# Experience with Punishment and Specific Deterrence: Evidence from Speeding Tickets

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**Abstract**

This paper analyzes how subjective experiences of being punished affect subsequent offending behavior. The context of our study is the enforcement of speeding tickets through speeding radars in a suburb of Prague. Using unique data that cover anonymized individual driving and ticketing histories of more than 170,000 car owners over 15 months, we evaluate drivers' responses to receiving a speeding ticket. In addition to evaluating the impact from the ticket, we ask whether the magnitude of any deterrent effect depends on the *delay* between the offense and the actual punishment. In answering these questions, we exploit high-frequency data and several quasi-experimental strategies that allow us to identify causal effects. We find evidence on a strong specific deterrence effect of receiving a ticket. Drivers reduce their speeding immediately after they receive the ticket and the reduction is sustained over several months. Moreover, we show that the effects are more than two times larger if the ticket is received within one month of the violation than with a longer delay. The results thus provide an original evidence on the role of celerity of punishment in specific deterrence.

**JEL Classification:** K14, K42, D80.

**Keywords:** Specific deterrence; Speeding; Punishment celerity; Re-offense rates; Regression Discontinuity Design.

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# 1 Introduction

The paper contributes to the literature on deterrence and punishment in two ways. First, it estimates the causal effect of the subjective experience of being punished on subsequent offending at the individual level. Second, it estimates how the magnitude of the deterrent effect depends on the delay between the offense and punishment. The context in which these effects are estimated is the enforcement of speeding tickets in a suburb of Prague, using data on individual driving histories from the speeding radars.

The deterrent effect of punishment has been empirically well established (see Chalfin and McCrary (2016) and Durlauf and Nagin (2011) for recent reviews). While a large body of research in the economics of crime has focused on 'general deterrence' – i.e., the deterrent effect from a threat of being punished – relatively little is known about 'specific deterrence' – the way in which personal experience of actually being punished shapes people and potentially deters them from re-offending in the future.

Economists have explored specific deterrence effects based on variation in the severity of punishment: variation in prison conditions (Chen and Shapiro 2007; Drago et al 2011) and the length of imprisonment (Ganong 2012; Maurine and Ouss 2009). DiTella and Shargrodsky (2013) compare criminal recidivism after imprisonment or electronic monitoring at home. Overall, the evidence suggests that specific deterrence effects from intensive margin variation in imprisonment are, at best, small and often seem to be dominated by criminogenic effects, e.g., through peer effects (Bayer et al 2009).

One important point which cannot be addressed in these studies is the question whether there is any specific deterrence effect from punishment *at the extensive margin*: compared to the case, where an offender remains unpunished, does detection and punishment induce any deterrent impact (beyond a potential incapacitation effect) on the future probability of re-offending? The criminological studies on the effect of imprisonment on recidivism (see Cullen, Jonson and Nagin (2010) for a recent review) suffer from the difficulties of identifying the causal effects of being imprisoned, all else equal, and the effects are again inevitably contaminated by the criminogenic effects of prisons. We try to address this issue in the context of traffic law violations.

We study individual-level reactions to receiving a speeding ticket using a very detailed, high-frequency data. We use a complete universe of measurements from speeding radars installed in a suburb of Prague. The data allows observing full driving history of each car (identified by an anonymous code to preserve privacy), both when speeding and also when driving below the speed limit. It also contains information on the dates when the driver was sent and then received the speeding ticket. We estimate the drivers' responses to the subjective experience of receiving the ticket in two ways. First, we use a regression discontinuity design that exploits the fact that speeding tickets are sent to drivers who exceed the official speed limit by at least 14 km/h. This *de facto* enforcement threshold has been set internally by the city officials, has never been publicly announced, and we do not observe drivers bunching just below this enforcement threshold. The comparison of the speeding behavior of drivers who received / did not receive the speeding ticket because they were just above / below this enforcement threshold provides a causal estimate of the deterrent effect of receiving the speeding ticket. Second, we estimate the driver's speeding on each drive-through the radar over several weeks before and after receiving the speeding ticket. The dataset allows us to control for a rich set of covariates affecting speed. The deterrent effect is estimated using dummies for each weekly interval preceding and following the delivery of the speeding ticket. We observe very large reduction in speeding immediately after receiving the ticket, and the reduction is fully sustained over a 12-week follow-up period.

Two recent papers studied the specific deterrence effects in the context similar to ours, that is, traffic law violations. Hansen (2015) finds that enhanced current penalties for aggravated drunk driving have a deterrent effect on future drunk driving irrespective of whether they imply enhanced penalties for future drunk driving. Studdert et al (2015) find substantial reductions in the likelihood of re-offending after receiving the ticket in a large sample of traffic violators in Queensland.

Our study also contributes to a second, under-researched question: the role of the celerity of punishment on the subsequent behavioral response. The importance of the temporal proximity of punishment is well established in psychology as a crucial element of child development (Watson 1924, Walters 1964, Aronfreed 1968), fostering the link between the undesirable behavior, punishment and self-control.[1] Economic models predict that a shorter delay between crime and punishment should have a stronger deterrent effect due to discounting (Davis 1988, Listokin 2007).[2] Empirical studies testing this prediction at the aggregate level include Pellegrina (2008) who finds a positive link between court delays and crime rates across Italian regions, and Dusek (2015) who finds a rather weak deterrent effect of a shortening of the criminal procedure in the Czech Republic on burglaries and embezzlements. The empirical research at the individual level, is, to our best knowledge, virtually non-existent. Nagin and Pogarsky (2001) elicit hypothetical responses to driving penalties in a survey. The variation in the celerity of punishment did not predict the hypothetical offending.

We contribute to this literature by studying the impact from delayed punishment in our context. More specifically, we exploit substantial quasi-experimental variation in the timing when speeding tickets are sent out to estimate how drivers' behavioral response vary with the gap between the day of the traffic violation and the day of receiving the ticket. We estimate the reduction in speeding after receiving the ticket as a function of, among other control variables, the dummies for each bi-weekly interval for the length of the delay between the offense and receiving the ticket. We find that the reduction in speeding is two-to-three times larger if the ticket is received within the first month since the offense, compared to the reduction if the ticket is received more than one month after the offense. Instrumental variable estimates which account for endogeneity related to the timing when tickets are actually delivered produce quantitatively very similar results.

The remainder of the paper is structured as follows. Section 2 introduces the institutional background of the study. Section 3 describes our data. In Section 4 we present our empirical strategies as well as our results: We implement an RD design to identify the specific deterrence effect from receiving a ticket (4.1); we evaluate short-run behavioral response patterns using high-frequency data in an event study around the day the ticket arrives at the driver (4.2); we exploit quasi-experimental variation in the time gap between the speeding violation and the delivery of the ticket to assess the role of the celerity of

[1] Similar evidence is provided by early animal studies on the effectiveness of punishment administration and delay (Azrin 1960, Church 1969).

[2] For a related study, see Lee and McCrary 2009.

punishment in shaping any deterrence effects (4.3); and we discuss heterogenous effects for different sub-groups of drivers (4.4). Section 5 concludes.

# 2 Institutional background

## 2.1 The radars

Ricany is a residential town of 15,000 inhabitants located just outside the city limits of Prague, the Czech Republic's capital. Several roads link the town to Prague and to smaller residential communities farther away from Prague; the town thus experiences heavy commuter traffic. On an average day, the five radars in the city (which will serve as the basis for our dataset) record 21,500 cars passing through. Not surprisingly, traffic safety is a major concern in the town. Measurements conducted prior to the implementation of the radars suggested that 30% of all cars were exceeding the speed limit, with 6% speeding by more than 10 km/h.[3]

In late 2013, the city council decided to install speeding radars at five locations. All the five locations are major commuter roads, inbound (towards the center of Ricany), and four of them are downhill. The locations for the radars were selected based on safety concerns and sufficient traffic intensity to warrant the cost of the radars. For *all cars* that pass by, the radars measure the average speed on a stretch of a road several hundred meters long. [4] This is important for the safety implications of observing a certain speed. Drivers who are recorded at a lower speed must drive slow over the entire stretch of the road covered by the radar. These radars therefore avoid the pitfalls of the more conventional radar speed guns, where the drivers may avoid being ticketed simply by slowing down just at the point of the radar and speeding again immediately past the radar.

The radars read-off the number plate of every car using an optical character recognition technology. Each morning, all the violations from the previous day are passed in a digital format to the city police. The police officer verifies visually that the number plate on the

[3]Own calculation based on speed measurement data provided by the town of Ricany.

[4]On each stretch, there is one radar at the entry point and one at the exit point. Each radar takes a snapshot of the car at the instant the car crosses the entry/exit mark. The number plates from both snapshot are automatically matched and the average speed is calculated straightforwardly.

snapshot corresponds with the number plate recorded, and excludes legitimately speeding cars such as police and ambulances. After these corrections the violations are then passed to the civilian city administrators for the enforcement of the fines.

Four radars were installed and started measurement in late August 2014. Each radar ran in a test mode for 6 to 8 weeks. During this period, the radars were collecting the standard information but no speeding violations were ever ticketed. The tickets started being sent out between Oct 13 - Nov 13 (different dates for each radar) and they penalized violations commited from Oct 6 - Oct 26 onwards (different dates for each radar). There were delays in obtaining the building permit for the fifth radar, which was put in the test mode on Nov 28, 2014, and violations committed from Dec 6 onwards were ticketed. Due to technical issues, several radars were out of operation for several hours per day or sometimes even for several days during the first 5 months since they were launched. Eventually, these issues were resolved. All radars were producing steady 24-hour measurements since March 2015. This led to an increase in the number of recorded violations, particularly on the busiest (fifth) radar, and a fast accumulation of case backlog at the city administration.

## 2.2 Speeding tickets: fines and enforcement

Penalties for speeding are stepwise increasing in the speed. Table 1 shows the fine structure and the corresponding speeding intervals by the offense severity. Speeding will trigger a small, intermediate or a high penalty, depending on whether the speed is up to 20km/h, 20 to 40km/h, or more than 40km/h above the limit, respectively. In case of a minor speeding violation (which is by far the most frequently observed case of speeding recorded in our data), the owner of the car is sent a ticket and asked to pay a fine of 900 CZK (approx. 36 USD, or 3.5 percent of the average monthly wage). For an intermediate speeding offense, the fine increases to 1900 CZK (approx. 76 USD).

