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

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Abstract

We study the impact of mandatory motorcycle helmet use laws on the severity and volume of road accidents in Uruguay by exploiting a change in the enforcement of the traffic law. Using a difference-in-differences design based on an unexpected change in policy, we report a sharp increase in helmet use and a five-percentage-point reduction in the incidence of serious or fatal motorcyclist accidents from a baseline of 11 percent. The benefits of helmet use are disproportionately borne by groups more likely to experience serious injuries, such as males or young drivers. We find no evidence of other responses in terms of either the volume or type of accident, suggesting that motorcyclists' behavior did not respond to differences in risk. We show that additional costs of enforcement for the relevant government agencies were negligible and estimate the health benefits of the policy.

Key words: Law enforcement; safety and accidents; helmet use.

JLE: I12; I18; R41; H89

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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 percent of gross domestic product (GDP) in most economies. These costs are particularly high in the case of low- and middle-income countries, which register 93 percent 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. For several decades, economists have studied the effectiveness of seat belt use laws, in particular, because they can in theory modify the actual and perceived risks of driver behavior. In turn, this could hypothetically induce unexpected changes in accidents that may render regulation ineffective or counterproductive. This is known as the Peltzman hypothesis, after a study showing 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.[1]

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.

1. 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 nineteen 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 the severity of motorcycle accidents, with estimates suggesting that helmet use leads to a five-percentage-point reduction in serious and fatal accidents (from a baseline probability of 11 percent) and a similar increase in the fraction of accidents resulting in minor injuries. This effect is of a similar magnitude to that observed for the reduction in serious accidents induced by seat belt use reported elsewhere.[2] Despite this large magnitude and, in contrast to the implications of the Peltzman hypothesis, we find no evidence of risk compensation by drivers. Neither accident volume nor the type of accident taking place changes as a result of the increase in helmet use.

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. Finally, we document differences in the effectiveness of helmet use on accident severity for different subpopulations and report that helmets appear to be more effective at reducing accident severity for the subpopulations more at risk of injury, such as males, young drivers, or victims of accidents taking place at night.

A small set of studies in economics have looked specifically at the effects of helmet use in traffic accidents.[3] Perhaps the closest to our work is Dee (2009), which provides estimates of the effect of the

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2. Centers for Disease Control and Prevention (CDC), "Policy Impacts: Seat Belts," December 2010 (www.cdc.gov/transportationsafety/seatbeltbrief/index.html).

3. Studies in the fields of accident prevention and medicine also look at this question using a variety of empirical methods. Some recent examples include Houston and Richardson (2008), Peng and others (2017), Olsen and others (2016), and Lee (2018).

introduction/removal of helmet use laws in U.S. states on fatalities, using a panel specification.[4] Total fatality effects are meant to incorporate the direct effect of helmet use plus potential compensating behavioral adjustments by drivers. Dickert-Conlin, Elder, and Moore (2011) find evidence of increased availability of organ donations by deceased motorcyclists in U.S. states that repeal mandatory use laws. Carpenter and Stehr (2011) find that the introduction of mandatory bicycle helmet use laws for the young results in reduced fatalities. They also report a substantial reduction in cycling.

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 United States. This is particularly important given that enforcement issues are especially acute in low- and middle-income countries.

Our paper also relates to previous studies in economics estimating the impacts of seat belt use on health outcomes for drivers or nondrivers. 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 U.S. state panel. They respectively study the impact of seat belt laws on fatalities and injuries for vehicle occupants and nonoccupants. While we also exploit longitudinal variation by jurisdiction to estimate our effects of interest, there are important differences relative to these studies. In particular, we look at mandatory helmet use instead of seat belt use, and we use administrative data on individual accidents to investigate effects on the types of accident 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 and other factors (for example, 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 U.S.-centered literature,

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

Aney and Ho (2019) study the impact of the Chinese Road Traffic Safety Law on the volume and severity of accidents and on fatalities. Our paper adds credible estimates of the effect of our policy of interest to the economics literature on policy solutions to traffic problems in the developing world.

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 five to twenty-nine 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.

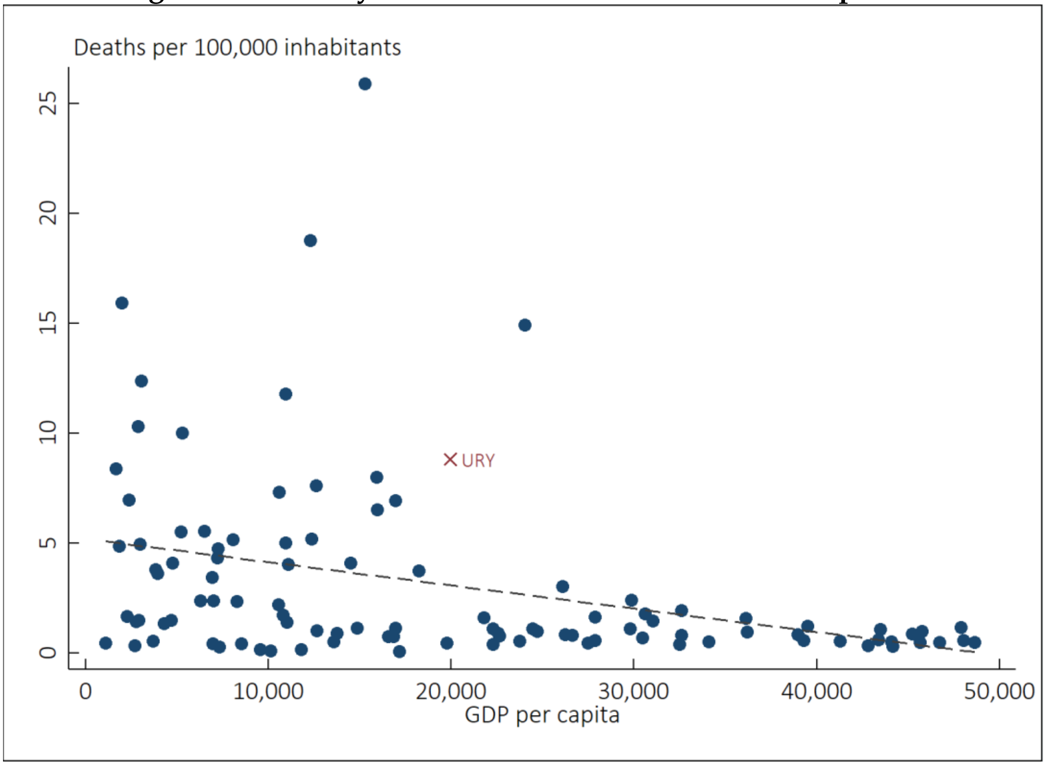
Globally, those using motorized two-and three-wheelers—mainly motorcycle riders—represent 28 percent of all traffic-related deaths. The heavy burden of deaths borne by these road users is, at least in part, a result of them being less physically protected than car occupants. This additional risk for motorcycle users also affects the distribution of traffic-related deaths worldwide, as motorcycle use is generally more prevalent in developing countries.[5] Figure 1 shows a negative relationship between fatalities in motorcycle accidents and GDP per capita.[6] 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 coexist with low use rates: Argentina, Bolivia, Iran, Peru, and Uganda all have mandatory helmet use laws, but in these countries over 30 percent of drivers, and roughly 60 percent of passengers, do not wear helmets (WHO, 2018). The situation is often worse: in India and China, helmets are used by 30

5. According to the 2014 Spring Pew Global Attitudes Survey, motorcycle ownership rates are regularly above 50 percent in developing East Asian economies, but less than 30 percent in developed countries.
6. Detailed information on per capita GDP and motorbike fatality rates by country can be found in table A1 in the appendix.

percent and 20 percent of drivers, respectively. Both countries have had mandatory helmet laws for over a decade.

Figure 1 – Motorcycle Fatalities and Economic Development



Source: World Health Organization and World Bank.

Note: Sample corresponds to countries with GDP per capita of less than USD 50,000 (measured in 2013 dollars, at purchasing power parity). Country marked with a cross corresponds to Uruguay. Dashed line corresponds to a linear regression estimated over the scatter plot.

That is not to say that mandatory helmet laws are universal. In the United States, many states only require helmet use for young riders (for example, under the age of twenty). 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 those helmets.

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. First, the helmet cushions the impact and therefore reduces the deceleration of the skull, which limits the speed of the impact between the brain and the skull. Second, 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 three 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 a 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

In November 2007, the Uruguayan Parliament approved a new National Traffic Law (Law 18,191) that required mandatory helmet use for motorcyclists in all nineteen departments of the country.[7] 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.[8] The refusal to enforce mandatory helmet use was partly based on electoral considerations, featuring prominently among the electoral promises in both departments. Both governors (*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 early 2013—when 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 percent and 21.2 percent for Soriano and Cerro Largo, respectively. The average for other departments stood at

7. A map of Uruguayan departments, including the percentage of helmet use, can be found in figure A1 in the appendix.

8. 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.

roughly 75 percent. Moreover, helmet use was particularly low in Mercedes (the capital city of Soriano) and Melo (the capital city of Cerro Largo)—respectively, 3.1 percent and 5.7 percent.[9]

In August 2013, Parliament approved the Misdemeanors Act (Law 19,120), 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 governor of Soriano informed citizens that 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."[10] On 1 November 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 type 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 the Misdemeanors Act by the National Parliament, and the governor specifically cited this approval 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 drunk driving—were enforced regularly and was often explicitly mentioned by the governor of Soriano before 2013 when defending his decision not to enforce the helmet laws.[11]

9. These cities have comparable numbers of registered motorcycles and automobiles per capita and similar helmet usage figures before 2013 (see table A2 in the appendix).
10 Interview in the internet media 180.com.uy, November 7th, 2014:
https://www.180.com.uy/articulo/51928_Besozzi-No-exigir-el-casco-pudo-ser-un-error
11. In statements to the news portal 180.com.uy, Soriano governor declared "We were betting on controlling drunk driving and speeding. We were strong with those (regulations) because 85 percent of accidents happened under the effect of alcohol or drugs or while speeding." November 7th, 2014:
https://www.180.com.uy/articulo/51928_Besozzi-No-exigir-el-casco-pudo-ser-un-error

According to administrative data on fines, the average number of fines issued by the Soriano traffic department before and after the policy change was stable.[12] 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.[13] 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 helmet or seat belt use as applicable. Locations in the original data set are reported with the latitude and longitude of each accident. We use location information to obtain the locality or town of each accident.

