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Abstract

Whether campaign advertising influence elections' outcome is an open question, a paradox given the amount spent on campaigning in general and TV advertising in particular. We argue that the "absence of documentation" is due to the focus of the empirical literature on the United States, where allocation of campaign spending and advertising is decentralized. Exploring a quasi-natural experiment produced by the Brazilian legislation, we overcome the omitted variable and reverse causality problems caused by decentralized allocation. Gubernatorial elections work in a runoff system. While in the first round candidates' TV time shares are determined by their coalitions' share of seats in the national parliament, candidates TV time is split equally if a second round is necessary. Using differences between the first and second rounds as a source of exogenous variation, we find a large causal effect of TV advertising on voting outcomes. Since TV advertising is the most important item in campaign expenditures, this result sheds light on the effect of campaign spending on elections outcome (JEL CODES: D72; C33)

KEYWORDS: TV Advertising; Campaign Spending; Election Outcomes; Endogeneity; Quasi-Natural Experiments

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1 Introduction

> More generally, for the vast subfield of voting behavior and elections, determining whether political campaigns influence individual vote choice and election outcomes has become a Holy Grail." Goldstein and Ridout on the Annual Review of Political Science (2004)

Political scientists, economists, advertisers and policy makers have held a long interest in how political campaigns affect voting behavior. The impact of campaign spending and electoral advertising has received special attention.[1] This paper contributes by estimating the impact of TV and radio advertising on the elections outcome using a unique empirical opportunity: a quasi-natural experiment provided by the Brazilian electoral legislation. In contrast with most received literature, we find a large impact. Since the most important item on campaign spending is TV advertising, the difference between campaign spending and TV advertising is largely immaterial (Ansolabehere and Iyengar (1996)).[2]

Why does advertising change voters' minds? The most common explanation is persuasion. Advertising might convince those predisposed to vote, or persuade those indifferent to make up their minds (see Goldstein and Ridout (2004)). Different literatures take different approaches towards why persuasion occurs. Part of the political science and the psychology literature emphasize emotion. By appealing to voters' prejudices, fears and hopes, advertising would alter voters' empathy towards candidates. The political economy literature offers two explanations within the rational voter paradigm. The first explanation is that advertising is informative

[1] A non-exhaustive list includes Welch (1981), Green and Krasno (1988), Abramowitz (1991) and Levitt (1994) on campaign spending; Finkel (1993), Zaller (1996) and Goldstein and Ridout (2004) on media effects, which includes TV advertising.

[2] As an illustration of how indistinguishable TV advertising and campaign spending are, we considered intuitive to use TV advertising as proxy for campaign spending. Political scientists, however, have done just the opposite, i.e., use spending as a measure for advertising. See Goldstein and Ridout (2004). Anecdotal evidence also suggests that money is mostly used to buy TV airtime. Consider the following passage extracted from a recent issue of The Economist, April 26th, 2008: "If he is the Democratic nominee, it would be hugely to Mr. Obama's advantage to shun public funds. Granted, back in the days when he did not realize what a cash-magnet he is, Mr. Obama piously vowed to accept public money if his opponent did. Now, having seen how useful it is to be able to saturate the airwaves with ads praising himself and damning his opponent, he has slithered away from that vow." Our emphasis.

about the candidate's quality (or of its opponents' defects). The second explanation is signaling, inspired in Milgrom and Roberts (1986) ideas on advertising spending as a signal of product quality. Advertising is a costly way to signal one's quality (see Prat (2002A, 2002B)). The issue of why advertising matter is beyond the scope of the paper. Our question takes a step back: does advertising matter at all? From a policy perspective it is important to disentangle the reasons, but the question is only relevant if advertising matters in the first place.[3]

The empirical literature has failed to document the impact of spending and media on election outcomes. Gerber (1998) and Levitt (1994) are two examples of papers that find no effect of campaign spending on election outcomes. Despite this "minimal effects" results, no consensus has emerged. Bartels (1993), for example, calls the literature on media effects "one of the most notable embarrassments of modern social science." Goldstein and Ridout (2004), in a more recent paper, show that the question is still open in the political science literature. In the game-theoretical literature, a wide range of important papers on elections and lobbying take for granted that money matters. Consider the often quoted Grossman and Helpman (1996), in which money from special interest is used by candidates to influence the decision of uninformed voters. Baron (1994) has a very similar result. Another influential paper based on the assumption that money matters is Snyder (1989).

Perhaps the best way to put the paradox is the following. If money and TV are irrelevant, how can we rationalize the large sums spent on campaigns in general, and on TV advertising in

[3] Within the "emotional persuasion" paradigm, it is hard to conceive that advertising would be welfare increasing. See Prat (2006) for a survey on rational voter models of campaign advertising and electoral outcomes and the different implications for welfare. As for empirics, little convincing evidence is available. An exception is Huber and Arceneaux (2007), who use neighboring districts in battleground and non-battle ground states as a source of variation to estimate the impact of TV advertising. They find little support for the informative hypothesis but strong support for persuasion. Their experiment convincingly accounts for the fact that advertising targeting is endogenous, but cannot control for the fact that the amount of TV advertising that candidates can air is endogenous itself. Thus, their evidence is better suited for appraising the reason why TV matters but not for measuring the magnitude of the impact.

particular?[4,5] Consider the following passage from *The Economist* describing Barack Obama's prospects early on the U.S. presidential primary campaign:

> Can all this charisma win him the nomination? The polls say probably not. He cannot match Hillary Clinton's organisation on the ground. He trails her by ten points or so in national polls of likely Democratic primary voters ..., and he leads in none of the early primary states. On the other hand, he raised more money for the primaries than Mrs Clinton in the first quarter of this year—an astonishing feat for a newcomer. So he must be taken seriously.[6]

The quote suggests that money matters. Despite trailing Mrs. Clinton in polls, Mr. Obama "must be taken seriously" because of his fund-raising performance. However, it is unclear why money matters. Are Mr. Obama's prospects brighter than polls suggest because he has lots of money to spend on convincing voters? Or is money a signal of his political strength above and beyond polls? Also on the *The Economist*, the story "Of Cash and Crushes," July 5th 2007 describes the odds of winning based on funds raised, not polls.

Even if campaign spending and TV advertising does influence election results, it is hard to

[4] In the 2000 election, roughly U$ 3 billion were raised and spent by candidates and party committees. How large this number is debatable. Tullock's puzzle posits that campaign contributions are low when compared with the federal governments' gross investment and consumption (some 590 billion), which is the (maximum) amount "up for grabs" by special interests. See Ansolabehere, Figueiredo and Snyder (2003). Nevertheless, consider GM, the third largest advertiser in the US in 2006, for a sense of the magnitudes involved. GM spent 2.2 billion dollars in measured media. US revenues were 201 billion. Thus, political campaign advertising intensity is not much lower than GM's. See Advertising Age, June, 25th, 2007. The theoretical literature has provided some "supply-driven" explanations, in which donors can manipulate policy at little or no cost (see Dal Bó (2007) and Helpman and Persson (2001)).

