HEALTH ECONOMICS
Health Econ. **24**: 26–40 (2015)
Published online 30 September 2013 in Wiley Online Library (wileyonlinelibrary.com). DOI: 10.1002/hec.2997

EFFECTS OF BICYCLE HELMET LAWS ON CHILDREN'S INJURIES

SARA MARKOWITZ[a,*] and PINKA CHATTERJI[b]

[a]*Department of Economics, Emory University, Altanta, Georgia, USA and NBER*
[b]*Department of Economics, University of Albany, Albany, New York, USA and NBER*

ABSTRACT

In recent years, many states and localities in the USA have enacted bicycle helmet laws. We estimate the effects of these laws on injuries requiring emergency department treatment. Using hospital-level panel data and triple difference models, we find helmet laws are associated with reductions in bicycle-related head injuries among children. However, laws also are associated with decreases in non-head cycling injuries, as well as increases in head injuries from other wheeled sports. Thus, the observed reduction in bicycle-related head injuries may be due to reductions in bicycle riding induced by the laws.

Received 22 May 2013; Revised 6 August 2013; Accepted 20 August 2013

KEY WORDS: injuries; helmet laws; cycling; children

1. INTRODUCTION

Cycling is one of the most popular recreational sports among children and adults. In 2010, there were about 40 million cyclists in the United States, 37 percent of whom were aged 7-17 years old (NSGA, 2012). Cycling, however, is an activity that can lead to potentially serious injuries and death, particularly among children. In 2009, bicycle accidents resulted in 782 deaths nationwide, and over 518,000 emergency room visits (CDC, 2012a, 2012b). Children aged 19 and under account for 57 percent of all bicycle injuries treated in emergency rooms and 15 percent of deaths. In fact, bicycle accidents are a leading cause of accidental death among children (CDC, 2012b).

Deaths and serious injuries from bicycle accidents frequently result from trauma to the head, and children are more likely than any other age group to die from a bicycle-related head injury (Safe Kids USA, 2012). Injured children also are more than twice as likely as injured adults to suffer from a head or facial injury (Rodgers, 2001). Based on 1994-2001 admissions data from the National Pediatric Trauma Registry (NPTR), the National Safe Kids Campaign estimates that almost half of children ages 14 and under who were hospitalized for a bicycle-related accident had a traumatic brain injury. Most (about 75 percent) of these hospitalized children were males (National Safe Kids Campaign, 2002).

Helmet usage reduces the probability of head trauma, but less than half of adults, teenagers, and pre-teen children report that they use helmets regularly (National Safe Kids Campaign, 2002; Thompson *et al*., 1989; CPSC, 1999; Carpenter and Stehr, 2011).[1] For this reason, since 1987, 21 states, the District of Columbia, and over a hundred localities have implemented mandatory helmet laws targeted at children of various age groups. Community and state-level studies offer suggestive evidence that helmet laws are effective in

*Correspondence to: Emory University, Economics, 1602 Fishburne Drive, Atlanta, Georgia, USA, 30322. E-mail: sara.markowitz@emory.edu

[1]A variety of analyses have shown that helmets are effective in reducing the risk of head and facial injury, with risk reductions ranging from 47 to 88 percent (Thompson *et al.*, 1989; Li *et al.*, 1995; Thompson *et al.*, 1996; Attewell *et al.*, 2001). There is, however, a recent debate about the efficacy of soft-shell helmets in protecting against brain injury (Curnow, 2005; Hagel and Pless, 2006).

increasing helmet usage.[2] However, there exists little information based on national data whether these state and local helmet regulations actually decrease head injuries from bicycle-related accidents.

To the best of our knowledge, only two studies exist based on national U.S. data that address the effects of bicycle helmet laws on fatalities from bicycle accidents, and no national study in the U.S. has examined the effects of helmet laws on injuries. Grant and Rutner (2004) examine the direct impact of helmet laws on juvenile fatalities that result from motor vehicle/bicycle accidents. They use 1975 to 2000 data from the Fatality Analysis Reporting System (FARS) and estimate models that include fixed effects to account for unobserved, time-invariant differences across states that might affect helmet legislation and fatalities from bicycle accidents. The authors find that a state-level helmet law is associated with a 15 percent reduction in fatalities among juveniles. Carpenter and Stehr (2011) update the fatality analysis of Grant and Rutner (2004) and analyze FARS fatality data spanning from 1991 to 2005. They find the laws are associated with a 19 percent reduction in child fatalities. These authors also explore the mechanism through which the laws reduce fatalities and find evidence that the laws are associated with increases in helmet usage but also reduced bicycle riding.

While the studies by Grant and Rutner (2004) and Carpenter and Stehr (2011) account for national and state trends that may confound their estimates, the results still must be interpreted carefully. First, the fatality results only pertain to deaths resulting from an accident on a public roadway. One important contribution of our paper is we examine the effect of helmet laws on a different outcome--bicycle-related injuries that require emergency room treatment. These visits are costly, and they are far more common than road-based fatalities. For example, in 2009, there were only 85 bicycle-related deaths nationwide reported in FARS for children under the age of 16, versus over 238,000 non-fatal bicyle-related injuries in emergency rooms for the same age group (NHTSA, 2009; CDC, 2012b). Based on the 2003 MEPS, the average payment for an emergency department visit for a patient under 18 years old was $423 (Machlin, 2006), and among all recreational sports, bicycle riding is the leading cause of pediatric emergency room visits (CDC 1995). Some of these emergency department visits lead to hospitalization, and the average charge associated with a pediatric hospitalization for a bicycle-related injury in the U.S. is estimated to be $18,654 (Shah *et al*., 2007). Given these costs, it is critical that we understand how helmet laws may be associated with such visits.

Our paper also makes a contribution over existing literature because our methods account for differences in the ages targeted by laws. Prior studies ignore such differences. As we discuss below, the age groups targeted by helmet laws vary widely across the country and across time, with states having varied age limits including ages 5, 12, 14, and 17. The prior two studies examine fatalities for children under 16, with the result that not all children represented in their treatment group are affected by the laws and that some children in the control group are affected by the laws. We take care to design our study so that the treatment and control groups are well defined and not overlapping. (Further below, we describe the difference-in-differences and difference-in-difference-in-differences models used to evaluate the laws.) Like the previous literature, we also take care to account for existing trends among all riders that may confound our estimates. We test for possible spillover effects into other age groups not covered by the laws, and we look for direct and spillover effects of local as well as state helmet laws.

