Corruption and Traffic Offenses: An Empirical Analysis

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Abstract

Do revelations of corruption lead to an increase in traffic offenses? To address this question, I leverage Brazil's anti-corruption program, *Programa de Fiscalização em Entes Federativos por Sorteios Públicos*, which exposed municipal-level corruption cases, to examine their potential impact on citizens' compliance with traffic laws. Using data on traffic offenses, I find that corruption has no effect on traffic violations. This finding remains consistent regardless of the severity of corruption cases, the age of drivers, the availability of local media, or whether corrupt politicians were legally punished or reelected. These results suggest that adherence to traffic rules is more likely driven by individual risk preferences and broader social and cultural factors rather than by political corruption.

Keywords: corruption, social norms, civic values, culture

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 & 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?

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 (measure of the extent to which countries adhere to the rule of law in practice) across different countries: countries with more road deaths (and thus lower compliance with traffic laws) have weaker rule of law (lower constraints on government power, more corruption, lower order and security, lower enforcement on government regulations, weaker civil justice, etc.). Is the implied negative correlation between compliance with the traffic law and corruption causal?

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 main finding remains consistent regardless of the magnitude of corruption cases, the age of drivers, the availability of local media, or whether corrupt politicians were legally punished or reelected.

To identify a causal effect, I exploit the random assignment of municipal audits and the staggered implementation of the program 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 (Borusyak et al., 2021; de Chaisemartin & D'Haultfœuille, 2020; Goodman-Bacon, 2021), I use a stacked-by-event approach (Cengiz et al., 2019; Deshpande & Li, 2019a), that focuses on comparisons between audited and non-audited municipalities. As a robustness check, I also apply the alternative estimator proposed by Callaway and Sant'Anna (2021) and obtain nearly identical estimates.

Additionally, I analyze and present evidence against alternative competing hypothesis to the lack of a causal relationship between corruption and traffic offenses. First, the absence of a causal effect of corruption on traffic law compliance could be attributed to citizens not being aware of the

corruption cases disclosed by the anti-corruption program. Using the same corruption data from Brazil's anti-corruption plan, Ferraz and Finan (2008) show that when corruption was revealed, there was a significant reduction in the likelihood of a mayor's reelection. This finding suggests that citizens did learn about the cases of corruption and were concerned about them. Moreover, when informed, they actively punished corrupt politicians at the polls.

Second, municipal corruption could lead to police corruption, resulting in more unrecorded interactions between police and citizens during traffic offenses. 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.

Third, corruption disclosures could affect federal resource allocation, potentially leading the 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 & Jackson, 2015), honesty (Ajzenman, 2021), political preferences (Dippel & Heblich, 2021), trust (Ananyev & Guriev, 2019; Depetris-Chauvin et al., 2020), unethical conduct (d'Adda et al., 2017; Garz & Pagels, 2018; Gulino & Masera, 2023), reproductive health and family planning preferences (Bassi & Rasul, 2017; Stroebel & van Benthem, 2012), and racial bias (Grosjean et al., 2020). 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 risk preferences 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 selected for auditing through random lotteries held publicly in conjunction with the national lottery in Brasília. The program targeted municipalities with populations of up to 500,000 residents. From the 24th lottery onward, state capitals were excluded from selection. Initially, the first two lotteries selected 5 and 26 municipalities, respectively. Subsequent lotteries randomly selected 60 municipalities each. By February 2015, 40 lotteries had been conducted, resulting in 2,241 audits across 1,913 municipalities. Of these, 1,619 municipalities were audited only once, 261 were audited twice, 32 were audited three times, and one municipality was audited four times. Over time, the frequency of lotteries decreased. In the program's final three years, only one lottery was held annually, with 60 municipalities audited each year.

During each audit, the CGU collected detailed information on all federal funds transferred to the municipality over the previous 3–4 years. A random selection of specific projects was then inspected[1]. CGU auditors, hired through a rigorous public examination process, earned highly competitive salaries, reducing their susceptibility to corruption compared to other federal-level bureaucrats (Avis et al., 2018).

Upon completing an audit, the CGU submitted a comprehensive report detailing all identified irregularities to its central office in Brasília. Additionally, a summary of findings was published online and shared with major media outlets. Each inspection typically lasted about ten days, though this duration varied depending on the municipality's size and the number of projects inspected (Ferraz & Finan, 2008). From the time a municipality was selected in a lottery, it generally took ten days for the audit to commence. Audit results became publicly available 6 to 12 months after the lottery. Once reports for all municipalities in a given lottery were finalized, the CGU announced their availability on its website and provided a summary of key findings.

Over time, the program underwent some modifications, such as limiting the number of sectors inspected in larger municipalities and adjusting the waiting period before a municipality could be re-audited. However, these changes did not alter the likelihood of a municipality being audited, conditional on its eligibility. The randomization process was conducted at the state level, ensuring a consistent probability of selection for municipalities within the same state. Smaller states typically had one or two municipalities selected per lottery, while larger states saw approximately ten municipalities drawn.

3. Data

To evaluate the corruption cases uncovered by Brazil's anti-corruption plan, I use information from the CGU's website[2]. This data contains information on the municipalities randomly selected to be audited between 2003 and 2015. Additionally, I utilize publicly available datasets from Avis et al.