The enforcement procedure, however, is analogous to a plea bargaining process. If the violator pays the stipulated fine, the case ends. The processing of the ticket up to this stage is also highly standardized and automated. For speeding violations up to 40km/h above the limit, however, the car owner may object the ticket.[5] The case then reverts to a more complicated administrative proceeding (aka "trial") in which the actual driver (not necessarily the car owner) has to be proven to be guilty. If the driver is convicted through this administrative proceeding, he is punished by a deduction of demerit points plus a fine which is assessed individually and is typically 2 to 3 times higher than the original fine (see Table 1). The speeding drivers therefore face the same trade-off as defendants in plea bargaining.[6] As we will see below, the vast majority of drivers (around 80%) do pay the fine right away.

Several aspects of the enforcement and speed measurement system will become crucial for our research design. Note first, that the speed limit is 50 km/h at four and 40 km/h at one of the five radars in Ricany. As the penalties are defined relative to the speed limit, we subsequently use a normalized variable which presents all speed measures as the difference relative to the speed limit. Second, one has to distinguish measured and adjusted speed. The measured speed is simply the speed computed by the radar and is measured with a precision of five decimal points. For enforcement purposes, the measured speed is adjusted downwards (in favor of the drivers): the adjustment procedure rounds down the measured speed to the next integer and then subtracts three km/h.[7] The outcome from the adjustment, which serves as a concession to prevent appeals (and generously accounts for possible measurement errors), determines whether the driver has exceeded the speed limit as well as the offense severity according to Table 1. In our data, both measured and adjusted speeds are recorded, and we use the measured speed as it is not confounded by rounding.

Third, in the our case, the speeding is only ticketed if the adjusted speed is at least 11 km/h above the speed limit. In practice, it implies that the actually measured speed has to be at least 14.00 km/h above the speed limit in order to be ticketed. This *de facto* enforcement threshold is not prescribed in any legislation. It has been set internally by

[5]For major speeding violations, the "plea bargaining" stage is not allowed. In this case, the actual driver must be convicted and the driver's license is suspended.

[6]The possibility to object the ticket law is sometimes abused by drivers to avoid the ticket; for example, one popular evasive strategy is based on claiming that the car owner lent the car to another person and giving a fake contact on that person in a faraway country.

[7]For example, a measured speed of 64.62 km is adjusted to 61 km/h. When the measured speed exceeds 104 km/h, four (and not three) km/h are subtracted.

the state police which had the authority to provide a regulatory approval of the radar installations. While the enforcement threshold is stated in the regulatory approval document, it has never been publicly announced in any way. Hence, we do not expect drivers to anticipate this threshold.

# 3 Data

The dataset is a full universe of the drive-throughs recorded by the radars. Upon each drive-through, the radar records the number plate, exact time of entering and exiting the radar zone, as well as the measured and the adjusted speed. The data provided to us has the number plate converted to an anonymous code by a deterministic hashing algorithm. As a result, the dataset contains a full driving history of each individual car, as recorded by the five radars. To emphasize, it contains all drive-throughs irrespective of speed, both above and below the speed limit. It therefore presents a unique opportunity to observe both illegal and legal driving of every driver. This is contrasted to conventional criminal recidivism studies where the former offenders are observed when rearrested or convicted. This feature also differentiates our data from other studies which exploit individual-level traffic violations (Hansen 2015, Studdert 2015). A lack of observations on re-offending in such data could be due to two causes: the former offender has "come clean" or he is simply not there. In the traffic enforcement context, it is possible that occasional drivers and strangers are disproportionately ticketed because they are unfamiliar with local conditions. Observing the full presence of drivers on the road is important for a proper estimation and interpretation of any deterrence effects. In particular, it will allow us to distinguish intensive margin (driving at lower speed) and extensive margin responses (not driving through this area anymore) to speeding tickets.

The second part of the dataset is an anonymized database of the ticket enforcement administration. For each instance of speeding above the enforcement threshold, it contains the same offense information as provided by the radars, and it records every administrative step in the enforcement process. Specifically, it records the day when the ticket was sent out, the mode of sending the ticket (conventional or electronic mail via "databox"[8] ), the date when the addressee received the ticket, subsequent tickets sent in the administrative proceedings if the car owner objected the initial ticket, the amount of the fine prescribed, and the amount and date when the fine was paid. It also contains rudimentary information about the person that was mailed the ticket (the car owner, the driver, or both, depending on the administrative procedures) - whether it is a private owner or a corporation, the municipality and zip code. If a speeding car is exempted from the ticketing system (police cars, ambulances, etc.), this is recorded, too.[9]

The data from the enforcement administration are merged to the radar data; hence for each violation, we observe the subsequent history of ticket enforcement. Some violations are not ticketed because they occurred during the test phase or are still on the backlog. Table 2 presents the basic summary statistics, separately for the full sample and for the subsample of drivers who have ever committed a violation (i.e., were measured at a speed above the enforcement threshold). The full sample contains over 7.5 million observations on more than 6,000,000 drivers. On average, people drive 5.67 km/h below the speed limit; 4.6 percent of all drive-throughs are measured at a speed above the limit. Speeding above the enforcement threshold is rare and occurs in only 3 out of 1000 drive-throughs. During the sample period, 20,248 drivers committed 24,636 enforced violations. The violators are more frequent and faster drivers. They drive above the speed limit 10.1 percent of the time and commit the violation 2.6 percent of the time. When ticketed, the average delay from violation to receiving the ticket was 54 days. Strong majority of drivers are actually punished in the sense that they pay the fine: 79% of the tickets have been paid.

[8]The databox is an e-government platform, akin to email, through which the government sends official documents in electronic forms. All corporations must have the databox and the government sends most of the official correspondence to corporations through the databox. Private persons may choose to have the databox as well but most choose not to. If the violator has a databox, the ticket is always sent through it.

[9]These exempted speeding violations are excluded from our analysis.

# 4 Empirical strategy and results

## 4.1 Regression discontinuity design

In the first part of our empirical analysis we estimate the causal deterrent effect of receiving a speeding ticket on subsequent driving behavior. In doing so, we exploit the discontinuity in the enforcement threshold that generates a large jump in the probability of being ticketed. We argue that the assignment of drivers into the 'treatment group' – i.e., driving slightly more than the publicly unknown enforcement threshold of 14.00 km/h above the speed limit, which will result in a ticket being issued (sooner or later) – and the 'control group' – i.e., speeding, but slightly below the enforcement threshold, which means that the drivers will not receive a ticket – is as good as random.

Our identification approach rests on several institutional features. First and most importantly, the actual enforcement threshold is not publicly known. As pointed out above, the cutoff is not prescribed by any law but was determined by the state police once the new radars started working.[10] People would realistically expect that exceeding the speeding limit by a small amount would be tolerated, but in this case they cannot be certain of the exact level of the tolerated speeding.

Even if drivers are aware of the thresholds (implicitly defined in Table 1), optimizing one's driving speed around a threshold is quite difficult. People first have to account for the adjustment formula (in order to respond to a threshold in terms of measured speed; see Section 2). Moreover, targeting a precise speed level over a driving distance of several hundred meters requires careful concentration (recall the radars' measurement technology, described above). This is further complicated by the fact that speedometers have measurement error, too. Hence, it is extremely difficult for drivers to target a desired speed, even if they attempted to do so.

Overall, the discussion suggests that we should not expect to see drivers' strategically responding to the enforcement threshold. To confront this conjecture with our data, we conducted McCrary's (2008) heaping analysis, which tests the null hypothesis of continuity

[10] This renders the cutoff quite different to those studied in Traxler et al. (2016), who observe bunching responses to well known (and stable) thresholds in the penalty function for speeding in Germany.

in the density distribution around the relevant cutoff. Figure 1 shows the distribution of the *maximum* speed of each car during the 6-month period since the enforcement of the tickets began. Visually, there is clearly no indication of bunching – a fact which is further confirmed in formal tests. Hence, we can reject the case of bunching.[11]

#### 4.1.1 RDD Implementation

Our design answers a specific question: what is the causal effect of being ticketed during a certain time period on the driving behavior during a subsequent period. To approach this question, we implement a regression discontinuity design (RDD) in a simple and straightforward way. The treatment variable $T_i$, defined at the driver level, is a dummy indicating whether a driver received a ticket during a certain period $t_1$. The continuous assignment variable is the maximum measured speed of driver $i$ during the assignment period $t_1$, $s'_i := \max(s_i)$. We then exploit the fuzzy discontinuity in the treatment probability: whenever $s'_i$ is below the enforcement threshold ($s'_i < 14.00$, captured by the enforcement dummy $D_i = 0$), this driver will not receive a speeding ticket: $T_i = 0$. In contrast, drivers with a speed of more that 14.00 km/h above the limit ($s'_i \geq 14.00$, and thus $D_i = 1$) are likely to get a ticket during the period $t_1$.

Exploiting the discontinuity in receiving a ticket, we then study the impact on a variety of outcomes, $Y_i$, during a follow-up period $t_2$:

- *Speeding*, captured by a dummy which indicates if the measured speed of a driver has ever exceeded the official speed limit. This includes both driving that should eventually be ticketed as well as driving that violates the law but is still below the enforcement threshold (and will therefore not be ticketed);

- *Speeding rate*, the fraction of drive-throughs at which the driver's measured speed was above the speed limit;

- *Violation*, another dummy, indicating if the driver drove faster than the enforcement threshold (and was thus eligible for a further ticket);

[11] We have also explored whether drivers might learn about the enforcement cutoff over time. At least during the first 12-month period after tickets were enforced, we do not observe any 'building up' of bunching at the enforcement cutoff.

