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The Costs of Avoiding Accidents.Selective Compliance and the 'Peltzman Effect' in Italy

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Abstract - We empirically investigate the deterrent and offsetting effects of the introduction of a point–record driving license (PDRL) in Italy. We find that the PDRL resulted in a sharp reduction of seat belt offenses, and in a noticeable decrease of road accidents. However, the reduction in occupant fatalities and injuries was associated with an increase in non-occupant ones, suggesting a remarkable "Peltzman effect". We then discuss whether a given enforcement design, by inducing drivers to make the best use of safety resources already available to them, may generate more external costs than would otherwise occur.

Keywords: offsetting behavior, point - record driving license, seat belts, traffic law enforcement, traffic fatalities.

JEL Classifications: D02; K32; K42;L51.

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1. Introduction♣

About forty years ago, Guido Calabresi (1970, p. 17) argued: "our society is not committed to preserving life at any cost". This unpleasant truth still holds today, especially for road traffic injuries. They represent the ninth most important cause of the global burden of disease and injury, and they are expected to become the fifth main determinant in about twenty years (WHO, 2009). The policy tools generally adopted to enhance road safety cover many different domains: mandatory use of safety devices, liability and insurance systems, speed limits, alcohol controlling policies and minimum legal drinking age, point-record driving licence system, highway infrastructure improvements, and so on.

While several analyses have focused on the stand-alone impact of specific policy options on road safety, little of this literature addresses the composite effect of the simultaneous adoption of complementary policies on drivers'incentives to drive safely. The estimation of the benefits deriving by a single policy tool (i.e. seat belt use), considered in isolation from other measures, hinges then on the key assumption that the behavior of the driver will remain unchanged in other domains (i.e. speeding), and vice-versa. This assumption is certainly open to challenge. As Peltzman (1975) outlined, regulation in one domain may affect agents' behavior in other domains, through a process of strategic adaptation. As a consequence, a richer conceptual framework that incorporates the possibility of drivers' adaptive behavior to changing incentives on several domains may be more appropriate to analyze the effects of the joint implementation of distinct policy options on road safety.

In this paper we focus our analysis on the interdependence between traffic law enforcement design and the mandatory use of safety devices, such as seat belts, in affecting drivers'incentives to take precaution. We are interested in understanding whether a given enforcement design, by inducing drivers to make the best use of safety

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resources already available to them, may generate more external costs than would otherwise occur.

Specifically, we empirically investigate whether drivers'response to the adoption of a new traffic law enforcement design - the point-record driving license (PRDL) – may induce them to take greater risk when driving (offsetting behavior), due to their best use of seat belts. To our knowledge this is the first paper dealing with this issue.

While a broad part of the empirical literature on the point–record mechanism has measured its deterrent effect on traffic offenses and road accidents, it has neglected its impact on promoting offsetting behaviors. To the best of our knowledge this is the first paper dealing with the Peltzman effect after the adoption of a new enforcement mechanism (PRDL), rather than after the introduction of mandatory seat belt law. Indeed, in the case we study, mandatory seat belt law was introduced well before the adoption of PRDL. This allowed us to isolate the deterrent effect of PRDL on traffic offenses, and specifically on seat belt offenses.

Our research question follows the pioneering argument proposed by Peltzman (1975), on drivers'behavioral response to automobile safety regulation. According to Peltzman, safety improvements - as those permitted by the mandatory usage of seat belts - may generate two opposite effects: on the one hand, the use of seat belts would reduce the potential hazards to the driver of driving fast; on the other, this reduction in hazards would entail the relative benefits of taking the safety precaution of driving carefully, inducing the driver to find desirable to drive faster or with less care, precisely when he makes use of seat belts.

We argue that, if the Peltzman's hypothesis is correct, then also the enforcement design should take into account the moral hazard potential associated to an increased use of safety devices as seat belts, induced by a reform in traffic law (Evans and Graham, 1991). Since the PRDL is increasingly adopted in most developed and developing countries, we believe it is crucial to understand whether the adoption of PRDL generates or exacerbates offsetting behaviors, when it increases drivers'compliance with seat belts use.

We are specifically interested in two questions. To what extent and through which channels does the mechanism of the point - record driving license increase compliance with traffic law? Does the induced compliance generate offsetting behaviors?

We focus our empirical analysis on the adoption of PRDL in Italy in 2003. Our findings clearly show the appearance of selective compliance after the introduction of PRDL, as drivers resulted to comply only with some traffic rules. Actually, a sharp reduction (-68.80%) in seat belt offenses (and thus a strong compliance with seat belt usage) is observed, whereas other traffic offenses have been only marginally affected or even unaffected by the adoption of a PRDL. This almost exclusive effect of PRDL on compliance with seat belt use constitutes a nice environment to measure the Peltzman hypothesis. We then investigated whether, by inducing drivers to make a better use of a safety device already available to them, and mandatory, a PRDL may have generated offsetting behaviors, thus resulting in perverse effects on road safety. In particular, we investigate whether an increase in seat belt use is associated to an increase in non-occupant fatalities and in drivers' hazardous behaviors.

Our findings show that, after the introduction of a PRDL regime in Italy, road accidents sharply decreased (-10%), whereas a strong 'offsetting effect' occurred, with reference to the relationship between occupant and non-occupant injuries (Cohen and Einav, 2003). In particular, we find that a 1% reduction in seat belt offenses is generally associated on the one side, with a reduction (- 0.13%) in occupant fatalities and injuries, and on the other with an increase (+ 0.18%) in non - occupant ones. In addition, after the introduction of the PRDL, the reduction in seat belt offenses generated slight, but nonetheless statistically significant, impacts on dangerous speeding and on driving under the influence of alcohol offenses.

We thus conclude that the deterrent effect of PRDL regime has been partially counterbalanced by the emergence of offsetting behavior. On the one side, we confirm the Peltzman hypothesis, as a sharp increase in seat belt use is associated with statistically significant offsetting behaviors. On the other, we suggest that a PRDL may generate the above offsetting behavior as long as it induces selective compliance with the use of safety devices - like seat belts - already available to drivers and mandatory.

The paper proceeds as follows. In Section 2, we survey the theoretical and empirical literature on offsetting behavior. In Section 3, we first describe the PRDL and the empirical literature on its impact, and then we briefly recall the Italian legislation on PRDL. In Section 4, we present the data and the methodology adopted in our empirical analysis. Section 5 outlines our main result. Section 6 discusses our main findings and concludes.

2. The Offset Hypothesis and the 'Peltzman Effect'

In his ground-breaking article, Sam Peltzman (1975) argued that the standard approach of the safety literature had the limit of taking as given the probability of an accident. Consequently, any policy design aimed at enhancing safety was 'measured' in terms of "the probability of surviving an accident". This was, for instance, the case of the mandatory use of seat belts in traffic law. According to Peltzman, the main consequence of the standard approach was to overstate the role of auto-safety regulation, by neglecting that "mandated devices substitute in part for safety which would have been purchased without regulation". In other words, in Peltzman's view, the standard approach failed to take into account the consequences of adaptive behavior to changing incentives, under a new regulatory regime. This point was illustrated through a simple graphical example, reported below in Figure 1.

Peltzman considered drivers behaving as if they were reacting to a 'demand for accident risk'. The assumption was that drivers face a sort of technological complementarity between accident risk and other driving outputs, as 'reduced travel time'. In Peltzman's words, "the typical driver may thus be thought of as facing a choice, not unlike that between leisure and money income, involving the probability of death from accident and [...] 'driving intensity'. More speed, thrills, etc., can be obtained only by forgoing some safety". This broad definition of 'driving intensity' has been later interpreted by several scholars also as 'aggressive driving', which is "any deliberate unsafe driving behavior performed with "ill intention or disregard to safety" (Tasca, 2000; Paleti, Eluru and Bhat, 2010). Several researchers in road safety have argued, and empirically tested, that drivers' desire to save journey time is a credible explanation of the reason why they drive fast (Fuller et al, 2009; Tarko, 2009; Peer, 2010). Since Peltzman's pioneering intuition, a wide consensus is reached today on the role played by speeding, i.e. by one of the major features of driving intensity or aggressive behavior, as a leading cause of traffic crashes and the injury severity sustained in the crashes (Andersson and Lundborg, 2007). According to the Peltzman's hypothesis, drivers face a trade-off between the level of 'driving intensity' they choose and the probability to avoid accidents and injuries. This is represented in Figure 1, where we report the Peltzman's graphical illustration, with the probability of death to driver on the vertical axis and driving intensity measured horizontally. The line A, in the Figure, thus represents, for any given safety device, all the possible combinations between driving intensity and its associated risk. Suppose that initially the driver is at point *X*, and that the mandatory use of seat belts is introduced. The safety device has the property to drop the risk curve from *0A* to *0B*. Now, if the driver maintains the previous degree of precautions, i.e. the ex-ante level of driving intensity, his death risk will be the one associated to a point like *Y*. However, if driving intensity is assumed to be a normal good, as Peltzman argued, the new equilibrium could be reached at a point like *Z* rather than *Y*, at which a higher risk is carried on relative to *Y*. In other words, since in *Y* the marginal benefits of taking precautions are reduced, the driver may want to increase it, thus reducing the effect of seat belts. This is precisely the Peltzman's offset hypothesis: while drivers may benefit from seat belts, other interested parties, such as pedestrians or passengers may face an increased risk, after the introduction of drivers' safety devices. The extent to which such offsetting behavior will apply, depends on a number of factors affecting the elasticity of the demand of driving intensity. According to Peltzman, 'whether the offset is partial, complete or even more than complete necessarily becomes an empirical matter'.

Since Peltzman's original contribution, many empirical works have investigated the existence of offsetting behaviors. The typical analysis performed has been devoted at empirically measuring risk taking behavior before and after the installation of technological innovations (Evans and Graham, 1991) such as drivers'seat belts, airbags, anti-lock brakes and so on. Some analyses have tested whether drivers who wear seat belts drive more recklessly. Given that the introduction of mandatory use of seat belts is generally associated with a reduction in overall traffic fatalities, scholars have long debated on how to disentangle the adverse behavioral feedbacks and the general observed trend in road accidents. One way of performing this analysis has been that of comparing (the reduction of) car occupants'injuries and (the increase of) non-occupants'injuries in road accidents (Cohen and Einav, 2003; Sen and Mizzen, 2007). The relationship between seat belt use and traffic fatalities is far from being conclusively ascertained. Indeed, while some studies document an increase in traffic fatalities as a consequence of an increase in seat belt use, some others reach completely different conclusions. Cohen and Einav (2003) found that an increase in seat belt use determines a reduction in traffic fatalities in general, thus finding no evidence of offsetting behaviors. Similarly, Houston et al. (1995), Sen (2001), and Young and Likens (2000) found that traffic fatalities in general benefit from the enactment of seat belt laws. Conversely, evidence of offsetting behaviors has been found by Garbacz (1990), and Sen and Mizzen (2007), who show a positive relationship between seat belt use and non-occupant death rates, as well as by Calkins and Zlatoper (2001) and Risa (1994) who show evidence of a positive relationship between occupant and non - occupant fatalities and seat belt use. Some investigations, such as Garbacz (1992) and Derrig et al. (2002), even find no relationship at all between seat belt use and traffic fatalities.

