Working papers series

WP ECON 18.14

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Department of Economics

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This version: November 2018

Abstract

Using a quasi-experimental approach, we study the causal effect of introducing a penalty-point system (PPS) on drivers, accidents, injuries and fatalities. We find that the PPS decreased the number of traffic offenders by 13.8%. In addition, the deterrence effect was directly related to the size of the point loss. The PPS reform also curbed PPS-related accidents, injuries and fatalities by 14,2%, 15.1% and 16.1%, respectively. These findings are robust to a battery of tests, including a placebo test with a fictitious reform date. Crucially, the timing of the PPS implementation had no effect on road incidents unrelated to PPS regulations.

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For helpful comments and technical guidance, we thank Lieutenant Colonel J. Moscoso. We thank Spanish *Dirección General de Tráfico* for providing us with the data set of micro data on road accidents. Financial support from MINECO of Spain through project ECO2016-76818, and from Junta de Andalucía through project SEJ-1512 is acknowledged.

"Deterrence is important not only because it results in lower crime but also because, relative to incapacitation, it is cheap. Offenders who are deterred from committing crime in the first place do not have to be identified, captured, prosecuted, sentenced, or incarcerated. For this reason, assessing the degree to which potential offenders are deterred by either carrots (better employment opportunities) or sticks (more intensive policing or harsher sanctions) is a first-order policy issue."

Aaron Chalfin and Justin McCrary (2017)

1. Introduction

According to the World Health Organization, road accidents truncate more than 1.25 million lives per year and cause non-fatal injuries to an additional 20 to 50 million individuals, generating costs as high as 3% of the gross domestic product in most countries (WHO 2018). To increase road safety, governments have applied a battery of measures from improving road infrastructure to monitoring drivers' behaviors and punishing their offences. The rational for implementing sanctions is that they increase individuals' expected cost of offending, hence discouraging reckless driving as well as deterring normal drivers from infringing road regulations.

Since the turn of the century, many governments have implemented a penalty-point system (PPS hereafter) in their traffic regulations such that drivers, who are assigned an initial amount of points, are sanctioned with losing a certain number of them when they commit a traffic violation. The number of points lost is directly related to the seriousness of the infringement and once the driver's point-credits are exhausted, her license is withdrawn and she is incapacitated during a compulsory deprivation period.[1] Despite its popularity[2], little is known on the *causal* effect of implementing such non-monetary sanctions as well as the effects of increasing the severity of the penalty (with greater point loss) on both drivers, accidents, injuries and fatalities.[3] This is the objective of the current paper.

[1] Note that this system is effectively identical to that existent in Australia, Canada, Japan, New Zealand and some states of the US, where drivers accumulate "demerit points" with traffic convictions until a threshold is reached (Bourgeon and Picard, 2007).

[2] About 44 countries use some form of PPS (ETSC, 2011).

[3] Previous studies on the benefits of implementing a new PPS in various countries provide crucial insights into the correlates of implementing a new PPS on accidents, injuries and fatalities, but fail to determine causality. Using country- as well as individual-level data for several countries and various methodologies, these studies have found short-lived beneficial effects of implementing a new PPS on accidents, injuries and fatalities—see Novoa et al. (2010) and Castillo-Manzano and Castro-Nuño (2012) and references therein.

This paper offers quasi-experimental evidence concerning the effects that the introduction and severity of PPS has on drivers, accidents, injuries and fatalities. Using a rich administrative micro-data set on drivers, vehicles and traffic violations committed in all interurban crashes in Spain from July 1 2004 to June 30 2008, this paper exploits exogenous variation in the implementation of a new point-record driving license in Spain to analyze whether this sharp increase in the non-monetary costs of offending in the road deters reckless driving. More specifically, we use the discontinuity in the number of point-credits lost—from no points lost to a positive amount ranging between 2 and 6 points—per traffic-violation type on July 1 2006, when the Spanish government implemented the PPS, to identify the effect of non-monetary sanctions on the number of drivers infringing the law based on a discontinuity-based approach. Figure 1 shows the time series of point-credits lost per traffic offenders type in Spain, showing the sharp discontinuity on July 1 2006.

The identifying assumption is that in the absence of this reform, the number of drivers committing PPS traffic violations would have changed smoothly on July 1 2006 in Spain. To put it differently, we argue that the offenses penalized by the PPS in Spain on the days just before the reform was implemented form a valid counterfactual for the amount of traffic violations on days just after the reform was implemented, conditional on differences in seasonal and business-cycle effects, province fixed effects, and a very flexible smooth time trend. This assumption seems reasonable as the Spanish government spent the first half of 2006 explaining the reasons behind the PPS, its objectives and implementation, as well as clarifying any questions or concerns citizens had about the PPS.

Our results are threefold. First, we find that the reform significantly reduced the number of drivers committing traffic violations punished by the PPS and the size of the reduction was directly related to the severity of the penalty. Indeed, we find that the PPS reduced PPS-related offenders by 13.8%. Offenders of highly severe infractions dropped by 29.5%, offenders of severe infractions dropped by 26.1% and those of mild infractions by 11.7%. These estimates are statistically significant at the 1% level and robust to a battery of sensitivity tests, including a placebo test with a fictitious reform date. Most importantly, we find no evidence that the PPS affected drivers involved in traffic incidents *not* punished by the PPS such as pedestrian's violations or accidents caused by drowsy driving, a sudden sickness while driving, or a sudden breakdown of the vehicle.

Second, we examine whether this reform had public health and policy implications by reducing road accidents, injuries and fatalities. We find that the PPS decreased the number of PPS-related road accidents by 14.2%, injuries by 15.1% and fatalities by 16.1%. Again, these estimates are statistically significant and robust to a battery of sensitivity tests, including the placebo test with the fictitious reform date. At the same time, we find no effect of the reform on non-PPS related road accidents, injuries or fatalities, suggesting that it is unlikely that our findings are driven by other confounding factors.

We also find that this causation persists and does not fade at least up until two years after its implementation, so that the aftermaths cannot only be confined to the months following the PPS implementation. Moreover, we rule out that our findings are driven by drivers being incapacitated due to driving-license suspension.

Since Becker wrote his 1968 influential model of criminal behavior, many researchers have estimated the degree to which punishments and apprehension probability deter crime— see Chalfin and MacCrary (2017) and references therein. To bypass the simultaneity (or reverse causality) bias, studies have used a quasi-experimental approach to estimate the causal effect of punishment and enforcement on crime reduction. Because much of this research has focused on violent or property crime reduction (Andenaes 1974, Levitt 1997 and 2002; McCrary 2002; Evans and Owens 2007; Worrall and Kovandzic 2007; Lin 2009; and Shi 2009, among others)[4], its findings are unlikely to apply to potential traffic offenders, providing limited information and policy guidance on how to reduce traffic offenders and increase road safety.

In recent years, several studies have estimated the causal effect of road-safety penalties and/or enforcement on traffic offences. They can be classified by whether they test general versus specific deterrence. In the case of general deterrence, individuals perceive that traffic laws are enforced and, hence, violating them comes with the risk of detection and punishment. In contrast, with specific deterrence, it is individuals' own experience of

[4] To instrument for police manpower, Levitt uses the timing of mayoral and gubernatorial elections and the number of fight fighters in his 1997 and 2002 papers, respectively; Evan and Owens (2007) use the size of federal Community Oriented Policing Services grants awarded to cities to promote police hiring; and Lin (2009) uses differential exposure to exchange-rate shocks based on the export intensity of the local industry. All these authors find an effect of police on crime that is disputed by McCrary (2002) who argues that Levitt's 1997 instrument was imprecise precluding the inference of a causal effect, and by Worrall and Kovandzic (2007) who use a smaller sample of cities than Evan and Owens (2007). Andenaes (1997) and Shi (2009) use a natural experiment in Nazi-ocupied Denmark and in Cincinatti (Ohio), respectively.

actually being punished that affects their future offenses. Studies testing specific deterrence (Hansen 2015; Gehrsitz 2017; Goncalves and Mello 2017; de Figueiredo 2015; Dušek and Traxler 2016**;** Studdert, Walter and Goldhaber-Fiebert 2017) focus on repeat offenders[5], which represent a small fraction of total traffic offenders.[6] Moreover, these studies focus on the subsample of speeding or driving-under-the-influence (DUI) recidivists, which are likely to respond quite differently to the rest of the population.[7]

Fewer studies have tested general deterrence and they focus (again) on drunk-driving penalties (Evans, Neville and Graham 1991; Briscoe 2004; Wagenaar et al. 2007), finding short-lived beneficial effects of increasing the certainty of punishment on alcohol-related fatalities, but no effects of increasing the severity of the punishment (in contrast with Becker's crime model's predictions).[8] Studies causally testing general deterrence on other traffic laws (beyond DUI targeting) are scarcer (Bar-Ilan and Sacerdote 2004; Tavares, Mendes and Costa 2008; and DeAngelo and Hansen 2014).[9] To the best of our knowledge, only De Paola, Scoppa and Falcone (2013) estimate the causal effect of implementing a new PPS, finding that it decreased road accidents and fatalities in Italy by 9% and 30%, respectively. Their findings appear to be driven by a sharp reduction in infractions sanctioned with 5-point losses such as wearing a helmet and fastening the seatbelt, as opposed to those sanctioned with 10-point losses such as speeding or DUI.

Our work contributes to the aforementioned quasi-experimental studies by utilizing

[5] Hansen (2015) studies the effect of harsher sanctions on driving-under-the-influence because of previous offenses' blood of alcohol content. Gehrsitz (2017) analyzes the impact of one-month driver's license suspension on second-time speeding offenders. De Figuereido (2015) estimates the effectiveness of DUI sanctions in curbing recidivism and vehicle crashes with some 200,000 blood-alcohol-content-level tests. Dušek and Traxler (2016) provide evidence that the effects of receiving a speeding ticket within a month of the violation are more than two times larger than if the ticket is received with a longer delay. Goncalves and Mello (2017) study the effect of harsher speeding monetary sanctions on future speeding offences of cited drivers.

