The role of legitimacy on deterrence: a speed-control intervention

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Abstract

The deterrence effect of legal sanctions has been widely studied and measured. Less attention has been given to the mechanisms by which some sanctions are more deterrent than others. This paper suggests one possible mechanism: the legitimacy of legal sanctions. I present a model in which individuals respond to a legal sanction for previously non-regulated behavior. An implication of the model is that more legitimate sanctions, those with higher social acceptance of the mandated behavior, cause a larger change in the targeted behavior. I test this hypothesis on the *Cámaras Salvavidas* Program, a speed-control intervention in Bogotá, Colombia. I estimate a Non-linear Difference in Difference equation, using the staggered implementation of the program as source of exogeneity. Legitimacy of legal sanctions is the source of heterogeneity. The program has a bigger effect in locations where the speed limit is more legitimate. It is shown that the result is robust to different ways of measuring legitimacy. Policy implications are discussed, suggesting that less strict but more legitimate sanctions might have a larger impact in reducing socially unwanted behavior.

1. Introduction

Modern societies have laws that sanction individuals who carry out socially undesirable actions as a deterrence mechanism. Economists have focused on understanding lawbreakers' incentives to design better legal sanctions (Chalfin & McCrary, 2017), while psychologists and lawyers have stressed the importance of social norms, especially when the aim is a behavioral change (Bicchieri, 2016). Understanding the mechanisms behind effective legal sanctions for behavior change is crucial (Bowles & Polania-Reyes, 2012). This is key for socially undesirable behaviors where states are limited in enforcing the law and must rely on voluntary cooperation (Tyler, 2004). Knowing which sanctions deter is not enough; we must also understand the underlying mechanisms.

This paper studies the legitimacy, the degree of acceptance of the prescribed behavior, of legal sanctions as a deterrence mechanism. Understanding this mechanism is crucial for addressing behaviors like tax evasion, gender discrimination, and speeding, where legal penalties have the dual aim: to punish and deter lawbreakers.

Road accidents are one of the public policy challenges where deterrence is more important. Since it is extremely difficult to impose legal sanctions to all individuals breaking traffic laws, sanctions are created so the threat of sanction works as a deterrent. I study the heterogeneous effect of legitimacy on the capacity of a speed-control intervention designed to reduce road accidents. I find that when the newly behavior mandated behavior is closer to the existing behavior, the intervention has a larger effect in reducing accidents. In short, that a gentle push might be more effective than a hard shove.

I develop a model in which legal sanctions generate a larger change in behaviors when they are perceived as legitimate. The underlying logic assumes that on top of their usual individual utility maximization (*a lá* Becker, (1968)) individuals incorporate a psychological cost of disobeying the law. This cost depends on the legitimacy of legal sanctions in the following sense: when legal sanctions are perceived as legitimate individuals experience a higher psychological cost of breaking the law.

The question on the capacity of laws to change social behavior is present on recent economic literature. Some works present theoretical models that support the idea that when laws are too far from social norms, they have a limited effect on behavioral change (Aldashev et al., 2011), or can even backfire (Acemoglu & Jackson, 2017) (Kahant, 2000). This as part of a wider literature on the intersection between law and social norms (Benabou & Tirole, 2011), particularly centered around the "expressive character of the Law"(Sunstein, 1996). This interaction between law and social norms has been studied on experimental[1] and quasi-experimental settings (Bursztyn, González & Yanagizawa-Drott, 2020, Galbiati et al., 2021) . I contribute to this strand of the literature by using empirical data that explore legitimacy of sanctions as the mechanism at play, and by suggesting a different avenue for legal changes backfiring.

Another strand of literature to which this paper is related focuses on *specific deterrence* and its application in road accidents. The influential prediction of Becker (1968), that an increase in expected punishment will reduce the prevalence of crime has been proven in multiple contexts, from car theft (di Tella & Schargrodsky, 2004), to late fees in movie-rentals (Haselhuhn et al., 2012) to school attendance (McMillen et al., 2019). A good summary of these findings, and its limitations, can be found on Chalfin & McCrary (2017). Specific deterrence has also been studied in the context of road accidents. When controlling for the endogeneity of police controls, the literature finds that deterrence depends positively on the perceived probability of being fined (Makowsky & Stratmann, 2011, de Angelo & Hansen, 2014, Luca, 2015, Banerjee et al., 2019; Dušek & Traxler, 2022; Traxler et al., 2018a) and on the monetary amount of the fines issued (Goncalves & Mello, 2022; Hansen, 2015; Rebollo-Sanz et al., 2021; Bar-Ilan & Sacerdote, 2004). However, there is wide variation on the magnitude and persistence of speed control effect on speeding behavior, clearly exemplified by highways in Germany where speed control effect is short on duration (Bauernschuster & Rekers, 2022) and highly dependent on incapacitation (Gehrsitz, 2017), while in its neighbor Czech Republic speed control

[1] Bicchieri et al., 2021; Bicchieri & Xiao, 2009; Govindan, 2021, Tyran & Feld, 2006.

seems to be long lasting and does not depend on incapacitation (Dušek & Traxler, 2022)[2]. It has also been found that while higher fines reduce recidivism, they are not imposed in the people in which they a bigger deterrence effect (Goncalves & Mello, 2022). The hypothesis that the legitimacy of sanctions can be a relevant explanation of why similar sanctions have heterogeneous deterrence effects comes from observing these differences that are hard to explain with existing models. It also comes from noting that the findings of these studies may not extend to low-income countries, where State capacity and thus the law enforcement capacity of transit authorities is considerably weaker.

This paper also relates to a more recent set of works that aim to evaluate the efficacy of *soft* sanctions on road fatality by suggesting legitimacy as an explanation for heterogeneous results. It has been found that while monetary sanctions are deterrent, there is no effect to non-coercive interventions that aim at showing information about the number or the severity of road accidents (Castillo-Manzano et al., 2012, Hall & Madsen, 2022). First randomized interventions find positive but quite small effects (Habyarimana & Jack, 2015; Lane et al., 2022), and the effect seems to depend les on new information than on social comparison (Chen et al., 2017; Lu et al., 2016).

A third strand of literature to which this paper is related focuses on how judges and other state agents in charge of punishing crime use their discretion. This literature, closer to the criminology discussion on policing, shows that sanctions that are not legitimate for the people in charge of enforcing them are less deterrent (Amaral et al., 2022). I contribute to this literature by looking at the question of how reasonable a legal sanction is from a different point of view, those of the individuals deciding whether or not to obey it. I also contribute to this literature by not focusing on the legitimacy of interactions with the police (Braga et al., 2018; Nagin & Telep, 2020), but on a less studied aspect, legitimacy of the behavior prescribed by the law.

The rest of the paper is organized as follows. Section 2 discusses the theoretical model in which the estimations are based. Section 3 describes the main features of the *Cámaras Salvavidas* program, and section 4 discusses the estimation strategy. Results are shown in section 5, where a set of robustness checks are also presented to show that I am really capturing legitimacy and not expected cost of sanctions. Section 6 does a brief exploration of the possible behavioral mechanisms in play. The paper closes with a discussion of the main findings on section 7 and offers some possible road to study legitimacy and deterrence in other contexts.

[2] This hypothesis of the importance of social norms is supported by the fact that there is considerable variation on the existence of social norms that punish speeding behavior. In Germany 21.22% of the surveyed think that other drivers consider speeding an acceptable behavior. For Czech Republic the number is 8.6% (Holoscher & Holte, 2019).

2. Model

As a guide to the empirical analysis that will be presented in the following sections, I now present a theoretical model of the possible effect of the introduction of a legal speed limit on the individual behavior of drivers. First, I model what happens in the utility decision of individual drivers. Then, this behavioral foundation is taken to the units of data I am interested on: accidents in roads.

2.1. Behavioral model

The main assumption in the model is that speed-control interventions introduce a psychological cost of breaking the law. This cost increases with the perceived legitimacy of the prescribed behavior (in this case, the speed limit). In the following sections I will discuss how to conceptualize and measure legitimacy, but to keep the simplicity of the model I will only work with a general notion of what legitimacy I, that is that the prescribed behavior, in this case the speed limit, is set at a fair and reasonable level.

2.1.1. No regulation scenario

It is safe to assume that I am not modelling the behavior of the average driver, I am modelling the behavior of the driver to whom the change in regulation will have some effect, the compliers. In the scenario where there are no legal speeding sanctions the driver *i* drives through road segment *r* at the speed S_{ir}. I will assume that the driver observes and exogeneous reference speed, at which is considered reasonable for him to move in segment *r*, denoted by S_r^{rs}. This reference speed is a function of road conditions, road design, and social norms on the acceptability of speeding. One could think of S_r^{rs} as "*the maximum speed at which (the driver perceives) is safe to drive at road r*".

I work under the assumption that S_r^{rs} is not perceived as a mere convention, but it has some normative expectations[3]. I do not deal with the creation and evolution of social norms, but with the way in which legal sanctions interact with existing behaviors at the time of legal changes[4].

[3] While not only the result of social norms, this reference speed is assumed to be increasing on social norms regulating speeding. The more social norms tolerate speeding, the higher S_r^{rs} will be. One could argue that even if S_r^{rs} is itself not a social norm, but a function of them, then it is necessarily endogenous. While it is true that all social norms are best responses of a game with multiple actors, and thus endogenous, in the short term and on a game with infinite players is safe to assume social norms are exogeneous to each player.

[4] Social norms regarding behavior on the road do change but in a longer time frame and from the intrinsic motivation of players, not from some external decisions by bureaucrats.
Anecdotal evidence of Bogotá also comes handy to explain the basis for our exogeneity assumption. Under the first administration of mayor Mockus (1995-1997) there was a massive government

Thus, the utility function of the driver in this context is decreasing on this cost and thus described by:

$$U\left(S_{ir,}\, S_r^{rs}\right) = \frac{-(\, S_{ir} - S_r^{rs})^2}{2} \quad (1)$$

As the utility function shows there is a cost for driving slower or faster than the reference speed[5]. This guarantees that the maximum speed chosen by the individual is obtained by the first order condition.

$$-(\, S_{ir}^{*} - \, S_r^{rs}) = \, 0 \quad (2)$$

The solution to (2) is thus $S_{ir}^{nr*} = S_r^{rs}$. In the context of no legal sanction on speeding behavior the optimal speed of each driver will be the observed reference speed at the road. The implicit assumption here is that everyone is equally responsive to social convention, and thus that all drivers consider reference speed equally acceptable. One could argue that this is not the case in most roads, since we have seen that some drivers have an individual *desire for speed* that for them is more important than social convention. As it will be shown on Table 1 of the Appendix none of the predictions of the model change by including an individual *desire for speed* on the utility function. Also, as I said at the beginning of this section, I am modelling the behavior of compliers, not average drivers, and for this fraction of the population is reasonable to assume all drivers as equally responsive to social convention.

2.1.2 Legal regulation scenario

I now move to the second scenario, where a speeding sanction is introduced for drivers exceeding speed level $\overline{S_J}$. I will assume that the legal limit is set at a lower speed than the optimal speed under the no regulation scenario: $S_{ir}^{nr*} \geq \overline{S_r}$. The utility of the driver is now described by:

$$U(S_{ir}, S_r^{rs}, \overline{S_r}, L_{ir}) = -\frac{(\, S_{ir} - S_r^{rs})^2}{2} - \, q(S_{ir}) - \frac{(\, S_{ir} - \frac{\overline{S_r}}{L_{ir}})^2}{2} \quad (3)$$

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campaign in order for drivers to improve their behavior, particularly for the respect of “zebras” (pedestrian crossings) on all traffic lights. This was a campaign aimed at cultural change that worked through shaming and social sanction and, one could argue, in fact moved the social norm equilibria. The Cameras program, on the contrary, has a much more limited campaign regarding speeding behavior as socially unacceptable. The Cameras program focuses more on imposing monetary sanctions to individual drivers. In fact, the basis for this work is the disdain for all *cultural interventions* on speeding by city officials who implemented the program (Bocarejo, 2022).

[5] To make it comparable to the regulation scenario one could call this the *reference speed-breaking cost:* $(S_{ir} - \, S_r^{rs})^2$. Note that It is reasonable to punish slower drivers since they also increase the risk of accidents for everyone. Of course, it is not equivalent to excessive speed but for simplicity I will not model different punishment for under and over speeding. A more elaborate model will necessarily start by punishing positive deviations higher.

Equation (3) has two new costs in the drivers utility: the expected cost of a speeding ticket and a *law-breaking cost.*

The expected cost of a speeding ticket is a function of the probability of receiving a fine at the chosen speed. I will represent this cost as $q(S_{ir}) = p(S_{ir})\varphi$, where $p(S_{ir})$ is the probability of getting a ticket at speed S_{ir} and the monetary and non-monetary costs of the fine is φ[6]. I will assume that drivers have some expectation of the value of $q(S_{ir})$ at every point in the road.

Since speeding behavior is now regulated through the law, drivers will now also have a *law-breaking cost* that reduces their utility. This is where the center of my theoretical approach lays. The essential assumption of this *law-breaking cost* conceptualization is that individuals want to see themselves or be seen by others as law abiding citizens. Thus, they suffer a psychological cost of being reminded that they are breaking the law, whether they are effectively punished or not. This cost is higher when individuals consider the legal sanction as something they should accept, i.e., a legitimate behavior prescribed by the State[7]. This Legitimacy is written as L_{ir} since is specific to each individual and road segment. It always takes positive values between 0 (no legitimacy) and 1 (perfect legitimacy). My central formulation is that under the *law-breaking cost* individuals want to follow not the legal speed but something they perceive as reasonable: a *legitimacy adjusted behavior (legitimacy adjusted speed in this case).*

The last term on (3) is thus defined as $(S_{ir} - \frac{\overline{S_r}}{L_{ir}})^2$. Drivers adjust the legal limit according to the legal limit to find their legitimate behavior, and this quantity is decreasing in L_{ir}: the more legitimate the intervention the closer drivers would want to follow the legal limit. In line with the literature on the "expressive character of the Law"(Sunstein, 1996) I am making the rather strong assumption that the posting of

[6] It is worth noting that according to Colombian law the amount of the fine does not increases with speed level, but the reason why $q(S_{ir})$ is increasing in S_{ir} is not relevant to my analysis.
One can argue that even if not in the Colombian law, in practice the amount of the fine is also increasing in speed. It is also true in many countries that driving with excessive speed can be a reason to be pulled over by police. Experience shows that even if the police has no technical means of verifying excessive speed, the interaction with the police can result in a ticket for a different reason. Thus, the value φ is in fact an expected value, but it is still a function of the speed chosen by the driver and can be anticipated by him or her. So, it is still a known function of speed.

[7] Literature in criminology has reached the following definition of legitimacy: "the public belief that there is a responsibility and obligation to accept and defer voluntarily to the decisions made by authorities" (Braga et al., 2018). One should note that legitimacy tends to place emphasis on physical encounters between citizens and the police, and thus on procedures that make police interventions. Here I am centered on the legitimacy of the behavior mandated by the law itself, and not on the policing mechanisms.

a limit itself, independent of how strong enforcement is, causes the *law-breaking cost* to activate[8].

