Telecracy: Testing for Channels of Persuasion

By GUGLIELMO BARONE, FRANCESCO D'ACUNTO AND GAIA NARCISO*

We consider the long-lived slant towards Berlusconi in political information on Italian TV. We exploit a shock to the slanted exposure of viewers: idiosyncratic deadlines to switch to digital TV from 2008 to 2012, which increased the number of freeview channels tenfold. The switch caused a drop in the vote share of Berlusconi's coalition by between 5.5 and 7.5 percentage points. The effect was stronger in towns with older and less educated voters. At least 20% of digital users changed their voting behavior after the introduction of digital TV. Our evidence is consistent with the existence of persuasion-biased viewers.

There is growing evidence that exposure to slanted information affects decision-makers' choices. This is true in several domains, such as political information (DellaVigna and Kaplan, 2007; Enikopolov et al., 2011), financial analyst forecasts (Malmendier and Shanthikumar, 2007), and product advertisements (Meyers-Levy and Malaviya, 1999). But are slants in information effective in the long run? If so, why do individuals not account for systematic slants over time?

* Barone is at the Bank of Italy and RCEA (e-mail: guglielmo.barone@bancaditalia.it). D'Acunto is at the Haas School of Business, UC Berkeley (e-mail: Francesco_dacunto@haas.berkeley.edu). Narciso is at Trinity College Dublin, CReAM and IIIS (e-mail: narcisog@tcd.ie). The views expressed are those of the authors and do not necessarily reflect those of the Bank of Italy. For very helpful comments and discussions, we thank Stefano DellaVigna, Ruben Enikolopov, Matthias Heinz, Philip Lane, Ross Levine, Ulrike Malmendier, Gustavo Manso, Enrico Moretti, Terry Odean, Ted O'Donoghue, Benjamin Olken, Torsten Persson, Maria Petrova, Josh Schwartzstein, Guido Tabellini and seminar participants at the 3rd TILEC Workshop (Tilburg), the 13th MOOD Workshop (EIEF), the X Media Economics Workshop, the 2012 European Economic Association Congress, the 2012 American Law and Economics Association Annual Meeting, the 2012 MILLS Workshop, and UC Berkeley (Haas). We also thank Michael O'Grady for excellent research assistance. D'Acunto gratefully acknowledges financial support from the White Foundation. All errors are our own.

To address these questions, we consider the long-lived slant towards former Prime Minister Silvio Berlusconi in political information on Italian TV (Durante and Knight, 2012). For 10 years within the period 1994 to 2011, Berlusconi controlled six out of seven national channels, due to his dual role as a media tycoon and prime minister. In the years when he was not prime minister, he directly controlled three channels and influenced the others through the executives he had appointed while in office. We exploit a quasi-random shock to the slanted TV exposure of Italian viewers: idiosyncratic deadlines to forcibly switch from analog to digital TV, from 2008 to 2012. At the deadlines, analog signals were switched off, and only digital signals kept on airing. The switch was imposed by the European Union. Switching deadlines were spatially heterogeneous and idiosyncratic: they were assigned to groups of Italian provinces based on the similarity of transmission infrastructures built in the 1950s. Digital TV improved transmission efficiency and increased the number of free national channels tenfold. 51 out of 78 new channels are aired by new media companies, which have no ties to Berlusconi or to the government.[1] After switching to digital TV, many Italian households changed their viewing habits. From October 2008 to May 2013, the average monthly share of viewers of old channels dropped from 86% to 66%. Over the same period, the share of viewers of new digital channels increased from 3% to 24% (Figure 1).

We employ a spatial regression discontinuity strategy to estimate the causal effect of the drop in exposure to media slant on voting behavior. Our main analysis uses Piedmont, the only Italian region where different cities switched to digital TV around elections. Towns in western Piedmont switched six months before the elections, while towns in eastern Piedmont switched six months after the elections. We show that the vote share of Berlusconi's coalition candidate

[1] Source: e-Media Institute and DGTVi.

dropped by between 5.5 and 7.5 percentage points after the switch. This effect is economically and statistically significant, and is robust to several alternative specifications and placebo tests. We estimate that at least 20% of digital users changed their voting behavior after the introduction of digital TV. Furthermore, we provide evidence on the validity of our results across regions and across elections.

To interpret the results, we first investigate which demographics stopped supporting Berlusconi's coalition once on digital TV. We find that the drop in the vote share of Berlusconi's coalition candidate was higher in towns with older and less educated voters. Moreover, turnout was a channel through which switching to digital TV affected voting. In treated towns with a high ratio of elderly, turnout was 3 percentage points lower, and the vote share of Berlusconi's candidate dropped by 2.3 percentage points more than in other treated towns. We argue that persuasion bias is a plausible explanation for our evidence.

This paper falls within the literature on persuasion in economics (DellaVigna and Gentzkow, 2010) and on media and political outcomes (Prat and Stromberg, 2011). In the short run, media bias may affect rational agents who do not know when information is omitted (Besley and Prat, 2006; Kamenica and Gentzkow, 2011). In the long run though, DellaVigna and Kaplan (2006) show that media bias does not affect Bayesian agents, while it affects persuasion-biased agents. In their model, the latter systematically fail to take into account the full extent of bias when updating their beliefs. To interpret our evidence, we propose a theoretical framework that builds on their model.

On the empirical side, Gentzkow and Shapiro (2006) describe a media slant of outlets that respond to the preferences of viewers. Cagé (2013) shows that higher competition among information sources may lead to a lower amount of

information produced in equilibrium, hence lower turnout at elections. In our setting, there is no increase in competition among information sources, because digital viewers sort from news programs to all-entertainment channels.

As in DellaVigna and Kaplan (2007) and Enikolopov et al. (2011), we show that media bias affects voting behavior. However, our contribution is novel in three ways. First, we look at lower exposure to a long-lived pervasive slant, instead of higher exposure to a new biased outlet. This allows testing if media slants are effective in the long run, in a setting where the slant is systematic and voters know who controls most TV channels. The long-lived slant also allows testing if any demographics are more affected by media slant than others. Mediating effects of demographics have not been detected in previous research that looked at short-run biased exposures. We do find that the elderly and the least educated are most likely to change their voting behavior after the long-run exposure drops. Second, because virtually all Italians were exposed to the slant before digital TV, we can estimate the fraction of the total population that changed their voting behavior once on digital TV. Third, the magnitude of our effect has the potential to change election outcomes: in 2010, the vote share of Berlusconi's coalition candidate dropped by at least 5.5 percentage points, out of an overall vote share of 52% in 2005.

In the rest of the paper, Section I describes the institutional setting, while Section II shows Italian households' reaction to the introduction of digital TV. In Section III, we propose a theoretical setting that differentiates between Bayesian and persuasion-biased voters, while in Section IV we discuss our identification strategy. Section V presents empirical results and robustness. In Section VI, we look at how many and which demographics were persuaded the most over time,

and in Section VII we interpret the magnitude of the effects. Section VIII investigates the debiasing mechanism and discusses alternative interpretations, and Section IX concludes.

I Institutional Setting

Television in Italy. Italian TV began airing on an analog infrastructure in the 1950s. The analog system consisted of seven national channels, 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 later renamed La7. Local channels aired at the town or regional level, covering local news and often not airing for 24 hours. Frequencies were assigned by the government, making TV a highly regulated industry. Given the limited penetration of satellite technology, Italian TV has been a de facto duopoly for decades. Rai and Mediaset alone were still attracting more than 86% of Italian viewers in 2008 (Figure 1).

Not only has Berlusconi owned three out of the seven national channels since the 1980s, but he also founded and has led a major political party since 1993. He has been Prime Minister three times: from 1994 to 1995, from 2001 to 2006 and from 2008 to 2011. In those years, he picked the main executives not only for the Mediaset network, but also for the governmental one. This situation raises concerns that a slant exists in favor of Berlusconi's party on Italian TV. Durante and Knight (2012) document such a slant, which is stronger when Berlusconi is in power.

The slant in Italian information on TV has lasted since 1994. Despite the gradual diffusion of the internet, more than 85% of Italians were still relying on TV as their unique or main source of political information in 2009.[2]

Digital TV. Since 2008, a new transmission technology has been put forward: terrestrial digital TV, which dramatically enhances transmission efficiency. Digital TV uses existing analog infrastructures, hence it avoids the high setup costs of cable and satellite TV. Recipients own a decoder, available for as low as 50 euro. Yet, to ensure that anyone could go digital, the government established a voucher plan to subsidize economically disadvantaged households.

II Shock to Bias Exposure and Viewers' Reaction

Switch to digital TV. Moving to digital TV from 2008 to 2012 represented a major shock to the supply of TV channels in Italy. 78 new free channels air on digital TV at the national level, 51 of which have no ties to Berlusconi or to the governmental network.[3] In 2006, the government regulated the transition from analog to digital TV, as mandated by European Union legislation.[4] 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 broadcasting was allowed. Households could have switched to digital TV before the deadline and were forced to do so by the switch-off date, otherwise their TV would have not displayed anything. Assigning switch-off dates to areas of the country was idiosyncratic to the purposes of our analysis. The criteria were the similarity of 1950s infrastructures, and the

[2] See "VIII Rapporto Censis/Ucsi sulla Comunicazione" (www.censis.it).

[3] Source: e-Media Institute and DGTVi.

[4] 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.

homogeneous move for north, center and south of Italy.[5] Therefore, the criteria could not be manipulated by national or local politicians, or by other local interest groups.

Viewers' reaction. Did the switch change the viewing habits of Italians? Panel A of Figure 1 shows the average monthly share of viewers of TV channels airing on both analog and digital TV over the period June 2008-May 2013. We plot the monthly average fraction of unique viewers for each type of channel in the time slot 6-8:30pm, during which news programs air on analog channels. The dashed line is the share for the six analog channels and is associated with the left axis. These channels dropped from a combined viewing share of 86% in June 2008 to 66% in May 2013 (-23%). The unreported viewing share of Berlusconi's network alone decreased from 40% to 31% over the same period (-23%).

The right axis shows the viewing share for the new digital TV channels (solid line). This share increased from about 3% to 24% over the period June 2008-May 2013. The overall share of Italians watching TV did not change during the process of analog switch-off (see Section VIII). Thus, more than 20% of viewers moved from previously existing channels to the new digital ones once the latter became available.

Sorting into all-entertainment channels. After the switch, viewers sorted out of news programs on slanted channels and into new, all-entertainment digital channels. Panel B of Figure 1 plots the average viewing shares of new digital channels by content in March 2010 for the slot 6pm to 8:30pm, when all evening news programs air on slanted channels. Figure A1 in the Online Appendix shows that the shares of the two major news programs have dropped by the same amount gained by the new digital channels. These drops are not driven by higher TV consumption. For instance, the number of unique viewers of the main Italian news

[5] See http://www.agcom.it/default.aspx?message=viewdocument&DocID=2708.

program (Tg1) dropped by almost 1.5 million between 2007 and 2011, as discussed in Section VIII. Figure A1 in the Online Appendix also shows that movers to digital channels did not sort into digital news programs.