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[1] For example, a municipality chosen for audit might have been investigated for school construction and medicine procurement, while another might have been audited for transportation services and school lunch provision.
[2] https://www.gov.br/cgu/pt-br/acesso-a-informacao/dados-abertos/arquivos/fiscalizacao-em-entes-federativos

(2018). These datasets provide detailed information on corruption cases at the municipality-lottery level, covering lotteries 22 through 38 conducted between July 2006 and March 2013. Lottery 39 occurred in February 2014. Hence, my main estimation sample spans the period between July 2006 and January 2014.

Figure 2 illustrates the distribution of corruption-related irregularities per service order (specific project 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) [3]. The datasets include records of all traffic offenses (including infraction's time, date, location, type, vehicle details and other characteristics) detected by the federal police on federal roads and highways since January 2007. Using PRF data combined with information on corruption cases disclosed by the anti-corruption plan, I construct a monthly panel dataset of municipalities. This dataset includes information on traffic offenses as well as details on whether and when each municipality was selected for an audit. To empirically address the main question of this paper, I restrict the analysis to municipalities never selected to be audited and municipalities selected only once and drawn from one of the lotteries 22 to 38[4]. The resulting estimation sample contains 1,795 municipalities observed over 85 periods (January 2007-January 2014). Within this sample, the treatment group—municipalities where corruption was identified following an audit—includes 285 municipalities, while the control group of never-audited municipalities consists of 1,510 municipalities.

Appendix Table A.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[5] (0.4 infractions per 1,000 inhabitants per month).

Four other data sources are used in this paper. Data on convictions of mayors, crackdowns and legal actions against corrupt municipal authorities is drawn from Avis et al. (2018). The datasets contain information about CGU–Federal Police crackdowns targeting municipalities as well as convictions of mayors for misconduct in public office. For each municipality, the data includes indicator variables denoting whether the municipality was subject to a crackdown in a given year,

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[3] https://www.gov.br/prf/pt-br/acesso-a-informacao/dados-abertos/dados-abertos-da-prf
[4] According to Figure 2 in all of these municipalities at least one case of corruption was disclosed after the audits.
[5] Illegal driving includes offenses such as tailgating, swerving, failing to yield or stop, illegal U turns, among others.

whether authorities implicated in corruption were arrested, and whether the mayor was convicted for misconduct in public office.

Political outcome variables, including reelection rates and mayoral characteristics (e.g., partisan alignment with the federal government), are obtained from the *Tribunal Superior Eleitoral* (National Electoral Court), which provides municipal election data for the period 2000 to 2012. This dataset includes candidate names, vote counts, election winners, and party affiliations. Using this data, I construct two indicator variables: one that equals one if the mayor in office during the audit belonged to the same party as the sitting president, and another variable that equals one if the mayor was reelected.

Demographic and socio-economic variables at the municipal level are derived from the 2000 Census conducted by the *Instituto Brasileiro de Geografia e Estatística* (IBGE; Brazilian Institute of Geography and Statistics). Using information from the Census I can create the following municipality-level characteristics: population, proportion of women, share of urban residents, proportion of youths (ages 18–24), income per capita, Gini index, percentage of individuals aged 18 and older with secondary education, and unemployment rate.

Additionally, I use data from the 2005 *Perfil dos Municípios Brasileiros* (a survey of municipal characteristics conducted by the IBGE) to gather information on the presence of key infrastructure in each municipality, such as universities, AM radio stations, FM radio stations, and TV stations. Furthermore, I incorporate state-level weather data from the *Instituto Nacional de Meteorologia* (National Institute of Meteorology) for the period under analysis. These weather variables include monthly averages for cloudiness, total rainfall, mean maximum temperature, and mean minimum temperature.

Appendix Table A.2 presents summary statistics for municipalities in the sample, comparing those randomly selected for audits to those not selected. It also shows the differences between these characteristics. While the anticorruption plan's randomization ensures that, on average, audited and non-audited municipalities are similar in both observable and unobservable characteristics, the randomization was performed across all municipalities in Brazil, not just the subset in my sample, which includes only those with traffic offense data from the Federal Highway Patrol. Therefore, it is crucial to verify whether the selection for audits remains exogenous in this sample. Results in Appendix Table A.2 shows that to a large extent the randomized selection is preserved; differences are not statistically significant or marginally significant (income per capita, share of youth and temperature). This evidence supports the conclusion that audited and non-audited municipalities in this sample are similar in observable characteristics, making it reasonable to assume similarity in unobservable traits as well.

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 & Li, 2019b). This method mitigates the recently identified challenges of two-way fixed-effects estimators in staggered adoption scenarios (Borusyak et al., 2021; de Chaisemartin & D'Haultfœuille, 2020; 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 (audits) 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,

$$y_{it} = \alpha_i + \delta_t + \beta_0 Corruption_{ig} + \beta_1 Corruption_{ig} \times post_{it} + \sum_{j=-74}^{j=80} \beta_j E^j + X'_{it}\gamma + \epsilon_{igt} \qquad (1)$$

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 $Corruption_{ig}$ is a binary indicator that equals 1 if municipality i was identified as corrupt (audited municipality) in audit wave g. Given the structure of the data, a municipality can appear several times both as treated and as control. Therefore, I include municipality fixed α_i (not collinear with the variable $Corruption_{ig}$). Additionally, $post_t$ is a dummy variable that takes value 1 for the periods after cases of corruption have been disclosed. E^j is a set of relative event-time dummies, that take the value of 1 if period t is j periods after (or before) the treatment. And 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 in the dataset 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,

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

Where the β_j's are the coefficients of interest as they measure the change in outcomes in treated municipalities j periods after (before) treatment, relative to the pre-treatment period, 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 (where corruption was found) in the absence of corruption disclosure. Although this assumption cannot be directly tested, the analysis of the results from estimating equation (2) provides an opportunity to assess its plausibility.