Finally, we also further the literature by considering the effects of helmet laws on injuries related to other sports in which a helmet may be worn. Some of the state helmet laws explicitly include other wheeled sports such as roller skating and scooter riding. It is also possible that the bicycle helmet laws create a norm of helmet wearing for other sports, such as skiing and ice skating, even when the laws do not specifically target those sports. To address this possibility, we generate injury rates by age for certain winter and wheeled sports, and examine the effects of the different types of helmet laws on injuries related to these sports. Bicycle helmet laws may have important spillover effects on injuries related to other sports, and this study is the first to examine such effects.

[2]Puder *et al*. (1999), for example, examine helmet usage in three counties with helmet laws targeted at different age groups (all ages, under 14 years old and under 12 years old). Compared to the county that mandated helmet use for all ages, the prevalence of non-use was 9 percent higher in the under-14 county and 28 percent higher in the under-12 county. Borglund *et al*. (1999) analyze helmet usage among 7 to 12 year old children (N = 154) before and after the passage of a state-level mandatory helmet law. They also find that helmet usage increased from 6 percent to 21 percent of children admitted to a trauma center for any bicycle-related injuries. It is not clear, however, whether increased helmet usage resulted from the law, since a public education campaign was introduced in the year before the law was enacted.

DOI: 10.1002/hec

2. DATA

2.1. Injury data

Data on injuries come from the National Electronic Injury Surveillance System (NEISS), which we obtained for the years 1991 to 2008. The NEISS is a data collection effort sponsored by the Consumer Product Safety Commission (CPSC) and is designed to gather information on consumer product-related injuries from the emergency departments of hospitals across the United States. These data are patient-level data on accidents and injuries involving any consumer product. A consumer product is defined as any article used by consumers in or around a home, school, or recreational area (US CPSC 2013).

NEISS hospitals are representative of all U.S. general hospitals with emergency departments. In 1997, a strata was added to include children's hospitals. During a survey redesign in 1997, a resampling method was used that maximized the probability of retaining hospitals from the previous sample. In our final data set, we observe injury data from 141 hospitals located in 42 states. Hospitals may enter and exit the sample; however, 95 hospitals (67 percent) are in the data for at least 10 years, and 50 (35 percent) are in for all eighteen years of our sample period. The remaining hospitals are in the sample for an average of 6 years and of these, when a hospital is in a state with a helmet law, half are in long enough to observe injuries both before and after the passage of the helmet law.

The NEISS provides comprehensive details on each consumer product-related injury. This information comes from the patient's report given to the emergency department clerk, nurse, or physician, and is entered into the patient's medical record. At the end of each day, a trained NEISS coordinator at each participating hospital reviews the day's medical records and determines which, if any, injuries from that day involve a consumer product that is listed in the NEISS Coding Manual. There are approximately 900 product codes listed. The NEISS coordinator is instructed to choose the most specific product code possible, and to abstract information from the medial record for the NEISS database. The abstracted data include the victim's age, injury diagnosis, body parts affected, type of consumer product associated with the injury (e.g. bicycle, skateboard), and a brief narrative describing the cause of injury (US CPSC, 2000).

From this information, we generate age-specific bicycle-related injury counts for each hospital in each year. Previous research indicates that helmet usage has the potential to prevent injuries to the head, brain, and scalp (Thompson *et al.*, 1996). Based on Thompson *et al.*, to best represent injuries that are potentially preventable by use of a bicycle helmet, we count only the injuries in which the most seriously hurt body part is the head, ear, or all parts of the body (at least 25 percent or more). This captures both internal head injuries (e.g., concussions) as well as external head injuries (e.g., laceration to scalp). Injuries to the face are not counted, since helmets are not likely to prevent injuries that most severely affect, for example, the mouth or nose. We limit the injury data to only those cases that involve a bicycle, mountain or all terrain bicycle, or a tricycle. These are codes 1202, 1301, 5033 and 5040 in the NEISS product code list. However, as the definition of these codes also includes bicycle accessories we took special care to include only those injuries that occurred while the person was riding on a bicycle (including young children riding with an adult). For example, head injuries involving a bicycle accessory such as an air pump were deleted. Injuries that occurred inside a house were also excluded as these are not expected to be prevented by a helmet. Another example of excluded injuries is those occurring to pedestrians who were hit by a cyclist. We used the accident narratives to assist us in determining which cases were relevant to our research question. To do this, we programmed certain keywords for an automated sorting.[3] When there were ambiguities, we read through the individual narratives and made a determination on a case-by-case basis.

After "cleaning" the data, we summed the individual cases to generate counts of bicycle-related head injuries by age, hospital, state, and year. These counts represent our main dependent variable, as we describe below. To account for exposure in the models, we use the total number of NEISS cases related to all

[3]It was not useful to search on a word such as "helmet" to drill down to injuries in which a helmet was or was not worn since most narratives do not include this level of information.

DOI: 10.1002/hec

consumer products in a hospital for each age. This total count has the advantage of being age, hospital, and year specific, just like the bicycle injury count, and at the same time providing a measure of the population served by the hospital.[4]

Table I shows some summary statistics for these counts and the other variables. We present mean values along with the minimum and maximum values for those ages 5-19 and again for ages zero through adult. Note that the averages are based on single year of age, hospital and year, which results in some very small values for injury counts. The average bicycle head injury count for ages 5-19 is 1.06. Zeros are quite prevalent in this data, as there are zero injuries reported for 56 percent of the observations in this age group. Note that the average injury count rises to 2.4 if zeros are excluded. Because of the distribution of the injury data, we believe that a count model is the best estimation technique. We describe our methodology in detail in the estimation section below.

Figure 1 shows trends in the national injury rates over time by age group. Head injury rates for children ages 5-11 show a dramatic decrease over time, falling from 21.2 injuries per 1000 cases in 1991 to 12.7 injuries per 1000 cases in 2008. The injury rate for children ages 0-4 also exhibits a downward trend, but it is much less pronounced, falling from 6.9 in 1991 to 5.3 in 2008. Teens ages 12-17 experience rates that initially increase slightly, rising from 9.1 in 1991 to 11.6 in 2000, and then falling to 8.7 in 2008. By contrast, the adult injury rate actually increases over time, rising from 3.7 to 5.1 over the time period presented. Since the trend appears to be different for adults versus children, we use caution when including adults in the comparison group, as we discuss further below.

2.2. Helmet laws

Information on the bicycle helmet laws comes from the Bicycle Helmet Safety Institute. We confirmed and expanded upon their information by consulting the state statutes. Table II lists each state, the effective date of any helmet law, the ages to which the law applies, and whether the helmet law pertains to other wheeled sports such as skateboards or roller skates.[5] Many cities and counties across the country also have local helmet laws. We account for these by gathering information on helmet laws for the county in which the NEISS hospital is located. These laws are fairly rare in the time span of our data. We observe local helmet laws for only 10 hospitals in 9 states, and therefore results for local laws should be treated as suggestive only. Not only is there limited variation in these laws, but the results are likely to be biased since hospitals admit patients from wider geographic areas than just counties; as a result, many of the patient injuries we observe could be under the jurisdiction of a different law than that of the hospital's county.

