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Jose Maria Cabrera, Felipe Carozzi, Alejandro Cid
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Centre for Economic Policy Research
33 Great Sutton Street, London EC1V 0DX, UK
Tel: +44 (0)20 7183 8801
www.cepr.org

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JEL Classification: I12, I18, R41, H89

Keywords: Helmet Use, law enforcement, Traffic Accidents

Jose Maria Cabrera - jmcabrera@um.edu.uy
Universidad de Montevideo

Felipe Carozzi - f.carozzi@lse.ac.uk
London School of Economics and CEPR

Alejandro Cid - acid@um.edu.uy
Universidad de Montevideo

Magdalena Blanco - m.blanco@um.edu.uy
Universidad de Montevideo

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Mandatory Helmet Use and the Severity of Motorcycle Accidents: No Brainer? †

Magdalena Blanco[1], José María Cabrera[2], Felipe Carozzi[3], Alejandro Cid[4]

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[1] UM Center for Applied Research in Economics at Universidad de Montevideo – m.blanco@um.edu.uy

[2] UM Center for Applied Research in Economics at Universidad de Montevideo – jmcabrera@um.edu.uy

[3] Corresponding author. Department of Geography and Environment. London School of Economics.
Houghton Street WC2A 2AE, London, United Kingdom – f.carozzi@lse.ac.uk

[4] UM Center for Applied Research in Economics at Universidad de Montevideo – acid@um.edu.uy

1. Introduction

Road traffic accidents are the leading cause of death for children and young adults worldwide. According to the World Health Organization, 1.35 million people die yearly in road accidents. The associated costs are estimated to account for roughly 3% of GDP in most economies. These costs are particularly high in the case of low- and middle-income countries, which register 93% of deaths (WHO, 2018). In an effort to curb the substantial human and material costs imposed by road traffic accidents, countries have implemented a panoply of different regulations, from mandatory seat belt and helmet use laws to vehicle speed limits among many others. For several decades, economists have studied the effectiveness of seat belt use laws in particular because these can in theory modify actual and perceived risks of driver behavior. In turn, this could hypothetically induce unexpected changes in accidents that may render regulation ineffective or counter-productive. This is known as the Peltzman hypothesis after this author showed evidence of increases in pedestrian accidents as a result of seat belt regulation in the United States (Peltzman, 1975). While evidence in support of the Peltzman hypothesis has been elusive in recent studies of the consequences of seat belt use, examples of inadvertent consequences of protection gear have been documented in other activities.[5]

In this paper, we study the impact of a change in the enforcement of mandatory helmet use regulation in Uruguay on the severity and volume of road accidents involving motorcyclists and other road users. Mandatory helmet use laws for motorcyclists are common but not universal and enforcement varies substantially between nations, with widespread enforcement issues in middle- and low-income countries. The potential effects of helmet use on the perceived consequences of speedy driving and other forms of risk taking are similar to those hypothesized in the case of seat belts. Yet there is limited evidence in the economics literature on the direct and indirect impact of helmet use enforcement on injury rates for motorcyclists. By using detailed administrative data on all reported road accidents in Uruguay, we can estimate these effects and study the impact of mandatory helmet use on the volume and severity of accidents taking place, both for motorcycles and other vehicles.

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[5] For example, Chong and Restrepo (2017) study the effect of protective gear in Ice Hockey on player behavior. Pope and Tollison (2010) find increased on-track accidents in NASCAR as a result of the introduction of new safety regulations.

Our empirical strategy is based on quasi-experimental variation in enforcement induced by changes in national laws in Uruguay. Mandatory helmet use was introduced in 2007 as part of the National Traffic Law, yet two departments – Uruguay is divided into 19 territorial jurisdictions called departments – refused to enforce this regulation. This situation changed when the Misdemeanors Act was passed by Parliament in 2013. As a consequence of this act, the department of Soriano started to enforce helmet use for motorcycle drivers and passengers. This induced an arguably exogenous change in enforcement that can be exploited for the purpose of our analysis.

We document the effect of this change in enforcement on the volume, type and severity of road accidents in the two years after 2013. Our findings indicate a substantial reduction in severity of motorcycle accidents, with a 5 to 7 percentage point reduction in serious and fatal accidents (from a baseline probability of 11.3%) and a similar increase in the fraction of accidents resulting in minor injuries. Contrary to what the Peltzman hypothesis suggests, we find no effect on either accident volumes or the type of accidents taking place as a result of the change in enforcement. Using our coefficients in combination with estimates of hospitalization costs in the country and the value of statistical life, we can obtain a rough estimate of the health benefits resulting from enforcement of the helmet use law. By comparing these with motorcycle registration numbers, we also compute the nuisance cost of helmet use that would be required to offset the health benefits of this policy.

A small set of studies in economics have looked specifically at the effects of helmet use in traffic accidents.[6] Perhaps the closest to our work is Dee (2009), which provides estimates of the effect of the introduction/removal of helmet use laws in US states on fatalities, using a panel specification.[7] Total fatality effects are meant to incorporate both the direct effect of helmet use plus potential compensating behavioral adjustments by drivers. Dickert-Conlin et al. (2011) find evidence of increased availability of organ donations by deceased motorcyclists in US states that repeal mandatory use laws. Carpenter and Stehr (2011) find evidence of reduced fatalities as a result of the introduction of mandatory bicycle helmet use laws for the young. They also report a substantial reduction in cycling.

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[6] Studies in the fields of accident prevention and medicine have also looked at this question using a variety of empirical methods. Some recent examples include Houston and Richardson (2008), Peng et al. (2017), Olsen et al (2016) and Lee (2018).

[7] Dee (2009) also provides complementary results using a within-vehicle specification similar in spirit to the analysis in Evans (1986).

Our paper contributes to this literature by testing for the effect of helmet use on accidents and injuries in a context in which the change in enforcement is induced by a national reform, and arguably affects helmet use only. Perhaps more importantly, we provide the first causal estimates of the effect of helmet use on injury severity outside of the USA. This is particularly important given that enforcement issues are especially acute in low and middle-income countries.[8]

Our paper also relates to previous studies in economics estimating the impacts of seat belt use on health outcomes for drivers or non-drivers. Motivated by the work in Peltzman (1975), Loeb (1995) uses time-series data for Texas to study the effect of seat belt use laws on the fraction of accidents resulting in serious injuries. Cohen and Einav (2003) and Carpenter and Stehr (2008) improve the empirical strategy by exploiting a US state panel. They respectively study the impact of seat belt laws on fatalities and injuries for vehicle occupants and non-occupants. While we also exploit longitudinal variation by jurisdictions to estimate our effects of interest there are important differences relative to these studies. Of course, we look at mandatory helmet use instead of seat belt use. In addition, the use of administrative data on individual accidents allows us to investigate effects on the types of accidents taking place. More importantly, we have information on injury type, which allows us to document impacts on serious and minor injuries and changes in composition between them.

Finally, our paper relates more broadly to the literature on policy solutions to the problem of road traffic accidents. Van Benthem (2015) uses historical changes in speed limits in the United States to obtain optimal limits, incorporating the impact of accidents as well as other factors (e.g. air pollution). Hansen (2015) uses regression-discontinuity methods to study the impact of punishment for driving under the influence on recidivism. In an exception to the largely US-centered literature, Aney and Ho (2019) study the impact of the Chinese Road Traffic Safety Law on the volume of accidents, fatalities and the severity of accidents. Our paper adds credible estimates of this change in policy to the economics literature on policy solutions to traffic problems in the developing world.

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[8] According to the 9.1 Penn World tables, per capita real PPP-adjusted GDP in Uruguay was 38% of that in the United States in 2013.

2. Background and Data

2.a. Road Accidents, Regulation and Enforcement

Road traffic accidents are the leading cause of death for children and young adults aged 5–29 years worldwide. The burden of road traffic injuries and deaths is disproportionately borne by vulnerable road users and those living in low- and middle-income countries, where the growing number of deaths is fueled by increases in transport motorization. Between 2013 and 2016, all low-income countries experienced an increase in the number of road traffic deaths (WHO, 2018). Despite the heavy costs imposed by road accidents, many countries still lack funded strategies, lead agencies and adequate enforcement of existing traffic regulation.

Globally, those using motorized two-and three-wheelers – mainly motorcycle riders – represent 28% of all traffic-related deaths. The heavy burden of deaths born by these road users is, at least in part, a result of them being less physically protected than car occupants. This additional risk for motorcycle users also affects the distribution of traffic-related deaths worldwide, as motorcycle use is generally more prevalent in developing countries.[9] Figure 1 shows a negative relationship between fatalities in motorcycle accidents and GDP per capita.[10] Our empirical analysis below focuses on Uruguay, which shows one of the worst rates in motorcycle accidents relative to its income level.

Tackling road safety problems in a context of increasing motorization is an important challenge for many developing economies. Even if adequate regulations are in place, these may be ineffective without the resources to ensure they can be successfully enforced. For example, in most countries helmet use is formally mandatory for motorbike drivers and passengers. Yet these regulations often co-exist with low use rates: Argentina, Bolivia, Iran, Peru and Uganda, all have mandatory helmet use laws and yet in these countries over 30% of drivers, and roughly 60% of passengers, do not wear helmets (WHO, 2018). The situation if often worse: in India and China, helmets are used by 30% and 20% of drivers, respectively. Both countries have had mandatory helmet laws for over a decade.

