Discussion Papers of the
Max Planck Institute for
Research on Collective Goods
2020/9

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May 2020

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Salience, Incentives, and Timely Compliance: Evidence from Speeding Tickets*

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May 2020

Abstract

This paper studies the enforcement of fines. We randomly assign 80,000 speeding tickets to treatments that increase the salience of the payment deadline, late penalties, or both. Stressing the penalties significantly and persistently increases payment rates. Emphasizing only the deadline is not effective. The findings from the RCT are consistent with a survey experiment which documents the treatments' impact on priors about parameters of the compliance problem. Exploiting discontinuous variation in fines, we then document a strong price responsiveness: a 1% increase in the payment obligation induces a 0.23 percentage point decrease in timely compliance. This semi-elasticity suggests that the impact of the salience nudges is equivalent to the effect of a 4–9% reduction in fines.

JEL Classification: K42, H26, D80.

Keywords: Enforcement; fines; timely compliance; salience; nudges; deadlines; perceptions; RCT; RDD.

*We would like to thank Mariana Blanco, Christoph Engel, Carlos Scartascini, Cornelius Schneider, Joel Slemrod, Johannes Spinnewijn and numerous conference and seminar participants for helpful comments and suggestions. Paulina Ockajova and Vlad Surdea provided excellent research assistance. The constructive cooperation with the town hall officials of Ricany and research funding from GACR (grant 17-16583J) and DFG (grant TR 1471/1-1) is gratefully acknowledged.

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1 Introduction

The effective collection of taxes is a key determinant of states' capacity to raise revenues (Kleven, 2014) and an important strand of research studies tax compliance and enforcement (Slemrod, 2019). Enforcement problems, however, are not limited to taxes. Various public sector entities face major challenges in collecting fines, service fees, or public utility bills (Fellner *et al.*, 2013; Szabó and Ujhelyi, 2015). Yet, the economic literature has paid little attention to these domains. Where the payment of fines is concerned, this seems particularly surprising, as monetary sanctions in law enforcement are increasingly ever more prevalent and their budgetary significance is non-trivial (Piehl and Williams, 2010).[1] Lofstrom and Raphael (2016) estimate that US local, county, and state governments collected \$15.3 billion in fine and forfeiture revenue in 2012. Makowsky (2019) notes that traffic tickets make up for more than 7% of Chicago's total revenues.[2]

One approach to improve enforcement outcomes are nudges. Numerous studies have highlighted the benefits of applying behavioral insights (e.g., Dwenger *et al.*, 2016; Hallsworth *et al.*, 2017; Perez-Truglia and Troiano, 2018) and many authorities have adopted behavioral tools (OECD, 2019). Against this trend, a growing body of evidence documents 'nudges that fail' (Sunstein, 2017). DellaVigna and Linos (2020) show that large-scale trials of US' Nudge Units yield much smaller effects than published, academic studies. In a similar vein, a recent meta-study of tax compliance interventions suggests that tax morale nudges are, on average, ineffective (Antinyan and Asatryan, 2019). The authors further report that nudges with a deterrence component work; however, the effect size is typically quite small. These findings allude to two important gaps in the literature. First, as stressed in Luttmer and Singhal (2014), we lack a coherent understanding of why certain behavioral interventions increase compliance in some contexts but not in others. Second, we lack evidence that compares the effect size of nudges with the impact of traditional economic incentives.

This paper tackles these two points in the context of fine enforcement. In a randomized control trial (RCT), we first evaluate the impact of different salience nudges on timely compliance with payment notifications for speeding. Evidence from a survey experiment explains why one of the salience nudges – which only emphasizes the payment deadline – is ineffective, whereas another one – which raises the salience of deterrence incentives – persistently increases payment rates. In a second step, we exploit exogenous variation in the level of fines to quantify the change in timely compliance due to a 1% increase in the payment obligation. The result allows us to compare the nudge effects to the 'price elasticity' of compliance.

Our study covers the universe of speeding tickets processed by an enforcement authority near Prague, Czech Republic. Any driver caught speeding by an automated speed camera system receives a notification demanding the payment of a fine (typically between \$40 and \$80) by a given deadline (within 15 days). Delayed or incomplete payments trigger additional enforcement measures that are costly for the ticketed individuals (in terms of late fees) and the authority alike (administrative costs). Similar to other domains where official payment notifications are regulated by numerous legal constraints, the notification used by the authority is a formalistic legal text. Two important

[1] There are numerous concerns associated with the increasing use of fines. For a discussion of political economy and social inequality aspects, see Makowsky and Stratmann (2009), Harris *et al.* (2010) and Makowsky (2019).

[2] Traffic tickets are costly to enforce. They involve many individuals paying, on average, relatively small amounts. The city of Berlin, for instance, issued 4.2 million traffic tickets in 2018; 75% were paid on time, generating a revenue of €82 million (Statistical Yearbook of Berlin 2019).

parameters, the payment deadline and the penalty for late payments, are hidden or only vaguely mentioned in the lengthy text. To mitigate potential frictions associated with the complexity of the text (Bhargava and Manoli, 2015) and to increase the salience of the two parameters, we augmented the notification by adding a one-page cover letter. Our RCT evaluates the impact of three different letters that emphasize (1) the payment deadline, (2) the late penalties for missing it, or (3) both attributes. The control group received only the basic notification. The trial was conducted between fall 2017 and summer 2019 and encompassed nearly 80,000 speeding tickets. The design and a pre-analysis plan were registered before the start of the trial (Dusek *et al.*, 2017).

The main findings from our RCT are as follows: First, increasing the salience of the deadline alone has no significant effect on the rate of payments made within the deadline. Second, emphasizing the late penalty produces a 1pp (percentage point) increase in timely compliance. Relative to the control group (78%), this corresponds to a 1.2% higher payment rate. The third cover letter, which increases the salience of both the deadline and the late penalty, raises compliance by 2pp or 2.6%. The effect is significantly larger than the one from solely stressing the late penalty. When the late penalty is salient, emphasizing the deadline thus seems to have a positive effect on compliance.

As noted above, the authority takes additional enforcement measures after the payment deadline has passed. These measures target non-compliant speeders. It is therefore not surprising that the treatment effects shrink over time. The impact of our most effective intervention is nonetheless persistent: even 100 days after the notification was delivered, the payment rate is 1.6pp (or 2%) above the rate from the control group. Our data further show that all treatment effects are driven by extensive margin responses: the treatments increase compliance by turning non-paying speeders into paying ones; partial or incomplete payments are hardly ever observed.

Why does an increase in the salience of the payment deadline only exert a limited effect on compliance behavior? To tackle this question we first introduce a theoretical framework (similar to Altmann *et al.*, 2017). Agents trade off the stochastic opportunity costs of paying the fine now against the option value of postponing the payment. The decision (when) to pay is shaped by the deadline. If the deadline passes, a late penalty applies which further increases the agents' costs. The nature of the payment notification suggests that the deadline and the costs of missing it might not be fully salient (Taubinsky and Rees-Jones, 2018). We thus model a case in which the treatments that increase the salience of these two parameters alter agents' prior (mis)perceptions and, in turn, influence payment behavior. The model predictions are ambiguous and depend on the distribution of prior (mis)perceptions and the direction in which the salience nudges alter them. Perceiving a higher late penalty or a tighter deadline would increase timely compliance. Both effects are reinforcing, which can explain why jointly emphasizing the deadline and the late penalty has a stronger effect.

To empirically quantify the distribution of and the treatments' impact on prior perceptions, we run a survey experiment which exposes respondents to the payment notification and, randomly, to one of the cover letters from our trial. In the control group, most subjects underestimate the penalty for missing the deadline. Emphasizing the late penalty corrects these misperceptions and, in turn, strengthens the incentive to pay on time. Misperceptions of the deadline itself are less pronounced and more symmetric: similar shares of respondents under- and overestimate the deadline. According to our model framework, making the 15-day deadline salient may then trigger opposing behavioral responses that cancel each other. The survey evidence thus offers an explanation for the results from our trial. Increasing the salience of the late penalties strengthens the incentive to comply.

Communicating solely the correct deadline, in contrast, fails to induce a significant increase in compliance as the treatment exerts opposing effects on perceptions. Only when the late penalty is salient (in the interacted treatment) do the 'gains' from correcting over-estimations of the deadline seem to dominate.

Our next step is to quantitatively assess the effects of our interventions. The results from the trial show that emphasizing the late penalty increases timely compliance by 1.2 – 2.6%. This range of effect sizes is remarkably similar to the average impact of deterrence messages reported in the literature (Antinyan and Asatryan, 2019). Advancing this literature, our setting allows us to benchmark the treatment effects relative to the impact of traditional economic incentives: we evaluate whether the rate of timely payments drops as the fine increases and by how much.

To do so, we take advantage of the fact that the fine for speeding is step-wise increasing in the speed and that, empirically, there is no heaping below the different speed cutoffs (Dusek and Traxler, 2020). Following a pre-registered regression discontinuity design (Dusek *et al.*, 2017), we then exploit jumps in the fine at different speed cutoffs. At a central cutoff, where the fine increases by 70%, we observe a 5.5pp drop in the rate of pre-deadline payments. The variation in the fine thus triggers an impact almost three times as large as the most effective nudge from our trial. The observed discontinuity implies that a one-percent increase in the fine causes a 0.23pp decline in timely compliance. This semi-elasticity indicates that increasing the salience of the late penalty has a similar effect as a 4.3% decline in the fine. The effect from the cover letter that jointly emphasizes the late penalty and the deadline is equivalent to an 8.7% drop in the fine. The discontinuity analysis thus provides two novel insights. On the one hand, the fine exerts a sizable influence on compliance behavior: a higher fine induces lower payment rates; this effect is stronger than the effect from the salience nudges. The impact of the penalty nudges, on the other hand, is quantitatively meaningful nevertheless: it is equivalent to the effect of a 4–9% reduction in the fine.

Our study makes several contributions to the literature. First, we provide a rare piece of evidence on the price elasticity of compliance. The estimated semi-elasticity from our RDD is very similar to the results from Berger *et al.* (2016), who find that a 1% increase in TV license fees raises the rate of non-compliance by 0.3pp. Kleven *et al.* (2011), who study bunching of pre- and post-auditing incomes at kink-points in the Danish income tax schedule, report elasticities of evasion w.r.t. the net-of-tax rate in a range between 0.08 to 0.25. Estimates from settings with a lower enforcement pressure document larger elasticities (Fack and Landais, 2016; Fisman and Wei, 2004; Kopczuk, 2012).

While our estimates are quantitatively similar to the findings from the taxation literature, there is a caveat in that the RDD estimates a local effect. Hence, one might question the comparison of RCT and RDD results. To address this concern, we show that the average treatment effects from the RCT hold among sub-populations of speeders in speed ranges around the relevant cutoffs. In addition, we replicate the main RDD estimate at a second speed cutoff. Reassuringly, we find a quantitatively very similar (but less precisely estimated) effect. Only at a third cutoff – at a much lower speed level – there is no discontinuity in compliance. This suggests that modest speeders are hardly sensitive to (small) variation in the fine. Consistently with this interpretation, our data further reveal that drivers ticketed at higher speeds have (beyond the discontinuous jumps in the fine) a much lower propensity to comply with the payment deadline. More pronounced non-compliance in one dimension (violation of the speed limit) is therefore correlated with more frequent non-compliance on a different dimension (failure to pay on time).

We further contribute to the literature by comparing nudge effects with the price elasticities of non-compliance. In fact, our estimates provide such a comparison within the same sample and context. Our findings indicate that an increase in the salience of late-penalties has a similar effect on timely compliance as a 4–9% decline in the fine. We are not aware of any similar quantification in the enforcement literature.[3] Research on energy consumption, which offers similar comparisons of price and non-price interventions, tends to report larger nudge effects. Based on estimates from the literature, Allcott (2011) notes that the impact of information nudges on households' energy consumption is equivalent to a 10–20% increase in short run electricity prices.[4] Holladay *et al.* (2019) find that social comparison nudges have similar effects on households' searches for (but not purchases of) energy-efficient durables as a \$50 – \$70 subsidy.

Our study is also one of the first to explicitly analyze the role of deadlines in timely compliance behavior. Despite the fact that deadlines are central policy parameters of the tax filing and payment process (Rees-Jones and Taubinsky, 2016; Slemrod *et al.*, 1997), they remain understudied. Our evidence shows that in a context in which equal shares of agents seem to over- and underestimate the deadline, increasing its salience alone is not a promising strategy: it produces an economically and statistically insignificant effect. Only in combination with an emphasis of the late penalty is there a positive effect from communicating the deadline.

The survey results, which document the deadline nudges' opposing impact on peoples' priors, also speak to the conflicting evidence on tax morale nudges (Luttmer and Singhal, 2014): descriptive social norm messages (statements such as "90% pay their taxes") may shift priors in opposing directions, too. Contextual differences in priors (and their distributions) can therefore explain why such messages work in some contexts (e.g. Bott *et al.*, 2020; Hallsworth *et al.*, 2017) but fail to increase compliance in other settings (e.g. Castro and Scartascini, 2015; Fellner *et al.*, 2013).[5] This argument further implies that, depending on priors, emphasizing a deadline might indeed be effective. In fact, De Neve *et al.* (2019) do report a positive deadline effect.[6] They test, among other things, the impact of reminders on taxpayers in Belgium who were late filing their tax returns. Reminders that emphasized the new deadline strongly increased the filing rate.[7] Together with the partial effectiveness of our deadline interventions, this suggests that deadlines can, in principle,

[3] De Neve *et al.* (2019) offer a related but distinct exercise: they compare simplification nudges with traditional enforcement measures. Similarly, Kleven *et al.* (2011) compare actual audits with threat-of-audit letters.

[4] A different result is provided by, e.g., Ito *et al.* (2018), who document strong and persistent price-effects but quantitatively small and short-lived effects of moral suasion nudges.

[5] Evidence supporting this argument is provided by Fellner *et al.* (2009). They report that priors and heterogenous treatment effects are correlated with local compliance rates. Their social norm message has a positive [negative] effect when local compliance is low [high], yielding a null result on average.

[6] A further related study is Heffetz *et al.* (2019). They examine the effect of reminders and deadlines on the payment of parking tickets. They focus on the role of heterogeneity in cognitive limitations.

[7] The setting in De Neve *et al.* (henceforth DISTL) differs from ours in numerous ways. In addition to context, sample and outcome, it is worth noting that we *add* a cover letter to a convoluted payment notification whereas DISTL *replace* a reminder that communicates many different cutoff dates with a very simple message ('submit your return within 14 days'). Ultimately, one cannot assess the role of deadline misperceptions for the difference in findings. Note further that, unlike DISTL, our design does not aim to isolate the effect from the simplification achieved by the cover letters. The effect from our basic, non-interacted treatments capture the joint effect of attribute salience and simplification. The small point estimates for the deadline treatment suggest that the benefits from simplification alone might be small in our context. However, our evidence is consistent with DISTL in that we find a positive effect from adding a deterrence message to simplified communication.

serve as an attractive policy tool to improve compliance – in particular, when the consequences of missing the deadline are sufficiently salient.

Finally, our results on the effectiveness of communicating the late penalty are consistent with Haynes *et al.* (2013), who document the impact of deterrent text messages on the collection of delinquent fines among a selected sample of non-compliant individuals. Our results are also consistent with the rich evidence from trials in tax enforcement[8] and many other domains (e.g., Dur and Vollaard, 2019). From an applied policy perspective, a simple cost benefit analysis shows that increasing the salience of the late penalties is a cost-effective instrument (Benartzi *et al.*, 2017): it reduces the caseload of the follow-up enforcement process (which saves administrative costs, processing time, etc.) and directly increases revenues, as it persistently increases payments. Summing up all fiscal benefits and costs, the most successful treatment yields a net revenue gain equal to around 125% of the monthly labor costs of an administrator handling the speeding tickets.

The remainder of the paper is organized as follows. After describing the institutional context, Section 3 discusses a theoretical framework for payment decisions under a deadline. The results of our RCT are presented in section 4, after introducing the experimental designs, our predictions as well as the findings from our survey experiment. In Section 5 we discuss the regression discontinuity design and report the empirical results. Section 6 concludes.

2 Institutional Background

We study the payment of speeding tickets issued by a local authority in Ricany, Czech Republic. The town serves as the administrative center for a larger suburban area south-east of Prague. Until July 2018 the authority managed 5 speed camera systems. During the second half of 2018 24 additional speed cameras were placed in the area. Most of the 29 camera systems are installed along road sections with speed limits of 50km/h (3 out of 29 are installed on stretches with a speed limit of 40km/h). The fully automated camera systems measure vehicle's average speed in measurement zones of several hundred meters. The enforcement authority then automatically processes the data on cars found to be speeding above a given cutoff.[9]

A common feature in traffic law enforcement are fines that are stepwise increasing in speed (Traxler *et al.*, 2018). The same applies in this context. A ride recorded at less than 20km/h above the speed limit (but above a certain enforcement cutoff) is handled as a *minor speeding* offense. Until July 2018, minor speeding was punished by a fine of 900 CZK (approx. $40 or 3% of the average monthly wage). Speeding at between 20 and 40km/h above the limit is classified as an *intermediate speeding* offense and was, until July 2018, subject to a fine of 1,900 CZK (approx. $80). Speeding at more than 40km/h above the limit triggers a very different enforcement procedure (and higher

[8]See, among many others, Bergolo *et al.* (2019); Brockmeyer *et al.* (2019); Kleven *et al.* (2011); Meiselman (2018); Slemrod *et al.* (2001). Pomeranz and Vila-Belda (2019) offer a comprehensive survey.

[9]Cameras placed at the entry and at the exit points of a road section record cars' number plates together with a precise time stamp. Using the travel time between the entry and exit points, the average speed is computed. Emergency vehicles like ambulances and police cars are later excluded from the automated enforcement process. Beyond this exemption and unlike in other studies (see, e.g., Makowsky and Stratmann, 2009), there is no scope for discretion by police officers. See Dusek and Traxler (2020) for further institutional details.

penalties). Such speeding offenses are rarely observed in our data. Our analysis therefore focuses on minor and intermediate speeding offenses.[10]

During the second half of 2018 new speed cameras were installed and the fine structure was gradually adjusted. During the following months, there was considerable variation in fine amounts, both within and between speed cameras.[11] Since 2019, minor offenses have been fined at 700 CZK for speeding by less than 12km/h above the limit and 900 CZK for speeding by 12–20km/h above the speed limit. Intermediate offenses have been fined at 1,500 CZK and 1,900 CZK, respectively, for speeding by 20–26km/h and 26–40km/h above the limit. A summary of the fine structure is provided in Table 1.

Table 1: Fine structure

Offense category	Speed (above speed limit)	Fines pre 2018/7	Fines 2018/7–12	Fines since 2019/1
Minor speeding	< 12km/h	900 CZK	500–700 CZK	700 CZK
	12 – 20km/h	900 CZK	700–900 CZK	900 CZK
Intermed. speeding	20 – 26km/h	1,900 CZK	1,100–1,900 CZK	1,500 CZK
	26 – 40km/h	1,900 CZK	1,400–1,900 CZK	1,900 CZK

Authorities send payment notifications ('speeding tickets') to the registered address of the vehicle's owner either by regular mail or by e-mail.[12] If the owner pays the stipulated fine within 15 days of receiving the notification, the case is closed. If the full fine is not paid by the deadline, the authority initiates a trial-like process. This typically begins 1–2 months after the initial notification and triggers further legal notifications, (e-)mails and phone calls. The car owner may be found liable for a violation committed with his car, raising the total payment due (the initial fine plus a late fee) to 1,500–2,500 CZK for minor speeding and to 2,500–5,000 CZK for intermediate offenses. Within these ranges, the authority has full discretion in determining the exact payment. The car owner may also be identified as driver and could then, in addition, be punished by demerit points.