Note that (3) is not a continuous function since $q\left(S_{ij}\right)$ is present only when $S_{ij} > \overline{S_J}$[9]. But I will assume this is the case for the rest of the analysis. The maximization of (3) thus becomes:

$$-\frac{\partial q\left(S_{ir}^{*}\right)}{\partial S_{ir}} - \left(S_{ir} - S_{r}^{rs}\right) - \left(S_{ir} - \frac{\overline{S_r}}{L_{ir}}\right) = 0 \quad (4)$$

Note that in this case the optimal speed will be an average between the social norm speed and this *legitimacy adjusted legal speed* minus the marginal cost of speeding.

$$S_{ir}^{lr*} = \frac{S_{ir}^{nr*} + \frac{\overline{S_r}}{L_{ir}} - \frac{\partial q\left(S_{ir}^{*}\right)}{\partial S_{ir}}}{2} \quad (5)$$

To measure the effect of this legal change in the optimal speed, one has to compare the optimal speed in (2) and in (5), when there is a change from no regulation to legal regulation of the behavior. It is easy to see that the optimal speed under legal norms is lower when the following condition holds.

$$S_{ir}^{nr*} > S_{ir}^{lr*}$$

if

$$L_{ir} > \frac{\overline{S_r}}{S_{ir}^{nr*} + \frac{\partial q\left(S_{ir}^{*}\right)}{\partial S_{ir}}} \quad (6)$$

This tells us that if enforcement is high and the expected cost is significant, then speed will be significantly lower under the law regulating scenario even for low levels of legitimacy. But if enforcement is not strong there might be cases where Legitimacy is so low that the optimal speed in fact increases after the introduction of the law[10]. As an analytical tool it is useful to consider this case where law is only expressive. It

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[8] Since we are dealing with compliers (who arguably have already some respect for the law) it is reasonable to assume that they will feel bad for behaving as unruly citizens. This assumption is particularly well suited to the program I use to test my hypothesis since the program is based not on surprising citizens but on making them aware of the need for lower speeds. The program is designed with road signs that tell drivers of the presence of each Camera and sufficient speed limit signs around it.

[9] Exploratory speeding is a reasonable assumption, particularly in low and middle income countries where state capacity for enforcement is very low, and thus the probability of receiving a speeding ticket is almost 0. If this were not the case in most roads in the city, there will be no public policy aimed at reducing the maximum speed of roads.

Note also that the discontinuity of $q(S_{ir})$ means that if the marginal probability of getting a fine is sufficiently high, then there is always a corner solution where $S_{ij}^{l*} = \overline{S_J}$.

[10] As I will explain later, the Cameras program only changed the value of $q(S_{ir})$ in certain very specific spots and hours of the day. For the rest of the city (and nighttime) there is no change in the probability of enforcement, which continues to be effectively absent. Thus, it is equivalent to the expected cost of a fine not entering the utility function.

is easy to see from (5) and (6) that the optimal speed in this case will be an average between the previous reference speed and the legal limit, and thus legitimacy will have to be bigger than the ratio between these two for the program to have its desired effect.

An important theoretical feature of the *law-breaking cost* is the possibility of legal norms backfiring in the sense of leading to a higher average speed than when no legal regulation existed. It is clear from condition (6) that backfiring is easier under low enforcement because the magnitude of the derivative is smaller and thus a lower level of Legitimacy is needed. But backfiring is also possible with some enforcement. This is another form of law backfiring for not considering social norms, also present in Kahant (2000) and Acemoglu & Jackson (2017) models of social norms[11]. Unlike those models that are based on the reluctance of enforcers or whistle-blowers to legally enforce legally mandated behaviors that are too far from their own social norms, in my model is the individual himself who decides to accept or challenge the legally mandated behavior.

Table 1 summarizes the model and its predictions. A model with a function $V_i(S_{ir})$, which is concave in S_{ir} and captures the individual tolerance for the risk of having an accident is presented in Table 1 of the Appendix. None of the implications of the model change significantly by that assumption.

[11] One could also think about the ideas presented in (Gneezy & Rustichini, 2000) on the effect of a fine on lateness behavior. Theirs is clearly a case in which moving from a social norm regulation to a form of punishment clearly backfires. In their experiment it could be that the amount of the fine was too little, but also that the legalization of a moral issue is not socially efficient. Something like there being no law-breaking cost in (3) and thus depending only in punishment. That is why when punishment is taken away, behavior is still regulated by law and thus behavior does not return to the previous social norm.

<table>
<tr><th colspan="4">No regulation scenario (before the Cameras program)
Reference speed: S_r^{rs}</th></tr>
<tr><td>Utility function</td><td>Maximization</td><td>Optimal speed</td><td></td></tr>
<tr><td>$U\left(S_{ir,}\, S_r^{rs}\right) = \frac{-(\, S_{ir} -\, S_r^{rs})^2}{2}$</td><td>$-(\, S_{ir}^* -\, S_r^{rs}) =\, 0$</td><td>$S_{ir}^{nr*} = S_r^{rs}$
Reference speed is the optimal speed</td><td></td></tr>
<tr><th colspan="4">Law regulation scenario (after the Cameras program).
First case: some enforcement p$(S_{ir}) > 0$ and legal limit: $\overline{S_r}$</th></tr>
<tr><td>Utility function</td><td>Maximization</td><td>Optimal speed</td><td>Effect of program</td></tr>
<tr><td>$U(S_{ir}, S_r^{rs}, \overline{S}_r, L_{ir}) = -\frac{(\, S_{ir}-\, S_r^{rs})^2}{2} - q(S_{ir}) - \frac{(\, S_{ir}-\left(\overline{S_r}+\frac{1}{L_{ir}}\right))^2}{2}$</td><td>$-\frac{\partial q(S_{ir}^*)}{\partial S_{ir}} - (\, S_{ir} -\, S_r^{rs}) - (\, S_{ir} - \frac{\overline{S_r}}{L_{ir}}) = 0$</td><td>$S_{ir}^{lr*} = \frac{S_{ir}^{nr*} + \frac{\overline{S_r}}{L_{ir}} - \frac{\partial q(S_{ir}^*)}{\partial S_{ir}}}{2}$</td><td>$S_{ir}^{nr*} > S_{ir}^{lr*}$
if
$L_{ir} > \frac{\overline{S_r}}{S_{ir}^{nr*} + \frac{\partial q\left(S_{ir}^*\right)}{\partial S_{ir}}}$ (6)

Then:
$S_{ir}^{nr*} > S_{ir}^{lr*} \approx \overline{s_r}$

Possibility of an increase in speed (“backfire”) when:

$S_{ir}^{nr*} < S_{ir}^{lr*}$
if
$L_{ir} < \frac{\overline{S_r}}{S_{ir}^{nr*} + \frac{\partial q\left(S_{ir}^*\right)}{\partial S_{ir}}}$ (6)
Then:
$s_{ir}^{lr*} > s_{ir}^{nr*} > \overline{s_r}$</td></tr>
</table>

Table 1. Behavioral model and its predictions.

This model is not speed-specific, but relevant for other domains where behavior goes from being non-regulated to a law-based regulation, and there is little enforcement capacity by authorities. One can think of a mental example that shows this model's usefulness: tipping restaurant workers. People tip restaurant workers not only because everyone does it, but because they feel a part of it is normative; they assume others will judge them as stingy for tipping to little or wasteful for tipping too much. My model suggests that when no law regulates tipping the optimal amount to tip is what everyone around you usually tips: the reference amount. In Colombia is 10% of the check (in the US is around 15%, once again customary behaviors are very local).

One day the government might decide that the custom is unfair to restaurant workers and thus there will be an expensive fine for everyone tipping below 20%. What happens to the average tip is now a question of enforcement and legitimacy of the sanctions. There is little to learn from the model if enforcement is high. If dinners are certain to get caught tipping under 20%, then they will not risk it: 20% is the new tip. The interesting case is when enforcement is very low. If dinners think they can get away with a tip below the legal mandate, they will try to do it because they don't like being legally forced to do anything. In the most common case, the average tip will be close to 20%, but lower. How much lower will depend on how legitimate dinners think the prescribed behavior is: the more legitimate the new norm the closer the average tip is to 20%. My model includes the special case where people think that the law is too greedy, or that this is absolutely no matter for the government to regulate, or both. In this case the average tip might go below the original 10%. That is the backfiring case of an illegitimate legal norm.

2.2 An application to road accidents:

The behavior I am modelling is speed but the one I make my hypothesis about, and the one will measure with data, is accidents. This decision is based on two considerations: one practical and one theoretical. The practical consideration is the lack of quality data for speed of vehicles. On the contrary, data for accidents is very precisely measured and makes different roads easily comparable.

The principal disadvantage it that my hypotheses will have to accept the notion that the main cause for road accidents is excessive speed. If the data does not support, my hypothesis a possible explanation is that this causality is in fact weaker than we have come to accept and most literature assumes. The contrary is not true, finding some support for my hypothesis does not automatically mean that speed is the main cause of accidents.

The prediction of the model is that optimal speed is the average between the reference speed and the newly introduced legal limit minus the derivative of the fine with respect to speed, as presented in (5). If one assumes that accidents are a function of $\beta * \text{speed}$, one can model the effect of the introduction of a legal limit as the heterogeneous effect of a treatment, where the treatment is the legal change and the source of heterogeneity is a measure of legitimacy:

$$Accidents_{rt} = \beta_0 + \beta_1 * Limits + \beta_2 * Limits * \frac{1}{Legitimacy_r} + \varepsilon_{rt} \text{ (7)}$$

$$\text{with } \beta_1 = \beta * -\frac{\partial q(S_{ir}^*)}{\partial S_{ir}}, \beta_2 = \frac{\beta}{2} \text{ (8)}$$

Where $Accidents_{rt}$ is the number of accidents in segment r and at times t and *Limits* is the treatment, the introduction of a speed limit, and $Legitimacy_r$ denotes a value that goes from 0 to 1. Note that this is a classical difference in differences estimation with heterogeneity, and all the assumption of such methodology must be verified. This equation is set at the road level, but my estimation will be at the segment level.

My model is agnostic about the general effect of the program on accidents, that is $\beta_1 + \beta_2\left(\frac{1}{Legitimacy_r}\right)$ could be positive or negative. Even if it is not the center of my analysis it is interesting to make a general evaluation of the program and see if the general result is a reduction of accidents[12]. This is captured by Hypothesis 1:

Hypothesis 1: The introduction of legal limits will result in a reduction in the number of accidents on all treated segments.

$$\beta_1 + \beta_2\left(\frac{1}{Legitimacy_r}\right) < 0 \;\forall\; Legitimacy_r$$

My model does make a prediction about the heterogenous effect of $Legitimacy_r$. If the general logic is that the more legitimate a sanction the more deterrent it is, for higher values of legitimacy the effect of the program should be bigger, that is a significant reduction of accidents. This is captured by my Hypothesis 2.

Hypothesis 2: The introduction of Cameras will result in a bigger reduction of traffic accidents on spots where legitimacy of the legal limit is higher.

$$\beta_2\left(\frac{1}{Legitimacy_rx}\right) < \beta_2\left(\frac{1}{Legitimacy_ry}\right) \;\forall\; Legitimacy_rx > Legitimacy_ry$$

The logic of a classical Difference in difference equation presented in (7) will need to be adjusted to the specific characteristics of the Program and the data we have. But for now, it keeps the main logic of the estimation and reflects how I evaluate both hypotheses. Once again, I am working under two assumptions. First, that all drivers are equal to the complier driver of my behavioral model. Second, that the main cause for accidents is speed and thus speed reductions will translate into a reduction on

[12] Even if this paper tries to do more than a policy evaluation of a Cameras Program, it is worth noting that causal evidence on the effect of Cameras on the number of accidents is still scarce. Some papers find a causal reduction of accidents and injuries (Ang et al., 2020), while others show that Cameras only change the type of accidents occurring. (Gallagher & Fisher, 2020).

the number of accidents. Figure 2 summarizes the chain of causality that this paper explores.

General Hypothesis

$\uparrow$ Legitimacy $\Longrightarrow$ $\uparrow$ Deterrence of legal sanctions

Behavioral Model

Introduction of a Speed limit
$\uparrow$ Legitimacy $\Longrightarrow$ $\uparrow$ Law- breaking cost

Behavioral prediction
$\uparrow$ Legitimacy $\Longrightarrow$ $\downarrow$ Speed

Application on road accidents

Segments with a speed limit
Speed limit $\Longrightarrow$ $\downarrow$ Accidents in segment **(Hypothesis 1)**
$\uparrow$ Legitimacy $\Longrightarrow$ $\downarrow\downarrow$ Accidents in segment **(Hypothesis 2)**

Summmary
$\uparrow$ Legitimacy $\Longrightarrow$ $\downarrow$ Speed $\Longrightarrow\downarrow$ Accidents in segment

Figure 2. Summary of the suggested causal relation and hypotheses.

Note again that the argument depends on the existence of the new legal limit activating the law-breaking cost[13]. Besides the potential punishment, laws have an expressive meaning. If the only thing that drivers care about is being or not being fined regardless of the perceived legitimacy of the sanction, the heterogeneity predicted by Hypothesis 2 is not present.

3. The *Cámaras Salvavidas* program in Bogotá, Colombia, and data sources

I use the case of *Cámaras Salvavidas* program in Bogotá, Colombia (Cameras) to study the general question of the impact of legitimacy on deterrence. Road fatalities are a public health problem, especially in the developing world. While low and middle income countries account for 60% of vehicles, they are responsible for 93% of road fatalities (WHO, 2018). Speed-control has been considered the most effective policy answer to this issue (Vecino-Ortiz et al., 2022)[14]. This strategy started in high income countries and soon was also implemented in middle and low income countries. Speed control actions in middle and low income countries have not been short of debate and even when they seem to be effective their acceptance is limited (Ang, Vieira & Christensen, 2020). Like most program of its kind the stated goal of *Cámaras Salvavidas* is to reduce traffic fatalities, not increase city revenue through fines (SDM, 2022).

[13] The model is agnostic about the reason why drivers will want to follow the law. Section 6 will dig deeper in the possible behavioral mechanisms in play in the relationship between law changes and behavior.
[14] Of the main four interventions in terms or reducing road fatalities (speed control, drunk driving control, helmet use for motorcycle users and seat belt use) speed control is the one that would lower fatalities mores.

As its name suggests the *Cámaras Salvavidas*[15] program aims to reduce traffic fatalities at high-risk spots. When the program was implemented, a reduction in the number and seriousness of accidents was expected as a result of higher enforcement of speed limits (Bocarejo, 2022)[16]. This deterrence effect is underlined by the use of road signs that tell drives of the presence of each Camera and sufficient speed limit signs around it. All this emphasizes to drivers that the goal is not to surprise them, but to make them aware of the need for lower speeds.

Technically the Cameras are electronic devices that detect all vehicles that circulate over the speed limit in the road. Such detections are then verified by a transit officer and, if identification of the vehicle is considered sufficient, results in a monetary fine to the registered owner of the vehicle. The program was formally established in December 2019, and the first Cameras were installed in June of 2020. Due to the difficulty of the bureaucratic process and the Covid pandemic, only by January 2022 were all the 91 Cameras of the program finally installed.

