Campaign Advertising and Election Outcomes: Quasi-Natural Experiment Evidence from Gubernatorial Elections in Brazil[§]

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Abstract

Despite the "minimal effects" conventional wisdom, whether and how campaign advertising influence elections outcome remains an open question. This is paradoxical because in the absence of a causal link from advertising to candidate performance, it is difficult to rationalize the amounts spent on campaigns in general, and on TV advertising in particular. However, most studies using US data suffer from omitted variable bias and reverse causality problems caused by the decentralized market-based method of allocating campaign spending and TV advertising. In contrast with received literature, we explore a quasi-natural experiment produced by the Brazilian electoral legislation, and show that TV and radio advertising has a much larger impact on election outcomes than previously found. In Brazil, by law, campaign advertising is free of charge and allocated among candidates in a centralized manner. Gubernatorial elections work in a runoff system. While in the first round, candidates' TV and radio time shares are determined by their coalitions' share of seats in the national parliament, the two most voted candidates split equally TV time if a second round is necessary. Differences in TV and radio advertising time between the first and second rounds are a source of exogenous variation to evaluate the impact of TV advertising on election outcomes. We find that a one percentage point increase in TV time causes a 0.247 percentage point increase in votes. Since TV advertising is the most important item in campaign expenditures, this result sheds light on the more general question of the effect of campaign spending on elections outcome.

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

JEL CODES: D72, C33

[§] The authors would like to thank Marco Antonio Bonomo, Fernando Blanco, George Avelino, Ciro Biderman, Miguel Foguel, Cláudio Ferraz, Helios Herrera, Paulo Arvate, Carlos Pereira and seminar participants at IPEA, USP-RP, FGV-SP, the Brazilian Econometric Society, NUY, and the Michigan State University / FGV-SP Workshop on Institutions, Policy Performance and Cooperation for insightful comments. Bernard Tamler, Mariano Lima and Edson Macedo provided superb research assistantship. Usual disclaimer applies.

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"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)

I – INTRODUCTION

Political scientists, economists, advertisers and policy makers have held a long interest in how political campaigns affect voting behavior. The impact of campaign spending in general, and of electoral advertising in particular, have received special attention.[1] Policy implications are almost self-evident. If money buys elections, campaign spending should be heavily regulated, assuming a policy goal of minimizing the influence of economic power on election results. If campaign spending is irrelevant, then policy discussions on public campaign finance are grounded on a false premise. The effect of TV advertising is equally important, for campaign managers and policy makers alike.

This paper estimates the impact of TV and radio advertising on the elections outcome. 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]

No consensus has emerged in the empirical literature. Despite the "minimal-effects" conventional wisdom that campaign spending and TV advertising have little impact on elections outcome, the paradox remains.[3] If money and TV are irrelevant, how can we rationalize the large sums spent on campaigns in general, and on TV advertising in particular?[4][5] As an illustration, consider the following passage from an article that describes Barack Obama's prospects on the 2007 U.S. presidential primary campaign:

[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, another name for 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).

[3] Gerber (1998) and Levitt (1994) are two examples of papers that find no effect of campaign spending on election outcomes. Bartels (1993), for example, calls the literature on media effects "one of the most notable embarrassments of modern social science." As quoted above, 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 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).

[4] 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.

[5] 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

> *"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 (see chart), 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 shows that money matters. Despite trailing Mrs. Clinton in polls, Mr. Obama "must be taken seriously" because of his fund-raising performance. However, it is not clear 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 just a signal of his political strength?[7]

Even if campaign spending and TV advertising influence election results, it is very hard to identify their impact. The quote hints at the reasons: omitted factors and reverse causality.[8] Reverse causality arises because stronger candidates should receive more contributions if donors expect their money to have an impact on policy making.[9] Heterogeneity in candidate quality, something difficult to control for, causes omitted variable bias, normally away from zero. 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.

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, with the direction of the bias undetermined.

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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.
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."

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

[7] Another good example is from *The Economist*, "Of Cash and Crushes," July 5th 2007. The story describes the odds of winning based on funds raised, not polls, and how these two could diverge as predictors of electoral performance. Similar examples abound.

[8] 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.

[9] If donations are purely ideological, or preference-based, then reverse causality does not arise.

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 and TV advertising are a choice of political actors, i.e. candidates and donors. The observed *quantums* 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 cross-sectional data are unpersuasive for several reasons. First, with pure cross-sectional data, it is hard to account to unobserved quality heterogeneity among candidates. Gerber (1998), for example, 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 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 legislative electoral races. Taking first-differences of repeated challengers eliminates most 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?