A key feature of the institutional set-up – which is also observed in other enforcement contexts (e.g. De Neve *et al.*, 2019) – is the poor communication of key parameters in the notification (officially titled 'summons to pay a prescribed amount'): it is highly convoluted and formalistic, full of legal terms and relatively lengthy (691 words, see Appendix A.4.2). It contains numerous legal extracts and information about the exact time, date and location of the traffic violation. The the payment deadline is poorly communicated and, in particular, the consequences of missing the deadline are not properly explained, but only stated in the vague phrase "*the office will continue investigating the offense*". Knowledge of administrative law is needed to understand that non-compliance implies that the offense will be processed under a different legal procedure, with various possible outcomes. Despite being explicitly stated, the salience of the payment deadline might be compromised, too, by the plethora of legal formalities in the text. The next section analyzes the implications of the limited salience of these attributes for individuals' payment decisions.

[10] As a concession to prevent appeals, the measured speed is rounded down to the next integer and then reduced by 3 km/h. The speed level cutoffs refer to this 'adjusted speed'. Our analysis consistently uses the accurately measured speed rather than the adjusted values.

[11] For some cameras zones, the authority imposed fines of 500–700 CZK for minor offenses. At other zones, it used fines of 1,100 or 1,400 CZK for intermediate offenses.

[12] The electronic mail is sent through an official e-governance platform called 'databox'. Almost all companies and some private individuals make use of this service.

3 Theoretical Framework

Appendix A.2 introduces a framework to analyze whether and when individuals pay their fines for speeding. The structure of the problem (which is similar to Altmann *et al.*, 2017), applies to many situations with notifications that communicate a payment deadline and consequences for non-compliance.

An individual receives a payment notification which stipulates a fine f, a payment deadline T, and an (expected) penalty for late payments, K. This late penalty K includes a late fee plus the costs associated with potential demerit points.[13] Paying the fine is associated with a transaction cost c_t, which is drawn from a given distribution. The total costs of paying the ticket in period t are then given by $C(t) = c_t + f$ for $t \leq T$. After the realization of c_t the individual decides whether to pay now, in period t, or whether to wait another period (and hope for a lower cost realization in the future). The opportunity to postpone the payment is constrained by the deadline T. If the deadline passes without having paid the fine, the individual's costs increase by K such that $C(t) = c_t + f + K$ for $t > T$. The optimal solution to the problem is characterized by a cutoff rule (see Appendix A.2; in line with our empirical analysis, we focus on the decision to pay or not to pay *before* the deadline). For any given period $t \leq T$, there exists a cutoff $\hat{c}_t$ such that the fine is paid whenever the cost c_t is below the cutoff. When the realized costs are higher, the agent prefers to wait for the next period $t + 1$.

Basic comparative statics. An important comparative static concerns the fine f. Does timely compliance – i.e. the rate of pre-deadline payments – decrease when the fine increases? Intuitively, one would expect to observe less compliance for higher fines. For a *ceteris paribus* increase in f, this is indeed what the model predicts. In practice, however, the discontinuous variation in f at the different speed level cutoffs might be accompanied by variation in the late penalty K.

The institutional features discussed in Section 2 suggests that we should observe a conjoint increase in f and K at the cutoff that separates minor and intermediate speeding offenses. An increase in K raises the costs of missing the deadline and thus works against the effect from higher fines. The overall effect of a joint increase in f and K on timely compliance is therefore ambiguous and remains an empirical question (see Appendix A.2 for a formal discussion of these comparative statics).[14] Conceptually, this ambiguity is similar to the classical income tax evasion model (Yitzhaki, 1974) and to compliance problems more generally (e.g. Berger *et al.*, 2016), where both the disincentives to comply (related to the tax rate) and the penalty for non-compliance (which is typically proportional to the tax rate) jointly increase.

Misperceptions. Accounting for the formalistic text of the payment notification, we consider the case where the deadline T and/or the costs of missing it, K, might not be fully salient to all individuals. Following Taubinsky and Rees-Jones (2018), we model individuals with (mis)perceptions $\tilde{T}(\theta_T) = \theta_T T$ and $\tilde{K}(\theta_K) = \theta_K K$, respectively. The θ-parameters (with $0 < \theta < \infty$) capture whether the deadline and/or the late pay penalty are over- ($\theta > 1$) or underestimated ($\theta < 1$). The full salience benchmark is given by $\theta_K = \theta_T = 1$.

[13] Under risk neutrality, K also covers the case in which an individual expects to get away with just paying the 'regular' fine f after the deadline with probability $1 - p$. With probability p, she would expect to pay f plus an additional penalty, κ. The expected late pay penalty would then equal $K = p\kappa$.

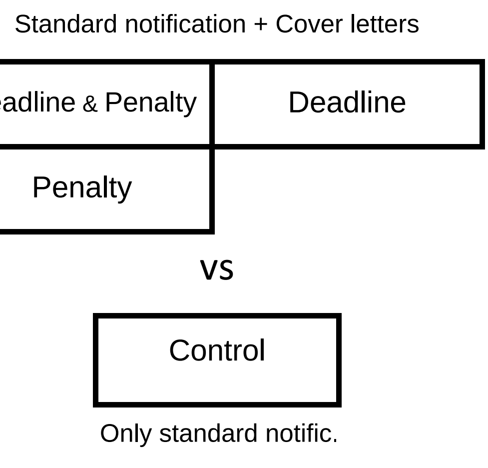
[14] In Section 5, we will empirically face an increase in f that is accompanied with a *decrease* in K. For this case, we predict an unambiguous drop in timely compliance (see Appendix A.2).

In Appendix A.2 we show that an individual's likelihood of paying the fine before the deadline T, is *cet. par.* increasing in θ_K. The higher the perceived costs of missing the deadline, the higher the chance of observing a pre-deadline payment. Individuals that over- [under-] estimate K are thus more [less] likely to comply with the deadline. In a similar vein, one can show that the likelihood of timely payments is *lower* for individuals with $\theta_T > 1$. An individual who overestimates the deadline length expects to have more opportunities to draw low costs c_t before the late penalty kicks in. In any period $t \leq T$, the incentive to pay immediately is lower (as compared to an agent who optimizes subject to the correct, shorter deadline T). As a consequence, individuals with $\theta_T > 1$ are less likely to pay by the (true) deadline. The case of an underestimation of the deadline ($\theta_T < 1$) is more complex and theoretically ambiguous.[15] It could lead to either a higher or a lower chance of paying before the (true) deadline (see Appendix A.2). We will return to this point in the next section.

4 Salience and Simplification

Motivated by the formalistic nature of the payment notification we evaluated whether the timely payments of the fines could be increased by increasing the salience of the relevant parameters of the choice problem. In cooperation with the enforcement authority, we randomly assigned speeding offenders to four different groups. The *control* (C) group received only the standard notification without any cover letter. Three treatment groups received a cover letter on top of the notification. These cover letters were brief and simple: depending on the treatment, they contained between 44 and 74 words (in contrast to the 691 words in the standard notification) to convey the main information. The treatments aimed at increasing the salience of the payment deadline, the consequences of missing it, or both.

Figure 1: Experimental design



[15] On the one hand, the individual feels 'more pressured' during the first $\tilde{T}$ periods (anticipating a lower option value from postponing the payment). Hence, compared to an agent who optimizes s.t. the correct deadline, the individual is more likely to have paid by period $\tilde{T}$. During periods $\tilde{T} < t \leq T$, on the other hand, the agent with correct deadline perceptions may have a higher propensity to pay (conditional on not having paid before): the proximity to the (correctly perceived) deadline increases the agent's willingness to pay despite higher opportunity costs and the individual with misperceptions anticipates a higher payment obligation $f + \tilde{K}$ for $t > \tilde{T}$. Hence, there might be a catch-up during this period such that the overall effect of underestimating the deadline on pre-deadline payments remains ambiguous.

Each cover letter briefly informed the recipient that she is summoned to pay a fine for speeding. The *Deadline* (D) treatment asked to "*Please pay the amount in full ... **within 15 days** after receiving this summons.*" The *Penalty* (P) treatment emphasized the consequences of non-compliance: "*If you do not pay the whole amount the office will continue investigating the offense. The amount that you will potentially have to pay **may be as high as 2,500 CZK***".[16] The *Deadline & Penalty* ($D\&P$) treatment, combined both of these two texts.

Our experimental design, which is summarized in Figure 1, can be interpreted as an incomplete (2×2) factorial design. As the authority refused to send out plain cover letters (emphasizing neither K nor T), there is no treatment cell with such a letter. The comparison of outcomes between the control and treatment groups will thus capture the joint effect of simplification and increased salience of either K, T or both.[17]

4.1 Predictions

Based on the framework from Section 3 we derive several predictions (see Appendix A.2). First, we expect the treatments that make the penalties more salient (the P and $D\&P$ treatments) to increase the salience of K. Relative to the control group, both treatments should reduce misperceptions, thus shifting $\tilde{K}$ towards the true K. This should decrease the rate of pre-deadline payments from drivers who overestimate K. The opposite prediction emerges for drivers who underestimate K. Among the latter, the treatments would *increase* timely payments (relative to the control).

Second, the impact from the D and $D\&P$ treatments, which make the deadline more salient, again depends on prior misperceptions. For drivers who overestimate the deadline T, the treatments should increase the rate of timely payments (i.e. payments within 15 days). As discussed in Section 3, the prediction for those who underestimate the length of the deadline is ambiguous. For this case we might observe a lower rate of timely payments.

Third, there is scope for an interaction effect. The impact of the $D\&P$ treatment on payment rates might be larger than the sum of the effects from the D- and the P-treatment, whenever both treatments work into the same direction. If drivers underestimate the late-pay penalty and overestimate the deadline (i.e. if $\tilde{K} < K$ and $\tilde{T} > T$) we could, in principle, observe a positive interaction.

4.2 Survey Experiment

The discussion highlights that the predicted treatment effects depend on prior (mis)perceptions and we have identified the direction in which the treatments should alter these perceptions. We study these points in a survey experiment, which exposed a sample of $N = 1{,}609$ individuals (aged 18 or above and holding a driving license) to the different treatments. The survey was conducted online among subjects in the Czech National Panel, a panel of respondents that is representative of the Czech population and is maintained by three professional survey providers.[18] Survey participants

[16] Bold font was also used in the actual cover letter (see Appendix A.4 for the full text). For intermediate speeding offenses (with speeds of 20–40km/h above the limit), the latter part would read "...as high as 5,000 CZK".

[17] De Neve *et al.* (2019) isolate 'pure' simplification effects. While these authors replace letters from tax authorities with more simple ones, the legal framework of our context prevented us from replacing the standard notification.

[18] Our survey was administered by one of the providers, NMS Market Research. Further information is available at www.narodnipanel.cz.

were on average 43 years old and half of them were females. Consistently with random treatment assignment, all observable characteristics are balanced across treatments (see Table A.1).

Participants were first exposed to a hypothetical scenario description, which explained that they were caught by a speed camera in a minor offense. In a second step, they were randomly assigned to either the control group or one of the treatment groups from our RCT. Based on that assignment, they were presented with speeding tickets with exactly the same text and graphical layout as the actual ticketed car owners in our RCT, that is, the standard notification (control group) or one of the three cover letters followed by the standard notification (treatment groups).[19] Thereafter (and without the opportunity to return to the text of the notification or the cover letter) the subjects were asked questions about their perceptions regarding the deadline (T) and the penalty for missing it (K). Acknowledging differences in incentives and sampling w.r.t. our RCT, the survey experiment provides proxies for the extent and direction of misperceptions (θ_T and θ_K in the control group) as well as the treatments' effect on these misperceptions.

Perceptions about the deadline. Perceptions about the deadline T were assessed with a question asking "when do you think you have to pay the full amount of the fine?". The four response options were within 7, 15, 30, or 60 or more days after the receiving the notification. Using binary response dummies as dependent variables, we estimate models of the structure

$$T7_i = \beta_0 + \beta_1 \text{Deadline}_i + \beta_2 \text{Penalty}_i + \beta_3 \text{Deadline\&Penalty}_i + X_i \gamma + \varepsilon_i \tag{1}$$

(where $T7_i$ indicates that subject i responded 'within 7 days'). The constant, β_0, indicates the share of subjects who gave this response in the control treatment. The treatment effects are captured by the coefficients β_1, β_2 and β_3, respectively. X_i is a vector of control variables. Table 2 presents linear probability model estimates of equation (1).

Table 2: Survey – Treatment effects on deadline perceptions

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Responses:	within 7 days		within 15 days		within 30 days		within 60+ days	
Deadline	-0.086***	-0.088***	0.169***	0.171***	-0.078***	-0.077***	-0.005	-0.005
	(0.020)	(0.020)	(0.029)	(0.029)	(0.023)	(0.023)	(0.007)	(0.007)
Penalty	-0.028	-0.029	-0.016	-0.015	0.051*	0.052*	-0.007	-0.007
	(0.023)	(0.023)	(0.033)	(0.033)	(0.028)	(0.028)	(0.007)	(0.007)
Deadline & Penalty	-0.073***	-0.073***	0.148***	0.148***	-0.076***	-0.075***	-0.000	-0.000
	(0.020)	(0.020)	(0.029)	(0.029)	(0.023)	(0.023)	(0.008)	(0.008)
Constant	0.131***	0.163***	0.691***	0.620***	0.166***	0.194***	0.012**	0.023*
	(0.017)	(0.028)	(0.023)	(0.044)	(0.019)	(0.037)	(0.006)	(0.013)
Controls	No	Yes	No	Yes	No	Yes	No	Yes
F-Tests (p-values):								
$D = P$	0.002	0.001	0.000	0.000	0.000	0.000	0.656	0.732
$D = D\&P$	0.394	0.343	0.421	0.377	0.909	0.913	0.503	0.453
$P = D\&P$	0.020	0.021	0.000	0.000	0.000	0.000	0.270	0.270

Notes: The table presents linear probability model estimates of equations following the structure from (1). N = 1,609. Control variables include age, gender, and education dummies. Robust standard errors in parentheses. ***/**/* indicate significance at the 1%/5%/10%-level, respectively.

[19] The names of the local authority and the speed camera location were replaced by generic placeholders.

A comparison of the constant across columns indicates the priors. In the baseline treatment, 69.1% of participants correctly indicate a 15-day deadline (Column 3). Hence, despite the formalistic structure of the notification, the deadline (which is mentioned repeatedly in the notification; see Appendix A.4.2) was sufficiently salient to a clear majority of survey participants. Put differently, there is only a modest level of deadline misperception: 13.1% underestimate the deadline length (Column 1) whereas 16.6% (Column 5) plus 1.2% (Column 7) overestimate it. The scope for the treatments to reduce misperceptions is therefore limited.

Notwithstanding, the estimates document that the D and $D\&P$ treatments have a consistent impact. The two treatments that make the 15-day deadline more salient increase the number of correct responses by 15pp and 17pp, respectively (Column 3). The P treatment, in contrast, has no effect. Our estimates further document that the increase in correct perceptions is achieved by a reduction in the fraction of respondents that underestimate the deadline (Column 1) and by an equally pronounced decline in the share of subjects that expect a deadline length of 30 days (Column 5). All these estimates remain virtually unchanged if we include control variables. This pattern is reassuring and consistent with successful randomization.

Perceptions about the late penalty. Let us now turn to the late penalty K, i.e. the late fee plus potential demerit points (on the interpretation of K compare fn. 13). Most respondents are well aware that not paying the fine before the deadline implies a late fee. In the control treatment, 81.2% expect an increase in the payment obligation (see Column 1, Table 3). Specifications (1) and (2) also show that the P- and $D\&P$-treatments, which highlight the penalty, further increase this share by 6pp and 9pp, respectively. (F-tests do not reject that these effects are of equal size; $p = 0.24$.) The D treatment, which emphasizes only the deadline, has no effect. This is a first piece of evidence suggesting that the two penalty treatments increase θ_K.

Table 3: Survey – Treatment effects on late fee perceptions

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Responses	Expect higher costs		$\leq$ 2000 CZK		2500 CZK		$\geq$ 3000 CZK	
Deadline	0.007	0.005	0.063*	0.058*	-0.039*	-0.037*	-0.025	-0.021
	(0.027)	(0.027)	(0.035)	(0.035)	(0.020)	(0.021)	(0.034)	(0.034)
Penalty	0.065**	0.064**	-0.295***	-0.298***	0.423***	0.423***	-0.127***	-0.125***
	(0.026)	(0.026)	(0.033)	(0.033)	(0.030)	(0.030)	(0.032)	(0.032)
Deadline & Penalty	0.091***	0.090***	-0.339***	-0.342***	0.467***	0.467***	-0.127***	-0.125***
	(0.024)	(0.024)	(0.032)	(0.031)	(0.029)	(0.029)	(0.032)	(0.032)
Constant	0.812***	0.834***	0.530***	0.477***	0.111***	0.131***	0.359***	0.391***
	(0.019)	(0.033)	(0.025)	(0.045)	(0.016)	(0.036)	(0.024)	(0.044)
Controls	No	Yes	No	Yes	No	Yes	No	Yes
F-Tests (p-values):								
D = P	0.024	0.022	0.000	0.000	0.000	0.000	0.001	0.001
D = D&P	0.001	0.000	0.000	0.000	0.000	0.000	0.001	0.001
P = D&P	0.242	0.238	0.127	0.126	0.208	0.206	0.999	0.992

Notes: The table presents LPM estimates of equations following the structure from eq. (1). N = 1,609. The dependent variable in Columns (1) – (2) is a dummy indicating that a subject responded *yes* to the question *'If you do not pay the full amount of the fine by the deadline, would you expect to pay a higher fine?'*. Columns (3) – (8) are based on a subsequent question regarding the expected total amount due, including the late fee (i.e. $f + K$). The dependent variable in Columns (3) and (4) captures responses indicating *1,500*, *2,000* CZK, as well as responses of subjects that answered *'no'* when asked if they would expect a higher fine if they did not pay by the deadline. Columns (5) – (6) indicate responses of *2500* CZK and Columns (7) – (8) pair responses with *3,000*, *3,500*, *4,500* and *more than 4,500* CZK. Control variables include age, gender, and education dummies. Robust standard errors in parentheses. ***/**/* indicate significance at the 1%/5%/10%-level, respectively.

Columns (3) – (8) explore responses to the question "what would be the total amount you would eventually have to pay?".[20] The constant terms from Columns (3), (5) and (7) indicate a pronounced level of misperceptions of the amount of post-deadline payment obligations. A larger share of respondents underestimate the costs of missing the deadline: 53% expect a total payment of 2,000 CZK at most (Column 3). In addition, 36% anticipate a total amount of 3,000 CZK or more (Column 7). The P and $D\&P$ treatments have a strong impact on these perceptions. The share of respondents expecting a payment of 2,500 CZK rises (from a baseline of 11%) by 42pp and 47pp, respectively (Column 5). The shift in perceptions is primarily due to a drop in underestimations (−30pp to −34pp, Column 3). P and $D\&P$ also reduce the overestimation (−13pp, Column 7). Again, the estimates remain unchanged when we control for individual characteristics.[21]

Similar effects are observed for the survey participants' expectations regarding demerit points (an additional element of the late penalty). In the baseline treatment, 32% expect to get demerit points for missing the deadline. In the P and $D\&P$ treatments, this rate increases by 30pp (see Appendix Table A.2). For this variable, we further observe an interesting correlation: within the control group, those who overestimate the deadline length are also more likely not to expect demerit points for missing the deadline. This observation provides some support for the (sufficient) condition for a positive interaction effect of the $D\&P$ treatment (i.e. $D\&P$ yielding a larger impact than the sum of the D and P treatments; see Section 4.1).