Insert Figure 1 here

III Theoretical Framework

We propose a theoretical framework based on DellaVigna and Kaplan (2006) to interpret the effect of the systematic media slant towards Berlusconi's coalition on voters' beliefs. Voters choose a candidate based on ideology and the perceived quality of contestants. In each electoral period t before the move to digital TV, the media report on two candidates: one for Berlusconi's coalition and the other for the center-left coalition. Voters do not observe the quality of candidates; rather, they infer it from the media reports to which they are exposed. In each period t, the differential quality of Berlusconi's candidate and the center-left candidate is q_t, where $q_t \sim N(0,1/\kappa)$. The media observe q_t, but they issue systematically slanted reports in favor of Berlusconi's candidate, $m_t = q_t + s$, where s is time invariant and drawn from the distribution $N(s_0,1/\sigma)$, and $s_0 > 0$.

Bayesian agents. Bayesian voters update their beliefs about the extent of media slant over time, based on media reports. That is, whenever they face a report, they realize it may be positive due to the superior quality of Berlusconi's coalition candidate or to the media slant. In period T, they will estimate the media slant to be

$$\hat{s}_T = \frac{\sigma s_0 + T\kappa \bar{m}_T}{\sigma + T\kappa}$$

where $\bar{m}_T = (\frac{1}{T}) \sum_{t=1}^{T} m_t$. This is because the voters can only estimate the extent of slant through the media reports, which are independently normally distributed.

$\hat{s}_T$ is thus a precision-weighted sum of signals. This weighted sum is also a normally-distributed random variable. $\hat{s}_T$ is a convex combination of the mean slant and the average media report, where weights are the precisions of the priors regarding the bias and the observed reports. Before the move to digital TV, Bayesian voters will estimate the differential quality of Berlusconi's coalition candidate as a precision-weighted average between the difference of the reported quality and the estimated amount of media slant, and the prior about the differential quality, which is zero:

$$\hat{q}_{T,pre} = \frac{\kappa * 0 + Q(m_T - \hat{s}_T)}{\kappa + Q}$$

where Q is the precision of the estimated quality $\hat{q}_T$, i.e. the reciprocal of $\hat{q}_T$'s variance. All quantities are derived in Appendix A.

Persuasion-biased agents. We now model non-Bayesian persuasion by introducing the category of persuasion-biased agents. These agents systematically underestimate the extent of slant in reports issued by the media. Whenever the media issue a report $m_t = q_t + s$, persuasion-biased agents think that the report is $m_t = q^{\lambda}_t + (1 - \lambda)\ s$, where $\lambda \in [0,1]$. This case embeds Bayesian updating for $\lambda = 0$. To simplify matters, we follow DellaVigna and Kaplan (2006) and assume that persuasion does not directly affect the estimation of the media bias $\hat{s}_T$ or of the precision of the estimated quality of politicians, Q. The differential quality of politicians estimated by a persuasion-biased agent at time T is given by:

$$\hat{q}^{\lambda}_{T,pre} = \frac{\kappa * 0 + Q(m_T - (1 - \lambda)\hat{s}_T)}{\kappa + Q}$$

In the following proposition we compare the effects of a systematic media slant on the estimated differential quality of politicians for Bayesian and persuasion-

biased agents. While media slant affects all agents in the short run, it only affects the estimation of quality by persuasion-biased agents in the long run.

Proposition I. *(i) For any finite T, a higher media slant increases the estimated differential quality of Berlusconi's coalition candidates by all voters. The increase is larger for voters more subject to persuasion bias (higher* λ*). (ii) In the long run (*$T \to \infty$*), media slant only affects voters subject to persuasion bias (*$\lambda > 0$*).*
Proof: see Appendix A.

Intuitively, the media slant has a direct, positive effect on the perceived quality of Berlusconi's coalition candidates for all voters. It also has an indirect, negative effect due to the media reports voters are exposed to over time, which increase their estimate of the media slant. At any finite point in time the direct effect is higher than the indirect one, leading to a positive effect of media slant in the short run. Because the indirect effect is lower for higher values of λ, the effect of media bias is larger for persuasion-biased agents in the short run. In the long run, Bayesian agents' estimation of the slant converges to the true slant *s*, hence they estimate the true differential quality of Berlusconi's coalition candidates despite the slant in the media. This is not true for persuasion-biased agents, who are always affected by the slant as long as they are exposed to news reports.

Switch to digital TV. We can think of the switch to digital TV as a sudden stop to the exposure to slanted news reports. Viewers who moved from analog to new digital channels sorted into all-entertainment programs (see Section II). Also, they did not sort into alternative sources of information, such as the internet or newspapers (see Figure A2 and Figure A3 in the Online Appendix). In the model, if voters do not observe any media reports, their estimated differential quality of Berlusconi's coalition candidate must equal their prior. This holds for both

Bayesian and persuasion-biased agents, because persuasion bias only affects the interpretation of media reports. Hence, after the shock to exposure, the estimated differential quality of Berlusconi's coalition candidate will be

$$\hat{q}_{T,post} = \hat{q}^{\lambda}_{T,post} = \frac{\kappa * 0}{\kappa} = 0$$

In the following proposition, we show that the change in the perceived quality of Berlusconi's coalition candidate after the stop to exposure to slanted media reports is higher for persuasion-biased agents than for Bayesian agents.

Proposition II. *(i) Once the exposure to slanted media reports stops, the drop in the perceived quality of Berlusconi's coalition candidates is larger for persuasion-biased agents than Bayesian agents. (ii) The higher the degree of persuasion* λ*, the larger the drop in perceived quality*.
Proof: see Appendix A.

Intuitively, the drop in perceived quality is higher for persuasion-biased agents, because their estimates of quality were biased upwards before the stop in exposure to the slant. Since this bias is a linear function of the persuasion parameter λ, the drop increases linearly in λ.

Difference-in-differences strategy. In Figure 2, we express the theoretical framework in a form that directly maps into the empirical strategy described in Section IV.

Insert Figure 2 here

We exploit the double difference in the perceived quality of Berlusconi's coalition candidate over time and across areas that switched to digital TV before and after the 2010 elections. The perceived quality before the switch is identical

for all viewers. After the switch, the perceived quality drops to the prior, that is zero, for viewers who switched to digital TV. For viewers who had not switched before the elections, this quantity is instead the estimated slant in news reports. Ultimately, the difference-in-differences quantity we aim to estimate is given by the perceived quality of Berlusconi's coalition candidate by non-switchers at the time of elections.

Assume that voting for Berlusconi's coalition candidates is a mapping of the perceived differential quality of candidates and of ideology. Also, ideology does not vary around the exogenous move from analog to digital TV. Proposition 2 implies that we should observe a drop in Berlusconi's coalition vote share after the move to digital TV, as long as not all voters filtered out the media slant in full. Moreover, we should observe a larger drop for voters who are more affected by persuasion bias.

In the rest of the paper, we design a test for these implications of the theoretical framework. We also show additional evidence about the effect of the stop in slanted exposure on the electoral support of Berlusconi's coalition.

IV Identification Strategy and Data

Spatial RD design. Our identification strategy is based on a natural experiment: idiosyncratic deadlines to forcibly switch from analog to digital TV in Italy around the 2010 regional elections. Near the switch date, the probability that households are on digital TV jumps to about one. As discussed in Section II, voters who switch before elections are not exposed to the Berlusconi media slant, while voters who switch before the elections are. Figure 3 describes the natural experiment. Piedmont is the only region where some towns (West, black) switched in autumn 2009, that is, six months before the elections. The other towns (East, white) went digital in autumn 2010, six months after the elections. Switch-

off dates were assigned at the level of provinces, i.e. governmental partitions between region and towns. The timing of the switch to digital TV by Western Piedmont households is particularly suitable to the analysis. According to survey evidence from Itanes (Italian National Election Studies), 37% of Italian voters decided who to vote for no earlier than a few weeks before the 2008 elections. Undecided and non-ideological voters are likely to be most responsive to information slant on TV.

Insert Figure 3 here

Estimating equation. 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. To test whether the electoral support for Berlusconi's candidate has changed after the stop to exposure to slanted media, we estimate variations of the following specification:

$$\Delta Berlusconi_{10-05ipb} = \alpha + \gamma Switch-off_p + \mathbf{X}'_{pre10ip}\delta + f(distance_i) + \Phi_b + \varepsilon_{ibp} \quad (1)$$

where $\Delta Berlusconi_{10-05ipb}$ is the change in the vote share of Berlusconi's candidate between 2010 and 2005 regional elections in town *i*, province *p*, along segment *b* of the treatment boundary, while $Switch-off_p$ is an indicator which equals one if province *p* is in Western Piedmont, i.e. it is a treated province. $\mathbf{X}_{pre10ip}$ is a set of town-level electoral and socio-demographic observables expressed in differences or levels. The full list of controls, which also include previous electoral performances of Berlusconi's coalition, is described in Appendix B. $f(distance_i)$ 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. Φ_b is a set of five border segment fixed effects as in Dell (2010). They average out unobserved characteristics common to towns at similar latitudes on each side of the border. Identification is based on three assumptions: i) all observable and unobservable characteristics vary smoothly at the 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; and iii) there is no sorting around the border, and all households are compliant with their assigned condition. We examine the plausibility of i) in Table 1, which reports summary statistics for town-level electoral and socio-demographic characteristics. Each panel of Table 1 shows means of variables for treated (*Switch-off*) and control (*No Switch-off*) towns. P-values for paired t-tests of the difference of means across groups are also reported.

Insert Table 1 here

The first panel shows statistics for the full sample, while others look at towns within 50km, 25km and 15km around the border. Election outcomes include the change in Berlusconi's candidate and main opponent vote shares across 2005-2000 and 2000-1995 regional elections. None of these changes are different across treated and control towns, neither for the full sample nor for towns close to the border. Socio-demographics include variables in differences and levels. Mean population in 2009 captures the size of towns before elections. It is not statistically different across treatment and control towns. Magnitudes differ because Western Piedmont includes Turin, which had more than 900,000 residents in 2009. The share of employees in manufacturing and in services is similar across groups. The same holds for the change in foreign residents and

income-adjusted recycling between 2009 and 2005. Both have been relevant topics in local Italian elections over the last decade.

As for assumption ii), one would ideally only rely on observations at the border. Unfortunately, there are not enough towns at the border to do that. Hence, we follow Dell (2009) and identify a causal effect with the model outlined in Equation 1.

Assumption iii) requires no sorting across the border. In our setup, moving from control to treated provinces, in order to gain access to digital TV, seems very implausible.

