Estimating the Impact of the Penalty Point System on Road Fatalities in Spain

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Abstract

Traffic accidents are a major public health concern since they are the leading cause of death for those aged 15-29 years and the ninth cause of death worldwide. In this paper we estimate the overall effect on traffic fatalities of the introduction of the Penalty Point System (PPS) in Spain in 2006, jointly with those of the publicity campaigns that went with it and the reform of the Penal Code to toughen the consequences of traffic offenses in 2007. We use a synthetic-control method that controls for differences in the distribution of control variables, changing business cycles conditions, the effect of unexpected policies or events that happen between the pre- and the post-PPS periods and the arbitrariness in the selection of the control group. We find that the introduction of the PPS and related initiatives lead to a reduction of almost 15% in traffic fatality rates in Spain during the first two years. The magnitude of the estimated effect monotonically increased over time until reaching a 40% reduction in fatality rates in 2009 and 2010.

Keywords: Penalty point system, causal effect, synthetic-control methods.

1. Introduction.

Traffic accidents are a major public health concern since they are the leading cause of death for those aged 15-29 years and the ninth cause of death worldwide (WHO, 2015). The deaths, disabilities and injuries provoked by traffic accidents have substantial individual and social costs like, among others, medical care, the losses of GDP due to premature death or permanent or temporary disability as a result of the accident or the quality of life losses of those having the accident and the sorrow and pain experienced by their relatives, friends and the society.

For Spain, Martínez-Pérez et al. (2015) estimated the cost of a fatal accident to be 1.4 million euros, with that for severe and minor non-fatal accidents being of 219,000 and 6,100 euros, respectively. When combining these estimates with the total number of accidents in each category in Spain in 2015, we find that the total cost of traffic accidents was above 1% of Spanish Gross Domestic Product.

Since empirical evidence confirms that the human factor accounts for almost 94% of total accidents (Singh, 2015), traffic laws in most countries have tried to modify driver behavior in the last decades by introducing a Penalty Point System (PPS)[i]. In a PPS, every driver implicated in a traffic offense is assigned a positive or negative (penalty) number of points, depending on the seriousness of the offense. When the total number of points accumulated or deducted reaches the allowed limit, the driving license is suspended or revoked. That is, the PPS is both a corrective and a preventive policy (Nolen and Ostlin, 2008).

Spain was one of the last European Union countries in implementing the PPS. The Spanish PPS came into force on July 2006 by subtracting points from the initial credit assigned to every driver for every traffic offense, as it is the case in other Southern

European countries like France and Italy. Additionally, in 2007 the Spanish Penal Code was reformed to toughen the consequences of traffic offenses.

In this paper we analyze the effect of the introduction of the PPS, and the surveillance measures, sanctions and publicity campaigns that went with it in 2006 and 2007 on road fatality rates in Spain. Throughout this paper we use the expression “effect of the PPS” to refer to the overall effect of the PPS and related initiatives introduced in 2006 and 2007.

To estimate the effect of interest there are some relevant concerns that we have to address. First, the Spanish PPS is a non-targeted policy (i.e. it applies to all drivers) and, thus, there is no contemporaneous control or comparison group in Spain that allows us to identify the causal effect of interest while controlling for both changing business cycles conditions and the effect of other non-targeted policies and events that happen between the pre- and the post-PPS periods. Additionally, there is no other European Union country that replicates the value of the time trend of road fatalities in Spain before 2006 even after controlling for common determinants of road fatalities. Thus, the comparison with other European Union countries does not identify the causal effect of interest.

To meet all these concerns we follow Abadie and Gardeazabal (2003) by using data from other European Union countries to construct a “synthetic” Spain without the PPS. The causal effect of interest can be identified by simply comparing the observed to the “synthetic” Spain.

The remainder of the paper is organized as follows. Section 2 reviews the literature. Section 3 describes the data. The identification strategy is presented in section 4. Sections 5 and 6 present and discuss the implications of our estimates, respectively. Section 7 discusses the limitations of our analysis and, finally, Section 8 concludes.

2. Literature review.

A PPS supplements traditional monetary sanctions. As shown by Bourgeon and Picard (2007), a system based on non-monetary sanctions such as the PPS might be more effective than monetary sanctions for traffic offenses. There is a general consensus about the main aims of the PPS. Firstly, this system discourages hazardous behavior since drivers can lose their driving license if they commit offences repeatedly. Secondly, it allows the traffic authorities to identify high-risk drivers and to withdraw them from the road before causing an accident. Finally, it might correct unsafe behavior as road users can regain points by attending driver improvement courses (SWOV, 2012). All the European Union countries but Belgium, Slovakia, Estonia, Lithuania, Portugal and Sweden have implemented the PPS.

There is now substantial evidence that the PPS is effective at lowering fatality rates, particularly so in developed countries. This effect is usually mediated by the effect of the PPS on some relevant dimensions of driver behavior like, among others, increasing seat belt and helmet usage rates and avoiding risky behaviors like speeding, driving under the effects of alcohol or drugs and using the mobile phone while driving.

That is the case, among other countries, for Italy, for which Zambon et al. (2007) found that the number of both fatalities and injured people declined by approximately 18% after the introduction of PPS. They also found that seat belt usage rates increased by at least 53% among both drivers and front passengers in the three months following the introduction of the PPS. Along these lines Farchi et al. (2007) found that the total number of hospital admissions, emergency department visits and traffic deaths lowered by 12%, 13%, and 7%, respectively, after the PPS came into force in Lazio, Italy. De Paola et al. (2013) also found that, after controlling for weather conditions, police patrols, speed cameras, gasoline prices, and unemployment rate, the number of road collisions and fatalities lowered by 9% and 30%, respectively, as a result of the introduction of the PPS in Italy.

