# Electoral Crime Under Democracy: Evidence from Brazil

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**Abstract**

This paper presents the first analysis of the relationship between electoral crimes and electoral performance in large democracies. Using a sample of candidates charged with electoral crimes in the race to municipal office in Brazil between 2004 and 2016 and employing an instrumental variables strategy, I find that being ultimately convicted by the Brazilian Electoral Court reduces the probability of election by 23.1 percentage points and convicted candidates' vote share by 13.3 percentage points. These results are robust to different estimation strategies and are not explained by changes in voter nor candidate behavior once an unfavorable ruling is made public. I further estimate the electoral payoff when crimes are not detected and whether voters impose differential punishment for substantial or procedural rule-breaking; while there is a significant gain in the probability of election and vote share (4.7 and 4.9 percentage points respectively) when candidates deploy substantial illegal tactics, voters impose the same electoral penalty regardless of candidates' charges. This result explains why candidates and parties would still employ illegal tactics while risking detection by the Brazilian Electoral Court.

**Keywords:** electoral politics; judicial politics; comparative politics; illegal behavior and the enforcement of law; political economy.

**JEL classification:** D72; K42; P48.

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# 1 Introduction

In democratic regimes, office-seeking politicians employ various tactics to get elected. They might promise voters more resources to increase the provision of local public goods, such as schools, hospitals, or roads; they can promote their candidacies by running ads on TV and, more recently, on social media; they might even meet with their constituents and ask for their vote based on their personal connection. While these tactics are different, sometimes complementary ways to win an election, they all characterize *play-by-the-rules* strategies, in which individuals follow legal provisions when running for office. Governments allow such electoral practices because they make electoral systems more competitive, and increase access to political office to more groups representing citizens. In this paper, however, I focus on illegal tactics to win elections and produce the first analysis of the effect of electoral crimes on ballot performance in large democratic regimes.

Scholars have not ignored these mechanisms used for winning elections. Lehoucq (2003) offers a comprehensive account of electoral fraud, which takes up a variety of forms, such as procedural rule-breaking, illegal campaigning, violence, and even unequivocal vote buying. In a more recent study, Gans-Morse et al. (2013) design a theoretical framework encompassing four types of clientelism practices (vote, turnout, and abstention buying, and double persuasion) and their adoption under five different institutional designs. They argue that the choice of illegal action is conditional on the design of electoral systems. For instance, in an environment of increased political polarization, we should expect to see more of turnout buying but less of vote buying.

Indeed, most studies looking into illegal electoral tactics have two common characteristics: first, they are largely concerned with coercive threats that prevent free and fair elections, as suggested by Mares and Young (2016); second, they focus heavily on non or partially democratic regimes, evidenced by the vast literature on electoral authoritarianism (Levitsky and Way, 2002; Gandhi and Lust-Okar, 2009; Ichino and Schündeln, 2012; Schedler, 2015; Asunka et al., 2017). This is a rich literature that helps understanding the use of elections for regime consolidation and continuity. Nevertheless, I address two unexplored issues that are supplemental to the established literature investigating electoral fraud.

The first contribution here is uncovering the effect of electoral crimes that are harder to detect or whose relationship with electoral outcomes is less known or well understood. For instance, politicians might use illegal forms of advertising or slush funds to spend beyond their campaign limits in order to win an election. Likewise, candidates and political parties might put forth candidacies for public office even if they do not meet all electoral requirements, a particular feature of Brazilian electoral law,[1] as a means of raising their profiles for future elections. These strategic moves are less easily understood than flat out vote buying, for example. The second contribution is precisely understanding how electoral crimes shape electoral outcomes in large democracies. Brazil is one of the top five largest democracies in the world and as such is an important research setting for understanding the use of illegal electoral tactics. Beyond just size, and despite a recent fallback, the

[1]For instance, parties need to file financial records proving they are financially solid and candidates must not have been convicted of crimes at the appellate level either at the state and federal judicial systems.

quality of Brazil's democracy makes it an important case study: since 2006, the country consistently ranks in the top 20% countries in the V-Dem Electoral Democracy Index (Coppedge et al., 2018).

Besides the electoral fraud scholarship, the present study contributes to the broader literature of political economy of development. Brazil has an unique institutional design in which the judiciary branch has an entire system of State (TRE) and Federal (TSE) electoral courts resolving electoral claims. Their mandate is to guarantee free and fair competition for public office, enforcing the Brazilian Electoral Code of 1965 and subsequent legislation, and to prevent that candidates not meeting legal requirements join electoral races. To the extent that the Electoral Courts are successful in rooting out this type of wrongdoing, we should expect more electoral accountability from office-holding politicians. Candidates would avoid illegal tactics to preserve their future career prospects. Understanding if electoral systems as such are effective should provide an important takeaway for countries sharing the same institutional design. Mexico, South Africa, and India are but a few of developing countries which also have a dedicated electoral authority similar to Brazil's. In addition, this paper investigates another source of judiciary power beyond settling legal disputes between economic and political agents; since every political candidate in Brazil needs a judicial authorization to run for office, the Electoral Court holds an enormous amount of power in shaping up political representation – an unusual role played by judiciary branches.

