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Police scandals and deterrence: an exploration from traffic offenses and accidents.

Juan José Ferro H.

Abstract

Increasing the deterrent effect of sanctions is a pressing public policy issue in many areas where state's capacity to punish all wrongdoers is limited. Criminologist have argued for increasing the legitimacy of institutions in charge of enforcing the law, i.e. the police, in order to have more deterrent sanctions. This paper uses traffic tickets and road accidents in Bogotá, Colombia to study whether sanctions imposed by more legitimate authorities are more deterrent. 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, or on any non-scandal day. Drivers that get their tickets just after a police scandal are 21% (11%) more likely to be in an accident in the following six months than drivers that get their first ticket just before a scandal (any other day). This main result is robust to the specific definition of a police scandal and the selected sample, but depends on the period of study. I use national media to evaluate if the effect is bigger for scandals in which the police is clearly at fault, with some evidence in this direction. I explore ticket payment, subsequent tickets and accidents to validate the behavioral hypothesis that sanctions by illegitimate authorities have a lower effect on changing behavior. My results suggest that increasing the legitimacy of existing sanctions is more urgent than increasing the number of ticketed drivers.

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.

This is particularly relevant for the case of traffic tickets, where the number of sanctions is small compared to the number of infractions. Traffic tickets are also an example of legal sanctions that aim not only at punishing an specific behavior, but they also aim to directly prevent other behaviors that are not easy to spot and/or prevent. Like other soft sanctions, traffic offenses aim to deter not only speeding or dangerous driving, but to prevent road accidents and fatalities.

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. They might also be less willing to cooperate in future encounters with authorities.

The more salient authority in charge of applying the law is the police. This paper explores how the perception of the police affects the deterrent capacity of its sanctions. It uses data from traffic offenses and road accidents in Bogotá, Colombia. I argue that when the police are considered an illegitimate authority, the deterrence effect of the sanction it imposes is smaller. On the contrary, when the police is perceived as a legitimate authority the deterrence effect of the sanctions is larger. I consider the police to be a legitimate author if it is perceived as proportional and fair in its application of the law. I will evaluate this hypothesis by comparing the behavior of drivers on the road. To evade the natural endogeneity between drivers that get tickets and drivers that do not, I will only compare drivers that do get tickets and will concentrate on first tickets.

In particular, I will 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. If my hypothesis is correct, drivers that get their tickets after a scandal are less prone to change their behavior at the police's calling and thus will have a higher probability of being involved in a road accident. My estimation strategy rests on the assumption that drivers' behavior on the road does not depend on their perceptions of the police, but the effect of a traffic ticket does.

This paper relates to different strands of the 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 in 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 possible 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. I also contribute to this literature exploring if automatic enforcement of the law (speed cameras) is affected equally by police legitimacy, or if this phenomena is exclusive to actual policemen on the street.

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 is centered on the causes of road accidents. Various explanations have been given for the increasing number of road accidents. 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.

Finally, 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 the academic literature.

The paper is organized as follows. In section 2, I explain the general context of accidents and tickets in Bogotá. Section 3 presents the identification strategy of the paper. Section 4 explains how the dependent variable is built and section 5 shows data for the selected sample. Results are presented in section 6. Section 7 discuss a "sentiment analysis" of the main findings. Mechanisms and heterogeneities are discussed in section 8. A discussion of the findings is presented on Section 9.

2. General context and background

2.1 Traffic offenses 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 has a fine of half of the mandatory minimum wage, but only around 45% of tickets are effectively paid, and there is not precise data on this issue[2].

[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.

[2] This number comes from a conversation with Juan Pablo Bocarejo, former head of the *Secretaría de Movilidad*, the office in charge of designing mobility public policy.

In Bogotá there are two other ways of enforcing traffic offenses, and both provide a good comparison to the policeman in the street standard of enforcement. First is the *Camaras Salvavidas* (Lifesaving Cameras, in Spanish), a camera enforcement of speed limit that generates tickets and sends it to the registered address of the owner of the vehicle. What is important to my research is that even if this program is managed by the police, drivers do not face a policemen in all the interaction. The second one is by *Agentes de Tránsito Civiles* (Civilian Transit Agents, in Spanish), a group of actors that are not police officers but can clearly impose tickets for some particular infractions. As will be seen, tickets form this source are marginal in number.

Figure 1 shows the yearly distribution of traffic offenses. Note that the Cameras program started in June 2020 and with the rollout of new cameras it became the primary source of tickets by March 2022. It is easy to see how the number of yearly tickets skyrocketed after the cameras program started. Since the probability of getting a ticket increases considerably after the introduction of the Cameras program, I will only use the period from June 2020 on my analysis[3]. My robustness check show that little of the result depends on this specific decision.

Note also that data is preliminary for 2023, since only tickets up to August are counted. In line with some findings from the literature, Cameras and policemen on roads work as compliments rather than substitutes when enforcing traffic offenses (Conover et al., 2023). Figure 2 compares the two sources that account for the majority of tickets, Cameras and Policemen on road, to show that there are no clear patterns in terms of monthly tickets that are common to both sources, and that the introduction of Cameras did not mean policemen on road reduced their work. In fact the early months of 2022 and 2023 show higher Policemen on Road tickets.

[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).

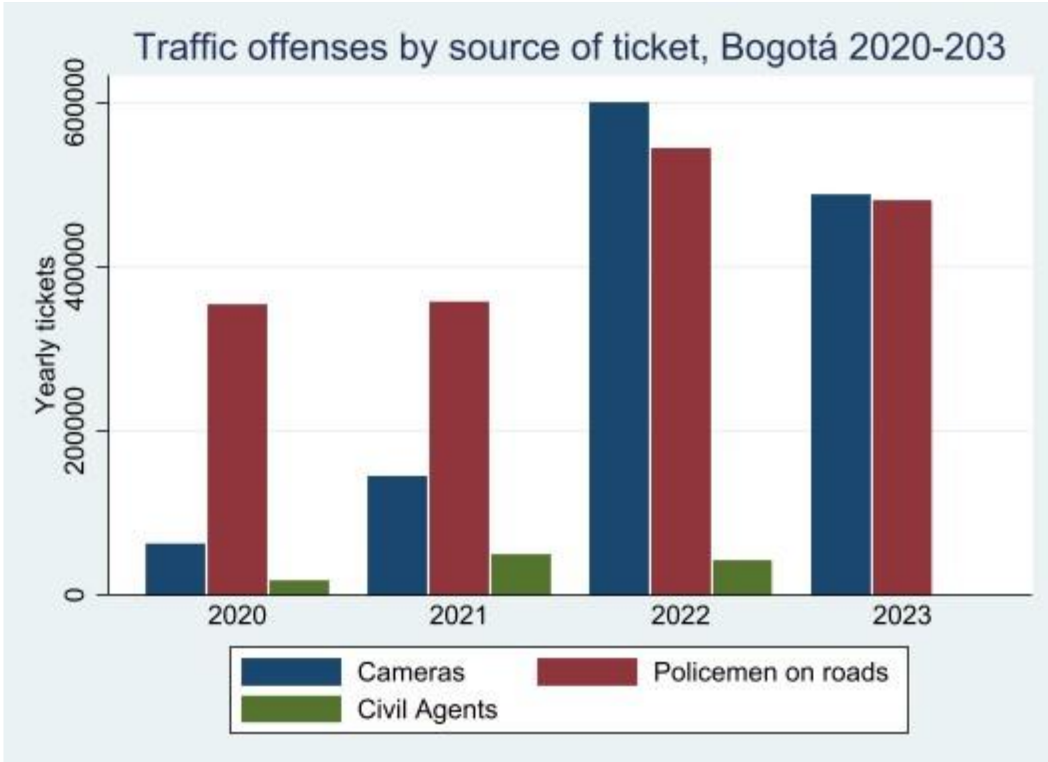


Figure 1. Traffic offenses by source of ticket. Data is preliminary for 2023, since only tickets up to August are counted.

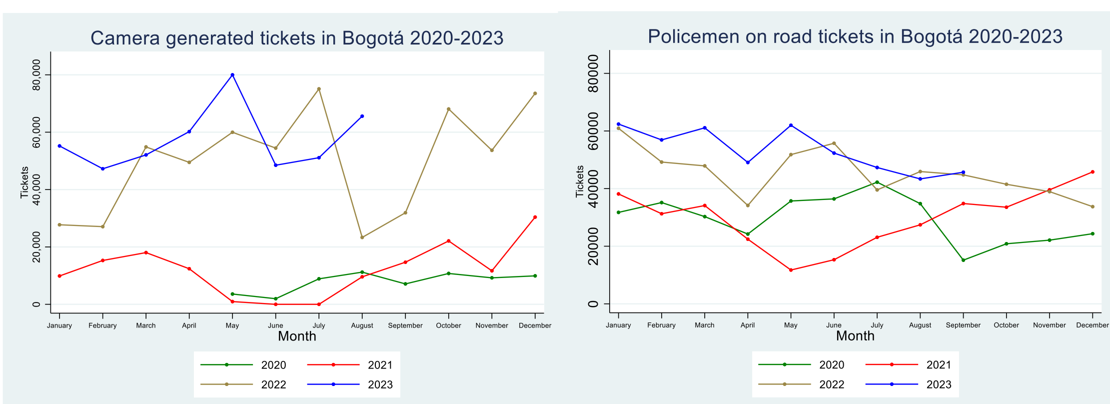


Figure 2. Month and year of tickets for Camera and Policemen on road tickets.

The introduction of Speed Cameras also changed the composition of the tickets. Figure 3 shows the main five infractions and its 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.