For Ireland, Healy et al. (2004) found a significant reduction in the number of road traffic accidents related to admissions in the National Spinal Injuries Centre in the first six months. However, this effect was not extended into the subsequent six months. Additionally, Donnelly et al. (2005) found that there was fewer femoral shaft fracture discharges linked to traffic accidents after the introduction of the PPS in Ireland.

Similar results have been obtained in other latitudes like, for example, Brazil. Poli de Figueiredo et al. (2001) found that the new Brazilian Traffic Code, that included a rigid PPS, provoked a reduction of 21% and 25% in the number of accidents and deaths, respectively.

For Spain, Aparicio-Izquierdo et al. (2011) found that the number of fatalities on the road within 24 hours after the accident, the number of excess speed sanctions from radar controls and the number of sanctions for non-use of protective systems (seat belt and helmet) lowered from before to after (until 2009) the PPS came into force. This evidence was interpreted as supportive of the hypothesis that the PPS, and the surveillance measures, sanctions and publicity campaign that went with it, succeed at modifying driver behavior and, thus, at lowering fatality rates in Spain.

Similar results are obtained in Castillo-Manzano et al. (2010) and Novoa et al. (2010). The former study used a before-after comparison and found that the PPS had an initial impact on deaths and vehicle occupants injured on highways and vehicle occupants injured in built-up areas of 18.2%, 15.3%, and 17.2%, respectively. However, these effects vanished in 40, 9, and 11 months, respectively. Novoa et al. (2010) used Poisson regression models adjusted for over-dispersion and they found that, controlling for time trend and seasonal pattern, fuel consumption, unemployment rate, and gross domestic product, Spanish PPS led to a 10% reduction in dead drivers and a 8% reduction in fatally injured people.

Additionally, Gras et al. (2014) found that Spanish drivers significantly lowered the relative frequency of some risky behaviors like speeding, driving under the effects of alcohol or drugs and using the mobile phone while driving after the introduction of the PPS.

However, there are also studies with rather negative results. Mehmood (2010) found that the demerit point system (DPS) had statistically no significant impact on the speeding behavior of drivers in Al Ain. In addition, as shown in the meta-analysis in Castillo-Manzano and Castro-Nuño (2012), PPS has a strong initial positive impact, but it seems to wear off in under eighteen months.

Preceding evaluations of the effects of the PPS in Spain share some limitations that call for caution when interpreting their estimates as causal effects. Firstly, while all these studies use time series techniques to control for business cycles and seasonality effects, their identification strategy is based on a before-after comparison that cannot control for the effect of other non-targeted policies and unexpected events that happen between the pre- and the post-PPS periods. Since the PPS applies to all drivers and preceding studies only use data for Spain, there is no contemporaneous comparison or control group that would approximate what the fatality rate would have been in Spain in the absence of the PPS after 2006. The lack of an appropriate counterfactual is the reason why preceding estimates should be taken with caution.

Additionally, our estimates confirm that there is no European Union country that follows a similar or parallel (to Spain) trend on road fatalities before 2006, even after controlling for common determinants of road fatalities and, thus, the comparison with other European Union countries is not useful to identify the causal effect of interest. To overcome these limitations we follow Abadie and Gardeazabal (2003) and use data for other European countries to construct a “synthetic” Spain without the PPS, that is, a valid counterfactual.

3. **Methodology**.

The causal effect of a policy (i.e., treatment) on those affected by policy (i.e., treated) can be identified by comparing, once conditioning on observed individual and aggregate characteristics that affect the outcome of interest, the average outcome for the treated after the policy comes into force, with the average value that would have been otherwise observed for the treated in the absence of the treatment. Obviously, the latter value cannot be observed and, thus, the problem of evaluating the effect of a treatment becomes the problem of finding a suitable contemporaneous control or comparison group not affected by the policy under evaluation (i.e. controls) whose behavior approximates that of the treated in the absence of the treatment. The plausibility of the latter assumption is usually tested in the pre-treatment period.

The evaluation problem becomes more interesting when we deal with a non-targeted treatment that affects all the agents living in the same country or region, as it is the case with the PPS in Spain. An alternative in this situation would be to use data for other countries or territories to approximate the counterfactual and, thus, to estimate the causal effect of interest. The identification hypothesis in this case is that the other countries informs on what the average outcome would have been in the treated country in the absence of the treatment once conditioning on covariates. As before, the plausibility of this assumption can be tested in the pre-treatment period.

Alternatively, that could be the case that the treated and comparison countries do not show the same average value of the outcome variable, once conditioning on covariates, in the pre-treatment period, but that the average outcome variable increases at the same rate in both territories. In this case, the “difference-in-differences” method identifies the effect of interest using information on the treated and comparison countries.[ii]

Unfortunately, in our application there is no other European country for which the evolution of fatality rates in the pre-treatment period reproduces that for Spain or exhibits the same sensitivity to business cycles conditions. In other words, there is no country that allows us to identify the causal effect of the PPS that came into force in Spain in 2006. This might explain why preceding estimates of the effect of the PPS for Spain have not included another country or territory in order to approximate the counterfactual average fatality rate that would have been observed in Spain since 2006 in the absence of the PPS. Conversely, preceding estimates have used before-after comparisons that in some cases also control for differences over time in the distribution of some variables determining the outcome of interest. The main disadvantage of this approach is that it cannot control for other non-targeted policies or unexpected events that happen between the pre- and the post-PPS periods and, thus, the before-after estimator is likely to provide biased estimates of the causal effect of interest. Linden (2017) illustrates the bias caused when using the before-after or single-group interrupted time series method to identify the effect of Florida’s 2000 repeal of its motorcycle helmet law and California’s 1988 proposition to reduce cigarette consumption.