While the police report is filed by the officers 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 one or more vital organs are compromised. Death is registered to have happened as a consequence of an accident if it occurred either at the time of the accident or at the medical center within 30 days of the accident. During the period under consideration—from 2013 to 2015—203,725 people were involved in traffic accidents in Uruguay. Excluding pedestrians and accidents with missing location information, we have 149,873 observations in our database. Roughly 40 percent of those observations involved motorbikes. Twelve out of 100 people suffering motorbike accidents were seriously injured or killed, more than double the rate observed for other vehicles.[14] In the capitals of Soriano and Cerro Largo—Mercedes and Melo—3,378 persons suffered motorbike accidents in this period.

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12. This result is based on data from SUCIVE *(Sistema Único de Cobro de Ingresos Vehiculares).* The database has every fine for traffic offenses imposed in Soriano from 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 ($p = 0.66$).
13. National Road Safety Agency (*Unidad Nacional de Seguridad Vial*).
14. See table A3 in the appendix.

Table 1 shows the descriptive statistics for all reported motorbike accidents between 2013 and 2015, splitting the sample by helmet usage. Wearing a helmet is associated with a significantly lower probability of being seriously injured in motorcycle accidents, with riders wearing a helmet facing a 3.8 percentage point lower probability of being seriously injured or killed. Clearly, this figure does not account for the potential endogeneity of helmet use. Motorcyclists make several decisions when riding their motorcycle: how fast to go, whether to respect traffic signs, whether to drive under the effects of alcohol or drugs, and whether to wear a helmet. Thus, helmet usage is an endogenous choice variable. Riders who decide to use a helmet self-select themselves into this group, 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 that unhelmeted riders are disproportionately young, male, and riding at night. In the next sections of the paper, we 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, 2013–15

	No helmet			*Helmet*			*Mean differences*
Variable	*Mean*	*Std. dev.*	*N*	*Mean*	*Std. dev.*	*N*	
Serious injury or death	0.14	(0.34)	14,283	0.10	(0.30)	46,406	0.037***
Slight injury	0.60	(0.49)	14,283	0.69	(0.46)	46,406	–0.095***
Unharmed	0.27	(0.44)	14,283	0.21	(0.41)	46,406	0.058***
Male	0.75	(0.44)	14,225	0.68	(0.47)	46,301	0.067***
Age	26.87	(13.53)	12,520	31.77	(14.04)	44,790	–4.900***
At night	0.32	(0.47)	14,283	0.27	(0.44)	46,406	0.052***

Source: Data from *Unidad Nacional de Seguridad Vial* (UNASEV), Uruguay. *p < .1; **p < .05; ***p < .01.

In our difference-in-differences analysis below, we compare the evolution of accident volumes, helmet use, and health outcomes in Soriano with the rest of the country. Table 2 presents a series of pretreatment descriptive variables for four relevant groups. Soriano has a population of roughly 83,000 inhabitants. According to the 2011 census, there were 140 cars and 302 motorcycles per 1000 inhabitants in this department. The population and the number of cars per capita are lower in this department than in the average department in Uruguay. Conversely, Soriano has a large number of motorcycles per capita relative to the rest of the country. A substantial fraction of these differences can be attributed to Uruguay’s capital, Montevideo. In our robustness checks, we verify that the main results of this paper are robust to excluding Montevideo from the comparison group in our difference-in-differences sample. With regard to accidents and motorcycle accidents in particular, both accident severity and total accidents per capita are fairly similar between Soriano and other departments.

Table 2. Descriptive Variables by Department

Variable	*Soriano*	*Other departments*	*Other depts. (excl. Montevideo)*	*Cerro Largo*
Census 2011				
Mean population	82,594	177,960	110,855	84,698
Cars per 1,000	140	137	140	124
Motorbikes per 1,000	302	148	217	258
Accidents, UNASEV 2013				
All accidents				
Total per 1,000	9.3	16.3	15.0	15.8
Serious injury (%)	5.7	4.8	5.0	3.7
Minor injury (%)	36.3	34.6	35.8	37.3
Motorbike accidents				
Total per 1,000	5.6	4.8	5.5	6.0
Serious injury (%)	9.0	9.9	9.1	6.9
Minor injury (%)	56.6	69.0	68.5	70.4
Helmet (%)	6.5	78.8	75.5	20.1

Our accident data only includes *reported* accidents. We expect the coverage of our data to be reasonably comprehensive, particularly for accidents in which the participants were injured or the vehicles were damaged.[15] That said, some accidents are surely missing from the UNASEV source. Therefore, we work with a selected sample of the total population of drivers; we cannot observe or document the helmet use of riders who were not involved in accidents or accidents that were not reported. This has two significant implications for our empirical analysis. The first is that cross-sectional differences in accident volumes and in the type of accident taking place may induce some degree of endogenous selection. Here is where the change in policy allows us to devise an empirical strategy that avoids this issue. The second implication relates to the interpretation of our findings. Our estimates of the impact of helmet use on the probability of having a serious accident are made relative to the population of bikers involved a reported accident. We believe this is the population of

15. According to Law 18,191 (*Ley Nacional sobre Tránsito y Seguridad Vial*), all accidents resulting in personal or material damages must be reported to the relevant authorities. Third-party insurance is mandatory in Uruguay, and the associated payment can only take place if the accident was reported.

interest from a policy perspective, particularly given that we do not find an impact of helmet use on the volume of accidents. Nevertheless, the resulting estimates would be slightly lower in absolute terms if taken over the (unobservable) population of all accidents.

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 tackle these objectives by exploiting the abrupt change in enforcement of helmet use in the department of Soriano in November 2013. We do so in the context of a difference-in-differences framework where the evolution of accident volumes, helmet use, and accident severity in this department is compared with that of other locations in the country. The resulting difference-in-differences coefficients can be interpreted as an average treatment effect of the change in policy under the typical parallel trends assumption.

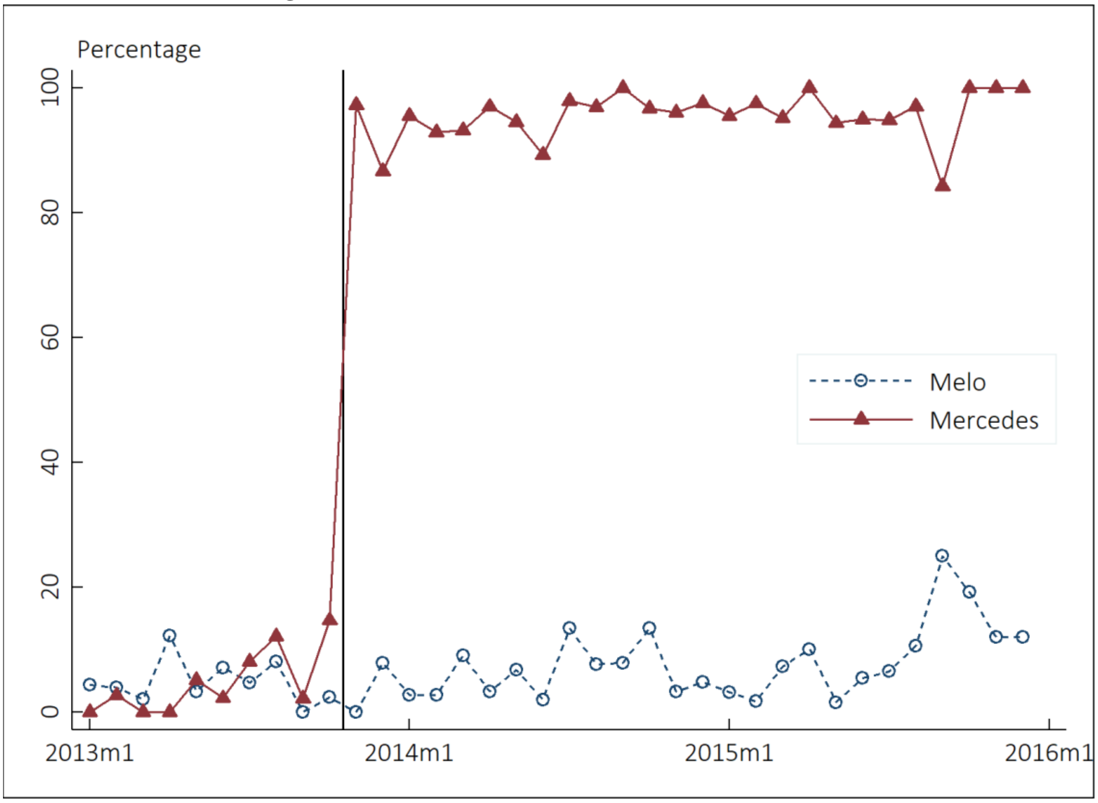
In addition to studying the impact of the change in enforcement on helmet use and accidents, we use our data to explore the heterogeneous impact of helmet use on different types of driver.

