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Speeding, Punishment, and Recidivism
Evidence from a Regression Discontinuity Design *

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Abstract

This paper estimates the effects of temporary driver's license suspensions on driving behavior. A little known rule in the German traffic penalty catalogue maintains that drivers who commit a series of speeding transgressions within 365 days should have their license suspended for one month. My regression discontinuity design exploits the quasi-random assignment of license suspensions caused by the 365-days cut-off and shows that 1-month license suspensions lower the probability of recidivating within a year by 20 percent. This is largely a specific deterrence effect driven by the punishment itself and not by incapacitation, information asymmetries, or the threat of stiffer future penalties.

JEL codes: I12, K42, R41

Keywords: Crime, Speeding, Deterrence, Regression Discontinuity

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1 Introduction

Worldwide, more than 1.2 million people die every year in traffic accidents (WHO, 2013). In 2010, traffic injuries in the US claimed the lives of 45,342 Americans and caused medical and work loss costs of more than $100 billion. Motor vehicle crashes are the leading cause of death for those aged 15 to 35 (CDC, 2015). Numerous laws and regulations, such as mandatory seatbelt use, speed limits, and blood alcohol concentration (BAC) limits, have been enacted in order to promote road safety. Most developed countries have an elaborate penalty catalogue in place that aims to punish and deter traffic offenders. Temporary license suspensions are a crucial component of these penalty catalogues. This measure not only incapacitates traffic offenders by taking them off the streets for a short period of time. It is also supposed to provide a "shot across the bows" by prompting offenders, who temporarily experience the inconveniences of life without a driver's license, to change their ways and drive more responsibly once they get their license back.

Yet, little is known about the effectiveness of temporary license suspensions. Economic theory provides a number of channels through which they may or may not affect driving behaviour. Gary Becker's (1968) model of the "rational criminal" predicts that temporary license suspensions should only have long-run effects if they affect the degree of future penalties and thus change the underlying cost-benefit trade-offs. The behavioral approach to the economics of crime (Jolls et al., 1998, among others) offers other channels through which temporary license suspensions might affect future behaviour even if they are one-off punishments. For instance, offenders might be backward-looking or might update their beliefs about the actual cost of punishments and the probability that an offense is detected.

Ultimately, determining the effectiveness and the mechanisms through which penalties affect criminal behaviour is, therefore, an empirical issue. As Levitt and Miles (2007) point out, the main challenge for empiricists is to distinguish causation from correlation. A naive comparison of the recidivism behavior of offenders who were punished for their criminal acts with those who were not, cannot shed much light on the question of how effective penalties are in deterring future crimes. After all, these penalties are not randomly assigned. People who get punished because they committed a crime might be intrinsically prone to committing crimes. Comparing their rates of recidivism to those of law abiding citizens is thus an apples to oranges comparison. Traffic transgressions are no exception to this problem. As shown below, a "naive" OLS regression yields a *positive* effect of license suspension on recidivism even when conditioning on age, sex, and state fixed effects.

The goal of this study is to overcome these challenges in answering a seemingly simple question. For that purpose, I exploit a rule in the German penalty catalogue for traffic violations which leads to a quasi-random suspension of some individuals' driver's licenses. This rule maintains that a person who commits two major speeding violations within 365 days, should have her license revoked for one month. This gives rise to a fuzzy regression discontinuity design where those to the left of the 365-day cutoff are likely to have their license revoked while those to the right of the cutoff retain theirs. The rule appears to be complex enough to prevent sorting to the right of the threshold. At the same time, the rule is very much enforced by the traffic authorities. Using rich administrative data, I find that a loss of license for one month reduces the probability of recidivating within a year by about 20 percent. The nature of the institutional framework also allows me to pinpoint the mechanism through which the penalty operates and to disentangle the effect of license suspensions from confounding punishments such as money fines and demerit points.

2 Background and Institutional Framework

Endogeneity issues in the relationship between punishment and criminal activity have been recognized since at least the late 1990s (Levitt, 1996). Ever since, economists have come up with various strategies to exploit sources of exogenous variation in order to isolate causal effects of punishment on recidivism. Kessler and Levitt (1999) exploit sentence enhancements that are exogenously induced by California's Proposition 8 to evaluate the effect of harsher sentences on crime. They find that harsher punishments reduce crime substantially. Helland and Tabarrok (2007) utilize idiosyncrasies in the same state's three strike policy to isolate a large and significant deterrence effect of the policy. Lee and McCrary (2017) use the fact that juvenile criminals tend to be sentenced as adults once they turn eighteen, and find only small deterrence effects of more severe penalties.

Drago et al. (2009) and Barbarino and Mastrobuoni (2014) analyze sentence reductions for certain parts of the Italian prison population due to a collective clemency bill. Their results suggest that a reduction in prison sentences actually reduced recidivism. Di Tella and Schargrodsky (2013) use Argentinian judges' ideological differences as an instrument in evaluating the effectiveness of electronic monitoring compared to incarceration. They find that this more lenient treatment reduces recidivism. Green and Winik (2010), Snodgrass et al. (2011) and Aizer and Doyle (2015) exploit the random assignment of criminals to judges with different incarceration tendencies (an identification strategy pioneered by Kling (2006)) and find that juvenile imprisonment increases recidivism probabilities and has adverse effects on human capital accumulation.

It is surprising how little research has been devoted to natural experiments in the area of traffic violations. After all, traffic fatalities are an enormous social problem. By way of comparison, more than ten times as many people died in vehicle crashes in Germany than were murdered in 2015. What is more, previous estimates regarding the effectiveness of punishments for property or violent crimes are unlikely to apply to traffic offenses which tend to unintentionally lead to damages and casualties. One reason for a lack of research in this area, is that the effects of incapacitation, of the mere threat of punishments, and of the punishment itself are hard to disentangle. Moreover, punishment usually consist of a mix of penalties. For example, the best study to date on the effect of penalties on road safety by Hansen (2015) is only able to isolate the combined reduced form effect of money fines, increases in insurance premia, license suspensions, home releases, and jail time in response to committing a driving under the influence (DUI) offense.[1]

My study therefore significantly advances the literature: rich administrative data enable me to estimate not only reduced form effects, but to also obtain a local average treatment effect. In other words, this is the first study to assess the effect of a punishment for traffic offenses on recidivism, as opposed to the effect of being assigned to punishment. Moreover, the nature of my natural experiment allows me to distinguish the effect of a 1-month driver's

[1]Studies by DeAngelo and Hansen (2014), de Figueiredo (2015) and Traxler et al. (2017) are the only other papers that I am aware of to find plausibly causal effects of penalties and/or enforcement on road safety.

license suspension from the confounding effects of other "treatments", such as money fines and demerit points which are usually also part of a penalty mix. Finally, the institutional setting allows me to credibly isolate the specific deterrence effect of the punishment from the general deterrence impact and the effect of incapacitation.

My source of exogenous variation stems from an idiosyncrasy in the German traffic penalty catalogue. This catalogue generally provides for three different types of penalties: money fines, (demerit) points entered into one's central traffic registry account, and license suspensions. The degree of the penalty is determined by the seriousness of the offense.[2] Small transgressions are fined with small financial penalties. For more severe transgressions, points in the central traffic registry are added. Points received for different transgressions and different types of transgressions accumulate. If a person does not commit a transgression for two years, all points are erased. If a person does commit a transgression within two years, however, new points are added to the existing stock and the two year expungement period starts afresh. An offender permanently loses her license once her stock of points rises to 18. Finally, for severe transgressions, temporary license suspensions are handed out on top of points and money fines. For instance, a person speeding 45 km/h over the limit will have her license suspended for 1 month. The penalty catalogue also distinguishes between offenses that took place on highways and offenses that took place in built-up areas, for example residential neighborhoods. Fines differ in severity for different types of transgressions ranging from aggravated DUIs to driving without appropriate snow chains.

The road traffic law (BKatV), which constitutes the legal basis for the penalty catalogue, has multiple additional provisions. One additional provision is for "persistent delinquencies." It maintains that "a temporary license suspension [of usually 1 month] shall ordinarily be handed out if the operator of a motor vehicle commits a speeding transgression of at least 26 km/h within 1 year after another speeding transgression of at least 26km/h has been committed and the corresponding penalty has obtained legal force" (§4 Abs. 2 Satz 2 BKatV; own translation). This provision will henceforth be referred to as the "365-day rule." For instance, a person who within a few months is caught twice exceeding the speed

[2] Table A1 in the Appendix provides an excerpt from the penalty catalogue, specifically for speeding offenses.

limit on a highway by 28 km/h falls under the 365-day rule and will have her license temporarily revoked even though, according to the penalty catalogue, she should on aggregate only be fined €160 and receive 6 demerit points (see Table A1). Note that the wording of the provision is tricky. If you read the above text carefully, you will notice that the period, in which an offender is at risk of falling under the 365-day rule, only begins after the penalty for the first transgression "has obtained legal force." The difference between the date of the transgression and the date on which the associated penalty obtains legal force might seem minor at first yet the median difference between those two dates is 66 days. In other words, the day count which determines whether an offender has her license suspended due to the 365-day rule does not start immediately after committing the first transgression but with a substantial time lag. This wrinkle in the law is a big source of confusion among offenders and - as we will see in Section 4 - prevents sorting to the right of the 365-day threshold. Fortunately, both the date of a transgressions and the date on which the corresponding penalties obtain legal force are recorded in the data, so I can properly ascertain which offenders fall under the rule.

The 365-day rule provides a cutoff that can be exploited in a regression discontinuity setting. For instance, this rule requires that a person who commits her second transgression within 365 days after the penalty for the initial transgression has obtained legal force, should have her licenses suspended for one month. A person who commits her second transgression on day 366 should keep her license. By comparing the recidivism behavior of these two groups of people, who should be very similar except for the degree of the penalty they receive, I can obtain an unbiased estimate of the effect of a 1-month driver's license suspension. My identification strategy rests on two assumptions that need to be met to guarantee internally valid estimates. First, drivers by and large must not be aware of this regulation or, at the very least, they should not gear their driving behavior accordingly. Data and statistical tests (McCrary, 2008) presented in Section 4 will support this assumption and will show that sorting to either side of the 365-day cutoff is not common, most likely because the wording of the provision is not straightforward. Second, traffic authorities must enforce this regulation. It is apparent from the above quote that the authorities only "shall ordinarily" hand out licenses suspension and thus have considerable wiggle room. But, Section 4 will also show that the authorities to a great degree adhere to this rule, thus creating a discontinuity in the assignment of drivers to license suspensions.

3 Data

The source of data for this study is the German central traffic registry ("Verkehrszentralregister" or VZR). The VZR is administered and maintained by the Federal Motor Transport Authority ("Kraftfahrt-Bundesamt" or KBA). The VZR contains an account with a unique ID for every traffic offender who has committed a transgression that was sanctioned with at least one demerit point. Offenses are usually first recorded by local traffic authorities. These local agencies (or in some instances the courts) then transmit information on the date and type of transgression as well as the corresponding penalty to the KBA. The transmissions also contain information about the date on which a penalty obtains legal force.

The VZR is an active registry. Persons who do not commit a traffic transgression for two years not only get their points total set to zero, but are erased entirely from the data base. If someone commits a transgression *after* her account has been erased due to this 2-year expungement period, she starts with a clean slate. That is, she receives a new ID and cannot be linked to former VZR entries. The point system was reformed on 1 May 2014. On this occasion, a dataset containing the VZR population as of 30 April 2014 was created. The KBA made an anonymized version of this excerpt available for on-site analysis. The data set contains more than 10.5 million entries pertaining to about 6.3 million distinct offenders.

