How Effective Are Non-monetary Instruments for Safe Driving? Panel Data Evidence on the Effect of the Demerit Point System in Denmark

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Abstract

Using unusually rich longitudinal traffic offense data, this paper exploits a reform that introduced a point recording scheme in Denmark to estimate the behavioral response of drivers to a non-monetary penalty based on demerit points. We find that drivers exhibit substantial behavioral response to each demerit point assigned to their driving license. We also find that drivers' effort, and hence response, increases with the number of demerit points accumulated. Drivers with demerit points reduced their frequency of traffic offenses by 15–30 percent. Similarly, drivers with one or more demerit points reduced their likelihood of committing traffic offense by 11-20 percent. The results also show that those drivers who are potentially more reliant on their car are more responsive.

Keywords: Non-monetary penalties, point recording, demerit points, behavioral response, deterrence, public road safety.

JEL Codes: D12, H76, K42, R41, R48.

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1. Introduction

Driving behavior plays a central role in instigating traffic accidents. Hence, affecting the driving behavior of individuals remains a top priority for policy makers around the globe. Standard economic and deterrence theory predicts that rational drivers will respond to policy instruments (or reforms) that increase the expected cost of committing traffic violations (Becker, 1968; Polinsky and Shavell, 1979). Following this justification, both monetary and non-monetary penalties are commonly used as key instruments to ensure public road safety. The most common monetary instruments involve traffic tickets (fines), whereas driving license revocation based on point recording is a recently emerging non-monetary instrument for safe driving. Many developed countries now use a point recording system for drivers violating traffic rules. In some countries, this point recording scheme is integrated with insurance premiums (see Dionne et al., 2013). Thus, the point recording scheme serves as incentive for safe driving by increasing the expected pecuniary cost in terms of forgone income associated with loss of driving privilege or through indirect costs on insurance premiums and license redemption. Although the implementation of the point recording system varies across countries, it is common that accumulating demerit points above some threshold in some specified period leads to revocation of the driving license. Therefore, the point recording scheme serves the dual purpose of incentivizing car users to drive carefully and incapacitating reckless drivers (Bourgeon and Picard, 2007). Denmark introduced the demerit point system (DPS) in September 2005. The point recording scheme in Denmark applies to different types of traffic violations and three demerit points in three years lead to conditional suspension of the driving license.

While policy instruments such as fines and the point recording scheme have long been used, little is known about the effect of these instruments on inducing safe driving and improving public road safety. This is mainly because most of these instruments are endogenously introduced, a problem that saddles evaluation techniques with simultaneity and reverse causality problems. A few recent studies, including those of Bourgeon and Picard (2007) and Dionne et al. (2011), provide theoretical foundations on the efficacy of the point recording scheme. Bourgeon and Picard (2007) present a theoretical model that demonstrates the effectiveness of the point recording scheme in different scenarios. Dionne et al. (2011) extend Bourgeon and Picard's (2007) model by linking the point recording scheme to insurance pricing, and provide some empirical evidence of moral hazard in public road safety. Despite their innovative empirical approach to test the prevalence of moral hazard in public road safety, Dionne et al. (2011) do not simultaneously capture (or disentangle) unobserved heterogeneity and behavioral response (moral hazard) effects.

This paper uses unusually rich longitudinal traffic offense data for Denmark to estimate the effect of non-monetary penalties on driving behavior. This longitudinal traffic offense data enables us to follow the driving behavior of individuals before and after the introduction of the point recording scheme in Denmark. This study adds at least two novel contributions to the existing literature on punishment and deterrence: (a) we estimate the behavioral response of drivers to a non-monetary penalty based on demerit points using a detailed longitudinal data which enables us to explicitly disentangle behavioral response (moral hazard) and unobserved heterogeneity (selection) effects associated with different groups of drivers; (b) in addition to evaluating the effectiveness of non-monetary incentives for safe driving, this study estimates enormously heterogeneous response of drivers based on varying level of intensity of treatment as well as other observable characteristics of drivers.

Our identification strategy exploits the longitudinal feature of the traffic offense data and uses standard difference-in-differences approach. We compare the driving behavior of individuals subjected to non-monetary penalties through demerit points (treatment group) with those drivers who are only subjected to monetary penalties (control group), before and after Denmark introduced the DPS. We exploit a specific rule introduced by the reform, which assigns speed offenders to one of the two types of penalties based on a 30 percent speed threshold. Accordingly, drivers exceeding the speed limit by more than 30 percent are subject to a demerit point and a fine, whereas those exceeding the speed limit by less than (or equal to) 30 percent are subject to only the fine. We measure individuals' driving behavior using the frequency of traffic violations committed and the probability of committing a traffic offense in some specified period. Our focus on the effect of traffic enforcement on driving behavior, rather than its effect on traffic accidents, provides a sharper picture of the ultimate effects of traffic enforcements. This is particularly appealing given that drivers' violation of traffic laws are the leading causes traffic accidents.

We find that drivers exhibit substantial behavioral response to each demerit point assigned to their driving license by reducing their frequency of traffic offenses and their probability of committing a traffic offense. We also find that drivers' effort, and hence response, increases with the number of demerit points accumulated. Depending on the number of points accumulated in the treatment period, affected drivers reduced their frequency of traffic offenses by 15–30 percent. Similarly, affected drivers reduced their likelihood of committing traffic violations by 11–20 percent. We also show that those drivers who are potentially more reliant on their car are more responsive to each demerit point assigned to their driving license. We find that the treatment effects are strong for those individuals who are self-employed and those who commute longer distances to their workplaces. Finally, while previous studies on the effect of monetary penalties for crimes and traffic offenses suggest that fines may be imposed differentially based on individuals' income and wealth, our results show that non-monetary penalties based on demerit points affect even those with high incomes and wealth. Rather, our results point that the effect of non-monetary penalties may differ based on the expected cost and consequences of these penalties for different groups of drivers. More broadly, our findings assert that non-monetary instruments – penalties that could affect driving privilege – are effective in inducing safe driving. This has some important implications for designing deterrence, public road safety and insurance policies.

We check the robustness of our results considering different specifications and allowing for enormously rich heterogeneity on the effect of the reform. We test the implication of our identifying assumption using a series of empirical exercises. More specifically, our identification strategy assumes that in the absence of the reform, the treatment and control group drivers would have exhibited similar (average) behavioral evolution (changes) in driving behavior. We believe that this assumption holds fairly well for the following reasons: (1) we find that treatment and control group drivers have statistically similar pre-treatment observable characteristics in terms of a large set of relevant variables. (2) Treatment and control group drivers exhibit similar pre-reform driving behavior. (3) Our placebo regression indicates that treatment and control group drivers share a statistically identical pre-reform time trend in driving behavior. We also document that once the demerit points assigned during the treatment period expire, on average, both groups exhibit similar driving behavior.

The rest of the paper proceeds as follows. Section 2 provides a brief review of related literature. Section 3 presents the institutional features associated with the reform and how the point recording scheme works in Denmark. In Section 4, we discuss the data and our sampling design. Section 5 presents our identification strategy and the main results. Section 6 provides concluding remarks and policy implications.

2. Review of Related Literature

Conceptually, this study lies within the general punishment and deterrence literature as we are interested in evaluating drivers' behavioral responses to a relatively severe punishment that could affect their driving privileges. The existing deterrence theory, particularly Becker's (1968) model, predicts that a rational road user will exhibit careful driving behavior if the expected cost of a traffic violation is greater than the net benefit of committing the traffic offense. This implies that drivers will respond to traffic enforcements that affect the expected cost of traffic violations, including reforms that change the amount of penalties for violating traffic rules, or enforcements that affect the probability of detection. However, empirical evidence on how these theoretically founded fines and apprehension probabilities affect driving behavior and hence public road safety is scant. Evaluating the effect of traffic tickets (or other traffic enforcements) on public safety and driving behavior suffers an endogeneity problem that corresponds to estimating the effect of police on crime. That is, greater number of police enforcement is allocated for areas where a higher number of traffic violations (or traffic accidents) are registered.

Despite the aforementioned challenge in estimating the effect of traffic enforcements on driving behavior and public road safety, few studies have used quasi-experimental approaches. Makowsky and Stratmann (2011) use an instrumental variable (IV) approach to circumvent the endogeneity of traffic tickets in estimating the effect of fines on public road safety. They use the financial standing of municipalities as an instrument for the number of traffic tickets issued and find that more tickets lead to fewer traffic accidents. DeAngelo and Hansen (2014) exploit a budget cut that led to police layoff to estimate the effect of police enforcement on traffic fatalities. Hansen (2015) evaluates the deterrence effects of punishments for driving under the influence of alcohol using regression discontinuity designs based on blood alcohol content (BAC) thresholds. Abouk and Adams (2013) investigate the effects of texting bans on traffic accidents in the United States and report short-term effects of the reform.[1]

Another emerging strand of the literature raises doubts concerning whether traffic tickets (or monetary instruments) serve public safety or revenue-generating agenda. Garrett and Wagner (2009) and Makowsky and Stratmann (2009) show that political motives and municipal economic conditions play substantial roles in defining the level of traffic enforcement through traffic tickets. Makowsky and Stratmann (2009) find that police officers discriminately impose traffic fines on non-resident drivers and they indicate that fines for speeding are not only serving public safety

[1] A few other studies evaluate the effect of local reforms and enforcements that change the penalty structure (see Bar-Ilan and Sacerdote, 2004) and speed limits (see Ashenfelter and Greenstone, 2004; Greenstone, 2002). Similarly, some other studies evaluate the effect of other traffic laws, including mandatory seat belt use (see Carpenter and Stehr, 2008; Cohen and Einav, 2003; Levitt and Porter, 2001).

agenda but also revenue generating agenda. Furthermore, the effects of monetary instruments for deterring crime (or traffic offenses) are expected to vary depending on the income and wealth of drivers (see Levitt, 1997; Polinsky, 2006; Polinsky and Shavell, 1991). These claims concerning the effectiveness of monetary penalties justify the need for alternative non-monetary instruments to improve public road safety.