Official Brazilian numbers are considerably lower. According to Transparência Brasil, an affiliated of Transparency International, total official campaign receipts in the 2002 national elections were close to R$400 millions (roughly U$ 110 million at the 10/02 exchange rate). However, there is evidence of significant covert campaign contribution and money rose by other illegal means. In 2005, for example, a large scale political scandals unveiled a large scheme of funneling money from state enterprises to fund political campaigning. See *The Economist*, June, 2nd, 2005 "Brazil bribery scandal: Jeffersonian democracy, tropical style."

[5] Levitt (1994) presents a couple of reasons. Perhaps the opportunity cost of raising money is low compared to the benefit of winning elections. Another reason is misinformation: politicians confound correlation with causality. We find both explanations lacking.

[6] See *The Economist*, "The Campaign's Brightest Start," June 14th 2007. Our emphasis.

identify their impact. The quote hints at the reasons: omitted factors and reverse causality.[7] Reverse causality because, in anticipation of high probability of winning, donors will contribute more generously if they expect their money to buy policy influence. Heterogeneity in candidate quality, something difficult to control for, causes omitted variable bias. All the following reasonable stories will prevent causal interpretation: more able candidates are better at raising money; donors prefer to donate to more competent candidates; and lobbyists prefer to put their money on candidates likely to win. Furthermore, the bias in OLS will tend to be away from zero.

Another source of trouble is unobserved electoral district preferences. A republican candidate running in a predominantly democratic district will face difficulty in raising money and getting votes. Or democrats in predominantly democratic district will not bother to raise money. Either way, if the district political inclinations are not properly controlled for, results will again be biased. In this case the direction of the bias is undetermined.

The econometric difficulties are particularly acute with U.S. data. Campaign financing – and consequently TV time allocation – is done in a decentralized, market-based way. Funding, spending, TV advertising and voting are all choices of political actors, i.e., candidates, donors and voters. Thus, they are jointly determined. The observed quantities are equilibrium values. As we know, estimating underlying parameters observing equilibrium values is notoriously challenging.

Although the literature has recognized these issues, it has been only partially successful at solving them. Papers that attempt to measure the impact of campaign spending or TV advertising on election outcomes using only cross-sectional data are not persuasive for several reasons. First, studies with pure cross-sectional data cannot account for unobserved quality candidate heterogeneity. Gerber (1998) try to control for the quality of contenders by including biographical information, such as dummies for whether the contender previously occupied an elected office, which capture only one possible dimension of quality. Gerber (1998) also use contender wealth to instrument for campaign expenditures, hoping to account for reverse causality. However, successful business men and politicians share many (unobserved) characteristics. Thus, if

[7] Psychologists and political scientists mention another problem, not related to the decentralized allocation: measurement. Exposure is one necessary condition. Having sufficient cognitive ability, which is almost never observable, is another. See Goldstein and Ridout (2004) and section II.

quality is not convincingly controlled for to begin with, wealth will correlate with unobservable determinants of elections outcome. Welch (1981) use demographic characteristics of the district, such as inequality and educational attainment, arguing that they determine donations but not voting behavior, which is quite debatable. Welch (1981) uses party affiliation and Gerald Ford's share of votes in the district as proxies for district preferences. Although these variables most certainly capture some district preferences, they are far from perfect. Much better if the same district is observed more than once, which demands a panel approach.

Levitt (1994) is arguably the most successful paper in the literature. Pairs of contestants are observed in different US congressional electoral races. Taking first-differences of repeated challengers eliminates time-invariant heterogeneity among candidates, thus mitigating omission and reverse causality problems. The question then is: what source of variation is left to estimate the impact of campaign spending? Put it differently, if candidate and district characteristics are really constant across races, why does campaign spending vary at all?

In fact, except for small random variations, we expect fund-raising to be roughly constant across races. Starting from an equilibrium situation, small variations in campaign spending would have small impact on electoral performance.[8] Not surprisingly, that is precisely Levitt [1994] finds.[9] A conundrum arises. If the identification strategy is successful, then all variation in campaign spending would be idiosyncratic, and one would expect a trivial impact on election outcomes. A related point is made by Prat (2006). Even after candidate quality is controlled for, the relationship between campaign spending and electoral outcomes should be interpreted as "the effect on electoral outcome of an extra dollar of campaign spending net of the political cost of persuading lobbies to donate the extra dollar". The problem arises precisely because lobbies' decision to donate is a choice, and therefore endogenous.

Using a unique empirical opportunity produced by the Brazilian electoral legislation, we

[8] There are other theoretical reasons to expect a zero impact when looking at equilibrium situations. Suppose a wealthy but unknown candidate faces a popular opponent whose support base is poor. Under reasonable assumptions about the production function of votes as a function of advertising, the equilibrium has the former with large disbursement strategy, and the later with a low disbursement, and votes are split. In this case, the raw relationship between spending and votes is indeed zero, although spending influence voting

[9] This "irrelevance result" arises in formal models. See Prat (2002). An analogy could be made with an ordinary industry: using GM and FORD advertising outlays and their market shares, one would probably find no relationships between these two variables. Why would they spend this money?

present a solution to this empirical conundrum. In Brazil, gubernatorial elections work in two-round system. The runoff round happens if no candidate reaches 50% of the votes in the first round, in which case the two most voted candidates face each other again to decide the winner. For a period of time before each round, TV and radio broadcasters are obliged by law to air candidates' advertising, free of charge. No additional paid TV/Radio advertising is allowed. The law determines that, before the first round, time is allocated among candidates according to the coalitions' share of representation at the National parliament. In the second round, however, the two runner-off candidates split the time equally. Hence, the electoral legislation produces a quasi-natural experiment, and the difference between first and second round TV and radio time share is a source of exogenous variation to estimate the effect of TV advertising on elections outcome.

Our procedure resembles Levitt's in the sense that we compare the same pair of contestants in different races. However, there are major differences. While in Levitt's paper races were at least two years apart, second rounds are no more than twenty-eight days after the first round. Hence, we are much more confident that candidates' and electoral districts' characteristics are in fact controlled for. More importantly, TV time is allocated by law. The first round TV time allocation reflects the political of strength candidates' coalition in the national parliament. State-level and national-level political strength are associated, and therefore the 1st round TV time is endogenous. However, differences between first and second rounds TV time shares are exogenous because of the TV time split rule in the second round. In summary, our quasi-natural experiment consists in observing, over a short period of time, two races in which the same candidates have different TV time shares, and second round allocation of TV time is outside the candidates' control.