- *Violation rate*, the fraction of drive-throughs at which the speed exceeded the enforcement threshold;

- *Average* and *90th percentile of the measured speed* among all drive-throughs of driver $i$;

To estimate the effect of speeding tickets on these outcomes, we estimate the following equations:

$$T_i = \delta^T D_i + g^T_+(s'_i)D_i + g^T_-(s'_i)(1 - D_i) + \epsilon^T_i \tag{1}$$

and

$$Y_i = \delta^Y D_i + g^Y_+(s'_i)D_i + g^Y_-(s'_i)(1 - D_i) + \epsilon^Y_i \ , \tag{2}$$

where the dummy $D_i$ indicates if a driver's maximum measured speed $s'_i$ is above the enforcement cutoff, and $g^T_\cdot(s'_i)$, $g^Y_\cdot(s'_i)$ are functions that capture how $T_i$ and $Y_i$ vary with $s'_i$. These functions are allowed to differ below and above the enforcement cutoff and will take up any unobserved factors that jointly vary with the drivers' speed $s'_i$ and influence $Y_i$ or $T_i$, respectively. We will use local linear regressions to non-parametrically estimate these equations.

The key parameter estimated in equations (1) and (2) are $\delta^T$ and $\delta^Y$. Akin to an instrumental variable regression, $\delta^T$ measures the 'first-stage' variation induced by the enforcement cutoff: the discontinuous increase in the probability of receiving a ticket during $t_1$, once the maximum speed surpasses the enforcement cutoff. Equation (2), in contrast, delivers $\delta^Y$, which measures the 'reduced form' effect of the 'treatment' (i.e., the non-zero risk of receiving a speeding ticket) on the outcome during $t_2$. By comparing the first-stage and the reduced form effects, we obtain a Wald estimator for the local average treatment effect from receiving a ticket on $Y_i$:

$$\beta^Y = \delta^Y / \delta^T. \tag{3}$$

In our implementation of this approach, we will consider two alternative assignment periods ($t_1$) – 3-months and 6-months since the first day from which the violations were ticketed (Oct 6, 2014) – as well as two response periods ($t_2$) – a 3- and 6-month window following the initial assignment period.

### 4.1.2 RDD Graphical Evidence

We start out with our analysis by providing graphical evidence. Figure 2 verifies that the probability of receiving a ticket indeed jumps discontinuously at the enforcement threshold. Each dot in the graph represents the mean observation for bins of a 1 km/h width. The graph further depicts fitted lines from local linear regressions (with a bandwidth chosen according to Imbens and Kalyanaraman, 2012) together with the 95% confidence interval. The figure shows that the probability of receiving a ticket after speeding during the first 3-months increases from zero to 38.3 % (i.e., $\delta^T = 0.383$ for $t_1 = 3$months). This relatively low number reflects the fact that a substantial fraction of drivers who were speeding with more than 14km/h above the limit were not ticketed during the period $t_1$. This occurs either because they committed a violation during a period when no tickets were enforced on "their" radar (recall the staged start of ticket enforcement, etc.), or they committed the violation close to the end of the assignment period and have not received their tickets yet.[12]

Figures 3 to 8 depict the reduced-form effect for the key outcome variables. Considering a 3-month assignment ($t_1$) and a 3-month follow-up period ($t_2$), the first figure displays a statistically significant reduction in the probability that a driver is observed speeding (above the legal speed limit): at the cutoff, the probability speeding drops from 65.5 % to 57.5 %. Figure 8 indicates that drivers with a maximum observed speed $s_i'$ just below the enforcement threshold during the assignment period drove 6.1 % of the time above the speed limit during the follow-up period. This percentage falls again significantly, to 3.9 % for drivers just above the enforcement threshold.

Likewise, the probability of committing a violation (driving at a speed above the enforcement threshold) drops from 15.1 to 6.9 %, and the rate of violations from 0.31 to 0.11 % (see Figures 5 and 6). The means speed drops by 1 km/h and the 90th percentile

[12] A further implication from Figure 2 is the fact that the risk of receiving a ticket is *not* increasing in the speed $s_i'$.

speed by 1.8 km/h (Figures 7 and 8). All of these results are robust to different bandwidth choices (i.e., when we apply the procedure from Imbens and Kalyanaraman, 2012, or Calonico et al., 2014).

### 4.1.3 RDD Estimations

Tables 3 to 5 report the Wald estimates from our RDD (equation 3). In addition to $\beta^Y$, the tables present $\delta^T$ and $\delta^Y$ for different outcomes $Y_i$ and for different combinations of the 3-months and 6-months assignment and follow-up periods ($t_1$ and $t_2$). All specifications consider an optimal bandwidth selected according to Imbens and Kalyanaraman (2012). The sample is composed by all drivers with an observed maximum speed $s_i'$ within a 15km/h window around the enforcement cutoff.

Table 3 shows the effects on subsequent speeding (binary outcome) and the speeding rate. Using $t_1 = t_2 =$ 3-months, we get a Wald estimate indicating that receiving a ticket reduces a driver's subsequent probability of speeding by 25.69 percentage points. As the local baseline probability is 65.5 %, the treatment effects implies a substantial reduction in speeding by 39 percent. Regarding the speeding rate, we estimate a 5.3 percentage point drop in the fraction of drive-throughs with a speed above the limit. All these effects are very precisely estimated.

Qualitatively similar results are obtained when the outcome variables are the probability of violation and the violation rate (Table 4). For the 3-month violation probability, the estimated effect of 19.91 would imply an eradication of violations for a person who was actually ticketed (the baseline probability at the threshold is 15.1 percent).

Comparing the 3-month and the 6-month assignment/outcome period, we obtain quite different point estimates. On the one hand, this is due to the fact that a much higher fraction of speeders receive a ticket during the first 6-month as compared to the first 3-month after the enforcement started working (during the latter period, the process of sending out tickets was still slow). $\delta^T$ increases from 39 (for the 3-month period) to more than 50 percent (for the 6-month period), thus increases the denominator in equation (3) and mechanically reduces the Wald estimates. What is more interesting, however, is the fact that the reduced form estimates vary, too. During months 4 to 6 after the start of the enforcement system, the average speeder who was just above the enforcement cutoff during the assignment period is now 10 percentage points less likely to speed (as compared to drivers with $s'_i$ just below the threshold). If we consider $t_1 = t_2 =$ 6-months, we observe a significantly smaller drop in the likelihood of speeding, by 4 percentage points (during months 7 to 12 after enforcement started). This suggests that the first tickets coming from the new radars had initially a stronger impact.

Strikingly, this trend is reverted for the second outcome variable studied in Table 3. Concerning the rate of speeding we observe a slighly stronger reduced form effect when we use $t_1 = t_2 =$ 3-months (yielding $\delta^Y = -0.02$) rather than a 6-months window (which gives us $\delta^Y = -0.04$). This suggest a differential treatment impact: while there is a stronger impact on the the binary speeding outcome during the shorter time window, it is weaker for the rate of speeding. One interpretation is that many drivers stop speeding quickly after receiving a ticket. At the same time, those who continue speeding, reduce the incidences of speeding more so in the mid run.

## 4.2 Event Study: Short-Run Responses after receiving a Ticket

The second part of our analysis estimates the immediate behavioral response after receiving a ticket and its medium-run persistence. In doing so, we exploit high-frequency data around the time when a driver receives a ticket. For each ticketed driver, we define the time axis such that "day one" represents the day when the driver received her (first) ticket from one of the five radars. We then analyze and compare her speeding behavior before and after receiving the ticket.

### 4.2.1 Graphical Evidence

Figures 9 through 12 visualize the behavioral responses plotting several outcome variables from the raw data. Each point in the graphs represents an average of all observations for all drivers who received a (first) ticket, binned into 7-day intervals, either before or after receiving the ticket.[13] Figure 9 depicts the rate of speeding, i.e., the fraction of drive-

[13]Note that the sample of drivers may vary between the different weeks. We will address this point in our parametric analysis.

throughs occurring above the speed limit. Between 21 to 25 % of drive-throughs before the ticket are above the limit. This fraction drops to 9 %, immediately after the ticket arrives. Over the 12-week period presented in the graph, it continues to gradually declines to 6 %.

In a similar fashion, figure 10 shows an immediate and sustained fall in the rate of violations, i.e., the fraction of drive-throughs above the enforcement threshold, from between 10-15 % to mere 1 %. Figure 11 shows an immediate and sustained reduction in the average speed at which the ticketed drivers drive, by 2.5 km/h and figure 12 a reduction in the 95th percentile of speed from 17-19 km/h above the speed limit to 5-6 km/h above the limit. Hence, the graphical evidence suggests that drivers respond strongly and swiftly, without any delay. Moreover, the figures indicate that the behavioral responses seem to be long-lasting – at least during the three months period considered in the figures.

#### 4.2.2 Regression Analysis

In a next step, we evaluate the behavioral response patterns parametrically. The regressions presented below will provide very precisely estimated magnitudes of the reduction in speeding after the ticket while controlling for a rich set of driving conditions and other variables. Our analysis builds on a sample of all drive-through observations going 4 weeks before and 12 weeks after each a driver received a ticket. The sample for the first ticket includes 155,775 observations (drive-throughs) from 7,811 drivers. To capture the pattern in behavioral responses, we estimate an equation which models speeding outcomes on each drive-through by a series of dummies for the weekly intervals during $w$-weeks before or after the ticket:

$$Y_{it} = \sum_{w=-3}^{12} \beta_w D_{wit} + \gamma X_{it} + \lambda_r + \lambda_m + \lambda_d + \lambda_h + \lambda_i + \epsilon_{it}. \tag{4}$$

$Y_{it}$ is the outcome of driver $i$ on a drive-through at time $t$. The key right-hand side variables are the set of dummies $D_{wit}$ indicating each weekly interval before and after receiving the ticket. The week zero, the last week up to receiving the ticket, is the omitted category. Hence the coefficients $\beta_w$ have the interpretation of the expected difference in the outcome in each week from the last week before receiving the ticket, after partialling out the influence of many other factors.