To sum-up, our survey results indicate a relatively high share of correct deadline perceptions. The D and $D\&P$ treatments further raise the salience of the correct deadline. This is reflected in an equally sized drop in over- ($\theta_T > 1$) and underestimations ($\theta_T < 1$) of the deadline length. Our framework from Section 3 suggests that these adjustments in perceptions may have opposing effects. The data therefore suggests that the impact of the D and $D\&P$ treatments on timely payments are ambiguous: the treatment effects might be small or even zero.

We find more pronounced misperceptions regarding the late penalty (fines and demerit points). A clear majority underestimates K. The P and $D\&P$ treatments' impacts on the perceived penalties are also stronger than the comparable impact of the deadline treatments on the deadline. Likewise, the penalty treatments also alter perceptions in opposing directions. Quantitatively, however, the dominant effect is clearly an increase in the perceived costs of missing the deadline. Moreover, the penalty treatments have unambiguous effects on other important dimensions of K (demerit points and the likelihood p, see fn. 13). Based on the survey evidence and our theoretical framework we therefore predict the treatments to increase timely compliance. Finally, there is some evidence of a negative correlation between θ_K and θ_T. This renders the case of a positive interaction effect (i.e. the $D\&P$ treatment having a larger impact than the sum of the D and P treatment) more plausible.

4.3 Sample and Implementation of RCT

Between November 2017 and August 2019 we randomly assigned N = 78,882 speeding tickets to one of our four treatments. Major speeding offenses (with a speed of more than 40km/h above the

[20] The question does not directly refer to the parameter K but asks about $f + K$. Our motivation for doing this is that both the notification and our cover letters refer to the higher total payment obligation – i.e. to the level rather than the difference.

[21] The D treatment seems to shift misperceptions towards an underestimation of the payment obligations (Columns 3–6). Note, however, that these effects are small and only weakly significant.

limit; see Section 2) were excluded from the randomization. During the expansion of the camera system in the second half of 2018, there was a glitch. Due to a programming error, speeding offenses recorded between August and November 2018 were randomized among only three treatments (C, D, and P), with no single observation for the $D\&P$ treatment. After the error was corrected, tickets were again randomly assigned to all four treatments. To re-balance the number of observations per treatment, we over-proportionally allocated cases to the $D\&P$-treatment for several months.[22]

Table 4: RCT – Summary statistics and balancing tests

	Full Sample	Control	Deadline	Penalty	Deadl& Penalty	F-Test$^{(a)}$	F-Test$^{(b)}$
Initial Cameras	0.230	0.231	0.241	0.242	0.208	0.000	—
	(0.421)	(0.422)	(0.428)	(0.428)	(0.406)		
Speed (km/h)	64.53	64.52	64.49	64.59	64.54	0.342	0.973
	(5.115)	(5.230)	(5.252)	(5.170)	(4.808)		
Fine (CZK)	836.3	836.6	837.0	837.3	834.5	0.653	0.152
	(245.4)	(248.3)	(246.0)	(246.7)	(240.7)		
Low Severity Offense	0.940	0.937	0.939	0.937	0.946	0.000	0.816
	(0.238)	(0.243)	(0.240)	(0.243)	(0.226)		
Number Plate: Prague	0.482	0.482	0.480	0.487	0.478	0.351	0.575
	(0.500)	(0.500)	(0.500)	(0.500)	(0.500)		
Number Plate: Central Bohemia	0.271	0.272	0.268	0.268	0.274	0.439	0.648
	(0.444)	(0.445)	(0.443)	(0.443)	(0.446)		
Company Car	0.458	0.449	0.454	0.461	0.467	0.002	0.074
	(0.498)	(0.497)	(0.498)	(0.498)	(0.499)		
Sent Electronically	0.400	0.392	0.398	0.401	0.409	0.004	0.137
	(0.490)	(0.488)	(0.489)	(0.490)	(0.492)		
Weekend	0.377	0.377	0.375	0.377	0.379	0.899	0.988
	(0.485)	(0.485)	(0.484)	(0.485)	(0.485)		
Sent-Received (days)	5.040	5.067	5.030	4.989	5.070	0.596	0.663
	(6.662)	(6.790)	(6.139)	(6.696)	(6.960)		
Number of tickets	78,882	20,399	19,025	19,012	20,446		
Number of cars	72,502	18,720	17,571	17,562	18,649		

Notes: Sample mean (with standard errors in parentheses) by treatment. The last two columns present the p-values from F-Tests of treatment balance: in (a) we run linear regressions with $x_i = \beta_0 + \beta_1 \mathrm{D}_i + \beta_2 \mathrm{P}_i + \beta_3 \mathrm{D\&P}_i + \varepsilon_i$ and then test the H0: $\beta_1 = \beta_2 = \beta_3 = 0$. In (b) we augment the regressions by including sending-week and speed camera fixed effects.

For each speeding violation we observe, among other characteristics, the date and time of the speeding offense, the vehicle's speed, the level of the fine, the date the ticket was generated, sent and received and the date when the fine was eventually paid. The average speed of ticketed offenses is 65km/h (roughly 15km/h above the limit). After being sent, it takes about 5 days for an average ticket to be received by the car owner. Around 45% of offenses are committed by company-owned vehicles, which means that the tickets are sent (typically electronically) to the company the car is registered to. The rest are privately owned vehicles. It is important to note that the bulk of our observations come from low severity offenses. Only 6% of offenses are of medium severity. Finally, note that around 10% of cars received two (or more) speeding tickets during our sample period. We independently randomized each offense, such that the treatment sequences are random, too.

As a consequence of the implementation issues mentioned above, our treatments are not fully balanced over time and space (between speed cameras). The latter point is also reflected in Table 4.

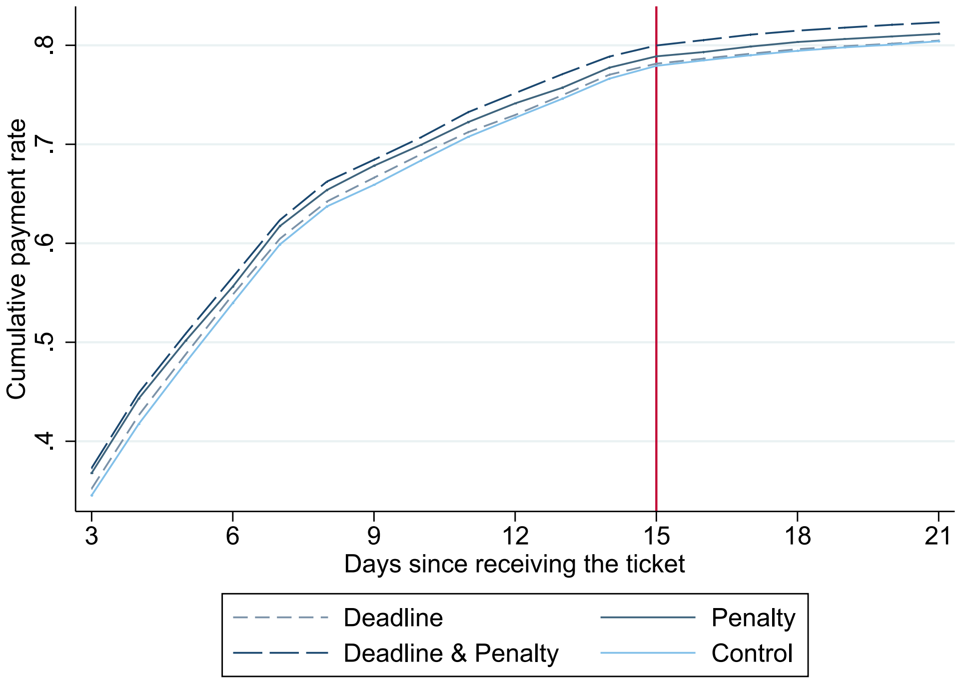
[22]Specifically, the probabilities of assigning an offense to the C, D, P and the $D\&P$ treatment were 20, 15, 15, and 50%, respectively. These proportions were maintained until May 2019. Between May and August 2019, we reverted back to an equal weight (25% per treatment).

In the $D\&P$-treatment, fewer observations are from the initial five speed cameras (21 vs 24%, see Table 4). F-tests indicate several additional imbalances. This is due to the average characteristics of the offenses observed during the time period when the $D\&P$ treatment was over-weighted in the randomization (see above). During this time period, the speed cameras recorded slightly more company cars (who also receive their tickets electronically) and more low-severity offenses. However, these imbalances vanish once we condition on time and space: after including sending-week and speed camera fixed effects, the second set of F-tests reported in Table 4 looks much more reassuring. In our estimations below we account for the imperfect randomization by including not only a large vector of observable characteristics but also a full set of sending-week and speed camera dummies. We will see that the imbalances do not seem to influence any of our results.

4.4 RCT Results

Figure 2 presents the cumulative payment rate across treatment conditions over the first 21 days after receiving the notification. (Figure A.1 in the Appendix covers 100 days.) The figure reveals small but systematic treatment differences.

Figure 2: RCT – Cumulative response rates by treatment



Notes: The figure illustrates, for each treatment, the cumulative payment rates (full amount) during the first 21 days after receiving the notification. The payment deadline (15 days) is indicated by the red vertical line.

The payment rates in treatment D are hardly distinguishable from those observed in the control group. By the 15 day deadline, 77.9% have paid the full amount in the control group. Treatment D increases this rate by a mere 0.2pp. Treatments P and $D\&P$, in contrast, produce a visibly positive effect: after just 3 days, payment rates are 2pp higher than in the control group. During days 6–15, the $D\&P$-treatment effect remains at this level but the P treatment effect shrinks. Within the 15 day period, the two treatments induce a 2.0pp (+2.6%) and 0.9pp (+1.2%) higher payment rate, respectively.

Next we estimate the treatment effects on the probability of paying within different time periods. We run linear probability model (LPM) estimates of the equation

$$\text{Pay}_{i,\tau} = \beta_{0,\tau} + \beta_{1,\tau}\text{Deadline}_i + \beta_{2,\tau}\text{Penalty}_i + \beta_{3,\tau}\text{Deadline\&Penalty}_i + X_i\gamma_\tau + \varepsilon_{i,\tau}, \qquad (2)$$

where $\text{Pay}_{i,\tau}$ is a dummy indicating whether the payment was made in full within $\tau = \{7, 15, 30, 100\}$ days after receiving the notification. The treatment effects on the outcome variables are captured by the β-coefficients. We control for X_i, a vector of car and offense characteristics, which, as discussed above, includes sending-week and speed camera zone fixed effects.

Table 5: RCT – Treatment effects on payments (LPM estimates)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Pay-7days		**Pay-15days**		Pay-30days		Pay-100days	
Deadline (β_1)	0.58	0.63	0.22	0.27	0.02	0.12	0.00	0.12
	(0.49)	(0.48)	(0.41)	(0.39)	(0.38)	(0.36)	(0.37)	(0.35)
Penalty (β_2)	1.85***	1.87***	0.96**	0.93**	0.71*	0.73**	0.50	0.54
	(0.49)	(0.48)	(0.42)	(0.40)	(0.39)	(0.37)	(0.37)	(0.35)
Deadline & Penalty	2.46***	2.38***	2.06***	2.02***	1.78***	1.78***	1.75***	1.59***
(β_3)	(0.49)	(0.50)	(0.42)	(0.41)	(0.39)	(0.38)	(0.37)	(0.36)
Constant (β_0)	59.88***	—	77.93***	—	81.55***	—	83.50***	—
	(0.39)		(0.37)		(0.36)		(0.35)	
Controls & FEs	N	Y	N	Y	N	Y	N	Y
F-Tests (p-values):								
$\beta_1 = \beta_2$	0.010	0.011	0.074	0.096	0.077	0.099	0.180	0.229
$\beta_1 = \beta_3$	0.000	0.001	0.000	0.000	0.000	0.000	0.000	0.000
$\beta_2 = \beta_3$	0.224	0.326	0.011	0.009	0.008	0.007	0.001	0.005
$\beta_3 = \beta_1 + \beta_2$	0.962	0.860	0.144	0.158	0.064	0.085	0.022	0.070

Notes: The table presents LPM estimates of equation (2) with alternative dependent variables. The dependent variable is an indicator for payment of the full amount within $\tau = \{7, 15, 30, 100\}$ days after receiving the notification. Specifications 2, 4, 6 and 8 contain dummies for the different speed cameras and the week the speeding ticket was sent as well as a vector of control variables. These controls account for the level of the fine, the severity of the speeding offense, the measured speed, whether the ticket was sent by regular (vs. electronic) mail, whether the vehicle is owned by a company, the region where the car is registered, the day of the week and the hour of the day when the offense took place, as well as the number of days between the offense and when the ticket was received. The number of observations for all specifications is 78,882. The estimated coefficients (β_0–β_3) and the corresponding standard errors (in parentheses) are multiplied by 100 and can be interpreted as percentage point values. Standard errors are clustered at the vehicle level. ***/**/* indicate significance at the 1%/5%/10%-level, respectively.

The estimates from Table 5 confirm the descriptive evidence from above. The D treatment has a positive but imprecisely estimated effect on payments during the first 7 days. The treatment's effect on the rate of pre-deadline payments is statistically insignificant. Column (4) documents a point estimate of +0.27pp (with an upper bound of the 95%-confidence interval of 1.0pp). This finding is consistent with the survey evidence from Section 4.2, which suggests that (i) most individuals have a correct perception of the deadline and (ii) that treatment D corrects deadline misperceptions by reducing both under- and overestimations of the deadline.[23] This two-way adjustment of misperceptions has opposing effects on timely compliance. As a result, the average treatment effect on payments within 15 days is very close to zero.

[23] Further evidence along these lines is provided by a duration analysis (see Table A.5 and the discussion below). The estimates indicate that the pre-deadline hazard (i.e. the probability of paying conditional on not having paid before) in the D treatment are weakly above those in the control group (in particular during the first week after receiving the treatment); in the post-deadline period (days 15–30), in contrast, the hazard is slightly below that observed for the control. This pattern is in line with the treatment's impact on deadline under- and overestimations.

The penalty treatment P raises the rate of pre-deadline payments by around 1pp and the interacted $D\&P$-treatment by 2pp. F-tests indicate that both effects are significantly larger than the effect from the D-treatment. These results are again consistent with the survey evidence, which indicates that there is more scope for altering misperceptions regarding the costs of missing the deadline (rather than misperceptions about the deadline itself). Many individuals underestimate the late penalty, such that both the P- and the $D\&P$-treatment raise the perceived costs on average (see Table 3). The observed effects are thus in line with our predictions: raising θ_K increases timely compliance.

Note that the $D\&P$-treatment has a significantly stronger impact than the P-treatment ($p \approx 0.01$; see the F-tests reported in Columns 3 and 4, Table 5). This suggests that adding emphasis to the deadline increases compliance when the late penalty is already salient. This point is consistent with the idea of a positive interaction from jointly increasing the salience of the deadline and the late penalty. Based on the estimates reported in Table 5, however, we cannot reject that the effect size of the $D\&P$-treatment equals the sum of the D- and the P- treatments – either for the 7 day or the 15 day outcome window (Columns 1–4). For the latter case, the F-test of the H0: $\beta_3 = \beta_1 + \beta_2$ yields $p \approx 0.15$.[24] This also implies that we cannot reject $\beta_3 - \beta_2 = \beta_1$: jointly making the deadline and late penalties salient has a larger but statistically indistinguishable effect from emphasizing only the deadline.

Robustness and extensions. How robust are these findings? First, it is reassuring to note that the estimates are virtually unaffected when we add controls. Hence, the imbalances associated with the imperfect implementation of the RCT seem to have little influence on average treatment effects. Second, when we consider payments for different outcome periods (with τ smaller or larger than the deadline T), we observe – consistently with Figure 2 – stronger treatment effects within one week and smaller effects on cumulative payment rates within 30 or 100 days. It is important to emphasize, however, that payments in the post-deadline period are very difficult to interpret because they are shaped by additional enforcement activities (which are largely unobserved in our data). This caveat is certainly relevant for the 100 day period, but also applies to the 30 day window: after just three weeks, the authority may reach out to a non-compliant car owner via phone or mail. Hence, any additional enforcement effort beyond the notification naturally works towards reducing the treatment effects. We nevertheless observe a persistently positive effect of the $D\&P$-treatment. Even 100 days after the delivery of the payment notification – and after up to 85 days of post-deadline enforcement actions – the payment rate in this treatment is 1.6–1.7pp (+2%) higher than in the control group (Columns 7 and 8, Table 5).[25] This finding is remarkably similar to the dynamic effects reported in De Neve *et al.* (2019). We return to discussing the implications of this persistency in Section 4.5. Finally, we also examined different corrections for multiple hypothesis testing (List *et al.*, 2019; Romano and Wolf, 2005). The results from this sensitivity analysis, which are reported in Table A.3, suggest that the inference from Table 5 remains qualitatively robust when we account for multiple testing.

[24] F-tests for outcome periods beyond the deadline indicate significant differences (Table 5, Columns 5–8). Note, however, that payments in the post deadline period are difficult to interpret (see below).

[25] The constant reported in Column (7) of Table 5 reveals a 100-day payment rate of 83.5% (in the control group). Compared to other domains, this is a high collection rate (see Fig. 2.2 in Piehl and Williams, 2010).

Exploring intensive margin responses, we next studied partial (rather than full) payment of fines. It turns out that 99.9% of all observed payments cover the full amount. We therefore obtain very similar estimates to those reported above (see Panel A, Table A.4). Put differently, there is no scope for intensive margin responses to drive the results from above. The treatments work at the extensive margin, turning non-paying speeders into paying ones. Next we examined whether the repeated treatment of car owners with multiple tickets influences our results. When we replicate the LPM estimates for a sample with just one ticket per car (the first treatment), the estimates remain again almost unchanged (see Panel B, Table A.4).

We also conducted a duration analysis to explore the exact timing of payments. In particular, we estimated hazard models with and without time-varying treatment effects. The results, which are reported in Table A.5 in the Appendix, corroborate our findings from above. Consistently with Figure 2, the duration analysis documents equally strong, positive effects from the P- and the $D\&P$-treatments during the first 7 days after receiving a speeding ticket. During days 8–15, the hazard rate in the $D\&P$-treatment (i.e. the probability of paying the fine conditional on not having paid before) remains roughly 6% above the corresponding rate in the control group (see Columns (3b) and (4b), Table A.5). For the D-treatment, in contrast, there is no difference in conditional payment rates during this period. Hence, it is the payment decisions in the pre-deadline week that lift the effect size of the $D\&P$-treatment above the P-treatment. The estimates further show that the treatment effects are concentrated in the pre-deadline period: after day 15, there are no statistically significant differences in hazard rates.[26]

In an additional step, we assessed the heterogeneity of the effects. Running our main LPM estimates (with full payments within 15 days as the dependent variable) on various sub-samples, we detect little heterogeneity. Only for the $D\&P$-treatment do we observe a significantly stronger treatment effect on private car owners compared to company cars (+3pp vs. +1pp; see Table A.6). A very similar pattern is observed for speeding tickets delivered by regular versus electronic mail. Given that almost all companies receive speeding tickets via e-mail (see fn. 12) and almost all private owners receive them via regular mail, we cannot pin down whether the form of delivery or the type of receiver drives this heterogeneity. Ortega and Scartascini (2020), who experimentally vary the way in which taxpayers with outstanding tax debt are approached, find that e-mails have a stronger impact on payment rates than letters. If this finding were to generalize to our context, it would imply that the observed pattern mainly reflects lower responsiveness of corporations than private car owners.

Finally, we examined whether the estimated treatment effects vary over time. Comparing tickets that were sent early or late within a given calendar month, we do not detect any systematic differences relative to the average treatment effects reported above. This suggests that short run liquidity constraints (which could be tied to pay check intervals) do not seem to be main drivers of non-compliance. This might be due to the fact that the region is relatively well off (many ticketed drivers are local) and that a 15 day payment deadline allows for a reasonable time period to adjust to negative income shocks.