Care must be taken in interpreting the results of the all of the helmet laws. The degree to which the laws are enforced may vary widely across localities. Also, the penalties for violating the law tend to be very minor. In many cases, the penalty for the first offense is a verbal warning, and if a fine is imposed, it is often waived if a helmet is purchased. Given this, it is not clear whether an effect of the law reflects the effects of the actual law itself or whether it reflects any educational efforts, public campaigns, or changes in attitudes towards risk that accompany the helmet laws.

2.3. Other variables

Injury rates may vary across geographic areas simply because of factors such as differences in weather, temperature, or road conditions. We account for the influence of these factors by including some additional state-level

[4]We considered some different options for the exposure variable. One is the age-specific population of the county of the hospital, but this number may not be a fair representation of the hospital's population, particularly for urban areas. Another option is to use the total number of emergency room visits for any cause as provided by the American Hospital Association. However, this data is not age-specific. Given this, we believe the total count of NEISS cases for all consumer products is the best available.

[5]Currently, several states have helmet laws for downhill skiing. However, these laws were passed starting in 2011, which is outside of our study period.

DOI: 10.1002/hec

Table I. Summary statistics

	Ages 5-19				All ages 0-Adult			
	Mean	Std. Dev.	Min	Max	Mean	Std. Dev.	Min	Max
Bicycle head injury count	1.06	1.96	0	25	1.26	3.39	0	94
Wheeled sports head injury count	0.28	0.70	0	11	0.28	0.76	0	18
Winter sports head injury count	0.24	0.78	0	25	0.26	1.26	0	67
NEISS total case count*	81.24	93.32	1	912	167.52	510.55	1	13347
State helmet law	0.24		0	1	0.26		0	1
Local helmet law	0.04		0	1	0.04		0	1
Vehicle miles	85.25	71.92	1.16	329.27	85.24	71.94	1.16	329.27
Percent rural roads	0.71	0.17	0.18	0.98	0.71	0.17	0.18	0.98
Average rainfall	3.20	1.15	0.45	6.25	3.20	1.15	0.45	6.25
Average temperature	53.90	7.63	36.53	72.48	53.90	7.64	36.53	72.48
Unemployment rate	5.28	1.35	2.30	9.50	5.28	1.35	2.30	9.50
Real per capita income in $1,000 s	35.15	5.47	21.55	56.82	35.15	5.47	21.55	56.82
Hospital beds	230.21	236.38	0	1603	230.13	236.44	0	1603
Teaching hospital	0.35		0	1	0.35		0	1
Trauma hospital	0.31		0	1	0.31		0	1
Hospital beds missing	0.04		0	1	0.04		0	1
Teaching hospital missing	0.03		0	1	0.03		0	1
Trauma hospital missing	0.13		0	1	0.13		0	1
N	25319				33745			

*NEISS total case count represents the number of injuries for any consumer related product by age, hospital, and year. The minimum values reflect the fact that there are small numbers of injuries for particular ages in small hospitals.

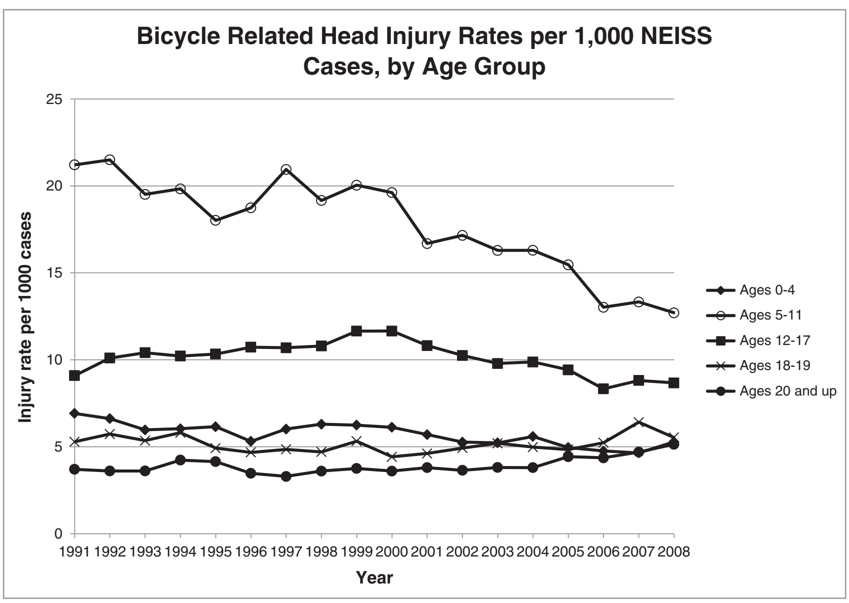

Figure 1. Bicycle Related Head Injury Rates per 1,000 NEISS Cases, by Age Group

variables in the models described below. First, we include the yearly average temperature and rainfall in the state in all models. These data come from the National Climatic Data Center of the U.S. National Oceanic and Atmospheric Administration. Next, we include the percentage of each state's highways classified as urban roadways. Heavy traffic volume on urban roadways may make riding more dangerous and

Table II. Description of bicycle helmet laws, by state

State name	Year law effective	Age applicable	Other wheeled toys?	State name	Year law effective	Age applicable	Other wheeled toys
Alabama	1995	Under 16		Missouri	No law		
(Alaska)	*(No law)*			Montana	No law		
Arizona	No law			Nebraska	No law		
Arkansas	No law			Nevada	No law		
California	1987	Passengers under 5		New Hampshire	2006	Under 16	
	1994	All under 18		New Jersey	1992	Under 14	
	2003	All under 18	Yes		1997	Under 14	Yes
Colorado	No law				2005	Under 17	Yes
Connecticut	1993	Under 12		*(New Mexico)*	*(No law)*		
	1997	Under 16		New York	1989	Under 5	
(Delaware)	*(1996)*	*(Under 16)*			1994	Under 15	Yes
	(2008)	*(Under 18)*		North Carolina	2001	Under 16	
(District of Columbia)	*(2000)*	*(Under 16)*		North Dakota	No law		
Florida	1997	Under 16		Ohio	No law		
Georgia	1993	Under 16		Oklahoma	No law		
(Hawaii)	*(2001)*	*(Under 16)*		Oregon	1994	Under 16	
Idaho	No law			Pennsylvania	1991	Under 5	
Illinois	No law				1995	Under 12	
Indiana	No law			Rhode Island	1996	Under 9	
Iowa	No law				1998	Under 16	Yes
Kansas	No law			South Carolina	No law		
(Kentucky)	*(No law)*			South Dakota	No law		
Louisiana	2002	Under 12		Tennessee	1994	Under 16	
(Maine)	*(1999)*	*(Under 16)*			2000	Under 16	
Maryland	1995	Under 16		Texas	No law		
	2001	Under 16	Yes	Utah	No law		
Massachusetts	1990	Under 5		*(Vermont)*	*(No law)*		
	1993	Under 13		Virginia	No law		
	2004	Under 17		Washington	No law		
Michigan	No law			*(West Virginia)*	*(1996)*	*(Under 15)*	
Minnesota	No law			Wisconsin	No law		
Mississippi	No law			Wyoming	No law		