The fines imposed by Cameras share two relevant characteristics with the rest of transit offenses in Colombia: the monetary fine does not increase with multiple offenses, and they do not limit the right to circulation of vehicles. Shortly before the start of the program a judicial ruling introduced another relevant characteristic[17]: the legal right to be cleared of the fine if the State cannot prove that the owner was effectively the driver. Taking advantage of this possibility has a cost to the owner, as all legal procedures do, but is significantly lower that the fine and it is smaller for frequent offenders that learn to navigate the judicial system. Even under this ruling a good number of drivers decided not to pay their fines, around 40% of fines where effectively paid, so the program was not all a *placebo* intervention. In short, that ruling turned a strong sanction into a softer one and lowered the expected cost of a fine. These three characteristics help me argue that in this context deterrence does not get confused with incapacitation and increase the external validity of my findings.

Since the general argument of the previous sections depends on Cameras making speed limits salient and thus drivers aware of breaking the law, one should verify that fines issued by the program are a significant number. In fact, speed fines imposed by Cameras grew rapidly. Speeding tickets went from 3,932 in all 2019 to 58,914 in 2020 (and 143,488 in 2021)[18]. But this change only occurred at the specific

[15] One should note that *Salvavidas* in Spanish means lifesaving. The name of the program in English will be something like Lifesaving Cameras. This is relevant in as much as it signals that the program is aimed at reducing fatalities, not at increasing city revenue through fines.
[16] The Program was implemented as a direct application of the Vision Zero approach to road fatalities. https://visionzeronetwork.org/about/what-is-vision-zero/
[17] Here I am referring to Ruling C 038 of February 6th, 2020, of the Corte Constitucional de Colombia. Magistrate: Alejandro Linares Cantillo. Late in 2022 this ruling was reversed, but this legal change falls outside of my studied period.
[18] Speeding tickets also increased as a percentage. In 2020, speeding accounted for only 21.35% of all the traffic fines in the city. By 2021 Cameras accounted for 29.71% of all traffic fines in Bogota even with the reduction of circulation caused by Covid.

spots of the Camera: more than 95% of all speeding tickets were given by the Cameras. This reinforces the argument that drivers will necessarily have to be very aware of the location and speed limit of each Camera even if they know that the expected cost of a fine is low. Or that they are not going to pay the fine they could receive.

Bogota's *Secretaría de Movilidad* (Mobility Office) makes public its database of all accidents that occur in the city since 2017[19]. All accidents are geolocated and sorted according to their severity. For this paper all accidents occurring between January 2019 and June 2022 will be used. Data on traffic offenses is also public, with geolocation and type of infraction. Cameras are georeferenced and the first date they started issuing tickets is easy to find. All Cameras installed after June 2020 will be used, taking advantage of the staggered implementation of the program.

For speed in roads, the periodic measures are captured and published *Secretaría de Movilidad* will be used. These periodic measures are captured at short road segments and are available for some months both before and during the period of study.

Speed related social norms will be measured by the *Encuestas de Percepción de Riesgo Vial* (Road Risk Perception Surveys) that were done in the city in 2018 and 2019. It tries to measure the perception of all actors involved in the mobility of the city. This information was shared on request by the Mobility Office, and is used in the Appendix.

4. Identification strategy

In section 2 I presented the application of my model as a classical difference in differences estimation with heterogeneity. Some elaborations are needed due to the nature of the Cameras program. First has to do with the timing of the intervention. As I explained in the last section the location of each Camera is clearly endogenous, but the exact moment in which it starts operating is not. Conditional on a location having a Camera, the moment in which it starts issuing tickets is random and cannot be anticipated by the road users[20]. From the workings of the bureaucratic process of implementing the program we can be certain that Cameras and road signs about the speed limit where installed simultaneously and there was no gap between installation and operation.

I will take advantage of this staggered intervention as source of exogeneity, noting that road segments where Cameras have not yet been installed are good controls for road segments where a Camera is already working. These not-yet treated spots

[19] All databases are available in the following link: https://datos.movilidadbogota.gov.co/. For this paper all data was retrieved on May 1st of 2023.

[20] Nor were Cameras located in spots with higher accidentality installed first. The bureaucratic process made the timing of the installation almost a random occurrence. This is also a common occurrence in Colombia for the installation of surveillance cameras. (Gómez, Mejía and Tobón, 2021)

work as controls before the treatment and as treated units after the first date the Camera starts imposing tickets. It is also desirable to build never-treated units in locations that are as similar as possible to the location of each Camera. Using this never-treated spots allows me to have an estimation with higher statistical power, an important feature since there is a relatively low number of treated spots. Having never-treated units also allows me to use the last group of treated units.

The *Cameras* program targets spots and roads that have a high prevalence of accidents. For this purpose, the first 46 locations were chosen, and the gradual installation of Cameras started in June of 2020. By January 2022 there were 84 spots where speed excess is detected by Cameras[21]. Figure 3 shows the exact location of each camera in the map of Bogotá.

[21] While the program evolved to enforce six different traffic violations, at its onset it was focused only on speeding. Cameras installed in 2020 had speeding as the only infraction, while the ones in 2021 were focused on it. For Cameras installed in 2022 only the ones that do detect speeding will be used in the analysis. This explains why only 84 of the existing 91 Cameras will be used on the analysis.

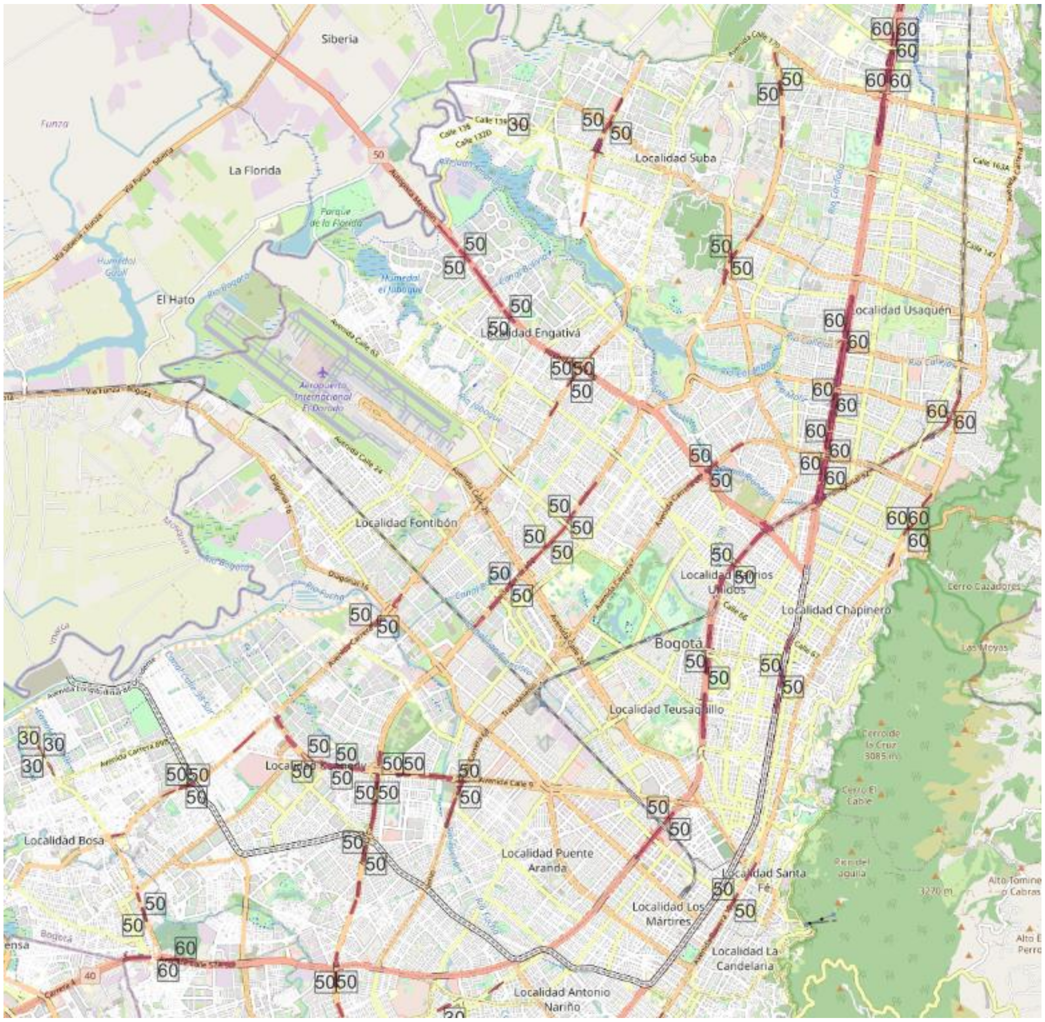

Figure 3. Location of Cameras. Label includes the maximum speed detected at each location. Red segments with no label are never-treated units.

The rollout of the program was staggered. Table 4 shows the starting month of each Camera. Note that there is a significant number of Cameras starting in the last period of anaylisis, thus making worse the problem of bad comparissons present in standard Two Way Fixed Effect estimation as discussed in (Goodman-Bacon, 2021) and Callaway y Sant 'Anna (2021).

Time distribution of treated segments		
Month	Treated road segments	Percentage
June 2020	13	15.5%
July 2020	6	7.1%
August 2020	15	17.9%
September 2020	10	11.9%
October 2020	2	2.4%
November 2020	2	2.4%
August 2021	4	4.8%
January 2022	32	38.1%

Table 4. Start date for treated road segments

As it was anticipated in Section 2 my units of analysis are road segments. Road segments are built as the 300 meters segment, 150 meters before and 150 meters after the specific location of each Camera. Never-treated controls were built as follows. For each Camera a road segment of the same dimension as the treatment one was chosen 700 meters apart from the camera location, in each direction. It has the same dimension of a treated segment and is always located in the same road, which guarantees the similarity between both road segments.

The dependent variable is road accidents. A total of 15735 accidents occurred in the control and treatment units, 5016 of those were serious accidents (with at least one injured person).

In line with Hypothesis 1, my independent variable is the treatment of having a Camera working in each road segment. In line with Hypothesis 2, for each treated segment, I will create a measure of legitimacy, and estimate the heterogenous effect it has on accidents. I have delayed the content of my legitimacy variable enough: a detailed explanation of the construction of that variable is now necessary.

4.1 Legitimacy variable

I consider the fact that the model does not require a specific definition of Legitimacy as an advantage. Since this is an area where the literature has no stablished definitions one can be creative on how to conceptualize and measure legitimacy of the prescribed behavior, in this case the speed limit. What follows is my approach, striking a balance between the data I have and my intuitions on the behavior on the road.

I assumed that individuals prefer no regulation to legal regulation. Drivers are not happy to accept a change on their reference speed, and thus they will compare the new legal limit speed to the reference speed when no regulation was in place. I am measuring the feeling of "*while I would like to drive faster the limit is sensible",* or its most common opposite *"the limit is too low, one could drive much faster in this road*" that some drivers experience when they see the signs telling them of a new maximum speed.

As it is clear from (6), Legitimacy in the behavioral model is a function of the ratio between the legal limit and the optimal speed before the regulation was put in place. I take the speed limit at each segment and then divided by the reference speed. For each segment *r* where a Camera is located, I take some value of the distribution of observed speeds in three periods before the program started (October and December 2019, and May 2020). I use the 99th or 97th percentile at each segment where a Camera is located as a measure of the reference speed (remember that form (2) $S_{ir}^{nr*} = S_r^{rs}$). This guarantees that I am capturing some aspirational speed, and not the effect of traffic and congestion that forces drivers to go at speeds lower than the reference speed.

In summary:

$$LL_r = \frac{Legal\ limit\ at\ segment\ r}{Observed\ speed\ at\ segment\ r}$$

$$LL_r = \frac{\overline{S_r}}{\overline{S_r^{rs}}}$$

One could argue that perceptions about the role of government in regulating speed could also affect the legitimacy variable. In the Annex I present a way of incorporating these considerations in the construction of the legitimacy variable. The available data is far from ideal, but it is an interesting extension of my model.

4.2 Main Regression

From the various alternatives for estimating the effect of a staggered intervention presented in the recent econometric literature, the only one that allows us to capture the heterogeneous effect of an intervention depending on a unit characteristics is the one suggested by Wooldridge (2022). It also has the additional advantage of allowing for a non-linear relationship when the dependent variable behaves as count data and has zeros in many time periods by using a Poisson pseudo-maximum likelihood regression (PPML) (Correia et al., 2020). This fact alone makes this a better option that the ones suggested by Callaway y Sant 'Anna (2021), Sun & Abraham (2020) or Roth & Sant'anna (2022), that only allow for a lineal relation.

The heterogeneous effect of legitimacy on the impact of the cameras Program in accidents can be estimated through a PPML regression like the following:

$$\log(\mathrm{E}(A_{rt})) = \mu_t + \gamma_r + \sum_{g=g_i}^{G} \sum_{t\geq 0}^{T} \beta_{gt} D_{rt} + \sum_{g=g_i}^{G} \sum_{t\geq 0}^{T} \tau_{gt} D_{rt} (L_r - \overline{L_g}) \quad (9)$$

Where A_{rt} corresponds to the number of accidents or serious accidents (with at least one person injured) at road segment *r* during month *t*. μ_t corresponds to a month fixed effect, γ_r corresponds to road segment fixed effects. The third element of the sum corresponds to interactions for a treatment group and a time period with an indicator variable D_{rt} that takes the value of 1 if it is a period at which the road segment has already started treatment ($t > 0$) and 0 if it has not. In short, this term makes all the *valid* interactions for time and treated groups. Thus, a coefficient β_{gt} is obtained for each for each group $g_i \epsilon (G)$ and time period where the road segment is being treated. From an average of those β_{gt} the common effect of the program is obtained, and this could be positive or negative.

The fourth element of the sum described in (9) is my main interest. It refers to the same *valid* group and period combinations interacted with the indicator variable, but additionally with the difference between my measure of legitimacy L_{rl} and the average of that value across all cameras $\overline{L_g}$ for the same treatment group $g_i \epsilon (G)$. This demeaned variable measures the effect of one unit deviation on the legitimacy variable. All coefficients τ_{gt} then capture the heterogeneous effect of legitimacy of sanctions on the number of accidents. For each form of Legitimacy a single coefficient will be computed by averaging all marginal effects for road segment and time period interactions. Errors are clustered at the road level in all estimations. A bootstrap procedure with 20 replications will be done to obtain standard errors for all estimates.

Note that equation (9) does not have Legitimacy, L_r, as an independent variable in itself as most regressions interested in identifying a heterogeneity do. This has to do with the nature of our Legitimacy variable that both only exists after the introduction of the sanction and only affects road segments where a Camera is present. Since legitimacy is defined in relation to a speed limit, never treated units technically don’t have a Legitimacy value and treated units only have it after they start receiving the treatment. Since Legitimacy varies by segment, but not in time including it before the treatment will be useless, since it will be captured by segment´s fixed effects. For these reasons it does not make sense conceptually to have Legitimacy as an independent variable in itself.

According to Hypothesis 2 the coefficient τ_{gt} should always be negative, meaning that the reduction of accidents is bigger on locations where the Cameras Program is more legitimate. Note that it is possible to include many forms of legitimacy by iterating the last term on equation (9). Theoretically one could include different forms of L_r, such as ${L_r}^2$, ${L_r}^3$ and ${L_r}^4$. The possible complications are computational, not theoretical. As se will see soon, it is hard to obtain convergence when the full set of powers of legitimacy is included.