3.a. Illustration: Mercedes and Melo

In early 2013, the cities of 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 both cities in figures 2 and 3.

Figure 2 plots the evolution of the percentage of people involved in a motorcycle accident who were reportedly wearing a helmet, for both cities. We use this variable as a proxy of helmet use. The initial levels of helmet use are remarkably low in both locations, oscillating under 10 percent. In November 2013, the rate of helmet use jumps to almost 100 percent in Mercedes, while the figures for Melo remain very low. This difference is sustained throughout the next two years and indicates that the change in enforcement prompted a persistent increase in helmet use in the city of Mercedes.

Figure 2: Helmet Use in Mercedes and Melo

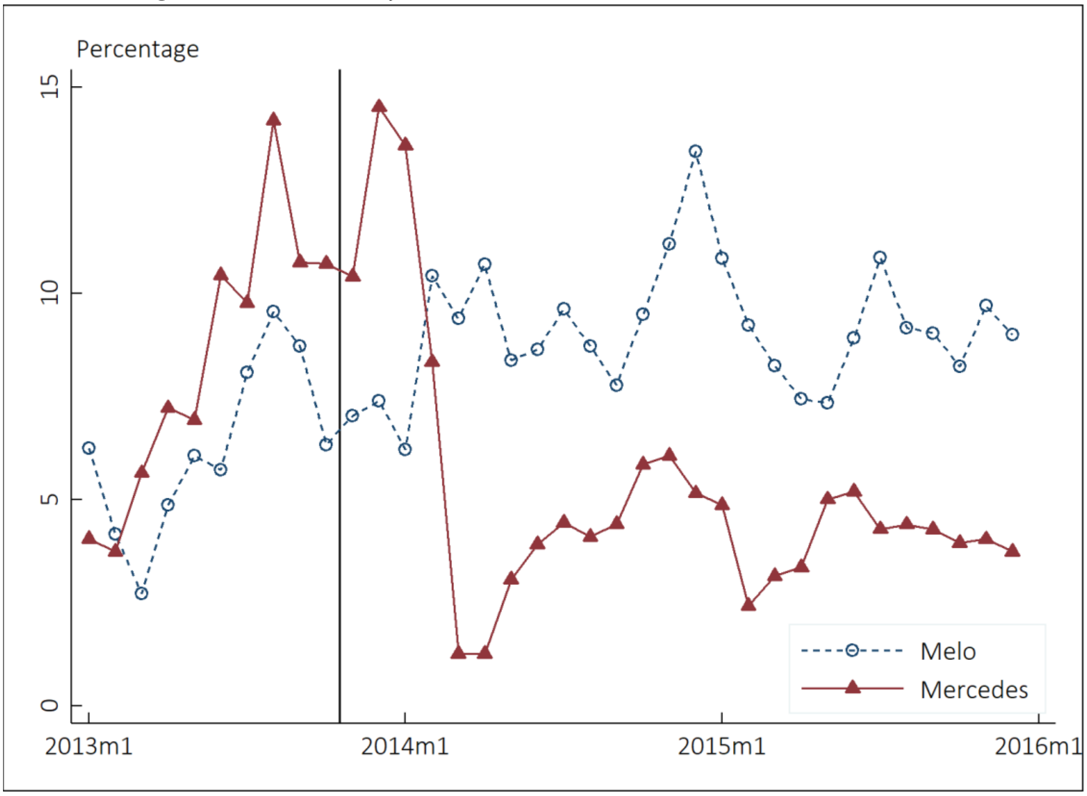


Source: Authors' calculations, based on data from UNASEV.

Note: Helmet usage is measured as the percentage of all motorbike accidents where the driver was wearing a helmet. Vertical line corresponds to November 2013.

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 three-month moving averages to smooth out some of the short-run fluctuations, but avoid smoothing between periods around November 2013. 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 in this city oscillated around 10 percent. Five months after the policy was introduced, serious injuries only occurred in 2 percent of motorbike accidents. Between late 2014 and 2015, the figure would recover to a level of around 4 percent. In this period, the rate of serious injury in Melo was twice as high as the rate in 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



Source: Authors' calculations, based on data from UNASEV.

Note: Serious injury or fatality is defined as a percentage of the number of motorcycle accidents in each city. Vertical line corresponds to November 2013. The series represent three-month moving averages, where averages are taken without crossing the vertical line.

In the figure, the decline in serious accidents in Mercedes does not occur immediately after the change in enforcement, but rather takes about five months to materialize. In the first three months after the introduction, there is an apparent 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. A closer look at the raw data reveals that this was largely motivated by an abnormally high rate in January 2014, which resulted from a relatively low number of motorcycle accidents (thirty) coupled with a relatively large number of serious injuries (eight). Given the low numbers involved in that month, we do not interpret this spike as being an outcome of the policy.

3.b. Difference-in-Differences Strategy

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 motorcycle accidents in all the country's localities in a difference-in-difference specification. We can thus incorporate data from all the towns and villages affected by the policy in the treatment group, while the comparison 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 to identify potential unintended consequences.

Before estimating the effect of the policy, we use a locality-month panel to estimate whether the parallel trend assumption is reasonable in this context and whether the volume and type of accidents were affected by the policy. The first exercise is necessary to give causal interpretation to the difference-in-differences estimates below; the second, to narrow down the potential mechanisms relating helmet use to the change in accidents.

Our data set on road accidents starts in January 2013, so we have ten months to test for differences in pre-trends between the treatment towns in Soriano and comparison towns throughout the country. With these ten months of data, we use our town-month panel and estimate the following specification:

$$Y_{jt} = \alpha_j + \delta_t + \eta \ Post_t \cdot T_j + \varepsilon_{jt} \quad (1)$$

where 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-year dummy variables. The coefficient η multiplies an interaction of a treatment dummy variable T_j, which takes a value of one for the localities of Soriano, and $Post_t$, a dummy variable taking a value of one between June and October 2013.[16] We cluster standard errors at the locality (town) level and consider alternative methods for inference below.

A value of η statistically different from zero indicates that there were differences in pre-trends of the dependent variable between treatment and comparison groups before the policy change. We consider a set of different outcomes to detect trends both in our main variables of interest (namely, helmet use and accident severity) and in other correlates (such as the driver's age and gender and when and where the accident took place). Results are reported in table 3. We find no evidence of statistically significant differential pre-trends in any of the main variables of interest and only marginally significant differences in two out of twelve coefficients. This indicates that the parallel trends assumption required for causal interpretation of our difference-in-differences coefficients below is plausible.