Note: States in parentheses are not in the NEISS data.

accidents more likely than in rural areas. Annual vehicle miles per capita are also included to provide a measure of automobile density. These highway characteristics come from the Federal Highway Administration of the U.S. Department of Transportation. The state annual unemployment rate and real per capita income are included to help account for economic conditions and income available for purchasing bicycles and helmets, and for using alternative modes of transportation. These variables are available from the U.S. Bureau of Labor Statistics.

The empirical models include hospital fixed effects which account for time-invariant hospital-specific characteristics. However, since we have eighteen years of data, there are many factors that can change during this long time span. We therefore include some time-varying hospital characteristics that may influence the injury rates for each hospital. These include: 1) the number of hospital beds (which represents hospital size), 2) an indicator for whether or not the hospital is a teaching hospital, and 3) an indicator for whether or not the hospital's emergency room is designated as a trauma center of any level. These data all come from the American Hospital Association (AHA). Indicators for missing values for these variables are also used as not all hospitals in the NEISS data have complete information from the AHA.

DOI: 10.1002/hec

3. ESTIMATION

The unit of observation in this data set is an age (a) in a NEISS hospital (h) in a year (t). The bicycle helmet laws vary by state, by the year the law becomes effective, and by the age group to which the law pertains. This gives us policy changes in multiple locations, time, and age groups. This situation is ideally analyzed using difference-in-differences (DD) or difference-in-difference-in-differences (DDD) models.

To help clarify the discussion below, we first discuss the terminology we use to describe the data. The term "law-state" is used to represent the 16 states and the 67 hospitals in those states that have a bicycle helmet law at some point during the study period. The term "no-law state" is used for the states and hospitals in those states that never have a bicycle helmet law during the study period. Next, we identify the "applicable ages", which is the range of ages that are required by law to wear helmets when riding (e.g. under 12). This is in contrast to the "non-applicable ages", which are the ages that are not covered by the law. Lastly, we use the terms "pre period" and "post period" to describe the years before and after the laws are in effect. These periods vary by state since each state enacts their helmet laws at different times.

The models use the injury counts of people of non-applicable ages as the comparison group. We use two age groupings as the basis for this comparison. The first includes children up through age 19. Note that we allow 19 year olds to be included as children since a few state laws extend to all children under age 18, so in order to have enough observations in the comparison group, we use the injury rates of 18 and 19 year olds. The second comparison group includes children of non-applicable ages plus adults ages 20 and up. We also have an issue of whether or not to include children under the age of 5. For most models, we will exclude these children since there are very few head injuries among the youngest children. However, as some of the early laws pertain only to those under age 5, we present some specifications that include these children.

Given the variations in age, location, and time, we ideally would like to estimate a multi-group, multi-period, multi-site, DDD model. However, the proper estimation of such a model would require interactions between 1) the age and the year indicators, 2) age and location indicators, and 3) location indicators and year indicators. The most complete model for children between ages 5 and 19 would include 15 ages, 18 years, and 141 hospitals, resulting in the inclusion of over 4700 main and interaction terms. A more collapsed specification using state indicators instead of hospitals would still result in over 1500 interaction terms. Unfortunately, it is difficult to get count models to converge with such full saturation.

Since the fully interacted DDD model is not feasible, we rely on a multi-group, multi-period, two-site DDD model. That is, we reduce the number of location interactions and replace state (or hospital) indicators with a single indicator variable for whether or not the hospital is located in a law-state. The resulting DDD model then includes age fixed effects, year fixed effects, the law-state indicator, interactions between age and year effects, interactions between age and the law-state indicator, and interactions between year indicators and the law-state indicator. This reduces the number of main and interaction effects to 301:

$$\begin{aligned} I_{aht} = f(&\alpha_0 + \alpha_1 policy\_indicator_{aht} + \alpha_2 LawStateIndicator_h + \alpha_3 Year\_dummies_t + \alpha_4 Age\_dummies_a \\ &+\alpha_5(Age\_dummies_a{*}LawStateIndicator_h) + \alpha_6(Year\_dummies_t{*}LawStateIndicator_h) \\ &+\alpha_7(Age\_dummies_a{*}Year\_dummies_t) + X_{ht}\beta + v_{aht}). \end{aligned} \quad (1)$$

In equation 1, I_{aht} is the injury count for age (a) in a hospital (h) in a year (t). For example, an observation is the number of injuries involving 5 year olds in Memorial Hospital in 1991. The vector X includes state and hospital specific characteristics that may determine injury rates as described in the data section. The coefficient on the policy indicator shows the effect of the law on injury counts of children of applicable ages in the post period. The comparison group includes children in law-states of non-applicable ages in the post period and children in non-law states of all ages.

We next modify equation 1 by replacing the law-state indicator with hospital (or state) fixed effects, but still using the law-state indicator to generate the interaction terms with age and year. This modification helps control for any time-invariant within state or hospital characteristics, and is our preferred specification:

DOI: 10.1002/hec

$$\begin{aligned} I_{aht} = f(&\alpha_0 + \alpha_1 policy\_indicator_{aht} + \alpha_2 Hospital\_dummies_h + \alpha_3 Year\_dummies_t + \alpha_4 Age\_dummies_a \\ &+\alpha_5(Age\_dummies_a{*}LawStateIndicator_h) + \alpha_6(Year\_dummies_t{*}LawStateIndicator_h) \\ &+\alpha_7(Age\_dummies_a{*}Year\_dummies_t) + X_{ht}\beta + v_{aht}). \end{aligned} \tag{2}$$

For comparison sake, we also present a three way fixed effects model that omits the interaction terms and simply controls for age fixed effects, hospital fixed effects and year fixed effects:

$$I_{aht} = f(\alpha_0 + \alpha_1 policy\_indicator_{aht} + \alpha_2 Year\_dummies_t + \alpha_3 Hospital\_dummies_h + \alpha_4 Age\_dummies_a + X_{ht}\beta + v_{aht}). \tag{3}$$

In equation 3, the coefficient on the policy indicator still shows the effect of the law on injury rates of children of applicable ages in the post period. The drawback to this estimation is that the only information that comes from the no-law states is a national trend. This specification does not difference out any treatment versus control group information from the no-law states and is essentially the same as a straightforward DD model on a sample of only the law-states. Indeed, in a separate table below, we show results from multi-group, multi-time period DD models on the sample of only law-states. The DD model has the advantage of generating a clean interpretation. It compares the injury rates of children of applicable ages in a hospital before and after the law, while netting out the trends generated from children of non-applicable ages in the same hospitals before and after the law.