Standard errors. In a spatial RDD framework, residuals may be correlated at the level of the provinces at which the treatment is assigned. We correct standard errors in three ways. First, we cluster them at the province level. There are 8 provinces; standard errors are likely biased downwards. As a finite-sample correction, we multiply the error terms by $\sqrt{\frac{C}{C-1}}$ where C is the number of clusters, to estimate the variance-covariance matrix. We then use critical values of a t-student distributed variable, with $C-1$ degrees of freedom, to establish statistical significance. If we use the rule of Donald and Lang (2007), results do not change. We alternatively account for the small number of clusters by wild-bootstrapping at the cluster level. Cameron et al. (2008) show that this method is reliable and superior to other asymptotic tests with data clustered in as few as five groups. To allow for comparability across methods, we derive standard errors as if the bootstrapped t-statistics was asymptotically normally distributed. These two frameworks assume that errors for towns in different provinces are uncorrelated. We therefore also use the procedure of Conley (1999), which allows for spatial dependence of unknown form across town-level residuals. We use a bandwidth of 0.25 degrees in longitude and latitude, that is, approximately 30km in each dimension. This gives more conservative standard errors than other bandwidths.

Weighting scheme. We estimate Equation 1 using both OLS and weighted least squares, where weights are the average logarithm of total voters in 2010 and 2005 elections. Electoral data are plausibly more precise in large towns (DellaVigna and Kaplan, 2007).

Baseline covariates. We add baseline covariates to reduce the sampling variability in the estimator (Lee and Lemieux, 2010). Detailed definitions and summary statistics for all covariates are in Appendix B. Electoral controls are changes in the vote share of Berlusconi's candidate across elections before digital TV was introduced. Voting data are from DWSIDE (DataWarehouse Sistema Integrato Dati Elettorali) by Osservatorio Elettorale at Consiglio Regionale del Piemonte. We add demographic controls at the town level from the 2001 Census (Istat), that is, the latest available before the introduction of digital TV, and more up-to-date demographics from sources described in Appendix B.

V Estimation Results

Baseline specifications. Figure 4 plots smoothed values for a kernel-weighted local polynomial regression of the change in the vote share of Berlusconi's candidate in Piedmont towns between 2010 and 2005 on the distance of towns from the treatment border. The dashed line represents the border between Western and Eastern Piedmont. The vote share dropped in treated towns (positive distance) more than in control ones.

Insert Figure 4 here

Table 2 shows results for estimating Equation 1. In Panel A, the RD polynomial is linear in distance. Columns (1) and (2) use the whole sample of Piedmont towns. In column (1), we estimate that the vote share of Berlusconi's candidate dropped by 4.7 percentage points more in the west than in the east in 2010,

compared to his share in 2005. This effect is statistically significant when standard errors are clustered at the province level and when allowing for spatial correlation of unknown form. In column (2), more weight is given to towns with more voters, whose data are plausibly more precise. The coefficients associated with the treatment indicator, as well as computed standard errors, are very similar to those in column (1). Columns (3) to (8) only use observations closer to the border. The coefficient of interest ranges between -5.5 and -7.0 percentage points for towns within 50km, 25km and 15km of the border. Results do not change if one approximates for smooth effects of distance using a third degree polynomial (see panel B). Coefficients on the indicator for treated towns range from -4.6 to -6.4 percentage points. All t-statistics are statistically significant at the 5% level or lower. In light of the theoretical framework in Section III, these results imply that not all voters had filtered out the media slant completely in the years of exposure from 1994 until 2010.

Insert Table 2 here

Specification tests and alternative explanations. In Table 3, we examine the robustness of results to alternative specifications and explanations.[6] Standard errors clustered at the province level and corrected as in Table 2 are reported below coefficients. In panel A, we relax the assumption that the relevant distance is Euclidean and consider the complete spatial structure of observations using a cubic polynomial in longitude and latitude. Magnitude of coefficients and statistical significance are similar to Table 2. In panel B, we consider two towns in the same cluster if they belong to the same province and to the same decile of the per capita regional income distribution. This procedure increases the number

[6] Table A1 in the Online Appendix includes additional results.

of clusters to 77, which makes the issue of non-convergence of clusters to their asymptotic distribution less compelling under customary standards. At the same time, it assumes that residuals are uncorrelated for towns in the same province but different deciles of income per capita. Estimated standard errors are also similar to Table 2. Panel C allows for heterogeneous treatment effects adding interactions of the variable *Switch-off* with the cubic distance polynomial. The estimated effect is one to two percentage points lower than in previous specifications, unless we limit the analysis to towns at 25km or closer to the border. We do not detect statistical significance if we only consider towns within 15km of the border. In panel D, we provide a difference-in-differences estimator, without exploiting the spatial dimension of the data. Some coefficients are smaller than those estimated in baseline specifications, but differences disappear for towns within 25km of the border. Statistical significance is unaffected.

In the last three panels of Table 3, we test alternative explanations. Turin and the towns around it have a peculiar manufacturing and urban structure. In panel E, we exclude them: results are similar to panel B of Table 2. Berlusconi's candidate in 2010 was a member of Lega Nord, a long-term ally in Northern Italy. This fact could affect our interpretation, if voters in the west know or trust this party less than others. In panel F, we add a dummy that equals one if there is a branch of Lega Nord in town and its interaction with *Switch-off*. If voters in the west voted less for the Berlusconi candidate because they knew him or his party less, the effect should be lower in towns where Lega Nord campaigned more actively. Unreported coefficients on both dummies are economically and statistically insignificant, whereas the main result is unaltered.

Insert Table 3 here

Placebo analysis. If the effect we document in Table 2 is due to the switch to digital TV, we should observe no effect of being a town in Western Piedmont on the performance of Berlusconi's candidate in earlier elections. In Table 4, columns (1) and (2) estimate Equation 1 using the change in the vote share of Berlusconi's candidate between 2005 and 2000 as the dependent variable. All households were on analog TV in 2005. Being a town in the west had no effect on the change in the vote share of Berlusconi's candidate. The same holds for the change in vote shares between 2000 and 1995, or 2005 and 1995. In columns (3) and (4), we use the change in the vote share of Berlusconi's party between 2009 and 2004 EU Parliament elections as a dependent variable.[7] Since EU Parliament elections were held in June 2009, they allow for an examination of voting behavior just three months before the west went digital and nine months before the 2010 elections. Moreover, Piedmont GDP growth was at its trough in June 2009, and it started to recover afterwards. This placebo test addresses concerns that differential effects of the economic crisis around the border drive the results. We find no effect of being a western town on the change in the vote share of Berlusconi's party between the 2009 and 2004 EU elections.

Insert Table 4 here

Following Imbens and Lemieux (2008), we propose a spatial placebo analysis using artificial borders. Under our interpretation of the treatment, we should find no effect when estimating Equation 1 using an artificial border within Western Piedmont, using only Western towns. In columns (5) and (6), we set the artificial border at 50km west from the true one. All towns to the west of it are assigned to an artificial treatment condition, and all towns to the east (and to the west of the true border) are assigned to an artificial control condition. We find no effect of

[7] Regional and EU elections have similar rules and turnout in Italy.

being an artificially treated town on the change in the vote share of Berlusconi's candidate. In columns (7) and (8), we only consider towns in Eastern Piedmont and set the placebo border at 50km east from the border. The placebo treatment group does not behave differently from other Eastern towns.

External validity. We now discuss the external validity of our results across space and time. First, we estimate the effect in a cross-regional setting that allows for exploitation of the idiosyncratic switch to digital TV. We compare towns in the province of Cuneo (Piedmont), which switched before 2010, with towns in the neighboring region Liguria, which switched after the 2010 elections. Both regions held elections on March 28th 2010. The change in the vote share of Berlusconi's coalition was indeed more negative in Piedmont towns. This difference is statistically and economically significant, ranging from -2.5 to -4.7 percentage points. Figure A6 in the Online Appendix depicts this effect. Moreover, Figure A7 in the Online Appendix shows that our baseline result holds for the 2011 Province elections. These findings support the external validity of our results.

VI Heterogeneity of the effects

Demographics and media slant. The 17-year long exposure to the Berlusconi slant in the media allows testing if some demographics are more likely than others to be affected by media slants over time. In our theoretical setting, demographics who are more likely to be affected by persuasion bias should change their voting behavior more after the drop to slanted exposure than others.

Age. First, we look at the elderly. Figure A4 in the Online Appendix shows that Italians aged 60 or over are more likely to watch TV every day than younger groups. Also, individuals aged 60 or over are not more likely than others to read newspapers or to listen to the radio (source: *Noi Italia – Culture and Leisure Time,* Istat). Moreover, aging of the brain has been shown to worsen cognitive

abilities (e.g, see Craik and Salthouse, 2008), and it correlates with lower quality of decision-making (Choi et al., forthcoming). Hence, two possibly unrelated characteristics of the elderly seem relevant: the extent of bias exposure before the shock and potential cognitive biases. Both characteristics imply that the effect of the drop in bias exposure was higher in towns with more elderly. In columns (1) and (2) of Table 5, Piedmont towns are sorted by the ratio of individuals aged 64 or over to the whole population. We look at the interaction between being in a treated town and in a town at the top of the elderly distribution. In treated towns with the highest ratio of elderly, the vote share of Berlusconi's candidate dropped by 2.3 percentage points more than in other treated towns. As a placebo corroboration, we sort towns by the ratio of population aged 16 to 24. Young voters are not more exposed to TV than others (see Figure A4 in the Online Appendix), and there is no evidence of different cognitive abilities compared to other age groups. In columns (3) and (4) of Table 5, we find no differential effect in towns with high or low ratios of young voters compared to other treated towns.

Insert Table 5 here

Education. We try to disentangle the extent of bias exposure from cognitive abilities by looking at education. People with high and low education do not differ in terms of hours of TV exposure (Istat 2010). But lower education may correlate with lower cognitive abilities (e.g, see Ceci, 1991). In columns (7) and (8) of Table 5, we show that in towns with least educated individuals, the effect of moving to digital TV was 1.8 to 2.1 percentage points larger than in other treated towns. This effect is less statistically robust than for the elderly, but the magnitudes are similar. Note also that the dummies for high percentage of elderly and low percentage of educated people are not highly correlated (0.1512, $p < 1\%$).

Social Capital. Social pressure affects the voting behavior of individuals, especially in areas with high social capital (e.g. Gerber et al., 2008). In columns (5) and (6) of Table 5, we use the ratio of individuals employed in non-profit organizations to proxy for social capital at the town level. Towns at the top or bottom of the social capital distribution did not behave differently than others. This holds true for alternative proxies of social capital: the number of non-profit organizations in a town (Guiso et al., 2008), a dummy equal to one for towns with a blood donation venue (Guiso et al., 2004) and the change in recycling over per capita income from 2005 to 2009. Incidentally, the coefficient on the level of social capital is statistically insignificant when added to our specifications.

Placebo interactions. Finally, we test whether other dimensions mediate or moderate the effect of lower bias exposure on the votes share of Berlusconi's candidate. The elderly and the least educated may have lower incomes than other demographics. We detect no mediating effect of income on the result (columns (9) and (10) of Table 5). In Table A2 of the Online Appendix, we sort towns based on several alternative observables and find no mediating or moderating role of any of those variables on the baseline effect.

