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

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Abstract

Whether campaign advertising influences election outcomes is an open question; a paradox given the amount spent on campaigning in general and TV advertising in particular. We argue that such "absence of documentation" is due to the focus of the empirical literature on the United States, in which the allocation of campaign spending and advertising is decentralized. We explore a quasi-natural experiment that enables us to mitigate the omitted variables and reverse causality problems caused by decentralized allocation. In Brazil, gubernatorial elections work in a two-round system. In the first round, candidates' TV time shares are determined by their coalitions' share of seats in the National Parliament. In the second round, TV time is split equally between the first-round winner and runner-up. Using differences between rounds as a source of variation, we find a large causal effect of TV advertising on election outcomes.

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

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1 INTRODUCTION

Scholars, advertisers and policy makers have long held an interest in how political campaigns affect voting behavior. However, measuring campaign effects has been an elusive task. Goldstein and Ridout (2004) summarize the literature and conclude that "...determining whether political campaigns influence individual vote choice and election outcomes has become a Holy Grail". The existing empirical research that uses field data suffers from identification issues, and often finds an intriguing "minimal effects" result. Examples of studies that recover a small impact of money and advertising abound: on campaign spending, see Levitt (1994) and Gerber (1998);[2] on advertising, see Finkel (1993), Gelman and King (1993) and Shaw (1999). In contrast, theoretical work on elections and lobbying takes it for granted that money and advertising matter (see, e.g., Snyder, 1989; Baron, 1994; Grossman and Helpman, 1996). If money and advertising are irrelevant, how can we rationalize the large sums spent on political campaigns in general, and on TV advertising in particular?

Difficulties in the existing empirical literature are due to limitations in dealing with identification issues caused by reverse causality and omitted variables. Most research consists of regressing election outcomes on a measure of either campaign spending or voters' exposure to advertising. Reverse causality occurs because donors expect their money to buy political influence, and thus contribute more generously to candidates with a high probability of winning. Unobserved heterogeneity in candidates' quality causes omitted variable bias. Another source of trouble is unobserved electoral district preferences. For example, a right-wing candidate running in a leftist district has difficulties in both raising money and getting votes. Most theoretical mechanisms indicate that correlation-based methods, such as Ordinary Least Squares (OLS), produce upwardly biased estimates (an exception occurs when a candidate is strong enough to make money unnecessary). Econometric difficulties are particularly acute with U.S. data because funding, spending and campaign advertising are all choices of the candidates and donors.

The literature recognizes the difficulties, but is only partially successful in solving them. Many studies adopt the strategy of using proxies to mitigate the omitted variables problems (Green

[2] The empirical literature focuses on US races. Because the most important item on campaign spending in the U.S. is TV advertising, the distinction between spending and TV advertising is immaterial (Ansolabehere and Iyengar, 1996). In fact, political scientists normally use spending as a proxy for advertising (Goldstein and Ridout, 2004).

and Krasno, 1988). However, it is unlikely that proxies will fully capture complex concepts such as the overall quality of a candidate or the preferences of an electorate. Some authors instrument for campaign spending or electoral advertising (Welch, 1981; Gerber, 1998; Rekkas, 2007). However, finding convincing instruments is difficult. Consider Gerber (1998) and Rekkas (2007). Both use lagged campaign spending at the district level as an instrument for current candidates' spending. The idea is that candidates tend to spend more in districts where previous election campaigns were costly. Yet, insofar as candidates run for the same position in multiple years, lagged campaign spending is correlated with current election outcomes, invalidating its use as an instrument.

Part of the literature resorts to experiments. Ansolabehere and Iyengar (1996), Valentino, Hutchings and Williams (2004) and Brader (2005) all provide laboratory evidence that TV advertising influences voters' behavior. Our results are in line with laboratory evidence in both direction and magnitude, although it is hard to compare magnitudes of lab experiments with real votes. Laboratory studies offer unparalleled internal validity, but have weak external validity. For this reason, we view our results as complements to the laboratory evidence.

From a methodological perspective, our work is closely related to papers that exploit natural experiments. Gerber et al. (2007) run a field experiment during the 2006 Texas gubernatorial race. They randomize the date and volume of TV and radio advertisements for one of the candidates, and evaluate how they affect voters' perception. They find a positive but transient advertising effect. In terms of magnitude, their estimates are smaller than ours. Some concerns remain, however. Randomization is a powerful tool, but Gerber et al. only randomize the advertisements for one of the candidates. More importantly, the experiment took place in January 2006, several months before the election. Voters' attention at this stage is not comparable to right before the election.[3] Given these differences, it is not surprising that Gerber et al. find a much smaller impact of advertising on voters' intention than we do.

Huber and Arceneaux (2007) use data on the 2000 U.S. Presidential campaign, and explore variation in advertisement within non battleground states to evaluate its impact over voters' perception and voting outcomes. They compare media markets bordering a contested state (high advertising) with "isolated" markets (low advertising). However, residents of areas close to a

[3] Indeed, about half of the respondents of the survey used by Gerber et al. declared to be unfamiliar with one of the candidates, and close to one third of the respondents declined to express a vote preference. In addition, the content of the ads used in the experiment was not typical of an election. For example, as Gerber et al. describe, the ads neither mention the candidate's opponents nor make any request for vote support.

contested state share many characteristics with their fellows across the border. Thus, the same elements that determine the TV advertising strategy in a competitive state also determine voting across the border, which may invalidate the identification strategy.

Levitt (1994) also uses field data to identify the effect of campaign spending on election results. He compares the same pairs of contestants in multiple U.S. congressional races. Using only differences in voting and spending between races, Levitt eliminates all unobserved heterogeneity that is constant over multi-year periods – thus mitigating problems associated with omitted variables and reverse causality. However, an important question arises: after first-differencing the data, what source of variation is left to estimate the impact of spending on election outcomes? If the identification strategy is successful, then candidates' fundraising should be roughly constant over the time – except for small random variations. Starting from an equilibrium situation, small variations in campaign spending would have a small impact on electoral performance. Not surprisingly, this is precisely what Levitt finds. A related point is made by Prat (2002, 2006), who presents a model in which campaign spending serves as a signal of candidates' quality. However, in order to obtain funds from contributors, candidates have to distort their platforms away from the median voter's ideal point. In equilibrium, the benefit of spending in terms of votes is offset by the policy bias needed to raise contributions. Thus, the net observed effect of campaign spending over election outcomes should be low.

We explore a quasi-natural experiment provided by the Brazilian electoral legislation. Gubernatorial elections work in a two-round system. A runoff happens if no candidate reaches 50% of the votes in the first round. In this case, the two top voted candidates advance to the second round. For a period of time before each round, TV and radio networks must air candidates' advertising free of charge, and no additional paid TV or radio advertising is allowed. First-round TV time is distributed among candidates according to their coalitions' representation at the National Parliament. In the second round, candidates split TV time equally. We explore the difference in TV time between rounds to estimate the impact of advertising on election outcomes. We consider only the pairs of candidates that make it to the second round, and take first-differences to control for time-invariant unobserved candidate and district heterogeneity.