Obviously not all observations in this dataset can be used for this study. For one, a sufficiently large follow-up period is required since my main outcome of interest is recidivism. More importantly, this study isolates exogeneous variation in the severity of punishment to assess the effect of license suspensions on recidivism. That is, only offenders who have a chance to be affected by the 365-day rule should be used for analysis. The following steps describe the selection process that identifies observations that become part of my "experiment."

First, it should be noted that the 365-day rule only applies to speeding transgressions of

26km/h or more. More precisely, a 1-month license suspension may be imposed on speeders whose offense by itself would not have resulted in a license suspensions. There are two cases that qualify for this rule: speeding 26-40 km/h above the limit on highways and speeding 26-30 km/h above the limit in built-up areas. Second, the rule can only affect persons who have previously committed another speeding transgression of 26km/h or more. Throughout this article, I refer to the earlier of these two transgressions as the "original transgression" and to the second transgression as the "treatment transgression." When I refer to the time difference between the treatment and the original transgression, I am referring to the number of days that have passed between the date on which the penalty for the original transgression obtained legal force and the date of the treatment transgression. In other words, throughout this paper, I account for the wrinkle in the law that delays the start of the day count. Only persons who have both an original transgression and a treatment transgression (that is two speeding offenses of 26km/h or more) can enter the final "discontinuity sample". I also require the time difference between both transgressions to be no more than 545 days and at least 186 days. In essence, this puts a 180-day window around the 365-day threshold that determines whether a 1-month driver's license suspension is issued. I assess the robustness of my results to different time windows around the cutoff in Section 5.

The outcome of interest is recidivism, namely the probability of committing yet another (third) offense. This requires a sufficiently large post-treatment time window. This time window has to be even larger since it can take a few months for transgressions to show up in the data. For example, offenders may appeal against prescribed penalties in court. Transmission to the KBA and recording information into the VZR also takes some time. On average, it takes about two months from the date of the actual offense until it shows up in the data base, after 5 months more than 99% of incidents actually show up in the data. My observational period ends on 30 April 2014. Accounting for an at-most 5 month delay and in order to evaluate a 12 months recidivism window, only offenders who have committed their treatment transgression before 1 December 2012 can be used in this analysis.

Another complication arises from the 2-year expungement period. Individuals who do not commit a traffic transgression for two years are erased from the data set. By inversion, individuals who still are in the data base must have committed an offense in the past two years. That is, individuals who committed an offense before 1 May 2012 must necessarily have committed another offense subsequently. For these individuals there will thus be no variation in the outcome. As a result, individuals whose treatment transgression predates May 1 2012 also need to be excluded from the discontinuity sample. The appendix, in particular Figure A1, provides additional information regarding the generation of my final sample.

Once these restrictions have been imposed, a "discontinuity sample" emerges. It consists of 31,400 persons. Each person has a treatment transgression that occurred between 1 May 2012 and 30 November 2012; each person also has a original transgression for which the date on which the corresponding penalty has obtained legal force predates the date of the treatment transgression by at least 186 days and at most 545 days. For about half the sample, 365 days or less passed between these two points in time. Borrowing terminology from the potential outcome framework (Angrist et al., 1996), these observations constitute the "treatment group". Members of this group fall under the 365-day-rule and should have their licenses suspended for 1 month. The remainder of the discontinuity sample constitute the "control group". Members of this group have committed similar transgressions but due to the timing of their offenses mostly retain their driver's license. Persons in both treatment and control group may or may not recidivate, that is may or may not have committed a (third) major traffic transgression after their treatment transgressions.

If the descriptive statistics of Table 1 are any indication, it appears as if license suspensions have an effect. Rates of recidivism are 24.4% and 26.5% for the treatment and control group, respectively. These differences are statistically significant at the 1% level (see row 10 and columns (1) and (2) of Table 1). The difference actually tends to increase the more I limit the sample to observations with treatment transgressions closer to the 365-day threshold. On the other hand, demographic factors that are reported with the data are quite balanced across treatment and control group, indicating a quasi-random separation of the sample. For instance, the average age in the treatment sample is 42.66 years, in the control sample it is 43.02. The null hypothesis that there is no difference in means cannot be rejected at the 1% level. The means for all other covariates are also very similar, and formal t-tests for differences in means fail to reject the null in the vast majority of instances.

I also distinguish between speeding-specific recidivism and general recidivism. The two bottom rows of Table 1 provide the corresponding means and standard deviations. Since 1-month license suspensions are quasi-randomly assigned due to a speeding incidence, one might hypothesize that speeding recidivism is particularly deterred. This turns out to not be the case, in fact speeding-specific recidivism is just as much affected as overall recidivism (for example DUIs, speeding, running a red light, and so on). All results for speeding-specific recidivism are therefore relegated to the appendix of this paper.

4 Methods

The goal of this study is to exploit the exogeneous variation in penalties induced by the 365-day rule. The rule maintains that a 1-month license suspension shall be levied on offenders who - accounting for the wrinkle in the law that delays the start of the day count - commit two major speeding transgressions within 365 days, but not on those who commit two such transgressions within 366 days or more. This will allow for the identification of the causal effect of a temporary license suspension on the probability of reoffending. My identification strategy will only be valid if the 365-day rule is actually applied and results in a discontinuity in the assignment to treatment. Figure 1 illustrates that this is indeed the case. The x-axis shows the running variable, the number of days that have passed between the date on which the penalty for the original transgression has obtained legal force and the date of the treatment transgression. For each bin, I calculate the fraction of offenders within that bin who have had their license suspended for 1 month. The position of each point relative to the y-axis yields information about these fractions.

If the 365-day rule was strictly applied, everybody to the left of the red vertical line should have her license suspended for 1 month in addition to the prescribed money and point penalties. Everybody to the right or on the line should keep their license and merely suffer the prescribed money and point penalties. Such a "sharp" separation into treatment and control group is not present in this case. There is, however, a big drop in the probability of having one's license temporary revoked at day 366. To the left of the cutoff around three quarters of offenders lose their license for 1 month and to the right of the cutoff a mere 1.7

percent of offenders are hit with a 1-month license suspension. In other words, there is a huge drop in the probability of having one's license suspended due to the treatment transgression once 365 days have passed since the penalty for the original transgression obtained legal force. Likely reasons for receiving the treatment on the "wrong" side of the cutoff are involvement in an accident or repeat offending in terms of non-speeding transgressions.[3] The reasons for imperfect compliance on the left hand side are numerous. In instances very close to the cutoff, judges may be sympathetic to appeals and choose to not invoke the 365-day rule. The same might be true in cases of hardship, for example for elderly or disabled drivers who have no other means of transportation than their vehicles. Similarly, offenders from rural areas, commuters who are dependent on their car, or professional truck drivers might be able to keep their licenses. In general, any penalty notice can be appealed in court and judges may override a suspension if the offender shows remorse or accepts a higher monetary and/or point penalty in lieu of the temporary license suspension. Note that this kind of selection is *not* a threat to my identification strategy. Rather it illustrates the local interpretation of any regression discontinuity coefficient.[4]

By and large, traffic authorities follow the 365-day rule which induces a big drop in the probability of having one's license temporarily suspended at the expected threshold. The suspension is also indeed for exactly one month. In the data, there were only 6 instances in which a 2 months suspension was imposed and 1 case in which a 3 months suspension was imposed due to aggravating circumstances. These observations were dropped from the data. Figure 2 plots the recidivism outcome of interest against the number of days that have passed between the date on which the penalty for the original transgression obtained legal force and the date of the treatment transgression. The running variable is aggregated into 3-day bins. The size of each circle indicates the number of observations in each bin. The position of each circle, relative to the y-axis, indicates the fraction of offenders within a bin who recidivated within 12 months. The recidivism period is extended to 13 months for those who actually have had their license suspended for 1 month in order to account for

[3]It is noticeable that cases in which exactly 366 days have passed between the original transgression and the treatment transgressions have their licenses suspended more frequently than most other cases to the right of the cutoff. The reason is that 2012 was a leap year which has led to confusion among the local traffic authorities as to whether these cases should fall under the 365-day rule.

[4]This issue will be further discussed in Section 7.

incapacitation effects as drivers without a license naturally have less of an opportunity to re-offend.[5]

In Figure 2 there is clearly a discontinuity at the 366-day cutoff. This jump yields a first rough estimate of the reduced form (intent-to-treat) effect. The graph suggests that offenders who are assigned to treatment (license suspensions) are three to four percentage points less likely to recidivate than offenders who are not assigned to treatment. Note that the lowess lines in both figures are merely superimposed to better visualize the pattern in the data but may very well suffer from boundary bias close to the 365-day cutoff. In order to obtain a visual estimate of the size of the treatment effect it is more important to focus on the position of the cloud of points, especially around the cutoff, than to study the position of the lowess lines at the boundary.[6]

Figures 1 and 2 make a compelling case that a) the 365-day rule invokes a quasi-random assignment of license suspensions and b) this assignment indeed has an effect on future recidivism behavior. However, the internal validity of any coefficient obtained through this setup would be in jeopardy if offenders were very much aware of the 365-day cutoff and geared their driving behavior accordingly. Fortunately, there is little indication that this is the case. First, anecdotal evidence suggests that the vast majority of drivers is not even aware of this rule. There is a vast amount of online forums in which repeat offenders who fall under the 365-day rule express their shock about their license suspensions. Second, the wrinkle in the provision that starts the day count only after the penalty for the original transgression has obtained legal force, makes it hard for offenders to keep track of whether they are still at risk of falling under the 365-day rule. That is, if they were aware of the exact wording of the law to begin with.

Data back up this claim. If at least some drivers were aware of the 365-day rule and all its wrinkles, and were able to keep track of the exact day count, one would expect "bunching" on the right-hand side of the 365-day cutoff. Drivers would drive more carefully than usual until the 365-day rule no longer applied to them. This would result in a spike

[5] It should also be noted that the treated group have to deposit their license for one month at the local traffic authority within 4 months of the date on which the punishment takes legal effect, but I do not observe the exact dates on which they turn in their license.

[6] Figure A2 in the Appendix repeats this analysis for speeding-specific recidivism. The relative magnitude of the jump is very similar.

of traffic transgressions on days 366-400. Yet Figure 3 gives little indication that this is indeed the case. The frequency of treatment transgressions (in 3-day bins) is very evenly distributed with around 100 transgressions per day on each side of the cutoff. Most notably, there is no spike in treatment transgressions from offenders whose penalties for their original transgression obtained legal force just a bit more than 365 days ago. One might be mildly concerned about the small drop in the transgression frequency on days 360-362. Yet, the frequency rebounds to above-average levels on days 363-365. Drops in frequency of even greater magnitude can also be observed elsewhere in the distribution. This visual analysis is consistent with McCrary's (2008) more explicit density test for manipulation at the cutoff. The test implies a log difference in height of -.021 with a standard error of 0.033. In other words, the null hypothesis that there is no manipulation at the cutoff cannot be rejected at any reasonable level of significance. Frandsen (2017) has pointed out that McCrary's (2008) density test might be inconsistent for discrete running variables, such as the day count in this application. He has developed a test with preferable finite sample properties for such a scenario. Frandsen's (2017) test yields a p-value of 0.224, thus confirming that manipulation at the cutoff is unlikely. By and large, there is no indication of any bunching or any increase in frequency just to the right of the cutoff.