Another important contribution in this study is the use of court documents as data. I collect and code judicial rulings from TRE and TSE courts on candidacies for municipal office in Brazilian elections between 2004 and 2016. For a subset of these documents, I implement a support-vector machine classification algorithm to find the exact allegations against candidates that prevent them from running for office. I split such allegations in two categories, substantial and procedural rule-breaking, to identify heterogeneous effects of electoral crime on performance for the 2012 and 2016 elections. This project forms part of a recent wave of studies using court documents to measure economic and political outcomes in development settings (Sanchez-Martinez, 2018; Lambais and Sigstad, 2018).

Using these court documents, I recover the causal effect of electoral crimes adopting an instrumental variables (IV) strategy. Since the judicial ruling on candidacies is composed of up to three instances of review, I can instrument decisions at the trial stage (first instance) for appellate decisions (second or third instance) for a subset of candidates who have untried appeals by election day. In such cases, the Brazilian electoral code establishes that candidates can be voted for, and that their votes should be counted, regardless of the charges brought against their candidacy in the first place. While the trial ruling is endogenous, e.g. potentially correlated with other factors determining electoral chances, appellate rulings issued *after* election day cannot influence electoral outcomes beyond their connection with trial decisions. Thus, for this subsample of candidates running for office who have an untried appeal standing at the time of election, I can identify the causal effect of crimes on performance.

The main IV result shows that a conviction for electoral crime reduces the probability of election and a candidate's vote share by 23.1 and 13.3 percentage points, respectively. These estimates are

statistically significant at the one percent level and significantly differ from OLS point estimates. These results are robust to the inclusion of covariates and fixed-effects, coefficient stability tests (Oster, 2019), Monte Carlo simulations of IV parameters, and reverse causality checks. Unauthorized candidates are also significantly further away from the election threshold in both proportional (city council) and majoritarian (mayor) systems, but this effect is indistinguishable from OLS estimates. These results indicate that an unfavorable candidacy ruling negatively impacts a candidate's chances but the actual effect size is only consistent in the probability of election and vote share estimates. Though we should be careful when comparing these results with studies looking at punishment for corruption in Brazil (Ferraz and Finan, 2008, 2011; Winters and Weitz-Shapiro, 2013), which is a more severe crime prosecuted by various other legal authorities and judicial bodies, the evidence here points to the same negative impact of (detected) illegal behavior on electoral performance.

I proceed further and test whether such illegal tactics, when undetected, have any electoral payoff. I find that candidates accused of a substantial breach of electoral law (e.g., candidates or parties have used illegal campaign strategies, channeled slush funds for campaign ads, have previous outstanding judicial convictions preventing them from running for office) increase their probability of election and vote share by about 4.8 percentage points compared to candidates accused of procedural rule-breaking. This result supports a positive electoral effect of adopting such campaign strategies. Finally, I test whether voters impose heterogeneous penalties in cases of substantial or procedural rule-breaking. I find, however, that voters are not sophisticated and do not differentiate them crime charges. Taken together, these results are an indication that candidates might risk punishment in exchange for the electoral benefit that is realized if they are not detected by the Brazilian Electoral Court.

In the remainder of this paper, I explain the institutional background allowing for causal identification in section 2, present the data in section 3, and discuss the theoretical mechanism underlying the relationship between electoral crimes and performance in section 4. Section 5 discusses the empirical strategy and section 6 presents the main results. In section 7, I investigate alternative explanations for the effect of electoral crimes on performance coming from changes in the behavior of voters, candidates, and judges. Section 8 discusses heterogeneous punishment effects. Section 9 concludes and suggests further avenues of research.

## 2 Institutional Background

The Brazilian Federal (TSE) and State Electoral Court (TRE) systems have existed intermittently since 1932 but only became institutionally relevant after the country's return to democracy in 1985. Since then, electoral courts have a fundamental role in guaranteeing free and fair elections. Their mandate is to enforce the Electoral Code of 1965 and subsequent legislation, particularly the law establishing conditions for ineligibility to public office (1990), the Law of Political Parties (1995), the Law of Elections (1997), and the Clean Slate Act of 2010.

These courts are have four main responsibilities: (i) electoral rule-making; (ii) judicial consultations clarifying and establishing jurisprudence for conflicting electoral norms; (iii) administration of the electoral process, which consists in publishing the electoral calendar, testing voting machines, distributing voting machines to all districts, counting and publishing electoral results; and, finally, (iv) conflict resolution on claims of breach of electoral law.