Our strategy resembles Levitt's (1994) in the sense that we compare the same pair of contestants in different races – two rounds of the same election in our case. There are two major differences, however. Levitt's races are at least two years apart. In contrast, the runoff is no more

than 28 days after the first round, increasing the confidence that most candidates' and districts' characteristics are constant over races. More importantly, TV time is determined by law, and the rule produces large changes in the distribution of TV time between rounds. In summary, we observe the same pair of candidates with different TV time shares in two races close to each other in time. In addition, the variation in TV time between rounds is largely outside the candidates' control. Unlike Gerber et al. (2007), we have TV advertising for both candidates during the entire campaign. We find a large impact of TV advertising on election outcomes. Using our preferred estimate, a one percentage point (p.p.) increase in a candidate's TV time share causes a 0.272 p.p. increase in her vote share. Using only first-round data, which is admittedly endogenous, we recover a stronger impact of advertising on election outcomes. This is precisely what theory predicts: OLS overestimates the impact of advertising.

We then investigate the determinants of advertising effects. The literature is almost silent about the circumstances under which media has an impact on electoral outcomes.[4] We can address this issue because of the large variation in demographics across cities in our sample. We find stronger media effects in poorly educated cities where TV penetration is high. We also find somewhat weaker evidence that advertising has a stronger impact in poorer and more unequal cities. These results are important for two reasons. The theoretical literature on lobbying normally assumes that candidates use contributions to advertise to uninformed voters (Baron, 1994; Grossman and Helpman, 1996). Our results provide support for this assumption. In addition, the fact that media effects vary with education and TV penetration sheds light on the underlying mechanism, and provides additional support for causal interpretation (Deaton, 2009).

Our experiment is not flawless. Other unobservable factors may change between rounds. While the first round is a multi-party race, only two candidates run in the second round. This structural difference raises a number of concerns regarding how strategic actors change their behavior between rounds. For example, defeated first-round candidates may forge informal alliances with runoff contestants, and candidates may change their fundraising or spending strategies between rounds. If such changes are systematically related with the rearrangement of TV advertising time between rounds, they may cause bias in our estimates.

We discuss several non-TV factors that threaten the identification of advertising effects. An extensive robustness analysis suggests that non-TV effects are not driving our findings. Naturally,

[4] Huber and Arceneaux (2007) is an exception.

we are unable to exhaust the list of potential threats, but we cannot think of any reasonable alternative story that rationalizes the results. Serious identification problems have been plaguing the empirical literature on campaign effects for a long time, and our experiment is an improvement within the field data literature.

The paper is organized as follows. Section 2 presents facts about political institutions in Brazil, with emphasis on gubernatorial elections and campaign advertising. In Section 3, we describe the data. Section 4 outlines the empirical strategy. Results are in Section 5, which also contains an extensive robustness analysis, as well as direct evidence on non-TV factors such as fundraising, spending and negative advertising. Section 6 discusses external validity, and includes results from mayoral races. Section 7 concludes.

2 ELECTORAL ADVERTISING IN BRAZIL

Brazil is a federal republic with 26 states and a federal district (the capital, *Brasília*). Both in the states and in *Brasília*, the executive branch is headed by a governor, who is elected by direct ballot in a two-round majority system every four years. Candidates must be supported by a party or a coalition of parties. The number of political parties is close to 30, but only a few are relevant at the national level. Voting is mandatory for all citizens between 18 and 64 years, and voluntary for citizens aged 16 and 17, and older than 65.[5] Consequently, turnouts are uniformly high across elections and across rounds. Averaging over the three election years in our sample (1998, 2002 and 2006), first and second round turnouts were 81.35% and 80.69%, respectively.

All candidates participate in the first round. If none reaches 50% of the votes, a runoff between the two first-placed candidates takes place some 28 days after the first round, and the candidate with the most votes wins. Table 1 shows the dates of first and second rounds of the three gubernatorial election years in our sample.

[TABLE 1 HERE]

Federal law mandates that part of the TV and radio daily grids must be allocated free of charge to campaign advertising for a period of 45 days preceding the first round of the elections. It is forbidden to buy additional airtime. Campaign advertising for gubernatorial races is aired on

[5] Failing to vote carries significant penalties. Besides fines, sanctions include ineligibility for public employment, for passport issuance, and for participation in government transfer programs.

Mondays, Wednesdays and Fridays, in two blocks of 25 minutes each, one at lunchtime and another at the evening prime time. Additionally, short ads of up to one minute are distributed along the grid, adding up to 30 minutes daily. In case of a runoff, advertising (again mandatory) starts 48 hours after the first-round results are officially announced, and ends on the Friday before the elections.[6] Second-round advertising is aired daily, in two blocks of 20 minutes each, at lunchtime and at the evening prime time. In the first round, gubernatorial elections compete for airtime with both presidential and legislative races. In the second round, it shares time only with the presidential race.[7] Gubernatorial races have 1,145 minutes of first-round airtime. The second-round airtime varies. The average in our sample is 920 minutes.

First-round TV time is allocated according to the following rule: 1) one third is equally divided among candidates; 2) the remaining is distributed proportionally to the share of seats that coalitions have in the lower house of the National Parliament at the beginning of the legislature.[8] If a runoff is necessary, advertising time is split equally between the two remaining contenders.[9]

3 DATA AND DESCRIPTIVE STATISTICS

We draw on three sources of data. Election data are from the federal electoral authority, the *Tribunal Superior Eleitoral* (TSE). We have: 1) the voting records of candidates for three gubernatorial elections (1998, 2002 and 2006) and three mayoral elections (1996, 2000 and 2004); 2) party coalitions supporting candidates; 3) the distribution of seats by coalition in the National Parliament at the beginning of the legislature; 4) a measure of negative advertising; and 5) funding and expenditure data by date for the 2008 mayoral elections. Items 2) and 3) allow us to compute the allocation of TV time according to the rule described above. Demographics are from the 2000 census. Finally, poll data are from IBOPE, the main polling institute in Brazil.

[6] Brazil has electronic ballot, and vote counting is very fast. Official results are normally announced within 24 hours after the polls are closed.
[7] Presidential races follow the same rules as the gubernatorial ones. A runoff was necessary in 2002 and 2006.
[8] Party switching occurs in Brazil. Allocation of campaign advertising time is determined by coalitions' share at the first day of the current legislature. We do not have this data. Instead, we observe the voting outcomes for the previous legislative elections, and use this to approximate the composition of the National Parliament at the beginning of the legislature. Elections and swear-in ceremonies are three months apart. Thus, our measure of advertising time has some noise but the amount of party switching is limited within this three-month period.
[9] The allocation of radio advertising time follows the same rules. Thus, the impacts of TV and radio are indistinguishable.

