Swiftness and Delay of Punishment*

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Abstract

This paper studies how the swiftness and delay of punishment affect behavior. Using rich administrative data from automated speed cameras, we exploit two (quasi-)experimental sources of variation in the time between a speeding offense and the sending of a ticket. At the launch of the speed camera system, administrative challenges caused delays of up to three months. Later, we implemented a protocol that randomly assigned tickets to swift or delayed processing. We identify two different results. First, delays have a negative effect on payment compliance: the rate of timely paid fines diminishes by 7 to 9% when a ticket is sent with a delay of four or more weeks. We also find some evidence that very swift tickets – sent on the first or second day following the offense – increase timely payments. These results align with the predictions of expert scholars that we elicited in a survey. Second, speeding tickets cause a strong, immediate, and persistent decline in speeding. However, we do not detect any robust, differential effects of swiftness or delay on speeding. This challenges widely held beliefs, as reflected in our survey. Yet, we document large mechanical benefits of swift punishment and provide a theoretical framework of learning and updating that explains our findings.

JEL Classification: K14, K42, D80.

Keywords: Law enforcement; celerity of punishment; swiftness; specific deterrence; speeding; payment compliance; expert survey.

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"*The more immediately a punishment is inflicted, the more ... useful it will be. [...] because the smaller the interval of time between the punishment and the crime, the stronger and more lasting will be the association of the two ideas, of crime and punishment.*" Beccaria (1764), Chapter XIX

"*Delay of punishment is of paramount importance and is probably largely responsible for the apparent ineffectiveness of our current punitive system.*" Singer (1970), p. 420

1 Introduction

A significant body of theoretical (Becker, 1968) and empirical research (Chalfin and McCrary, 2017) studies the effects of severity and certainty of punishment on crime. In contrast, the swiftness of punishment – celerity – receives only scant attention in economics. Despite a lack of evidence, there is a widely held belief among politicians and criminal justice practitioners that the celerity of punishment is a crucial policy parameter. Demands for '*swift* and certain' punishment are common around the world and often get heard. In the UK, for instance, both liberal and conservative governments explore strategies to facilitate swift responses of the justice system (Home Office, 2004; UK Ministry of Justice, 2012). In the U.S., the Speedy Trail Act of 1974 was in part motivated by using celerity as a crime control tool (Bridges, 1982).

Academic scholars assess swiftness as an important factor, too. This point is documented in Figure 1 (a), which is based on a survey of crime scholars who published in leading journals in Economics or Criminology, respectively. 48% of them think that swiftness plays a major role in shaping deterrence effects, 24% think that it plays at least some minor role.[1] At the same time, a majority of respondents acknowledge that the empirical foundation of their assessment is weak. Figure 1 (b) indicates that 35% are not aware of any evidence on the impact of swiftness, another 22% state that evidence is scarce. This paper aims at providing causal evidence on the effects of more or less swift punishment.

Figure 1 about here.

Exploiting (quasi-)experimental variation in the time between an offense and the communication of punishment in the form of a fine, we test whether swiftness amplifies behavioral responses – and thus makes punishment, in the words of Beccaria, 'more useful'. In addition, we examine whether delayed punishment diminishes any effects – as suggested by many scholars (see e.g. the quote from Singer, 1970). The context of our study is the enforcement of traffic laws. We leverage administrative data sets from an automated speed camera system in the Czech Republic. The data contain the exact time of an offense (i.e., a ride with certain speed above the limit) as well as the precise date when a speeding ticket is sent, delivered and, potentially, paid. A car ID allows us to follow cars over time and observe the measured speed of all of their rides across different camera zones. Based on these data, we study two sets of outcomes: (1) driving responses after receiving a ticket and (2) the payment of the fine specified in the speeding ticket.

[1] Among respondents, 54% of economists and 38% of criminologists assign a major role to swiftness (see Figure A.2). We discuss further details on the survey in Section 3.3.

Our empirical analysis exploits two sources of variation. First, we make use of a series of exogenous events at the start of the speed monitoring system. While the administrators were already generating speeding tickets, the regulatory approval to actually send them out was missing. The approval arrived in a staggered manner for different cameras, implying that some tickets (from some cameras) were issued swiftly, others with delays of one or two months. Once all cameras were approved, the amount of tickets overwhelmed the administration and created another wave of massive backlogs. Only after hiring additional staff and adjusting the work process, the delays in sending were curtailed. Conditional on a set of fixed effects, we use the variation among 14,250 speeding tickets from this *backlog period* to identify the effects of delayed tickets.

A second source of variation is based on an intervention where we, in cooperation with the authority, modified the administrative process. During this *randomization period*, we introduced a feature in the administrators' software that randomized the sequence at which another 11,250 tickets were processed. The procedure implied that, for a random subset of offenses from a given day, tickets would be sent swiftly, within one to four days after the offense. Tickets for the remaining offenses would be processed with modest delays of typically two to four weeks. This variation allows us to identify the impact of swift tickets.

The analyses yield two sets of findings. First, we document that delays have economically meaningful effects on payment compliance. For the backlog period, our estimates indicate that, relative to tickets sent between 5 days and 4 weeks after the offense, delays of one, two or three months cause a 7 to 9% (5 to 7 percentage point) drop in the rate of timely payments (i.e. payments within 15 days after delivery). For tickets sent swiftly, between one and four days after the offense, we observe a statistically insignificant 1% increase in timely payments. Only for tickets sent on the first and second day after an offense, we find weakly significant positive effects: compared to tickets sent between 5 and 14 days, such swift tickets tend to increase payment rates by about 3 to 6%. The effects from swiftness and delay on payment compliance are very close to the predictions indicated by the economists and criminologists covered in our survey.

Our preferred interpretation of these effects is that the celerity at which the fine is communicated signals how effective the enforcement process is. Drivers respond by updating their priors regarding the consequences of non-compliance (the risk of facing a late fee). Depending on the experienced swiftness or delay (relative to their priors), they might thus become more or less inclined to pay the fine on time.

A second set of results explores speeding responses. Based on within-car event study estimates, we document – in line with Dušek and Traxler (2022) – a pronounced and immediate behavioral response after receiving a ticket: the likelihood of speeding drops by 34 to 44% and the average travel speed declines by 3%. These responses are long-lasting and persistent over time. Yet, we do not detect differential effects from swiftness or delays. In the backlog period, where our main sample covers 540,000 rides from 9,100 repeatedly observed cars, tickets that are delayed by more than 4, 8 or 12 weeks do not have any different impact than tickets sent within four weeks. Similarly, the estimates for the randomization period (308,000 rides from 6,700 cars) do not indicate any differential impact of tickets sent within 4 days after the offense. Overall, we find no evidence that swiftness or delay would systematically shape the size of specific deterrence effects. This finding is in sharp contrast with the predictions of academic experts elicited through our survey: economics and criminologists both predicted that swiftness reinforces and that delays attenuate the effect of tickets on driving responses. These beliefs seem to echo Beccaria's idea that swiftness facilitates learning by making the link between an offense and the law enforcement response more salient – an idea that is not supported by our data.

Our evidence suggests that offenders learn their lesson, but do so irrespectively of the promptness of punishment. Drivers respond to *any* ticket: they update their priors about the future costs of speeding and, accordingly, adjust their driving behavior. The mere fact of receiving a ticket is crucial for shaping posterior beliefs. For payment compliance, in contrast, receiving a ticket on its own provides limited input to form expectations about the costs of non-payment. In the latter domain, swiftness and delay thus exert a meaningful influence on expectations and, in turn, payment decisions. For speeding choices, however, variation in swiftness and delay might only alter drivers' expectations about the timing of future sanctions. This dimension, which merely influences the discounting of expected costs, appears to play no significant role in shaping behavioral responses in our context.

While swift and delayed tickets are equally 'useful' in deterring future speeding, swiftness nevertheless generates a strong benefit: the earlier a ticket is sent, the sooner a driver responds. Hence, more swift punishment mechanically induces a desirable behavioral response – less speeding – at an earlier point in time. A further, policy relevant implication of our results is that sending tickets swiftly (or, at least, avoiding longer delays) yields fiscal benefits: the rate of timely payments increases, these payments arrive earlier, and the administrative costs associated with the enforcement of unpaid tickets decline. Neglecting any administrative constraints, our results imply that the authority should ideally send tickets within a day after the offense. While this target seems ambitious, technological progress and a growing scope for automated law enforcement could make it soon easy to achieve.[2] Beyond the context-specific implications, the idea that swift or delayed process management provides a signal about the strength of an institution offers a broader lesson. Enforcement authorities collecting tax debt or fines as well as private companies chasing outstanding payments should consider the potential costs and benefits from delayed or swift communication.

Related literature and contributions

As indicated by the responses to our survey (Figure 1 (b)), evidence on swiftness is indeed scarce. While there is persuasive evidence on the deterrent effects of more severe (e.g., Drago et al., 2009) and more certain punishment (e.g., Draca et al., 2011), the role of celerity has largely been ignored in economics. We are only aware of two quasi-experimental studies using aggregated,

[2] In Dubai and in Abu Dhabi, for instance, police has tested automated systems that send SMS notifications to motorist cell phones within minutes after traffic offenses (speeding, running a red light). Given that this can cause distractions by mobile phone while driving, this approach is not necessarily optimal.

regional data and one lab experiment, which provide mixed evidence on the effects of celerity.[3] More generally, and despite numerous analytical contributions studying dynamic aspects of crime (e.g., Davis, 1988; Lee and McCrary, 2017), there is a dearth of empirical investigations into the timing of law enforcement. An important exemption is Blanes i Vidal and Kirchmaier (2017), who study the effect of police response time on clearance rates.[4]

The gap in the literature can be attributed, in part, to the influence of Becker (1968). Empirical work on *general* deterrence has long dominated the field (Buonanno et al., 2022), which muted the attention given to the impact of different dimensions of punishment, such as celerity, on ex-post responses. Existing research on *specific* deterrence has primarily studied variation in prison conditions (e.g., Mastrobuoni and Terlizzese, 2022), sentence lengths (e.g., Drago et al., 2011), or the severity of punishment more generally (Hjalmarsson, 2009; Hansen, 2015; DiTella and Schargrodsky, 2013; Mueller-Smith and Schnepel, 2020; Bhuller et al., 2020). Our study is, to the best of our knowledge, the first to identify the role of celerity in shaping specific deterrence effects – i.e., offenders' responses after encountering either swift or delayed law enforcement. We also make a novel contribution by documenting spillover effects of the promptness of punishment for one type of non-compliance (speeding) on another (non-payment of fines).

The criminology literature offers much more work on celerity (reviewed in Nagin, 2013; Pratt and Turanovic, 2018). In addition to numerous correlational studies, there is also evidence from randomized correctional interventions. The latter, however, conjointly vary swiftness and other policy dimensions. The most prominent example along these lines is project HOPE (Hawaii Opportunity Probation with Enforcement), which tested 'certain and immediate punishment' for probationers (failing, e.g., urine tests). The encouraging findings from Hawken and Kleiman (2009) are often interpreted as large, positive celerity effects. Yet, the program simultaneously altered the severity, certainty and celerity of punishment. Hence, the evidence does not allow to isolate the impact of swiftness.[5] The challenge in isolating celerity effects is also highlighted by Andersen (2020). Exploiting a quasi-natural experiment that shortened the time from charges to imprisonment for juvenile offenders in Denmark, he finds that shorter case processing times led to *more* recidivism. The result, however, must be interpreted as the compound effect of aging, incapacitation, and specific deterrence. Our paper differs from these contributions in that our setting allows us to isolate the role from a swift or delayed communication of punishment in shaping specific deterrence effects.

Our study also relates to a broader research tradition on the role of feedback timing on, e.g., learning, educational outcomes and task performance. Early psychology studies on child development emphasized the importance of very close temporal proximity between wrongdoing

[3] Pellegrina (2008) documents a positive link between court delays and crime rates across Italian regions. Dušek (2015), who studies a procedural reform that shortened criminal procedure in the Czech Republic, finds weak effects on burglaries and embezzlements. The results from the lab experiment in Buckenmaier et al. (2021) point to a non-monotonic effect of celerity, with immediate and most delayed punishment being equally effective.

[4] The timing of punishment is also discussed in studies of three-strike laws (e.g., Helland and Tabarrok, 2007).

[5] This caveat applies to many interventions which include swiftness as one of many treatment dimensions (e.g., Kilmer et al., 2013; Engel et al., 2022). Note further that later trials testing swift-and-certain programs failed to replicate the findings from Hawaii (Cullen et al., 2016).

and punishment (Watson, 1924; Walters, 1964). Pratt and Turanovic (2018) questioned the practical relevance of these studies, which typically measure delay in seconds. Our results indicate that the positive effect of swift punishment on payment compliance is indeed limited to very swiftly communicated fines. While this confirms the importance of practical constraints, the result also highlights the potential benefits of leveraging swiftness as a policy instrument.

2 Setting, Data and Variation

Our study builds on data from speed cameras operated by a local authority in Ricany.[6] The town serves as an administrative center for a large area in the south-east of Prague, Czech Republic's capital. In 2014, the local authority installed fixed speed cameras along five major commuter roads. For all cars that pass though, the cameras measure the speed over a section (henceforth *zone*) of several hundred meters.[7] The system records data on *all* cars, independently of their speed, and thus allows tracking cars' driving histories at the five locations. The cameras are visible (see Figure A.1), but they neither look like 'regular' speed cameras nor is there any warning sign or 'flash' that would indicate the cameras' activity. Finally, note that it is very time-consuming for drivers to avoid the monitored road sections by choosing alternate routes.

Fines. As in most settings, the penalty for speeding is a stepwise increasing function of the measured speed (Traxler et al., 2018). For any speed below 14 km/h above the limit, no speeding ticket is issued. This cutoff thus implies that 'negligent' speeding is de facto tolerated. Speeding between 14 and 23 km/h above the speed limit is punished with a fine of 900 CZK ($ 40). Our analysis will focus on such *minor* offenses. Intermediate speeding offenses, with a measured speed between 23 and 43 km/h above the limit, result in a fine of 1900 CZK ($ 85).[8] There are no enhanced penalties for recidivism; the fine does not increase with subsequent offending, it is not communicated to car insurance providers and it is independent of a driver's income (Kaila, 2023).

Speeding Tickets. The local authority receives daily batches of new speeding offenses recorded by the speed cameras and administrates the sending of tickets to the registered owners of speeding cars. A speeding ticket communicates the day, time and place of the offense, the fine as well as the payment deadline: 15 days after the ticket delivery. If the full fine is paid on time, the case is closed. Otherwise the authority follows up with further enforcement steps. A late penalty applies, which may increase the total amount due up to 5,000 CZK ($ 220).[9]

[6] Dušek and Traxler (2022) and Dušek et al. (2022) provide further details on the camera system and its' administration.

[7] At the entry and at the exit point of each camera zone, a precise time stamp is recorded together with the vehicle's number plate. Based on these measures, the average speed is computed. At four road sections, the speed limit is 50km/h, one section has a limit of 40km/h.

[8] Offenses with a speed of more than 43 km/h above the limit are handled according to different procedures.

[9] The car driver may additionally be punished by a deduction of demerit points. However, this occurs very rarely in our setting.

2.1 Data

The local authority provided us with rich administrative data. First, the data cover the universe of all rides recorded by the speed cameras between 2014 and 2018. For 26 million rides, we observe the exact time of entering and exiting a camera zone, the measured speed and a number plate-based, anonymized ID (see fn. 7). As the camera system records all rides, irrespective of the speed, we can thus observe the entire driving history – speeding and non-speeding rides – of each single car that was ever recorded by one of the five speed cameras.

A second data provide detailed information on all 56,000 speeding tickets issued during the sample period. We observe the prescribed fine, the amount paid, the payment date as well as the sending day, dispatch mode (postal or electronic mail), and the date of delivery. Recall that the latter date marks the start of the deadline period.[10] The data also indicate the region where a car is registered, whether its' owner is an individual or a legal entity (i.e., a company).

Summary statistics for the raw data indicate that 13.5% of all rides are above the speed limit. Given the generous enforcement cutoff (see above), only 0.3% of rides result in a ticket (Table A.1, Column 1). The average offense is recorded with a speed of 17.32km/h above the limit; 89.7% are minor speeding offense. 60.2% of tickets are sent via postal-mail; 29.7% are sent to car owners from the local region, 47.9% to owners from nearby Prague (Table A.2, Column 1).

2.2 Celerity: Measurement and Variation

Our preferred measure for the celerity of punishment – operationalized by the communication of the fine – is the time gap between the speeding offense and the day a ticket is sent. Both of these days are clearly and saliently communicated in the speeding ticket. In the raw data, the averages sending–offense gap is 24.12 days (Table A.2, Column 1). Note that this measure does not account for the date when a ticket is delivered, because the delivery date is potentially endogenous.[11] The sending day, in contrast, is shaped by administrative processes which are, as we argue below, exogenous from an individual driver's perspective. Moreover, the time of sending also corresponds to the authority's main policy variable to influence the celerity at which offenders are exposed to a first law enforcement response. Yet, the results reported below are basically insensitive to using the time gap till delivery. This reflects that our preferred celerity measure is strongly correlated with the time gap between the offense and the actual delivery day ($r = 0.986$). The average [median] delivery time is 4.96 [3] days (Table A.2, Column 1); 34% of tickets sent by postal mail are delivered within two days and 50% of electronically sent tickets are delivered (i.e., opened) on the same date.

[10] For registered postal mails, the date tracks when the recipient signs a paper slip that confirms the delivery. For electronic mails, which are sent via an official e-government platform, the delivery date is when the message is first opened. E-mails are used for almost all companies; postal mails are mainly used for private car owners.

[11] An individual who gets notified to pick up a registered postal mail at the post office might have a sense that this is associated with a legal fine. Someone who is less inclined to pay the fine might also be less likely to (quickly) respond to the notification; she might pick up the ticket later, implying a longer delay. For postal mails, delivery time is also correlated with the spatial distance to Říčany. For tickets sent electronically, the delivery time is correlated with the frequency at which e-governance messages are opened.

Our sample covers two distinct periods in which different administrative procedures created distinctly different variation in the swiftness and delay at which speeding tickets are sent: a period of voluminous backlogs and catch-up as well as a period in which we randomized the sequence at which tickets were processed.

2.2.1 Backlog Period: Delayed Tickets

In September 2014, four speed cameras were fully functional, the administration was all set and the first offenses were recorded. Yet, each single camera zone required separate regulatory approval by the national police. These approvals were still pending and prevented the authority from sending out tickets. Eventually, the legal approvals were granted between mid October and early December 2014. Once an approval was given, all accumulated tickets from that camera zone were sent out in a matter of days. The staggered approval induced significant variation in the celerity of punishment: for a given offense week, some tickets were sent relatively swiftly (because the relevant speed camera was approved early on) while others tickets were delayed by several weeks. This point is illustrated in the box plots of Figure 2 (a): for the first six weeks of the sample, the delay in sending varied between one day and two months.[12]

Figure 2 about here.

In the first weeks of 2015, the caseload allowed administrators to process tickets relatively quickly. As a result, the median time till sending a ticket declined from about 20 to less than 5 days. In March 2015, the fifth speed camera zone started to operate. The new camera generated by far the most tickets and implied a substantial increase in the caseload. The daily influx of new offenses quickly exceeded the number of tickets sent out and, in turn, a large backlog started to accumulate: the median time gap from offense to sending increased from 5 to 90 days.

In summer 2015, the local authority took measures to eliminate the backlog. They hired additional staff and re-structured the work flow. One part of the staff would prioritize the oldest cases (defined by the earliest offense day); the other staff members would work on cases that were, at most, 50 days old. The additional manpower resulted in a quick reduction of the delay. As depicted in Figure 2 (a), the decision to work on different segments of delayed cases also generated sizable variation not only between but also within the last 12 offense weeks of this period.[13]

Figure 3 about here.

During the backlog period, the average time gap between an offense and the sending day was 51.6 days (with a SD of 33.4; Table A.2, Column 2). Figure 3 (a) visualizes the distribution in

[12] In line with the definition of our main estimation samples introduced below, the analysis focusses on a car's first ticket for a minor offense. Our main sample also excludes a few outliers with delays of more than 14 weeks (which are rare special cases).