Our experiment has one handicap. First and second rounds are different types of elections. For starters, the number of candidates is different. More importantly, defeated first round candidates forge informal alliances with second round contestants. We address this issue in different ways. By selecting sub-sample in which the first round looks like the second, we can assess how sensitive results are to the fact that configurations are different in the first and second rounds. Using poll information, we can partially account for second-round support and realignment of political coalitions. Results are not sensitive to these robustness procedures.

As a preview, we find a much large impact of TV advertising on elections' outcomes. Using our preferred estimate, one percentage point increase in TV advertising time shares causes a 0.272 percentage point increase in vote share. For a sense of practical relevance, in our sample the difference in TV time shares change 8 percentage points between first and second rounds. This implies a 2.176 percentage point change in voting share, meaning that TV alone would be enough to reverse the results 16% of the second rounds in the sample. When using only first-round data, which is admittedly endogenous, the magnitude of impact of TV time is almost twice as large. This is precisely what one would expect if unobserved candidate strength is not properly controlled for because the OLS bias tends to be away from zero.[10]

We then go one step further and explore the cross-section variation across voting units to investigate whether the impact of TV advertising changes according to city-level demographics. To the best of the our knowledge this is novelty of this paper, made possible by observing local voting records for a wide range of cities. All four "slope shifters": the influence of TV advertising is stronger in uneducated, poor and unequal cities where TV penetration is high. These results are important for three reasons. First, the decomposition is an additional falsification test for the hypothesis TV advertising causes election outcomes. It would be embarrassing if TV advertising had a stronger impact in educated places with low TV penetration. Second, the literature on lobbying normally makes the assumption that candidates use lobby money to advertise to uninformed voters (see Baron (1996) and Grossman and Helpman (1994)). The result that poor, educated voters are more susceptible to TV advertising provides support for this assumption. Although beyond the scope of our paper, these results are also in line with micro-founded models in which advertising convey information, since it has a larger impact on "uninformed" voters. Third, the decomposition of the advertising impact suggests that the sharp difference between our results and those in the received empirical may be due to reasons other than methodology or the data generating process. Levitt (1994) for example considered United States House of Representatives races. Compared to Brazilian states, US congressional districts tend to be smaller, both geographically and in terms of population. Perhaps TV advertising, and the attending money to pay for it, is more valuable in mass communication settings. For reaching smaller electorates, ground organization and person-to-person contact

[10] TV airtime applies to the whole state. Since we use municipal-level data on voting records, TV airtime does change across voting units of observations. Thus, the bias arising from different TV advertising efforts – which has an ambiguous sign - does not arise.

may be a superior means of communication. In this case, TV and money could have a smaller impact on electoral outcomes.

The paper has six sections, including this introduction. In section 2, we describe the Brazilian electoral system, with emphasis on the legislation on TV time allocation. Section 3 describes the data and section 4 outlines the estimation strategy. Results are in section 5. Section 6 concludes.

2 THE ELECTORAL SYSTEM AND ELECTORAL ADVERTISING IN BRAZIL

Brazil is a presidential federal republic comprising 26 states and one federal district, where the capital Brasília is. The executive branch of the federal government is headed by the President; the legislative branch consists of two houses – the Senate and the Chamber of Deputies. In states, the head of the executive branch is a governor, and the legislative branch consists of the State Assembly. The president and the governors are elected by direct ballot in two-round majoritarian system. Senators are elected by direct ballot in a one-round majoritarian system. Representatives at the Chamber of Deputies and the State Assemblies are elected by direct ballot in an open-list proportional system. Terms last four years, except for the senators, who serve eight-year terms.

Voting is compulsory in Brazil. The electorate is composed of three groups. First, all citizens between 18 and 64 years are automatically registered, and voting is mandatory for registered voters. Second, between 16 and 18 registering in optional, but voting is mandatory once registered. Finally, voting is optional for registered voters older than 64 years. Voters are subject to significant penalties for failing to vote. Besides fines, sanctions include becoming ineligible for public sector jobs, passport issuance and, more importantly, government transfers. Consequently, turnouts are uniformly high across elections and across rounds. Averaging over the three electoral cycles in our sample (1998, 2002 and 2006), first and second-round turnouts for gubernatorial elections were 81.35% and 80.69%, respectively.

Brazil has a multi-party system. The current number of active political parties in the country is close to thirty. However, only few parties are relevant at the national level – as data will show in the next section.

Gubernatorial elections work in a runoff system.[11] All registered candidates participate on the first round. If no candidate reaches 50% plus one of votes, a runoff between the two most voted candidates takes place some three weeks after the first-round. The candidate with most votes in the runoff is declared the winner. Table I shows the dates of first and second rounds of the three gubernatorial elections in our sample: 1998, 2002, and 2006.

Federal law mandates that over a period from 45 to 60 days preceding elections part of the TV and radio daily grids are allocated to political advertising free of charge. It is forbidden to buy and sell additional TV airtime. From 1998 onwards, political advertising was aired over a period of 45-day that ended in the Friday before election, that is, two days before voters go to the ballots. Gubernatorial advertising were aired on Mondays, Wednesdays and Fridays, in two blocks of 25 minutes each, one at lunchtime and another at prime night time. In case of runoff, political advertising is again mandatory, but for a period of roughly two weeks before elections.[12] In the first round, TV airtime is split among several different races. Gubernatorial elections compete with the presidential election, and the proportional ballots for the Federal Senate, the Federal Chamber of Deputies, and state assemblies. In the second round it shared time only with the presidential race. Thus, although the first-round TV campaign is longer, the total exposure is roughly the same between rounds.

In the first round, time is not equally allocated among candidates. From 1998 onwards, air time was allocated among parties or coalitions according to the following criteria: a) one third of the time equally divided among candidates; and b) two-thirds of time proportional to the number of representatives in the Chamber of Deputies. In case of a coalition, the total number of representatives in the coalition is considered. First-round coalitions have to be registered at

[11] Mayoral elections in cities larger than 200thd inhabitants also work on a runoff system. We concentrate on gubernatorial elections for two reasons. First, different cities are several observations on the same pair election-state. For mayoral elections, we would have only one observation for each pair election-city. Therefore data from mayoral elections are intrinsically noisier. Second, and more importantly, TV advertising should have a larger impact in elections in large, geographically disperse elections. For cities, particularly smaller ones, other means of campaign communications may be more important than TV and radio advertising. Additionally, presidential elections are also on the runoff system, but then there is data available for only three election years.