The main sample is composed of all gubernatorial races between 1998 and 2006 in which a runoff was necessary.[10] In total, we have 34 races in 18 different states. Table 2 has information about the sample, which covers all five regions in Brazil. States range from European-like Southern states such as *Rio Grande do Sul* to underdeveloped Northeastern states like *Ceará*. The three most populous states are included (*São Paulo*, *Minas Gerais* and *Rio de Janeiro*).

[TABLE 2 HERE]

Table 3 shows the distribution of the National Parliament by party for the 1994, 1998 and 2002 legislatures, which determine the allocation of TV time in the gubernatorial elections of 1998, 2002 and 2006, respectively. Some facts about the Brazilian political system emerge. More than 20 parties have representatives in the National Congress. Four are dominant: center-right *Partido da Frente Liberal* (PFL), centrist *Partido do Movimento Democrático Brasileiro* (PMDB), center-left *Partido da Social Democracia Brasileira* (PSDB), and leftist *Partido dos Trabalhadores* (PT). The effective number of parties is roughly seven.[11]

[TABLE 3 HERE]

Table 4 presents correlations between the distribution of seats by party at the National Congress and at the state legislatures. Out of 34 correlation coefficients, 23 are over 0.70. Thus, TV time allocation in gubernatorial races correlates with political strength at the state level, preventing the identification of the impact of TV using first-round data only.

[TABLE 4 HERE]

The average number of first-round candidates ranges from 6.23 in 1998 to 7.90 in 2006 (Table 5). The average number of effective candidates is 3.7. Thus, the first and second rounds are structurally different races, although not as much as the raw number of candidates suggests.

[TABLE 5 HERE]

Table 6 contains mean vote shares and advertising times of the first-round winner and the runner-up. Not surprisingly, winners have more advertising time on average. Inspection of columns (2) and (3) shows a closing gap: the mean difference in vote shares between the winner and runner-up shrinks between rounds. The difference in TV time also shrinks (it is zero in the runoff), suggesting a positive relation between the two variables.

[TABLE 6 HERE]

[10] Except for Brasilia, the federal district, which is in practice a city, and it is included in the mayoral races we present later in the paper.

[11] The effective number of parties is the inverse of the Herfindahl-Hirschman Index of vote concentration.

4 EMPIRICAL STRATEGY

The data has a panel structure. The cross-sectional unit is an election *e*, defined by the pair year (*t*) – city (*i*). The time-series unit is a round *r*. We observe cross-sectional units twice, once for each round, e.g., the first round in city *i* of the 1998 gubernatorial election is one observation ($r = 1$). The second round is another ($r = 2$).[12]

Our races are for governor, but the unit of analysis is a city. Using cities increases the number of observations and leverages the precision of our estimates, even after clustering observations at the state level. More importantly, it allows us to investigate how the impact of TV varies with demographics. We treat cities of different sizes equally because we are not interested in parameters representative of Brazilian states. Furthermore, when we decompose the impact of TV according to demographics it is more natural to treat observations equally. For robustness, we also implement a procedure that weighs cities according to their population as a percentage of the state population. This produces results that are representative at the state level.

Let *A* and *B* be the first-round winner and runner-up, respectively. Let $votes\_A_{er}$ and $votes\_B_{er}$ be the share of votes in the round *r* of election *e* of the first-round winner and runner-up, respectively. For example, $votes\_A_{e2}$ is the second-round vote share of the first-round winner of election *e*. Analogously, $TVtime\_A_{er}$ and $TVtime\_B_{er}$ are the shares of advertising time in round *r* of election *e* of the first-round winner and runner-up, respectively. We define:

$$dif\_votes_{er} = votes\_A_{er} - votes\_B_{er} \text{ and } dif\_TVtime_{er} = TVtime\_A_{er} - TVtime\_B_{er}$$

We normalize first-round votes to sum 1, making them comparable to second-round figures (by construction, runoff shares sum to one). Notice that $dif\_TVtempo_{e2} = 0$ for all *e* because $TVtime\_A_{e2} = TVtime\_B_{e2} = 0.5$ for all *e*. We estimate the following linear specification:

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

[12] Cities do not necessarily appear three times in the sample. A few cities were created over the 1998-2006 period. More importantly, not all state races required a runoff in all three election years.

γ is the parameter of interest. We test the hypothesis that $\gamma > 0$.

$round_r$ is a dummy that takes the value of one for $r = 2$. If $\omega < 0$, then the average voting gap closes between rounds. Controlling for this "round-specific effect" is crucial. Table 6 shows that the average gap in TV time (dif_TVtime_{er}) shrinks between rounds. Regardless of television, dif_votes_{er} may also shrink if runner-ups are unknown and voters pay closer attention in the second round. In the absence of $round_r$, the coefficient γ captures this average "shrinking effect". In summary, our procedure uses only how dif_TVtime_{er} changes (between rounds) differently in different races.[13]

X_e is a vector of time-invariant characteristics of race e, and includes city characteristics, election-year specific effects, and, more importantly, candidates' characteristics; because the runoff is held three weeks after the first-round, most determinants of electoral performance are in fact constant; ε_{er} contains all time varying unobserved determinants of the electoral outcomes.

Ex ante differences in candidates' strength affect election performance. First-round coalitions - which determine TV time allocation – are formed taking into account the expected chances of winning.[14] Thus, candidates' strength not only belongs to the right-hand side of (1), but also correlates with the first-round TV time allocation (dif_TVtime_{e1}). However, when we first-difference the data, *ex ante* candidate strength drops out from (1), along with all the other time-invariant factors in X_e. The estimated equation is:

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

Δ is a "difference-in-differences" transformation, i.e. the difference over rounds of the difference between the first-round winner and runner-up.[15]

In an ideally controlled experiment, we would observe the same pair of candidates twice (or multiple times) within a short period of time, and under identical election settings except for the TV time share. Our experiment violates the last condition. The first and second rounds are different races, and electoral conditions change between rounds. Scandals may emerge, although this is not a serious threat to identification because TV time is allocated *a priori*. If defeated candidates tend to

[13] The runoff dummy is analogous to period dummies in an ordinary panel. It controls for all "period-specific" effects and prevents the coefficient of interest from capturing spurious time effects.
[14] Members of the coalition normally share the spoils of victory in the form of positions in the elected administration.
[15] Because stronger candidates usually have more first-round TV time, dif_TVtime_{e1} tends to be large when the *ex ante* difference in candidates' strength is large. Since $dif\_TVtime_{e2} = 0$, Δdif_TVtime_e correlates with the unobserved differences in strength. But candidates' characteristics drop out after first-differencing and thus pose no threat.

support weaker runner-ups, then TV time will capture unobserved second-round support, biasing results away from zero. Voters may pay a closer attention to runner-ups once they make it to the second round. Fundraising, and consequently spending, ability may change between rounds. All these "non-TV effects" pose threats to our identification strategy. For the moment, we notice that the winner and runner-up receive together around 80% of the first-round votes (see Table 6). Thus the first round resembles a runoff.