5. Results

5.1. Main Results

Table 1 summarizes the results from estimating equation (1). Each column presents the key coefficients under varying fixed effects specifications[6]. Consistently across all models, the effect of corruption disclosure on traffic offenses is negligible and statistically insignificant[7]. 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 corresponds to a 5% increase.

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

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[6] 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.

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

$$y_{it} = \beta Corruption_{it} + \alpha_i + \delta_t + X'_{it}\gamma + u_{it}$$

Where y_{it} is the total number of traffic violations per 1,000 residents in municipality i in period t. $Corruption_{it}$ 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 corruption on traffic offenses.

dynamic effect; over nearly seven years following treatment, no significant difference between treated and control groups is observed in any period[8].

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 2 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. To investigate this further, this section explores the role of major corruption scandals, driver age, media coverage, the consequences of unpunished corruption, and the impact of reelected corrupt officials on the relationship between corruption and traffic offenses.

5.2.1 Major Corruption Scandals

Citizens' responses to corruption disclosures may vary based on the magnitude of the cases. 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 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.5 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.2 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.

5.2.2 The Age of Drivers

Numerous related studies have highlighted that individuals' responses can vary significantly with age (Ajzenman, 2021; Hays & Carver, 2014; Madestam & Yanagizawa-Drott, 2012). Specifically, exposure to events at a young age may have little impact on behavior, as individuals may

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

not be mature enough to internalize the experience, while exposure later in life may also be ineffective, as preferences, values, and beliefs are often firmly established and resistant to change.

In this paper, younger drivers, who are likely less engaged in political discussions, are expected to show a weaker response to the disclosure of corruption. While my data does not include information on drivers' ages, I can utilize the variation in the proportion of youths across municipalities as a proxy. Consequently, municipalities with a smaller proportion of youths are anticipated to exhibit a stronger impact of corruption on traffic offenses. To address this hypothesis, I estimate a triple-difference regression by adding the share of youths at the municipality level 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, in terms of traffic offenses, are not influenced by the age of the driver.

5.2.3 The Role of Media

Individuals in municipalities with local media outlets are significantly more likely to be aware of audit results. Conversely, municipalities without these local media sources, relying solely on national media, are less effective at disseminating information about specific corruption cases at the municipal level. Therefore, one might expect a stronger impact of corruption in municipalities with access to an AM radio, FM radio, local TV station, or local internet provider. However, as shown in Appendix Table A.7, there is no significant difference in the effect of corruption on traffic offenses between municipalities with local media outlets and those without them. In both cases, the estimated effect is close to zero and not statistically significant.

5.2.4 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[9], 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.3 provides the results of the event study specification for this analysis. Consistent

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[9] 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.

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.4).

6. Alternative Hypothesis

The findings in this paper suggest that revelations of corruption 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. While corruption typically receives extensive media coverage, it is possible that citizens were not informed about the specific findings of the CGU's audits. If this were the case, we would not expect to see any change in individuals' behavior following the audits. However, this hypothesis seems unlikely. Drawing on the same corruption data from Brazil's anti-corruption plan, Ferraz and Finan (2008) show that when corruption was revealed, there was a significant reduction in the likelihood of a mayor's reelection. This finding suggests that citizens did learn about the corruption cases disclosed by the CGU, were concerned about it and actively punished corrupt politicians at the polls.

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, 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.10, I show the effect of corruption on speeding offenses detected by fixed cameras with no police internation and on driving

under the influence offenses[10]. 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.10 show no 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.11, 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. Conclusions

This study investigated the relationship between the disclosure of corruption cases involving municipal authorities and compliance with traffic laws. 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

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[10] 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.

media presence, and cases where authorities were either reelected or not legally punished. 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, 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 risk preferences, along with broader social and cultural determinants, might play a more significant role in shaping adherence to these laws.

8. Declaration of generative AI and AI-assisted technologies in the writing process

During the preparation of this work the author used ChatGPT in order to improve language and readability. After using this tool/service, the author reviewed and edited the content as needed and takes full responsibility for the content of the publication.