[6] For instance, in Spain, from 2006 to 2016, 75% of drivers did not lose any point, and among those who did, 16% did so only once. Hence, 21% of drivers were repeat offenders.

[7] Nochajski and Stasiewicz (2006) explain that drunk-driving offenders, especially recidivist drunk drivers, tend to be an atypical kind of traffic offender. Moreover, Studdert, Walter and Goldhaber-Fiebert (2017) find that, over a 16-year period, less than 2% of the 11.6 million recorded offenses in Queensland were DUIs.

[8] Evans, Neville and Graham (1991) analyze the effectiveness of four different types of anti-drunk driving policy implemented across US states between 1975 and 1986. Briscoe (2004) analyzes the impact of doubling statutory penalties for drunk-driving offences in New South Wales, Australia. Wagenaar et al. (2007) examine the effects of statutory changes in DUI fines or jail penalties for first time offenders from 1976 to 2002.

[9] Tavares, Mendes and Costa (2008) find evidence that introducing an "on-the-spot" fine in Portugal curbed accidents, fatalities and injuries, and Bar-Ilan and Sacerdote (2004) find that red-light running was reduced after a fine was implemented San Francisco. DeAngelo and Hansen (2014) estimate the effect of a 35% decrease in roadway troopers on speeding citations, injuries and fatalities in the state of Oregon.

the universe of traffic violations involving an accident in the Spanish territory over a five-year period to estimate the causal effect of implementing a PPS. Importantly, we test general deterrence, which may well differ from studies using recidivists (that is testing specific deterrence). Crucially, we provide causal evidence of the effects of implementing PPS on both drivers, accidents, injuries and fatalities, finding that in addition to effective and cost-effective, PPS's impact increases with the severity of the sanction. As we provide evidence that incapacitation is unlikely to drive our results, our findings support Becker's theory of deterrence in a context of traffic violations. Finally, as the Spanish government is in the midst of considering a reform of the PPS (*El País*, July 12 2018), our findings could not be more policy timely.

The remainder of this paper is organized as follows. Section 2 describes the institutional background and the 2006 Spanish PPS. Sections 3 and 4 present our methodology and data set, respectively. Section 5 presents the main results of the PPS reform on traffic offencers and Section 6 presents the effects of the reform on road accidents, injuries and fatalities. Section 7 concludes with a cost-benefit analysis of the reform.

2. The Spanish Penalty Point System

In Spain, a 1990 law (Real Decreto 339/1990) determines the monetary fines to traffic violations in interurban roads.[10] Thus, interurban-road fines are set at the national level by the Department of Motor Vehicles (DGT, *Dirección General de Tráfico*) and, hence, do not vary across provinces. In contrast, in cities, the local government has discretion over the amount of the fine it charges for a given traffic violation.

Public health concerns[11] and EU directives (Dirección General de Tráfico 2005; and European Conference of Ministers of Transport, 2000) led the Spanish government to announce, on July 17th 2005, a law (Law 17/2005) introducing a penalty-point system (PPS) to traffic violations[12], on top of the monetary fines already charged. According to this law,

[10] Mild traffic violations are fined with €91 euros, serious violations with €92 to €301 euros and very serious violations with €302 to €602 euros.

[11] In 2004, Spain ranked fourth among fifteen European countries in the number of road-accidents and fatalities, accounting for 14% and 8% of the EU15 share, respectively, and amounting to 94,009 road-accidents and 4,749 deaths (Statistics extracted from CARE, EU road accidents database). In addition, the ratio of fatalities to accidents in Spain (5.1%) almost doubled that of the EU15 (2.8% on average).

[12] Similar PPS systems had been introduced in other 21 European countries such as the United Kingdom (1982), France (1992), Germany (1999) or Italy (2003).

starting on July 1st 2006, traffic-violation offenders would be sanctioned with losing a certain number of points from their initial endowment of 12 points, in addition to paying the monetary fine already charged for that particular traffic violation prior to this reform. As explained below, the amount of points lost ranges between 2 and 6 per traffic violation and is directly related to the seriousness of the violation. The total amount of points lost by the driver in a particular situation will depend on the seriousness of the offences committed. The law limits to 8 the maximum number of points lost per driver and day.

Once a driver has lost her 12 points, her driving license is withdrawn. To recover the license and regain 12 points, the offender has to pay and attend a 24-hour road-safety course, as well as pass a written test. Importantly, the test cannot be taken until at least 6 months after the last offence, and can only be taken once every two years. Prior to losing all her points, the driver can regain up to 6 points by attending a 12-hour road-safety course every two years. In 2006, the 24-hour course tuition was €320 euros and the 12-hour course, €170 euros.[13] Alternatively, offenders regain their balance of 12 points if they do not commit any traffic violation during a two-year period after their last offence (three-year period if the offence was very serious). Good drivers with no points lost during a three-year period are granted 2 additional points and a further point after four years, summing up to a maximum of 15 credit points.

The severity of the traffic violation determines the amount of points discounted from the initial score. For instance, traffic violations involving driving under the influence of alcohol or drugs, speeding over the legal limit, or reckless driving tend to be penalized with up to 6 points.[14] Traffic violations penalized with 4 points include: not respecting stop signs, police signals or the mandatory safety distance; driving without the appropriate license, not respecting cyclists, throwing objects that could start a fire or lead to an accident, and dangerous overtaking. Using the cell phone while driving, not buckling the seat belt, or making illegal road turns is punished with the loss of 3 points. Crucially for our analysis, the amount of the monetary fines was not affected by the reform.

[13] Prices are set at the national level and they are increased annually in January to account for changes in the Consumer Price Index. In 2018, the tuition is €405,22 euros for the 24-hour course (and €215,26 euros for the 12-hour course).

[14] The penalty loss could be smaller if the amount of alcohol in the blood or the speed level is below a certain threshold. For instance, the penalty is 4 points if the driver's alcohol level is between 0.25 or 0.50 mg per liter of blood.

This reform was widely publicized and well explained with a three-phase campaign that began as early as January 1st 2006. First, the government informed on the reasons for this law through advertising campaigns in radio, television, written-press and the internet, and, at the same time, certified those centers that would teach the new road-safety courses enabling offenders to redeem points. Second, the government explained how the PPS would work by publishing written-press articles and TV documentaries; creating a phone line where citizens could ask PPS-related questions; and mailing a letter explaining the details of the PPS to all drivers in Spain (about 14 million people). Third, during June 2006, under the slogan "*With the PPS we will all drive better*",[15] an aggressive marketing campaign hammered the idea that a change in drivers' behavior after July 1st 2006 would increase road-safety.

According to the European Commission (2010), the implementation of the PPS was transparent and the classification of offences and point losses associated to them were clear to drivers. Furthermore, drivers could (and can) consult their point status or any other frequently asked questions about the PPS in the Department of Motor Vehicles' website (www.dgt.es). Importantly, as soon as a driver's point record drops below 6, the Department of Motor Vehicles mails the offender a warning about the legal consequences of losing all the points and an explanation on how to redeem lost points.

By increasing the expected costs of committing traffic violations, the Spanish government's conjecture was that the PPS would work as an incentive device and deter normal drivers from infringing road regulations. Indeed, Montoro González, Roca Ruiz and Tortosa Gil (2008) present descriptive evidence that potential offenders perceived that the cost of committing a traffic offense had changed after the PPS reform.[16] In particular, drivers reported worrying that if their driving license were withdrawn they would need to find alternative transportation arrangements and would not be able to drive to work. They also worried about both the written exam and the cost of the road-safety course. Last but not least, according to these authors, Spaniards also underscored the social costs of being perceived as a dangerous driver by friends and family. In the rest of the paper, we analyze whether the reform succeeded in changing drivers' behaviors.

[15] In Spanish: "*Con el permiso por puntos vamos a conducir mejor*".
[16] The authors interviewed over 2,000 drivers in Spain between February 13 and March 2 2007.

3. Discontinuity-Based Model

Our aim is to explore whether the introduction of the PPS in July 2006 in Spain *caused* a reduction on the number of PPS-related traffic offenders and whether this reduction was steeper with the severity of the offence. Evidence of this would suggests that the Spanish reform, which mainly introduced non-monetary sanctions, was successful in modifying drivers' behavior, and hence would provide empirical evidence consistent with the theory of deterrence from Becker's crime model. For this purpose, we take advantage of a sharp discontinuity in the number of points lost per driver/day—from no points lost to a positive amount ranging from 2 to 8 points depending on the type and number of traffic-violations incurred by driver per day—that takes place on July 1st 2006, when the Spanish government implemented the PPS. Our model implements a regression discontinuity-based model, in which time is the running variable that determines whether drivers are exposed to treatment or not. More specifically, we use the following regression discontinuity-based model (DB hereafter):

$$Y_{i,t} = F(\alpha + \beta_0 PPS_t + \delta X_{it} + \sum_{k=2}^{50\times 12} \mu_k(\pi_i \times Q_m) +$$
$$[(1 - PPS_t) \times f_1(t)] + [PPS_t \times f_2(t)] + \varepsilon_{it}).$$

where Y_{it} is number of drivers committing a PPS-related traffic violation in province *i* and day *t*. Because the outcome variable is a count process adopting only positive integer values, we estimate model parameters using a negative binomial model as reflected by the term $F(\,)$.[17] The variable PPS_t is a dummy variable taking value one for all days after the implementation of the PPS (July 1st 2006) and zero otherwise. The vector X_{it} refer to variables capturing the province-specific business cycle such as the province quarterly unemployment rate and the province monthly price of gasoline (in logs). In addition, X_{it} also includes controls for the day of the week (Sunday through Saturday dummies) and day of the month, as well as an indicator of whether the day is a holiday in each province (as heavy traffic is more common during the holidays). We also control for the share of new cars' registrations to total car´s registered, the ratio of registered cars to total number of licensed drivers, the share of registered cars to total number of vehicles registered, and the

[17] The negative binomial model is a more flexible model than the Poisson as it relaxes the assumption of equality between the conditional mean and variance

share of female licensed drivers in each province and year. Finally, we control for province-specific seasonal patterns with monthly and province fixed effects, $\{Q_m\}_{m=1}^{12}$ and $\{\pi_i\}_{i=2}^{50}$, respectively, to account for the strong seasonality in traffic density and kilometers traveled (Rodríguez-López et al. 2016), as well as to account for socio-economic differences across the 50 provinces.