In fact, except for small random variations, we expect fund-raising to be roughly constant across races. Therefore, starting from an equilibrium situation, small variations in campaign spending would have small impact on electoral performance.[10] Not surprisingly, that is precisely Levitt [1994] finds.[11] A

[10] There are other theoretical reasons to expect a zero impact when looking at equilibrium situations. Suppose one candidate, wealthy and unknown, 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

[11] 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?

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.

Using a quasi-natural experiment produced by the Brazilian electoral legislation, we 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 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. Before the second round, however, the two runner-off candidates split the time equally. Hence, 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 two 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. Although state-level and national-level political strength are associated, the correlation is far from perfect. Consequently, one would not expect first-round TV shares to be similar to a decentralized equilibrium situation. Differences between first and second rounds TV time shares can be expected to have an impact, even if they are small. Finally, the differences are in fact large. In summary, our quasi-natural experiment consists in observing, over a short period of time, two different races in which candidates have different TV time shares, where allocation of TV time is largely outside the candidates' control.

Our setting is one feature away from a perfect experiment. 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 these issues in different ways. By selecting sub-sample in which the first round looks like the second, we can assess how sensitive results are the fact that the configurations are different across 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, the estimated impact of TV advertising time is much stronger then what received literature suggests. Using our preferred estimate, one percentage point change in the difference in TV advertising time shares causes a 0.241 percentage point change in the difference in vote share. For a sense

of practical relevance, between first and second rounds in the gubernatorial elections in our sample, difference in TV time shares change on average roughly 8 percentage points, which implies a change of 1.928 percentage point in the difference in voting. This means that changes in average TV share time alone would be enough to reverse the results 14% of the second rounds in the sample.[12]

In addition to evaluating how TV advertising time impacts elections outcome, we 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 authors' knowledge, this is novelty of this paper, made possible by the wide variation in city demographics for same election event.[13] The influence of TV advertising is stronger in larger, less educated, more urban, poorer and more unequal cities. After controlling for income and education, the impact of TV advertising is stronger where penetration of TV is deeper. All four "slope shifters" have the expected sign, and are significant both statistically and practically.

The game-theoretical literature on lobbying normally makes the assumption that candidates use lobby money to advertise to uninformed voters. Baron (1996) and Grossman and Helpman (1994) are two examples. The result that poorly educated low-income voters are more susceptible to TV advertising provides support for this untested assumption.

The paper is organized as follows. In section II, the Brazilian electoral system is described in detail, with emphasis on the legislation on TV time allocation. Section III describes the data and section IV outlines the estimation strategy. Results are in section V, and section VI concludes.

II – ELECTORAL ADVERTISING IN BRAZIL

In studies using U.S. data, the empirical evaluation of the impact of electoral advertising (the most important by-product of campaign expenditures) on elections' outcomes face two serious methodological problems. First, campaign expenditures and electoral advertising are related to candidates' characteristics such as ability, competence, or how the candidate's platform benefits special groups at the expense of voters. Not all, if any, of these characteristics are observable. Thus, omission of relevant covariates is a problem. Second, candidates that have a better chance of winning to begin with are more likely to receive campaign contributions. If this *ex-ante* electoral strength is unobservable, then a reverse causality bias arises.[14] In summary, campaign spending (and consequently TV advertising) is

[12] In five out of thirty-six second rounds in our sample the difference was less than 1.928.
[13] Although we consider gubernatorial elections, the electoral unit is city. See sections II and III.
[14] The task of controlling for strength is in fact more complicated than just described. Strength of candidacy unveils over the electoral process, so controlling for *ex-ante* strength might not be enough. One would need a measure of strength previous to every time a donation was made.

determined in decentralized market system, and observed quantities are equilibrium values produced by the strategic interaction of the choices of the agents.

In Brazil, in contrast, TV and radio advertising is determined in a centralized manner. The Brazilian electoral legislation produces an exogenous variation in TV advertising time, and allows us to circumvent the problems of omitted variable and reverse causality that plague most studies in the literature. Before continuing, we make a (very) short digression to describe the Brazilian political system.

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 of Brazil, while the legislative branch consists of two houses – the Senate and the Chamber of Deputies. In each state government, as well as in the government of the federal district, the head of the executive branch is a governor, and the legislative branch consists of the State Assembly. The president, the governors, the members of the Senate, the Chamber of Deputies, and the State Assemblies are elected by direct ballot every four years for four-year terms, except for the senators, who serve eight-year terms.[15] Brazil has a multi-party system. The current number of active political parties in the country is close to thirty. Of course, only some of such parties are relevant at the national level – as data will show in the next section.