16. By splitting the pre-period in half when studying pre-trends, we attempt to maximize estimate precision.

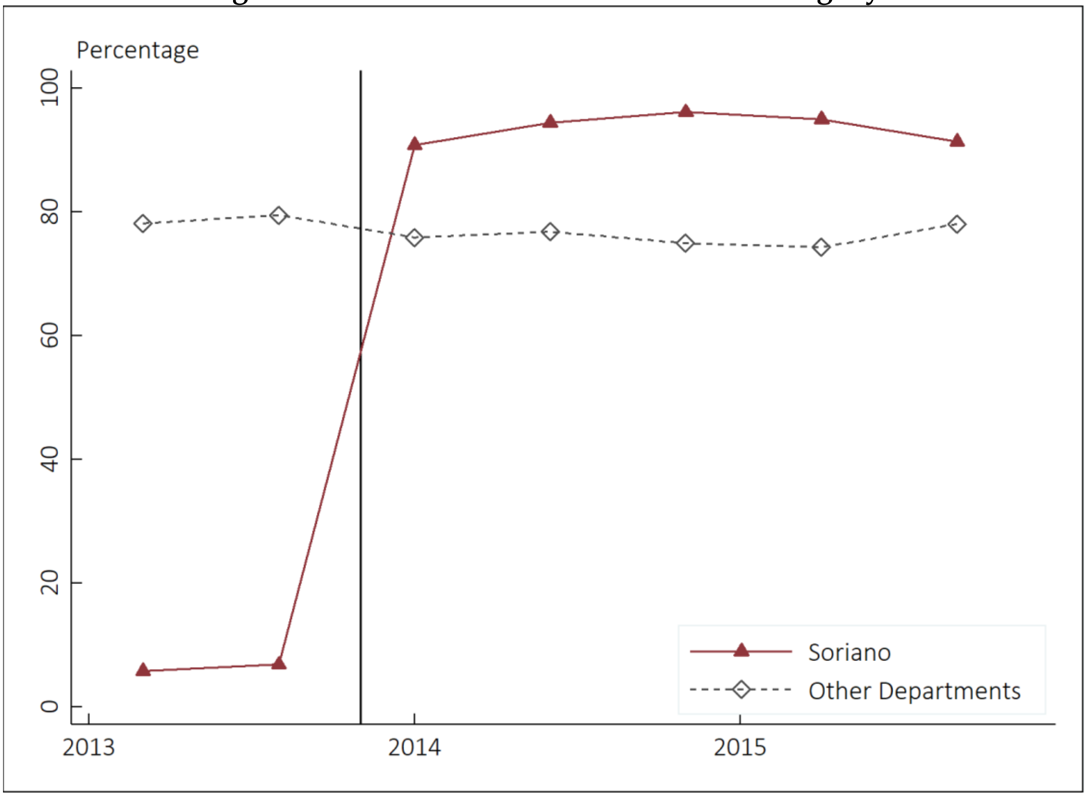
Table 3: Parallel Trends in Town Panel

Panel A Number of accidents	(1) Total Accidents	(2) Moto. Accidents	(3) Serious Moto Accidents	(4) Minor Moto Accidents
Post x Treatm.	2.754	1.700	0.220	0.673
	(2.362)	(1.415)	(0.215)	(0.537)
Observations	4,110	4,110	4,110	4,110
Panel B Shares (%)	(5) Helmet Share %	(6) Serious Accident %	(7) Serious Moto Accident %	(8) Minor Moto Accident %
Post x Treatm.	-0.096	0.044	0.122	-0.113
	(0.099)	(0.159)	(0.161)	(0.077)
Observations	1,192	1,385	1,192	1,192
Panel C Characteristics of drivers and accidents	(9) Youth	(10) Male	(11) Accident in urban area	(12) Accident at night
Post x Treatm.	0.092*	-0.028	0.005	-0.129*
	(0.053)	(0.129)	(0.050)	(0.077)
Observations	1,372	1,383	1,382	1,385

Note: All columns report the coefficient identifying differences in dependent variable trends between treatment and comparison groups in 2013. Estimates obtained using a town-month panel from January to October 2013. In Panel A, the dependent variables are the number of accidents for all type of vehicles in column 1, the number of motorbike accidents in column 2, and the number of serious or minor motorbike accidents in columns 3 and 4, respectively. In Panel B, the dependent variables are the share of reported motorbike accidents where a helmet was used in column 5, the ratio between serious motorbike accidents and all reported accidents or all reported motorbike accidents in columns 6 and 7 respectively, and the ratio of minor motorbike accidents over reported motorbike accidents in column 8. Finally, in panel C we report results for drivers and accident characteristics: youth is defined as age<25, male is the sex of the driver, urban takes the value of 1 if the accident took place in an urban area and 0 in a rural one, and the dummy for an accident at night varies through the year with the time of the sunset. In columns 5, 7 and 8, the sample is restricted to town-month pairs with reported motorcycle accidents. In columns 6 and 9-12 the sample is restricted to town-month pairs with reported road accidents. Regressions 1-10 and 12 control for month and town FE. Regression 11 controls for month and department FE. Standard errors clustered at the town level. *p<.1; **p<.05; ***p<.01

Further evidence on the absence of substantial differences in the pre-trends of our outcomes is presented in figures 4 and 5, built using our individual accident data and averaging within groups. Figure 4 describes the evolution of average helmet use in Soriano and the rest of the country. In both figures, we report five-month averages of the corresponding outcome so that this is comparable to the exercise reported in table 2, and we average the high-frequency fluctuations in the outcome over time.[17] The overarching message from these figures is the same as for figures 2 and 3: before the policy, there were no sizeable differences in trends between helmet use and serious accidents in the treatment and comparison groups, and this changed abruptly in 2014.

Figure 4: Helmet Use Soriano and Rest of Uruguay

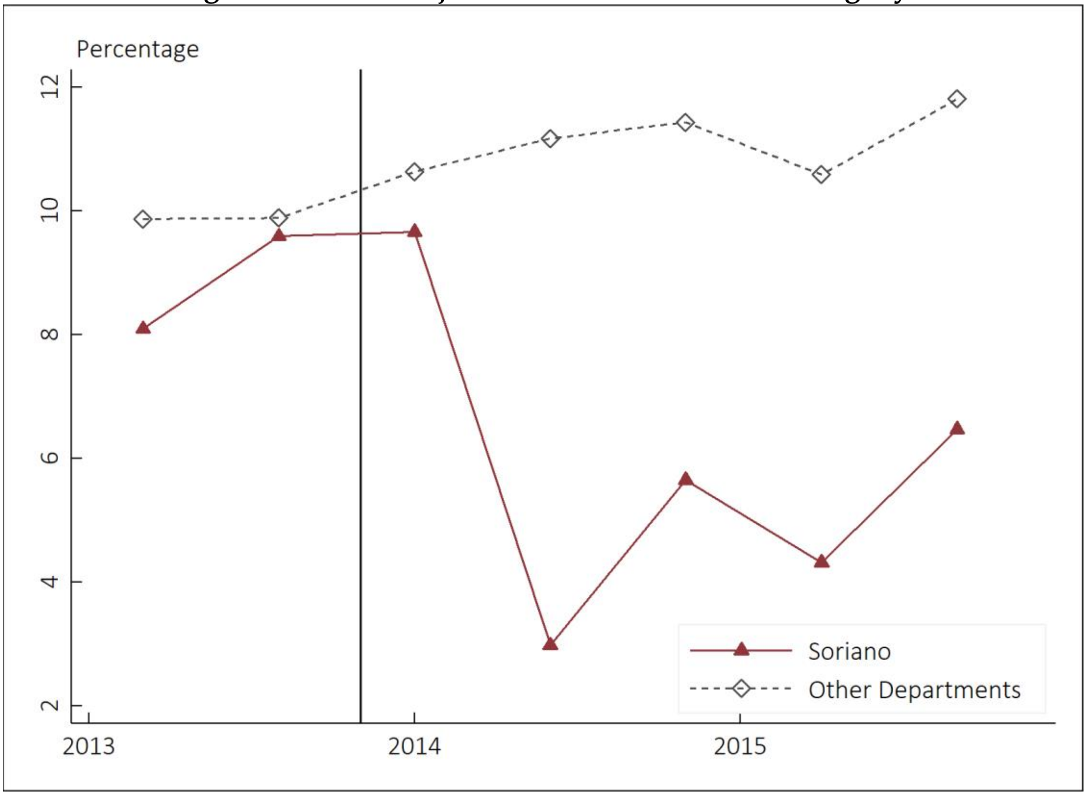


Source: Authors' calculations, based on data from UNASEV.

Note: Helmet usage is measured as the percentage of all reported motorcycle accidents in which the driver was wearing a helmet. Vertical line corresponds to November 2013. Frequency: five-month averages.

17. The total number of accidents in the comparison group is much larger than in Soriano, which results in a much smoother pattern at higher time frequencies.

Figure 5: Serious Injuries Soriano and Rest of Uruguay



Source: Authors' calculations, based on data from UNASEV.

Note: Serious injury or fatality is defined as a percentage of the number of motorcycle accidents in each city. Vertical line corresponds to November 2013. Frequency: five-month averages.

To obtain quantitative estimates of the effect of helmet use enforcement, we follow two different strategies. First, we use our localities panel to obtain difference-in-difference estimates of the effect of the change in enforcement on accident volumes and the types of accident occurring in different locations. For this purpose, we estimate a version of equation 1 in which the variable $Post_t$ is a dummy variable taking a value of one in the months after November 2013. Second, we use our data at the individual level to study the effect of enforcement on helmet use and accident severity. For this purpose, we restrict our sample to motorbike accidents and estimate the following:

$$Y_{it} = \alpha_j + \delta_t + \eta \ Post_t \cdot T_j + \varepsilon_{it} \quad (2)$$

where i is an index for individuals involved in an accident, $Post_t$ takes a value of one in the months after November 2013, and T_j takes a value of one if the accident took place in the department of Soriano. When we use our accident-level data set, the outcome Y_{it} is either a dummy variable taking a value of one if the biker was wearing a helmet, a dummy variable taking a value of one if the outcome from the accident was a serious injury, or a dummy variable taking a value of one if the outcome was a minor injury.

Finally, we can exploit the policy as a source of exogenous variation in helmet use to study the effect of helmet use on accident severity. To do so, we use the policy as an instrument for helmet use, so that equation 2 with a helmet dummy outcome is our first stage, and our second stage is given by

$$Severity_{it} = \alpha_j + \delta_t + \pi \, Helmet_{it} + \varepsilon_{it} \tag{3}$$

The additional assumption in this particular exercise is the exclusion restriction: the change in policy only affected accident severity via its impact on helmet use. Several results in the next section indicate that this may be a reasonable assumption in our context.

3.c. Difference-in-Differences: Results

Difference-in-differences estimates for the effect of the change in enforcement on accident volumes for different vehicles are reported in panel A of table 4. Point estimates are small in absolute value in all columns, at less than 0.01 of a standard deviation of the dependent variable. They are also statistically insignificant at conventional levels. We interpret these findings as evidence that the enforcement of helmet use in Soriano had no impact on total accidents, motorbike accidents, or accidents involving other vehicles.

Results for accident types are reported in panel B of the table. In this case, we compute the share of all accidents corresponding to collisions, falling (for example, from a motorbike), or other causes. We again find no statistically significant effect of increased enforcement on the type of accident taking place.[18] These findings are important because they suggest that changes in perceived risks for motorcyclists, as a result of changes in enforcement, did not have substantial effects on risk taking or observable measures of driver behavior, as predicated by hypotheses of risk compensation by drivers.

18 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.