In our preferred specification, the terms $f_j(t)$ are second-order polynomial functions [18] on t for $j = 1$ (before implementation of the PPS) and $j = 2$ (after implementation of PPS):

$$f_j(t) = \sum_{n=1}^{N_j} \beta_{jn} t^n.$$

The coefficient of interest, β_0, captures the causal effect of the PSS on the number of drivers committing traffic violations. Note that at the cutoff point, on July 1st 2006, $f_j(0) = 0$ for $j = 1, 2$. Hence, any causal effect associated with the introduction of the PPS will be absorbed by our coefficient of interest, β_0. A negative and statistically β_0 would provide evidence that the PPS decreased the number of offenders.

Lee and Lemieux (2010) and Hausman and Rapson (2017) argue that the parametric approach is a good approximation and improves precision as long as the conditional expectation of the unobserved determinants of Y_{it} are continuous over the threshold.[19] Henceforth, identification comes from assuming that the underlying, potentially endogenous, relationship between ε_{it} and the date is eliminated by the flexible functions $f_1(t)\ and\ f_2(t)$ that absorb any smooth relationship between the date and ε_{it}. To put it differently, the polynomial time trend, $f_j(t)$, controls for time-series variation in the number of traffic offenders that would have occurred in the absence of the PPS picking up smooth changes in the number of traffic offenders that take effect slowly over time and are caused by changes in

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[18] We selected the polynomial order N_j using the likelihood-ratio tests, the Akaike Information Criteria (AIC) and the Bayesian Information Criteria (BIC) as recommended by Lee and Lemieux (2010) and Hausman and Rapson (2017). The optimal polynomial order depends on the particular outcome variable. Because simpler models generally have greater precision than more complex ones, if the point estimate didn't change much between the models, we follow Jacob et al. (2016) and present results with the simpler model. Hence, our main specification uses second-order polynomials. Nevertheless, we check the robustness of our results by displaying them using one-order polynomial higher and lower.

[19] An alternative is a non-parametric approach such as the local linear regression (Hahn et al. 2001). However, Lee and Card (2008) argue that, with a discrete running variable, such a non-parametric approach is not advisable.

the composition of economic activity in Spain, or other policies that take effect slowly over time.

Crucially, these flexible polynomial time trends control for potential variation arising from observations further and further away from the threshold. We also allow the time trend in traffic offenders to differ on either side of the implementation date to increase flexibility in our specification. Because the power of analysis decreases when one allows the slope to vary on either side of the cutoff point, we will test the sensitivity of our results to the order of the polynomial.

After analyzing the effect of the reform on traffic offenders, we will also explore the effect of introducing the PPS on the number of road accidents, injuries and fatalities. For this purpose, the Y_{it} will be the number of PPS-related accidents, injuries or fatalities, depending on the outcome under study. In either case, we will also add as controls to the X_{it} vector the passengers' age.

4. Data

Our empirical analysis requires daily data on drivers' traffic violations, number of road accidents, injuries and fatalities. Fortunately, the Spanish Department of Motor Vehicles collects daily information on all drivers', passengers' and pedestrians' traffic violations involving an accident in the Spanish territory. Information is recorded on site by a police officer (*Guardia Civil de Tráfico*). The data are rich as it includes the following information: date and time, location, road type (urban or inter-urban), road conditions[20], traffic violations involved, and the accident type (collision, run over of pedestrians or cyclists). Individual characteristics include gender, age, type (driver, passenger, or pedestrian), existence of injury and fatality. In addition, we also observe type (passenger vehicle, SUV, truck, bike, or motorbike).[21]

We aggregated all interurban traffic offenders in a given day at the province level to obtain a panel where the unit of observation is the province and the time frequency is the day.

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[20] Examples of road conditions include the visibility conditions, and whether there was construction work.

[21] We also observe the date in which drivers' licenses were first issued, allowing us to determine the driver's experience at the time of the accident, the condition of the vehicle (breaks, lights, and age, among others), the number of passengers inside the vehicle and their position inside the car. In preliminary analysis, we included these controls and they had no effect on our reform estimates. Hence, these covariates were not used for the analysis but preliminary estimates are available to readers upon request.

As explained in Section II, in urban roads, there is discretion on the monetary fines charged for a given traffic violation. To avoid having variation in *monetary* fines across cities or time, we focus on traffic violations in interurban road as the fines (both monetary and non-monetary) are set at the national level and interurban monetary fines did not change over our period of analysis. We aggregated the data at the province level to increase precision as some outcomes (for instance, fatalities) are relatively infrequent.

For each province/day, we tabulated the number of drivers involved in a traffic violation (by severity measured with the number of points lost), the number of road accidents, injuries and fatalities. We classify them as PPS-related if at least one driver had violated a road regulation sanctioned by PPS after July 1st 2006. Prior to July 1st 2006, we classify PPS-related outcomes if at least one driver had violated a road regulation that would have been sanctioned by PPS had it been violated after July 1st 2006 (instead of before).

Since the PPS was implemented on July 1st 2006, we choose a sample period with a 2-year window around the implementation of the PPS, which leaves us with a data set containing 74,600 province/day observations.[22] The reason for using a 2-year threshold is that it allows us to better control for the seasonality in traffic offences.

We merged these data with province/day information regarding the local holidays for each province and year. In addition, we also merged our data with aggregate province/month variables measuring the local economy, namely the local unemployment rate and the fuel price index, from the Spanish Institute of Statistics.[23] Finally, we merged our data with aggregate province/year variables from the Department of Motor Vehicles measuring: new cars' registrations, the ratio of registered cars to total number of licensed drivers, the share of registered cars to total number of registered vehicles, and the share of female licensed drivers. Summary statistics of these variables can be found in Appendix Table A.1.

Descriptive Statistics

[22] For the year 2004, we use days from July 1st to December 31st (184 days*50 provinces=9,200 observations). For the years 2005 to 2007, we use the whole year (365 days*3 years*50 provinces=18,250 observations). For the year 2008, we use days from January 1st to July 1st (213 days*50 provinces=10,650 observations). Overall this adds to 74,600 observations.

[23] http://www.ine.es/ and http://www.cores.es/es/estadisticas. We also used average temperature and number of rainy days from National Weather Agency at the province level as controls in preliminary analysis. But since none of these two variables were statistically significant and adding them had no effect on the coefficient measuring the effect of the reform, we did not include them in the final analysis. Results available from authors upon request.

Table 1 summarizes descriptive statistics for traffic offenders, road accidents, injuries and fatalities. We present these summary statistics at the province/month level, even though our data frequency is at the province/day level. The first row displays the average number of drivers in a province/month committing traffic violations either sanctioned by the reform (column 2) or who would have been sanctioned by the reform had they been committed after July 1st 2006 instead of before (column 1). The third and fourth column shows the difference before and after the reform in absolute and relative terms, respectively.

Row 1 shows that the reform reduced the number of traffic offenders by 6.04%. Before the PPS reform, there was an average of 57.5 drivers in a province/month committing traffic violations that would have lost points had they committed the infraction after July 1st 2006. After the reform, the average number of PPS offenders decreased an average of 3.5 offenders to 54 offenders per province/month.

Rows 2 to 4 disaggregate the change in the number of offenders by the degree of severity of the violation measured by the total of points lost after the reform (and "potentially" lost before the reform) per driver per accident. Even though no points were lost in the two years prior to the reform, the pre-reform averages are (again) estimated using the points that would have been lost had the violation been done *after* the reform instead of before. As the PPS did not introduce "new" offences, but instead implemented non-monetary sanctions to offences that were already fined with monetary sanctions, it is not difficult to compute the number of points that the driver would have lost if the offences had been committed after the reform (instead of before the reform). For instance, the pre-reform average number of offenders in a province/month who would have lost 4 points in a given day had they committed the traffic violations after the reform (hereafter 4-point offenders) rounds to 50 offenders (shown in column 1, row 2). Rows 3 and 4 show the averages of 6- and 8-point offenders per province/month before and after the reform.[24]

Not surprisingly, the frequency of offenders decreases with the severity of the traffic violation both before and after the reform. Before the reform, the average number of 4-point offenders per province/month (49.7 offenders) is almost ten times higher than the average of 6-point offenders (5.22 offenders) and twenty times higher than the maximum point lost allowed by law per day (2.59 offenders). Similarly, after the reform, the average number of

[24] As explained in Section 2, by law, the maximum number of points lost is 8 points per day.

4-point offenders is close to ten times that of 6-point offenders and over twenty times that of 8-point offenders.

Comparing the pre-/post-reform change by offence type reveals a larger relative reduction in the number of offenders committing more severe offences (rows 3 and 4) than in those committing milder ones (row 2). Indeed, after the reform, the average number of 6- and 8-point offenders drops by 13% to 12%, a decline more than twice as large as the average drop in 4-point offenders (a 5% decline).