The variable $X_{it}$ is a measure of the speeding opportunity at a particular drive-through. The idea is to control for traffic congestion which makes speeding difficult or impossible. We exploit the observation on the car that had passed through the radar just in front of the driver whose behavior we are estimating. After analyzing the relationship between the probability of speeding and the time distance from the car in front, we define $X_{it}$ as a simple dummy equal to one if the car in front entered the radard zone 6 or more seconds ahead.[14]

In addition, we account for radar specific ($\lambda_r$) and time specific effects, including dummies for each calendar months of the year ($\lambda_m$), dummies for each days of the week as well as dummies for schooldays and holidays ($\lambda_d$), and dummies for each hour of the day ($\lambda_h$). Importantly, we also include driver fixed effects ($\lambda i$). Hence, we are controlling for unobserved heterogeneity in the speeding behavior at the driver level. Put differently, we are identifying the parameters of interests ($\beta_w$) by the within-driver variation in speeding behavior.

As outcomes variables, we consider similar measures as in Section4.1: *speeding* (a dummy equal to one if the measured speed exceeds the speed limit); *violation* (a dummy equal to one if the speed exceeds the enforcement threshold) and *relative speed* (difference between the measured speed and the speed limit). We emphasize that the estimated effects have the interpretation of the effects on the probability of speeding or violation *conditional on* a drive-through after ticket, as opposed to probability of a speeding or violation occurring after the ticket (which would be a product of both speeding conditional on a drive-through and the drive-through occurring as such).

The results from our estimates are reported in Table 6. All standard errors are clustered at the driver level. In each specification, the the estimated weekly effects are statistically significantly different from the (omitted) week zero at a 1 % level. The first

[14]The probability of speeding is increasing sharply in the time distance from the car in front from zero to 6 seconds, after which is almost levels off.

column reports the effects on the probability of speeding after receiving the first ticket (from these radars). The coefficient on the 1st week dummy implies that drivers reduce their probability of speeding by 9.46 percentage points during the first week – i.e., immediately after receiving the ticket. For the subsequent weeks, the effect gradually increases in magnitude; the drop in the speeding probability reaches 12.6 percentage points in the 12th week. Given the baseline probability of 20.7 %, the coefficients imply a vey large and sustained reduction in the probability of speeding by 50 to 60 %.

A very similar pattern is observed for the probability of violation (Table 6, column 2) and the relative speed (column 3). The probability of violation drops by 6.56 percentage points in the first week after receiving the ticket and gradually falls even further, down to 7.03 percentage point in the 12th week. Given the baseline probability of 9.7 %, the coefficients imply a reduction in the probability of violation by more than 70 %. The greater percentage reduction in the probability of violation than in the probability of speeding is consistent with the fact that the violation triggers the ticket while just speeding 'a bit' need not. Ticketed drivers reduce their speed by 2.1 km/h on average in the first week after receiving the ticket, and further down by (cumulative) 2.9 km/h by the 12th week.

Columns (4)–(6) of Table 6 report regressions estimated for a subsample of 786 drivers who received their second ticket. Here we define day one as the day of receiving this second ticket. One has to be cautious with the interpretation because this is of course a highly self-selected sample of drivers who were not deterred by the first ticket or who may have committed the second violation before receiving the first ticket. The estimates are, however, indicative at the very least. Again, all the weekly effects are highly significant – in an economic and statistical sense. For all three outcome variables, the profile of the response is similar to the one observed for the response to the first ticket: a substantial and immediate reduction in speeding followed by a further but small gradual decline over the 12 weeks of the post-ticket period considered. The effect size is still large but about one-half or two-thirds of the magnitudes estimated for the first ticket. Hence, the drivers who were ticked twice do respond to the 'new' ticket, however, their responses are quantitatively smaller.

### 4.3 Delay in punishment

In a third step, we empirically evaluate whether and by how much the magnitude of the behavioral response depends on the delay between committing the violation and receiving the ticket. First, we establish that there is substantial variation in the delay at which a ticket is delivered. Moreover, the delay is driven by three aspects which render the variation as good as random:

*(a) Delay in initial implementation of enforcement.* During the test phase, the city was waiting for the last regulatory approvals of the radars from the state police. The approvals were received and the fine administrators started sending tickets between October 13 and November 13, 2014 (depending on the radar). But the approvals applied retroactively to violations committed between 3 to 37 days prior to sending tickets (depending on the radar). These retroactive violations were processed very quickly because the backlog was minimal. Hence drivers who committed the violation just before the first tickets were mailed, received their tickets with a shorter delay than drivers who committed the violation weeks before the retroactive approval was given.

*(b) Natural variation in the delivery time* (i.e., the time between sending and receiving the ticket). The variation is due to weekends, holidays, absence (illness) or vacations of case workers, as well as due to delay in the mail.(There is a potential endogeneity issue with this source of delay which we return to later.)

*(c) Changes in the backlog*, which generate a variation in the time elapsed between the violation and sending of the ticket. Throughout most of the enforcement period, the fine administrators were processing the tickets from the oldest cases on the backlog forward. When the backlog accumulates, the delay in sending the tickets grows. For reasons discussed below, the backlog was accumulating and decumulating over time. Figure 13 illustrates the evolution of the distribution of the delay.

Initially, in October 2014, the administrators were able to process violations relatively quickly, hence most of the distribution of the delay is concentrated below 20 days, and the mode is mere 3 days. Gradually, delays due to absences, vacations, and a growing caseload caused an increase in the backlog. By January 2015, the distribution of the delay shifted to the right with a mode at 15 days. In March, the technical issues with the largest radar were resolved, which led to an increase in caseload. At the same time, the work pressure on the ticket administrators was increasing also due to the need to process the objections against the older tickets. This caused a huge accummulation in the backlog: the tickets to violators from May 2015 were sent with a delay of 55–100 days. In summer 2015, the city took several measures, including hiring part-time administrtors to process the backlog, and dividing the backlog such that the part-time administrators worked forward on the oldest cases, while the permanent staff worked on cases that were at most 50 days old. This led to a reduction in the delay, and also in a bi-modal distribution of the delay for violators from this period.

There is a natural concern that the fine administrators were sending the tickets to the most severe violators with a higher priority. We emphasize that this is not the case. The processing of tickets is strictly based on days. When a particular administrator starts processing the violations from a given day, she works only cases from that date until finishing them all because this is administratively convenient. Even the cases within a day are not sorted by speed.[15] In future work, we will document this point empirically, showing that – conditional on a given day of speeding – neither the speed at which a driver was measured violating the limit nor any other observable driver characteristics have a significant impact on the delay in the ticket.

#### 4.3.1 Estimation Approach

Exploiting the quasi-random variation in the delay of the ticket, we estimate a model which builds on a simplified version of equation (4):

$$Y_{it} = \beta_0 Post_{it} + \sum_{j=1}^{4} \beta_j \left( Post_{it} \times Delay_{ji} \right) + \gamma X_{it} + \lambda_r + \lambda_m + \lambda_d + \lambda_h + \lambda_i + \epsilon_{it} \quad (5)$$

The first right-hand side variable in the regression, $Post_{it}$, is a dummy equal to one if the drive-through occurs *after* the driver received the ticket – in the 'post-treatment

[15]This statement is based on our analysis of the software used in the enforcement process and numerous meetings with the city authorities.

phase'.[16] In addition, we include a set of interactions between the post-treatment dummy and the dummies $Delay_{ji}$. The latter capture the delay in the ticket, using two-week intervals. That is, $Delay_{1i}$ is an indicator for tickets being received in less than two weeks after the violation, $Delay_{2i}$ for ticket being received between two and four weeks after the violation, and so on (up to 4–6 and 6–8 weeks). The reference category are delays exceeding 8 weeks. In addition, we control for radar and time specific effects as well as for driver fixed effects and driving conditions (see discussion of equation 4, above). Similar as above, we consider all drive-troughs during a period of 4 weeks before and 12 weeks after a given driver received her first ticket.

The coefficient on the $Post_{it}$ dummy, $\beta_0$, has the interpretation of the average behavioral change (in speeding) after receiving the ticket as such, irrespective of the delay. Given how the delay dummies for the delay are specified, it is the effect of receiving the ticket with a delay of more than 8 weeks. The coefficients on the interaction terms, $\beta_1$ to $\beta_4$, have the interpretation of an *add-on* effect of receiving the ticket with a shorter delay. If, for instance, the ticket arrives within two weeks [between 2 to 4 weeks] as opposed to after 8 weeks or more, the behavioral response to the ticket is given by $\beta_0 + \beta_1$ [$\beta_0 + \beta_2$].

### 4.3.2 Results

Table 7 shows OLS estimates of equation 5. Standard errors are again clustered at the driver level. The first column reports the effects on the probability of speeding on a given drive-through for the first ticket received. The coefficient on the after-ticket dummy implies that the probability of speeding declines by 6.7 percentage points after receiving a ticket with a significant delay (of 8 weeks or more). If, in contrast, the ticket is received within two weeks after the violation, there is an additional deterrent effect of 11.5 percentage points. Hence, we would get 18.2 (rather than a 6.7) percentage point drop in the probability of speeding. The add-on effect of receiving the ticket between two to four weeks after the speed limit violation is 9.2 percentage points (resulting in an overall 15.9 pp drop). Interestingly, for a delay between 4–6 and 6–8 weeks, the coefficients are

[16] Building on the fact that the time profile of the response after receiving the ticket, as estimated in Table 6, is relatively flat, we use a simple post-treatment dummy rather than a full set of dummies for each week after receiving the ticket (as in equation 4).

very small and statistically insignificant. This implys that there is no additional deterrent effect from receiving the tickets with such a delay as compared to even longer delays exceeding 8 weeks.

The estimates of the probability of violation (column 2 in table 7) show an even greater relative magnitude of the effect of the delay. The coefficient on the baseline post-treatment dummy implies a 1.2 percentage point reduction in the probability of a violation. The additional effect of receiving the ticket within the first two weeks is a massive 12.6 percentage point reduction, while the add-on effect for a ticket that arrives within 2-4 weeks is a 11.3 percentage point drop. Similar as above, the effects for tickets being received between 4–6 or 6–8 weeks are again small and imprecisely estimated. With the probability of violation during the 4 weeks before the ticket being 11.9 % on average, the estimates imply that a ticket received within the first 4 weeks virtually eliminates violations, while the ticket received after more than four weeks does has an economically rather meagre effect (although statistically significant).