Table 4: Number and Type of Accidents in all Locations

A) Accidents by Vehicle	(1) Total Accidents	(2) Moto. Accidents	(3) Other Vehicles
Post x Treatm.	-0.034	-0.053	0.018
	(1.722)	(0.625)	(1.155)
Observations	14,796	14,796	14,796
B) Accidents by Cause	**Collision**	**Falling (e.g. from Motorbike)**	**Other**
Post x Treatm.	-0.066	0.059	0.007
	(0.075)	(0.069)	(0.052)
Observations	5,319	5,319	5,319

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 our individual-level data to obtain estimates of the effect of the change in enforcement on helmet use and accident severity. These are reported in table 5, where column 1 accounts for cross-sectional differences between treatment and comparison groups using a treatment dummy variable, and column 2 includes a full set of town dummy variables. In panel A, the coefficients show an increase of roughly 90 percent in helmet use as a result in the change in enforcement. This is in line with the results illustrated in figure 4, indicating that 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 –0.047, showing that the probability that a motorbike accident will result in a serious injury was reduced by approximately 4.7 percent 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 percent in this sample.

Panel C of the table shows our instrumental variables (IV) estimates of the causal effect of helmet use. These roughly coincide with the ratio between the reduced-form coefficients in panel B and the first-stage estimates in panel A. The effect of interest is roughly 5 percent, indicating that helmet use reduces the probability that a motorbike accident results in a serious or fatal injury by about 40 percent. 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 5: Differences-in-Differences Estimates for all Locations

	(1)	(2)
A) First-Stage	Helmet D.	Helmet D.
Post x Treatm.	0.902***	0.887***
	(0.0305)	(0.0373)
B) Reduced-Form	Serious D.	Serious D.
Post x Treatm.	-0.047***	-0.049***
	(0.012)	(0.011)
C) 2SLS Estimates (IV)	Serious D.	Serious D.
Helmet D.	-0.052***	-0.055***
	(0.012)	(0.011)
Observations	60,689	60,689
Vehicle	Motorbike	Motorbike
Town FE	No	Yes

Note: Columns 1 and 2 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. Both columns include month-year effects. Column 2 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 accident in which bikers are involved, or a change in accident severity conditional on accident type. Table 4 showed that the type of motorcycle accident 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 a value of one if an accident results in minor injuries and zero if the driver is unharmed as the dependent variable.[19] Results are reported in table 6. Instrumental variable estimates indicate that 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 6: Minor Injuries and Helmet Use

2SLS Estimates	Minor D.	Minor D.
Helmet use	0.037**	0.060***
	(0.018)	(0.014)
Observations	54,213	54,213
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

Based on the results reported in tables 5 and 6, we conclude that enforcement of the mandatory helmet use law led 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 accident. The fact that there are no discernible changes in the volume and type of accidents suggests that there are no other first-order behavioral responses to the law, at least in terms of driver behavior.[20] Therefore, we find that helmet use reduces accident severity and detect no evidence in support of the type of risk-compensating behavior associated with the Peltzman hypothesis.

3.d. Heterogeneous Effects

In this section, we study whether the policy change had different effects for different types of accident or victim. That is, we study whether the results from panel C in table 5 are heterogeneous across groups by reporting treatment effects for five subsamples, defined by age group, sex, being a driver (versus passenger), whether the accident occurred in an urban area, and whether it occurred at night.

19. Including serious injuries among the zeroes does not change the qualitative results of the exercise.
20. Using a subsample of the UNASEV data set, 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 on request).

We estimate equation 3 for each group by splitting the sample according to each characteristic and running a two-stage least squares (IV) regression where helmet use is instrumented with a dummy variable for treated localities in the post-treatment period. Table 7 shows the results for eleven different subsamples.[21]

The first stage results—not reported here, but available in the replication files—show that take-up is fairly uniform across subsamples, at around 0.9 for males and females, different age groups, accidents in urban or rural areas, and accidents during the night or during the day. The only subsample that has a lower take-up (0.6) are passengers (as opposed to drivers). In light of the findings in Grimm and Treibich (2016), these results indicate that the population induced to wear a helmet by enforcement of the corresponding law may differ substantially from the population of drivers who decide to wear a helmet spontaneously.

The broad picture of results from table 7 is that the benefits of helmet use on serious injuries are higher for the high-risk groups or accident types. When we split the sample by the age of the driver (columns 1 to 3) we find larger effects of helmet use on the young and old, and no effect at all on the middle-aged (between twenty-five and fifty-five years old). This may result from differences in risk attitudes and vulnerability by age, with young individuals being less risk averse (as shown in Dohmen and others, 2017) and relatively older drivers being more physically vulnerable. We find larger coefficients (in absolute value) for males than females and for accidents occurring the night. Globally, we interpret these findings as suggesting that helmets are particularly important for subpopulations that are more at risk of injury.

21. Qualitative results are similar if, instead of splitting the sample, we use a less flexible model with an interaction of the treatment variable and each observable characteristic.

Table 7. Heterogeneous Treatment Effects

	(1)	(2)	(3)	(4)	(5)	(6)
Explanatory variable	*Youth*	*Adults*	*Seniors*	*Males*	*Females*	*Drivers*
Helmet use	–0.081***	–0.007	–0.127***	–0.063***	–0.038***	–0.061***
	(0.001)	(0.018)	(0.014)	(0.015)	(0.010)	(0.009)
No. observations	24,544	25,996	6,770	41,991	18,535	51,437
	(7)	*(8)*	*(9)*	*(10)*	*(11)*	
	Passengers	*Urban*	*Rural*	*Night*	*Day*	
Helmet use	–0.017	–0.058***	0.042	–0.071***	–0.046***	
	(0.040)	(0.010)	(0.040)	(0.013)	(0.017)	
No. observations	9,160	51,830	8,859	16,826	43,863	

Note: The table shows two-stage least squares estimates, with locality fixed effects. Helmet usage is instrumented with a dummy equal to one for accidents in the treated localities in the post-treatment period. The dependent variable in all specifications is a dummy taking a value of one if the accident victim experienced a serious or fatal injury. In columns 1 to 3, we split the sample by age group: youth: under twenty-five years old; adults: from twenty-five to forty-nine years old; and seniors: fifty years old or older. In columns 4 and 5, we split the sample by sex. In columns 8 and 9, we split the sample by the type of locality where the accident occurred. In columns 10 and 11, the night or day variable is set for each day, based on the time of sunset. All columns include a full set of time fixed effects. The sample comprises all registered motorcycle accidents. Standard errors clustered at the locality level are in parentheses. *p < .1; **p < .05; ***p < .01.

4. Robustness Checks

In this section, we evaluate the robustness of the qualitative findings reported above by conducting five sets of 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; (b) we provide two falsification tests—one using Cerro Largo instead of Soriano as the treatment group and the other focusing on car accidents—for our main results; (c) we obtain alternative estimates using a triple interaction model accounting for differences in accident rates across all vehicles; (d) we exclude either Cerro Largo or Montevideo from the comparison group; and finally (e) we use a synthetic control for Soriano (see also appendix B).

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, and accident outcomes. Yet the choice to cluster at the level of localities has two issues. First, our treatment varies at the department level, not at the locality level. Since Bertrand, Duflo, and Mullainathan (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 nineteen departments in our sample.[22] Second, it is likely that our outcomes feature non-negligible spatial autocorrelation, so residuals in neighboring clusters will typically be correlated, violating the key assumption invoked to justify clustering at that level.

To address potential concerns with inference in our main tables, we report standard errors obtained using the spatial heteroskedasticity and autocorrelation consistent (HAC) robust standard errors proposed by 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 spatial dependence, analogous to an adjustment for heteroskedasticity and autocorrelation. Results for reduced-form difference-in-differences estimates on helmet use, the probability of an accident resulting in a serious injury, and the probability of an accident resulting in a minor injury are reported in table A4 in the appendix. We use a spatial kernel of 100 km in radius, so that the area of the uniform kernel is almost twice the size of the largest department in the country.[23] The main conclusions of our analysis are maintained with this inference method.

4.b Falsification Tests

We can use our accidents data to build two suitable placebos in order to validate our methodology. First, we can use the department of Cerro Largo as a placebo to test whether there were changes in either helmet use or accident severity in this department coinciding with the introduction of the Misdemeanors Act in 2013. For this purpose, we reproduce the equivalent of our reduced-form estimates using this department as the treatment and all other departments—excluding Soriano—as the comparison group. Results for this exercise are presented in table A5 in the appendix. As expected,

22. A growing literature proposes methods to conduct inference in the difference-in-differences setting when the number of clusters is small. However, these methods generally require having a large number of treated clusters, which is not the case in our paper (see MacKinnon and Webb, 2020).
23. The adjustment is carried out using the reg2hdfe spatial Stata command by Thiemo 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 find no evidence of a significant effect of the interaction term on serious accidents. While the local governments of Cerro Largo and Soriano both refused to enforce helmet use by motorcyclists before late 2013, it is only in Soriano—which changed enforcement in that period—that we observe a substantial change in accident severity.

Second, we can use data on automobile accidents to study whether changes the severity of these accidents responded to the change in policy in Soriano. We can only interpret this as a placebo if we assume that the change in helmet use does not affect the risks associated with car accidents. This assumption is perhaps reasonable given the results on accident volumes in table 4, although accident volumes might not sufficiently capture all of the possible changes in driver behavior or risks. The results indicate that the change in helmet use enforcement was not associated with changes in the severity of automobile accidents (see table A5).

