Behavioral responses to fines: direct and indirect deterrence

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Abstract

Governments use fines to deter socially unwanted behavior on a wide range of contexts. It is commonly assumed that, fixing the probability of sanction, the higher the price, the more deterrent a fine is. But with limited government capacity for effective enforcement, the fine is often only an invitation for behavioral change and perceived fairness of the fine price might affect deterrence. I explore this behavioral mechanism with data from automated speed cameras in Bogotá, Colombia. I use the fact that the speed threshold for issuing a ticket differs from the posted speed limit (and is unknown to drivers) as a source of exogenous variation, and the high visibility of speed cameras as a way to differentiate direct (speeding at cameras sites) and indirect deterrence (crashes/speeding elsewhere). With a regression discontinuity design I compare drivers whose speed just exceeded the unknown real limit, and thus get a ticket, with those driving just below the real limit, who do not get a ticket. I find that a ticket causally changes the direct behavior of drivers: ticketed drivers reduce all forms of speeding (direct deterrence). However, I find no change in the probability of crashes (indirect deterrence). Using the commercial price of the vehicle as a proxy for wealth, I also estimate heterogeneous effects based on the relative price of the fine. Drivers at higher wealth levels both reduce speeding and crash involvement, while drivers on lower wealth levels show no change in speeding, but an increase in crash participation. My findings are consistent with fines being an invitation by the government to broadly change behavior that is only accepted if it is perceived to be fair. This has specific public policy implications: speeding tickets might not be a well targeted policy for preventing road fatalities. I hypothesize that in many contexts lower fines might be a more behaviorally effective policy tool.

JEL Classification Codes: K42, D91, R41, D61, H76.
Keywords: fines, deterrence, direct deterrence, indirect deterrence, relative price, behavioral responses, traffic crashes, speed cameras.

1 Introduction

A central question for States is how to improve the deterrent effect of their legal sanctions. A common assumption is that increasing the price of monetary sanctions results in a higher expected cost for citizens and thus in lesser prevalence of the unwanted behavior (Becker, 1974). But this only applies in locations or times where the State has the capacity to enforce the law in the future. When State capacity for enforcement is limited, citizens have a wide discretion on whether to accept or reject the call to change their future behavior. One can assume that this behavioral change depends positively on the perception of fairness of the fine: the higher the amount the less fair a fine is perceived to be. Thus, when enforcement capacity is limited, higher monetary sanctions may generate weaker—or even counterproductive—deterrent effects.

This paper addresses a general research question regarding the effects of fines: What are the direct and indirect deterrent effects of fines? But most importantly it also explores a heterogeneous effect question: Do these effects vary depending on the relative price of the fine? When talking about relative price I am referring to the fine price relative to personal wealth, as a proxy for how fair the fine is considered to be. Take taxes as an example: the government fines citizens when they evade their income tax, but it also wants them to pay all other taxes, which are harder to audit. In contexts with little State capacity for enforcement citizens might know that the State can only audit them on their income tax. Thus, the global effect of the fine can be more negative than expected if frustrated citizens start evading other taxes.

Speeding tickets in Bogotá, Colombia are a good case study for this question. In Bogotá seeding tickets are mostly automated in certain locations with road signs and clear visibility[1]. Citizens know where speed is enforced and also know that in the rest of the city speed is not enforced. Thus, future speeding on Camera locations is a good measure of the direct deterrent effect of fines. Since speed is considered the main cause of crashes, exploring future crashes is a good way of measuring the indirect effect of the fine: speeding when the government cannot enforce speed limits. In addition, speed fines in Colombia have a fixed price [2], thus comparing it with the vehicle price, an objective measure of wealth, is a very good proxy for the relative price of the ticket. This variability helps me explore the heterogeneous behavioral effects of the fine price.

[1]This has to do with the State stating that it does not want to surprise drivers in any way. A few months before my sample study (May 2024) there was a public campaign to make cameras and speed signs even more visible by painting the poles of each Camera with bright colors. Camera locations are also visible in most transport apps like Waze.

[2]A very important element behind the behavioral model's intuition is the fact that fines in Bogotá are comparatively expensive. The fine is determined by the minimum monthly salary. For all drivers in my data a speeding fine is equivalent to half a monthly salary, or the equivalent of 80 working hours. For comparison, in New York city a speeding fine ranges from 45 to 600 dollars (from 3 to 36 working hours). A city like Buenos Aires has a much closer price, with speeding fines ranging from $217.800 to $1.452.000 Argentinian pesos (from 0.73 to 4 times the monthly salary of 296.832 Argentinian pesos).

Examining the effect of speeding tickets on behavior is difficult because of selection. Drivers that decide to exceed speed limits may differ in unobservable characteristics that are also related to risky behavior and crashes. But in the case of Bogotá, the administrative process for giving speed tickets offers a way to solve this issue. Speed tickets are not given to all vehicles that exceed the maximum speed posted on road signs, but to a different speed with an additional tolerance. Since this hidden threshold is not known to drivers, a discontinuity exists. I use a regression discontinuity design to make a valid comparison between only drivers that decide to exceed the speed limit. I compare drivers just below and just above the real limit that the State uses, and can interpret the effect as causal. This methodology is confirmed with a judge-IV on the traffic agent that does a manual verification of each ticket. This alternative estimation confirms my general findings.

I find that speeding tickets have a general direct deterrent effect, measured by the probability of speeding. This effect is considerable in magnitude both at the extensive and intensive margins. I do not find a significant effect on the indirect deterrence, measured with the probability of being in a crash as a proxy for speeding behavior where citizens know that the law is not being enforced. For a subsample I then use the price of the vehicle as a proxy for wealth, to explore my heterogeneous effect question. I find that the direct deterrent effect of speed reduction is explained mostly by vehicles above the median price. I then show that the null effect on indirect deterrence is really the combination of an (unexpected) increase in the probability of a crash for vehicles below the median and the (desired) decrease in the probability of a crash for vehicles above the median. Higher relative price does make fines less behaviorally efficient.

Road crashes are a major public health issue. Road traffic crashes are the leading killer of children and youth aged 5 to 29 years and are the 12th leading cause of death when all ages are considered (World Health Organization, 2023). While this paper tries to answer a more general question on the effect of fines on deterrence, the area in which it answers it can also give actionable and needed policy recommendations.

This paper contributes to four different strands of the economic literature. First, it relates to a wide literature on the intended and unintended effects of State policies on behavior. An important branch of the literature studies what citizens learn by interacting with law enforcement. Using a change on the punishment for recidivism, (Philippe, 2024) finds that learning about criminal law appears to be limited to people with direct involvement with the law. In the same direction, it has been shown that individuals do adjust their priors after receiving a speeding ticket (Dušek & Traxler, 2022). The question of whether a fine is just like any other price (Gneezy & Rustichini, 2000), and what type of price it is (Feldman & Teichman, 2008) has been explored in many dimensions. Another branch of this literature asks whether fairness considerations affect the efficiency of some policies. Taxation is a topic in which fairness considerations have shown to affect behavior in ways that are meaningful for policy design (Best et al., 2025; Perez-Truglia et al., 2024). This paper tries to bring questions of fairness (Kahneman et al., 1986) to the economic understanding of crime, using speeding tickets only as a data-rich case. I contribute to this literature by exploring how interactions with the law affect future behavior when citizens know the law is enforced (direct deterrence) but also when they know is not enforced (indirect deterrence), and with a novel and rich dataset that allows for precise estimations at the individual level.

This paper also relates to the specific literature on the analysis of the effects of financial fines and fees imposed by the State. There is ample public debate on the capacity of higher fines to increase revenue and the effect this has on recidivism. Some papers find a modest effect on revenue at the cost of higher recidivism (Giles, 2023). Others argue that even if the effect of sanctions is higher revenue, they have no effect on recidivism and thus are not justifiable ways of increasing deterrence (Finlay et al., 2024). The authors argue that the null effect on all relevant criminal behaviors is precisely estimated, not due to lack of power. In recent work, (Norris & Rose, 2024) argues that reducing legal financial obligations (LFOs) will impact municipal revenues without increasing deterrence. The evidence provided by (Bing et al., 2024) on a randomized payment of existing fines for criminals shows that this policy only reduces recidivism related to the financial costs of the LFO itself. These effects are not negligible and point to fines not having a deterrent effect but a significant cost of future engagement with the criminal system. I contribute to this literature by looking beyond recidivism and exploring a behavioral mechanism, perception of unfairness, as a possible explanation.

The question of the optimal amount of a fine has also been examined in the specific case of traffic tickets, but mainly through the lens of revenue, not deterrence like I do here. Some authors have shown that increasing fines has only a modest effect on timely payment (Traxler & Dušek, 2023) and recidivism (Dušek et al., 2022). A very detailed study of the effect of the price of fines is (Kaila, 2024), who uses the relative to income price of traffic tickets in Finland to show that there is in fact a reduction on recidivism for higher fines, but the effect is short lived and the fines need to be significantly more expensive. In their work, (Goncalves & Mello, 2024) also study the effect of harsher sanctions by exploiting the discretionary sanctions given by police officers. They find that harsher fines do reduce recidivism and, to a lesser degree, crash involvement, but in the aggregate discretion introduces inefficiency. This paper explores the opposite side of the same mechanism; fixed price fines that in practice are different depending on individual wealth.

Finally, this paper also contributes to a broader literature on the effect of traffic enforcement on behaviors (Angelo & Hansen, 2014; Hansen, 2015; Luca, 2015; Makowsky & Stratmann, 2011). In particular, my work adds to a growing literature that studies fines and enforcement in relation to factors that do not affect either fine amount or probability of enforcement but have been shown to have an effect on traffic crashes. Things like the political cycle (Bertoli & Grembi, 2021), or the stock market (Fry & Farrell, 2023; Giulietti et al., 2020) seem to have an effect on behavior. Close to this strand is a set of papers that explores behavioral responses to traffic enforcement and tries to derive policy recommendations to reduce road crashes and fatalities (Lu et al., 2016; Zhang et al., 2020). My work contributes by suggesting perceptions of fairness of sanctions as a mediator of changes in enforcement effectiveness. I also contribute to the literature on road injuries and fatalities by adding evidence from a city in the developing world, something that is still rare in the field.

The paper proceeds as follows. Section 2 presents the institutional background of speed fines in Bogotá, Colombia and the data used in the estimation. Section 3 discusses the estimation strategy and presents the evidence necessary for its validity. Section 4 presents all the results from the estimation on the general effect of speeding tickets with a fixed price. Section 5 presents results on the relative price of vehicles. Section 6 presents some robustness checks on the main estimation. Section 7 briefly discusses other behaviors that reinforce the credibility of the main findings. Finally, section 8 discusses the main findings in relation to fines as an effective deterrent strategy in other public policy areas and derives possible policy recommendations.

2 Institutional background and data

Automated speed enforcement started in Bogotá in 2020 with the staggered installation of cameras in the most dangerous zones, a successful intervention for reducing crashes (Ferro, 2024). The program was originally called Cámaras Salvavidas (Spanish for Lifesaving cameras) to emphasize that the objective of the program was not municipal revenue but behavioral change. After some debate on the legality of the fines, Colombia's higher court allowed for the fine to be given to the owner of the vehicle[3]. Two elements of the legal framework are crucial in the rest of the paper. First, tickets have a fixed price independent of the type of vehicle, the owner's wealth, or the speed at which the vehicle was traveling[4]. Second, there is no additional fine for recidivism.

The central database I will use for defining treatment includes all vehicles that exceeded the legal speed limit under a Speed Camera from August 16 to October 15, 2024. This dataset includes 179,612 distinct vehicles that passed a speed camera during the hours when tickets are issued (daylight). I will only be using vehicles that passed under the cameras once and had not received a speeding ticket in the previous six months, in order to separate the effect from behavioral effects of the previous interaction with the law. A total of 157,245 comprise the full sample used for estimation[5], equivalent to 87% of the plates that exceeded the limit. I will call this sample my Full sample.In my robustness checks section, I show that the results are unchanged when vehicles that passed more than once are included.

For my outcome variable I have a database with all tickets and crashes that occurred

[3] This is ruling C-321 of 2022 by Corte Constitucional del Colombia, Colombia's Supreme Court. It is important that more than two years have passed by the period my sample starts, so no driver is able to avoid paying the fine once it has been imposed

[4] technically there is an early payment, but all vehicles are subject to this, so this does not affect the differences on relative price. As I argued before, it is a considerable amount in absolute terms: one legal monthly salary

[5] Full distribution of the number of observation by vehicle is in this sample is shown on Table A.1. This is a particular problem since vehicles might circulate one time below the real limit and another one above the real limit, thus making it harder to classify them as treated or untreated in terms of the discontinuity. This is complicated by the fact that there is some delay on ticket notification, so both times can be before receiving a ticket. I will do robustness checks on this, but mostly just use vehicles that circulated once

in Bogotá[6]. I can see if each plate received a ticket (for speeding or another cause) or participated in a crash both before and many months after the period of study, until March 15 of 2025. Those two will be my main outcome variables.

Finally, I can access the registration record for all vehicles in Bogotá (and some that pay congestion pricing [7]) and match the plate with information on brand, model and year of the vehicle. I do a web scraping procedure of the most used website for selling used cars in the city to obtain the commercial value of each brand-model-year combination. All data on vehicle prices is taken from tucarro.com, the most used website for selling used vehicles. Vehicles that do not appear on that base are given an imputed value from a regression on year and cubic capacity on price for each class-brand combination. This leaves me with a total of 67,235 vehicles for which I can find their commercial price as a proxy for driver's wealth, naturally a subsample but big enough for the estimation to have statistical power. I will call this sample my Matched sample[8]. The heterogeneous effect of the relative price of the ticket can thus be estimated. Local effects and external validity questions will be analyzed in the Results section.