9. Acknowledgements

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Figure 1. Correlation between Traffic Related Deaths and The Rule of Law
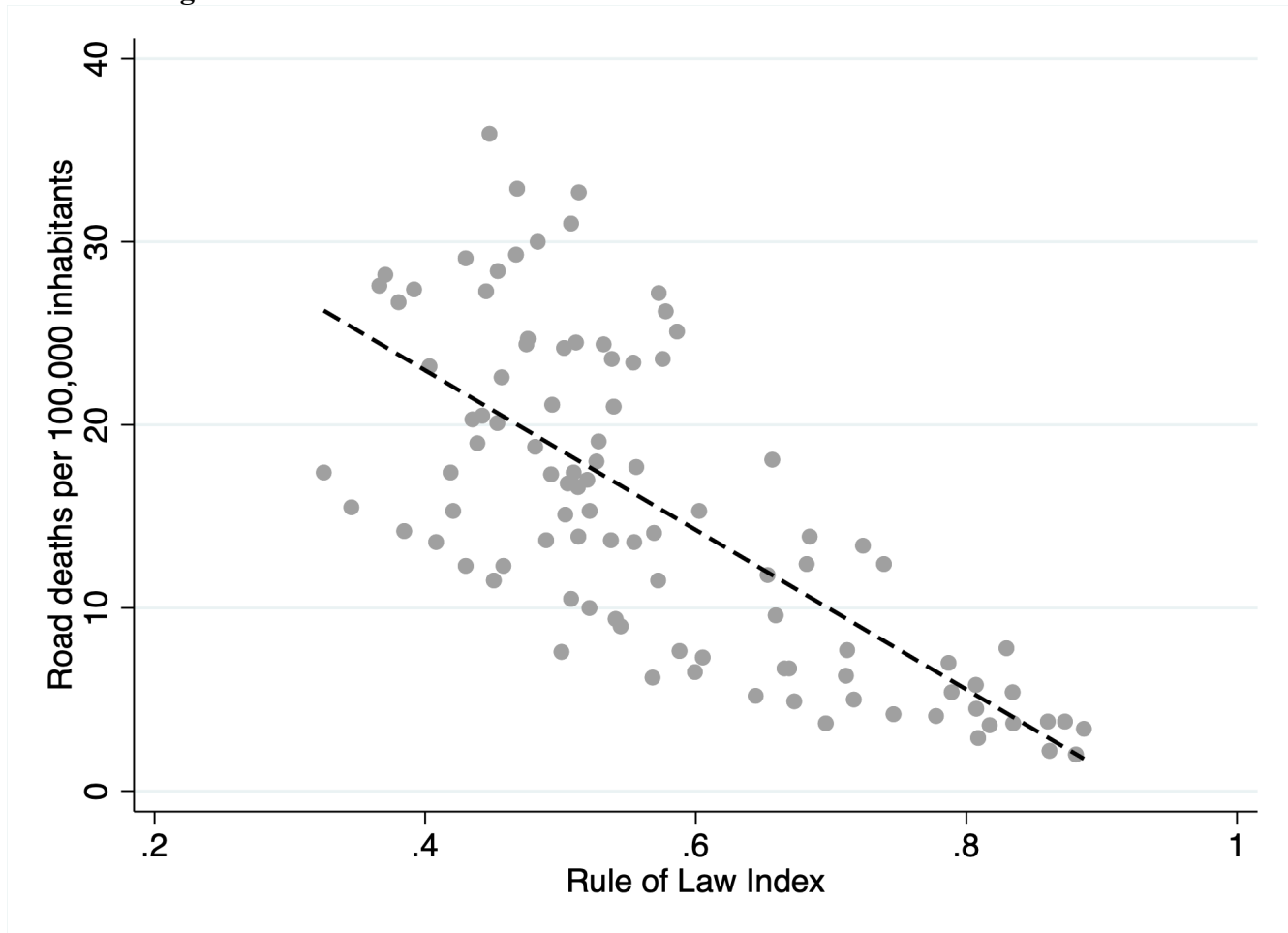
Notes: Data on road deaths is obtained from the World Health Organization Report 2016, while data on the rule of law index comes from the World Justice Project. The rule of law index considers eight factors: constraints on government powers, absence of corruption, open government, fundamental rights, 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
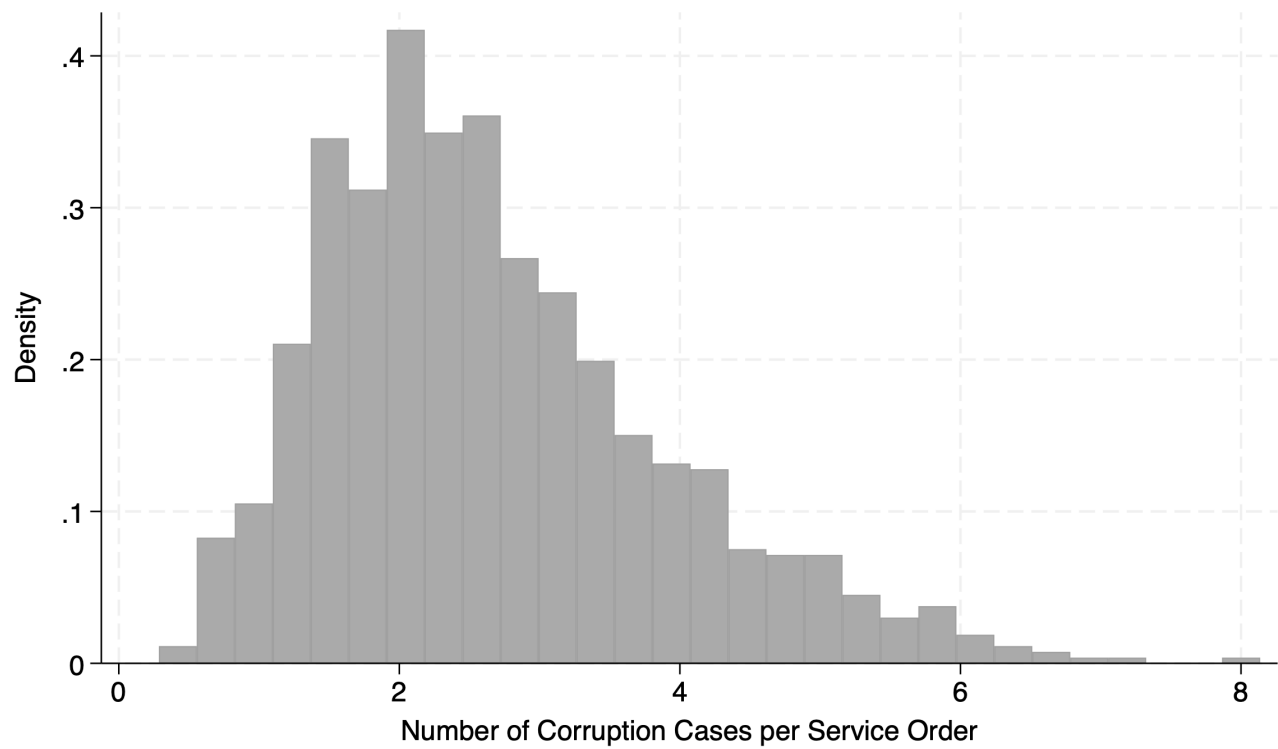
Notes: The data on audits and corruption disclosures is sourced from the CGU and Avis et al. (2018). The data is based on the audits conducted following lotteries 22 to 38 (July 2006-March 2013).