[12] From 1998 onwards, political advertising is transmitted starting 48 hours after the first-round results were officially announced by the state-level electoral authorities, the Tribunais Regionais Eleitorais (TREs), and again ended on the Friday before elections. Advertising was aired daily, in two blocks of 20 minutes each, one at lunchtime and another at prime night time.

the state-level electoral authorities (the TREs). Registered coalitions determine the TV time allocation and the number of representatives elected for the state assembly and the national congress. If a second round is necessary, the law mandates that the time be equally split among the two runner-off candidates. The difference in TV advertising time between rounds is explored as a source of exogenous variation in advertising time.

3 Data and Descriptive Statistics

Two pieces of electoral data are used, both publicly available from the national electoral authority, the Tribunal Superior Eleitoral (TSE). The first is the voting record of all candidates in three different gubernatorial elections (1998, 2002 and 2006), at the city-level. Second, the composition of the coalitions, which allows us to compute TV and radio advertising time for all three gubernatorial elections, according to the law described above.[13] Air time is used as a proxy for advertising exposure.[14] City-level demographics are from the 2000 census. Finally, poll data is from IBOPE, a well-established market research institute, with a long tradition in conducting electoral opinion polls.

Although we consider gubernatorial elections, the unit of observation is a city for two reasons. First, for the same election, we have several observations of voting behavior, which increases precision of estimation even after clustering observations at the state level. More importantly, it provides variation at the voting unit, which allows us to investigate how the impact of TV advertising varies with demographics such as educational attainment, income, inequality, and TV and Radio penetration.

[13] Party switching is common in Brazil, implying that representation at the time of elections is potentially different from representation that decided TV time allocation. Hence, TV reflects the strength of the coalition with noise. For an illustration, until the 2004 elections, TV time was based on composition at beginning of the legislature. For the 2006 elections, representation during the 2002 election was considered. Furthermore, for the elections before 2006, we only observe the composition at the end of the elected legislature, not the one sworn in (which is the relevant for the purposes of TV time allocation). Since some deputies do change parties in the interim, we observe TV time allocation with some noise. This is not a major concern since party switching in the interim is not such a relevant phenomenon.

[14] Clearly, being aired is only a condition to expose candidates to TV advertising. A finer measure of exposure is Gross Point Ratings (GRP), the number of times a TV set was turned at the time the advertising is aired. Unfortunately this information is unavailable.

The sample consists of all gubernatorial races in which a second round was necessary during three electoral cycles: 1998, 2002 and 2006. In total, there are 35 races in the sample, taking place in 18 different states and the Distrito Federal. Table II contains information on the characteristics of the sample.

The sample includes cities from all 5 Brazilian regions. States are very heterogenous in demography, ranging from Southern European-like states Rio Grande do Sul and Paraná to the very under-developed Northeastern states Maranhão and Ceará. Incidentally, it includes the three most populous states (Rio de Janeiro, São Paulo and Minas Gerais). Table III presents the distribution of Federal Chamber of Representatives by party, for the three relevant legislatures (1994, 1998 and 2002), which along with information on coalitions determine the first-round TV advertising time shares.

Some important characteristics of the Brazilian political system arise from table III. There are four main parties at the national level: Center-right Partido da Frente Liberal (PFL, later renamed DEM), centrist Partido do Movimento Democrático Brasileiro (PMDB), center-left Partido da Social Democracia Brasileira (PSDB), and leftist Partido dos Trabalhadores (PT). Additionally, there are at least four other relevant middle sized parties (PTB, PPB, PL and PSB), and several marginal parties. Concentration measures such as the Herfindahl-Hirschman Index, the C_4 and the C_2 suggest that the effective number of competitors is roughly 7. Therefore, first-round advertising time is neither too concentrated nor too dispersed. If the system was dispersed, time differences between the first and second rounds would all be very large, and it would hard to identify the effect of advertising on election outcomes.

While TV advertising time is determined at the local coalitions´ strength at the national level, local politics is what ultimately matters for gubernatorial elections. Table IV presents correlations between pairs of national and state level distributions of parliamentary seats.

Data shows that TV advertising time – determined by national political strength – also reflect state-level political strength. Although there is some variation (correlation coefficients ranges from 0.28 (Roraima, 1994) to 0.93 (Paraná, 2002)), twenty-three out of thirty-six of the estimated correlations are above 0.70. Therefore, it is difficult to identify the impact of TV using only first round variation in TV time. Table V contains descriptive statistics on the number of candidates per gubernatorial race.

The average number of first-round candidates, from 6.23 in 1998 to 7.90 in 2006, reflects the dispersion of the Brazilian party system. Note that the mean number of candidates minus one standard deviation is never less than 3.64. Thus, first and second round races are structurally different, a fact we have to take into account in our empirical strategy.

Table VI presents the first-round TV and radio times of the first and second placed candidates in elections that required a second round. Summary statistics on TV time and vote share hint at the results in section V. In the first-round, first placed candidates have more TV time share than second placed candidates on average. Inspection of columns (2) and (3) show that the difference in vote share between 1st and 2nd placed in the first round is larger than that of the second round, in relative and absolute terms. Since second round TV time is equally split, averages suggest a positive correlation between TV time and voting performance. Evidently, means can disguise candidate heterogeneity that may spuriously produce the results. Estimation strategy outlined in section IV solves these (potential) problems.

4 Empirical Strategy

Following Levitt (1994), we first-difference the same pair of candidates over two election races to control for time-invariant unobserved heterogeneity among candidates. There are, however, two major differences. First, in our case the two election cycles are in fact two rounds in the same election. This is a particularly interesting feature of the data. Since races are close in time, unobserved characteristics are likely to be constant. More importantly, second round TV time allocation is determined by legislation, not by choice of the participants in the political game. Therefore, the difference in TV time between rounds is exogenously given, after controlling for candidate fixed-effects. Therefore, the two major concerns about Levitt´s procedure – long interval between election cycles and non-random resource allocation – are greatly mitigated.