3 Estimated equation

My identification strategy relies on a fact that is unknown to drivers: tickets are not given at the speed posted on the road but on a different number. On the following I will call the legal limit posted on the road sign (50 km/h) as the posted speed limit, and the limit at which most tickets are really given ($50 + \epsilon$ km/h) as the real limit[9]. This discontinuity creates useful exogenous variation. In most of my applications, I estimate the following equation:

$$Result_i = f(z_i) + \tau D_i + \varepsilon_i \tag{1}$$

Where Result_i is one of the behavioral outcomes for vehicle i: a dummy that takes the value of 1 if the vehicle had at least one instance of excess speed in the six months after receiving a

[6]For the crash data I also use the data for the centralized health system that is specifically dedicated to traffic events. This data is under a system called SIRAS (System for the health services to victims of road crashes, its name in Spanish), and belongs to the national Minister of Health, not the city of Bogotá

[7]Some vehicles can pay to avoid a circulation restriction that exists in the city (*Pico y placa Solidario*). All the data I need is registered on these transactions.

[8]The decision to register the vehicle in Bogotá is surely related to behavior, as can be derived from the fact that the registry is growing in surrounding municipalities but not in Bogotá. This is particularly caused by the decision of new motorcycle owners to register their vehicle outside Bogotá, even if this is where they plan to use it. Thus, the effect seen on that sample might only extend to other drivers that decide to register their vehicles in Bogotá.

[9]In the sample there is only one Camera that has 30 as the maximum speed. It only accounts for 4,460 observations (1.82%). Speeds are adjusted accordingly. By agreement with the Mobility Office of Bogotá, I will not reveal the specific value of ϵ in order to protect public policy from undesired disclosure

ticket and 0 otherwise[10], or a dummy that takes the value of 1 if the vehicle had one crash in the six months after receiving a ticket and 0 otherwise. I use higher excess speed and serious crashes (those with sever injury or death) in a few estimations, and also use the total tickets or speeding infractions

$f(z_i)$ is a polynomial of degree 2 (Gelman & Imbens, 2019) for the variable that decides treatment; the speed at which vehicle i is circulating under a Speed Camera. As I said, all vehicles in the sample exceed the posted speed limit, but only some exceed the real limit. This reduces selection and allows me to identify causality, at the cost of identifying a local effect.

D_i is my treatment variable, which takes the value of 1 if the vehicle received a speeding ticket and 0 otherwise. The treatment is instrumented via the assignment, since this is a fuzzy design, and the discontinuity at the real speed limit is around an increase of 27 p.p. on the probability of receiving a ticket.

ε_i is an error term.

τ is my coefficient of interest. I will argue that it captures the causal effect of receiving a ticket in subsequent behavior. It is a local effect, since it only captures the effect of a ticket for vehicles around the cutoff, but those are the vast majority of observations in the sample[11]. For all estimations, I use the rdrobust package in STATA with the option to ignore mass points, as the variable that assigns treatment is discrete. The bias-corrected estimate will always be shown. When studying the heterogeneous effect of relative price on behavior I will run regression (1) separately for vehicles above and below the median vehicle price. Occasionally I will use the quintiles of the vehicle price distribution. When I use the vehicle price distribution I am not doing an intra-sample comparison but a comparison with all other vehicles registered in Bogotá. That is why I have many more vehicles above than below the median price in my sample.

Since the estimation wants to separate the effect of a possible ticket from any other change, I will only use the vehicles that have not received a ticket in the 90 days before passing under a Camera exceeding the legal speed limit. In the robustness section I show that nothing essential depends on this decision.

3.1 Validation of the Fuzzy Regression Discontinuity Design: Existence of discontinuity:

It might seem obvious to say, but I need to show the reader that there is indeed a discontinuity at the real limit. Figure 1 shows that enforcement does work with the real limit, not the one posted on the street signs. It also shows that the discontinuity is not 0 to 1, so my strategy

[10] Since the real limit is unknown to drivers, a future excess speed is the one above the posted limit, not above the real one

[11] 80% of observations pass under speed cameras inside the bandwidths selected

will be a fuzzy design. The reason behind this jump not being to 1 is mainly the quality of the pictures taken by the cameras. A police officer verifies that each picture is sufficiently clear to transform it into a ticket. Later, I will use the human component of this analysis to verify my estimate with a judge-IV type estimation. In summary, even if the behavior of driving at ($50 + \epsilon$ km/h) is very similar to driving at ($50 + \epsilon + 1$ km/h), and both are above the legal limit, the latter has four times the probability of receiving a ticket than the former.

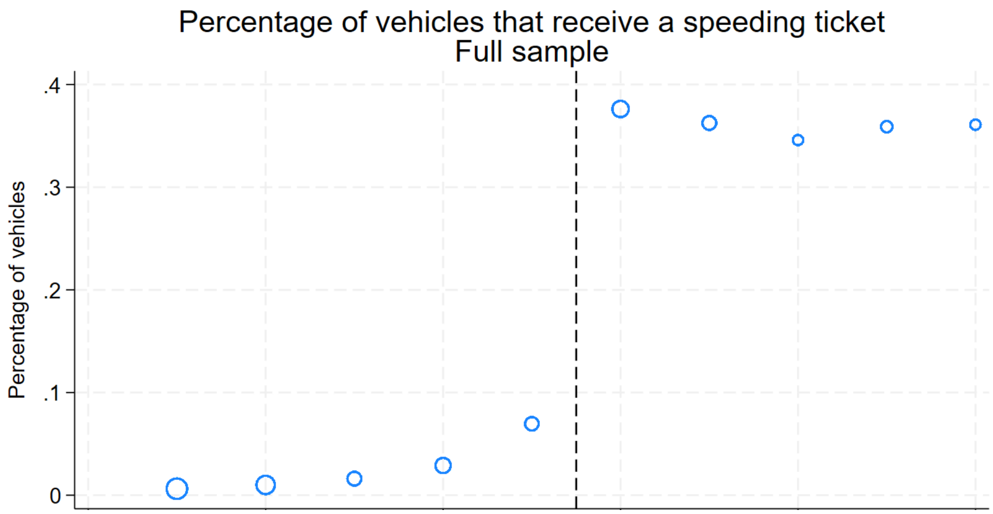

Figure 1: all vehicles in this graph are above the posted limit of 50 km/h. The size of the circle is proportional to the number of observations. Values are not displayed in the X axis by agreement with the Mobility Office of Bogotá

It is important to show that this discontinuity also exists in the matched sample and is similar for different values of vehicle prices. Figure A.1 plots this discontinuity for all matched vehicles and for vehicles above and below median price, used later in my relative price analysis. Figure A.2 does the same by quintiles of vehicle price. The differences are quite small, with a slight higher discontinuity for more expensive vehicles[12].

3.2 No manipulation

Now that I have shown that the discontinuity exists, I need to verify it is not caused by the behavior of drivers. In other words, drivers are not aware of the real limit. The frequency of vehicles at each speed level is shown in Figure 2 for the Full Sample and Figure A.5 for the Matched Sample. I also can show in Figure A.6 that there is no manipulation either below

[12]A very relevant question for local public policy is whether this deterrent effect of tickets varies by vehicle type. This is caused by motorcycles being a growing percentage of road injuries and fatalities both in Bogotá and in all Colombian cities, as in similar middle income countries. Figure A.3 and Figure A.4 show this for the Full and Matched sample. It can be seen that the discontinuity is smaller for motorcycles, and that this is considerably smaller in the case of the matched sample. This is less a threat for the identification strategy and more a fact to have in mind when obtaining public policy recommendations from results

or above the median vehicle price[13]. All these graphs show that the only possible violation of no manipulation occurs to the right of the cutoff, which is the undesired condition for drivers.

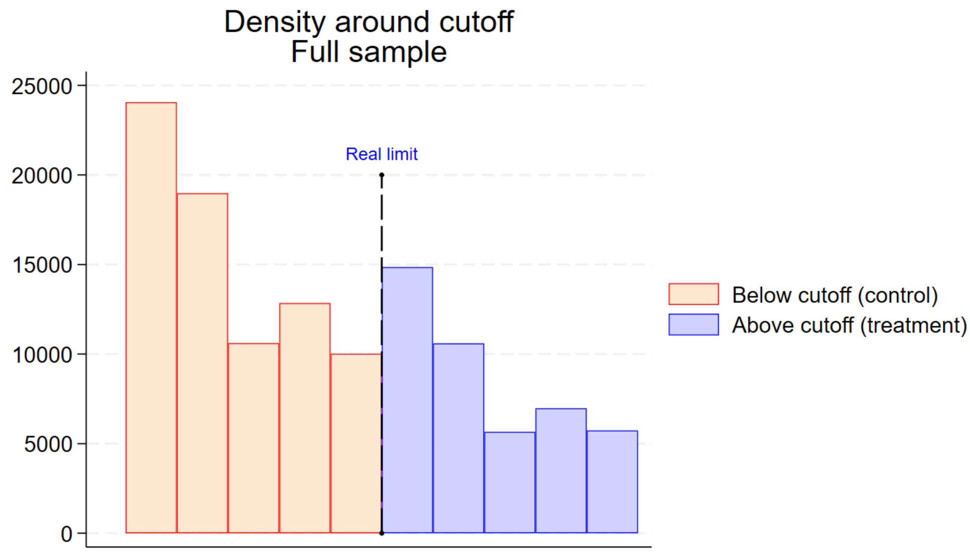

Figure 2: Frequency by condition in the Full Sample. All vehicles in this graph exceed the posted limit. Below cutoff (control) are vehicles observed below the real speed limit, and Above cutoff (treatment) are vehicles observed above the real speed limit. Values are not displayed in the X axis by agreement with the Mobility Office of Bogotá

More formal evidence of this can be given by running the manipulation test developed by (Cattaneo et al., 2018) shown in Figure 3. This is because of the discrete values of the assignment variable that does not allow for a McCrary test to be performed. Figure A.8 performs this test with different decisions regarding the polynomial degree. Figure A.9 shows that the test is also not significant in the matched sample. More importantly, all graphical evidence shows if there is a discontinuity it is to a higher density at the right of the cutoff. This is no evidence of manipulation since drivers do not want to get a ticket.

[13] A very stringent test of this lack of selection are vehicles that did receive a ticket in the prior 90 days, which I don't use for different reasons in my main estimation. In Figure A.7 I show that even drivers that have had an interaction with the law are not selecting themselves into not receiving a ticket. The same is true for vehicles that speed more than once in the study period. This group, arguably more speed enthusiastic, should bunch more at the real limit. But again, if anything the graph (not shown) shows there is some bunching to the right of the real limit

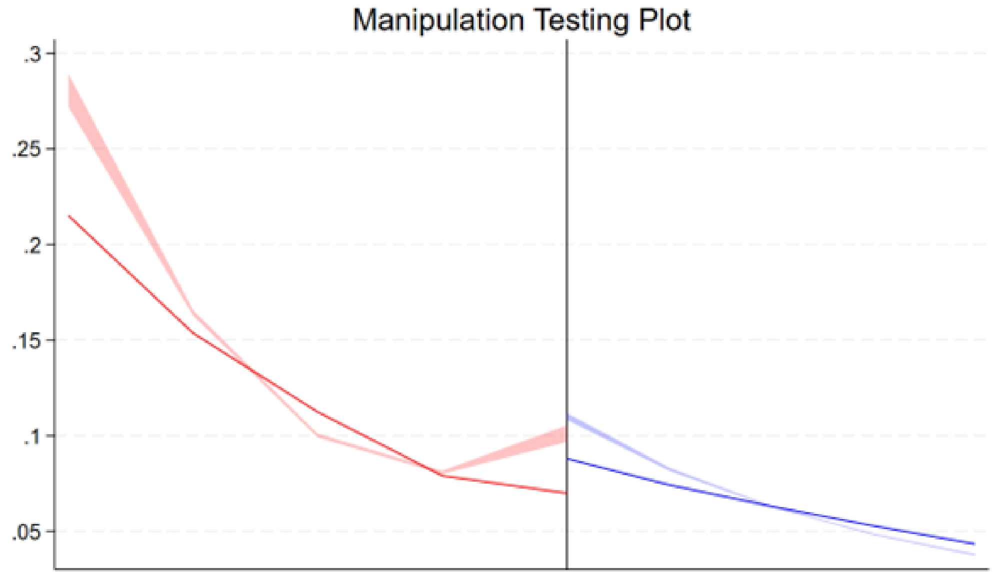

Figure 3: Manipulation test based on discontinuity at the real limit. Sample with polynomial of degree 2. Full Sample.

Another way to show that drivers are not selecting themselves into not receiving a ticket is to see what happens at the posted limit. One can see in Figure A.10 that there is bunching to the left of the posted limit, but to the right of the real limit. This is ratified by Figure A.11 in which I use the (Cattaneo et al., 2018) test at the posted limit and find that in this instance the null hypothesis of continuity can be rejected, since vehicles do bunch to the left. In summary, vehicles select into treatment at the posted limit, but not at the real limit. This is suggestive evidence that they don't know the existence of a real limit at which tickers are effectively given.

3.3 Smoothness on observables

The smoothness of observable variables of the vehicles involved at the real limit is another element necessary for the validity of the fuzzy regression discontinuity design. First, I will verify this first on the full sample, and then on the matched sample, in which naturally I have more knowledge of the vehicle characteristics. For all estimations I allow for optimal and different bandwidth below and above the cutoff.

3.3.1 Full sample

The first variable I explore is the hour of the day at which the ticket is received, a possible indications that drivers are different in the treatment and control groups. I can even compare if there is discontinuity in the provability of being in rush hour traffic, finding a slight difference. The same selection can be analyzed with day of the week as a continuous variable or a dummy if the ticket was given during the weekend. Since in Colombia, plates for motorcycles have a different configuration than those of other vehicles [14], I can observe the type of vehicle involved. I use a dummy that takes the value of 1 if the vehicle involved is a motorcycle. This variable is in fact discontinuous, and magnitude is considerable. It is also important to observe that treated and not treated do not differ in their road usage. I compare the number of times each vehicle passes under a Camera not speeding, before the first speeding infraction. This is a good proxy for their road usage, and one that is discontinuous at the real limit, but with a smaller magnitude than the motorcycle dummy. In the following I will add type of vehicle and previous observations as covariates that affect the treatment status. All this is shown in Table 1.