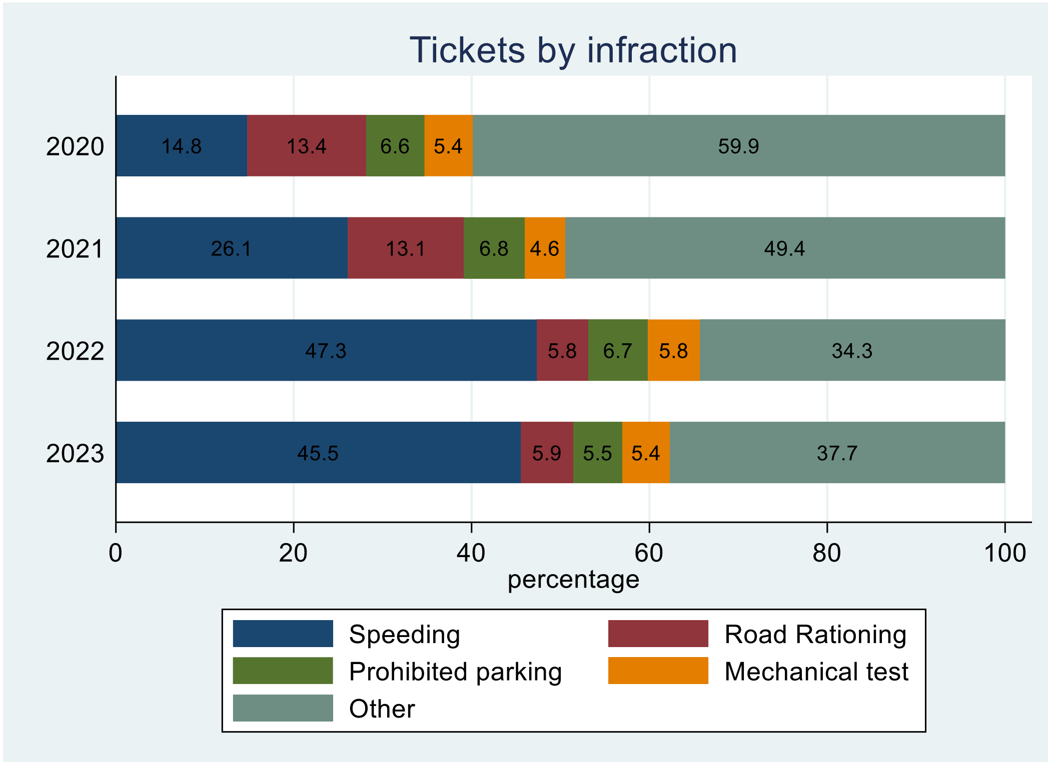
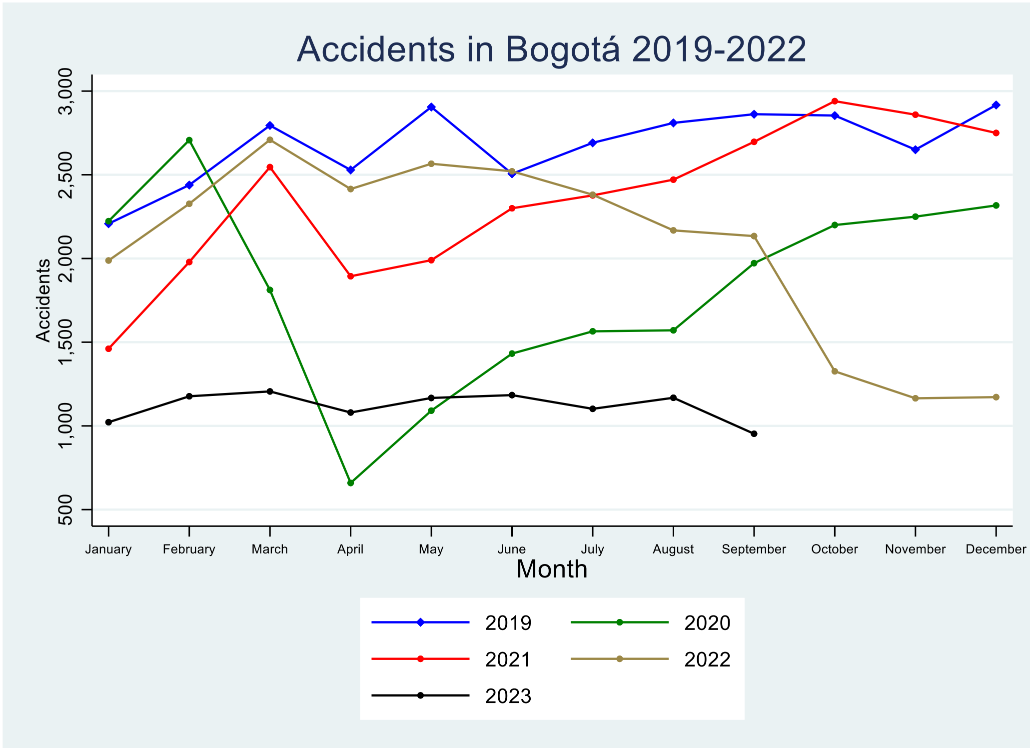


Figure 3. Tickets by year and type of infraction. "Road Rationing" refers to a congestion reducing system in which, depending on the registration number, cars cannot circulate on certain days and times. "Mechanical test" refers to a national mandatory technical revision of the vehicle that has to be renewed annually for all vehicles that have been circulating for at least 5 years.

2.2 Road accidents in Bogotá, Colombia:

Before presenting the estimation is important to evaluate the behavior of the accidents occurring in the city in the sample period. In Figure 4 it is easy to see the stationarity of the data, and to note that there is considerable lack of reporting from June 2022 on. This lack of reporting is centered around less serious accidents, as it can be seen in the data for only accidents that have at least one person injured. While this is not ideal, for it to affect my estimation underreporting will have to be related to behavior of citizens. There are no reasons to think this is the case.



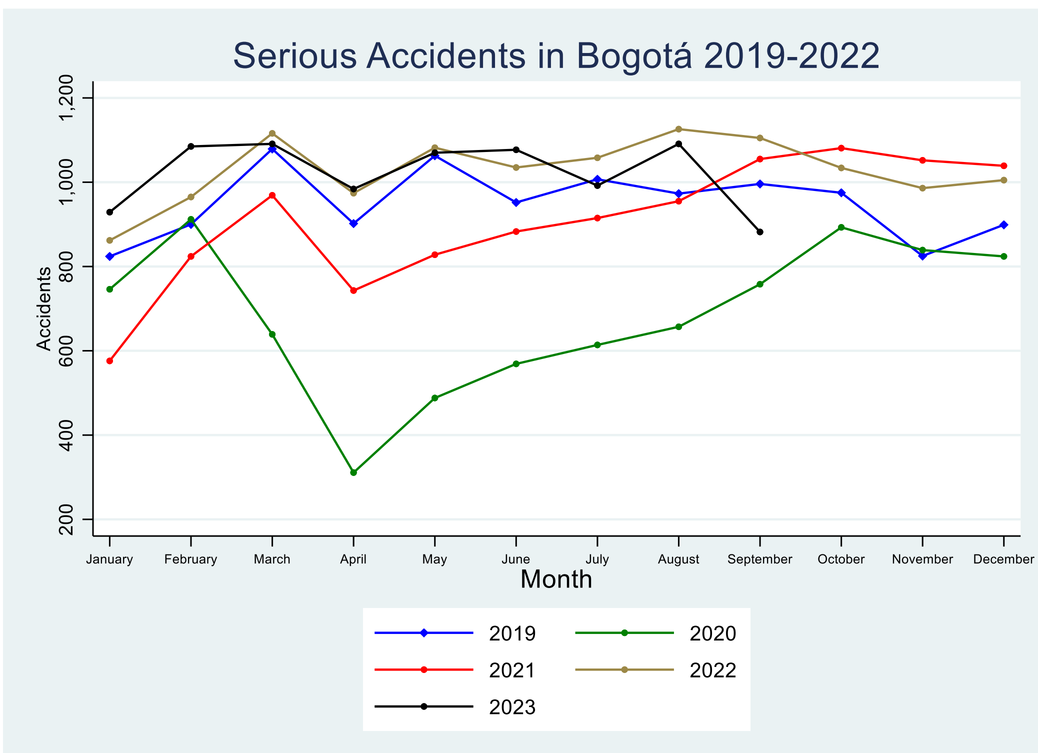


Figure 4. Number of traffic accidents and serious traffic accidents (with at least one person injured) occurring in the Bogotá by month and year.

Figure 4 also helps us understand the changing nature of traffic accidents. With 2019 as benchmark, one can see that by 2023 there is a reduction on total accidents but an increase in more serious accidents. As it is shown in Figure 1 of the Appendix this might be explained by the riskier profile and increasing presence of motorcycles in the city.

2.3 Data on accidents and traffic offenses:

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 also have data for average daily speeds for most days on the sample. All data has been given to me by *Secretaría de Movilidad*, the public office in charge of the enforcement of traffic law in the city, for research purposes.

Two possible questions about the data are worth tackling in advance. The first one has to do with identifying 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 short period of time in which most of vehicles don't change owners[4]. In the following, whenever I talk about drivers, I am really referring to their vehicles, under the assumption that vehicles are driven by one, and the same, individual.

The second one has to do with underreporting of road accidents. While some minor accidents are not reported, this is an unlikely occurrence since mandatory national insurance requires drivers to report all accidents, they have been involved in. Thus, it is safe to assume that the vast majority of accidents occurring in the city are included in my database.

To measure police legitimacy, I use two sources: Google Trends and national media. All of this information is publicly available and has been processed by me in the manner I explain in the following chapters.

2.4 Media Data:

As it has been used in recent years in social science, data on internet searches will be used 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

[4] 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 most of the vehicles don't 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.

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 the 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. As it has been shown 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 called *El Tiempo*. I use headlines on news in *El Tiempo* as a measure of negativity towards the police.

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 that does that 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 accident.

To avoid this selection effect, I will only compare drivers that do get tickets and see whether they are later involved in accidents or not. To avoid the selection caused by having an accident before the first ticket and the selection of drivers that get several tickets, I will only compare only drivers on their first ticket received. This will allow me to separate the effect of tickets and legitimacy from the experience of being in an accident.

The source of exogeneity I explore is the general sentiment toward the police in the city. Using Google Trends, I calculate the days when searches for the word "Police" are atypical. Days that are on the highest decile of searches, and the following n number of days, are considered "scandal days" and coded as 1. Days that are n days before the highest decile, and not in the n days following a day on the highest decile, are considered not scandal days and coded as 0. This is the *scandal and non-scandal days only* sample. As it will be clear in the results sections, in some estimations I code all days that are not scandal days as 0. This I call the *All days* sample.

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 accidentality 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 an accident less than six months after receiving a ticket, and 0 otherwise. But I will do my robustness on a longer time frame (a year) and the time to a second ticket.

$Scandal_{iot}$ 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. 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 an accident 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 accident[5].

X_t are a set of controls for the day in which the ticket was issued. Number of tickets and accidents per day are used to account for differential effect of police practices and/or behavior of drivers that might independently affect the chances of being in an accident[6].

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.

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. This might be important due to the fact that motorcycles have been shown to have an increasing percentage of accidents in the city lately.

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

[5] One paper that uses the same identification strategy is (Casey et al., 2018). A very similar one is also found on (Hall & Madsen, 2022)

[6] As I will argue later there are other possible controls that are "bad controls". In particular, average daily speed and total number of accidents in the six months following a scandal.

4. Police scandals:

To construct the scandal variable, I take all daily[7] values of the search word "policia"[8], 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. All days in the highest decile and the following n days are considered as scandal days. All days that are in the n days before a scandal day and are not themselves a scandal day, are considered not scandal days. By construction scandals last one day more than there previous days.