5. Results

5.1 Descriptive statistics for accidents and a note on external validity

In order for my strategy to make sense, treatment and control units must be comparable in terms of the dependent variable. Table 5 compares the behavior of drivers during the time where each segment with a Camera had not started issuing tickets to show that they are in fact very comparable. If anything, control segments have a slighter higher average of accidents and serious accidents.

Comparison between treated and control segments before each Camera started issuing tickets			
	All segments	Treated Segments	Control Segments (Never treated)
Number of segments	242	84	158
Accidents	14,251	3,348	10,903
Accidents per month (average) before treatment	1.589	1.539	1.605
Accidents = 0 percentage of month	38.18%	41.64%	36.34%
Serious Accidents	4515	1072	3443
Serious accidents per month (average) before treatment	0.503	0.493	0.507
Serious Accidents = 0 percentage of month	68.84%	66.50%	68.15%
Observations before treatment	8,970	2,176	6,794
Total observations	10,406	3,612	6,794

Table 5. Treatment and control segments

The period in which the Cameras policy is evaluated includes the emergency and lockdowns caused by COVID. The external validity of results shown here could be reduced by this fact. Figure 6 shows the tendency of road accidents occurring in Bogota during the analyzed years, 2019 to 2021. It is clear that 2020, the year in which the program started, is atypical due to the movement restrictions imposed by the lockdown of COVID-19. This atypical behavior extends also to some months in 2021, but from September on accidents return to their pre-pandemic levels[22]. Accidents in 2022 follow the same path that accidents in 2019. Thys atypical behavior will only be a threat to external validity if accidents reduced in a different way on treatment and control road segments, which is not the case as it be shown later. In any case, the studied period is sufficiently large for this threat to external

[22] Due to lag in reporting, only data until July 2022 will be used. The following months show an unrealistic descent.

validity to lessen. Only 29 of the 160 coefficients used to calculate effects come from 2020. Figure 2 of the Appendix shows the same information for serious accidents, where the effect of COVID is smaller.

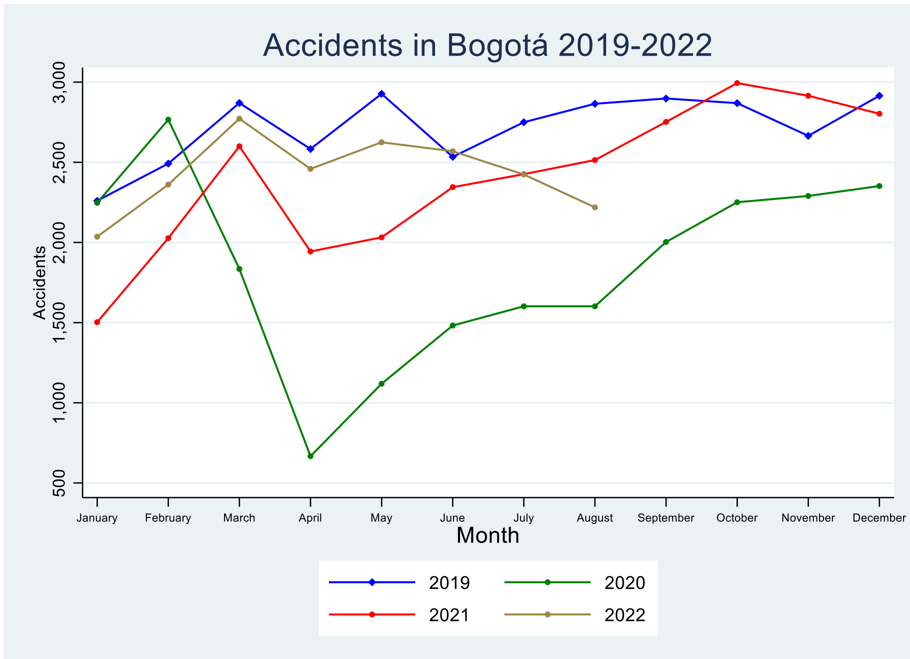

Figure 6. Accidents in Bogotá by month and year of occurrence.

5.2 Parallel trends assumption and general effect of the program:

Parallel trends assumption is harder to verify with staggered implementation. But one can offer visual indication by comparing segments entering treatment each month with those that are not yet or never treated units. Figures 7 and 8 summarise this information for accdidents (Figure 3 and 4 of the Appendix for serious accidents). As it is evident, trends don't seem completely parallel in a few months (September and October, 2020) but it seems that parallel trends hold for the months in which a majority of Cameras were installed: June and August 2020, and January 2022. More than 80% of Cameras started operating in only four of the months (June, August, September 2020 and January 2022). The last month, January 2022, concentrates more than 38% of the Cameras, as it was shown in Table 4.

A formal test for parallel trends is complicated by the fact that we have eight treatment groups and at least seventeen periods before treatment. A test of heterogenous trends (Wooldridge, 2022) is not significant at the 10% percent level if we exclude two groups for accidents (treated in Septemeber 2020 and August 2021) and three groups for serious accidents (treated in Septemeber 2020, October 2020 and August 2021). Those groups amount to less than 16% (accidents) or 20% (serious accidents) of treated units. As we will see later results are essentially the same if we use all treatment group or only the ones that guarantee this rather strong version of parallel trends.

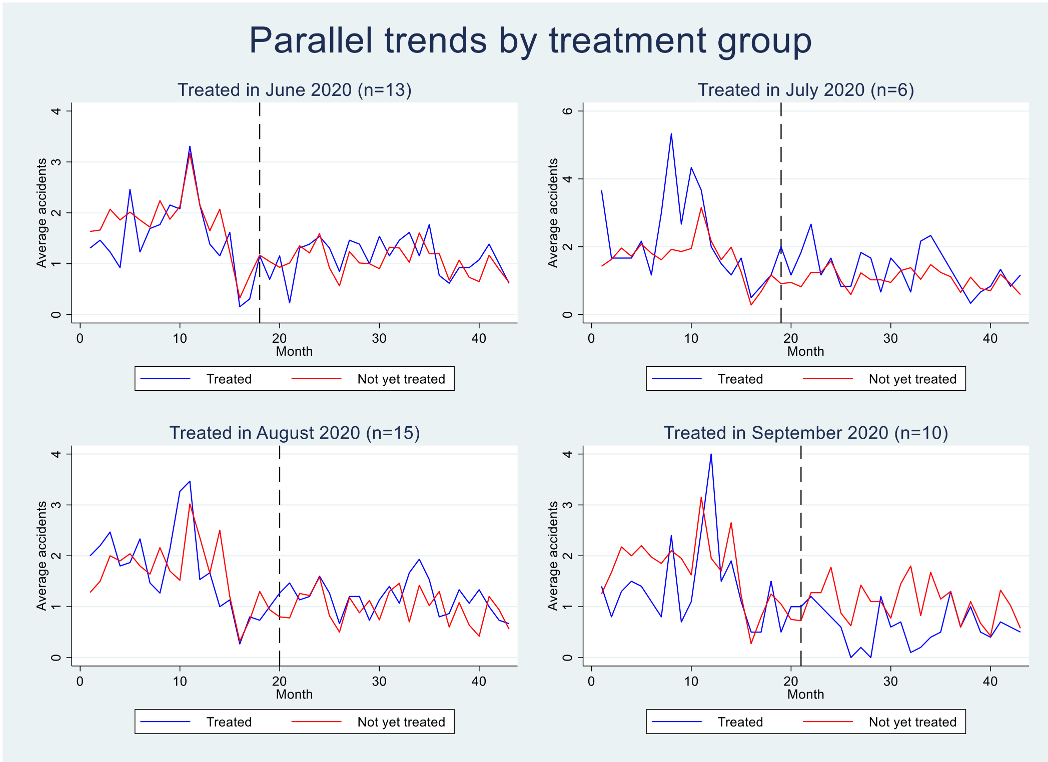

Figure 7. Trends by treatment group.

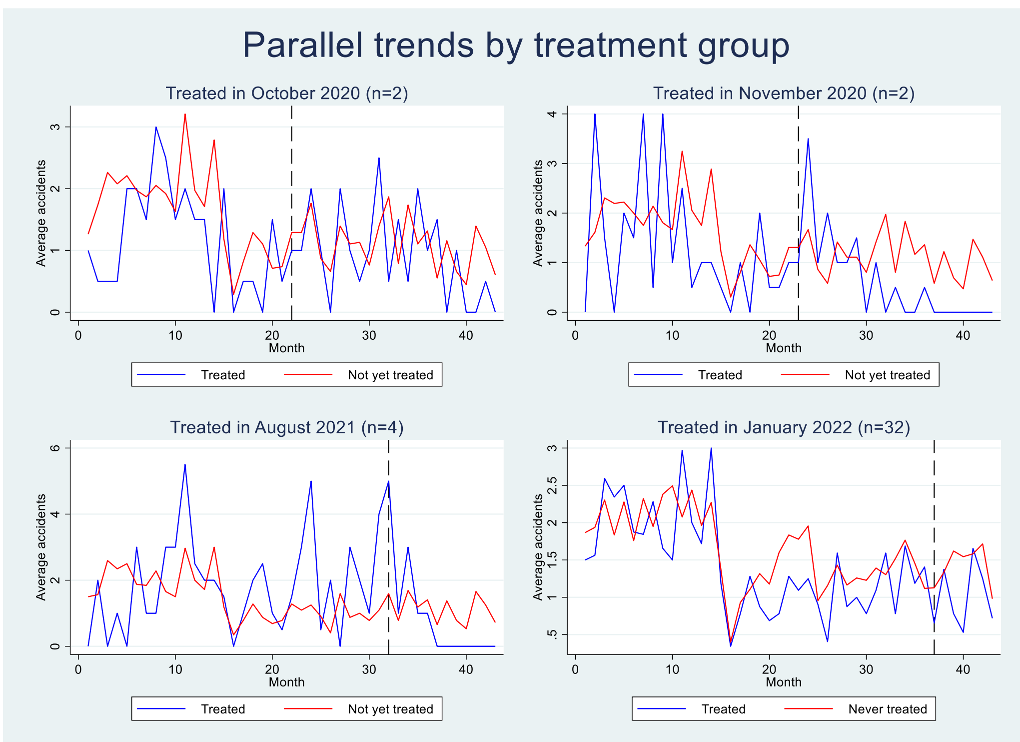

Figure 8. Trends by treatment group. (For the last treated group, the never treated are used as comparison)

This guarantees that both the general and heterogeeneous effect of the program defined in equation 9 identify a causal effect. I start by exploring Hypothesis 1[23]. As it is shown in Table 9 the general effect, that is the average of all marginal effects β_{gt}, is negative and significant at the 1% level for accidents and for serious accidents. There is, in any case, a considerable difference in the magnitude.

It is also clear that the result depend on the non-linearity of the Poisson distribution. Using the estimator by Callaway a Sant 'Anna (2021) or Roth & Sant 'Anna (2022) gives us a non-significant estimator with a similar magnitude. Across all estimation methods the magnitude is considerably higher for all accidents than only for serious ones, but this gap is smaller for the ones that assume linearity. This estimation was also done with the Inverse Hyperbolic of Sine transformation of the dependent variable (not reported). The sign and significance of the estimator does not change, but there is a reduction on the magnitude form columns 1 (from -17.7% to -8.75%) and 2 (from -5.06% to -3.8%)[24], and a smaller reduction on the rest of columns.

	General effect of the Cameras Program					
Dependent Variable	Accidents	Serious Accidents	Accidents (CSA)	Serious Accidents (CSA)	Accidents (RSA)	Serious Accidents (RSA)
	(1)	(2)	(3)	(4)	(5)	(6)
Average marginal effect β_{gt}	-0.506*** (0.048)	-0.115*** (0.031)	-0.205 (0.16)	-0.116 (0.08)	-0.206 (0.15)	-0.111 (0.08)
Average monthly change	-39.7%	-10.8%	-12.9%	-7.3%	-12.9%	-7.0 %
Location Fixed effects	X	X	X	X	X	X
Time Fixed effects	X	X	X	X	X	X
Control Segments (Never treated)	X	X	X	X	X	X
Observations	10406	10406	10406	10406	10406	10406

Table 9. Results of equation (9) are presented for the general effect of the Cameras Program. Columns 2 and 3 present the Callaway a Sant 'Anna (2021) estimator and columns 5 and 6 the estimator by Roth & Sant 'Anna (2022). Not-yet treated and never-treated are used as controls. Errors clustered at the road level.

5.3 Legitimacy variable:

Table 10 sumarizes the value of LL_r for all treated units with speed data for May 2020, the month preceding the start of the program[25]. One should note that for eight the cameras the value posted in the limit is higher than the 99th percentile speed.

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[23] Remember from Hypothesis 1 that I am interested in the general effect. Thus, what I study in this context is a less complex equation that (15) that is comparable to other estimation methods. Results shown in Table 9 columns 1 and 2 correspond to:

$$\log(\mathrm{E}(A_{rt})) = \mu_t + \gamma_r + \sum_{g=g_i}^{G} \sum_{t\geq 0}^{T} \beta_{gt} D_{rt}$$

Where the average of marginal effects β_{gt} will give us the effect of the program not considering its heterogeneity.

[24] Here I use the estimates of the Inverse Hyperbolic of Sine transformation as percentages.

[25] Since there is no speed data for three Cameras, these segments and the corresponding control segments will be dropped for the estimation of equation (9).

This explains numbers higher than 1. Note also that while there is a significante gap between the legitimacy of cameras at the 30 and 50 km/h mark, there is little difference between the 50 and 60 km/h limit, where most of Cameras are located.

	Summary of legitimacy variable			
Legitimacy	All treated segments	Maximum 30 km/h	Maximum 50 km/h	Maximum 60 km/h
Number of segments	81	3	48	50
Average	0.841	0.575	0.833	0.880
Standard deviation	0.149	0.052	0.153	0.118
Lowest value	0.521	0.521	0.608	0.729
Highest value	1.445	0.626	1.445	1.252

Table 10. Average mesaure of Legitimacy for all segments. The 99th percentile (May 2020 speed) is used for Legitimacy. Value shown for the 81 (out of 84) Camera locations with a value for speed.

5.4 Regression results

Table 11 presents the main results for the estimation of heterogeneous effect according to equation (9). It is the most important finding of this paper. The evidences seems to support Hypothesis 2. The best way to interpret the coefficient is to think in terms of the speed at which the limit is set. Increasing the legtimacy of the prescribed behavior, i.e. placing the limit one standard deviation higher (14% higher in percentage), will translate into a 14% reduction on accidents. This coeficient seems to be a lower bound.

My prefered specification is column (1), which include L_r as only source of heterogenity. The estimation is not convergent if more than one form of Legitimacy is included, so $L_r{}^2$, $L_r{}^3$ and $L_r{}^4$ are used in the following columns. If I drop the third term on (15), the general effect, convergence can be achieved by using L_r, $L_r{}^2$and $L_r{}^3$, as presented in column (5). But magnitudes are not credible in this case. Column (6) shows that dropping never treated variables maintains the effect, thus suggesting that the selection of controls is not what is driving results.