VII Interpretations of the results

Magnitude of the effect. Access to digital TV in Western Piedmont was close to 100% in March 2010. About 60% of eastern households were on analog TV in March 2010, whereas 40% were on digital TV. We divide the estimated coefficients by the difference in the probability of being exposed to the treatment across conditions, i.e. 60%. We estimate a lower and an upper bound using coefficients in Table 2, panel A, column (2) and Table 3, panel D, column (2), that is, -4.5 and -3.3 percentage points. Moving to digital TV reduced the vote

share of Berlusconi's candidate by between 5.5 and 7.5 percentage points. This is more than 10% of the weighted share of Berlusconi's coalition candidate in 2005 (52%). Assuming a homogeneous effect, had all eastern viewers moved to digital TV before the elections, Berlusconi's coalition's share in 2010 would have dropped by an additional 1.4 to 1.9 percentage points. In fact, the candidate won by a margin of less than 0.5 percentage points over his main opponent. Thus, the magnitude of the effect we document has the potential to change election results. To assess the plausibility of the magnitudes, it is helpful to compare them with extant estimations of media effects on voting. DellaVigna and Kaplan (2007) find that the introduction of Fox News in US towns has increased the vote shares of Republican candidates by 0.4 to 0.7 percentage points. Enikolopov et al. (2011) estimate that the availability of NTV in Russia, which ran a campaign against the governmental party in 1999, has decreased the party's vote share by 8.9 percentage points. As expected, our estimates fall in the middle of the range. On the one hand, Italy guarantees the freedom of the press (and free speech in general), as is the case in the United States. Several information sources other than TV exist which are not controlled by one political party. On the other hand, political information on Italian TV has been slanted towards Berlusconi's political stances since 1994 (See Section II).

Dissuasion rate. We next compute the dissuasion rate, that is, the share of viewers who were dissuaded from voting for Berlusconi's coalition candidate after moving to new digital channels. Similar to the persuasion rate of DellaVigna and Gentzkow (2010), we define the dissuasion rate in percentage points as follows:

$$d = 100 * \frac{b_T - b_C}{e_T - e_C} * \frac{1}{1 - b_0}$$

where b_T and b_C are the shares of viewers in the treated area (west) and the control area (east) who voted for Berlusconi's candidate in 2010, respectively; e_T and e_C are the share of viewers who had access to the treatment in the west and the east, respectively; and b_0 is the share of viewers who would have voted for Berlusconi's candidate regardless of whether or not they accessed the treatment, due to ideological reasons. To estimate $b_T - b_C$ we run Equation (1) including the change of the voting age population as a covariate, which controls for changes in turnout driven by the moving composition of the voting population. We use the estimated coefficient for towns 50km around the border, which is -5.6 percentage points. The coefficient is similar if we use the full sample or other subsamples. From the previous paragraph, we know $e_T - e_C = 1 - 0.4 = 0.6$. As suggested by DellaVigna and Gentzkow (2010), we approximate b_0 with the share of voters in the control area who chose Berlusconi's coalition in 2010, which was 0.54. Our resulting estimate of the dissuasion rate is $d = 20.3$. Thus, about one in five viewers who moved to new digital channels before 2010 changed their voting behavior after the s witch, that is, they were dissuaded from voting for Berlusconi's coalition candidate.

VIII Mechanisms

Transmission channels. Digital TV may have reduced the exposure to the Berlusconi slant in three ways. First, viewers may have accessed independent sources of news, being exposed to unbiased (or otherwise biased) information, as in Mullainathan and Shleifer (2005). However, the share of viewers watching digital news channels increased from 0.2% in October 2008 to only 0.8% in December 2010. Those watching all-entertainment channels soared from 1% to 11% over the same period. Besides, the move from news to entertainment channels was not paralleled by sorting into newspapers or the internet (see Figure

A2 and Figure A3 in the Online Appendix). Hence, viewers did not sort into alternative sources of information, on digital TV or on other media, after the switch. Second, those who did not go digital by the deadline could not access any TV signals. Their exposure to the slant was inhibited. This channel is not relevant: the number of households watching TV dropped temporarily at the 2009 switch date, but they were back to pre-switch levels before the 2010 electoral campaign started (see Figure A5 in the Online Appendix). Third, viewers may have moved from news programs on slanted channels to all-entertainment digital channels. This channel is indeed consistent with the viewing data, as discussed in Section II.

Explanations. We now assess to what extent a series of explanations alternative to persuasion bias may be consistent with the results.

TV consumption. Results may be driven by changes in TV consumption if digital TV has attracted new TV viewers. However, the percentage of households watching TV at least once a week was stable over time.[8] Hence, digital TV has not attracted new TV viewers. Besides, the viewing shares of the two major news programs dropped by the same amount gained by the new digital channels. These drops were not driven by a higher intensive margin of TV consumption (see Figure A1 in the Online Appendix).

Rational Inattention. TV may be the sole means reminding voters of upcoming elections. Once viewers stop watching the news, they may not pay the cost of learning the election date from non-TV sources. This could drive the results, if Berlusconi supporters were more likely to sort into new digital channels than others. In this case, the drop in the vote share of Berlusconi's coalition should be smaller in towns where the cost of learning election dates from non-TV sources is

[8] The percentages were 92.8% in 2007, 91.9% in 2010 and 91.6% in 2012.These percentages are yearly averages of monthly percentages of households who declare to watch TV in the Statistiche culturali surveys by Istat, available at http://dati.istat.it.

lower. But the effect is not smaller in towns with more newsagents per capita, with a local office of Berlusconi's coalition in town, or with more youngsters, who access the internet more than other demographics (see Figure A4 in the Online Appendix).

Cost of voting. Media slants should affect voters without strong ideological preferences more than others. These voters may also decide not to vote if the opportunity cost of voting is high. Digital TV and its contents may have increased the opportunity cost of voting. This explanation is consistent with our evidence, if voters without strong preferences were more likely to support Berlusconi's coalition in 2005 and earlier. If this is true, there should be more TV viewers on the day of 2010 elections than 2005 elections, since 36% of Italian households were on digital TV in March 2010, while no one was in 2005. According to Auditel data, the number of households watching TV on the day of the 2005 elections (April 3rd 2005) was, on average, 11.5 million in the morning slot (10am-1pm), 12.3 million in the afternoon slot (2:30pm-6:30pm) and 21.1 million in the evening slot (8pm-10:30pm). The number of households who watched TV on the day of the 2010 elections (March 28th 2010) was 7.4 million in the morning, 12.3 million in the afternoon and 18.5 million in the evening slot. Polling stations were open from 8am to 10pm both days. The aggregate data do not seem consistent with supporters of Berlusconi's coalition choosing to watch TV instead of voting in 2010. However, we do not observe data on TV viewership at the level of Piedmont provinces, which we should look at to convincingly rule out the cost of voting explanation. We cannot definitively dismiss this channel as a potential concurrent explanation of the documented effect of on voting.

Change in preferences. Durante et al. (2013) show that towns with early exposure to Berlusconi's network voted more for his party from 1994 to 2006. They argue that light entertainment contents have shaped beliefs over time, making voters more attracted to Berlusconi's party. Our natural experiment keeps

the light entertainment nature of TV contents constant, and provides a short-run treatment effect. Several shows broadcasted by digital TV had already appeared on Berlusconi's network from the 1980s onwards. Hence, the effect we document is hardly driven by a change in the ideological cues proposed by TV. Gentzkow (2006) and Prior (2005) document a lower interest in politics for viewers who are less exposed to news. This channel would be consistent with our results if Berlusconi's supporters sorted into digital TV more than others. But then we should observe a larger drop in towns with higher historical support for Berlusconi, which is inconsistent with columns (11) and (12) of Table 5.

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

Selective Attention. Schwartzstein (2012) proposes a model of selective attention to freely available information that produces persistently biased beliefs. Some voters may only attend to political information during electoral campaigns, and the debiasing process would be slow. This interpretation is consistent with our evidence, if we believe that two months of non-exposure to slanted information during the electoral campaign of 2010 was enough for voters to debias.

The Turnout Mechanism. Neither the center-left candidate nor third parties have systematically attracted the votes of Berlusconi's coalition candidate after the shock (see Table A3 in the Online Appendix). We thus examine the impact of the switch on turnout. In Table 6, we estimate Equation 1 using the change in the log of voters between 2010 and 2005 as a dependent variable. We add the change

in the log of voting population from 2005 to 2010 as a control. On average, turnout has not decreased more in treated towns (column (1) of Table 6). Turnout was about 3 percentage points lower in treated towns with the most elderly than in other treated towns, and than in control towns with most elderly (columns (2)-(5) of Table 6). Being in the former group was associated with a 2.3 percentage point larger drop in the vote share of Berlusconi's coalition candidate (columns (1) and (2) of Table 5).

The following debiasing mechanism is therefore consistent with our evidence: a) after moving to digital TV, individuals are less exposed to the Berlusconi slant; b) supporters of Berlusconi's coalition are less motivated to show up at elections. Turnout drops, especially in towns with more elderly, who are most affected by the slant; c) the vote share of Berlusconi's coalition candidate drops, especially in towns with more elderly. Evidence in line with points b) and c) is in Table 6 and Table 5, respectively.

IX Conclusions

Italians have been exposed to a slant in political information on TV since 1994. The introduction of digital TV caused a drop in the slanted exposure. In 2010, the drop has reduced the vote share of Berlusconi's coalition by between 5.5 and 7.5 percentage points. At least 20% of digital TV users have changed their voting behavior. Towns with more elderly and with less educated voters changed their behavior the most. The effects we document have the potential to change election results, and they are valid across geographic areas and elections. The results are consistent with the existence of persuasion-biased viewers. These viewers were affected by the slant towards Berlusconi as long as they were exposed to it. They changed their voting behavior once the exposure dropped.

This study suggests a motive for increasing competition in the TV market when information sources are slanted, and the slant does not consist of news omission: higher competition reduces slanted exposure, and individuals debias mechanically.

Our results imply policies should be implemented to help individuals, particularly those in the most vulnerable demographics, account for slants in the information they receive. Examples include voters and political information, investors and analyst forecasts, or patients and treatment recommendations. In our setting the slant affected voters despite awareness that Berlusconi controlled most TV channels. Thus, mandating disclosure of conflicts of interest is not a sufficient provision.

The results also pose questions that go beyond the scope of economic research. To what extent do elections legitimize representation in countries where information is systematically biased? If the effects of media bias are mediated, at least in part, by cognitive biases, is it lawful to exploit them? Which interventions to protect vulnerable groups are legitimate and which are excessively intrusive on free will? Recent media control concentration in countries like Hungary, France, Mexico and Thailand, as well as the establishment of large media conglomerates like Murdoch's News Corporation, makes these questions actual and relevant.

REFERENCES

Cage, J. “Media Competition and the Provision of Information.” Working Paper 2013.

Cameron, Colin, Jonah Gelbach, and Douglas Miller. “Bootstrap-based improvements for inference with clustered errors.” Review of Economics and Statistics 90(3) 2008, pp. 414–427.

Ceci, Sthephen, “How Much Does Schooling Influence General Intelligence and its Cognitive Components?” Developmental Psychology 27(5) 1991, 703-722.

Choi, Syngjoo, Shachar Kariv, Wieland Mueller, and Dan Silverman. “Who is (More) Rational?” American Economic Review, forthcoming.

