

Electoral Crime Under Democracy: Information Effects from Judicial Decisions in Brazil

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Abstract

This paper presents the first analysis of the relationship between information shortcuts of 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 candidates' vote share by 13.3 percentage points. The convictions serve as information signals about politician type and influence how voters cast their ballots. These results are robust to different estimation strategies and are not explained by changes in judge, voter, or candidate behavior over the judicial process. 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 engage in substantial rule-breaking, voters impose the same electoral penalty regardless of information type. Though voters use the information shortcuts to make voting decisions, they only partially act on the signals received about candidate type.

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

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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 elected office to a larger share of their citizenry. Nevertheless, an important question remains as to the illegal practices used to win an election in democratic regimes and how voters interpret these practices when they are made public. In this paper, I answer these questions and produce the first analysis of the effect of electoral crimes on electoral performance in large democracies.

Scholars have not ignored the use of illegal strategies in elections. Lehoucq (2003) offers a comprehensive account of the relationship between electoral fraud and election outcomes, discussing the various forms of crime, such as procedural rule-breaking, illegal campaigning, violence, and 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 the electoral system. For instance, in an environment of increased political polarization, we should expect to see more of turnout buying but less of vote buying. The majority of these studies looking into electoral crimes has two common characteristics: first, they are primarily 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 in Asunka et al., 2017; Gandhi and Lust-Okar, 2009; Ichino and Schündeln, 2012; Levitsky and Way, 2002; Schedler, 2015).

The first contribution of this paper is analyzing electoral crimes through an information shortcut angle. I posit that rulings by Electoral Courts in Brazil serve as signals of candidate type that help citizens make voting decisions. In a crowded candidate space, judicial sentences help distinguishing good and bad candidates. The information shortcut literature is well-established (Popkin, 1991; Lupia et al., 2000, and others), but to my knowledge this is one of the first attempts at using court documents as sources of information on electoral crimes. This paper also uncovers 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 the Brazilian electoral system (Novaes, 2018),¹ as a means of

¹For instance, one such requirement is that parties need to file financial records proving they are financially solid;

raising their profiles for future elections. These strategic moves are less easily understood than flat out vote buying, for example.

Besides the electoral fraud and information cues scholarship, the present study also advances the broader literature of political economy of development. Brazil has a unique institutional design in which the judiciary branch has an entire system of State (TRE) and Federal (TSE) electoral courts resolving electoral disputes. 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 electoral courts are successful in rooting out this type of wrongdoing, we should expect more electoral accountability from office-holding politicians. Candidates would also avoid breaching electoral law to preserve their future career prospects. Understanding if electoral systems as such are effective should provide an important takeaway for countries with similar institutional designs. Mexico, India, and South Africa are but a few developing countries which also have a dedicated electoral authority similar to Brazil's. These monitoring institutions are an important source of judiciary power beyond their traditional role of settling legal disputes; 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 the judiciary branch in developing countries.

The other contributions of this paper are methodological in nature. First, I use court documents as data. I collect and code TRE and TSE judicial rulings on candidacy authorization cases for candidates running for municipal office in Brazil between 2004 and 2016. For a subset of these documents, I implement a linear support-vector machine classification algorithm to find the exact allegations against candidates that prevent them from running for office. I split such allegations into two categories, substantial and procedural rule-breaking, to identify heterogeneous information effects of electoral crime on performance for the 2012 and 2016 elections. This approach aligns with a recent wave of studies using court documents to measure economic and political outcomes in development settings (Sanchez-Martinez, 2018; Lambais and Sigstad, 2018; Poblete-Cazenave, 2019).

Using these court documents, I recover the causal effect of criminal information shortcuts on voter response adopting an instrumental variables (IV) strategy. Since the judicial ruling on candidacy cases has 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 at the trial stage. While the trial ruling is endogenous, e.g., potentially correlated with other factors determining a candidate's electoral chances, appellate rulings issued *after* election day cannot influence electoral outcomes beyond their connection with trial decisions.

another is that candidates must not have been convicted of crimes at the appellate level either at the state or federal judicial system.

Thus, for this subsample of candidates running for office who have untried appeals standing at the time of election, I can identify the causal effect of candidates' criminal cues on their electoral performance.

The main IV result shows that revealing criminal information, i.e., convictions in court for electoral crimes, 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 estimates. These results are robust to the inclusion of covariates and fixed-effects, coefficient stability tests (Oster, 2019), and inclusion and exclusion restriction checks. Convicted candidates are also significantly further away from the vote threshold necessary for election in both proportional (city council) and majoritarian (mayor) systems, but this effect is indistinguishable from OLS estimates. These results indicate a negative and significant effect of the judicial ruling on electoral chances: voters interpret judicial convictions as negative signs of candidate type and punish them at the ballot box. This is a novel information shortcut effect, and supplements party label, interest group, and media effects documented elsewhere in the literature. These results closely align with similar studies documenting a negative effect of corruption information on voting decisions in Brazil (Ferraz and Finan, 2008, 2011; Winters and Weitz-Shapiro, 2013), though information type and research design change across studies, making it harder to directly compare effect sizes.

I test multiple alternative explanations for the effect on electoral performance. I show that changes in judicial sentencing behavior, candidate campaign strategies, and voter engagement cannot explain the hit to electoral performance, leaving punishment for electoral crimes as the only plausible explanation for the negative information effect. I further find that candidates accused of a substantial violation of electoral law (e.g., candidates or parties using illegal campaign strategies, channeling slush funds for campaign ads, having previous outstanding judicial convictions preventing them from running for office) increase their probability of election and vote share by 4.7 and 4.9 percentage points, respectively, when compared to candidates accused of procedural rule-breaking. This result indicates a positive payoff of employing such actions to convince voters. However, there is no support for heterogeneous information effects by crime type. Voters punish bad type candidates evenly, regardless of whether they had standing charges of substantial or procedural violations of electoral law. Despite holding candidates accountable for their electoral conviction, voters are not sophisticated enough to impose differential penalties by criminal information type.

In the remainder of this paper, I explain the institutional background in Brazil allowing for causal identification in section 2, I present the data in section 3, and discuss the theoretical grounds for the effect of criminal information in section 4. Section 5 discusses the empirical strategy of this paper, and section 6 presents its main results. Section 7 explores the exclusion restriction tests. In section 8, I investigate alternative explanations for the effect on performance. Section 9 discusses the heterogeneous punishment effects. Section 10 concludes and suggests further avenues of research.

2 Institutional Background

The Brazilian Federal (TSE) and State (TRE) Electoral Court 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 have four primary 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 votes 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 to which 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 calendar 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. These cases are called *registro de candidatura*, or candidacy registration in free translation. The review of these cases is the primary institutional feature that allows for causal identification of the effect of electoral crime information on performance.

An example helps illustrate this point. The most recent municipal election took place on Oct 2, 2016. The deadline for submitting all candidacy documents was Aug 15, 2016. Between Aug 15 and Sep 12, electoral courts reviewed and authorized each candidacy for mayor or city council. 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.² 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 Prosecution (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

²In Brazil, judges are appointed to the bench in state and federal courts when they pass nationally-competitive entrance examinations. They are automatically tenured after a two-year trial period; therefore, their entire career is independent of electoral politics.

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 council candidates.

The Sep 12³ date is the crucial institutional element 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.⁴ 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 to candidates who are eventually cleared of these charges. If candidates saw a final ruling before Sep 12, or if they have decided not to appeal their trial sentence, I cannot observe their performance because TSE will not load their information in the EV machines.⁵

Exogenous variation in convictions for electoral crimes comes from the timing according to which higher-level courts issue appeals sentences. 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 standing 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+ working EV machines reach 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 electoral situation, so on and so forth. It is not uncommon, therefore, to see final decisions come out only after election day has passed, especially in municipal elections.

When candidate appeals are not ruled in time for elections, the TSE loads candidate information (picture, name, voting number) on 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 ruling. Effectively, thus, the decisions at the appeals stage cannot affect electoral outcomes, since they are issued *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 with respect to local electoral conditions, 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 criminal information under restrictive conditions pertaining to municipal elections in Brazil. Local politicians have no control over the careers of electoral judges nor can they sanction judges. At any other electoral race, however, Congress, Senate, and State Governor candidates are much more powerful and can

³The exact day varies marginally every cycle. In 2018, for instance, the deadline for candidacy submission was Aug 15, last day for loading candidate information was Sep 17, and election day was Oct 7.

⁴Fujiwara (2015) describes this technology in detail.

