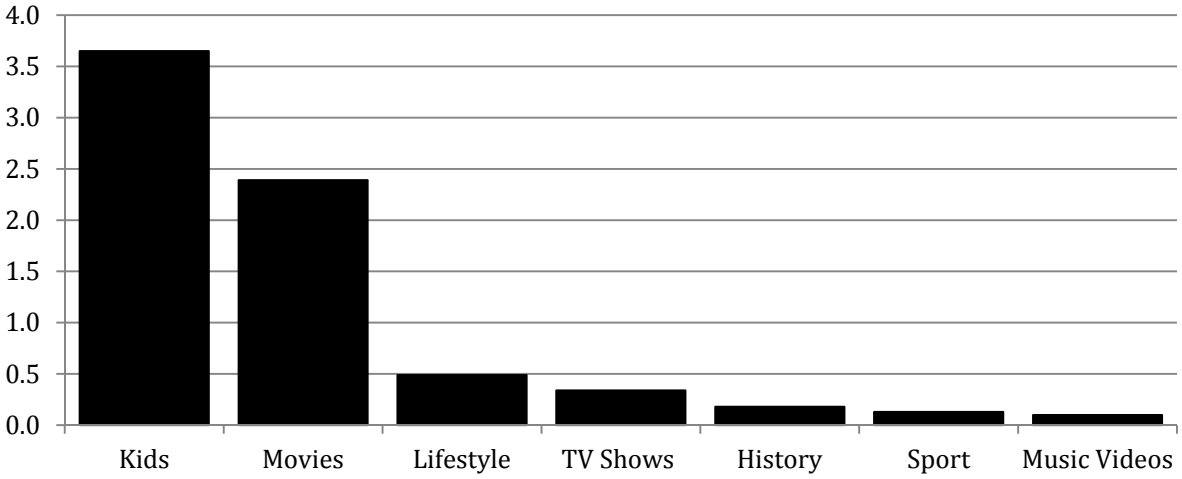
Figure I: **Shock to Bias Exposure and Viewers Reaction**

A. Viewing shares of Berlusconi-controlled channels (left) and new digital TV channels (right) around waves of deadlines to move to digital TV