	Heterogeneous effect of legitimacy on Cameras Program					
Dependent Variable	Accidents	Accidents	Accidents	Accidents	Accidents	Accidents
	(1)	(2)	(3)	(4)	(5)	(6)
Average marginal effect of $L_r\ \tau_{gt}$	-8.91*** (1.033)				-68.88 (99.16)	-9.28*** (1.28)
Average marginal effect of $L_r^2\ \tau_{gt}$		-5.02*** (0.87)			86.45 (134.66)	
Average marginal effect of $L_r^3\ \tau_{gt}$			-3.91*** (0.65)		-13.77 (36.33)	
Average marginal effect of $L_r^4\ \tau_{gt}$				-3.42*** (0.49)		
Average marginal effect of one sd increase in Legitimacy	-14.3%	-26.1%	-38.1%	-52.9%		-14.3 %
Location Fixed effects	X	X	X	X	X	X
Time Fixed effects	X	X	X	X	X	X
Control Segments (Never treated)	X	X	X	X	X	
Observations	10277	10277	10277	10277	10277	3483

Table 11. Results of equation (9) are presented for speed data (the 99th percentile on May 2020), with legitimacy as source of hetegoenity. Fixed effect at the location level. All errors clustered at the road level.

As I explained when discussing the parallel trends assumption, two of the groups seems to violate the heterogenous trend for this assumption. Table 5 of the Appendix presents the same results when we exclude those groups. Results are essentially the same for accidents.

If the results presented in table 11 are really capturing the heterogeneous effect of legitimacy,then the general effect of the program will be bigger for locations with more legitimacy. In Table 6 of the Appendix the sample is divided by terciles or quintiles of my demeaned legitimacy variable that is working as a source of heterogeneity, and the general effect of the program is measured for accidents. Consistent with Hypothesis 2, the higher the tercile or quintile the bigger the impact of the program. This is true even if these estimations have considerably less power.

Since we assumed a poisson distribution for the distributon of accidentes, the value $e^{(\beta_{gt}+\tau_{gt}\,(L_r-\overline{L_g}))}-1$ gives an idea of the total effect of the program in terms of percentages. Due to the non linearity, the total effect is bigger than the simple adittion of the general and heterogeneous effects. Figure 12 illustrates the central result of this paper, by showing the hypothetical effect of the program if Legitimacy value is decreased one standard deviation, it stays on the mean or is increased one or two standard deviations. It can be seen that an hypotehtical increase of a standard deviation in Legitimacy will translate into a 79% monthly reduction in accidentes, whereas the mean value of Legitimcy only translates into a 39% monthly reduction.

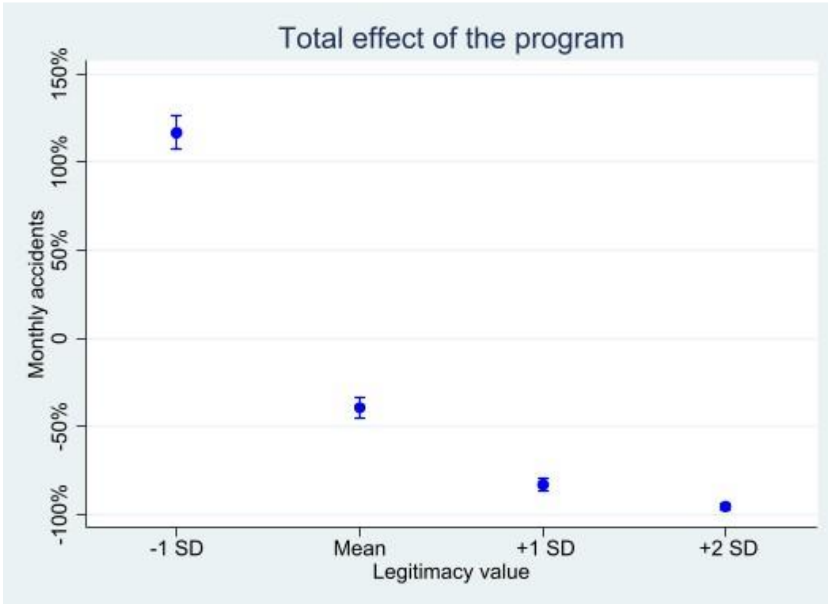

Figure 12. Total effect of the program for different values of Legitimacy. Counterfactual increases of the Legitimacy value (X axis) and the corresponding change in montlhy acidentes (Y axis) that will result.

To check the validity of my results I now discuss some estimations that support my identification strategy. A first possible critique of my estimation strategy is that control units that are located before drivers face a treated segment are affected by treatment, since drivers probably slow down some meters before facing a Camera. If this slowing down process starts more than 500 meters before a Camera, then SUTVA assumption might be violated. Table 7 of the Appendix presents the estimation of equation 9 with only control units that are located in the same direction, but after drivers face a Camera. It is reassuring to show that results barely change (with respecto to column 1 of Table 11), even with the significante reduction of observation units.

A second placebo has to do with the timing of the program. As table 8 of the Appendix shows, if I do a placebo treatment by moving the start of the treatment one or two years back, both the general effect of the program (Hypothesis 1) and heterogeneous effect of Legitimacy (Hypothesis 2) become not statistically significant. The fact that even when treatment is only moved back one year and to the nature of equation 9 many coefficients overlap, is particularly reassuring for my strategy. This is also evidence of the fact that the start date of each camera was as good as random, and did not depend on the level of accident at each location. Another form of this placebo estimation uses the timing at which each camera started treatment. Graph 9 of the Appendix shows the distribution of the p-values of a randomization of the cameras that start each month, preserving the same number of cameras that start treament each month. Even if this evidence is not particularly strong, a significant effect like the one I found is only present in only 20 percent of this random exercises.

A third element worht checking is the fact that the finding does not depend heavilty in any particular unit. For this I perform a “leave one out” excerise, where equation 9 its calculated iteratively droping a single unit and its controls. As Figure 10 of the Appendix shows there is little change in magnitude of dropping all but one of the units. If one of the units is not used, there is a dramatical reduction on our estimator, but the sign of the effect is still consistent with my hypothesis. All this points to the program having stronger effect is some units, as it is natural, but not depending on a few units to find an effect of legitimacy on deterrence.

5.5 Functional form and other robustnes checks

One could argue that what matters is not the speed limit, but previous speed itself. A simple robustness check on the results is to measure the effect of reference speed on itself, and not with relation to reference speed. This can be done by defining Legticmacy with a differente functional form. We could define it as:

$$LL_r = \frac{1}{Observed\ speed\ at\ segment\ r}$$

If results are really capturing an important dimension, one would expect the coeficient to be not significant. That is precisely what we find in column 1 of Table 13. Even if the magnitude is similar, the precision is considerabley smaller, specially when using the full set of controls (not reported). This helps us think that something relevant besides customary speed is being captured by my measure of legitimacy. An alternative functional form that includes other possible relevant dimensions of Legitimacy is explored in the second section of the Appendix.

There are some additional checks we can suggest to see that results shown in Table 13 are really capturing the effect of legitimacy. We go back to our initial functional form for Legitimacy and introduce some variations. First, we use a different percentile of the distribution of speed before the treatment started. Column 2 of Table 13 uses percentile 97th as the reference speed. Another check is to use data for different months, also prior to the start of the program. Columns 3 uses data for December 2019. A last check can be done using a different radio for road segments. Instead of a 300-meter segments, these are now built as 500-meter segments. This is shown in column 4.

	Heterogeneous effect of legitimacy, robustness checks			
Dependent Variable	Accidents	Accidents	Accidents	Accidents
	(1)	(2)	(3)	(4)
Difference with main specification	Functional form	97th Percentile speed data	December 2019 speed data	500 mt segment
Average marginal effect of $L_r\ \tau_{gt}$	-842.62 (645.13)	-7.98*** (1.63)	-7.58*** (3.58)	-2.43 (2.14)
Average marginal effect of one sd increase in Legitimacy		-14.2%	-14.1%	
Location Fixed effects	X	X	X	X
Time Fixed effects	X	X	X	X
Control Segments (Never treated)	X	X	X	X
Observations	10277	10277	10277	3483

Table 13. Results of equation (9) are presented for speed data on May 2020, with legitimacy as independent variable. For Column 2 the 97th percentile is used for Legitimacy. For Column 3 Speed Data for December 2019 is used for Legitimacy. For Column 4 The 99th percentile is used for Legitimacy varible, with road segments of 500 meters. All errors are clustered at the road level.

Results seem to be stable but sensible to road segment size. In particular, they do not seem to depend on using a particularly extreme value of the distribution, as an even bigger magnitude of the coefficient is found when using percentile 97th as the reference speed. Columns 3 also shows that using data for a different month for the Legitimacy variable only increases the magnitude of the coefficient. One should note that result for October 2019 (not reported) are contrary to my hypothesis, while an average of these three months shows essentially the same results. This points to October 2019 being an outlier.

On the contrary, increasing the dimension of road segments reduces the magnitude of the effect significantly with a small loss in precision. In fact, with a bigger treatment unit the result for serious accidents (not reported) is no longer significant. One should note, also, that the general effect of the program seems to be bigger in this scenario.

5.6 Is it really legitimacy? Some checks on causality.

In this section I explore if legitimacy is really behind the results. First I compare the effect of the program during day and night hours. Then I examine if legitimacy plays an heterogeneous effect on accidents that occur in hours of the day where speeds are on average faster. Then I look at hours where historically accidents have been more prevalent. Finally I look at rainy days as another form of verifying if it is really legitimacy that explains results.

The fact that cameras only give tickets during the day can help us in verifying Hypothesis 2. It is still questionable how much drivers know this fact, since anecdotal evidence says that most drivers in Bogotá are not aware of the possibility of exceeding speed limits at nighttime even under Cameras. One can even imagine that the same driver that has gotten a ticket during the day gets to decide if she accepts the legal limit or not when driving at night. If Hypothesis 2 is correct, and we assume drivers are aware that they are not going to receive a fine during nighttime hours, one would expect legitimacy to only play a role during the day. Note that of all accidents occurring in the city between 65% and 69% of all accidents occur during the day. As Table 9 of the Appendix shows this is similar for control and treatment units. This even if, as seen in Figure 11 to 13 of the Appendix, for the whole of Bogotá there are differences in the tendency and number of accidents during the day and night.

Table 14 presents the general and heterogenous effect of the Cameras Program separating the effect of accidents that occur during daytime and nighttime. I should note that for most of the analysis in the chapter if I include the general effect causes, the equation does not converge, so I present the coefficient for equation (9) without the general effect. As it is always the case, this coefficient is lower than when general effect is included. It is clear that the Cameras Program has both a bigger general and heterogeneous effect during the daytime. This difference in magnitude is considerably higher for the heterogeneous than the general effect. In fact, heterogeneity is three times as big during daytime than nighttime.

General and heterogeneous effect, Daytime and nightime

Dependent Variable	Accidents Daytime	Accidents Nighttime
	(1)	(2)
Average marginal effect β_{gt}	-0.115** (0.058)	-0.081** (0.034)
Average marginal effect of LL τ_{gt}	-1.580*** (0.4326)	-0.516** (0.2384)
Location Fixed effects	X	X
Time Fixed effects	X	X
Control Segments (Never treated)	X	X
Total accidents in sample	10744	4991
Observation units	10277	10277

Table 14. Results of equation (9) are presented for speed data on May 2020. The 99th percentile is used as reference speed. Daytime hours defined as between 6 am and 6pm and nighttime hours defined as between 7 pm and 5 am. All errors are clustered at the road level.

I now turn to speed during daytime, where I can be sure that all drivers are aware of the possibility of receiving a fine. Remember from our model that we assumed speed as a mediatior between legitimacy and accidents. If Hypothesis 2 is correct one would expect the limit to be binding only when traffic is fluid. If traffic is too congested for drivers to drive above speed limit, then legitimacy should play no role in a driver's decision. On the contrary, when traffic is fluid and drivers could make the decision to speed, one would expect the heterogeneous effect of legitimacy to be bigger. In short, the slower the drivers are already forced to drive the less important the program and legitimacy should be. Table 15 presents the result of that anaylisis. Daytime hours are divided in three quantiles over their 99th percentile speed all across the city. If an hour of the day is in the upper (lower) quantile is considered a high (low) speed hour. Results are slightly in line with Hypothesis 2. The heterogeneous effect of legitimacy is bigger on the hours of the day where drivers are more likely to speed, but the difference is minor. In fact, if only data for segments where treatment and control segments are located is used for dividing hours in quintiles, then there is no difference in the effect of legitimacy. One could argue that the number of drivers in the street for each hour is different, and thus that the relevant unit is congestion. While it will be interesting to have congestion measures, the relevant unit for how binding is the speed limit is the unadjusted distribution of speed.

If we look at hours of the day, there seems to be a very weak correlation between any measure of speed and the number of acciddents. This can be seen in Figure 14 of the Appendix. An interesting exercise is thus to analyze if the Cameras program has a bigger effect on hours where accidents were already more prevalent[26]. When dividing hours in terciles not by average speed but by the number of accidents occuring, the result is much more robust. Columns 3 and 4 divide hours of the day accoring to the frequency of accidents. In this case there seems a much bigger effect for hours where accdidents where already more prevalent. In the next section I will try to make sense of all this puzzling results. Results for serious accidents are shown in Table 11 of the Annex. Differences are esentially the same but magnitudes are considerably smaller and no heterogeneous effect is found if dividing hours according to the number of serious accidents in each hour. All this points at legitimacy playing a considerably smaller role on serious accidents.

[26] One should also note that higher speed and the most accident-prone hours show very little change from 2019 to 2022.

Heterogeneous effect of legitimacy, hours and rain days				
Dependent Variable	Accidents Tercile 1 Speed/hour	Accidents Tercile 3 Speed/hour	Accidents Tercile 1 Accidents/hour	Accidents Tercile 3 Accidents/hour
	(1)	(2)	(3)	(4)
Average marginal effect of LL τ_{gt}	-0.5846*	-0.8169**	0.145	-1.092***
	(0.34)	(0.337)	(0.213)	(0.371)
Average monthly reduction of one sd	-6.1%	-7.8%		-9.3%
Hours of the day	(11,12,16,17,18)	(6,7,13,14)	(6,8,9,10,18)	(7,13,14)
Location Fixed effects	X	X	X	X
Time Fixed effects	X	X	X	X
Control Segments (Never treated)	X	X	X	X
Accidents in sample	4382	3690	3947	2966
Observations	10277	10277	10277	10277

Table 15. Results of equation (9) are presented for speed data on May 2020. The 99th percentile is used as reference speed. Hours are divided in terciles depending on the average speed or the total number of accidents. All errors are clustered at the road level.

Another alternative for evaluating Hypothesis 2 is looking at rainy days in the city. Since rainy days force drivers to slow down, speed limits will also be less binding on such days. One would expect then that there will be a bigger effect for legitimacy on dry days compared to rainy days where drivers are already following the speed limit.

An econometric challenge arises when comparing months with different amounts of rain. I only have data for rain across all the city[27], so I divide the sample according to toal precipitation for each day. Days are classified as dry day, rain day or heavy rain days, and the full sample of days is divided according to each category. Since my time unit is months but the amount of rain days varies across months and years, a modified dependent variable is used. In this case the dependent variable is not the number of accidents at road segment *r* during month *t,* but the number of accidents per (dry, rainy or heavy rain) day at road segment *r* during month *t.* This is now a continuous variable but one in which the high number of zeros have a meaning. Thus, Poisson estimation is still the correct model for the data.

