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Police legitimacy and deterrence: an exploration from traffic offenses and crashes

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Abstract

Increasing the deterrent effect of sanctions is a pressing public policy issue in many areas where the state has limited capacity to punish all wrongdoers. If institutions in charge of enforcing the law are perceived as illegitimate, sanctions might lose their capacity to deter behavior. This paper uses local media, traffic tickets and road crashes in Bogotá, Colombia to study whether sanctions imposed by more illegitimate authorities are less deterrent. Using the fact that traffic control is done by the national police, I consider police scandals that are highly reported in the media as an exogeneous negative shock to the perceived legitimacy of the institution in charge of road traffic enforcement. My estimation compares the behavior of drivers that get their first tickets on days just after a police scandal with drivers that get their first tickets just before a scandal. Drivers ticketed just after a police scandal are more likely to not pay their fine and get a second ticket. Furthermore, they are 21% (11%) more likely to be in a crash in the following six months than drivers that get their first ticket just before a scandal (any other day). Finding the opposite effect when news shows the police as the victim also supports my hypothesis of loss of legitimacy causing state sanctions to be less deterrent. My results suggest that in this policy issue increasing the legitimacy of existing sanctions might be more effective than increasing the number of sanctions issued.

Keywords:

Traffic crashes; police scandals; deterrence; legitimacy; behavioral change.

JEL Classification:

H23; R41; D78; D91

1. Introduction:

Modern states impose legal sanctions in order to deter unwanted behavior. However, in many cases (discrimination, tax evasion), it is materially impossible for the state to punish all citizens that break the law. Thus, improving the deterrent effect of the few sanctions that are effectively imposed is a pressing public policy issue in many areas. States must maximize resources to achieve maximum deterrence from effectively imposed sanctions.

One intuitive avenue for increasing the deterrence effect of legal sanctions has been to improve the legitimacy of the enforcing institutions. If citizens recognize that the authority imposing sanctions on them is a legitimate actor that is working in society's best interest when enforcing the law, they might be willing to adjust their behavior. Also, criminology has widely argued that a more legitimate authority can also count on the voluntary cooperation from citizens (Owens & Ba, 2021).On the contrary, if those in charge of sanctioning unwanted behavior are perceived to be working on their own personal interest, citizens might not adjust their behavior besides evading possible new sanctions.

The more salient authority in charge of applying the law is the police. This paper explores how changes in the perception of the police affect the deterrent capacity of its sanctions. I use data from local media, traffic offenses and road accidents in Bogotá, Colombia. In particular, I compare drivers that get their first tickets on days immediately following a police scandal with drivers that get first tickets on days that precede a police scandal. In my preferred estimation I find that drivers that get their tickets just after a police scandal are 21% (11%) more likely to be in a crash in the following six months than drivers that get their first ticket just before a scandal (any other day). I also show that this differential effect of tickets after police scandals is seen in similar behaviors, ticket payment and subsequent accidents. I interpret these results as evidence for the hypothesis that citizens are less prone to change their behavior when the sanctions they receive come from an illegitimate authority.

The behavioral model behind my work assumes that engaging in unsafe driving behavior increases the probability of receiving a ticket as well as the probability of a crash. Tickets (speeding ones, in particular) typically have a deterrent effect, reducing the future amount of unsafe driving and therefore reducing the chances of future accidents. This effect might be weakened when the perceived legitimacy of the police force is challenged by scandals. Because of the selection of drivers into speeding behavior, I can not say anything here about the baseline effect of traffic tickets, but I can compare the differential effect of a ticket for those drivers that get their tickets just after a scandal (when legitimacy of the police is low) from those whose tickets is received just before a scandal (when legitimacy of the police is higher).

This paper relates to different strands of economics literature. First, it relates to research on the effect of certain scandals on the interactions between institutions and citizens. An often-explored topic is the effect of police scandals on the trust in the police, and consequentially in police behavior. After “Ferguson” and “Black Lives Matter” events, a decrease of police homicides (Campbell, 2023) and a reduction of police activities, de-policing, have been documented (Cheng & Long, 2022). It has also been argued that there is a causal effect of police scandals on less reporting of crimes (Ang et al., 2024). But this is not only relevant for the police. A similar question has also been explored in the U.S. Catholic Church, where scandals casually affect religious participation and charitable giving (Bottan & Perez-Truglia, 2015), with spillover effects to other religions (Frick et al., 2021). I contribute to this literature exploring the causal impact of police legitimacy not only on the behavior that is being sanctioned, traffic offenses, but also in the ones at which the deterrent effect is indirectly targeted: road accidents. This indirect deterrence is quite unexplored in the existing literature.

My research also relates to research on the role of legitimacy and trust on the interaction between individuals and institutions and among individuals. It has been shown that legitimacy of public actors helps authorities perform their duty through increased collaboration (Jácome, 2022) and less engagement with non-state actors (Acemoglu et al., 2020). Less trust in the police is related to higher levels of crime (Muchow & Amuedo-Dorantes, 2020). Procedural justice research has shown that even in short encounters during traffic stops, there is a causal effect on legitimacy of more procedural just interactions (Mazerolle et al., 2013) and a small but significant effect on procedural training on the reduction of most violent interactions of citizens with the police (Wood et al., 2020). The effect, some argue, is relatively small, and might not translate into cooperations with authorities (Sahin et al., 2017). This question has been answered in interactions between citizens. It has been shown that after a local corruption scandal, customers are more willing to steal at supermarkets (Gulino & Masera, 2023) and students more prone to cheating in their exams (Ajzenman, 2021). I fill a gap in the literature by exploring the effect of police legitimacy not only on immediate results, perceptions of police or violent encounters, but on subsequent behavior. In particular, behavior that does not involve authorities but the interaction with other citizens.

Another relevant research agenda centers on the causes of road accidents. There are multiple explanations in the literature for causes of road accidents, and a rigorous evaluation of them is necessary for a better public policy design. In particular, my work relates to literature that looks for causes that are not intuitively related to traffic accidents, like the political cycle (Bertoli & Grembi, 2021), or the stock market (Fry & Farrell, 2023) (Giulietti et al., 2020). Close to this strand is another set of papers that explores behavioral responses to traffic enforcement and tries to derive policy recommendations to reduce road accidents and fatalities (Zhang et al., 2020) (Lu et al., 2016). Some authors even explore if procedurally just policing might reduce subsequent traffic infractions and find that the effect is significant but small and depends on the age of the driver's age (Bates et al., 2023). I contribute to this literature by adding another possible cause for accidents, the lack of legitimacy of traffic authorities.

A final group of papers to which this article might be related is concerned about measuring (Tobon et al., 2022) and improving trust in the police (Abril et al., 2023). This has been studied with promising results in the context of body worn cameras and traffic stops (Demir et al., 2020; Demir & Kule, 2022). I contribute to this literature by measuring the effect of trust and engagement with the police in non-experimental data and not relying on self-reports but on actual behaviors of citizens.

On the methodological side this paper adds an additional point in the literature that tries to measure public opinion through social media and its effect on real-world results (Huang et al., 2023; Mellon, 2014; Müller & Schwarz, 2023; Stephens-Davidowitz, 2014,Colagrossi et al., 2023). I add a data point from the developing world that is rare in existing academic literature.

The paper is organized as follows. In section 2, I explain the general context of traffic offenses in Bogotá and how the independent variable of scandal days is built. In Section 3 I present the identification strategy of the paper. Section 4 presents results, and section 5 shows some robustness checks to specific decisions. In Section 6 I discuss a "sentiment analysis" of the main findings. I present a wider discussion of the main findings of the paper in Section 7.

2. General context and construction of scandal days variable.

2.1 Traffic offenses and accidents in Bogotá, Colombia

Most traffic enforcement in Colombia is done by the *Policía de Tránsito*, a branch of the national police[1]. In general, they are the ones that pull drivers over and give them a ticket according to the nature of the offense. Traffic tickets in Colombia have the paradoxical nature of being quite expensive but being rarely paid. A speeding ticket

[1] Two things are worth noting about police in Colombia. First is that it has only a national character. In Colombia there are no local police departments, but one national police that has local forces. Thus, police scandals arguably have a wider national impact than in other countries since event might have occurred in another city but are caused by the same police force. The second relevant aspect is that, due to the long history of violence in the country, it is common for the police to be in charge of many tasks that traditionally are reserved to the military and require some use of force. This negatively affects the legitimacy of the police, since it forces them to have more violent encounters with citizens.

has a fine of half of the mandatory minimum wage, but only around 45% of tickets in the sample are paid within one year of them being issued[2].

Figure 1 of the Appendix shows total yearly tickets. One can see a sharp increase in the number of tickets given by automated speed Cameras, called *Camaras Salvavidas* (Lifesaving Cameras, in Spanish). The program started in June 2020 and with the rollout of new cameras it became the primary source of tickets by March 2022. I will only use the period from June 2020 on my analysis in order to reduce selection caused by different enforcement probabilities[3]. Drivers that got a ticket before the Cameras existed are not comparable to drivers that get their tickets in the period I study.

The introduction of Speed Cameras also changed the composition of the tickets. Figure 2 of the Appendix shows the main five infractions and their distribution over the years. Note that speeding tickets account for an increasing percentage of speeding tickets, but there persists a significant percentage of tickets given for minor offenses. Since my argument relays on speeding tickets being the ones more closely related to accidents, this is also an argument for the sample I selected.

While tickets have increased significantly, accidents have continued to grow. Figure 3 of the Appendix shows that there is an increase in the number of serious accidents (does with at least one person injured). There was a legal change in the reporting of accidents from September 2022 onwards, by which only serious accidents continue to be reported[4]. While this is not ideal, the change in reporting affects drivers that received their ticket both before and after the scandal equally, thus it is not a threat to my identification strategy. As is shown in Figure 4 of the Appendix the riskier profile and increasing presence of motorcycles in the city can explain this apparent paradox of serious accidents increasing even with the growing number of speeding tickets.

2.2 Data and construction of scandal days variable.

I have data for all traffic offenses and road accidents occurring in Bogotá from 2020 to August 2023. Both traffic offenses and accidents have the registration of the vehicles involved, and this characteristic allows me to follow each vehicle in time. I

[2] In fact, most drivers only pay their tickets before selling their vehicles. This explains that only 45% of tickets get paid in the first year, but 60% are paid at any point in time.

[3] There is no reliable survey data from drivers after 2020. What we have is a survey that was done on drivers on 2018 and 2019. For both years around a third of respondents said they agreed or strongly agreed with the following statement “There is little chance of being stopped or ticketed by the police if I drive above the speed limit.” To be specific, in 2018 23% of drivers say they agreed with the statement and 7% said they strongly agreed. In 2019 the percentages were 29% and 9%, respectively. In sum, a significant number of drivers did not fear being punished for speeding. It seems safe to assume that this numbers reduced sharply after speed cameras were installed (Alcaldía de Bogotá, 2019). The chances of a speeding violation turning into a Speeding are really around 40%.

[4] The Ministry of Transportation (by Circular Externa 20224000000057) stated that simple accidents with no injuries do not require a formal police report.

also have data for average daily speeds for most days on the sample. The *Secretaría de Movilidad*, the public office in charge of the enforcement of traffic law in the city, provided me with the data for research purposes.

One question about the data is worth tackling in advance. I can only identify vehicle registration numbers (*placas*, in Spanish) rather than individual drivers. I grant that it will be ideal to have data on individuals, but there are serious concerns for the right to anonymity that prevent me from obtaining that information. But this is not a major concern since I am only looking at a brief period of time in which most of vehicles do not change owners[5]. In the following, whenever I talk about drivers, I am really referring to their vehicles, under the assumption that vehicles belong to an individual that is responsible for any fines. It is safe to assume that even if it is not the same driver that is riding the car, she will have known of the previous fine that her husband/coworker received. This fact is supported by the notion that in Bogotá, tickets are quite expensive.

I will use data on internet searches as a proxy for legitimacy of the police. It has been shown that Google Search data correlates strongly with the big topics in which citizens are interested in (Mellon, 2014), and reflects behaviors that have a significant effect on results like elections (Stephens-Davidowitz, 2014). Personal security and crime being something relevant for people's everyday life my assumption of searches related to the police reflecting police legitimacy and having an effect on their behavior thus seems reasonable. In this dimension, a paper that uses data in a similar way to what I try to do is the one by Muchow & Amuedo-Dorantes, (2020) that studies the effect of the fear of deportation by immigrants and its effects on domestic violence report to the police.

One should note that the period of study in a city like Bogotá was a time of increasing use, and social relevance, of social media. In this sense, social media is both a reflection and a driver of public opinion. Multiple studies show that the rhetoric present in social media has real effects in the world (Huang et al., 2023; Müller & Schwarz, 2023). Both searches and social media mentions are insufficient to show if salience of the police is because people recognize the police as a good or as a bad actor. For this purpose, I use national media. The most popular newspaper in Colombia, and Bogotá, is *El Tiempo*. I use headlines on news in *El Tiempo* as a measure of negativity towards the police.

[5] There are not public databases for verifying how often a vehicle changes owner. The best estimate is provided by Fenalco, an association of traders. The average vehicle changes owner every four years (FENALCO, 2022). So, it is safe to assume that a vast majority of the vehicles do not change hands six months after receiving a ticket. In fact, since unpaid tickets prevent drivers from transferring property of a vehicle, a significant group of drivers will only pay their tickets right before selling their car. Knowing that more than half of the tickets in the sample are unpaid a year after

To construct the scandal variable, I take all daily[6] values of the search word "policia"[7], Spanish for Police in Google trend from the start of year 2020 to August 2023 in the Bogotá region. I then divide the sample into 15 quintiles. *Scandal* days are all days in the highest decile and the following n days. *Not scandal* days are all days that are in the n days before a scandal day and are not themselves a scandal day. Scandals last one day more than the previous non scandal days in order to capture the exact day when police behavior occurred. All days that are not *Scandal* or *Not scandal* days are classified as *neutral*, meaning that there is no salience of police behavior in one direction or the other. I have two different samples. In my *scandal and non-scandal days only* sample I don't use neutral days. In some estimations I simply code *Not scandal* and *neutral* days that as 0. This I call the *All days* sample.

Figure 1 shows the number of scandal days with different duration of a scandal. Note that there are 1182 days in our sample, from June 1 of 2020 to August 31 of 2023. Note also that since scandal days are concentrated around particular events, there are more scandal than non-scandal days in my sample. I will come back to this issue in the first section of the Appendix.

Duration of a scandal	**1 day**	**2 days**	**3 days**	**4 days**
Scandal days = 1	90	117	142	161
Non scandal days = 0	27	44	54	68
Neutral days	1065	1021	986	953

Figure 1. Number of scandal and no scandal days, according to the assumed duration of a scandal on public perception.