Smoothness on observables. Full sample

	(1)	(2)	(3)	(4)	(5)	(6)
	Hour of the day	Rush hour dummy	Day of the week	Weekend dummy	Motorcycle dummy	Observations per day before
Bias-corrected	0.188	-0.0579*	0.0146	-0.0345	-0.0867***	-0.569**
	(0.248)	(0.0335)	(0.148)	(0.0338)	(0.0336)	(0.274)
Mean of not ticketed	11.80	0.350	4.156	0.350	0.341	3.682
N	136533	136533	136533	136533	136533	136533
N left of cutoff	52468	52468	52468	52468	52468	52468
N right of cutoff	57605	57605	57605	57605	57605	57605
BW left	4.500	4.500	4.500	4.500	4.500	4.500
BW right	9.500	9.500	9.500	9.500	9.500	9.500

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 1: Smoothness on observables. Full sample. Hour is a continuous variable with the time in which the vehicle passed under the camera. Rush hour is a dummy that takes the value of 1 if the vehicle passed under the camera at rush hours (6,7 am or 4,5,6 pm) and 0 otherwise. Day is a continues variable that goes from 1 (Monday) to 7 (Sunday). Weekend is a dummy that takes the value of 1 for Saturday and Sunday, and 0 otherwise. Motorcycle is a dummy that takes the value of 1 for motorcycles, and 0 otherwise. Observations per day before is the total times each vehicle passes under a Camera divided by the number of days before the speed excess

To better understand the composition of my sample I can also do a smoothness analysis on the real speed limit, not as a formal test but to understand what type of vehicles are selecting themselves into my sample. This analysis shows that, using the first time seen under a speed camera, speeders are most common early on the week, and on weekdays. Also, unlike at the real limit the coefficient for motorcycles is positive, suggesting that drivers of motorcycles

[14] Motorcycle plates end in a letter, whether all other vehicle's plates end in a number. As will be seen later, for the matched sample I have much more knowledge about the vehicle.

are more prone to speed, but by little. Additionally the coefficient for previous observations is negative, which can be interpreted as vehicles that decide to speed are less frequent road users [15]. All this points to the sample of speeders being different, but not to different than non-speeders.

3.3.2 Matched sample

In this case the total number of available observations is 67,235[16], around 40% of the Full Sample. In line with the Full Sample, in Table 2 most of the variables are smooth at the cutoff. It is also the case that vehicles in the sample tend to come not from the rush hour hours (and there is significance). There are also less motorcycles at the right of the cutoff, but one should note the significant difference in percentages between the two samples. Whereas in the Full Sample 34% of vehicles are motorcycles, this percentage is only 7% in the matched sample. Note also that the Matched Sample has a higher number of observations before the first excess, meaning that these vehicles are more frequent users of the road than the general sample. Both facts reinforce my argument about the Matched sample not being a representative slice of the Full Sample that I made based on the fact that where to register a vehicle is also a decision. Note also that there is no significant difference in terms of observations before the speed excess and price. Both facts are also central to my estimation later on. In this Matched Sample I can also evaluate the type of service which the vehicle is assigned to through a dummy that takes the value of 1 if a vehicle is used for public service (mainly taxis and similar services). I can also say a little more about the vehicle type, besides not being a motorcycle. In this case I construct a dummy that takes the value of 1 if a vehicle is an SUV like vehicle. Both of these additional variables are smooth at the cutoff.

[15] One can do this same analysis not with the first observation by plate, but by observations. Total observations around the posted limit are more than 20 million, and none of the variables are discontinuous. This is hard to interpret as proof that vehicles that speed are similar to those that do not, but differences are very subtle

[16] As I explained before, the vast majority of those (62,288 or 92% of the matched sample) correspond to vehicles that are registered in Bogotá. The rest are registered in other municipalities but pay an exemption for the congestion restriction that exists in the city that allow local authority access to the vehicle data

Smoothness on observables. Matched sample

	(1) Hour of day	(2) Rush hour dummy	(3) Day of week	(4) Weekend dummy	(5) Motorcycle dummy	(6) Observations per day before	(7) Price of vehicle	(8) Service type	(9) SUV dummy
Bias-corrected	-0.290	-0.0904**	0.196	-0.0193	-0.0582***	-0.232	-7.594*	-0.0203	-0.0633
	(0.287)	(0.0388)	(0.179)	(0.0406)	(0.0211)	(0.364)	(4.428)	(0.0294)	(0.0388)
Mean of not ticketed	11.95	0.336	4.320	0.402	0.0702	4.747	56.86	0.153	0.328
N	67235	67235	67235	67235	67235	67235	67235	67235	67235
N left of cutoff	26547	26547	26547	26547	26547	26547	26547	26547	26547
N right of cutoff	26911	26911	26911	26911	26911	26911	26911	26911	26911
BW left	4.500	4.500	4.500	4.500	4.500	4.500	4.500	4.500	4.500
BW right	9.500	9.500	9.500	9.500	9.500	9.500	9.500	9.500	9.500

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 2: Smoothness on observables. Matched sample. Hour is a continuous variable with the time in which the vehicle passed under the camera. Rush hour is a dummy that takes the value of 1 if the vehicle passed under the camera at rush hours (6,7 am or 4,5,6 pm) and 0 otherwise. Day is a continues variable that goes from 1 (Monday) to 7 (Sunday). Weekend is a dummy that takes the value of 1 for Saturday and Sunday, and 0 otherwise. Motorcycle is a dummy that takes the value of 1 for motorcycles, and 0 otherwise. Observations per day before is the total times each vehicle passes under a Camera divided by the number of days before the speed excess. Price of vehicle is the comercial price for a used car with the same brannd and model. Service typ is a dummy that takes the value of 1 for all vehicles that are in public service, and 0 otherwise. SUV dummy takes a value of 1 for SUVs and 0 otherwise

Also central to my estimation on the heterogeneous effect of relative price is verifying the smoothness below and above the median price. This is shown on Table A.2 and Table A.3. In general, there is smoothness, but there are some exceptions. In any case, as I do in my Full sample all the observables that are significant will be used as covariates for treatment assignment.

4 General results

In this section I show all the results of the estimations. In section 4.1 I present all results on direct deterrence, measured by subsequent speeding behavior. In Section 4.2 I present results on indirect deterrence, measured by future involvement in crashes. Chapter 5 is dedicated to the analysis of relative price.

4.1 Direct deterrence – Speeding

The most basic deterrent effect of a speeding ticket is to prevent speeding itself. Note that the assumption that drivers do not know the real speed limit implies that reoffending is measured by exceeding the posted limit. Since the risk of injury and death grows with the level of speeding it is also important to measure the deterrent effect that a ticket has on more serious infractions far above the posted and real limit. With my regression discontinuity I am able to measure both the extensive and intensive margins of moderate and major speeding.

Table 3 presents the results for equation (1) with different dependent variables related to speeding for a period of six month after a vehicle is observed speeding under a Camera. Moderate Speeding refers to instances where a vehicle was observed at a speed between 50 km/h and 60 km/h at least once. Major speeding refers to instances where a vehicle was observed at a speed higher than 60 km/h at least once. Odd columns refer to a dummy that measures if the vehicle ever had the behavior (extensive margin), while even columns measure the total number of times each vehicle had the behavior (intensive margin). Most, but far from all, vehicles commit a speeding infraction only once.

Tickets do have a causal effect of reducing all types of speeding behavior. This reduction is high in magnitude, both at the extensive (77% reduction in the chance of reoffending) and intensive margins (80% reduction in the total of future speeding instances). Very important is the fact that the result stands for major speeding, even if its a much frequent phenomena. In fact, the reduction is bigger in magnitude for Major speeding. I can even see (results not reported) that a fine has a negative effect on average speed in the future, pointing at drivers not only wanting to avoid fines or bunching at speed limits but reducing speeding altogether.

Effect of receiving a ticket. Full Sample

	(1) Moderate Speeding dummy	(2) Moderate Speeding continuous	(3) Major Speeding dummy	(4) Major Speeding continuous
Bias-corrected	-0.175***	-0.295***	-0.0651***	-0.0744***
	(0.0294)	(0.0639)	(0.0164)	(0.0250)
Mean of not ticketed	0.226	0.370	0.0555	0.0716
N	136533	136533	136533	136533
N Left of cutoff	52468	52468	52468	52468
N Right of cutoff	57605	57605	57605	57605
BW left	4.5	4.5	4.5	4.5
BW right	9.5	9.5	9.5	9.5

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 3: Direct deterrence. Full sample. Column 1 is a dummy that takes the value of 1 if the vehicle had at least one Moderate speeding (between 50 and 60Km/h), and 0 otherwise. Column 2 is a continuous variable, the total of times a vehicle was captured in Moderate speeding. Column 3 is a dummy that takes the value of 1 if the vehicle had at least one Major speeding (above 60 km/h), and 0 otherwise. Column 4 is a continuous variable, the total of times a vehicle was captured in Major Speeding

A central question for policy makers is the effectiveness of speeding tickets and other measures for speed control in the face of the growing number of motorcycles in middle income countries, and the risk they represent [17]. As I explained before, the existing data only allows me to build a dummy that says if a vehicle is a motorcycle or not. Table A.4 and Table A.5 show that deterrent effect is only present for not-motorcycles, which is in line with my behavioral mechanisms since motorcycle drivers are a group known to consider traffic enforcement of speed limits an unfair action by the government. I will argue that this null result is not due to lack of power, since I have significant results with smaller number of observations and less variation on the dependent variable.

[17] In Bogotá, motorcycle users were present in 45.5% of crashes with a death in 2015. This number has steadily grown to 68.5% in 2024.

I can also do this analysis in the matched sample. As Table A.6 shows the results are essentially the same, a general effect of reduction in all types of speeding. The only minor difference is the result for Major Speeding, which is slightly smaller in magnitude and less robust in the Matched Sample than in the Full Sample.

4.2 Indirect deterrence – crashes

As I argued in the introduction, speeding tickets have an indirect deterrent purpose explained by the fact that speeding both makes crashes more probable and more severe[18]. For policy makers this indirect effect is the most important one(Bocarejo, 2022). Measuring crashes in the whole city is a good proxy for whether drivers decide to speed less in the vast majority of locations where they know cameras are not located and can be certain not to receive a speeding ticket for doing so. This is the real test for the behavioral change that tickets are designed for.

Table 4 presents the results of equation (1) when the dependent variable measures crashes. Neither a dummy for having a crash (column 1), nor the continuous variable for total of crashes (column 2), nor the dummy for crashes with at least one fatality [19] (column 3) are significant. Nor is it useful to separate the sample into cars and motorcycles. The effect on the matched sample is equally null in all columns, so I do not report it.

[18] I can also add that speeders are much more crash prone that non-speeders. Using all observations in the city of Bogotá during my sample period, speeders are twice more likely than non speeders to ever be involved in a crash.

[19] I also include here those crashes that required immediate medical attention because of imminent risk of death, Triage I, according to the Triage categories defined by the Colombian Minister of Health. 8% of the crashes that required medical attention are considered Triage I

Effect of receiving a ticket on crashes

	(1) Any crash dummy	(2) Total crashes continuous	(3) Deadly crash dummy	(4) Cars dummy	(5) Motorcycles dummy
Bias-corrected	-0.00143	-0.00210	0.00441	-0.00337	0.000703
	(0.0131)	(0.0166)	(0.00359)	(0.0100)	(0.0328)
Mean of not ticketed	0.0363	0.0420	0.00258	0.01317	0.08106
N	136533	136533	136533	89037	47496
N Left of cutoff	52468	52468	52468	35012	17456
N Right of cutoff	57605	57605	57605	35827	21778
BW left	4.5	4.5	4.5	4.5	4.5
BW right	9.5	9.5	9.5	9.5	9.5

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 4: Indirect deterrence. Full sample. Column 1 (Any crash) is a dummy that takes the value of 1 if the vehicle had at least one crash in the following months, and 0 otherwise. Column 2 (Total Crashes) is a continuous variable; the total of times a vehicle had a crash. Column 3 (Deadly Crash) is a dummy that takes the value of 1 if the vehicle had at least one crash with a fatality or Triage I attention in the following months, and 0 otherwise. Column 4 (Cars) is the same as Column 1 restricting the sample to cars. Column 5 (Motorcycles) does the same for motorcycles.

A possible objection to these results could be the fact that the estimation is underpowered. I should start by noting that the same number of observations produced a very credible result for speeding. I should also point out that the number of crashes in the sample is small, but not negligible. There are 7,162 (108 of them deadly) crashes in my estimation sample. These are around 6.2% of the total 116,088 total crashes that occurred in the period of study. In addition, 3.6% of vehicles in the control sample do have a crash, so it is a rare but not extremely rare phenomena. I would argue that the effect is too small to be detected, were it big, my estimation would detect it.

5 Relative price

The central question of this paper is whether the deterrent effect of fines depends on their relative price. Many countries have fines that are relative to income or wealth, mainly out of justice considerations. I am thinking on a different dimension, effectiveness for preventing unwanted behavior. The most common assumption, and the one used for implementing the program in Bogotá, is that the more expensive a fine the better for road safety[20]. As I said before, this might not be true in contexts with low law enforcement.

Table 5 reproduces the main estimations on Moderate Speeding and Any crash, separating vehicles that are below and above the median of the commercial price of the vehicle as proxy for the owner's wealth. The reader shousl recall that this is not an intra-sample comparison, I am locating each vehicle on my Matched Sample in the full distribution of vehicles that are registered in Bogotá. Coefficients show that for vehicles below the median the ticket has no direct deterrence effect. Surprisingly it also has an unintended causal effect for indirect deterrence, making crashes more likely. For vehicles above the median a speeding ticket has the desired effect both in direct and indirect deterrence, causally reducing both speeding and crashes.

This means that the desired effects of a speeding fine are concentrated on the most wealthy, while fines have undesired effects on the less wealthy. Note that the result on indirect deterrence (an increase in crashes for the less wealthy) is particularly worrisome since vehicles below the median have a higher average rate of crashes. Expensive fines not only make some drivers worse off, they worsen outcomes where deterrence is needed most.