⁵There is no early voting in Brazil. Voters cast their ballot on a single day using the EV machines.

influence how electoral judges rule their cases. 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. The second limitation is that several candidates do not appeal their trial ruling and as such do not appear on the EV machine on election day. Thus, I cannot observe electoral performance for every candidate who has had their type revealed in the form of convictions for electoral crimes – just for the subgroup that filed an appeal or has had a third-party appeal their candidacy. It is likely that the latter candidates are heterogeneous in many dimensions when compared to candidates who have not appealed trial decisions, such as their political experience, or their drive to hold elected office. Excluded from this analysis, these candidates should be the object of future projects measuring the effect of judicial (criminal) information on electoral performance in developing countries, and this paper inaugurates such literature.

3 Data

The primary data source for electoral performance is TSE’s repository of electoral data. TSE publishes electoral results, vote counts, candidates’ 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. There are 9,470 candidates for mayor or city council in this sample; these candidates appealed, or had third-parties appealing, the trial ruling on their candidacy case. These candidates were displayed in the EV device and could have been voted for on election day. Their candidacy remained pending after elections; only if a favorable appeals ruling came out were the elected candidates allowed to take up office. I create three outcome measures from TSE’s data to measure electoral performance: (1) *the probability of election*, which is a binary variable taking up value one when the candidate received enough votes for election. 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 in 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: I can use them to estimate whether a candidate whose type has been revealed as bad is predicted to win or lose an election; conditional on having won (or lost) an election, outcome (3) describes the relative safety (or damage) resulting from the release of criminal information.

Next, I scrape court documents containing the allegations against each candidate from the TSE website. I developed software⁶ that downloads case files and sentences for all candidates in my sample. Though the information is public, due to data availability limitations at the TSE, 99.5 percent of court documents come from candidates in the 2012 and 2016 municipal elections. I match

⁶For the benefit of research transparency and replication, all programs and analysis scripts are freely available online on [GitHub](#).

court documents to candidates using an individual identifier provided by the Electoral Court so that I can recover all documents for each candidacy.

Table 1 reports the summary statistics of the sample. The average age is 46.3 years, and the overwhelming majority of candidates is male. Nine percent of them have any political experience, captured by whether they held any other elected office in the past. These candidates have reported, on average, campaign spending of R\$ 52,555. Using the current exchange rate, this is equivalent to ~\$15,000 per campaign. Sixty-four percent have seen an unfavorable ruling from the trial judge at their electoral district and 53.7 percent have had an unfavorable ruling after appealing their case to higher courts. Notice that all candidates have seen charges brought against them at trial, otherwise they would not have standing appeals by election day and would not be part of this sample; the conviction variables here capture unfavorable decisions issued by trial judges. If an electoral judge allowed a candidate to run for office, then either the trial or appeals variables become zero. Though not reported in table 1, I also collect information on candidates' marital status and education.⁷ These are categorical variables, and the most frequent marital status is married (62.6 percent) and education level is high school (30.8 percent). Finally, I report the means for the three outcomes in this analysis. The mean probability of election is 19.1, while the mean candidate's vote share and vote distance to cutoff are 10.1 and -4.1 percentage points, respectively.

4 Theory

Assume three representative agents are interacting in an election: voter A , candidate B , and judge J . They each have their utility function $f(X_c, \varepsilon_c)$ represented by a matrix of observed candidate characteristics X_c and unobserved characteristics ε_c . The former could be anything from policy positions, age, ethnicity, marital status, or campaign expenditures. A candidate's political ability, the deals they make with parties, supporters, or sponsors are the latter. In this setting, candidates' information is essential for determining the outcome of the election: A chooses candidates that maximize their utility $U_A = f_A(X_c, \varepsilon_c)$. For instance, A might prefer politically-aligned, more educated candidates because they believe these to be the most prepared candidates to take up office. Most importantly, A dislikes candidates who have a criminal record because it signals dishonesty. I make this explicit by separating criminal charges information c_c from matrix X_c in A 's utility function and setting the first derivative of f_A with respect to c_c to negative, as follows in equations (1) and (2):

$$U_A = f_A(X_c, c_c, \varepsilon_c) \quad (1) \quad \frac{\partial U_A}{\partial c_c} < 0 \quad (2)$$

In addition to the representative voter's preferences, I am also interested in candidate B 's preferences. B derives utility from holding elected office and is looking to adopt strategies that help their electoral endeavor. They cannot withhold or control specific characteristics, such as age, gen-

⁷I also have information on each candidate's party and use it as fixed-effects in the empirical sections.

der, ethnicity, but can choose amongst campaign expenditure levels (included in X_c) and electoral strategies that get them closer to winning an election. Some beneficial strategies are legal while others are not (b_c), and B adopts a mix of strategies such that the expected electoral payoff remains positive – there is a strictly positive risk of illegal strategy detection by judge J , and candidate B chooses strategies before judges authorize each political campaign.⁸ B 's utility is:

$$U_B = f_B(X_c, c_c, b_c, \varepsilon_c) \quad (3)$$

The focus in this paper is identifying how the disclosure of criminal records and illegal strategies, respectively summarized by c_c and b_c , impact a candidate's chances of election. More specifically, I am looking at the effect of releasing information on electoral crimes for electoral performance. The reasoning is straightforward. Voters dislike crimes and are likely to punish candidates who are found guilty of electoral violations (bad candidate type) in the run-up to an election. This mechanism would predict a negative first derivative $\partial U_B / \partial c_c < 0$ for c_c in equation (3): convictions on record hurt a candidate's chances to hold office. Some of this effect, however, could be offset by the boost in votes that would come from illegal strategies b_c . Suppose a candidate prints and distributes negative material on their opponents. The information in the advertisement is false, and such ad is not allowed in the jurisdiction where this office race is taking place. Though judges could eventually ban such material, once the information is out, it might hurt targeted opponents beyond reparation. In such hypothetical scenario, the strategy was illegal but benefited the candidate running the ad. In this case, $\partial U_B / \partial b_c > 0$. I want to test both effects on electoral performance.

4.1 Application to the Brazilian Context

The majority of the literature in electoral crimes is concerned with more severe actions, such as fraud or vote buying (Lehoucq, 2003). This paper is an important contribution to the scholarship by looking at other, more common, and more nuanced violations to electoral rules that are particular to large democracies, such as Brazil. To the extent that voters interpret the judicial information as a signal of candidate type, we should expect a change in voter behavior as a result of the disclosure of information.

As discussed in section 2, Brazil is a particularly interesting research setting because of the structure of its dedicated electoral court (TSE and TREs). All candidates need an authorization from the electoral judge in their district to run for office. This decision comes out as a judicial sentence, just like in any other legal case. The judge responsible for the electoral district verifies a candidate's application and issues an authorization based on compliance with the electoral code. Judges check whether a candidate's party has met all electoral requirements, whether candidates have met all criteria of the office to which they are running, and other legal provisions as established by electoral law.

⁸I assume a uniform distribution of detection risk across electoral districts for the reasons laid out in section 2, guaranteeing the independence and quality of electoral judges in Brazil. Note that applications of this simple model to other jurisdictions would likely change this assumption to adjust to features of other judiciary systems.

By coding statutes and judicial sentences, I recover both c_c and b_c . Sentence outcome, authorizing or dismissing a candidacy, makes up c_c ; legal reasons justifying the sentence make up b_c . In this study, there are two reasons why judges prevent candidates from running for office: (1) *procedural* rule-breaking, which are cases in which candidates are in breach of electoral law for trivial reasons. For instance, they could have forgotten to include a copy of their ID card in their application, or they could have missed a deadline in the application process. In either case, their candidacy is deemed incomplete, and they are not allowed to run for office; or (2) *substantial* rule-breaking, which are more severe cases in which either parties or candidates are in breach of more substantial elements of electoral law. Parties might not have kept, or presented, all financial records from previous elections, candidates might have an outstanding conviction for previous crimes, or they might have been convicted for running illegal campaign strategies against their opponents. Substantial cases are much more likely to be connected to campaign, office, or government crimes disliked by voters.