Column (3) of the table considers the relative speed on every drive-through. The estimates show a similar pattern as for the binary outcome variables. A delayed ticket reduces speed by 1.55 km/h. If the ticket arrives within the first two weeks, however, we estimate an additional decline by 2.59 km/h; for a delay of 2–4 weeks, we get an additional effect of 2.10 km/h. Beyond a delay of 4 weeks, there is again no significant difference as compared to tickets which are delayed by 8 or more weeks.

In columns 4–6 of table 7, we consider the effect of the delay on the response after the second ticket. The estimates suggest that the baseline deterrence effects are much smaller than the corresponding effects for the first ticket (similarly to the weekly effects in table 6). The effects of the delay, however, is very similar to the corresponding pattern observed for the first ticket.

The analysis therefore provides a clear cut and consistent pattern: the timing of the ticket is crucial in shaping the deterrence effect. The policy implications from our findings are straight forward: the ticket should arrive the driver within at most four weeks – the earlier the better. Once these 4 weeks period is passed, however, the ticket has a similar deterrent effect – irrespectively whether it is received earlier or later (within a 12 week period).[17]

### 4.3.3 Robustness

One potential concern regarding the delay of the ticket delivery concerns one potentially endogenous response margin which is available to drivers: In particular, late delivery may be partially due to driver being rather reluctant to pick up a speeding ticket at the post office after he received the notice in the mail.[18] Similarly, a driver might decide not to check his digital databox regularly. Figure 14 shows that the time from sending to receiving the ticket has a bimodal distribution, with tickets being delivered either very early or after 2 weeks (paper mail) or 10 days (databox).[19] A long delivery time may be symptomatic of a lack of diligence in personal affairs or an outright evasive behavior, which in turn may be correlated with the behavioral response to punishment. The drivers who are less willing to pick up the ticket may also be less willing to slow down after receiving the ticket, which would bias the estimated coefficients on the early delivery dummies downward.

We address the endogeneity issue by instrumenting the delay from the violation to receiving the ticket by the delay from the violation to *sending* the ticket. The instrument thus removes the variation in the delay that is controlled by the driver and leaves only the variation that is controlled by the ticket enforcers. Because the ticket enforcers prioritize solely by the backlog and do not prioritize by the driver's speed when sending the ticket, it is not correlated with the speed at violation.

Technically, the dummies for 0–2, 2–4, etc. week delay in *receiving* the ticket are instrumented by dummies for a 0–2, 2–4, etc. week delay in *sending* the ticket. Otherwise

[17] Early animal studies suggest that the effectiveness of punishment administration diminishes rapidly during the first 5 second after a behavior (Church, 1969); within one hour of the violation, however, immediate and non-immediate sanctions did not differ in its' effectiveness (Azrin, 1960).

[18] Tickets are sent via a 'registered mail'; the recipient has to sign a confirmation that he received the letter. When the postman does not find the recipient at home, he leaves a paper slip asking the recipient to pick up the registered letter at the post office. The driver than has up to two weeks to pick up the ticket; he may also guess from the paper slip that this mail might be a speeding ticket.

[19] The spike at 10 days in the databox delivery is caused by a legal fiction: If the recipient does not open the message in the databox within 10 days, the government regards it as delivered. So the observations with a 10-day delivery by the databox are drivers who did not actually open the message about the ticket, or did so at the very last moment.

the IV-regressions are identical to equation 5. The first-stage of the IV regressions shows that our set of dummies are very strong (with F-statistic on the excluded IVs ranging from 60 to 250). The results from the main stage, which are reported in Table 8, show that instrumenting has very little effect on the estimated coefficients: the IV estimates differ only marginally from the OLS estimates. The evidence therefore suggests that this layer of endogeneity is quantitatively and qualitatively irrelevant.

## 4.4 Heterogeneity of responses

Finally, we explore how the responses to the ticket differ between different groups of drivers. One dimension of heterogeneity is between private drivers and corporations. The dataset distinguishes whether the ticket was sent to a physical person or a legal person (corporations). The private persons pay the cost of the ticket directly and they also learn about the ticket at the time the ticket is received. In corporations, there is potentially an agency problem. Some corporations may pay the tickets on behalf of their drivers and may only imperfectly control the driving behavior of its employees. The actual drivers are likely to learn about the ticket with an additional delay after the corporation receives the ticket. One could therefore expect the private drivers to be more responsive to the punishment than the corporate drivers.

The first two columns of Table 9 show the estimates of the weekly speeding behavior around the time of receiving the ticket (equation 4) separately for private and corporate cars. The magnitudes of the reduction in the probability of speeding are essentially identical for the private drivers and corporations. The first two columns of Table 10 show the same breakdown of the effects of the delay (equation 5). Private drivers are more sensitive to the timely delivery; the likelihood of speeding on a given drive-through is 14.9 percentage points lower if the ticket is received within the first two weeks for them, while it is only 8.69 percentage points lower for corporate drivers, compared with receiving the ticket after more than two months. A somewhat less pronounced difference is observed for the ticket received between 2 to 4 weeks.

The second dimension of heterogeneity is based on whether the driver paid the ticket or not. Drivers who accept the punishment (pay the ticket) are possibly more law-abiding and hence more likely to slow down. In columns 3 and 4 of Tables 9 and 10, we estimate the speeding respones separately for drivers who paid the ticket within 90 days after receving the ticket and those who failed to pay by that time.[20] These regressions should be regarded as purely descriptive since the payment is an outcome variable occurring concurrently with the speeding after the ticket. The results suggest that drivers who do not pay the ticket do slow down nevertheless. The likelihood of speeding falls by 5.38 percentage points in the first week after the ticket and the size of the effect grows to 10.1 percentage points in the twelfth week. The speeding reductions for the drivers who paid the ticket are, however, between 30 to 91 percent greater in magnitude than for the non-paying drivers. The dependance of the speeding response on the delay in receiving the ticket is also slightly smaller in magnitude for the non-paying drivers (Table 10.)

# 5 Conclusions

Based on large and detailed micro-data on individual driving patterns we have studied specific deterrence effects from speeding tickets. Our results reveal strong and immediate deterrence effects from receiving a speeding tickets on subsequent speeding behavior. A ticket causes a very large drop in the probability of violating the speed limit and a pronounced decline in the average speed. Importantly, these behavioral responses seem long-lasting and are sustained over at least three to six months.

Using variation in the delay of ticket delivery, we further document the importance of the temporal proximity between the offense and the punishment. The specific deterrence is much larger (by a factor of 2–10), if the ticket is delivered quickly after the speeding violation. Once the delay is larger than 4 weeks, however, variation in the delivery does not alter the deterrence effect. In future work, we will analyze the mechanisms behind this effect. The policy implications, however, are already clear from the present analysis: early feedback is crucial to the deterrent effect of punishment.

[20] The average time from receiving to the payment is 10 days, conditional on the payment being made.

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Figure 1: Test of bunching at the enforcement threshold

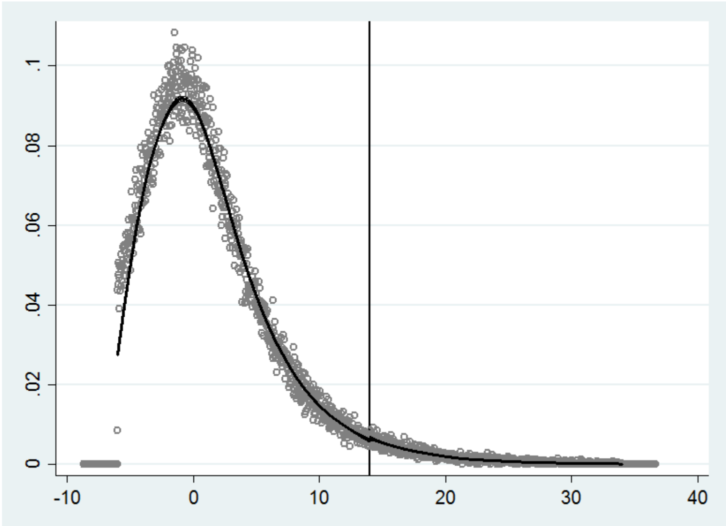


Figure 2: Treatment: Probability of being ticketed at the enforcement threshold

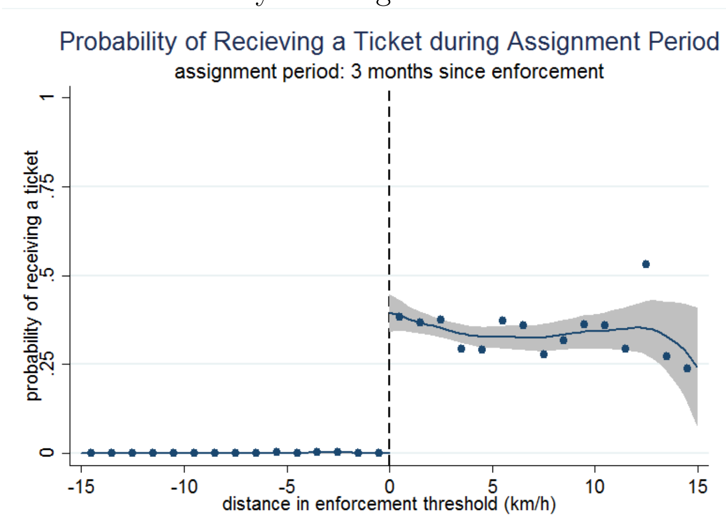


Figure 3: Outcome: Probability of speeding (above speed limit)

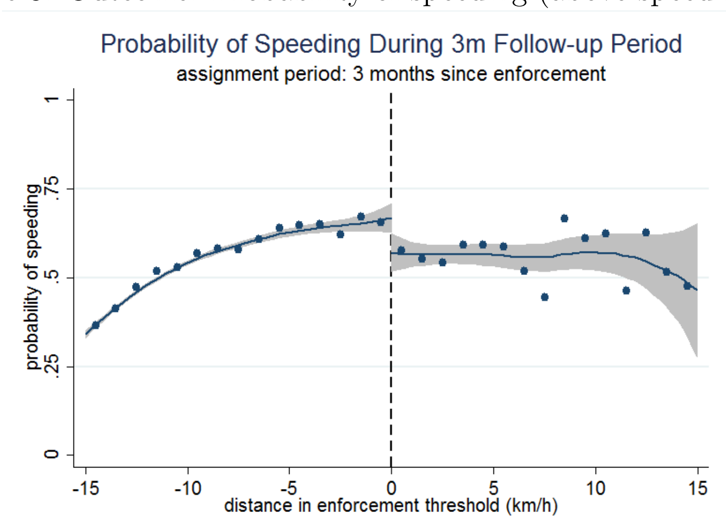

Figure 4: Outcome: Speeding Rate (above speed limit)

