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Intended and Unintended Consequences of Youth Bicycle Helmet Laws

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Abstract

More than 20 states have adopted laws requiring youths to wear a helmet when riding a bicycle. We confirm previous research indicating that these laws reduced fatalities and increased helmet use, but we also show that the laws significantly reduced youth bicycling. We find this result in standard two-way fixed-effects models of parental reports of youth bicycling and in triple-difference models of self-reported bicycling among high school youths that explicitly account for bicycling by youths just above the age threshold of the helmet law. Our results highlight important intended and unintended consequences of a well-intentioned public policy.

1. Introduction

Every year, emergency departments in the United States treat hundreds of thousands of bicycle-related injuries, with hundreds of these injuries resulting in deaths—usually because of head injuries (Rodgers 2000). Some large fraction of these deaths would likely have been prevented if the bicyclist had been wearing a properly fitted bicycle helmet, as there is ample medical evidence that helmets reduce the likelihood of serious head trauma and brain damage in bicycle accidents by as much as 85 percent, particularly among children (see, for example,

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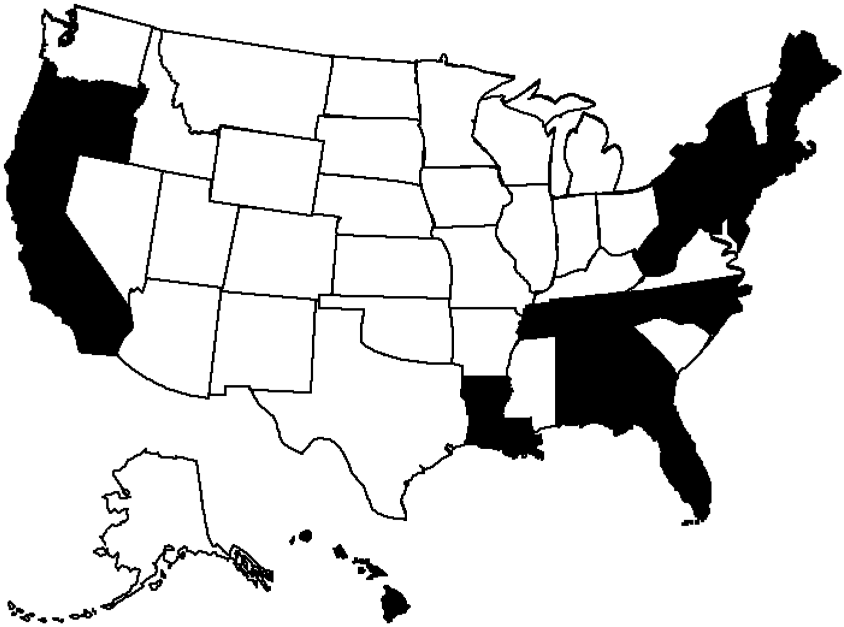

Figure 1. States with bicycle helmet laws as of 2005

Thompson, Rivara, and Thompson 1989). Despite this evidence, however, rates of bicycle helmet use are low, particularly among youths: about a third of youths ages 5–15 in the United States who rode a bicycle in the past 12 months did not wear a helmet; helmet use among older adolescents is particularly low. When asked why they do not wear bicycle helmets, youths commonly complain about the look and fit of bicycle helmets. A common complaint about the former is that bicycle helmets make youths look like geeks or nerds, while problems with fit generally include laments that helmets are too tight and induce sweating (Consumer Product Safety Commission 1995).

Because the technological efficacy of bicycle helmets is well accepted in the medical community, state and local governments have reason to try to increase helmet use. As with many public health policies, states' efforts in this area have reflected two distinct types of approaches: carrots and sticks. Examples of the former include bicycle helmet giveaways, educational outreach about the life-saving effects of helmets, and media campaigns designed to change social norms regarding bicycle helmets (for example, to make it seem "cool" to wear a bicycle helmet). The most direct example of the stick approach has been the adoption of laws that require youths to wear a helmet when riding a bicycle, a policy adopted by 21 U.S. states over the past 15 years. Figure 1 shows the geographic distribution of helmet laws as of 2005. Although helmet laws are largely coastal phenomena, over half of the U.S. youth population is now covered by a state bicycle helmet law (see Table A1). Most of these laws apply to youths under age 16 and impose modest penalties for violations (for example, verbal warnings, counseling, or a small fine). A recent quasi-experimental study returned evidence that helmet laws significantly reduced juvenile bicycle fatalities by about 11 percent during the 1990s (Grant and Rutner 2004).