Figure 5 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 5. Number of scandal and no scandal days, according to the assumed duration of a scandal on public perception. Note that the scandal day itself is not counted on each duration.

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 besides April all months of the year 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. A similar distribution is found for non-scandal days. For yearly distribution of 4 days scandals, 39% of scandal days (and 43% of non-

[7] 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.

[8] Spanish readers will note that a spelling mistake is present. The correct word to write in Spanish is policía, with the accent mark. This decision is taken based on the fact that the search volume with the mistake (mean of 64) is considerably higher than without the mistake (mean of 13). Both series are highly correlated.

scandal days) correspond to 2021. Similar values are found for other durations of scandals. All this seasonality is captured by the year-week fixed effects included in the regression. Figure 6 shows the distribution of scandal and non-scandal days for the 2 days scandals for year 2020. Figure 3 of the Appendix does the same for the full years. Some periods of intense unrest can be discerned. Note that scandal days are defined only for the period in which the Cameras work, so early days of 2020 with significant unrest 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.

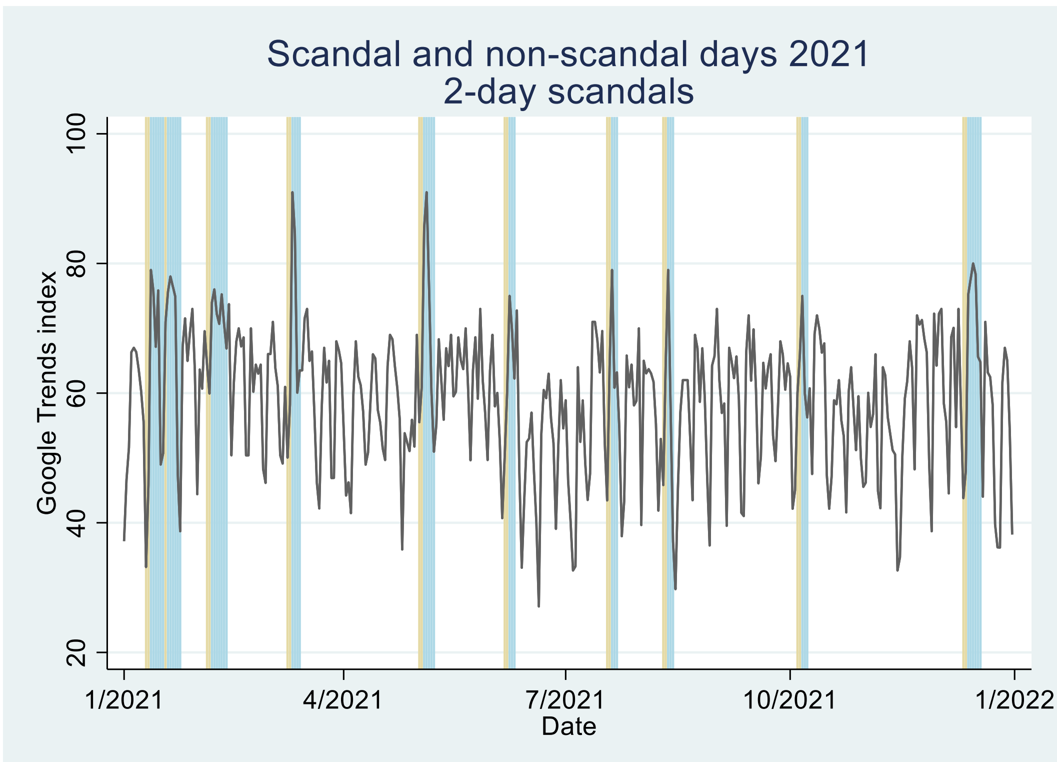


Figure 6. Distribution of scandals (in light blue) and non-scandal days (in light yellow) for the assumed duration of 2 days for scandals for year 2021.

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. Using the database of El Tiempo, the national newspaper, I select all the news that appear on a given day with the keyword Police. 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 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.

5. Vehicles sample:

In this section I present some data on my sample. Figure 7 shows the number of vehicles under the three possible conditions. Only rows 2 and 3 will be used for our estimation. In fact, since we are only using vehicles that received a ticket before being in an accident or never had an accident, 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 7. Number of vehicles by condition.

Figure 8 presents the average for all possible dependent variables used in the estimation. It also shows some variables that are used as fixed effects. Note how small the number of drivers that get 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 8. Dependent variables and controls of sample used in estimation.

6. Results:

In this section I show results on the short-term effect of police legitimacy on deterrence. Table 9 synthetizes the estimation of equation (1) with having an accident in the six months after a ticket as dependent variable. It is impossible to know with 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 accident 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 seem to be 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 mean probability of an accident in the sample, the effect is between 13% and 21% (columns 4 and 2) increase in the chance of being in an accident after receiving a ticket on a scandal day in comparison to a non-scandal day.

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 (0.11)	0.20 (0.08)**	0.12 (0.07)	0.13 (0.07)*
Mean probability of an accident (percentage)	1.00	1.01	1.03	1.05
Observations on sample	137,393	176,609	206,027	233,608
Fixed effects	X	X	X	X

Table 9. 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 make to extend the model is to code al 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. It also helps to better control for tickets and accidents per days, two important controls included in the regression that are higher in average on the *scandal and non-scandal days only* sample. Results shown in Table 10 and 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 an 11% increase in the chance of being in an accident is found on column 2. 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 seem to have a bigger effect of 3-day scandals. 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	0.04 (0.05)	0.10 (0.05)**	0.07 (0.05)	0.09 (0.05)*
Mean probability of an accident (percentage)	0.91	0.91	0.91	0.91
Observations on sample	1,282,608	1,282,608	1,282,608	1,282,608
Fixed effects	X	X	X	X

Table 10. 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.

I can also do the same exercise with all other dependent variables in order to proof that I am really capturing an effect of police scandal. A first logical step is to explore different durations for the dependent variable. Figure 11 uses column 2 of each table and estimates 3, 6, 9 and 12 months to the first accident. The fact that the effect is considerably higher for the 3 and 6 months time frames than for the 9 and 12 months frames, it is a sign that scandal are really in drivers memory for a significant period of time. But 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. This fading-out of the effect supports the behavioral mechanism I have in mind.

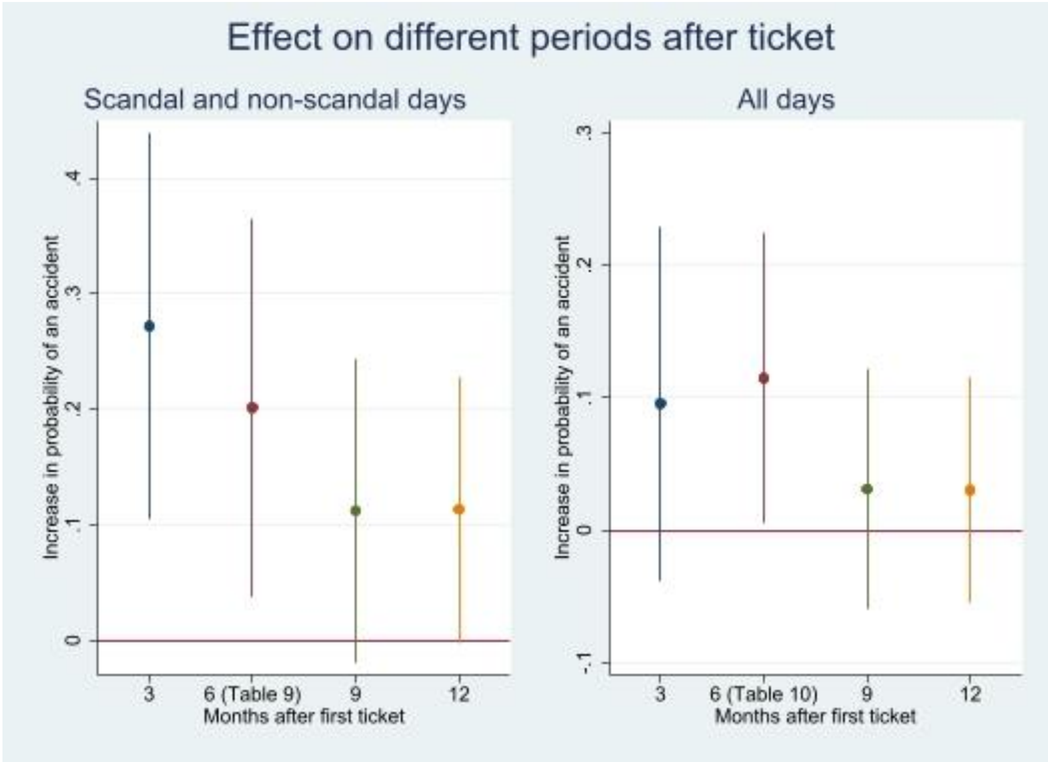


Figure 11. Dependent variable is 1 if the vehicle was involved in an accident less than three, six, nine or twelve months after the first ticket. Number of tickets issued and accidents per day are used as controls. All estimations include the following fixed effects for the ticket: year-week, day of week, type of infraction, source of the sanction and locality where it was issued. Fixed effect for the type of vehicle is 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 on a serious accident (where at least one person is injured). Figure 3 of the Appendix explores this aspect. Note that if dependent variable is being in a serious accident six months after the first ticket, the coefficient is negative and not statistically significant. When only non-serious accidents are considered, the effect is bigger than when all accidents are included. All hits point to the effect being driven by non-serious accident and it suggest that serious accidents are more of a random occurrence[9]. Once again, this supports the consistency of the effect found on my main estimation.

The effect is not driven by a subset of drivers, as it can be seen in Figure 4 of the Annex. Neither the drivers that get a second accident (0.84% of the sample) nor the ones that get a significant number of tickets 889% of the sample gets less than

[9] In any case a more consistent result is found if an ordered probit is estimated with dependent variable taking values of 0 (no accidents), 1 (non-serious accident) or 2 (serious accident). The marginal effect (not reported) of a scandal day is positive for being in accident or being in a serious accident, and significant at the 10% level.

4 tickets) explain the result. In fact, the magnitude of the coefficient is slightly bigger when those accident and ticket prone populations are not considered. I will go back to the fact that scandals seem to be less important for drivers that receive a significant number of tickets.

It is important to show that the result is not driven by the specific way in which the Google Trends index constructed with the search word police is used. A first possible question is if something is obtained by defining a set duration for scandal in driver's minds and not simply using the distribution of the Google Trends Index. Table 11 uses different ways to use this distribution, all pointing 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 last some days in the mind of drivers.