Nevertheless, we implement a procedure that accounts for non-TV effects, at least partially. The second-round TV campaign starts a few days after the first-round ballot. The first second-round polls are typically conducted one week after the first-round. We use the following modified dependent variable:

$$\Delta(dif\_votes_e)^* = (votes\_A_{e2} - votes\_B_{e2}) - (Poll\_A_{e2} - Poll\_B_{e2}) \qquad (3)$$

$Poll\_A_{e2}$ and $Poll\_B_{e2}$ are the statewide vote intentions in the first second-round opinion poll. All non-TV effects taking place within a week of the first-round are "priced-in" the first poll, and are thus captured by the modified dependent variable. One example is support from defeated candidates, which is normally announced within a day or two after the first round.

Unfortunately, poll data is not available at the city level. However, because the poll is representative at the state level, it is equivalent to a weighted average with frequency weights equal to the city population as a percentage of the state population. Thus, it is natural to weight observations according the city population when using the modified dependent variable.[16]

Section 5.2 discusses "non-TV effects" in depth and presents evidence that, although possible theoretically, competing effects are not relevant empirically in our sample.

5 RESULTS

5.1 *Main Results*

Table 7 shows the main results. Column (1) has the estimates of equation (4) using the whole sample. The point estimate means that a one percentage point change (p.p.) in the difference in TV

[16] We emphasize that the reason for weighting is not making results representative at the state level.

time share causes a 0.272 p.p. change in the difference in vote share. The estimated coefficient is significant at the 1% level. The effect is also significant in practice. Averaging out the three elections in the sample, the mean first-round TV time share difference is 7.7 p.p. (see Table 6). Thus, the mean impact of advertising on the vote difference (second-round difference is always zero) is $7.7 \times 0.272 \approx 2.10$. The mean gap between the first-round winner and runner-up shrinks by 3 p.p. between rounds. Thus, changes in TV advertising are responsible for closing 70% of the gap.

[TABLE 7 HERE]

To account for the fact that the first and second rounds are structurally different races, we focus on sub-samples of races whose first and second rounds look alike. In column (2), the sample is composed of races in which the third-placed candidate is not pivotal for the second-round race, i.e., her votes are not enough to turn the race around in favor of the runner-up in the second round. Results are stronger. In column (3), the sub-sample is composed of races in which the two first-placed candidates have more than 92.04% of the votes in the first round (75^{th} percentile). Results are stronger. In column (4), we focus on races in which the runoff was likely (winner had less than 45% of votes, the median). In this case, voters should have been paying attention to both the winner and the runner-up, and the first round would look more similar to a runoff. Results are again stronger.

In column (5), we weight observations by the city population as a percentage of state population, which produces an estimate representative at the state level. The impact of TV is now smaller, 0.179, but still significant statistically. TV advertising now represents $7.7 \times 0.179 \approx 1.38$ or 46% of the closing gap. Thus, the effect is still significant in practice. It is not surprising that the impact of TV is smaller when we give more weight to larger cities. As we show below, the impact of TV is stronger in less educated places, and larger cities are better educated. This is one of the reasons why we prefer non-weighted estimates. In column (6), we use the modified dependent variable ($\Delta dif\_TVtime_e^*$). The (statistically significant) impact of TV advertising is now 0.168, only slightly lower than 0.179 [column (5)], the comparable weighted figure with the original dependent variable.

Finally, we include the difference in the first poll of the first round to control for the *ex ante* differences in vote intention. Regardless of the dependent variable used [the original, the weighted original, or the weighted modified, columns (7) through (9)], results are similar to the uncontrolled version [columns (1), (5) and (6)].

5.2 *Non-TV Effects*

We now discuss non-TV effects, a major challenge to the causal interpretation of results.

5.2.A *First and Second-Round Fundraising and Campaign Expenditures*

Campaign expenditures and fundraising are major stumbling blocks to identification. Brazilian races are expensive, especially if one considers that TV airtime is free of charge (Samuels, 2001).[17] Candidates may withhold first-round expenditures to have larger sums to spend in the runoff. For example, if a runoff is clearly necessary and the runner-up spot is not contested, the winner and the runner-up may withhold first-round expenditures. A surprise runner-up may see a surge in fundraising revenue. All these possibilities pose challenges. We cannot control directly for expenditures and fundraising in our regression models, but we have data on them by date and candidate for the 2008 mayoral elections. Although these are not gubernatorial races, mayoral races follow similar electoral rules and have similar party structure. Table 8 shows first-round spending and fundraising as a proportion of total spending and fundraising (first plus second round) for several sub-samples. We have 28 races in which a runoff was necessary.

[TABLE 8 HERE]

Row (1) shows several facts. First, fundraising and spending occur mostly in the first round, which is expected because the first-round race is longer than the runoff. Expenditures are more concentrated in the first round than fundraising, suggesting that money raised in the second round is partially used to settle first-round debt. Thus, instead of withholding expenditures, candidates do the opposite. Winner and runner-up raise and spend similar amounts in the first round as a proportion of total expenses and receipts. Relatively higher runner-up spending in the second round would raise the suspicion that differences in TV time capture an increased ability to raise and spend money.

Weak runner-ups may improve their fundraising prospects when they advance to the second round. To test the hypothesis, we focus on elections in which the runner-up was relatively weak, i.e. races in which the first-round vote difference between the winner and runner-up is above the median [row (2)]. Weak runner-ups rise slightly more in the first round than in the whole sample (70.8%

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

versus 67.8%). Consequently, they spend slightly more in the first round (61.2% versus 60.3%). When runner-ups are weak, winners raise and spend *less* money in the first round [column (2)].

Strategizing expenditures between rounds is more valuable when a runoff is likely. In row (3), we focus on races in which the winner's vote share is below the median (41.34% in this sample). Both the winner and the runner-up spend less in the first round when a runoff seems likely, but the difference is small: 1.1 and 2.0 p.p. for the winner and runner-up, respectively. Thus, strategizing spending between rounds is unlikely to correlate with Δdif_TVtime_e.

The runner-up may have to overspend in the first round when the race for the second place is contested. If weak runner-ups are more contested by the third-placed candidates, then changes in spending between rounds may correlate with changes in TV time (but the bias is against finding an effect of advertising). Runner-ups do spend more when the first round race is contested, but the difference is small [row (4)].

5.2.B *Further Robustness Checks*

Table 9 contains additional robustness checks. Except otherwise noted, the benchmarks for comparison are those that include the first first-round poll as a control: for even-numbered columns in Table 9, the benchmark is 0.356 (column (7) in Table 7); and for odd-numbered columns it is 0.181 (column (9) in Table 7).[18]

[TABLE 9 HERE]

First-round winners may have better first-round campaigns and win most of the votes they aspire to win, leaving little improvement ahead for them. In contrast, runner-ups may have more fledgling campaigns and thus have more improvement to make for reasons other than an increase in TV time. We call this the "diminishing returns" hypothesis. Decreasing returns pose a threat because the runner-ups have less TV time in the first round (see Table 6). We restrict the sample to races in which the runner-ups have more TV time than the winner [column (1) and (2)]. When the original dependent variable is used, the estimated impact is a little smaller [0.293 in Table 9, column (1) versus 0.356 in Table 7, column (7)], but still significant statistically and in practice. When we use the modified dependent variable, results are stronger than the benchmark [0.342 in Table 9, column

[18] We report the original (non-weighted) dependent variable, which yields our main estimates of interest, and the modified (and weighted) dependent variable, our main robustness check. Results using the original weighted dependent variable are similar and available upon request.