Table 16 presents results for this exercise. In this case results are contrary to Hypothesys 2 in the sense that the more rain there is, the biggest the effect of legitimacy. In fact there is a diffeence in the effect of legitimacy when considering rainy days and those days where rain is heavier.

[27] Rain data was downloaded from https://app.climateengine.org/climateengine using Daily precipitation data from the CHIRPS database from January 1, 2019, to July 31, 2022, for all the city of Bogotá. Data was retrieved on August 10, 2023.

Heterogeneous effect of legitimacy, hours and rain days			
Dependent Variable	Accidents/day No rain	Accidents/day Some rain	Accidents/day Heavy rain
	(1)	(2)	(3)
Average marginal effect of LL τ_{gt}	-0.028	-0.098***	-0.211**
	(0.027)	(0.032)	(0.087)
Average monthly reduction of one sd		-1.3%	-2.7%
Location Fixed effects	X	X	X
Time Fixed effects	X	X	X
Control Segments (Never treated)	X	X	X
Accidents in sample	5649	5813	2904
Observations	10277	10277	10277

Table 16. Results of equation (9) are presented for speed data on May 2020. The 99th percentile is used as reference speed. Dependent variables is number of accidents per (dry, rain, heavy rain) day per month. Days are classified acording to total precipitation (0,0 for dry days, >0,0 for rain days, and over 75th percentile for heavy rain days). All errors are clustered at the road level.

In summary, the analysis in this section has shown us that legitimacy plays a bigger role during the day (when fines are present) than during the night (when no fines exist). It has also shown that there is little difference on the effect of legitimacy on hours of the day where average speed is faster, but there is a bigger effect of legitimacy in hours of the day where more accidents takes place both before and after the program. Finally legitimacy plays a bigger role on deterring accidents in days where there is some rain, and a bigger role when rain is heavier.

6. Possible behavioral mechanisms

Even if one accepts the findings that legitimacy, broadly defined, has an effect on deterrence there is still a question of the behavioral mechanism in play. The theoretical model alone is not sufficient to explain why legitimacy is important for drivers. I now offer some tentative interpretations of the findings in the last section, under the assumption that Hypothesis 2 is in general validated.

I interpret the difference in general and heterogeneous effect between daytime and nighttime as proof that legitimacy plays a role when a fine is possible. In that sense it is unlikely that drivers are under some pleasure of agency mechanism, by which lowering their speed has an effect on the behavior of others. On the other hand, it could be the result of self-image, a significant number of individuals want to consider themselves law-abiding citizens, and thus reduce their speed when they are reminded that they are breaking the law. It could also be public-image: individuals want to be seen by others as law abiding citizens, and thus they reduce their speed as long as others are watching them break the law. In short, legitimacy is important but only when there is sufficient probability of really receiving a fine.

The result on faster hours it is not by itself a proof that legitimacy does not play a role. If this where the case, we will not have the results on accident-prone hours or heavy rain days. What the data seems to show is that speed is not as determinant for accidents as it is commonly assumed. A first interpretation is that the program works not by reducing the distribution of speed but by changing other behaviors of drivers that play a role in accidentality.

A second interpretation has to do with selection, in the line of Goncalves & Mello (2022). The fact that legitimacy plays a bigger role on the times of the day when most accident occur might mean that the type of drivers that circulate at different times of the day are what explains the differential effect of legitimacy. Some drivers circulate at low speed, are prone to accidents but are more deterrable, and more so when the mandated behavior is legitimate. Other drivers circulate at higher speeds, are less prone to accidents but are less deterrable, and don't really take legitimacy of sanctions into account when deciding whether or not to obey traffic limits. One has to be careful of stereotyping, but natural candidates for this "hard to deter" population are drivers of motorcycles, a growing number in all Colombian cities[28]. This explanation also fits the general finding that across all specifications both Hypotheis 1 and Hypothesis 2 are less important when considering serious accidents.

Note that both explanations are complementary if we consider that the program tells drivers to drive slower, but they interpret it as a message to be more cautious. The more legitimate the message the more cautious drivers become. And the message is more effective on those who where less cautious (more accident prone), and has little or no effect on those that drive faster but are not accident prone. This version can help us make sense of the findings on rain. When drivers are forced to be cautious by road conditions, the Cameras reinforce this message. The more rain, the more cautious drivers already are so the program has a bigger effect.

Naturally, one could also think that these results point at the fact that legitimacy is less of a factor than what I have been arguing. The expected cost of a ticket is what is important, and what I think is captured as legitimacy is just a function of the risk of being punished for speeding. I do not think that this is a particularly useful reading of the results, and does not fully explain the previous section, but it is still a valid interpretation.

[28] Another source of heterogeneity worth exploring is the effect of the program depending on the type of vehicles involved in each accident. When I reduce the sample to only accidents where motorcycles are involved, both the general and heterogenous effect of the program are not statistically significant (at the 5% level). On the contrary, when the sample includes only accidents where no motorcycles are involved both the general and heterogeneous effects are negative and statistically significant, albeit smaller than in the full sample. This results are not reported, but available upon request.

7. Discussion

On this paper I examined the role of legitimacy, understood as the acceptance of the behavior prescribed by legal sanctions, on deterrence and behavioral change. This is a pressing question in a wide arrange of settings where States use legal mechanisms in order to not only punish some citizens, but to influence the behavior of all citizens. I worked under the general hypothesis that the sanctions that are more legitimate have a bigger effect on changing behavior. A behavioral model was presented to formalize this intuition. The essential assumption of the model is that after the introduction of a legal sanctions individuals evaluate the norm in comparison to their previous behavior. Citizens are more willing to follow the behavior suggested by the norm if they find the prescribed behavior closer to their previous custom.

I applied this behavioral model to a speed-reduction policy that was implemented to reduce road accidents: the *Cámaras Salvavidas* Program in Bogotá, Colombia. I presented the hypothesis that the introduction of Cameras results in a bigger reduction of traffic accidents on spots where legitimacy of the legal limit is higher. One general result and two specific findings are worth discussing.

The general result is that legitimacy did play a role in the deterrence of speed-control sanctions. Using different functional forms, the same result was found: accidents reduced further where legal sanctions where more legitimate. On my preferred specification getting the legal limit one standard deviation closer translates into a 14% reduction of average monthly accidents. Due to the fact that there is little identification power in the data, this seems to be a lower bound.

The most interesting finding of the causality checks that I performed is the fact that speed does not seem to be the only mediator on the effect of legitimacy. Speeding sanctions seem to affect behavior through additional behaviors[29]. Another possibility is the existence of selection, whereby legitimacy affect only certain type of drivers that are nor over-speeders but tend to be more accident prone. Both channels point at speeding sanctions acting as calls to drivers to be more cautious, and that call only heard by certain drivers.

The other specific finding worth discussing is the fact that across all estimations both the general and the heterogeneous effect is bigger for all accidents than for serious accidents. Speed-control interventions seem to be effective at reducing overall

[29] This finding seems to be in line with recent research on the Kangaroo Effect. Cameras seem to reduce speed on certain specific spots, but this behavior is compensated in other areas of the city (Valderrama et al., 2023).

accidents, but considerably less effective at reducing injuries and deaths[30]. Other complimentary policies are needed to reduce the human toll of accidents.

All in all, the results support the notion that policies aimed at reducing road accidents need to take previous behaviors and opinions into account. Policy makers need to understand the trade-off between strict measures that are illegitimate and softer measures that are more legitimate. The evidence shown here points to the latter ones having a bigger impact in reducing accidents.

That trade-off is not only present in policies aimed at reducing road accidents but in all cases where states impose sanctions with the goal of changing social behaviors. The results of this paper are worth exploring in other policy areas where a "gentle push"[31] might be better than a "hard shove"(Kahant, 2000).

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[30] This runs contrary to most findings in previous studies about the effect of Speed Cameras (De Pauw et al., 2014). It is worth exploring the reasons behind this, but the high number of motorcycles and the increasing accidentality of them seems a plausible explanation.

[31] Readers might note that in the Kahant paper the word used is *nudge,* not *push*. Recently nudge has gotten a non-punitive meaning that is inconsistent with the sanctions I have been talking about.

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Appendix

1. A model with *desire for speed*:

It is reasonable to assume that drivers care about something more than the social or legal norms surrounding them. Some papers (Banerjee et al., 2019; Dušek & Traxler, 2022) assume drivers obtain utility for speeding above the speed limit. This might be captured by a function $v_i(S_{ir})$, which is concave in S_{ir}. This function $v_i(S_{ir})$ captures the individual tolerance for the risk of having an accident and the perceived importance of not putting others at risk by speeding. This *desire for speed* is what is behind the exploratory speeding present in this literature.

Table 1 (Appendix) summarizes a model of this form of utility. Changes are small in relation to our baseline model. Note that in this scenario there is a smaller effect of legal norms in reducing optimal speed, and thus a bigger chance for backfire.

No regulation scenario (before the *Cameras* program) **Reference speed:** S_r^{rs}			
Utility function	**Maximization**	**Optimal speed**	
$U\left(S_{ir,} S_r^{rs}\right) = v(S_{ir}) \frac{-(S_{ir} - S_r^{rs})^2}{2}$	$\frac{\partial v(S_{ir}^*)}{\partial S_{ir}} = -(S_{ir}^* - S_r^{rs})$	$s_{ir}^{nr*} = s_r^{rs} + \frac{\partial v(S_{ir}^*)}{\partial S_{ir}}$ Optimal speed is higher than the social norm if derivative is positive (it is assumed to be).	
Law regulation scenario (after the *Cameras* program). **First case: some enforcement p$(S_{ir}) > 0$ and legal limit:** $\overline{S_r}$			
Utility function	**Maximization**	**Optimal speed**	**Effect of program**
$U(S_{ir}, S_r^{rs}, \overline{S_r}, L_{ir}) = v(S_{ir}) - \frac{(S_{ir} - S_r^{rs})^2}{2} - q(S_{ir}) - \frac{(S_{ir} - \left(\overline{S_r} + \frac{1}{L_{ir}}\right))^2}{2}$	$-\frac{\partial q\left(S_{ir}^*\right)}{\partial S_{ir}} - (S_{ir} - S_r^{rs}) - (S_{ir} - \frac{\overline{S_r}}{L_{ir}}) = -\frac{\partial v\left(S_{ir}^*\right)}{\partial S_{ir}}$	$S_{ir}^{lr*} = \frac{S_{ir}^{nr*} + \frac{\overline{S_r}}{L_{ir}} - \frac{\partial q(S_{ir}^*)}{\partial S_{ir}} + \frac{\partial v(S_{ir}^*)}{\partial S_{ir}}}{2}$	$S_{ir}^{nr*} > S_{ir}^{lr*}$ if $L_{ir} > \frac{\overline{S_r}}{S_{ir}^{nr*} + \frac{\partial q\left(S_{ir}^*\right)}{\partial S_{ir}} + \frac{\partial v(S_{ir}^*)}{\partial S_{ir}}}$ (6) Then: $S_{ir}^{nr*} > S_{ir}^{lr*} \approx \overline{s_r}$ Possibility of an increase in speed ("backfire") when: $S_{ir}^{nr*} < S_{ir}^{lr*}$

			if $L_{ir} < \frac{\overline{s_r}}{s_{ir}^{nr*} + \frac{\partial q(S_{ir}^{*})}{\partial s_{ir}} + \frac{\partial v(S_{ir}^{*})}{\partial s_{ir}}}$ (6) Then: $s_{ir}^{lr*} > s_{ir}^{nr*} > \overline{s_r}$

Table 1. Behavioral model and its predictions with a "desire for speed".

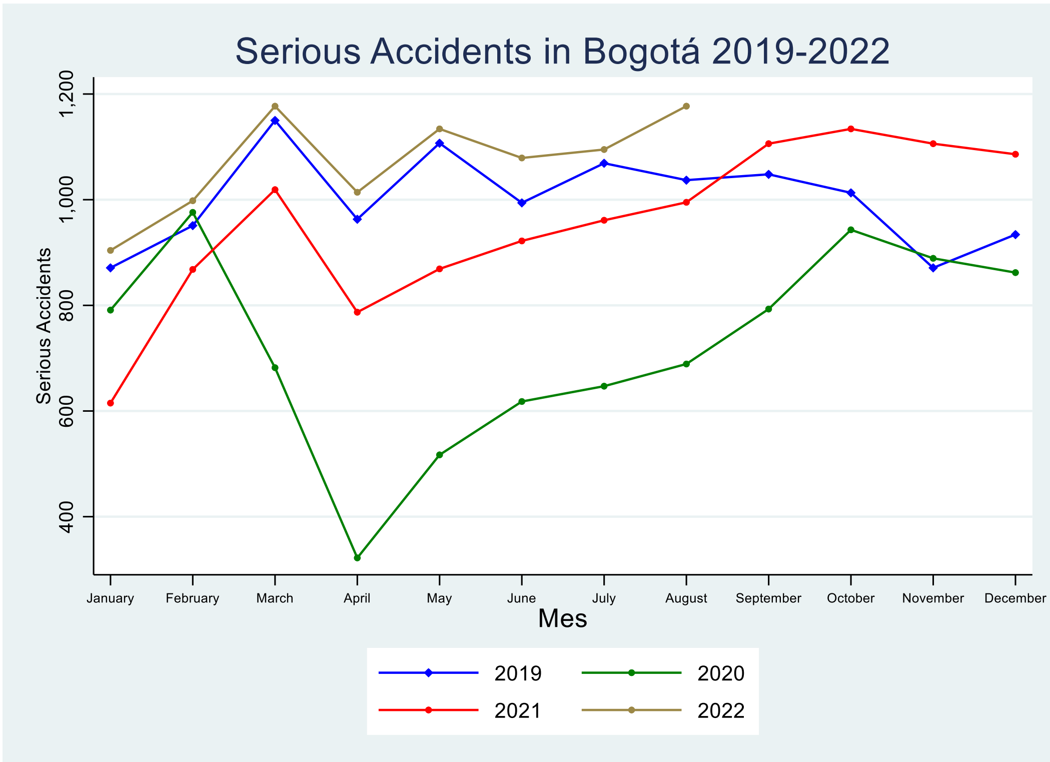

Figure 2. Serious accidents by year and month of occurrence.