A few recent studies, including those of Bourgeon and Picard (2007) and Dionne et al. (2011), made theoretical analysis on the effectiveness of the recently emerging non-monetary penalty based on a point recording scheme on public road safety. Bourgeon and Picard (2007) build a theoretical model and derive the properties under which the point recording system is effective mechanism to improve the driving behavior of motorists. They model the lifetime expected utility of drivers as a function of driving effort and a stock of demerit points, where accumulation in the latter term enters as a loss in utility. Dionne et al. (2011) extend Bourgeon and Picard's (2007) model by linking the point recording scheme to insurance pricing. Dionne et al. (2011) also provide some empirical evidence of moral hazard. That is, drivers who accumulate more demerit points drive more carefully. However, while Dionne et al.'s (2011) approach provides sufficient evidence of the prevalence of moral hazard or unobserved heterogeneity, it is not appropriate to quantify both of these simultaneously in their cross-sectional setting. Thus, our study is the first empirical attempt to quantify the behavioral response of drivers to a non-monetary penalty based on demerit points using longitudinal data enabling us to explicitly disentangle behavioral response (moral hazard) and unobserved heterogeneity (selection) effects.

3. Institutional Features of the Demerit Point System (DPS) in Denmark

Worldwide, speeding is a major cause of traffic accidents. The US National Highway Traffic Safety Administration reports that speed was a contributing factor in 31 percent of all fatalities registered in the United States in 2007 and estimates the economic cost of these speed-related crashes to be 40.4 billion US dollars each year (NHTSA, 2007). This is not different for Denmark, with speed being a contributory factor in up to 50 percent of all road accidents registered (Danish Road Safety Commission, 2000). Recent surveys show that more than 60 percent of drivers in Denmark admit exceeding the speed limit posted on the road (DaCoTA, 2012).[2] This problem has already gained local attention. For instance, in 2000 the Danish Road Safety Commission (DRSC) set a target of reducing the number of people killed and injured in traffic accidents by 40 percent in the period

[2] DaCoTA (2012) also reports that the average actual driving speeds on different types of Danish roads are above the speed limit.

2001–2012 (DRSC, 2000). The first and key instrument for achieving this target is enforcing increasingly strict speed control measures.

In Denmark, the police enforce speed limit regulations as well as other traffic laws. In doing so, they use automated mobile speed cameras in police vans. The rationale for using mobile speed cameras rather than fixed cameras is motivated by the aim to reduce speeds nation-wide, rather than speeds on specific road networks (Agustsson and Ottesen, 2000). This has important implications for our identification strategy because drivers cannot manipulate local traffic enforcement by the police unlike in the case of fixed cameras installed on some road networks – an approach used in many European countries – where drivers can avoid the networks affected by choosing alternatives. To signal the message that every driver is likely to be caught if driving beyond the speed limit everywhere, there are no signs concerning the location of speed cameras on the roads (Agustsson and Ottesen, 2000). When a driver is caught driving above the speed limit, the speed camera takes a photograph of the car's license plate and the face of the driver. Then, a letter containing the picture and fine is sent to the owner of the car, who may not be the driver who committed the offense. The owner of the car is obliged to disclose the identity of the driver who committed the traffic offense, and the fine is finally imposed on the offender.

As part of the efforts to achieve the aforementioned ambitious target, Denmark introduced a new penalty system, namely the demerit point system (DPS), in September 2005; inter alia, this measure targets drivers exceeding the speed limit beyond a specific threshold. The DPS in Denmark works by assigning a demerit point to a driver's license for each traffic violation committed and three demerit points in three years lead to conditional suspension of the driving license. Therefore, starting from September 1, 2005, speed offenders are subject to two types of punishment: (1) they receive a traffic ticket (fine) if they exceed the speed limit by less than (or equal to) 30 percent; (2) if they exceed the speed limit by more than 30 percent, they receive a fine that is slightly comparable had they exceeded the limit by 30 percent and a demerit point on their driving license. Drivers exceeding the speed limit by more than 60 percent are subject to conditional suspension of their driving license; while exceeding the speed limit by more than 100 percent leads to unconditional suspension of driving privileges for some specified period. The penalties for those drivers exceeding the speed limit by more than 60 or 100 percent predate the DPS reform and have not been changed. Furthermore, reports from the police department indicate that the national traffic code was not affected by the reform and there have not been changes in traffic enforcement associated with the DPS reform.

Importantly, the amount of the fine imposed as well as the assignment of demerit points are independent of any previous traffic violation record. The amount of the fine imposed varies slightly with the actual driving speed, but it is bounded above and below a particular level. For instance, an individual driving at a speed of 65 kilometers per hour (km/h) on a road with a speed limit of 50 km/h would be subject to a fine amounting to 1000 DKK (around 180 US dollars), while driving at 66 km/h on the same road would result in a fine of 1000 DKK and one demerit point.[3] Finally, if a driver accumulates three demerit points in three years, whether assigned for speed violations or other types of violations, his/her driving license is suspended conditionally. Each demerit point assigned expires after a period of three years. The DPS applies to all motor vehicles that require a driving license, but it is stricter for novice drivers (drivers who have held their driving license for less than three years), for whom only two demerit points in three years will result in conditional suspension. A conditional suspension means that you have to pass both practical and theoretical driving tests within a specified period (usually 6 months) to keep your driving license. Unconditional suspension of the driving license involves being deprived of driving privileges for some time and after this period elapses, it is necessary to pass both the practical and theoretical driving tests to get the license back. It should be noted that besides to the pecuniary costs associated with forfeiting driving privileges, for example in terms of foregone income, both driving tests involve substantial monetary costs.

Although the 30 percent threshold seems generous, 80 percent of the demerit points issued in the first few years following the introduction of DPS reform in Denmark have been assigned to drivers speeding beyond this threshold (ETSC, 2008). Similarly, more than 70 percent of speed offenses in the first two years after the introduction of DPS triggered the assignment of demerit points (Statistics Denmark, 2007).[4] This suggests that speed deserves attention in improving public road safety in Denmark.

The main motivation for the introduction of the point recording scheme in Denmark, as well as in other developed countries, is to discourage frequent traffic offenders. This is fairly plausible as traffic violations are disproportionally distributed across the driving populations of societies worldwide. To get a sense of the share of frequent offenders in the overall traffic violations registered in Denmark, Table A1 (in the appendix) shows the distribution of annual

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[3] This figure has been changed since then and the current rate (2012) for a similar offense is 2500 DKK (450 US dollar). See at www.sikkertrafik.dk

[4] See www.dst.dk/stattabel/384

offenses in the years 2001–2011, classifying whether offenses are committed by new offenders or frequent offenders. Frequent offenders are those drivers who have had previous offenses, whereas new offenders are those without any offense in the previous years. For instance, 28 percent of the offenses in 2005 were committed by drivers who had similar offenses in the years 2001–2004. The proportion of frequent offenders increases over the years because of the expansion of the reference time, thus approximately 35 percent of the traffic offenses in 2011 were committed by those drivers who had one or more traffic violations in the last 10 years. Overall, frequent offenders account for a good share of the traffic offenses registered in Denmark.

4. Data and Sampling Design

The data used in this study come from a combination of different administrative registers for Denmark. The driving behavior data come from annual traffic offenses recorded by the police, which is part of the crime register data for Denmark. From the whole annual crime register data, we extract offenses and verdicts associated with traffic violations, including traffic tickets (fines) issued, demerit points assigned, as well as other types of bans for traffic offenders. We merge this annual traffic offense register with detailed longitudinal administrative registers, including income/wealth, employment, education, and car registration. Merging these high quality administrative registers provides a rich set of relevant observable characteristics for our sample.

For the purpose of exploiting the specific feature of the reform with regard to speed and because speed offenses contribute to a large share of traffic accidents registered worldwide, our focus is on speed offenders. As we are interested in evaluating a reform introduced in September 1, 2005, we need a sufficient time window to observe drivers' differential penalty (or punishment) status. For brevity, we introduce three terms at this stage: *treatment period, treatment group,* and *control group* drivers. We assign the period September 1, 2005 to December 31, 2006, the immediate post-reform period, as the *treatment period*. Drivers committing speed offenses during this period were subject to a fine or demerit points together with a fine using the 30 percent speed threshold. We assign drivers to the *treatment* and *control* groups using this 30 percent speed threshold: those offenders above this threshold, and thus assigned demerit points, are our *treatment group* drivers; while those below (or equal to) the threshold and receiving only a fine, comprise the *control group*.

According to the three-year rule for the validity of demerit points in Denmark, the treatment period adopted here means that demerit points assigned during this time are not valid after

2009. Thus, we observe individuals' driving behavior before and after the treatment period (or reform), allowing for almost three *pre-reform* periods (32 months) and three *post-reform* follow-up periods (36 months). Implicitly, we follow the driving behavior of these drivers three years before the reform (2003–2005, to August 31) and three years after the treatment period (2007–2009). Thus, we compare the driving behavior of individuals assigned one or more demerit points (*treatment group*) during the treatment period to those receiving only a fine (*control group*), before and after the introduction of the reform. The following figure depicts our benchmark sampling design.