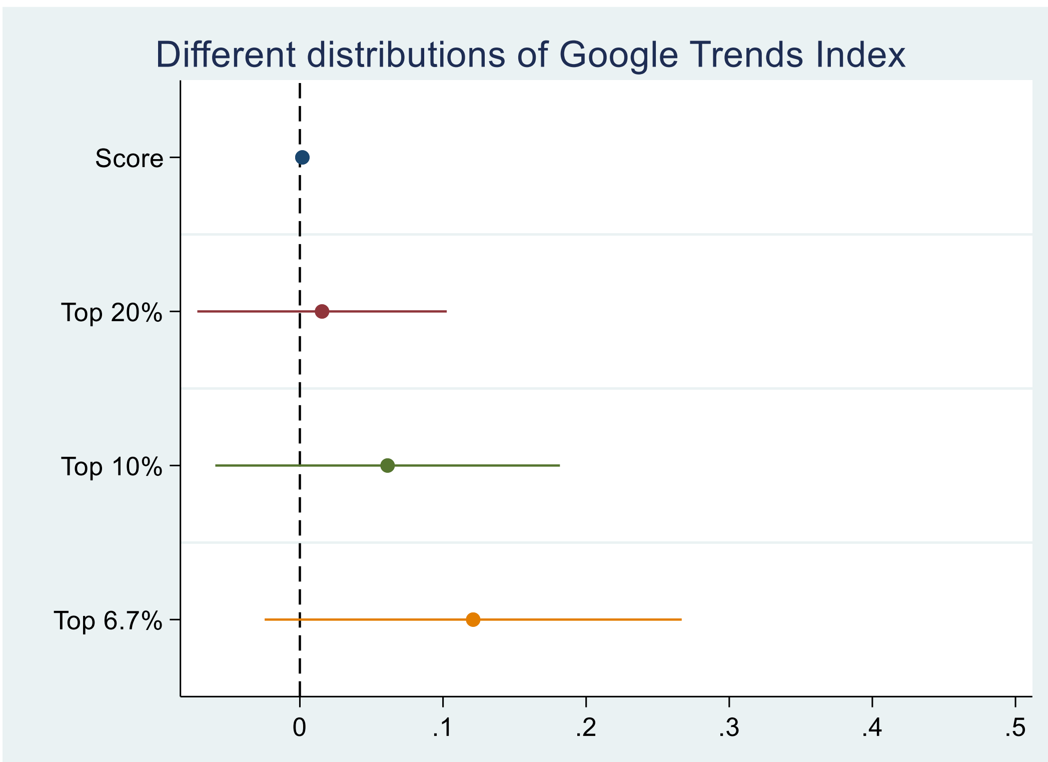


Figure 11. Dependent variable is 1 if the vehicle was involved in an accident less than six months after the first ticket. Number of tickets issued and accidents per day are used as controls. All estimations include the following fixed effects for the ticket: year-week, day of week, type of infraction, source of the sanction and locality where it was issued. Fixed effect for the type of vehicle is 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 very specific about the construction of scandal days is driving the effect. A whole chapter on 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 12 shows the effect when the number of quintiles ranges from 10 (top 10%) to 20 (top 5%). As it can be seen the effect is centered around the chosen distribution but is not specific to it. In fact, the effect is highest when the sample is divided in 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 become too small in that scenario.

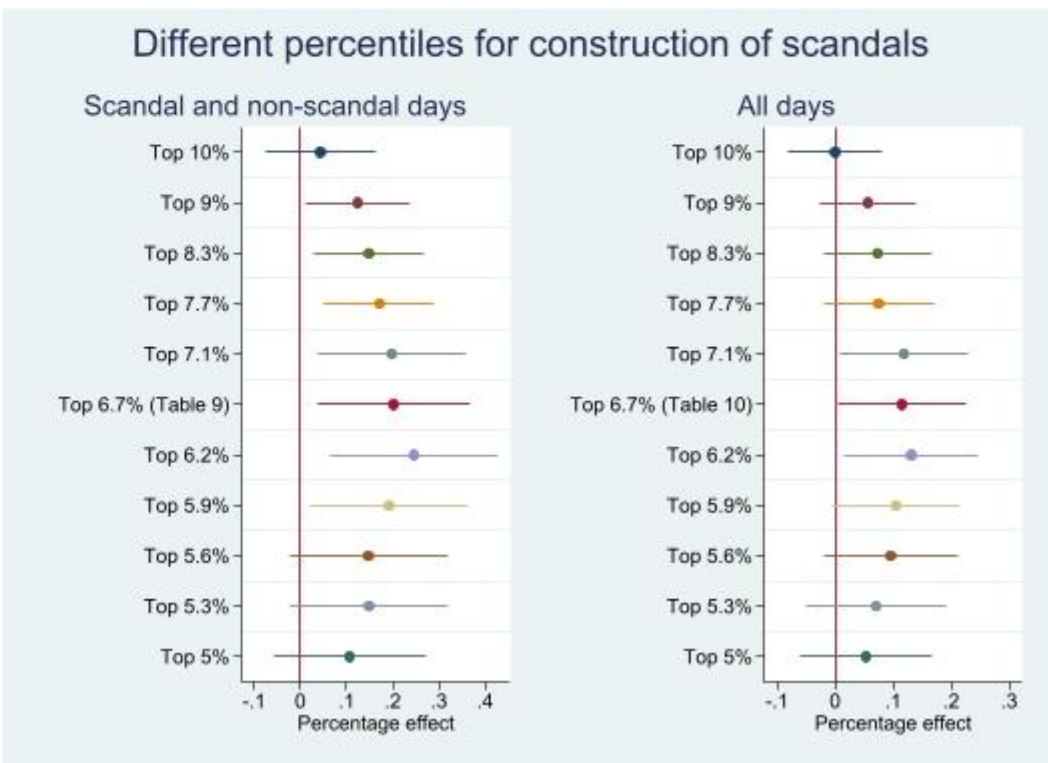


Figure 12. Dependent variable is 1 if the vehicle was involved in an accident less than six months after the first ticket. Number of tickets issued and accidents per day are used as controls. All estimations include the following fixed effects for the ticket: year-week, day of week, type of infraction,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

accident, by changing the percentile at which days are considered 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 13 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. Row 2 shows test if I am identifying a local effect. If the Google trends index not only contains searches within Bogotá, but the whole country, result changes significantly, which points to local perception driving the result. This is reassuring for my hypothesis.

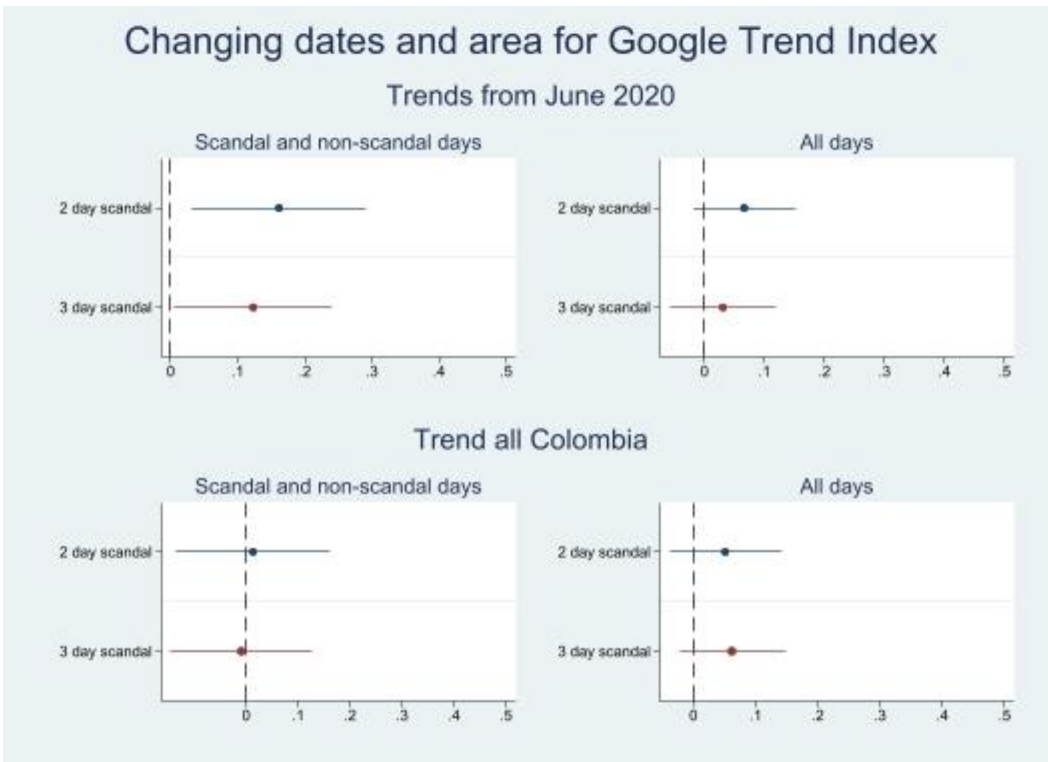


Figure 13. Dependent variable is 1 if the vehicle was involved in an accident less than six months after the first ticket. Number of tickets issued and accidents per day are used as controls. All estimations include the following fixed effects for the ticket: year-week, day of week, type of infraction,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

But the use of the Google Index is not the only decision I made. Figure 13 varies some of the estimation parameters to show that results are robust to some of the 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 accident 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 a first ticket and after six months it has not had an accident, this ticket is erased and the next ticket is used in the estimation as if it were a first ticket. This only increases the size of the sample in around nine thousand additional vehicles. Both changes only reduce the effect on the *All days* sample slightly.

Figure 14. 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 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 effect for the type of vehicle is also included. All errors clustered at the date level. Variation with respect to main sample is explained in the row title. 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.

7. 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 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 5 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, in average, there is a raw positive correlation between the Google Trends index score and the number of news items. Figure 6 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 have 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 a number of 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. When the same exercise is done on the *All days* sample, the result is the one found. 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[10]. Their data consists in three types of events: protests, riots and violence against civilians. Full details of the analysis are shown in the second subtitle of the Appendix, but the general conclusion is that scandal days have more conflicts than non-scandal days. And that is caused 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. Graph 7 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 similarly captured by 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[11].

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 8 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

[10] Data was downloaded from: https://acleddata.com/data-export-tool/ on May 6 of 2024.
[11] As readers might recall, past Twitter data has had some 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.

of a longer period of social unrest). When equation (1) is run 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 an accident increases from 20 to 24% (column 2) on the *scandal and non-scandal days only* sample. Similar changes occur on the *All days* sample.