[20] Juan Pablo Bocarejo, the Mobility Secretary at the time of the cameras implementation, has argued publicly and privately that cultural or educational interventions are not useful for the current challenge. Instead, he suggests that people should *feel pain in their pockets* in order to avoid risky behaviors(Bocarejo, 2022)

Effect of receiving a ticket on speeding and crashes. Matched sample

	(1) Below median Moderate Speeding	(2) Above median Moderate Speeding	(3) Below median Any crash	(4) Above median Any crash
Bias-corrected	-0.0231	-0.276***	0.0401**	-0.0385***
	(0.0546)	(0.0471)	(0.0181)	(0.0140)
Mean of not ticketed	0.218	0.268	0.0207	0.0154
N	24793	42442	24793	42442
Avg price in dollars	5379	19462	5379	19462
N left of cutoff	9837	16710	9837	16710
N right of cutoff	9937	16974	9937	16974
BW left	4.5	4.5	4.5	4.5
BW left	9.5	9.5	9.5	9.5

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 5: Direct and indirect deterrence. Matched sample. Columns 1 and 2 (speeding) are a dummy that takes the value of 1 if the vehicle had at least one Moderate speeding (between 50 and 60Km/h), and 0 otherwise. Columns 3 and 4 are a dummy that takes the value of 1 if the vehicle had at least one crash, and 0 otherwise. Non-smooth variables at cutoff used as covariates

Table A.7 reproduces the estimation below and above the median with the most costly results as dependent variables: serious speeding and deadly crashes. There is no change on the effect of direct deterrence only being present above the median, while there is a backfiring effect for both groups when talking about deadly crashes. But even if deadly crashes increase for both groups, both the magnitude of the coefficient and the base rate is much higher for vehicles below the median. Once again, the existing policy of a fixed high monetary cost for the ticket is increasing risks where deterrence is more needed.

It is necessary to verify whether this result holds when using alternative distributions of the relative price variable. A good option is to separate the relative price variable by quintiles. This comes at the cost of lower power, so results are naturally less robust than those with the median price. Table A.8 reproduces equation (1) with the dependent variable of a speeding dummy (column 1 of Table 3). One can see that the effect is concentrated on the higher quintiles of the distribution. The effect of a ticket is increasing on the quintile of vehicle price and significant above Quintile 2. This effect is robust to using the continuous variable of the total times a vehicle is captured under Minor Speeding, with the most negative coefficient also found in the highest quintile (not shown).Table A.9 does the same quintile estimation for the dummy of ever being in a crash. The desired effect is also increasing in the vehicle quintile but with less significant results. Quintile 5 is the only significant and negative result. But unlike the result on direct deterrence, there are at least some citizens that move in the undesired direction, those on Quintile 2 have a positive coefficient indicating an increase in crash involvement. Again, this groups are small, but even there the story is very consistent[21].

All the evidence shown in this section points in the same direction, relative price does matter and it suggests that tickets are more effective not when they are more, but less expensive. When one interacts this with base rates for each behavior, one can see that while speeding tickets seem well targeted at reducing speeding, they are poorly targeted at preventing injury and fatalities. A final obvious question for these results is how much they are caused by taxis and other public service vehicles[22]. I can exclude them from the Matched sample and all results (not reported) are in line with the story told in this chapter.

6 Robustness checks

In this section, I introduce some further evidence to support the credibility of the previous results. First, I present alternative estimations to show consistency. Second, I use an alternative method of estimating my results: an IV-judge type of estimation using data on the police agent in charge of verifying each speeding ticket.

6.1 Robustness to some decisions

There are many debatable decisions on my main estimation. The main one is whether to include vehicles that have more than one speeding (above the posted limit) during my sample period. Since the problem is really how to assign treatment, my choice is to consider vehicles that passed always below or above the real limit and use their average speed and first observation as treatment time. This estimation is called Repeated observations, one side on Figure 4. I find this method better to the convention of using the first time the vehicle exceeds as its treatment assignment. This because of the existing delay between speeding and the moment in which a vehicle is notified of the infraction, by which a vehicle can be treated and not know it for many days. Assignment based on first observations is called Repeated observations, first on Figure 4. Another relevant decision is whether to use vehicles that had a previous fine[23]. The final decision has to do with the period at which I study vehicles. I can study my sample on a longer period, from August to November, 2024.

[21] It is even consistent for serious crashes. As Table A.10 shows the effect of a ticket in serious crash might be positive in quintiles 1,3 and 5 but it is much higher for quintile 1 than quintiles 3 and 5

[22] Due to the licensing price, the high commercial price of a taxi might not be representative of the driver's wealth. Public service vehicles are around 15% of this Matched Sample

[23] As I argued before, I want to separate the effect of this fine from all previous fines. Including those vehicles that had a fine in the previous six months increases the sample to 145145 (from 136553), a 6.2% increase.

Figure 4 shows a comparison between the coefficient found in column 1 of Table 3 and the coefficient found with all these decisions that affect the sample. It is clear that, if anything, my main estimation errs by being too cautious, particularly when using vehicles that always respect or exceed the real limit.

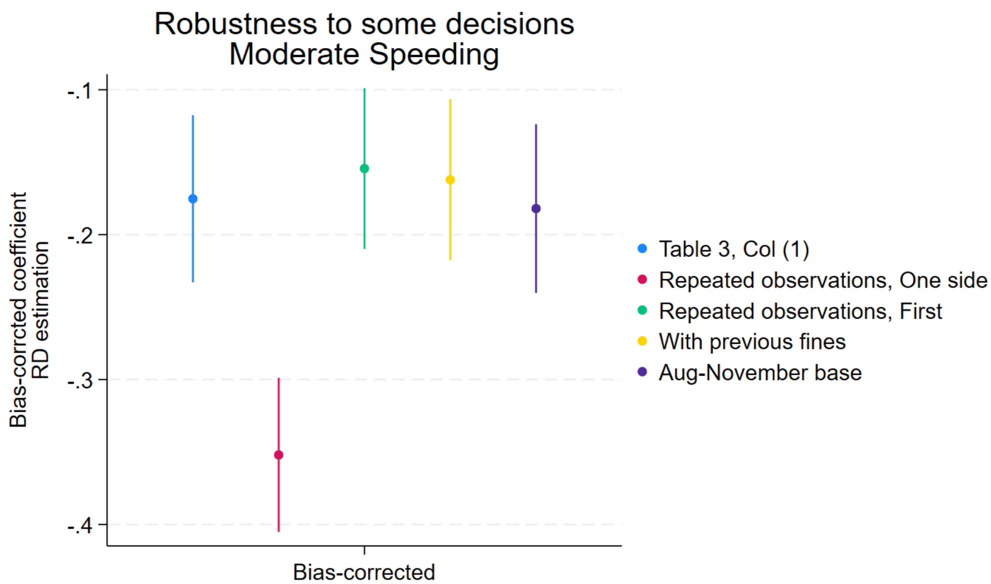

Figure 4: Table 3, Column 1 is shown as a reference. "Repeated observations, One Side" shows the coefficient using Vehicles that passed more than once and always above or below the cutoff and their first observation and average speed as treatment assignment. "Repeated observations, First" shows the coefficient using Vehicles that passed more than once using the first observations as treatment assignment. "With previous fines" uses the standard estimation, this time including vehicles that have had a ticket in the previous six months. "Aug-November" base makes the database bigger, by including all vehicles from August 16 to November 15, 2024.

Figure A.12 and Figure A.13 present the same calculations for other dependent variables: Major Speeding and Any crash. The coefficients are very stable to most of the decisions I make in the general case, but a longer period of study changes some of the results. In particular, the result for Major Speeding is less robust, and should be read with caution. I should point out that smoothness of the variables is less strong on bigger samples, the main reason I do not use the August to November database in the first place.

6.2 An IV-judge using police officer manual validation

According to Colombian law all speeding tickets must be subject to human verification. This has to do mainly with the fact that the technology for taking pictures of vehicles while speeding is not perfect, and some pictures are blurry. This task is done by trained police officers that must evaluate whether a specific picture should become a ticket. The pictures assigned to each policemen are random, and these police officers have no way to refuse their verification.

I have data for the identification of all pictures of vehicles that were circulating above the real speed limit (50 + ϵ km/h) during the months of October and November. I can also identify the agent in charge of reviewing each picture. As Figure 5 shows, there is considerable heterogeneity in the percentage of pictures that each agent converts into actual tickets, further enhanced by the different number reviewed by each agent. I should note that this is another sample, much more prone to speeding.

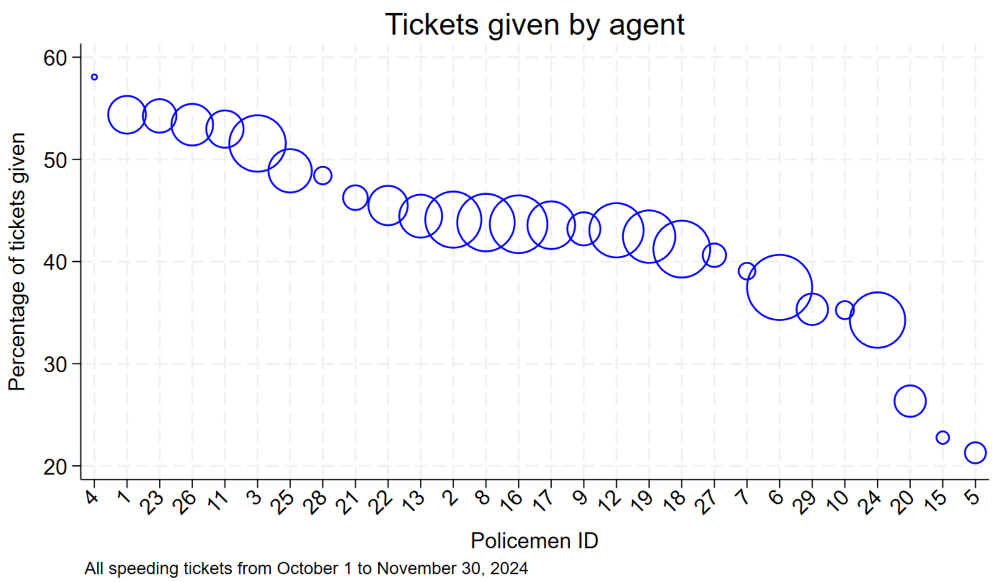

Figure 5: Policeman ID is a number to identify each police officer. The size of the circle is proportional to the number of pictures reviewed by each agent.

Figure 5 indicates that there is randomness associated to the Policemen in charge. For this to transform into a credible instrumental variable one would want to verify that these different rates are not related to any observable characteristic of the ticket, or the vehicle involved. Figure 6 shows that the strictness is stable for all Policemen across at least two meaningful variables, whether a vehicle is a motorcycle and those vehicles that drive under Major speeding, as previously defined.

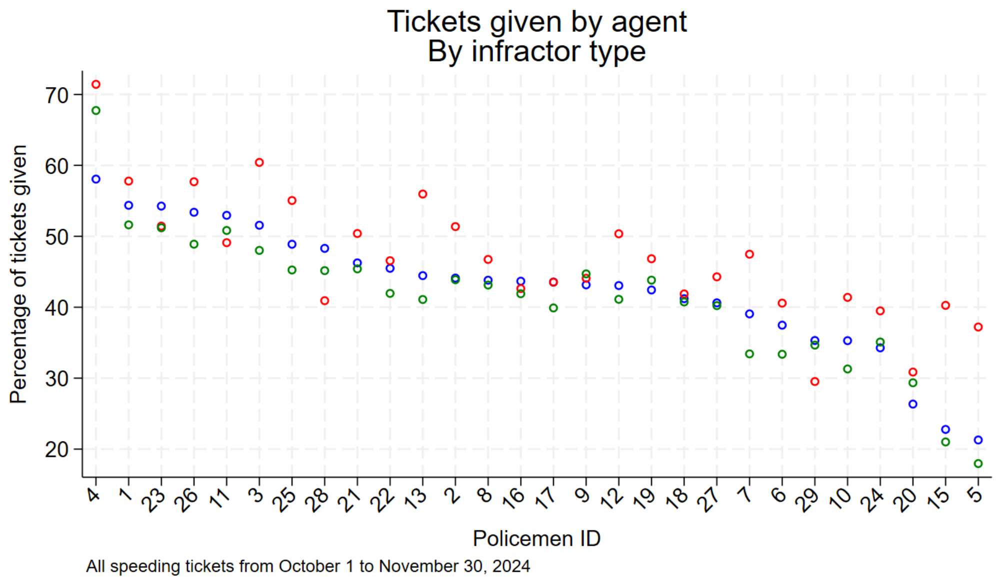

Figure 6: Policeman ID is a number to identify each police officer. Blue dots correspond to all pictures, red dots to pictures of motorcycles and green to vehicles under major speeding (40% of this subsample).

Having shown variation and some form of monotonicity I an use the random assignment of the Policemen in charge of reviewing each picture as an instrument of the ticket.In this manner I can capture the part of receiving a ticket that is due to the randomness of the assigned policemen. I also must emphasize that this is a different sample, consisting mostly of drivers that decide to exceed speeding limits by much[24]. Thus, in this estimation I am not finding a local effect, but the general effect on those drivers that really like to speed.

Table A.11 shows the result of an estimation with the same dependent variables as Table 3. As one can clearly see, the significance and sign of all the estimation is similar, but magnitudes are considerably smaller. While the effect of a speeding ticket was a 77% reduction in the likelihood of reoffending, in this case the effect is only 25%. This surely is caused by these drivers deciding to disobey speeding limits by a bigger amount than the other sample. These drivers like speeding more, so are less likely to slow down after being told to do so. In any case, there does not seem to be any backfiring of speeding tickets in terms of increasing probability of speeding for this alternative sample.

The results in Table A.11 are in line with the main finding of this paper of tickets having a causal effect on direct deterrence. The effect is in absolute terms of a smaller magnitude that can be attributed to selection, the love for speed of this sample[25]. One could use this estimation strategy to verify the second finding of this paper, no general indirect deterrent

[24] Note that these drivers face the same fine amount regardless of their observed speed

[25] One could also use this sample to verify a minor finding: effects being explained only by cars, not motorcycles. Table A.12 and Table A.14 do this. Note that in this case the effect is more precisely estimated for cars than motorcycles, but there is some for the latter. Once again, this does not cast doubt on previous findings but shows that major speeders are more similar between them, independent of the type of vehicle they drive

effect. As Table A.13 shows there is also a null effect on all dependent variables related to the chance of being involved in a crash. In this case the coefficients are positive in sign, but equally not significant.