4.c. Triple Differences-Model

Our baseline estimates are obtained by focusing specifically on motorbike accidents. This is motivated by our interested in the effect of helmet use on the health outcomes of the motorcyclist involved in the accident itself. However, we can use a larger sample including all accidents to obtain similar estimates in a triple-interaction model. The advantage of this alternative specification is that it can help us account for potential time-varying confounders that differentially affect all accidents in the treatment and comparison groups, such as broader trends in road behavior or idiosyncratic changes in the intensity of all forms of road regulation. To account for overall shifts in accidents across vehicle types when estimating our effect of interest, we estimate the following equation:

$$Y_{it} = \alpha_j + \delta_t + \beta\, Post_t \cdot T_i \cdot Moto_{it} + \gamma_1 Post_t \cdot Moto_{it} + \gamma_2 Moto_{it} \cdot T_i + \gamma_3 T_i \cdot Post_t + \varepsilon_{it} \qquad (4)$$

where T_i and $Post_t$ are defined as above and Y_{it} is either a dummy variable that takes a value of one if the motorcyclist in accident i was wearing a helmet or a dummy variable that takes a value of one if the motorcyclist suffered a minor accident. The variable $Moto_{it}$ takes a value of one if the victim involved in the accident is a motorcyclist. As in our baseline difference-in-differences specification, we also use variation in enforcement (captured by the triple interaction term) as an instrument for helmet use to obtain an estimate of the effect of helmet use on accident severity. The innovation relative to the specification in equation 2 comes in the form of the interaction term $T_i Post_t$, which accounts for changes over time in accident severity of all vehicles between treatment and comparison groups.

Estimates for the coefficient on the triple interaction term for the first-stage, reduced-form, and IV specifications are reported in table A6 in the appendix. Results are broadly consistent with those reported in table 5 using motorcyclists only. We interpret this as evidence that our baseline results are not driven by factors unrelated to helmet use enforcement affecting all vehicle accidents.

4.d. Alternative Comparison Groups

In this section, we test whether our results are robust to specific choices regarding the composition of the comparison group. Our baseline estimates use motorbike accidents in all recorded locations. However, certain locations may be ill-suited to act as controls. Cerro Largo, for example, is different from other departments because it did not enforce helmet use throughout the whole period. More importantly, the capital city of Montevideo is the largest urban area in the country, is characterized by a relatively more modest use of motorcycles, and has a high-density environment that is quite distinct from other localities in the country (see table 2).

Table A7 in the appendix presents IV estimates of the effect of helmet use on the probability of minor and serious injuries after excluding accidents taking place in Cerro Largo and Montevideo. Comparing these estimates with those reported in tables 4 and 5 indicates that these sample restrictions have little impact on our findings.

We can alternatively restrict our sample to accidents taking place in Mercedes and Melo only, so as to provide quantitative estimates of the effects illustrated in figures 2 and 3. Results for this exercise are qualitatively and quantitatively in line with those reported earlier (see table A8 in the appendix).

4.e. Synthetic Control

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

Appendix B discusses the implementation and results of applying this method using aggregate department-level data. Our results are qualitatively in line with our findings in the difference-in-differences analysis reported earlier. Soriano experienced a sustained reduction in accumulated serious motorbike accidents per capita after the fourth quarter of 2013. This reflects a change in accident severity and not accident volumes, which remained relatively stable throughout the period.[24]

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 the administrative costs of enforcement paid by the relevant agencies and the nuisance costs of wearing a helmet for motorcyclists. The latter is particularly hard to estimate, but we can calculate the magnitude of these costs that would reverse the change in benefits.[25] The outcome of the cost-benefit analysis can then be obtained relative to this benchmark.

The health benefits of the change in enforcement derive from a reduction in the volume of serious accidents and deaths. Paolillo and others (2016) document that roughly one and a half out of ten serious traffic accidents lead to a fatality. They also estimate the 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.[26] 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 2 of table 5 is 4.86 percent. The average number of yearly motorcycle accidents in Soriano is 610. Hence, the policy leads to a reduction of roughly twenty-nine

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24. See appendix B for details.

25. 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.

26. In the literature, there is considerable uncertainty about the value of life, depending on the method used, the age of the victim, and the country where it is estimated. According to U.S. EPA (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 calculating the benefits of preventing traffic fatalities is USD 9.1 million (in 2012 dollars). Considering that Uruguayan GDP per capita is 27 percent of U.S. GDP per capita, we employ a conservative value of USD 2,346,000 for our estimates.

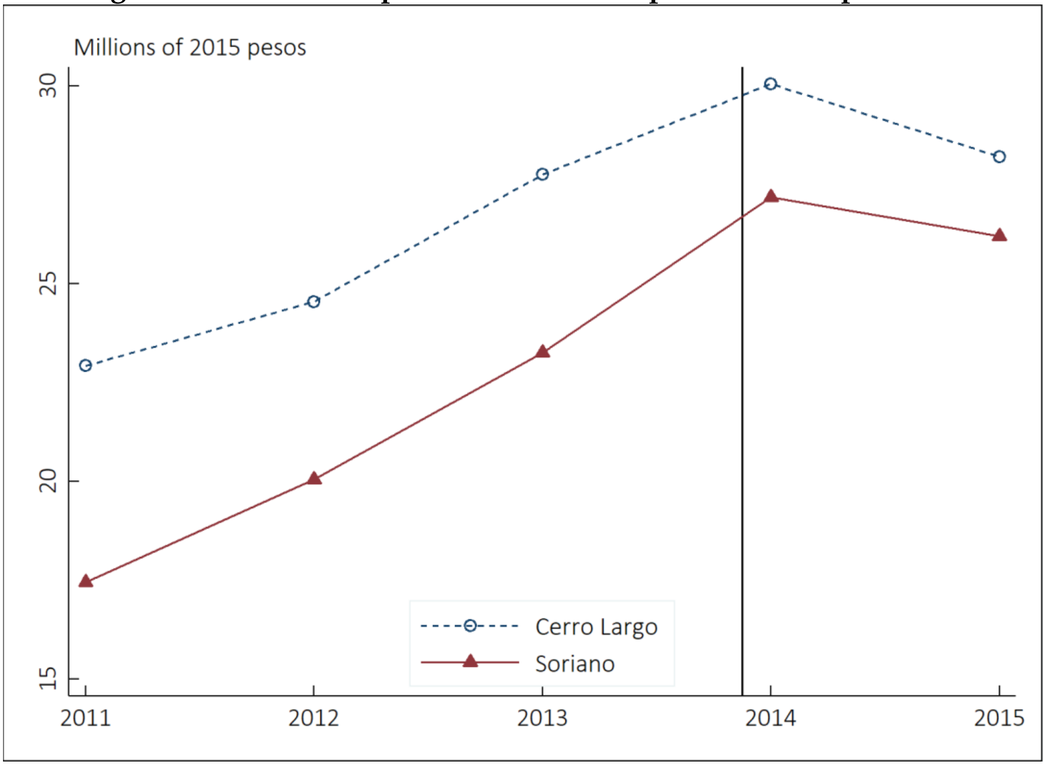
serious or fatal accidents per year. Using this number, we can compute the estimated health benefits from the policy as $29 \times 0.15 \times 2{,}346{,}000 + 29 \times 0.85 \times 7{,}437$. This yields a figure of USD 10,389,727 per year in benefits arising from reduced hospitalization costs and deaths alone. Assuming a 5 percent discount rate and a thirty-year time horizon (as in Dee, 2009), the present value of health benefits would be on the order of USD 160 million. This corresponds to USD 6,053 per motorcyclist.

Other health effects, such as the psychological costs and permanent disability resulting from serious accidents or the reduced work hours for hospitalized patients, are likely to be substantial. Therefore, we consider these figures to be an underestimate of total health benefits.[27]

On the cost side, public enforcement of the helmet law requires the use of traffic inspectors to detect and sanction violators. How much of Soriano's public resources were devoted to these tasks? Figure 6 reports the personnel expenses of the Soriano and Cerro Largo Transit Departments. The parallel trends observed before the enforcement of the law do not change afterward. 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 to enforce other transit rules (such as speed limits and traffic lights), and after the law was enforced, the same inspectors just added another complementary task—enforcement of the helmet law—to their daily activity. Information campaigns on helmet use were included in 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.

27. As discussed earlier, 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, so our estimate of the net health benefits would still be a lower bound of total health costs, even after accounting for the increased number of minor injuries.

Figure 6. Personnel Expense of the Municipal Transit Department



Source: Observatorio Territorio Uruguay (OPP).
Note: Vertical line corresponds to November 2013.

With regard to the nuisance costs of helmet use, there were 26,435 registered motorcycles in Soriano in 2013. The nuisance costs for registered motorcycles resulting from the policy should be proportional to this figure, scaled by the change in helmet use, which is 89 percent (see table 5). Our health benefits estimate is USD 10,389,727 per year, so the policy would have a positive net benefit for yearly nuisance costs per registered motorcycle under USD 442. 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 in 2013 can be explained on three grounds: large nuisance costs, moral hazard, or biased risk perception. First, if the nuisance costs of wearing a helmet—plus the pecuniary costs of owning one—are well above USD 442 a year, then the laissez-fair outcome is that rational motorcyclists will choose not to wear a helmet. Second, motorcyclists may not internalize the full cost of serious injuries because of the pervasiveness of health and disability insurance. If this is the case, even if the costs of helmet use are below USD 442 per year, it may still be privately optimal for drivers not to 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. They are not universal, however, and enforcement is often 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 led to a substantial reduction of roughly 5 percentage points in the rate of serious or fatal injuries. Given that the national base rate stands at roughly 11 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 accident 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 in the form of risk-compensating actions—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 hospitalization costs and the value of statistical life, we calculate an approximate measure of the health benefits resulting from the change in enforcement. Given that the direct enforcement costs for the involved traffic control agencies were largely unaffected by the policy, the main costs of increased helmet use are associated with the potential nuisance for riders. However, the nuisance costs would have to be substantial to offset the policy's health benefits.