[26] The estimation results hardly differ between Cox proportional hazard and complementary log-log models.

4.5 Cost-benefit analysis

The evidence from the RCT indicates that increasing the salience of the late penalty – in particular in the *D&P* treatment – has a statistically significant but quantitatively small effect on timely compliance.[27] A cost-benefit analysis reveals that the most successful treatment is still highly cost-effective.

The first benefit from increasing pre-treatment compliance relates to the resources the public authorities would spend on additional enforcement measures. Based on the time the administrators spend on follow-up actions when tickets are not paid within 15 days, the increase in timely payments caused by the *D&P* treatment (+2.06pp) translates into a reduction in administrative costs of at least 1.03 CZK per ticket (see Appendix A.3 for details). A second fiscal benefit is given by the gains in collected revenues. In addition to the increase in the payment of fines, one must account for the treatment's impact on late fees. Hence, in addition to timely compliance, the post-deadline payments (and the shift between late and timely payments) are relevant for assessing the fiscal implications. As discussed in Appendix A.3, the overall revenue consequences of the treatment can be approximated by the *D&P* treatment's impact on payments within 15 and 100 days. Based on the corresponding estimates from Table 5, the mean value of f and a proxy for K, we arrive at a revenue gain of 11.83 CZK per ticket. Together with the savings in enforcement costs, the *D&P* cover letter thus yields a marginal benefit of approximately 12.86 CZK per ticket.

The marginal costs of the treatment are zero for the 40% of tickets that are sent electronically (see Tab. 4). Accounting for the marginal costs of the remaining 60% of tickets, we arrive at fiscal costs of approximately 0.24 CZK per treatment cover letter (see Appendix A.3). Hence, the ratio of marginal fiscal benefits over costs is well above 50 – this result is mainly due to the near-zero costs (mirroring one of the key arguments for nudging; see, e.g., Benartzi *et al.* 2017). Summing up fiscal benefits and costs, the treatment yields a net revenue gain of 11.62 CZK per ticket. As the camera systems generate roughly 3,500 tickets per month, this would translate into monthly net benefits of roughly 44,000 CZK for the authority. This sum equals around 125% of the monthly labor costs of an administrator handling the tickets.

The above discussion omits several points that would enter a comprehensive welfare analysis of the intervention. Note first that the increase in collected revenues (11.83 CZK per ticket) represents a mere transfer between car owners and the authority. Neglecting this transfer, we are left with savings in enforcement costs of 1.03 CZK that come along with costs of 0.24 CZK. This would leave us with a social gain of 0.79 CZK per ticket. Second, the treatment impacts car owners and their choices in numerous, welfare relevant ways. Paying earlier, for instance, implies that some decision makers bear higher opportunity costs (see Section 3). At the same time, the treatment seems to correct misperceptions about the costs of non-compliance. The increase in timely compliance thus means that car owners avoid the (for some, surprisingly) costly follow-up enforcement process. In addition, the cover letter may be valuable in its own right. In fact, our survey finds that 57% of respondents found the information provided in the *D&P* treatment 'somehow useful' and 21% found it 'very useful'.[28] To the extent that the cover letters facilitate the processing of information, the

[27] Recall that the effect size is consistent with results from other RCTs testing behavioral strategies to enforce (tax) payments (see Antinyan and Asatryan, 2019).

[28] These responses resemble those documented by Allcott and Kessler (2019), who evaluate the welfare implications of home energy reports.

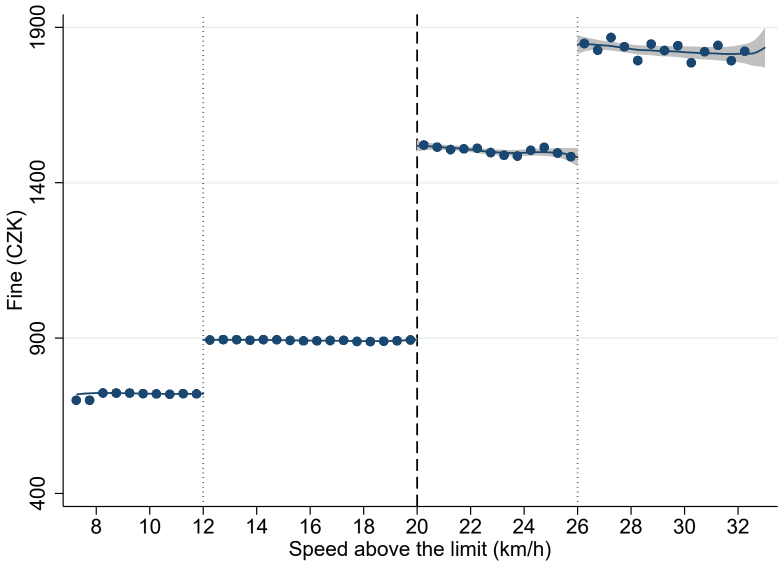
treatment may also reduce the time spent deciphering the content of the legal notification. The intervention might therefore reduce non-trivial compliance costs (see, e.g. Evans, 2003).

A quantification of these different channels and a fully-fledged (behavioral) welfare analysis is beyond the scope of this paper. However, two implications should be clear from our discussion. On the one hand, the intervention is welfare-improving as long as the net welfare impact on car owners is non-negative (or, in money-metric terms, larger than −0.79 CZK per ticket) – a condition that seems to be plausibly met. On the other hand, any net welfare gain would, most likely, be much smaller than what is suggested by the mere fiscal cost-benefit analysis.

5 RDD: Higher fines, lower compliance?

The results of the RCT show that increasing the salience of late penalties has a statistically significant but quantitatively small impact on timely compliance. This section studies the influence of the level of the fine f. To do so, we implement a pre-registered regression discontinuity design which exploits the increase in fines at the speed level cutoffs from Table 1.[29]

Figure 3: RDD – Discontinuities in fines (f)



Notes: The figure presents the observed fine for different levels of speed. The lines indicate local linear fits (with a bandwidth of 2.0km/h) together with 95%-confidence intervals. Each dot represents the mean fine in a 0.5km/h bin.

5.1 Discontinuous Policy Variation

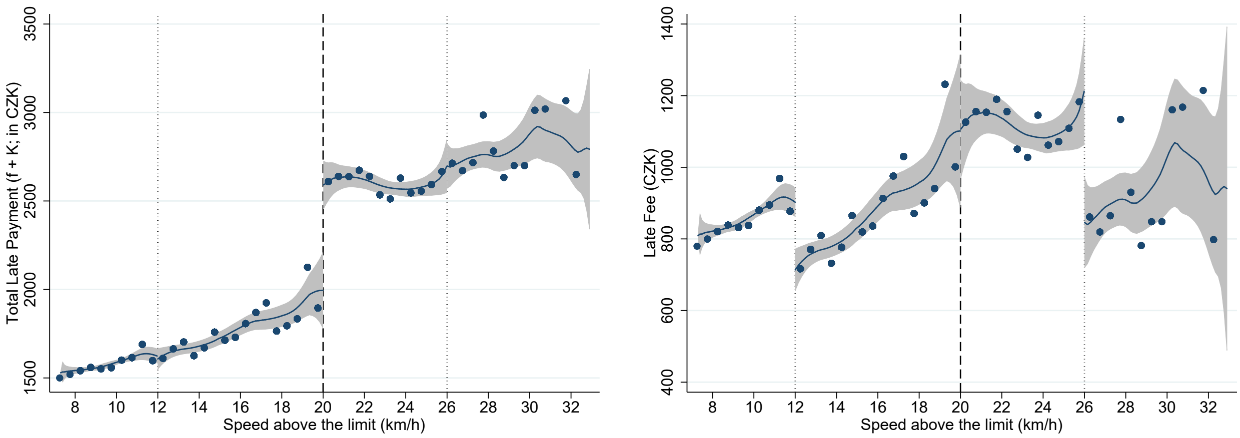
The discontinuous variation in fines is illustrated in Figure 3, which plots binned data as well as local linear regressions of the magnitude of the fine (in CZK) for different levels of speed above the limit. At the main cutoff, 20km/h above the speed limit, we observe a major jump in fines from approximately 900 to 1500 CZK (+623 CZK) on average, corresponding to a 70% increase

[29] Our pre-analysis plan specified the empirical approach for the cutoff at 20km/h above the speed limit. We apply the same, pre-registered method to the other two cutoffs (at 12 and 26km/h above the limit), which were only introduced ex-post by the authority and were thus not anticipated in Dusek *et al.* (2017).

(compare Table A.8 in the Appendix). The other two discontinuities are less pronounced: at the 12km/h cutoff, the fine increases by 24% (174 CZK); at the 26km/h cutoff, the increase is similar in relative terms (25%) but larger in absolute terms (368 CZK).

As discussed in Section 2, the cutoffs might be also associated with a discontinuous increase in the late fee. This appears particularly relevant for the main, 20km/h cutoff: in case of non-compliance with the payment deadline, the authority can impose total payments due $(f+K)$ of 1,500–2,500 CZK for minor speeding offenses and 2,500–5,000 CZK for intermediate offenses. To assess this point empirically, we explore a sample of 5,267 speeding tickets that were not paid in time and for which the administrative data contain information on the total amount due.[30] The empirically observed variation in total payments due is presented in the left graph of Figure 4. The graph reveals several interesting patterns.

Figure 4: RDD – Discontinuities in total payments due $(f + K)$ and late fees (K)



Notes: The left figure illustrates the empirically observed total payment (fine f plus late fee K) for a sample of 5,267 speeding tickets that were not paid in time. The right figure re-scales the pattern by subtracting the estimated fine for the different speed ranges (see Figure 3) from the total payment, thus yielding a proxy for the monetary component of the late penalty K (i.e. neglecting demerit points). The lines indicate local linear fits (with a bandwidth of 2.0km/h) together with 95%-confidence intervals. Each dot represents the mean total payment (left figure) or late penalty (right) in a 0.5km/h bin.

First, there is a clear positive trend in the total payments due. Given that the fine f is basically invariant within the speed ranges separated by the cutoffs (see Figure 3), this suggests that the imposed late fees are increasing in the speed. Second, we do not detect discontinuities in total payments due at the 12 or the 26km/h cutoff. In contrast, there is a clear discontinuity at the cutoff 20km/h above the limit: total payments due jump from 2,000 to roughly 2,600 CZK at this speed level. This observation implies that the discontinuity in $f + K$ solely mirrors the discontinuity in the fine f. Hence, there is no (additional) discontinuity in the late fee K at the main cutoff.

This last point is illustrated in the right graph of Figure 4. The figure plots a direct proxy for K, which is obtained from subtracting the basic fine f from the total payments (see Figure 3). As noted above, the data indicate that the authority does not impose any discontinuous increase in K at the main cutoff. At the same time, the fact that total payments due evolve smoothly around the other two cutoffs (documented in the left graph) implies that the authority defacto applies negative discontinuities in K at the 12 and 26km/h cutoff (right graph).

[30]This is clearly a non-randomly selected sample of tickets. Note further that entries for the total (late) payments due are sometimes missing in our data and that we lack systematic information on demerit points.

What do these patterns imply for the comparative statics discussed in Section 3? The main 20km/h cutoff provides variation in f while keeping K constant. Our model framework thus yields an unambiguous prediction: for speeding tickets that are marginally above this cutoff, the higher fine should induce a lower level of timely compliance. At the other two cutoffs, there is conjoint variation in f and K. Assuming that drivers correctly anticipate the pattern in K, the increase in the fine and the drop in the late fee both reduce the incentives for timely compliance (see Appendix A.2).[31]

5.2 Empirical Approach

To empirically test our prediction, we first estimate reduced form models of the structure

$$\text{Pay}_{i,15} = \delta^k D_i^k + \lambda^k(S_i) + v_i, \tag{3}$$

for each cutoff k. The dummies D_i^k indicate whether the speed S_i of a ticket is above a given speed level cutoff (i.e. k = 12, 20, or 26km/h, respectively); the flexible function $\lambda^k(.)$ (which is allowed to differ on the left and the right of a cutoff) captures the correlation between S_i and the payment rate. Similarly to the figures above, we estimate these functions non-parametrically. The coefficient of interest, δ^k, measures the discontinuity in timely compliance at cutoff k. The comparative statics from our model imply that all reduced form coefficients δ^k should be negative: a higher fine (and a drop in the late penalty) should result in a lower level of compliance.

In addition to the reduced form we also estimate the discontinuous variation in fines:

$$\log(\text{fine})_i = \gamma^k D_i^k + \kappa^k(S_i) + u_i. \tag{4}$$

The model yields the 'first-stage' coefficients γ^k. Together with the reduced form estimates from (3) one obtains Wald estimates $\beta^k = \delta^k/\gamma^k$ for each cutoff k. The estimates can be interpreted as semi-elasticities of timely compliance w.r.t. the fine f. However, our discussion above suggests that this interpretation only applies to the estimate at the main cutoff, where K is constant. At the other cutoffs, the estimated δ^k (and thus β^k) may reflect the effect of a joint change in f and K.[32]

Validity of design. For the RDD to be valid, the variation in measured speed around the cutoffs should be as good as random. There are many institutional features suggesting that this holds (see Dusek and Traxler 2020). Most importantly, the way speed is measured makes it extremely difficult for drivers to exactly target a certain speed level: each camera pair measures the average speed inside a fixed zone of several hundred meters. In order to aim at a speed just below a cutoff, a driver would have to either maintain a constant speed over the entire zone or (assuming prefect knowledge about the technology) target a certain travel time with a precision of some hundred milliseconds. Both scenarios seem implausible.[33] In addition, it is not clear whether drivers are aware of these cutoffs: while the cutoff separating minor and intermediate speeding (20km/h above

[31] As the variation in both f and K differs across all three cutoffs, one cannot (without further assumptions) predict at which cutoff we should observe a stronger drop in timely compliance.

[32] We cannot directly estimate the discontinuity in K in the full sample, as $f + K$ is only observed for the small, selected sub-sample analyzed in Figure 4 (see fn. 30).

[33] The measurement technology constitutes a major difference to the set-up in Traxler *et al.* (2018). Further challenges that complicate speed targeting are associated with the facts that (a) speedometers display travel speed with a certain error (with non-zero mean) and that (b) the fine-relevant speed is an adjusted measure derived from the recorded speed (see fn. 10) according to a formula that may not be fully salient.

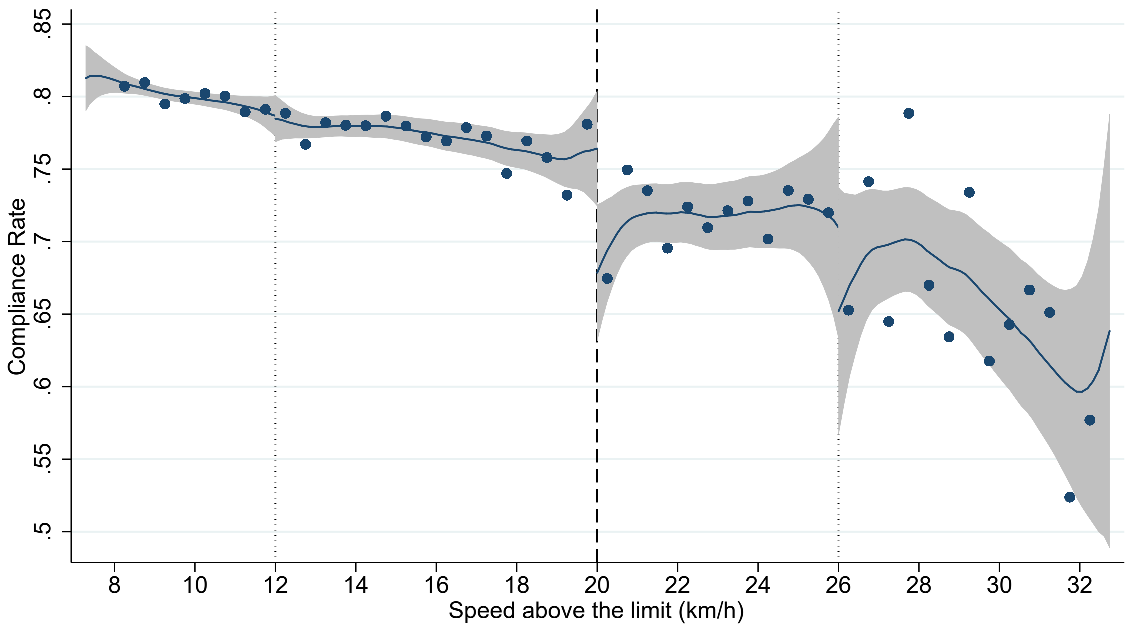
the speed limit) is stipulated in the law, the other two cutoffs were introduced ad-hoc by the local enforcement authorities and were never publicly communicated (before this paper). Hence, we doubt that ticketed drivers were strategically targeting speed levels below the cutoffs.

Consistently with this conjecture, we do not find any evidence of manipulation of the running variable. Figure A.2 in the Appendix reveals that the density of the running variable is smooth around the threshold: neither McCrary (2008)'s heaping test nor local-polynomial density estimators (Cattaneo *et al.*, 2020) indicate any bunching on the left of the three cutoffs. In addition, we do not detect any systematic discontinuities in observable characteristics around the cutoffs (see Table A.9). The evidence indicates that we can indeed exploit the speed variation around the cutoffs as local quasi-experiments.[34]

5.3 RD Results

Figure 5 displays local linear fits for the fraction of timely compliance and mean compliance rates for bins of 0.25km/h. The graph reveals a clear, discontinuous drop in timely compliance at the 20km/h cutoff. There also seems to be a drop in compliance at the 26km/h cutoff. The latter discontinuity, however, is less pronounced and the variance is higher. At the first cutoff (12km/h), the compliance rate evolves smoothly without any discontinuity. A further point worth noting is the negative trend depicted in the figure: independently of the discontinuities, drivers ticketed at higher speeds have a lower propensity to pay their fines before the deadline. More grave non-compliance in one dimension (violation of speed limit) is therefore correlated with more frequent non-compliance on a different dimension (failure to pay by the deadline).

Figure 5: RDD – Discontinuities in timely compliance



Notes: The figure presents the rate of timely compliance (the share of tickets paid within 15 days) for different levels of speed above the limit. The fitted lines indicate local linear estimates (for a bandwidth of 2.0km/h) together with 95%-confidence intervals. Each dot represents the average compliance rate in a 0.5km/h bin.

[34]Dusek and Traxler (2020) offer additional validity checks for the discontinuity at 20km/h. Among others, they show that drivers do not learn about this cutoff over time.