While the empirical literature on offsetting behaviors has generally focused on testing the hypothesis above, little of it explicitly addresses the relationship between enforcement design and the extent of offsetting behaviors (Campbell, 1988; Dee, 1998). Our focus here is on the impact of a specific enforcement design – the so-called point record driving license (PRDL) - on offsetting behaviors. Our purpose is trying to disentangle offsetting effects from the general trends observed in traffic law compliance, as well as in traffic fatalities.

3. The Point–Record Driving License

3.1 Description

A point-record driving license is a mechanism that imposes, besides fines, increasing nonmonetary sanctions (penalty points) for repeat offenders, by tracking drivers'offense histories through the progressive assignment of penalty points. Specifically, a PRDL can be defined as a mechanism through which authorities assign to a given infraction, a weight increasing with the number of previously detected infractions. The more serious are the offences, the greater is the number of points that are allocated against infringers. In some cases, as in traffic law enforcement in UK and in many US national legislations, infringers accumulate points up to a given threshold (totting-up system), after which the driving license is suspended. In some other countries, an opposite system holds, as drivers have an initial endowment of points, which they lose after violation occurs. In the latter case, once the original endowments of points is exhausted, a non-monetary sanction applies, i.e. drivers' licence suspension. As recently outlined by Bourgeon and Picard (2007), when individuals maximize the sum of their payoffs over different periods, drivers know that, under a PRDL system, if they have been caught committing a first offense, they will incur an immediate monetary sanction and a penalty which affects the initial endowments of points. Because of this record, drivers then know that any sanction they could face in the future for a subsequent offense, will be greater than it otherwise would have been. Indeed, the probability of having the driving license suspended increases with the penalty points cumulated (Haque, 1990; Polinsky and Rubinfeld, 1991; Polinsky and Shavell, 1998). Thus a PRDL is actually a mechanism that couples a system of *warnings* (Nyborg and Telle, 2004) with one of *specific enforcement* against repeat offenders. Under a PRDL the constraint faced by drivers is determined by the expected level of enforcement and by their current endowment of income and penalty points. In Peltzman's (1975) words, drivers then choose their 'driving intensity', according to the budget constraint they face over time, in terms of both fines and penalty points associated with that level of driving intensity. Thus, assuming that drivers know the enforcement probability (Polinsky and Shavell, 1998), they can calculate the risk of losing their license as the consequence of accumulating penalty points up to a critical threshold (Bourgeon and Picard, 2007).

A wide scholarly empirical research has investigated the impact of PRDL on drivers' incentives to comply with traffic law, reaching ambiguous results. The empirical literature available so-far outlines that, for any given level of detection, some deterrence effect is observed, under a PRDL regime. However, this effect is neither homogenous nor complete. In particular, a portion of infringers seems to change their behavior after a given threshold of penalty points has been reached. At the same time, some drivers systematically infringe traffic law rules even under a PPS. Several country-based studies provide evidence of positive impacts of a PPS in terms of its ability to increase deterrence (Haque, 1990; Zaal, 1994; Vaa, 2000; Zambon et al. 2008) and improve road safety (Poli de Figueiredo et al. 2001; Papaioannou et al. 2002; Hussain, 1990; and Zambon et al. 2007). Other empirical analyses show the potential of a PRDL to discriminate between different categories of drivers according to their offense propensity. Specifically, some analyses highlight that the PRDL allows to predict drivers' different likelihood of being involved in accidents or receiving convictions in subsequent periods on the basis of their respective points records (Chipman and Morgan, 1975; Chen et al. 1995; and Diamantopoulou et al. 1997). However, some recent empirical investigations show only temporary effects of PRDL on drivers' increased compliance (Lawpoolsri and Li, 2007; Benedettini and Nicita, 2009a, 2009b; Castillo-Manzano, Castro-Nuno, Pedregal, 2010).

3.2 The Italian legislation on PRDL

The Italian PRDL came into force in July 2003, following Decree Law No. 153/2003[1], with the aim of tackling the question of road safety in Italy. According to the European Commission, despite European countries display a common decreasing trend in traffic fatalities and injuries, Italy shows the second highest level of annual road fatalities among the 27 countries of the European Union, being second only to Poland, and above the most advanced European states, like Germany, France, and the UK[2]. Indeed, Italy emerged in 2007 as the country having the second highest number of road fatalities per year (5,131) among the 27 countries of the European Union, ranking second only to Poland (5,583 fatal accidents per year), and ahead of the most advanced European states, like Germany, France, and UK which recorded respectively 4,949, 4,620 and 3,823 road fatalities per year (Directorate for Energy and Transport, 2009). Similar remarks apply with reference to occupant and pedestrian fatalities whose relative dynamics is decreasing over time with reference to all EU – 27 economies. Actually, during the period 2001 – 2008 Italy displays the highest average number of passenger fatalities among EU – 27 countries[3], and ranks third with reference to both pedestrian and driver fatalities.

In Italy, the PRDL is characterized by the assignment to each driver of an initial credit of 20 points. Once a given offense is committed, the driver loses a number of points, which varies according to the severity of the offense committed. When the initial endowment of points is exhausted, the driver's license is not automatically suspended; drivers are merely required to attend a driving course and to pass a written and practical test within 30 days of the zeroing of their point endowment. The suspension occurs if, and only if, within the 30 days they fail to attend the driving course or do not pass the tests. During the time between the complete exhaustion of points and the driving tests, drivers are allowed to drive. Moreover, when several infringements are detected at once, no more than 15 points may be deducted, even though the total number of detected infractions could otherwise be enough to lead to the suspension of the driver's license. Nonetheless, the Italian Traffic Code also provides redemptive mechanisms such as the crediting of points every two years for

[1]Decree law no. 153/2003, "Modifiche ed integrazioni al codice della strada", published on the Italian Official Bulletin n. 149. Available at: www.parlamento.it/parlam/leggi/decreti/03151d.htm (in Italian).
[2]Directorate General for Energy and Transport, 2009. EU energy and transport in figures. Statistical pocketbook 2009. Available at: http://ec.europa.eu/transport/publications/statistics/doc/2009_energy_transport_figures.pdf .
[3] Source: our computation from Directorate for Energy and Transport, 2009. See supra note 2.

drivers who have kept a clean record. Specifically, if for two consecutive years a driver does not commit infractions entailing the deduction of points, the initial credit of 20 points is restored. When he does not commit infractions for two consecutive years and moreover he has maintained at least 20 points, he receives a further credit of two points[4].

Some recent empirical research has attempted to disentangle the deterrent effect of the introduction of PRDL in Italy. Farchi et al. (2007) evaluated the effectiveness of the introduction of the PRDL by studying the dynamics of admissions to hospital due to traffic accidents, in the region of Lazio. They found that the number of admission was 16% less than the previous year. Zambon et al. (2008), assessing the effects of the coming into force of the PRDL on the use of seat belts in the Veneto Region, in Italy, find that the new regime exerted a positive effect on the use of the seat belt. Benedettini and Nicita (2009, 2011) and De Paola *et al* (2010) also find a reduction in the trend of accidents after the introduction of PRDL in Italy. However, through *Lowess* estimates, Benedettini and Nicita (2009, 2011) found that the PRDL, in Italy, has exerted, *ceteris paribus*, a heterogeneous effect on traffic offenses as well as a vanishing one over time for some of them. Precisely, seat belt offenses are those violations that appear to have benefited the most by the introduction of the new sanction system, displaying a clear break in July 2003 as well as an indefinitely non-increasing trend over time. Conversely, offenses like speeding, dangerous speeding, driving under the influence of drugs, and driving under the influence of alcohol appear to have been positively affected only in a short - term window, around the time of the enactment of the new law. Indeed, they all display a clear indefinitely increasing trend over time beginning a few months after July 2003.

In the next sections, we empirically investigate whether the findings of Benedettini and Nicita (2009, 2011) are confirmed by a more accurate parametric analysis, by performing a regression discontinuity analysis. In other words, we examine through a parametric estimates whether and to what extent the introduction of a PRDL in Italy resulted in a reduction of road accidents and traffic offenses. Secondly, we study whether the type of drivers' reaction to the introduction of PRDL in Italy, has generated the kind of offsetting behavior described by Peltzman (1975).

[4] For a detailed analysis of the effectiveness of the Italian PRDL as an incentive - compatible mechanism, we refer to Benedettini and Nicita (2009, 2011) in which is accurately investigated the impact of PRDL on speeding offenses, disentangling the 'announcement effect' and the 'incapacitation effect'.

5. Empirical Analysis

5.1 Description of the data

Data on traffic law violations have been collected from the dataset of the Italian National Police, which provides evidence on the daily number of recorded tickets. Specifically, we focus on the following traffic law violations: (i) *speeding*[5]; (ii) *driving at a dangerous speed*[6]; (iii) *driving without seat belts*[7]; (iv) *driving under the influence of alcohol*[8]; and (v) *driving under the influence of drugs*[9].

These data are available with reference to the whole Italian road network, i.e. highways, state, regional, provincial, and municipal roads, from March 1st, 2001. Specifically, our analysis has been performed by employing monthly observations of the number of recorded tickets relative to the offenses mentioned in points (i) to (v).

To investigate the effects of the coming into force of the Italian PRDL on road accidents, and then to test the *Peltzman hypothesis* in our case, we used data on the monthly number of, respectively, road accidents and occupant and non–occupant fatalities and injuries. These data, provided by the Italian Institute of Statistics (2009), refer to the whole Italian road network and cover the period January 1st, 2001 – December 31st, 2008[10]. The monthly number of vehicle occupants involved in accidents refers to drivers and passengers who have suffered injuries or died as the result of an accident. Evidence on non–occupants relates to the monthly number of pedestrians who died or were injured in a crash.

In studying whether the introduction of a PRDL resulted in a reduction of offenses, we also took into account several factors, other than the change in the sanction policy itself, which may account in explaining the dynamics of traffic law violations. First of all, we considered the number of deployed cameras ($Cameras_t$) and police patrol cars ($Police_t$). Data on the implemented level of controls have been provided by the *Direzione della Polizia Stradale* (Traffic Police Directorate) and refer to the period January 1st, 2001 – September 30th, 2008[11]. In our regressions we control

[5]Art. 142 of the Italian traffic code, (ITC henceforth).
[6]Art. 141 of the ITC.
[7]Art. 172 of the ITC.
[8]Art. 186 of the ITC.
[9] Art. 186 of the ITC.
[10]At the time of writing, the Italian Institute of Statistics has yet to publish data on accidents occurring in 2009.
[11] Actually, recording rate is expected to be influenced by control intensity: i.e. as controls increase, the detection rate should increase as well.

for the implemented level of enforcement to capture changes in the enforcers'effort into detecting traffic offenses and thus to disentangle this effect from that of the point – record driving license which is our determinant of interest. Figures 3 and 4 illustrate, respectively, the time series of the monthly number of deployed police patrol cars and cameras. The vertical line individuates the coming into force of the new sanctioning system: i.e. July 2003. The dynamics of police patrol cars is clearly increasing over time[12]. Despite a different trend over time, similar remarks apply also with reference to cameras[13].