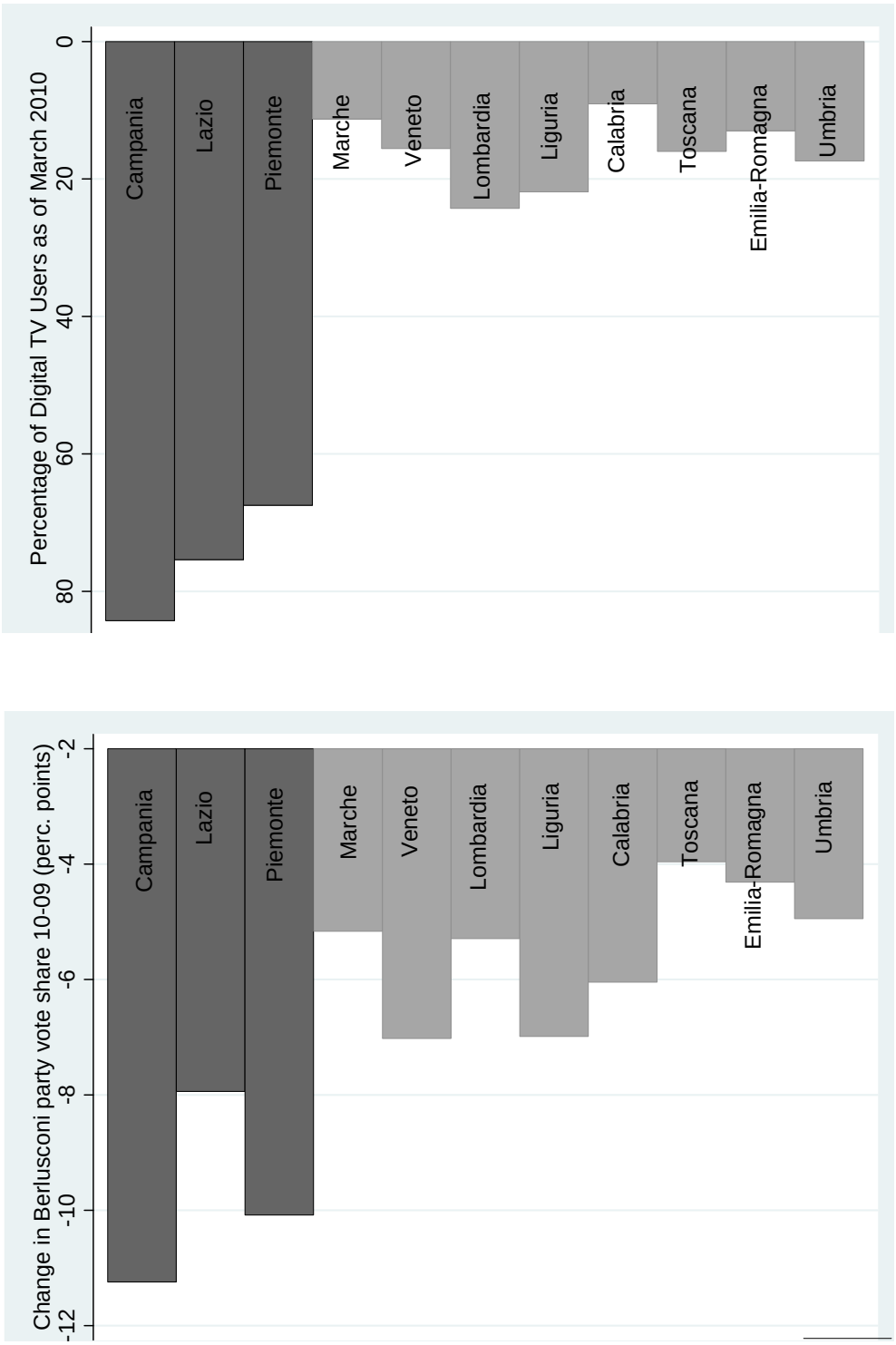


B. Daily Viewing shares of new digital TV channels by content as of March 2010 (6:30-8:30 pm)



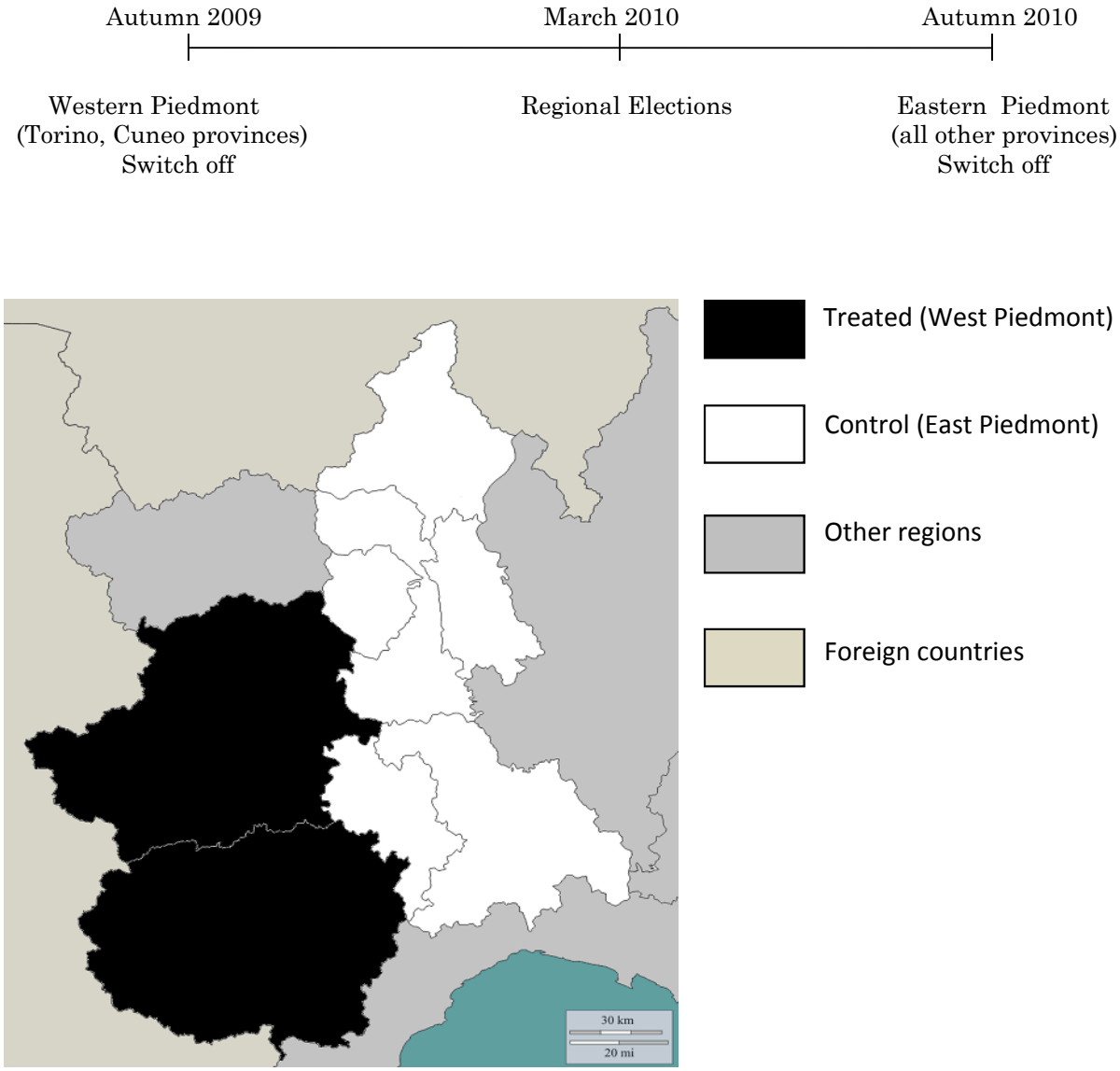
In Panel A, the left axis reports the average monthly viewing share of Berlusconi-controlled channels over from June 2008 to June 2011 (light blue line). The right axis reports the share of new digital TV channels over the same period (black line). "Wave 1", "Wave 2" and "Wave 3" refer to the first, second and third waves of deadlines to switch to digital TV. Areas affected by each wave are enlisted. Panel B shows the viewing shares of new digital TV channels by content from 6:30 to 8:30 pm daily in March 2010. This is the time slot when two major Italian news programs, Tg1 and Tg5, air on Berlusconi-controlled channels. Evidence of the drop in viewing shares of news programs is in the Internet Appendix.