A related threat to the internal validity of the design is differential sorting of offenders to either side of the cutoff. For instance, more experienced or habitual offenders might be more knowledgeable about the penalty catalogue and the 365-day provision and might sort to the right of the cutoff. The data, however, give little indication that this is indeed the case. Figure 4 plots the average number of prior offenses in 3-day bins against the running variable. There is no indication for either a jump or drop around the 365-day threshold.[7]

Overall, there is no indication of any sorting behavior. This suggests that the 365-day rule is obscure enough to lead to a random separation of offenders into a treatment and control group, yet it is enforced to such a degree that the take-up among those who are assigned to treatment is substantially higher than among those not assigned to a 1-month driver's license suspension. This gives rise to a fuzzy regression discontinuity (RD) design

[7]Figures A4a and A4b in the Appendix also fail to detect a break for average age - another good proxy for driving experience - or the percentage of female drivers in each 3-day bin.

which is implemented as a 2SLS instrumental variable regression. When picking a functional form, we want to be sure that what at first glance certainly looks like a jump in recidivism rates at the 366-day cutoff is not just a non-linearity in the data. A visual inspection of a graph that plots the outcome of interest against the running variable (such as Figure 2) provides a useful guide for picking the correct functional form. The graph reveals no obvious non-linearities. Not least for efficiency reasons, a linear functional form therefore seems to be appropriate. Nonetheless, I will also consider specifications using second and third order polynomials of the running variable and interactions of these polynomials with the treatment dummy. Gelman and Imbens (2014) show that polynomials of even higher order do more harm than good and even the cubic version of the model might be too much. It is still useful as a robustness check.

An alternative is a nonparametric approach, for example local linear regression. Lee and Card (2008), however, argue that with a discrete running variable, such a nonparametric approach is not advisable. My running variable, the number of days between the original and the treatment transgression, is discrete but takes on many distinct values which should mitigate concerns about the nonparametric approach. Nonetheless, this method is only used as a robustness check. Section 5 will show that it leads to results that are strikingly similar to those of the least flexible parametric specification. Lee and Lemieux (2010) also suggest that the standard errors should be clustered on the distinct values of a discrete running variable which is done throughout the paper. The second stage regression in my 2SLS model is modeled as follows:

$$Rec_i = \beta_0 + \beta_1 X_i + (\beta_2 X_i^2 + \beta_3 X_i^3) + \gamma_0 \hat{D}_i + \gamma_1 \hat{D}_i X_i (+\gamma_2 \hat{D}_i X_i^2 + \gamma_3 \hat{D}_i X_i^3) + \epsilon_i \tag{1}$$

where X_i is the running variable, that is the number of days that have passed between the date on which the penalty for the original transgression obtained legal force and the date of the treatment transgression of offender i. As is best practice in an RD setting (Lee, 2008), the running variable is centered around the cutoff. D_i is a dummy indicating whether, due to the treatment transgression, offender i had her license suspended for 1 month. This dummy is instrumented for (see below) thus the hat-superscript in equation (1). Rec_i is a dummy equal to one if offender i recidivates and commits a (third) offense within 12 months after the treatment transgression.[8] γ_0 is the coefficient of interest and yields the treatment effect of a 1-month license suspension on the probability of recidivating within a year. In order to assess more flexible functional forms, the polynomials and interaction terms in parentheses can be added to the model. Since assignment to treatment is fuzzy, a first stage regression, yielding the predicted values $\hat{D}_i$, is necessary:

$$D_i = \delta_0 + \delta_1 X_i + (\delta_2 X_i^2 + \delta_3 X_i^3) + \pi_0 T_i + \pi_1 T_i X_i (+\pi_2 T_i X_i^2 + \pi_3 T_i X_i^3) + \eta_i \qquad (2)$$

where $T_i = 1(X_i < 366)$. In other words, T_i indicates assignment to treatment and D_i indicates whether the treatment was in fact taken up. Of course, in the specifications using higher order polynomials, the first stage is constructed such that the model is exactly identified and $\{ D_i, D_iX_i, D_iX_i^2, D_iX_i^3 \}$ are instrumented for by $\{ T_i, T_iX_i, T_iX_i^2, T_iX_i^3 \}$.

All models are also run with a vector of covariates included in the regression. Controls are offender i's age, sex, her number of prior offenses, and regional dummies for her place of residence. This provides an additional check on the internal validity of my estimates. The covariates are balanced across treatment and control group so that the point estimates should not be affected by the inclusion of control variables. We will see in the next section that this is indeed the case.

5 Results

The reduced form results of Table 2 yield the intent-to-treat (ITT) effect, that is the effect of assignment to treatment. Offenders who committed their treatment transgression within 365 days after the penalty for the original transgression had obtained legal force, are about three percentage points less likely to recidivate within 12 months than offenders who do not fall under the 365-day rule. This finding is robust to the inclusion of covariates.

[8] As mentioned above, the evaluated recidivism period is extended to 13 months for those who actually receive the treatment in order to account for incapacitation effects as drivers without a license naturally have less of an opportunity to reoffend.

Changes to the functional form also have no effect on the point estimates, but result in small losses of precision. The average rate of recidivism is 25.4 percent, so these coefficients translate into a decrease in the rate of recidivism of about 12 percent. This result is also consistent with a visual analysis of Figure 2 which illustrates the ITT and would suggest an effect size of a similar magnitude.[9]

Second stage instrumental variable estimates for the effect of a 1-month license suspension on the probability of recidivating within 12 months are reported in Table 3. The linear model without any controls suggests that a 1-month license suspension reduces the probability of committing a major traffic transgression within the next year by 5 percentage points. Adding covariate controls does not alter this point estimate substantially. A model containing an additional quadratic term of the running variable and its interaction with the suspension indicator comes to virtually the same result. The coefficient is -.052 with a standard error of .021, and it is also robust to the inclusion of covariates. A cubic model yields similar results with a coefficient of -.057 and a standard error of .024. Given a mean recidivism rate of 25.4 percent, these coefficients translate into reductions in recidivating behavior by 19 to 22 percent.

Columns (3) through (10) assess the robustness of my results to picking an ever smaller time window around the 365-day cutoff. For instance, column (3) focuses on offenders who committed their treatment transgression between 276 and 455 days after the penalties for their original transgressions had obtained legal force. In essence, this creates a 90-day window to both sides of the 365-day cutoff. It is comforting to see that the point estimates remain very stable. If we further zoom in on the cutoff, the point estimates continue to hover around -.05. Not surprisingly, the standard errors inflate substantially as would be expected since the sample size shrinks with an ever closer window around the cutoff. Wooldridge (2009) also points out that there are more than just efficiency costs to limiting the sample to observations just around the cutoff. His simulations show that this might substantially bias the coefficient of interest. Therefore, the specification that uses the full sample (column (2)) is

[9] The reduced form results for speeding-specific recidivism are very similar in magnitude and are available from the author upon request.

the preferred specification.[10]

The results are also robust to different functional forms. Both quadratic and cubic specifications yield point estimates that are very similar to those of the linear specification. If we discount the findings from columns (9) and (10) which will likely suffer from both consistency and efficiency issues, the coefficient range stretches from -0.029 to -0.071 and is thus hovering around the preferred -.048 estimate yielded by the linear specification using covariates and the full discontinuity sample.[11] The second stage results are also strikingly different from what a "naive" OLS regression would imply. Appendix Table A3 shows that such an analysis would suggest a *positive* relationship between punishment and recidivism even after accounting for observable driver characteristics such as age, sex and state fixed-effects. In other words, the difference between correlations yielded by a naive OLS model and the plausibly causal effects of a regression discontinuity design turns out to be very substantial and illustrates the value added by design-based studies.

Finally, I also experiment with nonparametric local linear regression which assigns more weight to observations close to the threshold and sidesteps functional form issues. However, these advantages come at the cost of a loss of precision and should be viewed with some scrutiny when applied to a discrete running variable as in this study (Lee and Card, 2008). It is nonetheless comforting that Table 4 demonstrates that a nonparametric approach suggests that a 1-month license suspension reduces recidivism by 4.6 percentage points, an estimate that is virtually identical to the one yielded of my preferred parametric specification in column (2) of Table 3. This result is robust to different bandwidth selections. In fact, the two most popular algorithms by Imbens and Kalyanaraman (2012) and Calonico et al. (2014) respectively yield identical point estimates with differences in the number of observations used explaining differences in precision.[12]

[10]The results for speeding-specific recidivism are presented in Table A2 of the Appendix. They are similar in magnitude but - not surprisingly - less precise and therefore not quite as robust to changes in specification.

[11]I also conducted a Chi-Squared (Wald) version of the formal specification test suggested by Lee and Lemieux (2010). For this test, a set of dummies for 3-day bins of the running variable is added to the 2SLS regression. The p-values for a test for joint significance of these variables are reported below each coefficient in Table 3 and suggest that a linear version should be preferred not least for efficiency reasons.

[12]Appendix Figure A6a further tests for sensitivity with respect to bandwidth choice by plotting point estimates and 95% confidence intervals over a wide range of possible bandwidths. The point estimates all hover around -.05 and most of them are statistically significant at the 5% level. Similarly the point estimates for speeding-specific recidivism all hover around -.03 (see Figure A6b).

6 Mechanisms: Specific or General Deterrence

The key finding of this paper is that temporary driver's license suspensions substantially reduce recidivism. Yet, it is important to also pin down the mechanism through which punishment works. Criminologists broadly distinguish between specific and general deterrence. Nagin (2013) defines general deterrence as the effect of the "threat of punishment [that] may discourage criminal acts." Specific deterrence, on the other hand, refers to the impact of the actual experience of the punishment.

Moreover, in the context of this paper's identification strategy, simply learning about the 365-day rule might be another mechanism that could plausibly drive the results, but would limit their external validity. I vary the size of the recidivism time window to shed some light on this channel. The 2-year expungement period, the lag between transgression date and data entry, and the fact that treated offenders have 4 months to turn in their licenses limit the range of recidivism windows that can reasonably be evaluated to 6-15 months. In each case, the recidivism window for the treated is extended by an additional month to account for incapacitation effects. Figure 5 shows means-adjusted coefficients and confidence intervals for recidivism windows ranging from six to fifteen months. All results are obtained from a set of linear parametric regressions using covariates. The effect of a 1-month license suspension is statistically significant, negative, and notably stable over time although the point estimates are slightly larger for longer time windows. By and large, Figure 5 indicates that a 1-month license suspension reduces both short-run and long-run rates of recidivism by about 20 percent. This is not due to a short-run incapacitation effect but rather suggests that offenders are permanently deterred from committing traffic transgressions.[13]

This similarity and the general persistence of the effect indicate that the reduction in recidivism is not driven by the fact that the treatment group - as a result of being punished under the 365-day rule - simply becomes better informed about the peculiarities of the 365-day rule. Figure 6 provides additional evidence against this mechanism. The day count starts all over again for offenders who had their license suspended due to falling under the 365-day rule. In other words, these offenders are at risk of falling under the 365-day rule

[13]Similarly, Figure A7 suggests an effect range similar to the main results for speeding-specific recidivism.

for a second time. At the same time, these offenders should now be acutely aware of the specifics of the rule. If learning about the rule was a major driver of the observed reduction in recidivism, one would expect bunching to the right of the 365-day threshold between the time at which the penalty for the treatment transgression obtained legal force and the third transgression. Yet, this does not appear to be the case. McCrary's (2008) and Frandsen's (2017) formal tests confirm that there is indeed no evidence for manipulation around the cutoff.[14]

The case for general deterrence is also unconvincing. A license-suspension under the 365-day rule is a one-off punishment and has no legal implications for the stiffness of future penalties. In other words, the legal status of those just to the right of the cut-off is no different from the legal status of those to the left. Offenders in both groups have received the same penalty in terms of demerit points and money fines. Offenders in both groups will receive identical penalties for future transgressions. Furthermore, the institutional setting in which transgressions are recorded and speeding tickets are issued is such that it prevents police and traffic officials from giving preferential treatment to "unlucky" offenders who happened to land just to the left of the 365-days cut-off and went on to commit another traffic transgression. When enforcing speed limits, police officers generally do not pull over speeders but instead use sophisticated measuring equipment that records a driver's speed and takes a picture of both the driver and the license plate. This information is then automatically transmitted to the responsible authorities, matched to an offender's file, and a ticket is issued. Put differently, in the eyes of the law and law enforcement, no distinction between the treated and untreated can be made.[15]

All signs, therefore, point towards a specific deterrence effect, in which offenders are backward looking and become less likely to recidivate because they experienced the punishment. Table 5 provides some evidence for this hypothesis. It distinguishes between three groups of offenders: the treated who fell under the 365-day rule and indeed had their license suspended for 1 month; non-compliers who also fell under the rule but got to keep their

[14]The covariates are also balanced around the cutoff, a corresponding tabulation is available from the author upon request.