The relationship between control intensity and recorded traffic law violations is apparently not unambiguous by looking at Figures 2 - 4. Actually, we should expect that when the number of police patrol cars and cameras increase, recorded tickets raise as well. However, looking at Figure 2 we observe that only speeding, and driving under the influence of drugs and alcohol offenses register an increasing trend over time, while dangerous speeding and seat belt offenses appear as to be not correlated with the dynamics of cameras and police patrol cars.[14]. The regression analysis relative to the effect of the PRDL on traffic offenses helps to disentangle the relationship between recorded tickets and control intensity in an effective way[15]. Indeed, our findings confirm the correlations emerging from Figures 2 – 4 and show moreover that these correlations hold only with reference to the amount of deployed cameras. Specifically, it appears a positive and statistically significant correlation between the amount of speeding and drug driving offenses[16] and deployed cameras, while any

[12] The monthly average number of deployed patrols raises after July 2003. Actually, if we consider a two – years time window around the enactment of the new law, we have that the mean value of the of police patrol cars on duty is equal to 298677.90 before July 2003, and to 3158.21 in the 24 months after. Similarly, if we consider the whole period March 2001 – September 2008. The monthly average number of patrols is equal to 29468.80 in the period before the enactment of the new law, and to 357690.90 during the period July 2003 – September 2008.

[13] The monthly average number of cameras is equal to 3158.21 and to 3454.86 respectively in the two years before and after the coming into force of the PRDL. Same evidence if we consider the whole observation period March 2001 – September 2008. In the first sub – period the average number of cameras is equal to 3116.20, while it raises to 3234.59 during the sub – period July 2003 – September 2008.

[14]Actually, if we consider a two – years time window before and after the enactment of the point – record driving license we observe, respectively, the following monthly average number of tickets for the following traffic offenses: (i) speeding: 53925 and 75001; (ii) driving under the influence of drugs: 101.54 and 126.83; (iii) driving under the influence of alcohol: 1749.50 and 1929.50; (iv) seat belt offenses: 20830.46 and 9117.54; (v) dangerous speeding: 6742.21 and 5449.83. Moreover, these before and after averages confirm the vanishing effect over time of point – record driving license on some of the traffic offenses we consider.

[15]See Table 2.

[16]However the correlation with the recorded tickets for driving under the influence of drugs is statistically significant only at 10% level. Usually, drug driving offenses are detected by cameras as a consequence of speeding offenses. This may happen e.g. when mobile cameras are employed by police patrols.

statistically significant relationship occurs with reference to other offenses. Conversely, the amount of deployed police patrol cars seems to not affect the dynamics of recorded tickets despite the former dramatically increases over time. Findings on the effects of control intensity on recorded tickets are not of secondary importance given that enforcement is socially costly and it is crucial to channel public resources toward effective uses.

In our specifications, we also controlled for the potential volume of vehicles on roads by using data on the monthly number of circulating vehicles ($Veic_t$), as they appear from the register of the *Automobile Club d'Italia*[17].

In addition, our regressions include a variable capturing weather conditions, as measured by the average monthly level of precipitations occurred in Italy ($Prec_t$). These data have been obtained by averaging the daily amount of precipitation registered by each of the 187 meteorological stations located across the whole Italian territory[18].

In addressing empirically the effects of the adoption of the PRDL, we added in our regressions a further set of controls besides those just mentioned. We refer, first, to the monthly unemployment rate ($Unempl_t$), which has been used to capture the effect of economic conditions on traffic fatalities[19].

In addition, we employed the monthly variations in alcohol prices ($AlcPrice_t$) to control for the effect of alcohol consumption[20] on road accidents[21].

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[17]Automobile Club d' Italia (ACI), 2009. Dati e statistiche, (in Italian). Available at: http://www.aci.it/index.php?id=54. The monthly number of circulating vehicles has been computed by considering the total amount of circulating vehicles, as they resulted recorded at December 31st 2000, and by adding then, for every month, the number of newly registered vehicles minus the number of vehicles that have been removed from the register.

[18]Indeed In fact, weather conditions may be related to the number of recorded tickets and road accidents in several ways. Specifically, they may influence the frequency of accidents as well as drivers'perceived risk of accidents thus potentially affecting their willingness to violate traffic laws and, ceteris paribus, the effect on the number of recorded tickets. Data are provided by the European Climate Assessment & Dataset, ECA&D Available at: http://eca.knmi.nl. We would like to thank Vincenzo Scoppa for having provided suggestions on this control variable.

[19] Data on the monthly rate of unemployment come from the International Labour Organization database on labor statistics, LABORSTA. Actually, macro - economic conditions may affect the dynamics of accidents through different channels as well as in opposite directions. For example, an increase in the rate of unemployment may be associated with a reduction in traffic fatalities (Stuckler, 2009) and offenses, because of e.g. a reduction in people's opportunity costs (Cohen and Einav, 2003)), as well as in traveling (Ruhm, 2000). Conversely, a decrease in the unemployment rate, being associated with an improvement in well – being, may lead to the purchasing of safer vehicles and thus to a reduction of accidents (Sen and Mizzen, 2007). Or conversely, an increase in the unemployment rate may result in an increase in traffic fatalities and offenses because of an increase in alcohol consumption (see e.g. Carpenter and Dobkin, 2009; Arranz and Gil, 2009; Sen and Mizzen, 2007).

[20]We assume that alcohol demand is not inelastic, Fogarty (2009).

[21]Data on the variations in alcohol prices have been collected from the dataset used by the Italian Institute of Statistics to compute the yearly National Consumer Price Index Available at:

Data on the monthly level of resident population (Pop_t) and on the monthly number of resident males aged between 18 – 29 ($PopM1829_t$) are also included in regressions. Pop_t is a proxy for the overall potential volume of road users. $PopM1829_t$ accounts for the presence on roads of those individuals showing a higher probability of being involved in accidents: (i) male aged between 25 – 29. In Italy, this category of drivers displays the greatest likelihood of crash involvement (Italian Institute of Statistics, 2009); and (ii) younger male drivers, i.e. those aged between 18 - 24[22]. This in order to generalize with respect to the Italian scenario[23] and to take into account the findings of the literature on road safety which considers very young male as the most at risk of offense and accident involvement (Kirk and Stamatiadis, 2001; Lam, 2003; Masten and Hagge, 2004).

Finally we tested the robustness of our results by adding also the variable $GasPrice_t$ as proxy for traffic intensity. This variable captures monthly variations in gasoline prices. Although gasoline demand may be considered substantially inelastic (Brons et al. 2008), gasoline prices are usually employed in the literature on road safety as an exogenous proxy for traffic intensity. As with the price of alcohol price, data on changes in gasoline prices have been collected from the National Consumer Price Index dataset of the Italian Institute of Statistics[24].

To ensure that our time series are definitely free from seasonal effects (e.g. the dynamics of traffic tickets and road accidents may be driven by changes in traffic intensity because of holiday periods), we transformed both our dependent and independent variables by means of the moving average technique. Similar remarks apply for those controls like the unemployment rate, alcohol price, or precipitations, which, for differing reasons, are likely to be affected by seasonality. Given that our data refer to monthly observations, the time window employed for the moving average transformation is equal to twelve months.

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http://www.istat.it/salastampa/comunicati/in_calendario/precon/20100223_00/ . The reference year for variations in alcohol prices is 1995.

[22]We consider 18 as the lower bound of the interval because in Italy it represents the minimum driving age.

[23]Data on population come from the Italian Institute of Statistics Monthly Demographic Balance dataset and are available only from January 2002 onward (http://demo.istat.it/index_e.html). Monthly data on population for the year 2002 were kindly provided by Angela Silvestrini of the Italian Institute of Statistics. In fact, monthly data on population are publicly available only from January 2003 on. Data for the year 2001 are available only on a yearly basis. This is because after the October 2001 census, resident population in 2001 was recalculated to avoid problems of census representativeness. However, the computation was performed only on a yearly basis.

[24]See supra note 21.

The figures obtained thereafter were then transformed into logarithmic values in order to interpret the coefficient of our regressions as elasticities and to approximate as much as possible a normal distribution of the data.

Our balanced dataset covers the period March 2001 – September 2008. In Table 1 we report the main descriptive statistics concerning the variables employed in our regressions. Estimates have been performed by using STATA 10.

5.2 Methodology and empirical specification

We investigate the effects of the PRDL in two steps: first on traffic offenses and accidents and then on seat belt use and traffic fatalities.

A. The effects of the PRDL on traffic offenses and accidents

In this first step we elaborate on previous analyses on Italy (Benedettini and Nicita, 2009a, 2009b; De Paola et al. 2010), performing a Sharp Regression Discontinuity Analysis (Angrist and Pischke, 2009) and enriching the number of control variables. Specifically, the following specifications are drawn:

$$(1) \quad Off_t = \alpha + \gamma PRDL_t + \beta\Theta_t + \Omega t + \varepsilon_t$$

$$(2) \quad Acc_t = \delta + \theta PRDL_t + \psi\Theta_t + \mu t + u_t$$

where Off_t and Acc_t represent, respectively, the amount of recorded tickets and road accidents during month *t; $PRDL_t$* is a dummy variable which takes the value 1 for the months from the cutoff date (July 1st 2003) onward - i.e. from the entry into force of the PRDL onward - and 0 otherwise, which represents our treatment status; Θ_t is a set of controls including the variables $Cameras_t$, $Police_t$, $Veic_t$, $Prec_t$, $Unempl_t$, $GasPrice_t$, and $AlcPrice_t$, and the lagged dependent variable (up to three lags, arbitrarily chosen[25]) to account for autocorrelation; *t* is a variable accounting for the time trend.

Equation (1) has been estimated for all the five traffic offenses for which we evaluate the impact of PRDL: speeding ($Speed_t$); seat belt offenses ($Seat_t$); dangerous

[25]The decision to consider a number of lags up to three is the outcome of a trade – off between the willingness to control for autocorrelation and the necessity to ensure 'enough' degree of freedom, given the size of our sample, in order to guarantee efficient estimates as much as possible.

driving ($Dang_t$); driving under the influence of drugs ($Drug_t$); and driving under the influence of alcohol offenses (Alc_t).

The baseline regressions involve two years before and after the introduction of the PRDL, covering the period from July 2001 to June 2005.

We then tested the robustness of our results: (i) by considering different time windows around the entry into force of the new law, i.e. 12 months and 18 months before and after; (ii) by adding, in the specifications concerning road accidents, further controls, regarding population: i.e. Pop_t, and $PopM1829_t$. In all our specifications robust standard errors are computed to control for heteroskedasticity[26].

B. The effect of PRDL on seat belt use and traffic fatalities

In the second step, we estimate the relationship between seat belt use and traffic fatalities before and after the introduction of PRDL in Italy.

Among the several types of traffic law violations we considered, seat belt offenses are the only one which appear to have strongly benefited from the introduction of a PRDL (see Figure 2). Specifically, on the one side seat belt offenses' time series experiences a clear downward jump after the coming into force of the PRDL. On the other, relative to other infractions, it is the only time series that displays an indefinitely decreasing trend over time.