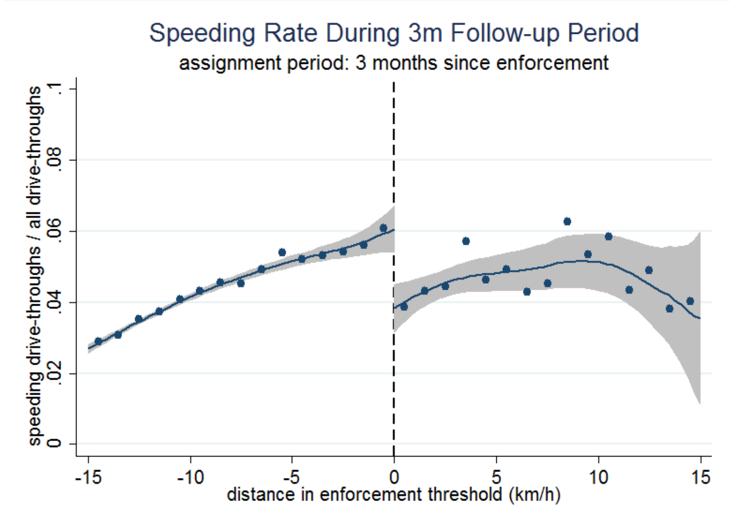


Figure 5: Outcome: Probability of violation (above enforcement threshold)

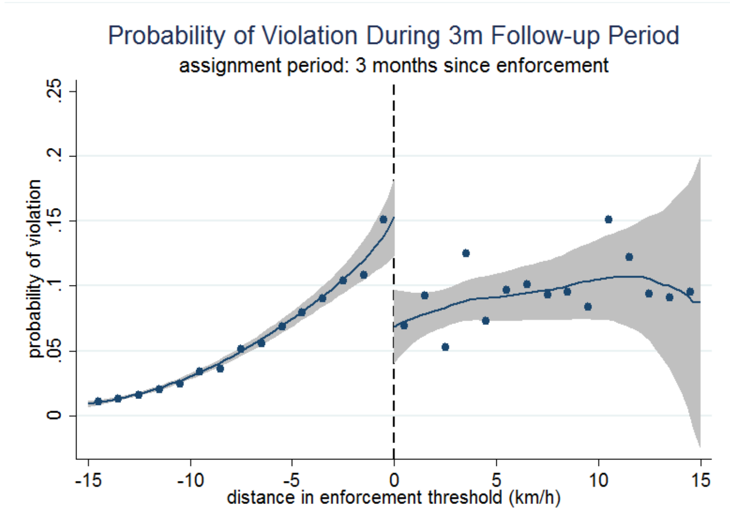


Figure 6: Outcome: Violation rate (above enforcement threshold)

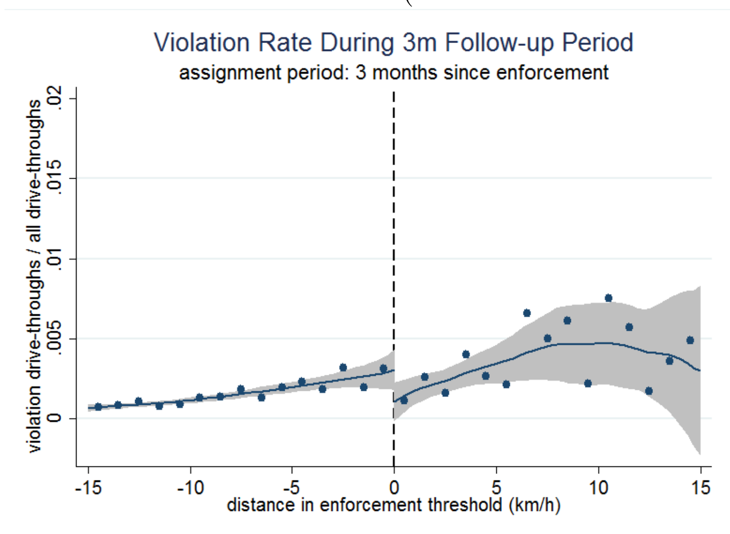

Figure 7: Outcome: Average speed (relative to speed limit)

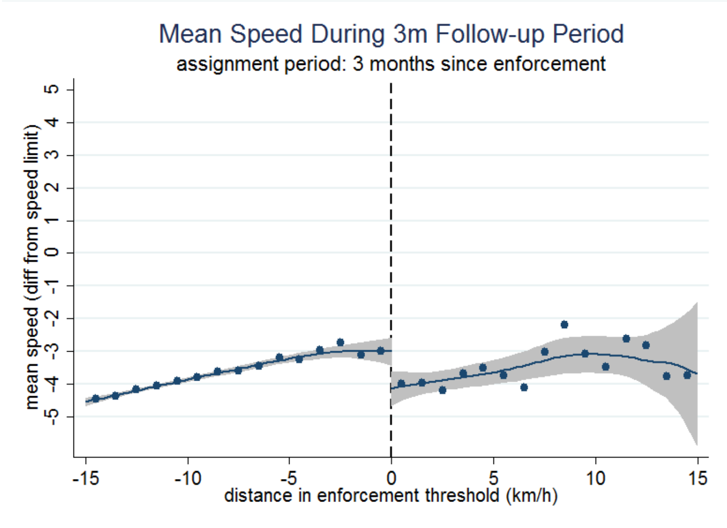


Figure 8: Outcome: 90th percentile speed (relative to speed limit)

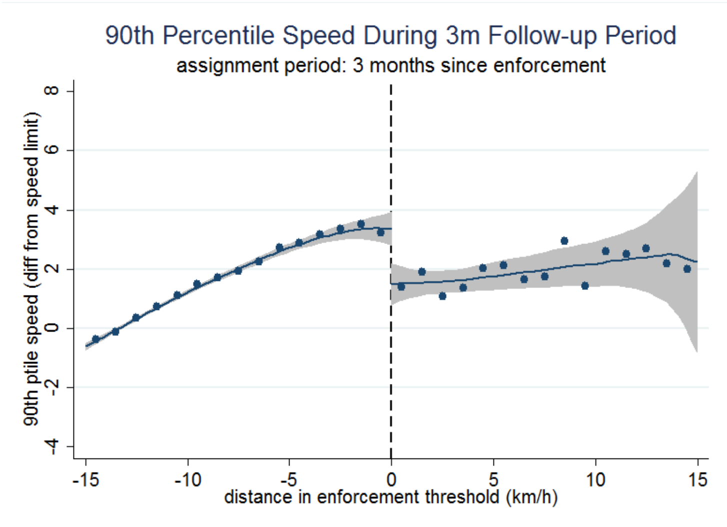


Figure 9: Rate of speeding, weekly average before/after receiving the 1st ticket)

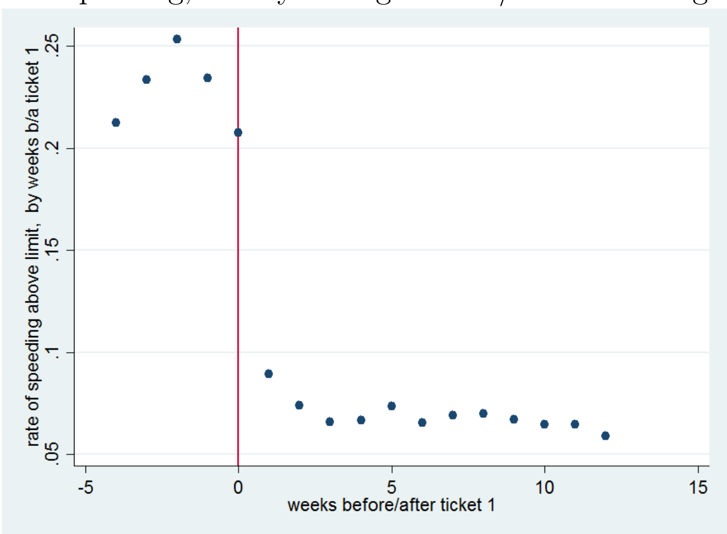

Figure 10: Rate of violation, weekly average before/after receiving the 1st ticket)

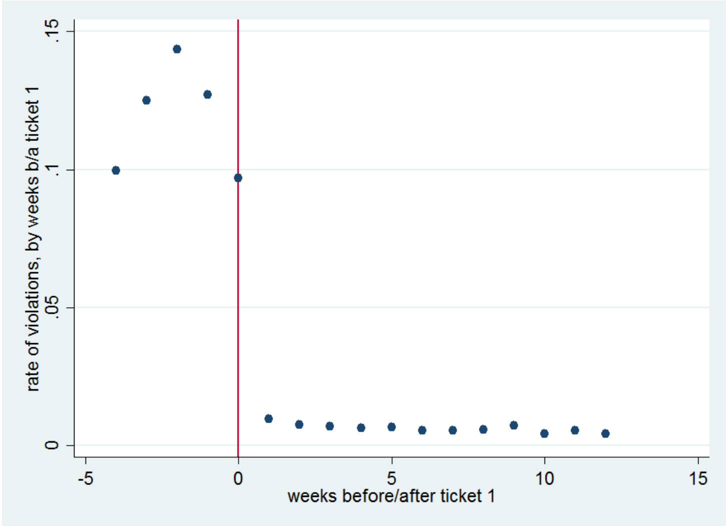


Figure 11: Speed, weekly average before/after receiving the 1st ticket)