The most interesting finding of this paper can also be verified by looking at the heterogeneous effect of the relative price of the fine. As Table 6 shows, on this Alternative sample the effect of a ticket on speeding behavior is also concentrated on the higher priced vehicles. It is noteworthy that there is no indirect deterrence on this sample of rspeed enthusiast[26]. Results (not shown) are similar for serious infractions. The result for minor speeding is robust to using quintiles, as it can be seen on Table A.15. Results by quintile are consistent but less trustworthy due to the loss of relevance of the instrument for smaller subsamples.

Effect of agent reviewing tickets on speeding and crashes. Alternative matched sample

	(1)	(2)	(3)	(4)
	Below median	Above median	Below median	Above median
	Minor	Minor	Any	Any
	Speeding	Speeding	crash	crash
Bias-corrected	0.00200	-0.0961***	-0.00623	0.00340
	(0.0356)	(0.0285)	(0.0116)	(0.00795)
Mean of not ticketed	0.169	0.217	0.0164	0.0153
N	17331	32146	17331	32146
Price in dollars	5379	19462	5379	19462
First stag Wald-F	16.19	31.44	16.17	31.44

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 6: Direct and indirect deterrence by relative price. Alternative matched sample.. Columns 1 and 2 (speeding) are a dummy that takes the value of 1 if the vehicle had at least one Moderate speeding (between 50 and 60Km/h), and 0 otherwise. Columns 3 and 4 are a dummy that takes the value of 1 if the vehicle had at least one crash, and 0 otherwise.

In summary, although the sample for the previous sections is different from this one, nor are the estimation strategies capturing the same causal effect, one can safely say that the results of this IV-judge strategy give more credibility to the causal effects found with the fuzzy regression discontinuity design. All evidence points in the same direction of tickets having a deterrent effect, and this effect being bigger for higher priced vehicles. This section also reinforces the idea that results are much more robust for direct than indirect deterrence, where there is little effect on this sample.

[26]This is consistent with what is shown in Table A.7, tickets being less effective for serious crashers. The sample of real speeders is more prone to serious crashes, for which tickets seem less effective.

7 Wake-up call or information? An exploration of mechanisms.

Speeding tickets are meant to prevent the most serious consequences of traffic crashes. As we have seen, in practice cameras seem very effective at reducing speeding itself but show little effect on reducing crashes. In this section I examine other behaviors to understand whether tickets induce broader behavioral changes and how local these effects are, both geographically and over time.

The first question I explore is whether the results are caused by a reduction in the frequency of driving. This is not good or bad in itself in terms of road safety, but it might have significant costs at the personal level if traveling creates welfare. I use the total number of times a vehicle passes under a Speed Camera after the speeding infraction as a dependent variable to measure this mechanism. Column 1 of Table 7 shows that speeding tickets do not affect road usage, suggesting that reduced driving is not driving the main results.

A second question I can address is how local the effect is. For this purpose I can use the location of each Camera. Note that due to the nature of traffic management in the city of Bogotá all cameras are not active during the six months following an speeding infraction. Instead of measuring each Camera I use the division of Bogotá into 20 *Localidades* (similar to boroughs). In Columns 2 and 3 of Table 7 I separate further speeding into close or far from the Camera at which a vehicle was captured speeding. I consider close any camera that is inside two levels of proximity, and far those occurring outside this range. Result (not shown) is robust to using just one level of proximity[27].

A third question, and the most interesting one in behavioral terms, is whether in general speeding fines are causing a change in driving behavior or only the need for being more aware of the locations and speed limit of speed cameras. This question can be addressed by looking at other behaviors on the road that are also punished by road authorities. This is complicated by the fact that speeding is by far the most common infraction in the period of study, even if this period coincides with a city wide campaign against parking in prohibited spaces. Column 4 and 5 of Table 7 explore both the extensive and the intensive margin of this dimension. It is reassuring that tickets also have a causal effect on other behaviors. This suggests a broader behavioral change, likely driven by drivers realizing that traffic laws are actively enforced in the city.

[27]There is also a final question about persistence of the effect of receiving a ticket in speeding.Table A.16 shows that the effect is not particularly concentrated in the first months, but seems to fade out after six months. The persistence of the effect after five months is also reassuring.

Behavioral mechanisms Full sample

	(1) Future observations	(2) Moderate Speeding nearby	(3) Moderate Speeding far	(4) Other tickets dummy	(5) Other tickets continuous
Bias-corrected	2.359	-0.105***	-0.0691***	-0.0867***	-0.125***
	(1.963)	(0.0267)	(0.0156)	(0.0228)	(0.0387)
Mean of not ticketed	21.013	0.176	0.0504	0.117	0.160
N	136533	136533	136533	136533	136533
N Left of cutoff	52468	52468	52468	52468	52468
N Right of cutoff	57605	57605	57605	57605	57605
BW left	4.500	4.500	4.500	4.500	4.500
BW right	9.500	9.500	9.500	9.500	9.500

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 7: Behavioral change. Full Sample.. Column 1 (Future observations) is a continuous variable that measures the total number of observations after the first speeding of a vehicle. Column 2 (Moderate speeding nearby) is a dummy that takes the value of 1 if the vehicle had at least one Moderate speeding (between 50 and 60Km/h) inside a range of two proximate Localidades and 0 otherwise. Column 3 (Moderate speeding far) is a dummy that takes the value of 1 if the vehicle had at least one Moderate speeding (between 50 and 60Km/h) outside a range of two proximate Localidades and 0 otherwise. Column 4 (Other tickets dummy) is a dummy variable that takes a value of 1 if the vehicle ever had another ticket for any cause after the first speeding instance, and 0 otherwise. Column 5 (Other tickets continuous) is a continuous variable that measures the total number of ticket for any cause after the first speeding instance.

Table A.17 examines how the results seen in columns 4 and 5 depend on relative prices. The results are consistent with previous findings, since even if there is a negative coefficient for both behaviors below and above the median price, the effects are slightly higher in magnitude and higher in significance for vehicles above the median price. Those vehicles also have lower rates of traffic infractions, particularly on the continuous dimension. Of course, the global effect of speeding tickets must consider a more general picture of behavior and sense of authority than just its effects on speeding.

8 Discussion and possible policy recommendations

Gary Becker wrote that "Fines are preferable to imprisonment and other types of punishment because they are more efficient" (Becker, 1992). In his analysis "the social cost of fines is about zero", and the optimal amount of a fine "depend[s] only on the marginal harm and cost and not at all on the economic positions of offenders"(Becker, 1974). This last sentence seems to be behind the decisions of many policy makers in a wide range of policy areas. This paper tested this intuition in the specific context of states with limited enforcement capacity. Becker's logic might need to be reconsidered in contexts where the State can impose a fine but it can do little to enforce it, making the perceived fairness of the fine more relevant for deterrence.

This paper shows that fines can be socially inefficient in a very particular way. I have shown that fines seem to be effective at direct deterrence: they reduce the specific behavior in locations where citizens know the law is being enforced. Fines seem less capable of generating indirect deterrence: they do not reduce the specific behavior in locations where citizens know there is no enforcement of the law. This general finding has some nuance when analyzing the fines in relation to personal wealth, using the relative price of the fixed fine amount. The direct deterrence effect is concentrated on higher quintiles of the wealth distribution, whereas the null effect of indirect deterrence is the combination of a deterrent effect at higher wealth levels and a backfire of fines at lower wealth levels. All of this is consistent with fines being a wake-up call that citizens receive and only accept if they perceive the fine to be fair or proportional, and can be extended (as I have tried in this paragraph) to many more behaviors than speeding when driving a car or motorcycle. In the context I study, relative price clearly matters. Expensive fines may be counterproductive, as lower fines could both reduce risky driving behavior and increase government revenue[28].

In general, the causal interpretation is stronger for the estimates of direct and indirect deterrence than for the heterogeneous effects related to the relative price of the fine. One would need additional evidence to argue that it is the relative price that is causing the inefficiency, since there might be other conditions correlated with relative price (education, driving skills, trust in government) that can be behind the result. Disentangling this is an interesting question for future work, but is not necessary for suggesting actionable corrections to existing policies. Even if the mechanism operates through factors correlated with relative price, the pattern is sufficiently clear that it calls for a reconsideration of current policy tools that ignore the cost of fines that are too expensive. In my opinion the discussion if relative price is acting in itself or as a proxy for something else, changes little on the counterintuitive findings I have shown.

The inefficiency of expensive fines for changing behavior can have additional negative societal consequences. As I explained before, in the institutional setting of Bogotá the only real effect of not paying a speeding fine is the exclusion from the formal market, both in the market for used vehicles and the formal labor market due to the risk of getting a wage garnishment for unpaid fines. Fixed monetary fines might turn into an additional barrier to formality for the poorest members of society, and it is natural to think other topics (taxes, labor law) where this might also happen in States with low enforcement capacity.

All this points to a clear policy recommendation: wealth-based fines. Speeding tickets might have a better effect if they were adjusted, to a lower value, for the less wealthy drivers. If one also considers the well-known fact that higher-income drivers have higher payment rates,

[28] This last idea can be supported with existing data on ticket payment, shown in Figure A.14

the total result of lower fines will be welfare-improving. At the same time, lower fines might reduce the barrier to formality that existing fines can be for the poor. Some might argue that if the State does not have the capacity to enforce traffic law it might also not have the capacity to measure the wealth of drivers. In that case the recommendation is simpler: cheaper fines[29].

I must also be careful about the interpretation of these findings in terms of law enforcement more broadly. The evidence shown does not imply that enforcement does not work. What this paper has shown is that it might not work through a very specific channel: the behavioral response of marginal speeders just above the unknown enforcement threshold. Most evaluations seem to show that in the aggregate speed control[30] and speed cameras work, and this might be easily explained by their effect on regular speed-limit obeying drivers both internationally (Ang et al., 2020) and specifically in Bogotá (Ferro, 2024). Additional policies may be needed for the subset of drivers who knowingly exceed speed limits despite understanding the level of enforcement. For the vast majority of drivers knowing that there is police enforcement of speeding laws is sufficient to positively influence their behavior. The optimal solution to too expensive fines is not the elimination of fines, but a less conventional set of policies.

Although not the main focus of this paper, the fact that the effects of enforcement are concentrated among automobiles and absent for motorcycles has important policy implications. This might mean that speeding tickets are not the right tool for the changing face of the public health challenge of road fatalities that some cities are facing, since motorcycles drivers are a growing percentage of road fatalities both as victims and perpetrators. Other interventions are needed if a city like Bogotá (and other in middle income countries) want to face the public health challenge of motorcycles (in all its dimensions: (Martínez-González et al., 2022). Both non-automated control and behavioral interventions are urgently needed.

[29]Of course, fines can also be too cheap. In that sense cheaper fines would backfire to the wealthiest drivers, and the optimal amount is not zero

[30]A good example of this is (DeAngelo & Hansen, 2014)

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Appendix

table0

A Figures

	Observations	
	Frequency	Percent
1	157245	87.5
2	17475	9.7
3	3384	1.9
4	887	0.5
5	348	0.2
6	133	0.1
7	55	0.0
8	32	0.0
9	19	0.0
10	12	0.0
11	10	0.0
12	2	0.0
13	7	0.0
15	2	0.0
26	1	0.0
Total	179612	100.0

Table A.1: Times each plate passed exceeding the limit under the cameras

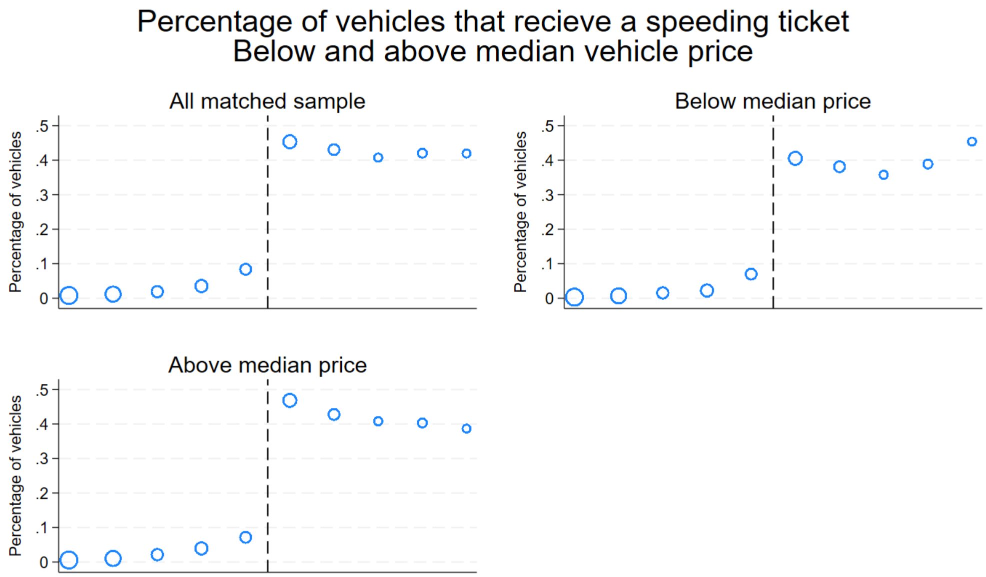

Figure A.1: Speed above limit is constructed as: vehicle observed speed-speed limit. Values are not displayed in the X axis by agreement with the Mobility Office of Bogotá