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. The regression is weighted by each municipality's share of the national population in 2000.

Table 1. Effect of Corruption on Traffic Offenses

Dependent Variable: All Traffic Offenses per 1,000 inhabitants

	(1)	(2)	(3)	(4)	(5)
Corruption	0.153	0.107***	-0.00283	-0.00317	-0.00320
	(0.162)	(0.0167)	(0.00642)	(0.00649)	(0.00650)
Corruption x *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
Observations	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 sample is restricted to municipalities that were either never selected for audits (*Corruption* = 0) or were selected once for an audit (during lotteries 22-38) and corruption was identified (*Corruption* = 1). 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 2. Effect of Corruption on Traffic Offenses by Type of Infraction

Dependent Variable: Specific Traffic Offenses per 1,000 inhabitants

	Major Traffic Offenses		Minor Traffic Offenses	
	(1)	(2)	(3)	(4)
Corruption	-0.00235	-0.00242	-0.000814	-0.000780
	(0.00559)	(0.00560)	(0.00245)	(0.00245)
Corruption x *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
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. The sample is restricted to municipalities that were either never selected for audits (*Corruption* = 0) or were selected once for an audit (during lotteries 22-38) and corruption was identified (*Corruption* = 1). 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

Appendix Figure A.1. Effect of Corruption on Traffic Offenses. Event Study Results. Callaway and Sant'Anna (2021)'s Estimator
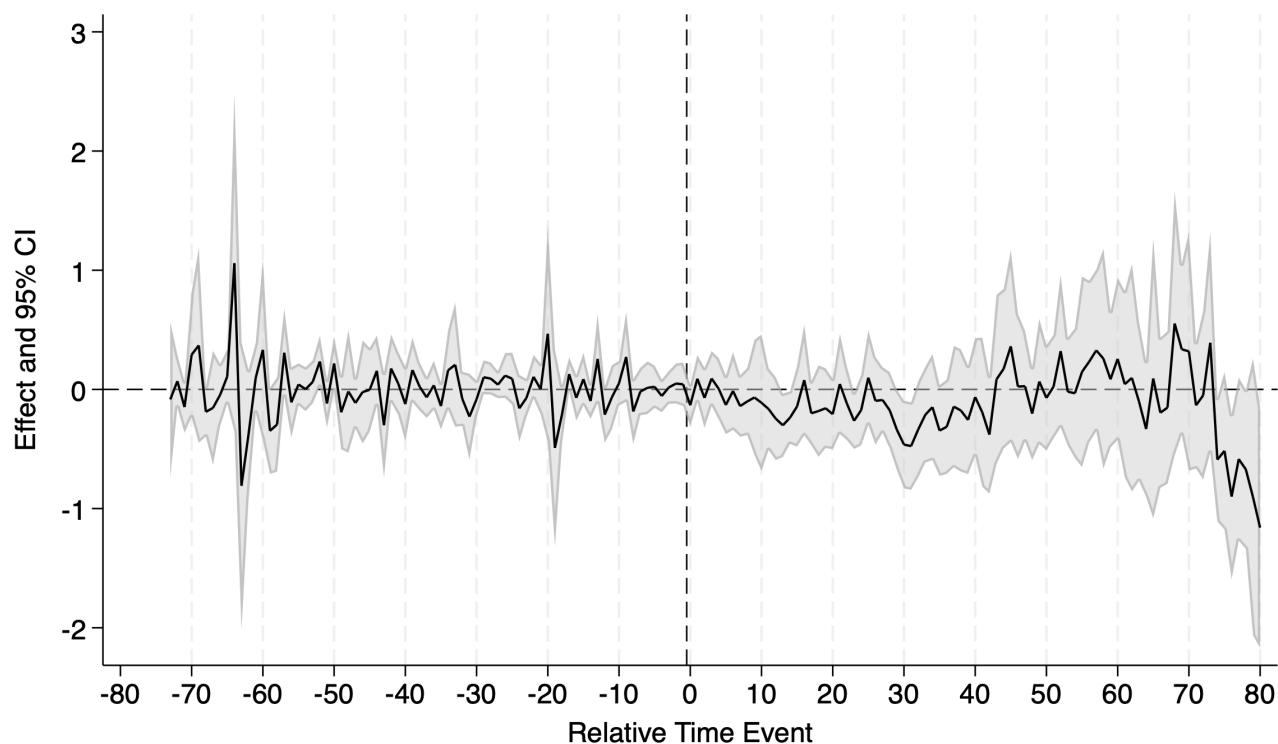
Notes: The data on audits and corruption disclosures is sourced from the CGU and Avis et al. (2018), while data on traffic offenses is obtained from the PRF. The sample is restricted to municipalities that were either never selected for audits (control group) or were selected once for an audit (during 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.