Another benefit of using candidacy cases to identify c_c and b_c is their standard sentences and penalties. Judges dismiss candidacies when they do not meet all requirements, whether the reasons are either substantial or procedural requirements; also, there is no jail time nor immediate financial penalties for candidates and parties, making the legal punishment for candidacy convictions uniform. In general, electoral cases take less time to conclude (17 months on average) than other cases in the Brazilian judicial system (26 months on average) (CNJ, 2018). Though standard sentences and penalties might not be ideal from a policymaking perspective, they create a subset of legal cases less susceptible to external influence and relatively stable in terms of the application of legal statutes and convictions.⁹

Finally, these cases allow for the testing of heterogeneous information effects by conviction type. If voters are sophisticated, not only they punish candidates with unfavorable trial rulings (*the conviction, or criminal information, effect*), but they also differentiate the punishment conditional on the crime (*the crime type effect*). One can reasonably expect that candidates charged with more severe crimes, such as illegal campaign spending, or convicted for previous crimes, signal a more systematic criminal behavior and should be punished more harshly than candidates missing deadlines or lacking hard copies of certain documents. Though judicial punishment is the same, the electoral punishment could still reflect the relatively more severe violations. There is substantial evidence in the literature against voter sophistication in other information contexts (Avis et al., 2018; Banerjee et al., 2010; Chong et al., 2015; de Figueiredo et al., 2011; Ferraz and Finan, 2011; Weitz-Shapiro and Winters, 2017; Winters and Weitz-Shapiro, 2013); this paper explores yet another mechanism of providing information to voters (judicial decisions) and investigates how they

⁹These cases, however, are often appended to other cases at the electoral court system and can create financial liabilities for candidates and their parties. The analysis of these other cases is beyond the scope of this study as they do not meet the criteria for causal identification developed here. There is also growing interest for electoral court reform in Brazil. Some experts criticize the fact that electoral justices do not have fixed appointments and thus do not specialize in electoral crimes; others say they that parties and candidates strategically avoid harsher punishments by requesting other court systems to move charges to electoral courts, knowing that their punishments are constrained to the electoral arena.

react to it.

5 Identification Strategy

In this paper, I adopt an instrumental variables (IV) approach. As described in section 2, I can only recover local average treatment effects (LATE) for the subsample of candidates who are charged with electoral crimes by the trial judge and have a standing, untried appeal on election date. Candidates who break the electoral code but are not detected are not part of this study, neither are candidates who have chosen not to appeal their trial sentence. For this sample of candidates, I estimate the following regression model in three ways and using three different measures of electoral performance:

$$y_i = \alpha + \rho \cdot c_i + X\beta + \sum \lambda_{i,k} + \varepsilon_i \quad (4)$$

The dependent variable y_i forms are: (1) the probability of election, taking up value one when either the mayor or city council candidate had enough votes for election in their district; (2) the total vote share of candidate i in their race; (3) the vote distance to the election cutoff, which is the percentage point margin between candidate i 's vote share and that of the single elected candidate (when running for mayor) or last elected candidate (when running for city council). Using outcome (1), I can measure the impact of crime on the most important outcome of any political campaign, i.e. being elected; outcome (2) serves as a measure of the overall impact of crime on candidate popularity; outcome (3) tells us about the relative benefit (or cost) of committing an electoral crime when candidates are trying to secure an electoral lead or narrow in on races in which they are trailing another candidate; X is the matrix of candidate characteristics, such as candidate age, gender, marital and education status, political experience, and campaign spending; $\sum \lambda_{i,k}$ is a set of k fixed-effects to capture any additional unobservable heterogeneity coming from party, election, and municipal factors shared by subsets of candidates.

The main independent variable is the binary indicator for convictions for electoral crime c_c at the electoral court system for candidate i . If a candidacy has been rejected by the trial judge responsible for that electoral district, c becomes one. I use convictions at trial in OLS regressions for benchmarking the biased effect on electoral performance; in reduced-form regressions, I replace convictions at trial for convictions on appeal – which becomes one when the candidate has seen an unfavorable ruling at higher courts within the electoral system. The reduced-form regressions hint at any potential correlation between instruments and outcomes beyond the channel via the endogenous decision at trial (discussed in section 7). I lastly estimate model (4) using two-stage least squares (2SLS) regressions, in which I instrument convictions at trial for convictions on appeal. Since I am looking at appellate court decisions issued after election day, the exclusion restriction is straightforward as I measure the instrument *after* observing the outcomes.¹⁰ Any effect of disclosing

¹⁰In addition to the temporal effect, the other theoretical arguments discussed in section 2 support the exogeneity of the instrument. Electoral judges are tenured state judges which have no ties to local politicians. Their wages,

appellate decisions only influences electoral performance via their relationship with convictions at trial. I address additional concerns about violations to the exclusion restriction in the following sections, but the baseline instrumental variables and the first-stage regression equations are:

$$y_i = \alpha + \rho \cdot \hat{c}_{i,\text{trial}} + X\beta + \sum \lambda_{i,k} + \varepsilon_i \quad (5)$$

$$c_{i,\text{trial}} = \alpha + \rho \cdot c_{i,\text{appeals}} + X\beta + \sum \lambda_{i,k} + \varepsilon_i \quad (6)$$

For every specification of equations (5) and (6), I estimate versions excluding and including individual characteristics (matrix X) and fixed-effects $\sum \lambda_{i,k}$. In addition to instrument validity tests, I also report coefficient stability tests across different specifications to demonstrate that selection on unobservables is not driving the results, as discussed in Altonji et al. (2005); Nunn and Wantchekon (2011); Oster (2019); Pei et al. (2019). I discuss and test other alternative, confounding explanations in the following sections and provide the empirical strategy at each stage of analysis.

5.1 Inclusion Restriction Checks

The first step in this analysis is guaranteeing I have a strong instrument for the endogenous regressor of interest (conviction at trial). Table 2 provides us anecdotal evidence on the relationship between convictions at either stage of the judicial review process. The overall reversal rate of trial decisions is 10.9 percent. Reversals come mostly from candidacy cases that had been denied by trial judges (16.6 percent). The unconditional Pearson correlation coefficient between convictions at trial and on appeals is .796. These results make intuitive sense given the presumed quality of judges and standard sentencing (both in substance and form) discussed in previous sections.

A more robust test, however, is reported in table 3. I present three first-stage regressions on the relationship between the endogenous variable (convictions at trial) and instrument (convictions on appeals). Across models progressively including candidate characteristics and municipal, electoral, and party fixed-effects, the coefficient on the instrument is always statistically significant (p -value $< .01$). The magnitude remains stable within the .738-.766 range, which means that a conviction on appeals explains roughly three-quarters of the outcome at trial. The positive relationship confirms the anecdotal evidence in table 2.

I additionally report each coefficient point estimate, confidence intervals (CIs), and F -statistics for all three regressions in figure 1. The inclusion of covariates and fixed-effects across models marginally shifts down the magnitude of instrument estimates. In all cases, however, the F -statistic of excluded instruments remains greater than industry standards at $F = 10$ (Bound et al., 1995). It means that the first-stage model is significantly predicting the candidacy outcome at trial and confidently partials out the causal effect of convictions on electoral performance.

In table 4, I present the Hausman tests for OLS consistency. I report the results for bivariate regressions between convictions at trial and on appeals for all outcomes.¹¹ Each row contains the

career prospects, and time on electoral bench are all independent of the action of mayors and city councilors.

¹¹I also run multivariate versions of Hausman tests, but there are no changes to p -values. Results are available

F -stat and p -values for the null of OLS consistency. I reject consistency for outcomes one and two (p -value $< .01$) when using the full sample and for outcome three when splitting the sample into city council and mayor candidates (also p -value $< .01$). Since the vote distance to election cutoff is much smaller when votes are spread out across many candidates in proportional elections (city council) than in majoritarian elections (mayor), the asymptotic equivalence between OLS and IV parameters in row 3 is entirely reasonable (p -value = .17; fail to reject H_0). In other words, conviction variables fail to explain electoral outcomes measured in such a way and thus carry over low predictive power to their regression models.

These tests confirm instrument choice and substantially support inclusion restriction conditions for causal identification under an IV design. After the results section, I also conduct exclusion restriction tests to provide further support for the information effects on performance in this paper.

6 Results

Table 5 reports the effect of criminal information shortcuts (via trial conviction) on the probability of election of each politician. For mayor candidates, this variable turns on when the candidate was the most voted in their election. For city council candidates, this variable turns on when the candidate has received enough votes to finish the election within the number of vacancies in their municipality. For instance, if a municipality has 12 seats in its city council, a candidate who received the same number, or more, votes than the 12th placed candidate has outcome value one.¹² It is the most important outcome and directly tests the first theoretical claim suggested in section 4, that is, voters would impose electoral penalties using information cues about criminal activity; another way to state this is that convictions for electoral crimes signal candidate type and influence how voters make their decisions.

