Do Revelations of Political Corruption Erode Citizens' Compliance with the Law?

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Abstract

This paper examines whether revelations of political corruption erode citizens' compliance with the law. I study Brazil's *Programa de Fiscalização em Entes Federativos por Sorteios Públicos*, which randomly audited municipalities and publicly disclosed cases of corruption, to test whether such revelations influence everyday law-breaking. Focusing on detailed records of traffic violations, I find no evidence that the disclosure of corruption cases affects citizens' compliance with traffic laws, regardless of case severity or infraction type. Moreover, these revelations do not appear to shift social norms around traffic behavior or perceptions of the costs of breaking rules. These findings suggest that, at least in the domain of traffic laws, adherence is shaped primarily by individual safety considerations and broader social and cultural factors rather than by political corruption.

Keywords: corruption, social norms, civic values, culture

JEL Classification: K42, D72, H70, Z13, O17

1 Introduction

Corruption can influence citizens' behavior by fostering dishonesty and eroding respect for rules and laws. Recent empirical evidence supports this connection: after corruption scandals, students are more likely to cheat on exams (Ajzenman, 2021), and supermarket shoppers are more inclined to steal (Gulino and Masera, 2023). Yet, an important question remains: can exposure to corruption cases drive changes in individual behavior that endanger both oneself and others, such as an increase in traffic violations?

The literature on traffic behavior highlights a variety of factors that influence the propensity to commit traffic violations, with social norms (Carter et al., 2014; Cestac et al., 2014; Fernandes et al., 2010; Mawanga and Ntayi, 2010) and the perceived cost of rule violations (Delavary Foroutaghe et al., 2020; Killias et al., 2016; Moolenaar, 2014; Nazif-Munoz et al., 2022) standing out as particularly influential. If the disclosure of political corruption undermines social norms related to law-abiding behavior[1] and/or shifts individuals' perceptions of the likelihood or severity of punishment and enforcement[2], an increase in traffic violations would be expected following such disclosures.

Moreover, to the extent that traffic-related deaths are, at least partly, determined by driving behavior, societies with lower compliance with traffic laws are more likely to have higher road deaths rates. Figure 1 shows a negative correlation between traffic-related deaths and the rule of law index[3] across countries: nations with higher rates of road fatalities,

[1]Theoretical contributions on the transmission of social norms through leadership (Acemoglu and Jackson, 2015; Tabellini, 2008) suggest that citizens may internalize and adopt the norms reflected in the behavior of their leaders. Empirically, Ajzenman (2021) provides evidence that exposure to political corruption wakens social norms related to honesty among secondary school students.

[2]A large body of research in political science document a negative correlation between corruption and confidence in public institutions (Anderson and Tverdova, 2003; Catterberg and Moreno, 2006; Chang and Chu, 2006; Clausen et al., 2011; Mishler and Rose, 2001; Seligson, 2002). These studies suggest that in contexts characterized by high levels of political corruption, citizens are less likely to trust authorities to uphold the rule of law. Consequently, individuals may become more inclined to circumvent laws and regulations, perceiving institutions as ineffective or biased.

[3]The rule of law index considers eight factors: constraints on government powers, absence of corruption, open government, fundamental rights, order and security, regulatory enforcement, civil justice, and criminal

indicative of lower compliance with traffic laws, tend to have weaker rule of law. However, does this observed negative correlation between compliance with traffic laws and corruption imply a causal relationship?[4] This question is particularly relevant for the developing world, which accounts for over 85% of all global road traffic fatalities (Nantulya and Reich, 2002), and where corruption levels remain relatively high (Olken and Pande, 2012).

In this paper, I empirically investigate whether the disclosure of corruption cases causally influences the propensity to commit traffic offenses. To address this question, I exploit Brazil's anti-corruption plan, *Programa de Fiscalização em Entes Federativos* (Monitoring Program with Public Lotteries), which exposed corruption cases at the municipal level, to examine their potential impact on citizens' compliance with traffic laws. By using traffic offense data from the *Polícia Rodoviária Federal* (Federal Highway Patrol; PRF), I find no evidence that corruption disclosure leads to an increase in traffic violations. This result is robust to the inclusion of different sets of controls (municipality fixed effects, time fixed effects, time-varying municipality characteristics relevant for driving behavior such as weather variables), categories of infractions, and type of model. Moreover, the main result remains consistent regardless of the magnitude of corruption cases.

In my empirical strategy, I exploit the random assignment of municipal audits and the program's staggered implementation to estimate a Difference-in-Differences (DiD) model. Random audit assignment ensures comparability between audited and non-audited municipalities, while the timing of audits, beyond municipal control, provides exogenous treatment variation. To address concerns about staggered DiD designs (De Chaisemartin and d'Haultfoeuille, 2020; Goodman-Bacon, 2021; Borusyak et al., 2024), I use a stacked-by-event approach (Cengiz et al., 2019; Deshpande and Li, 2019), that focuses on comparisons between audited and non-audited municipalities. As a robustness check, I also apply the al-

justice. A higher value of this index represents a stronger adherence to the rule of law.

[4] This observed correlation aligns with the findings of Fisman and Miguel (2007), who report that diplomats from high-corruption countries accumulated a greater number of unpaid parking violations.

ternative estimator proposed by Callaway and Sant'Anna (2021) and obtain nearly identical estimates.

Previous literature on the role of political corruption in shaping citizens' behavior has highlighted social learning as a potential channel, wherein individuals observe the behavior of their leaders and subsequently adjust their perceptions of social norms and their intrinsic values (Ajzenman, 2021). Building on this hypothesis, I examine potential heterogeneities based on drivers' age and local media availability. If political corruption induces a process of social learning, we should observe a differential effect on municipalities with local media outlets (where there is potentially a greater exposure to local corruption news) and an effect more pronounced on older drivers, likely more exposed to political discussion. However, I do not find heterogenous effects along any of these dimensions. An alternative mechanism suggests that corruption scandals may influence driving behavior by altering the perceived costs of breaking traffic rules—both material, such as the likelihood of receiving a fine, and social, including the risk of public punishment (Gulino and Masera, 2023). If this were the case, we would expect to observe differential effects in municipalities where corrupt officials faced no legal consequences and in municipalities where corrupt politicians were reelected in the subsequent election. However, my findings show no significant differences across these dimensions. These results suggest that political corruption neither shapes citizens' perception of social norms related to traffic rules nor influences their perceived costs of violating them. Instead, adherence to traffic rules is more likely driven by individual safety preferences and broader social and cultural factors rather than political corruption.

Finally, I examine and present evidence against alternative explanations for the absence of a causal relationship between corruption and traffic offenses. One possibility is that citizens were either unaware of the corruption cases disclosed by the anti-corruption program or that these disclosures did not provide new information. However, evidence suggests otherwise. Using the same corruption data from Brazil's anti-corruption initiative, Ferraz and Finan

(2008) show that when corruption was revealed, the likelihood of a mayor's reelection fell significantly. This indicates that citizens were indeed informed about the cases and cared about them, actively punishing corrupt politicians at the polls. Moreover, analysis of a national survey on corruption perceptions shows a striking shift: in 2001 (before the program began), citizens generally did not perceive municipal authorities as corrupt, whereas by the program's end, they overwhelmingly reported high levels of corruption among local officials.

A second alternative hypothesis is that municipal corruption may foster police corruption, leading to an increase in unrecorded interactions between law enforcement and citizens during traffic violations. This would mean no significant change in registered infractions after corruption scandals, even if corruption increased traffic violations. Offenses relying on police intervention, such as driving under the influence, might be affected, while speeding, detected by fixed cameras without police involvement, would remain unchanged. I provide empirical evidence that, as with total traffic offenses, there is no causal relationship between corruption and speeding offenses, suggesting that police behavior remained unchanged following revelations of corruption.

Another plausible hypothesis is that corruption disclosures could affect federal resource allocation, potentially leading the federal government to reduce funding for corrupt municipalities, thereby limiting police presence and contributing to underreporting of traffic violations. This response might vary based on political alignment, with non-aligned municipalities possibly facing greater resource cuts. My analysis shows that the main findings are unaffected by political alignment, suggesting that the federal government did not alter resource allocation in response to corruption revelations.

This paper builds on previous research investigating the impact of public figures, leaders, and public events on various aspects of behavior and decision-making, including social norms (Acemoglu and Jackson, 2015), honesty (Ajzenman, 2021), political preferences (Dippel and Heblich, 2021), trust (Ananyev and Guriev, 2019; Depetris-Chauvin et al., 2020), unethical conduct (d'Adda et al., 2017; Garz and Pagels, 2018; Gulino and Masera, 2023), reproductive health and family planning preferences (Bassi and Rasul, 2017; Stroebel and van Benthem, 2012), and racial bias (Grosjean et al., 2021). My findings contribute to this literature by showing that revelations of corruption do not substantially affect compliance with traffic laws and regulations, as such compliance is likely rooted in individual preferences towards safety and broader social and cultural factors.

2 Background

Between 2003 and 2015, Brazil's federal government implemented an anticorruption initiative known as the *Programa de Fiscalização em Entes Federativos* (Monitoring Program with Public Lotteries). This program, based on random audits of municipalities' use of federal funds, was managed by the *Controladoria-Geral da União* (Office of the Comptroller-General; CGU), an autonomous agency with ministerial rank dedicated to combating corruption at various levels of Brazil's administration.

Municipalities were randomly selected for audits through public lotteries held alongside the national lottery in Brasília, targeting those with populations under 500,000. From the 24th lottery onward, state capitals were excluded. Initially, 5 and 26 municipalities were selected; later lotteries drew 60 each. By February 2015, 40 lotteries had resulted in 2,241 audits across 1,913 municipalities; most audited once, with a few audited multiple times. In the program's final years, only one lottery was held annually.

During each audit, the CGU reviewed federal transfers from the previous 3–4 years and inspected a random subset of projects. Auditors, competitively selected and well-compensated (Avis et al., 2018), conducted audits that typically lasted ten days (Ferraz and Finan, 2008). Full reports were submitted to Brasília, while summaries were posted online and shared with the media. Results became public 6 to 12 months after selection.

Over time, the program introduced minor adjustments (such as limiting sectors inspected and spacing out repeat audits) but maintained the same audit probability by state. Randomization was conducted at the state level, with smaller states having 1-2 municipalities selected per lottery and larger states around 10.