A more formal analysis can also be done. In specific, for all of the days that are in the top decile of searches on Google Data, I search the internet archive of El Tiempo[12]. For each of the days in the sample I extract the headlines and lead for each news item that appear under the search word "police" in the Bogotá section of the archive. Then I use three widely used public sentiment analysis models[13] to measure the negativity towards the police in each day of the sample. Negativity is measured from 0 to 1, with 1 being the most negative sentiment. I use the maximum value of negativity for each day and scale this result by the total number of headlines each day.

Results (not reported) somehow support the hypothesis. If I classify scandal days by a dummy that takes the value of 1 for scandals higher than the 90th percentile on negativity. The interaction between this dummy and my scandal variable is positive and significant. In fact, it has a very high magnitude, that is difficult to interpret. One should note that this result is only true for the *scandal and non-scandal days only* and depends slightly on the sentiment model used.

Other possible distributions of my negativity variable are not conclusive. This might do with the fact that scandal days are significantly more negative in average, with 27% of scandal days (2-day scandals) on the highest decile.

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

8. A tentative exploration of mechanism: other behaviors and some heterogeneities.

It the behavioral mechanism suggested is that citizens do not accept the "wake up call" of an illegitimate authority, one should observe an effect of scandals in other

[12] An example of the digital archive can be seen here (this is the day with the highest Google Index value):
https://www.eltiempo.com/buscar/?q=Polic%C3%ADa&sort_field=desc&categories_ids_or=12&from=2020-09-10&until=2020-09-10&articleTypes=

[13] Model 1: "lxyuan/distilbert-base-multilingual-cased-sentiments-student"
Model 2: "z-dickson/multilingual_sentiment_newspaper_headlines"
Model 3: "pysentimiento/roberta-targeted-sentiment-analysis"

behaviors. The most obvious one is the decision to pay or to avoid payment for a ticket given by an illegitimate authority. This is particularly relevant since for most of the period studied around half of the tickets in Bogotá remained unpaid.

Figure 14 runs equation (1) with the dependent variable taken the value of 1 if the ticket was paid before one year had passed from the day of the ticket, or 0 otherwise. It is clear that for all possible durations of a scandal, tickets on scandal days are less likely to be paid. Magnitude is small, but not negligible. There is a 4% additional reduction on the probability of payment for first tickets received on scandal days. This is true for most of the possible durations of a scandal, even if not significant on the *All days* sample. Figure 9 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 is affected by the desire to pay the fine in order to legally sell a vehicle).

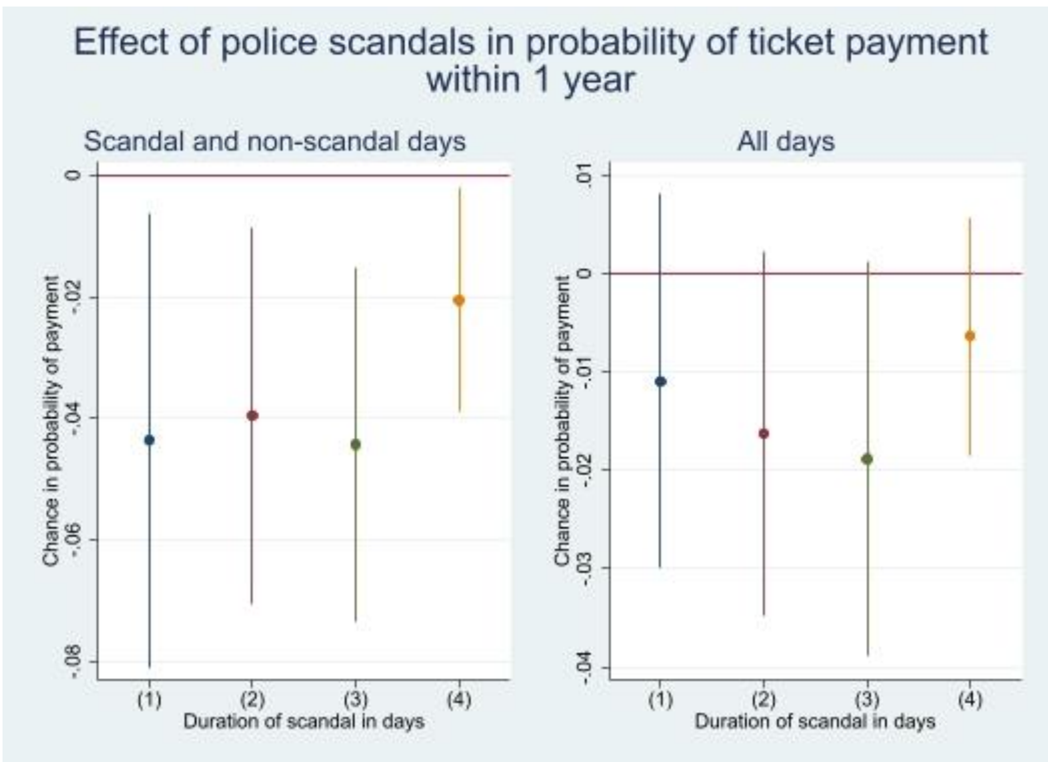


Figure 14. Dependent variable is 1 if the first ticket was paid in the following year (0 otherwise). Number of tickets issued and accidents per day are used as controls. All estimations include the following fixed effects for the ticket: year-week, day of week, type of infraction, source of the sanction and locality where it was issued. Fixed effect for the type of vehicle is also included. All errors clustered at the date level.

The fact that drivers behave worse after they perceive authorities as illegitimate should also be observed in behaviors similar to the one that caused the first ticket. Figure 15 shows the effect of receiving at ticket on a 2-day scandal day on subsequent driving. It can be seen that 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 are not rare behaviors since 42% and 43% of drivers get at least a second ticket (*scandal and non-scandal days only* sample and *All days* sample, respectively).

Even if magnitudes are small, around a 1% increase in all of these variables, they are statistically significant. One should note that there is no effect on the probability of being in a second accident, but this might be caused by the low rate (only 0.13%) of drivers in the sample that are involved in more than one accident.

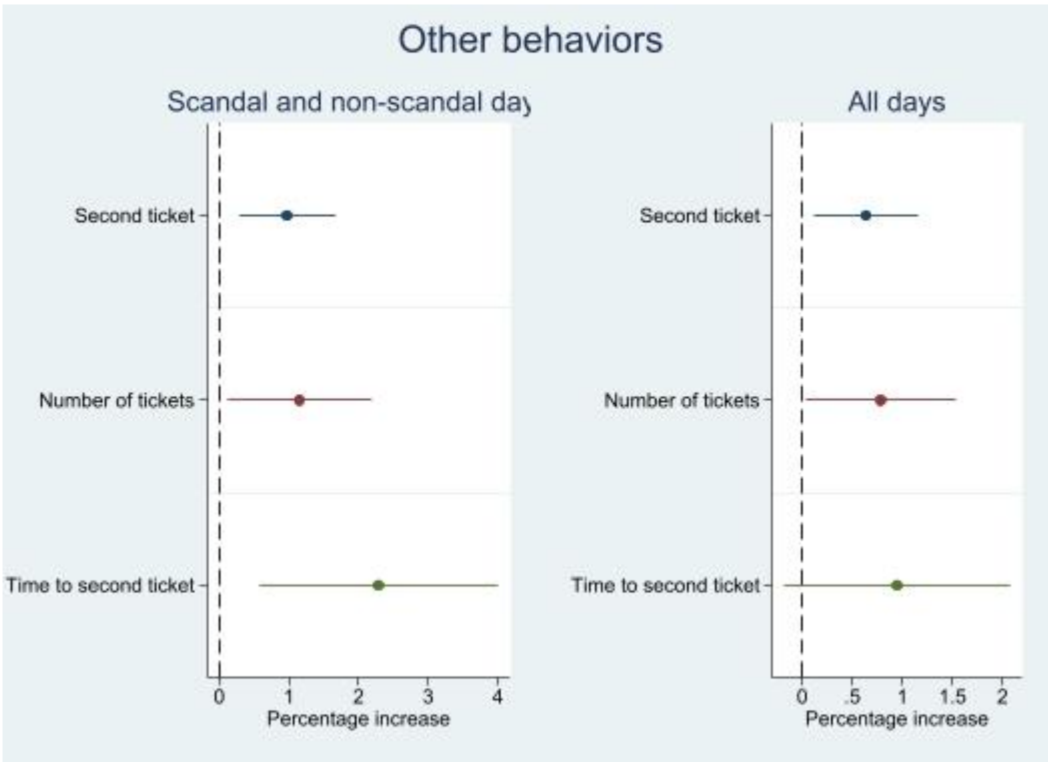


Figure 15. For first row dependent variable is 1 if the vehicle got at least a second ticket during the sample period. For second row dependent variable is the total number of tickets that the vehicle received. Third row dependent variable is the time between first and second ticket (coefficient is multiplied by -1 for presentation purposes). All coefficients presented as percentages. 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 effect for the type of vehicle is also included. All errors clustered at the date level. Variation with respect to main sample is explained in the row title. Column on the left corresponds to the sample used in Table 9, and column on the right to full sample used in Table 10.

Speed is considered the main cause of road accidents. That is also the case in Bogotá, and the reason why Cameras are targeted at speeding. But speed is in itself a decision by drivers, so the identification assumption that allows for a causal interpretation of equation (1) is harder to argue. With this caveats, one can still run equation (1) with the average speed in the city as a dependent variable and interpret this not as a casual but as a correlational finding. There is an additional data limitation, because changes in the methodology mean speed data is not reliable after November 2022. Results (not reported) consistently show that average speed is lower on scandal days. It is also true that if speed is used as a control, none of the findings of Table 9 and 10 change. In fact, coefficients only increase in magnitude and are more precisely estimated. I still will argue that in equation (1) speed is a bad control (in the Angrist & Pischke, 2008sense), so I consider this results more of a test in mechanisms. It seems clear that results shown in previous sections are not driven by people speeding on scandal days, or faster drivers being targeted by the police on those days.

As I argued early, the introduction of speed cameras greatly increased the chance that drivers in Bogotá received a ticket. An important question on the literature is whether automated enforcement of the law is perceived as more or less legitimate as tickets given by police officers. Theoretically this relationship can be argued either way. I can examine this heterogeneity by exploring if the source of the ticket has a differential effect on the deterrence of a ticket. The most precise way of measuring this effect will be to introduce an interaction between my scandal dummy and a source of ticket dummy that compares cameras to human agent tickets. Using the interaction and scandal days as source of exogeneity, coefficients are positive but not significant.

Another important heterogeneity, particularly in terms of public policy, is the type of vehicles involved in accidents. As it was said before, motorcycles are an increasing percentage of the vehicles involved in accidents. Even if the probability of being involved in an accident six months after getting a ticket is higher for vehicles, motorcycles are more than twice as likely of getting into a serious accident. Evidence is equally not conclusive in this case, with negative but not statistically significant coefficients on the effect of being a motorcycle driver.