Conley, Timothy. “GMM Estimation with Cross-Sectional Dependence.” Journal of Econometrics 105(1) 1999, 59–83.

Craik, Fergus and Timothy Salthouse. The Handbook of Aging and Cognition, 3rd ed. (Psychology Press, New York and Hove, 2008).

Dell, Melissa. “The Persistent Effects of Peru’s Mining Mita.” Econometrica 78(6) 2010, 1863–1903.

DellaVigna, Stefano and Ethan Kaplan. “The Fox News Effect: Media Bias and Voting.” NBER Working Paper series 2006.

DellaVigna, Stefano and Ethan Kaplan. "The Fox News Effect: Media Bias and Voting." Quarterly Journal of Economics 122(3) 2007, 1187–1234.

DellaVigna, Stefano and Matthew Gentzkow. "Persuasion: Empirical Evidence." Annual Review of Economics 2(1) 2010, 643–669.

Donald, Sthephen and Kevin Lang. "Inference with Differences-in-Differences and Other Panel Data." Review of Economics and Statistics 89 2007, 221–233.

Durante, Ruben and Brian Knight. "Partisan Control, Media Bias, and Viewer Responses: Evidence From Berlusconi Italy." Journal of the European Economic Association 10(3) 2012, 451–481.

Durante, Ruben, Paolo Pinotti, and Andrea Tesei. "The Political Legacy of News-Free Television: Evidence From the Rise of Berlusconi." Working Paper 2013.

Enikolopov, Ruben, Maria Petrova, and Ekaterina Zhuravskaya. "Media and Political Persuasion: Evidence from Russia." American Economic Review 111(7) 2011, 3253–3285.

Gentzkow, Matthew. "Television and Voter Turnout." Quarterly Journal of Economics 121(3) 2006, 931–972.

Gentzkow, Matthew and Jesse Shapiro. "Media Bias and Reputation." Journal of Political Economy 114(2) 2006, 280–316.

Gerber, A., D. Green, and C. Larimer. "Social Pressure and Voter Turnout: Evidence from a large scale field experiment." American Political Science

Review 102(1) 2008, 33–48.

Guiso, Luigi, Paola Sapienza, and Luigi Zingales. "The Role of Social Capital on Financial Development." American Economic Review 94(3) 2004, 526–556.

Guiso, Luigi, Paola Sapienza, and Luigi Zingales. "Long Term Persistence." 2008, p. NBER Working Paper No. 14278.

Hong, Harrison and Marcin Kacperczyk. "Competition and Bias." Quarterly Journal of Economics 125(4) 2010, 1683–1725.

Imbens, Guido and Thomas Lemieux. "Regression Discontinuity Design: a Guide to Practice." Journal of Econometrics 142(2) 2008, 615–635.

Istat, Aspetti della vita quotidiana, 2010.

Kamenica, Emir and Matthew Gentzkow. "Bayesian Persuasion." American Economic Review 101(6) 2011, 2590–2615.

Kline, Patrick and Enrico Moretti. "Local Economic Development, Agglomeration Economies and the Big Push." Quarterly Journal of Economics 129(1) 2014, pp.275-331.

Larcinese, Valentino, Jim Snyder, and Riccardo Puglisi. "Partisan Bias in Economic News: Evidence on the Agenda Setting Behavior of US Newspapers." Journal of Public Economics 95(9-10) 2011, 1178–1189.

Lee, David and Thomas Lemieux. "Regression Discontinuity Designs in

Economics." Journal of Economic Literature 48(2) 2010, 281–355.

Malmendier, Ulrike and Dervind Shanthikumar. "Are Investors Naive about Incentives?" Journal of Financial Economics 85(2) 2007, 457–489.

Meyers-Levy, John and Prashant Malaviya. "Consumers' Processing of Persuasive Advertisements: an Integrative Framework of Persuasion Theories." Journal of Marketing 63 1999, 45–60.

Mullainathan, Sendhil and Andrei Shleifer. "The Market for News." American Economic Review 95(4) 2005, 1031-1053.

Mullainathan, Sendhil, Joshua Schwartzstein, and Andrei Shleifer. "Coarse Thinking and Persuasion." Quarterly Journal of Economics 123(2) 2008, 577–619.

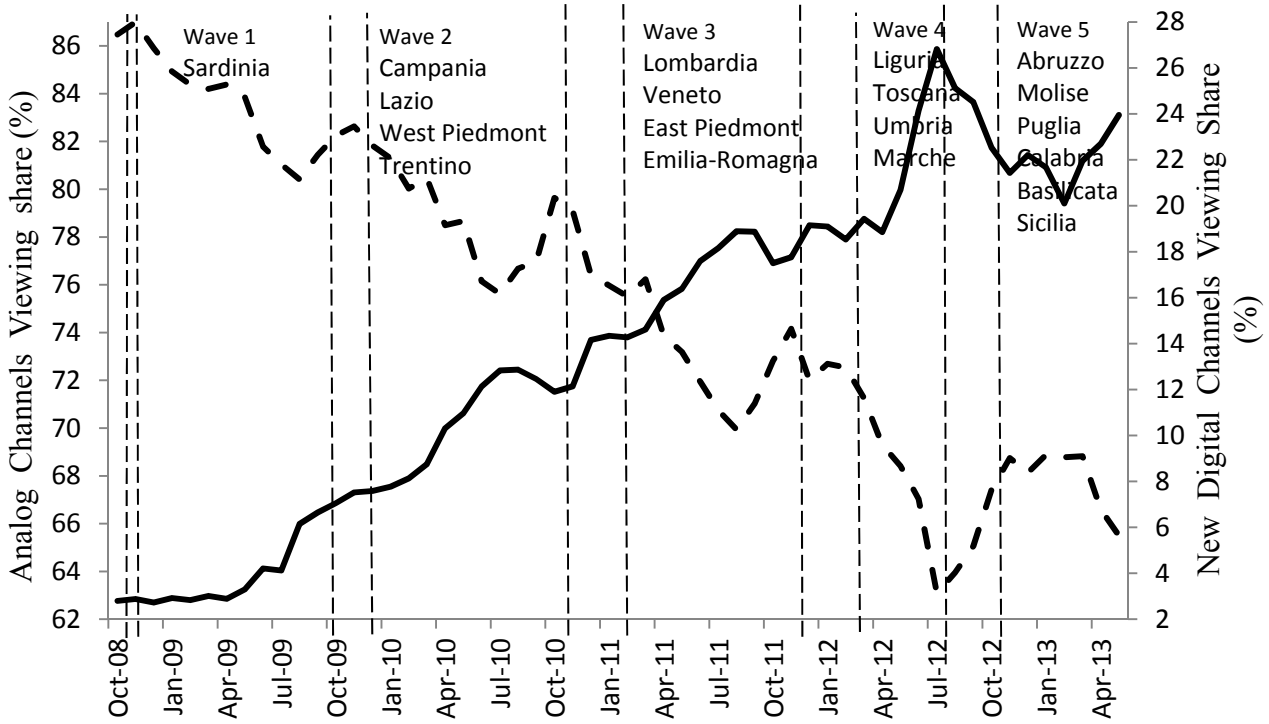
Prat, Andrea and David Stromberg. "The Political Economy of Mass Media." Working Paper 2011.

Prior, Markus. "News v. Entertainment: How Increasing Media Choice Widens Gaps in Political Knowledge and Turnout." American Journal of Political Science 2005, pp. 577–592.

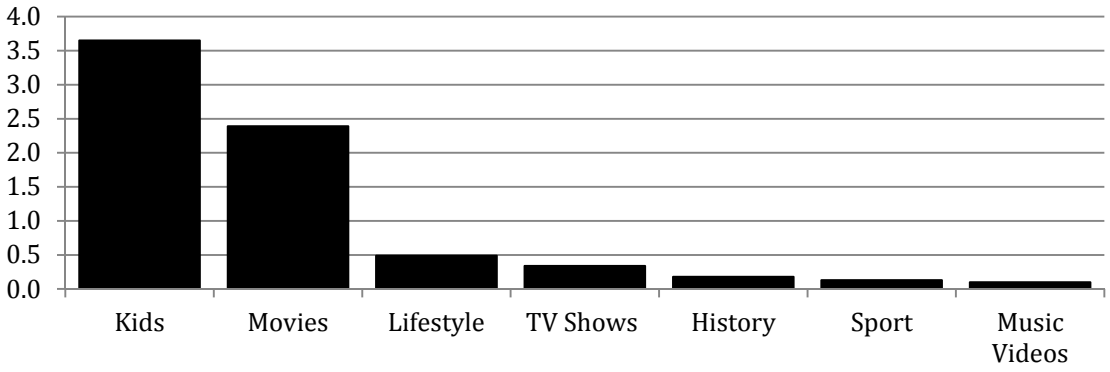
Schwartzstein, Joshua. "Selective Attention and Learning." Journal of the European Economic Association, forthcoming.

FIGURE 1 - SHOCK TO SLANTED EXPOSURE AND VIEWERS' REACTION

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



Panel B. Average daily viewing shares of new digital channels by content as of March 2010, 6pm-8:30pm slot



Notes. All data are from the *Nielsen/Auditel* monthly reports. The average daily viewing shares for each month are defined as follows (see Appendix 2): $\text{ViewingShare} = \frac{\sum_t \sum_m (\frac{\sum \text{Viewers}_m}{\sum \text{AllViewers}_m})/M}{T}$

FIGURE 2–THEORETICAL FRAMEWORK AND EMPIRICAL STRATEGY

	West (Treated)	East (Control)	Δ_{Space}
Before switch (2005)	$\boldsymbol{q_T^\lambda}=\frac{\kappa 0+ Q(m_T-(1-\lambda)\hat{s}_T)}{\kappa+Q}$	$\boldsymbol{q_T^\lambda}=\frac{\kappa 0+ Q(m_T-(1-\lambda)\hat{s}_T)}{\kappa+Q}$	$\boldsymbol{0}$
After switch (2010)	$\boldsymbol{q^\lambda_{T+1,West}}=\frac{\kappa 0}{\kappa}=0$	$\boldsymbol{q^\lambda_{T+1,East}}=\frac{\kappa 0+ Q_{+1}(m_{T+1}-(1-\lambda)\hat{s}_{T+1})}{\kappa+Q_{+1}}$	$-\boldsymbol{q^\lambda_{T+1,East}}$
Δ_{Time}	$-\boldsymbol{q_T^\lambda}$	$\boldsymbol{q^\lambda_{T+1,East}}-\boldsymbol{q_T^\lambda}$	$-\boldsymbol{q^\lambda_{T+1,East}}$

Notes. Each entry is derived from the theoretical framework of Section III.