3 Data

To evaluate the corruption cases uncovered by Brazil's anti-corruption plan, I use CGU data[5] on municipalities randomly audited between 2003 and 2015, along with detailed municipality-lottery data from Avis et al. (2018)'s publicly available datasets covering lotteries 22-38 (July 2006-March 2013). Hence, my main sample spans July 2006 to January 2014.

Figure 2 illustrates the distribution of corruption-related irregularities per service order (specific projects randomly selected to be inspected) in municipalities audited between lotteries 22 and 38. As shown in Figure 2, every audit revealed at least one irregularity associated with corruption. Therefore, since audits were randomly assigned, the corruption cases can be considered as randomly disclosed within the sample of municipalities eligible for auditing in lotteries 22–38. On average, auditors identified 2.7 acts of corruption per service order, with a maximum of 8 corrupt acts uncovered in a single order.

Data on traffic offenses comes from the official website of the *Polícia Rodoviária Federal* (Federal Highway Patrol; PRF).[6] Traffic violations offer a meaningful behavioral outcome to assess whether political corruption affects law-abiding behavior. The literature on traffic behavior identifies several key factors influencing individuals' propensity to violate traffic laws, particularly social norms (Carter et al., 2014; Cestac et al., 2014; Fernandes et al., 2010; Mawanga and Ntayi, 2010) and the perceived costs of rule-breaking, such as the likelihood

[5] Available at https://www.gov.br/cgu/pt-br/acesso-a-informacao/dados-abertos/arquivos/fiscalizacao-em-entes-federativos

[6] Available at https://www.gov.br/prf/pt-br/acesso-a-informacao/dados-abertos/dados-abertos-da-prf

or severity of punishment (Delavary Foroutaghe et al., 2020; Killias et al., 2016; Moolenaar, 2014; Nazif-Munoz et al., 2022). If the public disclosure of political corruption erodes trust in institutions or weakens social norms around compliance, we would expect to observe a rise in traffic violations as individuals adjust their behavior in response.

The datasets from the PRF records all traffic offenses detected by the federal police on federal roads since January 2007, including the time, date, location, type of offense, and vehicle details. Merging this with information on corruption cases disclosed by Brazil's anti-corruption program, I construct a monthly panel of municipalities. The analysis focuses on municipalities never audited and those audited only once through lotteries 22 to 38.[7] The final sample includes 1,795 municipalities observed over 85 months (Jan 2007-Jan 2014)[8], with 285 in the treatment group, where corruption was uncovered, and 1,510 in the control group.

Table 1 presents summary statistics for all infractions, both in aggregate and disaggregated by type, across all municipalities and by treatment status. On average, municipalities report 1.3 infractions per 1,000 inhabitants per month. Among the classified traffic offenses, the most common for both the treatment and control groups are speeding (0.2 infractions per 1,000 inhabitants per month) and illegal driving (0.4 infractions per 1,000 inhabitants per month).[9]

This paper uses four additional data sources. First, publicly available data from Avis et al. (2018) provide information on CGU-Federal Police crackdowns and mayoral convictions, including yearly indicators for crackdowns, arrests, and convictions. Second, political

[7]According to Figure 2 in all of these municipalities at least one case of corruption was disclosed after the audits.

[8]The PRF has jurisdiction over 2,211 municipalities (PRF, 2022). Among the municipalities randomly selected for auditing, 416 lack PRF presence and therefore provide no traffic offense data; these were excluded from the analysis. Although the resulting sample covers only a subset of Brazilian municipalities, their observable characteristics closely mirror those of the average municipality nationwide (see Appendix Table A.1).

[9]Illegal driving includes offenses such as tailgating, swerving, failing to yield or stop, illegal U turns, among others.

outcomes, such as reelection and partisan alignment, are drawn from the *Tribunal Superior Eleitoral* (National Electoral Court) for 2000–2012. Third, demographic and socio-economic variables come from the 2000 Census conducted by the *Instituto Brasileiro de Geografia e Estatística* (IBGE; Brazilian Institute of Geography and Statistics), including population, urban share, youth share, education, and income. Fourth, municipal infrastructure data (e.g., presence of universities, radio, and TV stations) comes from the *2005 Perfil dos Municípios Brasileiros* (obtained from the IBGE), and weather data from the National Institute of Meteorology.

Appendix Table A.2 compares audited and non-audited municipalities in the sample. Although randomization occurred nationwide, the sample in this study only includes municipalities with traffic offense data. Results show that audit selection remains largely exogenous; differences in characteristics (e.g., income, youth share, temperature) are mostly insignificant, supporting the assumption that both groups are similar in observable and likely unobservable traits.

4 Empirical Strategy

My identification strategy relies on the staggered implementation of audits across municipalities. Two key factors make the anti-corruption program suitable for identification. First, audits were randomly assigned to municipalities, ensuring that audited and non-audited municipalities are, on average, comparable. Second, municipal authorities had no control over the timing of auditor appointments or the number of service orders, making the treatment timing plausibly exogenous. This design enables a comparison of the changes in traffic offenses over time between municipalities audited earlier and those audited later or never audited.

To address the staggered treatment assignment, I employ a stacked-by-event design for estimation (Cengiz et al., 2019; Deshpande and Li, 2019). This method mitigates the recently identified challenges of two-way fixed-effects estimators in staggered adoption scenarios (De Chaisemartin and d'Haultfoeuille, 2020; Borusyak et al., 2024; Goodman-Bacon, 2021). I further demonstrate the robustness of my findings using the alternative estimator developed by Callaway and Sant'Anna (2021). The stacked design treats each wave of audits as a separate sub-experiment, generating distinct datasets for each of the 16 treatment (audit) waves. In these datasets, municipalities being audited in a given date are classified as treated, while those experiencing treatment in later years and those never receiving an audit serve as controls. Event-time dummies are created relative to the treatment year for each sub-experiment. By stacking these datasets, I can estimate the following specification,

$$y_{igt} = \alpha_i + \delta_t + \beta_0 Audit_{ig} + \beta_1 Audit_{ig} \times Post_t + \sum_{j=-74}^{j=80} \beta_j E^j + X'_{it}\gamma + \epsilon_{it} \tag{1}$$

Where y_{igt} is the total number of traffic violations per 1,000 residents in municipality i, during treatment wave g and period t. The variable $Audit_{ig}$ is a binary indicator that equals 1 if municipality i was randomly selected for audit in wave g, and 0 otherwise. In the lotteries under study (22–38), all audited municipalities were found to have irregularities. Therefore, in practice $Audit_{ig}$ and a "Corruption Found" indicator coincide, but throughout the paper I interpret the treatment as audit assignment, which is the randomized intervention.

Given the structure of the stacked dataset, a municipality can appear several times both as treated and as control. Therefore, I include municipality fixed effects α_i, which are not collinear with the treatment indicator $Audit_{ig}$. Additionally, $Post_t$ is a dummy variable that takes value 1 for the periods after the audit results were disclosed. E^j is a set of relative event-time dummies, equal to 1 if period t is j periods after (or before) treatment. X'_{it} is a matrix of time-varying controls that includes weather variables (rainfall, cloudiness, minimum and maximum temperatures) relevant for driving behavior. Standard errors are clustered at the municipality level (Bertrand et al., 2004) to account for potential serial correlation over time and the repeated inclusion of municipalities as both treatment and control units.

Moreover, I estimate a non-parametric event-study specification to analyze pre-trends and the dynamic evolution of the treatment effect,

$$y_{igt} = \alpha_i + \delta_t + \beta_0 Audit_{ig} + \sum_{j=-74}^{j=80} \beta_j \times E^j \times Audit_{ig} + \sum_{j=-74}^{j=80} \beta_j E^j + X'_{it}\gamma + \epsilon_{it} \tag{2}$$

where the β_j's are the coefficients of interest as they measure the change in outcomes in audited municipalities j periods after (before) treatment, relative to the pre-treatment period and relative to control municipalities.

The key identification assumption for β_1 in (1) and the β_j's in (2) to be interpreted causally is that the trend in traffic offenses in non-audited municipalities serves as a valid counterfactual for the trend in audited municipalities in the absence of audit disclosure. Although this assumption cannot be directly tested, the results from estimating equation (2) provide an opportunity to assess its plausibility.

While the identifying variation stems from the random assignment of audits, the interpretation of the treatment effect is tied to the disclosure of corruption. Because every audited municipality was found to have irregularities, the audit indicator effectively captures the revelation of corruption to the public. Thus, throughout the paper I interpret the estimated coefficients as the causal effect of corruption disclosure, leveraging the randomness of audit assignment for identification.

5 Results

5.1 Main Results

Table 2 summarizes the results from estimating equation (1). Each column presents the key coefficients under varying fixed effects specifications.[10] Consistently across all models, the estimated effect of being randomly audited, which in this context always entailed the public disclosure of corruption, is negligible and statistically insignificant.[11],[12] For example, the point estimate in column (5) indicates a mere 2% increase relative to the control group mean. This effect is minor and close to zero, as even a small shift from the 50th to the 51st percentile in the distribution of average traffic offenses across municipalities corresponds to a 5% increase.[13]

Additionally, Figure 3 presents results from the event-study specification in equation (2).

[10] As discussed in Section 4, a stacked-by-event design allows a municipality to appear multiple times, both as treated and as a control. Consequently, while the original panel consists of 152,572 municipality-month observations, this design results in a total of 2,185,102 observations.

[11] Appendix Table A.3 displays the results obtained using the standard two-way fixed effects estimator in the following form,

$$y_{it} = \beta Audit_{it} + \alpha_i + \delta_t + X_{it}'\gamma + u_{it}$$

where y_{it} is the total number of traffic violations per 1,000 residents in municipality i in period t. $Audit_{it}$ equals one for municipality i starting in period t when it is audited. α_i and δ_t are municipality and period fixed effects, respectively. X_{it} represents a matrix of time-varying control variables. And u_{it} represents the error term. As shown in Appendix Table A.3 results from this specification are identical to those using the stack-by-event design in specification (1), suggesting that the potential biases of the two-way fixed effects estimator in staggered adoption designs are minimal or absent in this context. Furthermore, given that there is left censoring in the dependent variable, I estimate a Tobit model for the two-way fixed effect specification. Results, presented in Appendix Table A.4, show no effect of revelations corruption on traffic offenses.

[12] One concern is that PRF records may capture infractions by transient drivers rather than local residents. While information on drivers' place of residence or birth is not available in the data, the records do include the origin of the vehicle's license plate. Nearly 70% of infractions are committed by vehicles registered in the same state as the municipality where the infraction occurred, suggesting this is not a major concern. As an additional robustness check, I restrict the sample to infractions committed by vehicles registered in the same state. Results are unchanged. Full estimates are available upon request.