[15]Neither are insurance premiums differentially affected across treatment and control groups as German data protection laws prohibit the KBA from transmitting detailed driving history information to insurance companies.

license; and the untreated who committed their second treatment transgression more than 365 days after the penalty for the original transgression had obtained legal force and hence also kept their license. The treated experienced both the punishment and learn about the 365-day rule. The non-compliers, on the other hand do not experience the punishment, but since they had to wriggle their way out of the license suspension, they are just as well informed about the 365-day rule as the treated. In other words, the statistically significant difference in recidivism, shown in the first row of Panel A in Table 5 cannot be explained by an information advantage. It should, however, be noted that this comparison no longer takes place in the realm of a randomized setting, as the non-compliers are a self-selected group. Nonetheless, descriptive statistics for treated and non-compliers are reasonably similar. For example, Panel A shows no statistically significant differences in the number of prior transgressions in either group. There is, however, a very substantial and statistically significant difference in the rate of recidivism which is 23.8 percent for the treated and 26.1 for non-compliers. In this the non-compliers also very much resemble the untreated who exhibit a recidivism rate of 26.4 percent.[16]

Panel B presents another piece of evidence in favour of a specific deterrence mechanism. It lists the fractions of different types of (third) speeding offenses that were committed by each group within 365 days after the penalty for the treatment transgression obtained legal force. Remember, that the 365-day rule only applies if speed limits are exceeded by 26-40 km/h (26-30km/h for built-up areas), but not if the limit is exceeded by less than that. For violations of the limit by more than 40km/h, license suspensions are issued regardless of the timing of the offense. Hence, if learning about the 365-day rule was driving the results, one would expect the treated and non-compliers to less frequently commit transgressions in the 26-40km/h range than the (presumably ignorant) untreated and to bunch in the 21-25 km/h range. Yet, there is no statistically significant difference in the fraction of third offenses in the 26-40km/h range across these three groups.

[16] Another specific deterrence mechanism might operate through enhanced penalties for driving without license. Such an offense would be flagged in the data and indeed none of the treated offenders in my sample are caught engaging in this behavior in their incapacitation month.

7 Conclusion

The impact of punishment on future criminal behavior has always been very hard to measure. Heavy penalties are usually only handed out to offenders who have committed serious crimes. Their recidivism rates are higher to begin with and the effect of punishment cannot easily be distinguished from the effect of unobservable characteristics of these offenders such as a lower risk aversion or self-control issues. This article has exploited a special provision in the German traffic law that results in a quasi-random assignment of 1-month driver's license suspensions to some traffic offenders but not to others. Using a fuzzy regression discontinuity design, I find that receiving the punishment reduces the probability of committing another offense within a year by about 20 percent. This effect is unlikely to be explained by information asymmetries or incapacitation. It indicates that traffic offenders are to some extent backward-looking and react to the punishment itself rather than to the mere threat of future punishment. In other words, temporary license suspensions appear have a large specific deterrent effect.

This, of course, is a "local" effect in two ways. First, it is local in the sense that the estimated effect is best interpreted as the effect on offenders with values of the running variable close to the 365-day cutoff. Yet, as the summary statistics in Table 1 show, there are few observable differences between offenders close and further away from the cutoff which leaves some room for generalizations. Second, a fuzzy RD design is implemented using instrumental variable analysis. As a result, I obtain a Local Average Treatment Effect (LATE), that is the effect for compliers. This is the group of people who only because they fell under the 365-day rule, had their license suspended for one month but would not have had their license suspended otherwise. The 365-day rule by its very nature only applies to repeat offenders who have committed two fairly serious speeding offenses. A 1-month license suspension may affect other types of offenders, for example first-time offenders, in a different way. Indeed, compared to the population of one-time offenders, my experimental sample is on average about 2 years younger and contains about twice the proportion of men.[17] The population of habitual offenders is, however, clearly the population that is the most interesting to policy

[17] A detailed table of summary statistics that compares the observable characteristics of one-time offenders and repeat-offenders is available from the author upon request.

makers so that my results might very well be seen as a case of "sometimes-you-get-what-you-want" (Angrist and Pischke, 2008).

The results of this article also have implications for the economic theory of crime. Driver's license suspensions due to the 365-day rule have no effect on the degree of penalties for future transgressions. As a result, they should not have an effect on the cost-benefit trade-off that rational criminals face in Becker's (1968) framework. Yet, this study provides compelling evidence that receiving such a penalty alters future behavior. My findings are thus more indicative of a backward-looking criminal who changes his ways due to a punishment he has already received. This finding is consistent with recent evidence on drunk driving (Hansen, 2015) which suggests that bounded rationality might predispose traffic offenders to being responsive to penalties and that much of this response operates through the specific deterrence channel. My study confirms this finding and shows that first-hand experience of penalties in general and license suspensions in particular are powerful tools for crime prevention. As such, these results are of great interest to policy makers who, as of now, have little reliable evidence regarding the effectiveness of penalties. This study suggests that taking offenders who were speeding, texting while driving, or drunk driving off the streets for a short period of time not only incapacitates them but has lasting effects on their driving behavior.[18] As a result, tweaks to penalty catalogues towards a more frequent imposition of temporary license suspensions are likely to offer large benefits in terms of avoided crashes, fatalities, and medical costs.

[18]A back of the envelope calculation suggests that starting to impose 1-month license suspensions for all speeding offenses in the 21-40km/h range, would cut the number of offenses by 160,000 through the specific deterrence channel alone.

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Tables and Figures

Table 1: Summary Statistics

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	Full Sample		±90 days		±60 days		±30 days		±10 days	
	[186-365]	[366-545]	[276-365]	[366-455]	[306-365]	[366-425]	[336-365]	[366-395]	[356-365]	[366-375]
Time	274.46**	451.69**	319.54**	409.44**	334.80**	395.02**	350.42**	380.59**	360.43**	370.53**
	(51.48)	(52.28)	(25.84)	(25.95)	(17.32)	(17.19)	(8.650)	(8.691)	(2.964)	(2.813)
Age	42.66	43.02	42.88	42.93	42.78	42.85	42.84	42.88	42.85	42.58
	(13.12)	(12.93)	(13.15)	(12.94)	(13.15)	(13.04)	(13.01)	(13.33)	(12.91)	(13.42)
Female	0.162**	0.148**	0.167**	0.144**	0.167	0.146	0.158	0.148	0.150	0.155
	(0.368)	(0.355)	(0.373)	(0.351)	(0.373)	(0.353)	(0.365)	(0.356)	(0.358)	(0.362)
Priors	2.014	2.004	2.000	2.018	1.986	2.019	1.995	1.993	1.947	2.003
	(1.224)	(1.183)	(1.194)	(1.207)	(1.185)	(1.214)	(1.208)	(1.223)	(1.185)	(1.225)
South	0.227	0.230	0.231	0.235	0.227	0.237	0.217	0.237	0.220	0.241
	(0.419)	(0.421)	(0.422)	(0.424)	(0.419)	(0.425)	(0.413)	(0.425)	(0.415)	(0.428)
North	0.208	0.209	0.208	0.207	0.212	0.208	0.222	0.208	0.190	0.212
	(0.406)	(0.406)	(0.406)	(0.405)	(0.409)	(0.406)	(0.415)	(0.406)	(0.392)	(0.409)
East	0.179	0.169	0.178	0.166	0.176	0.168	0.170	0.170	0.168	0.170
	(0.383)	(0.375)	(0.383)	(0.372)	(0.381)	(0.374)	(0.376)	(0.375)	(0.374)	(0.376)
West	0.366	0.372	0.363	0.371	0.366	0.367	0.370	0.364	0.399	0.364
	(0.482)	(0.483)	(0.481)	(0.483)	(0.482)	(0.482)	(0.483)	(0.481)	(0.490)	(0.481)
Foreign	0.0200	0.0201	0.0191	0.0201	0.0183**	0.0194**	0.0217	0.0209	0.0230	0.0132
	(0.140)	(0.140)	(0.137)	(0.140)	(0.134)	(0.138)	(0.146)	(0.143)	(0.150)	(0.114)
Recidivism	0.244**	0.265**	0.241**	0.268**	0.240**	0.267**	0.229**	0.265**	0.232	0.255
(Any)	(0.430)	(0.442)	(0.428)	(0.443)	(0.427)	(0.442)	(0.420)	(0.442)	(0.423)	(0.437)
Recidivism	0.175**	0.193**	0.174**	0.195**	0.178**	0.194**	0.175	0.195	0.182	0.182
(Speeding)	(0.380)	(0.395)	(0.380)	(0.396)	(0.383)	(0.396)	(0.381)	(0.397)	(0.387)	(0.388)
Observations	16,876	14,524	8,321	7,771	5,464	5,310	2,621	2,730	869	905

Notes: This is a table of means with standard deviations in parentheses. Columns (1) and (2) show descriptive statistics for the full analysis sample. In columns (3) and (4) descriptive statistics are listed for observations which have values in the running variable that fall into a 90 day window on either side of the 365-day cutoff. Columns (5) to (10) show means and standard deviations for observations with running variable values that are ever closer to the cutoff; **denotes a statistically significant difference in means at the 1% level.

Table 2: Reduced Form Regression Results

	(1)	(2)	(3)	(4)	(5)	(6)
Mean Rate of Recidivism (Any)			0.254			
(SD)			(0.183)			
Below Cutoff	-0.033^{**}	-0.031^{**}	-0.033^{**}	-0.031^{*}	-0.032^{*}	-0.032^{*}
	(0.009)	(0.009)	(0.012)	(0.012)	(0.014)	(0.014)
Observations	31,400	31,383	31,400	31,383	31,400	31,383
Convariates	No	Yes	No	Yes	No	Yes
Model	Linear	Linear	Quadratic	Quadratic	Cubic	Cubic

Notes: $^{+}$ / $*$ / $**$ indicate significance at the 1%/5%/10%-level.
Heteroscedasticity robust standard errors in parentheses, clustered by distinct values of the running variable.
Each column reports coefficients and standard errors from the reduced form OLS regression. Dependent variable is a dummy indicating whether a person commits any traffic offense within a year of her (second) treatment transgression. "Below Cutoff" is the main explanatory variable and is a dummy equal to one if the treatment transgression occurred within 365 days of the day on which the penalty for the original transgression had obtained legal force. Such a person is very likely to have her license suspended for 1 month.