One issue with all of the above models pertains to the quality of the control groups used for comparison. Ideally, a control group will be similar to the treatment group in many respects, but remain unaffected by the law. The quality of the control group can be questioned in the case of helmet laws and usage, since it is easy to argue that helmet laws may have spillover effects to non-applicable ages, especially among children. For example, public campaigns about the law may not highlight the age at which the law applies. Parents may require children of both applicable and non-applicable ages to wear helmets in response to the laws. The laws may create new social norms about riding for all ages. All of these possibilities call into question the appropriateness of individuals of non-applicable ages as the control group. We discuss this issue in further detail below.

Our empirical approach to answering the question of whether bicycle helmet laws are effective in reducing head injury rates among children relies on the weight of evidence provided by the different models outlined above. We will compare results from each, along with comparisons from using different definitions of the control group based on age. All models will be estimated with Poisson regression analysis, which is an appropriate technique for analyzing injury counts, particularly when there are a lot of zeros present in the dependent variable. To permit for overdispersion, standard errors are adjusted for heteroskedasticity of unknown form that includes a within-state cluster correlation (Cameron and Trivedi 2005; Bertrand *et al.* 2004). The advantage of the Poisson estimation is that the estimates are consistent regardless of whether the counts actually have a Poisson distribution (Wooldridge 2002).[6] Each model includes the log of the age-specific population as a right hand side variable to normalize for exposure. The coefficient on this log population is constrained to equal one.

4. RESULTS

4.1. Main specifications

Table III shows the results of bicycle helmet laws on injury counts among children ages 5-19. Four different models are shown, corresponding to equations 1, 2 and 3 above. Equation 2 is estimated twice--first with state

[6]The Poisson model is preferred to the negative binomial since the negative binomial estimates are not consistent if the variance specification is incorrect (Cameron and Trivedi 2005). Nevertheless, negative binomial models were tested and give similar results.

DOI: 10.1002/hec

Table III. Bicycle related head injuries, various poisson models, ages 5-19

Model	3 Way Fixed Effects		DDD With Indicator for Law States		DDD With State Fixed Effects		DDD With Hospital Fixed Effects	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
State helmet law	-0.176 (-3.36)	-0.180 (-3.73)	-0.214 (-4.31)	-0.214 (-4.38)	-0.155 (-2.88)	-0.155 (-2.83)	-0.137 (-2.69)	-0.136 (-2.67)
Local helmet law		-0.127 (-1.15)		-0.056 (-0.49)		-0.012 (-0.06)		-0.115 (-1.16)
Vehicle miles	0.003 (1.71)	0.003 (1.69)	0.0001 (0.33)	0.0001 (0.23)	0.003 (2.89)	0.003 (2.92)	0.003 (1.79)	0.003 (1.80)
Rural roads	-0.525 (-0.83)	-0.384 (-0.57)	-0.020 (-0.07)	-0.013 (-0.04)	-0.381 (-0.54)	-0.372 (-0.50)	-0.518 (-0.79)	-0.421 (-0.63)
Avg. rain	-0.040 (-1.35)	-0.039 (-1.34)	-0.039 (-1.68)	-0.041 (-1.81)	-0.039 (-1.10)	-0.038 (-1.09)	-0.033 (-0.98)	-0.031 (-0.91)
Avg. temp	-0.007 (-0.77)	-0.008 (-0.84)	-0.006 (-1.10)	-0.005 (-0.93)	-0.019 (-1.40)	-0.019 (-1.40)	-0.010 (-1.01)	-0.011 (-1.03)
Unemployment	-0.010 (-0.52)	-0.009 (-0.47)	-0.012 (-0.39)	-0.012 (-0.37)	-0.018 (-0.67)	-0.018 (-0.68)	-0.003 (-0.11)	-0.004 (-0.16)
Per capita income	-0.021 (-0.80)	-0.020 (-0.75)	-0.016 (-1.27)	-0.014 (-1.10)	-0.017 (-0.60)	-0.017 (-0.59)	-0.014 (-0.49)	-0.014 (-0.46)
Hospital beds	-0.0002 (-0.57)	-0.0002 (-0.56)	-0.0001 (-0.47)	-0.0001 (-0.51)	0.0001 (0.48)	0.0001 (0.48)	-0.0001 (-0.39)	-0.0001 (-0.40)
Teaching hospital	0.016 (0.30)	0.006 (0.12)	0.134 (1.50)	0.137 (1.55)	0.060 (0.60)	0.060 (0.62)	0.013 (0.22)	0.001 (0.02)
Trauma hospital	-0.008 (-0.16)	-0.021 (-0.43)	0.284 (3.72)	0.280 (3.79)	0.274 (3.72)	0.273 (3.81)	-0.006 (-0.12)	-0.018 (-0.39)

Notes: N = 25,319. Coefficients transformed into semi-elasticities shown. T-statistics in parentheses calculated with standard errors clustered at the state level. Models also include missing value indicators for hospital beds, teaching hospital and trauma hospital, and the log of the age-specific population, with coefficient constrained to equal 1 to normalize for exposure. Columns 1 and 2 include fixed effects for age, year and hospitals. Columns 3 and 4 include: fixed effects for age; fixed effects for year; an indicator for being in a law state; interactions between age indicators and year indicators; interactions between the indicator for being in a law state and year indicators; and interactions between the indicator for being in a law state and age indicators. Columns 5 and 6 include: fixed effects for age; fixed effects for year; fixed effects for states; interactions between age indicators and year indicators; interactions between the indicator for being in a law state and year indicators; and interactions between the indicator for being in a law state and age indicators. Columns 7 and 8 include: fixed effects for age; fixed effects for year; fixed effects for hospitals; interactions between age indicators and year indicators; interactions between the indicator for being in a law state and year indicators; and interactions between the indicator for being in a law state and age indicators.

DOI: 10.1002/hec

fixed effects and second with hospital fixed effects. We change the order in the table and present the results for the three way fixed effects first (equation 3). This shows a progression from the least inclusive to the most inclusive specification in terms of fixed effects and interactions. Models are also shown with and without the indicator for the presence of a local helmet law.