The details of the empirical implementation are as follows. The data has a panel structure. The cross-sectional unit is a race e, defined by the pair year (t) – city (i). The time-series unit is a round r. Cross-sectional units are observed twice, once for each round. For example, the first round in the city of Santos of the 1998 gubernatorial election in the state of São Paulo is one observation ($r = 1$). The second round is another ($r = 2$). Races e belong to the set defined by the cells of table II.[15]

[15] Not all cities appear three times in the sample for two reasons. First, cities are created (or extinguished) over the 1998-2006 period (see table II). Second, and more importantly, in not all states, and consequently in

A and B refer to the first-round winner and runner-up, respectively. Define $votes\_A_{er}$ as the share of votes of the first-round winner in the round r of race e. $votes\_B_{er}$ is defined analogously. Let $TVtime\_A_{er}$ and $TVtime\_B_{er}$ be the share of advertising time in round r of election e allocated to the first round winner and runner-up, respectively. Finally, define:

$$dif\_votes_{er} = votes\_A_{er} - votes\_B_{er}$$

and

$$dif\_TVtime_{er} = TVtime\_A_{er} - TVtime\_B_{er}$$

First round votes are normalized to sum 1 so that we can first and second round numbers are comparable. While by definition $\sum_{e \in state} dif\_votes_{e1} > 0$, it may be that the runner-up wins in some cities. $\sum_{e \in state} dif\_votes_{e2}$ may be negative if the runner-up comes back and wins the second round. By construction $TVtime\_A_{e2} = TVtime\_B_{e2} = 0.5$. Thus $dif\_TVtime_{e2} = 0$ for all e.

The goal is to investigate how the distribution of time affects the election outcomes. We have a mind a linear relationship given by:

$$dif\_votes_{er} = \alpha + \gamma dif\_TVtime_{er} + \omega round_r + \lambda X_e + \varepsilon_{er} \tag{1}$$

In equation (1), $round_r$ is a dummy that assumes the value 1 for $e = 2$. The parameter ω captures changes between rounds in average voting behavior. A positive ω means a "widening gap" effect: the first-round winner on average increases her advantage in the second round. X_e is a vector of characteristics of election e. They control for a wide range of the pair city-election year characteristics. Examples of such characteristics are (total) campaign spending, average candidate quality, city characteristics, election-year specific effects, and so on. Finally, ε_{er} contain all other determinants of the voting outcomes.

γ is the parameter of interest. It captures the effect of the difference in TV time shares on the difference in voting performance. We test the hypothesis that $\gamma > 0$. By first-differencing the data over rounds, X_e disappears from (1), and no omission bias arises as long as remains constant between rounds, which is reasonable. First-round coalitions are formed taking into account two factors, among other things: their impact on TV time shares, and the chances

not all cities, a second round was required in all three election years.

of winning.[16] Thus, first-round variation in TV time is correlates with unobserved candidate strength. Since the second round TV time cannot possibly respond to the probability of winning the election (they always split), reverse causality disappears when the data is first-differenced. The estimated equation is:

$$\Delta dif\_votes_e = \omega + \gamma \Delta dif\_TVtime_e + \Delta\varepsilon_e \tag{2}$$

where Δ is the difference between the second and first rounds.

The major challenge in interpreting the parameter γ as causal comes from the handicap that our quasi-experimental data have relative to the perfect experiment. In ideal controlled experiment, the same pair of candidates would be observed twice (or more times), with a short-span of time between observations and different TV time shares in each round of voting (characteristics present in our quasi-natural experiment), under identical voting settings. In other words, the ideal experiment would have everything but TV time constant over voting rounds.

Our experiment violates the last condition. Arguably, the first and second rounds are two different elections, and electoral conditions do change between rounds. One such example is the emergence of scandals, although it is not a serious threat to identification because TV time is allocated in pre-determined in a centralized manner. In this case, scandals - which are part of the error term $\Delta\varepsilon_{er}$ - can safely be assumed not to correlate with Δdif_TVtime_{er}. In addition, the short time span between rounds mitigates the impact of scandals As mentioned in the introduction, Levitt (1994) faces a far more serious problem due to the presence of scandals. Because campaign spending is allocated with a market mechanism in the United States, and because there is a two-year period between voting observations, scandals will probably cause both spending and voting.

While first-round political coalitions have to be registered at the electoral authority, second-round political alliances are informal and thus unobservable. It is not uncommon for defeated first-round candidates to support one of the two first round winners in exchange for positions in the future administration. Unobservable change in political strength between rounds is dangerous to our purposes if it correlates with how TV time share changes between rounds. In

[16] Members of the coalition normally share the spoils of victory in the form of positions in the elected administration.

other words, second-round political support - which is part of the error term $\Delta\varepsilon_{er}$ - may be correlated with Δdif_TVtime_{er}. Consider the following story. Assume defeated candidates tend to support runner-ups. Table VI shows runner-ups tend to have less first-round TV time than winners. In this case, TV time would capture unobserved second-round political alliances, and results would be biased away from zero. Although it is unlikely that third-placed candidates will systematically support the runner-up, we cannot dismiss this possibility.

The first and second placed candidates receive roughly 80% of the votes in the first round, which mitigates the impact of political support (see table VI). Nevertheless, we still follow two different strategies to deal with the question of second round political support. The first strategy involves selecting sub-samples of races that are closer to the perfect experiment, i.e., elections in which the first and the second round are even more similar. First, we define whether the third placed candidate in the first round is pivotal, in the sense that her support would change the first-round outcome if all her votes migrated to the runner-up. More precisely, define:

$$C\_pivotal_{er} = \begin{cases} 1, \text{ if } votes\_C_{er} - dif\_votes_{er} > 0 \\ 0, \text{ otherwise} \end{cases}$$

The model is estimated with the sub-sample of races in which $C\_pivotal_{er} = 0$. A second sub-sample is formed by the elections in which the runner-up and the winner had more than 92.04%, which is the 75th percentile of the distribution of the sum of runner-up and winner first-round vote shares.

Changing the dependent variable is the second strategy for dealing with second-round political support. The difference in vote intention measured by the first second-round is subtracted from the old dependent variable. Thus, the new dependent variable is similar to a "difference-in-difference-in-differences" of votes, i.e., the old difference-in-differences corrected for the vote intention in the beginning of the second round. It is important to notice that by using this new dependent variable we account for not only for political support but also for all changes that occurred between the end of the first round and the beginning of the second-round campaigning time, such as scandals. More precisely, the transformed dependent variable is:

$$\Delta dif\_votes^{*}_{er} = (votes\_A_{e2} - Poll\_A_{e2}) - (votes\_B_{e2} - Poll\_B_{e2}) \tag{3}$$

where $Poll\_A_{e2}$ and $Poll\_B_{e2}$ are the vote intentions in the first opinion pool of the second round.