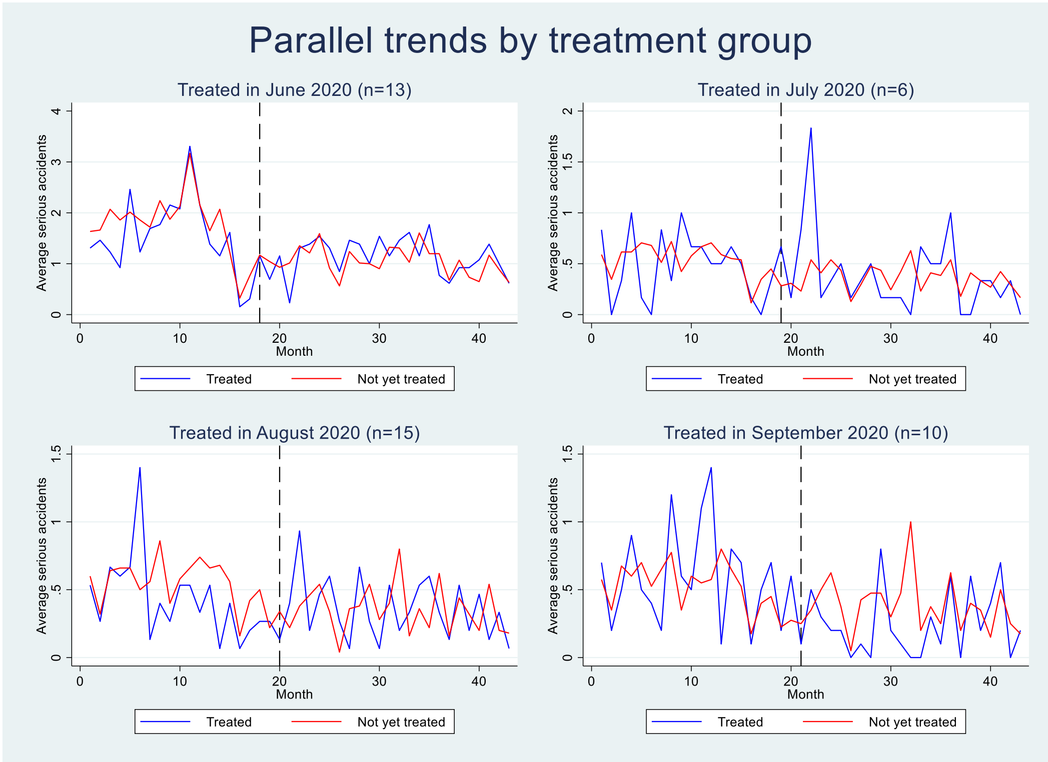

Figure 3. Trends by treatment group.

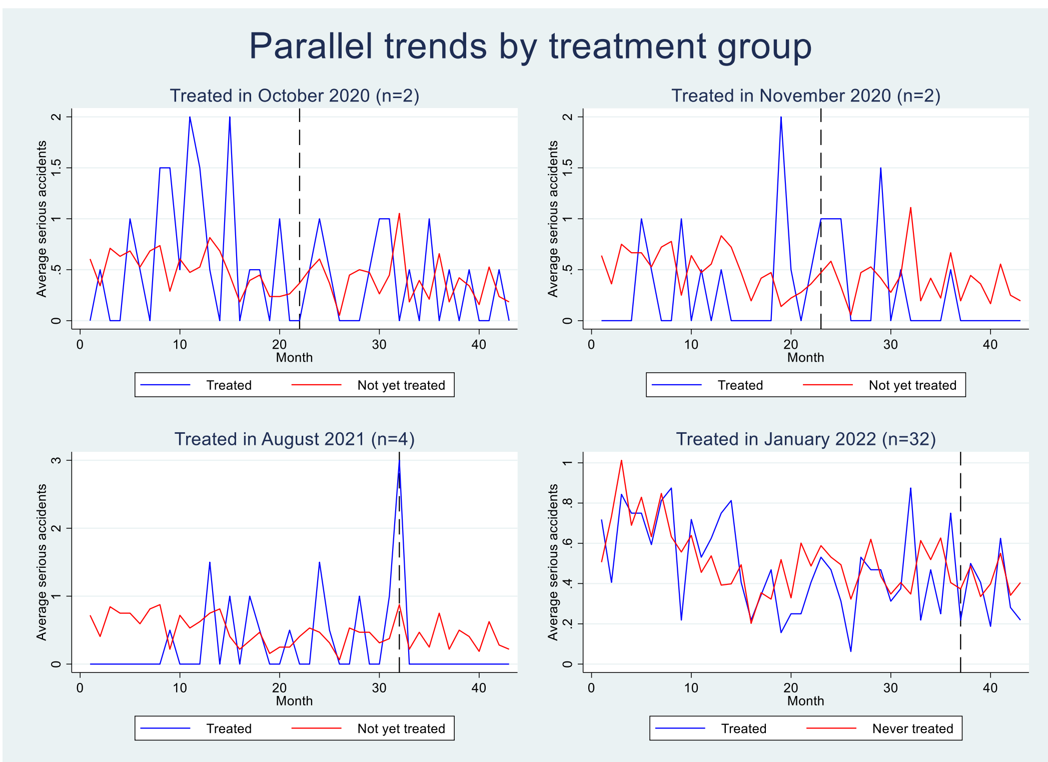

Figure 4. Trends by treatment group. (For the last treated group, the never treated are used as comparison)

Heterogeneous effect using only strict parallel trend groups		
Dependent Variable	Accidents	Accidents
	(1)	(2)
Average marginal effect of LL τ_{gt}	-10.79*** (2.51)	-11.02** (0.92)
Location Fixed effects	X	X
Time Fixed effects	X	X
Control Segments (Never treated)	X	X
Observation units	10277	3483

Table 5. Results of equation (9) are presented for speed data on May 2020, with legitimacy as independent variable. The 99th percentile is used for Legitimacy. All errors are clustered at the road level.

Heterogeneous effect of legitimacy, divided sample				
Dependent Variable	Accidents Tercile 1	Accidents Tercile 3	Accidents Quintile 1	Accidents Quintile 5
	(1)	(2)	(3)	(4)
Average marginal effect of LL τ_{gt}	-0.1326 (1.033)	-0.19504*** (0.108)	0.0114 (0.160)	-0.4250*** (0.151)
Location Fixed effects	X	X	X	X
Time Fixed effects	X	X	X	X
Control Segments (Never treated)	X	X	X	X
Observations	3526	3268	2193	1978

Table 6. Results of equation (9) are presented for the general effect of the Cameras Program with differerent subsamples and accidents as dependente variable. Not-yet treated and never-treated are used as controls.

General and heterogeneous effect, strict SUTVA assumption	
Dependent Variable	Accidents
	(1)
Average marginal effect β_{gt}	-0.195**
	(0.089)
Average marginal effect of LL τ_{gt}	-8.69***
	(3.20)
Location Fixed effects	X
Time Fixed effects	X
Control Segments (Never treated)	X
Observation units	7181

Table 7. Results of equation (9) are presented for the general effect of the Cameras Program with control units built under a strong SUTVA assumption. Column (1) thus has 84 treatment units and 86 control units (there are 158 controls in the standard estimation). Not-yet treated and never-treated are used as controls.

General and heterogeneous effect, Placebo treament		
Dependent Variable	Accidents Treatment 2018	Accidents Treatment 2019
	(1)	(2)
Average marginal effect β_{gt}	-0.076	-0.089
	(0.074)	(0.079)
Average marginal effect of LL τ_{gt}	0.25	-0.093
	(2.11)	(3.62)
Location Fixed effects	X	X
Time Fixed effects	X	X
Control Segments (Never treated)	X	X
Observation units	10320	10320

Table 8. Results of equation (9) are presented for the general effect of the Cameras Program with placebo tratment dates. Column (1) moves the start of treatment from june 2020 to june 2018, column (2) moves the start of treatment to june 2019. Not-yet treated and never-treated are used as controls.

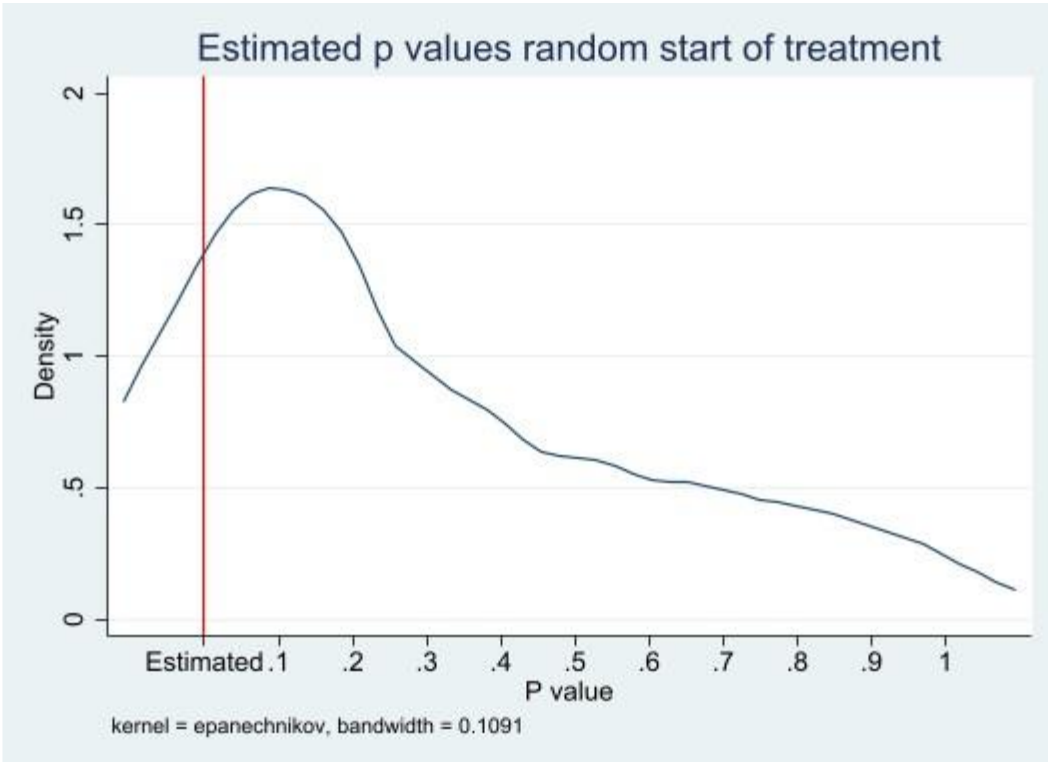

Figure 9. Results of equation (9) are presented for the general effect of the Cameras Program with placebo tratment dates. The number of cameras that start treatment each month is preserved with respect to the real program, but the location of each camera is randomized.

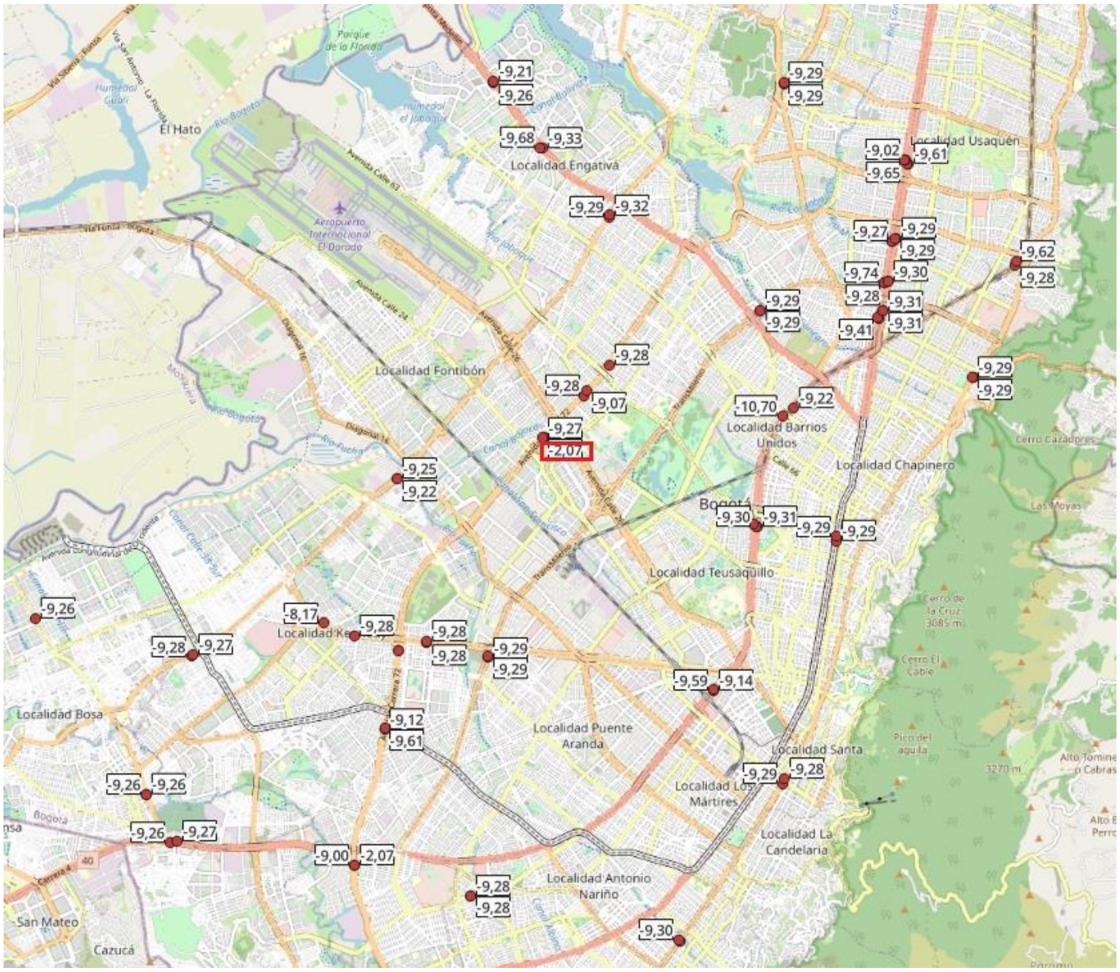

Figure 10. Equation 9 is iteratively estimated, dropping observations one at a time. Heterogeneous effect of Legitimacy is shown, with the number shown at each location the effect of the program without that specific locations. One should not that all coefficients are significant, except for the one on the red box.