Moving to public health outcomes, rows 5 to 7 show the average frequency of road accidents, injuries and fatalities per province/month before and after the reform for PPS-related road accidents. Interestingly, after the reform the average number of province/month road accidents decreased by 7.68% from 53.4 to 49.3 accidents per province/month. Similarly, the average number of injuries decreased by 9.3% from 89.2 to 80.9, and the average number of fatalities decreased by 26% from 4.12 to 3.05 deaths per province/month.

Figure 2 plots the evolution of PPS-related traffic offenders and by severity type over a 24-month window around July 1st 2006, when the PPS was implemented. Figure 3 plots the evolution of PPS-related accidents, injuries and fatalities over the same period. The horizontal axis shows the running variable (time), centered on July 1st 2006, highlighted by a vertical line. To gauge the importance of the discontinuity, the solid line represents a quadratic trend, estimated separately on either sides of the cut-off point. We observe a clear discontinuity in all the PPS-related outcomes. The direction and magnitude of this discontinuity is preliminary evidence of the causal effect of the reform on the outcomes of interest at the cutoff point.

5. The Effect of PPS on Traffic Violations Offenders

To explore whether the PPS reform has deterred drivers from committing traffic violations, Table 2 presents estimates of our DB model described in Section 3 using different specifications. In the first row of Table 2, we use as left-hand-side (LHS hereafter) variable the number of drivers committing PPS-related traffic violations, that is, violations that from July 1st 2016 on led to loosing driving-license points. The next three rows present estimates using as LHS variable the number of offenders based on the severity of their penalty, moving from those with a smaller penalty to those with a larger penalty.

Column 1 in Table 2 presents estimates from a DB model that only controls for province-specific seasonal patterns (with monthly and province fixed effects) and day-of-the-week and day-of-the-month dummies. Interestingly, all of the estimates of PPS-related traffic violations (rows 1 to 4) are negative and statistically significant at the 1% level or lower.

Column 2 is our preferred specification as it controls for province business cycle (with the unemployment rate and the oil prices) and local holidays. The economic interpretation of our estimates follows. According to estimates in row 1, column 2 in Table 2, the PPS reform reduced daily PPS-related offenders by 13.8% ($\approx e^{-0.149}-1$).[25] Moreover, we observe that the effect of the reform increases with the severity of the offence, supportive of the deterrence hypothesis. For instance, while the reform reduced the number of 4-point offenders by 11.7% (shown in row 2, column 2), its effect on the number of 8-point offenders was three times larger, namely a 29.4% ($\approx e^{-0.349}-1$) reduction (shown in row 4, column 2). The reduction on the number of 6-point offenders ranged in between at 23.9% ($\approx e^{-0.274}-1$).[26] Full specification of the models in column 2 of Table 2 are shown in Appendix Table A.2.

Column 3 presents a specification that adds other controls such as new cars' registrations, the ratio of registered cars to total number of licensed drivers, the share of registered cars to total number of registered vehicles, and the share of female licensed drivers. It is noteworthy that adding these controls has little effect on most of the coefficients, suggesting that omitted variable bias is unlikely a problem.[27]

On December 1st 2007, the Spanish government implemented a criminal reform, which may have affected drivers' behavior as, among other things unrelated to driving, it classified DUI, speeding and driving without a license as a felony (prior to this criminal reform, these traffic violations were considered very serious offences instead). Concerns that our estimates may be picking up this criminal reform instead of the PPS reform of July 1st 2006 are addressed in column 4 by re-estimating our preferred specification with a smaller

[25] The 95 confidence interval is [-18.7%, -8.9%].
[26] The estimated 95% confidence interval for each 4-, 6- and 8-point offenders are [-16%, -6%], [-34%, -17%] and [-41%, -17%], respectively.
[27] We tested whether coefficient estimates were statistically different from those of column 2 and we always accepted the null of equality of coefficient estimates. Henceforth, we chose the simpler model.

bandwidth period (restricted to 12 months around the reform, namely from July 1st 2005 to July 1st 2007), hence excluding the period that may have been affected by the December 1st 2007 reform. Doing so has little effect on most of our estimates of the reform on total number of offenders and offenders by type, with the exception of the impact on 6-point offenders, which now shows a reduction of 14.2% (instead of 23.9%), estimated with less precision (no longer significant at the 10% level). Comparing the estimates using two years (column 2) instead of one (column 4) also implies that the effects of the PPS reform were not short-lived as the effects were as large (or larger) during the second year after the implementation than the first year for practically all levels of severity.

Table 3 tests the sensitivity of our results to alternative specifications. Column 1 replicates column 2 in Table 2, our preferred specification. Our results are robust to using a different functional form: Poisson instead of negative binomial model (shown in column 2), and clustering the standard errors at the monthly- or daily-level instead of the province/month-level (shown in columns 3 and 4). Columns 5 and 6 re-estimate the effects of the reform replacing the second-order polynomial with a first- and third-order polynomial, respectively. Not surprisingly, the precision of our estimates declines with higher order polynomials.[28]

Moving back to Table 2, columns 5 and 6 present placebo estimates using our basic and preferred specification, respectively. Placebo estimates are estimated using data spanning from January 1st 2007 to December 31st 2009 and using as the reform date July 1st 2008 (instead of July 1st 2006). Most of the estimates are not statistically significantly and have the opposite sign, suggesting that our DB methodology is not capturing a spurious relationship.

As a falsification test, we use the number of drivers involved in car accidents caused by unfortunate events unrelated to drivers' infringing PPS-related road regulations (such as driver's sudden disease, pedestrians' offences, bad road conditions, or vehicle's sudden breakdown). Because these events are due to situations exogenous to (or independent of) the introduction of the PPS, we will use them as a falsification test. Table 4 shows a specification with only province, month, day of the week and day of the month fixed effects (column 1) as

[28] We tested whether the estimates from the model in column 6 (third-order polynomial) were statistically different from those of column 2 (second-order polynomial) and we could not reject the null hypothesis of equality of coefficient estimates. Henceforth, we chose the simpler model.

well as our preferred specification (column 2) using as LHS variable the number of drivers who are involved in car accidents caused by events unrelated to the PPS reform. Columns 3 and 4 in Table 4 show our preferred specification with lower and higher order polynomial, respectively. We observe that most of the estimates are not statistically significant. In general, the coefficient tends to be smaller in size (with the exception of vehicle breakdown) and frequently the opposite sign.

Anticipation effects

As pointed out by Lee and Lemieux (2010), our identification strategy could be biased if individuals anticipated or reacted sluggishly to the reform. In the case that drivers may have anticipated the PPS reform and adapted their behavior prior to the implementation of the reform (say when the law was announced a year earlier), our estimates would be downward biased as our pre-reform data would be picking up part of the *anticipated* reform. In the case of a sluggish adjustment to the reform, again the estimates would also be downward biased to the extent that drivers take longer to adapt to the reform.

Figure 4 explores the extent to which drivers anticipated or reacted sluggishly to the PPS reform by showing Google searches for the term "PPS" over our sample period.[29] Searches are normalized to 100 (where the peak equals 100). The first vertical line indicates the date the PPS law was approved in Parliament on July 1st 2005, and the second one indicates the date the PPS was implemented on July 1st 2006. While we observe a small pick during the second half of 2005 when first talks of the possible implementation of the PPS were discussed in the Spanish media, most Google searches took place during the first half of 2016 with a peak around July 2016. Searches quickly declined during the second half of 2007 and subsequently. Figure 4 does not seem to imply that there were either anticipation or lagged effects of the reform.

Nonetheless, to address potential concerns, column 7 in Table 2 re-estimates our preferred specification adding monthly Google searches for the term "PPS". Doing so has

[29] These data can be obtained in www.google.com/trends/. The term of search was for "*carnet por puntos*" or "*permiso por puntos*".

little effect on our coefficient of interest.[30]

We have also tested whether our estimates of interest are affected once we control for the Parliament's PPS approval on 1st July 2005. To do so, column 8 in Table 2 estimates our preferred specification adding a dummy variable that controls for the PPS announcement date. This way, we test whether the law impacted individuals' behavior prior to its implementation, confounding the announcement date (July 1st 2005) with the implementation of the PPS (July 1st 2006). As before, coefficient estimates for the impact of the PPS change little. Moreover, the coefficient of the July 1st 2005 dummy is not statistically significant in any of the models.[31] These findings combined with those presented for "Google-searches" do not suggest that our results could be biased by anticipation effects.

Incapacitation versus Deterrence

While we have interpreted our findings as the PPS deterring crime by raising the expected costs of violating traffic regulations, it is possible that, instead, they are the result of the PPS incapacitating reckless drivers. As Bourgeon and Picard (2007) explain, the PPS incapacitates drivers who have lost all their points since they are deprived from driving for a minimum of 6 months. In addition, as more dangerous drivers are more likely to accumulate points quicker, they are also deprived from driving more often, reducing the proportion of dangerous drivers on the road. To explore the extent to which this alternative explanation is likely in our context, Figure 5 plots the number (Panel A) and share (Panel B) of driving licenses suspended over our sample period. The vertical red line indicates July 1st 2016 when the PPS was implemented. It is noteworthy that the number of suspended licenses are small as they amount to less than 1% of the total of traffic licenses. Interestingly, the share of suspended driving licenses has been stable over time. Hence, while it is plausible that the incapacitation effect may make a difference in the longer run, it is dubious that it is driving our findings. To explore this further, column 9 in Table 2 adds to our preferred specification

[30] Coefficient estimates and the corresponding standard errors for the covariate *Google Searches* are -0.143*** (se=0.05), -0.134*** (se=0.05), -0.109 (0.09) and -0.309*** (0.14) for total offenders, 4-points offenders, 6-point offenders and 8-point offenders, respectively.