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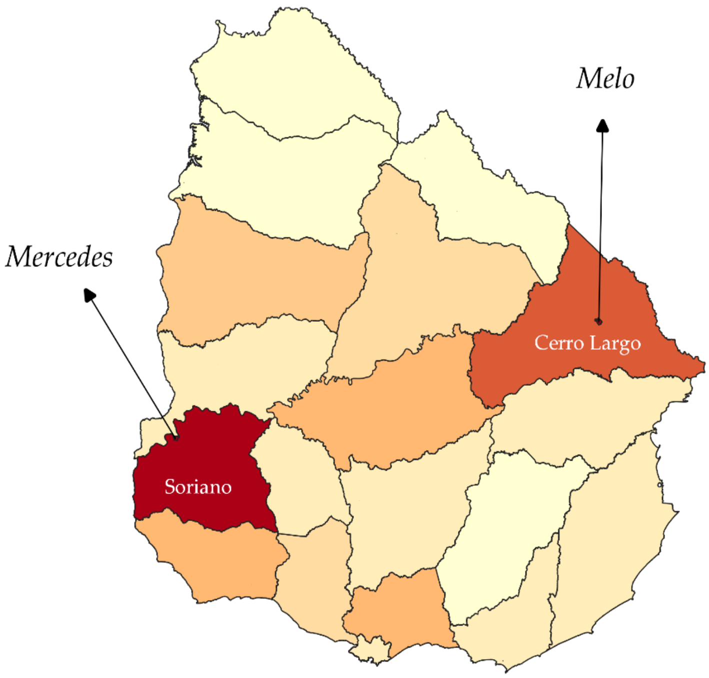
Appendix

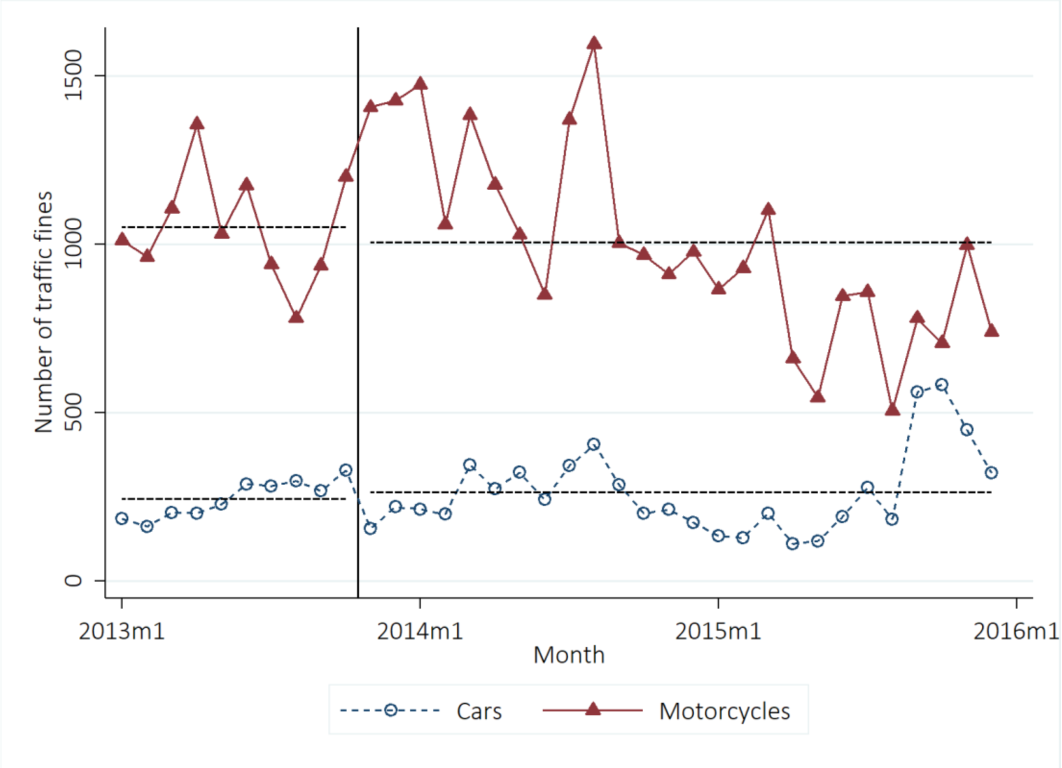
Figure A1 – Helmet Use by Department (Uruguay)



	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.

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

Country	Fatality rate	GDP per capita	Year	Country	Fatality rate	GDP per capita	Year
Albania	2.18	10,571	2013	Lesotho	0.33	2,723	2013
Angola	0.42	6,970	2013	Libya	1.10	22,322	2013
Australia	0.97	45,794	2013	Lithuania	0.80	26,661	2013
Austria	1.15	47,922	2013	Luxembourg	1.51	95,591	2013
Azerbaijan	0.05	17,172	2012	Macedonia	1.01	12,656	2013
Bahamas	2.38	29,878	2013	Malawi	0.45	1,099	2012
Bahrain	0.28	44,168	2013	Malaysia	14.90	24,034	2013
Bangladesh	1.46	2,935	2012	Maldives	0.50	13,607	2013
Barbados	1.12	16,980	2013	Mali	4.85	1,856	2013
Belgium	1.06	43,520	2013	Malta	1.44	31,069	2013
Belize	5.15	8,072	2013	Marshall Islands	3.79	3,851	2013
Benin	15.92	2,002	2012	Mauritius	3.73	18,244	2013
Bhutan	0.25	7,317	2013	Mexico	0.74	16,848	2012
Bolivia	2.36	6,303	2013	Moldova	1.46	4,700	2013
Bosnia & Herzegovina	1.71	10,826	2013	Mongolia	4.02	11,094	2013
Brazil	6.49	15,971	2012	Montenegro	1.13	14,870	2013
Bulgaria	0.72	16,571	2013	Morocco	4.31	7,240	2013
Cambodia	12.35	3,068	2013	Namibia	0.14	9,578	2012
Canada	0.51	44,101	2012	Netherlands	0.46	48,666	2013
Chile	0.87	22,579	2013	New Zealand	0.94	36,220	2013
China	5.16	12,368	2013	Nicaragua	4.07	4,780	2013
Colombia	7.59	12,634	2013	Norway	0.48	67,056	2013
Costa Rica	4.06	14,525	2013	Oman	0.57	43,387	2013
Côte d'Ivoire	4.93	2,980	2013	Paraguay	11.78	10,950	2013
Croatia	1.59	21,807	2013	Peru	0.14	11,829	2012
Cyprus	1.76	30,621	2013	Philippines	5.53	6,527	2013
Czechia	0.68	30,486	2013	Poland	0.97	24,719	2013
Denmark	0.47	46,727	2013	Portugal	1.62	27,900	2013
Dominican Republic	18.76	12,322	2013	Romania	0.44	19,797	2013
Ecuador	1.39	11,037	2012	Russia	0.81	26,240	2013
Egypt	0.07	10,157	2013	San Marino	3.10	59,764	2013
El Salvador	2.36	7,027	2013	São Tomé & Príncipe	10.30	2,883	2013
Estonia	0.42	27,496	2013	Serbia	0.88	13,760	2013
Finland	0.53	41,294	2013	Singapore	1.66	81,648	2013
France	1.20	39,524	2013	Slovakia	0.56	27,898	2013
Georgia	0.42	8,542	2013	Slovenia	1.08	29,797	2013
Germany	0.84	45,232	2013	South Korea	1.92	32,616	2013
Ghana	3.61	3,940	2012	Spain	0.78	32,604	2013
Greece	3.00	26,098	2012	Sri Lanka	7.32	10,596	2013
Guatemala	4.73	7,249	2013	Suriname	7.99	15,957	2013
Guyana	3.42	6,930	2013	Sweden	0.47	45,673	2013
Honduras	1.32	4,323	2013	Switzerland	0.68	60,109	2013
Hungary	1.07	24,463	2013	Tanzania	6.95	2,397	2013
Iceland	0.31	42,821	2013	Thailand	25.89	15,287	2012
India	5.50	5,251	2013	Trinidad & Tobago	0.36	32,500	2013
Iran	6.91	16,955		Tunisia	4.99	10,948	2013
Ireland	0.56	48,067	2013	Turkey	0.38	22,311	2013
Israel	0.48	34,129	2013	Uganda	8.38	1,667	2013
Italy	1.58	36,131	2013	United Arab Emirates	0.29	63,839	2013
Jamaica	2.33	8,309	2013	United Kingdom	0.55	39,308	2013
Japan	0.82	38,974	2013	United States	1.59	52,782	2012
Kazakhstan	0.54	23,773	2012	Uruguay	8.80	19,943	2013
Kenya	1.41	2,776	2013	Zambia	0.52	3,701	2013
Laos	10.00	5,294	2013	Zimbabwe	1.64	2,287	2013
Latvia	0.75	22,677	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	12,682	13,041
Total number of automobile or van	4,943	6,196
Number of motorcycle or moped per capita	0.302	0.252
Number of automobile or van per capita	0.118	0.120