We then investigate whether traffic fatalities benefited from the observed increased use of seat belts. In particular, we compare, on the one side, the number of individuals suffering from road accidents, and on the other the emergence of any offsetting behavior. To this end we have investigated: (i) the relationship between seat belt offenses (as a proxy of seat belt use) and occupant and non – occupant fatalities during the overall observation period March 2001 – December 2008, as a measure of the occurrence of *offsetting behaviors* or *offsetting effects* due to an increase in the usage of personal safety devices ; (ii) the impact, if any, of PRDL regime on the relationship between seat belt offenses and traffic fatalities; (iii) the channels through which the mentioned impact of PRDL, if any, prevented or exacerbated the occurrence of *offsetting behaviors.*

[26] In all the performed regressions heteroskedasticity has been taken into account by estimating robust standard errors through the White estimator. Specifically, using STATA 10 as software for our computations, the correction has been accomplished by means of the regression option *robust.*

In order to test the *Peltzman Effect* for Italy we have estimated the following two Log – Log models, by means first of an OLS and then of a 2SLS estimator, as in Cohen and Einav (2003), and Sen and Mizzen (2007):

$$(3)\ Log\,(Occ_t) = \alpha + \gamma Log(Seat_t) + \beta Log(\Theta_t) + \mu t + \varepsilon_t$$

$$(4)\ Log\,(Non - Occ_t) = \delta + \theta\, Log(Seat_t) + \psi\, Log(\Theta_t) + \Omega\, t + u_t$$

where Occ_t and $Non - Occ_t$ are, respectively, the monthly number of occupants and pedestrians injured or killed as the result of an accident during month *t*; and Θ_t is a set of controls including $Police_t$, $Cameras_t$, $Unempl_t$, $Veic_t$, $Prec_t$, $GasPrice_t$, $AlcPrice_t$, in the baseline regressions, and the variables Pop_t, and $PopM1829_t$, we used to test the robustness of our results. The robustness of the baseline specifications, originally estimated for the whole period (March 2001 – September 2008), has been then tested by using alternative time windows around the entry into force of the PRDL (i.e. two years, 18 months, and 12 months before and after).

The 2SLS estimator has been used to take into account the outlined influence of the introduction of the PRDL in increasing seat belt use and thus to take into account the potential endogeneity between the dependent variables and seat belt use (Cohen and Einav, 2003; Sen and Mizzen, 2007).

To this end, we regress, in the first stage of our 2SLS regressions, the endogenous variable $Log(Seat_t)$ on the set of regressors employed in the second stage plus the dummy variable $PRDL_t$ and a first order lagged dependent variable (Sen and Mizzen, 2007).

In all the estimated specifications, robust standard errors are computed to control for heteroskedasticity[27], time trend, the lagged dependent variable (up to three lags), and two dummies to account for seasonal peaks are also included. Specifically, dummies refer to the months August and November (where the dependent variable is $Non - Occ_t$ and to February) and July (where the dependent variable is Occ_t[28]).

Once ascertained the relationship between seat belt offenses and traffic fatalities, we proceeded further in our analysis, by investigating whether and to what extent the PRDL affected this relationship, in particular by encouraging offsetting behaviors.

[27]See supra note 26.

[28]The emergence of these peaks can be easily detected through a plot of the corresponding time series.

To this end we estimated the previous specifications for two different sub–samples (covering, respectively, two years before and two years after the introduction of the PRDL). In order to to understand which type of hazardous driving behavior, if any, has been promoted by the coming into force of the PRDL, we estimated the following Log-Log model by means of a 2SLS:

$$(5) \quad Log\,(Off_t) = \delta + \theta\, Log(Seat_t)*PRDL_t + \psi\, Log(\Theta_t) + \Omega t + u_t$$

In equation 5, Off_t is the number of recorded infractions at time t for speeding, dangerous speeding, driving under the influence of drugs, and driving under the influence of alcohol. $Log(Seat_t)*PRDL_t$ is an interaction term which captures contextually the occurrence of the two conditions we analyze (the variation in seat belt use, as proxied by the number of seat belt offenses $Log(Seat_t)$ and the adoption of the PRDL regime, captured as usual by the dummy variable $PRDL_t$). Actually, whether negative and statistically significant, the coefficient of this interaction term tells us that a 1% reduction in seat belt offenses determines an increase in traffic offenses related to hazardous driving behaviors, which is greater than it would have occurred in the absence of a PRDL, our reference category, by a percentage equal to the value of the mentioned coefficient. Θ_t includes a set of controls like $Cameras_t$, $Police_t$, $Veic_t$, $Prec_t$, $AlcPrice_t$, $Unempl_t$ and $GasPrice_t$.

We then exploited a 2SLS estimator in order to take into account the potential endogeneity existing between different driving behaviors[29]. Specifically, the variable $Log(Seat_t)*PRDL_t$ has been instrumented by using all the variables included in the second stage of our 2SLS regressions, i.e. the regressors in the right hand of eq. (5) plus the dummy variable $PRDL_t$ to account for the documented effect of the PRDL on seat belt offenses.

6. Estimation results

In this section we report the results of our econometric analyses concerning: (A) the effect on traffic offenses and road accidents of the introduction of the PRDL in

[29] For example it may happen that one drives faster because he or she wears a seat belts, i.e. offsetting behavior, but it might be also the case that one wears the seat belt because he or she decides to drive faster. Similar remarks apply for the other risky potentially dangerous behaviors we examined.

Italy on July 1st, 2003, and (B) the relationship between seat belt offenses and traffic fatalities.

A. The effects of the PRDL on traffic offenses and accidents

In Table 2 we illustrate the results of our empirical analysis. We present, first, the findings of our baseline regressions, and then the robustness checks consisting in the estimation of the original models using different time windows around the entry into force of the new law.

Our results show that the coming into force of the PRDL did not affect all offenses in the same way. Indeed, consistent with Figure 2, the traffic law violation that benefited the most from the new sanctioning policy is seat belt offense. Actually, when shifting from a regime without PRDL to one with PRDL, we observe a decrease of about 68.80% in seat belt offenses[30]. Besides, driving under the influence of alcohol and speeding offenses also benefited, though to a lesser extent, from the introduction of the PRDL, experiencing reductions of 21.41% and 20.68% respectively in the two years following its adoption.

Our robustness checks confirm the results of the baseline regressions and confirm also the vanishing effect (Benedettini and Nicita 2009, 2011) over time of the PRDL that occurred for some traffic offenses. Indeed, when moving from a 12 - months to a 18 - months and then 24 - months time window (Tables 3 and 2), the coefficient of the variable $PRDL_t$, capturing the treatment status, tends to decrease for all offenses with the exception of seat belt offenses. In addition, it emerges that for driving under the influence of drugs and dangerous speeding offenses the introduction of the PRDL exerted a deterrent effect only in the period immediately following the entry into force of the new law (i.e. when examining a 12 - months time window, see Table 3). Indeed, it appears that when considering broader time windows, the coefficient of our treatment variable loses significance. Unlike all other offenses, i.e. dangerous speeding

[30] To interpret in terms of elasticity the coefficient of a dummy variable of a Semi – Log model the following reasoning applies. Suppose we have the model: $\ln(Y_t) = \alpha + \sigma X_t + \beta D_t + u_t$, where α is the constant term, X is a discrete or continuous variable, and D is a dummy variable. We can then express Y_t as follows: $Y_t = Exp[\alpha + \sigma X_t + \beta D_t + u_t]$, which yields $Y_t = e^{\alpha + \sigma X_t + \beta D_t + u_t} = e^{\alpha} e^{\sigma X_t} e^{\beta D_t} e^{u_t}$.
If we want to know what is the % change in Y_t when D shift from 0 to 1 the following computation applies: $Y(D = 1) / Y(D = 0) = (e^{\alpha} e^{\sigma X_t} e^{\beta D_t} e^{u_t}) / (e^{\alpha} e^{\sigma X_t} e^{u_t}) = e^{\beta D_t} = e^{\beta} = 1 + \beta$. Since the ratio is approximately $(1 + \beta)$ the percentage increase in Y_t due to the enactment e.g. of a PRDL (i.e. D shifts from 0 to 1) is approximately β. For example if $\beta = 0.06$, then a shift of D from 0 to 1 raises the dependent variable by approximately 6%.

and driving under the influence of alcohol, the effect progressively decreased, while for seat belt offenses it tends to rise, albeit with a different magnitude. Specifically, it appears that the reduction in seat belt offenses rises from 0.42 offenses to 0.52 points in passing from a 12 - months to a two - years window.

Table 4 presents the results concerning the effect of the adoption of the PRDL on road accidents. Our estimates show that the adoption of PRDL is associated with a reduction of about 0.10 points in accidents, corresponding to a decrease of 10.23%. This percentage tends to rise, until about 15.95%, when, testing for the robustness of our results, we added further controls in our regressions. Robustness checks also show that actions aimed at reducing alcohol consumption – i.e. an increase in alcohol prices – enhance road safety. Indeed, it emerges that an increase rise of 1% in alcohol price is associated with a reduction of about 9.40% in road accidents. Our regressions also confirm that males aged 18 – 29 are significantly exposed to accident risk. Indeed an increase of 1% in the number of males aged between 18 – 29 in the population corresponds, *ceteris paribus*, to an increase of about 1.04% in accidents[31].

We then tested the robustness of our results by using different time windows around the enactment of the PRDL (see Table 5). These robustness checks further confirm our findings. In addition, when looking at the OLS estimates, it appears that the positive effect on accidents, as for offenses, is decreasing over time. Indeed, the coefficient of the variable $PRDL_t$ decreases when passing from a 12 - month to a 18 - months and then 24 - months time window. Moreover, the effect of alcohol prices tends to lose significance, while that relating to the influence of the level of resident population and of the number of males aged between 18 – 29 tend, generally, to maintain their statistical significance[32].

[31] The interpretation in terms of elasticity of the coefficient of a non – dummy variable of a Semi – Log Model, as model (1) and (2), does not require any computation. Actually, in this hypothesis the coefficient itself can be directly interpreted as an elasticity. In this case the coefficient, let's say β, is an approximation of: dln(y)/dln(x)=([y(t)-y(t-1)]/y(t-1))/([x(t)-x(t-1)]/x(t-1)) from which follows: ([y(t)-y(t-1)]/y(t-1)) = β*([x(t)-x(t-1)]/x(t-1)). Then, if we want to know what happens in the dependent variable when the independent variable faces a 1% variation, the following relationship holds: ([y(t)-y(t-1)]/y(t-1))= β*1%= β%

[32] Their coefficients appear to be not statistically significant only in the very short run, i.e. when a 12 - months time window, before and after the enactment of the PRDL, is considered. This probably occurs because of a reduced number of observations and consequently of degree of freedoms.

B. Seat belt use and traffic fatalities

Table 6 illustrates the baseline regressions concerning the relationship between seat belt use and traffic fatalities under a PRDL, distinguishing between occupant and non-occupand fatalities. Our OLS and 2SLS estimates show a significant *Peltzman effect* for Italy, as seat belt use resulted to be positively related to occupant fatalities but negatively related to non–occupant fatalities. Specifically, the positive and statistically significant coefficient of the variable $Log(Seat_t)$ tells us that an increase in seat belt offenses is related to an increase in occupant fatalities (or, equivalently, that a reduction in seat belt infractions, and thus an increase *ceteris paribus* in seat belt use, is associated with a reduction in occupant fatalities). Conversely, when non-occupant fatalities are considered, we observe a negative relationship with seat belt use. Specifically, the negative and statistically significant coefficient of the variable $Log(Seat_t)$ indicates that an increase in seat belt use (i.e. a reduction in seat belt offenses), is associated with an increase in pedestrian fatalities (correspondingly, it can be stated that a reduction in seat belt usage, and thus an increase in seat belt offenses, is associated with a reduction in non–occupant fatalities).

Specifically, we observe that an increase of 1% in seat belt use determines a reduction of 0.13% in occupant fatalities, but an increase of 0.18% in pedestrian injuries and deaths.

Moreover, the relevance of the observed offsetting behavior is further outlined by the analysis of two-years time window around the entry into force of the new law. Indeed, while the average number of occupant fatalities declined by an amount equal to 6%, the average number of non–occupant fatalities increased by an amount equal to 5%, after July 1st, 2003, i.e. after the adoption od PRDL.