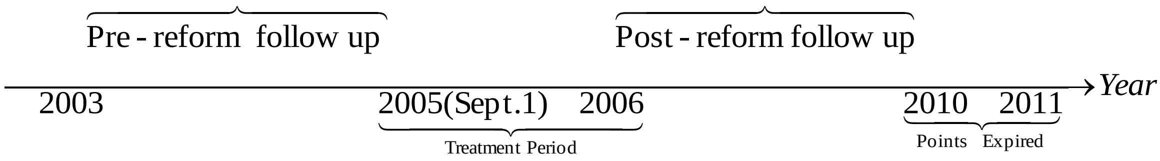

Figure 1: Illustration of the benchmark sampling design.

However, even with the detailed car ownership and exposure data we have, this sampling design may produce a slightly noisy sample due to the enormous variation in driving exposure and heterogeneous risk of committing traffic offenses among drivers. Thus, to ensure that drivers were continuous (or active) car users before our evaluation period and create a homogeneous sample of drivers, we assign the years 2001–2002 as *base* years and we sample those drivers who committed speeding offenses in the *base* years (2001–2002) and in the *treatment period*. Therefore, we introduce and mainly focus on the following sampling design (Figure 2).

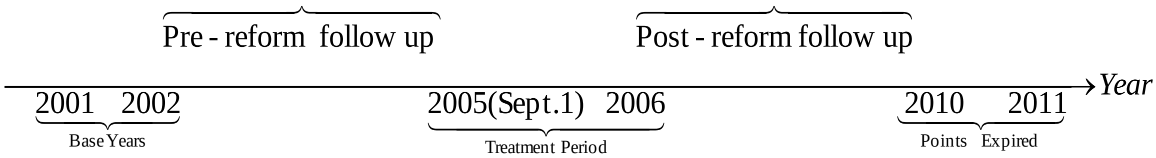

Figure 2: Illustration of the preferred sampling design

Our focus on frequent speed offenders by conditioning on traffic offenses in the base years deserves further justification: (1) worldwide, traffic offenses are disproportionally distributed across the population of drivers, so we need to balance the risk of committing a traffic offense by considering a specific group of drivers, in this case frequent speed offenders. (2) As shown in Table A1, frequent traffic offenders account for a substantial share of overall traffic violations. (3) The reform targets frequent offenders; hence, any evaluation strategy that aims to estimate the effect of this reform should focus on this type of drivers.[5] (4) Although we focus on this sampling design for the above reasons, we are able to establish all the empirical results even with the benchmark sampling design (see Section 5.4).

To reduce heterogeneity in our sample still further, we restrict the sample using some observable characteristics of drivers. First, we sample drivers aged 26–59 years old at the time of their first speeding offense in the *base* years (2001–2002) and with a car registered in their name before 2001.[6] Second, drivers should all hold a driving license when caught speeding in the base years (2001–2002). By design, this restriction means that at the time of treatment, all drivers will have held their driving license for more than three years, and hence will be subject to the three demerit points (in three years) rule relating to conditional suspension of the driving license. Third, we also exclude those drivers who have had other non-speed demerit points and those whose driving licenses were suspended in the *base* or *treatment* period. Using the above restrictions, we arrive at 5,490 drivers whose driving behavior we follow before and after the reform. Of these drivers, 75 percent received at least one demerit point associated with a speeding offense during the *treatment* period, whereas the remaining 25 percent were only fined. These proportions are consistent with the national reports discussed in Section 3. Decomposing these figures further, 71 percent of the drivers in our sample received only one demerit point for a speeding offense in the treatment period, whereas the remaining 4 percent received two demerit points for speeding offenses in the treatment period. It should be noted that if a driver accumulates more than two demerit points in the treatment period, his/her driving license will be suspended conditionally, and such drivers are then not included in our sample.

To sharpen the comparison in driving behavior between the treatment and control group drivers, and evaluate the long-lasting effects of traffic penalties, we also follow individuals' driving behavior after the validity of the demerit points assigned at the *treatment* period expires. We follow individuals' driving behavior in the period 2010–2011. This helps us to compare the driving behavior of the treatment and control group drivers once the demerit points assigned during the treatment period are no longer valid.

[5] This justification resembles Heckman's (1997) reasoning on the relevance of random assignment unless all individuals are eligible or comply for potential treatment.

[6] In Denmark, car owners pay substantial ownership tax. Thus, restricting the sample to those who own cars is expected to produce a sample of active car users.

We measure individuals' driving behavior using both intensive and extensive margins of driving: the frequency of traffic violations (including speed and non-speed offenses) committed in a specified period and the probability of committing a traffic offense within a certain period of time. As we are interested in the overall effect of the reform on driving behavior, we count traffic offenses involving speed and non-speed traffic violations. We focus on the most common traffic violations including: driving beyond the speed limit, failure to give right of way, and driving under the influence of alcohol. As expected, speed-related violations account for a large share of the offenses in our final sample.

5. Identification Strategy and Estimation

As we are able to follow the driving behavior of individuals before and after the reform, we use difference-in-differences (DID) approach to identify the causal impact of the demerit points assigned. This approach helps us to discern the behavioral response of drivers and time-invariant unobserved heterogeneity (selection) among different groups of drivers. We compare the driving behavior of individuals who are assigned one or more demerit points (*treatment group*) during the treatment period with those who only receive a fine (*control group*), before and after the introduction of the reform. Two issues in our sampling design deserve some emphasis: (a) both treatment and control group drivers are speed offenders, but the treatment group drivers exceeded the speed limit by more than 30 percent; (b) while the tendency to drive beyond the speed limit is potentially endogenous, there is some local randomization given by the 30 percent threshold. This natural experiment calls for regression discontinuity designs to identify treatment effects using those drivers speeding around the 30 percent threshold. Unfortunately, we lack information on actual driving speed in our data, except the fact that we can deduce some information on the range of actual speed from the fines we observe.[7] Thus, we will mainly employ DID approaches while also using some of the local randomization given by the 30 percent threshold to construct comparable treatment and control group drivers.

In this setting, identifying the causal impact of the demerit points hinges on the common trend assumption. That is, in the absence of the reform (or demerit point assignment), treatment and control group drivers would on average exhibit similar changes (evolution) in driving behavior. Using a series of empirical exercises, we justify that this assumption holds in our case. First, we show that treatment and control group drivers are statistically indistinguishable in terms of their

[7] We have done some robustness exercises using this information towards the end of the paper.

large set of pre-reform observable characteristics. Second, we show that both treatment and control group drivers exhibit statistically similar pre-reform driving behavior in terms of the frequency of traffic offenses and the probability of committing traffic violations. Third, our placebo regression in the next sections indicate that treatment and control group drivers share a statistically identical pre-reform time trend in driving behavior.

Table 1 provide descriptive statistics for a large set of pre-reform observable characteristics of treatment and control group drivers, measured in 2004. Table 1 compares those drivers with at least one demerit point (*treatment group*) associated with speeding offenses with those drivers without demerit points (*control group*). We consider an extensive set of relevant observable characteristics, including detailed demographic and socioeconomic characteristics, as well as driving exposure measures, including commuting distance to workplace. The first two columns of Table 1 present the means (with standard deviations in parentheses) of the variables for both groups, whereas the last column presents mean differences across both groups (with standard errors in parentheses). In none of the comparisons are we able to detect systematic and significant differences between the treatment and control group drivers, except for the indicator variable for drivers living in Odense.[8] It is not surprising to observe such a balanced composition (distribution) between both groups of drivers as we are considering a specific type of drivers – those who usually drive beyond the speed limit. We also partially attribute this balancing effect to the local randomization given by the 30 percent speed threshold. This nearly perfect balancing between our treatment and control group drivers suggests that our sampling framework instrumentally helped to construct meaningfully comparable groups of individuals.

[8] We expect that this minor difference would remain time-invariant.

Table 1: Descriptive Statistics of Treatment and Control Group Drivers

Background characteristics	(1) Treatment group (at least one point)	(2) Control group (no demerit points)	(3) Difference
Male driver	0.886 (0.318)	0.885 (0.319)	0.001 (0.010)
Age	40.976 (8.467)	40.692 (8.49)	0.284 (0.266)
Married	0.560 (0.497)	0.539 (0.499)	0.020 (0.016)
Self-employed	0.151 (0.358)	0.145 (0.352)	0.006 (0.011)
Unemployed	0.113 (0.317)	0.105 (0.306)	0.008 (0.010)
Education:			
Elementary	0.139 (0.346)	0.134 (0.340)	0.005 (0.011)
High school	0.039 (0.194)	0.046 (0.210)	-0.007 (0.006)
Vocational	0.454 (0.498)	0.453 (0.498)	0.002 (0.016)
Bachelor and above	0.239 (0.427)	0.244 (0.430)	-0.005 (0.013)
Other type of education	0.173 (0.378)	0.168 (0.374)	0.005 (0.012)
Income in 1000 DKK (gross)	444.029 (459.067)	438.230 (579.65)	5.798 (15.417)
Net wealth in 1000 DKK	109.676 (2104.034)	31.616 (1315.210)	78.060 (60.869)
Non-traffic offense in the last four years	0.254 (0.806)	0.272 (0.873)	-0.017 (0.026)
Commuting distance to work place (*km*)	25.892 (42.298)	27.979 (45.829)	-2.088 (1.435)
Commuting distance to work, below 100 km	17.784 (21.206)	18.438 (21.738)	-0.654 (0.730)
Immigrant	0.098 (0.297)	0.094 (0.292)	0.003 (0.009)
Driver lives in:			
Central Copenhagen	0.130 (0.336)	0.125 (0.330)	0.005 (0.01)
Copenhagen area	0.111 (0.315)	0.118 (0.323)	-0.007 (0.01)
Aarhus	0.035 (0.184)	0.039 (0.194)	-0.004 (0.006)
Aalborg	0.035 (0.183)	0.030 (0.172)	0.004 (0.006)
Odense	0.042 (0.202)	0.026 (0.159)	0.017 (0.006)***
Roskilde	0.010 (0.099)	0.009 (0.094)	0.001 (0.003)
Other towns and rural areas	0.637 (0.481)	0.653 (0.476)	-0.016 (0.015)
Number of individuals	4142	1348	

Notes: Columns 1 and 2 present means (with standard deviations in parentheses) of the variables for the treatment and control group drivers, respectively. Column 3 presents mean differences across both groups (with standard errors in parentheses). In this table, treatment group drivers are those with at least one demerit point associated with speed offenses in the treatment period, whereas control group drivers are those who committed speed offenses during the treatment period but only received fines. All variables are measured as of 2004, immediately before the reform. ***, **, * indicate that differences are significantly different from zero at the 0.01, 0.05, and 0.10 levels, respectively.