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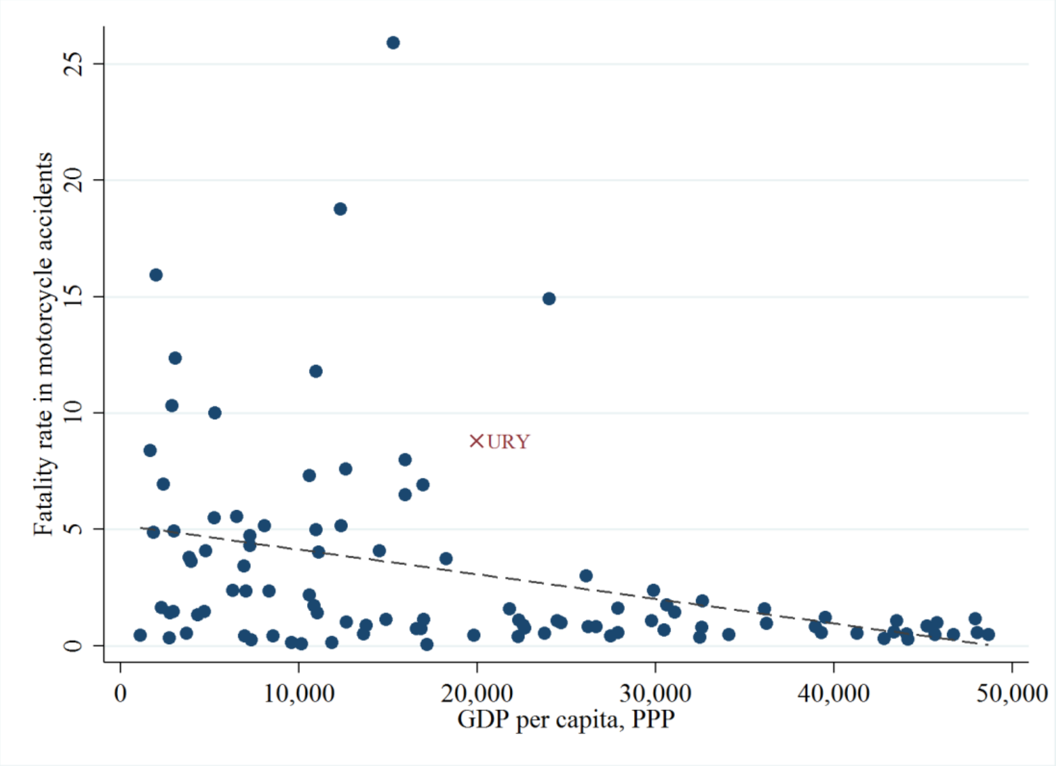
[9] According to the 2014 Spring Pew Global Attitudes Survey, motorcycle ownership rates are regularly above 50% in developing East Asian economies, but less than 30% in developed countries.

[10] Detailed information on p.c. GDP and motorbike fatality rates by country can be found in Appendix Table A1.

That is not to say that mandatory helmet laws are universal. In the United States, many states only require helmet use for young riders (e.g. under the age of 20). The states of Illinois, Iowa and New Hampshire do not require helmet use at all. In many of the countries that do have mandatory helmet laws, these laws do not specify standards for these helmets.

Figure 1 – Motorcycle Fatalities and Economic Development



Note: Dots correspond to countries with GDP per capita < 50.000. Vertical axis represents deaths of motorcyclists per 100.000 people. GDP per capita measured in dollars of 2013, at purchasing power parity. Dashed line corresponds to a linear regression estimated over the scatter plot. Sources: World Health Organization, World Bank.

2.b. Helmet Use and Motorbike Accidents

When a motorcycle is involved in a collision, the rider is often thrown from the vehicle. In this event, a motorcyclist that is wearing a helmet has a lower risk of suffering traumatic brain injuries. There are typically three reasons for this. In the first place, the helmet cushions the impact and therefore reduces the deceleration of the skull. In turn, this limits the speed of the impact between the brain and the skull. Secondly, a helmet spreads the force of the impact over a greater surface area so that it is not concentrated on a small area of the skull. Finally, helmets act as a mechanical barrier between the head and the object.

These three functions are met by combining the properties of four basic components of the helmet: *The shell* is the strong outer surface that distributes the impact over a large surface area. *The impact-absorbing liner* is the soft foam-and-cloth layer that sits next to the head. It helps keep the head comfortable and the helmet fitting snugly. Finally, the *retention system* or *chin strap* is the mechanism that keeps the helmet on the head in a crash.

In the event of an accident, bikers who do not wear helmets generate additional hospitalization costs by requiring greater number of medical and surgical interventions and longer recovery times. The disability that often results from these head injuries leads to additional individual and social costs (WHO, 2006).

2.c. Natural Experiment

On November 2007, the Uruguayan Parliament approved a new National Traffic Law – Law number 18.191 – which required mandatory helmet use for motorcyclists in all 19 departments of the country.[11] However, the departments of Soriano and Cerro Largo decided not to monitor the use of helmets – effectively ignoring this aspect of the law. The local governments of both departments were able to sustain differential enforcement because the Uruguayan Constitution devolves transit control to the departmental jurisdiction.[12] The refusal to enforce mandatory helmet use was partly based on electoral considerations, featuring prominently among the electoral promises in both departments. Both mayors (*intendentes*) continued to promote the enforcement of speed limits and other elements of the national traffic laws.

Perhaps as a result of the lack of enforcement, in the beginning of 2013 – where our sample period starts – both departments had substantially lower reported rates of helmet use than other parts of the country. The percentage of motorcycle accidents in which the biker was wearing a helmet was 7.9% and 21.2% for Soriano and Cerro Largo, respectively. The average for other departments

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[11] A map of Uruguayan departments including the percentage of helmet use can be found in Appendix Figure A1.

[12] Traffic inspectors are under the authority of local departmental governments and control traffic in urbanized areas. The national traffic police (*policía nacional de tránsito*) operates under the authority of the national government and focuses its attention on controlling traffic along national roads.

stood at roughly 75%. Moreover, helmet use was particularly low in Mercedes (the capital city of Soriano) and Melo (the capital city of Cerro Largo) – respectively, 3.1% and 5.7%.[13]

In August 2013, Parliament approved Law number 19.120 – the Misdemeanors Act – which includes an article establishing a specific punishment for motorcyclists not using a helmet, consisting of community work. In the months after the Misdemeanors Act was approved, the Mayor of Soriano informed his citizens the department would start enforcing mandatory helmet use. "The Misdemeanors Act forced my hand – he stated in a press interview – The local police chief asked me what to do because if they saw someone not wearing a helmet they would have to proceed." On November 1, 2013, the municipality of Soriano started monitoring motorcyclists. The department of Cerro Largo remained steadfast in its position, with the local government insisting on its jurisdictional priority. Cerro Largo does not, to this day, require helmet use for motorcyclists.

Two key assumptions are required to interpret the change in enforcement of the helmet use laws in Soriano as a natural experiment. The first assumption is that this change in policy is not correlated with previous or expected changes in helmet use or the volume and types of accidents in Soriano itself. We think this is a reasonable assumption in our context. The change in policy largely coincided with the approval of Misdemeanors Act by National Parliament; with this approval specifically cited by the mayor as motivating the decision. The Misdemeanors Act was a substantial change to *national* legislation and was not itself a response to the traffic policy decisions of Soriano or Cerro Largo. Importantly, changes in the existing or expected severity of accidents are not mentioned as prompting the shift in policy.

The second assumption is that the change in mandatory helmet use did not come with other differential changes in local traffic policy. During this period, other traffic regulations in Soriano - on speed limits or drink driving - were enforced regularly. This was often explicitly mentioned by the mayor of Soriano before 2013 when defending his decision not to enforce the helmet laws.[14] Using administrative data on fines, we find the average number of fines issued by the Soriano traffic

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[13] These cities have comparable numbers of registered motorbikes and automobiles per capita, and similar helmet usage figures pre-2013 (see Table A2 in the Appendix).

[14] In statements to the news portal 180.com.uy, Soriano mayor declared "We were betting con controlling drink driving and speeding. We were strong with those (regulations) because 85% of accidents were under the effect of alcohol, drugs, or happened when speeding".

department before and after the policy change was stable.[15] This helps us to interpret systematic variation in the volume and type of accidents in Soriano relative to other departments as a plausible outcome of helmet use policy alone.

2.d. Data

We employ data drawn mainly from the UNASEV database.[16] This includes detailed information about the universe of accidents recorded by the police authorities, including the date, time and location of each accident. The database includes information about the people involved in the accident, such as age, gender, role – if the person was a passenger or a driver – consequence of the accident – death, serious injury, minor injury or unharmed – and if the person wore a helmet or seat belt if applicable. Locations in the original dataset are reported with a latitude and longitude of each accident. We use location information to obtain the locality or town of each accident.

While the report is filled by the agents that intervene in the accident, the health consequences of the accident are recorded by medical service personnel. They are responsible for identifying if the person is slightly or seriously injured, with the difference depending on whether the person had one or more of their vital organs compromised. Deaths are registered to have happened as a consequence of an accident if the fatality is either at the time of the accident or at the medical center within 30 days of the accident taking place. During the period under consideration – from 2013 to 2015 – 203,725 people were involved in traffic accidents in Uruguay. Excluding pedestrians, we have 175,759 observations in our database (see Table A3 in the Appendix). Nearly 40 percent of those observations involved motorbikes, and 12 out of 100 people suffering motorbike accidents ended seriously injured or dead, more than doubling the rate observed for other vehicles (see Panel A of Table A3). In Mercedes and Melo, 3,378 persons suffered motorbike accidents in this period (see Panel B of Table A3).

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[15] This result is based on data from SUCIVE *(Sistema Único de Cobro de Ingresos Vehiculares).* The data has every fine for traffic offenses imposed in Soriano for the period January 2013 to December 2015. This encompasses 36,686 fines for motorcycles and 9,315 fines for cars. Figure A2 in the Appendix shows that the activity of traffic inspectors (reflected in the number of fines imposed on drivers) is not systematically different in the years before and after treatment. The difference in the average monthly number of fines to motorcyclists between periods is not statistically different from zero (p=0.66).

[16] National Division of Road Security (*Unidad Nacional de Seguridad Vial*).

Finally, in addition to data on accidents we collect information on daily time of sunset, school holidays and national holidays to account for time factors that could affect traffic volume and accidents.

In Table 1 we show the descriptive statistics for all reported motorbike accidents in this period, splitting the sample by helmet usage. Wearing a helmet is associated with a significant reduction in the probability of being seriously injured in motorcycle accidents. A rider wearing a helmet faces a 3.8 pp lower probability of being seriously injured or dead after a motorbike accident. These estimates do not account for the potential endogeneity of helmet use. A motorcyclist makes several decisions when riding her motorcycle: the speed, respecting traffic signs, whether or not she is going to drive under the effects of alcohol or drugs, and if she will be wearing a helmet. Thus, helmet usage is an (endogenous) choice variable. Riders who decide to use a helmet self-select themselves into the treatment, so there can be observable and unobservable factors that confound the use of a helmet and the severity of an accident. For example, Table 1 shows non-helmet riders are disproportionately young, male, and riding at night. In the next sections of the paper we will try to estimate the *causal* effect of using a helmet on the probability of serious injuries and fatalities.