FIGURE 3–NATURAL EXPERIMENT: SWITCH TO DIGITAL TV AND 2010 ELECTIONS

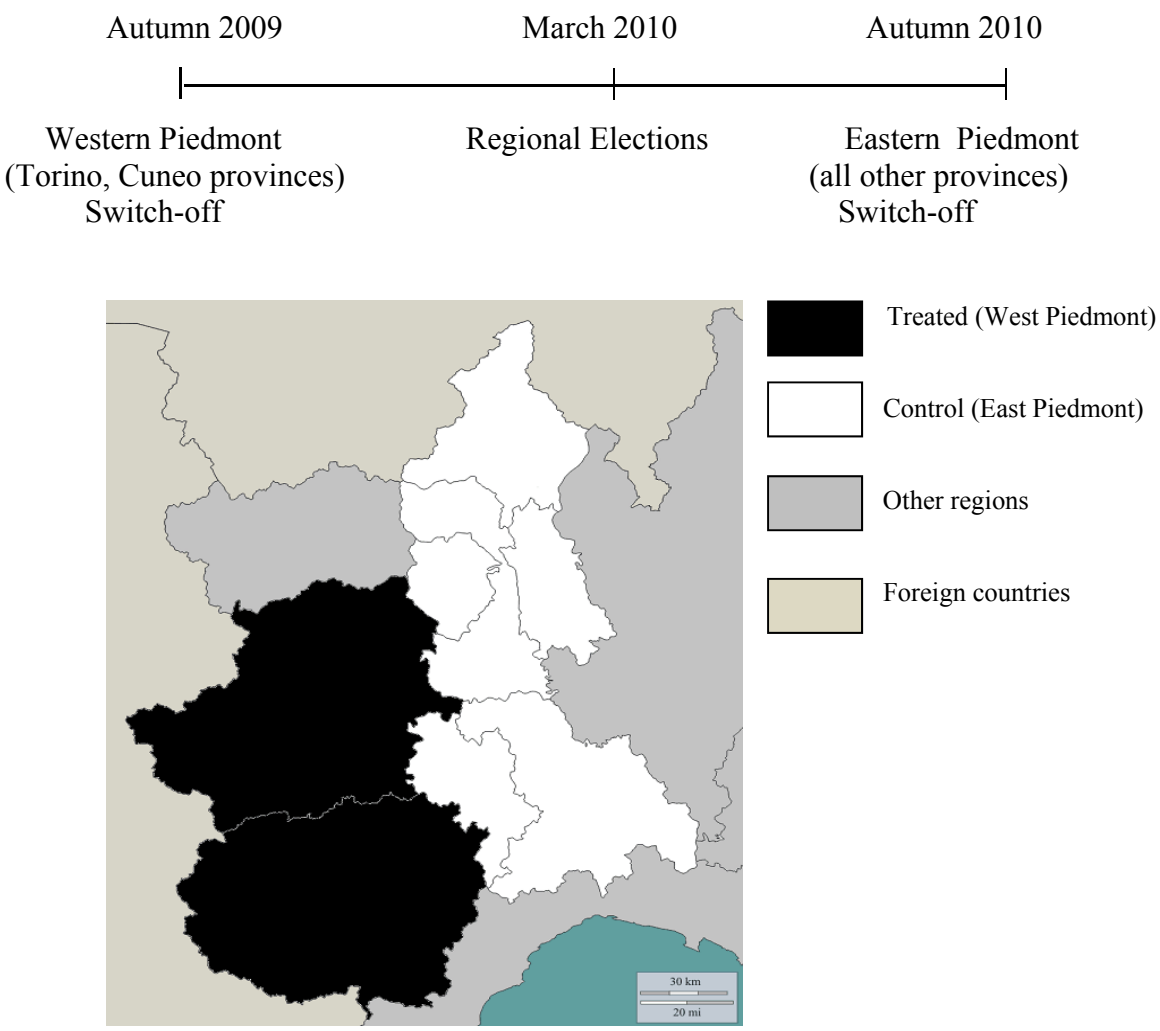
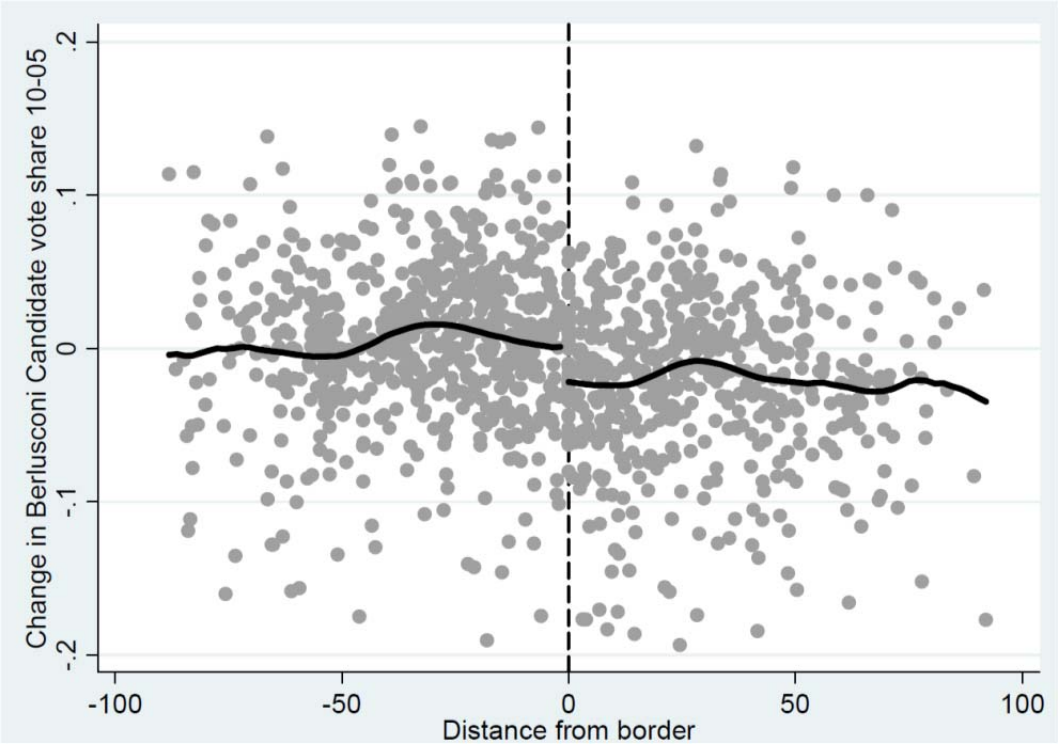


FIGURE 4–CHANGE IN BERLUSCONI'S COALITION CANDIDATE PERFORMANCE AROUND THE TREATMENT BORDER



Notes. Observations are all Piedmont towns. Distance is negative for control towns, positive for treated towns. Observations are trimmed at the 1-99 percentiles change in the vote share of Berlusconi's coalition candidate.

TABLE 1- SUMMARY STATISTICS

	Full Sample (< 100 km)			< 50 km			< 25 km			< 15 km		
	Treated	Control	p-value	Treated	Control	p-value	Treated	Control	p-value	Treated	Control	p-value
Panel A. Election outcomes												
Δ Berlusconi 05-00	-0.029	-0.035	0.761	-0.029	-0.032	0.879	-0.034	-0.038	0.842	-0.038	-0.040	0.932
Δ Berlusconi 00-95	0.177	0.150	0.493	0.168	0.145	0.521	0.161	0.154	0.871	0.156	0.170	0.751
Δ Main comp. 05-00	0.106	0.088	0.152	0.106	0.086	0.138	0.105	0.090	0.349	0.109	0.091	0.387
Δ Main comp. 00-95	0.041	0.045	0.758	0.039	0.048	0.491	0.035	0.055	0.237	0.038	0.054	0.271
Berl. Hist. Support	0.486	0.499	0.697	0.491	0.495	0.918	0.490	0.489	0.971	0.504	0.515	0.767
Panel B. Socio-demographics												
Population 09	5110	2432	0.194	5864	2443	0.157	6994	1933	0.173	3437	2367	0.279
Taxable Inc. p.c. 01	9388	9452	0.915	9534	9556	0.972	9600	9356	0.720	9701	9091	0.377
% manufacturing empl.	0.122	0.127	0.836	0.134	0.140	0.808	0.133	0.132	0.982	0.116	0.114	0.941
% services empl.	0.136	0.126	0.557	0.124	0.124	0.966	0.124	0.118	0.673	0.124	0.123	0.959
Δ unemployment 10-01	0.011	0.005	0.530	0.012	0.005	0.494	0.014	0.009	0.669	0.016	0.008	0.442
Δ % foreigners 09-05	0.871	0.785	0.532	0.898	0.812	0.599	0.973	0.747	0.294	1.002	0.825	0.405
Δ recycling 09-05	0.152	0.120	0.524	0.163	0.130	0.526	0.172	0.130	0.317	0.170	0.153	0.347
Observations	565	641	1206	457	471	928	287	265	552	193	151	344

Notes. This Table reports summary statistics for observables at the Piedmont town level before 2010 regional elections. Details about variable definitions and more summary statistics can be found in the Data Appendix. Each Panel reports the mean of a variable for Treated (*Switch-off*) and Control (*No Switch-off*) 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.

TABLE 2–EFFECT OF SWITCH-OFF TO DIGITAL TV ON THE VOTE SHARE OF BERLUSCONI'S COALITION CANDIDATE

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Full Sample	(< 100 km)	< 50 km		< 25 km		< 15 km	
Panel A. Distance from the border								
Switch-off	-0.047	-0.045	-0.055	-0.054	-0.063	-0.059	-0.071	-0.067
Cluster prov.	*0.009***	*0.008***	*0.009***	*0.009***	*0.011***	*0.012***	*0.008***	*0.009***
Wild bootstrap	*0.019***	*0.017***	*0.018***	*0.017***	*0.027***	*0.025***	*0.024***	*0.023***
Spatial HAC	*0.008***	*0.008***	*0.010***	*0.010***	*0.012***	*0.012***	*0.014***	*0.013***
R^2	0.397	0.394	0.424	0.418	0.437	0.433	0.527	0.518
Panel B. Cubic polynomial, distance from the border								
Switch-off	-0.061	-0.057	-0.050	-0.048	-0.051	-0.046	-0.064	-0.060
Cluster prov.	*0.012***	*0.012***	*0.013***	*0.013***	*0.009***	*0.009***	*0.013***	*0.013***
Wild bootstrap	*0.020***	*0.020***	*0.016***	*0.015***	*0.021***	*0.019***	*0.022***	*0.033**
Spatial HAC	*0.010***	*0.010***	*0.011***	*0.012***	*0.013***	*0.012***	*0.015***	*0.014***
R^2	0.404	0.399	0.425	0.419	0.441	0.439	0.535	0.526
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	928	928	552	552	344	344

Notes. Each observation is a town in Piedmont. In all columns, the dependent variable is the change in the vote share of Berlusconi's coalition candidate between 2010 and 2005. In Panel A, the RD polynomial in the distance of a town from the border is linear. In Panel B, it is cubic. Switch-off is a dummy that equals 1 for treated towns. In even columns, observations are weighted by the average of the log of voters in 2010 and 2005 elections. Clust. province s.e. are clustered at the province level, and corrected as suggested by Donald and Lang (2007). Wild bootstrap s.e. follow Cameron et al. (2008) but assume the wild boostrapped t-statistic is asymptotically normally distributed. Spatial HAC s.e. allow for spatial dependence of unknown form as in Conley (1999). *** Significant at the 1 percent level.** Significant at the 5 percent level.* Significant at the 10 percent level.