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	(0.23)	149,873	0.12	(0.32)	61,489
Slight injury	0.38	(0.48)	149,873	0.66	(0.47)	61,489
Unharmed	0.57	(0.50)	149,873	0.22	(0.42)	61,489
Male	0.73	(0.44)	149,419	0.70	(0.46)	61,326
Age	36.98	(16.18)	141,560	30.76	(14.12)	58,080
At night	0.26	(0.44)	149,873	0.28	(0.45)	61,489

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). "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: Spatial HAC Standard Errors

	(1) Helmet D.	(2) Serious D.	(3) Minor D.
Post x Treatm.	0.887***	-0.049***	0.053**
	(0.024)	(0.016)	(0.027)
Observations	60,689	60,689	54,213

Note: 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. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A5: Placebo/Falsification Tests

	(1)	(2)
Panel A: Cerro Largo	Serious D.	Serious D.
Post x Treatm.	0.011	0.013
	(0.010)	(0.010)
Observations	60,689	60,689
Vehicle	Motorbike	Motorbike
Location FE	No	Yes
Panel B: Automobiles	Serious D.	Serious D.
Post x Treatm.	-0.003	-0.003
	(0.003)	(0.003)
Observations	72,181	72,181
Vehicle	Car	Car
Location FE	No	Yes

Note: The dependent variable in all specifications is a dummy taking value 1 if the accident resulted in a serious injury and 0 if the driver was unharmed. In Panel A, we use a differences-in-differences specification where the treatment dummy takes value 1 in the department of Cerro Largo. In panel B, the sample is restricted to victims of car accidents only and the treatment dummy takes value 1 for accidents in Soriano. All columns include a full set of time effects. Standard errors clustered at the locality level in parentheses. *p<.1; **p<.05; ***p<.01

Table A6: Triple Interaction Model

	(1)	(2)
A) First-Stage	Helmet D.	Helmet D.
Post x Treatm. x Moto	0.883***	0.884***
	(0.030)	(0.028)
R-squared	0.628	0.662
B) Reduced-Form	Serious D.	Serious D.
Post x Treatm. x Moto	-0.048***	-0.047***
	(0.009)	(0.011)
C) TSLS Estimates (IV)	Serious D.	Serious D.
Helmet D.	-0.055***	-0.053***
	(0.009)	(0.012)
Observations	149,073	149,073
Vehicle	All	All
Town FE	No	Yes

Note: Triple-interaction term coefficients reported in panels A and V (see equation 4 in the text). In panel A, the dependent variable is a dummy taking value 1 if the accident victim is a motorcyclist wearing a helmet. In panel B, the dependent variable takes value 1 if the accident resulted in a serious injury. In panel C, we report IV estimates where the triple interaction term is used to instrument for helmet use. All columns include a full set of time effects. Standard errors clustered at the locality level in parentheses. *p<.1; **p<.05; ***p<.01

Table A7: Comparison Group Sample Restrictions

Panel A: Excluding Cerro Largo	(1) Minor D.	(2) Minor D.	(3) Serious D.	(4) Serious D.
Helmet D.	0.067***	0.087***	-0.052***	-0.054***
	(0.022)	(0.010)	(0.012)	(0.011)
Observations	58,343	58,343	58,343	58,343
Panel B: Excluding Montevideo				
Helmet D.	0.063***	0.083***	-0.053***	-0.055***
	(0.023)	(0.013)	(0.013)	(0.012)
Observations	42,511	42,511	42,511	42,511
Town FE	No	Yes	No	Yes

Note: All coefficients correspond to 2SLS estimates of the effect of helmet use on a measure of accident severity. The dependent variable in columns 1 and 2 is a dummy taking value 1 if the accident resulted in a minor injury and 0 if the victim was unharmed. The dependent variable in columns 3 and 4 corresponds to a dummy taking value 1 if the accident victim suffered a major injury. Panel A estimates obtained after removing observations from Cerro Largo from the comparison group. Panel B obtained after removing observations from Montevideo from the comparison group. All specifications include time effects. Standard errors clustered at the locality level in parentheses. *p<.1; **p<.05; ***p<.01

Table A8: Motorbike Accidents in Mercedes and Melo

	(1) Helmet D	(2) Serious D.	(3) Minor D.
Mercedes x Post	0.879***	-0.073***	0.059
	(0.019)	(0.022)	(0.041)
Observations	3,354	3,354	3,124
R-squared	0.768	0.021	0.025

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 Column 1, 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 Columns 2, the dependent variable is a dummy taking value 1 if the accident victim experienced a serious or fatal injury. In Column 3 the dependent variable is a dummy taking the value 1 if the accident victim experienced minor injuries and 0 if the driver was unharmed. Estimates from reduced-form regressions, as discussed in the text, including month-year effects. Sample restricted to motorbike accidents in Mercedes and Melo. Robust standard errors in parentheses. *p<.1; **p<.05; ***p<.01.

Appendix B – Synthetic Control

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).

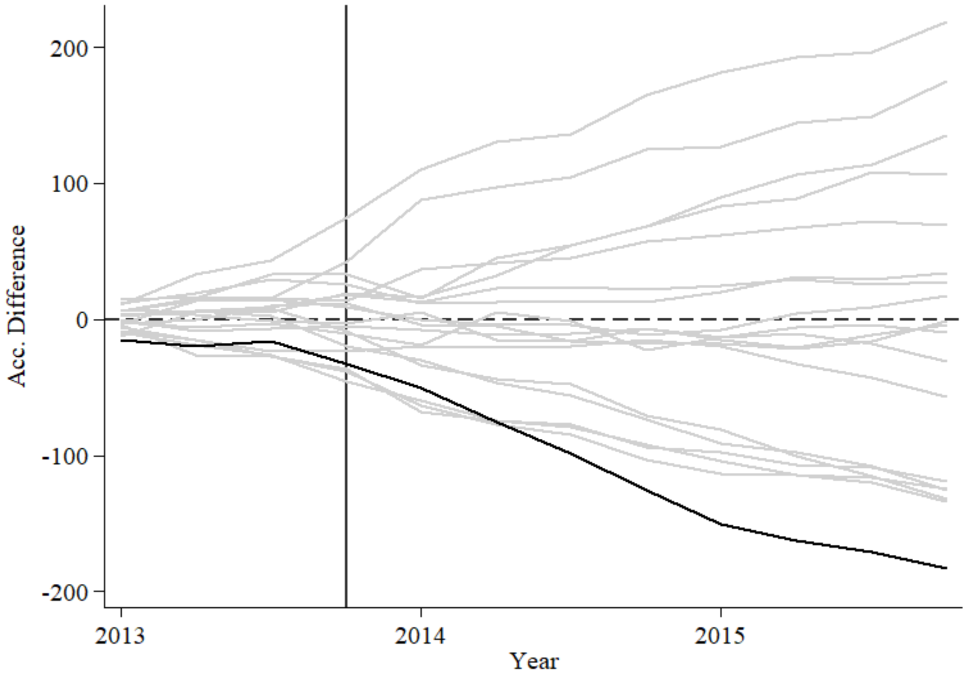
We construct the accumulated difference in serious accidents between the department of Soriano and our synthetic Soriano control. This is represented as the black solid line in Figure B1. We observe that in the months before November 2013, the line is flat. Note that only the first quarter is used to 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 B1 as solid grey lines.[28] 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

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

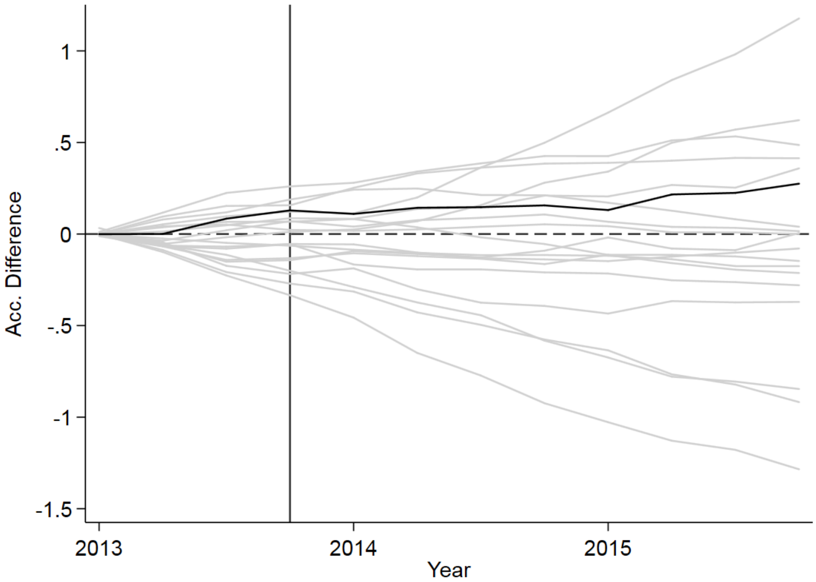
section 3.c. For this purpose, we modify the analysis above and build a synthetic department in order to match the number of motorbike accidents per capita before the policy was put in place in Soriano. Results from this exercise are reported in Figure B2 in this Appendix. The solid line represents the 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 4 – that the change in enforcement had no discernible effect on the number of motorbike accidents.

Figure B.1 – 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.

Figure B.2 – 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.