In this project, I am mostly interested in the courts' conflict resolution function and its underlying judicial review process. According to Brazilian Law, every individual running for office, at every level, has to submit proper documentation proving that they meet eligibility requirements for the office they are running; for instance, they should be 35 years of age or older to run for president or senator; executive-office holders, if running for any other elected office, must step down from their current post six months before election day. Every electoral cycle, the highest-level electoral court, TSE, establishes a schedule for submission of all these documents, which are reviewed at lower-level courts by electoral judges who issue rulings authorizing every single candidacy in the country. This is the main institutional feature that allows for causal identification of electoral crimes on performance.

An example helps illustrate this point. The most recent municipal elections took place on October 2, 2016. The deadline for submitting all candidacy documents was August 15, 2016. Between August 15 and September 12, electoral courts reviewed and authorized each candidacy for mayor or city councilor. The review process started at the electoral district in which the candidate is running for office, and their trial ruling comes out of the designated electoral judge for that district. These judges are part of the state court system and, when appointed to the electoral bench, are on leave from their original tenured positions at the state system. They serve on two-year mandates, with one reappointment allowed, such that they never oversee the same district for more than one electoral cycle. If either a candidate or someone else, such as opponents or the Office of Electoral Prosecutions (MPE), files an appeal to the trial ruling, the case is presented before a panel of three judges at the state electoral court TRE. There are seven appellate court justices in each state's TRE, serving up to four-year mandates, and they are immune to local politics. In any state, six of these judges are voted in by their fellow tenured judges at the state and federal court systems and the last member is appointed by the President of Brazil. If plaintiffs or defendants are unhappy with the appellate court decision, they can appeal their case before the federal court TSE, which serves as the third and final instance of judicial review for mayor and city councilor candidates.

The September 12[2] date is the key institutional feature that allows observing performance for politicians who violate electoral rules. It is the last day for entering candidate information onto electronic voting (EV) machines distributed at every single polling station in the country.[3] All candidates who have untried appeals by this date will have their information loaded, and thus can be voted for, in the EV machines on election day. Because of this feature, I can observe the electoral performance of candidates who eventually are convicted of electoral crimes and compare

[2]The exact day varies marginally every cycle. In 2018, for instance, deadline for candidacy submission was Aug 15, last day for loading candidate information was Sep 17, and election day was Oct 7.

[3]Fujiwara (2015) describes this technology in detail.

to candidates who are cleared of similar charges. If candidates have had a final ruling before September 12, or if they have decided not to appeal their trial sentence, I cannot observed their performance because TSE will not include their information in the EV machines.

Exogenous variation in convictions for electoral crimes comes from the timing according to which appeals sentences are issued by higher-level courts. Often, the high number of candidates running for municipal office, the judicial backlog, or the conditions of a particular electoral race make it difficult for electoral courts to hand out final decisions by Sep 12. Moreover, since candidates with outstanding appeals will have their information loaded onto EV machines regardless, there is no strong incentive for courts to issue decisions between then and Oct 2. In the lead-up to election day, judges and court officials are working around the clock making sure that 540,000+ EV machines are delivered to 450,000+ voting stations across the whole territory of Brazil; judges are ruling on smaller electoral cases that might or might not be appended to candidacy cases; court officials are meeting with political parties and discussing the local electoral situation, so on and so forth. It is not uncommon, therefore, that final decisions are issued only after election day has passed, specially in municipal elections, when stakes are lower than in federal elections.

When candidate appeals are not ruled in time for elections, candidate information (picture, name, voting number) is displayed in the EV machine but their votes are computed *sub judice* – their vote count will be considered valid only when the TRE or TSE publish their final decision on any individual's candidacy. Effectively, thus, the decisions at the appeals stage cannot affect electoral outcomes, since they are issued only after election day has passed, but they bear a strong relationship to the sentence handed out by the trial judge in each electoral district. Decisions at trial are mostly endogenous to electoral outcomes, but the use of appeals as instruments leaves out only their exogenous part – allowing for causal identification.

The primary limitation of this study is that I can only recover causal effects of electoral crimes under restrictive conditions pertaining to municipal elections in Brazil. At any other electoral race, both the trial and appeals stages are handled by the TRE and this might shape the way electoral judges issue rulings in response to the importance of the office for which a candidacy has been presented. For instance, senators are much more influential than city councilors and have a direct channel of communication with the President of Brazil, who is responsible for appointing one judge per TRE. Second, there are a number of candidates who do not appeal their trial ruling and as such do not appear on the EV machine on election day. Thus, I cannot observe their ballot performance. It is likely that these candidates are heterogeneous in many dimensions when compared to candidates who have outstanding appeals, such as their political experience, or their drive to hold elected office. These candidates should be the object of future projects measuring the effect of electoral crime on electoral performance in developing countries, and this paper inaugurates such literature.