(1) versus 0.152 in Table 7, column (9)]. Diminishing returns also pose a threat if the runner-up is much weaker than the winner. In columns (3) and (4) we focus on a sub-sample of strong runner-ups (more than 34% of votes, the median). Results are similar.

Regardless of TV advertising, voters and the media may take a "second look" at surprising runner-ups (surprises typically concern the runner-up, not the winner). The Electoral Law (Lei 9,504 from 1997) mandates that TV coverage is fair and balanced. Jurisprudence interprets this as: 1) equal coverage time of candidates' campaign activity in the news; and 2) all candidates must be invited to all debates with equal speaking time. Thus, coverage is uniform, and no TV coverage closing gap arises. Nevertheless, we address this concern with the following sub-sample exercise. If a "second-look effect" drives results, then the impact of TV should be smaller when voters know who the runner-up would be. Columns (5) and (6) focus on cases in which race for runner-up spot is not contested (difference between the runner-up and the third-placed is large). Results are similar.

Rounds differ in the total amount of TV time available for the winner and runner-up. In our sample, they have a total of 655 and 920 minutes in the first and second rounds, respectively.[19] To increase comparability between rounds, we select a sub-sample in which the winner and the runner-up have more than 50% of the first-round TV time. In this case, the average TV time they have is the same across rounds. Results in columns (7) and (8) are again similar to the benchmark figures.

Finally, members of relatively smaller parties that manage to advance to the second round will probably receive most of their parties' resources during the runoff. This may create bias because weaker runner-ups may attract more resources and make larger gains for reasons other than the larger jump in TV time. We exclude cases in which the runner-up's coalition participates in less than three runoff races in the whole country [columns (9) and (10)]. Results are similar.

5.2.C *Negative Campaigning*

In a multi-party runoff system, many candidates compete for the second place. Thus, part of the TV time in the first round may be aimed at knocking down contestants for the runner-up spot. This strategy suggests that the runner-up's real change in TV time between rounds is greater than the

[19] Gubernatorial races had 1145 minutes of first-round airtime in all three election years. Second round time varies: 1050, 825 and 1210 in 1998, 2002 and 2006. Since more races in our sample come from 1998 and 2002, the average TV second-round TV time in our sample is 920 minutes. The runner-up and the winner have 57% of the first round airtime.

change measured by TV advertising. In this case, we may overestimate the impact of TV: the runner-up does better once the attacks stop.

We believe that "negativeness" does not drive results for two reasons. First, we have a direct measure of "negativeness" in campaigning for some races. In Brazil, an electoral judge arbitrates litigation among candidates. The most common demand is for *Direito de Resposta*, i.e. the right to use the offender candidate's TV time to reply to "offensive" propaganda. The number of requests of *Direito de Resposta* is a proxy for the "negativeness" of first-round campaigns. Requests were compiled by the TSE for some states.[20] Among contested runner-up races (difference between the second and the third placed less than the median [11%]) for which we have data (11 races), the mean number of requests is 10.36. Among landslides, requests are slightly higher, 13.60. The sample size is quite small (23), but the data suggest that "negativeness" does not correlate with the competitiveness of the first-round race.

Second, some robustness checks also address the issue of "negativeness". For example, we expect more attacks when the runner-up spot is hotly contested. In columns (3) and (4) of Table 9 we focus on strong runner-ups. In column (5) and (6), we restrict the sample to races in which the runner-up is much stronger than the third-placed candidate. In both cases, the impact of TV is similar to the comparable ones in Table 7 [columns (7) and (9)].

V.3 *First-round Results*

Estimates using only first-round data are biased away from zero because stronger candidates have more first-round TV time. If our strategy works, the estimated impact of TV using only first-round data should be higher than in Table 7. We report first-round estimates in Table 10.[21] The impact of TV is 0.510 [column (1)], i.e., much higher than the benchmark estimate in Table 7, column (1) (0.272). Inclusion of the difference in vote intention in the first first-round poll reduces the impact of TV advertising to 0.406, as expected [column (2)]. Nevertheless, results are still higher

[20] The algorithm for collecting data is as follows. In the TSE database for judicial decisions (*jurisprudência* at http://www.tse.gov.br/internet/jurisprudencia/inteiro_teor_blank.htm), we search, by electoral regional court (TRE) and by gubernatorial race in our sample, for the expression *Direito de Resposta* in judgment decisions (*acordãos*). We use the number of hits as the measure of requests for *Direito de Resposta*. If the search came empty, we checked whether the search for any judicial electoral decisions is empty. If not, then we impute the number zero as the number of requests of *Direito de Resposta*. Otherwise the race is missing because data for that court-race pair have not been compiled.

[21] We cannot control for city and election year fixed-effects when using first-round data. The best emulation is a set of election year and city dummies.

than the comparable one in Table 7 [0.356 in column (7)]. Not surprisingly, *ex ante* stronger candidates perform better: the estimated coefficient on the difference in vote intention is positive (0.064) and statistically significant. The difference in the estimated impact of TV is partly due to different samples (polls were not conducted in all races). We re-estimate the model maintaining the sample constant and the results remain similar [column (3)].[22]

[TABLE 10 HERE]

V.4 *Demographic Determinants of TV Advertising Effect*

Documenting the mechanism through which TV advertising affects electoral outcomes is interesting *per se*. It also serves as a falsification test. We expect media effects to be stronger in less educated where TV penetration is high. The model is:

$$dif\_votes_{er} = \alpha + \eta Shifter_e + \gamma \cdot Shifter_e \times dif\_TVtime_{er} + dif\_TVtime_{er} + \omega \cdot round_r + \lambda \cdot X_e + \varepsilon_{er} \quad (4)$$

We take differences and estimate the following equation:

$$\Delta dif\_votes_{er} = \gamma \cdot Shifter_e \times \Delta dif\_TVtime_{er} + \Delta dif\_TVtime_{er} + \omega \cdot \Delta round_r + \Delta\varepsilon_{er} \quad (5)$$

Using the 2000 census, we match election-year data with city-level information. We consider six shifters: population, income per capita and its distribution, number of years of schooling, and TV and radio penetration. Table 11 has descriptive statistics. Table 12 contains the results. In columns (1) through (5), we include the interaction of each shifter with $\Delta difTVtime_{er}$, one at a time. The impact does not vary according to city size [column (1)].[23] TV advertising has a stronger impact in poorer [column (2)], more unequal [column (3)], and less educated [column (4)] cities. Seemingly surprising, TV and radio penetration are associated with a smaller impact of advertising [columns (5) and (6)]. However, TV and radio penetration capture income.