In all models in Table III, the coefficient on the state helmet law is negative and statistically significant. The magnitude varies based on the specification, with the least inclusive model corresponding to the largest magnitude. The magnitude falls with the inclusion of the interactions and the state fixed effects. Our preferred specification is shown in column 7 (with little difference when the local law is added in column 8.). This is the most inclusive DDD model with hospital fixed effects. Here, the coefficient shows that having a bicycle helmet law is associated with a reduction in the bicycle-related head injury count of 13.7 percent. In other words, considering a mean count of 1.06 injuries per age group, hospital and year, we can expect a decrease in this count of 0.145 injuries, down from 1.06 to 0.914 injuries.

In Table IV, we use the preferred specification (equation 2) to test whether the results are sensitive to the inclusion of children under age 5 and to the inclusion of adults. The included ages are specified in the row labeled "Sample". The protective effect of helmet laws is still apparent. The bicycle helmet laws are associated with a reduction in the bicycle-related head injury counts of a range of 14 to 20 percent. The local laws are also associated with a reduction in head injuries, but the coefficient is significant only at the 10 percent level in the models that include adults as part of the control group.

In Table V, we estimate DD models on the sample of law-states only. The purpose here is to demonstrate results within a simple in-hospital experiment. These models compares the injury rates of children of applicable ages in a hospital before and after the law, while netting out the trends generated from children of non-applicable ages in the same hospitals before and after the law. We vary the age groups included, and therefore vary the age definitions for the treatment and control groups. Results with adults included in the control group are not shown for brevity but are similar to those presented in the table. In all models, the coefficient on the state helmet laws is negative and statistically significant, with magnitudes similar to those of the previous tables.

4.2. Validity of control groups

One concern with the DDD and DD models regards the validity of the control groups, particularly since spillover effects of the laws to non-applicable age groups are possible. As described below, we provide some suggestive evidence that the control groups are valid.

Table IV. Bicycle related head injuries, DDD poisson model with hospital fixed effects, varying ages

Sample	Ages 0-19		Ages 0 - adult		Ages 5 - adult	
Control Group	Children of non-applicable ages (ages 0 to 19)		Children of non-applicable ages and adults		Children of non-applicable ages (ages 5 to 19) and adults	
	(1)	(2)	(3)	(4)	(5)	(6)
State helmet law	-0.144 (-2.93)	-0.143 (-2.92)	-0.197 (-4.33)	-0.198 (-4.27)	-0.193 (-4.04)	-0.195 (-3.94)
Local helmet law		-0.086 (-0.88)		-0.330 (-1.69)		-0.351 (-1.85)
N	32043		33745		27021	
Mean Injury Count	0.98		1.26		1.41	

Notes: Coefficients transformed into semi-elasticities shown. T-statistics in parentheses calculated with standard errors clustered at the state level. Models also include vehicle miles per capita; percent rural roads; average rainfall; average temperature; unemployment rate; real per capita income; number of hospital beds, indicators for teaching hospital and trauma hospital; missing value indicators for hospital beds, teaching hospital and trauma hospital; the log of the age-specific population, with coefficient constrained to equal 1 to normalize for exposure; fixed effects for age; fixed effects for year; fixed effects for hospitals; interactions between age indicators and year indicators; interactions between the indicator for being in a law state and year indicators; and interactions between the indicator for being in a law state and age indicators.

DOI: 10.1002/hec

Table V. Bicycle related head injuries, DD poisson models, law states only

Sample	Ages 0 to 19		Ages 5-19	
Control Group	Children of non-applicable ages (ages 0 to 19)		Children of non-applicable ages (ages 5 to 19)	
State helmet law	-0.168 (-3.54)	-0.166 (-3.51)	-0.175 (-3.69)	-0.173 (-3.67)
Local helmet law		-0.221 (-2.46)		-0.239 (-2.50)
N	14823	14823	11721	11721

Notes: Coefficients transformed into semi-elasticities shown. T-statistics in parentheses calculated with standard errors clustered at the state level. Models also include: vehicle miles per capita; percent rural roads; average rainfall; average temperature; unemployment rate; real per capita income; number of hospital beds, indicators for teaching hospital and trauma hospital; missing value indicators for hospital beds, teaching hospital and trauma hospital; fixed effects for age, year and hospital; the log of the age-specific population on the right hand side, with coefficient constrained to equal 1 to normalize for exposure.

We first collapse the data for the law-states by hospital and year into treatment and control groups, before and after the law. We then run simple two group, two period DD models. The effect of the law is reflected in the coefficient on the treatment group in the post period. In addition, the coefficient on the post period can be interpreted as the effects on injury rates of being in the control group in the post period. This coefficient will contain any spillover effects plus any other post-period effect that is not already captured by the other state and hospital level variables. Therefore, while is not a pure test of the spillover effects, an examination of this coefficient can provide suggestive evidence.

When doing this exercise, we limit the data to those of ages 5 and up in order to help eliminate difficulties in coding the groups that arise from changes in the applicable ages of the law. We also take care to account for changes in the applicable ages in some states over time by running different models with different assumptions about the applicable age, and by deleting some problematic states.[7]

Since the point of this simple DD model is to check on the quality of the control groups and see if we can observe any spillover effects of the laws, we tested four different control groups: First, we use teens ages 18 and 19 since this group serves as controls in all states; Second, we use children one, two or three years above the applicable age. For example, if the applicable age is ‘under 12’ in a state, children ages 12, 13, and 14 are designated as the control group. We expect that any spillover effects would occur among children closest in age to the applicable ages. The third control group is the same as the second; however, in these models, we limit the treatment group to children at the applicable age and one and two years younger. This generates a sample of children within six years of age, who should be similar in many respects, particularly riding habits. Lastly, we use only adults as the control group since no state law pertains to adults.

The results of this exercise uniformly show negative and significant coefficients for the treatment group in the post period, and small and statistically insignificant coefficients for the control groups in the post period. These insignificant coefficients are suggestive of a lack of spillover effects, although not definitive, since any positive post period trend in injuries could dampen any reduction in injuries that result from the law spillover to the older groups. We do note that this is a strong possibility when 18-19 year olds and adults are in the control group as Figure 1 shows some upward trends in the injury rates for these groups.

4.3. Checks for policy endogeneity and influence from small hospitals

Another concern with our estimation strategy is policy endogeneity where the laws are passed in response to high rate of injuries. To check for this problem, we ran our preferred DDD specification including the current year state law and an indicator for the next year’s law. The size and significance of the coefficient on the current

[7]Connecticut, Massachusetts and New Jersey all change the applicable age during the time span of our data. We run each model three times. The first uses the lowest age as the applicable age and ignores the subsequent age change. The second uses the highest age as the applicable age and ignores the lower age limit. Finally, we delete these three problem states to avoid the issue altogether. Results are similar across all models and are available upon request.