How did helmet laws achieve these reductions in fatalities? One direct possibility documented in previous public health research is that helmet laws increased youth helmet use. Since there is general agreement in the medical community on the technological efficacy of helmets in reducing serious brain injuries, an increase in helmet use is one plausible and intuitively appealing mechanism.[1] Despite this, the literature has not directly tested an alternative possibility: that mandatory helmet laws have reduced youth bicycling by increasing its costs.[2] Such a direct reduction in bicycling exposure would also be expected to reduce fatalities even in the absence of any changes in youth helmet use.

In this paper, we provide the first comprehensive quasi-experimental estimates of the effects of mandatory bicycle helmet laws in the United States on bicycling fatalities, helmet use, and bicycling behaviors among youths. Our main empirical approach is based on the differential timing of adoption of helmet laws across states, effectively comparing within-state changes in outcomes for residents of states adopting laws with outcomes for youths in states not adopting laws in that same year. We first confirm and extend previous fixed-effects research on bicycling fatalities by showing that helmet laws significantly reduced bicycling fatalities among youths ages 0–15 (those who were directly affected by most states' laws requiring helmet use by youths under 16) by approximately 19 percent. As expected, similarly specified fatality models among 16–30 year olds (who were not directly affected by the helmet laws) return much smaller and statistically insignificant effects of helmet laws. We also provide the first quasi-experimental evidence in the literature that an important mechanism behind the reductions in fatalities was an increase in helmet use: fixed-effects models using parental reports of children's helmet use suggest that helmet laws significantly increase youth helmet use by 29–35 percent. Similarly specified models using self-reports of high-school-aged youths also return evidence that helmet laws increase helmet use by 10–23 percent, although these estimates are less precise.

Our main contribution is to show that in addition to the increase in helmet use, there is also robust evidence for an unintended and previously undocumented mechanism: helmet laws produce modest but statistically significant reductions of 4–5 percent in youth bicycling participation. As with our helmet use estimates, we find this result both in fixed-effects models of parental reports

[1] For reviews of the medical literature on the lifesaving effects of helmets, see Towner et al. (2002) and Karkhaneh et al. (2006).

[2] We are not the first to consider the unintended consequences of safety regulations. Peltzman (1973) points out that policies designed to improve automotive safety may lead to more careless or aggressive driving.

of children's bicycling and in self-reports of high school youths' bicycling, and the bicycling effects are statistically significant in both data sets. Moreover, our analysis of the reduction in bicycling goes further by showing that this main result also obtains in triple-difference models that identify the effects of helmet laws by comparing changes in outcomes for youths under the age threshold of each state's helmet law (youths who were directly targeted by the laws) with the associated changes in outcomes for youths at or above the age thresholds (who were not directly affected by the laws) coincident with policy adoption. Estimates from these fully saturated models (with controls for age-by-state, age-by-year, and state-by-year fixed effects) provide particularly strong evidence that the significant reductions in bicycling associated with the adoption of helmet laws are robust. Overall, our results highlight that a full cost-benefit analysis of the laws should take into account the previously ignored reductions in bicycling associated with the adoption of helmet laws. More generally, our results highlight both the intended and the unintended consequences of a well-meaning public health policy.

The paper proceeds as follows: Section 2 outlines the data and empirical approach, Section 3 presents the results, and Section 4 concludes.[3]

2. Data Description and Empirical Framework

We use several data sources to estimate the effects of mandatory helmet laws on youth outcomes. We first revisit the relationship between helmet laws and bicycling fatalities by updating the estimates of Grant and Rutner (2005) with data from the Fatality Analysis Reporting System for 1991–2005.[4] The Fatality Analysis Reporting System (FARS) contains data on a census of bicycle fatalities involving an accident with a motor vehicle in the 50 states and the District of Columbia. Using these data, we create counts of the number of fatalities of bicycle riders among youths younger than 16 (since most state helmet laws apply