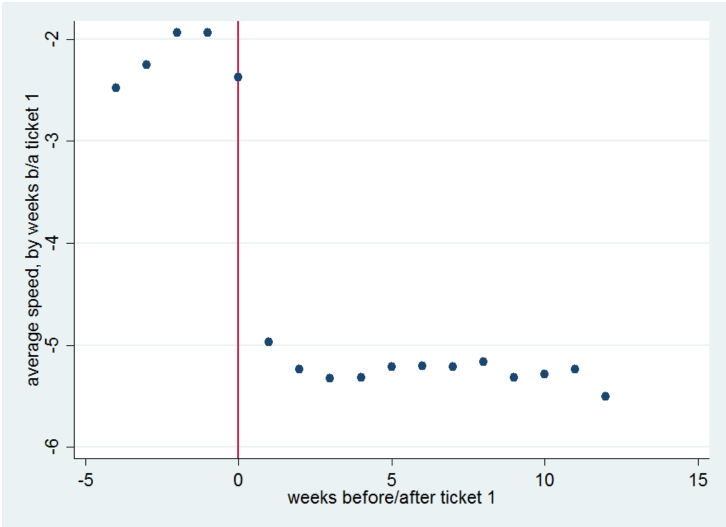


Figure 12: Speed, weekly 95th percentile before/after receiving the 1st ticket)

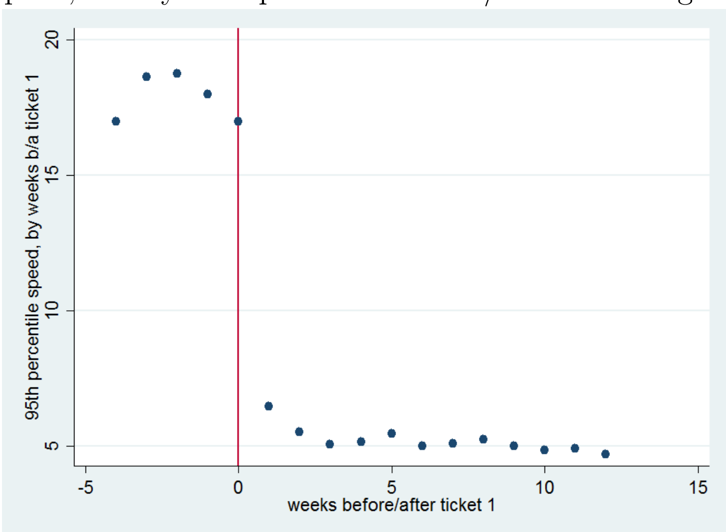

Figure 13: Variation in the delay from offense to punishment

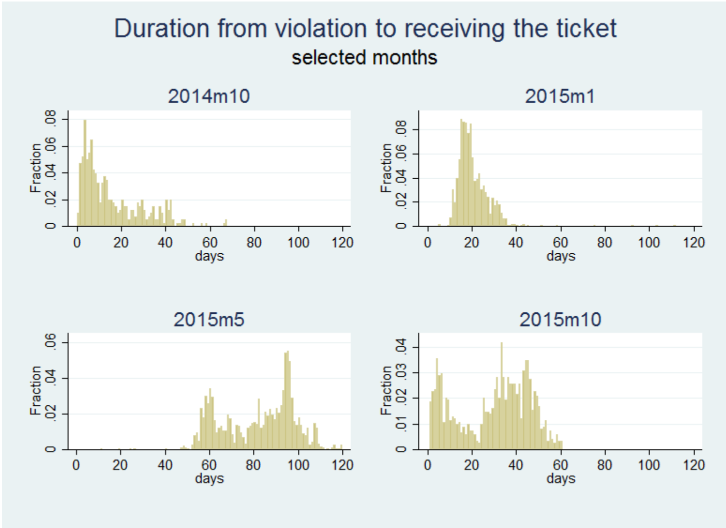


Figure 14: Potential endogeneity in the delay

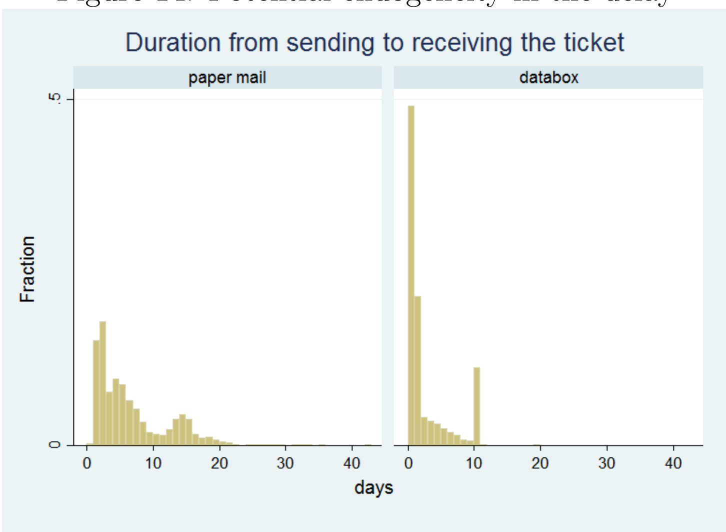

Table 1: Fine structure

| violation severity | low | medium | high |
|---|---|---|---|
| speed above the limit (km/h) | up to 20 | 21-40 | 41 and above |
| fine - "plea bargaining" (CZK) | **900** | **1900** | N.A. |
| fine - conviction (CZK + add-on) | 1500-2500 points | 2500-5000 points | 5000-10000 license suspended |

Table 2: Summary statistics

| | full sample | cars that ever violated |
|---|---|---|
| number of obs | 7,575,300 | 923,082 |
| number of cars | 613,587 | 20,248 |
| number of violations | | 24,636 |
| average speed (rel.) km/h | -5.67 | -4.51 |
| speeding rate | 0.046 | 0.101 |
| violation rate | 0.003 | 0.026 |
| | | |
| avg days from violation to ticket | | 54 |
| avg fine (CZK) | | 995 |
| fraction paid | | 0.79 |

Table 3: Regression discontinuity estimates – speeding.

| | | | follow-up period | | | |
|---|---|---|---|---|---|---|
| | | | speeding | | speeding rate | |
| | | | 3M | 6M | 3M | 6M |
| assignment period | 3M | outcome | −0.1012*** | | −0.0212*** | |
| | | | (0.0307) | | (0.0043) | |
| | | treatment | 0.3939*** | | 0.3939*** | |
| | | | (0.0243) | | (0.0243) | |
| | | wald estimate | −0.2569*** | | −0.0539*** | |
| | | | (0.0810) | | (0.0114) | |
| | 6M | outcome | | −0.0415** | | −0.0402*** |
| | | | | (0.0172) | | (0.0047) |
| | | treatment | | 0.5051*** | | 0.5051*** |
| | | | | (0.0148) | | (0.0148) |
| | | wald estimate | | −0.0822** | | −0.0796*** |
| | | | | (0.0347) | | (0.0095) |
| | number of drivers | | 109,469 | 178,340 | 109,469 | 178,340 |

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 4: Regression discontinuity estimates – violation.

| | | | follow-up period | | | |
|---|---|---|---|---|---|---|
| | | | violation | | violation rate | |
| | | | 3M | 6M | 3M | 6M |
| assignment period | 3M | outcome | −0.0784*** | | −0.0018*** | |
| | | | (0.0187) | | (0.0007) | |
| | | treatment | 0.3939*** | | 0.3939*** | |
| | | | (0.0243) | | (0.0243) | |
| | | wald estimate | −0.1991*** | | −0.0045*** | |
| | | | (0.0489) | | (0.0018) | |
| | 6M | outcome | | −0.0751*** | | −0.0034*** |
| | | | | (0.0119) | | (0.0010) |
| | | treatment | | 0.5052*** | | 0.5052*** |
| | | | | (0.0148) | | (0.0148) |
| | | wald estimate | | −0.1486*** | | −0.0068*** |
| | | | | (0.0239) | | (0.0020) |
| | number of drivers | | 109,469 | 178,340 | 109,469 | 178,340 |

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 5: Regression discontinuity estimates – speed.

| | | | follow-up period | | | |
|---|---|---|---|---|---|---|
| | | | average speed | | 90th perecentile | |
| | | | 3M | 6M | 3M | 6M |
| assignment period | 3M | outcome | $-1.1457$*** | | $-1.9458$*** | |
| | | | (0.2987) | | (0.3821) | |
| | | treatment | 0.4633*** | | 0.4633*** | |
| | | | (0.0279) | | (0.0279) | |
| | | wald estimate | $-2.4731$*** | | $-4.2004$*** | |
| | | | (0.6458) | | (0.8446) | |
| | 6M | outcome | | $-1.0252$*** | | $-1.9854$*** |
| | | | | (0.1902) | | (0.2364) |
| | | treatment | | 0.5456*** | | 0.5456*** |
| | | | | (0.0159) | | (0.0159) |
| | | wald estimate | | $-1.8790$*** | | $-3.6388$*** |
| | | | | (0.3481) | | (0.4451) |
| | number of drivers | | 109,469 | 178,340 | 109,469 | 178,340 |

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 6: The effects of receiving a speeding ticket.