[22] We discuss the estimates in columns (4) and (5) in Table 10 when we analyze the external validity of the results.
[23] This result is not in contraction with the difference between weighted and un-weighted estimates in Table 7, where the weight is city population as a percentage of state population, not the size of cities.

[TABLE 11 HERE]

[TABLE 12 HERE]

In column (6), we include interactions simultaneously. Now TV penetration has the expected sign: advertising has a stronger impact where TV penetration is deeper.[24] Education survives intact the inclusion of several factors. The coefficients on inequality and income maintain their sign, but are no longer statistically significant. In summary, advertising has a stronger impact where predicted.

The literature on lobbying commonly assumes that candidates take lobby money and use it to advertise to uninformed voters (Baron, 1994; Grossman and Helpman, 1996; Snyder, 1989). Our results support this assumption empirically. Advertising not only influences voters' choice, but its impact is stronger where voters are poorly educated, i.e. "uninformed".

6 DISCUSSION: EXTERNAL VALIDITY

The internal validity of our results is partially due to the specific nature of our empirical setting. In this section we discuss external validity. Before proceeding, a short digression on Brazilian gubernatorial elections in international perspective is warranted.

Brazil is a multiparty presidential federal republic. Elections for president, governor and mayor are held under a majority rule. Several countries share many of the characteristics of the Brazilian political and electoral system. Argentina, Colombia, Mexico, Nigeria, Peru, Russia, the United States, and Venezuela have federal systems with majority voting for all levels of government.[25] Except for the United States, all have multiparty political structures. A non-exhaustive list of countries with majority systems for president includes: Argentina, Austria, Bulgaria, Chile, Croatia, Ecuador, France, Finland, Ghana, Guatemala, Poland, Portugal, Romania, Indonesia, Serbia and Zimbabwe. Several of these countries have multiparty systems or are federal republics. Many entities share other electoral characteristics with Brazil. For example, similar to their Brazilian counterparts, American states are wide geographical areas with large populations. Under these conditions, TV is an important political advertising tool. Many local races throughout the world are held under a multiparty majority system, either in one round or two rounds. Arguably, TV is more important in gubernatorial and national elections. Nevertheless, as we show below, TV advertising is

[24] Radio penetration does not have enough variation across cities to estimate anything precisely (see Table 11).
[25] Although Colombia and Peru are not *de jure* federations, they are *de facto* federations because *Governadores de Departamento* are elected by direct ballot.

also important for local elections in Brazil, which increases the overall external validity of the results. In summary, Brazilian electoral institutions are not a peculiarity.

6.1 *Comparison with Races Decided in the First Round*

By construction, our design focuses on the races in which a runoff was necessary. Thus, it is a sample of sufficiently contested races. In terms of external validity, an important issue is whether races decided in the first round are systematically different from the sample races in dimensions other than contestability. For example, if runoffs occur in richer places, it is harder to extrapolate the results. We assess this possibility by estimating the following model:

$$\Pr(FirstRound = 1 \mid X_{st}) = f(X_{st}) \qquad (5)$$

s is a state and t is an election year (1998, 2002 and 2006). $FirstRound_{st}$ = 1 if the race st was decided in the first round, and 0 otherwise. X_{st} is a vector of state race characteristics, which includes the following demographics and electoral variables: 1) demographics (income per capita, growth of income per capita in the election year, income inequality, and education measured as the average number of years of schooling); 2) year and region dummies; 3) electoral variables (electorate size, number of candidates, and the lagged $FirstRound_{st}$). We present estimates for a linear probability model and Probit marginal effects.

[TABLE 13 HERE]

Estimates in Table 13 show that races decided in the first and second rounds are similar with respect to socio-economic demographics and political variables. Covariates are neither individually nor collectively significant in any of the four estimated specifications.

We also estimate the impact of advertising in races decided in the first round. External validity would be weakened if results were different than the first-round results among races in our main sample (i.e., races decided in the runoff). Table 10 contains the results. Comparing columns (4) and (5) to columns (1) and (2), we verify that results are similar.

6.2 *Mayoral Elections*

We introduce results from mayoral elections for two reasons. First, it is an opportunity to probe the robustness of results in another setting. Second, it serves as an additional test of external validity. Mayoral elections take place every four years and follow rules similar to gubernatorial races. Cities with more than 200,000 voters follow the same two-round system (remaining cities have one-round races). Voting is mandatory. The allocation of TV time is also similar.[26] The sample is composed of the 107 races that required a second round in the 1996, 2000 and 2004 election years.[27] Table 14 presents mean vote and TV time share for the mayoral races.[28]

[TABLE 14 HERE]

In contrast to gubernatorial races, the winner and runner-up have roughly the same TV time in the first round (compare with figures in Table 6). Correspondingly, no average closing gap arises. In addition, the winner and runner-up in mayoral races both have less TV time share than in gubernatorial races. Interestingly, they also have less first-round votes.

[TABLE 15 HERE]

Table 15 shows the results of a model similar to (2).[29] TV has a smaller impact than in gubernatorial races: the (statistically significant) coefficient in column (1) is 0.176 (versus 0.272, the most comparable estimate in Table 7). This is expected because Brazilian states are large and disperse geographically. Thus, far-reaching media such as TV should have a stronger impact on gubernatorial than on local races, where direct contact with the voter is more common and practical. Nevertheless, sample cities are large, and we still expect TV to influence election outcomes.

The remainder of Table 15 contains robustness checks. Column (2) has the pivotal exercise. Column (3) shows a subsample of races in which the first round is similar to the second round (sum of vote shares of the winner and runner-up is more than 69%, the median). In column (4), the sample is restricted to cases in which the runner-up has more TV time than the winner. All results are similar. Estimated standard errors are robust to clustering [columns (6) through (8)].

[26] In 2000 and 2004 the rule was exactly the same. In 1996 it was slightly different: candidates shared only a fifth equally, instead of a third.
[27] This includes the 1998 and 2002 races in Brasilia, the federal district.
[28] Polls were conducted in very few cities, preventing us from using the modified dependent variable.
[29] In contrast to gubernatorial elections, the source of variation in TV time is the same as the unit of observation (the city). Thus, there is no need to weight according to population to recover "representative" parameters. Nevertheless, estimated standard errors are robust to clustering at the city and year levels, as we show.

7 CONCLUSION

The "minimal effects" conventional wisdom is at odds with the perception of politicians, political analysts and general practitioners. Using quasi-experimental data, we find evidence that TV exposure in gubernatorial elections in Brazil has a strong effect on election outcomes, both practically and statistically. The magnitude – between 46% and 70% of the closing gap from first to second rounds in gubernatorial elections – shows that TV advertising is a major determinant of election outcomes. As expected, advertising has a stronger impact where voters are uneducated and have access to TV. Finally, advertising also matters in mayoral elections, corroborating the results from the gubernatorial races.