Gubernatorial elections work in a runoff system.[16] All properly registered candidates participate on the first round. If no candidate reaches 50% plus one votes, there is a second round, in which the two most voted first-round candidates participate. Second-round winner is the one who receives most valid votes.[17] There is a three-week interval between the first and the second round. Table I shows the dates of first and second rounds of the three gubernatorial elections in our sample: 1998, 2002, and 2006.

[15] Each state has three senators, two of them coincide, and overlap with the other: at every four year cycle either one or two, but never all three, seats are disputed.

[16] 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.

[17] Valid votes are those for any candidate, and blank votes. The other category is null votes.

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.		

Federal law mandates that, over a period from 45 to 60 days preceding elections, part of the TV and radio daily grids are allocated free of charge to political advertising.[18] In case of runoff, political advertising is again mandatory, but for a period of roughly two weeks before elections.[19] 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 collation, the total number of representatives in the coalition is considered.[20]

In case of second round, 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.

III –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 one 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

[18] 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.

[19] From 1998 onwards, political advertising is transmitted starting 48 hours after the first-round results are announced officially by the local electoral authority (the TREs), and again end 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.

[20] There is considerable party switching in Brazil. Thus TV time allocation depends on which point of the legislature one considers. Until the 2004 elections, the relevant representation was at the beginning of the current legislature. For the 2006 elections, representation during the 2002 election was considered. Thus, actual representation at the time of elections is potentially different from the representation that determined the TV time allocation. As we will see below, this poses no challenge to our identification strategy.

elections, according to the law described above. [21] Air time is used as a proxy for advertising exposure.[22] City-level demographics are from the 2000 census.

Although we consider gubernatorial elections, the unit of observation is a city. There are two major advantages in using cities as a unit of observation. First, for the same election, we have several observations of voting behavior, which dramatically increases precision of estimation. Second, it provides much more 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, etc.

The sample consists of all gubernatorial races in which a second round was necessary, during three elections: 1998, 2002 and 2006. In total, there are 35 gubernatorial races in the sample, which took place in 18 different states and the Distrito Federal.[23] Table II contains information on the distribution of the sample, across states and municipalities.

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)

[21] For the elections prior to 2006, we only observe the composition at the end of the *elected* legislature. Since representatives can switch parties in the months between they are elected and when they are sworn in, which is the one relevant to compute TV time allocation. Hence, we observe TV time allocation with some noise. This should not be a reason for concern since party switching in this period, although it happens, is not such a relevant phenomenon.

[22] Clearly, being aired is a necessary, but not sufficient, 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 private to broadcasters and perhaps candidates.

[23] Distrito Federal, which includes Brasília (the federal capital) and several other cities (called satellite-cities), has the same legal status as a state. Results are unchanged if the Distrito Federal is excluded.

The sample includes cities from all 5 Brazilian regions, and includes very heterogeneous states, 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). In general, heterogeneity poses challenges for estimation as it makes omitted variable bias all more likely. In our case, the procedure accounts for all time-invariant unobserved heterogeneity. This is, incidentally, all the relevant unobserved heterogeneity because the time lapse between cross-section observations is never more than four weeks. Heterogeneity, in our case, is in fact desirable, because it improves external validity. Finally, heterogeneity across voting units allows us to estimate a different impact of advertising according to characteristics of the cities (see section V). Table IIII presents the distribution of Federal Chamber of Representatives by party, for the three relevant legislatures (1994, 1998 and 2002).

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

Party	Year		
	1994	1998	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)

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)[24], 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 7. Therefore, first-round advertising time is neither too concentrated nor too dispersed. This not-so-concentrated structure is ideal as an empirical setting. Were the Brazilian system similar to the American one, i.e., bipartisan with very marginal small parties, there would be very little variation in TV and radio time between rounds.[25] 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 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.[26]

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)

[24] PFL was renamed Democratas (DEM) in the first semester of 2007.
[25] In fact, there would be very little use to two rounds.
[26] Tables A.I, A.II and A.III in appendix A contain the distribution of seats among parties in the State Assemblies of state-election year pairs in our sample.

Data shows that TV advertising time – determined by national political strength – also reflect state-level political strength. Although there is some variation (correlation coefficients vary from 0.28 (Roraima in 1994) to 0.93 (Paraná in 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 election. Table VI presents the first-round TV and radio times of the first and second placed candidates in elections that required a second round.

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)

The average number of first round candidates, from 6.23 to 7.90, 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, which is important for identification because otherwise there would not be sufficient variation in TV and radio time between rounds.

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)

Summary statistics on TV time and vote share already contain the story of the paper. In the first-round, first placed candidates have, on average, more TV time share than second placed candidates. 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 relevant heterogeneity and non-linearities that may spuriously produce the results. Estimation strategy outlined in section IV solves these (potential) problems.