[31] Coefficient estimates and the corresponding standard errors for the covariate *Law Announced_2005* are -0.003 (se=0.04), 0.011 (se=0.04), 0.005 (0.07) and -0.127 (0.11) for total offenders, 4-points offenders, 6-point offenders and 8-point offenders, respectively.

a variable that measures number of driving licenses suspended[32] or withdrawn during the previous year relative to the total number of driving licenses in that particular province.[33] If our estimates were driven by the incapacitation effect, adding this control would reduce significantly the size of our PPS-related β_0 coefficient. This is not the case as we find that, in this new specification, estimated PPS effects remain negative, large and statistically significant. Indeed, once potential incapacitation effects are taken into account, the introduction of the PPS explains a 29% decrease of 8-point offenders, 26% decrease of 6-point offenders and 11% decrease of 4-point offenders. As expected, the effect of "driving licenses suspended or withdrawn in the previous year" on the different traffic violations is negative but lacks precision.[34]

Alternative Explanations: Police Surveillance and Radar Controls

An important threat to our analysis would be that at the same time that the PPS is implemented, there is an increase in police monitoring directly by police or via radar controls. If the implementation of the PPS would have come hand in hand with an increase of police resources, we may have expected drivers to undertake more Google searches of the terms "radar controls and monetary sanctions" just as we saw for the term "PPS" in Figure 4. Figure 6 shows Google searches for the terms "traffic controls" and "monetary traffic sanctions" over our time period. There is no evidence that the timing of the PPS was accompanied by an increase in searches for any of these two alternative terms, suggesting that drivers were not affected by them.

Figure 7 shows the share of traffic patrols, namely the total number of roadway troopers over the total number of police force. While there is some fluctuation across years, this share ranges around 7% between 2004 and 2008, making it unlikely to explain the observed decrease in the number of PPS-related offenders. To confirm this, we re-estimated our baseline model controlling for the yearly share of roadway troopers and safety cameras by province (shown in column 7 in Table 3). If our findings were driven by an increase in

[32] As driving suspensions last 6 months, they are incapacitating drivers.
[33] Results do not differ if we include driving licenses suspended the current year instead of the previous year.
[34] Coefficient estimates and the corresponding standard errors for the covariate *deprivations in the previous year* are -0.073*** (se=0.03), -0.069*** (se=0.05), -0.055 (se=0.05), -0.117 (se=0.11) for total offenders, 4-point offenders, 6-point offenders and 8-point offenders, respectively. Hence, deprivations reduce the number of offenders but does not affect the effect of reform.

roadway troopers and/or safety cameras, our coefficients of interest would become considerably smaller when moving our baseline model (shown in column 1 in Table 3) to the model in column 7 in Table 3. As this is not the case, it is unlikely that these alternative hypotheses are behind our results.[35]

Classification by Offence Type and Heterogeneity

Table 5 re-estimates Table 2 but classifying traffic offenders by the type of offence (speed, DUI, and not obeying signals) instead of severity of the offence. Doing so reveals that the introduction of the PPS reduced speed violations by 41%, DUI by 29.7%, and not obeying traffic signals by 13.5%. These findings are noteworthy in that they seem to indicate that, in Spain, the introduction of the PPS led to a change of "cultural" driving habits by which, prior to the reform, speeding and driving under the influence was probably more socially accepted than after the reform. To put it differently, the PPS reform was mostly driven by drivers' change in driving habits such as speeding or driving under the influence.

6. PPS effects on Road Accidents, Injuries and Fatalities

In this section, we analyze whether the PPS was successful in increasing road safety by reducing road accidents, injuries and fatalities. For simplicity of exposure, we have pooled road accidents, injuries and fatalities in two groups by whether they are PPS-related or not as explained in the Data Section. Results are shown in Tables 6 and 7, respectively.

6.A. Road Accidents

Focusing first on the effect of the PPS on road accidents, it is worthwhile to note that PPS-related estimates in row 1, columns 1 to 3 in Table 6 range from -0.160 to -0.148 and are statistically significant at the 1% level. Moving from column 1 (which only controls for province, month, day of the week and day of the month fixed effects) to column 2 (which adds seasonal patterns and province-specific business cycle and local holidays) has a small effect on the PPS-related estimate suggesting that this effect is not very sensitive to the

[35] Except for the 8-points models, none of the coefficients on the covariates *yearly share of roadway troopers and safety cameras* are statistically significant at the 90% level, although the signs are negative as expected. For the 8-point offenders model, the coefficient on the covariate *yearly share of safety cameras* is negative and statistically significant (-0.0148***, se=0.00). The coefficient on *yearly share roadway troopers* is -0.0891, se=0.12.

inclusion of these additional covariates. The same happens when adding other covariates related to passengers', drivers' or vehicle characteristics (shown in column 3). Not surprisingly given our earlier findings on offenders, moving to the specification in column 4, which narrows the estimation period to one year around the PPS reform, only decreases our estimate of interest by a tad. Columns 5 to 9 show that this result is robust to alternative specifications.

We find that the PPS caused a drop of 14.2% ($\approx e^{-0.153}-1$) in the number of PPS-related road accidents (column 2). In contrast, the PPS did not affect PPS-unrelated road accidents as the coefficient of interest ranges between +0.054 and +0.190 and is never statistically significant at the standard significance levels (shown in Table 7). It is also important to underscore that we find no decrease in accidents when a fictitious reform date is used instead of July 1st 2006 (columns 10 and 11 in Table 6).

6.B. Injuries and Fatalities

Moving now to the effect of the PPS reform on injuries and fatalities, we observe from our preferred specification (shown in column 2 in Table 6) that the PPS reform reduced by 15.3% PPS-related injuries and by 16.4% PPS-related fatalities.[36] Our fatality findings are close to those by Izquierdo et al. (2011) who estimate the effect of the PPS in Spain on fatalities using an ARIMA model.[37] Both of these estimates are statistically significant at the 1% level and robust to alternative specification shown in columns 1, 3, 4 to 9. Most importantly, even when we constrained the sample period to July 1st 2005 to July 1st 2007 (hence, excluding the criminal-justice reform), we observe that the PPS reform reduced PPS-related injuries and fatalities by 12.8% (shown in column 4). Even though the PPS effect on fatalities loses significance, the size of the estimate is far from negligible. In contrast, the PPS reform did not decrease the PPS-unrelated injuries or fatalities (shown in Table 7). Finally, as in earlier findings, the placebo exercise (shown in columns 6 and 7 in Table 6) reinforces the causal interpretation of our analysis: centering the sample around July 2008 (that is with only post-reform data) does not yield a decrease of either outcome.

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[36] The 95% confidence interval for these estimated effects is [-29%, -3%] for fatalities and [-20%,-10%] for injuries.

[37] Izquierdo et al. (2011) only look at the effect of the Spanish PPS reform on fatalities.

7. Conclusion

This paper uses a quasi-experimental approach to study the causal effect that the introduction and severity of a penalty-point system (PPS) has on drivers, accidents, injuries and fatalities. Consistent with Becker's theory of general deterrence in a context of traffic violations, we find that the number of PPS-related traffic offenders declined by 13.8% after the reform and that the deterrence effect was directly related to the size of the point loss: offenders of highly severe infractions dropped by 29%, offenders of severe infractions dropped by 26% and those of mild infractions by 11.7%. We also estimate that the PPS reform curbed PPS-related accidents, injuries and fatalities by 14,2%, 15.1% and 16.1%, respectively. These findings are robust to a battery of sensitivity tests, including a placebo test with a fictitious reform date. Crucially, the timing of the PPS implementation had no effect on road incidents unrelated to PPS regulations.

Using estimates from Tables 1 and 6, we conclude that the PPS caused, on average, a *yearly* reduction in 399 fatalities and 8,095 non-fatal victims.[38] According to Maibach *et al.* (2008) and Korzhenevych *et al.* (2014), the monetary value of statistical life (VSL) in Spain is 1.8 and 0.058 million Euros per fatal and non-fatal victims, respectively.[39] Hence, the reduction in 399 fatalities and 8,095 non-fatal victims implies a yearly gain of 720 million Euros and 468 million Euros, respectively, amounting to a total benefit of 1,188 million Euros (with a 95% confidence band ranging between 394 and 1,881 million Euros).

At the same time, because the PPS reform only added non-monetary penalties to existent traffic violations, the cost of implementing the policy were relatively modest. More specifically, according to the Spanish government, the cost of implementing the PPS was at most 23.3 million Euros over the same period (*Memoria de Actuaciones de Seguridad Vial*, 2005 to 2008). Comparing costs and benefits, the social return of this reform was 51 euros for every 1 Euro spent to implement the PPS (i.e. 1,188/23.3). Overall, the PPS represented expected net gains of 1,164.7 million Euros, representing an efficiency gain of 0.15% of the GDP.[40]

[38] $399 = 4.12 \times (1\text{-}e^{-0.176}) \times 50 \times 12$, and $8{,}095 = 89.2 \times (1\text{-}e^{-0.164}) \times 50 \times 12$.

[39] These VSL are expressed in 2006 Euros using the GDP implicit deflator. The cost for non-fatal victim weights is a weighted average of severe (0,224 million) and mild injuries (0,017 million) using the following fraction of victims: 19,7% for severe and 80,3% for mild injuries from the Dirección General de Tráfico.

[40] According to INE, the 2006 nominal GDP was 752,142 million Euros. The total net gains from the reform, 1,164.7 Euros (1,164.7 =1,188-23.3) is expressed in 2006 Euros and accounts for 0.15% of GDP. This estimate

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is smaller than those estimated for the United States (Parry, Walls and Harrington 2007) in part because the VSL is much higher in the US than in Spain and because we did not include gains from reduction in property damage, travel delay, medical and emergency services costs, or the opportunity cost of reduced productivity.