Table 3: Treatment Effects by Specification and Time Window - Recidivism (Any)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	Full Sample		±90 days		±60 days		±30 days		±10 days	
Mean Rate of Recidivism	0.254		0.254		0.254		0.247		0.244	
(SD)	(0.183)		(0.184)		(0.187)		(0.186)		(0.183)	
Linear	-0.051^{**}	-0.048^{**}	-0.050^{**}	-0.048^{*}	-0.059^{**}	-0.059^{**}	-0.052^{+}	-0.053^{*}	−0.078	-0.082
	(0.014)	(0.014)	(0.019)	(0.019)	(0.021)	(0.021)	(0.027)	(0.026)	(0.060)	(0.054)
p-Value Wald Test:	0.9159	0.9169	0.9859	0.9785	0.9417	0.9377	0.3196	0.3725	0.8379	0.7889
Squared	-0.052^{*}	-0.050^{*}	-0.071^{**}	-0.073^{**}	-0.069^{*}	-0.071^{*}	−0.055	−0.052	−0.092	-0.084
	(0.021)	(0.020)	(0.026)	(0.025)	(0.030)	(0.029)	(0.052)	(0.049)	(0.079)	(0.075)
p-Value Wald Test:	0.9934	0.9895	0.9996	0.9992	0.9519	0.9479	0.3301	0.3665	0.8733	0.8612
Cubic	-0.057^{*}	-0.057^{*}	−0.047	−0.049	−0.053	−0.050	−0.037	−0.029	0.204	0.265
	(0.024)	(0.023)	(0.036)	(0.034)	(0.034)	(0.033)	(0.085)	(0.074)	(0.475)	(0.603)
p-Value Wald Test:	0.9962	0.9977	0.9836	1.000	0.9854	0.9879	0.7882	0.8149	0.9865	0.9998
Convariates	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
Observations	31,400	31,383	16,092	16,086	10,774	10,769	5,351	5,347	1,774	1,774

Notes: $^{+}/*/**$ indicate significance at the 1%/5%/10%-level.
Heteroscedasticity robust standard errors in parentheses, clustered by distinct values of the running variable.
Each column reports coefficients and standard errors from the second stage of a 2SLS instrumental variable regression. The outcome variable is a dummy equal to one if a person commits another major traffic transgression within 1 year after her treatment transgression. Coefficients are displayed for the main explanatory variable, a dummy indicating a 1-month license suspension following the treatment transgression. This dummy was instrumented for using a variable for whether the treatment transgression occurred within 365 days of the day on which the penalty for the original transgression had obtained legal force. In columns (1) and (2), the entire discontinuity sample enters the analysis. In columns (3) and (4) only observations with values of the running variable within a 90-day time-window on either side of the 365-day cutoff were used. Columns (5) through (10) further limit the sample. If indicated, controls for sex, age, the number of prior offenses and a set of dummies for the region of residence were included.
The reported p-values correspond to Wald specification tests. Small p-values indicate that a model is not appropriate and higher order polynomials of the running variable should be added to the regression equation.

Table 4: Non-Parametric Regression Results

	(1)	(2)
1-Month Suspension	-0.046	-0.046^{*}
	(0.029)	(0.019)
Observations Used	13,662	26,714
Selection Algorithm	CCT	IK
Bandwidth	76.44	152.2

Notes: $^{+}/*/**$ indicate significance at the 1%/5%/10%-level. Heteroscedasticity robust standard errors in parentheses, clustered by distinct values of the running variable.
Each column reports coefficients and standard errors from a local linear regression discontinuity (RD) model. A triangular Kernel function was used to construct the estimator. Coefficients yield the effect of a one month license suspension following the treatment transgression on the probability of committing another major traffic transgression within 1 year. Bandwidth was selected using algorithms developed by Calonico et al. (2014) (CCT) and Imbens and Kalyanaraman (2012) (IK), respectively.

Table 5: Descriptive Statistics by Treatment Status

	(1)	(2)	(3)
Panel A: Recidivism Rate and Offender Characteristics			
	Treated	Non-Compliers	Untreated
Recidivism (Any)	0.238	0.261	0.264
Recidivism (Speeding)	0.170	0.190	0.193
Age	42.247	43.765	43.013
Female	0.164	0.155	0.149
Num. Priors	2.027	1.977	1.982
South	0.248	0.169	0.227
North	0.201	0.226	0.209
East	0.162	0.225	0.170
West	0.375	0.344	0.374
Foreign	0.014	0.036	0.020
Observations	12,268	4,603	14,276
Panel B: (Third) Speeding Offenses within 365 days			
% <26km/h	0.525	0.493	0.500
% 26-40 km/h	0.384	0.402	0.394
% 40+ km/h	0.091	0.105	0.105
Observations	1,329	637	2,259

Notes: This is a table of means. The treated group consists of offenders who fell to the left of the 365-day cutoff and consequently had their license suspended for 1 month. Non-compliers are offenders who fell to the left of the 365-day cutoff but were able to retain their license. Untreated are offenders who fell to the right of the cutoff and consequently got to keep their license. Panel A shows recidivism rates and covariate values for each group. Panel B shows relative frequency of speeding offenses that were committed within 365 days of the penalty for the second offense obtaining legal force. The category for 26-40km/h excludes speeding offenses in excess of 30 km/h which were committed in built-up areas. These offenses are included in the 40+km/h category. Data source is the digital German traffic registry database as of 30 April 2014.