[13] A potential concern is that control municipalities geographically or culturally close to treated municipalities may also be indirectly exposed to information about corruption cases, which could contaminate the control group. To address this possible spillover, I re-estimated the main specifications excluding from the control group all municipalities that border a treated municipality. The results remain virtually unchanged, suggesting that the findings are not driven by spillover effects (see Appendix Table A.5).

The figure reveals no evidence of pre-treatment differences between the treated and control groups, supporting the validity of the identification assumption. Furthermore, there is no indication of a dynamic effect; over nearly seven years following treatment, no significant difference between treated and control groups is observed in any period.[14]

Although there is no observed effect on the overall level of traffic offenses, it is possible that citizens respond to corruption disclosures by adjusting their behavior specifically in relation to minor offenses, such as driving without proper identification or illegal parking. These offenses are primarily associated with violations of specific rules and regulations but lack significant externalities related to safety. Table 3 presents results for two categories of traffic offenses: major violations (e.g., driving under the influence, speeding, illegal driving, and driving without a seat belt) and minor violations (e.g., improper individual identification, illegal parking, and illegal equipment). The findings indicate no significant effect on either category of traffic violations, with point estimates that are small and close to zero.

5.2 Heterogenous Analysis

Although the primary analysis found no significant effect of corruption disclosure on traffic offenses, the overall result may conceal important variations within specific contexts or subgroups, making a heterogeneous analysis particularly valuable. Moreover, such an analysis allows for the examination of mechanisms previously highlighted in the literature on how corruption influences citizens' behavior. One such mechanism is social learning, where individuals observe their leaders, reassess their perceptions of social norms, and consequently adjust their own intrinsic values (Ajzenman, 2021). To test this hypothesis, I examine heterogeneities based on drivers' age and the availability of local media outlets at the municipal level. If corruption influences citizen behavior through social learning, its effects should be

[14] Appendix Figure A.1 shows nearly identical results using the alternative estimator developed by Callaway and Sant'Anna (2021).

more pronounced among older drivers, who are potentially more exposed to political discourse, and in municipalities with local media outlets, which are more likely to disseminate information about local leaders. An alternative mechanism involves a shift in perceived costs, including both material costs (such as the expected fine for violating a traffic rule) and social costs (such as the risk of public punishment) associated with breaking rules or regulations (Gulino and Masera, 2023). If corruption influences citizen behavior by altering the perceived costs of breaking the law, its impact should be more pronounced in municipalities where corrupt officials faced no legal consequences or were reelected in the subsequent election.

Finally, considering the variation in the number of corruption cases uncovered by the CGU during the analysis period (see Figure 2), I examine whether the impact of corruption on traffic offenses depends on the extent of corruption disclosed. Specifically, I assess whether major corruption scandals lead to changes in citizens' behavior.

5.2.1 The Age of Drivers and The Role of Media

Research shows that individuals' responses to public events vary with age (Ajzenman, 2021; Hays and Carver, 2014; Madestam and Yanagizawa-Drott, 2012). Younger individuals may be less affected by corruption exposure due to limited cognitive maturity and underdeveloped ethical frameworks. In contrast, older individuals, more engaged with political discourse, are more likely to internalize corruption disclosures. If social learning drives behavioral change, we would expect stronger effects among older individuals, whose greater political awareness makes them more responsive to norm violations by public officials.

Building on these hypotheses, I examine whether the impact of corruption on compliance with traffic laws varies based on drivers' age. While my data does not include individual age information, I use the variation in the proportion of youths across municipalities as a proxy. Accordingly, municipalities with a lower proportion of youths are expected to exhibit a stronger relationship between corruption and traffic offenses. To test this hypothesis, I employ a triple-difference regression, incorporating the municipal-level share of youths into in equation (1). Appendix Table A.6 shows the results of this exercise. The coefficient of interest, represented by the triple interaction between $Corruption_{ig}$, $Post_t$ and $ShareYouth_{ig}$ is not significantly different from zero. This suggests that individuals' responses to corruption are not influenced by the age of the driver.

Moving on to the role of media, individuals in municipalities with local outlets (such as AM/FM radio, local TV, or internet) are more likely to be informed about audit findings. In contrast, national media are less effective at conveying local corruption cases. If social norms are shaped by observing local leaders, one would expect stronger effects in municipalities with local media access. However, as shown in Appendix Table A.7, the impact of corruption on traffic offenses remains negligible and statistically insignificant regardless of media presence.

Beyond confirming the robustness of the main findings across subgroups, the results suggest that corruption disclosure does not trigger social learning related to traffic law compliance. In other words, political corruption does not appear to shift citizens' perceptions of traffic-related social norms.

5.2.2 Unpunished Corruption and Reelection Despite Corruption

Alternatively, one could argue that individuals' responses to corruption are shaped not just by the occurrence of corruption itself but by whether corrupt officials faced legal consequences, such as prosecution or conviction. When corrupt officials face no legal repercussions, it may send a message that illegal behavior carries little to no cost or that such behavior is socially tolerated. If this is the case, one would reasonably expect a greater impact of corruption on driving behavior in municipalities where corrupt officials avoided legal accountability.

To test this hypothesis, I compare audited municipalities where corruption was detected but no legal repercussions or actions were taken against municipal government authorities to municipalities that were never audited[15], using a specification like (1).

The results are presented in Appendix Table A.8. The coefficient of interest indicates no effect of unpunished corruption on driving behavior. Appendix Figure A.2 provides the results of the event study specification for this analysis. Consistent with prior findings, there is no evidence that citizens adjust their behavior in response to unpunished corruption.

Similarly, one could argue that individuals expect corrupt officials to face repercussions through the progression of their political careers. In other words, there is an expectation that corrupt politicians will not be reelected. If this hypothesis holds true, we would expect to observe a greater impact of corrupt acts on driving behavior in municipalities where the mayor in office during the corruption revelations was reelected in the immediate subsequent election. I test this hypothesis by comparing municipalities where corruption was detected and the mayor reelected in the subsequent election to municipalities that were never audited. Results presented in Appendix Table A.9 suggest that there is no effect on driving behavior of the reelection of corrupt municipal authorities. Similar results are found when an event study specification is estimated (see Appendix Figure A.3).

These results confirm the absence of a causal link between corruption and traffic offenses. Furthermore, they suggest that exposing political corruption does not appear to influence the perceived costs of breaking traffic laws.

5.2.3 Major Corruption Scandals

Citizens' responses to corruption disclosures may vary based on the magnitude of the corruption cases disclosed. In other words, it is plausible that only the most significant cases of corruption captured public attention, while instances of minor corruption went largely unnoticed. To test this hypothesis, I compare audited municipalities where the number of

[15] An alternative comparison examines municipalities where corrupt authorities faced legal consequences versus those where they did not. The results remain qualitatively similar to those in Appendix Table A.8 and are available upon request.

disclosed corruption cases falls within the top 25% of number of corruption cases (in audits conducted following lotteries 22 to 38) to municipalities that were never audited, using a specification similar to equation (1) Appendix Table A.10 presents the results of this analysis. Even in instances of significant corruption, there is no evidence to suggest that citizens reduce their compliance with traffic laws. Appendix Figure A.4 displays the results of the event study specification for this exercise. Consistent with the previous findings, there is no indication that major corruption cases influence driving behavior. [16]

6 Alternative Hypothesis

Although the literature on traffic behavior highlights social norms (Carter et al., 2014; Cestac et al., 2014; Fernandes et al., 2010; Mawanga and Ntayi, 2010) and the perceived cost of rule violations (Delavary Foroutaghe et al., 2020; Killias et al., 2016; Moolenaar, 2014; Nazif-Munoz et al., 2022) as key determinants of compliance with traffic laws, both of which could be negatively influenced by corrupt leadership, the findings in this paper suggest that corruption revelations do not lead to an increase in traffic offenses. In other words, individuals do not appear to alter their driving behavior in response to corruption. In this section I analyze three potential alternative hypotheses consistent with the results shown so far.

First, individuals may not have been aware of the audit results, or the disclosures might not have conveyed genuinely new information. Although corruption often receives broad media coverage, citizens may not have been exposed to the specific findings of the CGU audits, or they may not have perceived them as novel. In such a scenario, one would not expect any behavioral change following the audits. However, the hypothesis that information failed to reach citizens appears unlikely. Using the same corruption data from Brazil's anti-

[16] Similar findings emerge from a DiD regression that uses a continuous treatment variable capturing the number of disclosed corruption cases. These results are available upon request.

corruption program, Ferraz and Finan (2008) show that when corruption was revealed, the likelihood of a mayor's reelection declined significantly. This result indicates that citizens did, in fact, learn about the CGU's disclosures, were concerned about corruption, and actively punished corrupt politicians at the polls.

While direct data on perceptions of municipal corruption are not available at the municipal level, national-level evidence offers valuable insights into whether the CGU's disclosures constituted new information. According to the *Latinobarómetro* survey,[17] only 7.83% of respondents in 2001 identified corruption at the municipal or local level.[18] This suggests that, prior to the federal government's initiative examined in this paper, citizens generally did not perceive high levels of corruption at the local level.

By contrast, in the 2017 wave of the survey, after the federal anti-corruption initiative had ended, respondents were asked to rate municipal corruption on a scale from 0 (none) to 10 (a lot), only 16% answered between 0 and 5, while 80% responded between 6 and 10, with 41% selecting the maximum value of 10 (see Appendix Figure A.5). This sharp contrast underscores a significant shift over time: whereas municipal corruption was not widely perceived in 2001, by 2017 it was seen as pervasive. Accordingly, the disclosures of corruption cases by the federal audit program likely provided genuinely new information that contributed to shaping later perceptions of municipal governance.

Second, corruption within municipal government could foster corruption among police officers. If police officers became corrupt while citizens simultaneously reduced their compliance with traffic laws in response to corruption, this could result in more private, unofficial,

[17]The *Latinobarómetro* survey is an annual public opinion poll conducted across Latin America by the Corporación Latinobarómetro, a non-profit organization based in Santiago, Chile.