Abadie and Gardeazabal (2003) proposed a new approach called synthetic control method to identify the effect of interest when no control or comparison unit replicates the level or increment of the outcome variable for the treated in the pre-treatment period[iii]. In particular, they analyze the economic effects of the terrorist conflict in the Basque

Country. Since terrorism affected all Spanish regions and no other Spanish region replicates neither the value nor the growth rate of per capita income in the Basque Country before terrorism, the authors construct a “synthetic” Basque Country as the weighted average of the remaining Spanish regions that exactly reproduces the value of per capita income in the Basque Country in the pre-terrorism period. Equivalently, we use data on fatality rates for other European countries to construct the “synthetic” Spain that exactly reproduces the fatality rate in Spain before the PPS. The comparison between the observed and the “synthetic” Spain allows us to identify the effect of the PPS on road fatalities in Spain.

Synthetic control method has been used to identify the effects of road safety interventions such as prohibiting handheld cell phone use while driving (Sampaio, 2014) and liberalizing drinking bar hours (Green et al., 2014)

Some notation is useful at this point. Let J+1 be the number of countries used in the estimation, where j=1 for Spain and the remaining J countries are used to construct the counterfactual. Equivalently, index t indicates the year in which the data is measured, with $t^0=\{1,\ldots,t_0\}$ being the pre-treatment period and $t^1=\{t_0+1,\ldots,T\}$ the post-treatment period. Additionally, let y_{jt} be the outcome of interest (i.e. the number of fatalities) for country j measured in year t. Estimating the effect of the treatment on the treated unit (j=1) is our main objective. The effect of the PPS on fatalities in Spain, represented by δ, can be identified by estimating the following equation:

$$\delta = E\left[y_{jt} | j = 1, t \in t^1\right] - E\left[y^*_{jt} | j = 1, t \in t^1\right] \tag{1}$$

where y^*_{jt} is the number of fatalities that would have been occurred in Spain in the post-treatment period in the absence of the PPS, that is, the counterfactual. Since, as previously explained, the policy under evaluation is a non-targeted treatment and no other country considered in the estimation resembles the value or the time trend of the outcome variable for Spain in the pre-treatment period, equation (1) cannot be estimated.

Abadie and Gardeazabal (2003) proposes a solution to this identification problem that consists in looking for the non-linear combination of potential comparison countries that resembles the value of a large set of control variables determining the outcome of interest in the treated group in the pre-treatment period. This way of proceeding eliminates the arbitrariness in the selection of the control group.

Let W be a (J x 1) vector of weights $W = (w_2,\ldots,w_{J+1})'$ such that $\sum_{j=2}^{J+1} w_j = 1$ and $0 \leq w_j \leq 1$, for $j = \{2, \ldots, J+1\}$. Additionally, let X^1 be a vector of k pre-intervention covariates determining the value of y_{1t} for $\in t^0$, that is, the value of the outcome variable for Spain in the pre-treatment period. Equivalently, X^0 is a (k x J) vector of pre- intervention covariates determining the value of y_{jt} for $j \neq 1$ and $t \in t^0$, that is, the value of the outcome variable for countries different from Spain in the pre-treatment period. Our goal is to determine the value of W such that the values of the pre-intervention characteristics in the treated and comparison countries are as close as possible.

The optimization problem can be written as follows

$$min_{w \in \omega}(X^1 - X^0W)'V(X^1 - X^0W) \qquad (2)$$

where V is a diagonal matrix that indicates the relevance of each of the covariates in X in determining the value of the outcome variable y_{1t} for $t \in t^0$, and ω is the set of all possible weighting vectors. Once the optimal value of the weighting matrix has been identified, the counterfactual is estimated as follows:

$$E\left[y_{jt}^*\middle|j = 1, t \in t^1\right] = \sum_{j=2}^{J+1} w_j^*. E\left[y_{jt}\middle|t \in t^1\right] \qquad (3)$$

where w_j^* is the optimal weight for country *j*. The effect of interest can be estimated by simply comparing, in the post-treatment period, the average outcome of the treated and the synthetic treated groups, with the latter approximating the counterfactual average value of the outcome for the treated in the absence of the treatment.

$$\delta = E\left[y_{jt} \middle| j = 1, t \in t^1\right] - \sum_{j=2}^{J+1} w_j^* . E\left[y_{jt} \middle| t \in t^1\right] \qquad (4)$$

Linden and Adams (2011) provide an alternative to the synthetic estimator that identifies the effect of interest using interrupted time series data whenever the distribution of the outcome variable is correctly specified. In particular, they consider a simple regression model in which observations in the control group are weighted using the propensity score, defined as the probability of assignment to the treatment group conditioned on covariates, to replicate the distribution of the covariates in the treatment group in the pre-treatment period, i.e. in the absence of the treatment. The balancing property ensures that conditional on the propensity score pre-treatment covariates will be distributed equally in both groups (Rosenbaum and Rubin, 1984).

For comparability purposes, we estimate the effect of interest using both the synthetic estimator in Abadie and Gardeazabal (2003) and the regression-based weighting estimator in Linden and Adams (2011). While the latter estimator is easier to implement, the validity of the synthetic estimator does not rest on distributional assumptions.