[3] We do not provide a systematic review of the large public health and medical literatures that examine the effects of bicycle helmet laws and bicycle helmets on helmet use, injuries, and bicycling using physical observations of cyclists and/or analyses of cross-sectional data. Multiple reviews of the literature find that helmet laws increase helmet use among the groups targeted by the laws (see, for example, Attwell et al. 2001; Towner et al. 2002; Karkhaneh et al. 2006). Very few studies examine bicycle helmet laws in the United States; most research focuses on Australia or Canada (see, for example, Robinson 2003). An exception is Rodgers (2002), which uses a telephone survey of approximately 900 young cyclists across several states that did and did not have helmet laws. That study finds that, after controlling for individual demographic characteristics, state helmet laws are associated with statistically significant increases in the fraction of youths reporting that they "always" or "almost always" use a helmet when riding a bicycle, with effect sizes on the order of 18 percent. Notably, this study does not include noncyclists and, as such, cannot address whether bicycling rates are different in states that have helmet laws compared to states that do not have these laws. Also, because Rodgers (2002) uses a cross-sectional design, it cannot address concerns about unobserved state-specific characteristics that are correlated with both the presence of a helmet law and higher rates of helmet use.

[4] From 2000 to 2005, four additional states adopted youth bicycle helmet laws.

to children younger than 16) and ages 16–30 (as a specification check) in each state and year from 1991 through 2005.

To model the count nature of the outcome variables, we estimate negative binomial models on annual state-specific fatality counts (Cameron and Trivedi 1998).[5] In these models, we control for average per capita income and the state unemployment rate, and to account for exposure, we also enter the log of vehicle miles traveled per year in each state (U.S. Department of Transportation 1991–2005) and the log of the relevant state-year population. Our policy variable of interest is equal to one in states and years when any helmet law for youths under 16 is in place and zero otherwise.[6] To account for other state policies that may affect crash risk and driving and bicycling opportunities, we also control for variables representing the presence of a state graduated driver licensing (GDL) program with an intermediate phase, a primary enforcement seat belt law, a secondary enforcement seat belt law, a zero-tolerance drunk-driving law, a .08 blood alcohol content per se drunk-driving law, and two speed limits (65 mph and 70 mph or greater) for cars on rural interstates. If a policy variable changed in the middle of the year, we code it as the fraction of the year that the policy was in effect. The models also control for state and year effects such that the coefficient of interest on the helmet law variable is identified from within-state changes in fatalities coincident with adoption of a helmet law, controlling for the associated changes in fatalities for residents of states that did not adopt a helmet law in the same year. Throughout, we cluster standard errors at the state level (Bertrand, Duflo, and Mullainathan 2004).[7]

Our data on bicycling behaviors and helmet use for youths are from parental reports of child outcomes from the Centers for Disease Control and Prevention's (CDC's) Behavioral Risk Factor Surveillance System (BRFSS) over the period 1995–2000. These data include parental reports of bicycling behaviors and helmet use for youths as young as 5. The BRFSS surveys are random-digit-dialing telephone surveys, and adult respondents are asked a series of questions about the safety behaviors of the oldest child in their household between the ages of 5 and 15. Specifically, adults are asked, "How often within the last year did [child's name] wear a helmet when riding a bicycle?" Response options include "never wears a helmet," "rarely wears a helmet," "sometimes wears a helmet," "almost

[5] We use the nbreg procedure of Stata (version 10.1) to estimate a fixed-effects negative binomial model for the fatality analysis and interpret the coefficients as marginal effects. Although fixed-effects negative binomial models can yield inconsistent parameter estimates because of the problem of incidental parameters (Hausman, Hall, and Griliches 1984), fixed-effects Poisson models (which do not suffer from this problem) return results nearly identical to those presented here (Cameron and Trivedi 1998). Moreover, our fixed-effects negative binomial model is supported by recent research (Allison and Waterman 2002).

[6] Alternative fatality specifications that control for the relevant fraction of 0–15-year-olds who should have been directly affected by each helmet law (for example, defining the helmet law variable to equal 11/15 for laws that apply only to youths under 12) return very similar results.