IV – EMPIRICAL STRATEGY

Similarly to Levitt (1994), we follow the strategy of first-differencing 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 candidates race again over a three-week period, odds are that relevant characteristics are constant. Second, but equally important, 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 solved.

The details of the empirical implementation are as follows. The data has a panel structure. The cross-sectional unit is a pair election *e*, defined by the pair year (*t*) – city (*i*). The time-series unit is a round $r \in \{1,2\}$. 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.[27]

A and *B* refer to the statewide 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 election *e*. For example, $votes\_A_{e2}$ is the share of votes of the first-round winner in the second round of election *e*. $votes\_B_{er}$ is defined analogously. Define also $TVtime\_A_{er}$ and $TVtime\_B_{er}$ as the share of advertising time in round *r* of election *e* allocated to the first round winner and runner-up, respectively.

Finally, dif_votes_{er} e dif_TVtime_{er} are:

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

and

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

For comparability, first round votes are normalized to sum 1. While by definition $\sum_{e \in State} dif\_votes_{e1} \geq 0$, it may be that the runner-up wins in some cities ($dif\_votes_{e1} < 0$). $\sum_{e \in State} dif\_votes_{e2}$ can be negative, if the runner-up comes back in the second round. By construction $TVtime\_A_{e2} = TVtime\_B_{e2} = 0.5$. Thus $dif\_TVtempo_{e2} = 0$ for all *e*.

The goal is to investigate how the distribution of time affects the election outcomes. We estimate a linear relationship among the variables. The specification is

$$dif\_votes_{er} = \alpha + \gamma \cdot dif\_TVtime_{er} + \omega \cdot round_r + \lambda \cdot X_e + \varepsilon_{er} \qquad (1)$$

In equation (1), $round_r$ is a dummy that assumes the value 1 for second rounds, and X_e is a vector of characteristics of election *e*. They control for a wide range of the pair city-election year

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

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. The coefficient ω captures changes between rounds in average voting behavior. A positive ω means a "widening gap" effect: the first-round winner increases her advantage in the second round.

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 X_e remains constant between rounds, which is reasonable. Incidentally, first-differencing also significantly mitigates reverse causality problem.[28] First-round coalitions are formed taking into account two factors, among other things: their impact on TV time shares, and the chances of winning.[29] Thus, first-round variation in TV time is contaminated with the anticipation of electoral performance. 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:

$$\Delta(dif\_votes_e) = \gamma \cdot \Delta(dif\_TVtime_e) + \omega \cdot \Delta(turno_r) + \Delta(\varepsilon_{er}) \qquad (2)$$

where Δ is the difference between the second and first rounds.[30]

The major challenge in interpreting the parameter γ as causal comes from the shortages that our quasi-experimental data have relative to the perfect experiment.

In completely controlled experiment, the same pair of candidates would 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 quasi-natural 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, which in our case is not a serious threat to identification: since TV time is allocated in a centralized manner, and the time span between voting observation is short, scandals do not

[28] If national and state-level political strength were unrelated, reverse causality would not be an issue. Unfortunately, table IV suggest they are strongly correlated.

[29] Normally, members of the coalition share the spoils of victory in the form of positions in the elected administration.

[30] $\Delta(turno_r)$ is a vector of ones, and ω is the intercept of the linear relationship (2).

determine TV time allocation.[31] In this case, scandals - which are part of the error term $\Delta(\varepsilon_{er})$ - can safely be assumed not to correlate with $\Delta(dif\_TVtime_e)$.

A more serious problem arises from second-round political alliances, which are unobservable.[32] It is not uncommon that defeated first-round candidates will support one of the two first round winners. This unobservable change in political strength *between* rounds is dangerous to our purposes if it correlates with how TV time share changes between rounds, i.e., second-round political support - which are part of the error term $\Delta(\varepsilon_{er})$ - may be correlated with $\Delta(dif\_TVtime_e)$.[33] Although table VI suggest that first and second rounds are similar, we cannot dismiss the possibility that omission of second round political support will bias our results.[34]

We follow two different strategies to deal with the question of second round political support. The first strategy involves selecting sub-samples of elections that are closer to the perfect experiment, i.e., elections in which the first and the second round are very similar. This is done in two different ways. First, we define whether the third placed candidate in the first round is pivotal, in the sense that her support would change the second round outcome if all her votes migrated to the runner-up. More precisely, define:

$$C\_pivotal_{e1} = \begin{cases} 1, \text{if } votos\_C_i - dif\_votos_{i,1} > 0 \\ 0, \text{otherwise} \end{cases}$$

The model is estimated only with the sub-sample of elections in which $C\_pivotal_{e1} = 0$. 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.