We obtain our estimates using STATA software and the synthetic estimates using the code in Abadie et al. (2011). Finally, to properly distinguish between the causes and the consequences of the introduction of the PPS and related initiatives, the set of covariates used to explain y_{jt} is measured in *t-1*.

4. Data

We have constructed a panel with yearly information over the period 1995-2010 on fatalities and its main determinants for Spain and the European Union countries that do not implement the PPS during the sample period. These countries are Belgium, Estonia, Lithuania, Portugal, Slovakia Republic, and Sweden. We do not consider years after 2010 because the effects of the PPS are likely to be contaminated by the implementation of the Road Safety Strategic Program 2011-2020 and a methodological change in Spanish road safety statistics to report road deaths according to the EU common definition.[iv]

The dependent variable, that is, the variable in which we aim to estimate the effect of the implementation of the PPS in Spain, is the number of road fatalities per 100,000 inhabitants. To construct this variable, we combine data from the OECD Transport database on number of people who die immediately after the crash o within the next 30 days and data from Eurostat on the number of people living in each country in a given year.

Figure 1 and Figure 2 present the evolution of the dependent variable for Spain and for the comparison countries considered in the estimation. It turns out that fatality rates have decreased in all the countries, with the dispersion in fatality rates across countries lowering during the sample period.

Portugal and Lithuania stand out for showing the highest fatality rates at the beginning of the sample period, with values above 20 deaths per 100.000 inhabitants until the beginning of the current century. Conversely, Sweden is the country registering the lowest-low values of fatality rates during the whole sample period. Spain shows the same decreasing trend in fatality rates than the remaining countries considered, with average fatality rates in average values during the sample period.

Figure 1
Road fatality rates per 100,000 inhabitants in Spain, Estonia, Lithuania and Portugal

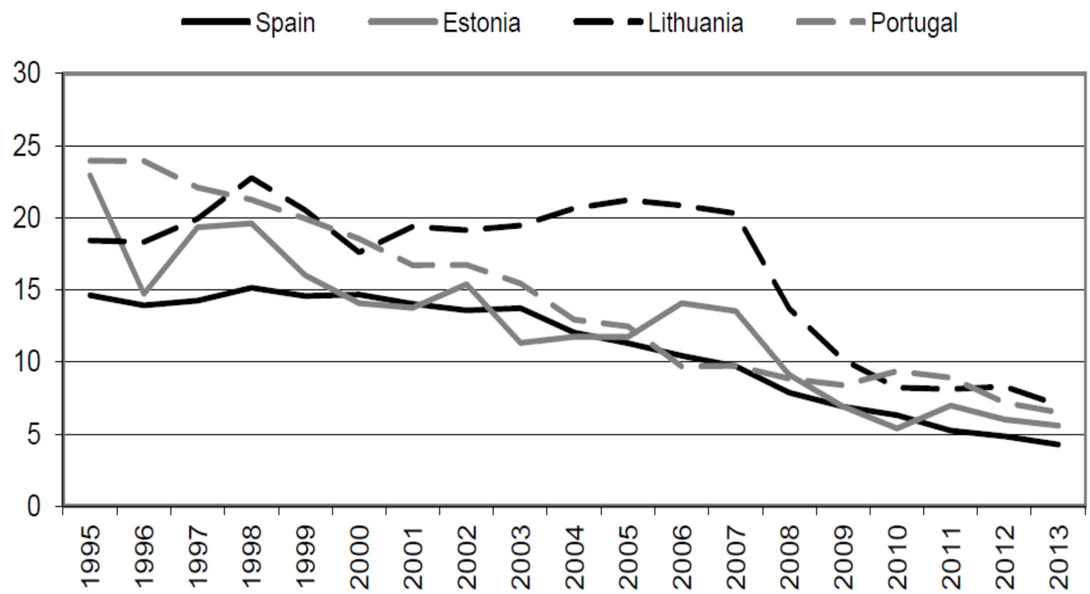
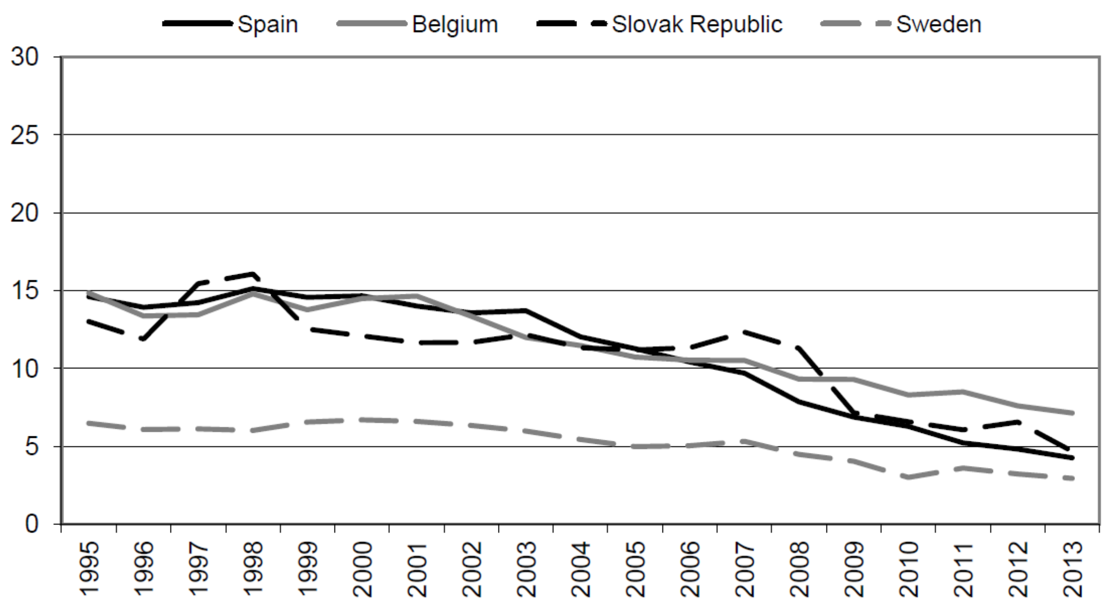