Figures 3–6 show the evolution of individuals' driving behavior across years. For simplicity, we can assign the evolution of driving behavior into four time windows relevant to our study: *base* years (2001–2002), *pre-reform* (2003–2005, to August 31), *post-reform* (2007–2009) and *expiration* period (2010–2011). In all figures, *treatment* period stands for the time September 1, 2005 to December 31, 2006. For this reason, the values for the year 2005 are computed for the eight months before the reform. In Figure 3, we plot the average annual frequency of traffic offenses for those drivers with at least one demerit point (treatment group) and those without demerit points (control group). For instance, those drivers who are assigned at least one demerit point committed, on average, around 0.17 traffic offenses in 2003, whereas the corresponding rate for those drivers without demerit points is 0.15. Figure 4 presents similar evidence but now restricting treatment to those drivers who are assigned one demerit point associated with speed offense in the treatment period. For both figures, the large values in 2001–2002 are artefacts of our sampling design because we condition on speed offense in the base years (2001–2002). Both figures indicate that except for the post-reform period, treatment and control group drivers committed similar numbers of traffic offenses in the base years, both pre-reform and once the demerit points expire. Figure 4 further sharpens this comparison as pre-reform differences between both groups diminish, while post-reform differences are more visible when we compare drivers with one demerit point with those without demerit points.

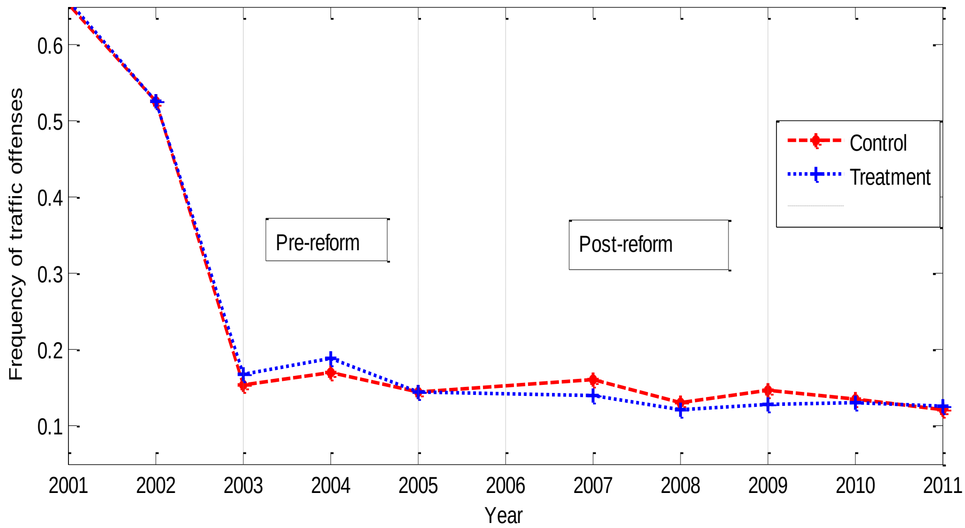

Figure 3: Evolution of driving behavior, measured by the average annual frequency of traffic violations. In this figure, treatment group drivers are those who were assigned at least one demerit point, whereas control group drivers are those who committed speed offenses but were only fined.

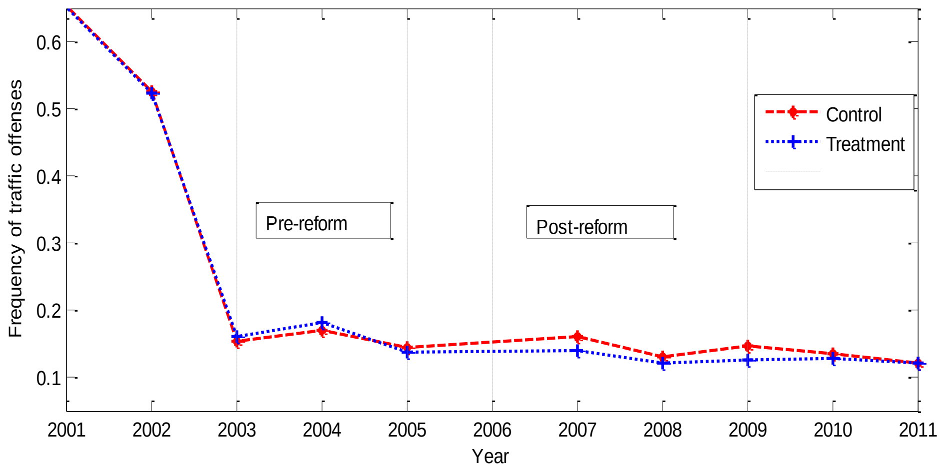

Figure 4: Evolution of driving behavior, measured by the average annual frequency of traffic violations. In this figure, treatment group drivers are those who were assigned one demerit point during the treatment period, whereas control group drivers are those who committed speed offenses but were only fined.

In Figures 5 and 6, we provide similar pieces of evidence for the extensive margin of driving behavior in terms of the proportion of drivers committing traffic violations. Both figures explicitly show that except for the post-reform period, treatment and control group drivers have comparable shares of traffic violations and this pattern approaches perfection when we compare drivers with one demerit point with those without demerit points (see Figure 6).

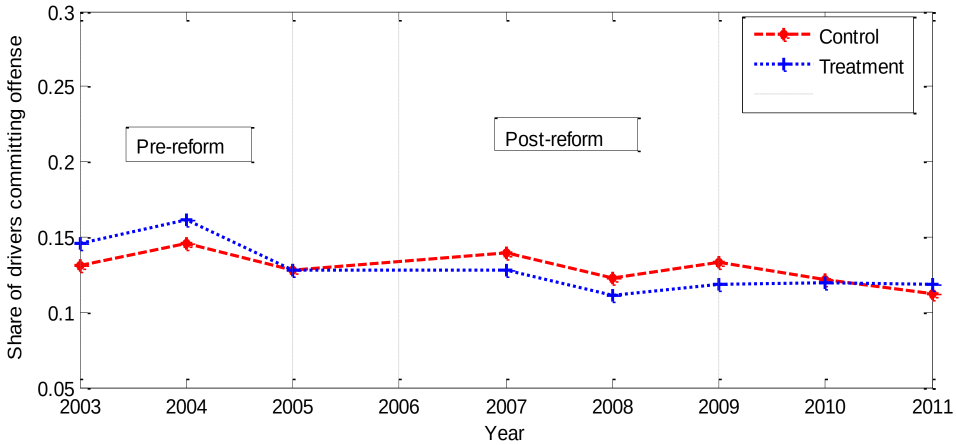

Figure 5. Evolution of driving behavior, measured by the proportion of drivers committing traffic violations every year. In this figure, the treatment group refers to those drivers who were assigned at least one demerit point during the treatment period, whereas control group drivers are those without demerit points.

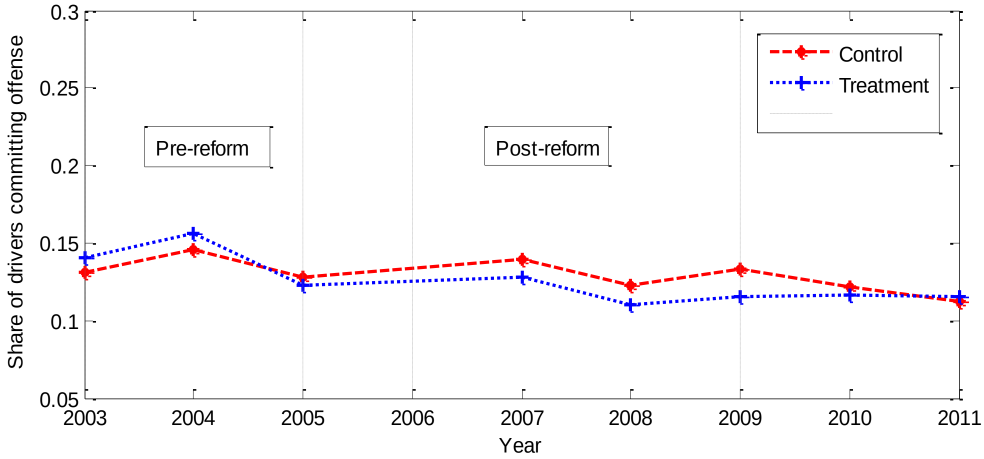

Figure 6: Evolution of driving behavior, measured by the proportion of drivers committing traffic violations annually. In this figure, treatment group drivers are those who were assigned one demerit point during the treatment period, whereas control group drivers are those without demerit points.

Table 2 compiles the graphical information given in Figures 3–6, together with some test statistics comparing differences in driving behavior at the relevant time windows mentioned above. The first two columns in Table 2 provide the mean outcomes of interest (measures of driving behavior) with standard deviations in parentheses for both groups of drivers. The last column of Table 2 presents mean differences across both groups (with standard errors in parentheses). Except for the post-reform period, all comparisons indicate that both groups exhibit statistically similar driving behavior in all time windows.