DOI: 10.1002/hec

year law is very similar to that in Table III, column 7, while the coefficient on the future law is very small and statistically indistinguishable from zero (results not shown). This provides some evidence that policy endogeneity is not an issue.

We also worry that the results presented thus far may be influenced by the injury counts in the small hospitals. That is, the rate within a small hospital could be very high if there are many bike related injuries relative to the number of overall NEISS cases. In results not shown, but available upon request we limit the sample first by excluding all observations in small hospitals and second, by excluding of all observations in small and medium size hospitals. Hospital size is defined by NEISS based on the number of emergency room visits. The results remain unchanged from previous models and indicate a decrease in the bicycle head injury count in the range of 11.5 to 14.3 percent after the enactment of a helmet law.

5. SAFER RIDING OR CHANGES IN RIDERSHIP?

A major concern with the results presented thus far is that we do not know whether the reduction in head injuries associated with the law arises from more children wearing helmets when they ride bicycles, from children riding more safely in general (say by avoiding street riding), or simply from a reduction in the number of children riding bicycles. Grant and Rutner (2004) find no evidence of a substitution to walking (as measured by pedestrian fatalities) or driving (as measured by vehicle miles) associated with helmet laws, while Carpenter and Stehr (2011) find a reduction in riding among high school students as a result of the laws. We test for such effects first by examining the effects of the laws on bicycle related injuries to other body parts and second, by examining the effects of helmet laws on injury rates of other popular sports.

To consider the effects of the laws on non-head injuries, we generate three bicycle related injury counts for children ages 5-19 (Table VI). The first is a count of all injuries to body parts other than the head, ear, or total body. The second is a count of injuries to the face, eye, mouth and neck, which should be most closely related to head injuries, but not preventable by a helmet. The third is an injury count pertaining to all body parts below the neck. The results for all three injury types are very similar. Bicycle helmet laws are associated with about a

Table VI. Non-Head bicycle related injuries and injuries from other sports, DDD poisson model, ages 5-19

	Non-Head Bicycle Related Injuries		
	Face/neck injuries	All body parts below neck	All non-head injuries
Bicycle helmet law	-0.090 (-2.10)	-0.085 (-2.43)	-0.088 (-2.60)
Mean Injury Count	1.31	4.35	5.66
		Wheeled Sports	
	Head injuries	All non-head injuries	Total injuries
Bicycle helmet law	0.255 (4.46)	0.100 (2.08)	0.111 (2.31)
Law includes wheeled sports	0.035 (0.45)	-0.031 (-0.95)	-0.023 (-0.76)
Mean Injury Count	0.28	3.29	3.56
		Winter Sports	
	Head injuries	All non-head injuries	Total injuries
Bicycle helmet law	-0.074 (-1.04)	-0.005 (-0.10)	-0.014 (-0.29)
Law includes wheeled sports	0.079 (1.15)	-0.055 (-0.73)	-0.041 (-0.60)
Mean Injury Count	0.24	1.54	1.78

Notes: N = 25,319. Wheeled sports includes scooters, skateboards, roller skates, in-line skates, and wheeled riding toys excluding bicycles and tricycles. Winter sports includes ice skating and ice hockey, snow skiing, and snowboarding. Coefficients transformed into semi-elasticities shown. T-statistics in parentheses calculated with standard errors clustered at the state level. Models also include: vehicle miles per capita; percent rural roads; average rainfall; average temperature; unemployment rate; real per capita income; number of hospital beds, indicators for teaching hospital and trauma hospital; missing value indicators for hospital beds, teaching hospital and trauma hospital; the log of the age-specific population, with coefficient constrained to equal 1 to normalize for exposure; fixed effects for age; fixed effects for year; fixed effects for hospitals; interactions between age indicators and year indicators; interactions between the indicator for being in a law state and year indicators; and interactions between the indicator for being in a law state and age indicators.

9 percent reduction in these injuries. Unfortunately, this exercise does not go far in answering the question since a decrease in these injuries is consistent with both a decrease in ridership and safer riding practices. What we can conclude from these results, however, is that there appears to be no substitution in the injury reports from the head (which may be protected by a helmet) to an unprotected body part. If this were the case, we would expect to see an increase in other injuries associated with the helmet laws.

Next we consider the effects of helmet laws on injury rates of other sports. Laws for helmets in four of the states in our sample explicitly cover other wheeled sports such as roller skates and scooters.[8] Even in the absence of these specific laws, it is possible that the bicycle helmet laws create a norm of helmet wearing for all sports with a risk of head injury. This can include winter sports such as skiing and ice skating in addition to the wheeled sports. In this case, we expect to see a decrease in head injuries for these other sports. However, if the bicycle helmet laws induce children to substitute away from bike riding toward the other sports, we may see an increase in injuries related to these sports.

Using the NEISS data and the same process described for bicycle injuries, we generate injury rates by age for certain wheeled and winter sports. Wheeled sports include scooters, skateboards, roller skates, in-line skates, and wheeled riding toys excluding bicycles and tricycles. Winter sports include ice skating and ice hockey, snow skiing, and snowboarding. Table I shows the injury counts for these sports, which are far less common than bicycle injuries. Trends in these injuries for children (not shown) show a distinct upward path over time.

The models shown in Table VI use the preferred DDD specification that includes the hospital fixed effects. The state helmet law indicator is defined in the same way as in the previous tables. We then add an incremental indicator for whether the state helmet law pertains to other wheeled sports too. We show results restricting the sample to ages 5-19; however, results that include adults in the control group are very similar. We also show results for injuries occurring to the head only and to all other body parts.

The results are striking in that the helmet laws are associated with an increase in injuries from wheeled sports and the laws that pertain specifically to wheeled sports have no effects on these injuries. These results are notable give that the estimates are net of national trends and net of trends for children of similar ages in non-law states. The results for the bicycle helmet law could be interpreted as reflecting a substitution effect away from bicycle riding towards the other wheeled sports in response to the laws.

The results for winter sports are also shown in Table VI. Here, neither the bicycle helmet law nor the wheeled sports helmet laws are associated with injuries for skiing and skating. This is some evidence against a norm being generated by the helmet laws that is broadly applied to winter sports.

6. DISCUSSION

In this paper, we examine the question of whether bicycle helmet laws are associated with reductions in head injury rates among children. We consider the effects of the laws directly on bicycle related head injuries, bicycle related non-head injuries, and injuries as a result of participating in other wheeled sports (primarily skateboarding, roller skating, and riding scooters). For 5-19 year olds, we find that the helmet laws are associated with a 13 percent reduction in bicycle related head injuries, but the laws are also associated with a 9 percent reduction in non-head bicycle related injuries and an 11 percent increase in all types of injuries from the wheeled sports.