TABLE 3–SPECIFICATION TESTS AND ROBUSTNESS

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Full Sample	(< 100 km)	< 50 km		< 25 km		< 15 km	
Panel A. Cubic polynomial, longitude and latitude								
Switch-off	-0.060	-0.057	-0.049	-0.048	-0.063	-0.062	-0.054	-0.053
Cluster prov.	0.013***	0.012***	0.011***	0.011***	0.008***	0.008***	0.013***	0.013***
R^2	0.425	0.422	0.428	0.425	0.455	0.451	0.456	0.437
*Panel B. Clustering by province*avg. income decile*								
Switch-off	-0.061	-0.057	-0.050	-0.048	-0.051	-0.046	-0.064	-0.060
s.e.	0.011***	0.011***	0.013***	0.012***	0.016***	0.014***	0.017***	0.016***
N. of clusters	77	77	69	69	58	58	50	50
R^2	0.404	0.399	0.425	0.419	0.441	0.439	0.535	0.526
Panel C. Heterogeneous Treatment Effects								
Switch-off	-0.038	-0.035	-0.043	-0.038	-0.057	-0.052	-0.047	-0.046
Cluster prov.	0.010***	0.008***	0.014**	0.013**	0.020**	0.020*	0.028	0.027
R^2	0.409	0.406	0.429	0.425	0.444	0.444	0.456	0.440
Panel D. OLS								
Switch-off	-0.034	-0.033	-0.033	-0.032	-0.053	-0.050	-0.069	-0.066
Cluster prov.	0.009***	0.008***	0.008***	0.009***	0.006***	0.005***	0.003***	0.004***
R^2	0.392	0.389	0.413	0.406	0.435	0.431	0.527	0.518
Panel E. Excluding Turin and neighboring towns								
Switch-off	-0.061	-0.057	-0.049	-0.047	-0.051	-0.047	-0.066	-0.062
Cluster prov.	0.012***	0.012***	0.013***	0.013***	0.009***	0.008***	0.014***	0.014***
R^2	0.403	0.399	0.425	0.419	0.440	0.437	0.536	0.527
Observations	1,194	1,194	916	916	544	544	341	341
Panel F. Lega Effect								
Switch-off	-0.061	-0.057	-0.051	-0.049	-0.053	-0.050	-0.070	-0.067
Cluster prov.	0.012***	0.012***	0.013***	0.013***	0.010***	0.009***	0.014***	0.014***
R^2	0.404	0.399	0.425	0.419	0.443	0.441	0.541	0.534
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	928	928	522	522	344	344

Notes. Each observation is a town in Piedmont. Switch-off is a dummy variable that equals 1 for treated towns. In even columns, observations are weighted by the average of the log of voters in 2010 and 2005. With the exception of Panel B, standard errors are clustered at the province level and corrected for downward bias as in Donald and Lang (2007). *** Significant at the 1 percent level. ** Significant at the 5 percent level. * Significant at the 10 percent level.

TABLE 4–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	
Panel 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*
$\dot{R}^2$	0.195	0.226	0.124	0.132	0.454	0.554	0.378	0.460
Panel 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*
$\dot{R}^2$	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

*Notes.*Each observation is a town in Piedmont. Switch-off is a dummy variable that equals 1 for treated towns. In all columns observations are weighted by the average of the log of voters in 2010 and 2005. In even columns, the analysis is limited t towns within 50km of the border in both directions. Cluster prov. standard errors are clustered at the province level, an corrected for downward bias as in Donald and Lang (2007). Spatial HAC s.e. allow for spatial dependence of unknown forn following Conley (1999). Unreported Wild bootstrap s.e. are larger than clustered and spatial HAC s.e. in all specifications *** Significant at the 1 percent level. ** Significant at the 5 percent level. * Significant at the 10 percent level.

TABLE 5– INTERACTION EFFECTS

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
	Elderly		Youngsters		Social Capital		Education		Income p.c.		Hist. support	
Switch-off	-0.049	-0.042	-0.056	-0.0466	-0.047	-0.037	-0.047	-0.036	-0.053	-0.043	-0.053	-0.043
Cluster prov.	*0.011***	*0.011***	*0.009***	*0.013***	*0.011***	*0.012***	*0.015***	*0.013***	*0.012***	*0.012***	*0.012***	*0.009***
Wild Bootstrap	*0.016***	*0.013***	*0.019***	*0.024**	*0.018***	*0.020**	*0.019***	*0.018**	*0.020***	*0.017***	*0.021***	*0.024**
Switch-off*Top 3	-0.023	-0.022	0.004	-0.001	-0.007	-0.006	-0.006	-0.009	-0.002	-0.004	0.004	0.005
Cluster prov.	*0.005***	*0.006***	*0.005*	*0.004*	*0.006*	*0.006*	*0.007*	*0.007*	*0.006*	*0.006*	*0.006*	*0.009*
Wild Bootstrap	*0.008***	*0.007***	*0.005*	*0.008*	*0.007*	*0.008*	*0.008*	*0.009*	*0.006*	*0.010*	*0.005*	*0.011*
Switch-off*Bottom 3	0.002	0.004	-0.008	-0.009	-0.010	-0.012	-0.018	-0.021	-0.007	-0.008	-0.014	-0.010
Cluster prov.	*0.004*	*0.008*	*0.012*	*0.011*	*0.006*	*0.007*	*0.008**	*0.009**	*0.006*	*0.006*	*0.003****	*0.006*
Wild Bootstrap	*0.006*	*0.006*	*0.013*	*0.014*	*0.007*	*0.008*	*0.009***	*0.013*	*0.012*	*0.011*	*0.006***	*0.006*
Top 3, Bottom 3	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes
Electoral controls	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes
Socio-dem. controls	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes
Border segment f.e.	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes
Half. dist. border	no	yes	no	yes	no	yes	no	yes	no	yes	no	yes

Notes. Each observation is a town in Piedmont. Switch-off is a dummy variable that equals 1 for treated towns, zero otherwise. Switch-off*Top 3 is 1 if a town is in the treatment group and above the top tercile of towns sorted by the variable indicated above each column. Switch-off*Bottom 3 is 1 if a town is in the treatment group and below the lowest tercile of towns sorted by the variable indicated above each column. Odd columns report results for the full sample. In even columns the analysis is limited to towns within 50km of 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, and corrected for downward bias as in Donald and Lang (2007). Wild bootstrap s.e. follow Cameron et al. (2008) but assume the wild boostrapped t-statistic is asymptotically normally distributed. *** Significant at the 1 percent level. ** Significant at the 5 percent level. * Significant at the 10 percent level.

TABLE 6–EFFECT OF SWITCH-OFF TO DIGITAL TV ON TURNOUT

	(1) Full Sample	(2) (< 100 km)	(3) < 50 km	(4) < 25 km	(5) < 15 km
Switch-off	0.018	0.033	0.027	0.024	0.036
Clust. province	*0.015*	*0.016**	*0.020*	*0.020*	*0.019*
Switch-off*H. Old		-0.030	-0.025	-0.044	-0.019
Clust. province		*0.006****	*0.008***	*0.018**	*0.009**
Switch-off*L. Old		-0.003	-0.005	-0.004	-0.016
Clust. province		*0.006*	*0.008*	*0.009*	*0.014*
Δ Voting pop. 10-05	0.214	0.213	0.239	0.229	0.269
Clust. province	*0.041****	*0.040****	*0.021****	*0.034****	*0.050****
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
Observations	1,206	1,206	928	552	344
R^2	0.207	0.222	0.245	0.297	0.327

Notes. 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 that equals 1 for treated towns. Switch-off*H. Old is 1 if a town is in the treatment group and above the top tercile of towns sorted by ratio of elderly. Switch-off*L. Old is 1 if a town is in the treatment group and below the bottom tercile of towns sorted by ratio of elderly. Standard errors are clustered at the province level. *** Significant at the 1 percent level. ** Significant at the 5 percent level. * Significant at the 10 percent level.

Appendix A

In this section we provide the derivations and proofs for Section III. First, we derive *Q,* that is, the precision of the estimated differential quality of Berlusconi's coalition candidate at time T. Viewers form a preliminary estimation of the differential quality:

$$\hat{q}_T^e = m_T - \hat{s}_T$$

$$= \frac{(q_T + s)(\sigma + T\kappa) - \sigma s_0 - T\kappa \frac{1}{T}\sum_1^T (q_t + s)}{(\sigma + T\kappa)}$$

$$= \frac{\sigma(s - s_0) + [\sigma + (T-1)\kappa]q_T - \kappa \sum_1^{T-1} q_t - \kappa T s}{(\sigma + T\kappa)}$$

The variance of this expression is the reciprocal of Q. Because $\hat{q}_T^e$ is an estimate of q_T, its variance does not depend on q_T:

$$Var[\hat{q}_T^e] = \frac{\sigma}{(\sigma + T\kappa)^2} + \frac{\kappa(T-1)}{(\sigma + T\kappa)^2} + \frac{\kappa^2 T}{\sigma(\sigma + T\kappa)^2} = \frac{1}{Q}$$

Now we can derive the expression for $\hat{q}_{T,pre}^{\lambda}$, that is, the estimated differential quality of Berlusconi's coalition candidates by agents affected by persuasion bias when exposed to the slanted media. Recall that this case embeds the Bayesian case for λ=0.

$$\hat{q}_{T,\boldsymbol{pre}}^{\lambda} = \frac{Q[m_T - (1-\lambda)\hat{s}_T]}{\kappa + Q}$$

$$= \frac{Q}{\kappa + Q}\left[\frac{(\sigma + \lambda T\kappa)s - (1-\lambda)\sigma s_0}{(\sigma + T\kappa)} + \frac{[\sigma + \big(T - (1-\lambda)\big)\kappa]q_T}{(\sigma + T\kappa)} - \frac{(1-\lambda)\kappa \sum_1^{T-1} q_t}{(\sigma + T\kappa)}\right]$$

Proof of Proposition I. (i) It suffices to take the derivative of $\hat{q}_{T,\boldsymbol{pre}}^{\lambda}$ with respect to *s,* and the second derivative of $\hat{q}_{T,\boldsymbol{pre}}^{\lambda}$ with respect to *s* and *λ*, and verifying that

both are strictly positive. $\frac{\partial \hat{q}_{T,\boldsymbol{pre}}^{\lambda}}{\partial s} = \frac{Q}{\kappa+Q}\frac{\sigma+\lambda T\kappa}{\sigma+T\kappa} > 0; \frac{\partial^2 \hat{q}_{T,\boldsymbol{pre}}^{\lambda}}{\partial s \partial \lambda} = \frac{QT\kappa}{(\kappa+Q)(\sigma+T\kappa)} > 0$ (ii) This follows from taking the limit of $\frac{\partial \hat{q}_{T,\boldsymbol{pre}}^{\lambda}}{\partial s}$ for T→∞ , which is zero for the case where λ=0, and λ for any $\lambda \in (0,1]$.