Our results concerning the relationship between traffic fatalities and seat belts usage appear to be robust to the introduction of further controls in the specifications (Tables 7 and 8), and to the adoption of different time windows[33] around the introduction of the PRDL (Tables 9 and 10).

The performed baseline estimates also reveal that macroeconomic conditions, as measured by the monthly unemployment rate $Unempl_t$, are negatively associated with occupant fatalities but show no relationship of any kind with non – occupant fatalities.

[33] We focused, respectively, on a 24 - months, 18 – months, and 12 - months time window around the introduction of the PRDL.

Also of interest is the role of different types of enforcement 'devices' on road fatalities. Actually, while occupant fatalities appear not to be sensitive to the number of cameras or police patrol cars deployed, non – occupant fatalities are positively related to the amount of police patrol cars on duty but negatively to the number of deployed cameras .

Moreover, our estimates point out that, generally, an increase in the number of circulating vehicles, as proxied by the variable $Veic_t$, is positively associated with the volume of both non – occupant and occupant fatalities. In addition, policies aimed at reducing alcohol consumption, through e.g. an increase in alcohol price, appear to benefit non – occupant fatalities, as pointed out by the negative and statistically significant coefficient of $AlcPrice_t$ (Table 8). When we test the robustness of our results by adding further controls, we observe that an increase in the number of males aged between 18 – 29 in the population is positively associated with the number of occupant fatalities, thus supporting the view that young male drivers have a substantially higher risk of being involved in accidents (Begg and Langley, 2001; Harre et al., 2000).

C. The impact of point-record mechanism on offsetting behaviors

Finally, Table 11 reports our major result, given by the existence of offsetting behavior measured by a statistically significant inverse relationship between seat belt offenses and non-occupant fatalities, in two different sub-periods: two years before and two years after the adoption of PRDL.

While we find no relationship between seat belt offenses (and thus *ceteris paribus* seat belt use) and occupant and non–occupant fatalities before the adoption of PRDL, a strong offsetting behavior is found within the two years after the adoption of the PRDL.

Seat belt use turns out to reduce occupant fatalities while increasing non–occupant fatalities. More precisely, in the two years after the coming into force of the PRDL regime, a reduction of 1% in seat belt offenses is associated with an increase of 0.34% in non – occupant fatalities and with a decrease of 0.28% in occupant fatalities. These results strongly confirm our hypothesis of a direct effect of the PRDL on offsetting behaviors. Our findings indeed suggest that large part of the effect of seat belt use on traffic fatalities, during the overall period (see Table 6). Finally, we also investigated the nature of the hazardous driving behaviors generated by seat belt use. We found that offsetting behaviors have occurred with reference to two types of driving behaviors: dangerous speeding and driving under the influence of alcohol (table 12).

Actually, the negative and statistically significant coefficient of the variable $Log(Seat_t)*PRDL_t$ tells us, first, that a decrease in seat belt offenses is positively associated with an increase in dangerous driving and driving under the influence of alcohol offenses; secondly, it shows that this increase in driving under the influence of alcohol and dangerous speeding is greater than it would have occurred in the absence of a PRDL, i.e. in the months before its introduction[34]. Specifically, this increase is equal to 0.02% for dangerous speeding and to 0.007% for driving under the influence of alchol offenses.

6. Discussion and Conclusions

In previous sections we have investigated the relationship between traffic law enforcement design and the emergence of offsetting behavior, focusing our empirical analysis on the Italian case, where a PRDL has been introduced in 2003.

In particular, we have analyzed first, (i) whether PRDL generates deterrence on drivers' infractions and, if so, (ii) whether the deterrent effect may in turn produce offsetting behaviors.

Following the empirical literature on the 'Peltzman effect', we have focused our analysis on the role that 'selective compliance' to traffic law, such as an increased use in seat belt, may play as a determinant of drivers' offsetting behavior, measured by non-occupant fatalities. We contribute to the empirical literature on the Peltzman effect, by investigating the relationship between the adoption of PRDL and the emergence of offsetting behaviors through the channel given by the increased use in seat belts.

Our findings show that:

(i) the introduction of PRDL system in Italy resulted to play a deterrent role on drivers' behavior;

(ii) such deterrent effect has been however 'selective' in Italy, as drivers reacted to the new system, by strongly increasing their use of seat belt, leaving almost unaffected other driving behaviors (as speeding, dangerous speeding and drunk driving);

[34] Being it an interaction term with the dummy $PRDL_t$ which captures the presence of the PRDL.

(iii) the observed selective compliance with seat belt, whereas reducing fatalities on the aggregate, has nonetheless generated additional fatalities (for non-occupants) as the result of offsetting behavior.

From the above results, we derive the following conclusions. First, the introduction of PRDL in Italy has increased deterrence even for those violations already sanctioned by the pre-existing enforcement regime. According to our analysis, Italian drivers appear to have changed their behavior under a PRDL system, almost exclusively reative to their seat belt usage, i.e. to a safety device that the empirical literature envisages as a potential source of offsetting behavior. It is then important to understand whether the observed selective compliance on seat belt usage and its subsequent offsetting effects, might be an important distinctive feature of a PRDL system, so-far neglected by the traffic enforcement literature.

Indeed, an interesting puzzle raised by our investigation concerns precisely the selective compliance issue, after the introduction of PRDL: if penalty points do exert a deterrent effect, why drivers only selectively complied with the law, by increasing their seat belt usage? In other terms, if drivers care about their 'budget' of penalty points - which is the main novelty introduced by the PRDL - why they start wearing seat belts, while continuing speed driving? Furthermore, in the case we analyze, the increase in seat belt use generated additional dangerous speeding and driving under the influence of alcohol.

The explanatory hypothesis that might be advanced for the observed selective compliance with seat belts is that the mechanism of PRDL may have itself induced only selective compliance on drivers' side. In this respect, a sort of 'substitution effect' might have occured. Indeed, extending the Peltzman's argument to the adoption of PRDL, one could argue that the adoption of this enforcement regime might have induced drivers to strategically adapt to the new sanctioning regime, by selecting their traffic violations so as to allocate the largest amount of penalty points to those traffic offenses that generate the highest benefits-costs ratio to the driver. Under this explanatory hypothesis, by complying selectively with the law, drivers may have 're-directed' to the most 'rewarding' traffic law violations (i.e. speeding, dangerous speeding, etc.), the penalty points that would have been 'spent' in seat belt offenses, in the absence of PRDL.

Under this interpretation, the decision to drive faster (Tarko, 2009), more intensely or with less precaution, is equivalent to a decision to "spend", in most rewarding uses, some of the of the penalty points and money 'saved" by drivers when enhancing their compliance with seat belts use. Should this interpretation reveal to be correct, this sort of strategic 'substitution effect' generated by a PRDL would then be responsible for the observed offsetting behavior, through the increased use of safety device.

Recent analyses conclude that drivers' choice of speed stems from a surrounding trade-off between time opportunity costs, the perception of accident risk, and the expectations about the actual level of enforcement (Tay, 2005; Tarko, 2009). Moreover, speeding behavior has revealed to be particularly responsive to offense-history sanction mechanisms (Redelmeier *et al.*, 2003).

Should the above interpretation reveal to be robust and convincing, it would outline an important drawback of the PRDL system that policy makers have to take into account: while saving lives within the car, the PRDL system might increase injuries for non-occupants.

The above explanatory hypothesis might nonetheless show two main weaknesses. First, it should be taken into account the circumstance that drivers react to the global 'institutional' environment of traffic rules (Dionne *et al*, 2011) and that disentangling monetary and non-monetary incentives in our analysis (attributing then only to penalty points the 'strategic substitution effect' under a PRDL system) is almost impossible, due to our data[35].

Moreover, as the introduction of PRDL in Italy was also associated to an increase of monetary sanction for seat belt offenses, it is extremely difficult to isolate the deterrent impact of each single incentive mechanism (fines, and point-record driver's licenses). Nonetheless, it is important to clarify that the significant and long-lasting increase observed in seat belt usage in Italy cannot be exclusively attributed to increase occurred in fines, for some violations, under the adoption of the PRDL. Indeed, should that be the case we would have observed a similar pattern with reference to all the infractions whose fines increased, i.e. dangerous speeding offenses[36]. However, for dangerous speeding offenses, we have observed an opposite trend relative the one that characterised seat belt infractions, with a limited and vanishing reduction over time of the corresponding tickets (see Table 2 and 3). This counterfactual example over the dynamics of dangerous speeding infractions, supports

[35] We thank one anonymous referee for having outlined this point.

[36] See Decree law no. 153/2003, "Modifiche ed integrazioni al codice della strada", published on the Italian Official Bulletin n. 149. Available at: www.parlamento.it/parlam/leggi/decreti/03151d.htm (in Italian).

the interpretation that what has had the relative major impact - once controlled for other relevant explanatory variables – in explaining the heterogeneous dynamics over time of traffic offenses is to be found in the specificity of the PRDL system.

Therefore, we can conclude that the dramatic change in drivers' seat belt usage in response to the coming into force of the new sanctioning system outlined a good case for testing the Peltzman hypothesis and its link with the enforcement design.

We leave to future research the verification of the above interpretation, and in particular the interdependence between the enforcement design associated to a PRDL system and the emergence of offsetting behaviors.

The main conclusion that our investigation allows us to derive is that, when the deterrent effect of PRDL is mainly captured by drivers' compliance on the usage of safety devices, as seat belts, some offsetting behaviors may arise, partially crowding out the benefits of PRDL. Given the widespread adoption of the point – record driving license, we believe this is an urgent message to be delivered to policy makers.

If a sort of 'substitution effect' among infractions, as the one here suggested, is associated to the PRDL system, then, in order to enhance deterrence, the enforcement design shall prevent this 'strategic substitution'. One possibility, to be explored, might be that of excluding penalty points from offenses related to the use of personal safety devices such as the seat belt, relying only to monetary sanctions. Our data do not allow to go further in this direction to test our explanatory hypothesis and our conjecture on the policy options needed to prevent offsetting behavior. However our results over the co-existence of PRDL and the emergence of offsetting behavior observed in Italy may shed new lights over a neglected consequence of one of the most diffused traffic enforcement system, and indicate an important direction for future research on road safety.

Figures

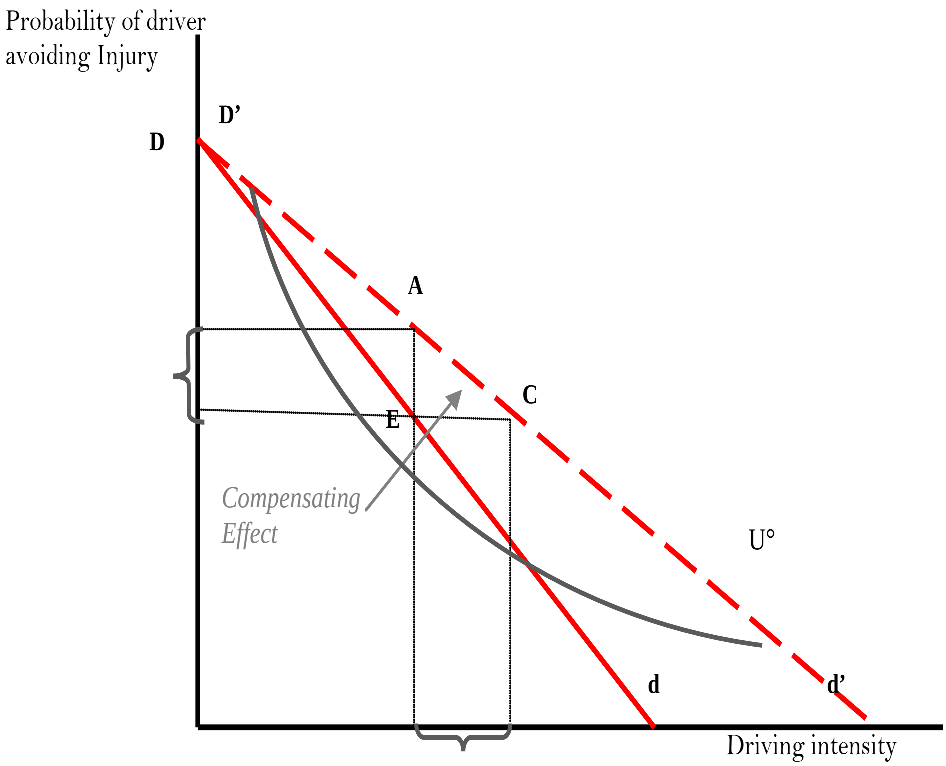

Figure 1: Compensating effect under a point – record driving license.