Table 1 – Descriptive Statistics: Motorbike Accidents by Helmet Use

	No helmet			Helmet			Mean Differences
Variables	Mean	SD	Obs.	Mean	SD	Obs.	
Serious injury or death	0.14	(0.35)	16,863	0.10	(0.30)	52,043	0.038***
Slight injury	0.60	(0.49)	16,863	0.69	(0.46)	52,043	-0.095***
Unharmed	0.26	(0.44)	16,863	0.21	(0.40)	52,043	0.057***
Male	0.75	(0.43)	16,789	0.68	(0.46)	51,922	0.063***
Age	27.22	(13.76)	14,801	32.02	(14.15)	50,242	-4.806***
At night	0.32	(0.47)	16,863	0.27	(0.44)	52,043	0.053***

Notes: Data from UNASEV (*Unidad Nacional de Seguridad Vial*, Uruguay). Period: 2013-2015. * p<.1, ** p<.05, *** p<.01

3. Empirical Analysis

Our empirical analysis has three main goals. The first is to evaluate the consequences of the change in enforcement of the mandatory helmet law, identifying effects on helmet use and the severity of road accidents. The second objective is to estimate the effect of helmet use itself on accident severity, using the policy change as a source of exogenous variation. Finally, we want to document any other noticeable changes in driving behavior resulting from the change in policy. We can tackle these objectives by exploiting the abrupt change in enforcement of helmet use in the department of Soriano. This strategy requires finding a suitable control group with which to compare this department.

For this purpose, we use two different strategies. On the first place, we illustrate the effect of the change in policy by conducting an event-study analysis of the evolution of helmet use and the incidence of serious accidents in Mercedes and Melo. These towns constitute, respectively, the administrative capitals of the departments of Soriano and Cerro Largo, both of which refused to enforce the mandatory helmet law in 2007, with Mercedes enforcing helmet use starting in November 2013. Next, we use data on the universe of accidents in Uruguay in a specification with locality fixed effects where localities in Soriano constitute the treatment region. The resulting difference-in-difference (DiD) coefficients can be interpreted as an average treatment effect of the change in policy under the typical parallel trends assumption.[17]

Alongside the results of these analyses, we also report the impact of the change in enforcement on the number and cause of accidents, as well as evidence on the shift from serious to minor injuries resulting from increased helmet use.

3.a. Event-Studies in Two Cities: Mercedes and Melo

In early 2013, Mercedes and Melo were the only department capitals in the country where municipal traffic inspectors did not enforce the helmet use law. As discussed above, Mercedes started enforcing that law in November 2013. To provide an initial illustration of the effects of the policy change, we report two event-study graphs comparing helmet use and the severity of motorbike accidents for Mercedes and Melo in Figures 2 and 3. Comparing both cities is especially relevant because motorbike accidents are more common in urban environments, and most traffic inspectors operate in and around urban areas.

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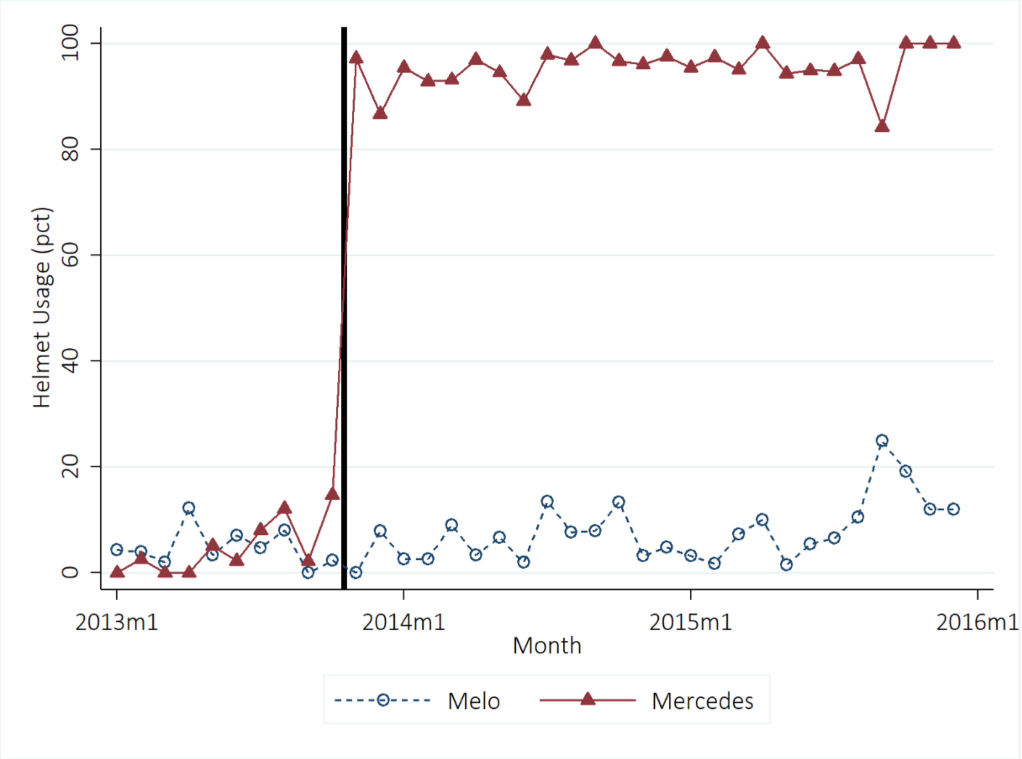
[17] In our robustness checks section, we use department-level data to conduct an analysis of the differential evolution of serious injuries in motorbike accidents in Soriano based on the synthetic control method (Abadie and Gardeazabal, 2003).

In Figure 2, we plot the evolution of the percentage of people involved in motorcycle accident who was reportedly wearing a helmet in both cities. We use this variable as a proxy of helmet use. The solid line corresponds to Mercedes, and the dashed line to the city of Melo. We observe initial levels of helmet use are remarkably low in both cities, oscillating under 10%. In the figure, November 2013 is indicated with a vertical solid line. Precisely in this period, the rate of helmet use jumps to almost 100% in Mercedes, while the figures for Melo remain very low. This difference is sustained throughout the next two years. This indicates that the change in enforcement prompted a persistent increase in helmet use in the city of Mercedes. This jump to a very high level of helmet use is consistent with the experience of other departmental capitals where helmet use is almost universal: The rate of helmet use is over 80% in the 12 out of the 17 departments in the country left after excluding Soriano and Cerro Largo.

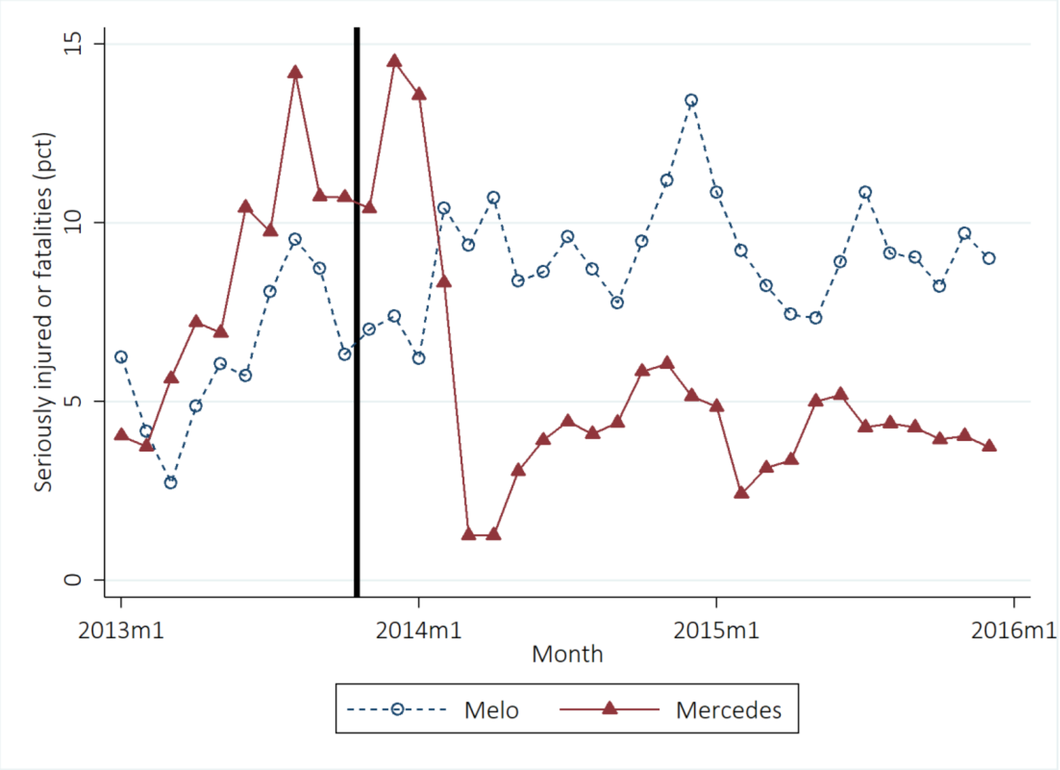
Figure 2: Helmet Use in Mercedes and Melo



Note: Helmet usage measured as the percentage of all motorbike accidents where the driver was wearing a helmet. Vertical line corresponds to November 2013. Source: own calculations based on data from UNASEV (*Unidad Nacional de Seguridad Vial*, Uruguay). Period: 2013-2015.

The evolution of the fraction of motorcyclists involved in accidents that experience serious or fatal injuries for both cities is reported in Figure 3. We report 3-months moving averages to smooth out some of the short run fluctuations, ensuring no smoothing is carried out across the start of the treatment period. We can observe that - before the change in enforcement – the fraction of serious accidents for both cities evolve in parallel with an upward trend, with the level being consistently higher in Mercedes. In the months before November 2013, the fraction of motorbike accidents resulting in serious injury for this city oscillated around 10%. Five months after the policy was introduced, serious injuries only occurred in 2% of motorbike accidents. Between late 2014 and 2015, the figure would recover to a level of around 4%. In this period, the rate of serious injury in Melo was twice as large as the one for Mercedes. The fact that this divergence broadly coincides with the change in policy indicates that the increase in enforcement resulted in reduced injuries for bikers.