It is important for my estimation that scandal days are not too heavily concentrated around specific dates, so I am really capturing the differential effect of police legitimacy and not any seasonality of the data. It is thus reassuring to note that all days of the week and all months of the year, but April, have both scandal and not scandal days. It is true that scandal days are somehow concentrated around the end of the year, with January accounting for 45% and December 9% of scandal days when using a duration of 4 days scandals, and similar numbers for other durations of scandals. Distribution for non-scandal days is similar. For yearly distribution of 4

[6] For periods longer than three months, Google Trends only shows weekly data. I first find the value for each week. Then I search each week independently to find daily values. The total daily value is the (normalized) product of the week and day score.

[7] Spanish readers will note that a spelling mistake is present. The correct word to write in Spanish is policía, with the accent mark. I take this decision based on the fact that the search volume with the mistake (mean of 64) is higher than without the mistake (mean of 13). Both series are highly correlated, and I will argue that most internet users make this minor mistake.

days scandals, 39% of scandal days (and 43% of non-scandal days) correspond to 2021. Similar values are found for other durations of scandals. In any case, all this seasonality will be captured by the year-week fixed effects included in the regression. Figure 2 shows the distribution of scandal and non-scandal days for the 2 days scandals for year 2020. Figure 6 of the Appendix does the same for the full years. One can see periods of intense unrest. I only define scandal days for the period in which the Cameras work, so early days of 2020, with significant unrest in the country, are not included as scandals in any of the regressions. In the second part of the Appendix I discuss whether scandals or social unrest are driving the results.

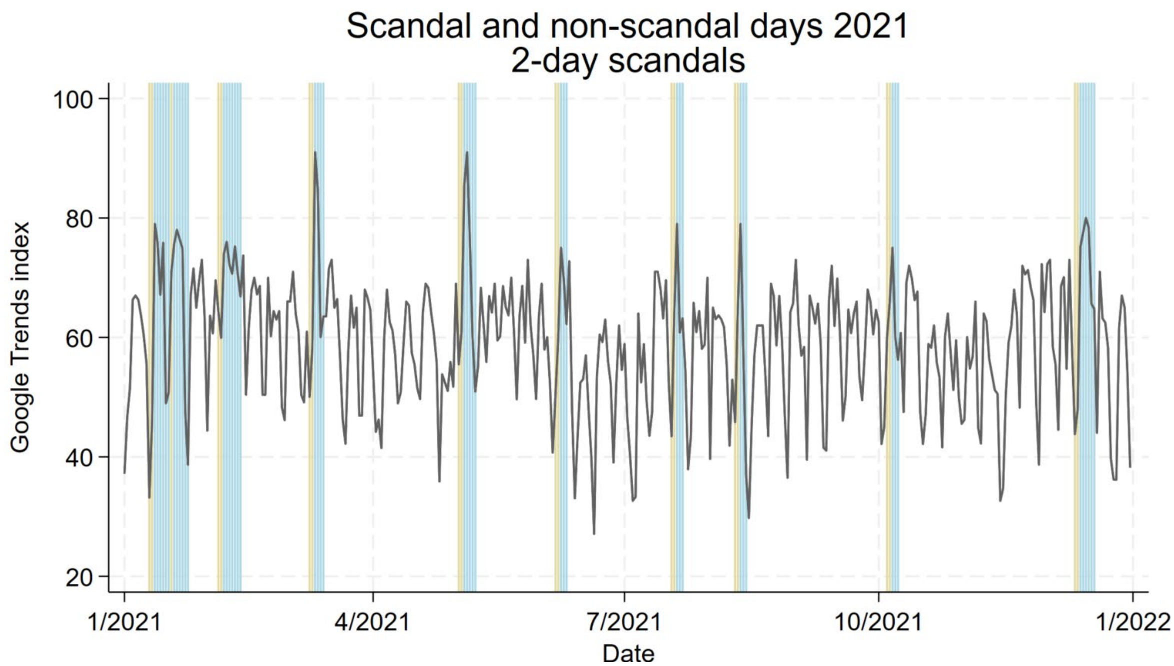

Figure 2. Distribution of scandals (in light blue) and non-scandal days (in light yellow) for the assumed duration of 2 days for scandals for the year 2021. Days not colored are neutral days.

A preliminary way to determine if scandal days are in fact caused by a stronger presence of the police in the mind of citizens is to compare this results with the results from print media. I search for the keyword Police on the database of El Tiempo, the main national newspaper. The number of news items on each day is an intuitive notion of how important the mentions for the police are. It is thus reassuring to observe in Figure 10 of the Appendix that for all definitions of scandal the average number of news items is bigger for scandal days than neutral days. Non scandal days have always the smaller number of average news items.

3. Identification strategy

The biggest challenge to any causal measure of the effect of traffic offenses on accidents or fatalities is selection. Drivers that do get traffic offenses might be significantly different than does who do not, and it is safe to assume that the source of this difference might have an effect on their probability of being in a car crash.

As I explained in the introduction, I avoid this selection effect by comparing drivers that do get tickets and measuring whether they are later involved in crashes or not. To further avoid the selection caused by having a crash before the first ticket and the selection of drivers that get multiple tickets, I will only compare only drivers on their first ticket. This will allow me to separate the effect of tickets and legitimacy from the experience of being in a crash or reoffending on a ticket.

For both of the samples I explained before, my estimation can be summarized in the following equation:

$$Y_{iot} = \beta * Scandal_{iot} + X_t + \theta + \mu + \pi + \rho + \tau + \delta + \varepsilon_{iot} \qquad (1)$$

Where the dependent variable Y_{iot} is any measure of behavior of vehicle *i* that received a ticket *o* after an offense on day *t.* My preferred variable is a dummy that takes the value of 1 for all drives involved in a crash less than six months after receiving a ticket, and 0 otherwise.

$Scandal_{iot}$ is my independent variable of interest. It is coded as 1 if the day *t* in which vehicle *i* received a ticket *o* is a scandal day *t*, and 0 when *t* is a non-scandal day (bleu and yellow in Figure 2). The coefficient of interest is β, which according to my hypotehsis should be positive, as it captures the causal increase in the chance of being involved in a crash because the traffic offense ticket was received on a scandal day, where police legitimacy is lower. My identification assumption is that after controlling for the number of tickets and accidents in any given day, and anything observable or unobservable related to the characteristics of the ticket and the type of vehicle (through fixed effects), receiving a ticket on a scandal or non-scandal day is as good as random to the probability of being in an crash[8].

X_t are a set of controls for the day on which the ticket was issued. I use the number of tickets and crashes per day to account for differential effect of police practices and/or behavior of drivers that might independently affect the chances of being in an crash. When I present my results, I will address the possible endogeneity of this variables[9].

θ is a fixed effect for the year-week in which the ticket was given.

μ is a fixed effect for the day of the week in which the ticket was given.

π is a fixed effect for the locality where the ticket was issued.

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[8] One paper that uses the same identification strategy is (Casey et al., 2018). (Hall & Madsen, 2022) use a very similar one.

[9] As I will argue later there are also other controls at the day level that are "bad controls." In particular, average daily speed.

ρ is a fixed effect for the source (Camera or Police) of the ticket.

τ is a fixed effect for the type of infraction of the ticket.

δ is a fixed effect for the type of vehicles, a dummy for motorcycles and not motorcycles.

ε_{iot} is an error term at the vehicle that got a ticket level.

4. Results

Before discussing estimation results, I present some general characteristics of my sample. Figure 3 shows the number of vehicles under the three possible conditions. In my estimation I only use rows 2 and 3. In fact, since we are only using vehicles that received a ticket before being in a crash or never had a crash, the total of different vehicles in the sample is slightly less than the sum of those two rows, exactly 1.405.713 vehicles.

Condition	**# of vehicles**
Had an accident, but no tickets	77.505
Had a ticket, but no accident	1.369.629
Had a ticket and accident	104.460

Figure 3. Number of vehicles by condition.

Figure 4 presents the average for all possible dependent variables used in the estimation. It also shows variables that I will use as fixed effects. Note how small the number of drivers that have had more than one accident is.

Full sample	
Dependent variables	
Had accident 6 months after first ticket	0.91 %
Had serious accident 6 months after first ticket	0.43 %
Had accident 12 months after first ticket	1.49 %
Had serious accident 12 months after first ticket	0.73 %
Average time between first and second ticket N= 550,177	223 days
Average time between first and second accident N=1481	2240 days
Year of first ticket	
2020 (June to December)	13%
2021	23%
2022	39%
2023 (January to August)	24%
Source of first ticket	
First ticket from a Camera	47%
First ticket from policemen on roads	53%
Type of vehicle	
Motorcycle	36%
Not motorcycle	64%
Type of infraction	
Speeding	47%
Other	26%
Pico y Placa (circulation in restricted times)	10%

Figure 4. Dependent variables and controls of sample used in estimation.

It the behavioral mechanism suggested is that citizens do not accept the “wake up call” of an illegitimate authority, and thus what matters is the condition on the day a ticket was received, one should observe an effect of scandals in other behaviors.

Figure 5 presents result for equation (1) with three results associated with driving: receiving a second ticket, the total number of tickets and the time to a second ticket. Even if magnitudes are small, around a 1% increase in all of these variables, they are statistically significant. One can safely say that drivers that get their first tickets after a scandal do behave worse after their ticket than drivers that get their tickets just before a scandal or at any other day. Drivers that get their first ticket on a scandal day are more likely to get a second ticket, have a higher number of total tickets and get their second ticket sooner. This is not rare behavior since 42% and 43% of drivers get at least a second ticket (*scandal and non-scandal days only* sample and *All days* sample, respectively). Once again, base rates are not the central issue here since I am looking at the differential effect of legitimacy.

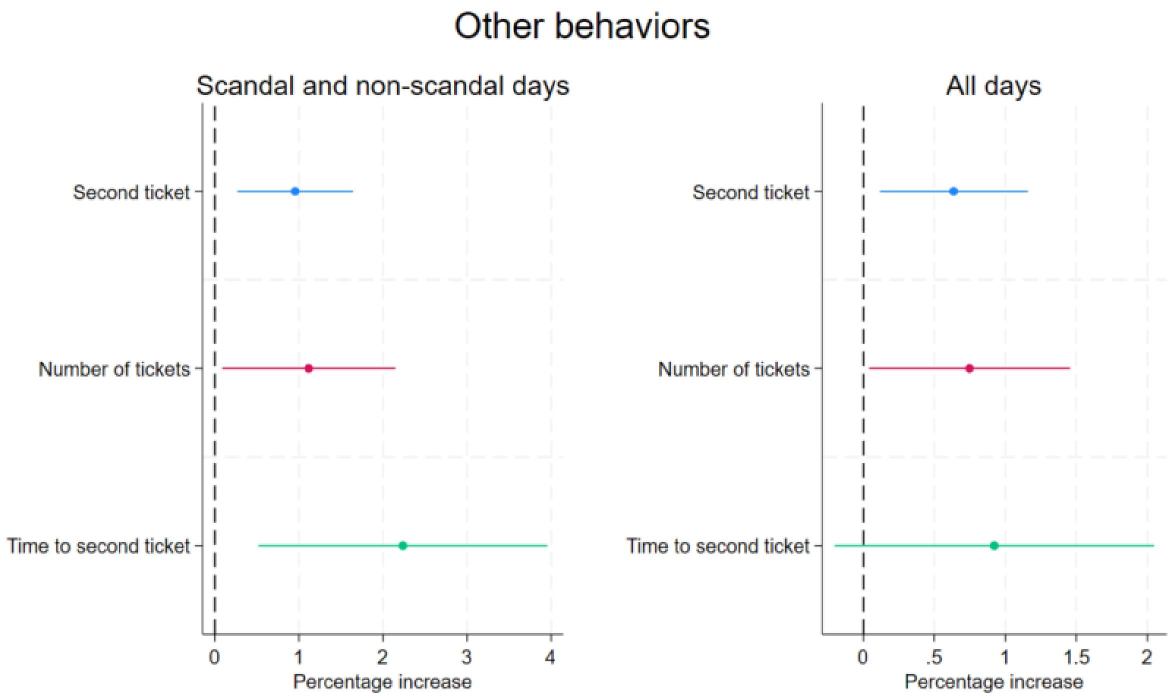

Figure 5. For first row dependent variable is 1 if the vehicle got at least a second ticket during the sample period. For second row dependent variable is the total number of tickets that the vehicle received. Third row dependent variable is the time between first and second ticket (coefficient is multiplied by -1 for presentation purposes). All coefficients presented as percentages. The number of tickets issued and accidents per day are used as controls. All estimations include the following fixed effects for the ticket: year-week, day of week, type of infraction, source of the sanction and locality where it was issued. Fixed effects for the type of vehicle are also included. All errors clustered at the date level. Row title explains the difference with main estimation. Column on the left corresponds to the sample used in Table 9, and column on the right to full sample used in Table 10.

A very strict reading of the previous result would say that these results could be related to the driving behavior that got drivers the ticket in the first place. A behavior that should be absolutely irrelevant is paying for the ticket, since this only could change because of fairness considerations. As Figure 6 shows, for all durations of a scandal, tickets on scandal days are less likely to be paid. Magnitude is small, but not negligible. There is a 0.4% additional reduction on the probability of payment for first tickets received on scandal days. While this number is small, it is very precisely estimated. This is true for most of the possible durations of a scandal, even if slightly not significant on the *All days* sample. Figure 6 of the Appendix shows that the result holds if dependent variable is the probability of ever paying for the ticket (a less strong measure of legitimacy since it captures the need to pay the fine in order to legally sell a vehicle).