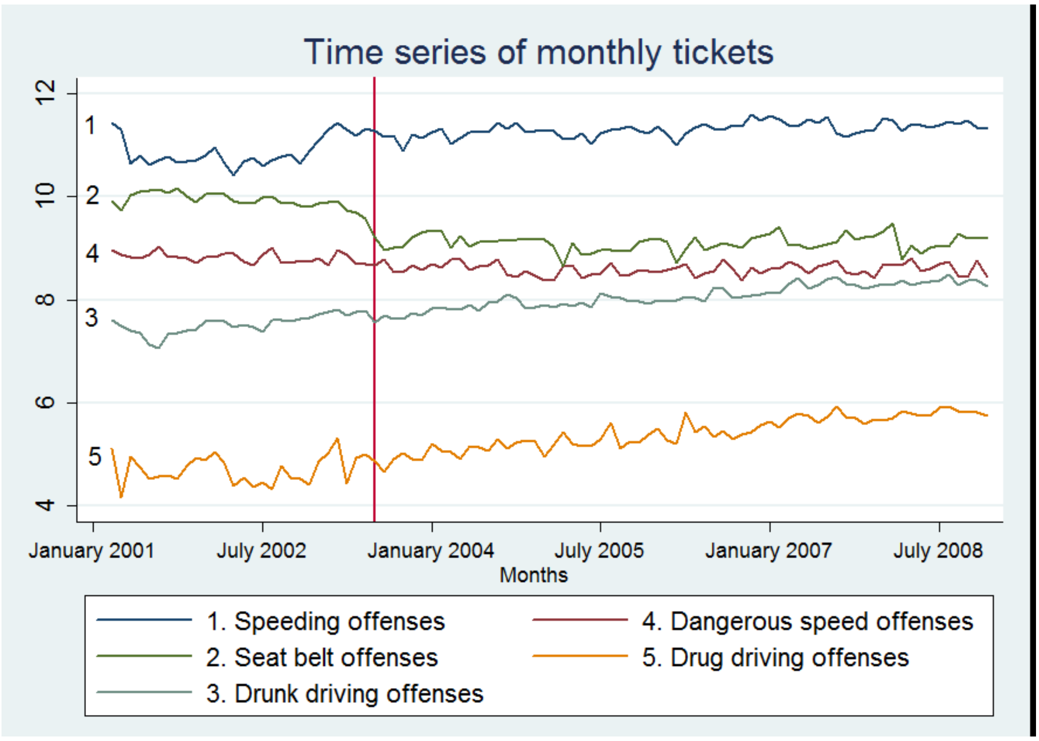

Figure 2: Traffic offenses in Italy. Period: March 2001 - December 2008. The red line indicates the coming into force of the point – record driving license. *Source*: Italian National Police dataset.

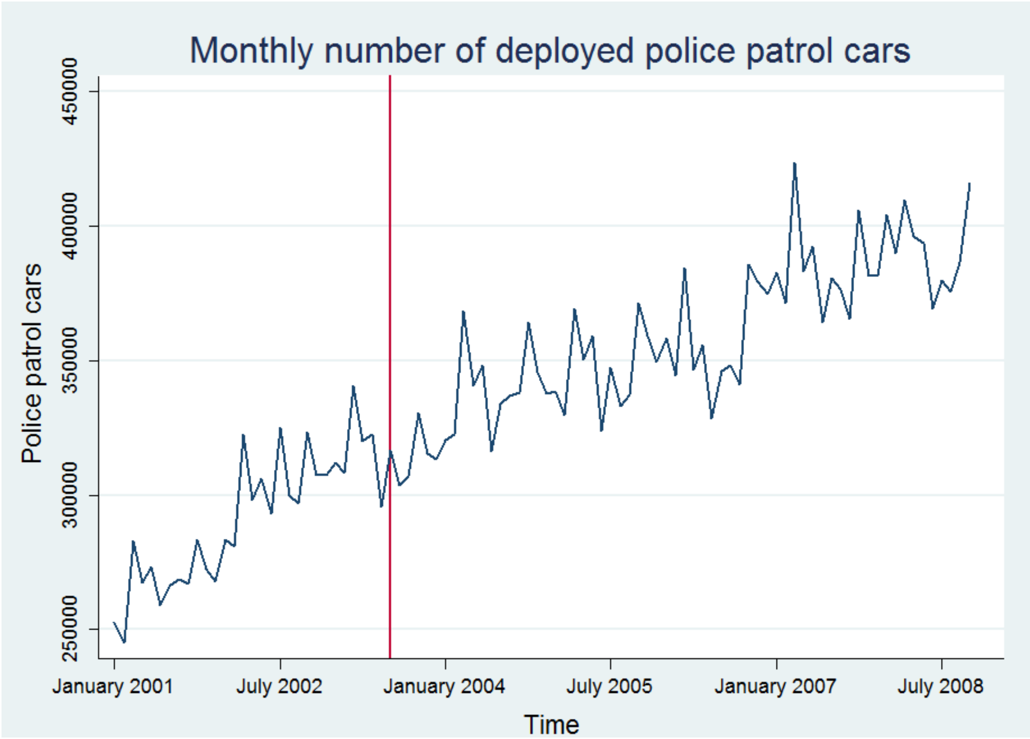

Figure 3: Police patrol cars on duty. Period: March 2001 - December 2008. The red line indicates the coming into force of the point – record driving license. *Source*: Traffic Police Directorate.

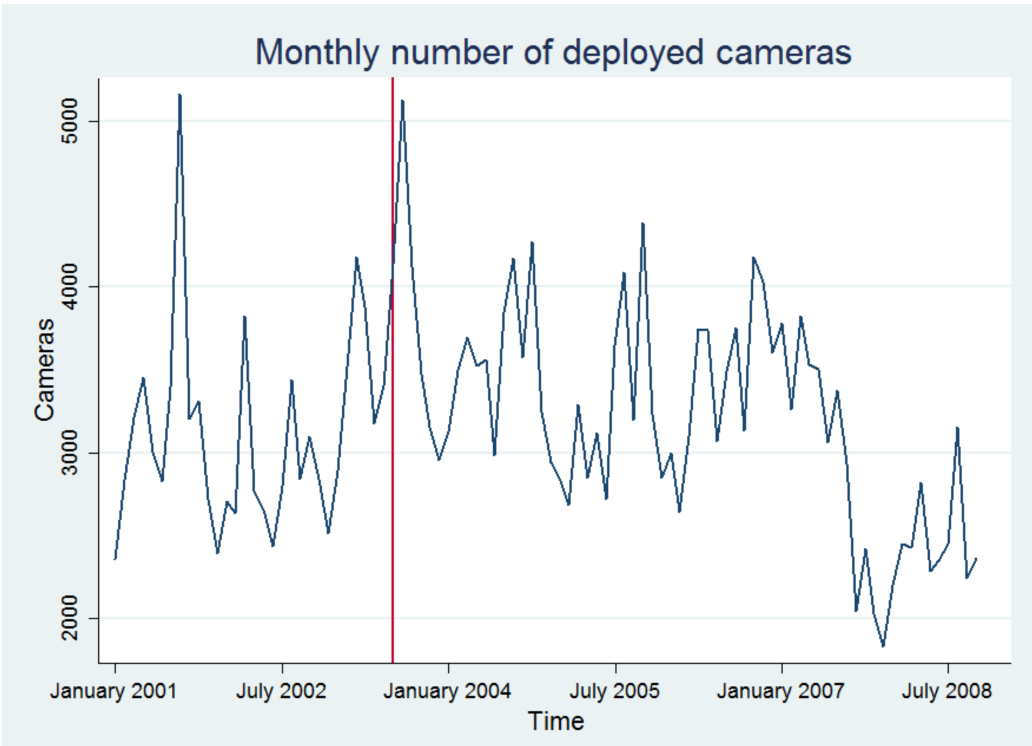

Figure 4: Deployed cameras. Period: March 2001 - December 2008. The red line indicates the coming into force of the point – record driving license. *Source*: Traffic Police Directorate.

Tables

Table 1: Descriptive statistics

Period March 2001 – September 2008

Variable	Observations	Mean	Sd. Dev.	Max	Min
Speeding offenses	91	74349.82	19880.07	30678	109379
Seat belt offenses	91	12834.63	6033.17	5973	28103
Drunk driving offenses	91	2057.65	463.6	1404	3410
Drug driving offenses	91	146.23	54.23	55	304
Dangerous speed offenses	91	5858.78	785.69	4187	8053
Road accidents	91	19452.76	2104.88	15869	23734
Occupant fatalities and inuries	91	26273.33	2843.71	20889	32102
Non – occupant fatalities and inuries	91	1660.14	300.83	930	2415
Police patrols cars	91	338296.7	39273.79	258895	423675
Cameras	91	3209.59	637.5	1833	5161
Vehicles	91	45000000	2266468	40900000	48500000
New registered vehicles	91	259225.2	54393.02	125212	476037
Precipitations	91	24.7	14.1	3.62	71.13
Unemployment rate	91	6.95	1.21	5	9.6
Alcohol price	91	129.31	6.98	117.23	143.57
Gasoline price	91	135.82	17.99	112.8	184.05
*Resident population**	81	58600000	880055.7	57000000	59900000
*18 – 29 years old male population **	81	4134125	237049	3937225	4573171

Table 2: The effect of PRDL on traffic offenses

	Period: July 2001 – June 2005				
Dep. Var.:	**Log(Speed$_t$)**	**Log(Seat$_t$)**	**Log(Drug$_t$)**	**Log(Alc$_t$)**	**Log(Dang$_t$)**
PRDL$_t$	-0.188** (0.073)	-0.524*** (0.110)	0.167 (0.195)	-0.194*** (0 .058)	-0.110 (0.086)
Log(Cameras$_t$)	0.920*** (0.246)	0.237 (0.186)	0.105 (0.291)	0.365* (0.199)	0.078 (0.181)
Log(Police$_t$)	-0.272 (0.528)	0.385 (0.430)	0.673 (0.536)	0.443 (0.268)	-0.369 (0.367)
Log(Veic$_t$)	-8.755 (11.031)	5.725 (8.981)	17.540 (15.577)	12.670 (9.293)	-4.261 (9.581)
Log(Prec$_t$)	0.028 (0.051)	0.054 (0.033)	-0.140* (0.073)	0.039 (0.026)	-0.002 (0.034)
Log(Unempl$_t$)	-0.641 (0.388)	-0.584 (0.435)	1.302* (0.680)	-0.080 (0.348)	-0.264 (0.386)
Log(AlcPrice$_t$)	9.378 (5.466)	-1.085 (4.084)	-0.388 (6.983)	-1.647 (4.715)	4.189 (5.301)
Log(GasPrice$_t$)	0.249 (0.827)	0.763 (0.721)	0.301 (1.309)	-1.370** (0.612)	-0.162 (0.877)
Obs.	48	48	48	48	48
R sq.	0.85	0.96	0.71	0.89	0.66

Notes: All specifications include a first order time trend and lagged dependent variable lags (up to 3 lags) to control for autocorrelation. Standard errors (reported in parentheses) are corrected for heteroskedasticity. The symbols ***, **, * indicate that coefficients are statistically significant, respectively, at the 1, 5, and 10 percent level.