The first second-round pool is typically conducted ten days after the first round, which gives enough time for the announcement of political supports, and for most of the pre-second-round-TV-campaign relocation of voters to take place. In this procedure, only variation in voting outcomes *above and beyond* the changes that normally occur because of rearrangements that normally occur between the end of the first-round campaign and the beginning of the second round campaign, *including most second round political support.*

Another issue concerns campaign funds and spending, which are unobservable. Sums spent in Brazil are large, especially if one considers that TV airtime is free of charge (see Samuels (2001)).[17] Insofar as most money is raised before and throughout the first-round campaign, available resources are accounted for in our procedure.[18] Of course, anticipating a likely second round, candidates may defer spending to the second round. Since information on the timing of expenditures is unavailable we cannot account for this possibility. Nevertheless, it is not obvious why second-placed candidates would defer more spending than first-placed candidates, especially because the second-round election is rather short and not much money can be spent on it. Finally, although no hard data on the type of expenditure is available, anecdotal evidence abounds that the single most important expenditure item is TV advertisement production costs.[19] Thus, under the unlikely scenario that runner-ups defer spending more that first-placed candidates, they would mainly be deferring expenditures on TV advertising. For these reasons, we are cautiously confident that unobserved campaign fund-raising and spending are not a serious threat to our identification strategy.

Finally, a word on the mechanism involved in allocating TV airtime. Consider again equation (1). Table IV shows that, on average, $dif\_TVtime_{e1} > 0$ because first-round winners have on average more TV time than runner-ups. By construction, $dif\_TVtime_{er} = 0$. Thus, one may wonder whether results arise mechanically if there is a natural closing gap between the first and second rounds. We implement several procedures to probe results to this possibility.

[17] In the 1994 presidential elections, Cardoso (the winning candidate) spent US$ 41 billion. For a comparison, in the 1996 American presidential race, Clinton spent roughly US$43 billion, and Dole some US$ 45 billion. Figures for gubernatorial races in Brazil are similarly high.

[18] There is no hard evidence on this. However, the little time between rounds (roughly three weeks) suggests that most money must be raised prior to the second round.

[19] See *Jornal do Brasil,* Tesoureiro se desdobra por verba, July 12th 1998 (in Portuguese).

5 Results

5.1 Main Results

Table VII contains the main results. Column (1) presents the estimates of model (2). The point estimate of the effect of TV time on election outcome is 0.272, and it is significant at the 1% level. This coefficient means that shortening the TV time gap one percentage point has a 0.272 percentage point impact on the difference in voting outcome. For a feeling of practical importance, consider the figures in table VI. Averaging out the three elections in the sample, the mean first-round TV time share difference is 7.7 percentage points, which implies an impact of on the voting difference (remember the difference is always zero in the second round). On average, the gap between the first-round winner and runner-up closed by 3 percentage points.[20] Thus, TV is responsible for 72% of the closing gap.

In column (2), the model is re-estimated for the sub-sample of elections in which the third placed in the first round was not pivotal for the second round election ($C\_pivotal_{er} = 0$). Results are, if anything, slightly stronger. In column (3), the procedure is re-estimated for all elections in which the (first round) winner and runner-up had 92.04% of the votes or more (the 75th percentile of the sum of the votes of the runner-up and winner). Results are again stronger.

Column (4) has the estimates when the dependent variable is $\Delta dif\_votes^{*}_{er}$. As expected, when using only the difference between second round votes and the first second-round opinion poll, the impact is lower than in columns (1)-(3). They are, however, similar (roughly 0.25 as opposed to roughly 0.29), and still precisely estimated.

From table VI, we know that runner-ups have on average less first-round TV time than winners. Thus, our procedure allocates more TV time to weaker candidates, on average. One reasonable concern is whether our results are sensitive to the fact that Δdif_votes_{er} is on average larger for the runner-ups. One way to deal with this issue is selecting a sample of cities in which Δdif_votes_{er} has zero mean and median. Results are again similar (column (5)).

Columns (6) through (8) deal with the same concern in other ways. In column (6), we

[20] From table VI, in first round, the differences between the winner and the runner-up was 6, 8 and 7 percentage points in 1998, 2002 and 2006, respectively. In the second round, it was 4, 6 and 2, respectively. The figure found is the average of (6 – 4 = 2), (8 – 6 = 2) and (7 – 2 = 5).

include the first first-round interview. If first-round runner-up were unknown in the beginning of the race, we expect them to gain voting intention faster than first-round winners through the race. Barack Obama's race against Hillary Clinton perfectly illustrates this "underdog" effect. Since runner-ups have on average less time in first-round, our results could arise spuriously because runner-ups become known throughout the race. Including the difference in the first first-round interview should capture the "underdog" effect". In column (6) we use the normal dependent variable. Results are similar to column (1). Column (7) contains the same model as in column (6) except that we restrict the sample to those races in which the runner-up had more first-round TV than the winner. If the results were driven mechanically by a "catch-up" effect, they should disappear in the sub-sample. In fact, they are stronger. Finally in column (8) we use the whole sample again but the dependent variable is substituted by the modified version (equation (3)). Results are again similar to the uncontrolled version (column (4)).

5.2 First-Round Results

As an additional test, we estimate a model that uses only first-round data and relate voting behavior to TV time directly by Ordinary Least Squares (OLS). For maximal comparability, city and election year dummies are included. The cross-sectional unit in model (2) is a pair city-election year. Evidently, with first-round data only one is not able to replicate controlling for city-election year (would imply including as many dummies as observations). The closest one can get is including a full set of election year and city dummies. Table VIII contains the results.

The impact of changing the difference in TV time share by one percentage point is now 0.451 percentage points. This point estimate is roughly 67% higher than the average estimated effect reported in table VII. Indeed, theoretical reasons behind the endogeneity of TV time suggest that the OLS bias is away from zero: intrinsically stronger and more able candidates receive more TV time and more votes. When political strength, quality and ability are unaccounted for, the estimated effect of TV advertising time is expected to be stronger.

In column (2), we include the last opinion poll before the first-round TV campaign started. This variable partially controls for *ex-ante* political strength. Two things are worth noticing. First, *ex-ante* political strength has the expected positive sign, and it is statistically significant, although only marginally ($p-\text{value} = 9.9\%$). Second, as predicted by theory, after including a measure of political strength TV time has a smaller impact on voting. Now the impact is 0.399. This difference, however, is partly due to different samples (polls were not conducted in some races).For this reason we re-estimate the model in column (1) restricting the sample to those observations that belong to the estimation in column (2). Although the difference is now less dramatic (0.399 versus 0.450), it still arises. In summary, including *ex-ante* strength mitigates OLS omission bias, as expected. But the bias still arises. Hence, using only variation between rounds is the proper way to control for omission and reverse causality.