Table 6: RDD – Discontinuities in timely compliance

	(1)	(2)	(3)	(4)	(5)	(6)
	Reduced Form Estimates			*Wald Estimates*		
Cutoff:	12km/h	20km/h	26km/h	12km/h	20km/h	26km/h
Estimate	0.004	-0.055**	-0.061	0.017	-0.225***	-0.277
	(0.015)	(0.025)	(0.059)	(0.067)	(0.078)	(0.270)
Relative Effect	0.47%	-7.10%	-8.52%	–	–	–
Y(left)	0.791	0.781	0.720	0.791	0.781	0.720
Optimal Bandwith	1.583	4.717	2.930	1.563	1.628	3.003
Effect. Obs./Left	13,503	8,590	1,138	13,320	1,963	1,178
Effect. Obs./Right	9,060	2,864	656	8,955	1,320	668

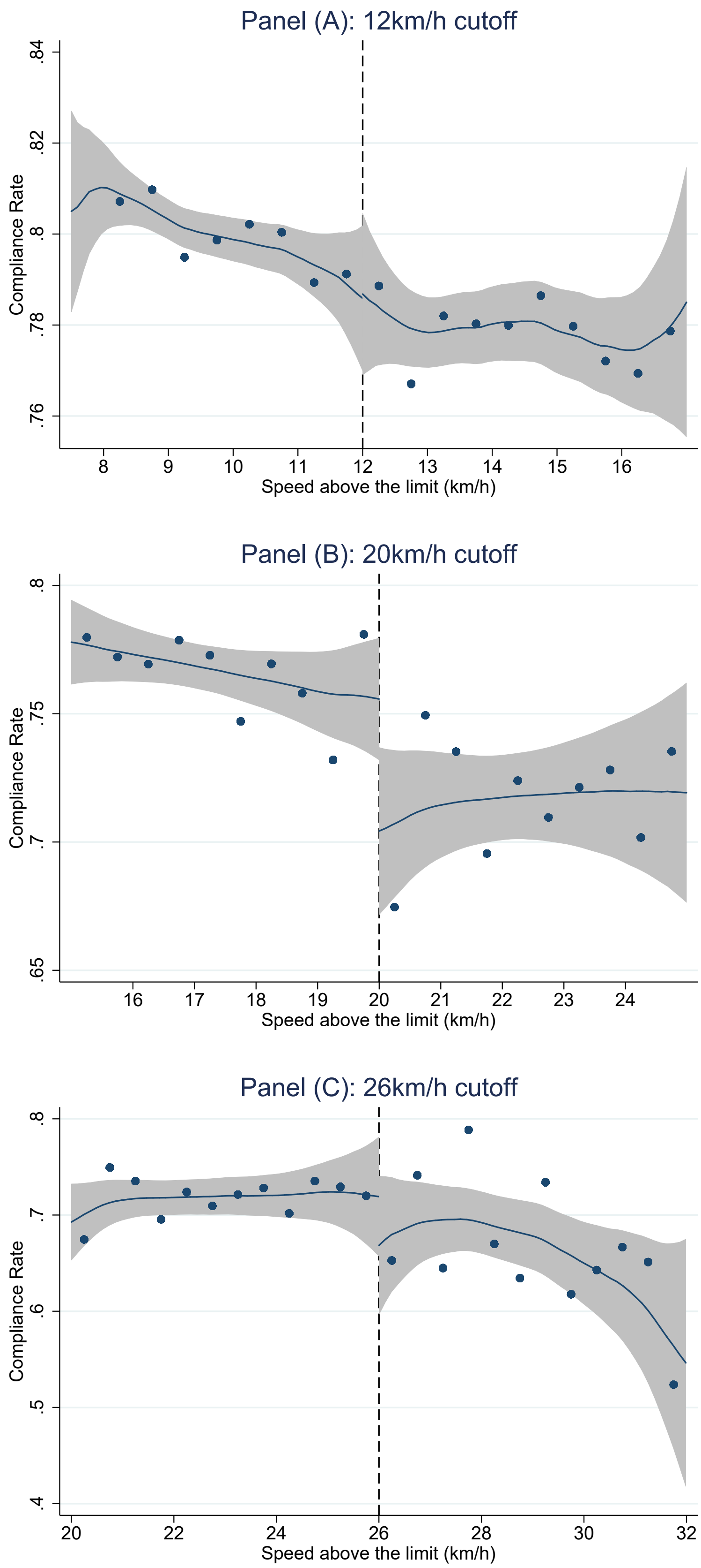
Notes: The table presents bias-corrected RD estimates (Calonico et al., 2014) with clustered standard errors. All specifications use MSE-optimal bandwidths estimated with a triangular kernel, local linear point estimators and local quadratic estimates for the bias correction. The dependent variable is an indicator for timely payment of the full fine. The table presents reduced form (specifications 1–3) and Wald estimates (4–6) for discontinuities at the 12, 20 and 26km/h cutoff, respectively. Specifications 1 – 3 further indicate the effect size relative to Y(left), the mean rate of timely compliance in the 0.5km/h bin below the cutoff. The overall number of observations is 78,882 and the effective number of observation (within the optimal bandwidth to the left and right of the cutoff) is indicated in the table. ***/** indicates significance at the 1%/5%-level, respectively.

The non-parametric fit in Figure 5 is based on a constant, ad-hoc bandwidth. A more refined approach is provided in Figure 6, which zooms into the three cutoffs and shows local linear estimates for MSE-optimal bandwidths. The corresponding reduced form estimates of equation (3) are reported in Columns (1)–(3) of Table 6. The analysis confirms the graphical evidence. While the compliance rate does not discontinuously change at the 12km/h cutoff, there is a statistically and economically significant 5.5pp drop in timely compliance at the 20km/h cutoff. Relative to the mean compliance rate in the 0.5km/h bin below this cutoff, this corresponds to a 7.1% decline (see Table 6, Column 2). A quantatively similar but less precisely estimated discontinuity is observed at the third cutoff: at 26km/h above the speed limit, the rate of timely payments drops by 6.1pp (8.5% relative to the rate below the cutoff). The latter discontinuity, however, is statistically insignificant, which also reflects the much smaller number of effective observations in this range of the speed distribution (see low panel of Table 6, Column 3).

The corresponding Wald estimates are reported in Columns (4)–(6) of Table 6.[35] While we obtain a null result at the first cutoff, the estimates yield very similar β-coefficients of –0.23 and –0.28 at the second (20km/h) and the third (26km/h) cutoff, respectively. The estimate at the main cutoff is statistically highly significant. The coefficient suggests that a 1% increase in the fine causes a 0.23pp drop in timely compliance. This semi-elasticity is very similar to estimates from the literature that studies the impact of high taxes or fees on (non-)compliance. Berger *et al.* (2016), for instance, estimate that a 1% increase in TV license fees raises the evasion rate by 0.3pp.

[35] The Wald estimates slightly differ from what would be obtained by simply dividing the reduced-form coefficients by the respective discontinuities from Table A.8. This is due to the fact that the MSE-optimal bandwidths for the Wald estimators (which jointly estimate γ^k and δ^k from (3) and (4)) differ from the optimal bandwidths of the reduced-form estimates.

Figure 6: RDD – Discontinuities in timely compliance by cutoff



Notes: For each of the three cutoffs, the figure illustrates the rate of timely compliance at different levels of speed above the limit. Fitted lines indicate local linear estimates with MSE-optimal bandwidths (Calonico *et al.*, 2014) together with 95%-confidence intervals. (Bandwidths are reported in Table 6.) Each dot represents the average compliance rate in a 0.5km/h bin.

Comparison of effect sizes. Let us now compare the effects of higher fines (obtained from the RDD) with the effects of increased salience (identified in the RCT analysis). The 5.5pp drop in timely compliance around the 20km/h cutoff is quantitatively much larger than the largest treatment effect observed in the RCT (2.06pp). The local variation in the fine around the main cutoff has an almost threefold impact on the rate of pre-deadline payments as compared to the most effective cover letter ($D\&P$). Expressed in terms of the estimated semi-elasticity, the $D\&P$ [P] treatment has an effect similar to an 8.7% [4.3%] reduction in fines. Hence, the variation in the fine f (with up to 70% changes) exerts a much stronger influence on timely compliance than the salience interventions from the RCT. The impact of the salience treatments is quantitatively meaningful nevertheless.

The simple comparison of effect sizes neglects the fact that the RCT estimates capture average treatment effects (ATEs) whereas the RDD yields local average effects (LATEs; see below). To address concerns regarding this discrepancy, we replicated our main RCT estimates for a sample of drivers with speeds around the 20km/h cutoff. The estimates, which are reported in Panel C of Table A.4, suggest that the ATEs from the RCT are quantitatively similar to the treatment effects observed in a sample of drivers around the main RDD cutoff.

Robustness. The RDD estimates reported above are confirmed in a series of robustness checks. The first sensitivity analysis explores alternative bandwidth selection algorithms. Figure A.3 plots the reduced-form estimates obtained for a broad range of bandwidths. The graphs show that the point estimates tend to be fairly insensitive to the bandwidth choice. An exception to this is the 20km/h cutoff, for which smaller bandwidths (in the range between 0.75 and 3.5km/h) would yield larger reduced form (and also Wald) effects.[36] The reduced form estimates reported for the MSE-optimal bandwidth might therefore represent a lower bound for the discontinuity in timely compliance at this cutoff.

A second sensitivity check concerns the changes in the discontinuities of fines over time. The results from above exploit data from the full sample period and thus reflect the *average* discontinuities. As the fine structure evolved over time (see Table 1), we observe a slightly different variation in fines for the time after 2018/7 (see Table A.8). In this period, new speed cameras were installed and generated most of the observations in our sample. The sample average is mainly shaped by this later period. Table A.7 provides our estimates for the post 2018/7 sample. The results (in particular, the Wald estimates) are quantitatively similar to our main estimates from Table 6.[37]

Variation between cutoffs. Our estimates point to similar semi-elasticities at the second and third cutoffs. At the first cutoff, however, we obtain a null result. These differences might reflect variation in local average treatment effects: the payment decisions of modest speeders (who were recorded with speeds around the first cutoff) may be less sensitive to variation in fines compared to more aggressive drivers with much higher measured speeds around the second or third cutoff. An alternative interpretation is that the effect is non-linear in the size of the discontinuities: the variation in the fine at the first cutoff (174 CZK) is far smaller than the jumps at the second and

[36] Given the variance around the cutoff illustrated in Figure 5, this observation is intuitive.

[37] We also considered the much smaller sample of the pre-2018/7 period . Consistently with the stronger discontinuity at the main cutoff that applied in this period (see Table 1) we find a stronger reduced form effect of –0.09pp at the 20km/h cutoff. The estimate implies a semi-elasticity of –0.13. Due to the limited number of relevant observations, however, the estimates are relatively imprecise (with z-statistics in the range of 1.5). Moreover, for higher speed ranges (where the sample is even smaller), the optimal bandwidth selection algorithms typically fail to yield a result.

third cutoffs (623 and 368 CZK, respectively). Our data do not enable us to discriminate between these two interpretations.[38]

Variation in late penalty. The Wald estimate for the first (12km/h) and third (26km/h) cutoffs are difficult to interpret, as these cutoffs are characterized by discontinuous variation both in the fine f and in the late penalty K (more specifically, the late fee). From above we know that the increase in fines and the decrease in late fees works towards reducing timely compliance. The results obtained at the first and third cutoffs might therefore overestimate the (local) semi-elasticity of timely compliance w.r.t. the fine. This concern is only relevant if decision makers anticipate the implicit discontinuities in K captured in the right graph of Figure 4. How salient are these discontinuities? To what extent do they shape payment behavior?

First, note that the reductions in late fees documented in Figure 4 were not public information (before this paper). Second, our survey evidence indicates that the late penalty K is not fully salient, suggesting that the discontinuities in K might not be salient either. If most individuals were unaware of the variation in K, then the Wald estimates at the first and the third cutoff would be valid measures for the impact of fines (which are clearly more salient) on compliance.

An indirect way of assessing the role of K builds upon the variation introduced in the RCT: the P and $D\&P$ treatments explicitly stressed the total amount an individual might have to pay if she missed the deadline. In fact, the communicated amount changed discontinuously at the 20km/h cutoff (from 'as high as 2,500 CZK' to 'as high as 5,000 CZK'; see Appendix A.4.1). Hence, in line with the legal regulations discussed in Section 2 (but in contrast to what is observed in Figure 4), the treatments signaled an *increase* in K at the cutoff. A higher late fee, in turn, should work towards increasing pre-deadline compliance (see Section 3 and Appendix A.2). If individuals are responsive to this information about K – which is only communicated in the P and $D\&P$ treatments – then we should observe a *smaller* drop in timely compliance among those confronted with these two treatments than among those in the control group and the D treatment.

To test this hypothesis, we estimate reduced form effects at the relevant cutoff, separately for each experimental group. The results, which are presented in Table 7, do not support the prediction from above. Compared to the control group, the point estimates even indicate *larger* discontinuities (in absolute terms) in the P and the $D\&P$ treatments. The same holds if we pool the these two treatments and compare them with the C- and P-groups. In both cases, the 95%-confidence intervals of the coefficients overlap. Hence, this exercise does not reveal a statistically significant impact from increasing the salience in the discontinuity of K on the estimated discontinuities in timely compliance. Individuals' pre-deadline payments appear to be primarily influenced by the fine f and not by K.

[38]Exploiting inter-temporal variation in fines at the different cutoffs (see Table 1), we do not obtain any clear evidence on non-linearities. In fact, the Wald estimate at the main cutoff seems to be relatively robust (see Table A.7). Note further that the analysis below plausibly rules out variation in (the salience of) K as a third possible explanation.

Table 7: RDD – Discontinuities at the 20km/h cutoff by treatment

Treatment(s):	(1) *C*	(2) *D*	(3) *P*	(4) *D&P*	(5) *C*, *D*	(6) *P*, *D&P*
Estimate	–0.018	–0.069	–0.055	–0.096*	–0.044	–0.069**
	(0.054)	(0.046)	(0.053)	(0.053)	(0.036)	(0.034)
Relative Effect	–2.45%	–8.36%	–7.18%	–12.19%	–5.32%	–8.94%
Y(left)	0.750	0.822	0.761	0.789	0.822	0.777
Optimal Bandwith	3.894	5.020	4.406	3.664	4.218	5.140
Observations (total)	20,399	19,025	19,012	20,446	39,424	39,458
Effect. Obs./Left	1,636	2,364	1,896	1,488	3,591	4,902
Effect. Obs./Right	702	716	684	564	1,383	1,466

Notes: The table presents bias-corrected RD estimates (Calonico et al., 2014) with clustered standard errors. All specifications use MSE-optimal bandwidths estimated with a triangular kernel, local linear point estimators and local quadratic estimates for the bias correction. The dependent variable is an indicator for timely payment of the full fine. Reduced form estimates for the discontinuity in compliance at the main cutoff (20km/h above the speed limit) are presented, separately for the different experimental groups (the control group C, and the D, P and $D\&P$ treatments) and for the C and D treatments (column 5) and the P and $D\&P$ treatments (column 6) pooled together, respectively. The table further indicates the effect size relative to Y(left), the mean rate of timely compliance in the 0.5km/h bin below the cutoff, the overall number of observations and the effective number of observations (within the optimal bandwidth to the left and right of the cutoff). **/* indicate significance at the 5%-/10%-level, respectively.

6 Conclusion

In this paper we have studied the enforcement of fines from speeding tickets. Our study combined three complementary research designs. First, an RCT evaluates the impact of simplifying cover letters that either increase the salience of a payment deadline, the penalties for late payments, or both. Emphasizing only the 15-day deadline does not yield a significant increase in payment rates. Stressing the late penalties increases timely compliance by about 1.2%. Jointly communicating the deadline and the late penalties raises timely payments by 2.6%. The latter treatment's impact is persistent over 100 days. Second, evidence from a survey experiment indicates both over- and underestimations of the true deadline. Making the deadline salient has opposing effects on priors and, in turn, fails to increase timely compliance. In contrast, interventions that make late penalties salient raise the perceived costs of non-compliance.

Our third research design, which exploits discontinuous variations in the fine, documents the influence of traditional economic incentives on compliance behavior. The central estimate from the RDD indicates that a 1% higher fine induces a 0.23pp drop in timely compliance. This semi-elasticity suggests that the effect sizes from late penalty nudges are equivalent to the impact of a 4–9% drop in the fine. The impact of these salience nudges is thus quantitatively meaningful. In fact, the most successful intervention yields a net revenue gain that equals approximately 125% of the monthly labor costs of an administrator handling speeding tickets. In light of these results, the local enforcement authority plans to apply this cover letter permanently.

The treatment effects from the RCT as well as the semi-elasticity identified in the RDD are quantitatively very similar to comparable estimates from the tax enforcement literature. It is nevertheless hard to assess the external validity of our findings for other enforcement problems. However, given that the collection of fines is non-trivial from a revenue perspective and yet understudied, our results seem relevant on their own.

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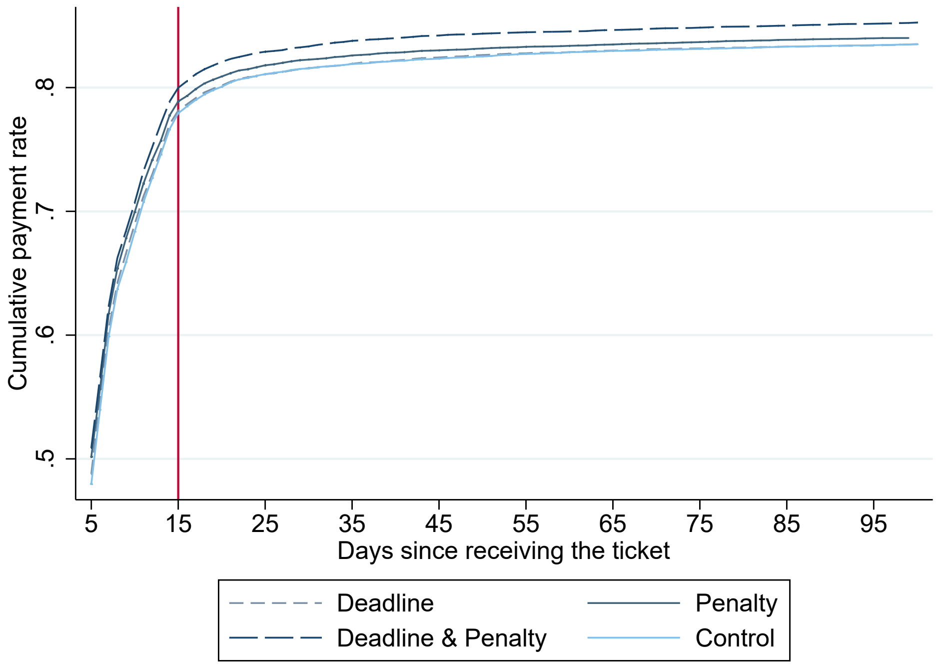
A Appendix

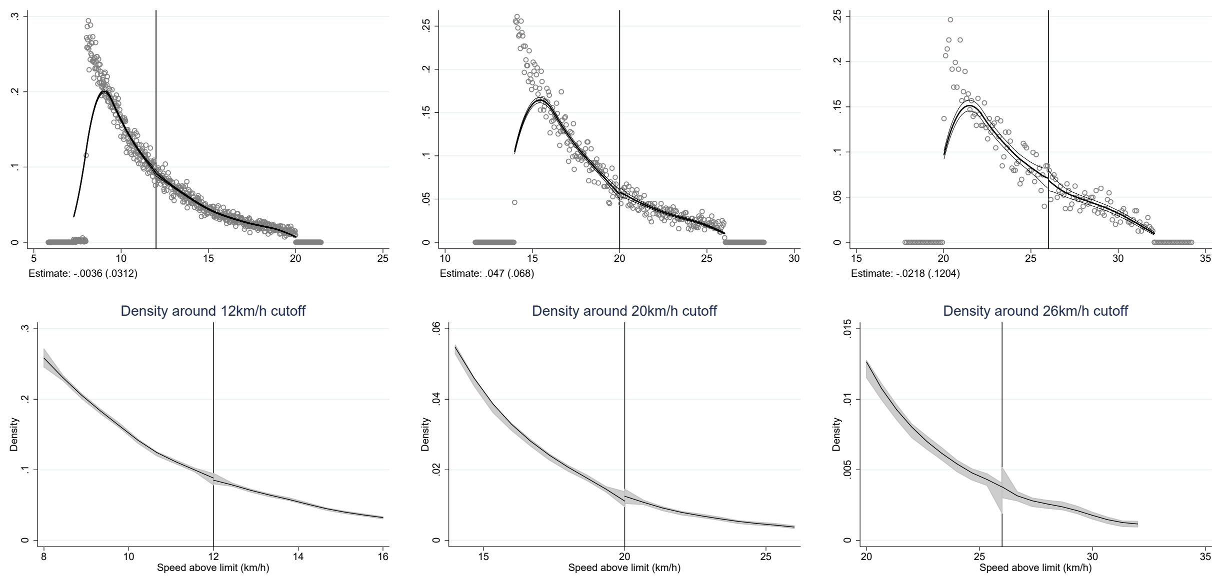
A.1 Additional Figures and Tables

Figure A.1: RCT – Cumulative response rates by treatment



Notes: The figure illustrates cumulative payment rates for the different experimental groups during the first 100 days after receiving the notification. The payment deadline (15 days) is indicated by the red vertical line.