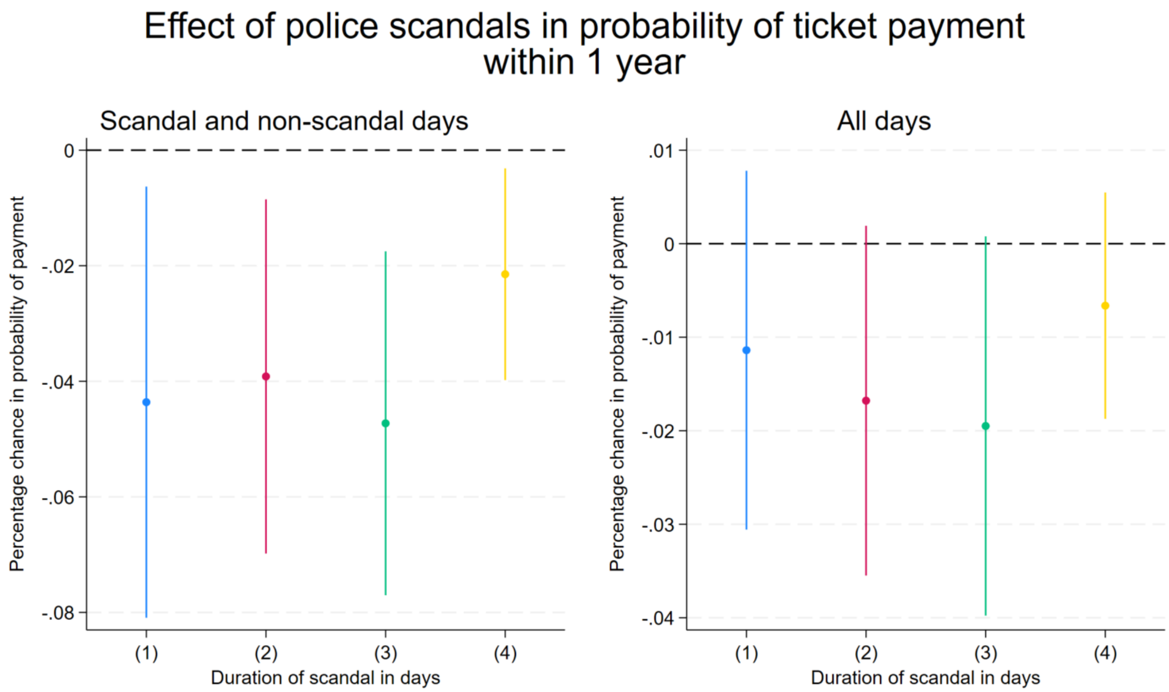

Figure 6. Dependent variable is 1 if the driver paid for the ticket in the following year (0 otherwise). The number of tickets issued and accidents per day are used as controls. All estimations include the following fixed effects for the ticket: year-week, day of week, type of infraction, source of the sanction and locality where it was issued. Fixed effects for the type of vehicle are also included. All errors clustered at the date level.

While the previous results are important, one needs to measure if this other behaviors translate into the most important variable when talking about road traffic; accidents. Table 7 synthetizes the estimation of equation (1) with having a crash in the six months after a ticket as dependent variable. It is impossible to know from the available data whether each vehicle has had accidents before the start of my sample. The decision taken is to consider the first six month of 2020 as a gap period, assuming that all vehicles that did not have an crash in those months do not have one previously. As I said before it is also a good decision to only consider the period after the Cameras Program started working since this program introduced a significant change in the probability of receiving a ticket, and thus probably on the behavior of drivers.

Results are in line with my hypothesis, since the sign and magnitude of the coefficient is the expected positive one. Note also that due to the low probability of an crash in the sample, the effect is between 14% and 21% (columns 4 and 2) increase in the chance of being in a crash after receiving a ticket on a scandal day in comparison to a non-scandal day. I should note that the results are essentially the same if I don’t control for the number of tickets and accidents per day on the date on which the ticket was received. I still think it is relevant to adjust for the level of control by the police (tickets per day) in order to separate the effect of police legitimacy from other reasons why future behavior might be affected, but I present those results in

Figure 7 of the Appendix. In Figure 8 of the Appendix I also show how scandal days do not predict either a higher number of tickets or crashes on each day. As Figure 9 of the Appendix shows it is more important to control for the total of crashes on scandal days, particularly for the *All days* sample.

Effect of police legitimacy on road accidents				
Dependent Variable	Accident Six months	Accident Six months	Accidents Six months	Accident Six months
	(1)	(2)	(3)	(4)
Duration of a scandal	1 day	2 days	3 days	4 days
Scandal days/non scandal days	90/27	117/44	142/54	161/68
β*100	0.11	0.21	0.14	0.15
	(0.11)	(0.08)***	(0.08)*	(0.07)**
Mean probability of an accident (percentage)	1.00	1.00	1.03	1.05
Observations on sample	137,393	176,528	206,192	233,865
Fixed effects	X	X	X	X

Table 7. Dependent variable is 1 if the vehicle was involved in an accident less than six months after the first ticket. Number of tickets issued and accidents per day are used as controls. All estimation include the following fixed effects for the ticket: year-week, day of week, type of infraction, source of the sanction and locality where it was issued. Fixed effect for the type of vehicle is also included. All errors clustered at the date level.

A natural way to extend the model is to code all days that are not scandals as non-scandal days, what I defined as the *All days* sample. This increases the sample size and allows for more power on the estimation, and to alleviate any concerns for the unequal distribution of scandal days driving the results. Results shown in Table 8 are essentially the same but with a naturally smaller magnitude, around half of the previous estimation, and some considerable losses in significance. It is natural for this to happen as I am comparing more dissimilar days, still on column 2 I find a 13% increase in the chance of being in a crash. From now on, column 2 is my benchmark estimation and will use it for all my robustness checks. As it occurs in both tables, 2-day scandals show a bigger effect than any other day, but 4-day scandals have a slightly bigger effect than 3-day scandals. One should note that the result is significant at the 10% level in all three of these durations. I discuss the issue of scandal duration on the first separate chapter on the Appendix.

Effect of police legitimacy on road accidents				
Dependent Variable	Accident Six months	Accident Six months	Accidents Six months	Accident Six months
	(1)	(2)	(3)	(4)
Duration of a scandal	1 day	2 days	3 days	4 days
Scandal days/non scandal days	90/1092	117/1065	142/1040	161/1021
β*100	0.06 (0.05)	0.12 (0.05)**	0.09 (0.05)*	0.11 (0.05)**
Mean probability of an accident (percentage)	0.91	0.91	0.91	0.91
Observations on sample	1,278,291	1,278,291	1,278,291	1,278,291
Fixed effects	X	X	X	X

Table 8. Dependent variable is 1 if the vehicle was involved in an accident less than six months after the first ticket. Number of tickets issued and accidents per day are used as controls. All estimation include the following fixed effects for the ticket: year-week, day of week, type of infraction, source of the sanction and locality where it was issued. Fixed effect for the type of vehicle is also included. All errors clustered at the date level.

5. Robustness checks

I can also do the same exercise with all other dependent variables in order to prove that I am really capturing an effect of police scandal. A first logical step is to explore different durations for the dependent variable. Figure 19 uses column 2 of each table and estimates 3, 6, 9 and 12 months to the first crash. The fact that the effect is considerably higher for the 3 and 6 months time frames than for the 9 and 12 months frames, is a sign that scandals last in the driver's memory for a significant period of time but eventually fade. Since the mechanism is past frustration with police scandals, then it is natural for it to diminish to the point that it is forgotten, as it occurs after 9 months on the *All days* sample. I argue that this fading-out of the effect supports the behavioral mechanism I have in mind.

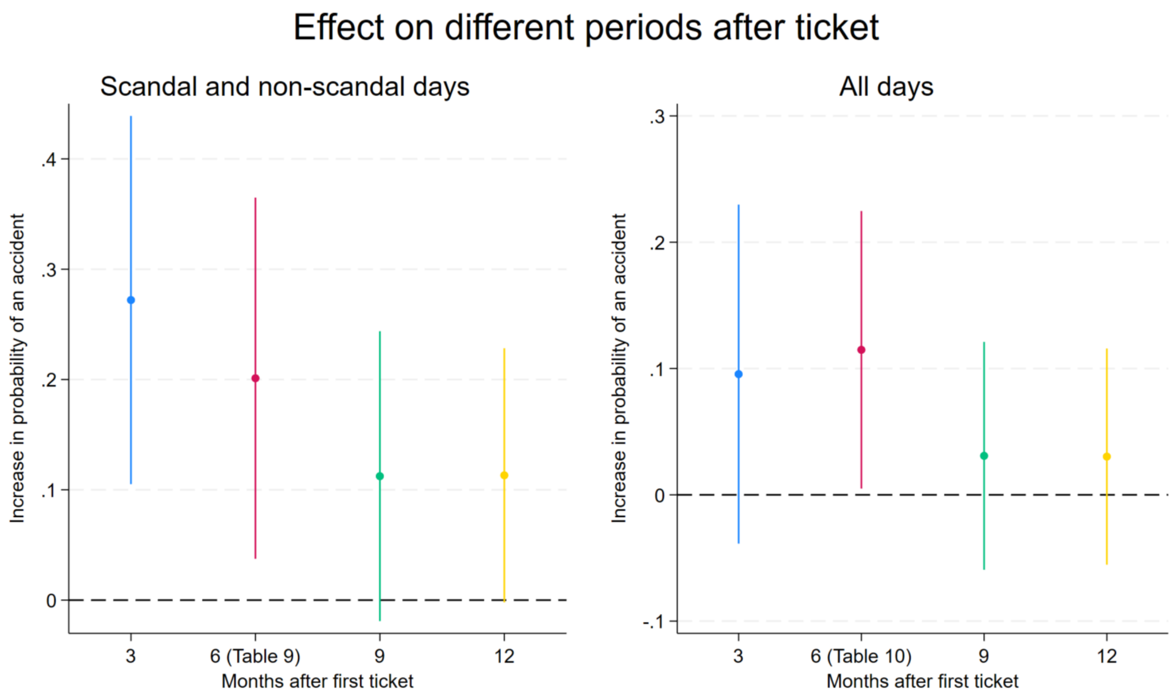

Figure 9. Dependent variable is 1 if the vehicle was involved in a crash less than three, six, nine or twelve months after the first ticket. The number of tickets issued and crashes per day are used as controls. All estimations include the following fixed effects for the ticket: year-week, day of week, type of infraction, source of the sanction and locality where it was issued. Fixed effects for the type of vehicle also included. All errors clustered at the date level.

Another interesting question is the effect that receiving a ticket on scandal days can have on the chance of being in a serious crash (where at least one person is injured). Figure 10 of the Appendix explores this aspect. Note that if dependent variable is a dummy for being in a serious crash six months after the first ticket, the coefficient is negative and not statistically significant. When the dependent variable only includes non-serious accidents, the effect is bigger than when all accidents are included. All hits point to the effect being driven by non-serious crashes and it suggests that serious crashes are more of a random occurrence[10]. Once again, this supports the consistency of the effect found in my main estimation since shows that the effect is not concentrated on the rare aggressive drivers, but in the most common ones.

Figure 10 of the Appendix shows that the effect is not driven by a subset of the sample. Neither the drivers that get a second crash (0.84% of the sample) nor the ones that get a higher number of tickets (89% of the sample gets less than 4 tickets) explain the result. In fact, the magnitude of the coefficient is slightly bigger when

[10] I find a more consistent result when I estimate and ordered probit. In this case the dependent variable takes values of 0 (no accidents), 1 (non-serious crash) or 2 (serious crash). The marginal effect (not reported) of a scandal day is positive for being in a crash or being in a serious crash, and significant at the 10% level.

those crash and ticket prone populations are excluded. I will go back to the fact that scandals seem less important for repeated offenders that get multiple tickets.

If my scandals conceptualization is wrong, one should find an effect by simply using the distribution of the Google Trends Index. Figure 10 uses this distribution in different ways. All point to the fact that tickets issued on days where the police actions are more salient are less efficient at reducing accidents. But simply using the distribution does not capture a statistically significant effect. In short, something important is obtained by assuming that scandal lasts some days in the mind of drivers.

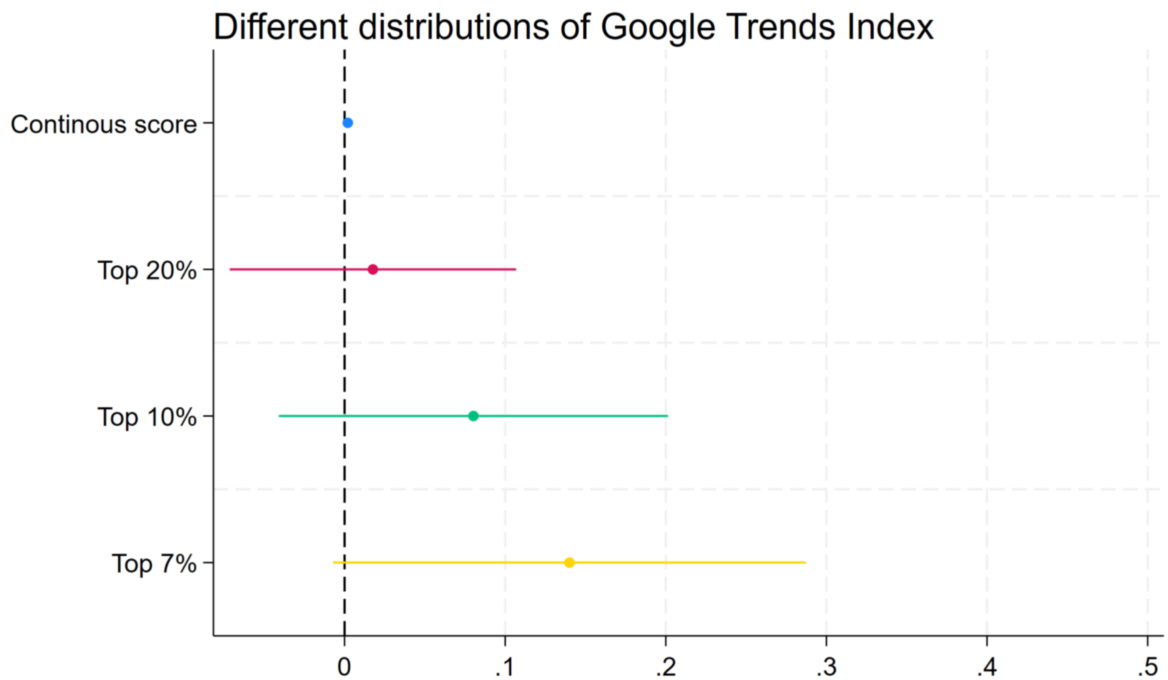

Figure 10. Dependent variable is 1 if the vehicle was involved in a crash less than six months after the first ticket. The number of tickets issued and crashes per day are used as controls. All estimations include the following fixed effects for the ticket: year-week, day of week, type of infraction, source of the sanction and locality where it was issued. Fixed effects for the type of vehicle are also included. All errors clustered at the date level. Coefficient shown corresponds to the effect of a one-point increase in the Google Index Score (row 1) difference between the top and bottom percentages of the distribution when the top group corresponds to top 10% (5 quintiles), top 10% (10 quintiles) or top 6.7% (15 quintiles) (row 2 to 4).