Brazil is the fourth largest democracy in the world, so results are relevant in practice. Gubernatorial elections involve reaching voters in large, geographically diverse states, which is not so different from gubernatorial or senate races in the United States. Furthermore, many other countries have state or provincial elections with similar characteristics. The use of large-scale TV advertising is a widespread phenomenon in modern democracies.

Our results have clear policy implications. A common concern in democracies is the influence of economic power on election outcomes. We show that money indeed can buy elections because TV advertising matters. But precisely because TV advertising matters, a centralized allocation of airtime may offset the influence of campaign spending.

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TABLES

Table 1 - Gubernatorial elections in Brazil, 1998-2002

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 2 - Sample

State	1998	2002	2006	Number of Municipalities
Amapá	X	X		16
Ceará		X		184
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 3 - Elected members of the Federal Chamber of Deputies, by party

	Year		
Party	**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)

Table 4 - Pairwise Correlation: Brazilian Chamber of Deputies and in the State Assemblies†

1994		1998		2002	
Amapá	0.61	Amapá	0.48	Goiás	0.77
Goiás	0.83	Ceará	0.68	Maranhão	0.57
Mato Grosso do Sul	0.82	Mato Grosso do Sul	0.75	Pará	0.72
Minas Gerais	0.80	Pará	0.87	Paraíba	0.82
Pará	0.87	Paraíba	0.70	Paraná	0.93
Rio de Janeiro	0.61	Paraná	0.87	Pernambuco	0.88
Rio Grande do Sul	0.60	Rio Grande do Norte	0.73	Rio de Janeiro	0.70
Rondônia	0.46	Rio Grande do Sul	0.62	Rio Grande do Norte	0.65
Roraima	0.28	Rondônia	0.80	Rio Grande do Sul	0.74
São Paulo	0.85	Roraima	0.55	Santa Catarina	0.89
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 5 - Number of first-round candidates

	Number of Candidates	
Year	Mean	Mean ± 1 Std. Dev
1998	6.23	(3.64, 8.82)
2002	7.86	(4.90,10.81)
2006	7.9	(5.82,9.98)

Source: Tribunal Superior Eleitoral (TSE)

Table 6 - 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 (12)	1st round winner	31%	43%	52%
	1st round runner-up	27%	37%	48%
2002 (14)	1st round winner	29%	41%	53%
	1st round runner-up	23%	33%	47%
2006 (10)	1st round winner	34%	44%	51%
	1st round runner-up	21%	37%	49%

‡: Statewide votes
Source: Tribunal Superior Eleitoral (TSE)

Table 7 - Dependent Variable: Δ(*Vote share_A - Vote share_B*)§

	First-Difference (2nd minus 1st round)								
	(1)	**(2)†**	**(3)††**	**(4)¥**	**(5)£**	**(6)£‡**	**(7)**	**(8)£**	**(9)£‡**
Δ(*Time share_A - Time share_B*)	**0.272**	**0.311**	**0.301**	**0.307**	**0.179**	**0.168**	**0.356**	**0.181**	**0.152**
	(0.077)* **	**(0.070)*** **	**(0.051)*** **	**(0.110)*** **	**(0.055)*** **	**(0.051)*** **	**(0.066)*** **	**(0.055)*** **	**(0.055)*** **
Δ(*First first-round poll*)							**-0.260**	**-0.138**	**0.094**
							(0.046)* **	**(0.060)** **	**(0.046)***
Constant	-0.002	0.003	0.051	-0.008	-0.035	-0.008	0.018	-0.029	-0.057
	(0.029)	(0.025)	(0.019)**	(0.034)	(0.019)	(0.020)	(0.081)	(0.023)	(0.053)
Number of Observations	7923	4537	1981	5463	7835	6991	7749	7663	6976
F-statistic	12.52	20.01	27.68	7.74	10.5	10.61	23.06	6.42	5.97
R^2	0.129	0.184	0.184	0.146	0.069	0.226	0.323	0.137	0.221

§ 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 the third-placed candidates are not pivotal (had less votes than difference between the winner and the runner-up)

†† 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).

¥: Sample restricted to races in which the winner had less than 45% of votes (less than the median).

£: Frequency weights: city population as % of the state population

‡ Modified Dependent Variable: Difference from 1st second-round opinion poll

*** = significant at the 1% level

** = significant at the 5% level

* = significant at the 10% level

Source: Tribunal Superior Eleitoral (TSE) and IBOPE.

Table 8 - Campaign Expenditures and Fund-Raising by Round 2008 Mayoral Elections

		Mean first-round EXPENSES as a share of total expenses†		Mean first-round FUND-RAISING as a share of total funds raised†	
		Winner	Runner-up	Winner	Runner-up
(1)	All sample (28)	67.2%	67.8%	60.3%	60.3%
(2)	Large First-Round Difference¥ (14)	65.5%	70.8%	59.7%	61.2%
(3)	Clear Second Round£ (14)	66.1%	65.7%	62.0%	64.1%
(4)	Tight race for second¥ (14)	64.0%	68.5%	59.1%	61.4%

†: Averages are computed attributing equal weight to cities. Number of observations in parentheses.

¥: Only races in which the difference of winner and runner-up vote share is larger than the median (10.83 percentage points).

£: Only races in which the winner had less the median share of votes (41.34%)

¥: Only races in which the difference between the runner-up and the third-place is less than the median (7.48 percentage points)

Source: Tribunal Superior Eleitoral (TSE)

Table 9 - Further Robustness Checks§

First-Difference (2nd minus 1st round)

	Runner-up more 1st round time than winner		Strong runner-up-¥		Runner-up >> third place ¥		Winner + Runner-up TV time more > 50%		No focused runner-up£	
	(1)	**(2)‡**	**(3)**	**(4)‡**	**(5)**	**(6)‡**	**(7)**	**(8)‡**	**(9)**	**(10)‡**
Δ(*Time share_A - Time share_B*)	**0.293**	**0.342**	**0.320**	**0.177**	**0.368**	**0.157**	**0.357**	**0.124**	**0.381**	**0.174**
	(0.084)* **	**(0.041)*** **	**(0.050)*** **	**(0.046)*** **	**(0.052)*** **	**(0.041)*** **	**(0.088)*** **	**(0.055)** **	**(0.089)*** **	**(0.061)*** **
Observations	1705	1476	3912	3610	5876	5403	3840	3299	3485	3161
R^2	0.112	0.032	0.308	0.079	0.361	0.041	0.281	0.066	0.337	0.032

§ Standard errors are computed with observations clustered at the state-election year level. First round vote shares are normalized to sum 1. Specifications include the difference in the first first-round poll, except for columns (3) and (4).

‡ Modified Dependent Variables: Differences from 1st second-round opinion poll; weighted by city population as a % of state

¥: Only races in which the difference beween the runner-up and the third-placed candidate is larger than 8.6 percentage points (75% less contested races)

¥: Runner-ups with more than 34% of the first-round votes (median).