Appendix Figure A.2. Effect of Major Cases of Corruption on Traffic Offenses. Event Study Results
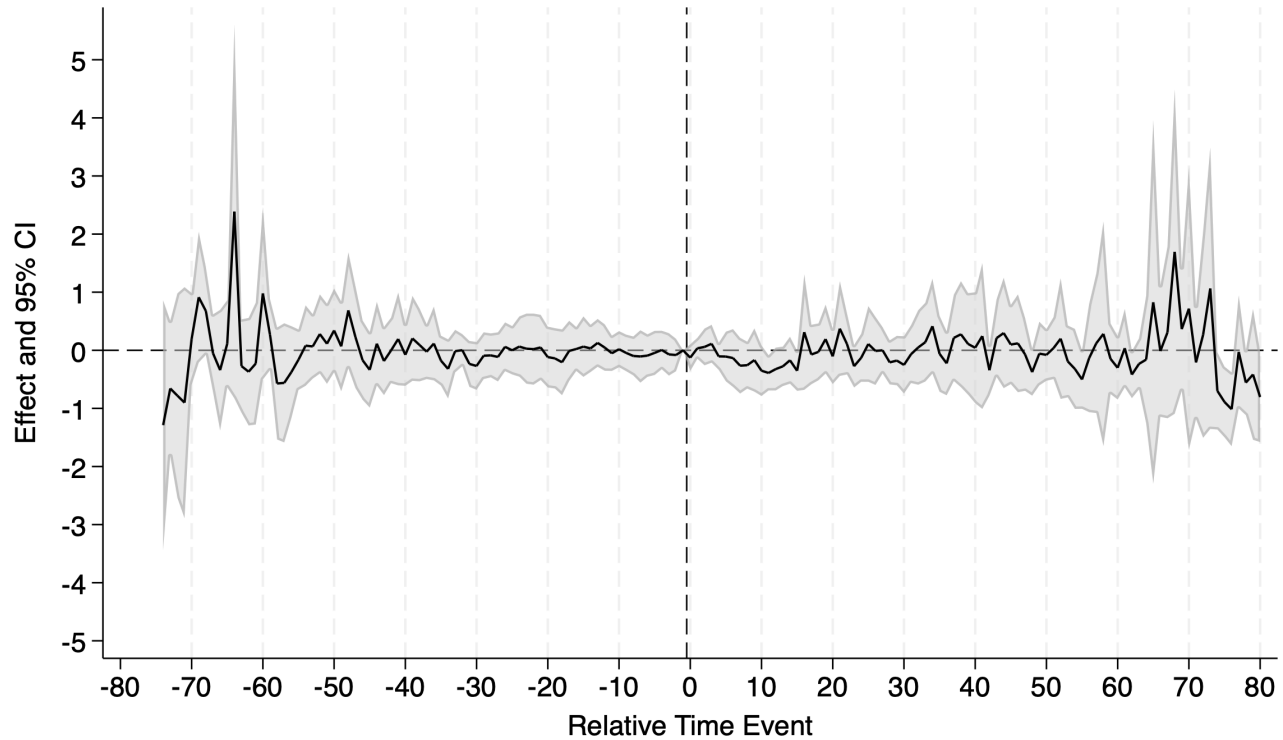
Notes: The data on audits and corruption disclosures is sourced from the CGU and Avis et al. (2018), while data on traffic offenses is obtained from the PRF. The sample is restricted to municipalities that were either never selected for audits (control group) or were selected once for an audit (during 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. The regression is weighted by each municipality's share of the national population in 2000.

Appendix Figure A.3. Effect of Unpunished Corruption on Traffic Offenses. Event Study Results
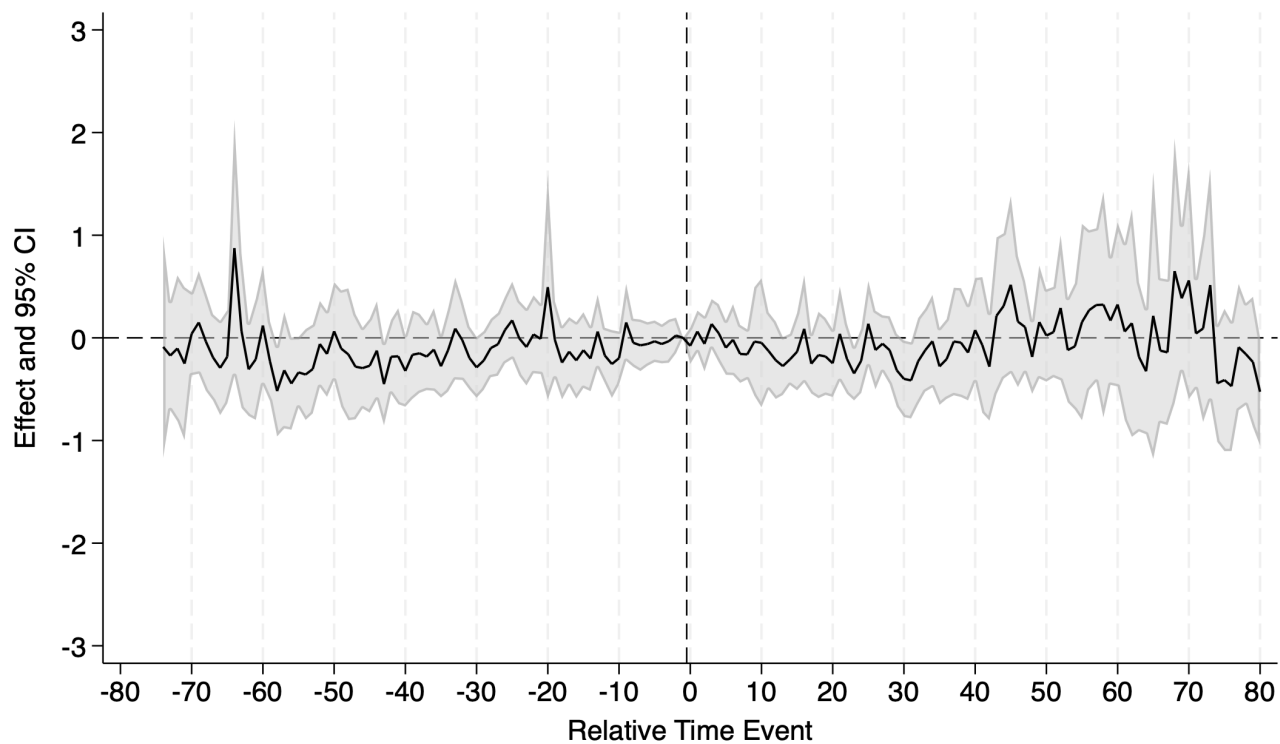