# 3 Data

The main data source for electoral performance is TSE's repository of electoral data. TSE publishes electoral results, vote counts, candidate's individual characteristics, and their candidacy's situation on election day for all elections since 1994. I focus on the municipal elections after the introduction of the EV machine in 2002 for even performance measures across elections and municipalities. My sample is composed of 9,470 candidates for mayor or city councilor who appealed, or had third-parties appealing, the trial ruling on their candidacy authorization. These candidates have been displayed in the EV device and could have been voted for on election day. Their candidacy remained pending after elections and they have only been allowed to take up office once a final ruling was issued. I create three outcome measures from TSE's data: (1) *the probability of election*, which is a binary variable taking up value one when the candidate received enough votes to be elected. For mayor candidates, under majoritarian rule, this means 50 percent plus one of all valid votes. For city council candidates, under proportional rule, this means having received enough votes to rank amongst the most voted candidates within the designated number of vacancies for each municipality; (2) *vote share* as a share of total valid votes; (3) *vote distance to election cutoff*, which is the percentage point distance between a candidate's vote share and the votes necessary for election. Outcomes (1) and (2) are make or break measures of electoral crime: we can use them to estimate whether a convicted candidate is predicted to win or lose an election; conditional on having won (or lost) an election, outcome (3) describes the relative safety (or damage) from employing an illegal electoral tactic.

Data on the reasons for rejecting candidacies are scraped from the TSE website, which makes all their court documents public. I have developed a web scraper that downloads the case number for each municipal candidacy and uses these numbers to find case files and sentences. Though I can find case numbers for all candidates in my sample, I cannot find case information for elections in 2004 and 2008 due to limitations in the TSE website. Thus, I produce heterogeneous treatment effect estimates only for the more recent elections in 2012 and 2016. For the benefit of research transparency, all programs and data are freely available online.[4]

I have already collected all data on candidates and electoral performance from the TSE repository. Candidate characteristics are age, gender, political experience, education, marital status, and the reported amount of funds they have spent on the campaign trail. There are, however, a few data management and transformation tasks remaining. I have coded political experience as a binary variable for when candidates report holding office at the time of elections, but I have yet to include as experience candidates who have run for office multiple times and, when running for city councilor, candidates who belong to the strongest coalition at the time of elections. Second, I need to collect further data on campaign expenditures; I have coded reported spending using the information in TSE's candidacy dataset, which has many missing values, but the comprehensive data on funding belongs is in TSE's campaign funds database.

[4] I maintain a github repository storing all programs, data, and manuscripts used in this project. For the time being, this page is set to private and the latter link only provides access to the web scrapers.

As far as obtaining the data from court documents go, I have already written the web scrapers for obtaining case numbers and information, but have only downloaded case numbers. The remaining steps for completing the data collection task are downloading all possible case information in HTML format, one per case, and parsing HTML files into human-readable text. Next, I should use text analysis software to read these judicial decisions and code them into the appropriate bins for each electoral infraction. Though I am yet to do this for the whole sample, I have completed this entire process for a random sample of 1,000 candidates in the 2016 election as a trial run, so I am well versed on the necessary steps for coding the remaining judicial cases used in this paper.

# 4 Theory

# 5 Empirical Strategy

## 5.1 Instrument Validity Checks

# 6 Results

## 6.1 Coefficient Stability Test

## 6.2 IV Parameter Simulations

# 7 Alternative Explanations

## 7.1 Heterogeneous Sentencing

## 7.2 Voter Behavior

## 7.3 Candidate Behavior

# 8 Heterogeneous Electoral Punishment

# 9 Conclusion

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# Appendix

## 9.1 Electoral Ruling Classification

# Tables and Figures

Table 1: Descriptive Statistics

| | N | Mean | St. Dev. | Min | Max |
|---|---|---|---|---|---|
| Age | 9,470 | 46.34 | 11.02 | 17 | 86 |
| Male | 9,470 | .793 | .405 | 0 | 1 |
| Political Experience | 9,470 | .091 | .287 | 0 | 1 |
| Campaign Expenditures (in R$) | 9,470 | 52,555 | 210,742 | 0 | 4,949,250 |
| Convicted at Trial | 9,470 | .641 | .480 | 0 | 1 |
| Convicted on Appeal | 9,470 | .537 | .499 | 0 | 1 |
| Probability of Election | 9,442 | .191 | .393 | 0 | 1 |
| Vote Distance to Election Cutoff (in p.p.) | 9,442 | −4.09 | 9.55 | −92.82 | 12.83 |
| Total Vote Share (in p.p.) | 9,442 | 10.13 | 17.98 | 0.00 | 100.00 |