Overall, the descriptive pieces of evidence given in Tables 2–3, as well as Figures 3–6, suggest that our treatment and control group drivers share similar pre-treatment characteristics and exhibit statistically similar pre-reform driving behavior. These tables and figures also show that once the demerit points assigned during the treatment period expire, on average, both groups exhibit similar driving behavior. This suggests that the demerit points have been effective as far as they are valid, suggesting the effect is applicable only for the period the demerit points are valid. Furthermore, although the pre-reform and post-reform periods are not exactly the same – the former including 32 months and the latter comprising 36 months – Table 2 also shows that the driving behavior of control group drivers did not change statistically in the two periods. This evidence is also reflected in Figures 3-6, as all show that the driving behavior of control group drivers remain stable before and after the reform. This is crucial evidence because one could claim that the reform could have affected even those in the control group drivers. This before-after comparison in driving behavior for the control group drivers in Table 2 and Figures 3-6 suggest that potential anticipation and general equilibrium effects of the reform can reasonably be ruled out. This further provides empirical evidence against the notion of "regression towards the mean".[9] These pieces of evidence suggest that we can reasonably interpret the only statistically significant post-reform difference in driving behavior between the two groups as a causal impact of the demerit points assigned.

Table 2: Comparison of Driving Behavior: Pre-reform, Post-reform and After Points Expire

Outcome variable (measure of driving behavior)	Treatment group	Control group	Difference
PANEL A: (treatment: at least one demerit point)			
Frequency of traffic offenses 2001–2002	1.185 (0.480)	1.180 (0.482)	0.006 (0.015)
Frequency of traffic offenses 2003–2005†	0.502 (0.827)	0.467 (0.763)	0.036 (0.025)
Share of drivers committing traffic offenses 2003–2005†	0.347 (0.476)	0.338 (0.473)	0.009 (0.015)
Treatment (September 1, 2005 to December 31, 2006)			
Frequency of traffic offenses 2007–2009	0.392 (0.667)	0.441 (0.710)	-0.050 (0.021)***
Share of drivers committing traffic offenses 2007–2009	0.306 (0.461)	0.336 (0.473)	-0.030 (0.015)**
Demerit points assigned expired (after 2009)			
Frequency of traffic offenses 2010–2011	0.257 (0.528)	0.254 (0.530)	0.002 (0.017)
Share of drivers committing traffic offenses 2010–2011	0.218 (0.413)	0.214 (0.411)	0.004 (0.013)
PANEL B: (treatment: one demerit point)			
Frequency of traffic offenses 2001–2002	1.176 (0.47)	1.180 (0.482)	-0.004 (0.015)
Frequency of traffic offenses 2003–2005†	0.483 (0.805)	0.467 (0.763)	0.016 (0.025)
Share of drivers committing traffic offenses 2003–2005†	0.338 (0.473)	0.338 (0.473)	-0.000 (0.015)
Treatment (September 1, 2005 to December 31, 2006)			
Frequency of traffic offenses 2007–2009	0.386 (0.662)	0.441 (0.71)	-0.056 (0.021)***
Share of drivers committing traffic offenses 2007–2009	0.302 (0.459)	0.336 (0.473)	-0.034 (0.015)**
Demerit points assigned expired (after 2009)			
Frequency of traffic offenses 2010–2011	0.249 (0.517)	0.254 (0.530)	-0.006 (0.016)
Share of drivers committing traffic offenses 2010–2011	0.213 (0.41)	0.214 (0.411)	-0.001 (0.013)

Notes: Columns 1 and 2 present the mean (with standard deviations in parentheses) outcomes for the treatment and control group drivers, respectively. Column 3 presents mean differences across both groups (with standard errors in parentheses). In Panel A, treatment group drivers are those with at least one demerit point associated with speed offenses in the treatment period, whereas in Panel B, treatment group drivers are those with one demerit point. † refers to counting considered until August 31, 2005 as the reform was introduced on September 1, 2005. ***, **, * indicate that differences are significantly different from zero at the 0.01, 0.05, and 0.10 levels, respectively.

5.1 Estimation

To quantify the effect of the demerit points assigned, we estimate the following DID equation:

$$y_{dt} = \beta_0 + \beta_1 DP_d + \beta_2 After + \beta_3 (DP_d * After) + \beta_4 X_d + d_m + \varepsilon_{dt} \qquad (1)$$

[9] The notion of regression towards the mean implies that if the treatment is random, a variable that assumes an extreme value in the first measurement will assume a value close to the mean in the second measurement due to the idea of mean reversion.

where y_{dt} stands for the measure of driving behavior, DP_d represents the treatment indicator for drivers who are assigned one or more demerit points during the treatment period, and $After$ indicates the post-reform period (2007–2009).[10] β_1 captures possible pre-reform differences in driving behavior between the treatment and control group drivers. The time trend indicator, $After$, captures certain aggregate time (trend) factors that could cause changes in driving behavior, even in the absence of the DPS reform. Our parameter of interest is β_3, an interaction effect of the treatment indicator and post-reform period. β_4 captures the effect of other pre-reform characteristics of drivers that could affect driving behavior. The observable variables in X_d are measured at 2004, immediately before the reform. d_m stands for geographic (municipality) dummies that absorb the exposure of drivers to traffic control. These geographic dummies comprise 272 indicator variables for the municipality in which the driver lived in 2004. The parameter in which we are interested, β_3, measures the "behavioral response" of drivers to a stricter non-monetary traffic enforcement based on demerit point assignment.

We estimate equation (1) for the two measures of driving behavior discussed in Section 4. We focus predominantly on two types of treatment: being assigned at least one and only one demerit point associated with a speed offense during the treatment period. This is simply because we have very few drivers who are assigned two demerit points (4 percent of our sample) during the treatment period. But to uncover the heterogeneous response of drivers based on the demerit points accumulated, we exploit this small share of drivers to construct a progressively increasing intensity of treatment. Therefore, we first consider those drivers with at least one demerit point in the treatment period as our treatment group and those without demerit points as the control group. Second, we restrict our treatment to those drivers with one demerit point and compare them with those drivers without demerit points (but fined). Third, we compare drivers with two demerit points with those who had only one demerit point. Fourth, we compare the response of drivers with two demerit points with those who had at most one demerit point.

To test (indirectly) the implication of our identifying common trend assumption, we undertake a placebo regression considering the following scenario: in the absence of the reform or its anticipation effect, both treatment and control group drivers would have had a similar pre-reform

[10] We also estimate a disaggregated version of equation (1) using full year dummies instead of aggregate post-reform dummy. But we prefer the latter aggregate specification to avoid some bias coming from working with very small fractions because a very small fraction of drivers commit traffic offenses every year.

trend in driving behavior. We split the pre-reform follow-up period into two time zones (2003–2004) and 2005 (until August 31). If there is no pre-reform differential time trend in driving behavior, estimating equation (1) using these data should yield an average placebo treatment effect close to zero.

As our outcome variables are both count and binary response variables, we estimate equation (1) using standard linear panel data models, as well as using Poisson regression (for the frequency of traffic offenses) and probit models (for the probability of committing traffic violations). The implied marginal effects from these non-linear regressions are given below each treatment effect estimated through our linear regression approach. Not surprisingly, the magnitude of treatment effects estimated using the linear panel data regressions are very comparable with the implied marginal effects from our Poisson and probit models.

In all our regressions, we estimate equation (1) without any control, as well as including additional controls and municipality dummies. Drivers living in the same municipality are exposed to similar traffic enforcement, suggesting that unobserved effects across drivers living in the same municipality can be correlated. Thus, as emphasized in Bertrand, Duflo, and Mullainathan (2004), we cluster standard errors at the municipality level.[11]

5.2 Main Results and Discussion

This sub-section presents the main estimates and associated discussion. Table 3 shows the estimation results of equation (1) for the intensive margin of driving behavior (frequency of traffic offenses), considering progressively varying levels of treatment. The first two columns provide treatment effects for those drivers who are assigned at least one demerit point, compared to those drivers without demerit points, estimated without control and with additional controls, respectively. The third and fourth columns in Table 3, present similar treatment effects but now for a slightly different treatment group: those drivers who are assigned one demerit point, compared to those drivers without demerit points, estimated without controls and with additional controls, respectively. The fifth column presents treatment effects comparing those drivers with two demerit points to those with one demerit point, while the sixth column compares drivers with two demerit points to those with at most one demerit point.

The DID estimates in Table 3 consistently tell a plausible story. The estimates without controls are almost identical to those with additional controls. The first column of Table 3 indicates

[11] We also had standard errors clustered at lower level, at household level, but this has very little effect compared to clustering at municipality level.

that those drivers who are assigned at least one demerit point reduced their frequency of traffic offenses by 0.085. Considering the pre-reform mean frequency of traffic offenses for this treatment group in Table 2 (0.502), this amounts to a 17 percent reduction in the number of traffic violations. This is a rather remarkable response. More intuitively, at least three points deserve emphasis in interpreting the estimates in Table 3. First, the estimates with varying intensity of treatment imply that drivers' efforts in relation to safe driving increase with the number of demerit points accumulated. For instance, comparing those drivers with one demerit point with those drivers without demerit points, we can see that the former reduced their frequency of traffic offenses by an order of 0.072 offenses. This amounts to a 15 percent reduction in the number of traffic offenses. The estimated treatment effect increases to 0.23 and 0.25, in columns 5 and 6, respectively. Relative to the pre-reform mean traffic violations for each treatment group (see Table 2), these effects translate to 28–30 percent reduction in the frequency of traffic offenses. These results are consistent with Dionne et al.'s (2011) theoretical model predictions, which generally show that the optimal effort level increases with the number of demerit points accumulated. Second, we also note that drivers' effort increases disproportionally in relation to the number of demerit points accumulated due to the limit on the maximum tolerable number of demerit points that drivers can attain. Comparing the treatment effects in columns 3 and 5, the estimates suggest that each additional demerit point has differential impact on victims depending on their previous driving record. This can be interpreted as increasing returns for each demerit point assigned to the driving license. Helland and Tabarrok (2007) evaluate a reform that is technically similar to our reform, the "three strikes" law introduced in California for serious crimes, and they find very comparable results to ours.[12] Third, integrating the estimates in Table 3 with the descriptive evidence on pre-treatment outcomes in Table 2, we observe that a larger share of the treatment effects come from post-treatment behavioral change.[13]

[12] California's "three strikes" reform punishes individuals with three strikes with longer prison sentences, and the DPS reform in Denmark suspends a driver's license once a driver accumulates three demerit points in three years.