Figure 3: Serious Injuries and Fatalities in Mercedes and Melo



Note: Seriously injured or fatalities is defined as a percentage over the number of motorcyclist accidents in each city. Vertical line corresponds to November 2013. The series represent 3 month moving averages, where averages are taken without crossing the vertical line. Source: own calculations based on data from UNASEV (*Unidad Nacional de Seguridad Vial*, Uruguay). Period: 2013-2015.

An interesting feature in Figure 3 is that the decline in serious accidents in Mercedes does not occur immediately but takes roughly 5 months after the change in enforcement. In the first three months after the introduction, there is an increase in the ratio of serious injuries. Given the changes reported in Figure 2, we know this transition is not induced by a slow and progressive change in helmet use. One plausible explanation is that bikers took some months to adapt to the limited visual and auditory acuity resulting from the use of a suitable helmet. In that adaptation period, serious accident rates could even exhibit a short-term increase.

These figures illustrate the association between the change in enforcement, the increase in helmet use and the sizeable reduction in serious and deadly motorcycle accidents. We quantify the size of these effects in the next section.

3.b. Difference-in-Difference Estimates

To estimate the size of the effects of the change in enforcement of the helmet laws in Soriano, we use data for the universe of road accidents in all the country's localities in a difference-in-difference specification. In this way, we can incorporate data from all the towns and villages affected by the policy in the treatment group. The control group is composed of all other towns in the country. The objective of the exercise is to obtain an average treatment effect that can be used to evaluate the benefits associated with the policy, as well as identifying potential unintended consequences.

Before moving to estimation of the treatment effects resulting from the policy, we use a locality-month panel to estimate a) whether the parallel trend assumption is reasonable in this context, and b) whether the volume and type of accidents were affected by the policy. The first exercise is necessary to give causal interpretation to the DiD estimates below. The second is required to narrow down the potential mechanisms relating helmet use to the change in accidents.

Our dataset on road accidents starts in January 2013, so we have 10 months to test for differences in pre-trends between the treatment towns in Soriano and control towns throughout the country. Using these 10 months of data we build a town-month panel and estimate the following specification:

$$Y_{jt} = \alpha_j + \delta_t + \eta \, \mathrm{t} \cdot T_j + \varepsilon_{it} \qquad (1)$$

Variable Y_{jt} represents the outcome variable in town j and month t, α_j represents a town fixed effect and δ_t is a set of month dummies. The coefficient η multiplies an interaction of a time trend and the treatment dummy T_j which takes value 1 for the localities of Soriano. Throughout of the analysis we cluster standard errors at the locality (town) level, and consider alternative methods for inference in section 4.a.

A value of η statistically different from 0 indicates that there were differences in pre-trends of the dependent variable between treatment and control groups before the policy change. We consider 4 different dependent variables in this exercise: the number of serious motorbike accidents, the fraction of motorbike accidents in which the rider reportedly wore a helmet, the ratio of serious motorcycle accidents to total reported accidents, and the ratio of serious motorcycle accidents to all motorcycle accidents. Results are reported in Table 2. We find no evidence of statistically significant differential pre-trends in any of the variables of interest. This indicates that the parallel trends assumption required for causal interpretation of our DiD coefficients below is plausible.

Table 2: Parallel Trends in Town Panel

	(1) Serious Motorbike Accidents	(2) Helmet Share of Motorbike Accidents	(3) Serious Motorbike Accidents over Total Accidents	(4) Serious Motorbike Accidents over Total Motorbike Accidents
Time x Treat.	0.0337	-0.0186	0.00112	0.0173
	(0.0330)	(0.0259)	(0.0213)	(0.0176)
Observations	4,660	1,374	1,574	1,374
Month Effects	Yes	Yes	Yes	Yes
Town FE	Yes	Yes	Yes	Yes

Note: All columns report the coefficient identifying differences in dependent variable trends between treatment and control groups in 2013. Estimates obtained using a town-month panel from January to October 2013. The dependent variable is the number of serious motorbike accidents in column 1, the share of reported motorbike accidents where a helmet was used in column 2, the ratio between serious motorbike accidents and all reported accidents in column 3, and the ratio of serious motorbike accidents over reported motorbike accidents in column 4. In columns 2 and 4, the sample is restricted to town-month pairs with reported motorcycle accidents. In column 3 the sample is restricted to town-month pairs with reported road accidents. Standard errors clustered at the town level. *p<.1; **p<.05; ***p<.01

We can also study whether the change in enforcement lead to a change in the quantity or types of vehicle accidents on the road. We use total number of accidents at the locality-month level and shares of different types of accidents to estimate whether localities in the department of Soriano experienced a differential change in these variables after November 2013.[18] Results for accident volumes for different vehicles are reported in panel A of Table 3 and show no significant effect of increased enforcement on accident volumes for total accidents, motorbike accidents or accidents involving other vehicles. Point estimates are negative and small in absolute value – less than 0.01 of a standard deviation of the dependent variable – in all columns. Results for accident types are reported in panel B of Table 3. In this case we compute the share of all accidents corresponding to

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[18] Specifically, we estimate $Y_{jt} = \alpha_j + \delta_t + \phi \, \mathrm{Post_t} \cdot T_j + \varepsilon_{jt}$, where T_j is a treatment dummy for Soriano, $\mathrm{Post_t}$ is a dummy taking value 1 after November 2013 and coefficient ϕ captures differences in the outcome of interest between treatment and control groups after treatment.

collisions, falling (e.g. from a motorbike) or other causes. We find no statistically significant effect of increased enforcement on the type of accidents taking place.[19]

Table 3: Number and Type of Accidents in all Locations

A) Accidents by Vehicle	(1) Total Accidents	(2) Moto. Accidents	(3) Other Vehicles
Post x Treatm.	-0.539	-0.157	-0.382
	(1.360)	(0.487)	(0.927)
Observations	16,776	16,776	16,776
B) Accidents by Cause	Collision	Falling (e.g. from Motorbike)	Other
Post x Treatm.	-0.0657	0.0610	0.00473
	(0.0749)	(0.0691)	(0.0527)
Observations	6,002	6,002	6,002

Note: Panel A estimates obtained from a month-locality panel including locality fixed effects and year-month effects. In column 1, the dependent variable is the total number of people involved in traffic accidents in a locality-month pair. In column 2, the dependent variable is the total number of people involved in motorcycle accidents and in column 3 the number of people involved in accidents for other vehicles. Panel B estimates obtained from a month-locality panel including localities with at least one accident in a month-locality pair. The dependent variable is the fraction of motorcycle accidents arising from collisions, falling (e.g. from the motorcycle), and other causes. All specifications include locality fixed effects and year-month effects. Standard errors clustered at the locality level in parentheses. *p<.1; **p<.05; ***p<.01

We now turn to the estimation of the effect of the change in enforcement and the use of helmets on serious and fatal injuries in motorcycle accidents using our difference-in-difference specifications. Having documented that accident volumes did not change with helmet use enforcement we can conduct this part of the analysis using individual accidents as our units of observation. We use the following two specifications:

$$Y_{it} = \alpha_j + \delta_t + \beta\ Post_t \cdot T_i \cdot Moto_{it} + \gamma_1 Post_t \cdot Moto_{it} + \gamma_2 Moto_{it} \cdot T_i + \gamma_3 T_i \cdot Post_t + \varepsilon_{it} \quad (2)$$

$$Y_{it} = \alpha_j + \delta_t + \beta\ Post_t \cdot T_i + \varepsilon_{it} \quad (3)$$

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[19] It is worth noting that the share of accidents by type is only defined for locality-month pairs featuring at least one accident. This implies that the sample used to produce the estimates in panel B of Table 3 is heavily selected. Yet the fact that there is no effect of increased enforcement on accident volumes, implies that this sample selection should not have a substantial effect on our estimates.

The outcome variable Y_{it} in both equations can be either a dummy for individuals wearing a helmet, or a dummy taking value 1 for individuals suffering major or fatal injuries as a result of the accident. Parameters α_j and δ_t, correspond to locality and month-year effects, respectively. Variable $Moto_{it}$ corresponds to a dummy taking value 1 if the individual involved in an accident i in month t was riding a motorbike.[20] Finally, T_i takes value 1 if the accident took place in one of the localities in Soriano – our treatment area – and $Post_t$ takes value 1 in all periods after November 1st 2013. In the case of model (2), we use a triple interaction term to identify the effect of the enforcement policy on our outcome by simultaneously comparing the before and after change in this outcome for bikers in Soriano with the corresponding change for other vehicles, and the change for bikers in the rest of the country. Model (3) is estimated on our subsample of bikers and is a standard differences-in differences specifications where the $Post_t$ and T_i variables have been replaced with time dummies and locality fixed effects. In the case of this model, the assumption required to estimate parameter β is the usual assumption of parallel trends in the absence of treatment.

In addition to estimating equations 2 and 3 directly, we can use them as first stages in a two stage least squares specification to estimate the effect of helmet use on serious injuries. The first stage will have the helmet dummy as the outcome, and the second stage outcome will be our serious injury dummy.

Estimates from these specifications are reported in Table 4. Columns 1 and 2 correspond to equation (2) and columns 3 and 4 correspond to equation (3). Columns 1 and 3 omit locality fixed effects, which are included in columns 2 and 4. In panel A, the outcome variable is our helmet use dummy. We observe consistent positive estimates for β of roughly 0.9 across specifications. This is in line with the results illustrated in Figure 2, indicating helmet use in Soriano went from close to zero to almost full compliance in a few months. Panel B provides reduced-form results for the effect of the enforcement of the mandatory helmet law on serious accidents. We find a negative and significant effect of roughly -0.05, which is interpreted as showing that the probability that a motorbike accident results in a serious injury was reduced by 5 percentage points as a result of the policy. This effect is large, as the baseline probability of having a serious or fatal injury for bikers is 11.3%.

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[20] Vehicles included in our accidents database are cars (48.9%), motorbikes/mopeds (39.8%), trucks (3.4%), buses (3.1%), bicycles (2.4%), carts (0.13%), and other vehicles (2.4%).

Panel C of Table 3 shows our IV estimates of the causal effect of helmet use. Note that these coincide with the ratio between the reduced-form coefficients in panel B over the first-stage estimates in panel A. The effects of interest oscillate around 5%, indicating that helmet use reduces the probability that a motorbike accident results in a serious or fatal injury by roughly 40 percent. Note that this estimated effect is slightly *larger* than the difference in probability of serious injury obtained from the mean comparison in Table 1. This suggests that helmet use is positively correlated with determinants of serious accident risk at the local level such as local density and urbanization.