| before/after ticket | 1st ticket speeding | 1st ticket violation | 1st ticket rel. speed | 2nd ticket speeding | 2nd ticket violation | 2nd ticket rel. speed |
|---|---|---|---|---|---|---|
| −3 weeks | 0.0154*** | 0.0136*** | 0.507*** | 0.0470*** | 0.0334*** | 0.790** |
| | (0.00535) | (0.00365) | (0.142) | (0.0118) | (0.00738) | (0.312) |
| −2 weeks | 0.0288*** | 0.0292*** | 0.568*** | 0.0667*** | 0.0447*** | 1.266*** |
| | (0.00525) | (0.00375) | (0.141) | (0.0117) | (0.00726) | (0.292) |
| −1 week | 0.0154*** | 0.0186*** | 0.601*** | 0.0279*** | 0.0271*** | 0.446 |
| | (0.00515) | (0.00362) | (0.135) | (0.0105) | (0.00709) | (0.326) |
| 1 week | −0.0946*** | −0.0656*** | −2.102*** | −0.0658*** | −0.0307*** | −1.678*** |
| | (0.00477) | (0.00286) | (0.136) | (0.00993) | (0.00526) | (0.305) |
| 2 weeks | −0.108*** | −0.0661*** | −2.316*** | −0.0642*** | −0.0272*** | −1.627*** |
| | (0.00489) | (0.00289) | (0.139) | (0.0110) | (0.00534) | (0.322) |
| 3 weeks | −0.115*** | −0.0661*** | −2.560*** | −0.0732*** | −0.0319*** | −1.647*** |
| | (0.00482) | (0.00288) | (0.139) | (0.0110) | (0.00526) | (0.325) |
| 4 weeks | −0.115*** | −0.0667*** | −2.517*** | −0.0708*** | −0.0312*** | −1.804*** |
| | (0.00492) | (0.00288) | (0.144) | (0.0114) | (0.00547) | (0.327) |
| 5 weeks | −0.107*** | −0.0648*** | −2.385*** | −0.0649*** | −0.0316*** | −1.497*** |
| | (0.00493) | (0.00283) | (0.141) | (0.0114) | (0.00561) | (0.343) |
| 6 weeks | −0.117*** | −0.0665*** | −2.507*** | −0.0635*** | −0.0333*** | −1.950*** |
| | (0.00492) | (0.00285) | (0.145) | (0.0117) | (0.00531) | (0.345) |
| 7 weeks | −0.115*** | −0.0677*** | −2.586*** | −0.0878*** | −0.0371*** | −2.310*** |
| | (0.00500) | (0.00283) | (0.145) | (0.0114) | (0.00564) | (0.349) |
| 8 weeks | −0.117*** | −0.0684*** | −2.653*** | −0.0758*** | −0.0350*** | −2.103*** |
| | (0.00512) | (0.00291) | (0.148) | (0.0117) | (0.00566) | (0.364) |
| 9 weeks | −0.118*** | −0.0669*** | −2.744*** | −0.0730*** | −0.0346*** | −1.317*** |
| | (0.00525) | (0.00292) | (0.150) | (0.0120) | (0.00590) | (0.372) |
| 10 weeks | −0.121*** | −0.0689*** | −2.720*** | −0.0743*** | −0.0365*** | −1.725*** |
| | (0.00522) | (0.00295) | (0.150) | (0.0127) | (0.00674) | (0.376) |
| 11 weeks | −0.122*** | −0.0688*** | −2.712*** | −0.0827*** | −0.0399*** | −1.894*** |
| | (0.00526) | (0.00299) | (0.150) | (0.0135) | (0.00654) | (0.370) |
| 12 weeks | −0.126*** | −0.0703*** | −2.942*** | −0.0874*** | −0.0367*** | −1.967*** |
| | (0.00522) | (0.00300) | (0.153) | (0.0127) | (0.00700) | (0.364) |
| Observations | 155,775 | 155,775 | 155,775 | 30,803 | 30,803 | 30,803 |
| R-squared | 0.293 | 0.344 | 0.312 | 0.238 | 0.207 | 0.262 |

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 7: OLS: The effects of the length of delay in receiving the ticket.

| | 1st ticket | | | 2nd ticket | | |
|---|---|---|---|---|---|---|
| | speeding | violation | rel. speed | speeding | violation | rel. speed |
| after ticket | −0.0668*** | −0.0118*** | −1.555*** | −0.0471*** | −0.00667 | −1.019*** |
| | (0.00742) | (0.00281) | (0.205) | (0.0108) | (0.00499) | (0.368) |
| ticket received | | | | | | |
| in 0–2 weeks | −0.115*** | −0.126*** | −2.588*** | −0.121*** | −0.107*** | −2.608*** |
| | (0.0102) | (0.00560) | (0.277) | (0.0204) | (0.0107) | (0.566) |
| in 2–4 weeks | −0.0918*** | −0.113*** | −2.098*** | −0.0946*** | −0.0924*** | −2.138*** |
| | (0.00945) | (0.00495) | (0.256) | (0.0177) | (0.00962) | (0.497) |
| in 4–6 weeks | 0.00331 | −0.00298 | −0.0616 | −0.0358** | −0.00864 | −1.205* |
| | (0.00950) | (0.00357) | (0.277) | (0.0182) | (0.00689) | (0.651) |
| in 6–8 weeks | 0.00119 | −0.00279 | 0.468 | 0.0176 | 0.00204 | 0.457 |
| | (0.0106) | (0.00397) | (0.310) | (0.0162) | (0.00623) | (0.586) |
| Observations | 155,775 | 155,775 | 155,775 | 30,803 | 30,803 | 30,803 |
| R-squared | 0.295 | 0.353 | 0.314 | 0.239 | 0.214 | 0.262 |

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 8: IV: The effects of the length of delay in receiving the ticket.

| | 1st ticket | | | 2nd ticket | | |
|---|---|---|---|---|---|---|
| | speeding | violation | rel. speed | speeding | violation | rel. speed |
| after ticket | −0.0595*** | −0.0124*** | −1.408*** | −0.0389*** | −0.00187 | −0.393 |
| | (0.00774) | (0.00297) | (0.226) | (0.0131) | (0.00588) | (0.467) |
| ticket received | | | | | | |
| in 0–2 weeks | −0.116*** | −0.118*** | −2.507*** | −0.167*** | −0.129*** | −4.096*** |
| | (0.0118) | (0.00704) | (0.324) | (0.0295) | (0.0171) | (0.815) |
| in 2–4 weeks | −0.0989*** | −0.116*** | −2.459*** | −0.0698** | −0.0782*** | −2.364** |
| | (0.0114) | (0.00622) | (0.332) | (0.0275) | (0.0125) | (0.927) |
| in 4–6 weeks | −0.00379 | −0.00595 | −0.0455 | −0.0380 | −0.0157 | −1.158 |
| | (0.0133) | (0.00495) | (0.389) | (0.0290) | (0.0114) | (1.011) |
| in 6–8 weeks | −0.0402* | −0.00437 | −0.228 | −0.0310 | −0.0265* | −1.810 |
| | (0.0210) | (0.00760) | (0.617) | (0.0351) | (0.0144) | (1.339) |
| Observations | 154,547 | 154,547 | 154,547 | 30,774 | 30,774 | 30,774 |
| R-squared | 0.263 | 0.258 | 0.290 | 0.235 | 0.206 | 0.259 |

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 9: Heterogeneity in the effects of receiving the ticket.

| before/after ticket | speeding private | speeding corporation | speeding ticket not paid | speeding ticket paid |
|---|---|---|---|---|
| −3 weeks | 0.0342*** | −0.00424 | 0.0303** | 0.0128** |
| | (0.00750) | (0.00765) | (0.0139) | (0.00577) |
| −2 weeks | 0.0386*** | 0.0185** | 0.0203 | 0.0312*** |
| | (0.00725) | (0.00759) | (0.0129) | (0.00570) |
| −1 week | 0.0197*** | 0.0109 | 0.0221* | 0.0144** |
| | (0.00693) | (0.00761) | (0.0125) | (0.00563) |
| 1 week | −0.0938*** | −0.0942*** | −0.0538*** | −0.103*** |
| | (0.00659) | (0.00690) | (0.0122) | (0.00516) |
| 2 weeks | −0.106*** | −0.109*** | −0.0631*** | −0.117*** |
| | (0.00675) | (0.00711) | (0.0128) | (0.00525) |
| 3 weeks | −0.117*** | −0.113*** | −0.0649*** | −0.125*** |
| | (0.00640) | (0.00727) | (0.0129) | (0.00513) |
| 4 weeks | −0.108*** | −0.121*** | −0.0759*** | −0.123*** |
| | (0.00665) | (0.00729) | (0.0130) | (0.00526) |
| 5 weeks | −0.104*** | −0.109*** | −0.0587*** | −0.116*** |
| | (0.00666) | (0.00731) | (0.0131) | (0.00529) |
| 6 weeks | −0.110*** | −0.122*** | −0.0717*** | −0.125*** |
| | (0.00674) | (0.00721) | (0.0129) | (0.00530) |
| 7 weeks | −0.113*** | −0.118*** | −0.0732*** | −0.124*** |
| | (0.00670) | (0.00753) | (0.0134) | (0.00534) |
| 8 weeks | −0.114*** | −0.120*** | −0.0711*** | −0.127*** |
| | (0.00699) | (0.00758) | (0.0139) | (0.00546) |
| 9 weeks | −0.117*** | −0.120*** | −0.0677*** | −0.128*** |
| | (0.00700) | (0.00792) | (0.0155) | (0.00548) |
| 10 weeks | −0.118*** | −0.123*** | −0.0952*** | −0.126*** |
| | (0.00712) | (0.00770) | (0.0147) | (0.00554) |
| 11 weeks | −0.120*** | −0.124*** | −0.0863*** | −0.129*** |
| | (0.00728) | (0.00768) | (0.0142) | (0.00566) |
| 12 weeks | −0.123*** | −0.129*** | −0.101*** | −0.131*** |
| | (0.00726) | (0.00758) | (0.0144) | (0.00557) |
| Observations | 80,808 | 74,967 | 25,282 | 130,493 |
| R-squared | 0.292 | 0.295 | 0.293 | 0.294 |

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 10: Heterogeneity in the effects of the delay.

| | speeding | | | |
|---|---|---|---|---|
| | private | corporate | ticket not paid | ticket paid |
| after ticket | −0.0603*** | −0.0732*** | −0.0420*** | −0.0738*** |
| | (0.00871) | (0.0130) | (0.0158) | (0.00822) |
| ticket received | | | | |
| in 0–2 weeks | −0.149*** | −0.0869*** | −0.107*** | −0.112*** |
| | (0.0133) | (0.0161) | (0.0257) | (0.0111) |
| in 2–4 weeks | −0.104*** | −0.0783*** | −0.0752*** | −0.0931*** |
| | (0.0119) | (0.0155) | (0.0211) | (0.0103) |
| in 4–6 weeks | −0.00534 | 0.0125 | 0.0148 | 0.00288 |
| | (0.0119) | (0.0158) | (0.0202) | (0.0106) |
| in 6–8 weeks | −0.0181 | 0.0266 | 0.00227 | −0.000868 |
| | (0.0135) | (0.0173) | (0.0229) | (0.0120) |
| Observations | 80,808 | 74,967 | 25,282 | 130,493 |
| R-squared | 0.295 | 0.297 | 0.295 | 0.296 |

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1