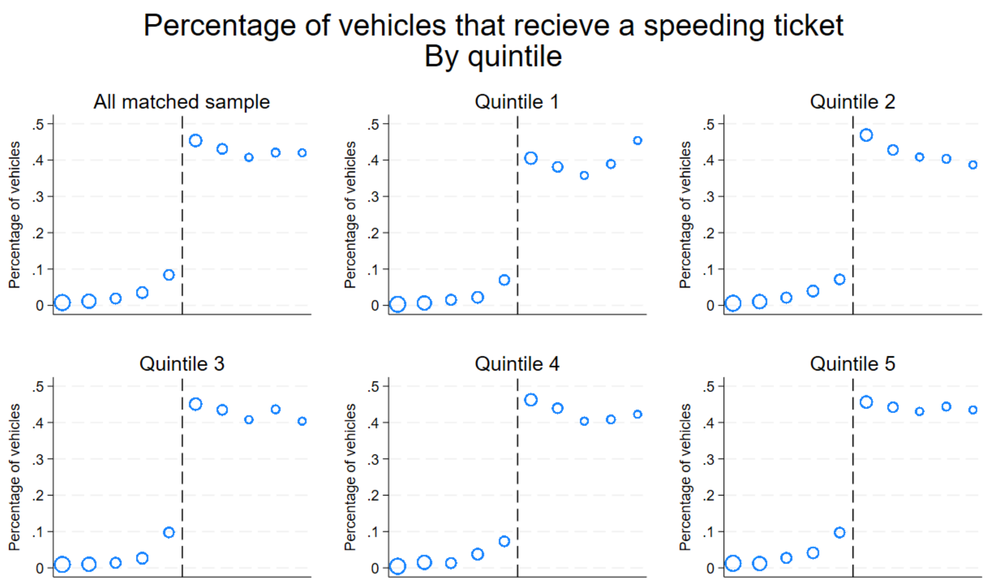

Figure A.2: Speed above limit is constructed as: vehicle observed speed-speed limit. Values are not displayed in the X axis by agreement with the Mobility Office of Bogotá

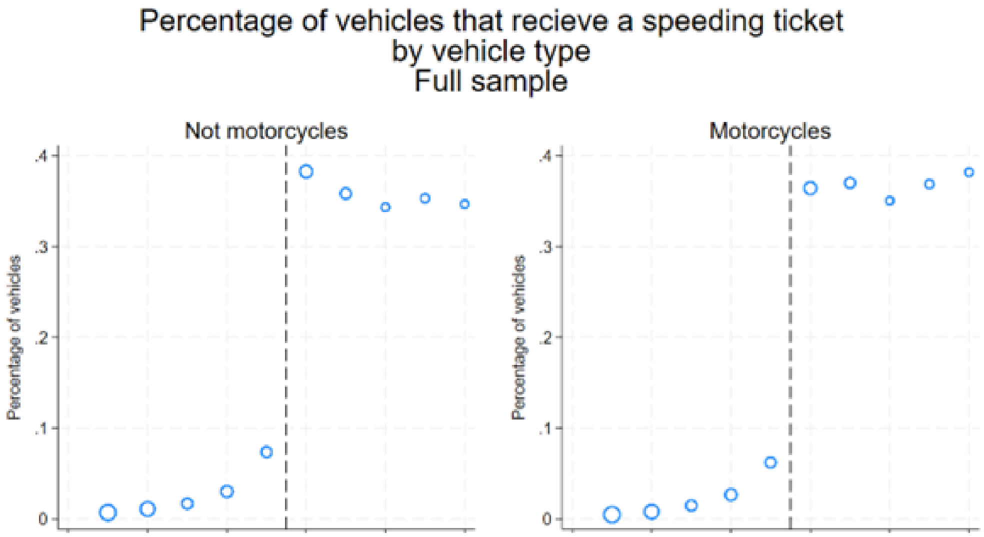

Figure A.3: Existence of the discontinuity by type of vehicle. Full Sample.

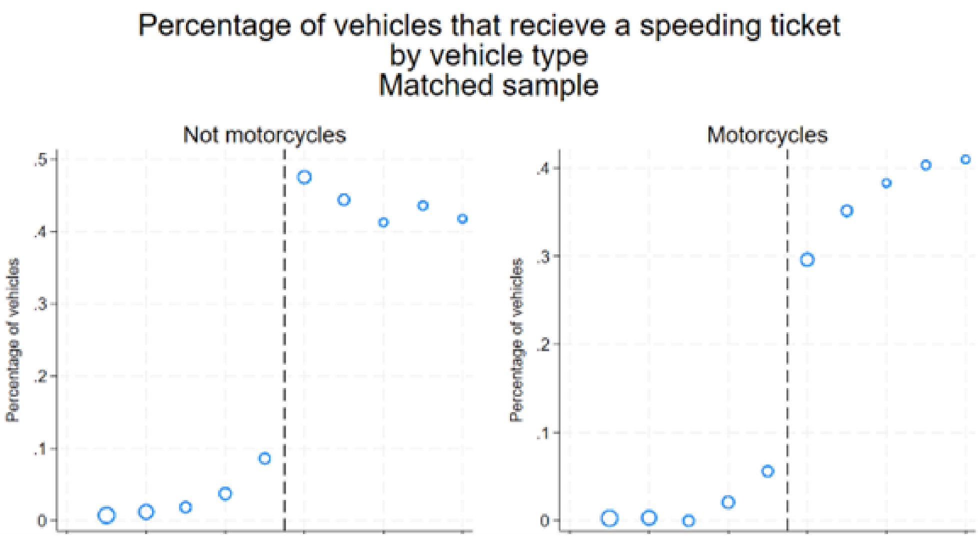

Figure A.4: Existence of the discontinuity by type of vehicle. Matched Sample.

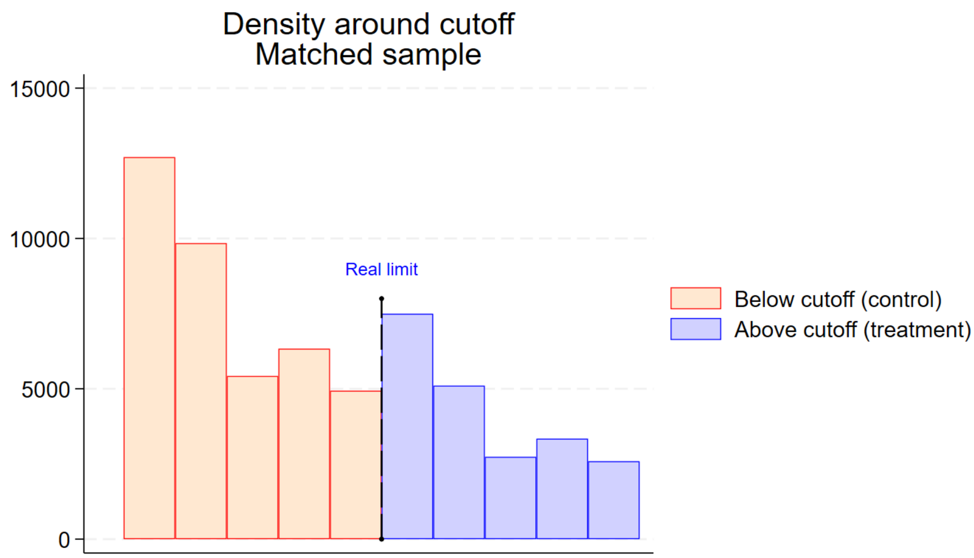

Figure A.5: Frequency by condition in Matched sample. All vehicles in this graph exceed the posted limit. Below cutoff (control) are vehicles observed below the real speed limit, and Above cutoff (treatment) are vehicles observed above the real speed limit. Values are not displayed in the X axis by agreement with the Mobility Office of Bogotá

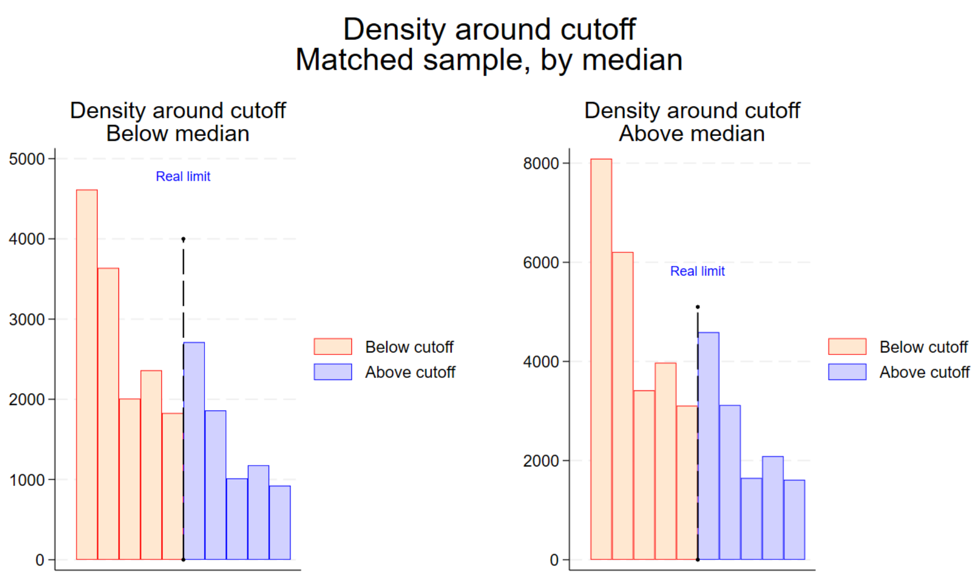

Figure A.6: Frequency by condition, and relative price. All vehicles in this graph exceed the posted limit. Below cutoff (control) are vehicles observed below the real speed limit, and Above cutoff (treatment) are vehicles observed above the real speed limit. Values are not displayed in the X axis by agreement with the Mobility Office of Bogotá

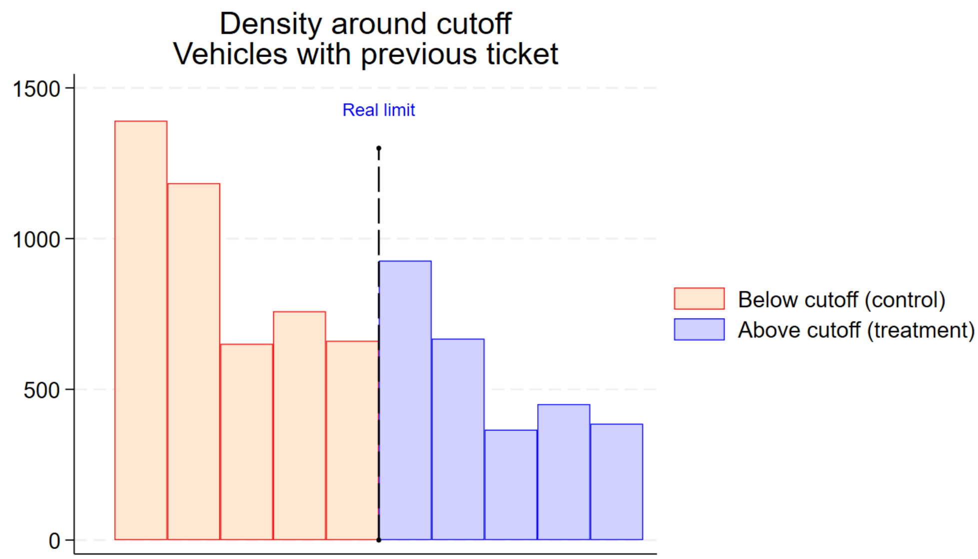

Figure A.7: Frequency by condition in Full sample. All vehicles in this graph exceed the posted limit, but these vehicles are not used in the main estimation. Below cutoff (control) are vehicles observed below the real speed limit, and Above cutoff (treatment) are vehicles observed above the real speed limit. Values are not displayed in the X axis by agreement with the Mobility Office of Bogotá.

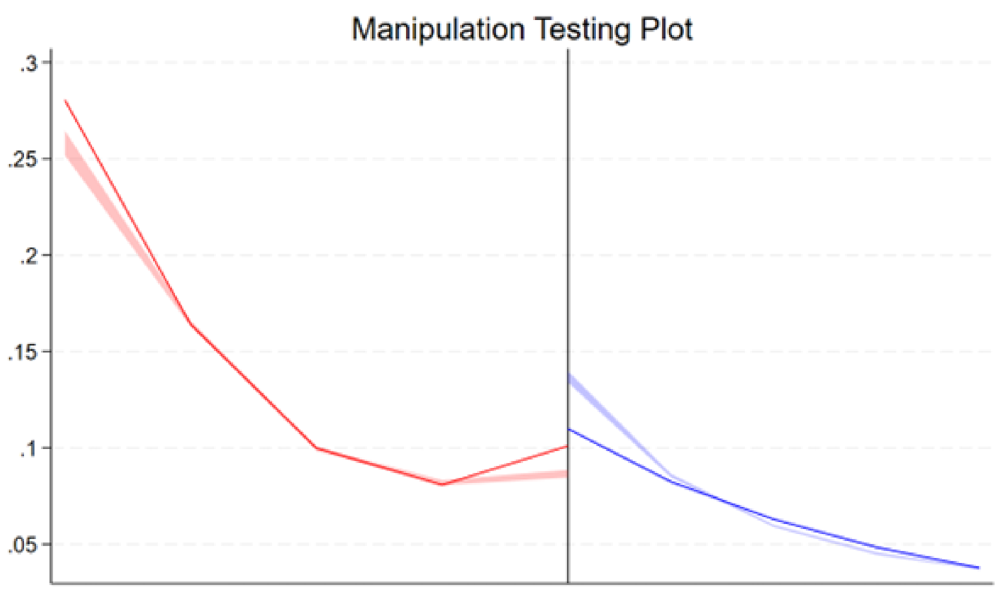

Figure A.8: Manipulation test based on discontinuity. Sample with polynomial of degree 3.

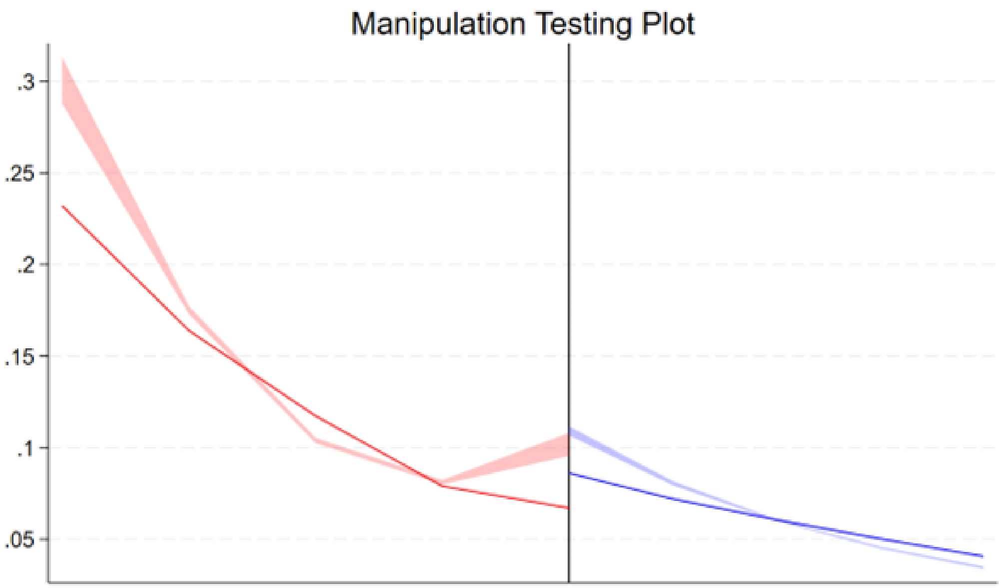

Figure A.9: Manipulation test based on discontinuity. Sample with polynomial of degree 2. Matched Sample.