Table 4: Differences-in-Differences Estimates for all Locations

	(1)	(2)	(3)	(4)
A) First-Stage	Helmet D.	Helmet D.	Helmet D.	Helmet D.
Post x Treatm. x Moto	0.885***	0.887***		
	(0.0290)	(0.0270)		
Post x Treatm.			0.904***	0.887***
			(0.0297)	(0.0366)
B) Reduced-Form	Serious D.	Serious D.	Serious D.	Serious D.
Post x Treatm. x Moto	-0.0490***	-0.0479***		
	(0.00932)	(0.0116)		
Post x Treatm.			-0.0450***	-0.0468***
			(0.0123)	(0.0111)
C) TSLS Estimates (IV)	Serious D.	Serious D.	Serious D.	Serious D.
Helmet D.	-0.0553***	-0.0541***	-0.0498***	-0.0528***
	(0.00954)	(0.0122)	(0.0123)	(0.0109)
Observations	174,696	174,696	68,906	68,906
Vehicle	All	All	Motorbike	Motorbike
Town FE	No	Yes	No	Yes

Note: Columns 1 and 2 estimated with the full sample of accidents in the UNASEV dataset (excluding pedestrian accidents). Columns 3 and 4 estimated using the sub-sample of motorcycle accidents. In Panel A, the dependent variable is a dummy taking value 1 if the victim of the accident was reportedly wearing a helmet at the time of the accident. In Panels B and C, the dependent variable is a dummy taking value 1 if the accident victim experienced a serious or fatal injury. Panel C reports instrumented variable estimates of the effect of helmet use on serious accidents as discussed in the text. Columns 2 and 4 include locality fixed effects. Standard errors clustered at the locality level. *p<.1; **p<.05; ***p<.01

The reduction in the prevalence of serious injuries as a result of motorbike accidents can operate through either a change in the type of accidents bikers are involved in, or a change in accident severity conditional on accident type. We have shown in table 3 that the types of accidents motorcyclists are involved in does not change with the enforcement of helmet use. If changes in accident severity are driving the effect on serious injuries, we would expect a positive effect on minor injuries as a result of the change in enforcement. Accidents that would have resulted in a serious injury if a helmet was not used may result in a minor injury instead. To explore this, we reproduce the previous analyses using an indicator taking value 1 if an accident results in minor injuries and 0 if the driver is unharmed as the dependent variable.[21] Results are reported in Table 5. Instrumental variable estimates indicate helmet use leads to a positive and significant effect on minor injuries, pointing to a transfer of serious to minor injuries as a result of the change in enforcement.

Table 5: Minor Injuries and Helmet Use

2SLS Estimates	Minor D.	Minor D.
Helmet D.	0.0340*	0.0572***
	(0.0181)	(0.0134)
Observations	61,148	61,148
Vehicle	Motorbike	Motorbike
Town FE	No	Yes

Note: The dependent variable in all specifications is a dummy taking value 1 if the accident resulted in a minor injury and 0 if the driver was unharmed. Sample of all registered motorcycle accidents. All columns include a full set of time effects. Standard errors clustered at the locality level in parentheses. *p<.1; **p<.05; ***p<.01

From the results reported in tables 4 and 5, we conclude that enforcement of the mandatory helmet use law lead to a reduction in serious or fatal accidents and an increase in accidents resulting in minor injuries. We interpret this as a concomitant change in the relative probabilities of both types of accidents. The fact that there are no discernible changes in the volume and type of accidents suggests there are no other first-order behavioral responses to the law, at least in terms of driver

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[21] Including serious injuries among the zeroes does not change the qualitative results of the exercise.

behavior.[22] Therefore, we find helmet use reduces accident severity and no evidence in support of the types of behavior associated to the Peltzman hypothesis.

4. Robustness Checks

In this section, we evaluate the robustness of the qualitative findings reported above by conducting three complementary exercises: (a) we validate the inference methods above using spatial heteroskedastic and autocorrelation consistent (HAC) standard errors for our reduced-form estimates of the change in enforcement, (a) we report results of the effect of the policy on accidents using a synthetic control for Soriano, and (b) we provide estimates of the effects illustrated in Figures 2 and 3 considering only the capitals of Soriano and Cerro Largo.

4.a. Spatial HAC Standard Errors

Throughout most of the analysis above, our inference is carried out using standard errors clustered at the level of individual localities. This is motivated by the fact that it is likely that there are locality level shocks to our dependent variables – accident volumes, helmet, use, accident outcomes. Yet the choice to cluster at the level of localities has two issues. In the first place, our treatment varies at the department and not the locality level. Since Bertrand et al. (2004), much of the difference-in-difference literature obtains standard errors clustered at the level of treatment, but this is not feasible in our case because there are only 19 departments in our sample.[23] In the second place, it is likely that our outcomes feature non-negligible spatial autocorrelation and, therefore, residuals in neighboring clusters will typically be correlated, violating the key assumption invoked to justify clustering at that level.

In order to deal with potential concerns with inference in our main tables and address these issues, we report standard errors obtained using the spatial heteroskedasticity and autocorrelation (HAC) robust standard errors proposed in Conley (1999) which are frequently used in much of the empirical literature in spatial economics. These standard errors are obtained by specifying a (typically uniform) spatial kernel and using these kernel weights to compute a variance-covariance matrix incorporating

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[22] Using a sub-sample of the UNASEV dataset, we also explore the effect of the change in enforcement on the number of pedestrians involved in traffic accidents. Difference-in-differences estimates are negative, small and statistically insignificant (results available upon request).

[23] A growing literature proposes methods to conduct inference in the DiD setting when the number of clusters is small. Yet these methods generally require having a large number of treated clusters which is not the case in our paper (see MacKinnon and Webb 2020).

spatial dependence, analogous to an adjustment for heteroskedasctity and autocorrelation. Results for reduced-form DiD estimates on helmet use, the probability of an accident resulting in serious injury and the probability of an accident resulting in a minor injury are reported in Table 6. We use a spatial kernel of 100km in radius, so that the area of the uniform kernel is almost twice the size of the largest department in the country.[24] The main conclusions from the analysis above are maintained despite the sensible increase in standard errors.

Table 6: Spatial HAC Standard Errors

	(1) Helmet D.	(2) Serious D.	(3) Minor D.
Post x Treatm.	0.887***	-0.0468***	0.0506*
	(0.0246)	(0.0155)	(0.0262)
Observations	68906	68906	61148

Standard errors adjusted as in Conley (1999) in parentheses. All specifications include a full set of time and locality fixed effects. We use a uniform spatial kernel with a radius of 100km and a serial correlation kernel cut-off of 3 months. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

4.b. Synthetic Control

The difference-in-difference estimates reported in the previous sections result from comparing changes in an outcome (e.g. serious accident rate) between locations in Soriano and the rest of the country. These control groups are obvious choices, but they are also arbitrary. We can use the data-driven synthetic control method – as described in Abadie et al. 2010 – to select a suitable control group and use that to estimate the difference in the rate of serious injuries induced by the policy.

For our synthetic control analysis, we use aggregated data at the department level. The outcome of interest is the number of victims of serious motorbike accidents per capita. The treatment group is the department of Soriano. Predictors for serious injury rates in the loss function include the number of motorbikes per capita, the share of rural population, the natural logarithm of population, average household income and the number of victims of serious motorbike accidents per capita in the first quarter of 2013. We use the algorithm described in Abadie and Gardeazabal (2003) to select the cross-sectional weights. The resulting weights take non-zero values for the departments of *Artigas* (0.448) and *Río Negro* (0.552).

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[24] The adjustment is carried out using the reg2hdfe spatial Stata command by Thimo Fetzer (Fetzer 2014), which is itself based on the previous implementation by Solomon Hsiang (Hsiang 2010). We thank these authors for making these codes available.

We construct the accumulated difference in serious accidents between the department of Soriano and our synthetic Soriano control. This is represented as the black solid line in Figure 4. We observe that in the months before November 2013, the line is flat. Note that only the first quarter is used to select the synthetic control, so the fact that there is no observable trend in the two subsequent pre-policy periods indicates no substantial change between the treatment and (synthetic) control departments before the enforcement of the mandatory helmet law in Soriano. Starting in the last quarter of 2013, we observe a progressive change in the accumulated number of serious accidents per capita. The line continues to diverge downward over time, and it reaches an estimated cumulative difference of – 183.1 serious accidents in the fourth quarter of 2015. While this method does not yield suitable standard errors for a conventional hypothesis test, we follow the synthetic control literature and use a permutation method to gain insights into whether this diverging trend could occur by coincidence. For this purpose, we construct a synthetic control for each of the other departments in our sample and calculate the accumulated difference in serious motorbike accidents per capita in each case. These are plotted in Figure 4 as solid grey lines.[25] We can observe that, while some of these lines diverge significantly from a flat path, none of them veers as far from this path as the solid black line for Soriano. This indicates that Soriano is an outlier in the trend of accumulated serious motorbike accidents per capita relative to all other departments. We interpret this as resulting from the enforcement of the mandatory helmet law in Soriano from November 2013.

We can also use the synthetic control method to determine whether the change in enforcement resulted in a change in accident volumes after 2013, echoing the analysis of accident volumes in section 3.b. For this purpose, we modify the analysis above and build a synthetic department in order to match the number of motorbike accidents per capita before the policy was put in place in Soriano. Results from this exercise are reported in Figure A3 in the Appendix. The solid line represents the evolution of the accumulated difference in the number of accidents per capita between Soriano and the synthetic control. The grey lines represent the same figures for other departments. We observe that the accumulated difference for Soriano is fairly flat and does not stand out relative to those from other departments. This confirms the notion – already illustrated in Table 3 – that the change in enforcement had no discernible effect on the number of motorbike accidents.