In columns 1-3 of table 5, I report the OLS estimates of the effect of information shortcuts via convictions at trial. The point estimates start at a 20.8 percentage point reduction on the probability of election but decrease to 16.3 percentage points in model 3, which includes candidate controls and fixed-effects. All effects are significant (p -value $< .01$). Therefore, regardless of the specification, there is a negative baseline relationship between revealing criminal information and performance. Unsurprisingly, the inclusion of covariates and fixed-effects soaks up some of the variation in the conviction variable, and controls for observed factors potentially correlated with convictions.

This biased result alone is interesting. It suggests that judicial decisions are also relevant infor-

upon request.

¹²City council elections are not necessarily decided in such manner; TSE tallies up all votes in a single election and divides them up by the number of seats available. All candidates who have more votes than this mark are automatically elected to office; remaining seats go to the coalitions who have rounded up more votes. Only rarely, however, all city councilors are elected like so. In most cases, votes are usually spread out across many candidates and coalitions, so being voted in as the last candidate within the number of available seats does guarantee their election and supports their coalitions to get further seats. In addition, this is a less strict way to define who is elected to city council such that, even if there are measurement errors in coding this outcome, the correct measurement would decrease the number of elected candidates and reinforce the conviction effect.

mation mechanisms influencing voter decision, a relationship yet undocumented in the literature. In Nicholson and Bowler (2019), there are many examples of information cues in the form of party labels, interest groups, or the media, but there are no examples of legal cues from court documents. This finding fills in an important gap in the literature by documenting the effect of disclosing information from a trustworthy source on voter behavior: in the U.S., people trust the judicial branch (68 percent) more than the executive (45 percent) and the media (41 percent) (Gallup, 2017, 2018); in Brazil, they trust the judiciary (24 percent) more than political parties (7 percent) and the executive (6 percent) (Ramos et al., 2017). The unbiased effect of judicial information, however, is harder to assess. First, though voters trust the judiciary more than other sources of information, the vocabulary and intricacies of legal cases might make it harder to interpret judicial decisions. For instance, voters might not understand the different electoral crimes and assign equal punishment to all convictions. Second, the information released in court documents is a function of the conditions of individual electoral races. Repeated candidates, or incumbents, are more exposed to legal action than new candidates. We explore this mechanism first, and address the interpretation of court documents in section 9.

A plausible hypothesis here is that some electoral races are more relevant than others and, as such, there is more competition for seats than otherwise. Candidates might even be less likely to play by the rules and bring many unfounded claims against their opponents in an attempt to disqualify them from the race. Alternatively, some races might have more skilled politicians than others. In any of these cases, the resulting claims against candidates are not randomly distributed, and are likely correlated with unobservable factors at each race to elected office. To partial out such confounding effects, I implement the instrumental variables strategy discussed in section 5, and report its results in columns 4-6. Note that all IV coefficients have significantly larger magnitudes than their OLS equivalents (again at the one percent level). They range from -27.2 to -23.1 percentage points in models 4 and 6, respectively. They suggest an upward bias in OLS estimates of about 6.2 (models 2 and 5) to 6.8 (models 3 and 6) percentage points; OLS predicts a smaller, weaker impact of information on performance. Along with the evidence of Hausman tests in section 5, these factors support IV consistency and its asymptotical convergence to the true, unbiased informational effect on voter response. For any given candidate, a conviction at trial alone would reduce their probability of election by 23.1 percentage points, according to my preferred model (column 6).

This result supports the negative information effect (suggested in section 4) and aligns with similar evidence in the literature. Ferraz and Finan (2008) report a smaller effect of seven percentage points for mayors when audit reports reveal corruption findings before elections in 2004. Though the effect here is larger for a less severe crime, the candidates in Ferraz and Finan (2008)’s sample are generally much more experienced than in this paper. The share of reelected mayors in Ferraz and Finan (2008) is 58.5, compared to 19.1 percent of experienced politicians in this sample, anecdotally suggesting that ability would indeed offset some of the negative information effects (see Winters and Weitz-Shapiro, 2013; Pereira and Melo, 2015).

In table 6, I report the results of the same regressions but on the vote share outcome. The OLS estimates are in columns 1-3 and show a negative and significant effect of criminal information on candidate's vote share, ranging from -12.9 to -9.9 percentage points. The IV effect is about 3.9-3.3 smaller than in the OLS. In my preferred model (column 6), the conviction effect significantly reduces vote share by 13.3 percentage points (p -value $< .01$). I should point out that the difference between OLS and IV parameters in this model is half the magnitude in the model for outcome one, which could create some skepticism about the marginal gain of using IV. The closer their magnitudes are, the smaller is the gain from trading bias (OLS) for consistency (IV). However, both the Hausman tests in section 5.1 and the fact that the 99 percent CIs around OLS and IV coefficients never overlap¹³ indicate IV as the best choice for measuring the information effect of electoral crimes.

The effect size on outcome two is larger than in comparable studies. Ferraz and Finan (2008) report a 10.4 percentage point decrease in vote share when mayors are running for reelection and have had corruption evidence released to the public prior to municipal elections in 2004. Chong et al. (2015) run an experiment before the municipal elections in three Mexican states in 2009 and find a 1.1 decrease in incumbent mayors' vote share when corruption information is revealed to the public. The differences in research design, however, explain why the effect is smaller in these studies. First, both Ferraz and Finan (2008) and Chong et al. (2015) are looking at the effect for incumbent politicians when there is evidence of corruption. These politicians are likely more skilled than the average and thus offset the negative impact of information with their ability. Second, they also only look at mayors, rather than city councilors, and the former have more visibility in local politics than the latter, which is another channel offsetting the negative impact of criminal information. When I reestimate the model in column 6 for the mayor-only sample (unreported here but available upon request), the information effect remains significant and negative but falls to 0.9 percentage points – marginally smaller than Chong et al. (2015). Thus, I have reason to believe the effect size here is consistent with a sample of less skilled local politicians.

I lastly investigate the information effect for outcome three, vote distance to election cutoff. This effect represents how much information disclosure helped getting away (or closer) to the number of votes needed for election. In this analysis, I split the sample into city council and mayor candidates because of the meaningful differences in each office race. Mayor elections follow majority rule; city council elections follow proportional rule. As such, the number of candidates is much smaller, and the votes needed for election much larger, in mayor elections. Therefore, the distance to election is not theoretically uniform across office type; in other words, a one percentage point distance is much harder to come by in city council rather than mayor races.

Table 7 presents the results. Columns 1-2 display OLS specifications and columns 3-4 display IV models. I only report regressions with individual controls and fixed-effects. I find that releasing information of conviction at trial has again a negative and significant effect (at the one percent

¹³OLS and IV pairwise 99 percent CIs are $(-11.9, -14.0)$ and $(-15.6, -18.0)$ for bivariate models; $(-7.5, -9.2)$ and $(-10.7, -12.8)$ for models including covariates; $(-8.6, -11.3)$ and $(-11.6, -14.9)$ for models including covariates and fixed-effects.

level) on the vote distance to the election cutoff across all models. For the city council sample, the IV coefficient points to 0.849 percentage point less in the distance to election than in the absence of a crime; for the mayor sample, this effect is 7.4 percentage points. Thus, candidates accused, and found guilty, of violating electoral law generally place further away from the necessary votes to guarantee election – in line with the impact of on outcomes one and two. Though the significance and direction of the effect align well with previous results, I am more skeptical about the effect size in table 7 compared to previous outcomes. The difference between OLS and IV parameters is much smaller, and their 95 percent CIs overlap. This means that trading off bias for consistency is not worth it for outcome three; with a sample size of 9,442 candidates, the OLS and IV coefficients perform equally well.

Despite my careful approach regarding the effect on outcome three, there is a robust, negative information effect on electoral performance across all models. The IV estimates in tables 5 and 6 point to the causal effect between disclosing judicial convictions for electoral crimes in two of the three performance measures. When different research designs are accounted for, these results align well with previous evidence in the literature. In the following sections, I conduct multiple robustness checks to support the negative, unbiased, and significant effect of judicial information.

7 Exclusion Restriction Checks

In section 5.1, I carried out four tests validating the inclusion restriction of convictions on appeal as an instrument for convictions at trial. In section 2, I also discussed how the TSE’s judicial review process qualifies for the exclusion restriction. Though there are no empirical tests for the exclusion restriction, I conduct two indirect checks which support my instrument choice: (i) coefficient stability tests following Altonji et al. (2005), Oster (2019), and Pei et al. (2019); and (ii) correlation tests including the instrument in the second stage.