[13] The backlog period can be thus decomposed in three phases: the lack of approvals at the start of camera system, the accumulation of a massive backlog and the crackdown on the backlog. This point is also illustrated in Figure A.4 (a), which presents the variation in the celerity conditional on the offense month.

the celerity of punishment. The distribution spans from one to almost one hundred days, with a pronounced mass at around 90 days. There is a non-trivial amount of tickets sent within 4 weeks, between 4 to 8, 8 to 12, and 12 to 16 weeks (see Table A.3). In contrast, there is only a limited number of swiftly sent tickets. Hence, the variation in this sample period is best suitable for studying the effect of moderate and long delays – relative to tickets sent within 4 weeks.

2.2.2 Randomization Period: Swift Tickets

In cooperation with the enforcement authority, we implemented several changes in the administrators' protocol for processing tickets. (Details of this protocol are discussed in Appendix A.3.) Starting in October 2015, we added a feature to the software interface which assured that, for a given offense day, offenses were ordered randomly. In addition, one staff member would focus on processing roughly a third of all cases from the most recent days. This implied that a random subset of offenders from a given day was 'treated swiftly', with a ticket being sent between one and four days. The remaining cases from that offense day would be added to the backlog pile. These tickets were processed by the other administrator, who always focussed on the oldest cases.

The variation in celerity resulting from this new protocol is documented in Figure 2 (b). For almost all offense weeks, the figure shows significant variation between tickets sent very swiftly and tickets sent with 20 or 30 days of delay.[14] Figure 2 (b) also shows that the upper bound of the distribution declined over time: the were no more longer delays, reflecting the enforcement authority's success in reducing the backlog. In January 2017, our processing and randomization protocol was discontinued.

In the randomization period, the average ticket was sent 10.6 days after the offense day (SD of 7.8; Table A.2, Column 3). The distribution in the celerity is presented in Figure 3 (b). As intended, the distribution exhibits a large mass at the lower end of the distribution: there is a large junk of tickets sent within 4 days after the offense. Motivated by the protocol described above, our later discussion will refer to these as swiftly sent tickets. The figure further indicates a significant mass of cases sent within 5 – 14 and 14 – 28 days. The comparison with the backlog period (Panel (a) of Figure 3) makes clear that the randomization period is much better suited to analyze the effects of swiftness. Note further that this period covers only modest delays (of up to 40 days) and only few observations with a delay of more than 4 weeks (see Table A.3).

3 Empirical Strategies

We estimate the effects of swiftness and delay on two different outcomes: (1) the timely payment of the fine and (2) speeding responses to receiving a ticket. We will also compare our estimates with the results from an expert survey that elicited predictions about the impact of swift and delayed tickets.

[14]The weeks with little or no swift tickets overlap with the Christmas, Easter and the Summer holidays.

3.1 Timely Payments

First, we study the impact of the celerity of punishment on the payment of fines. We constrain the sample to the first ticket assigned to a car and focus on minor speeding offenses. Accounting for the different nature of the variation in the backlog and the randomization period, we separately examine the two sample periods. For a given cross-section of cars, we then estimate linear probability models of the following structure:

$$Y_i = \alpha + \sum_{\tau} \beta^{\tau} Delay_i^{\tau} + \gamma X_i + \lambda_{W(i)} + \lambda_{T(i)} + \varepsilon_i. \qquad (1)$$

Our key outcome variable Y_i is a dummy indicating whether the fine for speeding ticket i is paid before the deadline, i.e., within 15 days after receiving a ticket. In an extension, we also examine payments within 100 and 365 days, respectively.

The variation in the swiftness or the delay of a ticket is captured by a set of dummies $Delay_i^{\tau}$, which indicate if a ticket i is sent, e.g., within 4 days of the offense, between 5 and 14 days, and so on. From Figure 3 we know that range and distribution of delays varies considerably between the backlog and the randomization period. Our main specifications thus employ slightly different sets of dummies for the two sample periods (see Table A.3). While we consistently include a dummy for 'swift tickets', which are sent within 4 days[15], one has to keep in mind that less than 6% of tickets in the backlog period fall into this category. In the randomization period, swift tickets account for 30% of the sample whereas only 3% of the tickets are sent with a delay of more than 4 weeks (see Table A.3).

To account for a possible fluctuations in liquidity within a calender month (tied to, e.g., salary or welfare benefit payment dates; Carr and Packham, 2019), all specifications add fixed effects for the week of the month when a ticket is sent (λ_W). We also control for variables X_i that coincide with what the administrative staff observes in the software while processing the tickets.[16] Our data further allow us to control for car-level characteristics that the ticket administrators *do not* observe: the average speed, the speeding rate and the number of rides in the time period before the offense.[17]

A possible challenge for the identification of the β^{τ} coefficients from equation (1) relates to inter-temporal variation in the composition of speeding offenders. At the beginning of our sample period, for instance, we observe a higher share of local cars which declines over time. While we can account for different number plate regions and many other observed characteristics, the residual variation could be correlated with the inter-temporal variation in the delay of tickets. To

[15] Recall that the 4 day cutoff is motivated by the administrative protocol used during the randomization period (see Section 2.2.2 and Appendix A.3).

[16] Specifically, X_i includes dummies indicating the camera zone, the hour of the day, the day of the week and a dummy indicating if the offense day was a public holiday. We also control for the offense speed (using a third order polynomial) and add dummies indicating whether the car is registered in the local region (Central Bohemia), Prague, or any other region, and a dummy indicating if the owner is a corporation (rather than a private individual).

[17] For 16 to 20% of cars, the speed cameras recorded no ride before the offense (Table A.2). To keep samples constant across specifications, we replace missing values of the pre-offense characteristics with zero and add a dummy for a missing pre-offense ride. Dropping the missing observations yields very similar results.

address this concern, we add fixed effects for the time period of the offense (λ_T). For the backlog period, we present specifications with either quarter or month of the offense fixed effects. We thus estimate celerity effects from the variation in swiftness and delay within a given offense quarter or month (as displayed in Figure A.4 (a)). For the randomization period we have sufficient variation to include day of the offense fixed effects.

Balancing Checks. We conduct several balancing checks to examine the variation in celerity. Firstly, we descriptively compare the distribution of the offense speed. For the backlog as well as for the randomization period, the speed distributions are very similar between offenders from different delay groups (see Figure A.5). This is important, as the offense speed is strongly correlated with our outcome variables. The pattern also suggests that, among minor offenses, administrators did not prioritize tickets with a higher offense speed. Interestingly, this is different for intermediate speeding offenses. The data suggest that administrators slightly prioritized intermediate offenses and send out tickets earlier, in particular during the backlog period. This motivates our focus on minor offenses.[18]

Secondly, we estimate models akin to equation (1) that use observable characteristics as outcome variables. Estimates for the backlog period (which are reported in Table A.4) suggest that car level characteristics (e.g., private vs corporate owners, the region where a car is registered) and the average offense speed are well balanced between swiftly sent and delayed tickets. At the same time, there are clear imbalances between the camera zones. The latter simply reflects that a non-trivial part of the variation in the backlog period is driven by the staggered approval of the different speed cameras.[19] Since all our estimates account for camera zone fixed effects, the between camera variation is absorbed. In fact, adding the camera dummies eliminates all other observed imbalances.

Balancing checks for the randomization period indicate that observable characteristics are very well balanced between swift and delayed tickets. Only for one out of 14 variables we observe significant differences between the different delay categories (see Table A.5). Overall, the analysis indicates that the randomization of the processing sequence and the accompanying admin protocol was successfully implemented.

3.2 Speeding Behavior

Our second analysis makes use of the ride-level data from the speed cameras to examine whether the celerity of punishment influences speed choices after receiving a ticket. Dušek and Traxler (2022) document that speeding tickets cause a strong and persistent decline in travel speed and the speeding rate (the share of rides recorded above the speed limit). The present paper now asks if these behavioral responses differ if a ticket is sent swiftly or with more or less delay.

[18] Since minor offenses account for about 90% of all tickets (Table A.2), the results reported below do not change qualitatively when we include intermediate offenses in our analysis (see Section 5.1.5).

[19] Given that the legal approval to send out tickets from a given camera arrived at different points in time, the amount of initially accumulated delay varied considerably between speed camera zones.

To answer this question, we build upon the event-study framework from Dušek and Traxler (2022). First, we define the event by the date when a ticket is delivered.[20] Including all rides (other than the speeding offense) observed for car i during 32 weeks before and 24 weeks after this event date, we use the within car observations to estimate the model

$$Y_{it} = \beta_0 \, Post_{it} + \sum_{\tau} \beta^{\tau} \left(Post_{it} \times Delay_i^{\tau} \right) + \lambda_i + \lambda_{z(it)} + \lambda_{T(it)} + \gamma X_{it} + \varepsilon_{it}. \tag{2}$$

Our two main outcome variables Y_{it} are the recorded travel speed s_{it} and a speeding dummy (an indicator for speed being above the limit) for a ride of car i at time t.[21] The right-hand side of the equation includes a comprehensive set of controls. In addition to car (λ_i) and camera zone fixed effects ($\lambda_{z(it)}$), we add a large set of dummies absorbing time-specific effects ($\lambda_{T(it)}$) and a vector controlling for time-varying driving conditions (related to, e.g., traffic density and weather; X_{it}).[22] The dummy $Post_{it}$ from equation (2) indicates rides observed after receiving a ticket. We interact this indicator with the same set of delay dummies used in the cross-sectional estimates (see equation (1) and Table A.3). Our parameters of interest, the β^{τ} coefficients, have the interpretation of an *additional* effect of either swift or delayed tickets, relative to a baseline category (tickets with a delay of 5 – 28 days in the backlog period, and 5 – 14 days in the randomization period). The parameters are identified from within-car variation in speed choices and between car variation in the swiftness or delay of tickets.

Complementary to equation (2), we also present graphical evidence based on models that are estimated separately for each delay category τ:

$$Y_{it} = \sum_{w=-32}^{24} \beta_w^{\tau} W_{w(it)} + \lambda_i + \lambda_{z(it)} + \lambda_{T(it)} + \gamma X_{it} + \varepsilon_{it}. \tag{3}$$

The dummies $W_{w(it)}$ indicate the pre- or post-event week w in which an observation is recorded. For a given delay group τ (and conditional on all fixed effects and covariates), the β_w^{τ}-estimates thus capture the expected difference in the outcome between week w and the last pre-event week (week zero, the omitted category).

As in Section 3.1, we focus on minor speeding offenses and examine cars' first ticket event. To assure meaningful within-car variation, the sample is restricted to cars with (a) at least one recorded ride during the 32 weeks before the event and (b) at least one ride during the next 24 weeks. To avoid mean reversion issues (Ashenfelter, 1978), we follow Dušek and Traxler (2022) and exclude the ride that generated the speeding ticket for each car in the sample. This leaves

[20] Focusing on the delivery rather than the sending day increases the precision of our estimates. Since the event-study estimates account for car fixed effects, we are not concerned about unobserved car level factors shaping the exact delivery day (see fn. 11).

[21] The speeding dummy indicates violations of the speed limit. We do not consider (re-)offending, i.e., a dummy for rides above the enforcement cutoff, as it is an extremely rare event (see Table A.1).

[22] We account for calendar month, day of the week, holiday and hour of the day fixed effects. As driving pattern differ between speed camera zones, all these dummies are interacted with the zone dummies. X_{it} accounts for temperature and precipitation (measured at a 10-minute frequency) and includes non-parametric controls for the traffic situation (measured by the time gap to the car j traveling ahead of car i).

us with a sample of $539,850$ rides from $9,069$ cars in the backlog period and $307,531$ rides from $6,687$ cars in the randomization period (see Table A.1, Columns 2 and 3, respectively).

3.3 Expert Survey

To compare our estimates with the predictions from experts, we ran an online survey among Economists and Criminologists. We invited 1,900 scholars who published articles on crime-related topics in general interests journals in Economics, in Law and Economics or Criminology journals between 2005 and 2019. In total, 293 authors opened the survey, 255 answered the first question and 203 completed the survey. (Appendix A.4 provides details on the sample.)

The core questions of the survey first introduced some information on the context and provided the mean for two outcome variables, the rate of timely payments and the drop in the speeding rate after receiving a ticket within 1–2 weeks after the offense. Respondents were then asked to indicate their predictions for the two outcomes for swift tickets, sent within 4 days, and for delayed tickets, sent $4-8$ weeks after the offense. The experts' predictions are presented in Section 5.

4 Theoretical Discussion

4.1 Timely Payments

Consider the decision to pay or not to pay the fine f before the deadline. In case of non-payment, a speeder might get away without any payment. With probability p, however, the authority will enforce the fine plus a late fee, k. Agents form perceptions about the risk p (which is not public information) and the late fee k (which is not properly communicated in the summons; see Dušek et al. 2022). We denote these perceptions by $\tilde{p}$ and $\tilde{k}$, respectively. For a given level of risk-aversion, a speeder is more likely to pay on time, the higher $\tilde{p}$ or the higher $\tilde{k}$.

We argue that the time gap between an offense and the date when a ticket is sent influences a speeder's perceptions. (Recall that the summons reminds the speeder about the exact offense day.) A swiftly delivered ticket might be perceived as a signal about a thorough, well-functioning enforcement authority. As compared to a more delayed ticket, swiftness might therefore increase $\tilde{p}$ (and, potentially, also $\tilde{k}$); the likelihood of a timely payment would increase. In contrast, a ticket that is delayed by many weeks signals that the enforcement process is poorly organized. Experiencing a long delay might thus lead to a lower $\tilde{p}$ (and, potentially, a lower $\tilde{k}$) which implies a lower probability to pay on time.[23]

In addition to this mechanism, which interprets swiftness and delay as signals of more or less strict payment enforcement, there are further channels that would yield similar predictions. First, there is a statute of limitations which constrains the authority to collect fines within one

[23]These effects will depend on agents' prior beliefs about the celerity at which tickets are sent. Some speeders might be positively surprised by a ticket send within two weeks, others might already perceive this as significant delay. Our estimates will capture average behavioral responses.

year after the offense. Long delays thus limit the period in which the authority can enforce the fine. If speeders are aware of this legal rule, the perceived risk $\tilde{p}$ should gradually decline with longer delays. Delay should thus reduce the rate of timely payments. Second, if speeders face memory limitations (as in, e.g., Altmann et al., 2022), one might argue that swiftness increases the chance that they still recall the circumstances of the speeding offense. If there is less doubt about the specific day and location of the associated ride, this might increase the propensity to pay. Finally, in the spirit of negative reciprocity (Fehr and Gächter, 2000), speeders might simply be less willing to pay within 15 days if a ticket arrives two or three months after the offense. This would again lower the rate of timely payments.

4.2 Speeding Behavior

Next we model speed choices. Let the benefits from a ride with speed s_t (net of, e.g., fuel costs) be given by the increasing and concave function $u(s_t)$. In period t, the driver anticipates a risk $\pi^t(s)$ that a speeding ticket with fine $f^t(s)$ will be issued. With probability q^t, the ticket is expected to be delivered (and paid) in period $t+1$.[24] With $1-q^t$, the ticket is expected to be delayed such that costs only occur in $t+2$. Allowing for quasi-hyperbolic discounting (Laibson, 1997), a risk neutral driver's expected utility as of period t is thus given by

$$u(s_t) - \beta\delta\, q^t\, c^t(s_t) - \beta\delta^2\,(1-q^t)\, c^t(s_t), \tag{4}$$

where δ is the discount factor, and $\beta \leq 1$ indicates a possible present bias. The expected fine for a ride at speed s_t, $c^t(s_t)$, is defined by the product $\pi^t(s_t)f^t(s_t)$ (and assumed to be non-decreasing and weakly convex in s_t). One can simplify (4) to $u(s_t) - \beta\delta\, c^t(s_t)\,[q^t\,(1-\delta)+\delta]$. For given expectations $c^t(.)$ and q^t, the optimal speed s_t^* is then characterized by[25]

$$\frac{\partial u(s_t^*)}{\partial s_t} = \beta\delta \frac{\partial c^t(s_t^*)}{\partial s_t}\,[q^t\,(1-\delta)+\delta]. \tag{5}$$

Dušek and Traxler (2022) show empirically that speeding tickets induce learning about the enforcement process.[26] After receiving a ticket in period τ, drivers update the expected (discounted) costs c^τ and, accordingly, adjust their optimal speed, s_τ^*. The question now is whether variation in swiftness or delay of a ticket influences the updating process and the subsequent driving responses. Given that *any* ticket induces an updating in c^τ (Dušek and Traxler, 2022), it seems plausible that the celerity of punishment does not shape the expected fine but primarily the posterior belief q^τ. Swift or delayed ticket would thus induce differences in the *discounted* costs of speeding (Davis, 1988). If $q^\tau_{\text{swift}} > q^\tau_{\text{delay}}$, it follows from (5) that, *cet. par.*, the specific deterrence effect of a swift ticket would be larger than the effect from a delayed ticket: one should expect a stronger drop in the speeding rate and a larger decline in the travel speed.

[24] For the sake of simplicity, we neglect (celerity effects on) payment non-compliance.

[25] We focus on a static (instead of a bandit) problem, implicitly assuming that exploratory speeding is too costly.

[26] They neither model nor study delays, implicitly assuming $\beta = \delta = q^\tau = 1$. Moreover, their evidence does not allow them to discriminate whether drivers update $\pi^\tau(s)$, $f^\tau(s)$ or both.

However, the empirically observed variation in delay (at most 100 days) might result in small differences in posterior beliefs q^τ, which would imply quantitatively limited behavioral responses. Yet, Åkerlund et al. (2016) and Mastrobuoni and Rivers (2016) find that (average) criminals strongly discount the future. If speeders indeed discount with $\delta \ll 1$ (note that q^τ directly interacts with $1 - \delta$ in equation 5), the experienced variation in celerity might induce meaningful differences in behavioral responses. If, on the contrary, speeders discount modestly with $\delta \approx 1$, differences in posteriors q^τ would hardly affect behavior (even if speeders were strongly present-biased; see Lee and McCrary 2017).

Similar predictions can be derived from Beccaria (1764), who emphasized that swiftness strengthens the link between a wrongdoing and its punishment: "*The smaller the interval of time between the punishment and the crime, the stronger ... will be the association of the two ... so that they may be considered, one as the cause, and the other as the unavoidable and necessary effect*" (Chapter XIX, Par. 2). This argument can be re-interpreted in terms of mental accounting (Thaler, 1985). If a ticket is sent swiftly, the speeding offense and the law enforcement response are booked in the same mental account. The association between the offense and the punishment is fully salient, updates in c^τ (and q^τ) should be more pronounced such that swifter tickets induce stronger behavioral responses. A delayed ticket, on the contrary, might be placed in a different mental account than the offense. The link between speeding and its costs would be dissolved (Walters, 1964), which would diminish or even preclude the updating of c^τ (and q^τ). Delayed tickets should thus trigger smaller responses.

Finally, scholars have also argued that more swift punishment might be *more* attractive as it reduces the time spent in uncertainty about whether or not a crime is punished.[27] Such an anxiety mechanism clearly requires that offenders are aware of having committed a wrong. While all speeders in our sample were significantly exceeding the speed limit (recall the non-trivial enforcement cutoff discussed in Section 2), some of them might not be fully aware of having committed a speeding offense until they receive a speeding ticket. Hence, we doubt that this channel is relevant in our setting.

5 Results

Sections 5.1 and 5.2 present the results for the payment and the speed outcomes, respectively. In each section, we separately examine the backlog and the randomization period and compare our findings with the experts' predictions. Section 5.3 links the empirical results to the theoretical discussion from Section 4.

[27] See Section 4.1 in Loughran (2019) for a summary. Buckenmaier et al. (2021) offer a formal analysis.

5.1 Timely Payments

5.1.1 Backlog Period

Table 1 presents our main estimates for the backlog sample. Specifications (1) – (3) include offense quarter, specifications (4) – (6) offense month fixed effects. All models account for week-of-month fixed effects and add control variables for offense and pre-offense characteristics (see Section 3). Across all specifications, the estimates reveal that delayed tickets significantly reduce the rate of timely payments. Our preferred model from column (6) indicates that tickets sent between 4 and 8 weeks after the offense result in a 5.3 pp (7%) lower payment rate than the 72.2% of timely payments observed in our baseline category (tickets sent between 5 days and 4 weeks). For tickets sent between 8 and 12 [12 and 16] weeks, payments decline by 6.6 pp [6.8 pp] (9%). The point estimates further indicate a small, positive but statistically insignificant effect of tickets sent within 4 days after the offense: specification (6) reports an imprecisely estimated 1.1 pp (1.5%) increase in the rate of timely payments relative to the baseline category.[28]

Table 1 and Figure 4 about here.