Notes: The data on audits and corruption disclosures is sourced from the CGU and Avis et al. (2018), while data on traffic offenses is obtained from the PRF. The sample is restricted to municipalities that were either never selected for audits (control group) or were selected once for an audit (during 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. The regression is weighted by each municipality's share of the national population in 2000.

Appendix Figure A.4. Effect of Corruption and Reelection on Traffic Offenses. Event Study Results
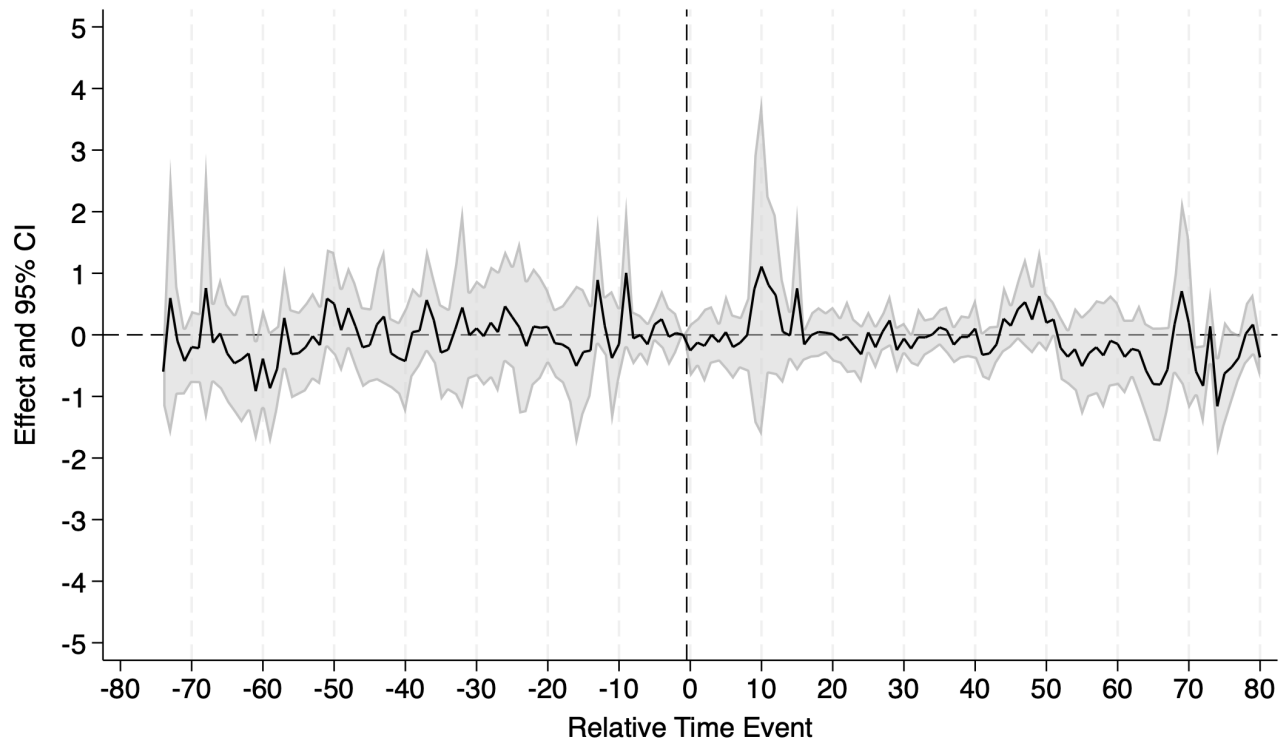
Notes: The data on audits and corruption disclosures is sourced from the CGU and Avis et al. (2018), while data on traffic offenses is obtained from the PRF. The sample is restricted to municipalities that were either never selected for audits (control group) or were selected once for an audit (during 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. The regression is weighted by each municipality's share of the national population in 2000.

Appendix Table A.1. Summary Statistics on Traffic Offenses per 1,000 inhabitants

	All		Treated		Control	
	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 Individual Identification	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 and 1,795 municipalities).

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

	Treated	Control	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 Maximum 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 Municipios 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.