Figure 2

Road fatality rates per 100,000 inhabitants in Spain, Belgium, Slovak Republic and Sweden



Regarding the covariates included in X, we include a measure of a country's level of development such as the log of per capita Gross Domestic Product (GDP) taken from the World Bank National Accounts, per capita measures of consumption of alcohol (World Health Organization), per capita measures of gasoline and diesel (International Road Federation) and the percentage of young people of a given sex in the total population (Eurostat). As shown by Garcia-Ferrer et al. (2007), road accidents are associated to the rate of vehicle utilization which, in turn, depends on the level of economic activity. That is the reason why we control for per capita GDP and for per capita consumption of gasoline and diesel. Furthermore, we also include the percentage of young people in total population since traffic crashes are the main cause of fatalities among young people (WHO, 2015). Finally, we control for per capita consumption of alcohol since almost 40% of both drivers and pedestrians who die in a road accident in Spain have consumed alcohol or other drugs (National Toxicology Institute, 2014).

Table 1 describes the covariates used in the estimation. These covariates have been selected for efficiency reasons because of their statistical significance at conventional significance levels when explaining fatality rates in the countries considered in the estimation. Remarkably, the estimation results in this paper are robust to the inclusion of the complete set of covariates. These estimates are available upon request to the authors.

Table 1
Descriptive statistics of the control variables used in the estimation (1995-2010).

	Belgium	Estonia	Lithuania	Portugal	Slovak Rep	Spain	Sweden
Per capita (pc) GDP	34,203.38 (2,514.78)	8,396.48 (2,389.33)	6,499.35 (1,994.64)	17,567,26 (1,235.75)	10,419.81 (2,207.23)	24,121,84 (2,294.14)	37,573.60 (4,263.54)
Gasoline consumption (pc)	199.83 (51.46)	214,19 (18.76)	128.42 (30.58)	180.82 (23.04)	112.29 (11.78)	188.63 (36.04)	434.82 (36.94)
Diesel consumption (pc)	546.17 (89.71)	226.40 (83.99)	184.81 (74.09)	340.31 (74.15)	163.48 (37.26)	471.78 (113.60)	298.92 (51.14)
Alcohol consumption (pc)	10.68 (0.37)	10.73 (2.53)	10.15 (2.70)	11.98 (0.61)	11.42 (0.98)	10.93 (0.66)	6.82 (0.30)
% Men aged 18-24	0.09 (0.00)	0,11 (0.00)	0.11 (0.00)	0.10 (0.01)	0.12 (0.00)	0.11 (0.01)	0.09 (0.00)
% Women aged 18-24	0.08 (0.00)	0.09 (0.00)	0.09 (0.00)	0.09 (0.01)	0.11 (0.00)	0.10 (0.01)	0.08 (0.00)

Note: The table reports average and standard deviations in brackets. Per capita GDP is measured in constant 2005 US$.

Finally, our estimates of the overall effect of the PPS and the reform of the Penal Code in 2007 on road fatalities in Spain are likely to provide a lower bound to the real effect since we cannot discard that the comparison countries have introduced other policies or measures intended to improve road safety during the sample period.

5. Estimates

Figure 3 presents the evolution over the estimation period of the fatality rate in the observed and synthetic Spain. To further clarify the discussion of the estimates, Figure 4 depicts the evolution over time of the gap between the observed and synthetic fatality rates for Spain. As previously argued, the relevance of this gap in the pre-treatment period provides a measure of the validity of the synthetic Spain as the counterfactual needed to identify the causal effect of interest.

Figure 3

Observed and predicted using the synthetic-control method road fatality rates for Spain.