The construction of scandal days captures a bigger effect than just the distribution of the Google Trend index. But it is also important for me to show that nothing specific about the construction of scandal days is driving the effect. A whole chapter in the Appendix deals with the assumed duration of a scandal. Here I show that the result is not dependent on the decision of dividing the sample into 15 quintiles and defining the start of a scandal as any day on the higher decile (in this case the top 6.7% of days). Figure 11 shows the effect when the number of quintiles ranges from 10 (top 10%) to 20 (top 5%). The effect is centered around the chosen distribution but is not specific to it. In fact, the effect is highest when I divide the sample into 16 quintiles. Both too big (10 quintiles, top 10%) and too small quintiles (20 quintiles, top 5%) make the effect disappear. One could argue that too big quintiles include days that are not really related to the behavior of the police. Harder is to explain why too small quintiles also lose the effect, but I would argue that the top 5% days are not only different to the 7% top days in terms of the behavior of police, but they are in terms of other things happening on the news cycle. Too small quintiles capture something other than police behavior and the number of drivers ticketed on scandal days becomes too small in that scenario.

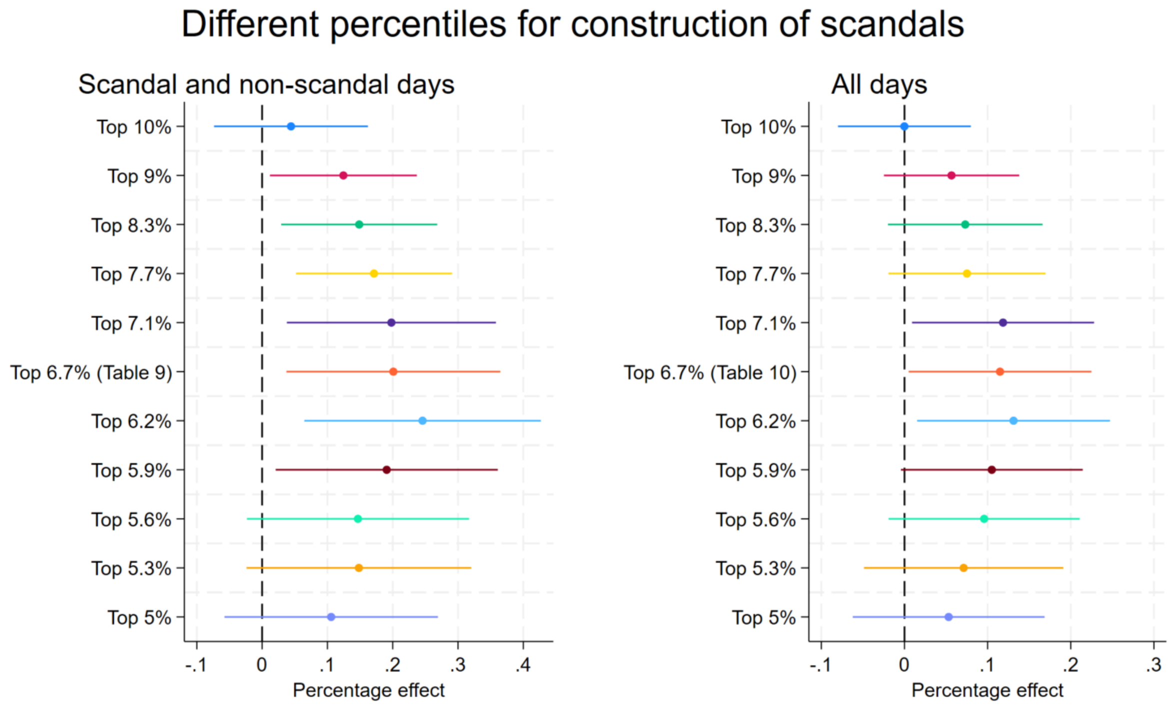

Figure 11. Dependent variable is 1 if the vehicle was involved in ac crash less than six months after the first ticket. The number of tickets issued and crashes per day are used as controls. All estimations include the following fixed effects for the ticket: year-week, day of week, type of infraction, source of the sanction and locality where it was issued. Fixed effects for the type of vehicle are also included. All errors clustered at the date level. The coefficient shown corresponds to the effect of scandals on the probability of having a crash, by changing the percentile I use for defining a scandal. Row 1 corresponds to dividing the distribution into 10 quintiles (top 10% of days) and row 11 into 20 quintiles (top 50% of days). Each row increases by one the number of quintiles (row 2= 11 quintiles, row 3=13 quintiles).

Finally, one should note that the Google trend index is a relative measure. The database I use starts in the second week of January 2020. Row 1 of Figure 14 shows that little of the result depends on where I start the database. Results holds if I only count scandals from June 2020. Same general result (not reported) is found if the database starts in December 2019. In Row 2 I examine whether I am identifying a local effect. Reassuringly, if the Google trends index not only contains searches within Bogotá, but the whole country, the result changes significantly. This points to local perception driving the result.

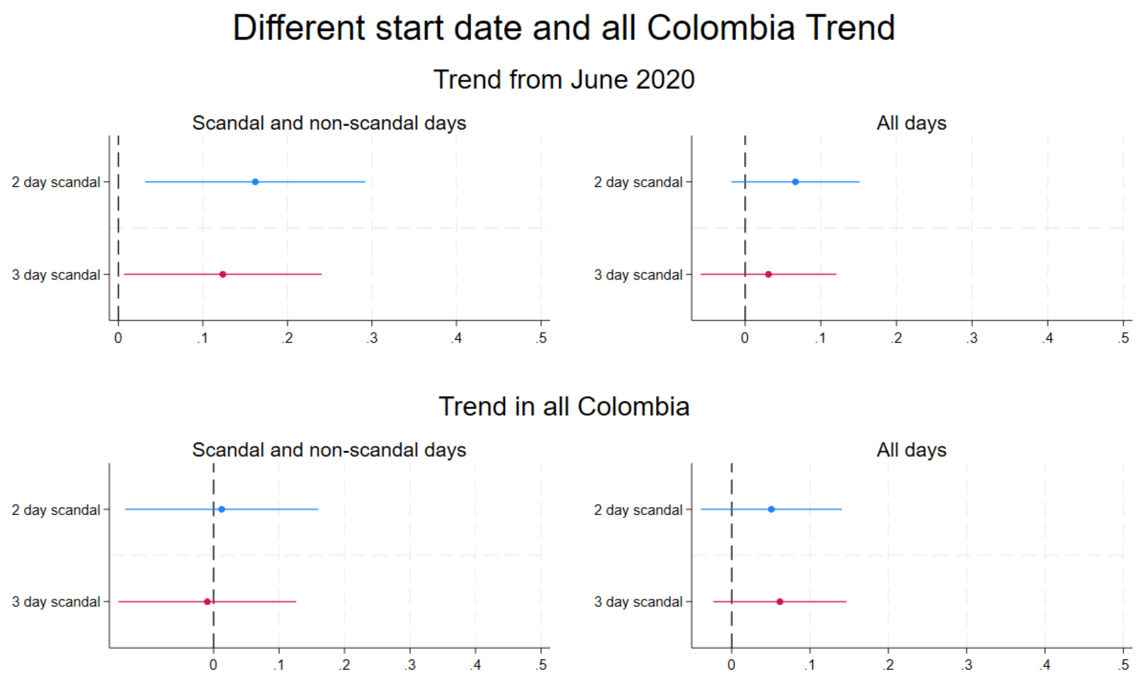

Figure 12. Dependent variable is 1 if the vehicle was involved in a crash less than six months after the first ticket. The number of tickets issued and accidents per day are used as controls. All estimations include the following fixed effects for the ticket: year-week, day of week, type of infraction, source of the sanction and locality where it was issued. Fixed effects for the type of vehicle are also included. All errors clustered at the date level. Row title explains the difference with main estimation. Column on the left corresponds to the *scandal and non-scandal days only* sample used in Table 9, and column on the right to *All days* sample used in Table 10.

Figure 13 varies some of the estimation parameters to show that results are robust to other decisions I make. All estimations are done with two-day and three-day scandals (Column 2 and 3 of Table 9 or 10) as baseline. Row 1 shows that the result is robust to starting the estimation in February, even though it risks including vehicles that got their first crash in the months previous to the start of my sample period. Row 2 allows for the effect of a ticket to “erase” after six months. If a vehicle gets the first ticket and after six months it has not had a crash, this ticket is erased and the next ticket is used in the estimation as if it were the first ticket. This only increases the size of the sample on around nine thousand additional vehicles. Both changes only reduce the effect on the *All days* sample slightly but keep the *scandal and non-scandal days only* effect very strong. The last row is very reassuring as it signals that it is not occasional drivers who drive the result.

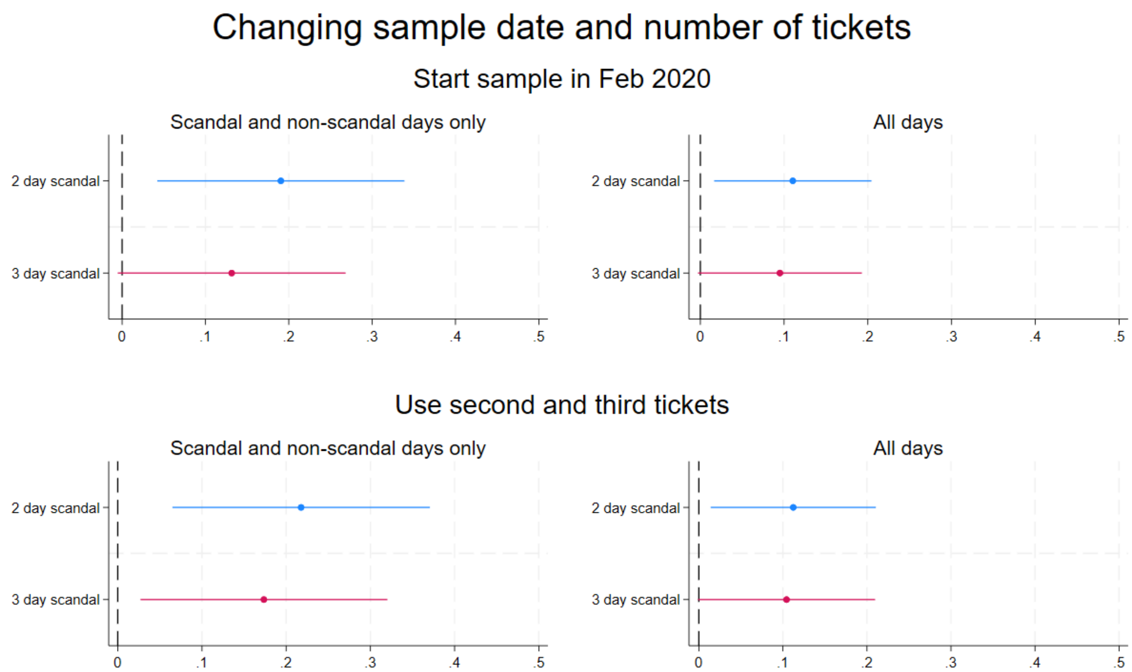

Figure 13. Dependent variable is 1 if the vehicle was involved in an crash less than six months after the first ticket. The number of tickets issued and crashes per day are used as controls. All estimations include the following fixed effects for the ticket: year-week, day of week, type of infraction, source of the sanction and locality where it was issued. Fixed effects for the type of vehicle also included. All errors clustered at the date level. Row title explains the difference with main estimation. Column on the left corresponds to the *scandal and non-scandal days only* sample used in Table 9, and column on the right to *All days* sample used in Table 10.

Most importantly, I can check whether another word related to public affair is causing the results. Table 11 and 12 of the Appendix use two similar words related to public affairs: *corrupcion* (corruption) and *alcaldia* (mayor's office)[11] and repeat the estimation on table 9. Constructing the independent variable in the same way leads to negative and non-significant coefficients for both words, across all eight different specifications of a corruption-scandal or mayor's office scandal. Results (not reported) are equally not significant if we repeat the estimation on Table 10. This reaffirms my idea that it is something specific to the authority in charge of implementing the law, the police, that explains the driving the result.

6. Is it really legitimacy? A "Sentiment analysis"

It is possible that even if my story is convincing and the effect does not depend on specific decisions, what I am capturing is not really about police legitimacy. I need to show sufficient evidence that the Google Trends Index I use really captures events

[11] Once again, I use both words with their incorrect spelling (corrupción and alcaldía, being the correct ones) since the incorrect forms is around five times more common in terms of total searches.

that might reduce police legitimacy. For this purpose, I can provide evidence using the digital database of *El Tiempo*, the most widely read (by far) newspaper in the country. Table 14 of the Appendix shows a manual exercise in which I check for headlines related to the police in the top 20 days with a higher score. Note that in fact the highest score is for a day in which the police killed a citizen during an arrest procedure, arguably the biggest police scandal in Bogotá's recent history. In total, in the top 10, there are 7 cases of misbehavior by the Police and 3 cases in which the Police was the victim. For the top 20, the Police was the aggressor in 13 days, the victim in 5, and 2 days are hard to classify since they refer to general insecurity.

I can do a more numeric analysis with the digital archive of El Tiempo: Using the search word police as a filter on the digital archive I can compare the average number of news between different types of days. As I did when building the Google Trend Index, my search word in the archive is Police (*policia*, in Spanish, the *El Tiempo* archive is not sensitive to spelling). A first element one wants to see in the data is that, on average, there is a raw positive correlation between the Google Trends index score and the number of news items. Figure 15 of the Appendix presents a scatterplot in which the two variables are positively correlated.

One can compare specific days. According to my hypothesis the most natural comparison is between days that define a scandal (top quintile) and any other day. Using a t-test in which the null hypothesis is that the number of news each days is equal, in all cases the p-value is smaller than 0.001 for the alternative hypothesis that days that define a scandal have more news related to the police. This is true if one uses all headlines that appeared in the newspaper, if one excludes news from other cities or if one only includes news on the Bogotá section. It is also true if one compares news that include the word Police or its synonyms in the headline. In total there are six headlines databases, and the result is the same for all of them. I argue that this is the best possible comparison, since my argument depends on the news of a top quintile day lasting in drivers mind for multiple days. But one can also compare news on scandal and non-scandal days.

In fact, when comparing scandal and non-scandal days the alternative hypothesis that days just before a scandal have more news related to the police than scandal days (*scandal an non-scandal days* sample) has a p-value smaller than 0.05 only in two of the six possible databases (only headlines with the world police in all sections of the newspaper and general interest sections and Bogotá section of the newspaper). In the other four databases there are no significant differences. I can also do this exercise on the *All days* sample. I find the same result. For all possible database of headlines, the p-value is smaller than 0.001 for the alternative hypothesis that days that scandal days have more news related to the police than non-scandal days.