The results from Table 1 are confirmed in Figure 4 (a), which visualizes the estimates from a model with more refined, weekly delay dummies. Relative to tickets sent between 5 and 14 days, one observes a significant drop in payment rates for tickets delayed by more than 3 weeks. The estimates indicate that the impact of delays on timely payments does not get lager with a longer delay: none of the estimates is statistically distinguishable from the coefficient obtained for a 3 to 4 weeks delay. The pattern suggests that the responses are not driven by the one year statute of limitations, which should induce a more gradual decline in payment rates.

5.1.2 Randomization Period

The randomization period offers considerable variation in swiftness and delay within more narrowly defined offense periods. In addition to specifications (1) – (3), which account for offense month fixed effects, specifications (4) – (6) of Table 2 thus include offense day fixed effects. The results from column (6) indicate a small, positive but statistically insignificant effect of tickets sent within 4 days after the offense: relative to tickets sent between 5 and 14 days, swift tickets increases the payment rate by 1.0 pp (1.3%; upper bound of the 95% confidence interval is 3.5 pp). The point estimate hardly varies across specifications.

Table 2 about here.

In contrast to above, we do not find any evidence for a negative effect of delays. For tickets delayed by more than 4 weeks, our preferred model (6) points to an insignificant 0.5 pp drop in the payment rate (relative to the baseline category). Recall, however, that less than 3% of tickets from this sample have such delays (see Tab. A.3 and Fig. 3). The insignificant effect of

[28] Recall that the backlog period covers only few swiftly sent tickets (see Table A.3).

modestly delayed tickets in the randomization period is also confirmed in Figure 4 (b), which presents estimates for weekly delay dummies. The figure also illustrates that the 95%-confidence intervals overlap with the negative delay effects found in the backlog period.

Making use of the fact that the randomization period includes a much larger number of swiftly sent tickets, we can conduct a more refined analysis of swiftness effects. We augment equation (1) to separately estimate the effect of tickets sent within one, two, three or four days after the offense. The corresponding estimates are presented in Table 3. Specification (6) suggests that tickets sent within one day yield a 4.2 pp increase in the payment rate (p-value: 0.066). Relative to tickets sent between 5 and 14 days (for which 76.5% of the fines are paid on time), this corresponds to a 5.8% increase. For tickets sent on the second day after the offense, the specification yields a quantitatively meaningful but statistically insignificant 1.8 pp (2.6%) higher payment rate.

Table 3 about here.

The effect of tickets sent one or two days after the offense turn out statistically significant in several robustness and sensitivity analyses. In fact, with a slight change of the reference group, the 'day one'-effect becomes significant at the 5%-, the 'day two'-effect at the 10%-level, relative to tickets sent between 4 and 14 days (see Table A.6). Overall, the evidence provides some support for a positive effect of swiftness on payment compliance. Tickets which are sent within one or two days after the offense tend to increase the rate of timely payments by around 3 to 6%.

5.1.3 Expert Predictions

Our results indicate that delays reduce the rate of timely payments: for the backlog sample, we estimate that tickets sent between 4 and 8 weeks after the offense result in roughly 5 pp lower payment rates (Tab. 1, Col. 6). For tickets sent swiftly, within 4 days, our estimates indicate a statistically insignificant 1 pp increase in the rate of timely payments (Tab. 1 and 2, Col. 6).

Figure 5 about here.

Figure 5 shows that most experts' predictions, especially those on the effect of swift tickets, are reasonably close to our estimates. For tickets sent within 4 days, the mean prediction is a 0.6 pp (median: zero) increase in the rate of timely payments (relative to the communicated baseline of 75% for tickets sent within 5 to 14 days).[29] For tickets sent with a delay between 4 and 8 weeks, the mean prediction is a 12.4 pp (median: 5.0 pp) drop in the payment rate (from 75 to 62.6%). Except for the fact that Criminologists predict a significantly stronger effect of delays than Economists ($p = 0.011$), the predictions are similar between the different groups of scholars (see Fig. A.11).

[29] For the backlog sample, our reference group are tickets sent between 5 days and 4 weeks – which differs from the baseline communicated in the survey (1 – 2 weeks). When we estimate effects of swift and delayed tickets relative to the latter reference group, the point estimates hardly change.

5.1.4 Long-run Payment Outcomes

The results reported above consider payments of the full amount of the fine within 15 days. As we observe hardly any partial payments, we obtain basically the same estimates when we consider a dummy for incomplete payments. Yet, one might ask how persistent the effects of swift and delayed tickets are when we consider payments over a longer time period. Before examining this question, it is worth noting that the predictions from Section 4 only hold for timely payments within the 15 days deadline. Moreover, payments after the deadline are difficult to interpret as follow-up enforcement, which targets offenders who did not yet pay the fine, works against finding any differences between more or less delayed tickets (see Traxler and Dušek, 2023).

With these caveats in mind, we re-run our main estimates for payments made within 100 and 365 days, respectively. In line with the endogenous enforcement responses noted above, the negative effects of delays observed in the backlog period become smaller and turn insignificant in most specifications (see Tabble A.7). At the same time, we find slightly larger and, in some specifications, statistically significant effects of swift tickets. For the randomization period, we observe a significant positive effect (+2 pp) of tickets sent within 4 days on payments within 100 days. The estimates also indicate that tickets delayed by more than 4 weeks reduce the 100 and the 365 day payment rates by 2 pp and 6 pp, respectively (see Table A.8).

5.1.5 Heterogeneity

Studying effect heterogeneity across different types of cars, we find little systematic variation. For the backlog period, we only observe some heterogeneity at which point of delay payment rates drop. This might reflect that different subgroups have heterogeneous priors regarding the expected delay of a ticket (see fn. 23). We also find some evidence for a slightly stronger effect of delays on private (vs company) cars and, vice versa, a weakly stronger impact of swiftness on company cars (Figures A.6 and A.7, respectively). We also compared our main estimates, which are based on low severity offenses, with speeding tickets for intermediate speeding offenses (see Section 2). The analysis points to a more pronounced impact of delayed tickets for intermediate speeding offenses (Figure A.8). As the number of these tickets is relatively small, however, the precision of these estimates is limited. The low number of observations also implies that our main estimates do not change when we pool all tickets from low and intermediate severity offenses.

5.2 Speeding Behavior

5.2.1 Backlog Period

We first examine the estimates for the event study model from equation (3). The estimated coefficients indicate that, after receiving the ticket, there is a pronounced 10 to 15 pp drop in the speeding rate and a 1 to 2 km/h drop in the average travel speed (see Figures A.9 (a) and A.10 (a)). Confirming the findings from Dušek and Traxler (2022), these responses occur immediately and are persistent over the 24-week outcome period. In contrast, the impact from swiftness and delay is not clear. For the different levels of delay, we obtain very similar weekly estimates. There is some indication that swiftly sent tickets might amplify the drop in speed and speeding rates (relative to tickets sent between 5 days and 4 weeks). However, the confidence intervals of the different delay groups (which are not presented in the figures) largely overlap.

Table 4 about here.

Panel A of Table 4 presents estimates for model (2) for the backlog period. For the baseline category (tickets sent between 5 days and 4 weeks), Columns (1) and (3) indicate a precisely estimated 10.8 pp (44%) drop in the speeding rate and a 1.4 km/h (3%) decline in the travel speed. Different sets of fixed effects yield virtually identical estimates (columns 2 and 4). The β^{τ} estimates for the interaction terms (which capture differential effects to the baseline) are typically very small and relatively precise for any delay of 4 or more weeks. The data thus suggest that delays do neither weaken nor strengthen the specific deterrence effect of tickets. Swiftly sent tickets, however, tend to amplify the impact of tickets. Specifications (1) and (3) indicate a (statistically insignificant) additional drop in speeding rates by 1.3 pp and a weakly significant 0.34 km/h additional decline in average speed. Alternative specifications of fixed effects (columns 2 and 4) yield similar estimates. Yet, we are cautious in emphasizing these estimates, as they are based on a small number of swift tickets in the backlog period and sensitive to robustness checks discussed below.

5.2.2 Randomization Period

For the randomization period, the event study graphs do not provide any clear evidence on a differential impact of swift tickets on speeding responses (see Figures A.9 (b) and A.10 (b); note the different scale between the corresponding figures for the backlog period). The estimates reported in Panel B of Table 4 confirm this observation. For the reference category, tickets sent between 5 days and 2 weeks, we estimate a 8.2 pp (37%) drop in the speeding rate and a 1.1 km/h (3%) decline in speed (see columns 1 and 3). Tickets sent within 4 days after the offense do not cause any additional effect on the speeding rate (columns 1 and 2). For the travel speed, the point estimates are weakly significant and positive, indicating a 0.17 and 0.19 km/h *smaller* effect from swift tickets (columns 3 and 4, respectively). For shorter (2 – 4 weeks) or longer delays (4 – 6 weeks), the estimates are typically small and provide no clear pattern on the role of delays for speeding responses.

In line with our analysis of timely payments in section 5.1.2, we also estimated more refined models that distinguish the effects of tickets sent on the first, second, third or fourth day after the offense. These estimates do not indicate any systematic, additional effects of such swiftly sent tickets. Overall, the data from the randomization period do not provide any evidence that swiftness would amplify the specific deterrence effect of speeding tickets.

5.2.3 Expert Predictions

Our estimates indicate that neither swiftness nor delay have a strong, consistent impact on speeding behavior. This finding markedly differs from the experts' predictions collected in our survey. Figure 6 documents that experts assigned a much more important role to swiftness and delay. In particular, swiftly sent tickets were expected to produce a 5.6 pp (median: 5.0 pp) larger drop in the speeding rate (15.6 pp as compared to a baseline effect of 10 pp for tickets sent within 1–2 weeks). Delayed tickets were, on average, predicted to produce a 3.4 pp (median: 5.0 pp) smaller drop in the speeding rate (6.6 pp instead of 10 pp).[30]

Figure 6 about here.

5.2.4 Robustness: Attrition

The sample requirement to observe at least one pre- and one post-ticket ride implies that the number of cars covered by the event study estimates from Table 4 is smaller than the corresponding cross-sectional samples used to analyze payment outcomes. In fact, the chances that a car meets our sample criterion might depend on the delay of the ticket itself.[31] For the backlog period, the data confirm that sample attrition is indeed correlated with the length of the delay (Table A.9, column 1). For the randomization period, where delays are much shorter, differential attrition is not an issue (column 3).

To assess the influence of attrition, we implement event study estimates that replace car fixed effects with a rich set of car and offense level controls (see fn. 16). The main advantage of these alternative models is that we can relax the sample requirement of observing one pre- *and* one post-event observation (other than the ride for which the ticked was issued). Relaxing the sample constraint increases and effectively eradicates differential attrition in the backlog period (Table A.9, column 2). At the same time, the estimates of models without car fixed effects in the larger samples yield results (see Table A.10) that are very similar to our main estimates. A notable exemption concerns the additional effect of swift tickets on speed, which becomes smaller and turns insignificant (columns 3 and 4 of Table A.10).

In additional sensitivity checks, we imposed more stringent (rather than more relaxed) sample conditions that also aim at alleviating differential attrition.[32] When we estimate our main event study model (including car fixed effects) in these more constrained samples, we again obtain very similar estimates. Overall, all our robustness analyses suggest that attrition plays a minor role in our event study setting.

[30] The predictions are similar across scholars from different disciplines (see Figure A.12).

[31] Consider, for instance a car that only drives in the area for four weeks but never returns thereafter. This car will not enter the estimation sample if the ticket is sent with a long delay.

[32] Among others, we restricted the sample (a) to cars for which a ride is recorded every 12, 8, or 4 weeks in the post-*offense* (rather than post-ticket) period and (b) to cars that are observed at least once within the first 2 weeks [4 weeks] and once after 6 weeks [14 weeks] after the offense. The different constraints assure that the sample includes, independently of when a ticket is sent, equally 'regular drivers'; the latter conditions further assure that cars appear at a point in time when they would receive a relatively swift ticket but also when they would get a delayed ticket.

5.2.5 Mechanical Effects of Swiftness and Delay

While speeding tickets generate a sharp and persistent drop in the travel speed and the speeding rate, the event study analyses indicate that these responses are not systematically affected by the swiftness or the delay of tickets. More swift punishment nevertheless generates an important benefit. Since speeding responses occur immediately after a ticket is delivered, the celerity at which a ticket is sent determines the *timing* of the response. To document this point, we re-run the event study analyses using the speeding offense (rather than the delivery of the ticket) as the event. The outcome period then starts right after the offense. Note that this also implies that there is no scope for differential attrition, since outcome windows are defined independently of any delay.

With this alternative definition of the event, we then re-estimate equation (3). The results for the backlog period are illustrated in Figure 7. The graph illustrates the mechanical effect of swiftness and delay: the specific deterrence effect – the drop in speed and speeding rates – is realized earlier if a ticket is sent more swiftly. For more delayed tickets, responses only manifest in later weeks. The mid- and long-run driving responses, however, are very similar for delayed or swiftly treated cars. After week 13, once all groups have received a ticket, the effects are statistically indistinguishable across different delay groups.[33]

Figure 7 about here.

To sum up, there are strong timing effects associated with the celerity of punishment. These mechanical effects are highly policy relevant. Swifter tickets reduce speeding earlier and are thus desirable – even if swiftness does not amplify the magnitude of behavioral response to a ticket.

5.3 Discussion

The empirical analysis provides two sets of results. First, in line with the experts' predictions, the swiftness or delay of tickets influences the timely payment of fines. Delayed tickets produce a non-trivial drop in payment rates. We also find evidence suggesting that very swiftly processed tickets, which are sent within one or two days after the offense, increase payment compliance.

Our preferred interpretation of these results is that swiftness and delay signal how well-organized the enforcement process is. Drivers respond to the signal by updating the expected costs of non-compliance (i.e., the risk of facing a late fee) and, accordingly, are more or less inclined to pay on time (see Section 4.1). The negative effect of delays could, in principle, also reflect the statute of limitation. If longer delays increase the perceived chance that the authority fails to complete the follow-up enforcement within the one-year limit, we should observe that longer delays cause gradually declining payment rate. This is rejected by Figure 4 (a), which shows that payment compliance drops if tickets are delayed by more than 3–4 weeks; longer delays, in

[33] Similar mechanical effects are observed for the randomization period (see Figure A.13). Due to the more modest delays in this sample, however, the differences in the onset of the effects are visually less pronounced.

contrast, do not amplify this effect. Moreover, very swiftly sent tickets should have no positive effect under the statute of limitation channel. Table 3 rejects this prediction.

The second main finding is that the celerity of punishment does not systematically influence the size of specific deterrence effects. Once a ticket arrives, drivers immediately reduce their speed. This implies that celerity has a mechanical effect on the timing of behavioral responses. In our simple model from Section 4.2, agents would update expected costs, $c^{\tau}(s)$, and adjust their optimal speed s^*_{τ} either in period $\tau = t + 1$ or $\tau = t + 2$, depending on whether the ticket arrives swiftly or with a delay. Figure 7 confirms this effect. The graph also illustrates that the mid-run decline in average speed and the drop in speeding rates is quantitatively similar for very different levels of swiftness or delay. This conflicts with the predictions of academic scholars, who anticipated large effects from swiftness and delay. While some results indicate that swiftness might amplify the specific deterrence effect, these estimates are weakly significant, sensitive to robustness checks and identified from a small number of swift tickets in the backlog period. The event study analysis for the randomization period, which has more power to identify swiftness effects, provides no support for a positive effect.

Within our model framework, the evidence suggests that the updating of $c^{\tau}(s)$ is independent from how swiftly or delayed a ticket arrives. This contradicts Beccaria's idea, that the celerity of punishment crucially shapes learning about the link between an offense and the law enforcement response. The experienced celerity might still influence expectations about the swiftness of future sanctions, q^{τ}. As discussed in Section 4.2, the behavioral implications from variation in q^{τ} might be quantitatively negligible as the timing of punishment varies by at most 100 days (Figure 3) and daily discount rates might be modest ($\delta \approx 1$).

6 Conclusions

Our study presents two main findings. First, we show that the celerity of punishment for one behavior – speeding – influences choices in a different dimension: the timely payment of the fine. Speeding tickets which are delayed by more than four weeks cause a 7 to 9% drop in timely payments. For very swift tickets, that are sent within one or two days after the offense, we find evidence suggesting that payment compliance increases by almost 3 to 6%. The evidence indicates that the swiftness or delay of a ticket is interpreted as a signal about how thoroughly or weakly the authority enforces fines.

Second, while all tickets produce an immediate and persistent drop in speeding, we find no systematic, robust impact of swiftness or delay on the magnitude of these driving responses. This conflicts with Beccaria's idea that a longer delay between the commission of a crime and its punishment would diminish specific deterrence effects. The evidence also differs from expert predictions: academic scholars expected large effects from both swiftness and delay. From a theoretical perspective, the data suggest that any ticket makes drivers update their priors about the expected costs from speeding.

The two findings highlight that the celerity of punishment has a different impact on different outcomes. When it comes to timely payments, receiving a ticket in itself conveys little information about the consequences of non-compliance. Hence, there is plenty of scope for swiftness and delay to influence posterior beliefs about the costs of non-payment and, in turn, payment decisions. For speeding outcomes, the mere fact of receiving a ticket is crucial for updating expectations regarding the costs of speeding. Variation in swiftness or delay may, at most, influence the expectations about the timing of future punishment.

Yet, we document clear timing effects associated with the celerity of punishment. As drivers immediately respond to tickets, swiftness mechanically reduces speeding earlier on. From a policy perspective, swiftness is therefore desirable, even if it does not amplify the magnitude of behavioral response to a ticket. A ticket sent out swiftly also generates – independent of any additional effects on payment compliance – an earlier payment. Swiftness thus generates an earlier drop in speeding and yields fiscal benefits for the authority.

How do we think about the external validity of our findings? Regarding the effects on timely payments, we argue that our setting shares important features with many economically relevant enforcement problems. The idea that swift or delayed process management provides a signal about the strength of an institution seems relevant for a vast range of settings, from the collection of fines, fees and taxes by public authorities to private firms chasing outstanding invoices. In all these cases, there could be non-trivial costs or benefits from delayed or swift communication. Our study might serve as a starting point to examine such under-researched effects in other areas.

Regarding the lack of any detectable effects from swiftness or delay on driving behavior, we acknowledge that there are several features of criminal justice processes which are not present in our setting. In addition to lower clearance rates (Blanes i Vidal and Kirchmaier, 2017), delays can also have costs in terms of diminished quality and reliability of evidence: accused persons' and witnesses' memories will be less clear as time passes. One might also argue that delayed punishment corresponds to a delayed resolution of uncertainty, which implies a prolonged period of anxiety for undetected offenders. While the latter channel is relevant for criminal acts where offenders are fully aware of their misdeeds, awareness about speeding offenses might be limited.[34] Overall, however, it is not clear why Beccaria's idea, that a shorter interval between punishment and misdeed strengthens specific deterrence, should not be applicable to our setting. The finding that offenders learn their lesson independently of the swiftness of punishment at least challenges widely-held beliefs, which are also mirrored in our survey responses, about the universal relevance of celerity.

[34] An offender would not be aware of a speeding offense if it is the result of temporary inattention. Recall, however, that minor speeding violations are tolerated in our context. All drivers in our sample were speeding by more than 14km/h above the limit (the enforcement threshold), which is typically a deliberate decision rather than an unnoticed lapse.