Finally, we change the dependent variable to account for all changes that occurred between the end of the first round and the beginning of the second-round campaigning time, including all announced political support, scandals, etc. The transformed dependent variable is:

[31] 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.
[32] In the first-round, coalitions tend are registered at the electoral authority (the TSE) precisely because they determine TV time.
[33] Assume defeated candidates tend to support runner-ups. As, 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. The reverse is also true, evidently, and we cannot tell the sign of the bias.
[34] Together the runner-up and the winner had 80%, 74% and 81% of the first-round votes. In this sense, the first and the second rounds look "alike."

$$\Delta(dif\_TVtime_e)^* = (votes\_A_{e2} - Poll\_A_2) - (votes\_B_{e2} - Poll\_B_2) \quad (3)$$

where $Poll\_A_2$ and $Poll\_B_2$ are the results of the first opinion pool of the second round.

The first second-round pool is typically conducted some ten days after the first round, which normally gives sufficient time for the political supports to be announced, and for most of the pre-second round TV campaign relocation of voters to take place.[35] 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.*

V – RESULTS

V.I *Main Results*

Table VII contains the estimation of several versions of model (2) (including version in which the dependent variable is as in (3)).

[35] See table A.IV for the field dates of the first second-round pool, when available.

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

	First-Difference (2nd minus 1st round)						
	(1)	(2)†	(3)††	(4)‡	(5)£	(6)	(7)‡
Δ(*Time share A - Time share B*)	**0.272**	**0.311**	**0.301**	**0.247**	**0.273**	**-0.149**	**0.038**
	(0.077)* **	**(0.070)*** **	**(0.057)*** **	**(0.104)** **	**(0.080)*** **	**(0.186)**	**(0.349)**
Δ(*Time share A - Time share B*)^2						**0.386**	**0.192**
						(0.160) **	**(0.299)**
Constant	-0.002	0.003	0.051	-0.089	-0.004	-0.091	-0.203
	(0.029)	(0.025)	(0.019)**	(0.031)***	(0.031)	(0.046)*	(0.086)**
Number of Observations	7925	4537	1982	7379	7379	7925	7379
F- statistic	12.52	20.01	27.68	5.64	11.54	12.12	3.25
R^2	0.129	0.184	0.184	0.120	0.138	0.165	0.129

§ All standard errors (in parentheses) are robust to clustering within the state-election year pair. 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
£: Same model as in column (1), same sample as in column (5)
*** = significant at the 1% level
** = significant at the 10% level
* = significant at the 10% level
Source: Tribunal Superior Eleitoral (TSE) and IBOPE.

Column (1) the first 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 roughly 7.7 percentage points, which implies an impact of $7.7 \times 0.272 \cong 2.08$ 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.[36] Therefore, TV is responsible for almost 70% of the closing gap.

[36] 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).

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 ($C\_pivotal_{e1} = 0$). 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. Again, results are, if anything, stronger.

In columns (4) are estimates when the dependent variable is $\Delta(dif\_TVtime_e)^*$. As expected, when using only the difference between second round votes and the first second-round opinion poll, the estimates are lower than those in columns (1)-(3). They are, however, similar (roughly 0.25 as opposed to roughly 0.29), and still precisely estimated. Since polls are not available for all states at all election races, the sample is somewhat different. Column (5) shows that sample selection is not driving results.

From table VI, 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 Thus, it is important to evaluate the relevance of the non-linearities of the impact of TV. Columns (6)-(7) present the estimates of quadratic specifications for both the original and the modified dependent variables. Coefficients suggest that the relationship between TV time and votes is, if anything, weakly convex. Nevertheless, the linear and the quadratic model are very similar in many respects. The quadratic model does not fit the data much better than the linear model, and *F*-statistics are in fact lower. For the model with the original dependent variable, the point estimate of the impact of TV advertising is positive for 80% of the sample, and one can reject the null hypothesis at the 5% for roughly 50% of the sample. For the model with the modified dependent variable, the estimated impact is always positive, and statistical rejection occurs again in 50% of the times.

Scatterplots and the fitted curves in both cases are depicted in figures I and II. They make it clear that the linear and the quadratic relationships are quite similar.