Table 2: Electoral Crime Rulings

| | *Appeals* | | Percent |
|---|---|---|---|
| *Trial* | Affirmed | Reversed | Reversed |
| Not Convicted | 3380 | 22 | 0.6 |
| Convicted | 5059 | 1009 | 16.6 |

Table 3: First-Stage Regressions

| | Outcome: Convicted at Trial | | |
|---|---|---|---|
| | (1) | (2) | (3) |
| Convicted on Appeal | .766*** | .753*** | .738*** |
| | (.006) | (.007) | (.009) |
| Individual Controls | - | Yes | Yes |
| Fixed-Effects | - | - | Yes |
| Observations | 9,470 | 9,470 | 9,470 |
| Adjusted-$R^2$ | .633 | .649 | .861 |
| $F$-stat | 16,364.9*** | 1,094.0*** | 21.7*** |

*Note:* First-Stage regressions here report the correlation between being convicted at trial and being convicted on appeal for all candidates who have had their candidacy challenged under charges of electoral irregularities. I present results including and excluding individual politician characteristics; municipal, electoral, and party fixed-effects; and use robust standard errors. *p<0.1; **p<0.05; ***p<0.01

Table 4: Hausman Test of Instrument Strength

| Outcome | Hausman Statistic | $p$-value |
|---|---|---|
| 1. Probability of Election | 109.28 | .000 |
| 2. Total Vote Share | 205.57 | .000 |
| 3. Vote Distance to Election Cutoff: | 1.88 | .170 |
| 3.1. City Councilor | 65.44 | .000 |
| 3.2. Mayor | 93.43 | .000 |

Figure 1: Instrument Point Estimates and CIs

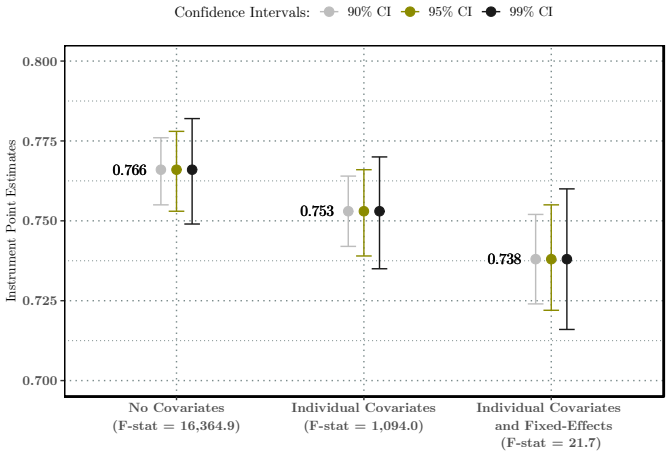


Table 5: The Effect of Electoral Crime on the Probability of Election

| | Outcome: Probability of Election | | | | | |
|---|---|---|---|---|---|---|
| | OLS | OLS | OLS | IV | IV | IV |
| | (1) | (2) | (3) | (4) | (5) | (6) |
| Convicted at Trial | −.208*** | −.151*** | −.163*** | −.272*** | −.213*** | −.231*** |
| | (.009) | (.009) | (.014) | (.011) | (.010) | (.016) |
| Individual Controls | - | Yes | Yes | - | Yes | Yes |
| Fixed-Effects | - | - | Yes | - | - | Yes |
| Observations | 9,442 | 9,442 | 9,442 | 9,442 | 9,442 | 9,442 |
| Adjusted-$R^2$ | .065 | .149 | .303 | .059 | .143 | .300 |
| *F*-stat | 653.58*** | 104.02*** | 2.46*** | 707.35*** | 108.9*** | 2.47*** |

*Note:* The regressions here estimate the effect of being convicted at trial on the probability of election for all candidates who have had their candidacy challenged under charges of electoral irregularities. Columns 1 and 4 display models not including individual candidate characteristics; columns 2 and 5 include age, gender, marital status, education level, political experience, and the amount spent in their campaign; columns 3 and 6 also include municipal, electoral, and party fixed-effects. I report robust standard errors for all specifications in this table. *p<0.1; **p<0.05; ***p<0.01

Table 6: The Effect of Electoral Crime on the Total Vote Share

| | Outcome: Total Vote Share (in p.p.) | | | | | |
|---|---|---|---|---|---|---|
| | OLS | OLS | OLS | IV | IV | IV |
| | (1) | (2) | (3) | (4) | (5) | (6) |
| Convicted on Appeal | −12.945*** | −8.316*** | −9.943*** | −16.804*** | −11.765*** | −13.254*** |
| | (.418) | (.337) | (.529) | (.478) | (.399) | (.624) |
| Individual Controls | - | Yes | Yes | - | Yes | Yes |
| Fixed-Effects | - | - | Yes | - | - | Yes |
| Observations | 9,442 | 9,442 | 9,442 | 9,442 | 9,442 | 9,442 |
| Adjusted-$R^2$ | .119 | .379 | .606 | .109 | .371 | .602 |
| *F*-stat | 1,278.91*** | 361.57*** | 6.15*** | 1,360.8*** | 368.19*** | 6.14*** |