5.3 Demographic Determinants of TV Advertising Effect

In this subsection we investigate some possible determinants of the impact of media exposure. In levels, the model is:

$$dif\_votes_{er} = \alpha + \eta Shifter_e + \gamma dif\_TVtime_{er} + \tag{4}$$

$$+\kappa Shifter_e \times dif\_TVtime_{er} + \omega round_r + \lambda X_e + \varepsilon_{er} \tag{5}$$

When first-differences are taken (or election specific dummies are included), the shifter disappears because it does not vary between rounds. The estimated model is:

$$\Delta dif\_votes_{er} = \alpha + \gamma \Delta dif\_TVtime_{er} + \tag{6}$$

$$+\kappa \Delta Shifter_e \times dif\_TVtime_{er} + \Delta\varepsilon_{er} \tag{7}$$

Five shifters are considered: income per capita, income distribution, a measure of educational attainment, TV and radio penetration. Using the 2000 census, we match election-year data with demographics of the city, and estimate the parameters in the right-hand side of (5). Table IX shows some descriptive statistics on the cities that compose the sample. Results are in table X.

In columns (1)-(5), each of the following variables are interacted with Δdif_TVtime_{er}: income per capita, Gini, Schooling, TV and Radio penetration. As expected, the impact of TV advertising is larger in poorer (column (1)), more unequal (column (2)), and less educated (column (3)) cities. Seemingly surprising, TV penetration and Radio penetration are associated with a smaller impact of TV/Radio advertising (columns (4)-(5)).

In column (6), all variables are included. Four new facts arise. First, TV penetration has now the expected sign: cities where TV penetration is deeper the impact of TV advertising is stronger. This suggests that the estimate in column (5) captures income. Second, radio has no impact. This is probably due to the fact that there is too little variation in radio penetration (see table IX). Third, the estimated coefficient on income is no longer statistically significant, although it has the "correct" sign. Education, on the other hand, survives intact to inclusion of several factors. Income distribution is marginally significant. When radio is excluded (column (7)), there is enough variation to estimate the impact of income distribution with some precision (p − value = 9.8%). In summary, evidence suggests that the impact of TV advertising is more pronounced in less educated places, where income distribution is more unequal, and with a deeper TV penetration.

A common result in the theoretical literature on lobbying is that, in equilibrium, candidates take lobby money and use it to advertise to uninformed voters (see Baron (1994), Grossman and Helpman (1994) and Snyder (1989), among others). Evidently, implicit is the assumption that advertising in fact affects voting behavior. Our results show that this widely used assumption has empirical support. Not only advertising do influence voters' choice, but this is particularly true for in poorly educated places, where the proportion of "uninformed" voters is higher.

6 Conclusion

From received literature we received a conventional wisdom on the academic literature that campaign spending and media have "minimal effects" on elections outcome. This conventional wisdom is at odds with the perception of politician and political analysts, as the quote on Barack Obama's prospects show. Using quasi-experiment data, we find evidence contrary to the academic conventional wisdom: TV exposure in gubernatorial elections in Brazil have a strong on elections outcome, both practically and statistically. The magnitude - 70% of the closing gap from first to second round in gubernatorial elections - suggests that, while TV advertising cannot always predict elections outcome, it is a major determinant.

How can one reconcile our results with a large literature that says differently? The empirical literature on campaign spending and media effects suffer from either one of two problem It is either not persuasive that the omission and reverse causality problems are properly solved or, in the way of solving, throw so much variation away that it is difficult to estimate anything precisely. In contrast with the literature we have access to a source of significant exogenous variation.

Another avenue for reconciliation is in table IX. Brazil is a poorly educated highly unequal country where a high access to TV. Hence, it scores high in three factors that magnify the impact of political advertising.

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Table I – Calendar of Gubernatorial elections in Brazil		
Year	**First round**	**Second round**
1998	October, 4th	October, 25th
2002	October, 6th	October, 27th
2006	October, 1st	October, 29th

Source: Tribunal Superior Eleitoral (TSE) and Lei N° 9.504, September 1997.

Table II – Sample Distribution

State	1998	2002	2006	# of municipalities
Amapá	X	X		16
Ceará		X		184
Distrito Federal	X	X		1
Goiás	X		X	242
Maranhão			X	217
Mato Grosso do Sul	X	X		77
Minas Gerais	X			853
Pará	X	X	X	143
Paraíba		X	X	223
Paraná		X	X	399
Pernambuco			X	185
Rio de Janeiro	X		X	91†
Rio Grande do Norte		X	X	167
Rio Grande do Sul	X	X	X	467††
Rondônia	X	X		53†††
Roraima	X	X		15
Santa Catarina		X	X	293
São Paulo	X	X		645
Sergipe	X	X		75

†In 2006, the state of Rio de Janeiro had 92 municipalities
††In 2006, the state of Rio Grande do Sul had 466 municipalities
†††In 2002, the state of Rondônia had 52 municipalities
Source: Tribunal Superior Eleitoral (TSE)

Table III: Elected members of the Federal Chamber of Deputies, by party

Party	Year 1994	Year 1998	Year 2002
PPR	51	0	0
PDT	34	25	21
PT	50	59	91
PTB	32	31	26
PMDB	107	83	76
PSC	3	2	1
PL	13	12	26
PPS	2	3	15
PFL	89	105	84
PMN	4	2	1
PRN	1	0	0
PP	34	0	0
PSB	15	18	22
PSD	3	3	4
PV	1	1	5
PRP	1	0	0
PSDB	63	99	70
PC do B	10	7	12
PPB	0	60	48
PRONA	0	1	6
PSL	0	1	1
PST	0	1	3
PSDC	0	0	1
Total	513	513	513
Herfindhal-Hirschman Index	1227	1403	1179
C_4	60%	68%	63%
C_2	38%	40%	34%

Source: Tribunal Superior Eleitoral (TSE)

Table IV - Pairwise Correlation: Brazilian Chamber of Deputies and in the State Assemblies†

Year					
1994		1998		2002	
Amapá	0.61	Amapá	0.48	Goiás	0.77
Distrito Federal	0.34	Ceará	0.68	Maranhão	0.57
Goiás	0.83	Distrito Federal	0.63	Pará	0.72
Mato Grosso do Sul	0.82	Mato Grosso do Sul	0.75	Paraíba	0.82
Minas Gerais	0.80	Pará	0.87	Paraná	0.93
Pará	0.87	Paraíba	0.70	Pernambuco	0.88
Rio de Janeiro	0.61	Paraná	0.87	Rio de Janeiro	0.70
Rio Grande do Sul	0.60	Rio Grande do Norte	0.73	Rio Grande do Norte	0.65
Rondônia	0.46	Rio Grande do Sul	0.62	Rio Grande do Sul	0.74
Roraima	0.28	Rondônia	0.80	Santa Catarina	0.89
São Paulo	0.85	Roraima	0.55		
Sergipe	0.89	Santa Catarina	0.86		
		São Paulo	0.91		
		Sergipe	0.83		

†: Correlation between the seat distribution of the State Assembly and the National Chamber of Deputies
Source: Tribunal Superior Eleitoral (TSE)