[13] We emphasize this effect as a general behavioral response of drivers, rather than attributing it to improvement in driving behavior, because our reduced form equation does not explicitly inform the behavioral mechanism.

Table 3: Difference-in-Differences Estimates for the Frequency of Traffic Offenses

Explanatory variables	At least one demerit point (1) Without controls	At least one demerit point (2) With controls	One demerit point (3) Without controls	One demerit point (4) With controls	Two points vs. one (5) With controls	Two points vs. at most one (6) With Controls
After reform	-0.025	-0.023	-0.025	-0.024	-0.096***	-0.077***
	(0.027)	(0.027)	(0.027)	(0.027)	(0.014)	(0.013)
Treatment	0.036	0.044*	0.016	0.027	0.312***	0.318***
	(0.023)	(0.023)	(0.024)	(0.023)	(0.067)	(0.065)
Treatment* After reform	-0.085***	-0.085***	-0.072***	-0.072***	-0.232***	-0.251***
	(0.028)	(0.028)	(0.028)	(0.028)	(0.072)	(0.072)
Poisson: marginal effects	-0.085***	-0.083***	-0.072***	-0.071***	-0.211***	-0.230***
	(0.030)	(0.029)	(0.030)	(0.029)	(0.070)	(0.070)
Other controls	No	Yes	No	Yes	Yes	Yes
Constant	0.467***	-0.093*	0.467***	-0.090*	-0.152	-0.068
	(0.022)	(0.049)	(0.022)	(0.051)	(0.096)	(0.048)
R-squared	0.004	0.048	0.003	0.048	0.063	0.051
No. of individuals	5490	5490	5250	5250	4142	5490
No. of observations (*N*T*)	10980	10980	10500	10500	8284	10980

Notes: Column 1 represents the DID estimates of equation (1) for the frequency of traffic offenses without controls. Colum 2 includes the controls in Table 1 and 272 municipality dummies. In columns 1 and 2, the treatment group comprises those drivers with at least one demerit point, whereas control group drivers are those without any demerit points. Columns 3 and 4 consider a slightly different treatment group, comprising drivers with only one demerit point, compared to drivers without demerit points (but fined). Columns 5 and 6 compare those drivers with two demerit points to drivers with one and at most one demerit point, respectively. The fourth row of this table presents marginal effects from a Poisson regression evaluated at the sample means. In all regressions standard errors are clustered at municipality level and given in parentheses. ***, **, * indicate that estimates are significantly different from zero at the 0.01, 0.05, and 0.10 levels, respectively.

Table 4 presents similar evidence using the extensive margin of driving behavior: the probability of committing traffic offenses. As in Table 3, treatment effects are estimated considering varying levels of treatment. For instance, column 1 of Table 4 indicates that comparing those drivers with at least one demerit point to drivers without demerit points, the former decreased their likelihood of committing traffic violations by 4 percentage points. Considering the pre-reform share of traffic violations for this treatment group in Table 2, this corresponds to an 11 percent reduction in the probability of committing traffic violations. Consistent with the results in Table 3, the estimates in Table 4 show that drivers' effort in relation to avoiding traffic violations increases with the number of demerit points accumulated. Columns 5–6 of Table 4 indicate that drivers' response through the extensive margin of driving behavior reaches 9–10 percentage points, an effect amounting to an 18–20 percent reduction in the share of drivers committing traffic offenses.

Table 4: Difference-in-Differences Estimates for the Probability of Committing Traffic Offenses

	At least one demerit point		One demerit point		Two points vs. one	Two points vs. at most one
Explanatory variables	(1) Without controls	(2) With controls	(3) Without controls	4) With controls	(5) With Controls	(6) With controls
After reform	-0.001	-0.001	-0.001	-0.001	-0.035***	-0.026***
	(0.016)	(0.017)	(0.016)	(0.017)	(0.009)	(0.008)
Treatment	0.009	0.014	0.000	0.006	0.147***	0.145***
	(0.014)	(0.014)	(0.014)	(0.014)	(0.029)	(0.028)
Treatment * After reform	-0.039**	-0.039**	-0.034*	-0.034*	-0.090**	-0.098**
	(0.018)	(0.018)	(0.018)	(0.018)	(0.042)	(0.042)
Probit: marginal effects	-0.039**	-0.041**	-0.034*	-0.035*	-0.092**	-0.101**
	(0.021)	(0.021)	(0.021)	(0.021)	(0.047)	(0.046)
Other controls	No	Yes	No	Yes	Yes	Yes
Constant	0.338***	-0.042	0.338***	-0.041	0.371***	-0.030
	(0.013)	(0.030)	(0.013)	(0.032)	(0.040)	(0.029)
R-squared	0.004	0.044	0.001	0.045	0.055	0.046
Number of individuals	5490	5490	5250	5250	4142	5490
No. of observations (*N*T*)	10980	10980	10500	10500	8284	10980

Notes: Column 1 represents the DID estimates of equation (1) for the probability of committing traffic offenses without controls. Colum 2 includes the controls in Table 1 and 272 municipality dummies. In columns 1 and 2, the treatment group comprises those drivers with at least one demerit point, whereas control group drivers are those without any demerit points. Columns 3 and 4 consider a slightly different treatment group, comprising drivers with only one demerit point, compared to drivers without demerit points (but fined). Columns 5 and 6 compare those drivers with two demerit points to drivers with one and at most one demerit point, respectively. The fourth row of this table presents marginal effects from a probit regression evaluated at the sample means. In all regressions standard errors are clustered at the municipality level and given in parentheses. ***, **, * indicate that estimates are significantly different from zero at the 0.01, 0.05, and 0.10 levels, respectively.

Table 5 provides the placebo regression results to test the implication of our identifying assumption. Table 5 shows DID estimates using the pre-reform driving behavior data. We can see that treatment and control group drivers share identical pre-reform time trend in driving behavior.[14] This suggests that the main results given in Tables 3 and 4 are not driven by differential time trends in driving behavior among the treated and control group drivers.

[14] The significant negative time trend we observe in our placebo experiments is an artifact of our design, a design that includes a shorter time span for the post-reform period.

Table 5: Placebo Differences-in-Differences Estimates Using Pre-reform Data

Explanatory variables	Frequency of traffic offenses				Probability of committing offenses			
	At least one demerit		one demerit point		At least one demerit		One demerit point	
	(1) Without Controls	(2) With Controls	(3) Without Controls	(4) With Controls	(5) Without Controls	(6) With Controls	(7) Without Controls	(8) With Controls
After reform	-0.179***	-0.178***	-0.179***	-0.178***	-0.119***	-0.118***	-0.119***	-0.118***
	(0.023)	(0.023)	(0.023)	(0.023)	(0.016)	(0.016)	(0.016)	(0.016)
Treatment	0.036	0.040*	0.021	0.027	0.024*	0.027**	0.014	0.018
	(0.023)	(0.023)	(0.024)	(0.023)	(0.014)	(0.014)	(0.014)	(0.014)
Treatment*After reform	-0.036	-0.036	-0.027	-0.027	-0.024	-0.025	-0.019	-0.020
	(0.028)	(0.028)	(0.029)	(0.029)	(0.019)	(0.019)	(0.020)	(0.020)
Constant	0.323***	0.050	0.323***	0.051	0.247***	0.033	0.247***	0.035
	(0.018)	(0.034)	(0.018)	(0.033)	(0.012)	(0.026)	(0.012)	(0.026)
R-squared	0.034	0.069	0.033	0.067	0.030	0.068	0.029	0.066
No. individuals	5490	5490	5250	5250	5490	5490	5250	5250
No. observations	10980	10980	10500	10500	10980	10980	10500	10500

Notes: This table presents placebo regression results using pre-treatment driving behavior data. In this estimation, we split the pre-reform period into two periods, labeling the period 2003–2004 pre-reform and the eight months in 2005 post- reform period, and estimate a DID regression of the type in equation (1). Columns 1–4 represent estimation results for the frequency of traffic offenses, while columns 5–8 are estimates for the probability of committing traffic violations. Odd columns are estimates without any control, while even columns include the same additional controls as in our main estimations. In all regressions, standard errors are clustered at the municipality level and given in parentheses. ***, **, * indicate that estimates are significantly different from zero at the 0.01, 0.05, and 0.10 levels, respectively.

To sum up, the overall treatment effects estimated provide interesting insights on drivers' responses to more severe non-monetary punishments, penalties that may affect their driving privileges. More generally, the overall results are consistent with standard economic and deterrence theory. The results complement previous theoretical studies in the deterrence literature, which generally argue that complementing monetary fines with non-monetary penalties can provide effective deterrence, particularly when the wealth of individuals is unobservable (see Levitt, 1997; Polinsky, 2006). Our results are also consistent with the recent theoretical predictions on the effectiveness of the point recording scheme (see Bourgeon and Picard 2007; Dionne et al. 2011). The progressively increasing treatment effects in Tables 4-5 are consistent with Bourgeon and Picard's (2007) theoretical model, according to which the lifetime expected utility of a driver is negatively related to the expected change in demerit point accumulation. Implicitly, our findings suggest that complementing monetary penalties with non-monetary instruments attached to driving privilege appears to be effective in inducing safe driving behavior.