Before using the content of the news in El Tiempo, I want to show two additional sources that can help validate that the Google index, and thus my scandal days variable, is really capturing something meaningful about police legitimacy. The first one is the Armed Conflict & Event Data Project (ACLED, from now on). This database processes and stores data on protests and local conflict around the world. I use their data for the period of study on conflict in Bogotá[12]. ALCED data consists of three types of events: protests, riots and violence against civilians. Full details of the analysis is shown in the second subtitle of the Appendix, but the general conclusion is that scandal days have more conflicts than non-scandal days. This result is captured by the prevalence of riots, not only by protests. This is true both for the *scandal and non-scandal days* sample and the *All days* sample. All this points to the Google Trend Index as capturing relevant episodes of social unrest at the local level.

The other source is the messaging app Twitter (now called X). I have a representative sample of 28,534 Twitter users for the duration of the National Strike (from April 28 to June 29, 2021). I can measure how closely my Google trends index maps into the variation on number of tweets, retweets or likes for any given day. Figure 16 of the Appendix shows that trends are closely matched only when retweets about the police are used. It is particularly relevant that when using tweets about the police the existing peak is similar in both sources, thus giving support to my construction of the scandal variable. One should bear in mind that the Twitter data is not available for the full period of my analysis[13].

There might be many reasons for people to be searching for news about the police. Distinguishing those reasons is a way to further test my hypothesis. If my findings are related to police legitimacy the effect of police scandals should be higher when it is clear that the police misbehaved. On the contrary, if people search the world police because someone attacked a policeman the effect should be smaller.

I can do a first manual analysis in which I select four cases in which the police were clearly the victim of someone else's actions. I select four dates in which policemen were violently attacked, either on or off-duty (Table 17 of the Appendix shows specific days and headlines). Note also that these four are pure police scandals (this is the reason for not including Jan 17 and 18 of 2022, which are part of a longer period of social unrest). When I run equation (1) with this adjustment, results (not reported) are in line with more negative scandals having a bigger effect and are still significant. Probability of being in a crash increases from 20 to 24% (column 2) on the *scandal and non-scandal days only* sample. Similar changes occur in the *All days* sample.

In summary, while not definitive there are reasons to think that the scandal variable is really capturing negative sentiment towards the police. One should note that media

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[12] I downloaded the data from: https://acleddata.com/data-export-tool/ on May 6 of 2024.

[13] Past Twitter data has had severe limitations recently. While is not ideal to only have some days on sample, it is hard to think that the Google Trends Index matches Twitter on this period but not on the unobserved dates.

is an imperfect measure since it only shows what the media presents, but data is scarce on what citizens actually read.

7. Discussion.

This paper tried to answer if the deterrent capacity of police sanctions weakens when the police is perceived as less legitimate. The case used to study this question was police tickets and road accidents in Bogotá, Colombia. Police legitimacy was measured using police scandals, assuming that when the police is present on the public debate it is usually because of police misbehavior. Data from Google trends (and national media) was used to measure the days in which police scandals occurred, and their effect on driver behaviors. To avoid endogeneity, the estimation includes only drivers that got a first ticket in the sample period. Placebo estimations using other words that capture the general sentiment of public opinion showed that the behavior is only affected by the direct authority imposing the sanction, and that this instantaneous illegitimacy is capable of affecting multiple behaviors.

Tickets issued by the police just after a police scandal are less effective at preventing traffic accidents, and the magnitude has real world importance. In comparison to receiving a ticket just before a police scandal, receiving a ticket after a scandal increases the chance of being in a crash in the following six months by 21%. This conclusion was robust to comparing scandal days to the days just before a scandal (non-scandal days) or comparing scandal days to any other day in the sample. It is also quite robust to different decisions on the time in which the sample is studied, and slight variations on the dependent variable.

The evidence shown points to legitimacy affecting in measurable ways the work done by the police. It seems that the effectiveness and deterrence of police work is not independent of how the public perceives their actions. The fact that in this context the more negative news of police actions make the speeding sanctions even less deterrent is a clear indication of this. Future work should measure if there is a trade-off between severity of sanctions and their legitimacy, and what is the optimal way to decide police enforcement of the law.

These results also give important lessons on the public policy challenges around road accidents and fatalities, pointing to a tradeoff between reach and deterrence. Most road policing strategies in the developing world try to increase the number of tickets given on the roads. This paper suggests that this strategy might be ineffective if police legitimacy remains low. It might make more sense to concentrate efforts on increasing police legitimacy with the existing number of tickets, rather than giving tickets to a higher number of drivers. Future research could measure if a similar logic operates in other public policy areas where the state's capacity to punish all wrongdoers is also severely limited.

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Appendix:

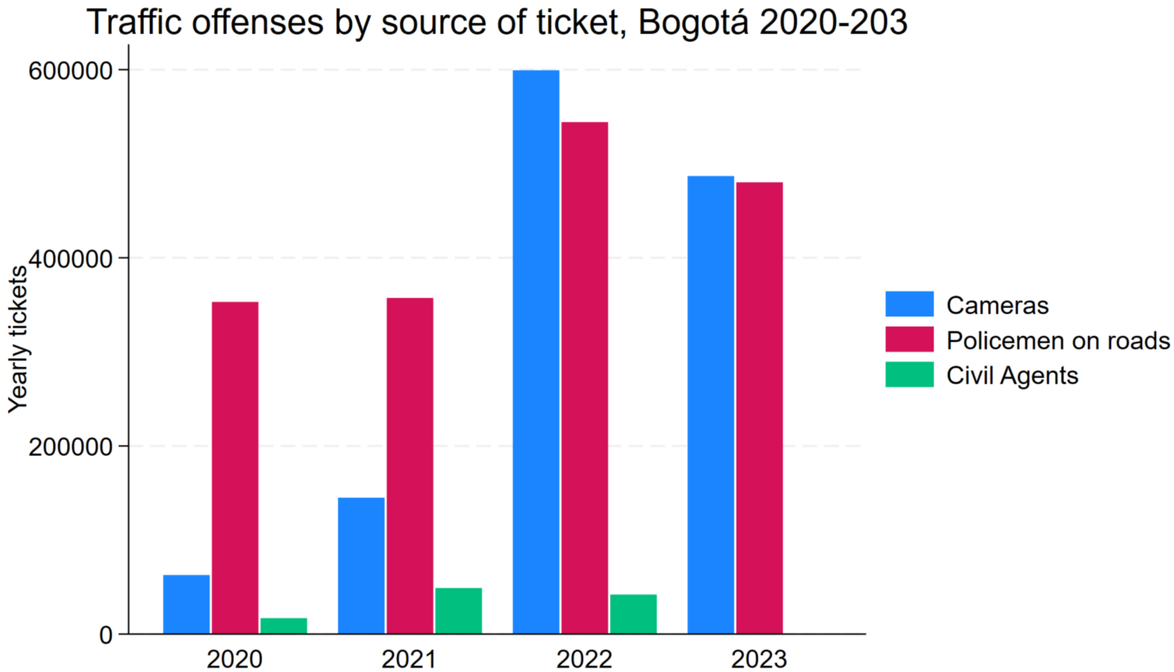

Figure 1. Traffic offenses by source of ticket. Data is preliminary for 2023, since August is the last month with available data.

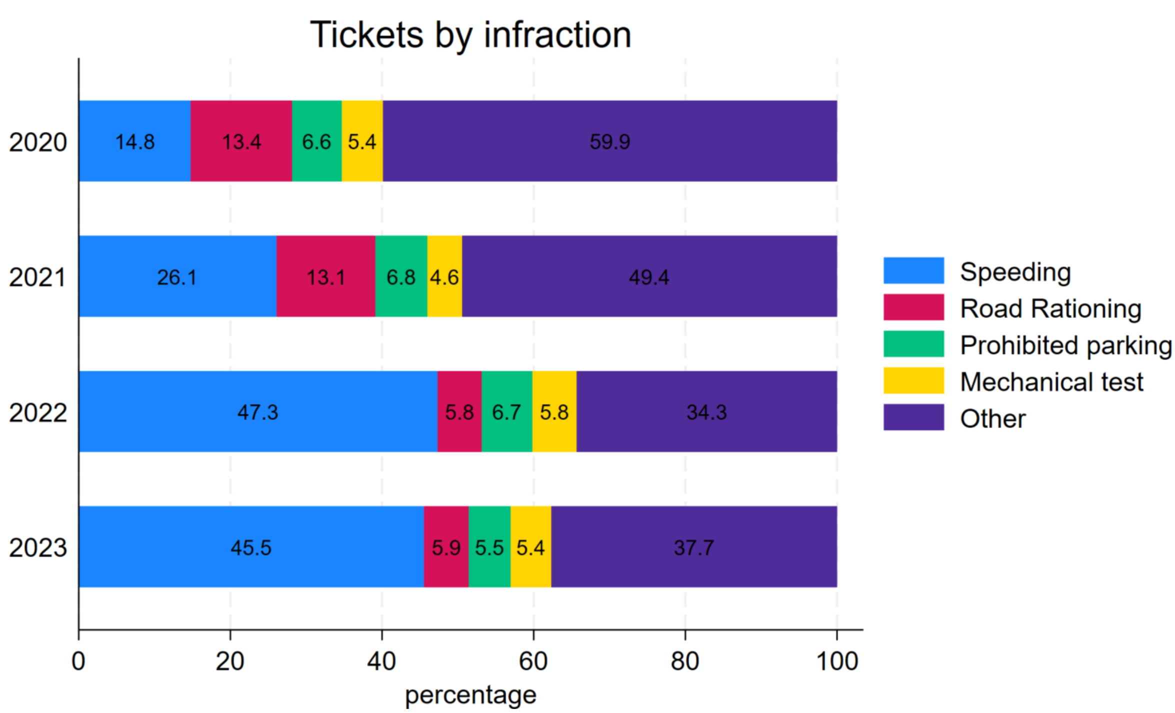

Figure 2. Tickets by year and type of infraction. “Road Rationing” refers to a congestion reducing system in which, depending on the registration number, cars cannot circulate on certain days and times. “Mechanical test” refers to a national mandatory technical revision for all vehicles that have been circulating for at least 5 years.

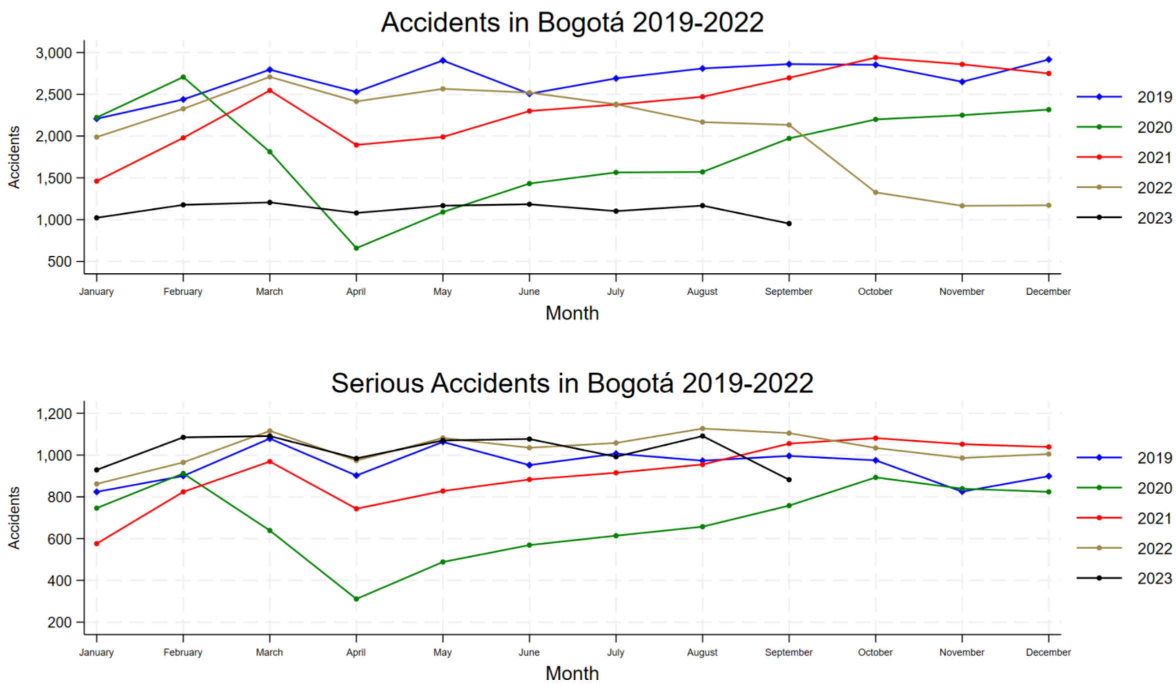

Figure 3. Number of traffic accidents and serious traffic accidents (with at least one person injured) occurring in Bogotá by month and year.

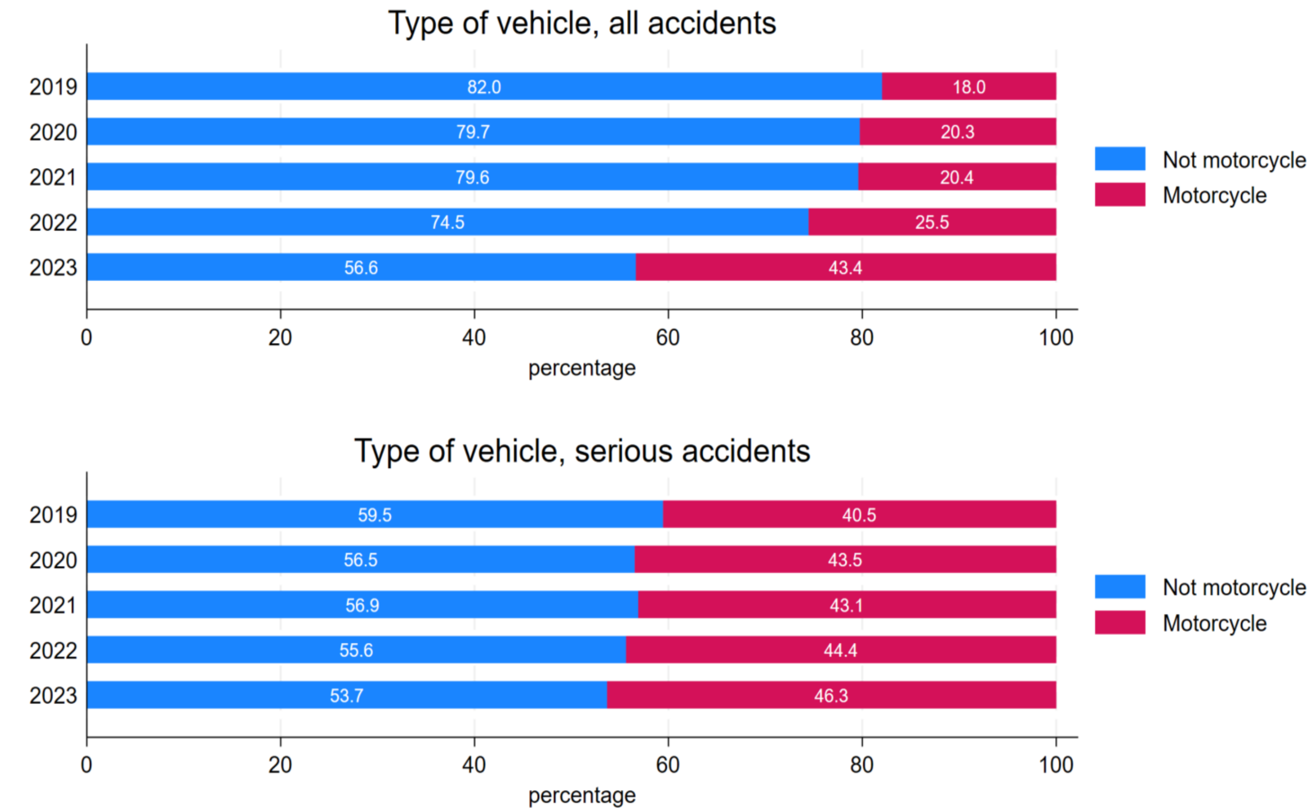

Figure 4. Participation of motorcycles in all accidents and serious accidents.