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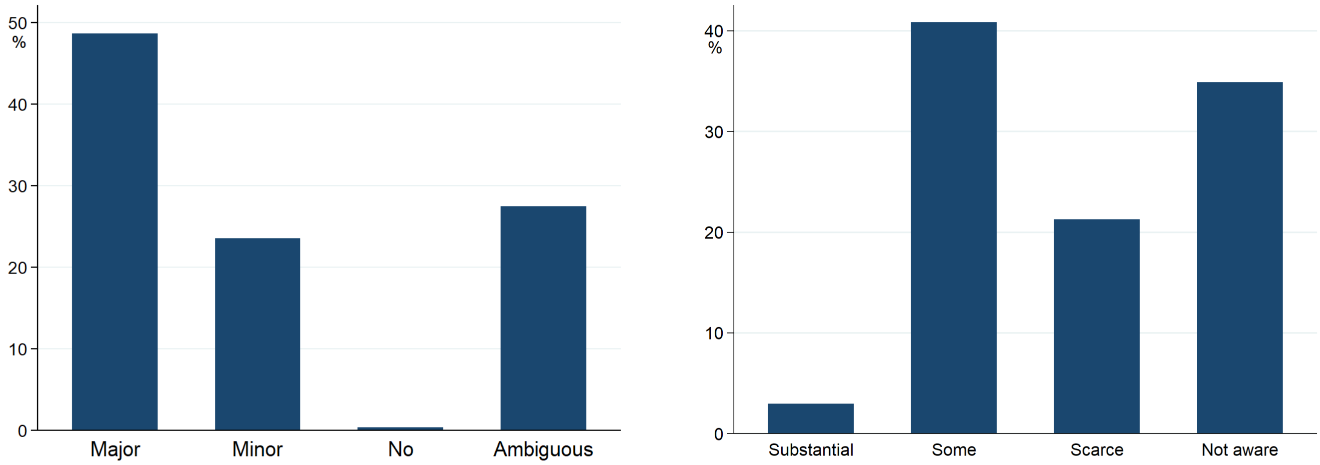
Figures

Figure 1: Survey Responses: Role of and Evidence on Swiftness

(a) Role of swiftness in determining (specific) deterrent effects

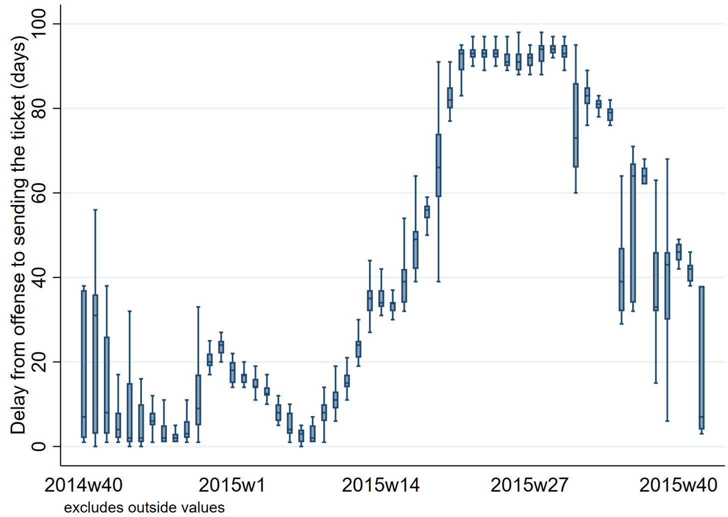
(b) Existing evidence on the role of swiftness



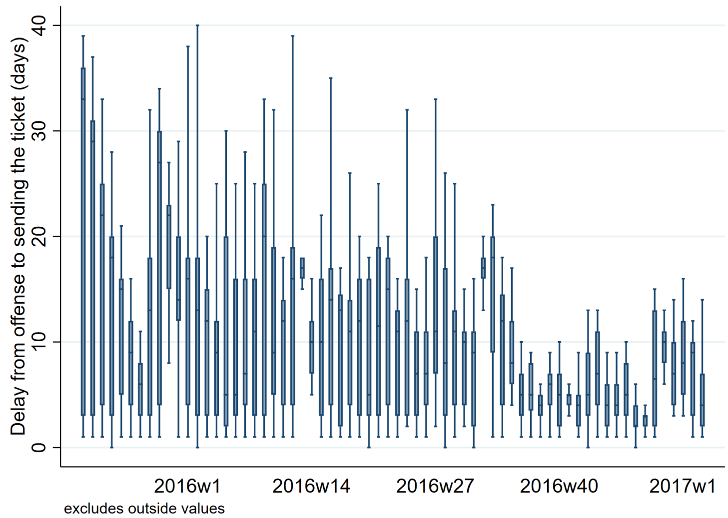
Notes: Panel (a) shows responses to the questions "How would you assess, at a very general level, the role of swiftness in determining the (specific) deterrent effect of punishment? Swiftness plays a .... role." ($N = 255$); Panel (b) considers the question: "How would you assess the empirical evidence on the importance of swiftness for the (specific) deterrent effect of punishment? There is ... evidence" ($N = 235$). Further details on the survey are provided in Sections 3.3. Sample decompositions are provided in Appendix Figures A.2 and A.3.

Figure 2: Variation in Delay by Offense Week

(a) Backlog Period (October 2014 – October 2015)



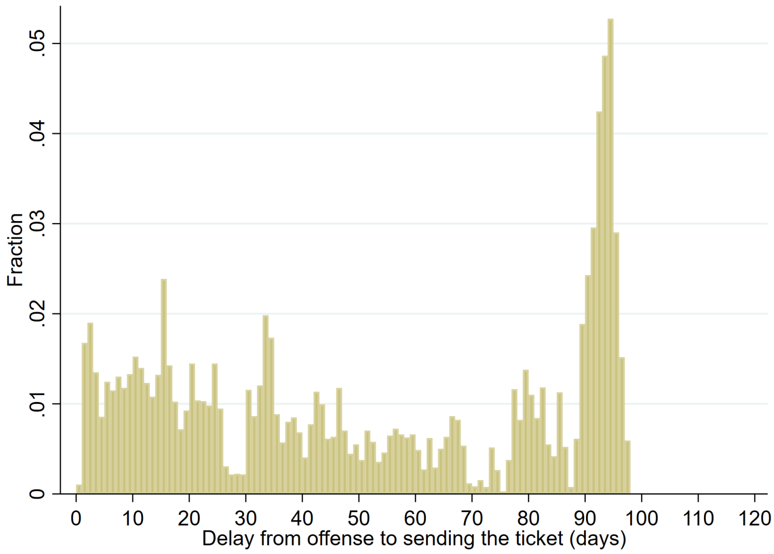
(b) Randomization Period (October 2015 - January 2017)



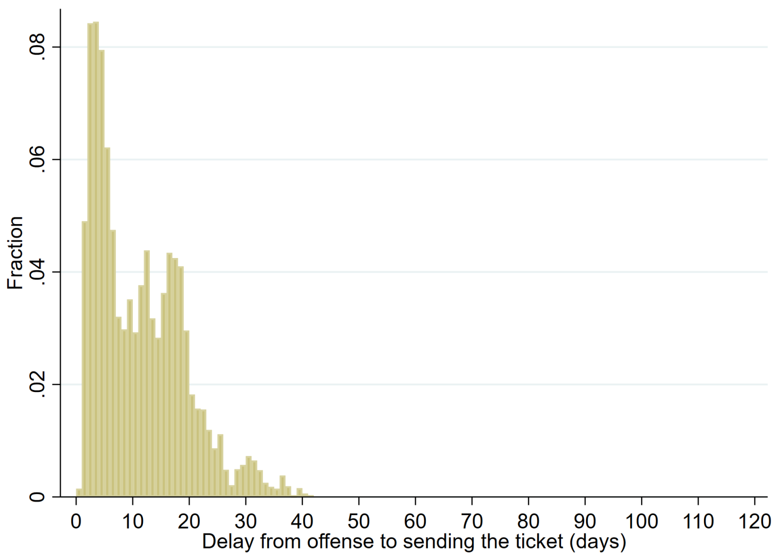
Notes: Box plots of the time gap between the tickets' offense and sending day, conditional on the week of the offense. Sub-figure (a) is for the backlog period, sub-figure (b) for the randomization period.

Figure 3: Distribution of Delay

(a) Backlog Period



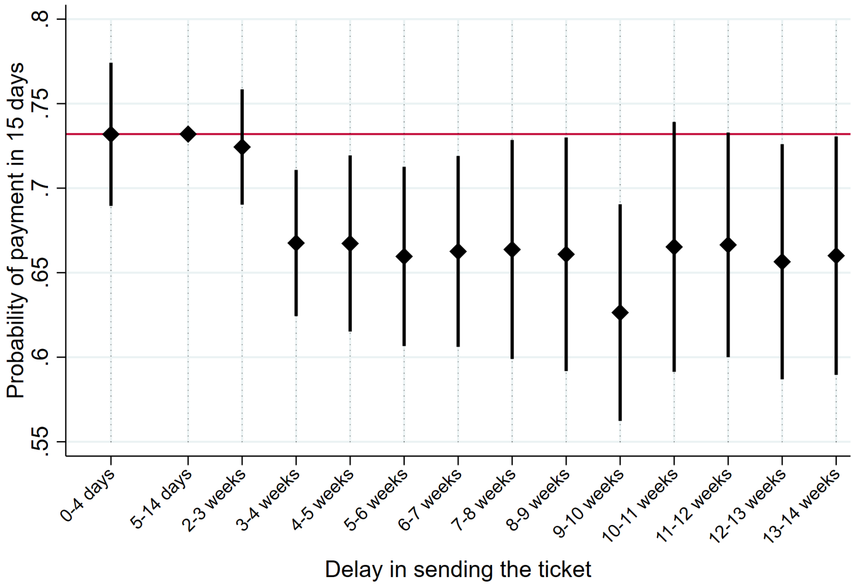
(b) Randomization Period



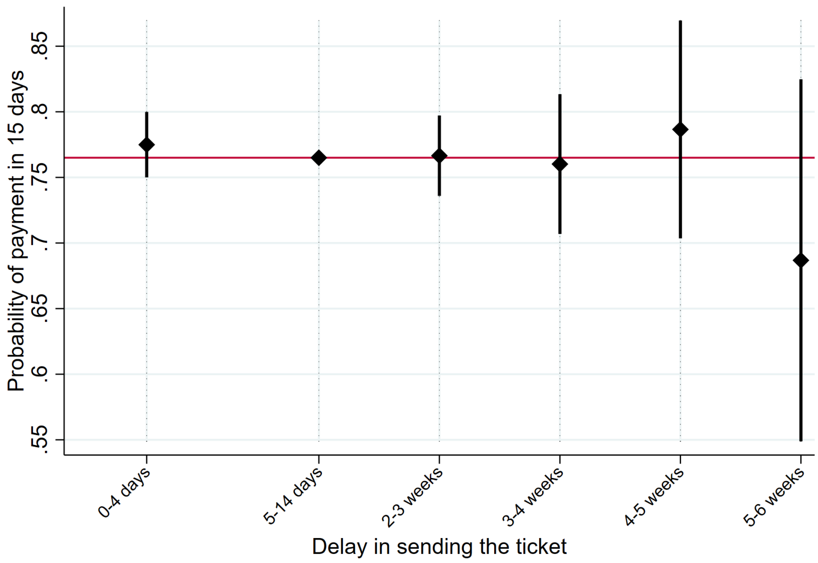
Notes: Distribution of the time gap between the tickets' offense and sending day. Sub-figure (a) is for the backlog period, sub-figure (b) for the randomization period.

Figure 4: Estimated Effects on Timely Payment

(a) Backlog Period

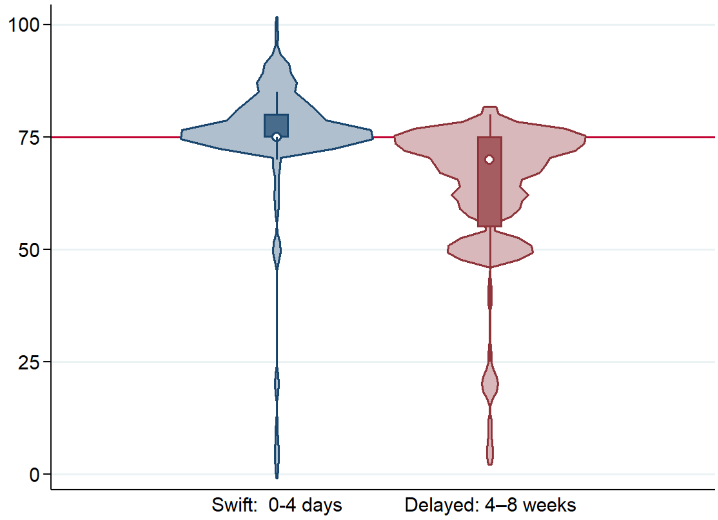


(b) Randomization Period



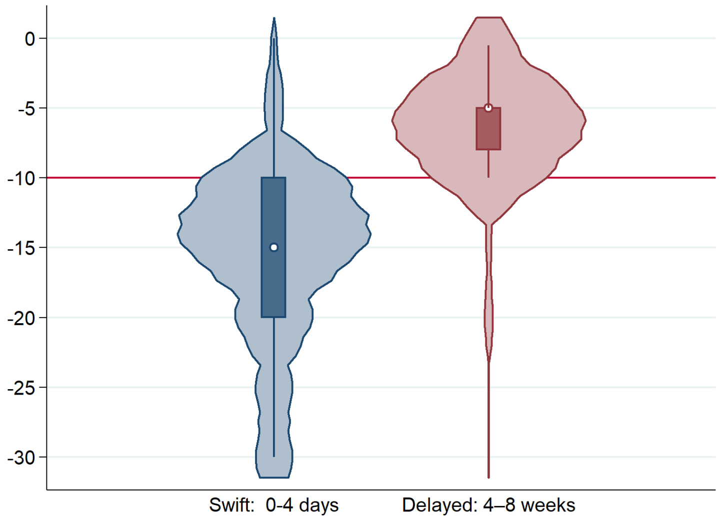
Notes: The figures present the estimated effect of swiftness and delay on timely payments as well as the corresponding 95%-confidence intervals. The estimates are obtained from a extension of (1) that uses weekly delay dummies and includes offense month and week-of-month fixed effects as well as the full set of control variables. The reference category are tickets sent within 5–14 days. Sub-figure (a) is for the backlog period, sub-figure (b) for the randomization period.

Figure 5: Predicted rate of timely payments for swift and delayed tickets



Notes: Violin plots indicating median and interquartile ranges of predictions recorded in the survey. The survey introduced that, among speeding tickets sent within 1-2 weeks after the offense, approximately 75% are paid on time. Participants were then asked to predict payment compliance rates for tickets sent *within 0–4 days* (swift tickets) and *within 4–8 weeks* after the offense (delayed tickets). The mean predicted payment rate is 75.63% for swift and 62.56% for delayed tickets. $N = 203$.

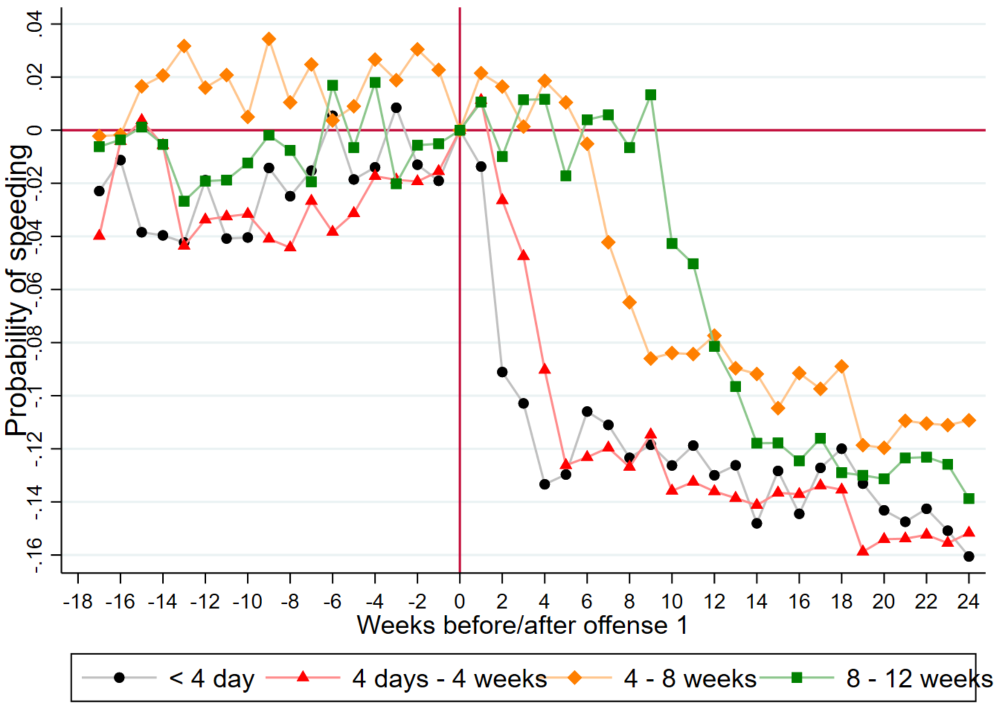
Figure 6: Predicted change in speeding rate for swift and delayed tickets



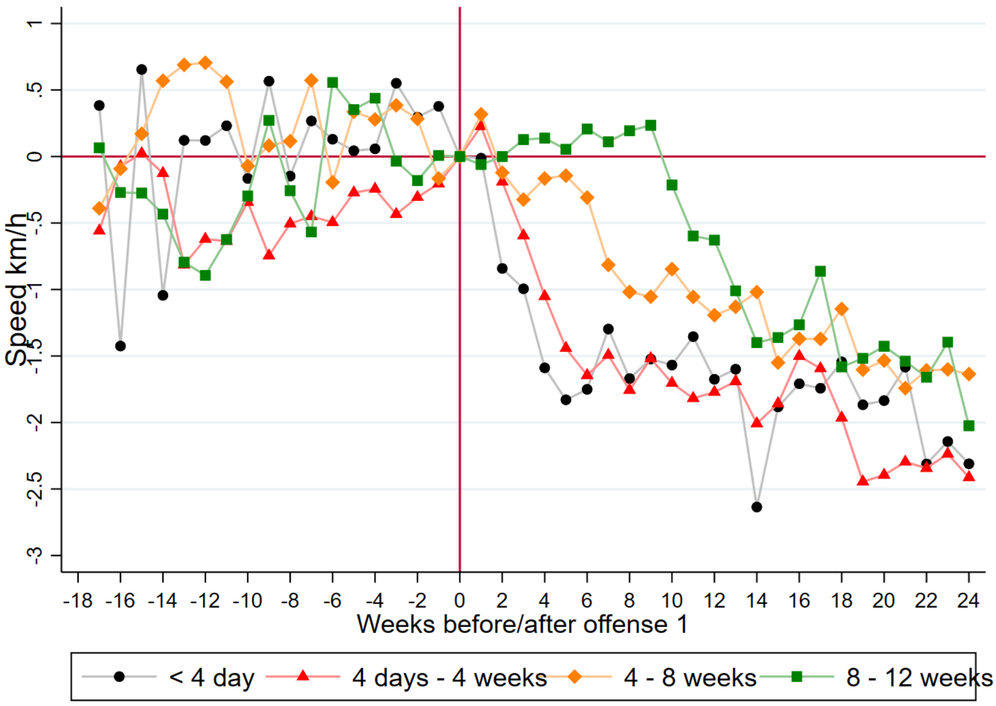
Notes: Violin plots indicating median and interquartile ranges of predictions recorded in the survey. The survey introduced the find that speeding tickets sent within 1-2 weeks after the offense trigger a 10pp (percentage point) drop in the speeding rate. Participants were then asked to predict the effect of tickets sent *within 0–4 days* (swift tickets) and *within 4–8 weeks* after the offense (delayed tickets). The mean predicted effect is a 15.55pp drop for swift and a 6.57pp drop for delayed tickets. Sample is $N = 206$.

Figure 7: Event Study Estimates for Date of Offense: Backlog Period

(a) Speeding Rate



(b) Travel Speed



Notes: In the spirit of equation (3), the figures presents event study estimates for the speeding indicator. Different from our main estimates, the event is defined by the *offense date*. Sub-figure (a) is for the backlog period, sub-figure (b) for the randomization period.

Tables

Table 1: Estimates: Effect on Timely Payment, Backlog Period

	(1)	(2)	(3)	(4)	(5)	(6)
	Offense quarter FEs			Offense month FEs		
Sent 0 – 4 days	0.023	0.015	0.013	0.017	0.013	0.011
	(0.018)	(0.019)	(0.019)	(0.020)	(0.021)	(0.021)
Sent 4 – 8 weeks	–0.040*	–0.053**	–0.052**	–0.041*	–0.054**	–0.053**
	(0.022)	(0.022)	(0.022)	(0.023)	(0.023)	(0.023)
Sent 8 – 12 weeks	–0.045*	–0.058**	–0.058**	–0.056**	–0.065**	–0.066**
	(0.025)	(0.025)	(0.025)	(0.027)	(0.027)	(0.027)
Sent 12 – 16 weeks	–0.047*	–0.063***	–0.063***	–0.057*	–0.068**	–0.068**
	(0.024)	(0.024)	(0.024)	(0.031)	(0.032)	(0.032)
Baseline mean: (5 days – 4 weeks)	0.722					
Offense controls	N	Y	Y	N	Y	Y
Pe-offense contr.	N	N	Y	N	N	Y
Obs.	14,251	14,251	14,251	14,251	14,251	14,251

Notes: The table presents linear probability model estimates of equation (1) for the backlog period. Specifications (1) – (3) account for offense quarter, specifications (4) – (6) for offense month fixed effects. All specifications include sending week-of-month dummies. The omitted category (reference period) are tickets sent between 5 days and 4 weeks. Robust standard errors are in parenthesis.