7.1 Coefficient Stability Tests

The most common way to address omitted variable bias is to include controls in the regression of interest. In this paper, I repeatedly report parameter estimates progressively including candidate controls, party, municipal, and election fixed-effects. In many cases, however, the set of controls does not fully identify confounding effects. In fact, scholars rarely use the full set of confounding factors; instead, they use the *observed*, available confounders. Unless available variables fully capture the confounding set, selection on unobservables could still explain a significant portion of the parameters we are estimating in linear models.

Oster (2019) formalizes this point. She suggests that coefficient stability across regression models is only a reliable indication of unbiasedness if scaled by changes in the amount of regression variation explained by independent variables. In other words, the coefficient of interest should move relatively less than R^2 , indicating the stability of effect size as the researcher shifts explained variation from the error term to the matrix of independent variables. The following equation in

Oster (2019) translates this idea:

$$\beta^* = \tilde{\beta} - \delta \cdot [\beta^0 - \tilde{\beta}] \cdot \frac{R_{\max} - \tilde{R}}{\tilde{R} - R^0} \quad (7)$$

Where β^* is the bias-adjusted coefficient; $\tilde{\beta}$ is the coefficient in the unrestricted regression; β^0 is the coefficient in the restricted regression; \tilde{R} and R^0 are their respective R^2 . In this setting, we are interested in adjusting δ and R_{\max} such that we can test how $\tilde{\beta}$ fares against β^* : δ is the ratio of the regression variance explained by unobserved and observed controls; R_{\max} is the theoretical population variance explaining the outcome. Altonji et al. (2005) and Oster (2019) suggest that $\delta = 1$ is a reasonable threshold for coefficient stability, which means that unobservable and observed variables are equally able to explain $\tilde{\beta}$.

To support the exclusion restriction, I modify equation (7) such that I can compare across OLS and IV coefficients. Finding evidence that unobserved variation is unable to explain the shift in magnitude between OLS and IV coefficients means that the instrument does not add bias to regression results, and thus provides support for the exclusion restriction. To do this, I calculate δ under different R_{\max} assumptions to check how each δ fares against the unitary threshold proposed by Altonji and Matzkin (2005) and Oster (2019). Any $\delta > 1$ would mean excessive selection on unobservables is required for β_{iv} to be the biased effect, and not the true effect, of criminal information on performance. Rearranging equation (7) and assigning subscripts to coefficients, I estimate the following equation in table 8:

$$\delta = \frac{\tilde{\beta}_{iv} - \beta_{ols}^*}{\beta_{iv}^0 - \tilde{\beta}_{iv}} \cdot \frac{\tilde{R} - R^0}{R_{\max} - \tilde{R}} \quad (8)$$

I compute δ by varying R_{\max} in two ways: $R_{\max} = R_{ur}^2 + (R_{ur}^2 - R_r^2)$ and $R_{\max}^2 = 2 \cdot R_{ur}^2$. These magnitudes are listed in the top row of table 8. When the calculation of R_{\max} yields a nonsensical $R_{\max} > 1$, I truncate it at $R_{\max} = 1$. In column 3, I calculate the necessary R^2 for $\beta_{iv} = \beta_{ols}$ assuming $\delta = 1$. I do not truncate R^2 in column 3 to provide an indication of the degree of variation required to make β_{iv} converge to β_{ols} . Both of these validation exercises follow Oster (2019).

Except for outcome two, all δ 's are greater than one for $R_{\max} = R_{ur}^2 + (R_{ur}^2 - R_r^2)$ in column 1 of panels A and B. This means that the variance of unobservable variables would have to be more powerful than the variance of observed variables to explain β_{iv} , a hypothesis rejected in Altonji et al. (2005) and Oster (2019). The same is true when $R_{\max} = 2 \cdot R_{ur}^2$. Though the δ for outcomes one and two in panel A suggests selection is a problem, my preferred models come from panel B, where all δ 's are greater than one; the inclusion of the full set of fixed-effects represents a substantial improvement in observed variation and fixes potential selection in outcome one and two models. Therefore, I find no support for selection effects driving IV estimates, and this result serves as another indication in favor of the exclusion restriction for convictions on appeals.

7.2 Instrument Included in Second-Stage

Having confirmed the consistency of IV estimates, the second test in support of the exclusion restriction is relatively straightforward. I replace the endogenous variable (convictions at trial) for the instrument (convictions on appeals) in the second-stage regression, which is thus transformed in an OLS regression where the variable of interest is the ruling issued by the appellate panel. Such reduced-form approach is only recommended if we are able to reject OLS consistency in favor of IV – which is the case here. In figure 2, there are four panels, one for each of the outcomes discussed in section 6. In light gray, I plot the OLS coefficient on convictions at trial and its 99 percent CI for all outcomes and regression models, yielding a total of 12 estimates and CIs. I do the same for convictions on appeal, the reduced-form OLS model, and produce the same estimates and CIs (in black).

It is evident that the instrument is significantly correlated with the outcome of interest. No 99 percent CI includes zero. This is expected when the instrument passes inclusion restriction tests. The more important result, however, is the similarity of point estimates using either trial and appeals convictions. Except for vote distance in the mayor candidates sample, all point estimates of appeals coefficients fall inside trial CIs. The correlation between either variable, covariates, and fixed-effects is the same, meaning that the instrument is not adding any more variance to the trial regression. In other words, there is additional support for $cov(z, y) = 0$. Almost all of the effect of instrument z on outcome y occurs via its correlation with x . Along with the evidence in the previous section and the institutional design of judicial review of candidacy registration cases, I can confidently say that there is no independent effect of the instrument on the outcomes of interest.

8 Alternative Explanations

There are additional threats to validity beyond the inclusion and exclusion restriction. In this section, I first explore how strategic changes of judge sentencing behavior, voter engagement, or candidate campaigning behavior during the judicial review process could explain the information effect. If they are true, then what I am picking up is something different than voter punishment for candidate’s bad type. Secondly, I provide a final robustness check in which I show the validity of the IV effect even in the presence of weaker correlations between the endogenous variable and the instrument.

8.1 Heterogeneous Sentencing across Review Stages

The first alternative explanation to the criminal information effect is the potential change in judges’ sentencing behavior over case duration. Both elections and judicial review coincide in time, and judges could change how they sentence candidates based on campaign promises, policy positions, or even as a response to electoral results: judges might have a hard time issuing sentences preventing candidates who received the most votes from taking up office. To test this mechanism, I should provide indication that judicial decision-making factors are not differentially affecting trial and

appeals sentencing, i.e., that conviction signals are the same over the electoral period. Judges should be using the same criteria, and weighing them the same, when reviewing candidates' cases at trial and on appeal. To that end, I implement a modified version of Pei et al. (2019)'s covariate balancing test, which constitutes in regressing variables of interest on other covariates.

The test is as follows. First, I run two independent regressions with conviction variables on the left-hand side. The respective dependent variables are conviction at trial and on appeal. I report these regressions in columns 1 and 2 of table 9. Besides including the same covariates as before, I also include the main electoral outcome, whether the candidate had enough votes to take up office, on the right-hand side. It is the most important sentencing factor, as judges might be less willing to convict candidates once they know these people have had enough votes to take up office. I also include party-fixed effects, and cluster standard errors at the municipality-election pair level to account for the shared variation of standard errors at the judge level (each judge oversees one electoral district one election at a time). Next, I run a t -test on the difference between each parameter across regressions. Columns 3-6 respectively report the difference in coefficients, the joint distribution of standard errors, t -stats, and the p -value of each test. The null hypothesis is that the parameter difference is zero, meaning that the factor has an even effect on the trial and the appeals decision-making process.

The results in table 9 support homogeneous sentencing over the judicial review process. The difference in parameters is not statistically significant at any industry standard. No p -value is smaller than .20 (column 6). Moreover, it does not seem that being elected to office (outcome one) changes the way judges rule on a particular candidate's case; the difference of .044 is not significant (p -value = .61). This is strong evidence in favor of homogeneous sentencing, as judges do not seem to be changing their sentencing behavior over time. This is consistent with the institutional design of the electoral court system in Brazil. In local elections, trial rulings are issued by electoral district judges, who face both career and monetary incentives independent of local politics; appeals are decided by a panel of three judges at the state level, who are appointed to the electoral bench by fellow judges in state and federal systems, and the President of Brazil. Therefore, there is no evidence that heterogeneous sentencing would be driving the effect discussed in section 6.

8.2 Voter Disengagement Effect

A second source of concern is whether voters also change their behavior once they learn candidates' trial outcomes. While this could mean they change their votes for someone else (my hypothesis), this could also mean disengagement of the political process altogether. In this scenario, rather than punishing candidates for criminal behavior, voters would become frustrated by politicians. The mechanism behind the information effect would be disengagement rather than punishment (Pavão, 2018; Chong et al., 2015).