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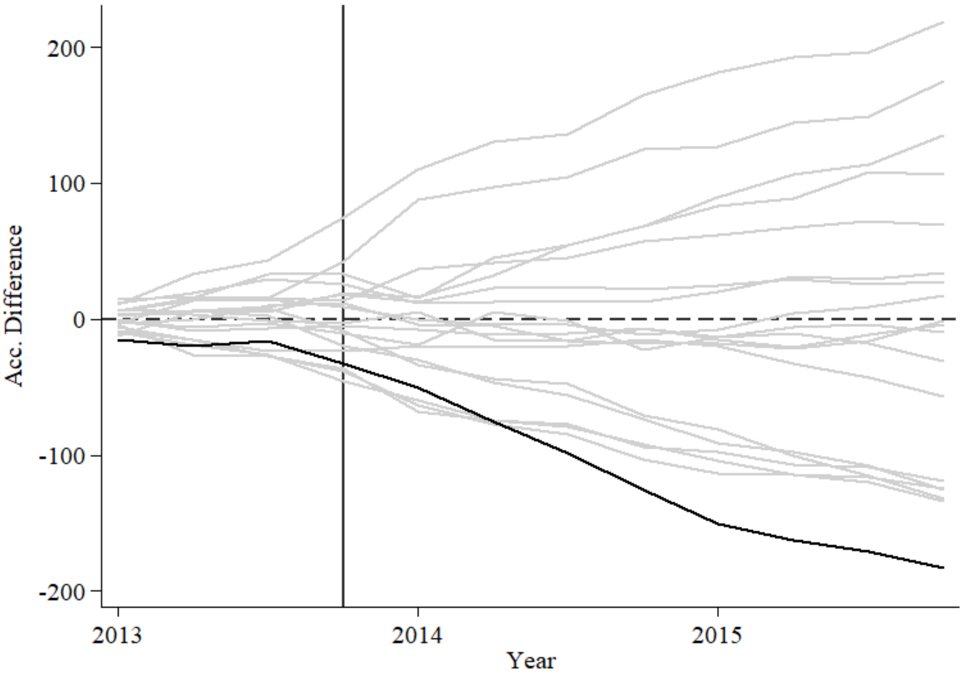
[25] Note that the department of Soriano is not included as a potential control unit in this exercise.

Figure 4 – Synthetic Control: Accumulated Serious Accidents p.c.



Note: Solid line represents accumulated difference in the per capita number of motorbike accidents resulting in serious injuries between the department of Soriano and a synthetic Soriano control constructed using the method detailed in the text. Grey lines represent the accumulated difference between observed numbers and synthetic controls for other departments.

4.c Mercedes and Melo

We can use data for accidents taking place in the department capitals of Soriano and Cerro Largo – Mercedes and Melo – to provide quantitative estimates corresponding to the effects illustrated in Figures 2 and 3. For this purpose, we estimate:

$$Y_{it} = \alpha + \beta(Mercedes_i \cdot Post_t) + \gamma_1 Mercedes_i + \gamma_2 Post_t + \delta_t + \theta' X_{it} + \epsilon_{it} \qquad (4)$$

The outcome variable Y_{it} be either a dummy taking value 1 if the individual i involved in an accident in month t was wearing a helmet, or a dummy taking value 1 if the individual suffered major or fatal injuries as a result of the accident. Variable $Mercedes_i$ takes value 1 if an accident took place in the city of Mercedes (as opposed to Melo), and $Post_t$ takes value 1 after November 2013. Variables δ_t and X_{it} corresponds to date or driver level controls, respectively. In δ_t we include dummies for public holidays, day of the week, hour of the day, month of the year and year effects. In X_{it} we include age and gender of the driver.

Estimates for β under different sets of controls are reported in Table 6. In Panel A, we report the effect of the increase enforcement on helmet use. As illustrated in Figure 2, this effect is positive and large, with the probability of using helmet increasing by roughly 87 percentage points after the policy was introduced. In Panel B, we report that the probability of having a serious or fatal injury was reduced by roughly 7 percentage points in Mercedes after the change in enforcement. Reassuringly, in both cases the coefficients are stable across specifications, indicating that controls have very little impact on our estimates. These results show the change in enforcement was both successful in promoting helmet use and in reducing accidents.

Table 7: Motorbike Accidents in Mercedes and Melo

	(1)	(2)	(3)
Panel A		Use of helmet	
Mercedes x Post	0.871***	0.869***	0.867***
	(0.019)	(0.019)	(0.020)
Observations	3,354	3,354	3,175
R-squared	0.764	0.769	0.768
Panel B		Serious injuries and fatalities	
Mercedes x Post	-0.070***	-0.072***	-0.068***
	(0.022)	(0.022)	(0.023)
Observations	3,378	3,378	3,198
R-squared	0.006	0.025	0.032
General Contr.	No	Yes	Yes
Driver Contr.	No	No	Yes

Note: The variable Mercedes x Post is a dummy that takes the value 1 when the accident took place in Mercedes after November 1st, 2013. In Panel A, the dependent variable is a dummy for helmet use. In Panel B, the dependent variable is a dummy taking value 1 if the accident resulted in a serious or fatal injury. General controls include a dummy for school and public holidays; a full set of day-of-week-specific fixed effects, hour-of-day fixed effects, month-of-year fixed effects and year fixed effects. Driver controls include a dummy for gender and the age of the driver. Sample restricted to accidents in Mercedes and Melo. Robust standard errors in parentheses. *p<.1; **p<.05; ***p<.01.

If helmet use is behind the reduction in serious or fatal injuries, we expect a positive effect on minor injuries as a result of the change in enforcement. Accidents that would have resulted in a serious injury if a helmet was not used may result in a minor injury instead. To explore this, we reproduce the previous analyses using an indicator taking value 1 if an accident results in minor injuries as the dependent variable, and 0 if an accident ends with no reported physical harm. Results are reported in Table A4. We estimate model 4 and find a substantial increase of roughly 8 percentage points in the probability that motorbike accidents result in minor injuries in the city of Mercedes after the helmet law was enforced. This points to a transfer of serious to minor injuries as a result of the change in enforcement, echoing the results of Table 5.

5. Discussion and Conclusions

5.a. Valuation of the Change in Enforcement of the Mandatory Helmet Use Law

We can use our estimates and additional information on health and administrative costs to outline a cost-benefit analysis of helmet use laws for Uruguay. The main benefits of the policy arise from the reduction in serious injuries and fatalities from motorcycle accidents. The main costs relate to: i) the administrative costs of enforcement paid by the relevant agencies and, ii) the nuisance costs of wearing a helmet for motorcyclists. The latter is particularly hard to estimate, but we can calculate which could be the magnitude of the costs that would be required to reverse the change in benefits.[26] The outcome of the cost-benefit analysis can then be obtained relative to this benchmark.

Health benefits resulting from the change in enforcement can be due to a reduction in the volume of serious accidents, and a reduction in the volumes of deaths. Paolillo et al. (2016) documents that roughly 1.5 out of 10 serious traffic accidents lead to a fatality. The same source estimates average intensive care hospitalization costs for serious traffic accidents in Uruguay to be USD 7,437. A conservative estimate for the value of a statistical life is USD 2,346,000 dollars.[27] We obtain health benefits by multiplying these figures times an estimate of the absolute reduction in serious injuries. The coefficient on the reduced-form effect of the policy on serious accidents in column 4 of Table 3 is

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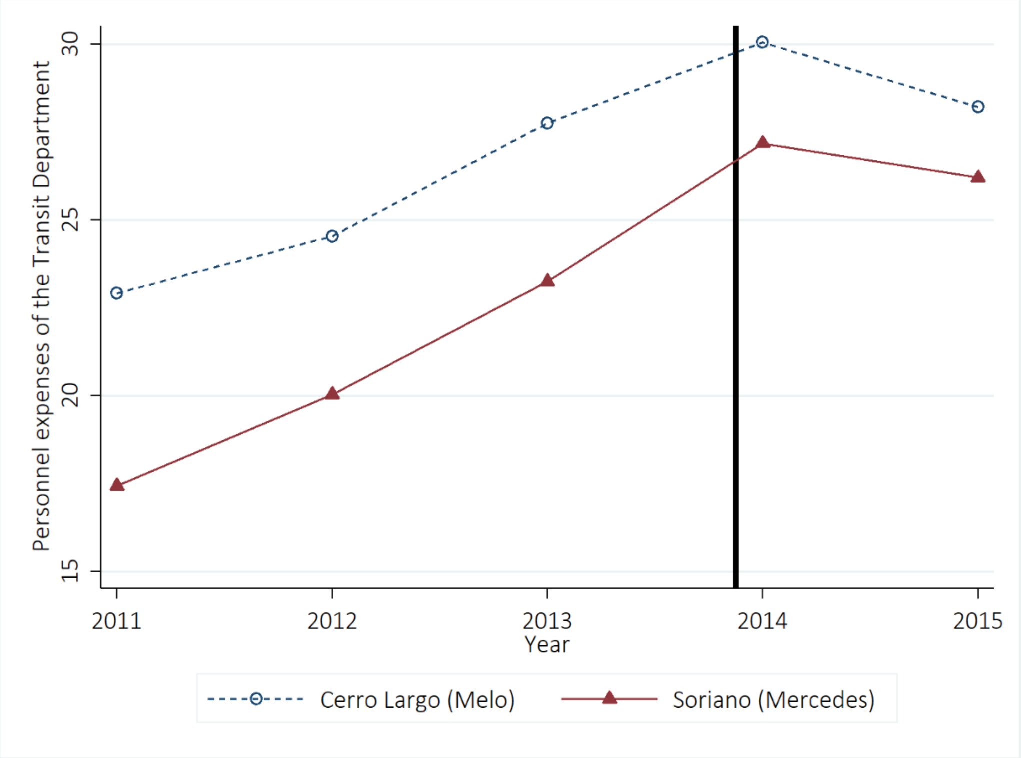
[26] Standard revealed-preference valuation tools, such as the opportunity cost or compensating differential methods, cannot be applied to measure nuisance costs because there are no other markets compensating for these costs, or pricing a similar bad.

[27] In the literature, there is considerable uncertainty about the value of life, depending on the method used, the age of the victim, or the country where it is estimated. According to U.S. E.P.A. (2014), a recommended default central value of a statistical life (VSL) is around USD 8.7 million (in 2014 dollars). The U.S. Department of Transportation (2013) indicates that, on the basis of the best available evidence, the VLS that should be used for benefits of preventing traffic fatalities is USD 9.1 million (in 2012 dollars). Considering that Uruguayan GDP per capita is 27 percent of the US GDP per capita, we employ a conservative value of 2,346,000 dollars for our estimates.

4.7%. The average number of yearly motorcycle accidents in Soriano is 610. Hence, the policy leads to a reduction of roughly 29 serious or fatal accidents per year. Using this number, we can compute estimated health benefits from the policy as 29 × 0.15 × 2,346,000 + 29 × 0.85 × 7,437. This yields a figure of USD 10,389,727 per year in benefits arising from reduced hospitalization costs and deaths only. Assuming a 5% discount rate and a 30 year time horizon (as in Dee 2009), the present value of health benefits would be in the order of USD 160 million. This corresponds to USD 6,789 per capita.