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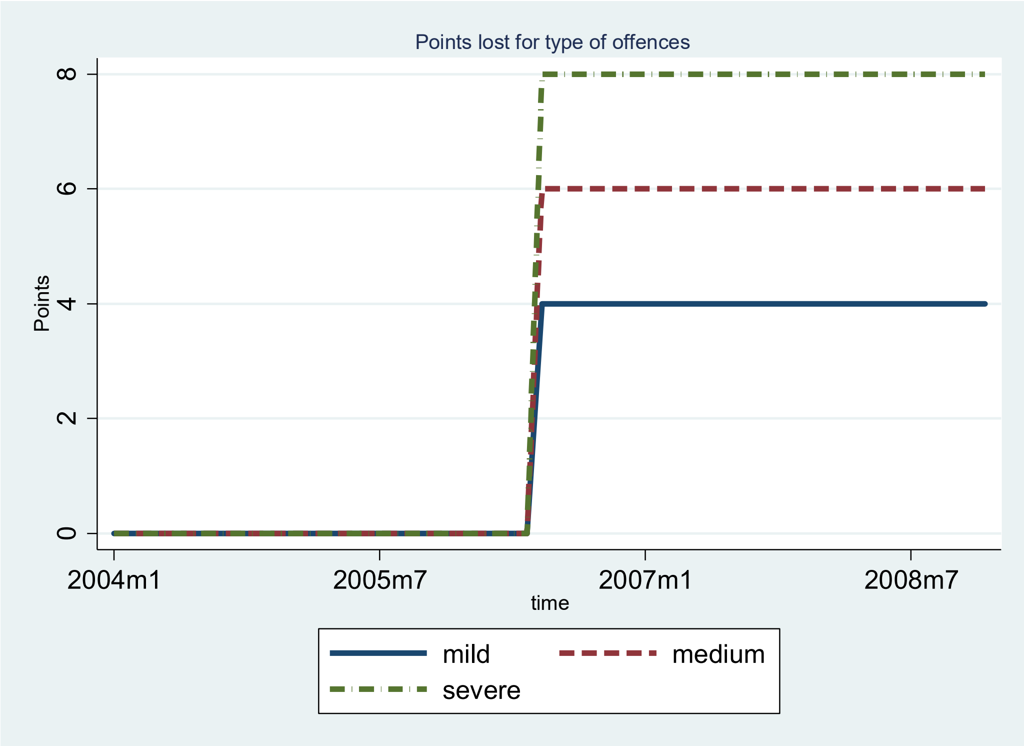
Tables and Figures

Figure 1: Points lost for type of offences



Source: Own Elaboration

Table 1: Descriptive Statistics
(Monthly averages per province)

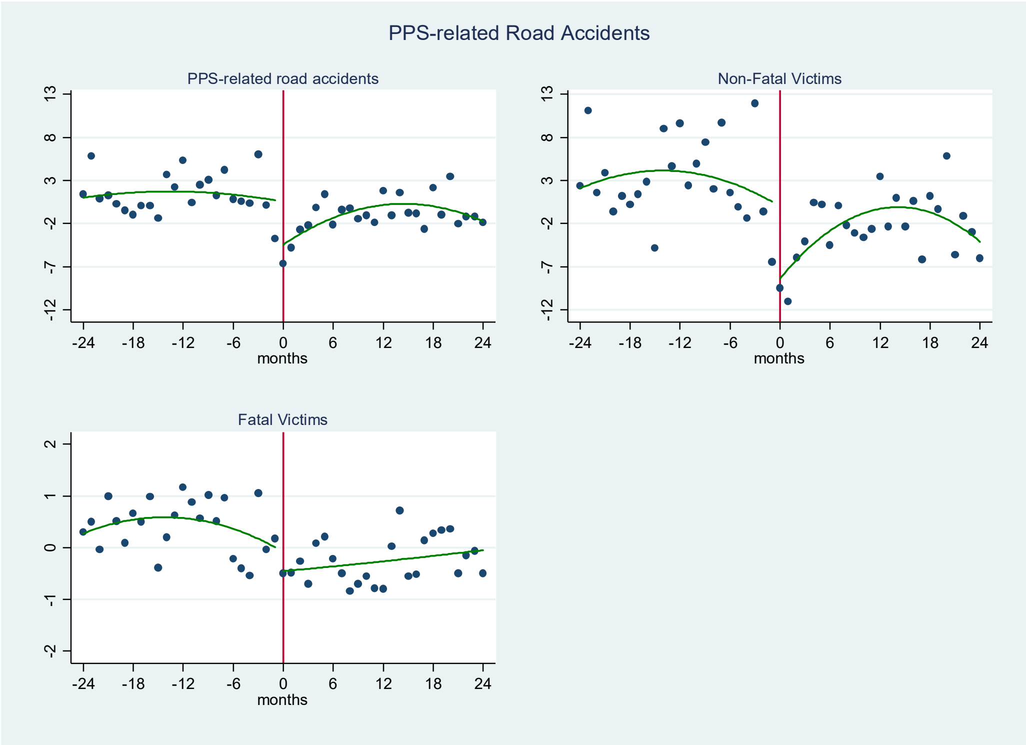
	Before PPS July 2004-June 2006 Mean (Sd)	After PPS July 2006-June 2008 Mean (Sd)	Diff Absolute	Diff % change
Total offenders	**57.5 (44)**	**54.0 (45)**	**-3.47*****	**-6.04%**
By points lost:				
4-point offenders	49.7 (38)	47.2 (40)	-2.47**	-4.97%
6-point offenders	5.22 (4.6)	4.52 (3.8)	-0.69***	-13.21%
8-point offenders	2.59 (2.8)	2.28 (2.4)	-0.31***	-11.96%
PPS-related road accidents				
PPS-related road accidents	53.4 (40)	49.3 (40)	-4.10**	-7.68%
Fatalities	4.12 (3.3)	3.05 (2.6)	-1.07***	-25.97%
Injuries	89.2 (66)	80.9 (63)	-8.30***	-9.30%

Source: DGT and own calculations.
These statistics reflect mean (standard deviations in parenthesis). The symbols *** and ** indicate that the pre-post-reform differences are statistically significant at the 1% and 5% level, respectively.

Figure 2. Traffic Offenders Before and After the PPS Reform

Note: Following Lee and Lemieux (2009) and Hausman and Rapson (2017), we deseasonalize the time series by regressing the outcomes of interest on monthly dummies, quarterly local unemployment rate, and monthly local gas prices, and then plotting the residuals. The fitted line uses a second-order polynomial trend.

Figure 3. Accidents, Injuries and Fatalities Before and After the PPS Reform



Note: Following Lee and Lemieux (2009) and Hausman and Rapson (2017), we deseasonalize the time series by regressing the outcomes of interest on monthly dummies, quarterly local unemployment rate, and monthly local gas prices, and then plotting the residuals. The fitted line uses a second-order polynomial trend.

Table 2. DB Estimates of the PPS on Number of Offenders

	Model Specification			One-year bandwidth	Placebo tests [F(t)=2 & F(t)=3]		Google	Law Approval 2005	Incapacitations
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Offenders (Total)	-0.156***	**-0.149***	-0.143***	-0.127***	0.106	0.082	-0.143***	-0.148***	-0.144***
	(0.03)	**(0.03)**	(0.03)	(0.04)	(0.06)	(0.06)	(0.03)	(0.03)	(0.03)
4-point offenders	-0.132***	**-0.125***	-0.121***	-0.105**	0.107	0.108	-0.118***	-0.119***	-0.119***
	(0.03)	**(0.03)**	(0.03)	(0.04)	(0.07)	(0.07)	(0.03)	(0.03)	(0.03)
6-point offenders	-0.300***	**-0.303***	-0.274***	-0.154	-0.009	-0.176	-0.299***	-0.300***	-0.300**
	(0.06)	**(0.06)**	(0.06)	(0.09)	(0.18)	(0.18)	(0.06)	(0.07)	(0.06)
8-point offenders	-0.346***	**-0.349***	-0.341***	-0.472***	0.089	0.018	-0.334***	-0.414***	-0.343**
	(0.09)	**(0.09)**	(0.09)	(0.13)	(0.29)	(0.32)	(0.09)	(0.10)	(0.09)
Main Covariates:									
Province and month FE	X	X	X	X	X	X	X	X	X
Day of the week and day of the month FE	X	X	X	X	X	X	X	X	X
Local holidays		X	X	X			X	X	X
Business cycle and traffic intensity		X	X	X			X	X	X
Other Covariates									
Share of new Cars (%)			X						
Share of cars (%)			X						
Share of female drivers (%)			X						
Google Index							X		
Law Approval								X	
Incapacitation									X
Sample period	2004-08	2004-08	2004-08	2005-07	2007-09	2007-09	2004-08	2004-08	2004-08
Sample size	74,600	74,600	74,600	38,050	38,100	38,100	74,600	74,600	74,600

Notes: The unit of observation is province-day. Standard errors (in parenthesis) are clustered at the province-month level. The symbols ***, **, * indicate that coefficients are statistically significant at the 1%, 5%, 10% level, respectively. A polynomial trend interacted with the PPS has been included in all cases, imposing a possible break before and after July 1st 2006. The order of the polynomial is two except for one-year bandwidths –columns from 4 to 6-, where the polynomial order is one. As 2008 was a leap year, the sample used to estimate the placebo test has 38,100 observations (instead of 38,050).