[18]In the 2001 wave, respondents were asked the following question: "Do you believe there is corruption in any government institution, or do you believe there is none? If you think there is, name them, or indicate if you have not heard enough to give an opinion." The results showed that only 7.83% of responses referred to local or municipal authorities, 11.43% to state authorities, and 11% to parliament. Meanwhile, 8.11% stated that there were no corrupt institutions, and 36.13% answered that they did not know (see Appendix Figure A.5).

and unregistered interactions between police officers and citizens during traffic offenses. Consequently, we would not expect significant changes in the number of registered infractions in the data following corruption scandals. However, this hypothesis primarily affects traffic offenses that rely heavily on police intervention, such as driving under the influence or driving without proper identification, where police effort is crucial. In contrast, offenses like speeding detected by fixed cameras—where no interaction between the offender and police officers occurs—should remain unaffected even if police officers are corrupt. Therefore, if this hypothesis is valid, we would observe a null effect on infractions requiring police intervention and a significant effect on speeding. In Appendix Table A.11, I show the effect of corruption on speeding offenses detected by fixed cameras with no police intervention and on driving under the influence offenses.[19] Both estimates are almost identical, showing no effect and consistent with prior findings. Furthermore, given these results, it seems unlikely that police officers altered their behavior in response to corruption revelations.

Third, the disclosure of corruption cases can have widespread effects, influencing not only citizens' behavior but also the allocation of resources by the federal government. In some instances, the federal government may respond to municipal corruption by reducing the resources allocated to the affected municipality. This reduction could result in fewer police officers, potentially compromising law enforcement capabilities. If this decline in police presence coincides with an increase in traffic offenses, it may lead to underreporting, as the lack of enforcement would fail to reflect the actual rise in violations. This hypothesis seems unlikely since the lack of police officers should not affect the registration of speeding offenses detected by fixed cameras. As described previously, Appendix Table A.11 show no

[19]The database of infractions contains information on whether a police officer approached the driver during a traffic offense and whether the offender signed an offense report. For this exercise, I focus on speeding offenses detected by cameras where no police officer approached the vehicle, and the offender did not sign an offense report. This ensures that the speeding offenses considered involved no police intervention. In contrast, all DUI offenses required a police officer to approach the vehicle and the offender to sign an offense report.

effects on speeding offenses.

Furthermore, the federal government's response to municipal corruption might be more pronounced when municipal and federal authorities belong to different political parties, intensifying the resource constraints faced by non-aligned municipalities. As a result, if the federal government addresses municipal corruption by allocating comparatively fewer resources to corrupt, non-aligned municipalities, and if corruption did reduce compliance with traffic law, we would expect corruption disclosures to have a more significant positive impact on traffic offenses in politically aligned municipalities. This is because resource reductions are either absent or less severe in aligned municipalities compared to their non-aligned counterparts. In Appendix Table A.12, I test this hypothesis by incorporating an indicator variable for political alignment into equation (1). This indicator variable equals one if municipal and federal authorities belong to the same political party and zero otherwise. I then estimate a triple-difference regression to assess the potential differential impact of political alignment. Results show that the effect of corruption on traffic offenses is small, statistically insignificant, and close to zero for both aligned and non-aligned municipalities. This evidence suggests that a change in federal allocation of resources as a consequence of municipal corruption seems unlikely.

7 Conclusion

This study investigated the relationship between the disclosure of corruption cases involving municipal authorities and compliance with traffic laws. Understanding the relationship between these two phenomena is crucial for the developing world, where countries are marked by high rates of traffic fatalities, often linked to driving behavior, and persistently high levels of corruption. The analysis reveals that corruption disclosures do not affect compliance with traffic laws, even in cases involving significant scandals. This finding holds across different subgroups, including different age demographics, municipalities with greater media presence, and cases where authorities were either reelected or not legally punished. These findings indicate that political corruption does not affect citizens' perceptions of social norms regarding traffic rules or their perceived consequences of violating them.

Moreover, alternative explanations for the lack of a causal effect were systematically ruled out. The evidence indicates that citizens were aware of the corruption cases disclosed, police enforcement behavior remained unchanged despite the exposure of corruption, and federal government resource allocation was unaffected by these disclosures. These results suggest that compliance with traffic laws, as a reflection of civic and social norms, is likely influenced by factors beyond immediate political and institutional contexts. Individual safety preferences, along with broader social and cultural determinants, might play a more significant role in shaping adherence to these laws.

8 Declaration of competing interest

The author declares that he has no known competing financial interests or personal relationships that could have appeared to influence the work reported in this paper.

9 Funding Declaration

The author received no external funding for this work.

10 Data Availability Statement

The data that support the findings of this study are available from the author, upon request.

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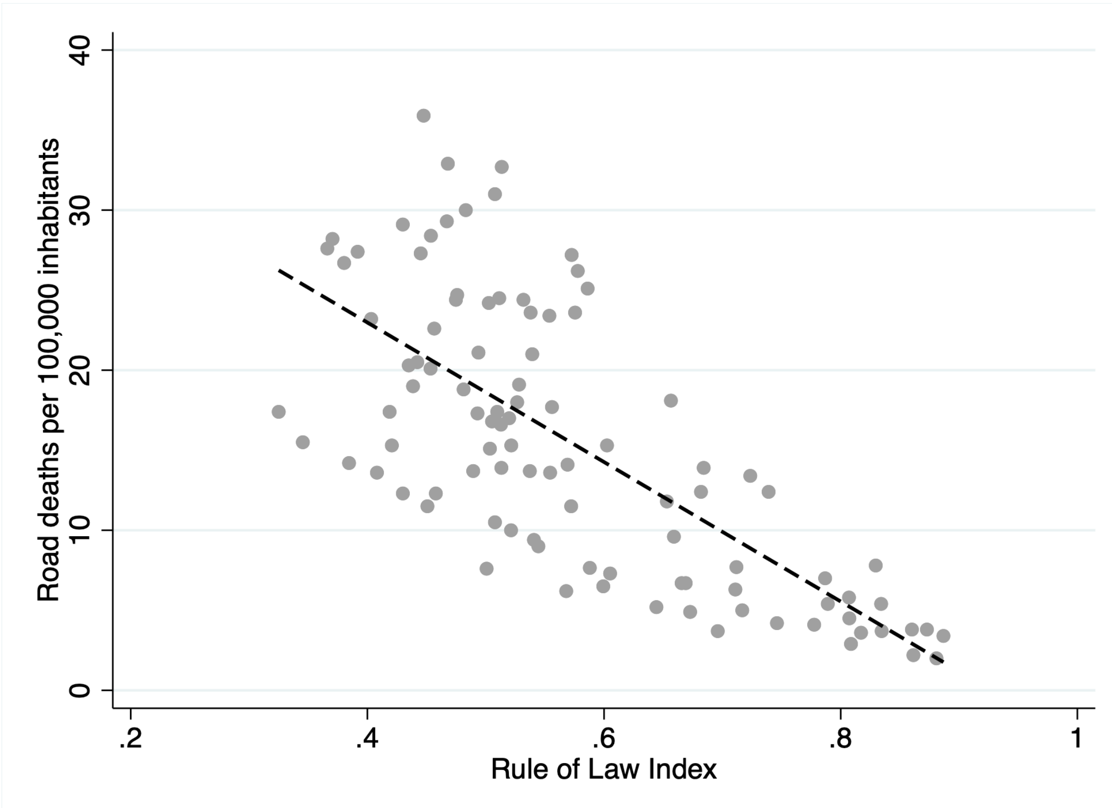
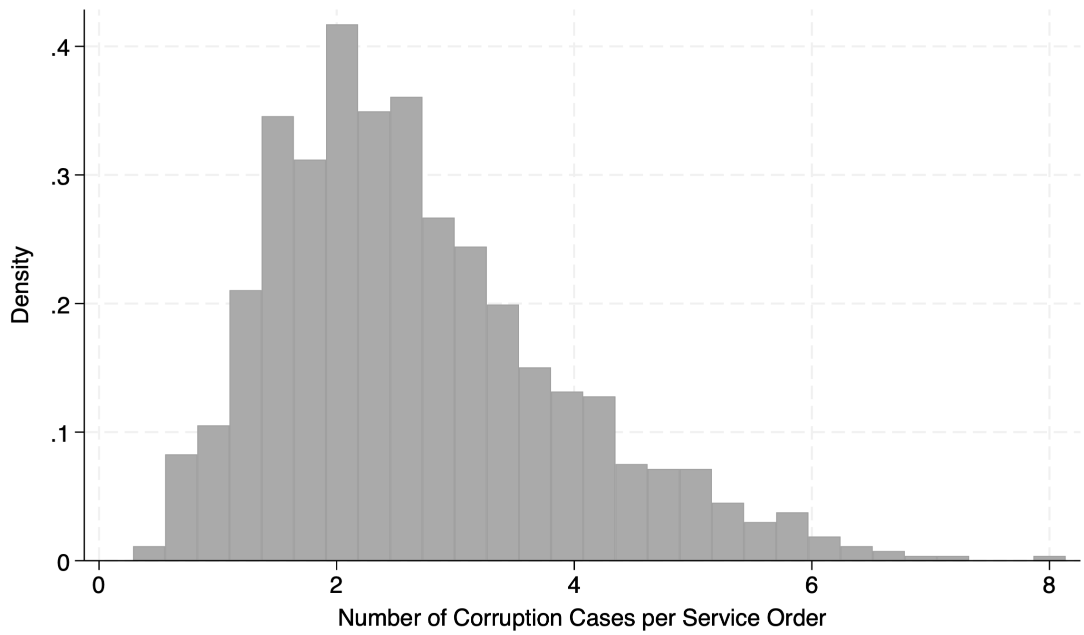
Figure 1: Correlation between Traffic Related Deaths and The Rule of Law



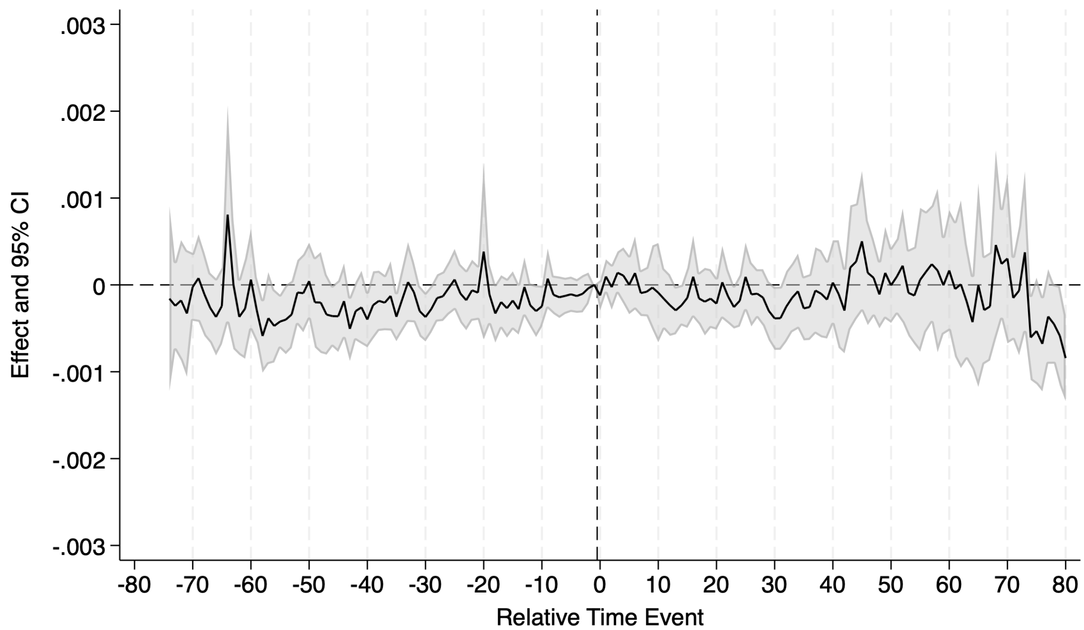
Notes: Data on road deaths is obtained from the World Health Organization Report 2016, while data on the rule of law index comes from the World Justice Project. The rule of law index considers eight factors: constraints on government powers, absence of corruption, open government, fundamental rights, order and security, regulatory enforcement, civil justice, and criminal justice. A higher value of this index represents a stronger adherence to the rule of law. The list includes 99 countries: 19 from Africa, 24 from Asia, 28 from Europe, 16 from North and Central America, 2 from Oceania and 10 from South America.