It is worth noting that other health effects, such as psychological costs and permanent disability resulting from serious accidents, or reduced work hours for hospitalized patients, are likely to be substantial. Therefore, we consider these figures to be an underestimate of total health benefits.[28]

Figure 5: Personnel expense of the Municipal Transit Department



Note: Personnel expenses of the transit departments expressed in millions of 2015 Uruguayan pesos. Lines correspond to the evolution of expenses over time for Soriano (triangles) and Cerro Largo (circles). Vertical line corresponds to November 2013. Source: *Observatorio Territorio Uruguay (OPP).*

Public enforcement of the helmet law requires the use of traffic inspectors to detect and to sanction violators. How much of Soriano's public resources were devoted to these tasks? Figure 5

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[28] As discussed in section 3, the reduction in serious and fatal injuries comes at the expense of an increase in minor injuries. Minor injuries will impose costs of their own, although by definition they will not require hospitalization. These unaccounted costs are arguably higher for serious accidents, and so our estimate of net health benefits would still be a lower bound of total health costs, even after accounting for the increased number of minor injuries.

reports personnel expenses of the Transit Department of the departments of Soriano and Cerro Largo. The parallel trends observed before the enforcement of the law do not change after it. In other words, Soriano achieved an abrupt increase in the compliance with the helmet law after 2013 without an escalation in personnel costs. Consulted officials at the Soriano transit authority stated that enforcement of the law did not involve the deployment of additional human resources. Inspectors were already deployed within the city of Mercedes in order to enforce other transit rules (speed limits, traffic lights, etc.) and, after the law was enforced, the same inspectors just added another complementary task –the enforcement of the helmet law- to their daily activity. Information campaigns on helmet use were included on traffic safety campaigns already in place before the policy change. Hence, it is not surprising that we do not identify a significant administrative cost of enforcement in this case.

There were 26,435 registered motorcycles in Soriano in 2013. Nuisance costs of helmet for registered motorcycles resulting from the policy will be equal to this figure scaled by the change in helmet use, which is 89% (see panel A of Table 3). Our health benefits estimate is USD 10,389,727 per year. So, for yearly nuisance costs per registered motorcycle under USD 442, the policy would have a positive net benefit. Because our estimate of health benefits is probably downward biased, this is a lower bound for break-even nuisance costs per motorcyclist.

Given this discussion, low levels of helmet use in the absence of appropriate enforcement during 2013 can be explained on three grounds: large nuisance costs, moral hazard or biased perception of risks. First, if nuisance costs of wearing a helmet – plus pecuniary costs of owning one – are well above USD 442 a year, then the laissez-fair outcome is that rational cyclists will choose not to wear a helmet. Second, cyclists may not internalize the full costs of serious injuries because of the pervasiveness of health and disability insurance. If this is the case, even if costs of helmet use are below USD 442 per year, it may still be privately optimal for drivers to not use a helmet. Finally, it is not obvious that motorcyclists have an accurate perception of the risks of driving without a helmet. The same outcome of low helmet use without enforcement would be observed if motorcyclists' subjective probabilities of serious accidents are lower than actual probabilities.

5.b. Conclusions

Mandatory helmet use laws for motorcyclists are a feature of transit regulation in many jurisdictions. Yet these are not universal, and enforcement can often be extremely poor, particularly in low- and middle-income countries. This paper shows that changes in enforcement can lead to a substantial alleviation of the deleterious health consequences of motorcycle accidents. Our difference-in-differences estimates indicate that changes in the enforcement of helmet use laws in Uruguay lead to a substantial reduction of roughly 5 percentage points in the rate of serious or fatal injuries. Given the national base rate stands at roughly 12 percent for this period, this effect is sizeable. The reduction in serious accidents takes place at the expense of an increase of minor injuries, pointing squarely to a net reduction in accident severity. Accident numbers and the type of accidents taking place – both for motorcycles and other vehicles – do not appear to be affected by the change in policy. This further alleviates concerns that behavioral responses to helmet use – such as increased driving speeds or more reckless conduct by motorcyclists – counter the direct effect of using a helmet to prevent head trauma.

Combining our reduced-form estimates of changes in accident severity with costs of hospitalization and the value of statistical life, we calculate an approximate measure of the health benefits resulting from the change in enforcement. Given that direct enforcement costs by the involved traffic control agencies were largely unaffected by the policy, the main costs of increased helmet use are associated to the nuisance these implements may generate for drivers. Substantial nuisance costs would be necessary to compensate for the policy's health benefits.

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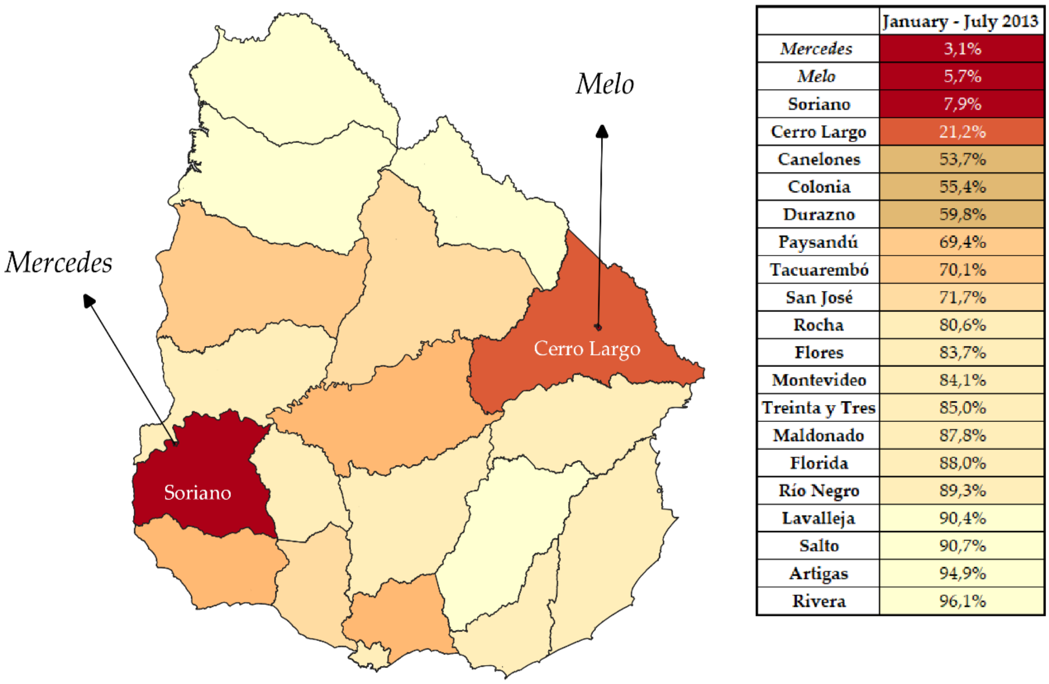
Appendix

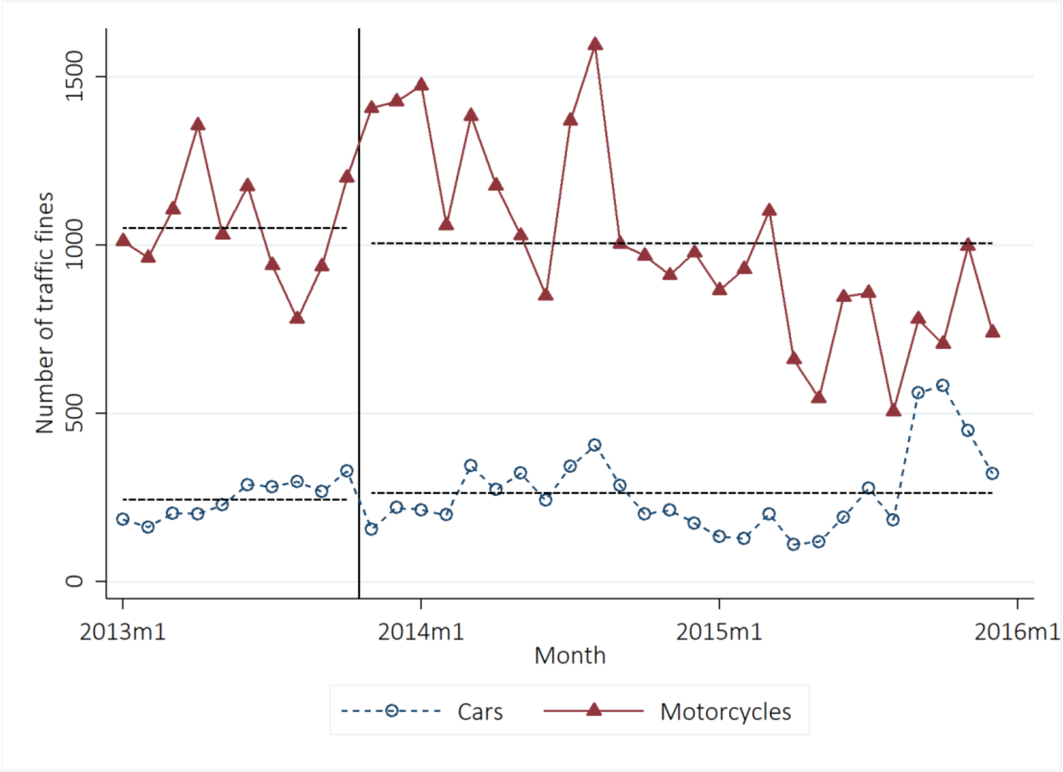
Figure A1 – Helmet Use by Department (Uruguay)



	January - July 2013
Mercedes	3,1%
Melo	5,7%
Soriano	7,9%
Cerro Largo	21,2%
Canelones	53,7%
Colonia	55,4%
Durazno	59,8%
Paysandú	69,4%
Tacuarembó	70,1%
San José	71,7%
Rocha	80,6%
Flores	83,7%
Montevideo	84,1%
Treinta y Tres	85,0%
Maldonado	87,8%
Florida	88,0%
Río Negro	89,3%
Lavalleja	90,4%
Salto	90,7%
Artigas	94,9%
Rivera	96,1%