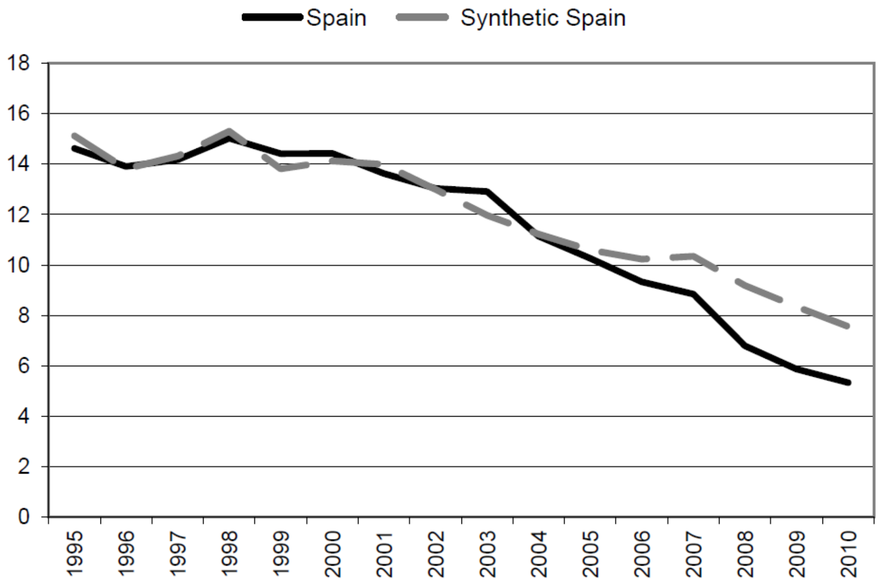
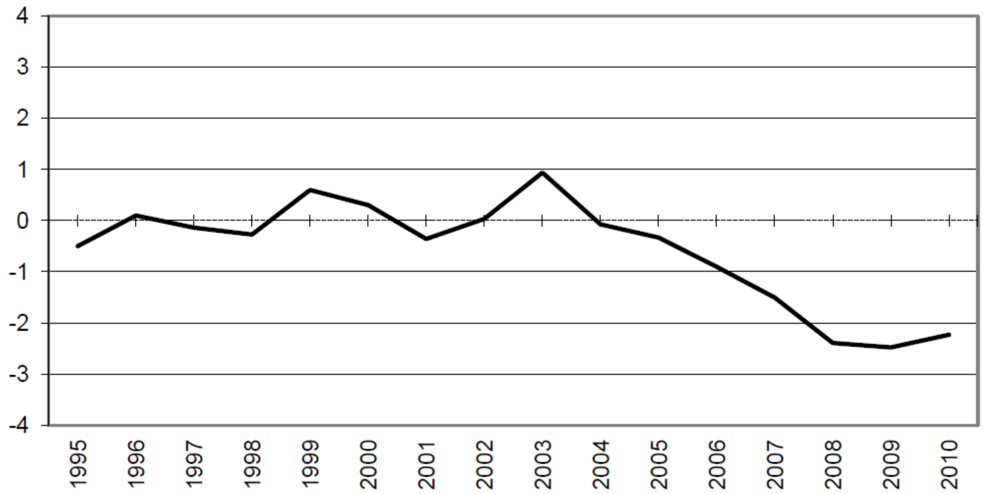


Figure 4

Difference between observed and predicted using the synthetic-control method road fatality rates for Spain.



According to these figures, the synthetic Spain estimated using the methodology in Abadie and Gardeazabal (2003) provides a reasonable approximation to the average fatality rate that would have been observed in Spain after 2006 in the absence of the PPS.

That is the case since the value of the gap in the pre-treatment period is 0.03 on average terms and it does not exceed in absolute value the threshold of 0.5 in seven out of the eleven years in the pre-treatment period and only in one year it exceeded the value of 0.9 percentage points. The synthetic Spain is obtained by combining fatality rates for Belgium (73.1%), Portugal (7.7%), Slovak Republic (17.8%) and Sweden (1.4%).

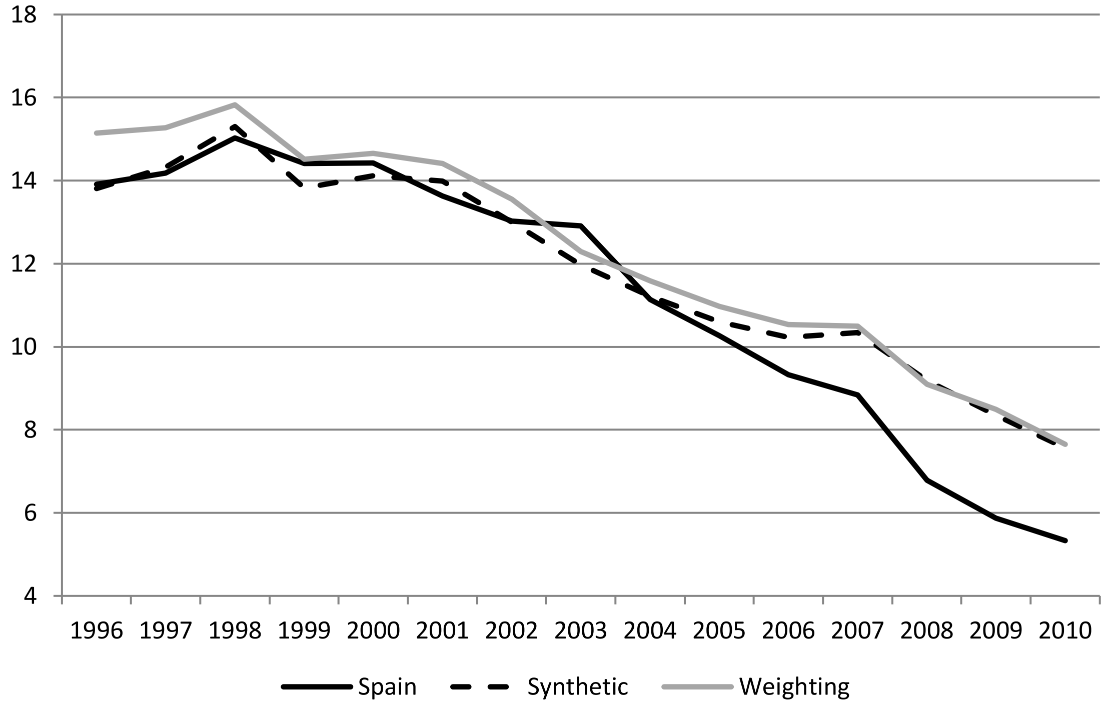
It is worth pointing out that the magnitude of the gap in the pre-treatment period also informs on whether estimates of a given magnitude are more or less common in the absence of the treatment. Following Meyer (1995), this, in turn, allows us to shed light on the statistical significance of the estimated effects in the post-treatment period.[v]

While the gap in fatality rates between the observed and the synthetic Spain remains close to zero until 2006, from 2006 onwards the gap follows a monotonically increasing trend in absolute value. Additionally, the values of the gap from 2006 onwards cannot be explained on the basis of those estimated for the pre-treatment period. Thus, our estimates suggest that the introduction of the PPS in 2006 and the surveillance measures, sanctions and publicity campaigns that went with it and with the reform of the Penal Code that came into force at the end of 2007 succeed at lowering fatality rates in Spain.