I cannot disentangle this effect by only looking at the main results from section 6. Instead, I have to look at other election outcomes to check for signals of disengagement. The first signal comes from voter turnout. If voters are frustrated with the electoral process, e.g., they believe

candidates are dishonest because they observe convictions for electoral crimes, then one plausible reaction is simply skip voting. Though Brazil has mandatory voting in place, the costs of not voting are negligible. Voters only have to fill out a no-show form, either online or in person. If they do not, they have to pay a \$1 fine. In this case, a decrease in voter turnout if the TSE convicts more candidates would be evidence of disengagement. Another related signal is the number of invalid votes in each municipal election. Voters could show up to the ballot but intentionally cast a blank vote or type in a non-existing candidate number in the electronic voting machine, both qualifying as invalid votes in Brazil. Thus, another evidence of disengagement could come from a higher number of invalid votes when there are more convicted candidates running for office.

I report the results of these tests in table 10, where I aggregate up voter turnout and invalid votes to the party and election-level. The most accurate aggregation of the conviction variables in this scenario would be the share of invalid candidacies over the total number of candidacies at each race. Since the ratio of invalid versus valid candidacies is very small overall,¹⁴ the variance of this new variable would also be very small, making it harder to reject the null between the new instrumental variable and the disengagement outcomes. I could mistake the null effect coming from low variation for the (true) null effect of no correlation. Instead, I want to make it easy to reject the null, for if I eventually reject the null I am more certain that the effect comes from no correlation rather than low variance. Therefore, I construct measures of the share of invalid candidacies, both at trial and on appeals, *over the total number of office vacancies in every election*.¹⁵ I then run IV regressions for each disengagement outcome and aggregation level. In columns 1 and 3, I find no relationship between convictions and voter turnout for both aggregation levels. This means that the number of invalid candidacies per party or election does not affect turnout. Voters still cast their ballots regardless of the number of invalid candidacies. In columns 2 and 4, the share of invalid candidacies has a significant and positive relationship with invalid votes. Voters still get out to vote, but once at the voting station, they cast more invalid votes when there are more convicted candidates (either per party or election). However, the effect size is almost meaningless. A one percentage point increase in the share of invalid candidacies by party only reduces invalid votes by 0.222 percent; similarly, one percentage point increase in the share of invalid candidacies by election only reduces invalid votes by 0.134 percent. Together, these results do not point to disengagement as the main driver behind the criminal information effect.

8.3 Candidates Quit Campaigning

The third confounding effect would come from campaign responses after candidates receive an unfavorable trial ruling. Candidates who receive such ruling might anticipate the eventual disqualification of their candidacy by the appellate panel such that they, partially or entirely, give

¹⁴I had an initial sample of 200,000 candidates for all elections and ended up with about 10,000 candidates after narrowing them down to candidates who could be voted for but had an outstanding appeal on election day.

¹⁵In mayor races, for instance, this means that the share of invalid candidacies might yield values greater than one for the simple reason that there is always just one open spot for mayor in each municipality. These tests place a higher bar for rejecting the voter disengagement hypothesis, providing yet more confidence in my results.

up campaigning. As a consequence, the hit to electoral performance would come from an effort rather than a punishment effect. I believe this to be a minor problem for the simple reason that my sample only contains candidates who remained engaged in their race to office – evidenced by their filing of an appeal against their trial sentence. This is a limitation of the instrumental variables design more generally, and it is likely that the candidates in this sample are not the same as the population of candidates in local elections in Brazil, but it stills fends against claims of candidate disengagement. Second, any strategic candidate believing they have a shot at election would do well to keep campaigning since the judicial penalty (dismissal of candidacy) is small compared to the benefit of holding office. Candidates would be willing to take a gamble with small risk but high reward.

Nevertheless, I provide anecdotal evidence to dismiss concerns about candidates making such large shifts in their campaign strategies. In table 11, I compare the mean campaign expenditure across various subgroups of candidates. Campaign expenditures do not fully capture the extent to which campaigns are adjusted, but are a good proxy to understand campaigning behavior. I first compare mean spending for candidates by the type of trial and appeals ruling they receive. The mean spending of candidates with favorable rulings at trial (R\$84,766, or ~\$22,000) is much higher than candidates with unfavorable rulings (R\$34,497, or ~\$8,600). The same is true for outcomes on appeals. I can thus anecdotally claim that there is an association between campaign spending and judicial outcomes, which is not surprising; for precisely this reason, I control for campaign spending in all regressions of this paper. The more interesting result, however, is the bottom row of table 11. For the subgroup of candidates who received an unfavorable ruling at trial, campaign spending is not associated with better outcomes on appeal. Campaign spending is statistically the same (p -value = .961), and their respective means are R\$34,346 and R\$34,527. Unfortunately, I cannot observe expenditure dates, which would be a better proxy for campaign engagement, but the indirect evidence here is that candidates who needed to reverse a trial ruling have not differently spent money on their campaigns. If they had spent more, we would see an opposite engagement effect trying to revert unfavorable rulings. If they had spent less, we would find evidence in favor of candidate disengagement. None of these explanations apply, and this is additional evidence that the main effect can be attributed to voter punishment.

8.4 IV Estimate is Contingent on Small Portion of Data

Though the inclusion restriction tests support convictions on appeal as instruments for convictions at trial, a final source of concern is that the instrument is *too strongly* correlated with the endogenous variable. A simple, unconditional Pearson correlation check yields a .796 correlation across conviction types. The appellate decision estimate in the first-stage regression from table 3, column 3, returns a .738 correlation in the presence of all covariates and fixed-effects. For three-quarters of the sample, the distribution of trial outcomes is fully identified by the distribution of appeals outcomes. A valid concern, therefore, is whether the difference in estimates across OLS and IV is only coming from a small portion of the data for which trial outcomes are reversed, – 17.2 percent

of the whole sample.

To address this concern, I investigate how IV estimates change under different correlation coefficient assumptions for the first stage. The reasoning is straightforward. Suppose I have an unobserved distribution of correlations between convictions at trial and on appeals, ranging from 0 to 1. For a weak enough correlation p_{weak} , the instrument is weak and does not meet inclusion restriction criteria. I cannot identify causal effects. Alternatively, for an identity correlation $p = 1$, the instrument becomes meaningless as the endogenous variable and instrument are co-linear. Similarly, I cannot identify causal effects. However, there is a pair of correlations $(p_{\text{low}}, p_{\text{high}})$, such that $\{p_{\text{weak}} < p_{\text{low}} \leq p_{\text{high}} < 1\}$, for which the instrument both meets the inclusion restriction and is a consistent estimator of the causal effect of interest. Since the empirical draw of β_{iv} has a high correlation with the endogenous variable, and is asymptotically consistent, I can simulate effect sizes for the interval of strong instruments with correlations starting at p_{low} and ending at $p_{\text{high}} = p_{\text{instrument}} = .738$, and compare them to the observed OLS coefficient. If there is no overlap between CIs of OLS and the simulated IV parameters, then I am more confident that the IV effect is the true causal effect.

In practical terms, the simulation starts with the random draw of a correlation coefficient between $p_{\text{weak}} = 0$ and $p_{\text{instrument}} = .738$ (empirical value), which I call $p_{\text{simulated}}$. I then create a simulated instrument with the same mean and standard deviation of the original variable, but whose correlation with the endogenous variable is $p_{\text{simulated}}$. Next, I run first and second-stage regressions using the simulated instrument, and store all coefficients, standard errors, and first-stage F -statistics.¹⁶ I repeat this process 10,000 times to recover the entire distribution of correlations between 0 and .738 and their simulated IV coefficients. After this process, I discard all weak instruments and their estimates using the Bound et al. (1995) cutoff of $F = 10$. The remaining values correspond to the distribution of correlation coefficients for strong instruments $\{p_{\text{low}}, .738\}$, plotted on the y -axis of figure 3, and their effect size β_{iv} , plotted on the x -axis.

The first important result is no overlap between OLS and IV 95 percent CIs. The strong instrument IV parameters have a mean of $-.231$ with a standard error of $.018$. It is the same effect but larger standard error than in the empirical distribution (s.e. = $.016$). Thus, I again confirm the consistency of the IV estimate in the empirical sample. Second, and most importantly, the IV estimate remains statistically significant even when the correlation coefficient, measured by the first-stage coefficient between conviction at trial and on appeals, drops to $.525$. That is a 28 percent drop in correlation. It means that a third more of the affirmed trial decisions would have to be overturned to render the IV effect inconsistent – an unrealistic high number of reversals. To put this in perspective, the overall reversal rate in this sample is 17.2 percent; the highest reversal rate in the U.S. is 14.1 percent (U.S. Courts, 2018).¹⁷ It is yet another evidence in support of the consistent, unbiased IV effect of crimes on electoral performance.