Figure 2: Distribution of the Number of Corrupt Acts per Service Order



Notes: The data on audits and corruption disclosures is sourced from the CGU and Avis et al. (2018). The data is based on the audits conducted following lotteries 22 to 38 (July 2006-March 2013).

Figure 3: Effect of Corruption on Traffic Offenses. Event Study Results



Notes: The data on audits and corruption disclosures is sourced from the CGU and Avis et al. (2018), while data on traffic offenses is obtained from the PRF. The sample is restricted to municipalities that were either never selected for audits (control group) or were selected once for an audit (during lotteries 22–38) and corruption was identified (treatment group). The darker line illustrates the estimated effects for each period (estimated β_j's from specification (2)), while the shaded area indicates the 95% confidence interval. The estimation includes municipality fixed effects, period fixed effects, and event-time fixed effects, with $j = -1$ serving as the omitted (reference) period. Regressions are weighted by each municipality's share of the national population in 2000.

Table 1: Summary Statistics on Traffic Offenses per 1,000 inhabitants

	All		Audited		Non-Audited	
	Mean	Std. Dev	Mean	Std. Dev	Mean	Std. Dev
All Infractions	1.299	3.594	1.465	3.429	1.272	3.620
Speeding	0.193	2.214	0.186	2.291	0.194	2.201
Driving Without Seat Belt	0.097	0.314	0.113	0.292	0.095	0.318
Driving Under the Influence	0.017	0.055	0.023	0.047	0.017	0.056
Illegal Equipment	0.132	0.478	0.168	0.609	0.126	0.453
Illegal Driving	0.369	1.190	0.352	0.874	0.371	1.235
Improper ID	0.083	0.171	0.112	0.185	0.078	0.168
Illegal Parking	0.030	0.146	0.027	0.123	0.030	0.149
Other Infractions	0.378	0.914	0.484	1.005	0.361	0.896

Notes: Data on traffic offenses comes from the *Policia Rodoviaria Federal* (PRF). The unit of observation is at the municipality-month level. It covers the period January 2007 to January 2014. The total number of observations is 152,575 (85 periods $\times$ 1,795 municipalities).

Table 2: Effect of Corruption on Traffic Offenses

Dependent Variable: All Traffic Violations per 1,000 inhabitants

	(1)	(2)	(3)	(4)	(5)
Audit	0.153	0.107^{***}	-0.00283	-0.00317	-0.00320
	(0.162)	(0.0167)	(0.00642)	(0.00649)	(0.00650)
Audit × *Post*	0.0497	0.113	0.0338	0.0336	0.0338
	(0.154)	(0.104)	(0.107)	(0.107)	(0.107)
R^2	0.001	0.566	0.569	0.569	0.570
N	2,185,102	2,185,102	2,185,102	2,185,102	2,185,102
Municipality Fixed Effects	No	Yes	Yes	Yes	Yes
Period Fixed Effects	No	No	Yes	Yes	Yes
Event Time Fixed Effects	No	No	No	Yes	Yes
Controls	No	No	No	No	Yes

Notes: The data on audits and corruption disclosures is sourced from the CGU and Avis et al. (2018), while data on traffic offenses is obtained from the PRF. The treatment variable is an indicator equal to one for municipalities randomly selected for audit during lotteries 22–38 and zero for those never audited. In this period, all audited municipalities were found to have corruption cases; therefore, the audit indicator coincides with a "corruption found" variable. Identification comes from the random assignment of audits, while the results are interpreted as the causal effect of corruption disclosure. Each column presents estimates of equation (1) using different sets of fixed effects. Control variables, measured at the state–month level, include average cloudiness, total rainfall, maximum temperature, and minimum temperature. Regressions are weighted by each municipality's share of the national population in 2000. Standard errors, clustered at the municipality level, are reported in parentheses. *** $p < 0.01$

Table 3: Effect of Corruption on Traffic Offenses by Type of Infraction

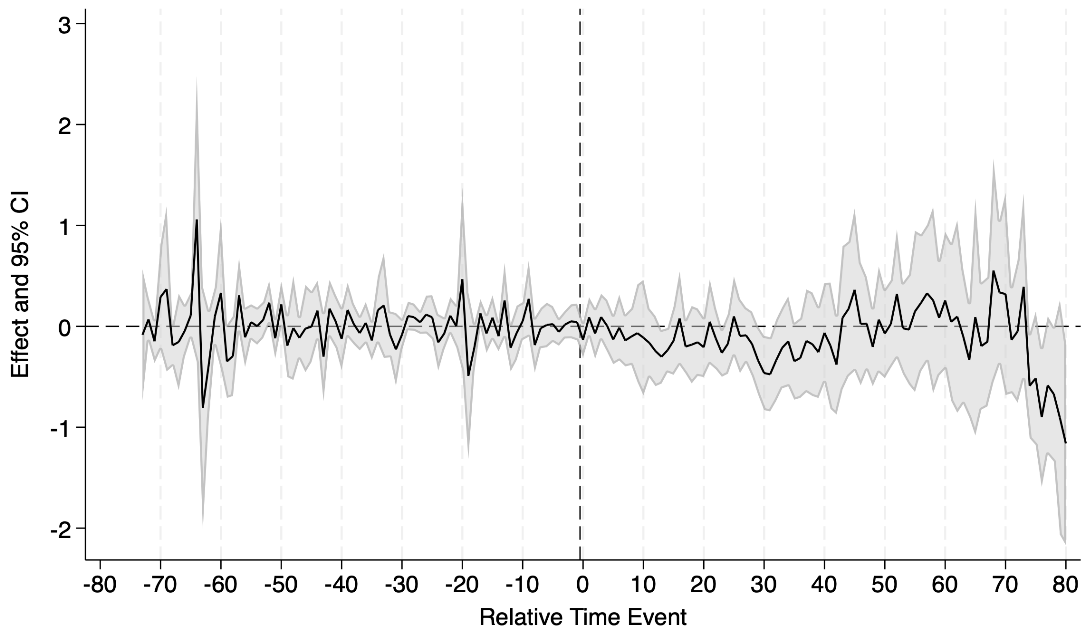
Dependent Variable: Specific Traffic Violations per 1,000 inhabitants

	Major Traffic Violations		Minor Traffic Violations	
	(1)	(2)	(3)	(4)
Audit	-0.00235	-0.00242	-0.000814	-0.000780
	(0.00559)	(0.00560)	(0.00245)	(0.00245)
$Audit \times Post$	0.0862	0.0860	-0.0526	-0.0522
	(0.0892)	(0.0892)	(0.0432)	(0.0434)
R^2	0.450	0.450	0.677	0.677
N	2,185,102	2,185,102	2,185,102	2,185,102
Municipality Fixed Effects	Yes	Yes	Yes	Yes
Period Fixed Effects	Yes	Yes	Yes	Yes
Event Time Fixed Effects	Yes	Yes	Yes	Yes
Controls	No	Yes	No	Yes

Notes: The data on audits and corruption disclosures is sourced from the CGU and Avis et al. (2018), while data on traffic offenses is obtained from the PRF. The treatment variable is an indicator equal to one for municipalities randomly selected for audit during lotteries 22–38 and zero for those never audited. In this period, all audited municipalities were found to have irregularities; therefore, the audit indicator coincides with a "corruption found" variable. Identification comes from the random assignment of audits, while the results are interpreted as the causal effect of corruption disclosure. Each column presents estimates of equation (1) using different sets of fixed effects. Control variables, measured at the state–month level, include average cloudiness, total rainfall, maximum temperature, and minimum temperature. Major traffic offenses include driving under the influence, driving without a seat belt, illegal driving, and speeding. Minor traffic offenses include improper individual identification, illegal equipment, illegal parking, and other infractions. Regressions are weighted by each municipality's share of the national population in 2000. Standard errors, clustered at the municipality level, are reported in parentheses.