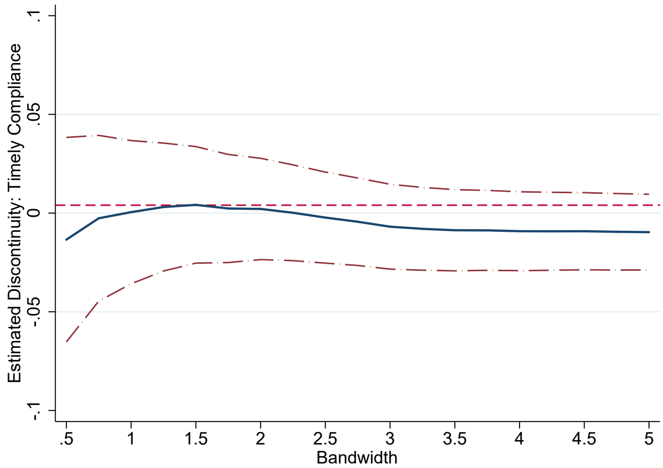
Figure A.2: Density of Speed Distribution around Cutoffs



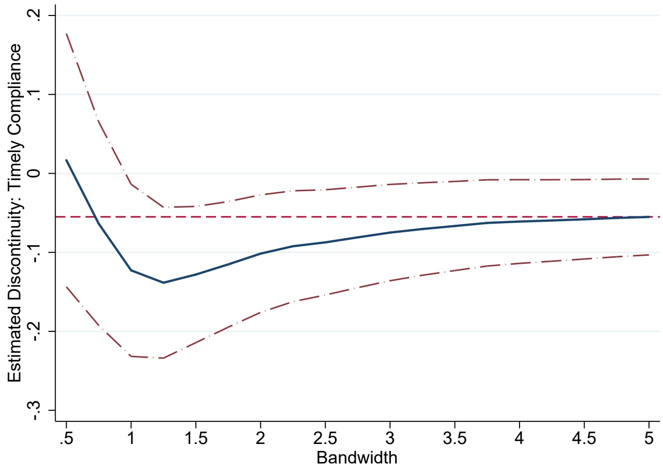
Notes: The figures illustrate the distribution of the measured speed around the 12km/h, 20km/h and 26km/h speed cutoffs. The three figures in the first row include estimates from the McCrary (2008) heaping test. The three figures in the second row are based on the local-polynomial density estimator from Cattaneo *et al.* (2020), with the p-values for the corresponding manipulation tests being $p = 0.859$, $p = 0.587$, and $p = 0.152$, respectively.

Figure A.3: RDD – Reduced form estimates for alternative bandwidths

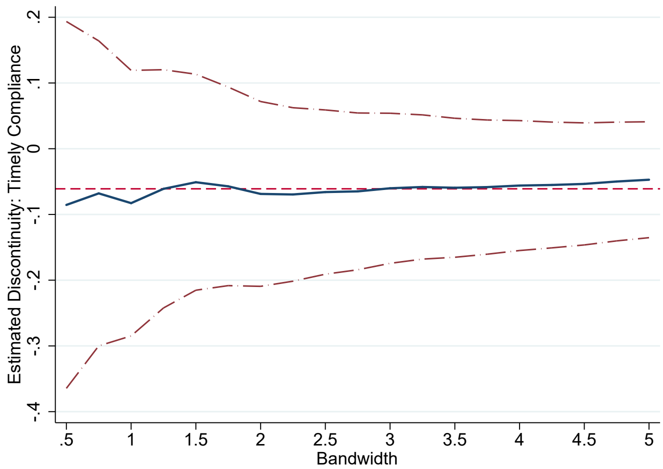
(a) Cutoff: 12km/h



(b) Cutoff: 20km/h



(c) Cutoff: 26km/h



Notes: For each of the three cutoffs, the figure plots reduced-form estimates and 95%-confidence intervals for local linear RD estimates (with a triangular kernel and local quadratic estimates for the bias correction; Calonico et al., 2014) with clustered standard errors. Within each panel, we exogenously vary the bandwidth from 0.5 to 5.0km/h (in steps of 0.25km/h). The dependent variable is an indicator for timely payment of the full fine. The dashed horizontal lines indicate the reduced form estimates obtained for the MSE-optimal bandwidth (reported in Table 5).

Table A.1: Survey – Descriptive statistics

	Control	Deadline	Penalty	Deadline & Penalty	Total
Age	43.51	43.52	43.74	43.86	43.66
	(15.03)	(13.99)	(13.80)	(14.29)	(14.28)
Male	0.510	0.523	0.481	0.500	0.503
	(0.501)	(0.500)	(0.500)	(0.501)	(0.500)
Apprenticeship education	0.381	0.450	0.406	0.405	0.410
	(0.486)	(0.498)	(0.492)	(0.491)	(0.492)
Elementary school education	0.0842	0.0628	0.0529	0.0756	0.0690
	(0.278)	(0.243)	(0.224)	(0.265)	(0.254)
High school education	0.324	0.339	0.355	0.329	0.337
	(0.469)	(0.474)	(0.479)	(0.471)	(0.473)
University or higher education	0.210	0.148	0.186	0.190	0.184
	(0.408)	(0.356)	(0.390)	(0.393)	(0.388)
city of 100,000 + inhabitants	0.223	0.206	0.219	0.227	0.219
	(0.417)	(0.405)	(0.414)	(0.419)	(0.414)

Notes: Sample size is *N=1,609*. Standard errors in parentheses.

Table A.2: Survey – Treatment effects on expectations of addition of demerit points

	(1)	(2)	(3)	(4)	(5)	(6)
Responses:	Yes		No		Maybe	
Deadline	0.015	0.018	-0.072**	-0.076**	0.057**	0.058**
	(0.033)	(0.033)	(0.035)	(0.035)	(0.026)	(0.027)
Penalty	0.308***	0.309***	-0.300***	-0.301***	-0.008	-0.008
	(0.034)	(0.034)	(0.033)	(0.033)	(0.024)	(0.024)
Deadline & penalty	0.310***	0.311***	-0.298***	-0.299***	-0.012	-0.012
	(0.033)	(0.033)	(0.033)	(0.033)	(0.024)	(0.024)
Constant	0.324***	0.287***	0.535***	0.567***	0.141***	0.146***
	(0.023)	(0.045)	(0.025)	(0.046)	(0.017)	(0.033)
F-Tests (*p*-values):						
$D = P$	0.000	0.000	0.000	0.000	0.014	0.013
$D = D\&P$	0.000	0.000	0.000	0.000	0.008	0.007
$P = D\&P$	0.955	0.964	0.938	0.935	0.859	0.868
Controls	-	Yes	-	Yes	-	Yes

Notes: The table presents LPM estimates following the structure of equation (1). $N = 1{,}609$. The dependent variable in Columns (1) – (2) is a dummy indicating that an individual responded *yes* to the question *'If you do not pay the full amount of the fine by the deadline, would you expect to be added demerit points within the demerit point system?'*. The dependent variable in Columns (3) – (4) captures responses indicating *no*. Columns (5) – (6) indicate responses of *maybe*. Control variables include age, gender, and education dummies. Robust standard errors in parentheses. ***/**/* indicate significance at the 1%/5%/10%-level, respectively.

Table A.3: RCT – Treatment Effects with Multiple Hypothesis Testing

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Pay-7days		**Pay-15days**		Pay-30days		Pay-100days	
Deadline (β_1)	0.58	0.63	0.22	0.27	0.02	0.12	0.00	0.12
	(0.238)	(0.188)	(0.593)	(0.494)	(0.951)	(0.747)	(0.996)	(0.732)
	[0.453]	[0.144]	[0.788]	[0.505]	[0.993]	[0.758]	[0.998]	[0.758]
	{0.465}		{0.801}		{0.994}		{0.995}	
Penalty (β_2)	1.85	1.87	0.96	0.93	0.71	0.73	0.50	0.54
	(0.000)	(0.000)	(0.021)	(0.019)	(0.068)	(0.050)	(0.180)	(0.125)
	[0.000]	[0.000]	[0.030]	[0.004]	[0.081]	[0.011]	[0.174]	[0.037]
	{0.000}		{0.116}		{0.284}		{0.581}	
Deadline & Penalty	2.46	2.38	2.06	2.02	1.78	1.78	1.75	1.59
(β_3)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]
	{0.000}		{0.000}		{0.000}		{0.000}	
Constant (β_0)	59.88	—	77.93	—	81.55	—	83.50	—
Comparison of treatment effects (p-values, using correction from List *et al.* 2019):								
$\beta_1 = \beta_2$	{0.061}		{0.332}		{0.354}		{0.581}	
$\beta_1 = \beta_3$	{0.000}		{0.000}		{0.000}		{0.000}	
$\beta_2 = \beta_3$	{0.521}		{0.034}		{0.028}		{0.002}	
Controls & FEs	N	Y	N	Y	N	Y	N	Y

Notes: The table re-prints the LPM point-estimates from Tab. 5. Dependent variable is an indicator for a payment of the full amount within $\tau = \{7, 15, 30, 100\}$ days. Specifications 2, 4, 6 and 8 contain control variables. The number of observations for all specifications is 78,882. The estimated coefficients (β_0–β_3) are multiplied by 100 and can be interpreted as percentage point values. In addition to the estimated treatment effects, the table presents three different sets of p-values:

(1) The (round brackets) contain the basic p-values based on robust standard errors, clustered at the vehicle level but without accounting for multiple testing.

(2) In [squared brackets], we report p-values for the Romano-Wolf multiple hypothesis correction (Romano and Wolf, 2005, 2016), implemented with the `rwolf` package from Clarke *et al.* (2019) (5000 bootstraps). Note that this step-down procedure (which controls the familywise error rate but – different to and thus more powerful than, e.g., Bonferroni or Holm corrections – considers the dependence structure of the test statistics by re-sampling from the original data) allows to account for clustering.

(3) Finally, the {curly brackets} contain the p-values from the multiple testing correction proposed by List *et al.* (2019) (i.e., results obtained from the `mhtexp` package, again with 5000 bootstraps). The later package allows to include tests for pairwise treatment comparisons and the corresponding p-values are reported in the lower panel of the table. Note, however, that the package does neither account for clustered standard errors nor does it allow to add controls. Hence, these corrections are only available for specifications 1, 3, 5 and 7.

Table A.4: RCT – Robustness and extensions (LPM estimates)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Controls & FEs	N	Y	N	Y	N	Y	N	Y
Panel A. Any Payment								
	AnyPay-7days		**AnyPay-15days**		AnyPay-30days		AnyPay-100days	
Deadline (β_1)	0.0062	0.0067	0.0025	0.0029	-0.0003	0.0006	-0.0005	0.0007
	(0.0049)	(0.0048)	(0.0041)	(0.0039)	(0.0038)	(0.0036)	(0.0036)	(0.0033)
Penalty (β_2)	0.0186***	0.0187***	0.0095**	0.0091**	0.0062	0.0062*	0.0039	0.0042
	(0.0049)	(0.0048)	(0.0041)	(0.0040)	(0.0038)	(0.0036)	(0.0036)	(0.0034)
Deadline & Penalty	0.0245***	0.0238***	0.0201***	0.0199***	0.0161***	0.0163***	0.0152***	0.0132***
(β_3)	(0.0049)	(0.0050)	(0.0042)	(0.0041)	(0.0038)	(0.0037)	(0.0037)	(0.0035)
Constant (β_0)	0.5993***	—	0.7802***	—	0.8249***	—	0.8473***	—
	(0.0039)		(0.0037)		(0.0035)		(0.0034)	
Number of Obs.	78,882	78,882	78,882	78,882	78,882	78,882	78,882	78,882
Panel B. Full Payment; One (first) Observation per Car								
	Pay-7days		**Pay-15days**		Pay-30days		Pay-100days	
Deadline (β_1)	0.0028	0.0037	-0.0015	-0.0011	-0.0040	-0.0031	-0.0031	-0.0019
	(0.0055)	(0.0053)	(0.0045)	(0.0042)	(0.0041)	(0.0039)	(0.0039)	(0.0036)
Penalty (β_2)	0.0174***	0.0182***	0.0079*	0.0078*	0.0045	0.0048	0.0031	0.0037
	(0.0054)	(0.0053)	(0.0044)	(0.0042)	(0.0041)	(0.0038)	(0.0039)	(0.0036)
Deadline & Penalty	0.0244***	0.0227***	0.0213***	0.0208***	0.0180***	0.0177***	0.0178***	0.0157***
(β_3)	(0.0053)	(0.0056)	(0.0043)	(0.0043)	(0.0039)	(0.0040)	(0.0037)	(0.0037)
Constant (β_0)	0.6246***	—	0.8065***	—	0.8432***	—	0.8628***	—
	(0.0038)		(0.0031)		(0.0029)		(0.0027)	
Number of Obs.	62,782	62,782	62,782	62,782	62,782	62,782	62,782	62,782
Panel C. Full Payment within 15 days; Sample with varying Speed Range								
	20 ± 3km/h		20 ± 4km/h		20 ± 5km/h		20 ± 6km/h	
Deadline (β_1)	0.0057	0.0107	0.0164	0.0185	0.0067	0.0060	0.0124	0.0133
	(0.0153)	(0.0149)	(0.0127)	(0.0123)	(0.0109)	(0.0105)	(0.0094)	(0.0091)
Penalty (β_2)	0.0079	0.0116	0.0196	0.0214*	0.0110	0.0123	0.0154	0.0160*
	(0.0155)	(0.0151)	(0.0128)	(0.0123)	(0.0108)	(0.0104)	(0.0094)	(0.0090)
Deadline & Penalty	0.0104	0.0118	0.0200	0.0204	0.0191*	0.0163	0.0242**	0.0232**
(β_3)	(0.0152)	(0.0155)	(0.0129)	(0.0131)	(0.0112)	(0.0113)	(0.0095)	(0.0096)
Constant (β_0)	0.7393***	—	0.7375***	—	0.7471***	—	0.7486***	—
	(0.0120)		(0.0101)		(0.0086)		(0.0076)	
Observations	6,425	6,425	9,188	9,188	12,462	12,462	16,459	16,459

Notes: The table presents LPM estimates of equations following the structure of (2). The dependent variable in *Panel A* is an indicator for *any payment* (including incomplete payments below the prescribed fine) within $\tau = \{7, 15, 30, 100\}$ days. *Panel B* considers again the main outcome variable (full payment) but only includes one observation per car (i.e. the first time a car is treated). Hence, the number of observations reduces to 62,782 cars (= tickets). The dependent variable in *Panel C* is full payment within 15 days (Pay-15days). The specifications vary the estimation sample around the high-fine cutoff (20km/h above the speed limit), including speeding tickets with a speed S_i in the range $20 - \sigma \leq S_i < 20 + \sigma$ km/h above the speed limit and $\sigma \in \{3, 4, 5, 6\}$. For all three panels, specifications 2, 4, 6 and 8 contain dummies for the week the speeding ticket was sent and a vector of control variables (compare Table 5). Standard errors are clustered at the vehicle level in Panels A and C. Panel B reports robust standard errors. ***/**/* indicates significance at the 1%/5%/10%-level, respectively.

Table A.5: RCT – Duration analysis (time until payment)

	(1)	(2)	(3a)	(4a)	(3b)	(4b)	(3c)	(4c)	(3d)	(4d)
Panel A. Cox estimations										
	time-invariant		0–7 Days		8–15 Days		16–30 Days		31+ Days	
Deadline	1.011	1.016	1.024*	1.029**	0.997	1.003	0.923*	0.930	1.034	1.044
	(0.010)	(0.010)	(0.013)	(0.013)	(0.020)	(0.020)	(0.042)	(0.043)	(0.090)	(0.090)
Penalty	1.038***	1.042***	1.055***	1.058***	1.023	1.027	0.957	0.959	0.908	0.910
	(0.011)	(0.011)	(0.013)	(0.014)	(0.021)	(0.021)	(0.044)	(0.044)	(0.083)	(0.082)
Deadline & Penalty	1.071***	1.064***	1.083***	1.071***	1.059***	1.057***	0.995	1.008	1.091	1.113
	(0.011)	(0.012)	(0.014)	(0.014)	(0.022)	(0.021)	(0.046)	(0.046)	(0.097)	(0.098)
Controls	N	Y	N	Y	N	Y	N	Y	N	Y
Panel B. Cloglog estimations										
	time-invariant		0–7 Days		8–15 Days		16–30 Days		31+ Days	
Deadline	1.001	1.009	1.025*	1.030**	0.996	1.004	0.918*	0.926	1.035	1.046
	(0.012)	(0.013)	(0.014)	(0.014)	(0.022)	(0.022)	(0.045)	(0.045)	(0.091)	(0.092)
Penalty	1.018	1.022	1.057***	1.061***	1.025	1.030	0.954	0.957	0.908	0.910
	(0.013)	(0.013)	(0.014)	(0.014)	(0.023)	(0.023)	(0.046)	(0.047)	(0.083)	(0.083)
Deadline & Penalty	1.060***	1.066***	1.086***	1.073***	1.064***	1.063***	0.995	1.010	1.091	1.114
	(0.014)	(0.015)	(0.015)	(0.015)	(0.024)	(0.023)	(0.049)	(0.049)	(0.098)	(0.099)
Controls	N	Y	N	Y	N	Y	N	Y	N	Y

Notes: The table reports estimated hazard ratios (together with standard errors, clustered at the car level) based on the Cox proportional hazard model (Panel A) and complementary log-log regressions ('Cloglog', Panel B; see Sueyoshi, 1995), respectively. Specifications (1) and (2) estimate time-invariant treatment effects on the timing of payments. (For the Cox model, this is based on an equation with the structure $h(t) = h_0(t)exp(\beta_0+\beta_1\text{Deadl}+\beta_2\text{Penalty}+\beta_3\text{Deadl\& Penalty}+\mathbf{X}\gamma')+\varepsilon)$. Specifications (3) and (4), in contrast, present estimates for time-varying treatment effects on the hazard rate during days 0–7 , 8–15, 16–30, and more than 30 days after receiving the notification. Hence, the different columns (3a–3d) [and, analogously, for (4a–4d)] present the coefficients from one given estimation. (The estimation equation is $h(t) = h_0(t)exp(\beta_0 + \beta_1^1\text{Deadl}^{D0-7} + \beta_1^2\text{Deadl}^{D8-15} + \beta_1^3\text{Deadl}^{D16-30} + \cdots + \beta_2^1\text{Penalty}^{D0-7} + \cdots + \beta_3^1\text{Deadl\& Penalty}^{D0-7} + \cdots + \mathbf{X}\gamma') + \varepsilon$.) Columns (3a) and (3b), for instance, report treatment-specific hazard ratios for the first and second week after receiving the ticket, respectively. Analogously to the LPM estimates, specifications 2 and 4 contain dummies for the different speed camera zones and the week the speeding ticket was sent, and the full set of control variables (see Table 5). The number of observations for all specifications is 78,882 speeding tickets with the timing of the full payment observed over 60 days. $^{***}/^{**}/^{*}$ indicate significance at the 1%/5%/10%-level, respectively.