Table 2: Estimates: Effect on Timely Payment, Randomization Period

	(1)	(2)	(3)	(4)	(5)	(6)
	Offense month FEs			Offense day FEs		
Sent 0 – 4 days	0.006	0.009	0.009	0.009	0.010	0.010
	(0.010)	(0.010)	(0.010)	(0.013)	(0.013)	(0.013)
Sent 2 – 4 weeks	0.002	0.002	0.002	0.001	0.001	0.001
	(0.011)	(0.011)	(0.011)	(0.015)	(0.015)	(0.015)
Sent 4 – 6 weeks	0.011	0.020	0.020	–0.010	–0.006	–0.005
	(0.032)	(0.032)	(0.032)	(0.038)	(0.037)	(0.037)
Baseline mean: (5 days – 2 weeks)	0.765					
Offense controls	N	Y	Y	N	Y	Y
Pe-offense contr.	N	N	Y	N	N	Y
Obs.	11,232	11,232	11,232	11,229	11,229	11,229

Notes: The table presents linear probability model estimates of equation (1) for the randomization period. Specifications (1) – (3) account for offense month, specifications (4) – (6) for offense day fixed effects. All specifications include sending week-of-month dummies. The omitted category (reference period) are tickets sent between 5 days and 2 weeks. Robust standard errors are in parenthesis.

Table 3: Refined Estimates: Effect on Timely Payment, Randomization Period

	(1)	(2)	(3)	(4)	(5)	(6)
	Offense month FEs			Offense day FEs		
Sent day 1	0.035*	0.039**	0.038**	0.040*	0.042*	0.042*
	(0.018)	(0.019)	(0.019)	(0.023)	(0.023)	(0.023)
Sent day 2	0.022	0.023	0.023	0.021	0.018	0.018
	(0.015)	(0.016)	(0.016)	(0.019)	(0.019)	(0.019)
Sent day 3	–0.001	0.004	0.004	0.010	0.011	0.011
	(0.016)	(0.016)	(0.016)	(0.019)	(0.019)	(0.019)
Sent day 4	–0.022	–0.019	–0.019	–0.017	–0.014	–0.013
	(0.016)	(0.017)	(0.017)	(0.019)	(0.019)	(0.019)
Sent 2 – 4 weeks	0.002	0.003	0.002	0.004	0.003	0.003
	(0.011)	(0.011)	(0.011)	(0.015)	(0.015)	(0.015)
Sent 4 – 6 weeks	0.012	0.021	0.021	–0.007	–0.004	–0.003
	(0.032)	(0.032)	(0.032)	(0.038)	(0.038)	(0.038)
Baseline mean: (5 days – 2 weeks)	0.765					
Offense controls	N	Y	Y	N	Y	Y
Pe-offense contr.	N	N	Y	N	N	Y
Obs.	11,232	11,232	11,232	11,229	11,229	11,229

Notes: The table presents linear probability model estimates for the randomization period, based on an augmented version of equation (1). Specifications (1) – (3) account for offense month, specifications (4) – (6) for offense day fixed effects. All specifications include sending week-of-month dummies. The omitted category (reference period) are tickets sent between 5 days and 2 weeks. Robust standard errors are in parenthesis.

Table 4: Event-Study Estimates for Backlog and Randomization Period

	(1)	(2)	(3)	(4)
Outcome:	Speeding Rate		Speed	
	A. Backlog Period			
Post Ticket	-0.108***	-0.108***	-1.37***	-1.47***
	(0.004)	(0.005)	(0.08)	(0.08)
Post ×	-0.013	-0.017**	-0.37**	-0.34**
Sent 0 – 4 days	(0.008)	(0.009)	(0.17)	(0.17)
Post ×	-0.001	0.003	-0.02	-0.02
Sent 4 – 8 weeks	(0.006)	(0.006)	(0.12)	(0.12)
Post ×	-0.002	0.006	0.09	0.08
Sent 8 – 12 weeks	(0.007)	(0.008)	(0.13)	(0.14)
Post ×	-0.005	0.005	0.09	0.10
Sent 12 – 16 weeks	(0.006)	(0.007)	(0.12)	(0.13)
Pre-ticket mean:	0.248	0.248	43.734	43.734
Number of Cars	9,069	9,069	9,069	9,069
Number of Rides	539,850	539,850	539,850	539,850
	B. Randomization Period			
Post Ticket	-0.082***	-0.087***	-1.10***	-1.17***
	(0.004)	(0.005)	(0.07)	(0.08)
Post ×	-0.004	-0.003	0.19*	0.17*
Sent 0 – 4 days	(0.006)	(0.006)	(0.10)	(0.10)
Post ×	-0.010*	-0.004	-0.12	-0.04
Sent 2 – 4 weeks	(0.006)	(0.006)	(0.10)	(0.11)
Post ×	-0.001	0.012	0.36	0.34
Sent 4 – 6 weeks	(0.017)	(0.016)	(0.24)	(0.24)
Pre-ticket mean:	0.241	0.241	44.548	44.548
Number of Cars	6,687	6,687	6,687	6,687
Number of Rides	307,531	307,531	307,531	307,531

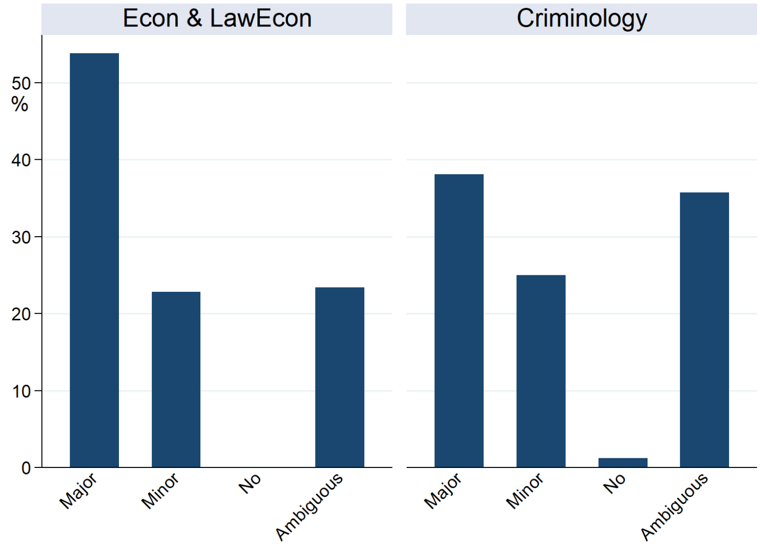
Notes: The table presents estimates of equation (2) for the speeding rate (columns 1 and 2) and the travel speed (in km/h; columns 3 and 4). Panel *A* present the results for the backlog period, where the non-interacted effect (Post Ticket) captures the impact of tickets from the reference group, i.e. tickets sent between 5 days and 4 weeks. Panel *B* presents the results for the randomization period, where the reference group are tickets sent between 5 days and 2 weeks. All regressions include car and camera zone fixed effects; day of the week, holiday and hour of the day fixed effects interacted with the zone dummies; variables measuring weather (temperature and precipitation, measured at a 10-minute frequency) and driving conditions capturing traffic density (dummies for different time gaps to the car j traveling ahead of car i). To control for seasonal variation, the regressions also include interactions of camera zone fixed effects with month-of-the-year dummies (columns 1 and 3) or with calendar month dummies (columns 2 and 4). Two-way clustered standard errors (by car and camera zone-hour) are in parenthesis.

A Online Appendix

A.1 Figures (Online Appendix)

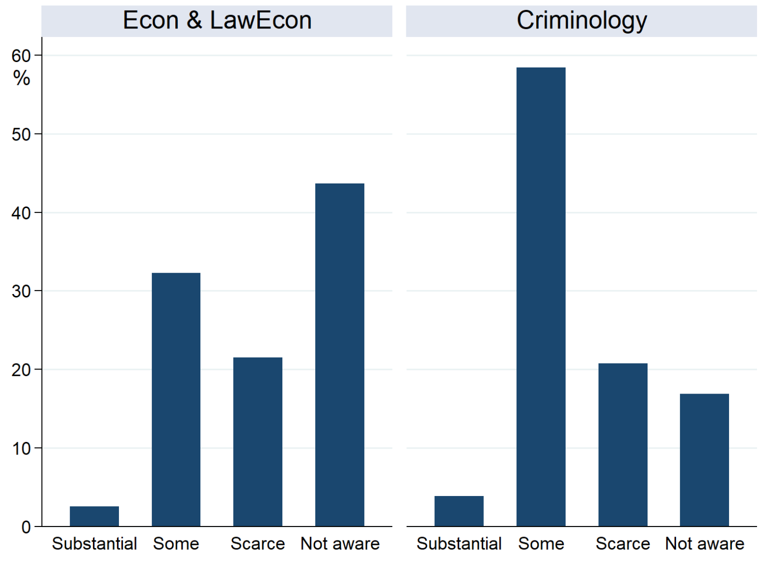
Figure A.1: Picture of the entry point to speed camera zone No. 4

Figure A.2: Role of swiftness in determining (specific) deterrent effects – Split by sample



Notes: Responses from expert survey (split by sample): "How would you assess, at a very general level, the role of swiftness in determining the (specific) deterrent effect of punishment? Swiftness plays a .... role." Econ & LawEcon Sample: $N = 171$; Criminology Sample: $N = 84$; $N = 255$ overall.

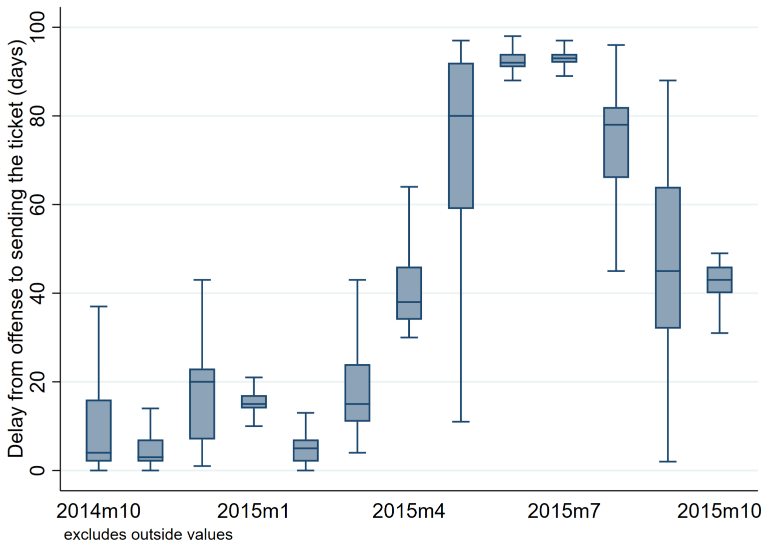
Figure A.3: Existing evidence on the role of swiftness – Split by sample



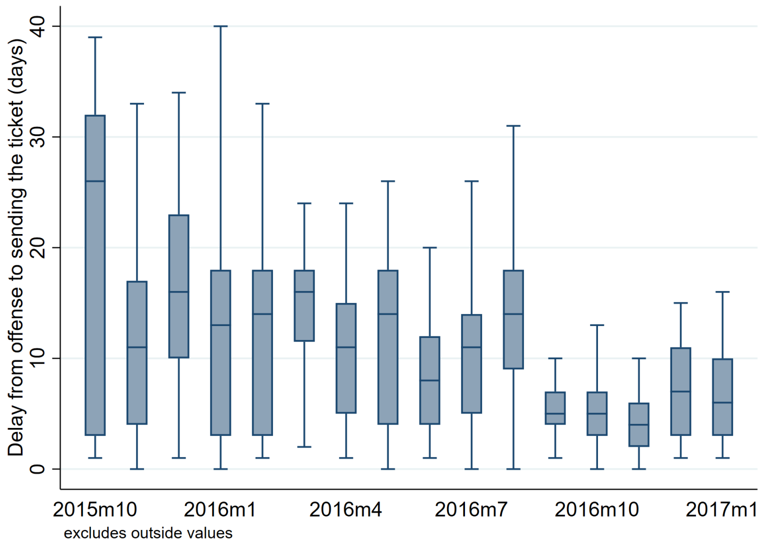
Notes: Responses from expert survey (split by sample): "How would you assess the empirical evidence on the importance of swiftness for the (specific) deterrent effect of punishment? There is..." Econ & LawEcon Sample: $N = 158$; Criminology: $N = 77$; $N = 235$ overall.

Figure A.4: Variation in Delay by Offense Month

(a) Backlog Period (October 2014 – October 2015)



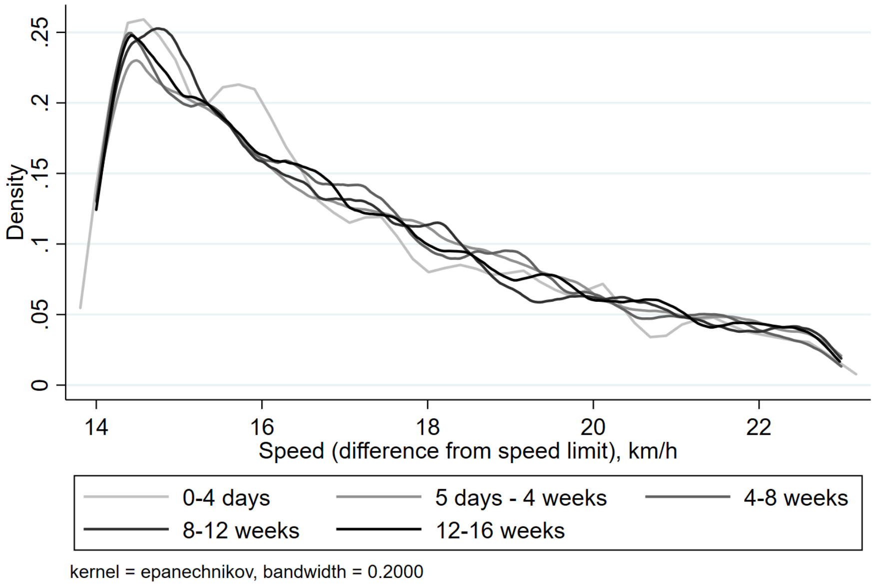
(b) Randomization Period (October 2015 - January 2017)



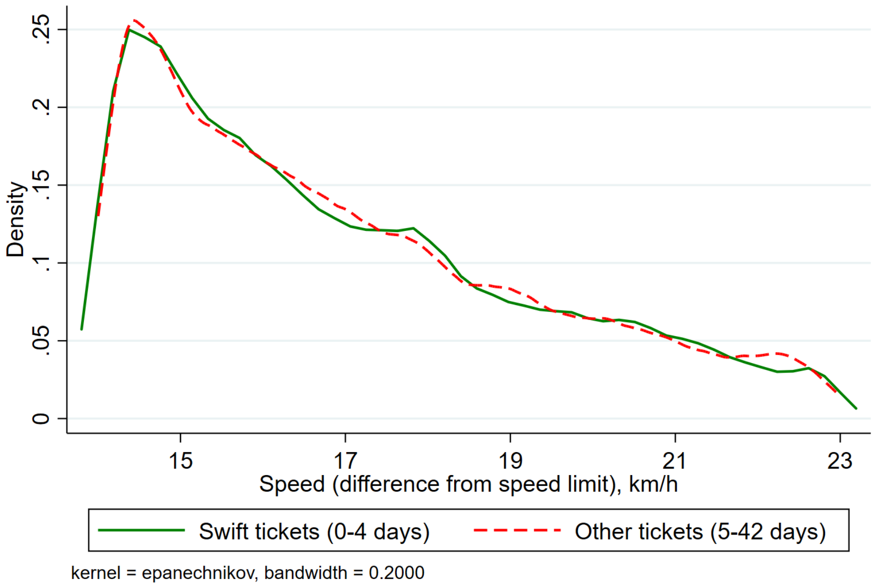
Notes: Box plots of the time gap between the tickets' offense and sending day, conditional on the month of the offense. Sub-figure (a) is for the backlog period, sub-figure (b) for the randomization period.

Figure A.5: Distribution of Offense Speed by Delay

(a) Backlog Period



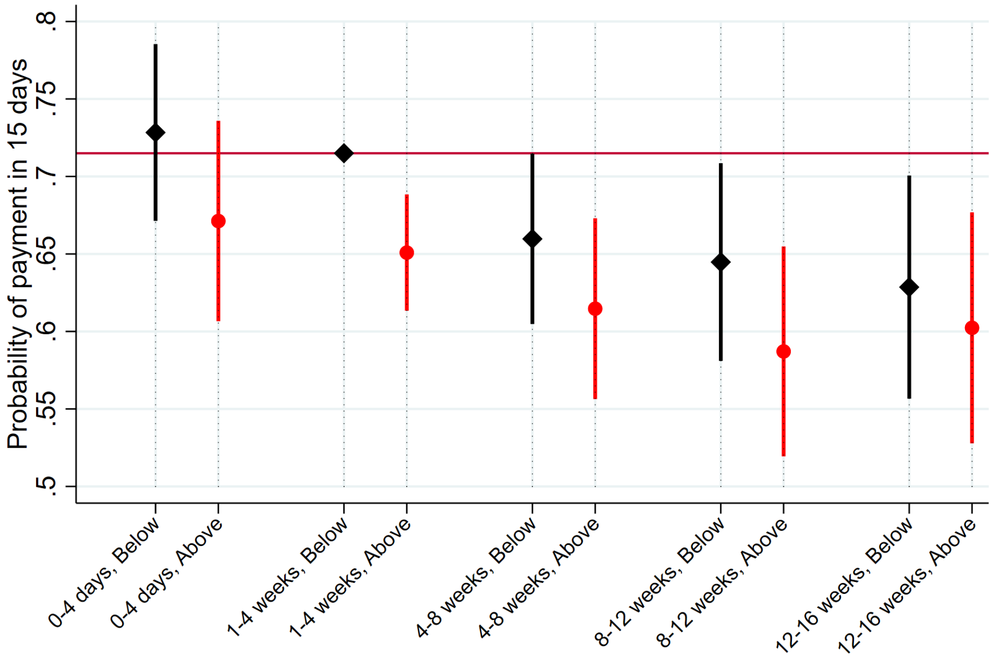
(b) Randomization Period



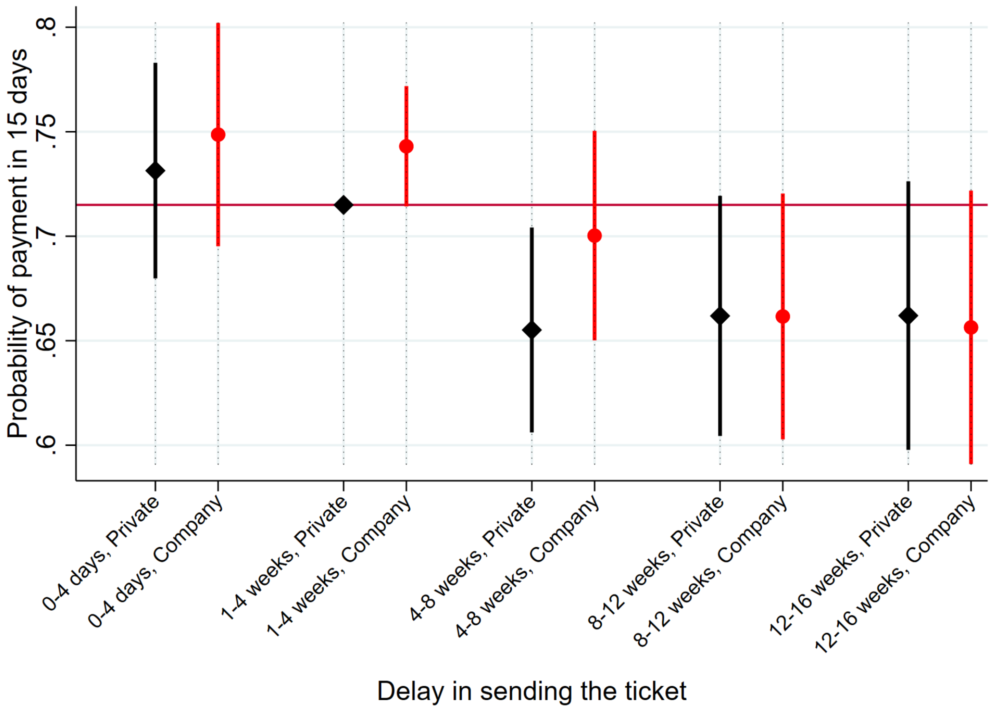
Notes: The figure illustrates the distribution of the offense speed in the different delay categories. Sub-figure (a) is for the backlog period, sub-figure (b) for the randomization period. Note that the left end of the distribution is driven by boundary effects: the drop in the density simply reflects that, by definition, tickets are only observe for a speed of more than 14km/h above the limit.