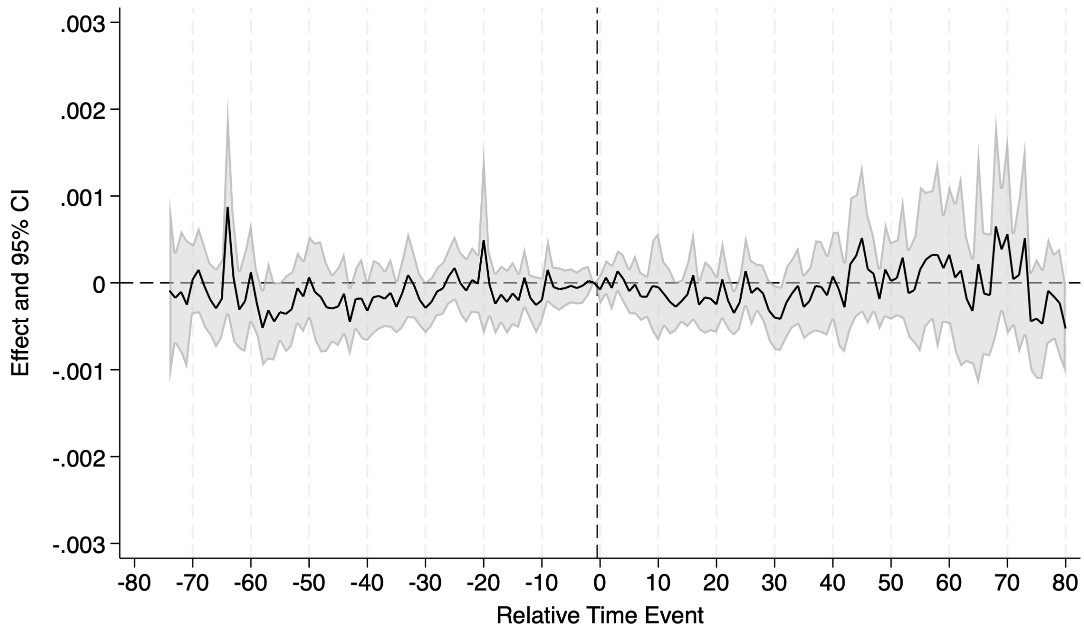
A Appendix Tables and Figures

Figure A.1: Effect of Corruption on Traffic Offenses. Event Study Results. Callaway and Sant'Anna (2021)'s Estimator.



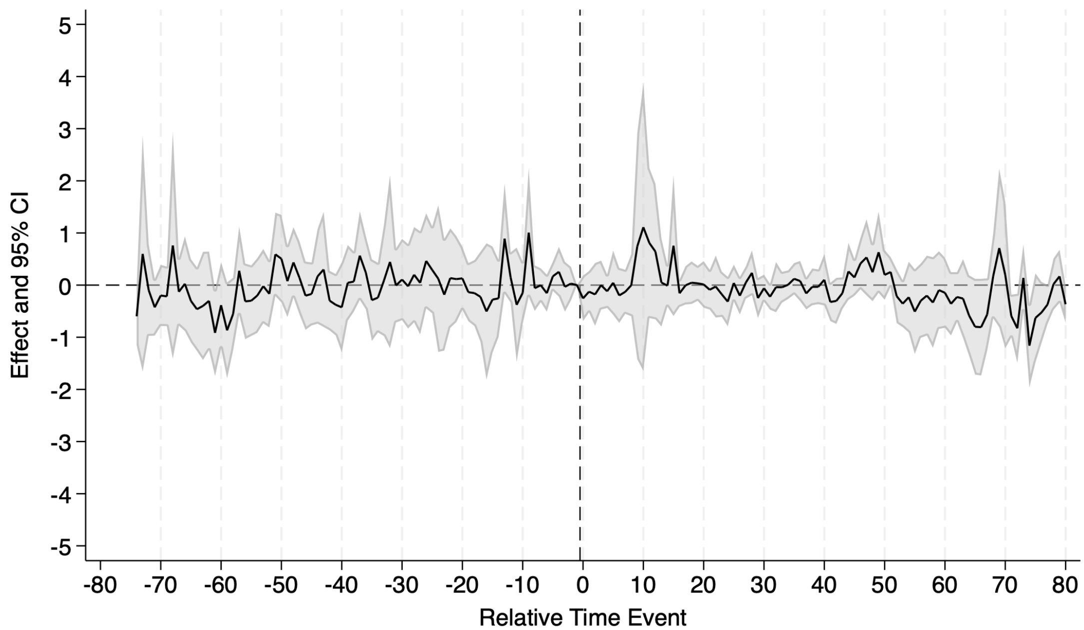
Notes: The data on audits and corruption disclosures is sourced from the CGU and Avis et al. (2018), while data on traffic offenses is obtained from the PRF. The sample is restricted to municipalities that were either never selected for audits (control group) or were selected once for an audit (during lotteries 22-38) and corruption was identified (treatment group). Estimation was performed using Callaway and Sant'Anna (2021)'s estimator. The darker line illustrates the estimated effects for each period, while the shaded area indicates the 95% confidence interval. The estimation includes municipality fixed effects, period fixed effects and event time fixed effects. Estimation is weighted by each municipality's share of the national population in 2000.

Figure A.2: Effect of Unpunished Corruption on Traffic Offenses. Event Study Results



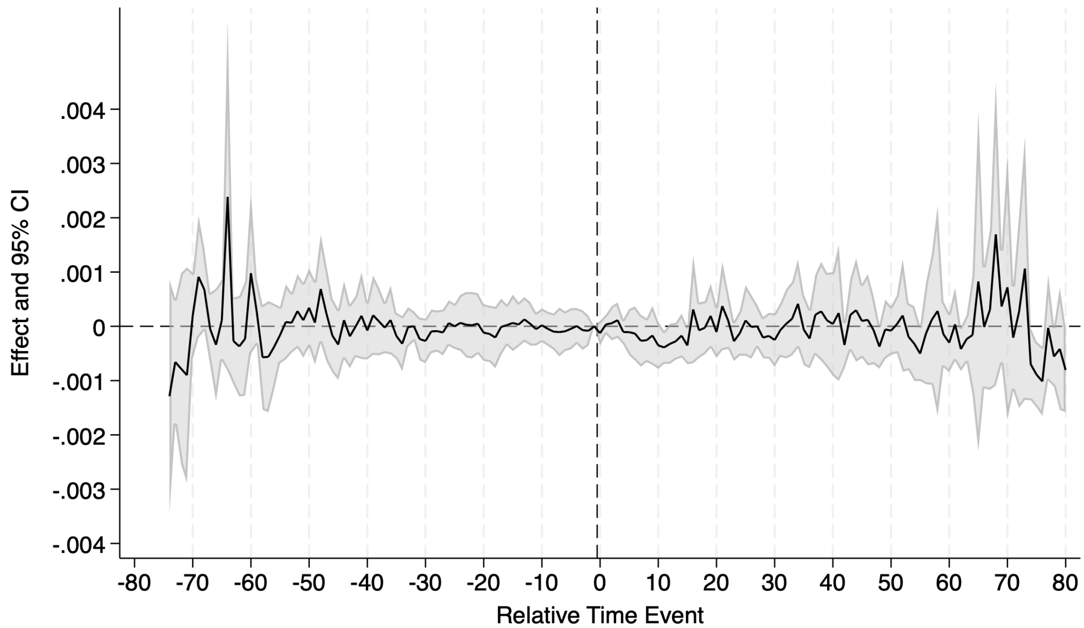
Notes: The data on audits and corruption disclosures is sourced from the CGU and Avis et al. (2018), while data on traffic offenses is obtained from the PRF. The sample is restricted to municipalities that were either never selected for audits (control group) or were selected once for an audit (during lotteries 22-38), corruption was found but no legal action (nor prosecution or conviction) was taken against municipality authorities (treatment group). The darker line illustrates the estimated effects for each period (estimated β_j's from specification (2)), while the shaded area indicates the 95% confidence interval. The estimation includes municipality fixed effects, period fixed effects and event time fixed effects, with $j = -1$ serving as the omitted (reference) period. The regression is weighted by each municipality's share of the national population in 2000.

Figure A.3: Effect of Corruption and Reelection on Traffic Offenses. Event Study Results



Notes: The data on audits and corruption disclosures is sourced from the CGU and Avis et al. (2018), while data on traffic offenses is obtained from the PRF. The sample is restricted to municipalities that were either never selected for audits (control group) or were selected once for an audit (during lotteries 22-38), corruption was found, and the municipality's mayor was reelected (treatment group). The darker line illustrates the estimated effects for each period (estimated β_j's from specification (2)), while the shaded area indicates the 95% confidence interval. The estimation includes municipality fixed effects, period fixed effects and event time fixed effects, with $j = -1$ serving as the omitted (reference) period. The regression is weighted by each municipality's share of the national population in 2000.

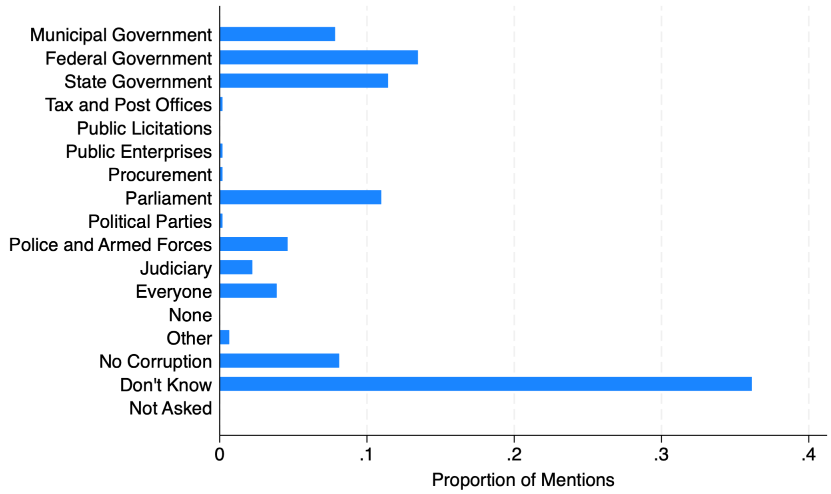
Figure A.4: Effect of Major Cases of Corruption on Traffic Offenses. Event Study Results



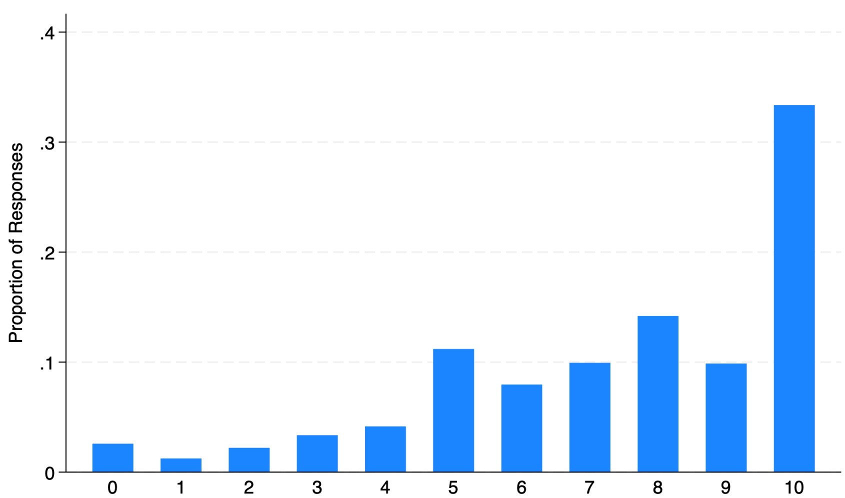
Notes: The data on audits and corruption disclosures is sourced from the CGU and Avis et al. (2018), while data on traffic offenses is obtained from the PRF. The sample is restricted to municipalities that were either never selected for audits (control group) or were selected once for an audit (during lotteries 22-38) and the number of corruption cases was in the top 25% of the number of corrupts acts found during audits following lotteries 22-38 (treatment group). The darker line illustrates the estimated effects for each period (estimated β_j's from specification (2)), while the shaded area indicates the 95% confidence interval. The estimation includes municipality fixed effects, period fixed effects and event time fixed effects, with $j = -1$ serving as the omitted (reference) period. The regression is weighted by each municipality's share of the national population in 2000.