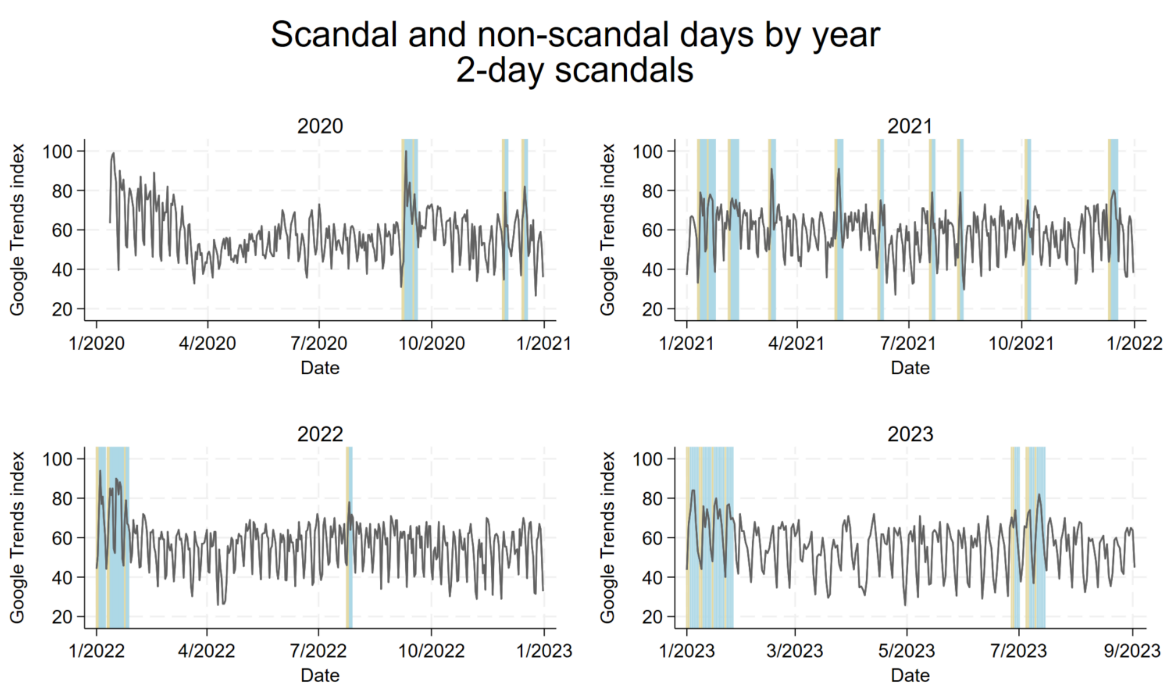

Figure 5. Distribution of scandals (in light blue) and non-scandal days (in light yellow) for the assumed duration of 2 days for scandals.

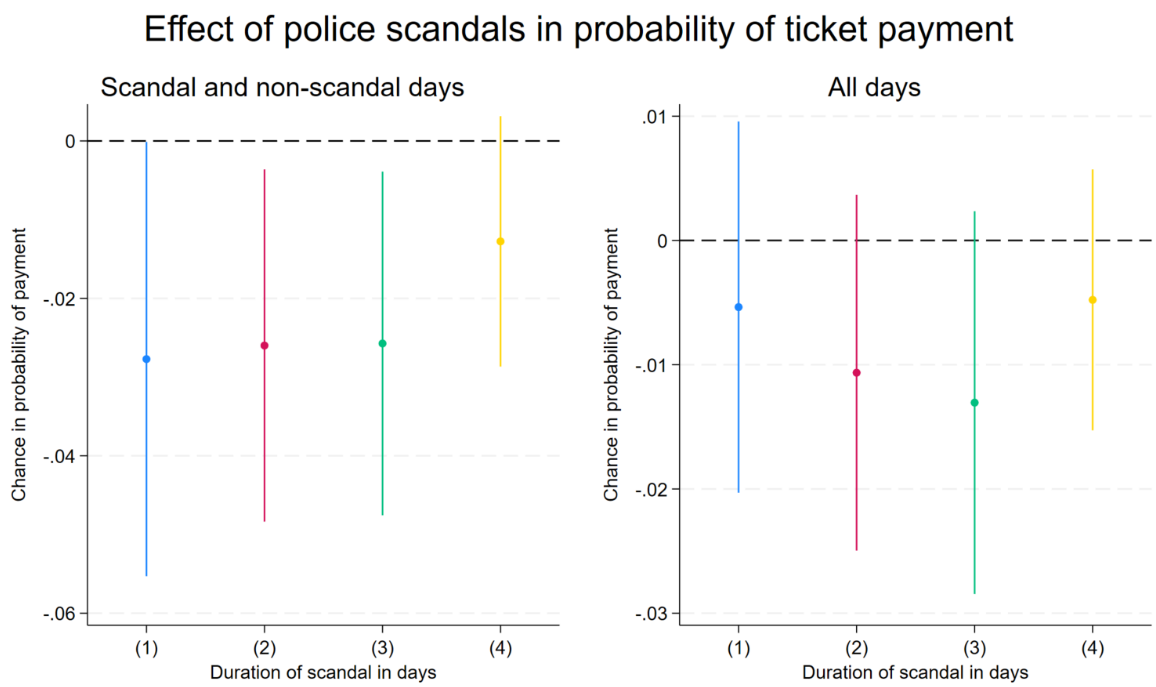

Figure 6. Dependent variable is 1 if the driver ever paid for its first ticket. The number of tickets issued and accidents per day are used as controls. All estimations include the following fixed effects for the ticket: year-week, day of week, type of infraction, source of the sanction and locality where it was issued. Fixed effects for the type of vehicle are also included. All errors clustered at the date level.

Effect of police legitimacy on road accidents

Dependent Variable	Accident Six months	Accident Six months	Accidents Six months	Accident Six months
	(1)	(2)	(3)	(4)
Duration of a scandal	1 day	2 days	3 days	4 days
Scandal days/non scandal days	90/27	117/44	142/54	161/68
β*100	0.11	0.20	0.13	0.15
	(0.12)	(0.08)**	(0.07)*	(0.07)**
Mean probability of an accident (percentage)	1.00	1.00	1.03	1.05
Observations on sample	137,393	176,528	206,192	233,865
Fixed effects	X	X	X	X

Table 7. Dependent variable is 1 if the vehicle was involved in an accident less than six months after the first ticket. Number of accidents per day used as controls. All estimation include the following fixed effects for the ticket: year-week, day of week, type of infraction, source of the sanction and locality where it was issued. Fixed effect for the type of vehicle is also included. All errors clustered at the date level.

Figure 7. Table 7 without control for the total number of tickets

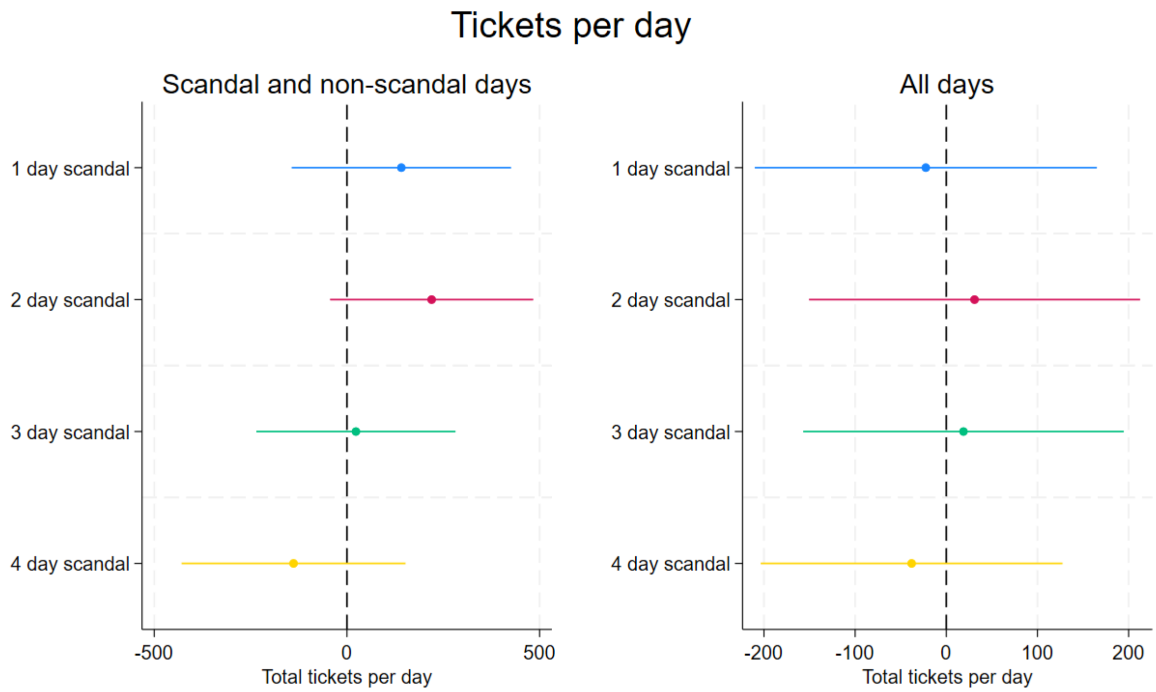

Figure 8. Dependent variable is the total number of tickets per day. Controls for year, month, week, and day of week included for each date.

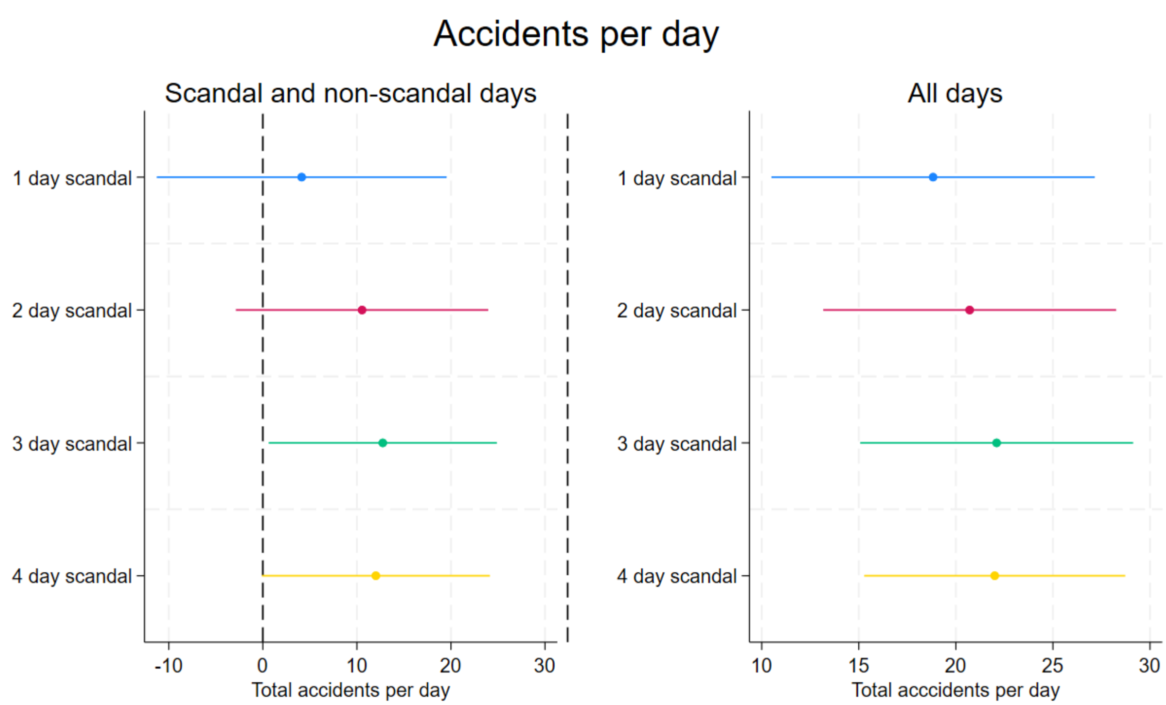

Figure 9. Dependent variable is the total number of accidents per day. Controls for year, month, week, and day of week included for each date.

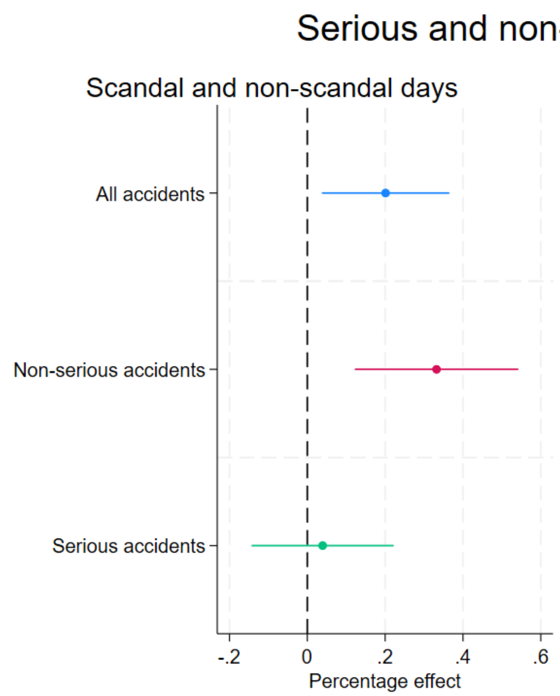

Figure 10. Dependent variable is 1 if the vehicle was involved in a non-serious or a serious crash six months after the first ticket. The number of tickets issued and crashes per day are used as controls. All estimations include the following fixed effects for the ticket: year-week, day of week, type of infraction, source of the sanction and locality where it was issued. Fixed effects for the type of vehicle also included. All errors clustered at the date level.