£: Excludes races in which the runner-up party had 2 or less candidates running in runoffs in other states

£: Only races in which the difference in the first first-round poll is less than 11 percentage points (the median)

*** = significant at the 1% level

** = significant at the 5% level

* = significant at the 10% level

Source: Tribunal Superior Eleitoral (TSE) and IBOPE.

Table 10 - Dependent Variable: *Vote share_A - Vote share_B* §

	OLS: First-Round Only†				
	Runoff Necessary			Runoff not Necessary	
	(1)	(2)	(3)	(4)	(5)
Time share A - Time share B	**0.510**	**0.406**	**0.453**	**0.615**	**0.494**
	(0.101)* **	**(0.096)*** **	**(0.108)*** **	**(0.171)*** **	**(0.142)*** **
First Poll A - First Pool B		**0.064**			**0.190**
		(0.028) **			**(0.085)** **
Constant	0.089	0.289	0.323	0.387	0.196
	(0.028)***	(0.032)***	(0.037)	(0.075)***	(0.093)**
Number of Observations	7923	7649	7649	8250	6970
R^2	0.209	0.219	0.217	0.337	0.333

§ 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, TV time and vote intention 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 11 - Descriptive Statistics, city-election pairs in the sample§

	Median	Mean	Std Dev	Obs
Income per Capita †	4.47	5.42	5.88	7835
Population (in thousands)	9.64	32.5	209.84	7835
Gini ††	0.55	0.55	0.06	7835
Years of Schooling †††	4.46	4.41	1.21	7835
Radio ‡	88.68	85.03	12.21	7854
Television ‡	85.98	81.78	50.11	7854

§: 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 radio or television set

Source: Instituto Brasileiro de Geografia e Estatística (IBGE)

Table 12 - Dependent Variable: Δ(*Vote share_A - Vote share_B*)§

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Δ(*Time share A - Time share B*)	0.152	0.519	0.753	1.028	0.204	0.169	1.007
	(0.376)	(0.108)***	(0.199)***	(0.242)***	(0.278)**	(0.088)*	(0.450)**
Log (populacao)* Δ(*Time share A - Time share B*)	0.013						0.048
	(0.039)						(0.042)
Log (Income)†* Δ(*Time share A - Time share B*)		-0.178					-0.023
		(0.059)***					(0.036)
Log (*Gini*)* Δ(*Time share A - Time share B*)			0.787				0.257
			(0.278)***				(0.206)
Log(Schooling) †† * Δ(*Time share A - Time share B*)				-0.525			-0.650
				(0.159)***			(0.210)***
Log (*TV*)††† * Δ(*Time share A - Time share B*)					-0.274		0.170
					(0.087)***		(0.086)**
Log (*Radio*)†††† * Δ(*Time share A - Time share B*)						-0.596	0.058
						(0.230)***	(0.283)
Constant	-0.003	-0.011	-0.007	-0.011	-0.005	-0.007	-0.012
	(0.029)	(0.028)	(0.029)	(0.028)	(0.029)	(0.029)	(0.028)
Number of Observations	7835	7835	7835	7835	7835	7835	7835
F - statistic	6.40	12.57	9.34	13.26	12.74	12.79	8.46
R^2	0.133	0.179	0.150	0.191	0.152	0.163	0.209

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

† *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

* = 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 Estatística (IBGE)

Table 13 - (Not)Explaining the Necessity of a Second Round Dependent Variable = 1 if the Election is Decided in the First Round, 0 otherwise

	OLS			Probit Marginal Effects
	(1)	(2)	(3)	(4)†
GDP per capita (in thds of Reais)	-0.022	-0.029	-0.010	-0.010
	(0.027)	(0.027)	(0.033)	(0.034)
GDP per capita Growth (in %)	0.009	0.007	0.005	0.628
	(0.014)	(0.013)	(0.017)	(1.658)
Years of Schooling (in years)	0.001	0.069	-0.022	-0.027
	(0.073)	(0.100)	(0.118)	(0.121)
Inequality (Gini Index)	1.301	1.534	0.911	0.945
	(1.703)	(1.817)	(2.064)	(2.293)
Electorate Size (in millions)		0.003	-0.012	-0.014
		(0.013)	(0.016)	(0.016)
Number of Candidates		-0.027	-0.023	-0.023
		(0.028)	(0.033)	(0.034)
Previous Election Decided in the First Round?		-0.199	0.145	0.160
		(0.123)	(0.135)	(0.131)
Constant	-0.088	-0.495	0.262	
	(1.219)	(1.349)	(1.559)	
Region Dummies?	No	No	Yes	Yes
Year Dummies?	No	No	Yes	Yes
Number of Observations	81	81	81	81
p-value of joint significance ‡	0.603	0.417	0.323	0.517
p-value joint significance of additional regressors‡		0.776	0.992	
R^2	0.037	0.080	0.146	0.112

§ White-Huber standard errors, unless otherwise noted.

†: Pseudo-R^2 reported.

‡: p-value is computed using the cumulutative probabilty of χ^2(# restrictions tested) variable.

*** = significant at the 1% level

** = significant at the 5% level

* = significant at the 10% level

Source: Tribunal Superior Eleitoral (TSE) and Instituto Brasileiro de Estatística e Geografia (IBGE)

Table 14 - Descriptive Statistics: TV time and vote shares, mayoral races

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†
1996 (32)‡	1st round winner	22%	39%	54%
	1st round runner-up	23%	30%	46%
2000 (32)¥	1st round winner	23%	41%	55%
	1st round runner-up	23%	30%	45%
2004 (43)	1st round winner	25%	40%	54%
	1st round runner-up	26%	32%	46%

†: Observation is a city. Means computed giving equal weight to cities. Number of races in parentheses.
‡: Includes Brasilia 1998. ¥: Includes Brasilia 2002
Source: Tribunal Superior Eleitoral (TSE)

Table 15 - Mayoral Elections

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*)	**0.176**	**0.112**	**0.126**	**0.227**	**0.176**	**0.176**	**0.176**
	(0.044)* **	**(0.050)** **	**(0.044)*** **	**(0.134)***	**(0.044)*** **	**(0.034)*** **	**(0.017)*** **
Constant	-0.053	-0.104	-0.049	-0.076	-0.053	-0.053	-0.053
	(0.013)***	(0.017)***	(0.013)***	(0.039)*	(0.013)***	(0.012)***	(0.006)***
Number of Observations	107	21	54	56	107	107	107
F-statistic	16.27	3.81	8.07	2.86	15.70	26.09	103.06
R^2	0.132	0.139	0.122	0.067	0.132	0.132	0.132

§ White-Huber standard errors, unless otherwise noted. 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 69% of the votes in the first round (median of the sum of votes of winner and runner-up).

£: Only races in which the runner-up had more TV first-round time than the winner
¥: In columns (5), (6) and (7), standard errors clustered at the state-election year, state and year levels, respectively.
*** = significant at the 1% level
** = significant at the 5% level
* = significant at the 10% level
Source: Tribunal Superior Eleitoral (TSE) and IBOPE.

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