¹⁶I only use the most important outcome, the probability of election, in this test.

¹⁷Surprisingly, the Brazilian judiciary system has little information on reversal rates. The last official report came out in 2009 and did not have any information on reversal rates at the Electoral Court system at the time of publication.

9 Heterogeneous Electoral Punishment

Besides the primary effect, I want to identify whether voters impose heterogeneous punishment by information type. This is called the *crime type* effect. In an ideal world, substantial electoral code violations should be met with harsher electoral penalties than procedural violations. While it makes sense to expect that punishment should fit the crime, the literature is filled with cases in which this is not true. Voters might not punish candidates as expected because they trade off crimes for public goods (Pereira and Melo, 2015); they might not trust the source of information on criminal behavior, or might not understand the information being disclosed (Winters and Weitz-Shapiro, 2013; Weitz-Shapiro and Winters, 2017); they might even lack options, and end up voting for the *least* dishonest candidate (Pavão, 2018). None of these studies, however, looks at the disclosure of legal information in the form of judicial decisions. This information might be more credible but also harder to understand.

To estimate such effect, I collect and code court documents for all candidates running for municipal office in 2012 and 2016, and a few for 2004 and 2008. For 2016, I also have official TSE classification of conviction types.¹⁸ There are eight conviction categories, and I group them in 4 classes: violations to the Law of Elections (1995), the Law of Political Parties (1997), the Clean Slate Act (2010), and documentation problems. Next, I use a linear support-vector machine classification model in which every unigram and bigram in each sentence is used to predict conviction classes. Once the classification algorithm has been trained on 2016 data, I apply the model to predict classes in earlier elections, *which were not disclosed by the TSE*, in a standard application of supervised machine learning.¹⁹ To guard even further against sentence misclassification, I group the first three classes as *substantial* rule-breaking and the last class as *procedural* rule-breaking. It is arguably the most consistent distinction in sentence type and is well-suited for identifying heterogeneous effects. Procedural rule-breaking are cases in which candidates lack a trivial requirement to run for office, such as missing documentation, not meeting deadlines, or not having submitted notarized documents. For instance, they could have forgotten to include a copy of their ID card in their application, or they could have missed a deadline in the application process. Substantial rule-breaking are more severe cases in which either parties or candidates are in breach of more substantial elements of electoral law, such as using illegal campaign strategies, channeling slush funds for campaign ads, having previous outstanding judicial convictions preventing them from running for office. These crimes come from the legislation under the jurisdiction of the electoral court: the Electoral Code of 1965, 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.

I test the crime type effect by including b_i in equation (5). The parameters describing each relationship are the conviction effect (ρ_0), the crime type effect (ρ_1), and their interaction (ρ_2).

¹⁸Though documents are public, the TSE had many errors in the system that prevented me from downloading almost all case files for 2004 and 2008.

¹⁹I discuss this classification algorithm in more detail in appendix A.

Equation (9) summarizes these relationships:

$$y_i = \alpha + \rho_0 \cdot \hat{c}_{i,\text{trial}} + \rho_1 \cdot b_i + \rho_2 \cdot \hat{c}_{i,\text{trial}} \times b_i + X\beta + \sum \lambda_{i,k} + \varepsilon_i \quad (9)$$

Note that, in the absence of the conviction at trial, ρ_1 is just the raw association between breaching electoral law and electoral performance, and represents the potential gain of engaging in an activity prohibited by law. To make the interpretations clearer, I lay out the four alternative explanations of the joint effect of conviction and crime type in table 12: if $\rho_1 = 0$, engaging in an electoral crime has no electoral payoff; if $\rho_1 > 0$, engaging in an electoral crime has a positive electoral payoff; if $\rho_2 = 0$, voters punish candidates the same, regardless of the accusation against them; if $\rho_2 < 1$, voters fit punishment to the crime; in other words, substantial violations receive larger electoral penalties.

Table 13 reports results of the heterogeneous information effect by crime type. The first result that stands out is that the criminal information effect ρ_0 remains significant and negative. When compared to the outcomes in the primary results section, one also notices that the magnitude falls by approximately as much as the electoral gain of substantially breaching electoral law. For instance, in the main results, the probability of election falls by 23.1 percent when crimes are disclosed by the electoral court, which is very close to the subtraction of electoral gain (4.7 percentage points) from the information effect in table 13: $-17.6 - 4.7 = -22.3$. The same is true for vote share: the main effect of -13.1 is very close to the subtraction of ρ_1 from ρ_0 : $-7.4 - 4.9 = -12.3$. These results provide strong support for the offsetting, positive electoral effect of substantially breaching electoral law. I fail to reject, however, the homogeneous punishment, regardless of conviction type. The ρ_2 effect is only significant in the model for outcome two, which is not strong enough of an evidence to claim that voters fit electoral punishment to crimes. Therefore, the most suited explanation to the heterogeneous effect is in the upper right quadrant of table 12. While there is an electoral gain of engaging in substantial electoral rule-breaking, voters are not sophisticated enough to discern between these types of information. Beyond the concerns expressed by Weitz-Shapiro and Winters (2017) regarding the credibility of the source of information, these results also point to yet another dimension of voters response to crime disclosure: information complexity. Voters might be discerning credible from poor sources of information, and punishing politicians for it, but the complexity of a judicial ruling might still represent a barrier to the proper level of punishment. Addressing this issue is beyond of the scope of this paper, but should prove a fruitful avenue of future research.

10 Conclusion

This project aims at uncovering the effect of electoral crime information on performance. Supplemental to existing literature looking at severe electoral fraud and electoral malfeasance in non or partially democratic regimes, I provide evidence on less known, less understood electoral practices that can also shape the results of elections. I also suggest that judicial information is an important

cue for voters in addition to other information shortcuts documented in the literature (party labels, interest groups, and the media, for example). I measure the information effect in Brazil, one of the largest and highest quality electoral democracies in the world at the time of the study. Further contributions of this paper are the use of court documents as data and causal identification using the institutional design of judicial review in the Brazilian electoral court system.

I find substantial and significant negative effects of criminal information on electoral performance. Being convicted of an electoral crime reduces the probability of election for mayor and city council candidates in Brazil by 23.1 percentage points. It also reduces vote share by 13.3 percentage points. These results are robust to a series of checks on the inclusion and exclusion restriction, and to strategic changes of judge, voter, and candidate behavior. Contrary to expected, I find that voters do not impose differential punishments conditional on the severity of the released information. There is a positive, independent effect of engaging in substantial rule-breaking on performance, about 4.8 percentage points both in the probability of election and vote share, but when interacted with the information effect, the type of crime does not impose additional punishment beyond what the conviction had caused. I suggest that information complexity is another dimension influencing voter punishment decisions which should be explored in future research.

This study is relevant for multiple policy reasons. I offer additional evidence claiming the existence of a negative relationship between electoral crime information and electoral performance beyond just corruption. Knowing that voters punish bad behavior, skilled politicians and policymakers can increase monitoring, detection, and prosecution of electoral crimes as a means of weeding out low-quality office-seeking candidates. A final implication of this project is a discussion on the effectiveness of electoral oversight authorities in the first place, a feature shared by other developing countries such as Mexico, India, and South Africa. While on the one hand they might prevent low-quality candidates from running, and eventually, being elected, they might simultaneously create barriers to entry that are detrimental to political competition and to the democratic process in developing countries.