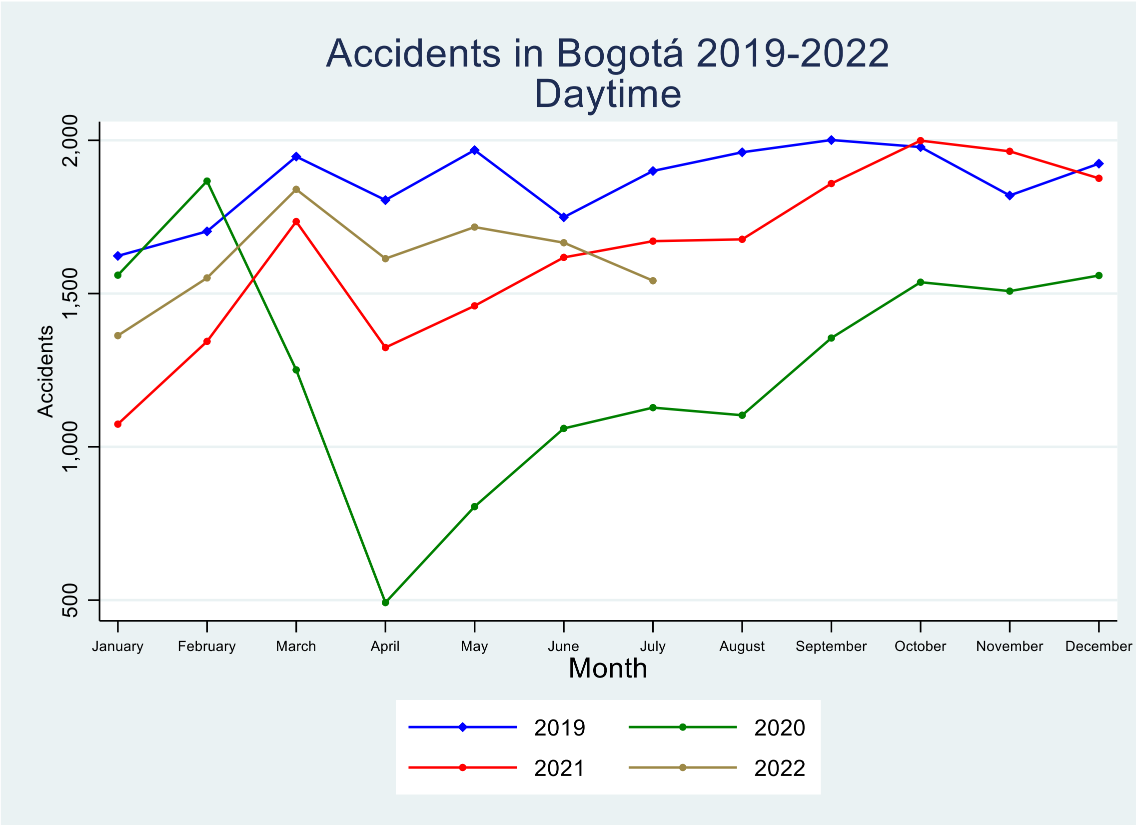

Figure 11. Accidents by month and year of occurrence (Daytime)

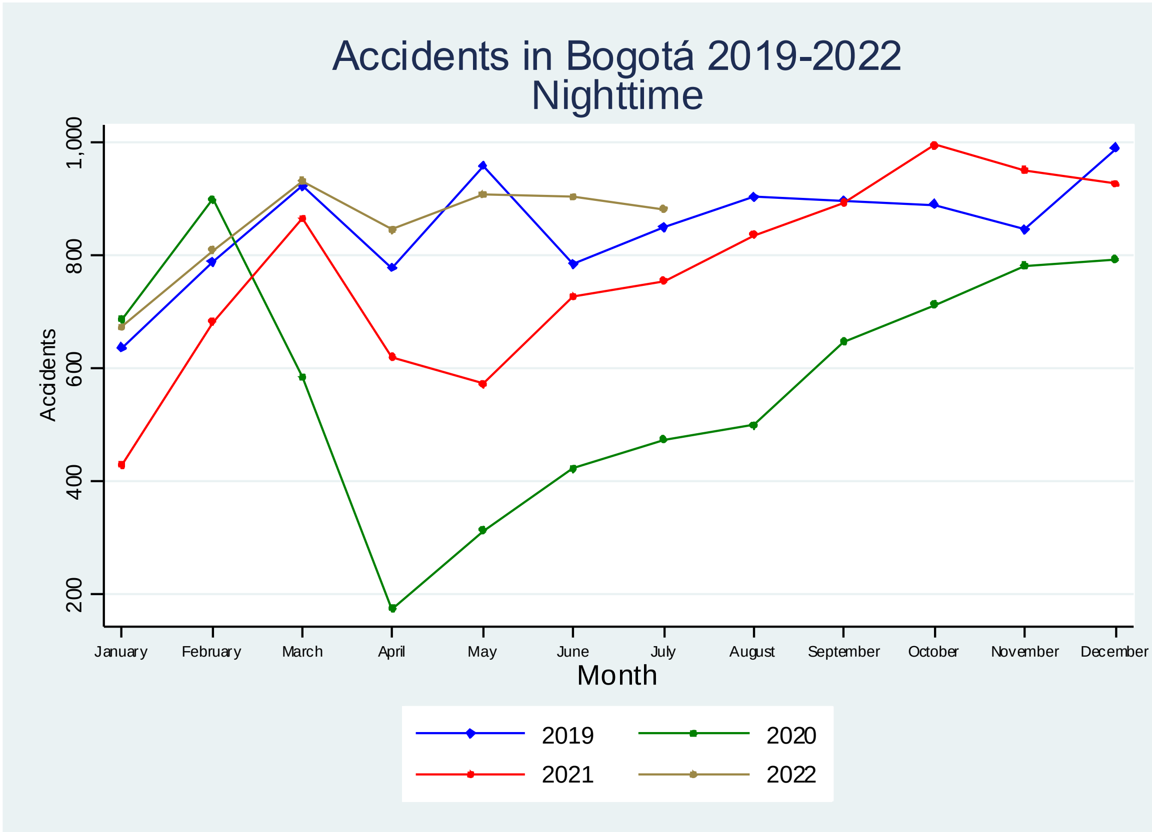

Figure 12. Accidents by month and year of occurrence (Nighttime)

Accidents occurring daytime and nighttime			
	Full Sample	Daytime	Nighttime
Accidents	15735	10744 (65%)	4991 (35%)
Serious accidents	5016	3120 (62%)	1896 (38%)

Table 13. Distribution of accidents between daytime and nighttime.

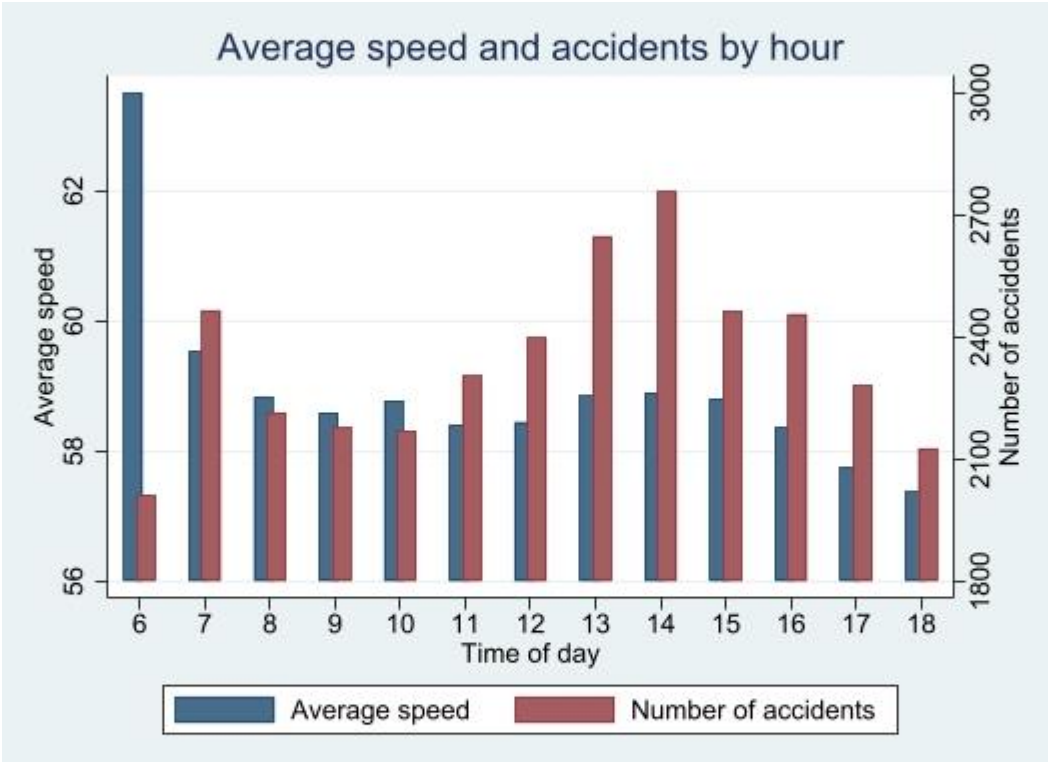

Figure 14. Average speed and accidents by hour of the day. Data from January 2019 to May 2020 used, only for roads where Cameras are located.

2. An alternative form of Legitimacy

According to our model drivers compare the behavior prescribed by the norm with their social custom. In an alternate definition of Legitimacy drivers will also evaluate the new legal limit with respect to how important they perceive the State's regulation of speed to be. This is captured by SR_{ir}, which measure State's Regulation acceptance is a function of some measure of how much the individual perceives speeding behavior as an area where the government should intervene. I am modelling the feeling some drivers have that, *"it is necessary for the State to punish over speeders"* or its opposite "*the government should not punish drivers for speeding, while roads are full of potholes (or traffic is slow, or some people drive without mandatory insurance, etc.)*".

I also need to operationalize a measure of $SR_{ir.}$ As it was mentioned above, I have survey data for creating a measure of support of the government at the locality level. The *Encuesta de Percepción de Riesgo Vial* (Road Risk Perception Survey) was done both in 2018 and 2019 by the city of Bogotá. I will use the questions that more closely measure how supportive a person is on the intervention of the State in issues related to speeding. As a Survey this is far from perfect, but it is the only data available for at least some areas of the city.

All questions are codified as categorical variables with higher values indicating a higher number in open questions or more support for each statement. The assumption I am working with is that a higher average number on open questions or more support for statements all signal a smaller value of SR_{ir}. The following four questions will be used, and an average of the answers at the locality where each segment with a Camera is located will be used.

Open questions:

- How many people do you think died in traffic accidents last year?
- In general, how often do drives exceed speed limits in highways?
- ¿Do you think the reduction of speed limits has contributed to the reduction of road accidents?

Support for statements:

- How important are the following solutions for road safety: to install more speed sensors in the city.

Full wording of questions is presented in Table 15.

$$SR_{rl} = Support\ for\ governemnt\ interventions\ at\ segment\ r\ in\ locality\ l$$

Social norms on the State's intervention on speeding behavior.

Question	**Coding**
How many people do you think died in traffic accidents last year?	1 Less than 100 2 Between 101 and 400 3 Between 401 and 700 4 Between 701 and 1000 5 Between 1001 and 2000 6 More than 2000
In general, how often do drives exceed speed limits in highways?	1 Never 2 Rarely 3 Frequently 4 Always
¿Do you think the reduction of speed limits has contributed to the reduction of road accidents?	1 Not at all 2 A little 3 Somehow 4 A lot
How important are the following solutions for road safety: To install more speed sensors in the city.	1 Not important 2 A bit important 3 Moderately important 4 Important 5 Very Important

Table 15. Full wording to questions on social norms.

5.4 State Regulation acceptance

State's regulation acceptance is measured at the locality level. Since I don't have individual data, the rather strong assumption I am working with is that drivers perceive the local norms when driving through a Camera. Cameras are located in 16 of 20 localities of Bogotá. There is little relation between the location of each Camera and its starting date. Only for one of the treatment groups (August 2021, with 4 cameras) all cameras are located in the same locality.

For measuring State's regulation acceptance, the average of the four dimensions will be used. At the locality level this measure is positive and highly correlated with all of the dimensions of local social norms. Figure 16 shows the value at the locality level for the 16 localities that have at least one Camera.

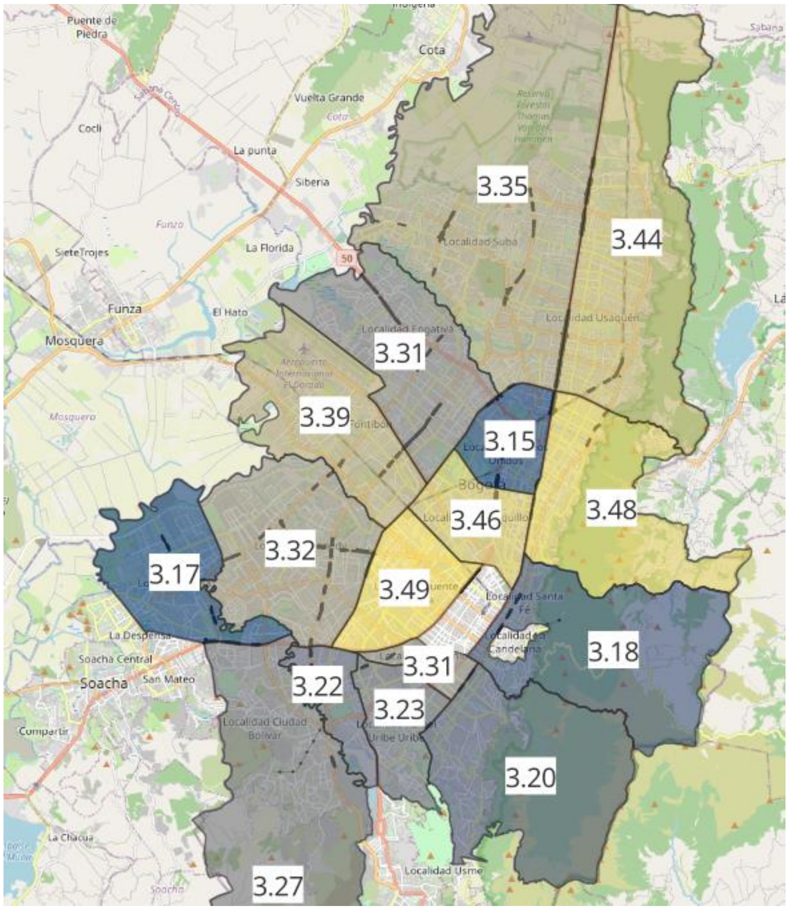

Figure 16. State's regulation acceptance dimension. Value for the State's regulation acceptance dimension is shown at the locality (*Localidad*) level, with dark blue as the lower value and yellow as the higher value. Table 6 of the Appendix shows the number of Cameras at each locality.

5.5 Adjusted Legitimacy variable

Following this logic I can build an Adjusted legitimacy variable that takes into account State's regulation acceptance.

$$Adjusted\ LL_r = \frac{Legal\ limit\ at\ segment\ r}{Observed\ speed\ at\ segment\ r} * Support\ for\ governemnt\ interventions\ at\ segment\ r\ in\ locality\ l$$

$$Adjusted\ LL_r = \frac{\overline{S_r}}{S_r^{rs}} * SR_{rl}$$

After combining both dimensions into one single variable we have a more robust measure of the comparisson between the new legal limit and the existing social norms at each location at the onset of the program. Table 17 summarizes the values for his alterantive Legitimacy variable. Note that since the average of State's regualtion acceptance goes from 3.155 to 3.55, the exact number will be around three times bigger than the unadjusted Legitimacy variable.

	Summary of Ajusted legitimacy variable			
Legitimacy	All treated segments	Maximum 30 km/h	Maximum 50 km/h	Maximum 60 km/h
Number of segments	81	3	48	50
Average	2.81	1.86	2.77	2.97
Standard deviation	0.50	0.20	0.51	0.38
Lowest value	1.66	1.66	1.95	2.42
Highest value	4.80	2.07	4.80	4.09

Table 17. Average mesaure of legitimacy for all segments. The 99th percentile (May 2020 speed) is used for Legitimacy dimension and average at locality for State's regulation acceptance dimension. Value shown for the 81 (out of 84) Camera locations with a value for speed.

Table 18. presents the result of the estimation of equation (9) with this alternative definition of Legitimacy. Columns (1) and (6) of Table 11 are reproduced. Note that the result is essentially the same. Although this adjusted legitimacy is harder to interpret, magnitude seems to be larger.

Heterogeneous effect of legitimacy on Cameras Program Adjusted Legitimacy		
Dependent Variable	Accidents	Accidents
	(1)	(2)
Average marginal effect of $AL_r\ \tau_{gt}$	-3.44*** (0.45)	-3.71*** (0.12)
Average marginal effect of one sd increase in Legitimacy	46.7%	47.5%
Location Fixed effects	X	X
Time Fixed effects	X	X
Control Segments (Never treated)	X	
Observations	10277	3483

Table 18. Results of equation (9) are presented for speed data (the 99th percentile on May 2020), with legitimacy as source of hetegoenity. Fixed effect at the location level. All errors clustered at the road level.