Figure A.5: Citizens' Perceptions of Corruption, 2001 and 2017

(a) Answers to *"Do you believe there is corruption in any government institution, or do you believe there is none? If you think there is, name them, or indicate if you have not heard enough to give an opinion."* Survey in 2001



(b) Answers to *"On a scale from 0 to 10, where 0 means 'none' and 10 means 'a lot', how much corruption do you think there is in the municipal goverment?"* Survey in 2017



Notes: Data are from the 2001 (Panel A) and 2017 (Panel B) waves of the *Latinobarómetro* survey, which provides nationally representative responses. Panel A reports responses to question P28N from the 2001 questionnaire, while Panel B presents responses to question P42NC from the 2017 questionnaire.

Table A.1: Mean Characteristics of Municipalities with PRF data and all Municipalities in Brazil

	All Municipalities in Brazil	Municipalities with PRF Data
Population	29811.5	33339
	(181063.2)	(88228.7)
Share of Women (%)	49.81	50.02
	(1.640)	(1.536)
Share of Urban Population (%)	59.2	62.17
	(23.920)	(23.650)
Share of Youth (%)	13.15	13.33
	(1.425)	(1.356)
Log (Income Per Capita)	5.662	5.697
	(0.583)	(0.556)
Gini Coefficient	0.547	0.551
	(0.0687)	(0.0649)
Share of Individual Aged 18+ with High School Education (%)	13.05	13.57
	(7.155)	(7.123)
Unemployment Rate (%)	9.592	10.21
	(5.636)	(5.521)
Has University	0.31	0.401
	(0.463)	(0.490)
Has AM Radio	0.217	0.28
	(0.412)	(0.449)
Has FM Radio	0.513	0.584
	(0.500)	(0.493)
Has TV Station	0.107	0.149
	(0.309)	(0.356)
Average Cloudiness	5.54	5.515
	(0.521)	(0.523)
Average Total Rain	118	116.1
	(32.63)	(32.37)
Average Maximum Temperature	29.35	29.44
	(3.08)	(3.127)
Average Minimum Temperature	18.45	18.48
	(2.984)	(3.00)
Municipalities	5562	1795

Notes: Data is from the 2000 Census and the *2005 Perfil dos Municipios Brasilerios*. The table shows means and standard deviations (in parenthesis) of several characteristics for municipalities with PRF data and for all municipalities in Brazil.

Table A.2: Mean Comparisons between Non-Audited (Control) and Audited Municipalities (Treated)

	Audited Municipalities	Non-Audited Municipalities	Difference
Population	29431.5	34076.5	-4644.9
	(44639.4)	(94212.7)	(3357.7)
Share of Women (%)	49.96	50.04	-0.0716
	(1.471)	(1.548)	(0.0723)
Share of Urban Population (%)	60.28	62.53	-2.251
	(23.50)	(23.67)	(1.579)
Share of Youth (%)	13.49	13.30	0.184*
	(1.353)	(1.355)	(0.0731)
Log (Income Per Capita)	5.628	5.710	-0.0820*
	(0.544)	(0.557)	(0.0319)
Gini Coefficient	0.548	0.551	-0.00261
	(0.0636)	(0.0651)	(0.00450)
Share of Individual Aged 18+ with High School Education (%)	12.86	13.70	-0.835
	(6.533)	(7.223)	(0.475)
Unemployment Rate (%)	10.47	10.16	0.317
	(5.795)	(5.468)	(0.345)
Has University	0.396	0.401	-0.00483
	(0.490)	(0.490)	(0.0275)
Has AM Radio	0.295	0.277	0.0173
	(0.457)	(0.448)	(0.0243)
Has FM Radio	0.582	0.585	-0.00231
	(0.494)	(0.493)	(0.0286)
Has TV Station	0.144	0.150	-0.00647
	(0.352)	(0.358)	(0.0241)
Average Cloudiness	5.552	5.508	0.0448
	(0.534)	(0.521)	(0.0444)
Average Total Rain	115.2	116.2	-1.069
	(36.82)	(31.47)	(4.143)
Average Maximun Temperature	29.78	29.38	0.403*
	(2.997)	(3.147)	(0.193)
Average Minimum Temperature	18.97	18.39	0.573*
	(2.997)	(2.993)	(0.230)

Notes: Data is from the 2000 Census and the 2005 *Perfil dos Munícipios Brasilerios.* The table shows means and standard deviations (in parenthesis) of several characteristics by municipalities audited and municipalities non-audited in my sample. The total number of municipalities is 1,795, with 285 in the treatment group (audited municipalities) while 1,510 municipalities in the control group (non-audited municipalities). The difference in means is shown in column (3), with standard errors in brackets. * $p < 0.05$

Table A.3: Effect of Corruption on Traffic Offenses. TWFE Estimations

Dependent Variable: All Traffic Violations per 1,000 inhabitants

	(1)	(2)	(3)	(4)
$Audit_{it}$	0.292	0.291***	0.00442	0.00380
	(0.188)	(0.0858)	(0.0910)	(0.0910)
R^2	0.001	0.560	0.564	0.564
N	152,575	152,575	152,575	152,575
Municipality Fixed Effects	No	Yes	Yes	Yes
Period Fixed Effects	No	No	Yes	Yes
Controls	No	No	No	Yes

Notes: The data on audits and corruption disclosures is sourced from the CGU and Avis et al. (2018), while data on traffic offenses is obtained from the PRF. The treatment variable ($Audit_{it}$) equals one for municipality i starting in period t when it is audited. In this period, all audited municipalities were found to have corruption cases; therefore, the audit indicator coincides with a "corruption found" variable. Identification comes from the random assignment of audits, while the results are interpreted as the causal effect of corruption disclosure. Control variables, measured at the state-month level, include average cloudiness, total rainfall, maximum temperature, and minimum temperature. Regressions are weighted by each municipality's share of the national population in 2000. Standard errors, clustered at the municipality level, are reported in parentheses. *** $p < 0.01$

Table A.4: Effect of Corruption on Traffic Offenses. Tobit Estimations

Dependent Variable: All Traffic Violations per 1,000 inhabitants

	(1)	(2)	(3)	(4)
$Audit_{it}$	0.182	0.476***	-0.002	-0.002
	(0.273)	(0.122)	(0.130)	(0.130)
Pseudo-R^2	0.001	0.212	0.214	0.214
N	152,575	152,575	152,575	152,575
Municipality Fixed Effects	No	Yes	Yes	Yes
Period Fixed Effects	No	No	Yes	Yes
Controls	No	No	No	Yes

Notes: The data on audits and corruption disclosures is sourced from the CGU and Avis et al. (2018), while data on traffic offenses is obtained from the PRF. The treatment variable ($Audit_{it}$) equals one for municipality i starting in period t when it is audited. In this period, all audited municipalities were found to have corruption cases; therefore, the audit indicator coincides with a "corruption found" variable. Identification comes from the random assignment of audits, while the results are interpreted as the causal effect of corruption disclosure. Control variables, measured at the state-month level, include average cloudiness, total rainfall, maximum temperature, and minimum temperature. Regressions are weighted by each municipality's share of the national population in 2000. Standard errors, clustered at the municipality level, are reported in parentheses. *** $p < 0.01$

Table A.5: Effect of Corruption on Traffic Offenses, Excluding Neighboring Control Municipalities

Dependent Variable: All Traffic Violations per 1,000 inhabitants

	(1)	(2)	(3)	(4)	(5)
Audit	0.172	0.103***	-0.00185	-0.00228	-0.00232
	(0.166)	(0.0190)	(0.00656)	(0.00664)	(0.00665)
Audit × *Post*	0.0562	0.120	0.0367	0.0366	0.0369
	(0.155)	(0.106)	(0.110)	(0.110)	(0.110)
R^2	0.001	0.589	0.592	0.592	0.592
N	1,510,542	1,510,542	1,510,542	1,510,542	1,510,542
Municipality Fixed Effects	No	Yes	Yes	Yes	Yes
Period Fixed Effects	No	No	Yes	Yes	Yes
Event Time Fixed Effects	No	No	No	Yes	Yes
Controls	No	No	No	No	Yes

Notes: The data on audits and corruption disclosures is sourced from the CGU and Avis et al. (2018), while data on traffic offenses is obtained from the PRF. The treatment variable is an indicator equal to one for municipalities randomly selected for audit during lotteries 22–38 and zero for those never audited. In this period, all audited municipalities were found to have corruption cases; therefore, the audit indicator coincides with a "corruption found" variable. Identification comes from the random assignment of audits, while the results are interpreted as the causal effect of corruption disclosure. The sample includes treated municipalities and a control group consisting only of municipalities that are not adjacent to any treated municipality. Each column presents estimates of equation (1) using different sets of fixed effects. Control variables, measured at the state–month level, include average cloudiness, total rainfall, maximum temperature, and minimum temperature. Regressions are weighted by each municipality's share of the national population in 2000. Standard errors, clustered at the municipality level, are reported in parentheses. *** $p < 0.01$

Table A.6: Effect of Corruption on Traffic Offenses. The Role of Drivers' Age

Dependent Variable: All Traffic Violations per 1,000 inhabitants

	(1)	(2)
$Audit \times Post$	0.0338	0.878
	(0.107)	(1.613)
$Audit \times Post \times ShareYouth$		-5.308
		(10.26)
N	2,185,102	2,185,102
Municipality Fixed Effects	Yes	Yes
Period Fixed Effects	Yes	Yes
Event Time Fixed Effects	Yes	Yes
Controls	Yes	Yes

Notes: The data on audits and corruption disclosures is sourced from the CGU and Avis et al. (2018), while data on traffic offenses is obtained from the PRF. The treatment variable is an indicator equal to one for municipalities randomly selected for audit during lotteries 22–38 and zero for those never audited. In this period, all audited municipalities were found to have corruption cases; therefore, the audit indicator coincides with a "corruption found" variable. Identification comes from the random assignment of audits, while the results are interpreted as the causal effect of corruption disclosure. Variable $ShareYouth$ is the proportion of individuals aged 18 to 24 in the municipality's population. Control variables, measured at the state-month level, include average cloudiness, total rainfall, maximum temperature, and minimum temperature. Regressions are weighted by each municipality's share of the national population in 2000. Standard errors, clustered at the municipality level, are reported in parentheses.