*Note:* The regressions here estimate the effect of being convicted at trial on the total vote share for all candidates who have had their candidacy challenged under charges of electoral irregularities. Columns 1 and 4 display models not including individual candidate characteristics; columns 2 and 5 include age, gender, marital status, education level, political experience, and the amount spent in their campaign; columns 3 and 6 also include municipal, electoral, and party fixed-effects. I report robust standard errors for all specifications in this table. *p<0.1; **p<0.05; ***p<0.01

Table 7: The Effect of Electoral Crimes on the Vote Distance to Election Cutoff

| | Outcome: Vote Distance to Election Cutoff (in p.p.) | | | |
|---|---|---|---|---|
| | OLS | IV | OLS | IV |
| | (1) | (2) | (3) | (4) |
| Convicted at Trial | $-.575^{***}$ | $-.849^{***}$ | $-5.172^{***}$ | $-7.381^{***}$ |
| | (.064) | (.075) | (1.905) | (2.184) |
| Individual Controls | Yes | Yes | Yes | Yes |
| Fixed-Effects | Yes | Yes | Yes | Yes |
| Sample | City Council | City Council | Mayor | Mayor |
| Observations | 7,100 | 7,100 | 2,342 | 2,342 |
| Adjusted-$R^2$ | .431 | .428 | .384 | .382 |
| $F$-stat | $3.54^{***}$ | $1.86^{***}$ | $3.55^{***}$ | $1.85^{***}$ |

*Note:* The regressions here estimate the effect of being convicted at trial on the distance to the election cutoff for candidates who have had their candidacy challenged under charges of electoral irregularities. All models include individual candidate characteristics and municipal, electoral, and party fixed-effects. Since election rules differ by office type, I split the sample into city council candidates (columns 1 and 2) and mayor candidates (columns 3 and 4). I report robust standard errors for all specifications in this table. $^{*}$p<0.1; $^{**}$p<0.05; $^{***}$p<0.01

Table 8: Coefficient Stability Test For Various $R^2_{max}$

| | Individual Covariate Models | | | Individual Covariate and Fixed-Effects Models | | |
|---|---|---|---|---|---|---|
| *Outcomes:* | $R^2_{ur}+(R^2_{ur}-R^2_r)$ | $2 \cdot R^2_{ur}$ | $R^2$ for $\beta = 0$ | $R^2_{ur}+(R^2_{ur}-R^2_r)$ | $2 \cdot R^2_{ur}$ | $R^2$ for $\beta = 0$ |
| Probability of Election | 2.63 | 1.50 | - | 3.55 | 3.10 | - |
| | (.24) | (.30) | (.37) | (.96) | (1.00) | (2.10) |
| Vote Share | 1.80 | 1.23 | - | 7.26 | 7.26 | - |
| | (.64) | (.76) | (.85) | (1.00) | (1.00) | (2.73) |
| Vote Distance to Cutoff (City Councilor) | 6.72 | 5.05 | - | 63.08 | 63.08 | - |
| | (.22) | (.25) | (.75) | (1.00) | (1.00) | (25.78) |
| Vote Distance to Cutoff (Mayor) | 2.44 | 1.23 | - | 4.66 | 4.66 | - |
| | (.27) | (.35) | (.39) | (1.00) | (1.00) | (1.61) |