Table A.6: RCT – Heterogeneity analysis (LPM estimates)

	(1)	(2)	(3)	(4)	(5)	(6)
Interaction with:	company car		e-mail delivery		local number plate	
Deadline	0.0001	0.0022	0.0015	0.0023	-0.0027	-0.0021
	(0.0055)	(0.0052)	(0.0052)	(0.0049)	(0.0057)	(0.0054)
Penalty	0.0099*	0.0090*	0.0075	0.0073	0.0057	0.0070
	(0.0055)	(0.0052)	(0.0052)	(0.0049)	(0.0057)	(0.0055)
Deadline & Penalty	0.0291***	0.0311***	0.0253***	0.0280***	0.0182***	0.0173***
	(0.0056)	(0.0053)	(0.0053)	(0.0051)	(0.0057)	(0.0055)
Deadline × Interaction	0.0048	0.0011	0.0022	0.0009	0.0102	0.0099
	(0.0083)	(0.0080)	(0.0085)	(0.0082)	(0.0082)	(0.0079)
Penalty × Interaction	-0.0002	0.0006	0.0058	0.0049	0.0082	0.0050
	(0.0084)	(0.0080)	(0.0086)	(0.0083)	(0.0083)	(0.0080)
Deadline & Penalty	-0.0178**	-0.0234***	-0.0105	-0.0190**	0.0047	0.0062
× Interaction	(0.0084)	(0.0080)	(0.0086)	(0.0082)	(0.0084)	(0.0079)
Interaction Term	-0.0161**	0.0120	-0.0259***	-0.0381***	-0.0207***	-0.0149**
	(0.0075)	(0.0092)	(0.0079)	(0.0099)	(0.0074)	(0.0073)
Constant	0.7865***	—	0.7894***	—	0.7892***	—
	(0.0044)		(0.0041)		(0.0044)	
Controls & FEs	no	yes	no	yes	no	yes
Observations	78,882	78,882	78,882	78,882	78,882	78,882

Notes: The table presents LPM estimates that build upon the structure of equation (2), but interact each treatment dummy with a binary interaction term: an indicator for (i) cars owned by corporations (incl. single-person businesses organized as limited liability partnerships; columns 1–2), for (ii) speeding tickets sent via electronic mail (*data box*, see fn. 12; columns 3–4), and for (iii) cars with a 'local' number plate (from Central Bohemia; columns 5–6). Standard errors, clustered at the car level, are in parentheses. ***/**/* indicate significance at the 1%/5%/10%-level, respectively.

Table A.7: RDD – Discontinuities in timely compliance in post-2018/7 sample

	(1)	(2)	(3)	(4)	(5)	(6)
	Reduced Form Estimates			*Wald Estimates*		
Cutoff:	12km/h	20km/h	26km/h	12km/h	20km/h	26km/h
Estimate	-0.005	-0.051*	-0.031	-0.021	-0.206**	-0.134
	(0.014)	(0.027)	(0.060)	(0.059)	(0.091)	(0.260)
Relative Effect	-0.626	-6.572	-4.348			
Y(left)	0.788	0.770	0.704	0.788	0.770	0.704
Optimal Bandwith	1.910	4.742	3.200	1.805	1.633	3.228
Effect. Obs./Left	15,063	7,718	1,179	14,022	1,764	1,193
Effect. Obs./Right	9,340	2,602	655	8,904	1,189	658

Notes: The table – which replicates Tab. 6 for the post 2018/7 period – presents bias-corrected RD estimates (Calonico et al., 2014) with clustered standard errors. All specifications use MSE-optimal bandwidths estimated with a triangular kernel, local linear point estimators and local quadratic estimates for the bias correction. The dependent variable is an indicator for timely payment of the full fine. The table presents reduced form (specifications 1–3) and Wald estimates (4–6) for discontinuities at the 12, 20 and 26km/h cutoffs, respectively. For specifications (1) – (3) the table further indicates the effect size relative to Y(left), the mean rate of timely compliance in the 0.5km/h bin below the cutoff. The sample is constrained to the post 2018/7 period. The overall number of observations is 69,514 and the effective number of observations (within the optimal bandwidth to the left and right of the cutoff) is indicated in the table. **/* indicate significance at the 5%/10%-level, respectively.

Table A.8: RDD – Discontinuity in fines

Cutoff:	**12 km/h**		**20 km/h**		**26 km/h**	
Sample:	*full*	*2018/7+*	*full*	*2018/7+*	*full*	*2018/7+*
	(1)	(2)	(3)	(4)	(5)	(6)
fine (absolute val.)	173.829***	196.438***	623.138***	572.193***	368.147***	390.851***
	(2.134)	(1.479)	(11.899)	(9.820)	(24.726)	(23.933)
Relative Change	24.13	28.21	69.71	64.08	24.81	26.76
Y(left)	720.4	696.4	893.9	892.9	1484	1461
Optimal Bandwith	1.123	1.456	1.729	1.571	1.573	1.569
Observations	78,882	69,514	78,882	69,514	78,882	69,514
Effect. Obs./Left	9,066	10,817	2,118	1,673	557	512
Effect. Obs./Right	6,712	7,449	1,388	1,152	390	365
	(7)	(8)	(9)	(10)	(11)	(12)
log(fine)	0.221***	0.249***	0.523***	0.493***	0.226***	0.240***
	(0.003)	(0.002)	(0.009)	(0.008)	(0.017)	(0.017)
Optimal Bandwith	1.104	1.470	1.675	1.596	1.432	1.444
Observations	78,882	69,514	78,882	69,514	78,882	69,514
Effect. Obs./Left	8,859	10,944	2,042	1,701	502	478
Effect. Obs./Right	6,622	7,511	1,356	1,165	352	330

Notes: The table presents bias-corrected RD estimates (Calonico et al., 2014) with clustered standard errors. All specifications use MSE-optimal bandwidths estimated with a triangular kernel, local linear point estimators and local quadratic estimates for the bias correction. In specifications (1) – (6), the dependent variable is the fine (absolute CZK-values); in specifications (7) – (12) it is log(fine). Every second specification considers the post 2018/7 sample, i.e. when the fine scheme was adjusted (see Table 1). The top panel further indicates the change in fines relative to Y(left), the mean fine in the 0.5km/h bin below the cutoff. In addition, the table indicates the overall and the effective number of observations (within the optimal bandwidth to the left and right of the cutoff). *** indicate significance at the 1%-level.

Table A.9: RDD – Balance of observable characteristics around the cutoffs

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	Company Car	Number Plate: Prague	Centr.Bohem.	Tuesday	Wednesday	Thursday	Friday	Saturday	Sunday	Holiday
Panel A1. 12km/h cutoff										
Estimate	-0.016	-0.032*	0.006	0.007	-0.013	-0.016	0.015	0.008	0.003	-0.014*
	(0.022)	(0.017)	(0.015)	(0.011)	(0.013)	(0.011)	(0.013)	(0.013)	(0.014)	(0.007)
Y(left)	0.465	0.485	0.274	0.112	0.133	0.133	0.128	0.202	0.181	0.0584
Opt. Bandw.	1.019	1.706	1.884	1.506	1.296	1.802	1.292	1.988	1.430	1.576
Panel B1. 20km/h cutoff										
Estimate	0.004	0.030	0.004	-0.004	0.027	-0.000	-0.028	0.010	0.026	-0.002
	(0.025)	(0.030)	(0.027)	(0.016)	(0.020)	(0.020)	(0.020)	(0.024)	(0.023)	(0.012)
Y(left)	0.484	0.499	0.261	0.122	0.105	0.137	0.124	0.219	0.190	0.059
Opt. Bandw.	5.462	4.054	3.635	4.934	3.190	3.577	4.037	4.154	4.477	4.687
Panel C1. 26km/h cutoff										
Estimate	-0.007	0.001	-0.021	0.004	0.044	-0.022	-0.022	-0.046	0.026	0.000
	(0.053)	(0.057)	(0.053)	(0.029)	(0.033)	(0.038)	(0.035)	(0.042)	(0.042)	(0.023)
Y(left)	0.433	0.500	0.207	0.127	0.0867	0.107	0.187	0.213	0.207	0.0533
Opt. Bandw.	4.412	3.499	2.730	4.335	4.293	3.258	5.355	3.812	4.442	4.694

	(11)	(12)	(13)	(14)	(15)	(16)	(17)	(18)	(19)	(20)
	Offense–Sent (days)	Direction outbound	E-Mail	12-6am	6-9am	9am-12pm	12-3pm	3-6pm	6-9pm	9pm-12am
Panel A2. 12km/h cutoff										
Estimate	0.737**	-0.020	-0.016	-0.005	0.023*	0.003	-0.007	-0.001	-0.001	-0.009
	(0.372)	(0.016)	(0.021)	(0.008)	(0.014)	(0.014)	(0.015)	(0.015)	(0.012)	(0.008)
Y(left)	7.627	0.620	0.411	0.0440	0.116	0.246	0.249	0.168	0.120	0.0581
Opt. Bandw.	1.344	1.322	1.105	1.459	1.026	1.853	1.648	1.152	1.525	1.478
Panel B2. 20km/h cutoff										
Estimate	0.732	-0.004	0.001	0.027	-0.011	0.030	0.022	-0.012	-0.024	-0.030*
	(0.709)	(0.025)	(0.028)	(0.019)	(0.020)	(0.023)	(0.024)	(0.018)	(0.018)	(0.018)
Y(left)	7.840	0.592	0.427	0.095	0.112	0.196	0.234	0.135	0.137	0.090
Opt. Bandw.	3.374	4.781	4.632	3.623	3.226	4.347	3.995	5.049	4.842	3.248
Panel C2. 26km/h cutoff										
Estimate	0.122	0.017	-0.007	0.006	0.037	-0.003	-0.051	-0.002	-0.014	0.021
	(1.316)	(0.063)	(0.058)	(0.037)	(0.033)	(0.032)	(0.045)	(0.032)	(0.043)	(0.035)
Y(left)	9.653	0.580	0.367	0.153	0.0933	0.153	0.207	0.107	0.173	0.113
Opt. Bandw.	4.691	2.627	3.695	4.094	3.821	5.219	3.611	5.011	3.431	4.448

Notes: The table presents bias-corrected RD estimates (Calonico et al., 2014) with clustered standard errors. Considering two sets of outcome variables, the different panels present estimated discontinuities at the speed-level cutoffs 12 (Panel A1 and A2), 20 (B1 and B2) and 26km/h (C1 and C2) above the speed limit, respectively. All specifications use MSE optimal bandwidth estimated with a triangular kernel, local linear point estimators and local quadratic estimates for the bias correction. Y(left) indicates the mean of the dependent variable in the 0.5km/h bin below the cutoff. Number of observations: 78,882. **/* indicate significance at the 5%/10%-level, respectively.

A.2 Theoretical Framework

This Appendix offers a closer discussion of the model framework and the comparative statics outlined in Section 3. Note that the framework is similar to the models studied in Altmann *et al.* (2017) and Heffetz *et al.* (2019). It follows the logic of job-search models (e.g., McCall 1970) and can be easily expanded to a broad set of economically relevant choices of costly task completion under a deadline (e.g. submitting work before a due date or buying a product before a price increase; Altmann *et al.*, 2017).

We consider an agent who receives a speeding ticket in period $t = 0$. Paying the stipulated fine f involves transaction and opportunity costs c_t. These costs are i.i.d. according to a given distribution $F(c)$, which is continuous and increasing in its support $[\underline{c}, \infty]$.[39] In every period t, after the realization of the costs, the agent faces a binary choice: to pay or not to pay the fine f.[40] As long as the fine remains unpaid, the agent re-considers the problem in the next period, $t + 1$.[41] Postponing the payment beyond the deadline T implies additional costs K (a late fee and, potentially, demerit points). $C(t)$, the costs of paying the speeding ticket in period t, are then given by

$$C(t) = \begin{cases} c_t + f & \text{if } t \leq T \\ c_t + f + K & \text{if } t > T. \end{cases}$$

The agent pays the ticket in period t if the present costs are lower than the expected costs of postponing the choice. Formally, this condition is given by

$$-c_t - f \geq \delta V_{t+1}, \tag{A-1}$$

where V_{t+1} is the option value of 'waiting' for the next period and δ is the discount factor. Note that our empirical application uses discrete periods in terms of days. Hence, the (day-to-day) discount factor should be reasonably close to unity.

From (A-1) follows that the agent pays if and only if the costs c_t are sufficiently low:

$$c_t \leq \hat{c}_t := -\left(f + \delta V_{t+1}\right) \tag{A-2}$$

and the probability that the agent pays the fine in a pre-deadline period t – conditional on not having done so before – is given by

$$h_t := F(\hat{c}_t). \tag{A-3}$$

With the cutoff rule from (A-2), the option value V_t for a pre-deadline period $t < T$ is

$$V_t = \int_0^{\hat{c}_t} (-c - f) dF(c) \; + \; (1 - h_t)\delta V_{t+1}. \tag{A-4}$$

[39] By assuming independency we neglect liquidity constraints (which would imply inter-temporally correlated draws). Section 4.4 discusses evidence suggesting that liquidity constraints are not an issue in our context.

[40] Anticipating the empirical fact that partial payments are defacto unobserved we neglect incomplete payments.

[41] We abstract from cognitive limitations ('forgetting' about the speeding ticket; see Altmann *et al.* 2017).

The first term of V_t captures the expected costs $C(t)$ conditional on cutoff $\hat{c}_t$; the second term measures the expected option value from further postponing to $t+1$ (which happens with probability $1-h_t$). As discussed below, $V_{t+1} < V_t < 0$. The RHS of (A-2) is thus positive as long as $\delta \approx 1$.[42]

If the agent has reached period $t = T$ without having paid before, the cutoff becomes

$$\hat{c}_T := -\left(f + \delta V_{T+1}\right) \quad \text{with} \quad V_{T+1} = \int_0^{\hat{c}_{T+1}} (-c - f - K) dF(c) + \left(1 - F(\hat{c}_{T+1})\right) \delta V_{T+2}. \qquad \text{(A-5)}$$

The increase in $C(t)$ at the deadline T provides an incentive to pay before the deadline. Once the deadline gets closer, the option value of further postponing the payment shrinks: there are fewer periods left that could yield a low cost draw before the payment obligation increases from f to $f + K$. From this follows $V_{t+1} < V_t$ (recall that $V_t < 0$).[43]

Based on the definition of h_t from (A-3), the cumulative payment rate (or the chance that an agent has paid the fine) by period t is

$$s_t := 1 - \Pi_{\tau=0}^{t}(1 - h_\tau), \qquad \text{(A-6)}$$

where $\Pi_{\tau=0}^{t}(1-h_\tau) = (1-h_0)(1-h_1)\ldots(1-h_t)$ measures the probability of not having paid until t. The pre-deadline compliance rate – the share of tickets paid before the deadline – is given by s_T.

A.2.1 Comparative static: fine.

How does s_T respond to a change in the fine f? To tackle this question, we first explore how V_T changes with f. Applying Leibniz's rule of integral differentiation and re-arranging yields

$$\frac{\partial V_T}{\partial f} = \underbrace{-F(\hat{c}_T)}_{A} + \underbrace{\left(1 - F(\hat{c}_T)\right) \delta \frac{\partial V_{T+1}}{\partial f}}_{B} + \underbrace{\left(-\hat{c}_T - f - \delta V_{T+1}\right) \frac{\partial F(\hat{c}_T)}{\partial \hat{c}_T} \frac{\partial \hat{c}_T}{\partial f}}_{C}. \qquad \text{(A-7)}$$

Term A is negative. Term B must be non-positive: a post-deadline option value cannot increase with an increase in f. From (A-5) follows $\hat{c}_T = -f - \delta V_{T+1}$; term C is therefore zero and it follows that $\frac{\partial V_T}{\partial f} < 0$. As f increases, the option value falls. Repeating this argument for period $T-1$ one can easily show that $\frac{\partial V_{T-1}}{\partial f}$ is negative, too. Note further that any decrease in the option value must be smaller than the present cost form a marginal increase in f. In absolute terms, we then have $\mid \partial V_{t+1}/\partial f \mid < 1$. From this follows $-1 < \frac{\partial V_t}{\partial f} < 0$ for any $t \leq T$.

Equipped with these properties we can now evaluate the effect on the compliance rate. As noted before, the first-order increase in the present costs from a marginally higher fine f works towards lowering $\hat{c}_t$. This effect will dominate the effect of the declining option value (which tends to increase $\hat{c}_t$). Formally, we get $\frac{\partial \hat{c}_t}{\partial f} = -1 - \delta \frac{\partial V_{t+1}}{\partial f} < 0$ (as $\delta \leq 1$). An increase in the fine should therefore lower the hazard rate h_t (for $t \leq T$) and, in turn, the pre-deadline payment rate s_T: a higher fine causes a decrease in timely compliance.

[42] To assess this condition, recall first that δ is the day-to-day discount factor (see above). Second, let us consider the case of $\delta = 1$. We can then re-arrange $-\left(f + V_t\right)$ to obtain $-f + f + \int_0^{\hat{c}_t} c\, dF(c) - (1 - h_t)\left(V_{t+1} + f\right)$. This must be positive since $V_{t+1} < -f$.

[43] Our analysis will focus on the pre-deadline period. To close the model, however, we might further assume that the late penalty K will increase over time. This would be the case, for instance, if the (perceived) enforcement pressure increases over time. (Using the notation from fn. 13, one could assume $p(t)\kappa$ with $p(t+1) > p(t)$ for some $t+1 > T$.) This would assure that option values decline over time, even after the deadline has passed.

A.2.2 Comparative static: late penalty.

Next, consider a *ceteris paribus* increase in the late penalty K. Differentiating $\hat{c}_T$ w.r.t. K yields

$$\frac{\partial \hat{c}_T}{\partial K} = -\delta \left(\underbrace{-F(\hat{c}_{T+1})}_{A} + \underbrace{(1 - F(\hat{c}_{T+1}))\,\delta \frac{\partial V_{T+2}}{\partial K}}_{B} + \underbrace{(-\hat{c}_{T+1} - f - K - \delta V_{T+2}) \frac{\partial F(\hat{c}_{T+1})}{\partial \hat{c}_{T+1}} \frac{\partial \hat{c}_{T+1}}{\partial K}}_{C} \right). \tag{A-8}$$

From the Envelope Theorem and the discussion above it follows that terms A and B are negative and term C is zero (it follows from (A-5) that $\hat{c}_{T+1} = -(f + K + \delta V_{T+2})$). As the sum $A + B < 0$ is multiplied with $-\delta$ we get $\partial \hat{c}_T / \partial K > 0$. This result is intuitive: an increase in K causes an increase in the payment rate at the deadline.

Does the same hold for pre-deadline periods $t < T$? In these periods, there is no direct increase in the payment obligation for $t + 1$. The counterpart of expression A from (A-8) is missing and we get

$$\frac{\partial \hat{c}_t}{\partial K} = -\delta \left((1 - F(\hat{c}_{t+1}))\,\delta \frac{\partial V_{t+2}}{\partial K} \right) \quad \text{for } t < T \tag{A-9}$$

(where we have already omitted term C). The option value V_{t+2} is declining in K: with a marginal increase in the late penalty, postponing becomes less attractive as the future costs of the speeding ticket, conditional on missing the deadline, increase. Hence, $\partial \hat{c}_t / \partial K > 0$. From this it follows that $\partial h_t / \partial K > 0$ for $t \leq T$, which ultimately means that s_T is increasing in K. The rate of pre-deadline payments is therefore increasing in K.

A.2.3 Conjoint variation of f and K.

Empirically, our setting might be characterized by discontinuities with a conjoint increase in both f and K. For this case, the results from above imply that the overall impact on timely compliance is ambiguous: the two changes would work in opposite directions and it is unclear which one dominates. For an *increase* in f that is accompanied by a *decrease* in K, however, the model predicts an unambiguously negative effect on the rate of pre-deadline payments – both changes weaken the incentives to pay before the deadline.

A.2.4 Misperceptions.