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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
Total Vote Share (in p.p.)	9,442	10.13	17.98	0	100.00
Vote Distance to Election Cutoff (in p.p.)	9,442	-4.09	9.55	-92.82	12.83

Table 2: Electoral Crime Rulings

<i>Trial</i>	<i>Appeals</i>		Percent
	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*** (.006)	.753*** (.007)	.738*** (.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

Figure 1: Instrument Point Estimates and CIs

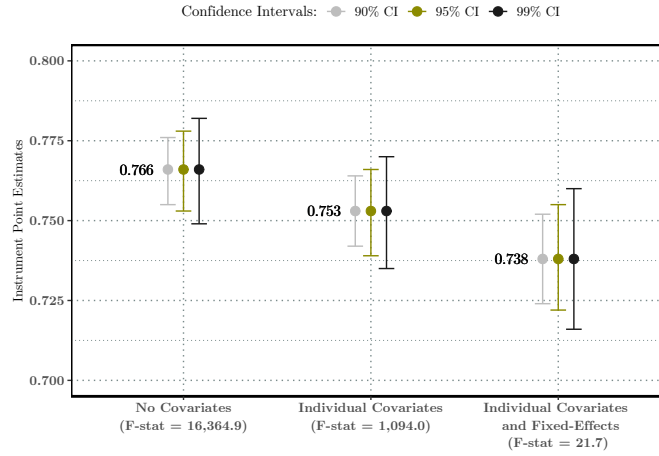


Table 4: Hausman Test of Instrument Strength

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

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

	Outcome: Probability of Election					
	OLS (1)	OLS (2)	OLS (3)	IV (4)	IV (5)	IV (6)
Convicted at Trial	-.208*** (.009)	-.151*** (.009)	-.163*** (.014)	-.272*** (.011)	-.213*** (.010)	-.231*** (.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 (1)	OLS (2)	OLS (3)	IV (4)	IV (5)	IV (6)
Convicted on Appeal	-12.945*** (.418)	-8.316*** (.337)	-9.943*** (.529)	-16.804*** (.478)	-11.765*** (.399)	-13.254*** (.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 (1)	IV (2)	OLS (3)	IV (4)
Convicted at Trial	-.575*** (.064)	-.849*** (.075)	-5.172*** (1.905)	-7.381*** (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 Tests: δ 's and R^2 for $\beta_{ols} = \beta_{iv}$

	Panel A: Individual Covariate Models			Panel B: Individual Covariate and Fixed-Effects Models		
	(1)	(2)	(3)	(1)	(2)	(3)
	$R_{ur}^2 + (R_{ur}^2 - R_r^2)$	$2 \cdot R_{ur}^2$	R^2 for $\beta_{ols} = \beta_{iv}$	$R_{ur}^2 + (R_{ur}^2 - R_r^2)$	$2 \cdot R_{ur}^2$	R^2 for $\beta_{ols} = \beta_{iv}$
Probability of Election	1.05 (.23)	0.63 (.29)	- (.46)	1.69 (.96)	1.49 (1.00)	- (3.07)
Vote Share	0.68 (.64)	0.48 (.74)	- (.99)	2.05 (1.00)	2.05 (1.00)	- (3.01)
Vote Distance to Cutoff (City Councilor)	7.74 (.21)	6.05 (.23)	- (2.11)	20.51 (1.00)	20.51 (1.00)	- (24.86)
Vote Distance to Cutoff (Mayor)	2.64 (.23)	1.56 (.29)	- (.64)	1.21 (1.00)	1.21 (1.00)	- (1.51)

Note: In each panel, I compare the unrestricted coefficient for the model in the panel title ($\tilde{\beta}$) against the restricted coefficient for the bivariate model (β^0). The different outcomes are summarized across rows. Columns 1 and 2 in each panel display conditions for R_{\max} calculations in the row just above table content. The first value in each cell is the δ for each model. R^2 values are reported inside parentheses. While I cap R^2 at one for δ calculations, I do not cap it for calculations of the necessary R^2 to yield $\beta_{ols} = \beta_{iv}$; therefore, some nonsensical $R^2 > 1$ appear in column 3 in each panel.

Figure 2: Instrument Correlation with Covariates

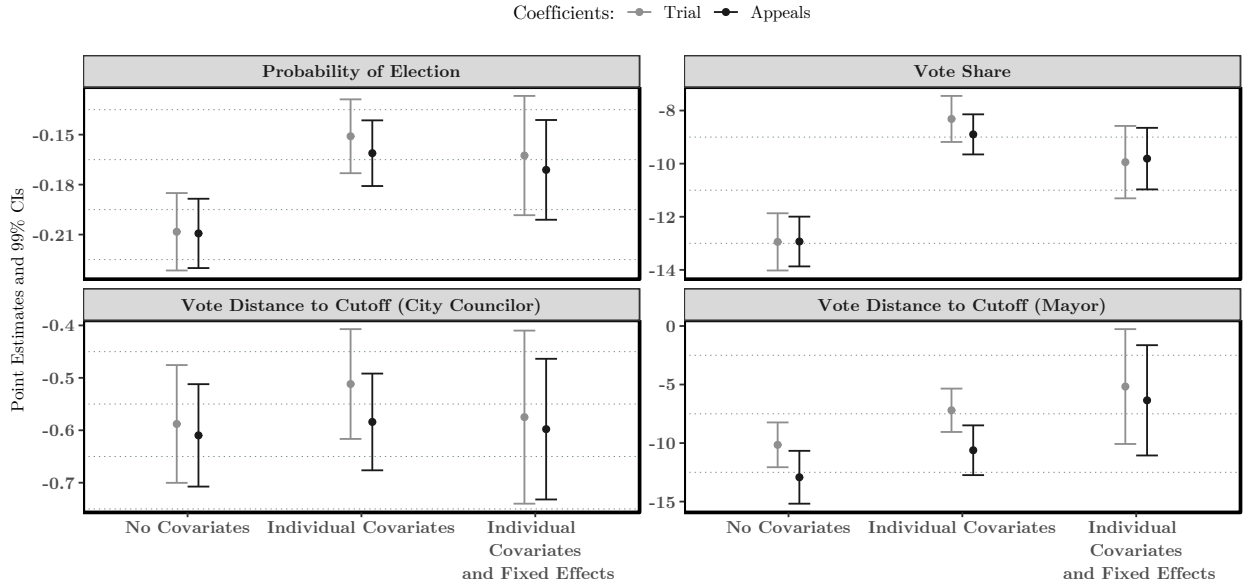


Table 9: Heterogeneous Sentencing across Trial and Appeals

	(1)	(2)	(3)	(4)	(5)	(6)
	β_{trial}	β_{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 $H_0: \beta_{\text{difference}} = 0$ 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.

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 (.007)	.222*** (.076)	-.001 (.009)	.134* (.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

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

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

Figure 3: Simulation of IV Point Estimates

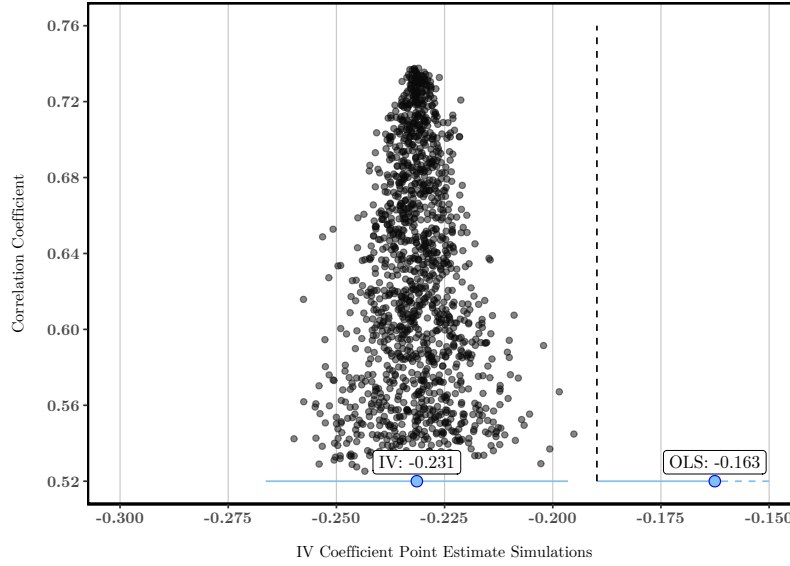


Table 12: Voter Sophistication and Benefit of Rule-Breaking

ρ_1 : Substantial Violation		
ρ_2 : Convicted at Trial \times Substantial Violation	$\rho_1 = 0$	
	$\rho_2 = 0$	
	1. Violation carries no electoral benefit. 2. Voters impose same penalty for different electoral violations.	1. Violation helps candidate get elected. 2. Voters impose same penalty for different electoral violations.
	1. Violation carries no electoral benefit. 2. Voters impose harsher electoral penalties for substantial violations.	1. Violation helps candidate get elected. 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*** (.020)	−7.369*** (.719)	−.713*** (.084)	−6.653*** (2.101)
Substantial Violation	.047** (.024)	4.939*** (.723)	.089 (.103)	.169 (1.524)
Convicted at Trial × Substantial Violation	−.014 (.028)	−4.952*** (.915)	.015 (.111)	1.644 (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 A). 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

A Appendix: Electoral Ruling Classification

(TBU)