Figure 2: Simulation of IV Point Estimates

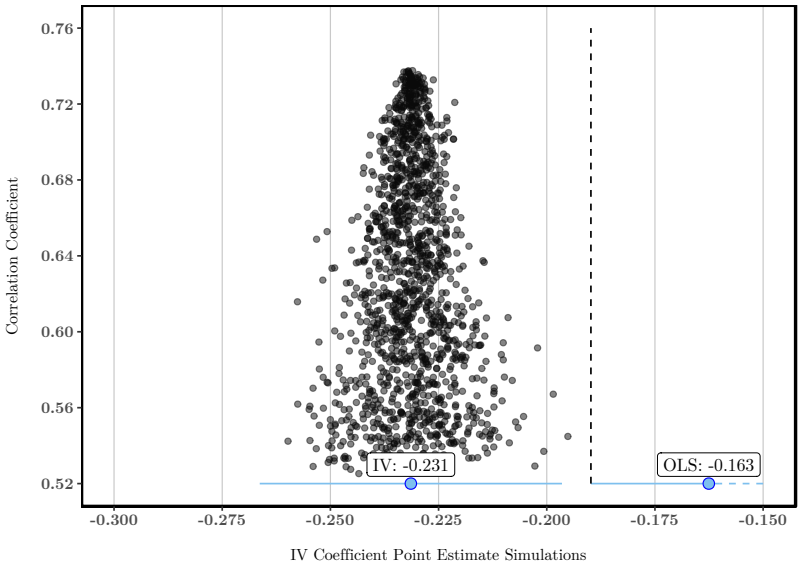


Table 9: Heterogeneous Sentencing across Trial and Appeals

| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| | $\beta_{\text{trial}}$ | $\beta_{\text{appeals}}$ | $\beta_{\text{difference}}$ | s.e. | $t$-stat | $p$-value |
| Elected to Office | −.223 | −.267 | .044 | .085 | .510 | .610 |
| Age | −.001 | .000 | −.001 | .003 | −.424 | .671 |
| Male | .029 | .022 | .007 | .039 | .176 | .861 |
| Political Experience | −.089 | −.013 | −.076 | .079 | −.964 | .335 |
| Campaign Expenditures (ln) | −.029 | −.028 | −.001 | .029 | −.034 | .973 |
| Marital Status: | | | | | | |
| Divorced | −.006 | .026 | −.032 | .038 | −.839 | .402 |
| Legally Divorced | .066 | .028 | .039 | .048 | .795 | .427 |
| Single | −.008 | .043 | −.051 | .040 | −1.276 | .202 |
| Widowed | .029 | −.011 | .040 | .064 | .626 | .532 |
| Educational Levels: | | | | | | |
| Completed ES/MS | −.160 | −.234 | .074 | .090 | .819 | .413 |
| Incomplete ES/MS | −.116 | −.259 | .143 | .134 | 1.066 | .286 |
| Can Read and Write | −.066 | −.286 | .220 | .174 | 1.268 | .205 |
| Completed HS | −.191 | −.259 | .068 | .085 | .799 | .424 |
| Incomplete HS | −.108 | −.264 | .156 | .132 | 1.180 | .238 |
| Completed College | −.218 | −.300 | .083 | .099 | .833 | .405 |
| Incomplete College | −.177 | −.270 | .093 | .125 | .742 | .458 |

*Note:* In this table, I report the coefficients of two regressions using the same covariates on the probability of receiving an unfavorable ruling at trial (column 1) and on appeals (column 2). I then recover the distributions of the differences in betas and test H0: $\beta_{\text{difference}} = 0$ for all covariates in the regressions (columns 3-6). Robust standard errors are clustered at the municipal-election pair level (equivalent to the judge-level error shared by all candidates in one municipality during one election period); party-fixed effects are included in both regressions but are not reported here.