Next, we consider agents with (mis)perceptions of the deadline, $\tilde{T}(\theta_T) = \theta_T T$, and the late penalty, $\tilde{K}(\theta_K) = \theta_K K$. The θ parameters, with $0 < \theta < \infty$, capture whether T and/or K are over- or underestimated. It is straightforward to extend the cutoff rule from (A-2) to the case of (mis)perceptions. If agents optimize based on $\tilde{K}(\theta_K)$ and $\tilde{T}(\theta_T)$, one can substitute these values for the actual K and T, respectively. The survey evidence from Section 4.2 indicates that the treatments from our RCT alter these perceptions. Let us next derive the behavioral implications.

A.2.5 Treatment effect on (mis)perceptions: late penalty.

How does an increase in $\tilde{K}(\theta_K)$ (via an increase in θ_K) influence pre-deadline compliance? The analysis from above implies that an increase in $\tilde{K}$ will have the same effect as an increase in

K: timely compliance should increase. If the P- and the $D\&P$-treatments push θ_K upwards, we therefore predict an increase in timely compliance (relative to the control treatment). The opposite prediction emerges for agents that *overestimate* K (i.e. $\theta_K > 1$). For this case, the treatments would lower $\tilde{K}$ and, in turn, timely compliance should decline.

Section 5.3 ('*Variation in late penalty*') analyzes treatment-induced variation in the salience of K. The analysis exploits the fact that the P- and the $D\&P$-treatment emphasize the late fee: for a speed marginally below the main cutoff (20km/h above the limit), the cover letters stress a payment obligation 'as high as 2,500 CZK'. For a speed marginally above the cutoff, the letter notes payments 'as high as 5,000 CZK' (see Appendix A.4.1). It seems plausible that speeders in these treatment groups (with recorded speeds marginally above or below the cutoff) perceive a higher discontinuity in K at the cutoff (relative to those in the control- and the D- treatments).

At the 20km/h cutoff, there is variation along two dimensions: first, there is a discontinuous increase in the fine f. This policy variation (which is, plausibly, fully salient and clearly symmetric across treatment groups) should lower pre-deadline compliance rates (see A.2.1 above). Second, there might also be a discontinuous increase in the *perceived* late penalty $\tilde{K}$. From above we know that an increase in $\tilde{K}$ strengthens the incentive to pay before the deadline. The perceived discontinuity in K – and thus the incentive to comply – should be more pronounced in the P- and $D\&P$-treatments. For these two treatments we therefore predict a smaller drop (or even an increase) in pre-deadline compliance around the 20km/h cutoff (relative to the C- and D-treatments).

A.2.6 Treatment effect on (mis)perceptions: deadline.

To assess variation in the perceived deadline $\tilde{T}(\theta_T)$, we must discuss several basic properties from our model framework.[44] Note, first, that for any given pre-deadline period t, the option value V_t is increasing in T. A longer deadline offers more opportunities to draw a low cost c_{t+s} in future periods $t + s \leq T$. A higher option value, in turn, lowers the incentive to pay in period t. From (A-2) it then follows that $\hat{c}_t$ decreases. Hence, $h_t = F(\hat{c}_t)$, the probability to pay in a given period $t < T$ (conditional on not having done so before), drops if the deadline T is extended. Under two arbitrary deadlines $T'' > T'$ we therefore have

$$h_t \mid^{\tilde{T}=T'} > h_t \mid^{\tilde{T}=T''} \text{ for any } t \leq T' \tag{A-10}$$

(where $h_t \mid^{\tilde{T}}$ indicates the hazard rate under a given deadline perception). From this we can immediately derive predictions regarding the impact of a *treatment induced decline in deadline misperceptions*. Consider an agent who initially overestimates the deadline and perceives $\tilde{T} = T'' > T$. Whenever the D- and the $D\&P$-treatments lower perceptions towards T' (with $T \leq T' < T''$), it follows from (A-10) that the pre-deadline payment rate s_T must increase. In each period $t \leq T$, the agent considers a lower option value and is therefore more likely to pay (conditional on not having done so before). Reducing an over-estimation of the deadline (i.e. $\theta_T \downarrow$ starting from $\theta_T > 1$) therefore increases the rate of timely compliance (i.e. payments before the actual deadline T).[45]

[44] Appendix A in Altmann *et al.* (2017) offers a complementary analysis of variation in T.

[45] Note that the cases of $T' \leq T < T''$ (i.e. when the treatment induces the perceived deadline to drop below the true deadline) and $T' < T'' \leq T$ is less clear. Section 4.2 suggests, however, that both scenarios are of limited empirical relevance.

Let us turn to a *treatment induced increase in the perceived deadline* (starting from an underestimation; i.e. $\theta_T \uparrow$ departing from $\theta_T < 1$). To evaluate this case, a further model property becomes important. Consider two agents, one of whom perceives a 'short' deadline T', and one who perceives a longer deadline $T'' = T' + s$ with $s > 0$. From the cutoff rule (A-5) it follows that the first agent's conditional probability h_t of paying in a given pre-deadline period $t = T' - \tau$ (that is $\tau \geq 0$ periods before the perceived deadline T') is exactly the same as the second agent's probability $h_{t'}$ in period $t' = T'' - \tau = T' + s - \tau = t + s$. Formally, this is

$$h_t \mid^{T'} = F(\hat{c}_t) \mid^{T'} = h_{t'} \mid^{T''} = F(\hat{c}_{t'}) \mid^{T''} \quad \text{with } t' = t + s \text{ and } T'' = T' + s.$$

The intuition behind this property is straightforward. Conditional on not having paid before, the probability of paying τ periods before the perceived deadline is independent of the time that has passed. What matters is τ, the 'distance' to the perceived deadline, but not time t in itself. From this follows that

$$\Pi_{t=0}^{T'}(1 - h_t \mid^{T'}) = \Pi_{t=s}^{T''}(1 - h_t \mid^{T''}). \tag{A-11}$$

Under a short deadline T', the probability of not having paid the fine between periods $0 \leq t \leq T'$ is exactly the same as the probability of an agent with a perceived deadline T'' not having paid between periods $s \leq t \leq T'' = T' + s$.

Making use of these properties, we can now evaluate the treatment effect on an agent who initially underestimates the deadline, $T' = T - s$, and then updates her perceptions to $T'' = T' + s = T$. Under the perception T', the pre-deadline compliance rate can be decomposed as:

$$s_T \mid^{T'} = 1 - \underbrace{\Pi_{t=0}^{T'}(1 - h_t \mid^{T'})}_{A1} \underbrace{\Pi_{t=T'+1}^{T}(1 - h_t)}_{B1} \tag{A-12}$$

(compare definition A-6). With a perceived deadline T'', the corresponding pre-deadline compliance rate can be presented as

$$s_T \mid^{T''} = 1 - \underbrace{\Pi_{t=0}^{s-1}(1 - h_t \mid^{T''})}_{B2} \underbrace{\Pi_{t=s}^{T''}(1 - h_t \mid^{T''})}_{A2} \quad \text{with } T'' = T \tag{A-13}$$

From (A-11) it follows that A1 = A2. If B1 > B2, we would thus get $s_T \mid^{T'} < s_T \mid^{T''}$. Vice versa, if B1 < B2, we would obtain $s_T \mid^{T'} > s_T \mid^{T''}$. Let us next discuss the expressions B1 and B2.

The term B2 measures the probability of not paying the fine during periods $0 \leq t < s$ for an agent who optimizes subject to the correct deadline $T'' = T$. The term B1 measures the corresponding probability during the first s periods *after* a perceived deadline T' (with $T' = T - s$), i.e. in periods $T' < t \leq T' + s = T$. It turns out that the comparison between B1 and B2 is ambiguous.

To see this, recall first that periods $(T', T]$ are – from the misperceiving agent's perspective – the first s periods after the perceived deadline T'. Note further that the relevant cutoff in a post-(perceived-)deadline period τ is $\hat{c}_\tau := -(f + K + \delta V_{\tau+1})$ where $V_{\tau+1}$ is the post-deadline option value. If K (or $\tilde{K}$) is large relative to the difference between $V_{\tau+1}$ and V_{t+1}, it follows from (A-2) that we might get $c_\tau < c_t$ and, in turn, $h_t \mid^{T''} > h_\tau$ for $\tau = T' + 1 + s$ and $t = 0 + s$ (with $0 \leq s < T - T'$). Stated more intuitively: if the disincentive to pay associated with the late penalty dominates any differences in the pre- and post-deadline option values, we should see lower conditional payment probabilities during periods $(T', T]$ (after the perceived deadline) as compared to the very first periods $[0, s)$ (under a correct deadline perception). If this were the case, condition B1 > B2 could hold. In principle, however, we could get the opposite and B1 < B2 would hold. Without imposing further assumptions, we therefore have an ambiguous prediction:[46] A treatment-induced correction of a deadline underestimation (i.e. $\theta_T \uparrow$ towards $\theta_T = 1$) could trigger either an increase or a decrease in the pre-deadline compliance rate.[47]

A.3 Cost-Benefit Analysis

This subsection provides details on the cost-benefit analysis – in particular, the fiscal costs and benefits from the most effective intervention (the *D&P* cover letter) – introduced in Section 4.5. Note that the additional input parameters – the information on the processing time of follow-up enforcement, the labor costs of administration and further details reported below – were obtained from the traffic authorities in Ricany.

One benefit from increasing pre-treatment compliance comes from saving the public authorities' resources spent on enforcement. As mentioned in Section 2, the administrators follow-up with additional enforcement steps if a ticket is not paid in time; they compile and send further legal notifications (often more than one) and may communicate with the car owners via e-mail, phone, or in person. These administrative steps take about 15 minutes per ticket when offenders comply quickly after the first follow-up step (and much longer for protracted cases). Accounting for the labor costs of the administrators – the average hourly labor costs of an administrator are about 200 CZK – the costs for processing an unpaid ticket thus amount to at least 50 CZK. In turn, the increase in timely payments caused by the *D&P* cover letter (+2.06pp) translates into a reduction of at least 1.03 CZK in administrative costs per ticket.

A further, direct fiscal benefit from the *D&P* treatment is given by the gains in collected revenues. These gains are comprised, on the one hand, by the treatment-induced increase in the probability of collecting any payment. On the other hand, we must account for the potential 'loss' in late fees (which would have occurred for post-deadline payments). Let us discuss these two components in more detail.

First, note that we cannot directly estimate the treatments' effects on ultimate payments. The RCT ended in August 2019 and our data-set records payments made until December. The final payment outcomes, however, might take more than one year to materialize. Analyzing data from the start of our trial nevertheless indicates that non-compliance within 100 days (the longest time window used in our analysis) is a good predictor for long-run non-compliance: for 9 out of 10 tickets that are unpaid within 100 days (and for which we are able to observe a one-year outcome period), there is no payment recorded even after the first 100 days. We therefore use the estimated treatment effects on (full) payments within 100 days to approximate the revenue gains.

[46] To arrive at an unambiguous comparative static, we would need further structure to specify post-deadline (relative to pre-deadline) option values (see also fn. 43).

[47] This ambiguous result (which assumed $T'' = T$) applies to any upward shift in deadline-perceptions with $T' < T'' \leq T$. The case of $T' < T < T''$ (an 'overshooting' in perceptions) is even more complex. However, the results from Section 4.2 suggest that this latter case is empirically not very relevant.

As pointed out above, we also have to account for the treatment-induced shift in the post-deadline payments of $f + K$ (see Section 3). To illustrate this point, it is useful to introduce some formal structure. Let s_0^τ denote the cumulative payment rate during the first τ days after receiving a ticket in the absence of the treatment (control group benchmark). Let s_3^τ indicate the corresponding rate observed for the *D&P* treatment. Following the discussion from above, we use payments within 100 days as a proxy for the final revenue effect. Neglecting the timing of payments (i.e. zero interests and no discounting), the treatment effect on revenue collected from fines and late fees is then given by

$$[s_3^{15} f + (s_3^{100} - s_3^{15})(f + K)] - [s_0^{15} f + (s_0^{100} - s_0^{15})(f + K)]. \tag{A-14}$$

That is, share s_3^{15} of speeding offenders pays the basic fine by the deadline and an additional share $(s_0^{100} - s_0^{15})$ pays the basic fine plus the late fee $(f + K)$. After some manipulation, the difference between post- and pre-treatment revenues from equation A-14 becomes

$$(s_3^{100} - s_0^{100})f + [(s_3^{100} - s_0^{100}) - (s_3^{15} - s_0^{15})]K \tag{A-15}$$

which can be expressed as

$$\beta_{3,100} f + (\beta_{3,100} - \beta_{3,15})K. \tag{A-16}$$

In the last expression, $\beta_{3,15}$ and $\beta_{3,100}$ denote the *D&P* treatment's effect on the probability that a (full) payment is made within 15 or 100 days, respectively (see eq. 2). Intuitively, an increase in the rate of payments within 100 days yields revenue gains from collecting the basic fine f. In addition, the expression accounts for the share of those tickets that are already paid within 15 days and thus avoid the late fee K. Using the estimates from columns (3) and (7) in Table 5 to quantify the effect sizes, the mean values of f (830 CZK) and K (870 CZK), equation A-16 implies a revenue gain of around 11.83 CZK per ticket. Together with the savings in enforcement costs (1.03 CZK, see above), the *D&P* cover letter thus yields a fiscal benefit of approximately 12.86 CZK per ticket.

To assess the the marginal costs of printing and sending the cover letters, first note that 40% of tickets are sent electronically (see Table 4). Given that the fixed costs for the necessary adjustment in the software were negligible, we consider zero marginal costs for these cases. For the remaining 60% of treatments, the costs are at most 0.4 CZK (paper and printing; sending costs are unaffected). Hence, an 'average' treatment cover letter costs at most 0.24 CZK. Summing up the fiscal benefits and costs, the *D&P* treatment yields a net fiscal gain of 11.62 CZK per ticket on the margin.

A.4 Cover letters and notifications

A.4.1 Cover letters

The texts used in the different cover letters was the following (English translation):

Deadline (D) treatment:

Dear Sir/Madam,

We summon you to pay the prescribed amount for a speeding violation. We encourage you to carefully read the information contained in the attached pages and take appropriate action.

Please pay the amount in full and make sure it is credited to the city's account **within 15 days** after receiving this summons.

The city office of Ricany, legal division, department of fines

Penalty (P) treatment:

Dear Sir/Madam,

We summon you to pay the prescribed amount for a speeding violation. We encourage you to carefully read the information contained in the attached pages and take appropriate action.

If you do not pay the whole amount the office will continue investigating the offense. The amount that you will potentially have to pay **may be as high as CZK 2,500**.[48] In addition, the driver may be **added points** within the demerit point system.

The city office of Ricany, legal division, department of fines

Deadline & Penalty ($D\&P$) treatment:

Dear Sir/Madam,

We summon you to pay the prescribed amount for a speeding violation. We encourage you to carefully read the information contained in the attached pages and take appropriate action.

Please pay the amount in full and make sure it is credited to the city's account **within 15 days** after receiving this summons.

If you do not pay the whole amount the office will continue investigating the offense. The amount that you will potentially have to pay **may be as high as CZK 2,500**.[49] In addition, the driver may be **added points** within the demerit point system.

The city office of Ricany, legal division, department of fines

A.4.2 Standard notification

The following pages present the legal notification sent to all car owners in our RCT (in English translation from the Czech original).

[48] For medium-severity speeding violations (i.e. speeding at more than 20km/h above the limit), this part reads 'as high as CZK 5,000'.

[49] This part is also adjusted for medium-severity speeding violations (see previous footnote).

Docket number: XXXX Proceedings number: XXXX

The town authority of Ricany

Legal division,

Office of speeding cameras and vehicle weights

Contact official: name, phone Car operator: name, address

Date:

Summons for the payment of a prescribed amount under § 125h, paragraph 1, of Act No. 361/2000 Coll. on the Road Traffic and Amendments to Other Laws, as subsequently amended.

The town authority of Říčany, legal division, department of fines, as the town authority of a municipality with extended jurisdiction, competent to the administrative proceedings under provisions of section **§ 125h, paragraph 1, of Law No. 361/2000** on Road Traffic and on the Amendments to Other Laws, as subsequently amended (hereinafter "road traffic law"), **summons** the operator of the vehicle with license plate ####### (hereinafter "vehicle"), with which road traffic law was breached on **03.01.2016** at **14:57** on Říčanská street (a segment situated between Kolovratská and Březinova streets, a stretch of 335,360 metres) in the municipality of Říčany in the direction from Voděrádky,

for the payment of the amount

of **900 Kč** (nine hundred Czech korunas) to the bank account 35-320390319/0800, variable symbol 9116001251, (message for the recipient: XXXX), and to do so within 15 days of the delivery of this summons. Kindly pay attention to crediting the said account with the prescribed amount within the given time limit.

The illegal act was committed by an unspecified driver, who did not respect the traffic sign "speed limit – 50km/h", and as such violated section §125c, paragraph 1, letter c), item 4 of the road traffic law. This violation appears to satisfy the definition of an offense under section **§18, paragraph 4** of the road traffic law. The offense was documented by an automated speed measurement system without the presence of a human operator.

Based on the findings of the town authority of Ricany, legal division, department of fines, you are the operator of the vehicle with license plate ####### and, according to the provisions of section **§ 10, paragraph 3** of the road traffic law, you are obliged to ensure that, while the vehicle is in use on the road, all driver's duties and road traffic rules are followed as prescribed by this law. This particular offense constitutes a breach of the driver's duties and, concurrently, of the road traffic rules. The offense shows indicia of a violation under the road traffic law; there was no traffic accident.

Because the above-mentioned offense may, in accordance with the legal provisions, be handled by issuing a fine on the spot and, as previously mentioned, other conditions were also fulfilled for the special procedure by the administrative authority for the application of the strict liability of the vehicle operator under the provisions of section **§ 125h, paragraph 1** of the road traffic law, the prescribed amount of **900 Kč** has a legal basis according to the provisions of section **§ 125h, paragraph 2** of the road traffic law.

Notice:

1. If the prescribed amount is paid by the due date, i.e. if it is be credited to the stated account within 15 days of the delivery of this summons, the town authority of Říčany, legal division, department of fines, will defer the case in accordance with the provisions of section **§ 125h, paragraph 5** of the road traffic law. Otherwise, it will continue investigating the offense.
2. If the prescribed amount is paid after the due date, the town authority of Říčany, legal division, department of fines, will, in accordance with the provisions of section **§ 125h, paragraph 7** of the road traffic law, immediately return it to the vehicle operator and will continue investigating the offense.
3. Concurrently, in accordance with the provisions of section **§ 125h, paragraph 6** of the road traffic law, we instruct you that if you do not pay the prescribed amount, you may report, in writing, the details of the identity of the driver who was driving the vehicle at the time of the offense to the town authority of Říčany, legal division, department of fines, and you may do so within 15 days of the delivery of this summons. You may use the attached form to report these details. Providing the identity details of the driver is considered, according to section § 125h, paragraph 6, as a provision of an explanation.
4. If you neither pay the prescribed amount nor make use of your right to report the details of the identity of the vehicle's driver nor provide any other explanation within the given time limit, the town authority of Říčany, legal division, department of fines, will continue investigating the offense.

If you consider it necessary to provide an oral explanation on record, you may visit our offices during office hours (Monday and Wednesday 07:30-12:00 and 12:30-18:00; Tuesday and Thursday: 07:30-12:00) at Olivova 1800, Říčany.

Upon payment of the prescribed amount, no record is made. No demerit points will be imposed on the vehicle operator within the framework of the demerit point system for drivers under the provisions of sections § 123a to § 123f of the road traffic law. This summons is not an administrative adjudication.

Appeal against this summons is not admissible.

Contact officials' signatures.

Attachments:

- Photograph from the location of the speed measurement
- Form for reporting the identity details of the vehicle's driver at the time of the offense
- Postal order for payment