These results are checked in a variety of ways. Through variations on DDD and DD models, we show that the estimated reduction in head injuries resulting from helmet laws is robust to changes in the definition of the control group, to changes in the type of fixed effects included (state versus hospital), and to changes in the

[8]Rhode Island also has a wheeled sports law, but NEISS hospitals in Rhode Island exit the sample before the law.

DOI: 10.1002/hec

samples of states and hospitals evaluated. We also provide some limited evidence of a "clean" control group, that is, one where the laws do not have spillover effects to children of non-applicable ages.

To what do we attribute the observed changes in head and other injuries? Unfortunately, it is difficult to identify the mechanisms at work with our injury data, since we cannot distinguish between a decrease in riding versus a change in safe riding behaviors. Our results fit both stories. That is, if the laws decrease bicycle riding we would see a decrease in both head injuries and injuries to other body parts, which we do. If the helmet laws promote safer riding practices in general and awareness of the risks of riding, we would also see the decrease in both head and non-head injuries. Our evidence in support of the decrease ridership theory comes from the observed increase in injuries in other wheeled sports that is associated with the bicycle helmet laws. We note that Carpenter and Stehr (2011) also find some evidence of the substitution effect using survey data on bicycle riding among high school students.

The uncertainty surrounding the mechanism at work also extends to the question of whether the laws themselves are responsible for the reduction in injuries, or rather, we are observing the effects of education campaigns, or changes in attitudes towards risk that accompany the helmet laws. As we noted earlier, the enforcement and penalties may vary widely across localities so where these factors are lax, the laws may have less to do with the reductions in injuries than the safety awareness that accompany the passage of the laws.

The mechanism aside, perhaps what is most important is an estimate of the total effect on injuries associated with the helmet laws. Considering the different offsetting results, we run our preferred specification on injury counts for 1) all head injuries and 2) total (all head and body) injuries arising from cycling and wheeled sports. The net effects of the helmet laws are small and are not statistically different from zero. However, they do point to a net reduction, be they imprecisely estimated, with a 6 percent reduction in all head injuries and a 2 percent reduction in total injuries (results not shown).

The findings from this paper indicate that while bicycle helmet laws are widespread and thought to be effective, the net effect of these laws on health outcomes is actually not straight-forward. It is clear that there are offsetting behaviors and unintended consequences of these laws, and these effects need to be considered by policymakers.

ACKNOWLEDGEMENTS

Funding for this project was provided by grant number R03 HD060972-01 from the National Institutes of Health. We would like to acknowledge the help of Tom Schroeder for providing the NEISS data, and to Diane Nesson for coding the laws.

REFERENCES

Attewell RG, Glase K, McFadden M. 2001. Bicycle helmet efficacy: a meta-analysis. *Accident Analysis and Prevention* **33**: 345–352.

Bertrand M, Duflo E, Mullainathan S. 2004. How much should we trust differences-in-differences estimates? *Quarterly Journal of Economics* **119**: 249–275.

Bicycle Helmet Safety Institute. Helmet laws for bicycle riders. Available from: http://www.helmets.org/mandator.htm (Accessed 6/10/08).

Cameron AC, Trivedi PK. 2005. *Microeconometrics: Methods and Applications*. Cambridge University Press: New York.

Carpenter C, Stehr M. 2011. Intended and Unintended Consequences of Youth Bicycle Helmet Laws. *Journal of Law and Economics* **54**(2): 305–324.

Centers for Disease Control and Prevention. 1995. Injury-control recommendations: bicycle helmets. MMWR Morb Mortal Wkly Rep 44(RR-1); 1–17.

Centers for Disease Control and Prevention. 2012a. National Center for Health Statistics. Underlying Cause of Death 1999-2009 on CDC WONDER Online Database, released 2012.

DOI: 10.1002/hec

Centers for Disease Control and Prevention. 2012b. Nonfatal Injury Data on WISQARS. Available from: http://www.cdc.gov/injury/wisqars/nonfatal.html

Consumer Product Safety Commission. 1999. National Bike Helmet Use Survey, April.

Consumer Product Safety Commission, Division of Hazard and Injury Data Systems. 2000. NEISS The National Electronic Injury Surveillance System: A Tool for Researchers, March.

Consumer Product Safety Commission, Division of Hazard and Injury Data Systems. 2013. NEISS Codebook.

Curnow WJ. 2005. The Cochrane Collaboration and bicycle helmets. *Accident Analysis and Prevention* **37**: 569–573.

Grant D, Rutner SM. 2004. The effect of bicycle helmet legislation on bicycling fatalities. *Journal of Policy Analysis and Management* **23**(3): 595–611.

Hagel BE, Pless IB. 2006. A critical examination of arguments against bicycle helmet use and legislation. *Accident Analysis and Prevention* **38**(2): 277–278.

Li G, Baker SP, Fowler C, DiScala C. 1995. Factors related to the presence of head injury in bicycle-related pediatric trauma patients. *The Journal of Trauma: Injury, Infection and Critical Care* **38**: 871–875.

Machlin SR. 2006. *Expenses for a hospital emergency room visit, 2003. Statistical Brief #111. January*. Agency for Healthcare Research and Quality: Rockville MD.

National Highway Traffic Safety Administration. 2009. Traffic Safety Facts 2009 Data Bicyclists and Other Cyclists. DOT HS 811 386.

National Safe Kids Campaign. 2002. A national study of traumatic brain injury and wheel-related sports, May.

NSGA. 2012. 2010 Youth Participation in Selected Sports with Comparisons to 2001. Available from: http://www.nsga.org/files/public/2010YouthParticipationInSelectedSportsWithComparisons.pdf (accessed 5/17/12)

Rodgers GB. 2001. The Bicycle Study. Unpublished manuscript, Consumer Product Safety Commission.

Safe Kids USA. 2012. Bicycling and Skating Safety Fact Sheet. Available from: http://www.safekids.org/our-work/research/fact-sheets/bicycle-safety-fact-sheet.html (Accessed 5/17/12)

Shah S, Sinclair SA, Smith GA, Xiang H. 2007. Pediatric hospitalizations for bicycle-related injuries. *Injury Prevention* **13**: 316–321.

Thompson DC, Nunn ME, Thompson RS, Rivera FR. 1996. Effectiveness of bicycle safety helmets in preventing serious facial injury. *JAMA* **276**: 1974–1975.

Thompson RS, Rivara FP, Thompson DC. 1989. A case-control study of the effectiveness of bicycle safety helmets. *The New England Journal of Medicine* **21**(320): 1361–1367.

Wooldridge JM. 2002. *Econometric analysis of cross section and panel data*. MIT Press: Cambridge.

DOI: 10.1002/hec