Figure A.6: Heterogeneity Analysis (1): Effects on Timely Payment in the Backlog Period

(a) Below vs Above Median Pre-Offense Speed



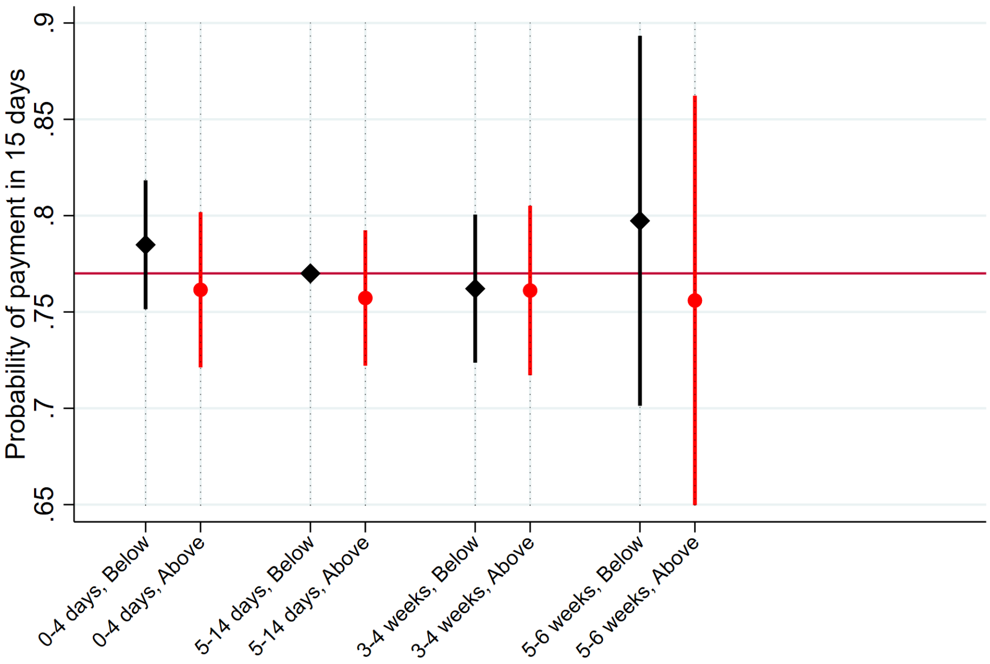
(b) Private vs Company Cars



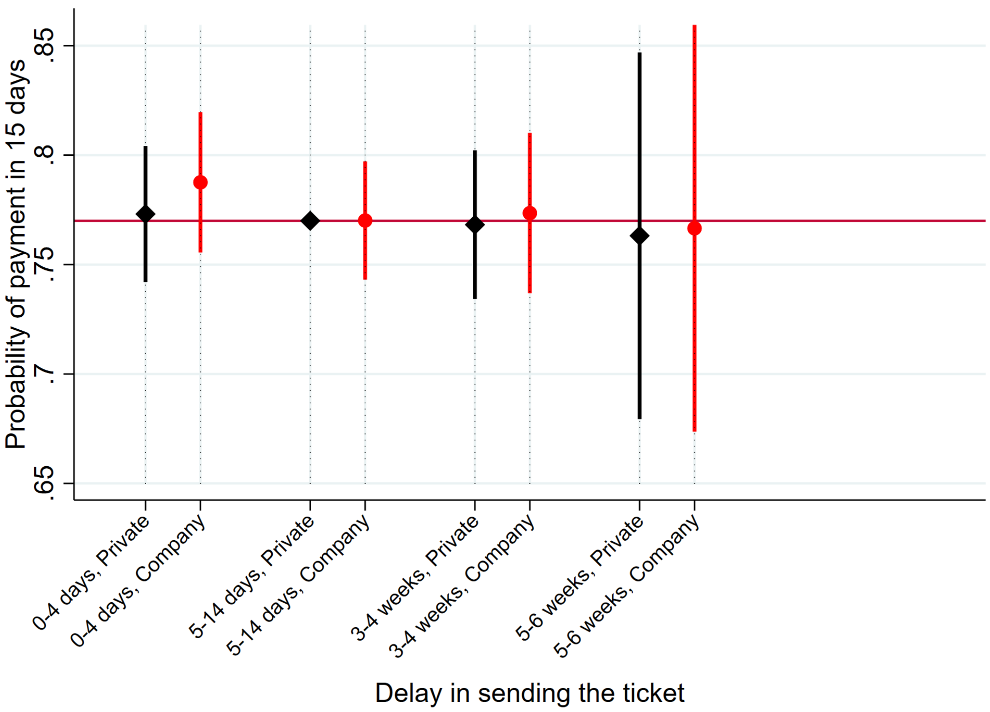
Notes: The figures presents estimates from interacted versions of the model from equation 1 for the backlog period. All specifications include offense month and sending week-of-month dummies (analogous to specification (6) from Table 1). Panel (a) compares cars with an average pre-offense speed below and above the median. Panel (b) compares private and company cars.

Figure A.7: Heterogeneity Analysis (2): Effects on Timely Payment in the Randomization Period

(a) Below vs Above Median Pre-Offense Speed



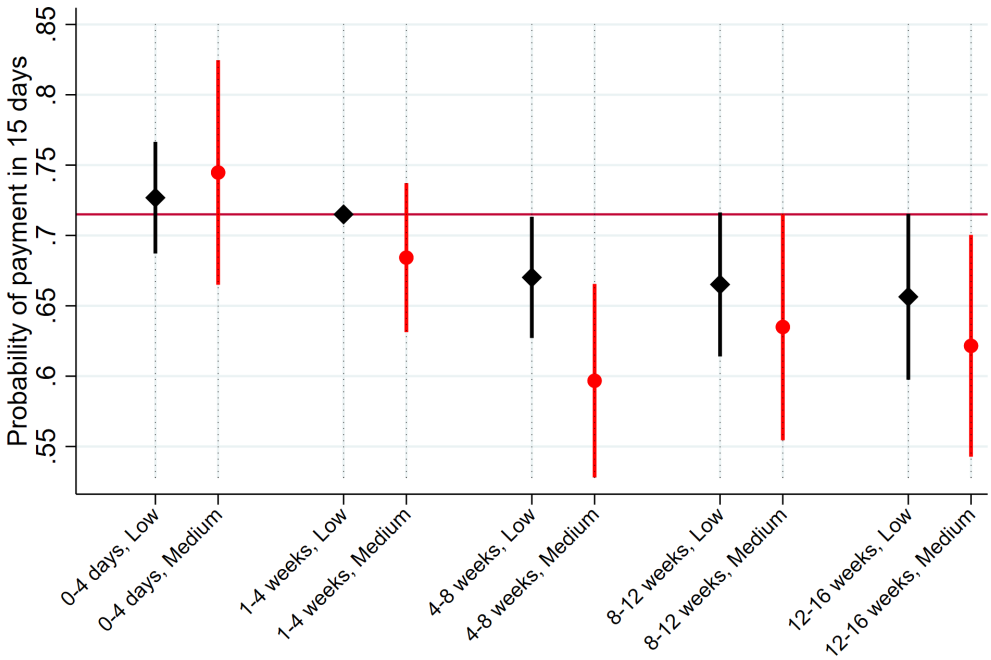
(b) Private vs Company Cars



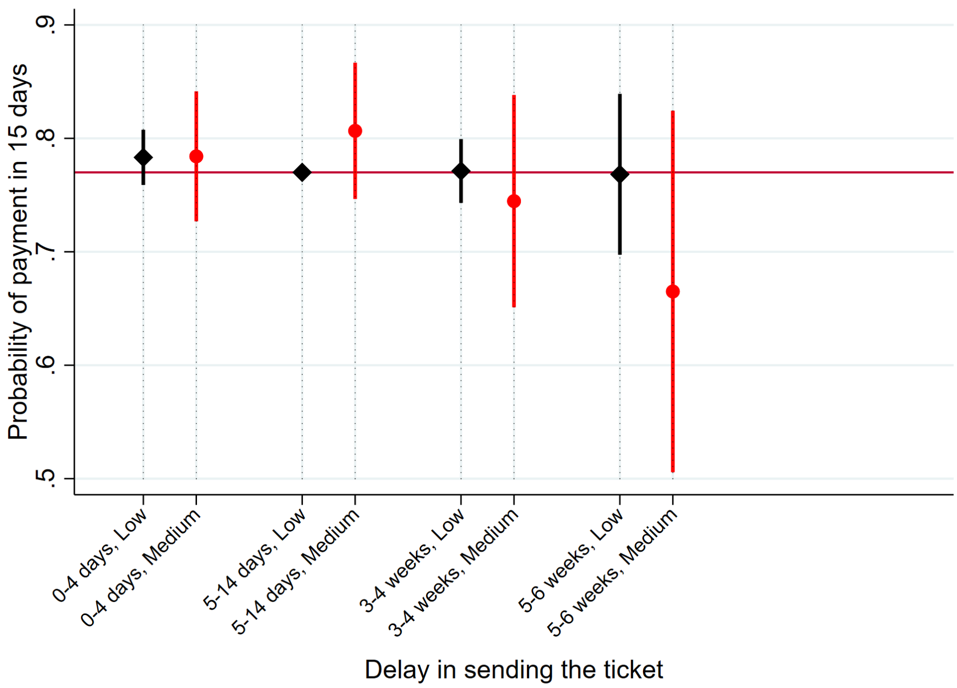
Notes: The figures presents estimates from interacted versions of the model from equation 1 for the randomization period. All specifications include offense day and sending week-of-month dummies (analogous to specification (6) from Table 2). Panel (a) compares cars with an average pre-offense speed below and above the median. Panel (b) compares private and company cars.

Figure A.8: Effects on Timely Payment for Minor and Intermediate Speeding Offenses

(a) Backlog Period



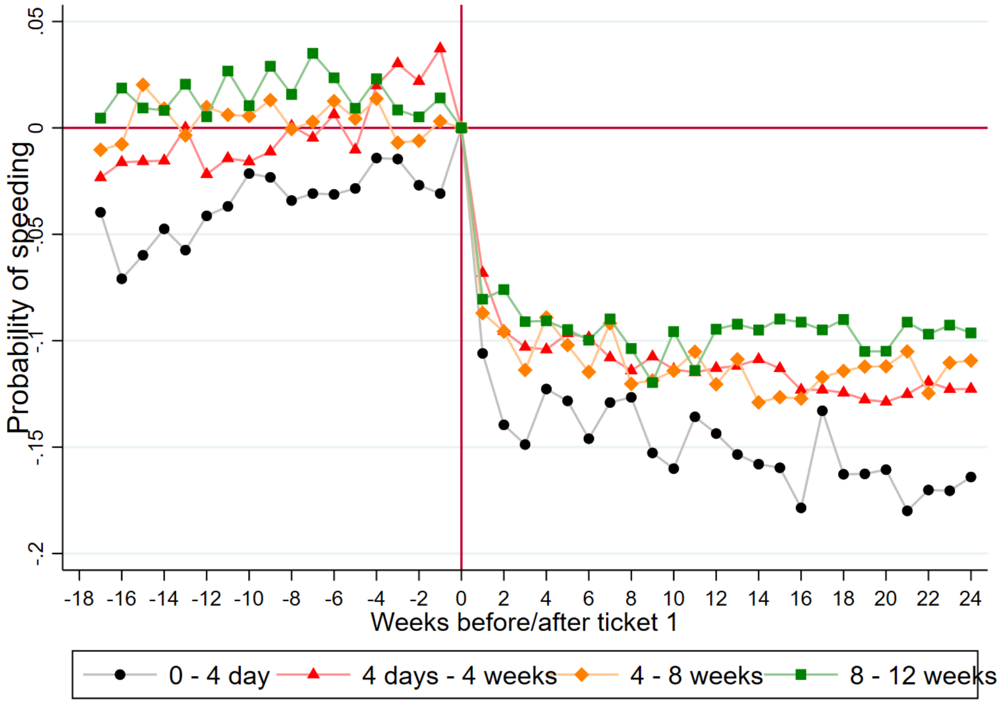
(b) Randomization Period



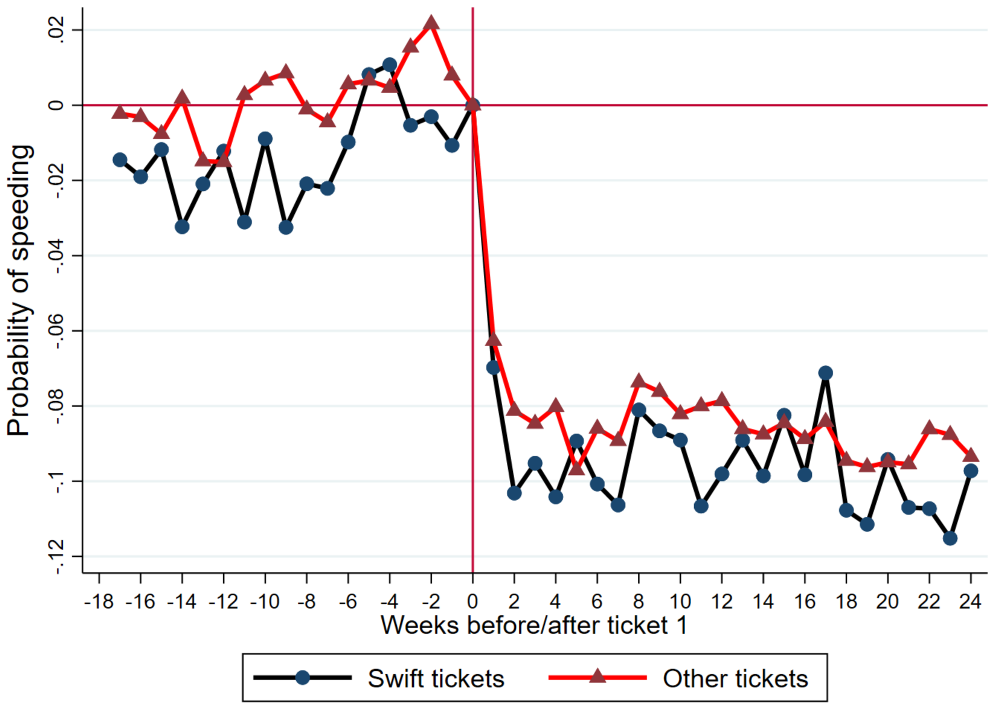
Notes: The figures presents estimates from interacted versions of the model from equation 1 for the backlog period (panel a) and the randomization period (panel b). For the former, the model includes offense *month* and sending week-of-month dummies (analogously to Table 1, Column 6). For the latter, the model includes offense *day* and sending week-of-month dummies (analogously to Table 2, Column 6). The estimates compare the effect of swiftness and delay for minor offenses ('Low') with the results obtained for intermediate speeding offenses ('Medium', which were excluded from our main analysis sample).

Figure A.9: Weekly Event Study Estimates: Speeding Rate

(a) Backlog Period



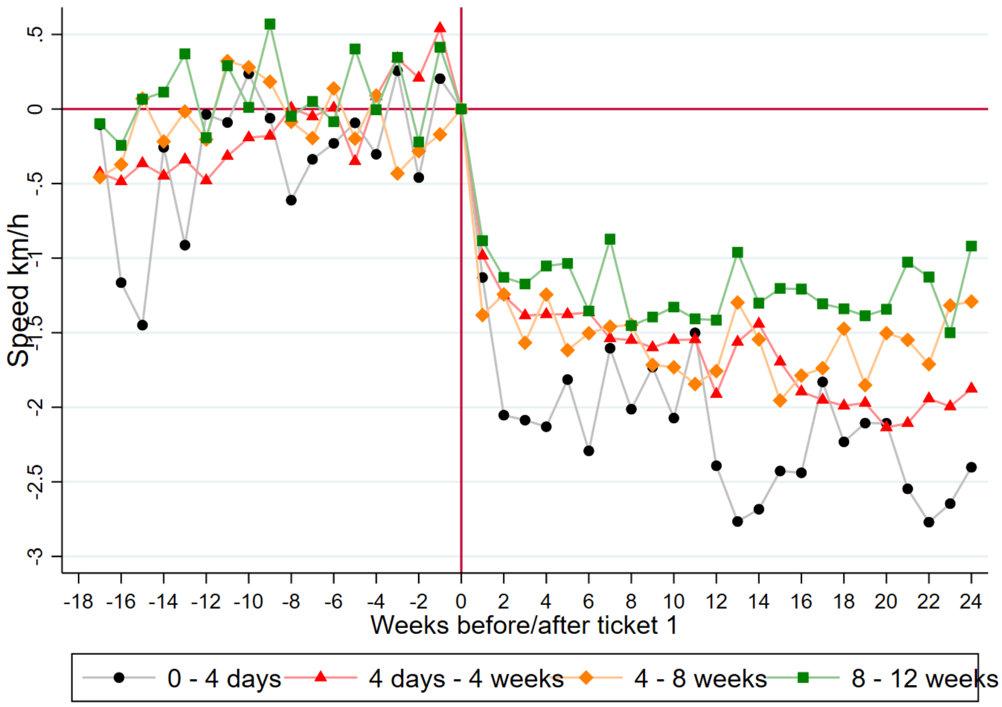
(b) Randomization Period



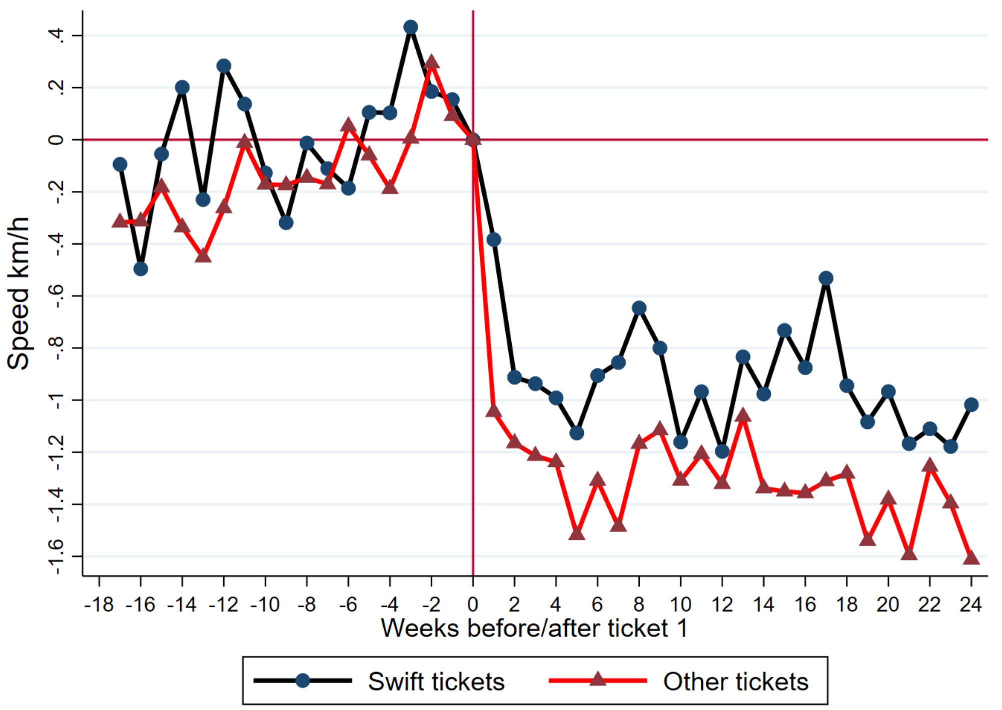
Notes: The figures presents estimates from equ. (3) for the speeding indicator. The coefficients illustrated for week -17 pools the period from week 34 to 17 before the ticket is delivered. Subfigure (a) is for the backlog period, subfigure (b) for the randomization period. Note that the latter subfigure uses a more refined scale.