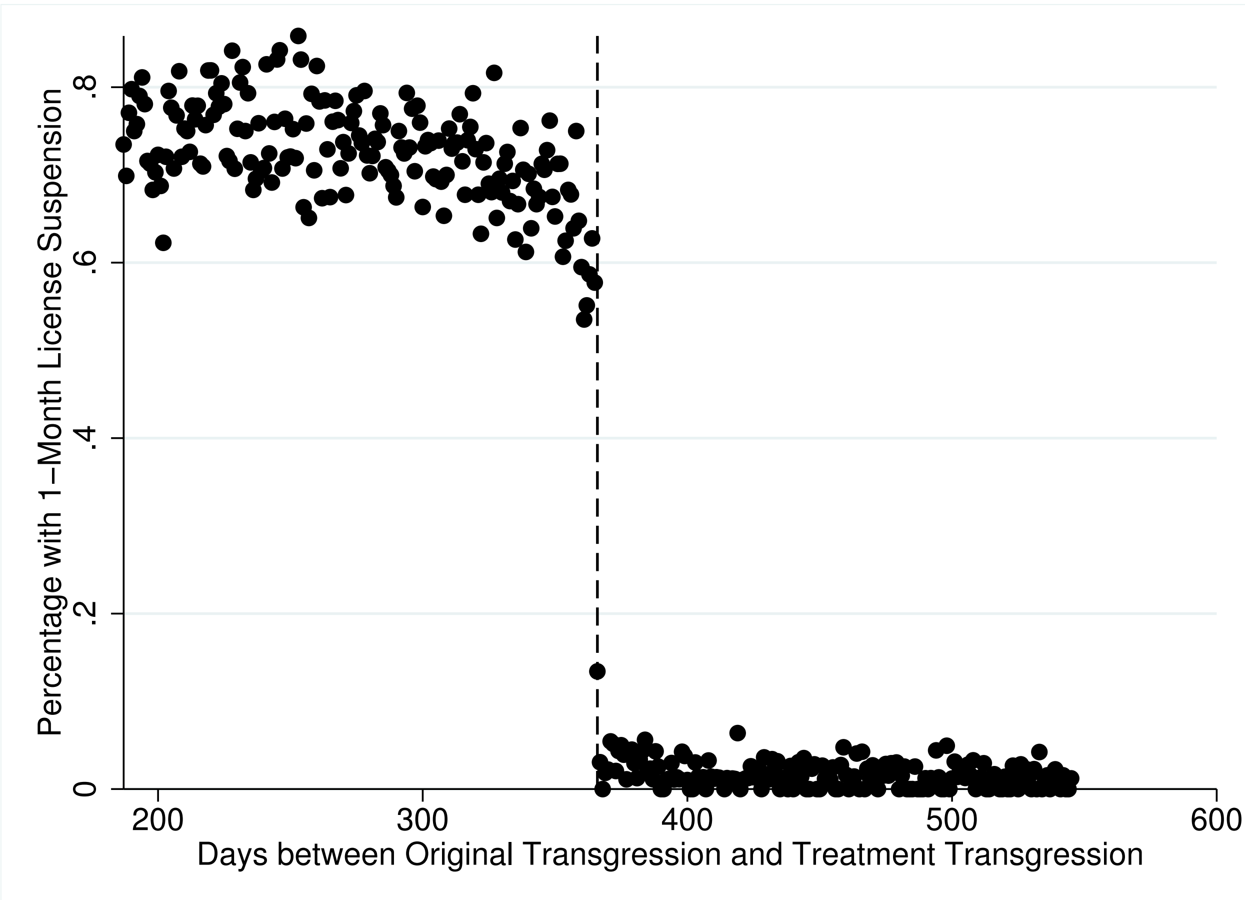


Figure 1: Treatment Probability by Time

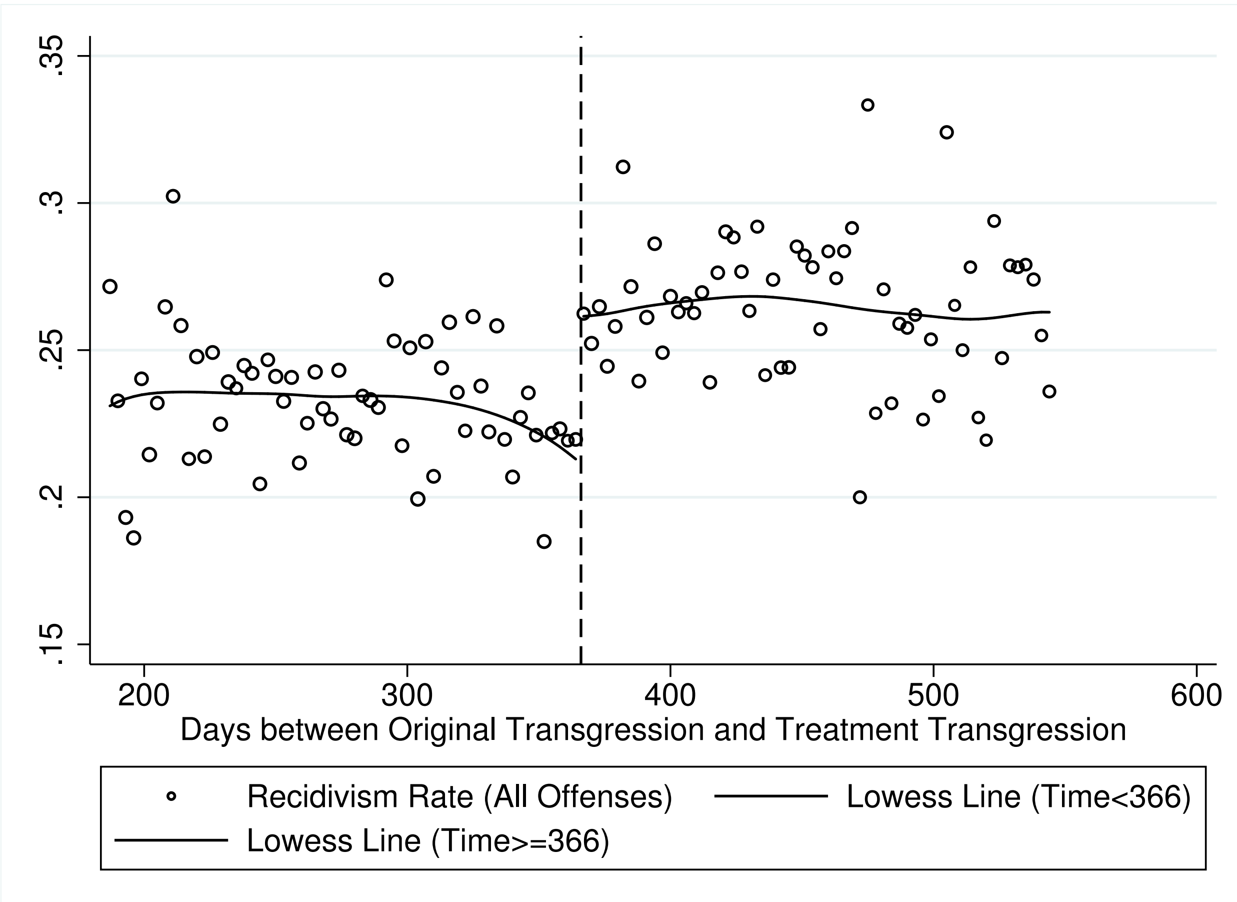


Figure 2: Rate of Recidivism by Time

Figure 3: Distribution of Time until Treatment Transgression

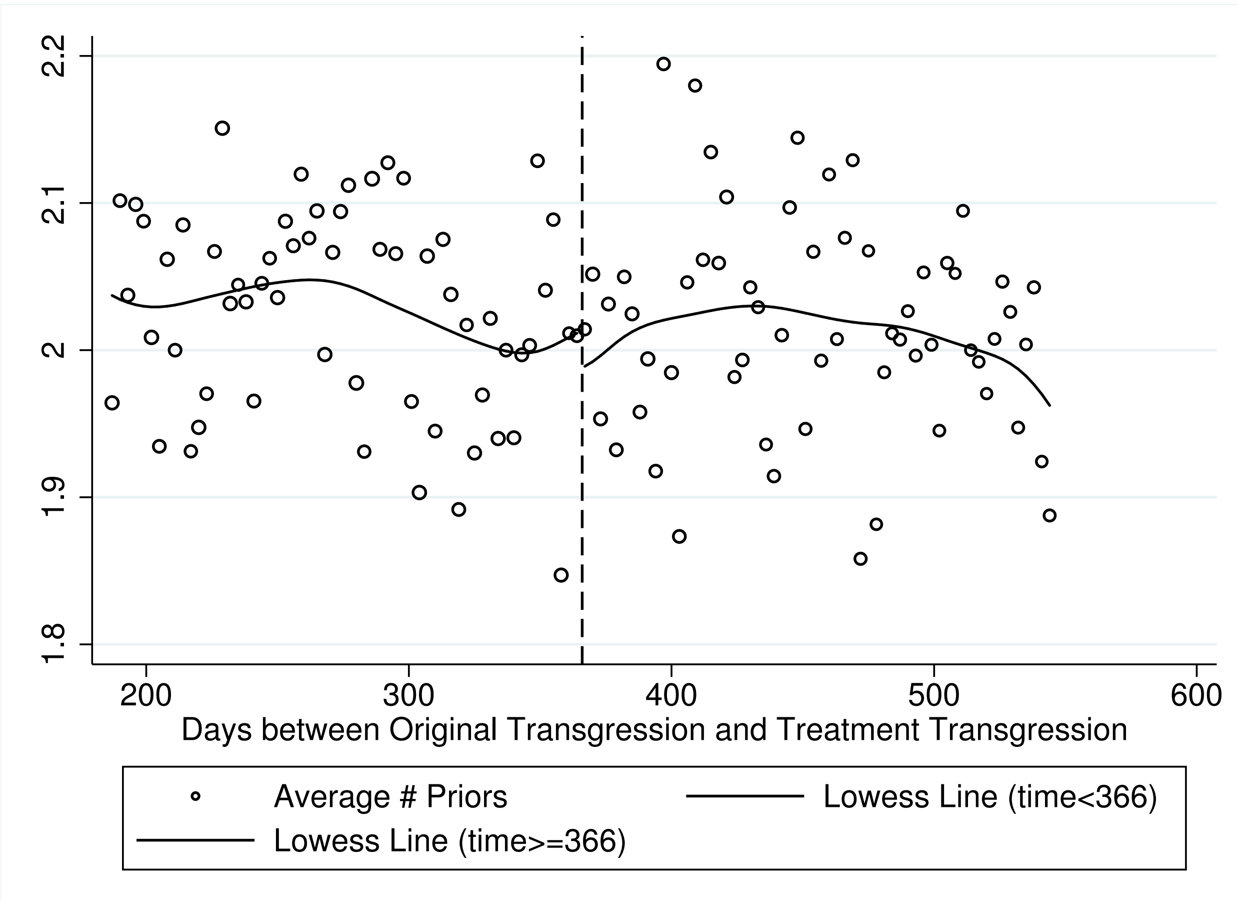


Figure 4: Non-Outcome (Number of Prior Offenses) by Time

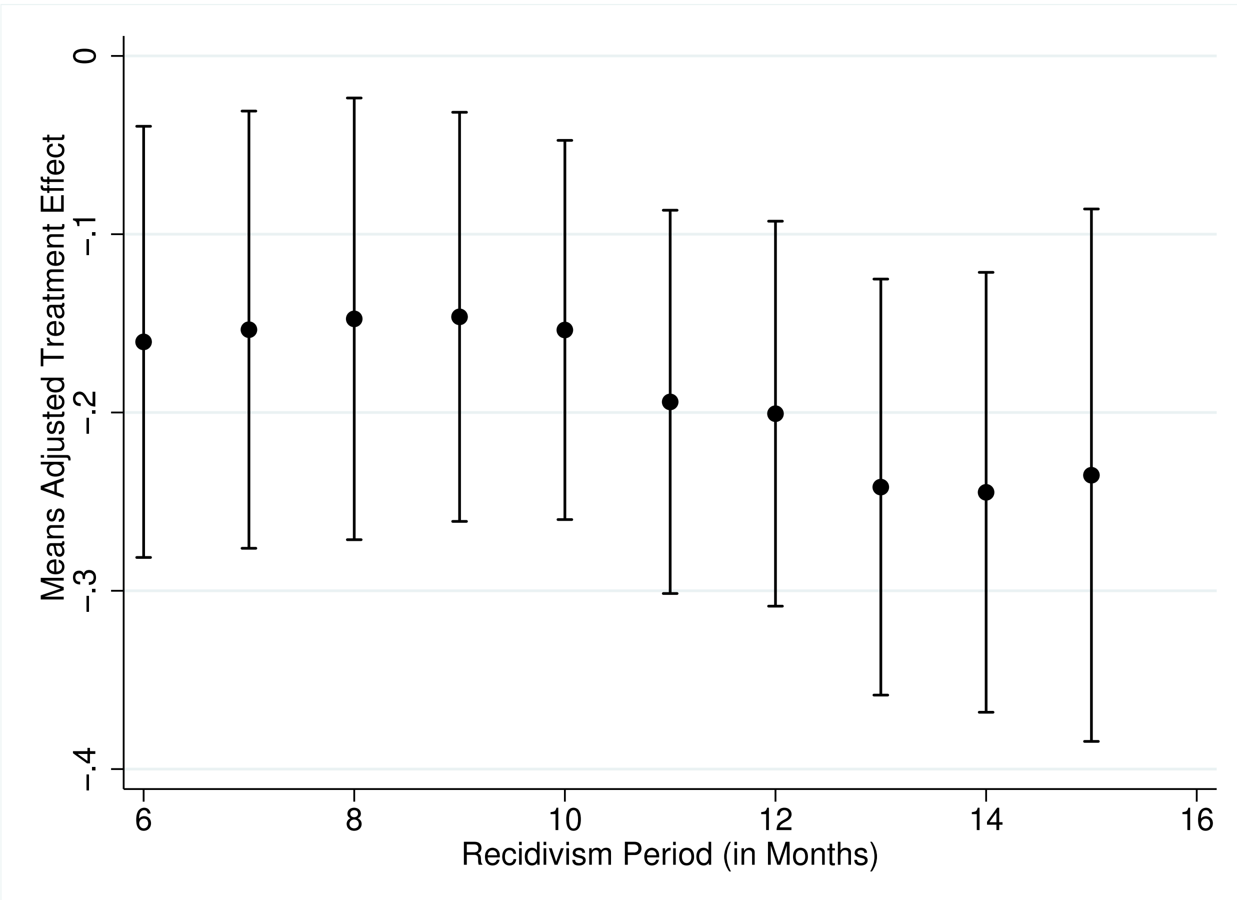


Figure 5: Treatment Effect by Recidivism Time Window

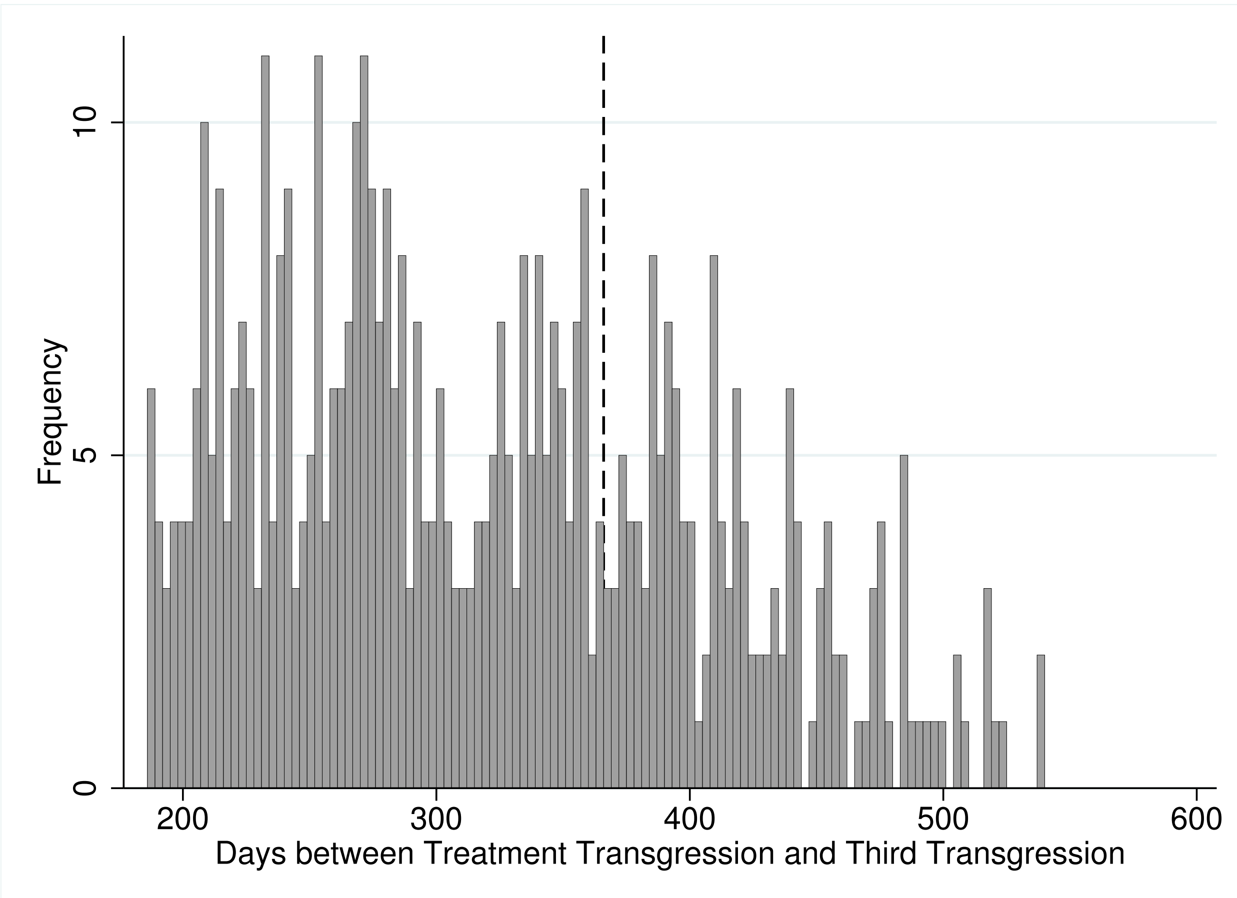


Figure 6: Distribution of Time until Third Transgression

Appendix

Table A1: Excerpt from the German Traffic Penalty Catalogue: Speeding Offenses

	Penalization	
Transgression by:	Highways	Built-Up Areas
$\leq$ 10 km/h	10€	15€
11 - 15 km/h	20€	25€
16 - 20 km/h	30€	35€
21 - 25 km/h	70€, 1P	80€, 1P
26 - 30 km/h	80€, 3P	100€, 3P
31 - 40 km/h	120€, 3P	160€, 3P, 1M
41 - 50 km/h	160€, 3P, 1M	200€, 4P, 1M
51 - 60 km/h	240€, 4P, 1M	280€, 4P, 2M
61 - 70 km/h	440€, 4P, 2M	480€, 4P, 3M
$\geq$ 71 km/h	600€, 4P, 3M	680€, 4P, 3M