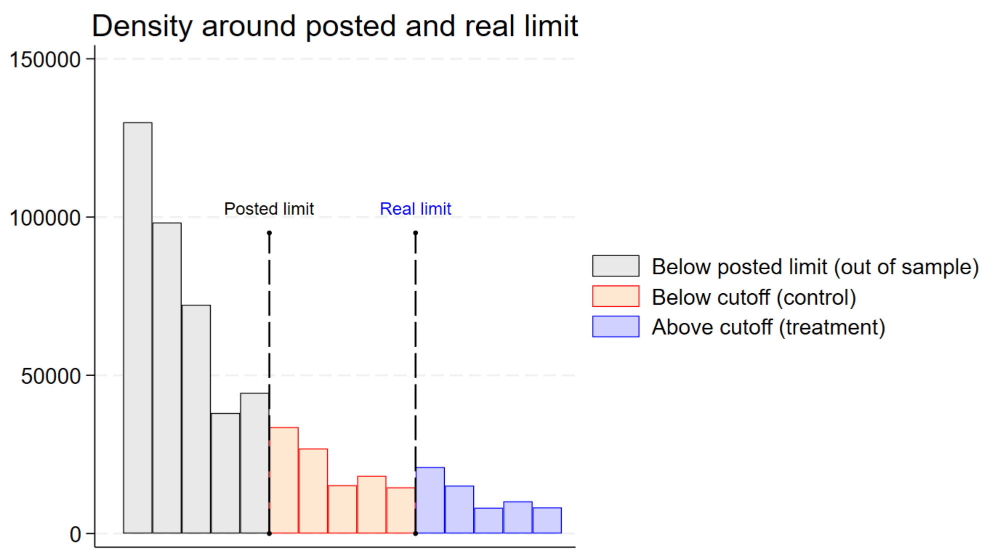

Figure A.10: Discontinuity around posted and real limit. Below posted limit (out of sample) are vehicles that were not observed speeding, thus are not included in my sample to avoid selection. Below cutoff (control) are vehicles observed below the real speed limit, and Above cutoff (treatment) are vehicles observed above the real speed limit. Full sample. Values are not displayed in the X axis by agreement with the Mobility Office of Bogotá

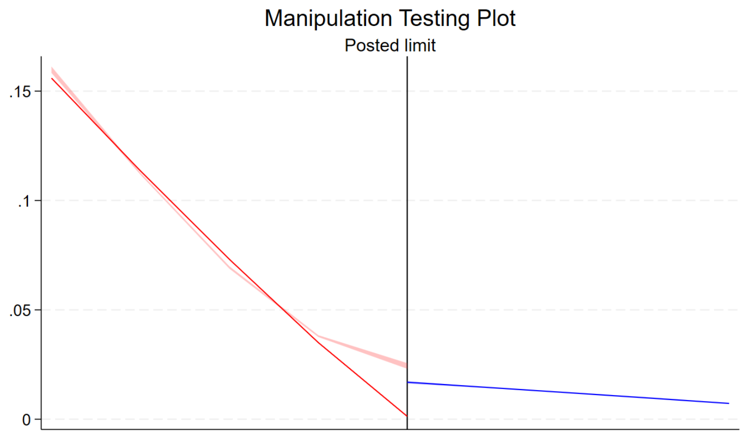

Figure A.11: Manipulation test based on discontinuity at the posted limit. Sample with polynomial of degree 2.

Smoothness of observables Matched sample. Below median price

	(1) Hour of day	(2) Rush hour dummy	(3) Day of week	(4) Weekend dummy	(5) Motorcycle dummy	(6) Observations per day before	(7) Price of vehicle	(8) Service type	(9) SUV dummy
Bias-corrected	-0.793*	-0.136**	0.450	0.0212	-0.125**	-0.477	0.745	-0.0936***	0.0348
	(0.469)	(0.0628)	(0.288)	(0.0657)	(0.0504)	(0.448)	(1.187)	(0.0358)	(0.0383)
Mean of not ticketed	11.85	0.336	4.341	0.406	0.175	3.845	21.46	0.0835	0.0957
N	24793	24793	24793	24793	24793	24793	24793	24793	24793
N left of cutoff	9837	9837	9837	9837	9837	9837	9837	9837	9837
N right of cutoff	9937	9937	9937	9937	9937	9937	9937	9937	9937
BW left	4.5	4.5	4.5	4.5	4.5	4.5	4.5	4.5	4.5
BW right	9.5	9.5	9.5	9.5	9.5	9.5	9.5	9.5	9.5

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A.2: Smoothness on observables. Below median price. Hour is a continuous variable with the time in which the vehicle passed under the camera. Rush hour is a dummy that takes the value of 1 if the vehicle passed under the camera at rush hours (6,7 am or 4,5,6 pm) and 0 otherwise. Day is a continues variable that goes from 1 (Monday) to 7 (Sunday). Weekend is a dummy that takes the value of 1 for Saturday and Sunday, and 0 otherwise. Motorcycle is a dummy that takes the value of 1 for motorcycles, and 0 otherwise. Observations per day before is the total times each vehicle passes under a Camera divided by the number of days before the speed excess. Type of service takes the value of 1 if a vehicle is used for public service (mainly taxis and similar services) and 0 otherwise. SUV dummy takes the value of 1 if vehicle is an SUV and 0 otherwise.

Smoothness of observables Matched sample. Above median price

	(1) Hour of day	(2) Rush hour dummy	(3) Day of week	(4) Weekend dummy	(5) Motorcycle dummy	(6) Observations per day before	(7) Price of vehicle	(8) Service type	(9) SUV dummy
Bias-corrected	0.0318	-0.0618	0.0445	-0.0429	-0.0208**	-0.0779	-11.49*	0.0269	-0.116**
	(0.364)	(0.0493)	(0.228)	(0.0516)	(0.00986)	(0.517)	(6.129)	(0.0414)	(0.0526)
Mean of not ticketed	12.01	0.336	4.307	0.399	0.00820	5.281	77.80	0.194	0.465
N	42442	42442	42442	42442	42442	42442	42442	42442	42442
N left of cutoff	16710	16710	16710	16710	16710	16710	16710	16710	16710
N right of cutoff	16974	16974	16974	16974	16974	16974	16974	16974	16974
BW left	4.5	4.5	4.5	4.5	4.5	4.5	4.5	4.5	4.5
BW right	9.5	9.5	9.5	9.5	9.5	9.5	9.5	9.5	9.5

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A.3: Smoothness on observables. Above median price. Hour is a continuous variable with the time in which the vehicle passed under the camera. Rush hour is a dummy that takes the value of 1 if the vehicle passed under the camera at rush hours (6,7 am or 4,5,6 pm) and 0 otherwise. Day is a continues variable that goes from 1 (Monday) to 7 (Sunday). Weekend is a dummy that takes the value of 1 for Saturday and Sunday, and 0 otherwise. Motorcycle is a dummy thah takes the value of 1 for motorcycles, and 0 otherwise. Observations per day before is the total times each vehicle passes under a Camera divided by the number of days before the speed excess. Type of service takes the value of 1 if a vehicle is used for public service (mainly taxis and similar services) and 0 otherwise. SUV dummy takes the value of 1 if vehicle is an SUV and 0 otherwise.

Effect of receiving a ticket. Not motorcyles

	(1) Moderate Speeding dummy	(2) Moderate Speeding continuous	(3) Major Speeding dummy	(4) Major Speeding continuous
Bias-corrected	-0.245***	-0.382***	-0.0814***	-0.0923***
	(0.0381)	(0.0848)	(0.0204)	(0.0314)
Mean of not ticketed	0.250	0.418	0.0571	0.0736
N	89037	89037	89037	89037
N Left of cutoff	35012	35012	35012	35012
N Right of cutoff	35827	35827	35827	35827
BW left	4.5	4.5	4.5	4.5
BW right	9.5	9.5	9.5	9.5

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A.4: Direct deterrence. Not motorcycles, full sample. Column 1 is a dummy that takes the value of 1 if the vehicle had at least one Moderate speeding (between 50 and 60Km/h), and 0 otherwise. Column 2 is a continuous variable, the total of times a vehicle was captured in Moderate speeding. Column 3 is a dummy that takes the value of 1 if the vehicle had at least one Major speeding (above 60 km/h), and 0 otherwise. Column 4 is a continuous variable, the total of times a vehicle was captured in Major Speeding

Effect of receiving a ticket. Motorcyles

	(1) Moderate Speeding dummy	(2) Moderate Speeding continuous	(3) Major Speeding dummy	(4) Major Speeding continuous
Bias-corrected	-0.0424	-0.123	-0.0341	-0.0407
	(0.0456)	(0.0920)	(0.0274)	(0.0412)
Mean of not ticketed	0.177	0.274	0.0524	0.0678
N	47496	47496	47496	47496
N Left of cutoff	17456	17456	17456	17456
N Right of cutoff	21778	21778	21778	21778
BW left	4.5	4.5	4.5	4.5
BW right	9.5	9.5	9.5	9.5

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A.5: Direct deterrence. Motorcycles, full sample. Column 1 is a dummy that takes the value of 1 if the vehicle had at least one Moderate speeding (between 50 and 60Km/h), and 0 otherwise. Column 2 is a continuous variable, the total of times a vehicle was captured in Moderate speeding. Column 3 is a dummy that takes the value of 1 if the vehicle had at least one Major speeding (above 60 km/h), and 0 otherwise. Column 4 is a continuous variable, the total of times a vehicle was captured in Major Speeding

Effect of receiving a ticket. Matched Sample

	(1) Moderate Speeding dummy	(2) Moderate Speeding continuous	(3) Major Speeding dummy	(4) Major Speeding continuous
Bias-corrected	-0.186***	-0.254***	-0.0531***	-0.0554**
	(0.0361)	(0.0729)	(0.0191)	(0.0265)
Mean of not ticketed	0.228	0.337	0.0565	0.0687
N	67235	67235	67235	67235
N Left of cutoff	26547	26547	26547	26547
N Right of cutoff	26911	26911	26911	26911
BW left	4.5	4.5	4.5	4.5
BW right	9.5	9.5	9.5	9.5

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A.6: Direct deterrence. Matched sample. Column 1 is a dummy that takes the value of 1 if the vehicle had at least one Moderate speeding (between 50 and 60Km/h), and 0 otherwise. Column 2 is a continuous variable, the total of times a vehicle was captured in Moderate speeding. Column 3 is a dummy that takes the value of 1 if the vehicle had at least one Major speeding (above 60 km/h), and 0 otherwise. Column 4 is a continuous variable, the total of times a vehicle was captured in Major Speeding

Effect of receiving a ticket

	(1)	(2)	(3)	(4)
	Below median	Above median	Below median	Above median
	Major	Major	Any deadly	Any deadly
	Speeding	Speeding	crash	crash
Bias-corrected	-0.0133	-0.0761***	0.0204***	0.00767***
	(0.0282)	(0.0255)	(0.00524)	(0.00248)
Mean of not ticketed	0.0481	0.0606	0.00161	0.00111
N	24793	42442	24793	42442
Avg price in dollars	5379	19462	5379	19462
N left of cutoff	9837	16710	9837	16710
N right of cutoff	9937	16974	9937	16974
BW left	4.5	4.5	4.5	4.5
BW left	9.5	9.5	9.5	9.5

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A.7: Direct and indirect deterrence. Matched sample. Columns 1 and 2 are a dummy that takes the value of 1 if the vehicle had at least one Major speeding (above 60 km/h), and 0 otherwise. Columns 3 and 4 are a dummy that takes the value of 1 if the vehicle had at least one crash with a fatality or Triage I attention in the following months, and 0 otherwise. Non-smooth variables at cutoff used as covariates

Effect of receiving a ticket					
	(1)	(2)	(3)	(4)	(5)
	Quintile 1 Moderate Speeding	Quintile 2 Moderate Speeding	Quintile 3 Moderate Speeding	Quintile 4 Moderate Speeding	Quintile 5 Moderate Speeding
Bias-corrected	-0.0649	-0.0815	-0.195**	-0.213***	-0.213**
	(0.116)	(0.0742)	(0.0879)	(0.0670)	(0.0750)
Mean of not ticketed	0.170	0.240	0.244	0.263	0.278
N	7065	11848	12824	17225	18273
Price in dollars	2295	5999	8553	12845	29482
N Left of cutoff	2692	4794	5141	6854	7067
N Rift of cutoff	3046	4603	5010	6820	7430
BW left	4.5	4.5	4.5	4.5	4.5
BW right	9.5	9.5	9.5	9.5	9.5

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A.8: Direct deterrence by quintile of vehicle price. Matched sample Dependent variable is a dummy that takes the value of 1 if the vehicle had at least one speeding between 50 and 60km/h, and 0 otherwise. For each quintile, variables that are not smooth at cutoff are used as covariates

Effect of receiving a ticket

	(1)	(2)	(3)	(4)	(5)
	Quintile 1	Quintile 2	Quintile 3	Quintile 4	Quintile 5
	Any crash	Any crash	Any crash	Any crash	Any crash
Bias-corrected	0.0307	0.0503***	0.0335	-0.0247	-0.0661***
	(0.0612)	(0.0164)	(0.0223)	(0.0193)	(0.0239)
Mean of not ticketed	0.0443	0.0112	0.0113	0.0142	0.0177
N	7065	11848	12824	17225	18273
Price in dollars	2295	5999	8553	12845	29482
N Left of cutoff	2692	4794	5141	6854	7067
N Rift of cutoff	3046	4603	5010	6820	7430
BW left	4.5	4.5	4.5	4.5	4.5
BW right	9.5	9.5	9.5	9.5	9.5

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A.9: Indirect deterrence by quintile of vehicle price. Matched sample . Dependent variable is a dummy that takes the value of 1 if the vehicle had at least one crash, and 0 otherwise. For each quintile, variables that are not smooth at cutoff are used as covariates.