* Data on *Pop$_t$* and *PopM1829$_t$* are available since January 2002.

Table 3: The effect of PRDL on traffic offenses
Different time – windows around the enactment of the PRDL

	Period: July 2002 – June 2004				
Dep. Var.:	**Log($Speed_t$)**	**Log($Seat_t$)**	**Log($Drug_t$)**	**Log(Alc_t)**	**Log($Dang_t$)**
$PRDL_t$	-0.234** (0.116)	-0.422*** (0.158)	-0.475** (0.156)	-0.348*** (0.095)	-0.259** (0.115)
Log($Cameras_t$)	0.100** (0.406)	-0.272 (0.423)	-0.093 (0.258)	0.488 (0.268)	-0.010 (0.264)
Log($Police_t$)	-0.935 (1.087)	0.395 (0.823)	-0.103 (0.900)	-0.010 (0.426)	-1.208 (0.707)
Log($Veic_t$)	-22.423 (29.780)	-14.155 (1.439)	-10.859 (19.849)	7.782 (19.071)	-9.129 (16.697)
Log($Prec_t$)	-0.017 (0.053)	-0.013 (0.053)	-0.193* (0.095)	0.021 (0.037)	0.011 (0.074)
Log($Unempl_t$)	-0.288 (0.624)	-0.207 (0.801)	1.848*** (0.498)	-0.086 (0.353)	-1.299** (0.382)
Log($AlcPrice_t$)	18.786 (18.687)	12.756 (11.627)	18.790 (10.259)	-1.183 (11.950)	2.487 (8.873)
Log($GasPrice_t$)	0.876 (1.249)	1.310 (1.090)	-1.861 (1.144)	0.008 (0.911)	0.746 (0.990)
Obs.	24	24	24	24	24
R sq.	0.86	0.97	0.93	0.90	0.73
	Period: January 2002 – December 2004				
Dep. Var.:	**Log($Speed_t$)**	**Log($Seat_t$)**	**Log($Drug_t$)**	**Log(Alc_t)**	**Log($Dang_t$)**
$PRDL_t$	-0.267** (0.105)	-0.468*** (0.115)	0.178 (0.425)	-0.227*** (0.044)	-0.174 (0.111)
Log($Cameras_t$)	1.063*** (0.344)	-0.008 (0.270)	-0.178 (0.425)	0.440*** (0.123)	-0.150 (0.234)
Log($Police_t$)	-0.980 (0.752)	0.496 (0.520)	0.285 (0.801)	-0.066 (0.404)	-0.505 (0.746)
Log($Veic_t$)	-5.842 (13.200)	10.270 (8.078)	27.736 (18.155)	23.475*** (6.734)	-7.953 (11.798)
Log($Prec_t$)	0.034 (0.081)	-0.002 (0.029)	-0.141 (0.108)	0.028 (0.024)	-0.035 (0.049)
Log($Unempl_t$)	-0.454 (0.531)	-0.207 (0.435)	1.677** (0.711)	0.187 (0.287)	-0.570 (0.432)
Log($AlcPrice_t$)	9.078 (6.832)	-4.080 (4.985)	-1.675 (8.414)	-7.860** 3.403	6.783 (5.556)
Log($GasPrice_t$)	0.333 (1.152)	0.714 (0.647)	-0.237 (1.946)	-0.693 (0.609)	-0.081 (1.187)
Obs.	36	36	36	36	36
R sq.	0.85	0.97	0.91	0.91	0.65

Notes: All specifications include a first order time trend and lagged dependent variable lags (up to 3 lags) to control for autocorrelation. Standard errors (reported in parentheses) are corrected for heteroskedasticity. The symbols ***, **, * indicate that coefficients are statistically significant, respectively, at the 1, 5, and 10 percent level.

Table 4: The effect of PRDL on accidents

	Period: July 2001 – June 2005		
Dep. Var.:	**Log(Acc_t)**	**Log(Acc_t)**	**Log(Acc_t)**
$PRDL_t$	-0.098*** (0.029)	-0.103*** (0.033)	-0.148*** (0.030)
Log($Cameras_t$)	-0.053 (0.057)	-0.138* (0.078)	-0.076 (0.067)
Log($Police_t$)	-0.036 (0.106)	0.068 (0.182)	-0.004 (0.176)
Log($Veic_t$)	-1.046 (2.339)	-2.532 (2.598)	-1.321 (2.537)
Log($Prec_t$)	-0.012 (0.012)	-0.023* (0.013)	-0.019 (0.011)
Log($Unempl_t$)	0.156 (0.101)	0.256** (0.109)	0.154 (0.093)
Log($AlcPrice_t$)	-0.153 (1.243)	0.292 (1.635)	-9.394* (4.634)
Log($GasPrice_t$)	0.107 (0.140)	-0.003 (0.197)	0.152 (0.226)
Log(Pop_t)		1.949 (1.930)	32.455** (13.571)
Log($PopM1829_t$)			1.035** (0.454)
Obs.	48	42	42
R sq.	0.63	0.68	0.79

Notes: All specifications include a first order time trend and lagged dependent variable lags (up to 3 lags) to control for autocorrelation. Standard errors (reported in parentheses) are corrected for heteroskedasticity. The symbols ***, **, * indicate that coefficients are statistically significant, respectively, at the 1, 5, and 10 percent level.

Table 5: The effect of PRDL on accidents – Robustness checks
Different time – windows around the enactment of the PRDL

	Period: January 2002 – December 2004		
	(I)	**(II)**	**(III)**
$PRDL_t$	-0.116*** (0.035)	-0.129*** (0.036)	-0.165*** (0.034)
$Log(Cameras_t)$	-0.045 (0.085)	0.001 (0.087)	0.021 (0.089)
$Log(Police_t)$	0.130 (0.161)	0.029 (0.168)	-0.052 (0.174)
$Log(Veic_t)$	-3.061 (2.861)	-5.805 (3.335)	-4.486 (3.162)
$Log(Prec_t)$	-0.015 (0.012)	-0.017 (0.013)	-0.015 (0.012)
$Log(Unempl_t)$	0.151 (0.135)	0.157 (0.125)	0.144 (0.11)
$Log(AlcPrice_t)$	1.868 (1.407)	0.816 (1.607)	-9.325* (4.952)
$Log(GasPrice_t)$	-0.060 (0.181)	0.023 (0.176)	0.142 (0.219)
$Log(Pop_t)$		5.103 (3.288)	35.829** (14.266)
$Log(PopM1829_t)$			1.009** (0.447)
Obs.	36	36	36
R sq.	0.73	0.76	0.80
	Period: July 2002 – June 2004		
Dep. Var.:	**$Log(Acc_t)$**	**$Log(Acc_t)$**	**$Log(Acc_t)$**
$PRDL_t$	-0.123*** (0.019)	-0.129*** (0.023)	-0.139*** (0.027)
$Log(Cameras_t)$	-0.180* (0.095)	-0.122 (0.116)	-0.128 (0.120)
$Log(Police_t)$	0.101 (0.149)	0.041 (0.180)	-0.047 (0.218)
$Log(Veic_t)$	-1.938 (3.912)	-3.835 (5.324)	-4.153 (5.556)
$Log(Prec_t)$	0.079 (0.015)	0.001 (0.017)	-0.002 (0.018)
$Log(Unempl_t)$	0.256* (0.137)	0.244 (0.147)	0.275 (0.155)
$Log(AlcPrice_t)$	0.936 (1.960)	0.493 (1.940)	-6.468 (6.054)
$Log(GasPrice_t)$	-0.003 (0.202)	0.069 (0.236)	0.140 (0.253)
$Log(Pop_t)$		3.259 (4.002)	28.061 (21.257)
$Log(PopM1829_t)$			0.780 (0.647)
Obs.	24	24	24
R sq.	0.91	0.92	0.92

Notes: All specifications include a first order time trend and lagged dependent variable lags (up to 3 lags) to control for autocorrelation. Standard errors (reported in parentheses) are corrected for heteroskedasticity. The symbols ***, **, * indicate that coefficients are statistically significant, respectively, at the 1, 5, and 10 percent level.

Table 6: Seat bealt use effect on occupant and non – occupant fatalities and injuries

Period: March 2001 – September 2008				
Dep. Var.:	Log (Occupant fatalities and injuries)		Log (Non - occupant fatalities and inj.)	
	OLS	**IV**	**OLS**	**IV**
Log($Seat_t$)	0.130*** (0.038)	0.128*** (0.036)	-0.179*** (0.048)	-0.180*** (0.046)
Log($Police_t$)	0.073 (0.228)	0.078 (0.215)	0.377* (0.210)	0.371* (0.196)
Log(Cam_t)	-0.046 (0.050)	-0.043 (0.047)	-0.075 (0.077)	-0.073 (0.072)
Log($Unempl_t$)	-0.179* (0.096)	-0.180** (0.090)	0.086 (0.150)	0.084 (0.140)
Log($Veic_t$)	5.337** (2.064)	5.058** (2.068)	-1.035 (3.686)	-1.162 (3.601)
Log($Prec_t$)	-0.001 (0.017)	-0.0003 (0.016)	0.017 (0.017)	0.017 (0.016)
Log($FuelPrice_t$)	0.333 (0.226)	0.349 (0.218)	0.159 (0.223)	0.163 (0 .222)
Log($AlcPrice_t$)	3.073 (2.403)	2.483 (2.348)	-12.180*** (2.434)	-12.267*** (2.396)
Obs.	91	91	91	91
R sq.	0.54	0.54	0.73	0.73

Notes: All specifications include a first order time trend and two dummy variables accounting for the peaks displayed by the relative time series in February and July (Occupant fatalities), and in November and August (Non – Occ. fatalities) . Standard errors (reported in parentheses) are corrected for heteroskedasticity. The symbols ***, **, * indicate that coefficients are statistically significant, respectively, at the 1, 5, and 10 percent level.