Our estimates have some crucial implications for designing public road safety and insurance policies. For instance, the results suggest potential directions for improving market imperfections between insurance companies and car users, as the former suffer from information asymmetry regarding individuals' driving behavior. Along this line of reasoning, Dionne et al. (2011) find that a reform that integrates a point recording scheme with insurance pricing in Canada has resulted in a substantial reduction in the frequency of traffic violations.

5.3 Allowing for Heterogeneous Effect of Demerit Points

This section investigates whether treatment effects vary based on various theoretical considerations and observable characteristics of individuals. Since we have very few drivers who are assigned two demerit points in the treatment period, we focus on the first two types of treatments: at least one demerit point and one demerit point.

First, we aim to investigate if the responses of drivers vary based on the expected (ultimate) cost of losing their driving license. We expect that the importance of keeping driving license may vary across drivers and that for some drivers suspension of their driving license may even amount to losing their job. To uncover these types of behavior, we split our sample into two based on drivers' labor supply status: wage earners and self-employed. We expect that self-employed drivers are highly reliant on their cars and will strive hard to keep their driving license valid. The treatment effects reported in Table 6 support this premise, indicating that self-employed individuals, covering around 15 percent of the sample, are more responsive than wage earners. For instance, considering the intensive margin of driving behavior, column 1 of Table 6 indicates that the average response of self-employed drivers is almost three-fold that of wage earners. Similar evidence is reported using our second measure of driving behavior, the share of drivers committing traffic violations. Along a similar line of reasoning, those individuals commuting longer distances to their workplace may be more reliant on their cars and hence more responsive to each demerit point assigned to their driving license. Therefore, we also split the sample into two using the median commuting distance of drivers (to their workplace). The middle rows in Table 6 show that as hypothesized, those who commute longer distances are slightly more responsive, both in the intensive and extensive margins of driving. These pieces of evidence imply that drivers' efforts in relation to safe driving depend on the expected (or ultimate) cost of losing their driving license. This is consistent with the standard Becker (1968) model of criminal behavior and rational criminality.

A second question worth investigating in this section is whether drivers' responses vary with income and wealth. The effects of monetary instruments for deterring crime (or traffic offenses) are expected to vary depending on the income and wealth of drivers (see Levitt, 1997; Polinsky, 2006; Polinsky and Shavell, 1991). Under Becker's (1968) standard theory of deterrence, a socially optimal fine should be set at its maximum, even to the level of wealth of individuals, and coupled with a low probability of detection. Polinsky and Shavell (1991) demonstrate that if the wealth of individuals is heterogeneous, the optimal fine is less than the maximum fine. Implicitly, in cases in which monetary penalties account for a very small fraction of drivers' income and wealth, these incentives are not sufficient to induce safe driving behavior. Does this hypothesis hold for non-monetary penalties based on demerit points? To investigate this, we split our sample into two using the median gross income and median net wealth. The last rows in Table 6 suggest that treatment effects are slightly larger for those drivers with higher income. This may be because those drivers with higher incomes are more reliant on their cars and strive to keep their licenses valid.[15] Coming to the sample split based on wealth, we are unable to detect a substantial differential effect. Generally, these exercises suggest that while monetary penalties may induce little effect on individuals with higher income and wealth, non-monetary incentives address even those with higher income and wealth. This implies that a non-monetary penalty based on point recording is potentially effective instrument to ensure public road safety even when the wealth of individuals is unobservable and monetary penalties represent a small share of drivers' income (or wealth).

Overall, the empirical exercises to uncover heterogeneous treatment effects provide interesting policy implications. While previous studies on the effect of monetary fines for crimes (or traffic offenses) propose that fines may be imposed differentially based on individuals' socioeconomic status, including income and wealth, our results hint that this might not be the optimal choice in the case of non-monetary instruments. Rather, our results point that the effect of non-monetary penalties may differ based on the expected cost and consequences of these penalties for different groups of drivers.

[15] Splitting the sample using median income also serves as a robustness exercise. In Denmark, fines vary slightly with the actual driving speed, although there is little variation in fines due to the ceiling and bottom limits. Theoretically, this implies that the treatment group drivers were generally subject to a slightly higher fines because they were driving at higher speeds than the control group drivers. If the slight variation in fines can affect driving behavior, we would expect that low-income drivers should respond to a greater extent.

Table 6: Heterogeneous Treatment Effect Based on the Expected Cost of Losing Driving License and Socioeconomic Background of Drivers

Sample stratification	Frequency of traffic offenses		Probability of committing traffic violations	
	At least one demerit point	One demerit point	At least one demerit point	One demerit point
Stratification by labor market participation				
Wage earners	-0.069**	-0.054*	-0.024	-0.018
	(0.032)	(0.032)	(0.021)	(0.021)
Self-employed	-0.184**	-0.177**	-0.131***	-0.127**
	(0.088)	(0.089)	(0.051)	(0.051)
Stratification by commuting distance:				
Short: less than the median (11.5 km)	-0.073*	-0.053	-0.022	-0.013
	(0.041)	(0.040)	(0.031)	(0.031)
Long: greater than the median (11.5 km)	-0.094**	-0.086**	-0.053**	-0.050**
	(0.042)	(0.042)	(0.025)	(0.025)
Stratification by income (gross):				
Low income: less than the median	-0.046	-0.038	-0.022	-0.016
	(0.043)	(0.041)	(0.027)	(0.025)
High income: greater than the median	-0.124***	-0.106**	-0.056**	-0.052*
	(0.041)	(0.042)	(0.028)	(0.028)
Stratification by wealth (net wealth)*:*				
Low: less than the median	-0.094**	-0.078*	-0.036	-0.030
	(0.041)	(0.042)	(0.025)	(0.026)
High: greater than the median	-0.077*	-0.065	-0.042	-0.037
	(0.041)	(0.040)	(0.028)	(0.028)

Notes: Each row represents a separate DID estimation of equation (1) and estimates represent treatment effects in each regression. In all regressions we employ the additional controls included in our main results. Standard errors are clustered at the municipality level.

5.4 Alternative Sampling Design and Robustness Exercises

This section considers the benchmark sampling design presented in Section 4 to ensure that our sampling design is not driving the results. Instead of considering frequent offenders by conditioning for an offense in the *base* years (2001–2002) and *treatment* period, we drop the former conditioning and construct another very large sample based on the latter conditioning.[16] Using this large sample we are able to establish all the empirical results discussed in Section 5.2. Table A2 presents DID estimates for the frequency of traffic offenses, while Table A3 presents corresponding estimates for the probability of committing traffic offenses in the specified period. Generally, the treatment effects in Tables A2 and A3 are similar to the main results in section 5.2, except for slight differences in the magnitude of the effects, potentially attributable to the heterogeneous driving

[16] All previous caveats and restrictions are used in constructing this sample.

exposure and risk of committing traffic offenses among drivers in the large sample. For instance, those drivers with at least one demerit point during the treatment period reduced their frequency of traffic offenses by an order of 0.025 offenses, amounting to a 10 percent reduction in the frequency of traffic offenses. Treatment effects based on the extensive margin of driving behavior provide similar evidence (see Table A3).

Finally, using the full sample and for those drivers with only one traffic offense in the treatment period, we undertake another robustness exercise that corroborate our main findings. Although we are not able to observe the actual speed, we can deduce an approximate range of the actual speed from the amount of fine received and hence restrict the sample to those speed offenders around the 30 percent threshold. Thus, we restrict our sample to those drivers who received a fine of 1000 or 1500 DKK, fines used to be levied for those drivers exceeding the speed limit by 20-40 percent. Estimating a DID equation for this sample for both measures of driving behavior provides very similar estimates with those given in columns 3-4 of Tables A2-A3 (see Table A4).

6. Concluding Remarks and Policy Implications

This paper evaluates the effectiveness of a recently emerging non-monetary penalty aimed at inducing safe driving. We use unusually rich longitudinal traffic offense data, which enables us to observe individuals' driving behavior before and after Denmark introduced the non-monetary penalty based on a point recording system in 2005. We compare the driving behavior of individuals affected by the reform (treatment group) with those not affected (control group), before and after the reform. We use a difference-in-differences approach to disentangle the behavioral response of drivers and unobserved heterogeneity among different groups of drivers. To our knowledge, this study is the first empirical work that quantifies drivers' behavioral response to a more severe non-monetary penalty based on demerit points. We measure driving behavior using both intensive and extensive margins of driving: frequency of traffic offenses committed and probability of committing traffic violations in a specified period. We first establish that treatment and control group drivers are statistically indistinguishable in terms of a large set of pre-reform observable characteristics, as well as pre-reform driving behavior. We also show that treatment and control group drivers share identical pre-reform time trend in driving behavior.

We find that drivers with demerit points (treatment group) substantially reduced their frequency of offenses as well as their likelihood of committing traffic violations. As theoretically expected, we also find that drivers' effort in relation to safe driving increases with the number of demerit points accumulated. Depending on the number of demerit points accumulated in the treatment period, drivers with demerit points reduced their frequency of traffic offenses by 15–30 percent. Similarly, those affected drivers reduced their likelihood of committing traffic violations by 11–20 percent. Notably, these estimates represent a substantial behavioral response and have several policy implications. Interestingly, some of our descriptive pieces of evidence further suggest that the effect of the demerit points is only applicable for the period for which they are valid, as the treatment effects seem to vanish once the demerit points expire. In a broader sense, these behavioral responses can be attributed as evidence of the moral hazard problem, consistent with the theoretical predictions in Bourgeon and Picard (2007) and Dionne et al. (2011).