Table V - Descriptive statistics: number of 1st round candidates

Year	Number of Candidates	
	Mean	Mean ± 1 Standard Deviation
1998	6.23	(3.64, 8.82)
2002	7.86	(4.90,10.81)
2006	7.90	(5.82,9.98)

Source: Tribunal Superior Eleitoral (TSE)

Table VI – Descriptive statistics: TV time and vote shares

Year	Candidate‡	Mean time share in the first round of the elections†	Mean vote share in the first round of the elections†	Mean vote share in the second round of the elections†
1998	1st placed in the 1st round	31%	43%	52%
	2nd placed in the 1st round	27%	37%	48%
2002	1st placed in the 1st round	29%	41%	53%
	2nd placed in the 1st round	23%	33%	47%
2006	1st placed in the 1st round	34%	44%	51%
	2nd placed in the 1st round	21%	37%	49%

†Averages were computed attributing equal weights across state

‡ Statewide votes

Source: Tribunal Superior Eleitoral (TSE)

Table VII - Dependent Variable: Δ(*Vote share_A - Vote share_B*)§

	First-Difference (2nd minus 1st round)							
	(1)	**(2)†**	**(3)††**	**(4)‡**	**(5)£**	**(6)**	**(7)¥**	**(8)‡**
Δ(*Time share_A - Time share_B*)	**0.272**	**0.311**	**0.301**	**0.247**	**0.326**	**0.277**	**0.519**	**0.237**
	(0.077)***	**(0.070)*****	**(0.057)*****	**(0.104)****	**(0.174)***	**(0.080)****	**(0.135)*****	**(0.099)****
Δ(*First first-round poll*)						**-0.009**	**0.094**	**0.056**
						(0.079)	**(0.046)***	**(0.027)***
Constant	-0.002	0.003	0.051	-0.089	-0.004	-0.004	-0.057	-0.097
	(0.029)	(0.025)	(0.019)**	(0.031)***	(0.034)	(0.038)	(0.053)	(0.029)***
Number of Observations	7925	4537	1982	7379	3640	7402	1760	7115
F- statistic	12.52	20.01	27.68	5.64	3.51	6.04	10.95	4.96
R^2	0.129	0.184	0.184	0.120	0.100	0.138	0.242	0.127

§ Standard errors are clustered at the state-election year level. First round vote shares are normalized to sum 1.

† Sample restricted to elections in which *C_pivotal* = 0

†† Sample restricted to elections in which the sum of the winner and the runner-up votes were more than 92.04% of the votos in the first round (75th percentile of the sum of votes of winner and runner-up).

‡ Modified Dependent Variables: Differences from 1st second-round opinion poll

£: Sub-sample with 0 mean and median of Δ(*Time share_* A - *Time share_B*)

¥: Only races in which the first-round runner-up had a larger share of votes than the the first-round winner

*** = significant at the 1% level

** = significant at the 5% level

* = significant at the 10% level

Source: Tribunal Superior Eleitoral (TSE) and IBOPE.

Table VIII - Dependent Variable: *Vote share_A - Vote share_B* §

	OLS: First-Round Only†		
	(1)	(2)	(3)
Time share A - Time share B	**0.507** **(0.152)*****	**0.399** **(0.145)*****	**0.450** **(0.165)*****
First Poll A - First Pool B		**0.067** **(0.042)***	
Constant	0.067 (0.028)**	0.072 (0.021)***	0.088 (0.025)***
Number of Observations	7925	7651	7651
R^2	0.051	0.072	0.101

§ Standard errors (in parentheses) are robust to clustering within the state-election year pair. All models include a full set of city and year dummies.

† First round vote shares are normalized to sum 1.

§: Same sample as in columns (2).

*** = significant at the 1% level

** = significant at the 5% level

* = significant at the 10% level

Source: Tribunal Superior Eleitoral (TSE) and IBOPE

Table IX - Descriptive Statistics, city-election pairs in the sample§

	Median	Mean	Standard Deviation
Income per Capita †	4.47	5.42	5.88
Gini ††	0.55	0.55	0.06
Years of Schooling †††	4.46	4.41	1.21
Television ‡	85.98	81.78	50.11
Radio ‡	88.68	85.03	12.21

§: Observation is a race, i.e., a city-election pair.

†Annual income per capita in thousands of 2000 dollars

†† Gini belongs to the interval [0,1]

††† Years of Schooling is the average number of years of schooling

‡ % of households in which there is a television set

Source: Instituto Brasileiro de Geografia e Estatística (IBGE)

Table X Dependent Variable: Δ(*Vote share_A* - *Vote share_B*)§

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Δ(*Time share A* - *Time share B*)	2.109	0.753	1.028	0.203	0.167	1.986	1.955
	(0.605)***	(0.198)***	(0.242)***	(0.078)**	(0.088)*	(0.734)***	(0.630)***
Log (Income)†* Δ(*Time share A* - *Time share B*)	-1.595					-0.640	-0.624
	(0.518)***					(0.439)	(0.444)
Log (*Gini*)* Δ(*Time share A* - *Time share B*)		0.388				0.320	0.317
		(0.194)**				(0.204)	(0.189)*
Log(Schooling) †† * Δ(*Time share A* - *Time share B*)			-0.525			-0.509	-0.505
			(0.159)***			(0.184)***	(0.161)***
Log (*TV*)††† * Δ(*Time share A* - *Time share B*)				-0.274		0.168	0.175
				(0.087)***		(0.089)*	(0.100)*
Log (*Radio*)†††† * Δ(*Time share A* - *Time share B*)					-0.596	0.029	
					(0.230)***	(0.295)	
Constant	-0.011	-0.007	-0.011	-0.005	-0.007	-0.014	-0.014
	(0.028)	(0.028)	(0.028)	(0.029)	(0.029)	(0.028)	(0.028)
Number of Observations	7837	7837	7837	7836	7836	7836	7836
F - statistic	12.610	9.360	13.280	12.700	12.650	6.620	6.420
R^2	0.179	0.150	0.195	0.152	0.162	0.206	0.206

§ All standard errors (in parentheses) are robust to clustering within the state-election year pair. First round vote shares are normalized to sum 1.

† *Income* is per capita income in municipality in 2000 dollars.
†† *Years of Schooling* is the average number of schooling years among the 15-64
††† *TV* is the percentage of households with at least one television set
†††† *Radio* is the percentage of households with at least one radio
£: Same as in table VII, column (1) except that the same is the same as in the other columns of table X.
* = significant at the 10% level
** = significant at the 5% level
*** = significant at the 1% level
Source: Tribunal Superior Eleitoral (TSE) and Instituto Brasileiro de Geografía e Estatística (IBGE)

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