Finally, an important question for public policy is the different causes for accidents occurring during daytimes and nighttime. Accident occurring at night are much more deadly, with twice the probability of having deaths and an 8-percentage point increase on the chance of injuries. When dependent variable is the probability of being in an accident during daytime or nighttime, results (not reported) show a consistent pattern: the effect is concentrated on accidents occurring during the day.

9. Conclusion.

This paper tried to answer if the deterrent capacity of police sanctions is reduced 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 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 the endogeneity, only drivers that got a ticket were included in the sample, and only the effect of their first ticket was measured.

The hypothesis was mostly verified. The tickets issued by the police just after a police scandal are less effective at preventing traffic accidents, and the magnitude has practical considerations. In comparison to receiving a ticket just before a police scandal, receiving a ticket in a scandal increases the chance of being in an accident 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.

However, a natural extension of the hypothesis could not be verified by studying the general sentiment toward the police with data on national media. The fact that the effect of police scandals is not robustly higher when newspaper headlines show a more negative view of the police is not in itself a rebuttal to the hypothesis of this paper. It can be attributed to the difficulty of measuring sentiment toward the police with existing sentiment models, and to the difficulty of obtaining data on what headlines are effectively read by citizens.

It was also shown that other relevant behaviors are also causally affected by scandal days. Drivers that get their tickets in days with lower police legitimacy are more prone to getting a second ticket, have more total tickets and get their second tickets earlier. This result does not seem to be a causal result of speed and seems to be concentrated on daytime accidents.

One can synthetize the finding for accidentality, speed, and time of the day in a behavior in which drivers want to avoid interactions with the police when they presume that police is a non-trustworthy actor. This makes drivers avoid contact as much as possible (thus they reduce speed), but if this interaction occurs and they do get a ticket, it has little effect on their subsequent risky behavior.

I also examined whether the result of scandal days depended on the ticket being given by a police officer in person[14]. Results were inconclusive. I was also unable to contribute to another important debate in terms of public policy, whether the effect is bigger or smaller for motorcycles.

All these findings are relevant for understanding the importance of police legitimacy. All the evidence shown points to legitimacy affecting in measurable ways the work done by the police. But more importantly, these results give

[14] A powerful argument in favor of automated enforcement is the fact that is less susceptible to being affected by scandals of the police.

important lessons on the public policy challenges around road accidents and fatalities.

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 is not increased. It might make more sense to increase the legitimacy of the police to get a bigger deterrent effect of the tickets already given.

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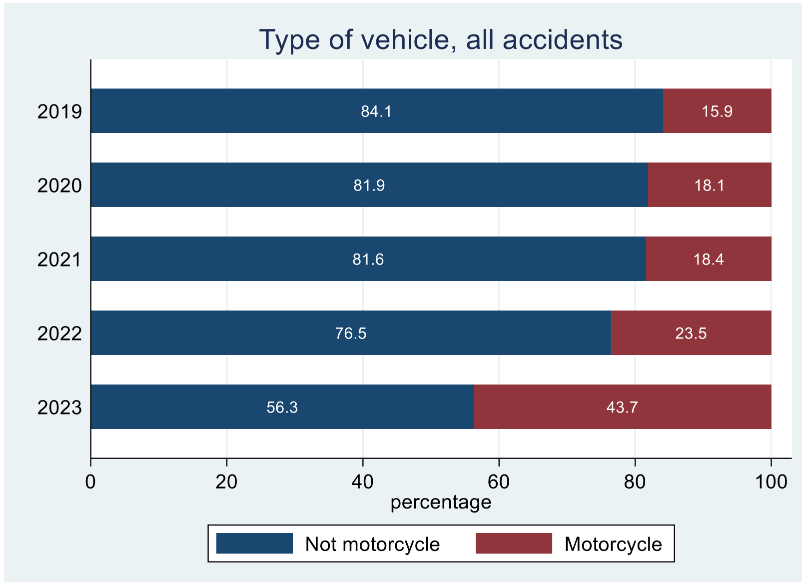
Appendix:



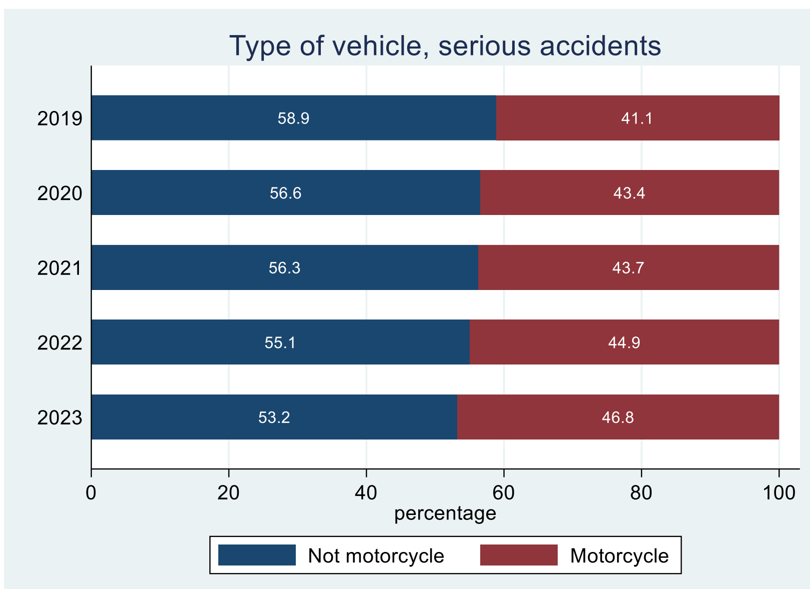


Figure 1. Participation of motorcycles in all accidents and serious accidents.

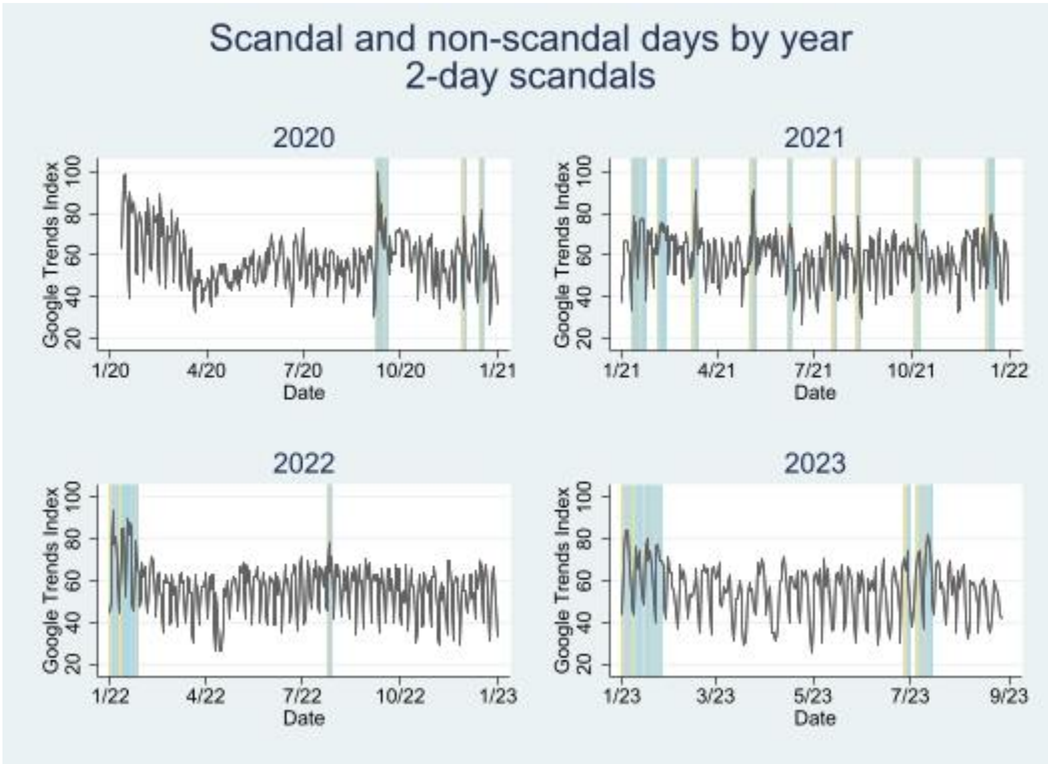


Figure 2. Distribution of scandals (in light blue) and non-scandal days (in light yellow) for the assumed duration of 2 days for scandals.

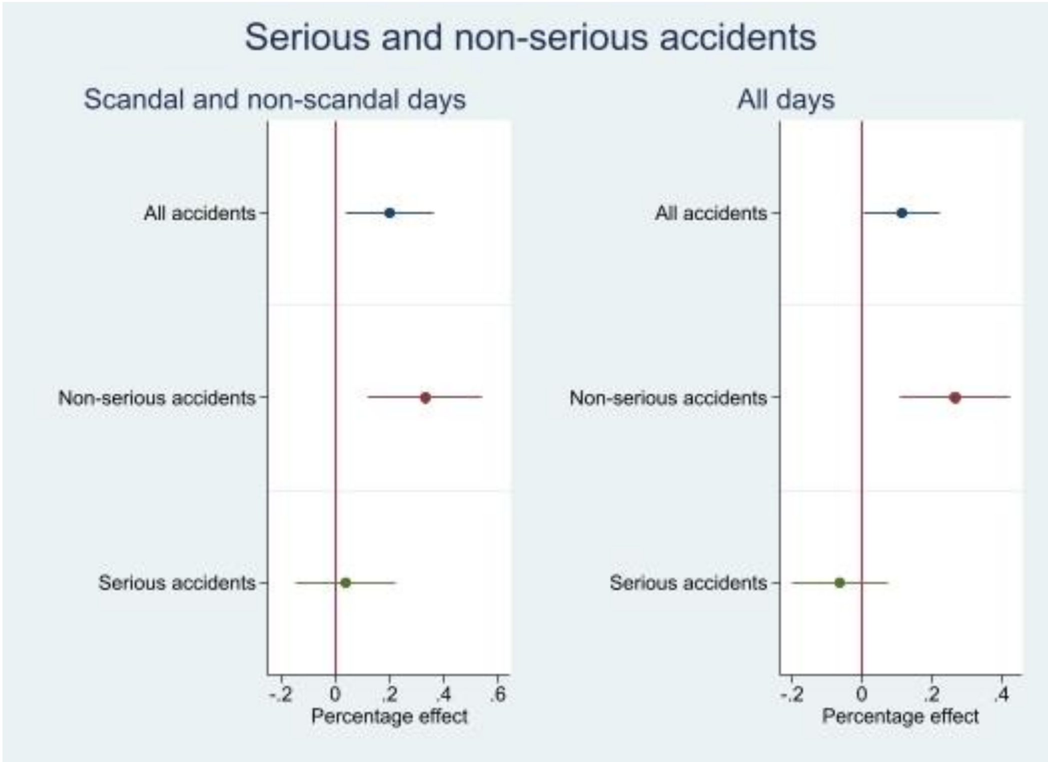


Figure 3. Dependent variable is 1 if the vehicle was involved in an accident a non-serious or a serious accident six months after the first ticket. Number of tickets issued and accidents per day are used as controls. All estimations include the following fixed effects for the ticket: year-week, day of week, type of infraction, source of the sanction and locality where it was issued. Fixed effect for the type of vehicle is also included. All errors clustered at the date level.