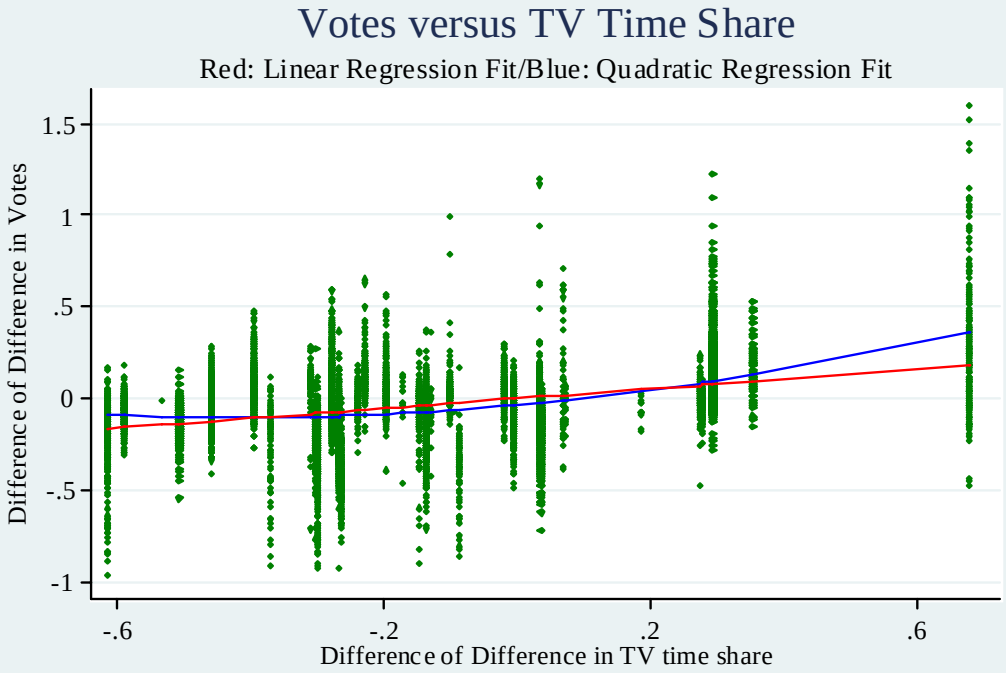

Figure I: Original Dependent Variable

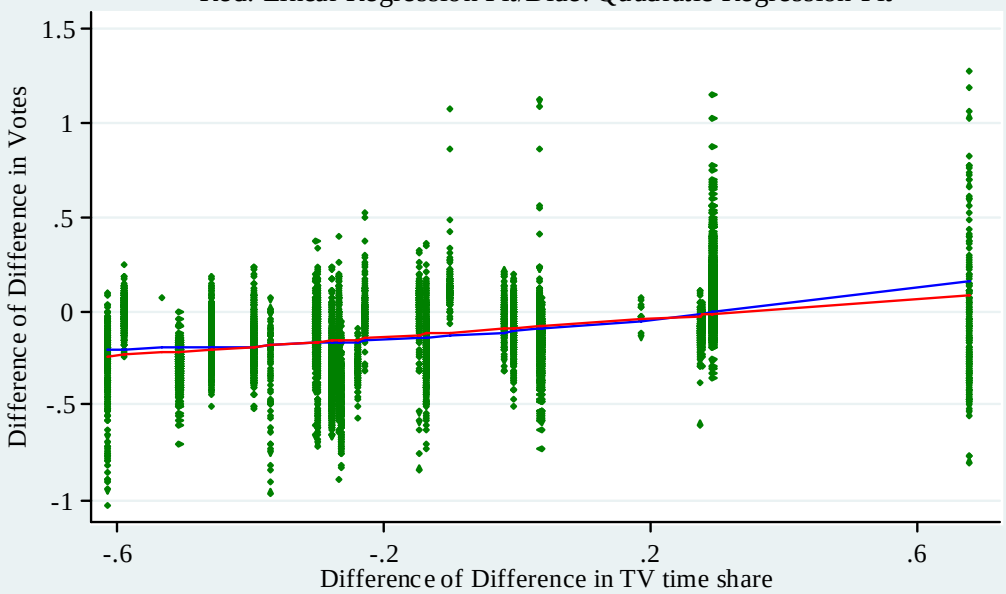

Figure II: Modified Dependent Variable

Finally, for comparison we estimate a model that uses only first-round data, and relate voting to TV time directly by Ordinary Least Squares (OLS). For maximal comparability, city and election year dummies are included.[37] Table VIII contains the results.

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

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

§ All standard errors (in parentheses) are robust to clustering within the state-election year pair.
† 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

The impact of changing TV time share difference by one percentage point is 0.451 point. This point estimate is roughly 67% higher than the average estimated effect reported in table VII. The reasons why endogenous suggest that simple OLS procedure will have a bias away from zero: (intrinsically) stronger, more able candidates receive more TV time *and* more votes, implying that the straight OLS bias is *away from zero*. When political strength, quality, ability, etc are accounted for, the estimated effect of TV advertising time should drop, as it does. As expected, the effect is now is even stronger.