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

Dependent Variable: All Traffic Offenses per 1,000 inhabitants

	(1)	(2)	(3)	(4)
Corruption	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
Observations	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 sample is restricted to municipalities that were either never selected for audits or were selected once for an audit (during lotteries 22-38) and corruption was identified. *Corruption* is a binary variable with time variation that equals one for audited municipalities starting in the period when it was audited. 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.

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

Dependent Variable: All Traffic Offenses per 1,000 inhabitants

	(1)	(2)	(3)	(4)
Corruption	0.182	0.476***	-0.002	-0.002
	(0.273)	(0.122)	(0.130)	(0.130)
Pseudo R2	0.001	0.212	0.214	0.214
Observations	152575	152575	152575	152575
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 sample is restricted to municipalities that were either never selected for audits or were selected once for an audit (during lotteries 22-38) and corruption was identified. Corruption is a binary variable with time variation that equals one for audited municipalities starting in the period when it was audited. Control variables, measured at the state-month level, include average cloudiness, total rainfall, maximum temperature, and minimum temperature. Estimation was performed using a Tobit model. 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.

Appendix Table A.5. Effect of Major Cases of Corruption on Traffic Offenses

Dependent Variable: All Traffic Offenses 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 x *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
Observations	2087612	2087612	2087612	2087612	2087612
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 sample is restricted to municipalities that were either never selected for audits (*High Corruption* = 0) or were selected once for an audit (during lotteries 22-38) and the number of corruption cases was in the top 25% of corrupt acts found during audits following lotteries 22-38 (*High Corruption* = 1). 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.

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

Dependent Variable: All Traffic Offenses per 1,000 inhabitants

	(1)	(2)
Corruption x *Post*	0.0338	0.878
	(0.107)	(1.613)
Corruption x *Post* x *ShareYouth*		-5.308
		(10.26)
Observations	2185102	2185102
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 sample is restricted to municipalities that were either never selected for audits (*Corruption* = 0) or were selected once for an audit (during lotteries 22-38) and corruption was identified (*Corruption* = 1). Variable *ShareYouth* is the proportion of individuals aged 18 to 24 in the municipality's population. 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.

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

Dependent Variable: All Traffic Offenses per 1,000 inhabitants

	(1)	(2)	(3)
Corruption x *Post*	0.0542	-0.0453	0.0721
	(0.187)	(0.227)	(0.128)
Corruption x *Post* x *TV Station*	-0.0410		
	(0.206)		
Corruption x *Post* x *Radio Station*		0.0906	
		(0.252)	
Corruption x *Post* x *Internet Provider*			-0.0469
			(0.178)
R^2	0.570	0.570	0.570
Observations	2185102	2185102	2185102
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 sample is restricted to municipalities that were either never selected for audits (*Corruption* = 0) or were selected once for an audit (during lotteries 22-38) and corruption was identified (*Corruption* = 1). 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. 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.

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

Dependent Variable: All Traffic Offenses per 1,000 inhabitants

	(1)	(2)	(3)	(4)	(5)
(*Corruption & No Legal Action*)	0.179	0.102***	-0.00812	-0.00851	-0.00858
	(0.173)	(0.0166)	(0.00612)	(0.00620)	(0.00621)
(*Corruption & No Legal Action*) x *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
Observations	2175911	2175911	2175911	2175911	2175911
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 sample is restricted to municipalities that were either never selected for audits (*Corruption & No Legal Action* = 0) 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 (*Corruption & No Legal Action* = 1). 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.

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

Dependent Variable: All Traffic Offenses per 1,000 inhabitants

	(1)	(2)	(3)	(4)	(5)
(*Corruption & Major Reelected*)	0.538	0.126***	0.0187	0.0186	0.0188
	(0.476)	(0.0265)	(0.0223)	(0.0223)	(0.0224)
(*Corruption & Major Reelected*) x *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
Observations	2072053	2072053	2072053	2072053	2072053
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 sample is restricted to municipalities that were either never selected for audits (*Corruption & No Mayor Reelected* = 0) or were selected once for an audit (during lotteries 22-38), corruption was found, and the municipality's mayor was reelected (*Corruption & Mayor Reelected* = 1). 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.

Appendix Table A.10. Effect of Corruption on Speeding and Driving Under the Influence Offenses

Dependent Variable: Specific Traffic Offenses per 1,000 inhabitants

	Speeding Offenses		Driving Under the Influence Offenses	
	(1)	(2)	(3)	(4)
Corruption	0.0023	0.0022	-0.0001	-0.0001
	(0.0030)	(0.0030)	(0.0001)	(0.0001)
Corruption x *Post*	0.002	0.001	0.001	0.001
	(0.030)	(0.030)	(0.003)	(0.002)
R^2	0.311	0.311	0.334	0.334
Observations	2185102	2185102	2185102	2185102
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 sample is restricted to municipalities that were either never selected for audits (*Corruption* = 0) or were selected once for an audit (during lotteries 22-38) and corruption was identified (*Corruption* = 1). 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.

Appendix Table A.11. Effect of Corruption on Traffic Offenses. The Role of Political Alignment

Dependent Variable: All Traffic Offenses per 1,000 inhabitants

	(1)	(2)
Corruption x *Post*	0.0338	0.0196
	(0.107)	(0.119)
Corruption x *Post* x *SameParty*		-0.111
		(0.143)
Observations	2185102	2185102
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 sample is restricted to municipalities that were either never selected for audits (*Corruption* = 0) or were selected once for an audit (during lotteries 22-38) and corruption was identified (*Corruption* = 1). *SameParty* is an indicator variable that takes value one if municipal authorities and federal government authorities belong to the same party, and zero otherwise. 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.