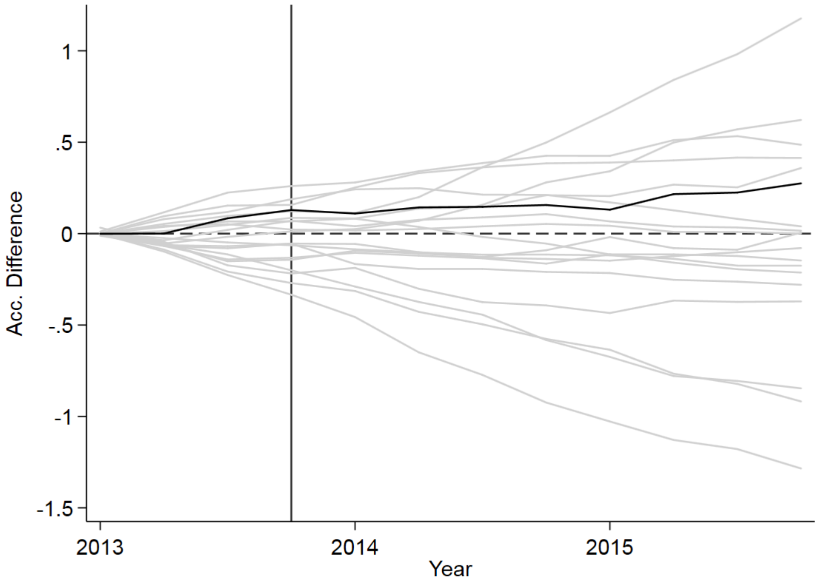
Note: Polygons representing the 19 departments of Uruguay. Shades correspond to helmet use as measured by the fraction of motorbike accidents where the riders were wearing a helmet. The table also includes data for Mercedes (the capital city of Soriano) and Melo (the capital city of Cerro Largo).

Figure A2 – Number of traffic fines in Soriano



Note: Horizontal lines represent the average number of monthly traffic fines in Soriano, for cars and motorcycles, calculated before and after the change in policy. Source: own calculations based on data from Source SUCIVE (*Sistema Único de Cobro de Ingresos Vehiculares*). Period: 2013-2015.

Figure A3 – Synthetic Control: Accumulated Number of Motorbike Accidents p.c.



Note: Solid line represents accumulated difference in the number of motorbike accidents per capita between the department of Soriano and a synthetic Soriano control constructed using the method detailed in the text. Gray lines represent the accumulated difference between observed numbers and synthetic controls for other departments.

Table A1– Fatality rate in motorcycle accidents and GDP per capita.

Country	Fatality rate	GDP per capita	Year
Albania	2.18	10,571	2013
Angola	0.42	6,970	2013
Australia	0.97	45,794	2013
Austria	1.15	47,922	2013
Azerbaijan	0.05	17,172	2012
Bahamas	2.38	29,878	2013
Bahrain	0.28	44,168	2013
Bangladesh	1.46	2,935	2012
Barbados	1.12	16,980	2013
Belgium	1.06	43,520	2013
Belize	5.15	8,072	2013
Benin	15.92	2,002	2012
Bhutan	0.25	7,317	2013
Bolivia	2.36	6,303	2013
Bosnia & Herzegovina	1.71	10,826	2013
Brazil	6.49	15,971	2012
Bulgaria	0.72	16,571	2013
Cambodia	12.35	3,068	2013
Canada	0.51	44,101	2012
Chile	0.87	22,579	2013
China	5.16	12,368	2013
Colombia	7.59	12,634	2013
Costa Rica	4.06	14,525	2013
Côte d'Ivoire	4.93	2,980	2013
Croatia	1.59	21,807	2013
Cyprus	1.76	30,621	2013
Czechia	0.68	30,486	2013
Denmark	0.47	46,727	2013
Dominican Republic	18.76	12,322	2013
Ecuador	1.39	11,037	2012
Egypt	0.07	10,157	2013
El Salvador	2.36	7,027	2013
Estonia	0.42	27,496	2013
Finland	0.53	41,294	2013
France	1.20	39,524	2013
Georgia	0.42	8,542	2013
Germany	0.84	45,232	2013
Ghana	3.61	3,940	2012
Greece	3.00	26,098	2012
Guatemala	4.73	7,249	2013
Guyana	3.42	6,930	2013
Honduras	1.32	4,323	2013
Hungary	1.07	24,463	2013
Iceland	0.31	42,821	2013
India	5.50	5,251	2013
Iran	6.91	16,955	
Ireland	0.56	48,067	2013
Israel	0.48	34,129	2013
Italy	1.58	36,131	2013
Jamaica	2.33	8,309	2013
Japan	0.82	38,974	2013
Kazakhstan	0.54	23,773	2012
Kenya	1.41	2,776	2013
Laos	10.00	5,294	2013
Latvia	0.75	22,677	2013
Lesotho	0.33	2,723	2013
Libya	1.10	22,322	2013
Lithuania	0.80	26,661	2013
Luxembourg	1.51	95,591	2013
Macedonia	1.01	12,656	2013
Malawi	0.45	1,099	2012
Malaysia	14.90	24,034	2013
Maldives	0.50	13,607	2013
Mali	4.85	1,856	2013
Malta	1.44	31,069	2013
Marshall Islands	3.79	3,851	2013
Mauritius	3.73	18,244	2013
Mexico	0.74	16,848	2012
Moldova	1.46	4,700	2013
Mongolia	4.02	11,094	2013
Montenegro	1.13	14,870	2013
Morocco	4.31	7,240	2013
Namibia	0.14	9,578	2012
Netherlands	0.46	48,666	2013
New Zealand	0.94	36,220	2013
Nicaragua	4.07	4,780	2013
Norway	0.48	67,056	2013
Oman	0.57	43,387	2013
Paraguay	11.78	10,950	2013
Peru	0.14	11,829	2012
Philippines	5.53	6,527	2013
Poland	0.97	24,719	2013
Portugal	1.62	27,900	2013
Romania	0.44	19,797	2013
Russia	0.81	26,240	2013
San Marino	3.10	59,764	2013
São Tomé & Príncipe	10.30	2,883	2013
Serbia	0.88	13,760	2013
Singapore	1.66	81,648	2013
Slovakia	0.56	27,898	2013
Slovenia	1.08	29,797	2013
South Korea	1.92	32,616	2013
Spain	0.78	32,604	2013
Sri Lanka	7.32	10,596	2013
Suriname	7.99	15,957	2013
Sweden	0.47	45,673	2013
Switzerland	0.68	60,109	2013
Tanzania	6.95	2,397	2013
Thailand	25.89	15,287	2012
Trinidad & Tobago	0.36	32,500	2013
Tunisia	4.99	10,948	2013
Turkey	0.38	22,311	2013
Uganda	8.38	1,667	2013
United Arab Emirates	0.29	63,839	2013
United Kingdom	0.55	39,308	2013
United States	1.59	52,782	2012
Uruguay	8.80	19,943	2013
Zambia	0.52	3,701	2013
Zimbabwe	1.64	2,287	2013

Note: Data sources: Fatalities rate in motorcycle accidents, from World Health Organization. GDP per capita, at purchasing power parity, from World Bank.

Table A2 – Descriptive Statistics for Mercedes and Melo

	Mercedes	Melo
Total population	41,974	51,830
Total number of motorcycle or moped	12,420	12.828
Total number of automobile or van	4,900	6,130
Number of motorcycle or moped per capita	0.296	0.248
Number of automobile or van per capita	0.117	0.118

Note: Own calculations based on Uruguayan National Census 2011. Uruguay is divided in 19 departments. Mercedes is the capital of Soriano Department, and Melo is the capital of Cerro Largo Department. Both cities show similar ratios of motorcycles and automobiles.

Table A3 – Descriptive Statistics for Accident Database

Panel A - All localities

	All vehicles			Motorbikes		
Variables	Mean	SD	Obs.	Mean	SD	Obs.
Serious injury or death	0.06	(0.25)	175,759	0.12	(0.33)	69,969
Slight injury	0.38	(0.49)	175,759	0.66	(0.47)	69,969
Unharmed	0.56	(0.50)	175,759	0.22	(0.41)	69,969
Male	0.74	(0.44)	175,198	0.70	(0.46)	69,774
Age	37.05	(16.19)	166,139	31.01	(14.26)	66,073
At night	0.26	(0.44)	175,198	0.28	(0.45)	69,969

Panel B - Mercedes and Melo

	All vehicles			Motorbikes		
Variables	Mean	SD	Obs.	Mean	SD	Obs.
Serious injury or death	0.04	(0.20)	6,183	0.07	(0.26)	3,378
Slight injury	0.38	(0.48)	6,183	0.62	(0.49)	3,378
Unharmed	0.58	(0.49)	6,183	0.31	(0.46)	3,378
Male	0.67	(0.47)	6,160	0.60	(0.49)	3,363
Age	35.33	(17.14)	5,836	29.66	(15.42)	3,189
At night	0.26	(0.44)	6,183	0.28	(0.45)	3,378

Note: Own calculations based on UNASEV (*Unidad Nacional de Seguridad Vial*, Uruguay). Medical service personnel are responsible for identifying if the person is slightly or seriously injured, with the difference depending on whether the person has one or more of their vital organs compromised. Deaths are registered to have happened as a consequence of an accident if the fatality is either at the time of the accident or at the medical center within 30 days of the accident taking place. "At night" is a dummy variable that takes the value "1" if the accident occurred at night. "Male" and "Age" refer to the person that suffered the accident. Data: period 2013-2015.

Table A4: Minor Injuries and Helmet Use

Mercedes and Melo	(1) Minor D.	(2) Minor D.	(3) Minor D.
Mercedes x Post	0.118***	0.0829**	0.0806**
	(0.0396)	(0.0376)	(0.0381)
Observations	3,354	3,354	3,175
R-squared	0.009	0.017	0.037
Gen. Contr.	No	Yes	Yes
Driv.Contr.	No	No	Yes

Note: Coefficients estimated with the sample of reported motorcycle accidents in Mercedes and Melo that end either on minor injuries or with unharmed individuals. The dependent variable in all specifications is a dummy taking value 1 if the accident resulted in a minor injury, and 0 if an accident ends with unharmed individuals. We include driver level controls and an additional set of dummies to account for school holidays and day-of-the-week effects. Standard errors clustered at the town level in parentheses. *p<.1; **p<.05; ***p<.01