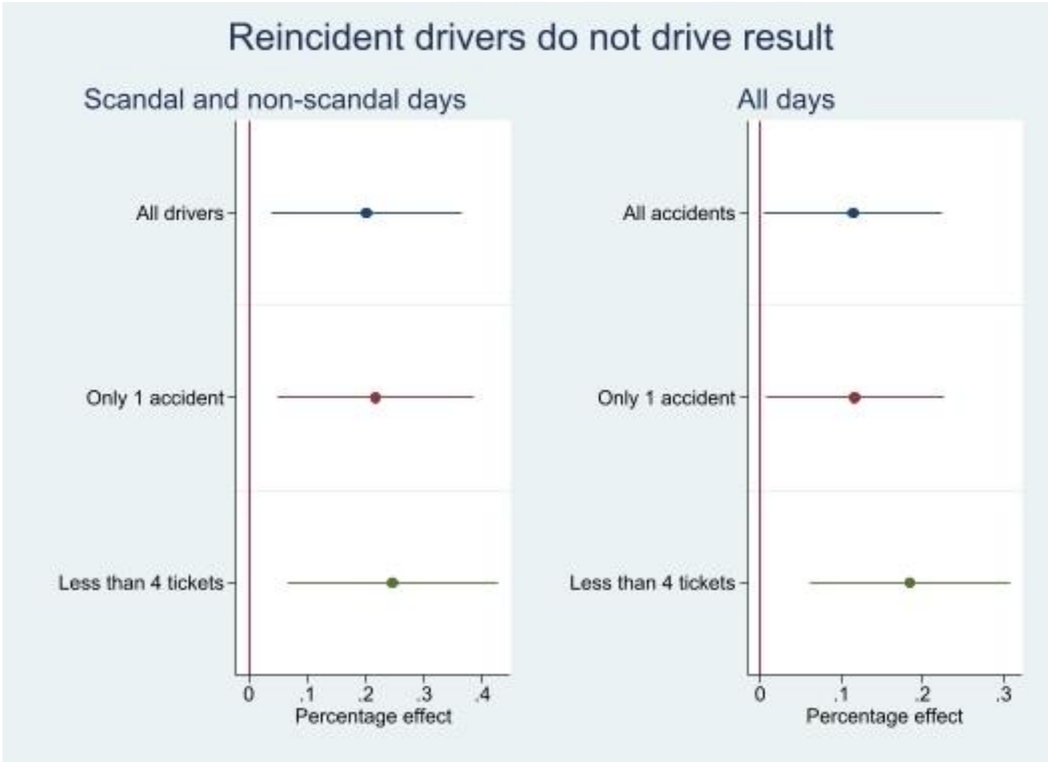


Figure 4. Dependent variable is 1 if the vehicle was involved in an accident six months after the first ticket. Row 1 uses all sample, row 2 excludes all drivers that are involved in more than one accident and row 3 all drivers that get more than 4 tickets in the sample. Number of tickets issued and 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 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 after the mortal beating of a man is demanded.
11	Jan 14-2022	General insecurity	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 delincuentes en Bogotá	Policemen dies while facing 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	Two more policemen to be

			policías por homicidio de Javier Ordóñez	tried 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 5. Dates and newspaper headlines for the Top 20 days in the Google Trends Index.

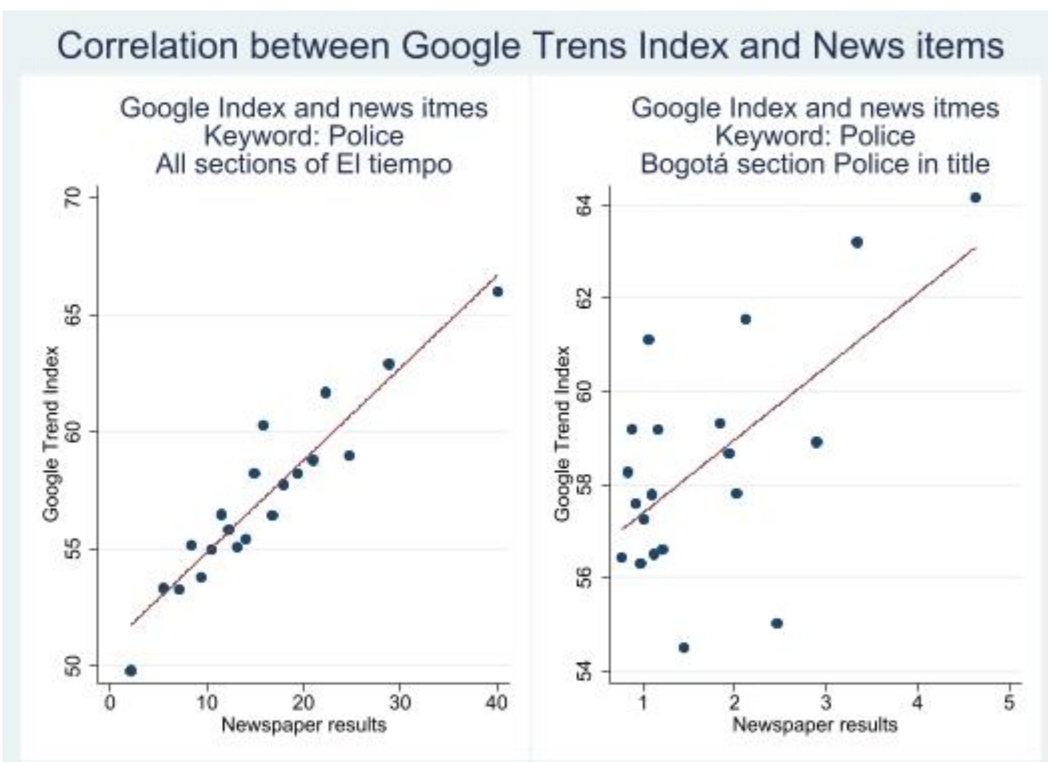


Figure 6. A binscatterplot graph is shown between the Google Trends Index for each day and the number of news items in the El Tiempo webpage. The scatterplot controls for year, week of the year day of the week and number of accidents and tickets each day. Column 1 uses all section of the news paper, 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.

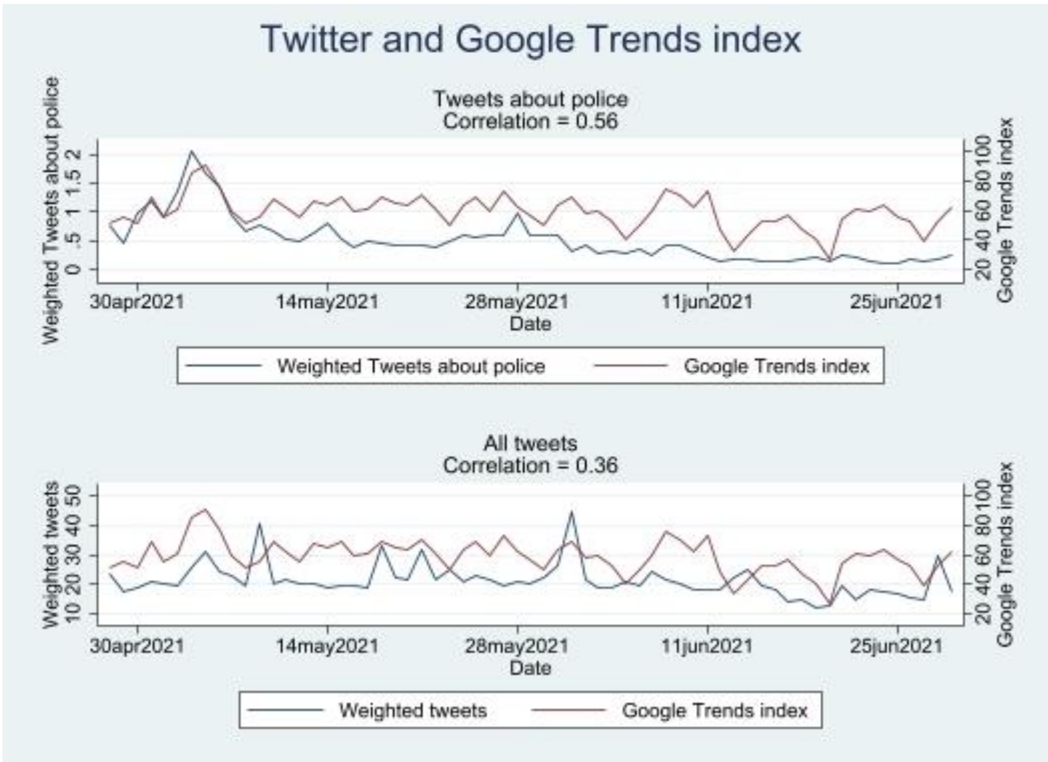


Table 7. A graph showing the value of Google Trend Index and the number of weighted tweets each day is shown. Weighted Tweets gives the value of its total retweets to any tweet and sums it over the date the tweets were written. Values for Weighted tweets are in millions. Tweets about the police are the 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 8. Dates and newspaper headlines for select days in which a policemen was the victim of an act of violence.