In column (2), we include the last opinion poll before the first-round TV campaign started. This variable should help controlling for before election political strength (it is part of the

[37] 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.

set of controls X_e). Two things are worth noticing. First, *ex-ante* political strength has the expected sign (positive), and it is statistically significant, although only marginally (p – 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.[38] 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.

Finally, notice that, while the estimate in column (3) is lower than those in columns (1), (2) and (4) of table VIII, it is still *higher* than all estimates in table VII. This suggests that, although including *ex-ante* vote intention with first-round variation mitigates the problem of omission, some omission is likely to still be there. These results increase our confidence that using only *between* rounds variation is the proper way to control for omission and reverse causality.

V.II *Demographic Determinants of TV advertising effect*

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

$$\begin{aligned} dif\_votes_{er} &= \alpha + \eta Shifter_e + \gamma \cdot Shifter_e \times dif\_TVtime_{er} \\ &+ dif\_TVtime_{er} + \omega \cdot round_r + \lambda \cdot X_e + \varepsilon_{er} \end{aligned} \tag{4}$$

When first-differences are taken (or election specific dummies are included), the shifter disappears. The estimated model is:

$$\begin{aligned} \Delta dif\_votes_{er} &= \gamma \cdot Shifter_e \times \Delta dif\_TVtime_{er} \\ &+ \Delta dif\_TVtime_{er} + \omega \cdot \Delta round_r + \Delta \varepsilon_{er} \end{aligned} \tag{5}$$

Five *SHIFTERS* are considered: income per capita, income distribution, a measure of educational attainment, level of urbanization, and size measured by population. 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.

[38] Polls were not available for all races in our original sample.

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 Estatistica (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
††† *TV* is the percentage of households with at least one television set
†††† *Radio* is the percentage of households with at least one radio
††††† Electricity is the percentage of households with at least access to electrical power
£: 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 Geografia e Estatistica (IBGE)

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 significant statistically, 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, which seems irrelevant, 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.

VI –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 sharp contrast with the perception of politician and political analysts, as the quote on Barack Obama's prospects show. Using a quasi-experiment data, this paper finds evidence contrary to the academic conventional wisdom: TV and radio exposure in gubernatorial elections in Brazil have a significant effect on elections outcome, both practically and statistically. The magnitude - half the average closing gap from first to second round in gubernatorial elections - suggests that, while TV advertising cannot always predict elections outcome, it is a major determining factor.

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 poorly educated where TV penetration is very high and income is very poorly distributed. Thus, it scores high in three factors that magnify the impact of political advertising.

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APPENDIX A

Table A.I - Number of elected members of the State Assemblies in the 1994 elections, by party and by state

Party	State											
	Amapá	Distrito Federal	Goiás	Mato Grosso do Sul	Minas Gerais	Pará	Rio de Janeiro	Rio Grande do Sul	Rondônia	Roraima	São Paulo	Sergipe
PPR	0	0	3	0	0	6	4	11	0	3	7	3
PDT	1	1	0	2	5	1	9	7	3	0	2	1
PT	0	5	2	2	5	3	4	4	1	0	13	2
PTB	2	1	1	3	6	2	2	8	1	5	6	1
PMDB	2	1	8	4	9	9	5	9	3	0	17	3
PSC	0	0	0	0	0	0	2	0	2	1	0	0
PL	2	1	3	1	4	2	3	0	1	0	4	0
PPS	0	1	0	0	1	0	0	0	0	0	0	0
PFL	4	0	3	3	6	3	1	0	1	1	5	5
PMN	0	0	0	0	1	0	2	0	3	0	0	1
PRN	0	1	0	0	0	0	0	0	1	0	0	0
PP	0	6	2	3	9	2	3	0	0	1	0	1
PSB	1	0	0	0	1	0	3	3	0	0	1	0
PSD	1	0	3	0	1	0	1	0	0	1	2	0
PV	0	0	0	0	1	0	0	0	0	0	1	0
PRP	0	0	0	0	0	0	0	0	0	0	1	0
PSDB	0	2	3	1	6	2	10	1	1	1	12	1
PC do B	0	0	1	0	0	0	1	1	0	0	1	0
PPB	0	0	0	0	0	0	0	0	0	0	0	0
PRONA	0	0	0	0	0	0	1	0	0	0	1	0
PSL	0	0	0	0	0	0	0	0	0	0	0	0
PST	0	0	0	0	0	0	0	0	0	0	0	0
PSDC	0	0	0	0	0	0	0	0	0	0	0	0
PGT	0	0	0	0	0	0	0	0	0	0	0	0
PT do B	0	0	0	0	0	0	0	0	0	0	0	0
Total	13	19	29	19	55	30	51	44	17	13	73	18