Figure 3: Instrument Correlation with Covariates

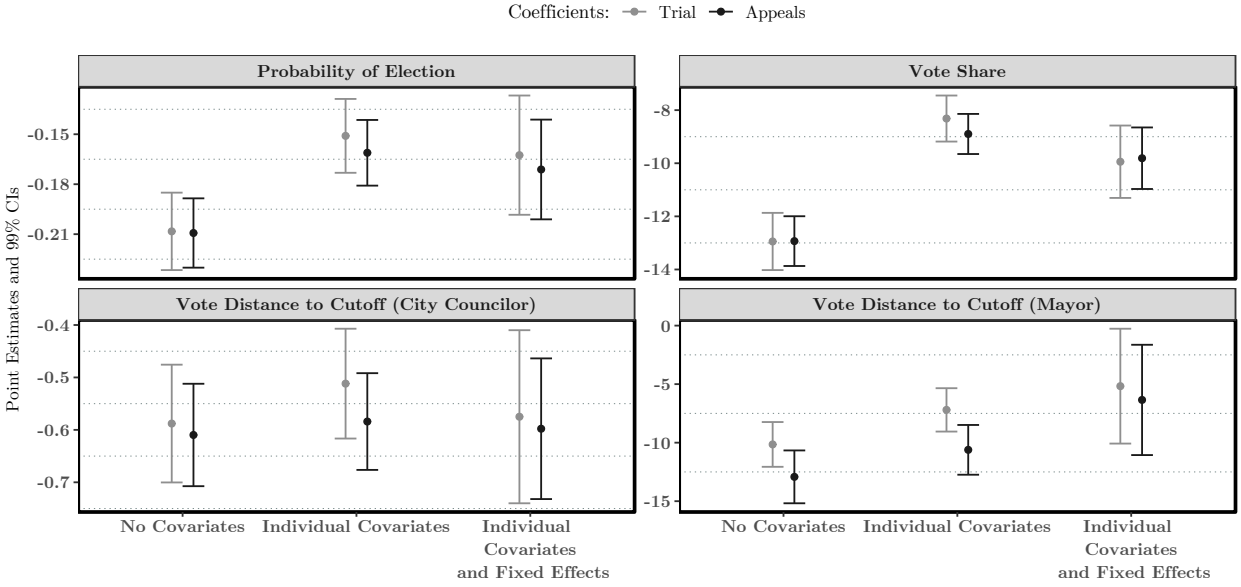


Table 10: The Effect of Electoral Crimes on Voter Engagement

| | Party-Level | | Election-Level | |
|---|---|---|---|---|
| | Outcome: Voter Turnout (percent) | Outcome: Invalid Votes (percent) | Outcome: Voter Turnout (percent) | Outcome: Invalid Votes (percent) |
| | (1) | (2) | (3) | (4) |
| Share of Candidacies Invalid at Trial | .003 | .222*** | −.001 | .134* |
| | (.007) | (.076) | (.009) | (.070) |
| Individual Controls | - | - | - | - |
| Fixed-Effects | Yes | Yes | Yes | Yes |
| Observations | 5,322 | 5,322 | 3,757 | 3,757 |
| Adjusted-$R^2$ | .997 | .973 | .995 | .946 |
| $F$-stat | 214.3*** | 354.1*** | 81.8*** | 124.5*** |

*Note:* The regressions here estimate the effect of the share of candidates convicted at trial overall the total office vacancies on voter turnout and the number of invalid votes (both logged). I aggregate observations up to the party and election level and control for municipality and election year fixed-effects. I report robust standard errors, clustered by elections and municipalities, for all specifications in this table. *p<0.1; **p<0.05; ***p<0.01

Table 11: Campaign Expenditure Across Ruling Group

| | *Mean Campaign Spending in Ruling Group (in R$)* | | | |
|---|---|---|---|---|
| *Stage* | Favorable | Unfavorable | *t*-stat | *p*-value |
| Trial | 84,766 | 34,497 | 9.45 | .000 |
| | [3,402] | [6,068] | | |
| Appeals | 73,275 | 34,658 | 8.62 | .000 |
| | [4,389] | [5,081] | | |
| *Unfavorable Ruling* | Affirmed | Reversed | *t*-stat | *p*-value |
| Trial | 34,346 | 34,527 | −0.05 | .961 |
| | [5,059] | [1,009] | | |

*Note:* This table reports t-tests across different subsamples of candidates. The number of observations in each group is reported inside the squared brackets.

Table 12: Voter Sophistication and Benefit of Rule-Breaking

| | | $\beta_1$: Substantial Violation | |
|---|---|---|---|
| | | $\beta_1 = 0$ | $\beta_1 > 0$ |
| $\beta_2$: Convicted at Trial × Substantial Violation | $\beta_2 = 0$ | **1.** Violation carries no electoral benefit.<br>**2.** Voters impose same penalty for different electoral violations. | **1.** Violation helps candidate get elected.<br>**2.** Voters impose same penalty for different electoral violations. |
| | $\beta_2 < 0$ | **1.** Violation carries no electoral benefit.<br>**2.** Voters impose harsher electoral penalties for substantial violations. | **1.** Violation helps candidate get elected.<br>**2.** Voters impose harsher electoral penalties for substantial violations. |

Table 13: Heterogeneous Effect of Electoral Ruling

| | Full Sample | | City Councilor | Mayor |
|---|---|---|---|---|
| | Outcome: Probability of Election | Outcome: Vote Share (in p.p.) | Outcome: Vote Distance to Cutoff (in p.p.) | Outcome: Vote Distance to Cutoff (in p.p.) |
| | (1) | (2) | (3) | (4) |
| Convicted at Trial | −.176*** | −7.369*** | −.713*** | −6.653*** |
| | (.020) | (.719) | (.084) | (2.101) |
| Substantial Violation | .047** | 4.939*** | .089 | .169 |
| | (.024) | (.723) | (.103) | (1.524) |
| Convicted at Trial | −.014 | −4.952*** | .015 | 1.644 |
| × Substantial Violation | (.028) | (.915) | (.111) | (2.562) |
| Individual Controls | Yes | Yes | Yes | Yes |
| Fixed-Effects | Yes | Yes | Yes | Yes |
| Observations | 4,717 | 4,717 | 3,465 | 1,252 |
| Adjusted-$R^2$ | .375 | .697 | .499 | .380 |
| $F$-stat | 2.54*** | 6.84*** | 3.70*** | 1.73*** |

*Note:* The regressions here include the severity of the accusation brought against candidates running for municipal office. I recover the accusations from court documents and identify ruling type using linear support-vector machine classification (details in appendix REF). In columns 1-4, I report the coefficients on ruling outcome (row 1), type (row 2), and their interaction (row 3). All regressions include municipal, electoral, and party fixed-effects. Robust standard errors are displayed inside parentheses. *p<0.1; **p<0.05; ***p<0.01