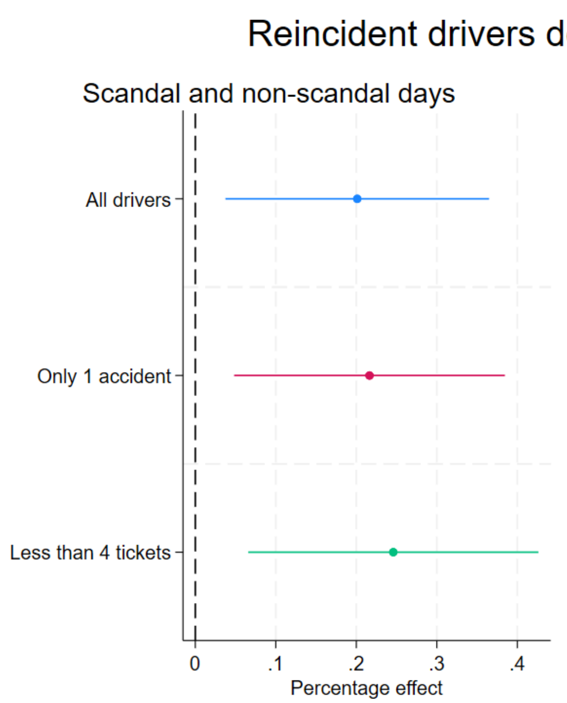

Figure 11. Dependent variable is 1 if the vehicle was involved in a crash six months after the first ticket. Row 1 uses the full sample, row 2 excludes all drivers that are involved in more than one crash and row 3 all drivers that get more than 4 tickets in the sample. The number of tickets issued and accidents per day are used as controls. All estimations include the following fixed effects for the ticket: year-week, day of week, type of infraction, source of the sanction and locality where it was issued. Fixed effects for the type of vehicle also included. All errors clustered at the date level.

Effect of legitimacy on road accidents. Corruption as Keyword

Dependent Variable	Accident Six months	Accident Six months	Accidents Six months	Accident Six months
	(1)	(2)	(3)	(4)
Scandal days/non scandal days	195/38	235/60	270/80	300/96
β*100	-0.04	-0.07	-0.06	-0.03
	(0.08)	(0.06)	(0.06)	(0.06)
Mean probability of an accident (percentage)	1.13	1.16	1.16	1.15
Observations on sample	174,179	216,557	259,298	301,127
Fixed effects	X	X	X	X
Fixed effects	X	X	X	X

Table 12 Appendix. Dependent variable is 1 if the vehicle was involved in an accident less than six months after the first ticket. Number of tickets issued and accidents per day are used as controls. All estimation include the following fixed effects for the ticket: year-week, day of week, type of infraction, source of the sanction and locality where it was issued. Fixed effect for the type of vehicle is also included. All errors clustered at the date level.

Effect of legitimacy on road accidents. Mayor's office as Keyword

Dependent Variable	Accident Six months	Accident Six months	Accidents Six months	Accident Six months
	(1)	(2)	(3)	(4)
Scandal days/non scandal days	126/33	159/50	185/64	208/73
β*100	-0.01	-0.04	-0.02	-0.01
	(0.01)	(0.09)	(0.09)	(0.008)
Mean probability of an accident (percentage)	1.39	1.35	1.31	1.32
Observations on sample	83,329	108,789	131,929	153,740
Fixed effects	X	X	X	X
Fixed effects	X	X	X	X

Table 13 Appendix. Dependent variable is 1 if the vehicle was involved in an accident less than six months after the first ticket. Number of tickets issued and accidents per day are used as controls. All estimation include the following fixed effects for the ticket: year-week, day of week, type of infraction, source of the sanction and locality where it was issued. Fixed effect for the type of vehicle is also included. All errors clustered at the date level.

Ranking	Date	Event type.	Original Headline in Spanish	Translation

1	Sept-10-2020	Killing of citizen by the police	¿Quién era el abogado Javier Ordóñez, muerto tras abuso policial?	¿Who was Javier Ordoñez, killed by police abuse?
2	Jan-4-2022	Civic Unrest	Plan de intervención en tres portales de TransMilenio	Interventions Plan for Transmilenio Portals.
3	Mar11-2021	Policemen killed on duty	Policía le rinde homenaje a joven patrullero asesinado en Bogotá	Police honors officer killed on duty
4	May-05-2021	Civilian protest-police abuse	Capturan a mayor de la Policía por asesinato de joven en protestas	Police officer captured for killing of juvenile during protests
5	Jan-17-2022	Policemen killed off duty	En un acto de intolerancia fue asesinado un policía	Policemen killing caused by intolerance
6	Jan-18-2022	Policemen killed off duty	En un acto de intolerancia fue asesinado un policía	Policemen killing caused by intolerance
7	Jan20-2022	Possible police abuse	Policías les habrían apropiado 6 choques eléctricos a mujeres transgénero	Policemen presumably used electric pistol on transgender woman
8	May-04-2021	Civilian protest-police abuse	ONU acusa a policía de amenazas, agresiones y disparos	UN accuses Police of threats, aggressions and shootings.
9	Jan 21-2022	Possible police abuse	Investigan denuncia de presunto abuso policial contra mujeres trans	Alleged police abuse against trans women is investigated
10	Jan 12-2022	Possible police abuse	Piden ratificar pena a policías por muerte de un hombre que recibió golpiza	Ratification of conviction of policemen is demand after mortal beating of a citizen.
11	Jan 14-2022	General crime	La delincuencia tiene azotado el sector de los restaurantes en Bogotá	Restaurant business is under the whiplash of robbers
12	Mar 12-2021	Policemen killed on-duty	Policía muere tras enfrentar a	Policemen dies while facing

			delincuentes en Bogotá	criminals in Bogotá
13	Sep 13-2020	Police abuse	ONU DD. HH. verifica casos de exceso policial en Bogotá en protestas	UN Human Rights office verifies police abuse in Bogotá during protests.
14	Jan 4-2023	Police faith expression	Esto responde el director de la Policía a los que lo critican por profesar su fe	Police Director responds to criticism over the expression of his religious faith
15	Jan 5-2023	General insecurity	Policía sigue el rastro de asesino de joven en TransMilenio	Police is behind the murderer of teenager in Transmilenio
16	Dec-16-2020	Police abuse	Imputarán cargos a otros dos policías por homicidio de Javier Ordóñez	Two more policemen to be indicted on the Javier Ordoñez case
17	July 12-2023	Policemen attacked on duty	Policía agredido en aeropuerto El Dorado narra su versión: 'Evité una tragedia'	Policemen attacked at the airport gives his version of events: "I prevented a tragedy."
18	Jan-19-2022	Possible police abuse	Policías les habrían apropiado 6 choques eléctricos a mujeres transgénero	Policemen presumably used electric pistol on transgender woman
19	Jan-13-2022	Possible police abuse	Piden ratificar pena a policías por muerte de un hombre que recibió golpiza	Ratification of conviction of policemen after the mortal beating of a man is demanded.
20	Jan-6-2022	Civic Unrest	Molinos, ¿un nuevo frente de acción de la primera línea?	Molinos, ¿A new action space for the "Primera Línea" movement?

Table 14. Dates and newspaper headlines for the Top 20 days in the Google Trends Index.

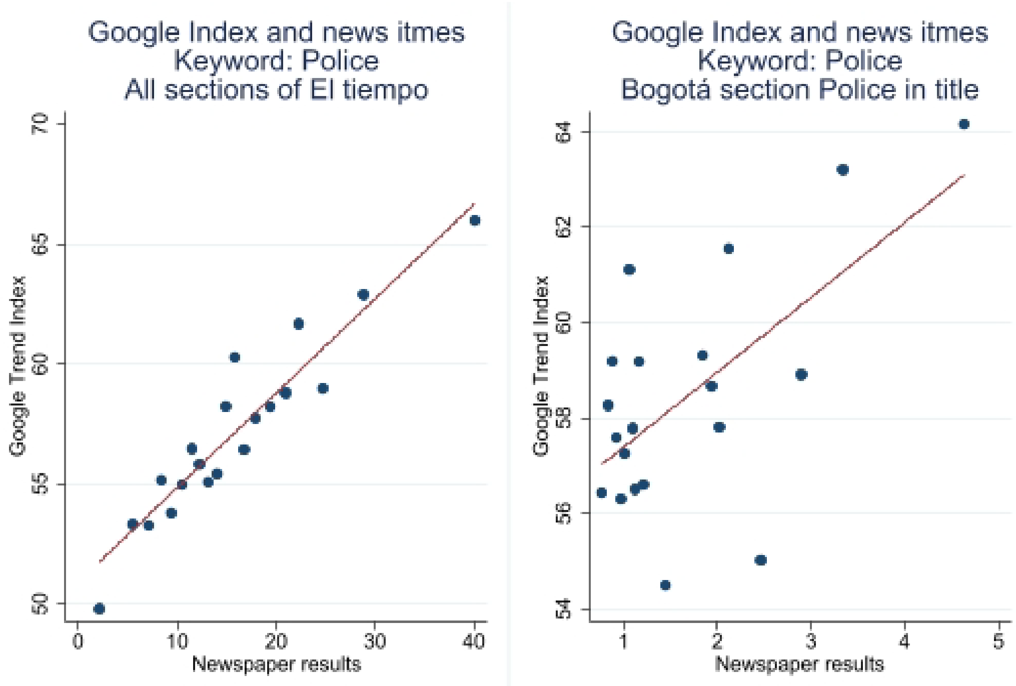

Figure 15. A binscatterplot graph is shown between the Google Trends Index for each day and the number of news items in the El Tiempo webpage. The scatterplot controls for year, week of the year day of the week and number of accidents and tickets each day. Column 1 uses all sections of the newspaper, and all news items that appear after searching for the keyword. Column 2 keeps only the Bogotá section of the newspaper and only news items that have the word Police, or its synonyms, on the headline.

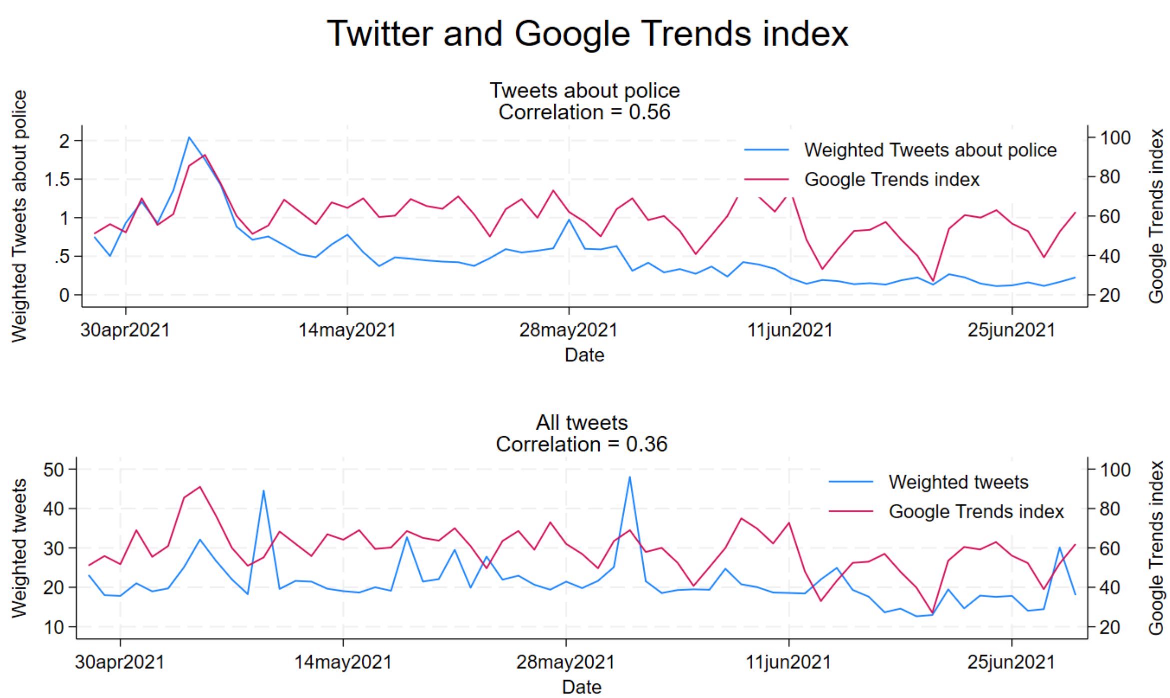

Figure 16. Value of Google Trend Index and the number of weighted tweets each day. Weighted Tweets gives the value of its total retweets to any tweet and sums it over the date the tweets were posted. Values for Weighted tweets are in millions. Tweets about the police are the ones that have a mention of police and its synonyms in the text.

Date	El Tiempo Headline (English translation)
March 11, 2021	Conmoción en Bogotá: la historia del patrullero asesinado en un robo. (Bogotá in shock: Policeman killed during a robbery)
August 12, 2021	Policía Humberto Sabogal fue herido cuando intentaba requisar a dos hombres en barrio Ciudad Berna. (Policeman Humberto Sabogal was killed while searching two men at ciudad Berna neighborhood)
July 26, 2022	Familia llora asesinato de patrullero bogotano en manos del Clan del Golfo (Family cries for the assassination of policeman by the “Clan del Golfo”)
July 10, 2023	Atención: pasajero golpeó de manera brutal a un policía en el aeropuerto El Dorado (Brutal beating of policeman at the El Dorado Airport)

Table 17. Dates and newspaper headlines for select days in which a policeman was the victim of an act of violence.

1. How long does a scandal last?

It is clear that I find the biggest coefficients when constructing 2-day scandals. This in itself is not an issue, but it is also clear that in general 4-day scandals show more precise estimations than 3-days scandals. In short duration of scandals seems not to be single-peaked. In this section I look at the causes for this unintuitive finding and to argue that the general result does not depend on a very narrow assumption about how long a scandal lasts in driver’s minds.

Figure 18 shows the average Google Trend Index for different days after a scandal. This is using only clean days, when scandals do not persist (closer to year 2021 than 2020 in Figure 6). Note that there is a significant drop from a scandal day to any of the following days up to 3 days. There is a slight jump in 4 days after a scandal that continues for 5 and 6 days. This might explain why 4-day scandals show higher coefficients than 3-day scandals, since days with higher total are included (coefficient for daily index score in row 1 of Figure 18 is positive). Results (not reported) are essentially the same if days following a scandal are not clean days.