Source: Tribunal Superior Eleitoral (TSE)

Table A.II - Number of elected members of the State Assemblies in the 1998 elections, by party and by state

Party	State													
	Amapá	Ceará	Distrito Federal	Mato Grosso do Sul	Pará	Paraíba	Paraná	Rio Grande do Norte	Rio Grande do Sul	Rondônia	Roraima	Santa Catarina	São Paulo	Sergipe
PPR	0	0	0	0	0	0	0	0	0	0	0	0	0	0
PDT	3	1	1	2	3	2	3	1	6	3	4	1	6	1
PT	1	3	4	1	3	3	4	1	11	2	0	5	13	0
PTB	1	2	2	3	3	0	9	1	9	2	2	1	4	1
PMDB	3	5	4	4	7	16	7	8	9	4	2	9	8	5
PSC	0	1	1	0	0	0	1	0	0	2	0	0	0	0
PL	2	1	1	1	3	0	0	2	0	1	0	0	5	0
PPS	0	4	1	2	1	0	0	0	0	0	0	0	3	1
PFL	2	2	1	2	3	4	12	4	2	2	3	8	11	3
PMN	0	0	0	0	0	0	0	0	0	0	0	0	0	1
PRN	0	0	0	0	0	0	0	0	0	0	0	0	0	0
PP	0	0	0	0	0	0	0	0	0	0	0	0	0	0
PSB	3	1	1	0	1	0	2	1	1	0	0	0	2	2
PSD	2	0	1	0	2	0	0	0	0	0	0	0	0	0
PV	0	0	0	0	0	1	0	0	0	0	0	0	1	0
PRP	0	0	0	0	0	0	0	0	0	0	0	0	1	0
PSDB	1	18	1	6	7	5	5	1	2	3	1	3	20	3
PC do B	0	1	0	0	0	0	0	0	0	0	0	0	2	0
PPB	0	2	1	0	4	1	7	2	11	2	4	9	9	2
PRONA	0	0	0	0	0	0	0	0	0	0	0	0	2	0
PSL	1	0	0	0	0	1	0	0	0	0	3	0	0	0
PST	0	0	0	0	0	0	0	0	0	0	0	0	0	0
PSDC	0	0	0	0	0	0	0	0	0	0	0	0	0	0
PGT	0	0	0	0	0	0	0	0	0	0	0	0	0	0
PT do B	0	0	0	0	0	0	0	0	0	0	0	0	0	0
Total	19	41	19	21	37	33	50	21	51	21	19	36	87	19

Source: Tribunal Superior Eleitoral (TSE)

Table A.III - Number of elected members of the State Assemblies in the 2002 elections, by party and by state

Party	State									
	Goiás	Maranhão	Pará	Paraíba	Paraná	Pernambuco	Rio de Janeiro	Rio Grande do Norte	Rio Grande do Sul	Santa Catarina
PPR	0	0	0	0	0	0	0	0	0	0
PDT	0	4	2	1	5	3	3	1	6	0
PT	3	2	5	4	8	4	8	2	12	8
PTB	0	2	4	2	3	1	1	1	6	2
PMDB	8	2	7	8	7	6	11	3	8	7
PSC	0	1	0	0	1	2	2	0	0	0
PL	1	1	4	1	2	2	3	1	0	1
PPS	1	1	1	1	2	1	1	0	3	0
PFL	3	13	0	3	7	7	4	4	1	7
PMN	0	0	0	0	0	0	0	0	0	0
PRN	0	0	0	0	0	0	0	0	0	0
PP	0	0	0	0	0	0	0	0	0	0
PSB	1	1	1	2	2	4	11	2	2	0
PSD	1	6	2	0	0	2	0	0	0	0
PV	0	0	0	0	0	1	3	0	0	0
PRP	1	0	0	0	1	0	0	0	0	0
PSDB	10	3	6	10	4	4	4	0	3	2
PC do B	1	0	1	0	0	1	1	0	1	0
PPB	4	1	2	2	4	3	5	7	8	8
PRONA	0	0	0	0	0	0	2	0	0	0
PSL	0	0	0	0	2	1	2	0	0	0
PST	1	0	2	0	0	0	0	0	0	0
PSDC	1	0	0	0	0	1	0	0	0	0
PGT	0	1	0	0	0	0	0	0	0	0
PT do B	0	0	0	0	0	0	2	0	0	0
Total	36	38	37	34	48	43	63	21	50	35

Source: Tribunal Superior Eleitoral (TSE)