In particular, we find that the overall effect of the introduction of the PPS and related measures at the end of the year 2007 amounts to a reduction of fatality rates in Spain of 14.7% with respect to what would have been otherwise observed in the absence of those initiatives. Additionally, in the next two years, that is, in 2008 and 2009, the effect of the PPS and related initiatives amount to a 29.5% reduction in the fatality rate. The fatality rate in Spain in the absence of the PPS and related initiatives in 2010 would have been 2.2 points higher than the observed rate of 5.3 deaths per 100,000 inhabitants.

Next, we compare these estimates to those obtained using the estimator proposed in Linden and Adams (2011). Figure 5 compares the evolution of the observed fatality rates to those in the counterfactual Spain estimated using the synthetic-control and the regression-based weighting methods, respectively. Equivalently, Figure 6 compares the difference between the observed fatality rate and those estimated using the two estimation methods, respectively.

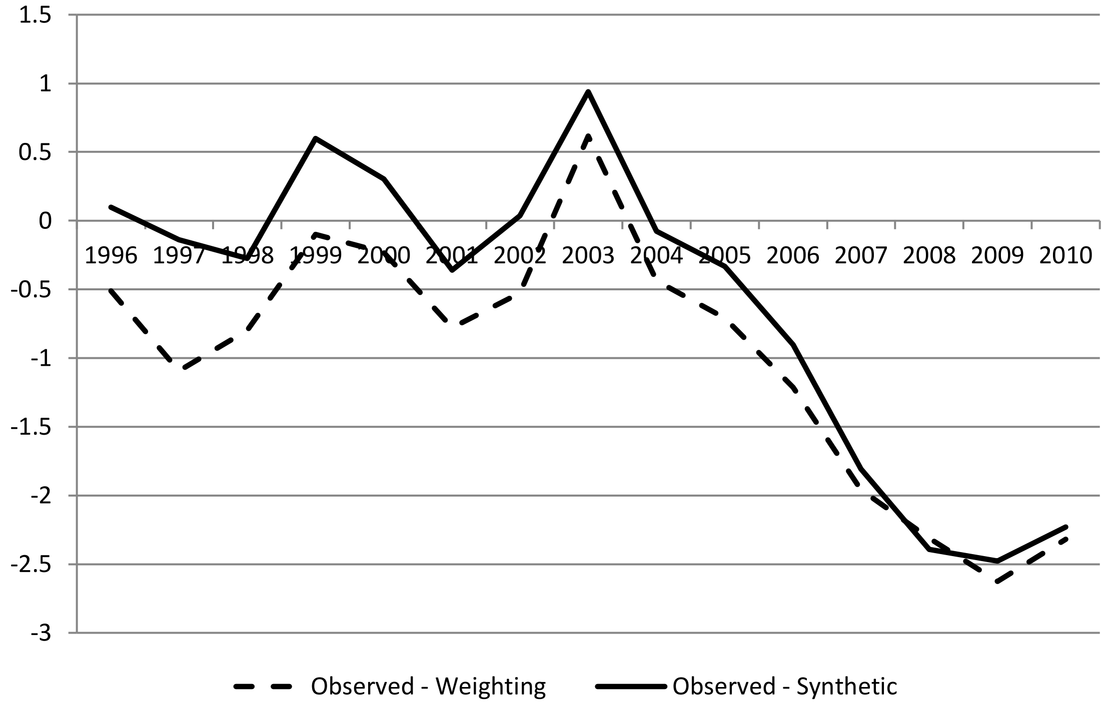
Figure 5
Observed and predicted using the synthetic-control and the regression-based weighting methods road fatality rates for Spain.



It turns out that the two estimation methods yield similar results about the effect of the PPS when looking at the post-PPS period. However, the synthetic estimator better approximates the observed fatality rate in the pre-PPS period and, thus, it provides a more reasonable approximation to the fatality rate that would have been observed in Spain from 2006 onwards in the absence of the PPS and related initiatives.

Figure 6

Difference between the observed and predicted using the synthetic-control and the regression-based weighting methods road fatality rates for Spain.



Accordingly, the summary statistics in Table 2 attest that the synthetic Spain replicates the value of the fatality rate in Spain in the pre-treatment period better than the regression-based weighting estimator.

Table 2

Predictive accuracy of road fatality rates in Spain using the synthetic-control method and he regression-based weighting

	Observed	Synthetic	Weighting
Per capita (pc) GDP	10.04	10.12	10.45
Gasoline consumption (pc)	210.16	209.32	289.16
Diesel consumption (pc)	418.95	418.9	301.15
Alcohol consumption (pc)	10.89	10.94	11.43
% Men aged 18-24	0.11	0.10	0.10
% Women aged 18-24	0.10	0.09	0.08

6. Discussion

The results of this research suggest that the introduction of the PPS in 2006, jointly with surveillance measures, sanctions, publicity campaigns and the reform of the Penal Code in 2007, succeed at lowering road fatality rates in Spain. The estimated effect on road fatalities increased in absolute value over time from 2007 until the end of the estimation sample in 2010. While fatality rates lowered in Spain by approximately 17% in 2007 due to the introduction of the policies under evaluation, the magnitude of the estimated effect monotonically increased over the following years until reaching a reduction of 41-42% on fatality rates in 2009 and 2010.