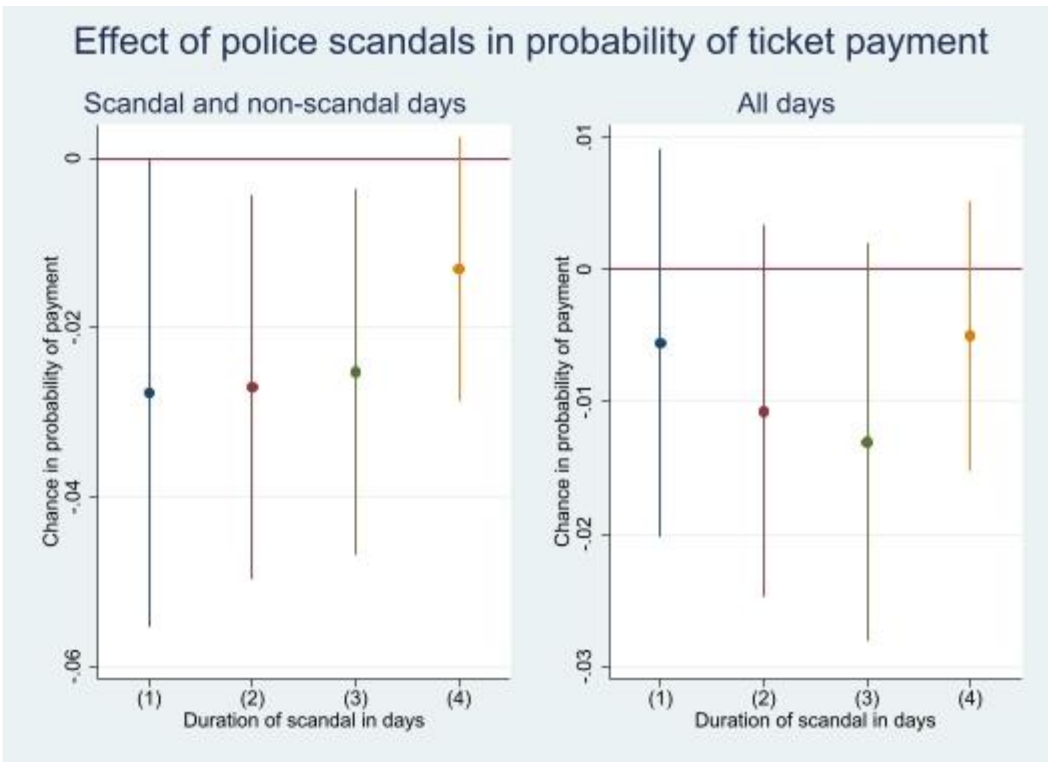


Figure 9. Dependent variable is 1 if the first ticket was ever paid. Number of tickets issued and accidents per day are used as controls. All estimations include the following fixed effects for the ticket: year-week, day of week, type of infraction, source of the sanction and locality where it was issued. Fixed effect for the type of vehicle is also included. All errors clustered at the date level.

1. How long does a scandal last?

It is clear that the biggest coefficients are found 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 4 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 11 is positive). Results (not reported) are essentially the same if days following a scandal are not clean days.

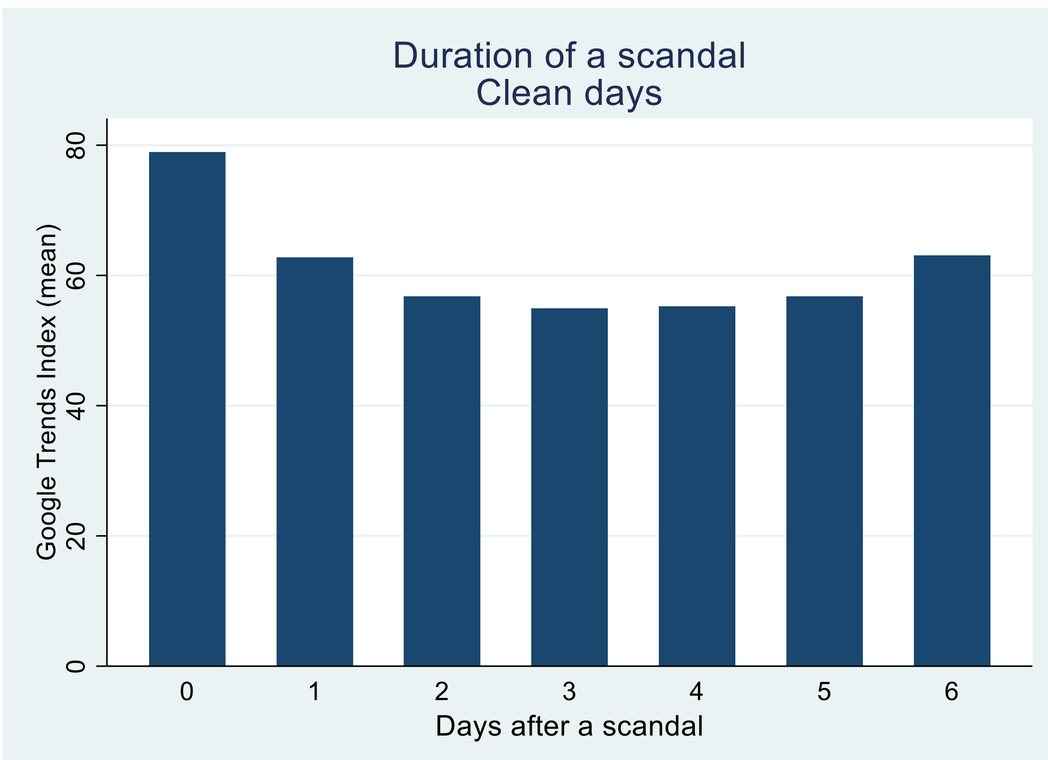


Figure 4. Average score for Googe Index searches of the world Police are shown for some days after a scandal. Only clean days are used, those that only follow a scandal that persists for a short period of time.

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 5 all scandal durations vary only after the scandal day, but the number of days prior to the scandal that are 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. The same pattern is found if previous days are fixed at 1 or 3 days before. Note also that this does not affect the most critical test of Table 10, since in that case all non-scandal days are coded as zeros.

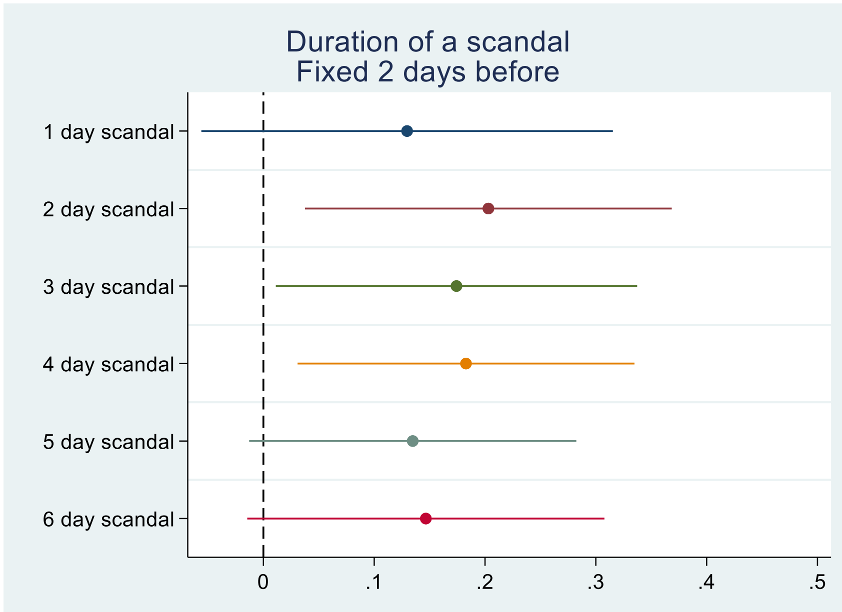


Figure 5. Coefficient for equation (1) are shown with the scandal day variable constructed as usual, but the days before a scandal duration fixed at 2 days. Dependent variable is 1 if the vehicle was involved in an accident less than six months after the first ticket. Number of tickets issued and accidents 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 effect for the type of vehicle is 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 6, 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.

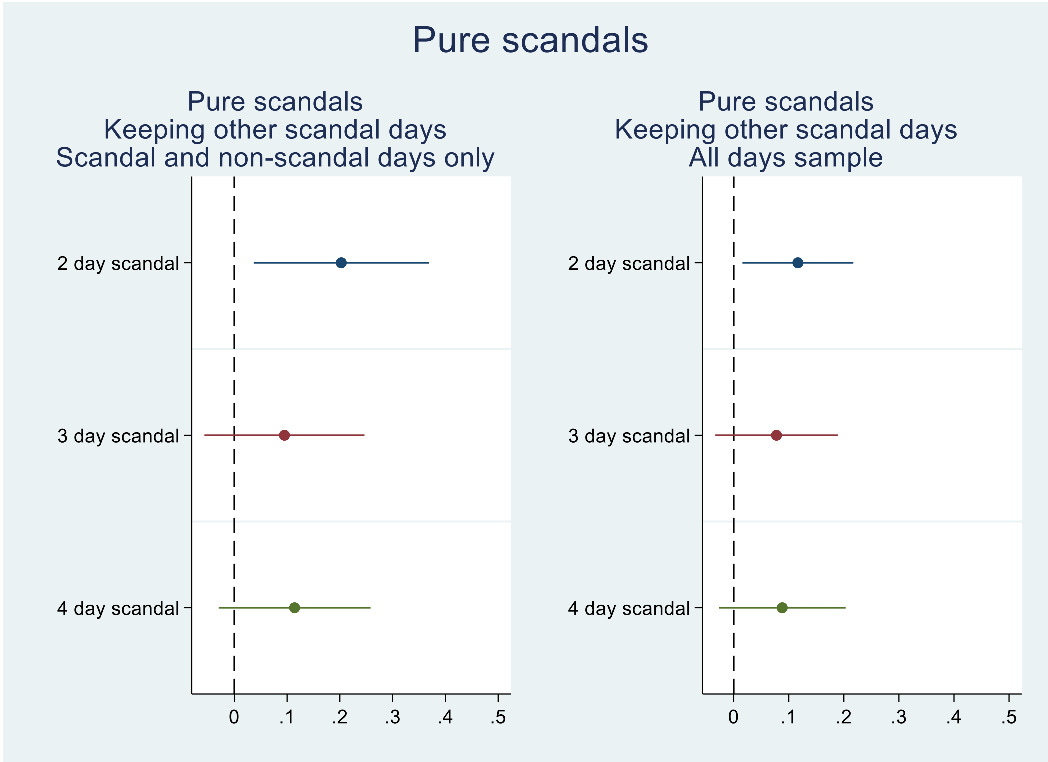


Figure 6. Coefficient for equation (1) are shown with the scandal day variable constructed only with pure scandals. Dependent variable is 1 if the vehicle was involved in an accident less than six months after the first ticket. Number of tickets issued and accidents per day are used as controls. All 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 effect for the type of vehicle is 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 in 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".

In some months during 2021 the whole country of Colombia lived what was called a "National Strike". A series of street protest 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: no Camera generated tickets were given between May 9 and August 12 of 2021. This 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 7 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 an accident. Using column (1) there was an increase of 40% in the chance of being in an accident 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 it can be seen in column (3) with the same 40% in the chance of being in an accident. 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 (0.10)***	0.26 (0.11)**	0.37 (0.10)***	0.42 (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 1 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.