Note that the drop in the perceived quality of Berlusconi's coalition candidate is

$$\Delta \hat{q}^{\lambda} = \hat{q}_{T,post}^{\lambda} - \hat{q}_{T,\boldsymbol{pre}}^{\lambda} = -\hat{q}_{T,\boldsymbol{pre}}^{\lambda}$$

Proof of Proposition II.(i) $\hat{q}_{T,\boldsymbol{pre}}^{\lambda} > \hat{q}_{T,\boldsymbol{pre}}^{\lambda=0}$ for any λ > 0, but $\hat{q}_{T,post} = \hat{q}_{T,post}^{\lambda} = 0$. Hence the drop in perceived quality is more negative for persuasion-biased agents (λ > 0) than for Bayesian agents (λ = 0). *(ii)* One can compute $\frac{\partial \Delta \hat{q}^{\lambda}}{\partial \lambda} = -\frac{\partial \hat{q}_{T,\boldsymbol{pre}}^{\lambda}}{\partial \lambda} = -\frac{Q}{\kappa+Q}\left[\frac{sT\kappa+\sigma s_0+kq_T+k\sum_1^{T-1} q_t}{\sigma+T\kappa}\right] < 0$.

Appendix B

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

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

- Precincts: number of electoral precincts in a town.
- Δ Berlusconi 05-00: change in the vote share of Berlusconi's coalition candidate between 2005 and 2000 regional elections.
- Δ Berlusconi 00-95: change in the vote share of Berlusconi's coalition candidate between 2000 and 1995 regional elections.
- Δ Berlusconi EU 09-04: change in the vote share of Berlusconi's party list between 2009 and 2004 European Parliament elections.
- Share Berlusconi Prov. pre10: vote share of Berlusconi's coalition candidate in the closest province's elections before 2010.
- Δ csx 05-00: change in center-left candidate vote share between 2005 and 2000 regional elections.
- Δ csx 00-95: change in center-left candidate vote share between 2000 and 1995 regional elections.

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

- Δ unemployment 10-01: change in unemployment rate between 2010 and 2001.
- Δ unemployment 09-05: change in unemployment rate between 2009 and 2005 at the province level.

- Δ perc. foreign 09-05: change in the percentage of foreign residents between 2009 and 2005. (Rete Unitaria della Pubblica Amministrazione in Piedmont (RUPAR), available at http://www.ruparPiedmont.it/infostat/index.jsp)
- Δ abs. foreign 09-05: change in the absolute number of foreign residents between 2005 and 2009. (Rete Unitaria della Pubblica Amministrazione in Piedmont (RUPAR), available at http://www.ruparPiedmont.it/infostat/index.jsp).
- Δ milk prod quotas 10-08: change in EU milk production quotas (liters) assigned to Piedmont farms at the provincial level.
- Δ recycling inc 09-05: change in percentage recycling over average taxable income. (Sistema Piedmont, http//www.sistemaPiedmont.it/webruc/raccoltaRifiutiReportAction.do?btnAggiorna=aggiornaComuniDaComune)
- Events environment 09-05: number of interventions to address major pollution events. (Anagrafe Regionale Siti Contaminati, available at: http://www.regione.Piedmont.it/ambiente/bonifiche/home.htm)
- Newsagents pop 09: number of newsagents per 1,000 inhabitants. (Regione Piedmont, Osservatorio Commercio, available at: http //www.regione.P iedmont.it/commercio/ossC ommercio.htm)
- Tabacchi pop 09: number of liquor stores (Tabacchi) per 1,000 inhabitants. (Regione Piedmont, Osservatorio Commercio, available at: http //www.regione.Piedmont.it/commercio/ossC ommercio.htm)
- Arci: dummy equal to one if Arci clubs exist in town, i.e. leftish meeting points for elderly and youngsters. (Arci Piedmont, available at: http //www.arciPiedmont.it/affiliatiPiedmont)
- Acli: dummy equal to one if Acli clubs exist in town, i.e. catholic meeting points for elderly and youngsters. (Acli, available at :

http //www.acli.it/index.php?option=comgoogle&view=advanced&id=5&Itemid=141)

- Avis: dummy equal to one if a blood donation station exists in a town. (AVIS, available at http : //www.avis.it/usr view.php/I D = 1403)
- Density 01: inhabitants per squared km from 2001 Census.
- Male 01: share of men over all inhabitants from 2001 Census.
- Hsize 01: average number of components per household from 2001 Census.
- Manufacturing 01: number of employees in manufacturing from 2001 Census.
- Services 01: number of employees in services from 2001 Census.
- Tourism 01: percentage of days hotel rooms are occupied over the whole year from 2001 Census.
- Banking 01: number of checking accounts per 100 inhabitants from 2001 Census.
- Cars 01: number of cars per 100 inhabitants from 2001 Census.
- Students 01: number of high school students from 2001 Census.
- Health care efficiency 01: number of days × patients needed to recover over one year from 2001 Census.
- Chemists 01: average number of inhabitants per each pharmacy from 2001 Census.
- Disp. income 01: average disposable income per inhabitant from 2001 Census.
- Farms 01: number of farms from 2001 Census.
- Large HH 01: number of households with 5 or more components from 2001 Census.
- Retired 01: number of inhabitants from 65 to 79 years old from 2001 Census.
- Very Old 01: number of inhabitants older than 80 years old from 2001 Census.

Finally, we report the definition of the viewing share plotted in Figure 1, which is computed by Auditel/Nielsen over the whole Italian territory:

$$ViewingShare_{analog} = \frac{\sum_t \sum_m (\frac{\sum ViewersAnalog_m}{\sum AllViewers_m})/M}{T}$$

where $\sum ViewersAnalog_m$ is the sum of all individuals in the Auditel/Nielsen representative sample who watch any of the six analog channels in each minute *m* of the daily slot 6-8:30pm, $\sum AllViewers_m$ is the total number of viewers on analog, digital or satellite TV channels in minute *m*, *M* is the number of minutes in the slot 6-8:30pm, and *T* is the number of days in the month.

APPENDIX TABLE–SUMMARY STATISTICS FOR ALL VARIABLES

	Full Sample (< 100 km)			< 50 km			< 25 km			< 15 km		
	Treated	Control	p-value	Treated	Control	p-value	Treated	Control	p-value	Treated	Control	p-value
Panel A. Election outcomes												
Δ Berlusconi 05-00	-0.029	-0.035	0.761	-0.029	-0.032	0.879	-0.034	-0.038	0.842	-0.038	-0.040	0.932
Δ Berlusconi 00-95	0.177	0.150	0.493	0.168	0.145	0.521	0.161	0.154	0.871	0.156	0.170	0.751
Δ Berlusconi EU 09-04	0.006	0.002	0.686	0.011	0.001	0.421	0.015	-0.003	0.168	0.015	-0.008	0.113
Berlusconi Prov. Pre 10	0.502	0.527	0.652	0.507	0.517	0.883	0.508	0.483	0.661	0.508	0.481	0.641
Δ Main comp. 05-00	0.106	0.088	0.152	0.106	0.086	0.138	0.105	0.090	0.349	0.109	0.091	0.387
Δ Main comp. 00-95	0.041	0.045	0.758	0.039	0.048	0.491	0.035	0.055	0.237	0.038	0.054	0.271
Precints	5.335	2.841	0.190	6.020	2.817	0.157	7.146	2.321	0.183	3.601	2.695	0.276
Panel B. Socio-demographics												
Δ unemployment 10-01	0.011	0.005	0.530	0.012	0.005	0.494	0.014	0.009	0.669	0.016	0.008	0.442
Δ % foreigners 09-05	0.871	0.785	0.532	0.898	0.812	0.599	0.973	0.747	0.155	1.002	0.825	0.405
Δ abs. foreigners 09-05	176.6	71.50	0.158	206.0	71.59	0.119	269.8	54.26	0.137	106.7	72.68	0.142
Δ recycling 09-05	0.152	0.120	0.524	0.163	0.130	0.526	0.172	0.130	0.317	0.170	0.135	0.347
Events environment 09-05	0.727	0.757	0.943	0.849	0.713	0.742	0.969	0.513	0.429	0.622	0.589	0.939
Newsagents pop 09	1.244	1.088	0.090	1.045	1.111	0.614	0.963	1.111	0.387	1.053	1.019	0.818
Tabacchi pop 09	1.471	1.427	0.883	1.242	1.463	0.225	1.157	1.367	0.209	1.143	1.317	0.401
Arci	0.145	0.200	0.356	0.149	0.170	0.749	0.136	0.125	0.901	0.119	0.106	0.882
Acli	0.237	0.098	0.317	0.223	0.098	0.327	0.202	0.079	0.262	0.161	0.106	0.598
Avis	0.285	0.201	0.342	0.287	0.221	0.475	0.244	0.170	0.404	0.197	0.185	0.889
Observations	565	651	1206	457	471	928	287	265	552	193	151	344

[continues]

APPENDIX TABLE - CONTINUED–SUMMARY STATISTICS FOR ALL VARIABLES

	Full Sample (< 100 km)			< 50 km			< 25 km			< 15 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	217.9	115.5	0.248	237.4	120.1	0.285	187.7	124.5	0.362
Male 01	0.498	0.489	0.097	0.495	0.489	0.219	0.494	0.489	0.413	0.494	0.491	0.533
Hsize 01	2.239	2.212	0.535	2.295	2.212	**0.033**	2.297	2.214	0.058	2.307	2.243	0.054
Manufacturing 01	676.5	359.3	0.223	783.6	379.7	0.185	871.9	282.3	0.161	498.1	328.5	0.279
Services 01	858.6	397.4	0.121	995.9	408.9	0.147	1198	311.2	0.158	488.1	405.6	0.472
Tourism 01	0.071	0.061	0.529	0.066	0.048	0.195	0.054	0.042	0.375	0.051	0.049	0.903
Banking 01	44.74	36.91	0.240	45.58	38.25	0.315	41.03	36.92	0.435	38.41	39.99	0.783
Cars 01	58.82	59.66	0.513	59.52	60.28	0.562	59.47	60.30	0.645	59.74	59.94	0.905
Students 01	180.5	77.55	0.174	207.3	83.90	0.177	251.1	47.74	0.171	98.92	70.55	0.277
Health care eff. 01	5351	2624	0.237	6065	2934	0.277	7175	1272	0.187	2497	2231	0.777
Chemists 01	1522	1066	0.195	1687	1033	0.106	1543	953.9	0.229	1439	1066	0.432
Disp. Income 01	13897	14239	0.587	13940	14489	0.464	13856	14059	0.817	13894	13901	0.993
Farms 01	114.5	87.79	0.517	117.6	102.5	0.720	111.6	126.1	0.755	122.6	148.6	0.560
Large HH 01	71.56	33.36	0.085	81.25	33.17	0.069	91.95	28.22	0.129	51.49	35.37	0.149
Retired 01	754.0	401.9	0.228	862.0	415.7	0.193	1044	336.1	0.193	486.7	398.9	0.481
Very Old 01	219.9	136.2	0.265	247.8	140.7	0.230	294.9	115.9	0.216	135.2	140.3	0.866
Observations	565	641	1206	457	471	928	287	265	552	193	151	344