Effect of receiving a ticket

	(1) Quintile 1 Any deadly crash	(2) Quintile 2 Any deadly crash	(3) Quintile 3 Any deadly crash	(4) Quintile 4 Any deadly crash	(5) Quintile 5 Any deadly crash
Bias-corrected	0.0556***	0.00557	0.0216***	-0.00271	0.0141***
	(0.0172)	(0.00559)	(0.00423)	(0.00171)	(0.00527)
Mean of not ticketed	0.00326	0.000928	0.00117	0.000813	0.00131
N	7065	11849	12823	17226	18272
Price in dollars	2295	5999	8553	12845	29482
N Left of cutoff	2692	4794	5141	6854	7067
N Rift of cutoff	3046	4603	5010	6820	7430
BW left	4.500	4.500	4.500	4.500	4.500
BW right	9.500	9.500	9.500	9.500	9.500

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A.10: Indirect deterrence by quintile of vehicle price. Matched sample. Dependent variable is a dummy that takes the value of 1 if the vehicle had at least one deadly crash, and 0 otherwise. For each quintile, variables that are not smooth at cutoff are used as covariates.

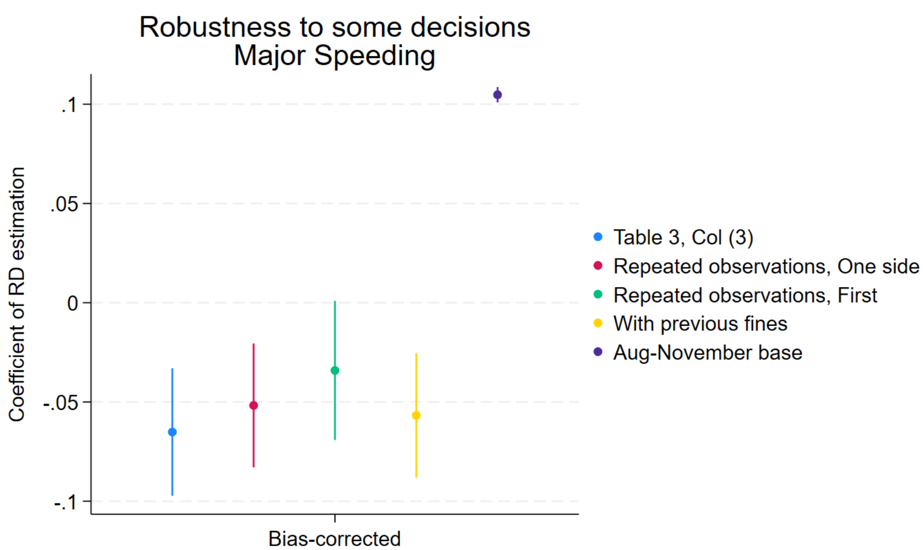

Figure A.12: Table 3, Column 3 is shown as a reference. “Repeated observations, One Side” shows the coefficient using Vehicles that passed more than once and always above or below the cutoff and their first observation and average speed as treatment assignment. “Repeated observations, First” shows the coefficient using Vehicles that passed more than once using the first observations as treatment assignment. “With previous fines” uses the standard estimation, this time including vehicles that have had a ticket in the previous six months. “Aug-November base" makes the database bigger, by including all vehicles from August 16 to November 15, 2024.

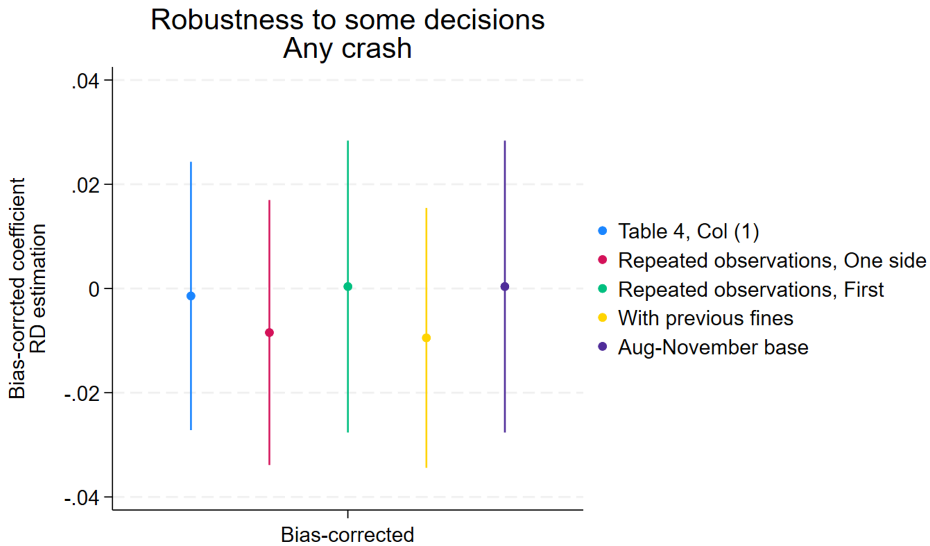

Figure A.13: Table 4, Column 1 is shown as a reference. "Repeated observations, One Side" shows the coefficient using Vehicles that passed more than once and always above or below the cutoff and their first observation and average speed as treatment assignment. "Repeated observations, First" shows the coefficient using Vehicles that passed more than once using the first observations as treatment assignment. "With previous fines" uses the standard estimation, this time including vehicles that have had a ticket in the previous six months. "Aug-November base" makes the database bigger, by including all vehicles from August 16 to November 15, 2024.

Effect of agent reviewing ticket

	(1) Moderate Speeding dummy	(2) Moderate Speeding continuous	(3) Major Speeding dummy	(4) Major Speeding continuous
Ticket	-0.0416***	-0.0840***	-0.0201**	-0.0257**
	(0.0158)	(0.0279)	(0.00897)	(0.0127)
Mean of not ticketed	0.164	0.241	0.0470	0.0604
N	132668	132668	132668	132668
First stage Wald-F	82.56	82.56	82.56	82.56

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A.11: Direct deterrence. Alternative sample.. Column 1 is a dummy that takes the value of 1 if the vehicle had at least one Moderate speeding (between 50 and 60Km/h), and 0 otherwise. Column 2 is a continuous variable, the total of times a vehicle was captured in Moderate speeding. Column 3 is a dummy that takes the value of 1 if the vehicle had at least one Major speeding (above 60 km/h), and 0 otherwise. Column 4 is a continuous variable, the total of times a vehicle was captured in Major Speeding

Effect of agent reviewing ticket. Not motorcycles

	(1) Moderate Speeding dummy	(2) Moderate Speeding continuous	(3) Major Speeding dummy	(4) Major Speeding continuous
Ticket	-0.0446**	-0.103***	-0.0159	-0.0236*
	(0.0185)	(0.0328)	(0.00994)	(0.0138)
Mean of not ticketed	0.179	0.265	0.0451	0.0572
N	94536	94536	94536	94536
First stage Wald-F	65.40	65.40	65.40	65.40

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A.12: Direct deterrence. Alternative sample.. Column 1 is a dummy that takes the value of 1 if the vehicle had at least one Moderate speeding (between 50 and 60Km/h), and 0 otherwise. Column 2 is a continuous variable, the total of times a vehicle was captured in Moderate speeding. Column 3 is a dummy that takes the value of 1 if the vehicle had at least one Major speeding (above 60 km/h), and 0 otherwise. Column 4 is a continuous variable, the total of times a vehicle was captured in Major Speeding

Effect of agent reviewing ticket

	(1) Any crash dummy	(2) Total crashes continuous	(3) Deadly crash dummy	(4) Cars dummy	(5) Motorcycles dummy
Ticket	0.00484	0.00837	-0.0000405	0.00118	0.00267
	(0.00664)	(0.00816)	(0.00182)	(0.00463)	(0.0182)
Mean of not ticketed	0.0246	0.0280	0.00194	0.0103	0.0630
N	132668	132668	132668	94536	38132
First stage Wald-F	82.56	82.56	82.56	65.40	28.93

t statistics in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A.13: Indirect deterrence. Alternative sample.. Column 1 (Any crash) is a dummy that takes the value of 1 if the vehicle had at least one crash in the following months, and 0 otherwise. Column 2 (Total Crashes) is a continuous variable; the total of times a vehicle had a crash. Column 3 (Deadly Crash) is a dummy that takes the value of 1 if the vehicle had at least one crash with a fatality or Triage I attention in the following months, and 0 otherwise. Column 4 (Cars) is the same as Column 1 restricting the sample to cars. Column 5 (Motorcycles) does the same for motorcycles. This table is comparable to Table 4.

Effect of agent reviewing ticket. Motorcycles

	(1) Moderate Speeding dummy	(2) Moderate Speeding continuous	(3) Major Speeding dummy	(4) Major Speeding continuous
Ticket	-0.0235	-0.0109	-0.0273*	-0.0280
	(0.0238)	(0.0393)	(0.0159)	(0.0229)
Mean of not ticketed	0.123	0.176	0.0519	0.0691
N	38132	38132	38132	38132
First stage Wald-F	28.93	28.93	28.93	28.93

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A.14: Direct deterrence. Alternative sample.. Column 1 is a dummy that takes the value of 1 if the vehicle had at least one Moderate speeding (between 50 and 60Km/h), and 0 otherwise. Column 2 is a continuous variable, the total of times a vehicle was captured in Moderate speeding. Column 3 is a dummy that takes the value of 1 if the vehicle had at least one Major speeding (above 60 km/h), and 0 otherwise. Column 4 is a continuous variable, the total of times a vehicle was captured in Major Speeding

Effect of agent reviewing ticket. Minor Speeding. By quintile

	(1) Matched Sample	(2) Quintile 1	(3) Quintile 2	(4) Quintile 3	(5) Quintile 4	(6) Quintile 5
Ticket	-0.0673***	-0.0339	0.0811	-0.0868**	-0.102**	-0.107***
	(0.0226)	(0.0670)	(0.0495)	(0.0417)	(0.0447)	(0.0439)
Mean of not ticketed	0.200	0.119	0.179	0.195	0.214	0.227
N	48230	3562	8772	10223	12150	13523
First stage Wald-F	46.28	3.551	8.688	13.11	12.64	13.37

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A.15: Direct deterrence by price quintile. Alternative sample.. Dependent variable is a dummy that takes the value of 1 if the vehicle had at least one instance of Minor speeding, and 0 otherwise.

Effect of receiving a ticket. By month

	(1) Minor Speeding any time	(2) Minor Speeding Month 1	(3) Minor Speeding Month 2	(4) Minor Speeding Month 3	(5) Minor Speeding Month 4	(6) Minor Speeding Month 5	(7) Minor Speeding Month 6
Bias-corrected	-0.175***	-0.0216*	-0.0811***	-0.0139	-0.0481***	-0.0466***	0.0187*
	(0.0294)	(0.0127)	(0.0158)	(0.0153)	(0.0153)	(0.0139)	(0.00971)
Mean of not ticketed	0.222	0.0352	0.0539	0.0488	0.0481	0.0382	0.0194
N	136533	136533	136533	136533	136533	136533	136533
N left of cutoff	52468	52468	52468	52468	52468	52468	52468
N right of cutoff	57605	57605	57605	57605	57605	57605	57605
BW left	4.500	4.500	4.500	4.500	4.500	4.500	4.500
BW right	9.500	9.500	9.500	9.500	9.500	9.500	9.500

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A.16: Behavioral change. Full Sample.. Column 1 (speeding) is a dummy that takes the value of 1 if the vehicle had at least one Moderate speeding (between 50 and 60Km/h) any time after the first speeding instance and 0 otherwise. Columns 2 to 7 measure the same variable by month.

Effect of speeding tickets on other behaviors. Matched Sample

	(1)	(2)	(3)	(4)
	Below median	Above median	Below median	Above median
	Other tickets	Other tickets	Other tickets	Other tickets
	Dummy	Dummy	Continuous	Continuous
Bias-corrected	-0.0853*	-0.0650**	-0.194**	-0.186***
	(0.0448)	(0.0329)	(0.0766)	(0.0537)
Mean of not ticketed	0.132	0.106	0.189	0.137
N	24793	42442	24793	42442
Price in dollars	5379	19462	5379	19462
N left of cutoff	9837	16710	9837	16710
N right of cutoff	9937	16974	9937	16974
BW left	4.5	4.5	4.5	4.5
BW right	9.5	9.5	9.5	9.5

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A.17: Behavioral change. Matched Sample. Column 1 and 2 are a dummy variable that takes a value of 1 if the vehicle ever had another ticket for any cause after the first speeding instance, and 0 otherwise. Column 3 and 4 are a continuous variable that measures the total number of ticket for any cause after the first speeding instance.

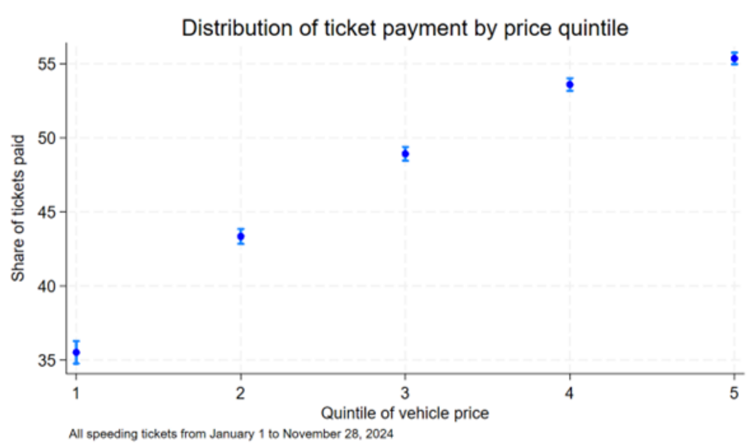

Figure A.14: Ticket payment by quintile of vehicle price. .