We also document differential behavioral responses among drivers based on their observable characteristics. We find that drivers' responses to each demerit point assigned to their driving license depend on the expected (or ultimate) cost of losing their driving license. The results indicate that self-employed individuals and those commuting longer distances to workplace are more responsive to each demerit point assigned to their license, potentially due to the large expected pecuniary cost in terms of forgone income associated with losing their license. This is consistent with the standard Becker's (1968) model of criminal behavior and rational criminality. Our results also suggest that while monetary penalties (fines) are conceived to have little impact on individuals with high income and wealth, non-monetary penalties based on point recording address even those with higher income and wealth. This provides further appeal for using non-monetary instruments, particularly in cases in which the income and wealth of drivers are unobservable.

More broadly, the estimated treatment effects carry some important implications for designing deterrence, public road safety and insurance policies. In view of general deterrence theory, our results are consistent with the theoretical predictions in the models of Levitt (1997) and Polinsky (2006), which generally propose that complementing fines with non-monetary penalties, including imprisonment, is more effective in deterring crime, particularly if the wealth of individuals is unobservable. Furthermore, while previous studies on the effect of monetary penalties for crimes suggest that fines may be imposed differentially based on individuals' socioeconomic status, including income and wealth, our results point that the effect of non-monetary penalties may differ based on the expected cost and consequences of these penalties for different groups of drivers. In the context of public road safety, our findings confirm Bourgeon and Picard's (2007) theoretical prediction, which indicates that complementing fines with point recording provides optimal deterrence, while the former alone may not effectively deter reckless driving. The substantial heterogeneous effort, and hence response, of drivers based on the number of demerit points accumulated is also consistent with the theoretical prediction of Dionne et al.'s (2011) model.

Our findings provide interesting insights on the effectiveness of non-monetary penalties in deterring reckless driving behavior beyond the context of Denmark. However, in the context of Denmark, our findings suggest that provided a larger share of traffic violations in Denmark trigger demerit point assignment, further initiatives on integrating the point recording scheme with insurance pricing might be worthwhile. Dionne et al. (2013) provide empirical experiences of integrating demerit points with insurance pricing in Quebec, Canada.

In this study, we focus on estimating the effect of the reform on driving behavior, not on traffic accident incidence. This amounts to estimating the effect of an intervention on the proximate causes of traffic accidents. Although we are not able to provide the ultimate effect of the reform on traffic accidents, one could translate our treatment effects into effects of the reform on traffic accident incidence using some assumptions on the obviously strong association between traffic violations and traffic accidents. Thus, our results still corroborate existing studies that compare alternative policy instruments to reduce traffic accidents (see Parry, 2004). Similarly, due to our reduced form approach, we are not able fully to observe the mechanisms by which drivers respond to the demerit points assigned to their driving licenses. It is fairly intuitive to uncover whether drivers respond by improving their driving behavior or by compromising their driving demand (reducing car use). We interpret either case as a behavioral response, but further refinement concerning the mechanisms by which drivers respond would enhance our understanding of the effectiveness of these instruments. For instance, Dee et al. (2005) find that the introduction of a graduate driver license program (GDL) in the US has been effective in reducing teenage fatality rate, but Karaca-Mandic and Ridgeway (2010) show that this works by reducing the prevalence of teenagers on the road, not by improving teenagers' driving behavior. Thus, further empirical studies on the way drivers respond to the demerit points assigned to their licenses could help policy makers understand the effectiveness of these penalties and reforms.

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Appendix

Table A1: Annual Distribution of Traffic Offenses Committed by Different Groups of Drivers

Offenses	Year										
	2001	2002	2003	2004	2005	2006	2007	2008	2009	2010	2011
Total offenses	59227	47105	52855	72334	96079	90034	80755	79628	80669	85125	84151
New offenders	-	-	42662	56434	68796	67260	55600	53285	51931	53056	50795
Frequent offenders	-	-	10193	15900	27283	22774	25155	26343	28738	32069	33356
Share of frequent offenders	-	-	0.106	0.166	0.284	0.237	0.262	0.274	0.299	0.334	0.347

Notes: The figures are computed focusing on the most common traffic violations including: driving beyond the speed limit, failure to give right of way, and driving under the influence of alcohol. Frequent offenders are those drivers who have had previous offenses, whereas new offenders are those without any offense in the previous years.

Table A2: DID Estimates Using Alternative Sampling Design for the Frequency of Traffic Offenses

	At least one demerit point		One demerit point		Two points vs. one	Two points vs.at most one
	(1) Without controls	(2) With controls	(3) Without controls	(4) With Controls	(5) With Controls	(6) With controls
Explanatory variables						
After reform	0.005	0.005	0.005	0.005	-0.014***	-0.009**
	(0.007)	(0.007)	(0.007)	(0.007)	(0.004)	(0.004)
Treatment	0.013**	0.013**	0.003	0.004	0.256***	0.256***
	(0.007)	(0.006)	(0.006)	(0.006)	(0.024)	(0.024)
Treatment*After reform	-0.025***	-0.025***	-0.019***	-0.019**	-0.157***	-0.161***
	(0.007)	(0.007)	(0.007)	(0.007)	(0.032)	(0.032)
Constant	0.235***	0.044***	0.235***	0.046***	-0.048**	0.050***
	(0.008)	(0.016)	(0.008)	(0.014)	(0.022)	(0.015)
R-squared	0.000	0.036	0.000	0.035	0.007	0.040
No. of individuals	49865	49865	48475	48475	37608	49865
No. of observations (*N***T*)	99730	99730	96950	96950	75216	99730

Notes: This table provides DID estimates for the full sample. Column 1 represents estimates of equation (1) for the frequency of traffic offenses without controls. Column 2 includes the controls in Table 1 and municipality dummies. In columns 1 and 2 the treatment group comprises drivers with at least one demerit point, whereas control group drivers are those without any demerit points. Columns 3 and 4 consider a slightly different treatment group: drivers with only one demerit point. Columns 5 and 6 compare drivers with two demerit points to those with one and at most one demerit point, respectively. In all regressions, standard errors are clustered at the municipality level and given in parentheses. ***, **, * indicate that estimates are significantly different from zero at the 0.01, 0.05, and 0.10 levels, respectively.

Table A3: DID Estimates Using Alternative Sampling Design for the Probability of Committing Traffic Offenses

	At least one demerit point		One demerit point		Two points vs. one	Two points vs.at most one
Explanatory variables	(1) Without controls	(2) With controls	(3) Without controls	(4) With Controls	(5) With Controls	(6) With controls
After reform	0.012**	0.012**	0.012**	0.012**	0.003	0.005*
	(0.005)	(0.005)	(0.005)	(0.005)	(0.003)	(0.003)
Treatment	0.010**	0.009**	0.003	0.004	0.150***	0.150***
	(0.004)	(0.004)	(0.004)	(0.004)	(0.013)	(0.013)
Treatment*After reform	-0.013**	-0.013**	-0.010*	-0.010*	-0.083***	-0.086***
	(0.006)	(0.006)	(0.006)	(0.006)	(0.018)	(0.018)
Constant	0.184***	0.053***	0.184***	0.055***	-0.039**	0.056***
	(0.006)	(0.012)	(0.006)	(0.012)	(0.016)	(0.012)
R-squared	0.000	0.031	0.000	0.0299	0.033	0.033
No. of individuals	49865	49865	48475	48475	37608	49865
No. of observations (*N*T*)	99730	99730	96950	96950	75216	99730

Notes: This table provides DID estimates for the full sample. Column 1 represents DID estimates of equation (1) for predicting the share of drivers committing traffic offenses without controls. Column 2 includes the controls in Table 1. In columns 1 and 2 the treatment group comprises drivers with at least one demerit point, whereas control group drivers are those without any demerit points. Columns 3 and 4 consider a slightly different treatment group: drivers with only one demerit point. Columns 5 and 6 compare drivers with two demerit points to those with one and at most one demerit point, respectively. In all regressions, standard errors are clustered at the municipality level and given in parentheses. ***, **, * indicate that estimates are significantly different from zero at the 0.01, 0.05, and 0.10 levels, respectively.

Table A4: Further Robustness Exercises

Panel A: DID estimates by restricting the sample around the threshold				
	Frequency of traffic offenses		Probability of committing offenses	
Explanatory variables	(1) Without controls	(2) With controls	(3) Without controls	(4) With controls
After reform	0.004	0.004	0.013**	0.013**
	(0.007)	(0.007)	(0.005)	(0.005)
Treatment	-0.000	-0.003	0.003	0.001
	(0.006)	(0.006)	(0.005)	(0.005)
Treatment*After reform	-0.017**	-0.017**	-0.011*	-0.011*
	(0.008)	(0.008)	(0.006)	(0.006)
Constant	0.217***	0.237***	0.171***	0.187***
	(0.005)	(0.019)	(0.004)	(0.014)
R-squared	0.000	0.019	0.000	0.017
No. individuals	30339	30339	30339	30339
No. observations (*N*T*)	60678	60678	60678	60678

Notes: This table presents DID estimates for the full sample and those drivers with single offense in the treatment period, by restricting the sample to those drivers who exceeded the speed limit by 20-40 percent. Columns 1–2 represent estimation results for the frequency of traffic offenses, while columns 3–4 are estimates for the probability of committing traffic violations. Odd columns are estimates without any control, and even columns include the same additional controls as in our main results. Standard errors are given in parentheses. ***, **, * indicate that estimates are significantly different from zero at the 0.01, 0.05, and 0.10 levels, respectively.