Each cell contains information on the penalization for speeding offenses. There are three types of penalties: Fines as measured in Euros (€), central registry points (P), and temporary license suspensions in months (M). A person who has accumulated 18 points will have her license revoked permanently. All points are erased if a person remains without a traffic transgression for 2 years.

Table A2: Treatment Effects by Specification and Time Window - Speeding Recidivism

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	Full Sample		±90 days		±60 days		±30 days		±10 days	
Mean Rate of Recidivism	0.184		0.185		0.187		0.186		0.183	
(SD)	(0.387)		(0.388)		(0.390)		(0.389)		(0.387)	
Linear	−0.031**	−0.030*	−0.019	−0.019	−0.035+	−0.035*	−0.011	−0.011	−0.010	−0.011
	(0.012)	(0.012)	(0.017)	(0.017)	(0.018)	(0.018)	(0.024)	(0.023)	(0.041)	(0.039)
p-Value Wald-Test:	0.8774	0.8470	0.9799	0.9646	0.9551	0.9514	0.4606	0.5001	0.9505	0.9473
Squared	−0.023	−0.022	−0.045*	−0.045*	−0.006	−0.006	0.012	0.012	0.019	0.019
	(0.018)	(0.018)	(0.022)	(0.022)	(0.026)	(0.026)	(0.040)	(0.040)	(0.046)	(0.046)
p-Value Wald-Test:	0.9919	0.9851	0.9986	0.9974	0.9722	0.9696	0.4896	0.5043	0.9652	0.9750
Cubic	−0.019	−0.020	0.019	0.018	0.005	0.008	0.037	0.044	0.052	0.072
	(0.020)	(0.019)	(0.031)	(0.031)	(0.029)	(0.030)	(0.055)	(0.049)	(0.152)	(0.174)
p-Value Wald-Test:	0.9758	0.9756	0.9977	1.000	0.9993	0.9993	0.8224	0.9218	0.9578	0.9544
Convariates	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
Observations	31,400	31,383	16,092	16,086	10,774	10,769	5,351	5,347	1,774	1,774

Notes: $^{+}/*/**$ indicate significance at the 1%/5%/10%-level.
Heteroscedasticity robust standard errors in parentheses, clustered by distinct values of the running variable.
Each column reports coefficients and standard errors from the second stage of a 2SLS instrumental variable regression. The outcome variable is a dummy equal to one if a person commits another major speeding offense within 1 year after her treatment transgression. Coefficients are displayed for the main explanatory variable, a dummy indicating a 1-month license suspension following the treatment transgression. This dummy was instrumented for using a variable for whether the treatment transgression occurred within 365 days of the day on which the penalty for the original transgression had obtained legal force. In columns (1) and (2), the entire discontinuity sample enters the analysis. In columns (3) and (4) only observations with values of the running variable within a 90-day time-window on either side of the 365-day cutoff were used. Columns (5) through (10) further limit the sample. If indicated, controls for sex, age, the number of prior offenses and a set of dummies for the region of residence were included.
The reported p-values correspond to Wald specification tests. Small p-values indicate that a model is not appropriate and higher order polynomials of the running variable should be added to the regression equation.

Table A3: Naive OLS - Effect of License Suspension on Recidivism

	(1)	(2)	(3)
Suspension	0.010^{**}	0.002^{*}	0.003^{**}
	(0.001)	(0.001)	(0.001)
Female		-0.074^{**}	-0.079^{**}
		(0.001)	(0.001)
Age		-0.002^{**}	-0.002^{**}
		(0.000)	(0.000)
Observations	2,170,728	2,170,079	2,170,079
State Dummies	No	No	Yes

Notes: $^{+}$/ $*$ / $**$ indicate significance at the 1%/5%/10%-level. Heteroscedasticity-robust standard errors in parentheses. Each column reports coefficients and standard errors from an OLS regression. The estimates in this table are based on the population of *all* of the about 2 million offenders who committed a major traffic violation in Germany between May 2012 and December 2012, some of which were punished with a temporary license suspension. Dependent variable is a dummy that equals one if a person recidivates within 1 year of an initial traffic transgression. The suspension variable indicates whether a person's license was temporarily suspended after the initial traffic transgression.

Appendix Notes on Generation of Analysis Sample: Figure A1 below illustrates which observations can be used for the analysis. The final analysis sample is comprised of offenders with at least two speeding transgression. One, the so-called “treatment transgression” must have occurred between 1 May 2012 and 1 December 2012 (dashed frame) thus allowing for a sufficient follow-up period. Another transgression, the so called “original transgression,” must have occurred prior to the treatment transgression and the penalty for this original transgression must have obtained legal force between 186 and 545 days before the date of the treatment transgression. Persons whose transgressions over time are illustrated by circles are part of the final sample. Persons whose transgressions over time are illustrated by triangles do not become part of the final sample. Only observations who have their treatment transgression within the dashed frame (1 May 2012 or later, but before 1 December 2012) can be used for the analysis.

Person A at the top of Figure A1 whose transgressions over time are illustrated by triangles has a transgression within the framed time period. But this is her original transgression, not her treatment transgression. Her (second) treatment transgression is outside the dashed frame, as indicated by a second triangle. Moreover, this person reoffends as there is yet another triangle further to the right. I might fail to observe this recidivism event, however, since it takes place after 1 December 2013 and thus may not yet show up in the data. Person A is therefore excluded from my sample. Person B’s treatment transgression, on the other hand, falls into the framed area, giving me a sufficiently large time-window to detect recdivating behavior. In this instance, person B indeed recidivates within 12 months which I observe. Person C is similar and will also be included in my sample. The main difference to person B is that person C does not recidivate. Since I allow for a sufficiently large follow-up period, I can be sure that the lack of another offense for this person is not due to lagged reporting. Finally, person D and everyone else who has their treatment transgression prior to 1 May 2012, by virtue of the expungement period, must necessarily have recidivated. Otherwise, they would no longer be in the sample. That is, there is no variation in the outcome for these observations, leading me to drop person D.