Table 3: Sensitivity Analysis: DB Estimates of the PPS on Number of Offenders

	Baseline model	Poisson	Std. errors clustered at: Month level	Day level	Polynomial order: F(1)=1	F(t)=3	Controlling for Enforcement
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Offenders (Total)	**-0.149*** **	-0.147***	-0.149***	-0.149***	-0.031	-0.133***	-0.156***
	(0.03)	(0.03)	(0.04)	(0.02)	(0.02)	(0.05)	(0.03)
4-point offenders	**-0.125*** **	-0.120***	-0.125***	-0.125***	-0.015	-0.117***	-0.130***
	(0.03)	(0.03)	(0.03)	(0.02)	(0.02)	(0.05)	(0.05)
6-point offenders	**-0.303*** **	-0.303***	-0.303***	-0.303***	-0.127***	-0.249**	-0.332**
	(0.06)	(0.06)	(0.06)	(0.06)	(0.04)	(0.09)	(0.06)
8-point offenders	**-0.349*** **	-0.350***	-0.349***	-0.349***	-0.148**	-0.250*	-0.346***
	(0.09)	(0.09)	(0.08)	(0.08)	(0.06)	(0.14)	(0.09)
Main Covariates:							
Province and month FE	X	X	X	X	X	X	X
Day of the week and day of the month FE	X	X	X	X	X	X	X
Local holidays	X	X	X	X	X	X	X
Business cycle and traffic intensity	X	X	X	X	X	X	X
Standard errors clustered at:	Province x months	Province x months	months	days	Province x months	Province x months	Province x months

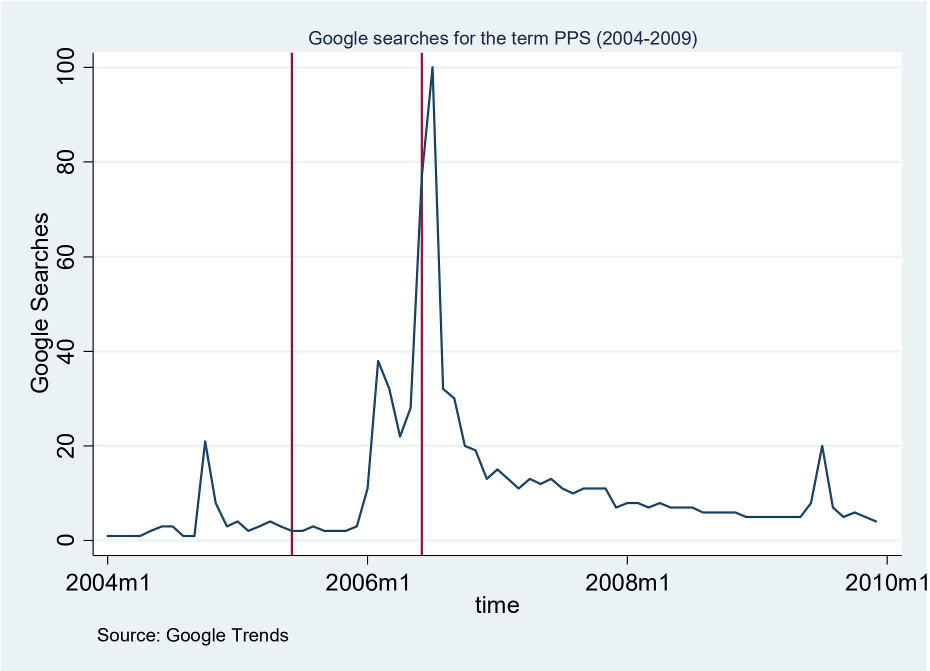
Notes: Sample period 2004-2008 for all models (74,600 observations). The unit of observation is province-day. Standard errors are clustered at the province-month level. The symbols ***, **, * indicate that coefficients are statistically significant at the 1%, 5%, 10% level, respectively. The order of the polynomial is two. A polynomial trend interacted with the PPS has been included in all cases, imposing a possible break before and after July 1st 2006. All regressions incorporate a constant term.

Table 4: Falsification Test
DB Estimates of the PPS on Number of PPS-Unrelated Events (2004-2008)

			Polynomial order:	
			F(t)=1	F(t)=3
	(1)	(2)	(3)	(4)
PPS-unrelated events	-0.013	0.001	-0.010	0.039
	(0.11)	(0.11)	(0.08)	(0.17)
Drowsy driving or sudden disease	0.059	0.057	0.046	0.195
	(0.11)	(0.11)	(0.08)	(0.17)
Pedestrian infractions	-0.013	0.011	-0.010	0.039
	(0.11)	(0.11)	(0.08)	(0.17)
Bad Road Conditions	0.107*	0.075	0.200***	0.118
	(0.06)	(0.06)	(0.05)	(0.10)
Car´s break down	0.494	0.517	0.268	0.627
	(0.34)	(0.33)	(0.22)	(0.49)
Main Covariates:				
Province and month FE	X	X	X	X
Day of the week and day of the month FE	X	X	X	X
Local holidays		X	X	X
Business cycle and traffic intensity		X	X	X

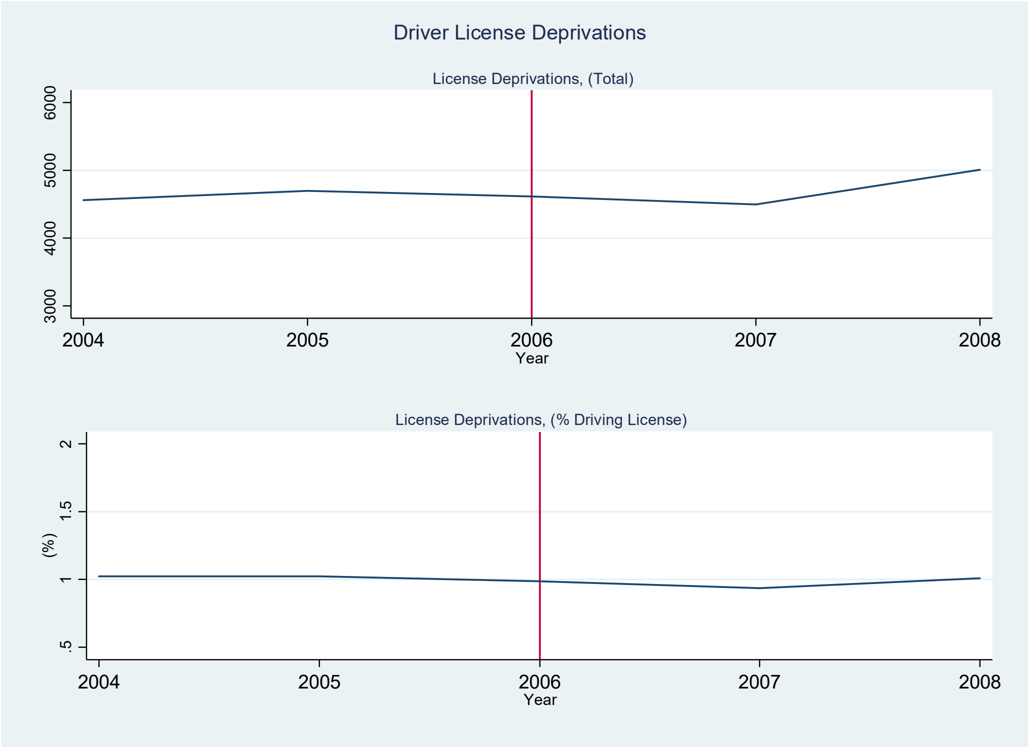
Notes: The unit of observation is province-day. Standard errors are clustered at the province-month level. The symbols ***, **, * indicate that coefficients are statistically significant at the 1%, 5%, 10% level, respectively. The polynomial order is two. A polynomial trend interacted with the PPS has been included in all cases, imposing a possible break before and after July 1st 2006. All regressions incorporate a constant term.

Figure 4: Google searches for the term PPS (2004-2009)



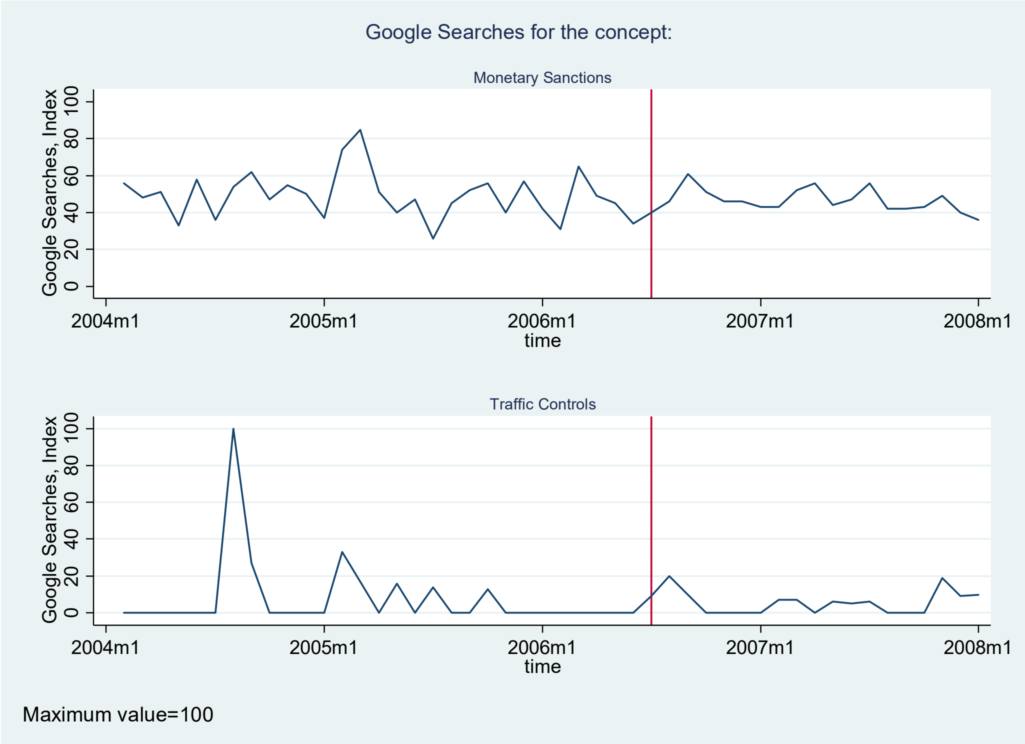
Note: Source Google Trends and Own Calculations