Figure A.10: Weekly Event Study Estimates: Travel Speed

(a) Backlog Period

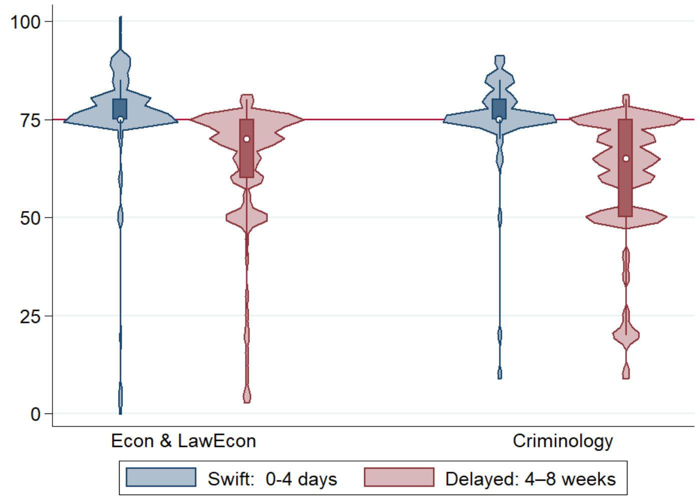


(b) Randomization Period



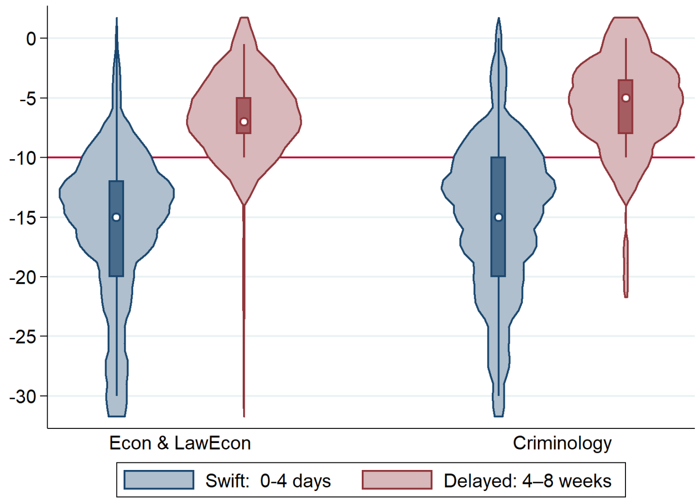
Notes: The figures presents estimates from equ. (3) for the travel speed (in km/h). The coefficients illustrated for week −17 pools the period from week 34 to 17 before the ticket is delivered. Subfigure (a) is for the backlog period, subfigure (b) for the randomization period. Note that the latter subfigure uses a more refined scale.

Figure A.11: Predicted rate of timely payments for swift/delayed tickets – Split by sample



Notes: Violin plots (indicating median and interquartile ranges of predictions recorded in the survey) split by samples. The survey first introduced that the payment rate for speeding tickets sent within 1-2 weeks after the offense is 75%. Thereafter, the survey asked participants to predict the payment rate for tickets sent *within 0–4 days* (swift tickets) and *within 4–8 weeks* after the offense (delayed tickets). Among the Econ & LawEcon sample ($N = 136$), the mean predicted payment rate is 75.85% for swift and 63.81% for delayed tickets. In the Criminology sample ($N = 67$), the mean predictions are 75.22% (swift) and 60.02% (delayed), respectively.

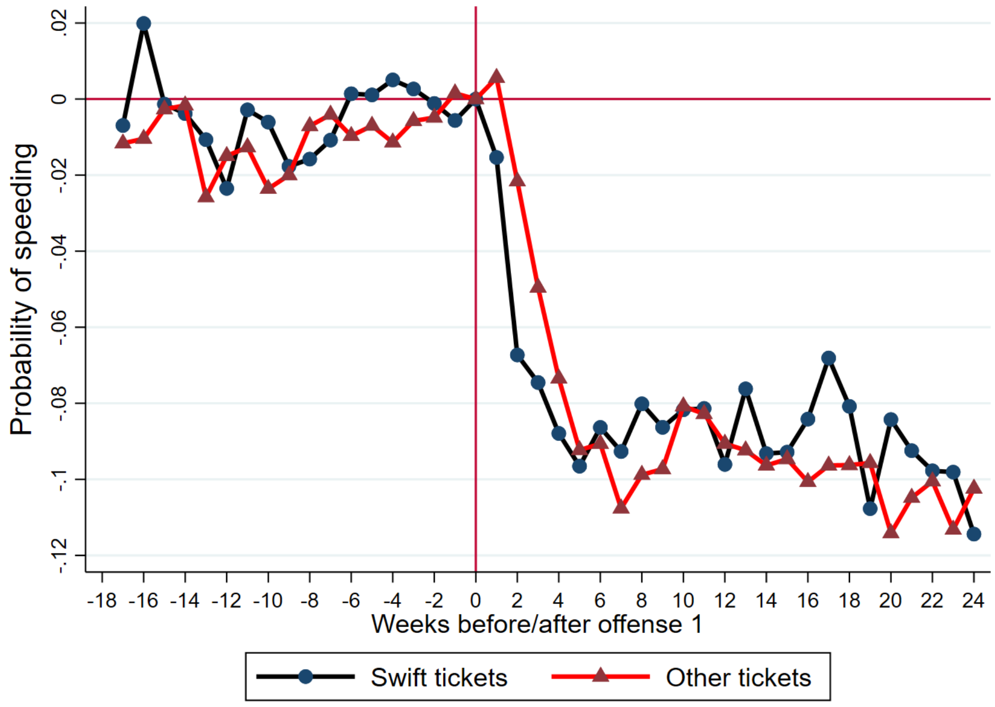
Figure A.12: Predicted change in speeding rate for swift/delayed tickets – Split by sample



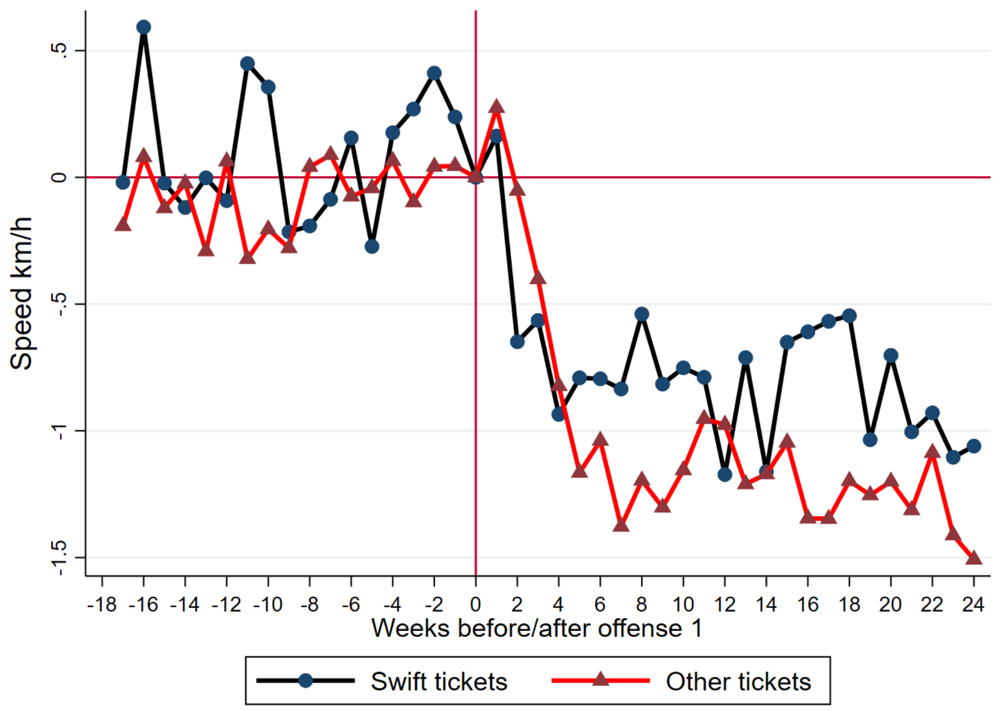
Notes: Violin plots (indicating median and interquartile ranges of predictions recorded in the survey) split by samples. The survey first introduced the findings of a recent study: speeding tickets sent within 1-2 weeks after the offense would trigger a 10 percentage point drop in the speeding rate. Participants were then asked to predict the effect of tickets sent *within 0–4 days* (swift tickets) and *within 4–8 weeks* after the offense (delayed tickets). Among the Econ & LawEcon sample ($N = 138$), the mean predicted effect is a 15.82pp drop for swift and a 6.83pp drop for delayed tickets. In the Criminology sample ($N = 68$), the mean predictions are 14.99pp (swift) and 6.04pp (delayed), respectively.

Figure A.13: Event Study Estimates for Date of Offense: Randomization Period

(a) Speeding Rate



(b) Travel Speed



Notes: In the spirit of equ. (3), the figures presents event study estimates for the travel speed (in km/h). The event is defined by the offense date. Sub-figure (a) is for the backlog period, sub-figure (b) for the randomization period.

A.2 Tables (Online Appendix)

Table A.1: Summary Statistics – Ride Level

	Raw Data	Backlog Period	Randomiz. Period
Speed (relative to limit)	-5.869	-4.923	-4.962
	(7.879)	(8.808)	(8.270)
Speeding	0.135	0.227	0.210
	(0.342)	(0.419)	(0.407)
Offending	0.0026	0.0058	0.0031
	(0.0513)	(0.0699)	(0.0494)
Drive hour 0 – 6	0.0402	0.0404	0.0442
	(0.196)	(0.197)	(0.206)
Drive hour 6 – 12	0.394	0.398	0.393
	(0.489)	(0.489)	(0.488)
Drive hour 12 – 18	0.406	0.379	0.384
	(0.491)	(0.485)	(0.486)
Drive hour 18 – 24	0.159	0.183	0.179
	(0.366)	(0.386)	(0.383)
Weekend	0.204	0.193	0.197
	(0.403)	(0.395)	(0.398)
Number plate: Local	0.453	0.485	0.408
	(0.498)	(0.500)	(0.492)
Number plate: Prague	0.400	0.423	0.458
	(0.490)	(0.494)	(0.498)
Number of cars	1,353,213	9,069	6,687
Number of rides	26,134,767	539,850	307,531

Notes: The table reports sample means (and standard deviations in parentheses) at the ride level. Columns 1 is for the raw data, and columns 2 and 3 cover the event-study sample from the backlog and the randomization periods, respectively. The latter samples only include rides from cars with at least one recorded pre- and one post-ticket ride.

Variable Descriptions (including variables from Tables A.2, A.4 and A.5): *Speed* indicates the measured speed (in km/h), relative to the speed limit; *Speeding* is a dummy indicating that the measured speed is above the speed limit; *Offending* is a dummy indicating that the measured speed is above the enforcement cutoff (14 km/h above the speed limit). *Minor offense* is a dummy indicating an offense with a speed between 14 and 24km/h above the limit. *Hour* and *Weekend* are dummies based on the hour and day of the offense. *Local* (Central Bohemia) and *Prague* are number plate based indicators for the region in which the car is registered. The residual category are the remaining number plate regions of the Czech Republic. *Company* is a dummy indicating that the car is registered for a legal entity (typically a company). *Day sent* counts the time lag between the offense and the sending day; *Day delivered* measures the time lag between sending and delivery. *Sent via e-mail* is a dummy for electronically sent speeding tickets (residual category are tickets sent via postal mail); *Week of the month sent* indicates the week within a calendar month when the ticket was sent. Variables labeled with *Pre* are based on pre-offense observations recorded by the speed cameras: *Missing* indicates that no pre-offense rides were recorded (i.e., the offense occurred at the 1st ride). *Pre: Speeding* and *Pre: Speed* indicate the pre-offense speeding rate and the average speed (relative to the speed limit); *Pre: Rides* and *Pre: Frequency* are the number of pre-offense rides and the driving frequency (per day). Variables labeled with *Post* are analogously defined, but based on rides recorded post-ticket. *Fine paid* are dummies indicating the payment of the fine within 15, 100 and 365 days, respectively.

Table A.2: Summary Statistics – Offense Level

	Raw Data	Backlog Period	Random. Period
Minor offense	0.897	1.000	1.000
	(0.304)		
Speed (relative to limit)	17.32	17.01	16.97
	(4.77)	(2.34)	(2.32)
Day sent	24.12	51.61	10.64
(relative to offense day)	(30.40)	(33.42)	(7.80)
Day delivered	4.96	4.72	4.55
(relative to sending day)	(6.11)	(5.81)	(5.15)
Week of the month sent	2.59	2.49	2.70
	(1.14)	(1.13)	(1.19)
Sent via e-mail	0.398	0.353	0.372
	(0.490)	(0.478)	(0.483)
Drive hour 0 – 6	0.113	0.109	0.096
	(0.316)	(0.312)	(0.294)
Drive hour 6 – 12	0.295	0.267	0.327
	(0.456)	(0.442)	(0.469)
Drive hour 12 – 18	0.292	0.297	0.292
	(0.455)	(0.457)	(0.455)
Drive hour 18 – 24	0.300	0.327	0.286
	(0.458)	(0.469)	(0.452)
Number plate: Local	0.297	0.342	0.289
	(0.457)	(0.474)	(0.453)
Number plate: Prague	0.476	0.446	0.447
	(0.499)	(0.497)	(0.497)
Company car	0.479	0.452	0.456
	(0.500)	(0.498)	(0.498)
Pre(-offense): missing	0.161	0.204	0.166
	(0.367)	(0.403)	(0.372)
Pre: speeding rate†	0.282	0.286	0.290
	(0.257)	(0.276)	(0.262)
Pre: average speed†	–3.911	–3.851	–3.788
	(4.816)	(5.203)	(4.727)
Pre: number of rides†	38.03	22.98	34.45
	(93.34)	(44.48)	(79.79)
Pre: driving frequency†	1.489	1.716	1.308
	(2.437)	(2.552)	(2.362)
Post(-ticket): speeding rate†	0.290	0.302	0.292
	(0.252)	(0.265)	(0.258)
Post: average speed†	–3.734	–3.595	–3.755
	(4.817)	(5.075)	(4.732)
Post: re-offense rate	0.207	0.113	0.079
(within 1 year)	(0.405)	(0.317)	(0.269)
Fine paid in 15 days	0.731	0.740	0.765
(within the deadline)	(0.443)	(0.438)	(0.424)
Fine paid in 100 days	0.829	0.844	0.879
	(0.376)	(0.363)	(0.326)
Fine paid in 365 days	0.871	0.888	0.915
	(0.336)	(0.316)	(0.279)
Number of tickets	55,965	14,251	11,232

Notes: The table reports sample means (and standard deviations in parentheses) at the offense level. Column (1) includes all speeding tickets (low and intermediate speeding offenses) from our sample period. Column (2) and (3) cover the main sample for the backlog and the randomization period. They only contain minor speeding offenses and focus on a cars' 1st ticket. Variables marked with † are only defined for subsets of cars with at least one recorded pre-offense or one post-treatment ride, respectively. Variable descriptions are provided in Table A.1.

Table A.3: Delay Dummies for Backlog and Randomization Period

Backlog Period		Randomization Period	
Sent 0 – 4 days	843 (0.059)	Sent 0 – 4 days	3,337 (0.297)
5 days – 4 weeks	3,829 (0.269)	5 days – 2 weeks	4,318 (0.384)
Sent 4 – 8 weeks	3,117 (0.219)	Sent 2 – 4 weeks	3,244 (0.289)
Sent 8 – 12 weeks	2,295 (0.161)	Sent 4 – 6 weeks	333 (0.030)
Sent 12 – 16 weeks	4,167 (0.292)		
Total	14,251 (1.000)	Total	11,232 (1.000)

Notes: The table illustrates the set of delay dummies employed in our baseline estimates for the Backlog and the Randomization Period. The table further present the absolute as well as the relative number of observation in each delay category within the two cross-sectional samples.

Table A.4: Balancing Checks – Backlog Period

	(1) Speed	(2) Company	(3) Zone 1	(4) Zone 2	(5) Zone 3	(6) Zone 4	(7) Zone 5
Sent 0 – 4 days	–0.220**	–0.012	–0.111***	0.030***	0.137***	0.041***	–0.096***
	(0.105)	(0.014)	(0.013)	(0.011)	(0.009)	(0.006)	(0.017)
Sent 4 – 8 weeks	–0.014	–0.034**	0.037***	–0.025**	–0.022**	–0.013**	0.022
	(0.116)	(0.016)	(0.014)	(0.012)	(0.010)	(0.007)	(0.019)
Sent 8 – 12 weeks	0.011	–0.043**	0.035**	–0.021	–0.016	–0.014*	0.016
	(0.138)	(0.019)	(0.017)	(0.015)	(0.012)	(0.008)	(0.022)
Sent 12 – 16 weeks	0.024	–0.039*	0.026	–0.025	–0.019	–0.008	0.027
	(0.159)	(0.022)	(0.019)	(0.017)	(0.014)	(0.009)	(0.026)
F-test (*p*-values)	0.338	0.204	0.000	0.013	0.000	0.001	0.000
Obs.	14,251	14,251	14,251	14,251	14,251	14,251	14,251

	(8) Local	(9) Prague	(10) Pre:miss	(11) Pre:speeding	(12) Pre:speed	(13) Pre:rides	(14) Pre:frequ
Sent 0 – 4 days	0.017	–0.009	–0.003	0.018	0.642**	–0.956	–0.215*
	(0.021)	(0.022)	(0.018)	(0.014)	(0.262)	(2.236)	(0.129)
Sent 4 – 8 weeks	–0.039*	0.040*	0.034*	0.017	0.271	–8.338***	–0.002
	(0.023)	(0.024)	(0.020)	(0.015)	(0.287)	(2.453)	(0.141)
Sent 8 – 12 weeks	–0.037	0.041	0.027	–0.003	–0.041	–2.650	–0.030
	(0.027)	(0.029)	(0.024)	(0.018)	(0.340)	(2.908)	(0.167)
Sent 12 – 16 weeks	–0.039	0.046	0.006	0.000	–0.418	–2.170	–0.051
	(0.032)	(0.033)	(0.027)	(0.021)	(0.395)	(3.374)	(0.194)
F-test (*p*-values)	0.418	0.501	0.288	0.286	0.019	0.001	0.567
Obs.	14,251	14,251	14,251	11,349	11,349	11,349	11,349

Notes: The table presents balancing checks for the backlog period. All estimates include month of the offense and week of the month fixed effects. The *p*-values are based on post-estimation tests of the joint significance of all delay dummies. The omitted category (reference period) are tickets sent between *5 days and 4 weeks*. The dependent variables are described in the notes to Table A.1.

Table A.5: Balancing Checks – Randomization Period

	(1) Speed	(2) Company	(3) Zone 1	(4) Zone 2	(5) Zone 3	(6) Zone 4	(7) Zone 5
Sent 0 – 4 days	0.010	0.006	–0.008	–0.000	0.006	0.002	–0.001
	(0.070)	(0.009)	(0.007)	(0.008)	(0.005)	(0.003)	(0.011)
Sent 2 – 4 weeks	0.085	0.010	–0.009	–0.016*	–0.000	–0.004	0.030**
	(0.081)	(0.011)	(0.009)	(0.009)	(0.005)	(0.004)	(0.013)
Sent 4 – 6 weeks	0.391**	0.011	–0.016	0.011	0.011	–0.002	–0.004
	(0.193)	(0.026)	(0.020)	(0.022)	(0.013)	(0.010)	(0.032)
F-test (p-values)	0.145	0.821	0.627	0.210	0.401	0.329	0.068
Obs.	11,229	11,229	11,229	11,229	11,229	11,229	11,229

	(8) Local	(9) Prague	(10) Pre:miss	(11) Pre:speeding	(12) Pre:speed	(13) Pre:rides	(14) Pre:frequ
Sent 0 – 4 days	–0.006	0.034**	0.001	0.006	0.206	–4.214	–0.124
	(0.013)	(0.015)	(0.011)	(0.009)	(0.155)	(2.616)	(0.078)
Sent 2 – 4 weeks	–0.023	–0.047***	0.003	0.011	0.307*	–4.627	–0.154*
	(0.016)	(0.017)	(0.013)	(0.010)	(0.182)	(3.058)	(0.091)
Sent 4 – 6 weeks	0.023	0.069*	0.059*	0.020	0.189	–6.715	0.027
	(0.037)	(0.040)	(0.031)	(0.025)	(0.447)	(7.529)	(0.224)
F-test (p-values)	0.359	0.000	0.278	0.663	0.373	0.340	0.261
Obs.	11,229	11,229	11,229	9,365	9,365	9,365	9,365

Notes: The table presents balancing checks for the randomization period.All estimates include offense day and week of the month fixed effects. The p-values are based on post-estimation tests of the joint significance of all delay dummies. The omitted category (reference period) are tickets sent between *5 days and 2 weeks*. The dependent variables are described in the notes to Table A.1.