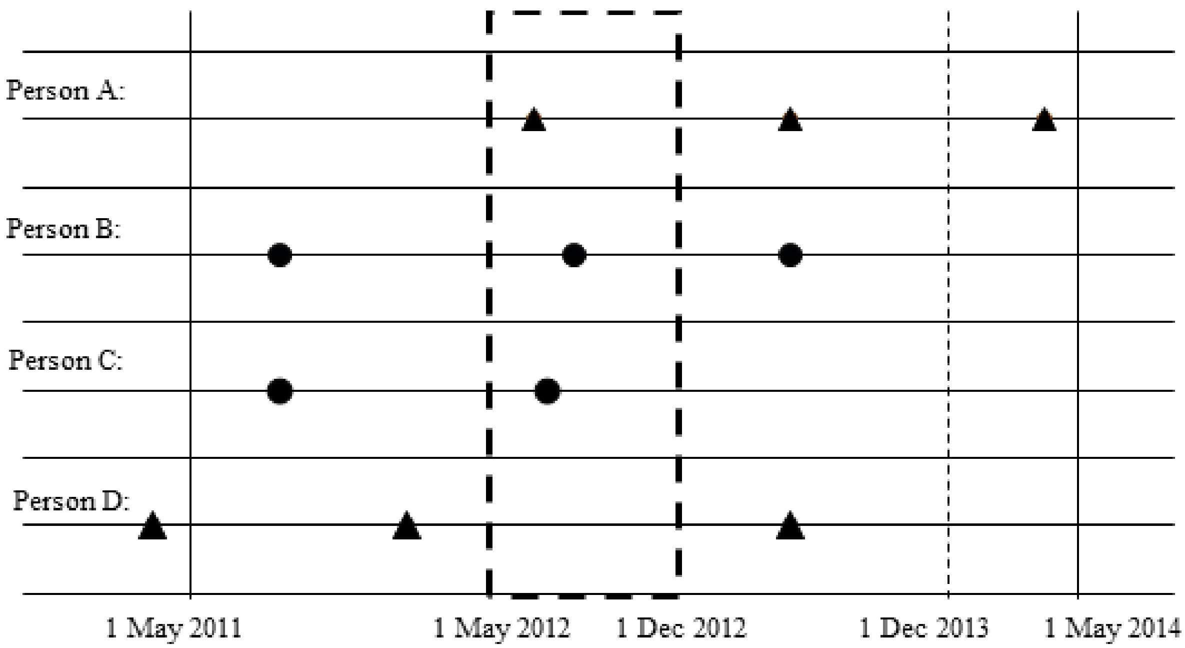


Figure A1: Data Selection from the Central Traffic Registry

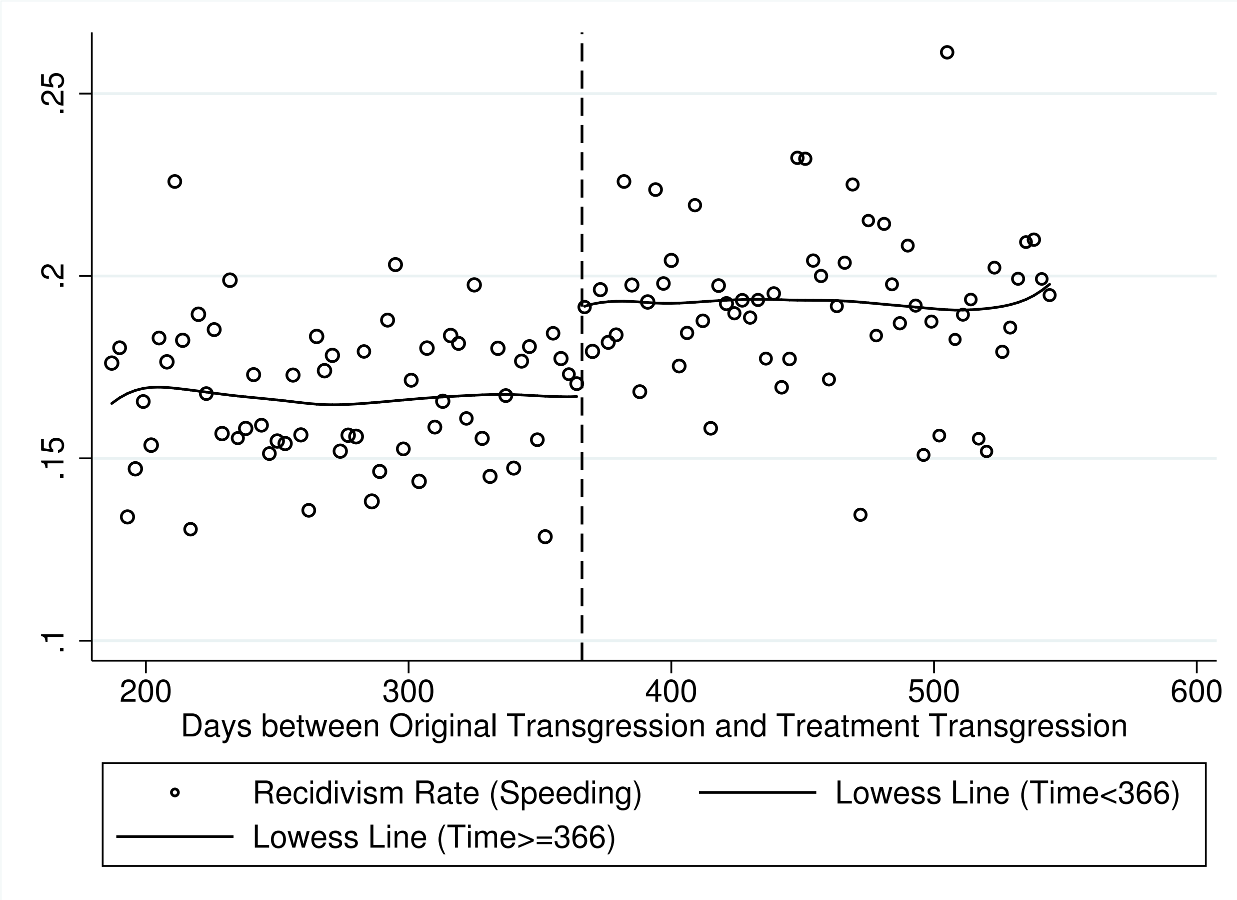
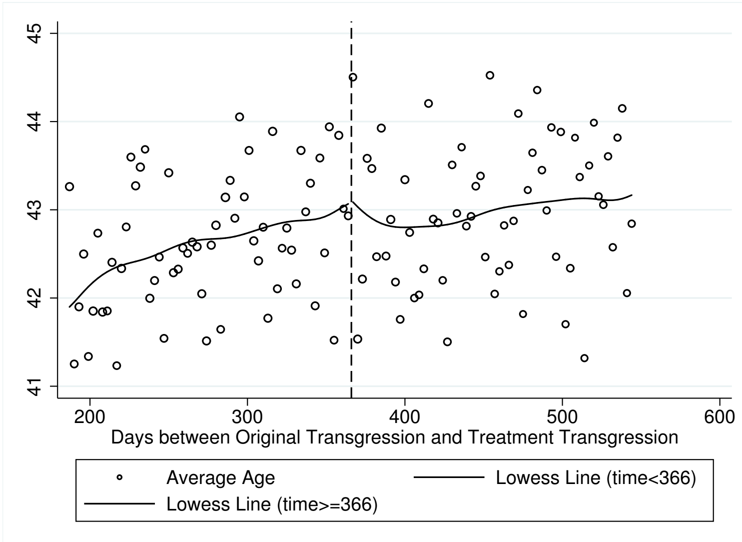


Figure A2: Speeding Recidivism by Time

(a) Average Age



(b) Percentage Female

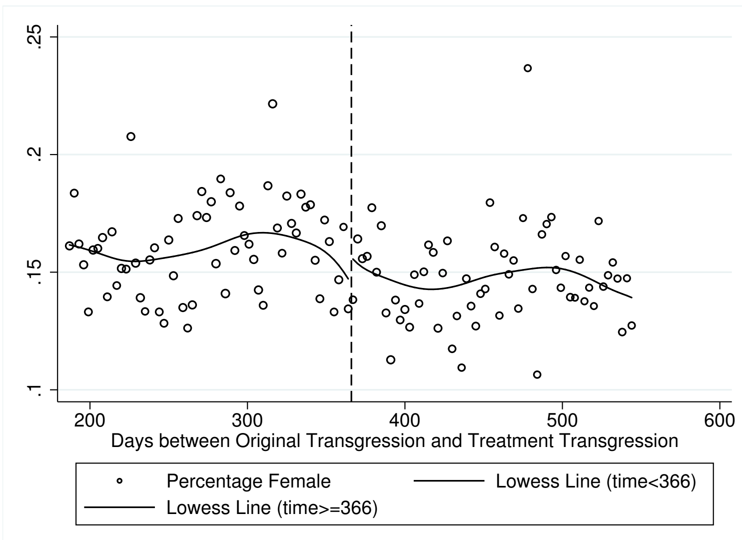
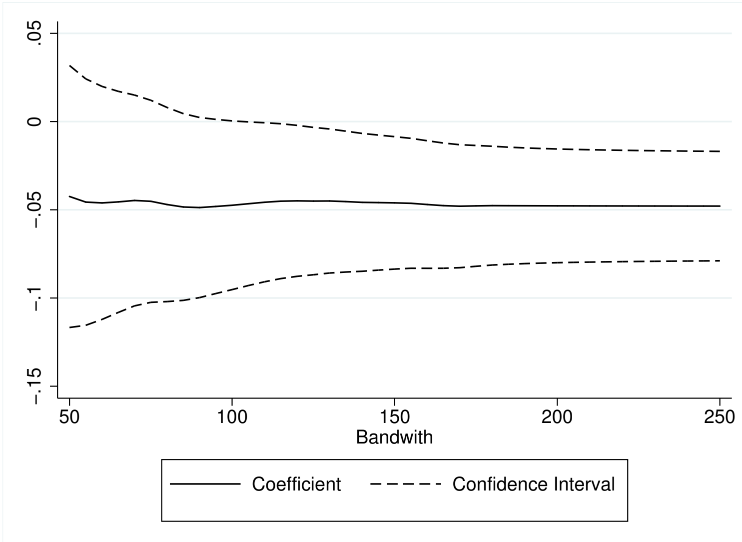


Figure A3: Non-Outcomes by Time

(a) Rate of Recidivism (Any)



(b) Rate of Recidivism (Speeding)

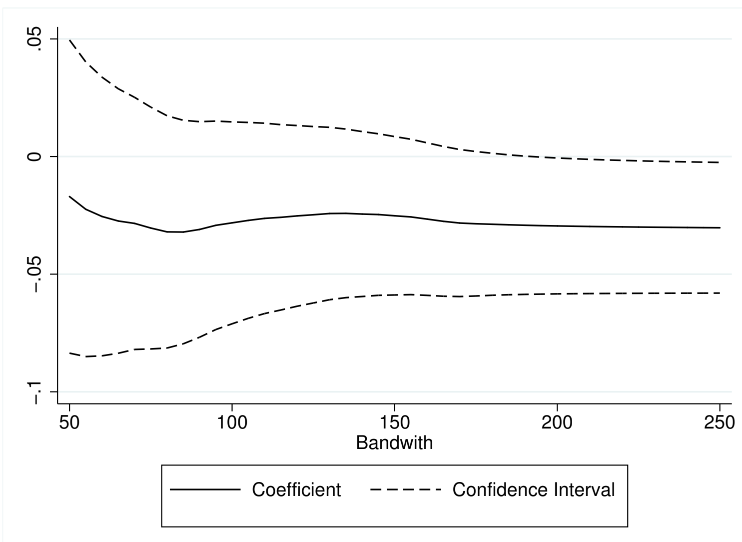


Figure A5: Treatment Effect by Bandwidth

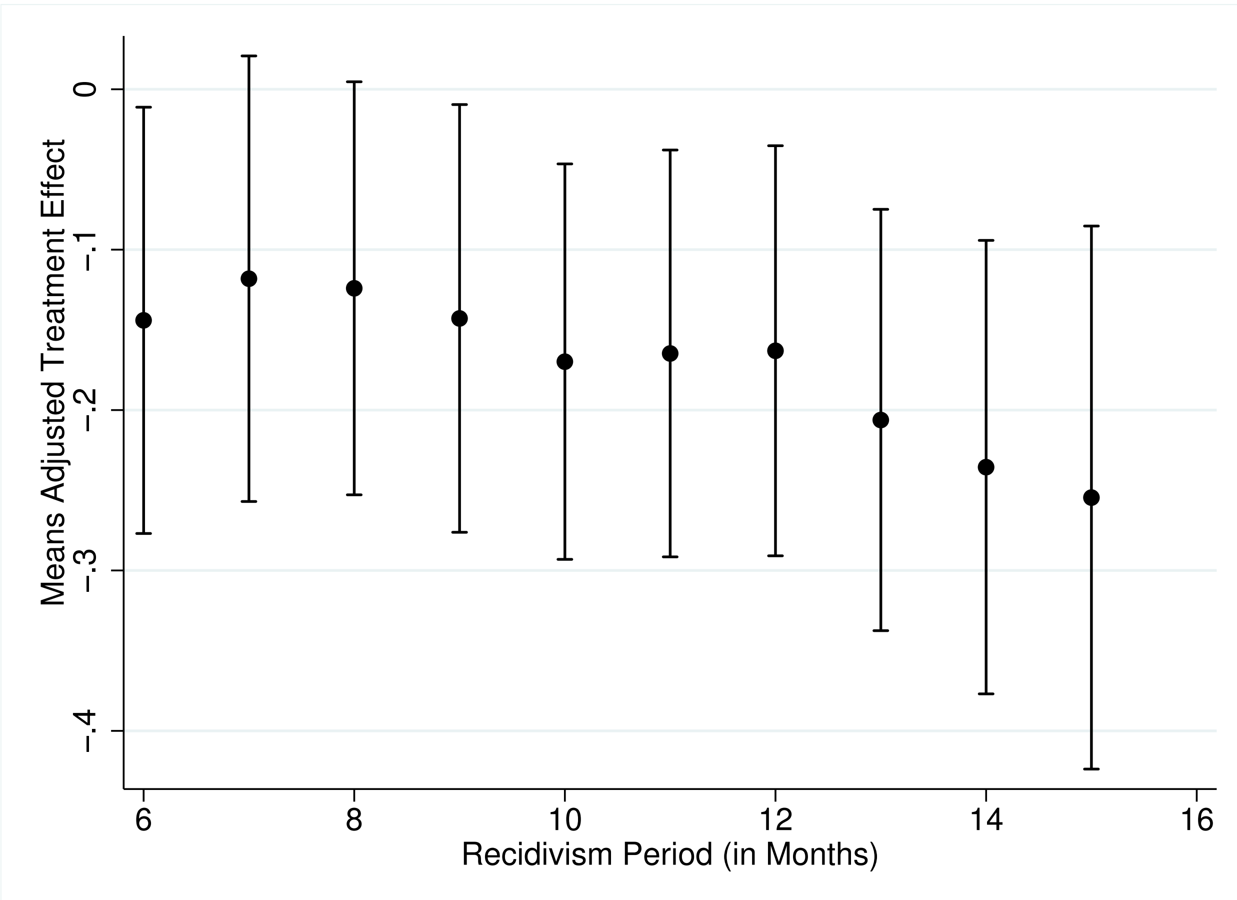


Figure A7: Speeding-Specific Recidivism by Follow-Up Window