Figure 5: Number and Share of Licensed Suspended



Source: DGT and own calculations

Figures 6: Google Searches for Monetary Sanctions and Traffic Controls



Note: Source Google Trends and Own Calculations

Figure 7: Number of Police in Traffic Patrols relative to Total Police Force (%)

Note: Source *Ministerio del Interior* an *own calculations*

Table 5. Estimates of the PPS by type of offense (2004-2008)

	Model Specification			One-year bandwidth	Placebo tests [F(t)=2 & F(t)=3]		Google	Law Approval 2005	Incapacitations
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Speed	-0.526***	**-0.537*****	-0.505***	-0.365***	0.356	0.182	-0.533***	-0.530***	-0.528***
	(0.07)	**(0.07)**	(0.07)	(0.10)	(0.22)	(0.23)	(0.07)	(0.08)	(0.08)
Alcohol	-0.354***	**-0.353*****	-0.363**	-0.369***	-0.088	-	-0.351***	-0.357***	-0.363***
	(0.06)	**(0.06)**	(0.06)	(0.09)	(0.14)	-	(0.06)	(0.07)	(0.06)
Not obeying signals	-0.153***	**-0.146*****	-0.140***	-0.128***	0.115*	0.086	-0.139***	-0.143***	-0.139***
	(0.03)	**(0.03)**	(0.03)	(0.04)	(0.06)	(0.07)	(0.03)	(0.03)	(0.03)
Main Covariates:									
Province and month FE	X	**X**	X	X	X	X	X	X	X
Day of the week and day of the month FE	X	**X**	X	X	X	X	X	X	X
Local holidays		**X**	X	X			X	X	X
Business cycle & traffic intensity		**X**	X	X			X	X	X
Other Covariates									
Share of new Cars (%)			X						
Share of cars (%)			X						
Female drivers (%)			X						
Sample period	2004-08	2004-08	2004-08	2005-07	2007-09	2007-09	2004-08	2004-08	2004-08
Sample size	74,600	74,600	74,600	38,050	38,100	38,100	74,600	74,600	65,400

Notes: The unit of observation is province-day. Standard errors (in parenthesis) are clustered at the province-month level. The symbols ***, **, * indicate that coefficients are statistically significant at the 1%, 5%, 10% level, respectively. A polynomial trend of order two is interacted with the PPS has been included in all cases, imposing a possible break before and after July 1st 2006. All regressions incorporate a constant term. As 2008 was a leap year, the sample used to estimate the placebo test has 38,100 observations (instead of 38,050). For alcohol offences results for the placebo exercise using polynomial degree three can not be presented due to convergence problems

Table 6: DB Estimates of PPS Effects on PPS-Related Road Accidents, Fatalities and Injuries

				Robustness							
						S.e clustered at		Polynomial Order		Placebo tests [F(t)=2 & F(t)=3]	
	Model Specification			One-year Bandwidth	Poisson	months	days	F(t)=1	F(t)=3		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
Accidents	-0.160***	**-0.153*****	-0.148***	-0.119***	-0.134***	-0.134***	-0.134***	-0.159***	-0.109*	0.104	0.087
	(0.03)	**(0.03)**	(0.03)	(0.04)	(0.05)	(0.04)	(0.03)	(0.03)	(0.06)	(0.06)	(0.07)
Fatalities	-0.176**	**-0.176****	-0.167**	-0.138	-0.181**	-0.176**	-0.176**	-0.126**	-0.297**	0.323	0.268
	(0.08)	**(0.08)**	(0.08)	(0.19)	(0.08)	(0.08)	(0.06)	(0.05)	(0.12)	(0.23)	(0.23)
Injuries	-0.168**	**-0.164*****	-0.176**	-0.138***	-0.168***	-0.164***	-0.164***	-0.052***	-0.124***	0.155**	0.156*
	(0.03)	**(0.03)**	(0.03)	(0.07)	(0.03)	(0.03)	(0.03)	(0.02)	(0.05)	(0.07)	(0.08)
Main Covariates:											
Province and month FE	X	**X**	X	X	X	X	X	X	X	X	X
Day of the week and day of the month FE	X	**X**	X	X	X	X	X	X	X	X	X
Local holidays		**X**	X	X	X	X	X	X	X		
Business cycle and traffic intensity		**X**	X	X	X	X	X	X	X		
Other Covariates											
Share of new Cars (%)			X								
Share of cars (%)			X								
Female drivers (%)			X								
Passengers´ Characteristics			X								
Sample Period	2004-08	**2004-08**	2004-08	2005-07	2004-08	2004-08	2004-08	2004-08	2004-08	2007-09	2007-09
Sample size	74,600	**74,600**	74,600	38,050	74,600	74,600	74,600	74,600	74,600	38,100	38,100

Notes: The unit of observation is province-day. Standard errors are clustered at the province-month level. The symbols ***, **, * indicate that coefficients are statistically significant at the 1%, 5%, 10% level, respectively. The polynomial order is two except for one-year bandwidth. The polynomial trend interacted with the PPS has been included in all cases, imposing a possible break before and after July 1st 2006. All regressions incorporate a constant term. As 2008 was a leap year, the sample used to estimate the placebo test has 38,100 observations (instead of 38,050).

Table 7: DB Estimates of PPS Effects on PPS-Unrelated Road Accidents, Fatalities and Injuries

	Model Specification		Polynomial Order	
			One order lower	One order higher
	(1)	(2)	(3)	(4)
Accidents	**0.102**	0.054	0.065	0.190*
	(0.14)	(0.08)	(0.06)	(0.11)
Fatalities	**-0.113**	-0.139	-	-0.217
	(0.10)	(0.11)	-	(0.40)
Injuries	**0.054**	0.012	0.179***	0.151
	(0.07)	(0.06)	(0.05)	(0.10)
Main Covariates :				
Province and month FE	**X**	X	X	X
Day of the week and day of the month FE	**X**	X	X	X
Local holidays	**X**	X	X	X
Business cycle	**X**	X	X	X
Province traffic intensity	**X**	X	X	X
Other Covariates				
Province/year drivers' and cars' composition		X		
Passengers´ Characteristics		X		

Notes: The unit of observation is province-day. Standard errors are clustered at the province-month level. The symbols ***, **, * indicate that coefficients are statistically significant at the 1%, 5%, 10% level, respectively. The polynomial order is two. The polynomial trend interacted with the PPS has been included in all cases, imposing a possible break before and after July 1st 2006. All regressions incorporate a constant term.

APPENDIX TABLES

Table A.1: Sample Characteristics

	Before PPS July 2004- June 2006 Mean	After PPS July 2006-June 2008 Mean
Seasonal and Business-Cycle Covariates		
Price of diesel (average per liter, province/month)	2.19 €	2.22 €
Local unemployment rate (province/quarter)	9.5%	9.2%
Holidays (% per month, includes Sundays, province/day)	18%	21%
Other Covariates		
New cars' registrations (% over the total registered, province/year)	7.0%	5.9%
Ratio of registered vehicles to total number of licensed drivers (province/year)	1.13	1.18
Share of registered cars to total number of registered vehicles (province/year)	72%	71%
Share of female drivers (province/year)	37%	38%

Note: The unit of observation depends on the covariates as explained in the main text.

Table A.2: Baseline Specification: Detailed Results for Traffic Offenders

VARIABLES	(1) PPS-Offenders (Total)	(2) 4-point PPS-offenders	(3) 6-point PPS-offenders	(4) 8-point PPS-offenders
PPS=1	-0.149***	-0.125***	-0.303***	-0.349***
	(0.03)	(0.03)	(0.06)	(0.09)
F(t)*(PPS=1)	0.006***	0.006***	0.004	0.008**
	(0.00)	(0.00)	(0.00)	(0.00)
F(t)*(PPS=0)	-0.0014	-0.001	-0.004	-0.014***
	(0.00)	(0.00)	(0.00)	(0.00)
$F(t)^{\wedge 2}$*(PPS=1)	-0.008***	-0.001***	-0.006*	-0.006
	(0.00)	(0.00)	(0.00)	(0.01)
$F(t)^{\wedge 2}$*(PPS=0)	0.002*	0.001	0.006*	0.0139***
	(0.00)	(0.00)	(0.00)	(0.01)
Fuel prices (logs)	0.017***	0.015***	0.036***	0.0145
	(0.01)	(0.01)	(0.01)	(0.02)
Local unemployment rate	-0.911**	-0.797	-2.172***	-0.818
	(0.33)	(0.35)	(0.79)	(1.09)
Holiday (=1)	0.021	-0.020	0.234***	0.331***
	(0.02)	(0.02)	(0.04)	(0.06)
Registered vehicles to total # of drivers	-0.554***	-0.512***	0.871*	0.516
	(0.20)	(0.21)	(0.49)	(0.60)
Constant	-3.290	-2.994	-6.235*	-17.400***
	(1.57)	(1.72)	(3.62)	(4.86)
Ln(α)	-2.157***	-2.020***	-2.010***	-1.807***
	(0.04)	(0.04)	(0.21)	(0.33)
Observations	74,600	74,600	74,600	74,600
All FE	YES	YES	YES	YES

Notes: The unit of observation is province-day. Standard errors (in parenthesis) are clustered at the province-month level. The symbols ***, **, * indicate that coefficients are statistically significant at the 1%, 5%, 10% level, respectively. A polynomial trend interacted with the PPS has been included in all cases, imposing a possible break before and after July 1st 2006. The order of the polynomial is two. Robust standard errors in parentheses. The term α is the over dispersion parameter so the larger α is, the greater the over dispersion. This term confirms the presence of over dispersion, that is, the Poisson specification is, itself, rejected in favor of the negative binomial model.