Table A.7: Effect of Corruption on Traffic Offenses. The Role of Media

Dependent Variable: All Traffic Violations per 1,000 inhabitants

	(1)	(2)	(3)
Audit × Post	0.0542	-0.0453	0.0721
	(0.187)	(0.227)	(0.128)
Audit × Post × TV Station	-0.0410		
	(0.206)		
Audit × Post × Radio Station		0.0906	
		(0.252)	
Audit × Post × Internet Provider			-0.0469
			(0.178)
R^2	0.570	0.570	0.570
N	2,185,102	2,185,102	2,185,102
Municipality Fixed Effects	Yes	Yes	Yes
Period Fixed Effects	Yes	Yes	Yes
Event Time Fixed Effects	Yes	Yes	Yes
Controls	Yes	Yes	Yes

Notes: The data on audits and corruption disclosures is sourced from the CGU and Avis et al. (2018), while data on traffic offenses is obtained from the PRF. The treatment variable is an indicator equal to one for municipalities randomly selected for audit during lotteries 22–38 and zero for those never audited. In this period, all audited municipalities were found to have corruption cases; therefore, the audit indicator coincides with a "corruption found" variable. Identification comes from the random assignment of audits, while the results are interpreted as the causal effect of corruption disclosure. *TV Station*, *Radio Station* and *Internet Provider* are dummy variables indicating the presence at the municipality level of a local TV station, a local radio station and a local internet provider, respectively. Each column presents estimates of equation (1) using different sets of fixed effects. Control variables, measured at the state-month level, include average cloudiness, total rainfall, maximum temperature, and minimum temperature. Regressions are weighted by each municipality's share of the national population in 2000. Standard errors, clustered at the municipality level, are reported in parentheses.

Table A.8: Effect of Unpunished Corruption on Traffic Offenses

Dependent Variable: All Traffic Violations per 1,000 inhabitants

	(1)	(2)	(3)	(4)	(5)
(*Audit & No Legal Action*)	0.179	0.102***	-0.00812	-0.00851	-0.00858
	(0.173)	(0.0166)	(0.00612)	(0.00620)	(0.00621)
(*Audit & No Legal Action*) $\times$ *Post*	0.0163	0.0896	0.0106	0.0105	0.0110
	(0.165)	(0.111)	(0.114)	(0.114)	(0.114)
R^2	0.001	0.566	0.570	0.570	0.570
N	2,175,911	2,175,911	2,175,911	2,175,911	2,175,911
Municipality Fixed Effects	No	Yes	Yes	Yes	Yes
Period Fixed Effects	No	No	Yes	Yes	Yes
Event Time Fixed Effects	No	No	No	Yes	Yes
Controls	No	No	No	No	Yes

Notes: The data on audits and corruption disclosures is sourced from the CGU and Avis et al. (2018), while data on traffic offenses is obtained from the PRF. The treatment variable is an indicator equal to one for municipalities randomly selected for audit during lotteries 22–38 where no legal action (prosecution or conviction) was taken against municipal authorities ((*Audit & No Legal Action*) = 1) and zero for municipalities never selected to be audited ((*Audit & No Legal Action*) = 0). Municipalities randomly selected for audit in which legal actions were taken against authorities are excluded from these regressions. Each column presents estimates of equation (1) using different sets of fixed effects. Control variables, measured at the state-month level, include average cloudiness, total rainfall, maximum temperature, and minimum temperature. Regressions are weighted by each municipality's share of the national population in 2000. Standard errors, clustered at the municipality level, are reported in parentheses. *** $p < 0.01$

Table A.9: Effect of Corruption and Reelection on Traffic Offenses

Dependent Variable: All Traffic Violations per 1,000 inhabitants

	(1)	(2)	(3)	(4)	(5)
(*Audit & Major Reelected*)	0.538	0.126***	0.0187	0.0186	0.0188
	(0.476)	(0.0265)	(0.0223)	(0.0223)	(0.0224)
(*Audit & Major Reelected*) $\times$ *Post*	-0.101	0.0584	-0.0234	-0.0233	-0.0201
	(0.342)	(0.150)	(0.150)	(0.150)	(0.149)
R^2	0.0001	0.816	0.823	0.824	0.825
N	2,072,053	2,072,053	2,072,053	2,072,053	2,072,053
Municipality Fixed Effects	No	Yes	Yes	Yes	Yes
Period Fixed Effects	No	No	Yes	Yes	Yes
Event Time Fixed Effects	No	No	No	Yes	Yes
Controls	No	No	No	No	Yes

Notes: The data on audits and corruption disclosures is sourced from the CGU and Avis et al. (2018), while data on traffic offenses is obtained from the PRF. The treatment variable is an indicator equal to one for municipalities randomly selected for audit during lotteries 22–38 where the municipality's mayor was reelected ((*Audit & Major Reelected*) = 1) and zero for municipalities never selected to be audited ((*Audit & Major Reelected*) = 0). Municipalities randomly selected for audit in which the mayor was not reelected are excluded from these regressions. Each column presents estimates of equation (1) using different sets of fixed effects. Control variables, measured at the state-month level, include average cloudiness, total rainfall, maximum temperature, and minimum temperature. Regressions are weighted by each municipality's share of the national population in 2000. Standard errors, clustered at the municipality level, are reported in parentheses. *** $p < 0.01$

Table A.10: Effect of Major Cases of Corruption on Traffic Offenses

Dependent Variable: All Traffic Violations per 1,000 inhabitants

	(1)	(2)	(3)	(4)	(5)
High Corruption	-0.0583	0.104***	-0.00385	-0.00378	-0.00386
	(0.235)	(0.0181)	(0.00936)	(0.00944)	(0.00947)
High Corruption × *Post*	-0.210	0.0421	-0.0265	-0.0263	-0.0289
	(0.191)	(0.101)	(0.104)	(0.104)	(0.105)
R^2	0.001	0.576	0.580	0.580	0.580
N	2,087,612	2,087,612	2,087,612	2,087,612	2,087,612
Municipality Fixed Effects	No	Yes	Yes	Yes	Yes
Period Fixed Effects	No	No	Yes	Yes	Yes
Event Time Fixed Effects	No	No	No	Yes	Yes
Controls	No	No	No	No	Yes

Notes: The data on audits and corruption disclosures is sourced from the CGU and Avis et al. (2018), while data on traffic offenses is obtained from the PRF. The treatment variable is an indicator equal to one for municipalities randomly selected for audit where the number of corruption cases was in the top 25% of corrupt acts found during audits following lotteries 22-38 (*High Corruption* = 1) and zero for those never audited. Each column presents estimates of equation (1) using different sets of fixed effects. Control variables, measured at the state-month level, include average cloudiness, total rainfall, maximum temperature, and minimum temperature. Regressions are weighted by each municipality's share of the national population in 2000. Standard errors, clustered at the municipality level, are reported in parentheses. *** $p < 0.01$

Table A.11: Effect of Corruption on Speeding and Driving Under the Influence Offenses

Dependent Variable: Specific Traffic Violations per 1,000 inhabitants

	Speeding Offenses		DUI Offenses	
	(1)	(2)	(3)	(4)
Audit	0.0023	0.0022	-0.0001	-0.0001
	(0.003)	(0.003)	(0.0001)	(0.0001)
Audit $\times$ *Post*	0.002	0.001	0.001	0.001
	(0.03)	(0.03)	(0.003)	(0.002)
R^2	0.311	0.311	0.334	0.334
Observations	2,185,102	2,185,102	2,185,102	2,185,102
Municipality Fixed Effects	Yes	Yes	Yes	Yes
Period Fixed Effects	Yes	Yes	Yes	Yes
Event Time Fixed Effects	Yes	Yes	Yes	Yes
Controls	No	Yes	No	Yes

Notes: The data on audits and corruption disclosures is sourced from the CGU and Avis et al. (2018), while data on traffic offenses is obtained from the PRF. In Columns (1) and (2) the dependent variable is the number of speeding offenses (per 1,000 inhabitants) detected with fixed cameras where there were no police intervention (police officers did not approach the driver and the driver did not sign an offense report at the time the infraction took place). In Columns (3) and (4) the dependent variable is the number of DUI offenses. The treatment variable is an indicator equal to one for municipalities randomly selected for audit during lotteries 22–38 and zero for those never audited. In this period, all audited municipalities were found to have corruption cases; therefore, the audit indicator coincides with a "corruption found" variable. Identification comes from the random assignment of audits, while the results are interpreted as the causal effect of corruption disclosure. Control variables, measured at the state-month level, include average cloudiness, total rainfall, maximum temperature, and minimum temperature. Regressions are weighted by each municipality's share of the national population in 2000. Standard errors, clustered at the municipality level, are reported in parentheses.

Table A.12: Effect of Corruption on Traffic Offenses. The Role of Political Alignment.

Dependent Variable: All Traffic Violations per 1,000 inhabitants

	(1)	(2)
Audit × *Post*	0.0338	0.0196
	(0.107)	(0.119)
Audit × *Post* × *Same Party*		-0.111
		(0.143)
N	2,185,102	2,185,102
Municipality Fixed Effects	Yes	Yes
Period Fixed Effects	Yes	Yes
Event Time Fixed Effects	Yes	Yes
Controls	Yes	Yes

Notes: The data on audits and corruption disclosures is sourced from the CGU and Avis et al. (2018), while data on traffic offenses is obtained from the PRF. The treatment variable is an indicator equal to one for municipalities randomly selected for audit during lotteries 22–38 and zero for those never audited. In this period, all audited municipalities were found to have corruption cases; therefore, the audit indicator coincides with a "corruption found" variable. Identification comes from the random assignment of audits, while the results are interpreted as the causal effect of corruption disclosure. *Same Party* is an indicator variable that takes value one if municipal authorities and federal government authorities belong to the same party, and zero otherwise. Each column presents estimates of equation (1) using different sets of fixed effects. Control variables, measured at the state-month level, include average cloudiness, total rainfall, maximum temperature, and minimum temperature. Regressions are weighted by each municipality's share of the national population in 2000. Standard errors, clustered at the municipality level, are reported in parentheses.