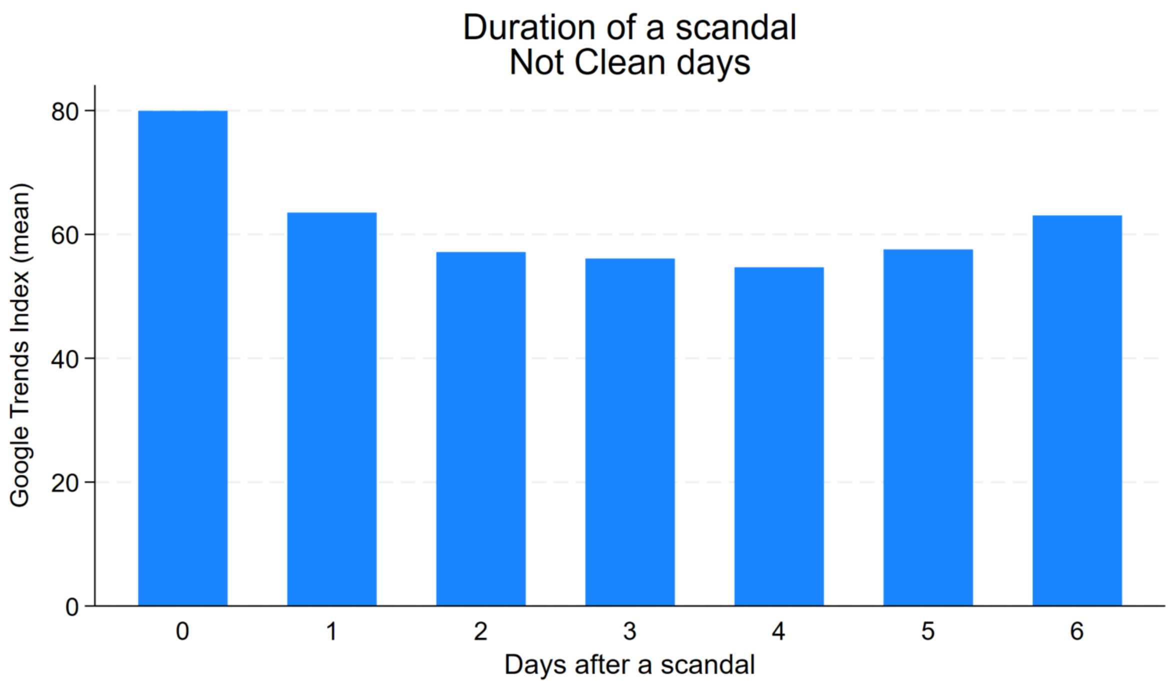

Figure 18. Average score for Googe Index searches of the world Police for some days after a scandal. Only clean days are used, those that only follow a scandal that persists for a brief period of time.

Another way to show that the result I am finding does not depend on a specific duration is to fix the number of days before a scandal. In Figure 19 all scandal durations vary only after the scandal day, but the number of days prior to the scandal coded as 0 stays fixed. I have added 5-day and 6-day scandals to show that they are not significant. Note that the highest coefficient is still for 2-day scandal, but 4-day scandals are still higher than 3-day scandals. All other durations give non-significant estimates. I find the same pattern when previous days are fixed at 1 or 3 days before. Note also that this does not affect the most critical test of Table 8, since in that case all non-scandal days are coded as zeros.

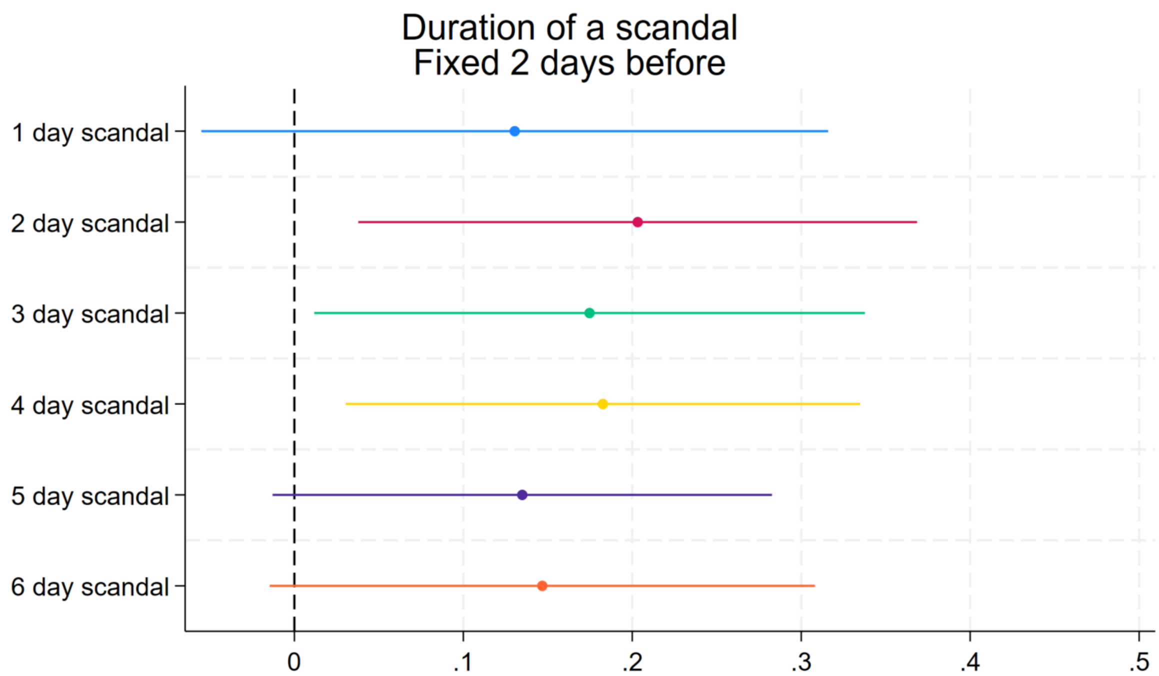

Figure 19. Coefficient for equation (1) are shown with the scandal day variable constructed as usual, but the days before a scandal duration fixed at 2 days. Dependent variable is 1 if the vehicle was involved in a crash less than six months after the first ticket. The number of tickets issued and accidents per day are used as controls. All estimations include the following fixed effects for the ticket: year-week, day of week, type of infraction, source of the sanction and locality where it was issued. Fixed effects for the type of vehicle also included. All errors clustered at the date level.

A last check that can be done is to perform the estimation olny with pure scandals, those in which there are no more than n-days as a scandal even if the Google Trend index is kept low for various days. This strategy has the cost of losing a lot of information for days in which scandals cluster around for certain dates, since in this case only the first of these scandals is kept. For this reason I also keep scandal days coded as 1, even if they are not at the start of the scandal. Results (not reported) do not change if these days are also taken from the sample. As it can be seen in Figure 20, the same general pattern is found here. 2 day scandals show the biggest effect, while 4-day scandals show a slightly higher coefficient. In this case, though, only 2-day scandals are significant at the conventional levels.

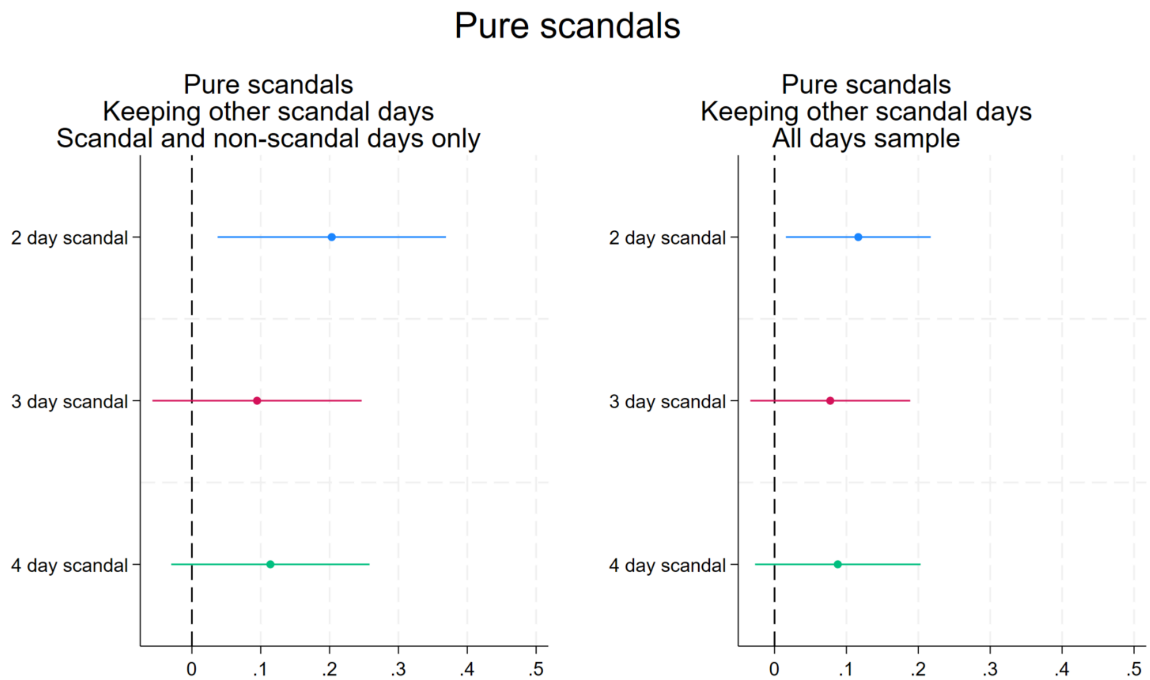

Figure 20. Coefficient for equation (1) are shown with the scandal day variable constructed only with pure scandals. Dependent variable is 1 if the vehicle was involved in a crash less than six months after the first ticket. The number of tickets issued and accidents per day are used as controls. All estimations include the following fixed effects for the ticket: year-week, day of week, type of infraction, source of the sanction and locality where it was issued. Fixed effects for the type of vehicle also included. All errors clustered at the date level.

In sum, even if it is hard to explain why scandal duration is not “single peaked” the general result stands. The finding that legitimacy plays a role on the deterrence of traffic offenses does not derive from ad-hoc decisions about the duration of a scandal. The main question of this paper is not the duration of a police scandal on the mind of citizens, but if I had to give an answer based on the findings it will clearly be: 2 days.

2. Use of ACLED database.

To test whether my Google trends index captures something relevant about the actions of the police I use all episodes captured by the ACLED database. There are 325 days with at least 1 protest, 171 days with a riot and 38 days with violence against civilians.

A first way to test the validity of the scandal variable is to add the number of events for each day and measure whether days that define a scandal (those on the higher quintile) have on average higher number of events. The p-value of a t-test of means is higher than 0.99 for the hypothesis that days that define a scandal have a higher number of events.

The same comparison can be made not with the total number of events, but with the probability of having a protest or a riot. In both cases the p-value of a t-test of means is higher than 0.9 for the hypothesis that days that define a scandal show higher probabilities. But interestingly, the p-value is higher for riots than protests. This points at the scandal days definition capturing not minor unrests (such a protest for police inaction) but real conflicts at the local level (such as violent clashes after perception of police abuse). Note that nothing in this evidence is a judgement on police behavior. Perceptions about police actions are powerful enough.

All this evidence supports the idea that the scandal variable is really capturing something relevant in terms of legitimacy, but it opens the question how much it depends on the police actions and how much on civic unrest.

3. Is it police legitimacy or social unrest? An exploration from the "National Strike."

During some months of 2021, the whole country of Colombia lived a "National Strike" (*Paro Nacional*, in Spanish). A series of street protests against the national government resulted in streets closings and frequent clashes with the police. This occurred all around the country and Bogotá, being the capital of the country, was one of the most affected cities. This can be seen in the data: there are no Camera generated tickets between May 9 and August 12 of 2021. This is mainly because police resources where required in controlling public order across the city. In addition, some cameras were vandalized and could not work.

In order to measure the effect of unrest in the city in the deterrence on legal sanctions, one can compare drivers getting their ticket during the same dates in the year of the strike (2021) to other years (2020, 2022, and 2023). Table 20 presents this data. Results seem to support the effect of unrest being significant. An even finer comparison will be to have all drivers that do get their tickets from policemen on roads, since Camera generated tickets where absolutely absent during these months. Still, there might be some selection in this samples. Since during the National Strike average daily tickets are 25% of the average daily tickets for other years. "Policemen on roads" average daily tickets where 40% of the average daily tickets for other years. In sum, drivers that got ticketed in these times might be significantly different.

To solve for this selection and apply the same logic of scandals persisting in the mind of drivers, one could compare the month following the National Strike. In this case the reduction on tickets is lower. For all tickets, the daily average is 75% of the average for other years in the month following the National Strike. For policemen on roads, the number is 83% of the daily tickets. This result is presented in in column (3), for all tickets, and column (4) for only policemen on road tickets.

All results show that unrest affects the chance of being in a crash. Using column (1) there was an increase of 40% in the chance of being in a crash by getting a ticket during the National Strike in comparison with the same date in the following years. The effect is also present in the month just after the national strike, as shown in column (3) with the same 40% chance of being in an crash. The effect seems to be caused by policemen on roads, as seen in columns (2) and (4). The effect is even bigger if the full sample is used, instead of only the same dates of the National Strike or the month after in different years.

	Effect of National Strike on trafiic deterrence			
Dependent Variable	Accident Six months	Accident Six months	Accidents Six months	Accident Six months
	(1)	(2)	(3)	(4)
Sample	National Strike All tickets	National Strike Policemen on road	Month after N.S. All tickets	Month after N.S. Policemen on road
β*100	0.38	0.26	0.37	0.42
	(0.10)***	(0.11)**	(0.10)***	(0.14)**
Mean probability of an accident	0.87	1.12	0.87	1.10
Observations on sample	362,872	194,783	106,086	62,217
Fixed effects	X	X	X	X

Table 20 Appendix. Dependent variable is 1 if the vehicle was involved in an accident less than six months after the first ticket. Number of tickets issued and accidents per day are used as controls. All estimation include the following fixed effects for the ticket: week, day of week, type of infraction, source of the sanction and locality where it was issued. Fixed effect for the type of vehicle (motorcycle or not) is also included. All errors clustered at the date level. Column (1) compares vehicles getting any ticket during the National Strike with vehicles getting their tickets in different years during the same dates. Column (2) compares vehicles getting "policemen on roads" tickets during the National Strike with vehicles getting their tickets in different years during the same dates.Column (3) and (4) do the same during the month after the National Strike with vehicles getting their tickets in different years during the same dates.