Table 7: Occupant fatalities and injuries – Robustness checks

	Period: January 2002 – September 2008			
Dep. Var.:	Log (Occupant fat. and inj.)		Log (Occupant fat. and inj.)	
	OLS	**IV**	**OLS**	**IV**
Log($Seat_t$)	0.132*** (0.0492)	0.132*** (0.045)	0.150*** (0.053)	0.156*** (0 .049)
Log($Police_t$)	0.089 (0.288)	0.088 (0.263)	0.131 (0.267)	0.127 (0.240)
Log(Cam_t)	-0.043 (0.057)	-0.042 (0.052)	-0.057 (0.058)	-0.059 (0.053)
Log($Unempl_t$)	-0.176 * (2.531)	-0.176* (0.097)	-0.258** (0.123)	-0.264** (0.112)
Log($Veic_t$)	6.206** (0.019)	6.179*** (2.319)	7.271*** (2.659)	7.458*** (2.428)
Log($Prec_t$)	-0.0004 (0.019)	-0.0004 (0.018)	-0.003 (0.019)	-0.004 (0.017)
Log($FuelPrice_t$)	0.217 (0.263)	0.217 (0.241)	0.258 (0.246)	0.263 (0.223)
Log($AlcPrice_t$)	4.026 (3.024)	4.005 (2.77)	3.281 (2.851)	3.157 (2.600)
Log(Pop_t)	-1.737 (2.926)	-1.699 (2.680)	3.953 (4.497)	5.012 (4.105)
Log($PopM1829_t$)			0.390* (0.236)	0.435* (0.215)
Obs.	81	81	81	81
R sq.	0.52	0.52	0.54	0.54

Notes: All specifications include a first order time trend and two dummy variables accounting for the peaks displayed by the relative time series in February and July. Standard errors (reported in parentheses) are corrected for heteroskedasticity. The symbols ***, **, * indicate that coefficients are statistically significant, respectively, at the 1, 5, and 10 percent level.

Table 8: Non - occupant fatalities and injuries – Robustness checks

	Period: January 2002 – September 2008			
Dep. Var.:	**Log (Non - occupant fat. and inj.)**		**Log (Non - occupant fat. and inj.)**	
	OLS	**IV**	**OLS**	**IV**
Log($Seat_t$)	-0.246*** (0.0569)	-0.245*** (0 .052)	-0.243*** (0.055)	-0.241*** (0.0503)
Log($Police_t$)	0.704** (0.278)	0.703** (0.255)	0.701** (0.278)	0.704*** (0.252)
Log(Cam_t)	-0.086 (0.080)	-0.086 (0.073)	-0.087 (0.080)	-0.088 (0.074)
Log($Unempl_t$)	-0.007 (0.149)	-0.001 (0.136)	-0.017 (0.152)	-0.016 (0.139)
Log($Veic_t$)	-1.846 (3.850)	-1.824 (3.529)	-1.783 (3.903)	-1.598 (3.569)
Log($Prec_t$)	0.030 (0.018)	0.030* (0.017)	0.030 (0.019)	0.030 (0.017)
Log($FuelPrice_t$)	0.110 (0.278)	0.109 (0.255)	0.114 (0.275)	0.114 (0.250)
Log($AlcPrice_t$)	- 9.904*** (2.985)	-9.990*** (2.734)	-10.072*** (3.0356)	-10.049*** (2.7532)
Log(Pop_t)	-5.468 (3.879)	-5.434 (3.553)	-4.696 (5.030)	-4.413 (4.554)
Log($PopM1829_t$)			0.051 (0.261)	0.065 (0.237)
Obs.	81	81	81	81
Rsq.	0.75	0.75	0.75	0.75

Notes: All specifications include a first order time trend and two dummy variables accounting for the peaks displayed by the relative time series in November and August. Standard errors (reported in parentheses) are corrected for heteroskedasticity. The symbols ***, **, * indicate that coefficients are statistically significant, respectively, at the 1, 5, and 10 percent level.

Table 9: Robustness checks – Occupant fatalities and injuries
Different time – windows around the enactment of the PRDL

Period:	July 2002 – June 2004		January 2002 – December 2004		July 2001 – June 2005	
Dep. Var.:	Log (Occupant fat. and inj.)		Log (Occupant fat. and inj.)		Log (Occupant fat. and inj.)	
	OLS	IV	OLS	IV	OLS	IV
Log(Seat$_t$)	0.209* (0.121)	0.217*** (0.084)	0.264 *** (0.092)	0.259 *** (0.076)	0.374*** (0.090)	0.372*** (0.078)
Log(Police$_t$)	-0.029 (0.376)	-0.064 (0.267)	-0.185 (0 .358)	-0.165 (0.293)	-0.731** (0.329)	-0.722*** (0.286)
Log(Cam$_t$)	-0.402** (0.078)	-0.397*** (0.0534)	-0.312*** (0.047)	-0.313*** (0.039)	-0.229*** (0.055)	-0.230** (0.048)
Log(Unempl$_t$)	-0.036 (0.198)	-0.029 (0.139)	0.060 (0.131)	0.059 (0.108)	0.190 (.1464123)	0.191 (0.127)
Log(Veic$_t$)	-5.499 (6.205)	-5.905 (4.392)	-5.006 (3.320)	-4.513 (2.685)	-5.139 (4.349)	-4.884 (3.768)
Log(Prec$_t$)	0.011 (0.018)	0.011 (0.013)	0.005 (0.012)	0.005 (0.010)	-0.08 (0.016)	-0.008 (0.013)
Log(FuelPrice$_t$)	1.692** (0.761)	1.625*** (0.545)	1.066** (0.483)	1.065*** (0.394)	0.331 (0.500)	0.334 (0.433)
Log(AlcPrice$_t$)	73.984*** (16.475)	71.981*** (11.776)	49.714*** (9.180)	50.088*** (7.507)	9.741 (6.992)	9.917 (6.065)
Obs.	24	24	36	36	48	48
Rsq.	0.91	0.91	0.87	0.87	0.71	0.71

Notes: All specifications include a first order time trend include and two dummy variables accounting for the peaks displayed by the relative time series in February and July. Standard errors (reported in parentheses) are corrected for heteroskedasticity. The symbols ***, **, * indicate that coefficients are statistically significant, respectively, at the 1, 5, and 10 percent level.

Table 10: Robustness checks – Non - occupant fatalities and injuries
Different time - windows around the coming into force of the PRDL

Period:	July 2002 – June 2004		Jan. 2002 – Dec. 2004		July 2001 – June 2005	
Dep. Var.:	Log (Non – occ. fat. and inj.)		Log (Non – occ. fat. and inj.)		Log (Non – occ. fat. and inj.)	
	OLS	IV	OLS	IV	OLS	IV
Log(Seat$_t$)	-0.415*** (.171)	-0.441*** (0.115)	-0.184 0.148	-0.186 (0 .121)	-0.241* (0.121)	-0.241** (0.105)
Log(Police$_t$)	1.261* (0.653)	1.294 *** (0.457)	0.973 * (0.524)	0.982** (0.426)	0.735* (0.368)	0.729** (0.319)
Log(Cam$_t$)	-0.895*** (0.226)	-0.951*** (0.176)	-0.485*** (0.127)	-0.488*** (0.104)	-0.336** (0.132)	-0.333*** (0.114)
Log(Unempl$_t$)	0.746 (0.431)	0.736** (0.319)	0.685*** (0.228)	0.692*** (0.186)	0.734*** (0.203)	0.728*** (0.176)
Log(Veic$_t$)	-5.804 (10.754)	-10.894 (8.138)	8.713 (5.556)	8.989** (4.522)	10.926** (5.344)	10.684** (4.639)
Log(Prec$_t$)	-0.034 (0.045)	-.0309 (0.035)	-0.0169 (0.0239)	-0.017* (0.020)	0.001 (0.021)	0.001 (0.018)
Log(FuelPrice$_t$)	1.351 (1.196)	1.760** (0.888)	-0.990 (0.972)	-0.975 (0.794)	0.211 (0.708)	0.205 (0.613)
Log(AlcPrice$_t$)	31.039 (30.681)	43.235* (23.230)	-20.590 (13.822)	-20.009 (11.325)	-3.902 (7.540)	-4.142 (6.521)
Obs.	24	24	36	36	48	48
Rsq.	0.89	0.89	0.82	0.83	0.80	0.80

Notes: All specifications include a first order time trend two dummy variables accounting for the peaks displayed by the relative time series in August and November. Standard errors (reported in parentheses) are corrected for heteroskedasticity. The symbols ***, **, * indicate that coefficients are statistically significant, respectively, at the 1, 5, and 10 percent level.

Table 11: Seat belt offenses and traffic fatalities before and after the introduction of the PRDL

Period:	Jul 2001 – Jun 2003	Jul 2003 – Jun 2005	Jul 2001 – Jun 2003	Jul 2003 – Jun 2005
Dep. Var.:	Log(Occ. fatalities and injuries)		Log(Non - occ. fatalities and injuries)	
Log($Seat_t$)	0.031 (0.174)	0.279* (0.144)	-0.666 (0.416)	-0.336* (0.188)
Log($Police_t$)	0.163 (0.301)	-0.318 (0.721)	0.734 (0.843)	1.489** (0.521)
Log(Cam_t)	-0.094 (0.077)	-0.351* (0.171)	0.068 (0.3409)	-0.679*** (0.218)
Log($Unempl_t$)	0.005 (0 .202)	0.408 (0.281)	0.573 (0.809)	0.416 (0.283)
Log($Veic_t$)	4.781 (4.514)	-10.646 (11.053)	42.023* (20.881)	17.187*** (5.555)
Log($Prec_t$)	0.000 (0.012)	-0.057 (0.056)	-0.0256 (0.056)	0.038 (0.044)
Log($FuelPrice_t$)	0.394 (0.281)	3.923 (2.142)	-0.287 (1.021)	1.672 (1.846)
Log($AlcPrice_t$)	46.213*** (8.001)	12.255 (14.100)	-43.880 (44.059)	-3.992 (9.003)
Obs.	24	24	24	24
Rsq.	0.94	0.74	0.80	0.90

Notes: All specifications include a first order time trend and two dummy variables accounting for the peaks displayed by the relative time series in February and July (Occupant fatalities), and in November and August (Non – Occ. fatalities) . Standard errors (reported in parentheses) are corrected for heteroskedasticity. The symbols ***, **, * indicate that coefficients are statistically significant, respectively, at the 1, 5, and 10 percent level.

Table 12: PRDL and offsetting behaviors

Period: March 2001 – September 2008				
	Log($Speed_t$)	Log($Dang_t$)	Log(Alc_t)	Log($Drug_t$)
Log($Seat_t$)$PRDL_t$*	0.002 (0.007)	-0.021*** (0.006)	-0.007** (0.004)	0.014 (0.010)
Log($Police_t$)	-0.259 (0.294)	-0.199 (0.191)	-0.009 (0.198)	-0.283 (0.357)
Log($Cameras_t$)	0.750*** (0.112)	0.012 (0.094)	0.021 (0.090)	0.100 (0.156)
Log($Veic_t$)	0.035 (0.027)	0.0131258 (0.021)	0.2.66 (1.995)	-0.082*** (0.031)
Log($Prec_t$)	-1.195 (1.421)	0.327 (2.310)	0.011 (0.020)	6.589*** (2.397)
Log($GasPrice_t$)	0.225 (0.463)	-0.102 (0.265)	-0.425* (0.236)	0.771 (0.524)
Log($Unempl_t$)	0.105 (0.161)	0.250 (0.152)	0.116 (0.149)	0.640*** (0.206)
Log($AlcPrice_t$)	7.083*** (1.368)	2.283 (1.607)	2.021 (1.302)	-0.102 (2.227)
Obs.	91	91	91	91
R sq.	0.74	0.52	0.91	0.84

Notes: All specifications include a first order time trend and lagged dependent variable lags (up to 3 lags) to control for autocorrelation. Standard errors (reported in parentheses) are corrected for heteroskedasticity. The symbols ***, **, * indicate that coefficients are statistically significant, respectively, at the 1, 5, and 10 percent level.

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