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



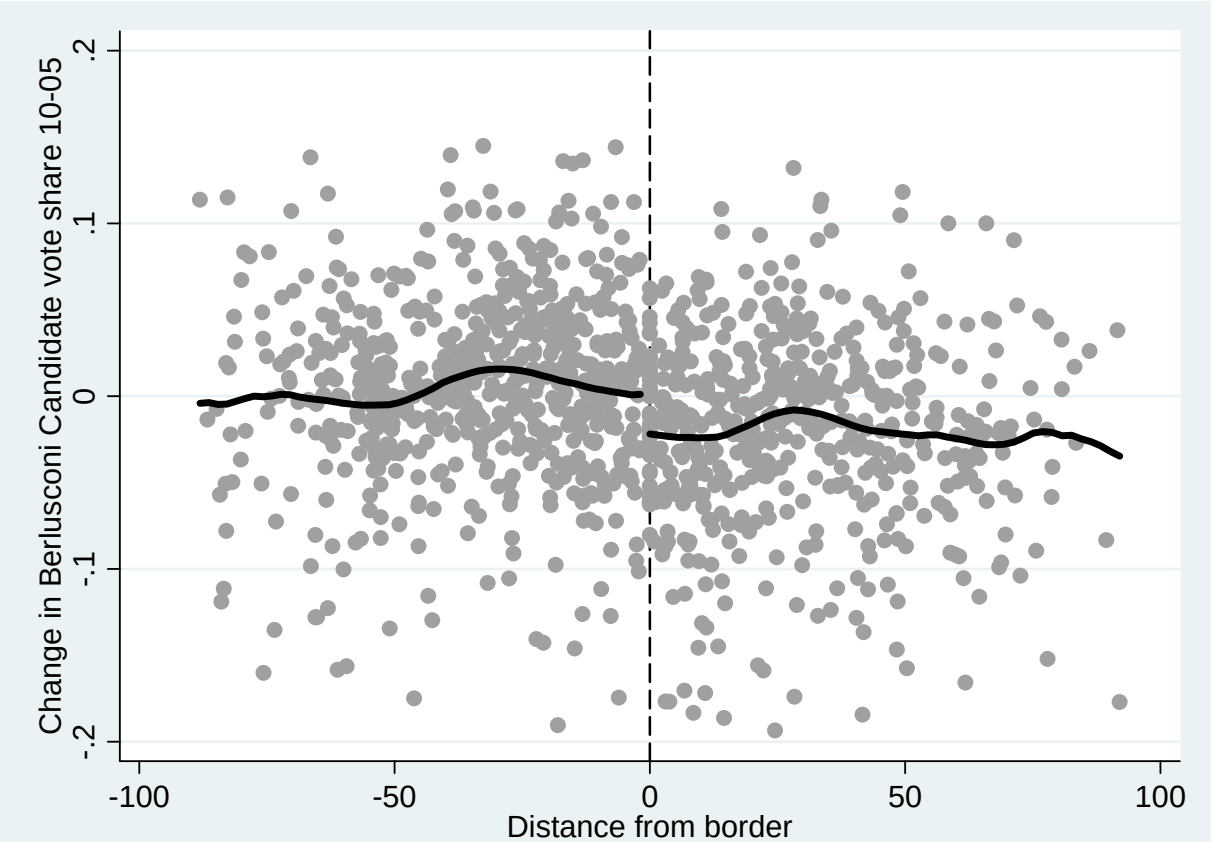
This picture compares the ratio of digital TV users and Berlusconi Party performance across Italian Regions where elections were held in March 2010. The top graph shows the percentage of digital TV users as of March 2010 for each region. The bottom graph shows the change in Berlusconi party vote share 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.

Figure III: **Natural Experiment: Switch to digital TV in Piedmont and 2010 Regional 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]