These results also suggest that existing evidence for Spain underestimates the effect of the PPS and other initiatives implemented in 2006 and 2007 on fatality rates both in magnitude and length. As previously argued, preceding evaluations of the effects of the PPS in Spain use before-after comparisons that cannot control for the effect of other non-targeted policies or unexpected events or changes in business cycles conditions that happen between the pre- and the post-PPS periods. Conversely, our identification strategy controls for: *i*) differences in the distribution of control variables that affect fatality rates both over time and among countries; *ii*) changing business cycles conditions and *iii*) the effect of other non-targeted policies and events; *iv*) arbitrariness in the selection of the control group. Thus, our estimates are likely to provide a more reliable approximation to the causal effect of the introduction of the PPS and related initiatives on fatality rates in Spain.

As the meta-analysis in Castillo-Manzano and Castro-Nuño (2012) suggests, the effect of the introduction of the PPS on road fatalities quickly declines over time unless there are other complementary enforcement measures like the ones implemented in Spain.

Finally, our estimates are likely to provide a lower bound on the causal effect of the PPS and related initiatives on road fatalities since some of the comparison countries also introduced policies intended to improve road safety during the sample period.

7. Limitations

The analysis presented in this paper is - to date - the most comprehensive approach to estimating the causal effect of the introduction of the PPS, and related initiatives, on fatality rates in Spain. It includes several aspects previous studies have overlooked like, for example, the need of an adequate comparison group to control for unexpected changes in business cycles conditions and the effect of other non-targeted policies. While our approach provides a more reliable approximation to the causal effect of interest, it also comes with some limitations.

First, the effect of the introduction of the PPS and associated publicity campaigns on road fatalities cannot be disentangled from that of the reform of the Penal Code in 2007. That is the case since the latter initiative is introduced only one year after the introduction of the PPS in Spain and we analyze yearly fatality rates. In other words, the post-PPS and pre-reform of the Penal Code period includes only one observation and, thus, we cannot precisely estimate what the yearly fatality rate in Spain would have been from 2007 onwards with the PPS but in the absence of the reform of the Penal Code. The lack of a sufficiently large pre-treatment period makes it impossible to ascertain whether estimates of a given magnitude are more or less common in the absence of the treatment and, thus, we cannot accurately estimate the effect of the treatment.

Second, while our estimates are robust to the inclusion of a larger set of control variables, we cannot reject the hypothesis that there are other relevant to fatality rates control variables that have not been recorded in our analysis and that would have changed our estimates.

Finally, while our research provides an estimate of the overall effect of the introduction of the PPS and related initiatives, the aggregate data that we use does not allow us to analyze the heterogeneity of that effect with respect to drivers' personal characteristics or to the type of vehicle. That information would allow to define a more efficient road safety policy by identifying the collectives of drivers that modified their behavior in response to the introduction of the PPS and related initiatives.

8. Conclusions

Spain was one of the last European Union countries in implementing the Penalty Point System (PPS). The Spanish PPS came into force on July 2006. Additionally, in 2007 the Spanish Penal Code was reformed to toughen the consequences of traffic offenses. This paper has analyzed the effect of the introduction of the PPS, and the surveillance measures, sanctions and publicity campaigns that went with it and whose effect cannot be separately identified, on fatality rates in Spain.

Our estimates provide a more reliable approximation to the causal effect of interest since they control for several aspects previous studies have overlooked like, for example, the need of an adequate comparison group to control for unexpected changes in business cycles conditions and the effect of other non-targeted policies.

We find that the introduction of the PPS and related initiatives lead to a strong initial reduction of almost 15% in traffic fatality rates in Spain in 2007. Additionally, in the next two years, that is, in 2008 and 2009, the effect of the PPS and related initiatives amounted to a 30% reduction in the fatality rate. The fatality rate that would have been observed in Spain in the absence of the PPS and related initiatives in 2010 would have been 2.2 points higher than the observed rate of 5.3 deaths per 100,000 inhabitants.

These results suggest that existing evidence for Spain underestimates the efficacy of the PPS and related initiatives implemented in 2006 and 2007 on fatality rates both in magnitude and length.

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Declarations of interest

Declarations of interest: None

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Notes

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i This also suggests that traffic system has to be made more suitable for the limitations of humans by building safer roads or constructing safer cars, for example.

ii See Hirano et al. (2003) for a detailed discussion on the properties of the different econometric estimators available for identifying causal effects.

iii See Bouttell et al (2018) for a detailed discussion on advantages, assumptions and limitations of synthetic control method when evaluating public health interventions.

iv The figures of fatalities related-accident were computed within 24 hours after the occurrence until 1993. From 1993 to 2010 that number was augmented with a correction factor that was a function of the number of seriously injured people that could die within the 30 days following the accident. From 2011 and onwards the reported statistic is the real number of people died within the 30 days following the accident. Our sample period is chosen such that the estimates are not affected by these methodological changes

v Meyer (1995) suggested that the implementation of an estimator over the pre-treatment period allows the researcher to examine if movements of a given magnitude are more or less common than standard errors suggest. A similar reasoning can be used in the current context to infer about the statistical significance of the estimated effects.