Table A.6: Alternative Reference Group: Effect on Timely Payment, Randomization Period

	(1)	(2)	(3)	(4)	(5)	(6)
	Offense month FEs			Offense day FEs		
Sent day 1	0.039**	0.042**	0.042**	0.043*	0.045**	0.044**
	(0.018)	(0.019)	(0.019)	(0.022)	(0.022)	(0.022)
Sent day 2	0.026*	0.027*	0.027*	0.024	0.021	0.020
	(0.015)	(0.015)	(0.015)	(0.019)	(0.019)	(0.019)
Sent day 3	0.003	0.008	0.008	0.014	0.015	0.014
	(0.015)	(0.016)	(0.016)	(0.019)	(0.019)	(0.019)
Sent 2 – 4 weeks	0.006	0.006	0.006	0.008	0.006	0.006
	(0.011)	(0.011)	(0.011)	(0.015)	(0.015)	(0.015)
Sent 4 – 6 weeks	0.018	0.026	0.026	–0.002	0.000	0.001
	(0.032)	(0.032)	(0.032)	(0.038)	(0.037)	(0.037)
Baseline mean: (**4** days – 2 weeks)	0.761					
Offense controls	N	Y	Y	N	Y	Y
Pe-offense contr.	N	N	Y	N	N	Y
Obs.	11,232	11,232	11,232	11,229	11,229	11,229

Notes: The table presents linear probability model estimates for the randomization period, based on an augmented version of equation (1). Specifications (1) – (3) account for offense month, specifications (4) – (6) for offense day fixed effects. All specifications include week-of-month dummies. The omitted category (reference period) are tickets sent between 4 days and 2 weeks. Robust standard errors are in parenthesis.

Table A.7: Estimates: Long-Run Effects on Timely Payment, Backlog Period

	(1)	(2)	(3)	(4)	(5)	(6)
	Offense quarter FEs			Offense month FEs		
	Payments within 100 days					
Sent 0 – 4 days	0.025	0.018	0.017	0.025	0.022	0.022
	(0.015)	(0.015)	(0.015)	(0.017)	(0.017)	(0.017)
Sent 4 – 8 weeks	–0.015	–0.024	–0.022	–0.002	–0.009	–0.008
	(0.019)	(0.019)	(0.019)	(0.020)	(0.019)	(0.020)
Sent 8 – 12 weeks	–0.019	–0.026	–0.025	–0.022	–0.028	–0.028
	(0.021)	(0.021)	(0.021)	(0.023)	(0.023)	(0.023)
Sent 12 – 16 weeks	–0.022	–0.034*	–0.032	–0.031	–0.037	–0.038
	(0.020)	(0.020)	(0.020)	(0.026)	(0.026)	(0.026)
Baseline mean:	0.821					
	Payments within 365 days					
Sent 0 – 4 days	0.028**	0.021	0.021	0.026*	0.023	0.022
	(0.014)	(0.014)	(0.014)	(0.015)	(0.015)	(0.015)
Sent 4 – 8 weeks	–0.002	–0.007	–0.006	0.004	–0.000	0.001
	(0.018)	(0.018)	(0.018)	(0.018)	(0.018)	(0.018)
Sent 8 – 12 weeks	–0.011	–0.016	–0.015	–0.025	–0.029	–0.029
	(0.019)	(0.019)	(0.019)	(0.021)	(0.021)	(0.021)
Sent 12 – 16 weeks	–0.001	–0.009	–0.009	–0.028	–0.033	–0.033
	(0.019)	(0.019)	(0.019)	(0.023)	(0.024)	(0.024)
Baseline mean:	0.851					
Offense controls	N	Y	Y	N	Y	Y
Pe-offense contr.	N	N	Y	N	N	Y
Obs.	14,251	14,251	14,251	14,251	14,251	14,251

Notes: The table presents linear probability model estimates of equ. (1). In the top panel, the dependent variable indicates payments within 100 days; in the bottom panel, it is payments within 365 days. Specifications (1) – (3) account for offense month, specifications (4) – (6) for offense day fixed effects. All specifications include sending week-of-month dummies. The omitted category (reference period) are tickets sent between 5 days and 4 weeks. Robust standard errors are in parenthesis.

Table A.8: Estimates: Long-Run Effects on Timely Payment, Randomization Period

	(1)	(2)	(3)	(4)	(5)	(6)
	Offense month FEs			Offense day FEs		
	Payments within 100 days					
Sent 0 – 4 days	0.008	0.009	0.009	0.022**	0.022**	0.021**
	(0.008)	(0.008)	(0.008)	(0.010)	(0.010)	(0.010)
Sent 2 – 4 weeks	–0.009	–0.007	–0.007	–0.001	0.001	0.000
	(0.009)	(0.009)	(0.009)	(0.012)	(0.012)	(0.012)
Sent 4 – 6 weeks	–0.021	–0.017	–0.017	–0.029	–0.027	–0.028
	(0.027)	(0.027)	(0.027)	(0.031)	(0.031)	(0.031)
Baseline mean:	0.881					
	Payments within 365 days					
Sent 0 – 4 days	0.003	0.003	0.003	0.010	0.011	0.010
	(0.007)	(0.007)	(0.007)	(0.008)	(0.008)	(0.008)
Sent 2 – 4 weeks	–0.010	–0.008	–0.008	–0.006	–0.003	–0.004
	(0.007)	(0.008)	(0.008)	(0.010)	(0.010)	(0.010)
Sent 4 – 6 weeks	–0.049**	–0.046*	–0.046*	–0.061**	–0.059**	–0.060**
	(0.024)	(0.024)	(0.024)	(0.028)	(0.028)	(0.028)
Baseline mean:	0.918					
Offense controls	N	Y	Y	N	Y	Y
Pe-offense contr.	N	N	Y	N	N	Y
Obs.	11,232	11,232	11,232	11,229	11,229	11,229

Notes: The table presents linear probability model estimates of equ. (1). In the top panel, the dependent variable indicates payments within 100 days; in the bottom panel, it is payments within 365 days. Specifications (1) – (3) account for offense month, specifications (4) – (6) for offense day fixed effects. All specifications include sending week-of-month dummies. The omitted category (reference period) are tickets sent between 5 days and 4 weeks. Robust standard errors are in parenthesis.

Table A.9: Sample Size and Attrition in the Event Study

Backlog Period			Randomization Period		
Sample:	(1) Baseline Estimates	(2) Estimates w/o car FEs		(3) Baseline Estimates	(4) Estimates w/o car FEs
Sent 0 – 4 days	614 [0.728]	759 [0.900]	Sent 0 – 4 days	2,051 [0.615]	2,899 [0.869]
5 days – 4 weeks	2,765 [0.722]	3,414 [0.892]	5 days – 2 weeks	2,419 [0.560]	3,679 [0.852]
Sent 4 – 8 weeks	1,978 [0.635]	2,754 [0.884]	Sent 2 – 4 weeks	1,847 [0.569]	2,755 [0.849]
Sent 8 – 12 weeks	1,341 [0.584]	2,011 [0.876]	Sent 4 – 6 weeks	196 [0.589]	273 [0.820]
Sent 12 – 16 weeks	2,371 [0.569]	3,611 [0.867]			
Total	9,069 [0.636]	12,549 [0.881]	Total	6,687 [0.595]	9,606 [0.855]

Notes: For each delay category of the backlog and the randomization period, the table tabulates the total number of observations and [in squared brackets] the fraction of observations from the cross-sectional sample (from our analysis of payment outcomes) that is included in different event study samples. The samples in columns (1) and (3) correspond to the baseline estimation samples from Tables 4. Columns (2) and (4) describe the alternative samples used in the estimates without car fixed effects reported in Table A.10.

Table A.10: Event Study Estimates without Car Fixed Effects

	(1)	(2)	(3)	(4)
Outcome:	Speeding Rate		Speed	
	A. Backlog Period			
Post Ticket	-0.100***	-0.105***	-1.32***	-1.40***
	(0.005)	(0.005)	(0.09)	(0.08)
Post ×	-0.015*	-0.016*	-0.27	-0.28
Sent 0 – 4 days	(0.009)	(0.009)	(0.17)	(0.17)
Post ×	-0.004	-0.002	-0.15	-0.13
Sent 4 – 8 weeks	(0.007)	(0.007)	(0.13)	(0.12)
Post ×	0.002	0.002	0.02	0.08
Sent 8 – 12 weeks	(0.008)	(0.008)	(0.15)	(0.15)
Post ×	-0.003	-0.001	-0.04	0.01
Sent 12 – 16 weeks	(0.007)	(0.007)	(0.13)	(0.13)
Pre-ticket mean:	0.248	0.248	43.765	43.765
Number of Cars	12,549	12,549	12,549	12,549
Number of Rides	539,644	539,644	539,644	539,644
	B. Randomization Period			
Post Ticket	-0.092***	-0.093***	-1.19***	-1.21***
	(0.004)	(0.005)	(0.09)	(0.09)
Post ×	-0.001	0.000	0.18*	0.17*
Sent 0 – 4 days	(0.007)	(0.006)	(0.10)	(0.10)
Post ×	-0.004	-0.002	-0.03	-0.03
Sent 2 – 4 weeks	(0.007)	(0.006)	(0.11)	(0.11)
Post ×	0.008	0.010	0.43*	0.46*
Sent 4 – 6 weeks	(0.016)	(0.016)	(0.25)	(0.24)
Pre-ticket mean:	0.243	0.243	44.602	44.602
Number of Cars	9,607	9,607	9,607	9,607
Number of Rides	319,247	319,247	319,247	319,247

Notes: The table presents estimates of event analysis regressions without car fixed effects. Panel *A* present the results for the backlog period, where the non-interacted effect (Post Ticket) captures the impact of tickets from the reference group, i.e. tickets sent between 5 days and 4 weeks. Panel *B* presents the results for the randomization period, where the reference group are tickets sent between 5 days and 2 weeks. The sample includes all rides of ticketed cars from 32 weeks prior to receiving the ticket to 24 weeks after (except for the ride that generated the ticket). Instead of using car fixed effects, we control for the same car and offense characteristics used in estimating equation (1) (see fn. 16). Analogously to the estimates reported in Table 1, Panel A accounts for offense quarter (columns 1 and 3) and offense month fixed effects (columns 2 and 4). In line with Table 2, Panel B includes offense month (columns 1 and 3) and offense day fixed effects (columns 2 and 4). As in our main estimates reported in Table 4, all estimates include camera zone fixed effects; day of the week, holiday and hour of the day fixed effects interacted with the zone dummies; variables measuring weather (temperature and precipitation, measured at a 10-minute frequency) and driving conditions capturing traffic density (dummies for different time gaps to the car j traveling ahead of car i). To control for seasonal variation, the regressions also include interactions of camera zone with calendar month dummies. Two-way clustered standard errors (by car and camera zone-hour) are in parenthesis.

A.3 Ticket Administration Process

This section provides more details on the administrative protocol used in the randomization period. As noted in the main text, one of the staff members would work on the most recent offenses. New cases arrive every working day during the morning. Using the processing software, the administrator in charge of the most recent cases would then select a specific offense day (e.g., the last day) and would work down the list of randomly sorted offenses from that day. (The software displays the day, time, camera zone and the measured speed – variables that we observe as well.) The administrator was instructed to process approximately 30–40% of the offenses from a given day. The remaining cases would go to the backlog pile.

For days with few offenses, the 30–40% target could be typically accomplished within one working day. Hence, offenses from, say, a not-too-busy Monday that get randomly sorted to the top 30% of entries in the list, would be processed on Tuesday (i.e., one day after the offense). On days with a higher caseload, however, the administrator would spread her work on the top-of-the list cases from a given day over multiple days. The processing on cases from a given day might also get interrupted (and delayed) by working on cases from a more recent offense days. As a consequence, we often observe that cases from 'recent' offense days are processed and sent with a delay of three or four days.

The updated data batches typically cover the last 24 hours. This implies that some offenses from the early morning hours of a given day might be processed on the very same day – if the workload allows the administrator to process cases from that day. Note further that administrators do not work on weekends (or public holidays). Offenses recorded on a Friday afternoon (or Saturday) would thus be processed the earliest on the next Monday, i.e., 3 days (or 2 days) after the offense. Hence, swiftness varies with the weekday of the offense. Our estimation strategy accounts for this feature by adding day-of-the-offense fixed effects.

The other administrative staff (typically 3 out of 4 administrators) focuses on the backlog, i.e., on the oldest cases. They allocate the (remaining 60–70% of) offenses committed during a given day. They work down the (again, randomly sorted) list until they finish the assigned day and then turn to another day. Finally, note that a part of these administrators's time is also devoted to enforcement steps targeting non-complying car owners, processing objections to the tickets, or communicating with offenders who call with questions about their tickets.

A.4 Expert Survey

The invitation to participate in our survey was sent to three sets of authors of publications in (1) Economics, (2) Law & Economics and (1) Criminology journals. The following subsection details how we arrived at these different sets of authors.

(1) *Economics.* We exploit on Jennifer Doleac's *Database of crime-related papers published in economics journals.*[1] This list covers Economics of Crime articles – broadly defined – from general interest and top field journals in Economics:[2]

> American Economic Review (excluding P&P); Econometrica; Journal of Political Economy; Quarterly Journal of Economics; Review of Economics Studies; Journal of the European Economic Association; AEJ: Applied Economics; AEJ: Economic Policy; AER: Insights; Review of Economics and Statistics; Economic Journal; Journal of Labor Economics; Journal of Public Economics; Journal of Urban Economics; Journal of Policy Analysis and Management; Journal of Human Resources; Journal of Development Economics; Journal of Economic History; Explorations in Economic History; Journal of Health Economics.

Focussing on articles published between 2005 and 2019 (the last 15 years covered at the point in time we started working on the survey), we first extracted the list of authors. We then ran several scrapping scripts and did extensive searches to manually compile the authors' email addresses. We managed to find emails for 95% of these authors.

(2) *Law & Economics.* A second sample is based on articles published in seven field journals in Law & Economics and Legal Studies:

> Journal of Law & Economics; Journal of Law, Economics, & Organization; Journal of Legal Studies; Journal of Empirical Legal Studies; American Law & Economics Review; European Journal of Law & Economics; International Review of Law & Economics.

Between 2005 and 2019, these journals published roughly 2,700 articles. Using several key words,[3] we narrowed down this list to 332 articles on crime, recidivism or, more specificity, traffic law enforcement and inter-temporal aspects of deterrence. We compiled email addresses for almost 98% of the 500 unique authors of these articles.

(3) *Criminology.* We also covered authors of articles published in 14 top-ranked Criminology journals:

> Criminology; Journal of Experimental Criminology; Journal of Criminal Law & Criminology; Journal of Criminal Justice; Journal of Quantitative Criminology; Journal of Research in Crime & Delinquency; Justice Quarterly; Criminology & Public Policy; Criminology & Criminal Justice; Crime & Delinquency; Criminal Justice & Behavior; Annual Review of Law & Social Science; European Journal of Criminology; Legal & Criminological Psychology.

Starting from almost 7,000 articles published in these journals between 2005 and 2019, we selected contributions based on the same set of key-words applied to the Law & Economics sample (see

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[1] The spreedsheet is available here (at jenniferdoleac.com/resources/).

[2] Note that the list also covers the Journal of Law and Economics. We assigned authors that published in this journal (but not in any other Econ journal) to the Law & Econ sample (see below).

[3] Crime; deterr∗; enforc∗; policing; swiftness; ('velocity' or 'celerity' or 'swift∗' or 'delay∗' or 'discount∗' or 'inter-temp∗') & ('punish∗' or 'sanction∗' or 'sentenc∗'); 'swift fine∗'; 'delayed fine∗'; speeding; speeders; drivers; traffic (−drug∗); reoffend∗; recidiv∗.

Table A.11: Sample description

Sample	Invited	Opened Survey	Completed Survey
Economics	758	128 (16.89%)†	83 (64.84%)‡
Law & Economics	365	72 (19.73%)	53 (73.61%)
Criminology	783	93 (11.88%)	67 (72.04%)
Total	1,906	293 (15.37%)	203 (69.28%)

Notes: † Fraction of invited authors that started the survey; ‡ fraction that completed the survey conditional on starting it.

above). This yielded roughly 1,500 articles with 1,000 unique authors. We again web scraped and searched email addresses. Eventually, we found data for almost 80% of the authors. The slightly lower coverage reflect a much higher share of practitioners (from, e.g., police forces or the judiciary system) among the authors publishing in Criminology journals. Finding email accounts of these practitioners turned out to be much more difficult than for scholars working in Academia. The latter (i.e., academic researchers), still form the largest part of our third sample.

Sample Consolidation and Decomposition. A small number of authors were identified in two or, in a few cases, even all three samples. This reflects that several scholars publish their work in Economics, Law & Economics and Criminology outlets. Despite the interdisciplinary nature of this subgroup, we nevertheless assigned all authors to one unique sub-sample. Any author who published at least one article in the general interest/top field Economics journals (other than the Journal of Law & Economics; see fn. 2) was assigned to the 'Economics' sample. Anyone who co-authored at least one publication in the Law & Economics outlets (but not in the general interest/top field Econ journals) was assigned to the 'Law & Economics' sample. Finally, authors who published in Criminology journals (but neither in Econ nor Law & Econ journals) were assigned to the 'Criminology' sample. The resulting composition of the sample is displayed in Table A.11.[4]

Overall, we compiled 1,918 email addresses of unique authors. In 12 cases, emails were neither delivered nor did we find any properly working, alterative email address. This left us with an invited sample of 1,906 authors: more than 750 authors from the Economics and the Criminology sample and more than 350 Law & Econ scholars (see Table A.11). 293 authors, slightly more than 15% of the invited authors responded and opened the survey link. 255 answered at least the first question, 203 (69.3% of those who opened the survey link or 80.2% of those who started answering) completed the survey.

In examining heterogeneous responses across different fields, we will pool responses from scholars publishing in Economics and Law&Econ journals and consider differences to Criminologists (see, e.g., Fig. A.11). This approach is mainly motivated by the small sample of respondents publishing (only) in Law&Econ journals (see Tab. A.11).

[4]The number of authors publishing in two or the three subsets of journals is limited. Hence, alternative assignment approaches (e.g., based on the number of published articles in the different journal groups) yield similar decompositions.

A.4.1 Survey Questions

Q1 How would you assess, at a very general level, the role of swiftness in determining the (specific) deterrent effect of punishment?

- Swiftness plays a major role
- Swiftness plays a minor role
- Swiftness plays no role
- The role of swiftness is ambiguous

Q2 How would you assess the empirical evidence on the importance of swiftness for the (specific) deterrent effect of punishment?

- There is a substantial body of evidence
- There is some evidence
- There is scarce evidence
- I am not aware of any evidence

Q3 Consider the enforcement of speed limits by automated speed cameras. A speeding ticket, which communicates a fine of roughly $40, is mailed with some delay after the offense.

A recent study finds that the likelihood that a ticketed driver is speeding on subsequent rides through the same speed cameras drops after receiving a ticket.

For a ticket sent **within 1-2 weeks** after the offense, the speeding rate drops by approximately 10 percentage points (from 30% to 20%).

Which drop would you expect if the ticket is sent **within 0–4 days** after the offense?

The speeding rate will drop by ____ percentage point.

Please enter numeric values, up to one decimal point.

Q4 Which drop would you expect if the ticket is sent **within 4–8 weeks** after the offense?

The speeding rate will drop by ____ percentage point.

Please enter numeric values, up to one decimal point.

Q5 A related study on the same data documents non-compliance with fine payments.

Among tickets sent **within 1-2 weeks** after the offense, approximately 75% are paid on time (i.e., within two weeks after delivery).

Which payment rate would you expect for tickets sent **within 0–4 days** after the offense?

The payment rate will be ____ %.

Please enter numeric values, up to one decimal point.

Q6 Which payment rate would you expect for tickets sent **within 4–8 weeks** after the offense?

The payment rate will be ____ %.

Please enter numeric values, up to one decimal point.