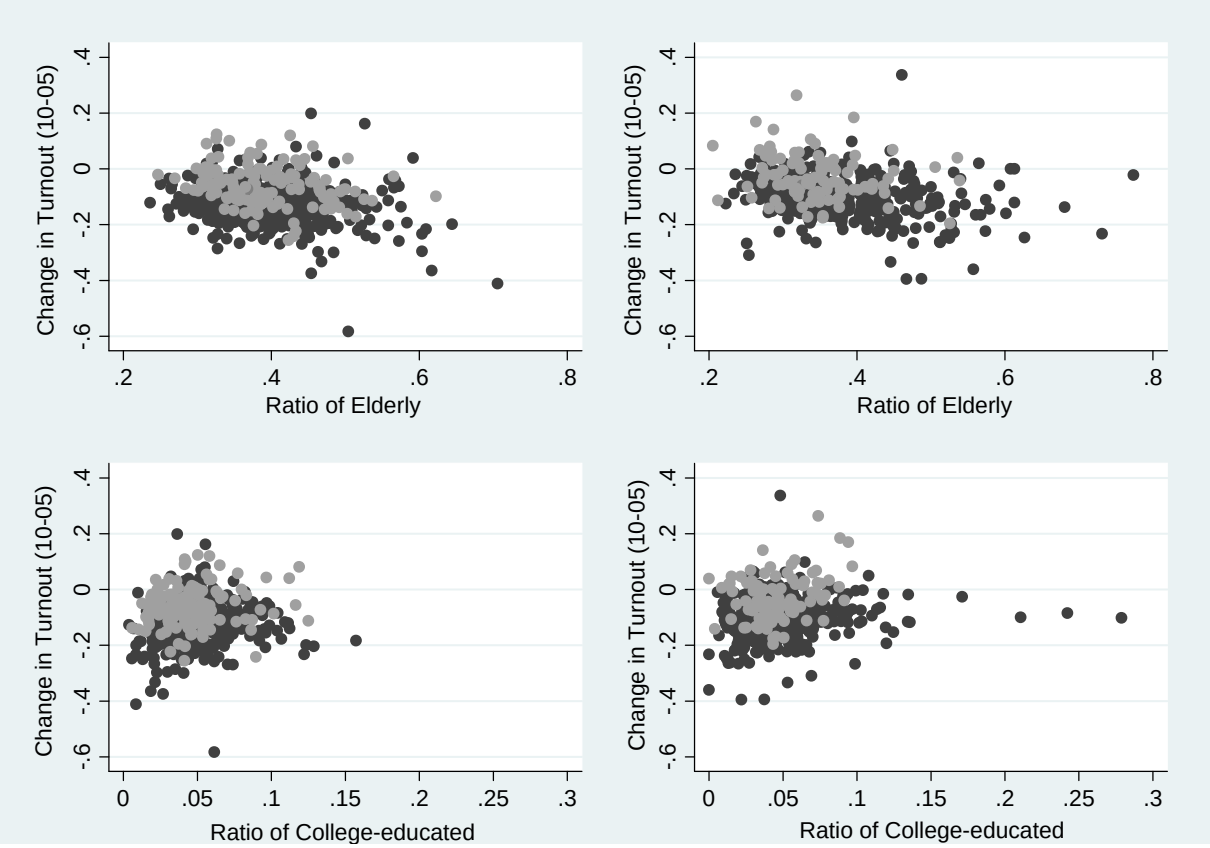
Figure IV: **Change in Berlusconi candidate performance around treatment border**



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. Distances are 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.

Figure V: **Debiasing Mechanism**

Eastern towns (control) Western towns (treated)



The graph below plots the change in turnout between 2010 and 2005 regional elections in Piedmont against the ratio of elderly (top graphs) and the ratio of college graduates (bottom graphs) for each town in Piedmont. Turnout is the change in logarithm of voters in 2010 and 2005. Graphs on the left refer to control towns (East), while those on the right refer to treated towns (West). Each point is a town in Piedmont. Grey points correspond to towns where Berlusconi candidate in 2010 obtained more absolute votes than in 2005. Black points correspond to towns where Berlusconi candidate in 2010 obtained less absolute votes than in 2005.

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.

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 limiting the analysis to 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 Cameron and Miller (2011). 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%.

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 limiting the analysis to 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 Cameron and Miller (2011) for downward bias. Significance is as follows: *10%, **5%, ***1%.

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 even columns the analysis is limited to towns within 50Km from the border in both directions. 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 Cameron and Miller (2011) for downward bias. Spatial HAC s.e. allow for spatial dependence of unknown form following Conley (1999). Significance is as follows: *10%, **5%, ***1%.

Table V: **Interactions 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 Cameron and Miller (2011) for downward bias. Wild bootstrap s.e. follow the procedure suggested by Cameron and Miller (2011). Significance is as follows: *10%, **5%, ***1%.

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 Cameron and Miller (2011). Significance is as follows: *10%, **5%, ***1%.

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