[7] For completeness, we also present robust standard errors without state clustering.

always wears a helmet," and "always wears a helmet."[8] Importantly, adults can also indicate that the youth never rides a bicycle. We exclude a small number of adults who report that they do not know or refuse to answer the bicycle helmet question, and we restrict attention to adults with no missing demographic information. We use responses to this question to create a variable called Bicycle Rider that equals one if the parent chose any of the five responses. To measure helmet use, we follow previous research by creating a measure, Frequent Helmet Use, that equals one if the parent reports that the youth wore a helmet either almost always or always. We also create Continuous Helmet Use, a measure that equals 1 for "always wears a helmet," .75 for "almost always wears a helmet," and so forth, through 0 for "never wears a helmet."

We supplement the parental reports of children's bicycling behaviors using restricted-use versions of the national Youth Risk Behavior Surveillance System (YRBSS) over the period 1991–2005. These surveys are coordinated every other year by the CDC and are administered by paper and pencil to high school students at schools in the spring. These data provide standard demographic characteristics, information on bicycle riding and helmet use, and state of residence (provided by the CDC in response to a confidential request). For the YRBSS analysis, we restrict attention to youths with no missing data for demographic characteristics or helmet use.[9]

Like the BRFSS, the core YRBSS questionnaire in each year since 1991 has included the following question: "When you rode a bicycle during the past 12 months, how often did you wear a helmet?"[10] Response options include "did not ride a bicycle," "never," "rarely," "sometimes," "most of the time," and "always."[11] We consider the same outcomes as in the parental reports: Bicycle Rider and Frequent Helmet Use.

To estimate the effect of the mandatory helmet laws in the BRFSS and YRBSS data, we first follow the basic approach in the fatality model described above and estimate two-way fixed-effects models on the group targeted by most helmet

[8] In the Behavioral Risk Factor Surveillance System (BRFSS) data, the question about helmet use was in an injury control topical module that was asked only in a handful of states for the even-numbered years between 1995 and 2000. For the odd-numbered years, however, we observe large samples of adults across all 50 states and the District of Columbia.

[9] Notably, the national YRBSS survey was not explicitly designed to produce estimates that are representative at the state level. Despite this, previous studies have used these data in state policy evaluations such as ours (see, for example, Gruber and Zinman 2001; Carpenter and Cook 2008; Carpenter and Stehr 2008).

[10] In 1991–97, the core YRBSS questionnaire also included a lead-in question about intensity of bicycle riding in the previous year. Specifically, youths were asked, "During the past 12 months, how many times did you ride a bicycle?" Response options included zero times, between one and 10 times, 11–20 times, 21–39 times, and 40 or more times. Unfortunately, over this shorter sample period we do not have enough state policy changes or sufficient statistical power to estimate precise helmet law effects. Models that were specified in a fashion similar to our main estimates returned results that were qualitatively similar to those we present here (namely, state helmet laws were associated with reductions in youths' bicycling intensity).

[11] Note that these response options are worded in a slightly different fashion than those in the BRFSS questionnaire, although we treat them similarly.

laws—those younger than 16 in the YRBSS (or, alternatively, parents of youths under 16 in the BRFSS).[12] The two-way fixed-effects model for youths 15 or younger in both data sets amounts to ordinary least squares estimation of the following:

$$Y_{ist} = \beta_0 + \beta_1 \boldsymbol{X}_{ist} + \beta_2(\text{Helmet Law})_{st} + \beta_3 \boldsymbol{Z}_{st} + \beta_4 \boldsymbol{S}_s + \beta_5 \boldsymbol{T}_t + \varepsilon_{ist}, \quad (1)$$

where Y_{ist} denotes the outcomes of interest (Frequent Helmet Use, Continuous Helmet Use, and Bicycle Rider).[13] The term $\boldsymbol{X}_{ist}$ is a vector of individual student characteristics that includes age, grade, and race dummies in the YRBSS. In the BRFSS, $\boldsymbol{X}_{ist}$ is a vector of parental characteristics that includes age, marital status, and race dummies. The term $\boldsymbol{Z}_{st}$ is a vector of other potentially relevant state policies and characteristics, including the state unemployment rate, a dummy for the presence of a mandatory seat belt law, and a dummy for any state GDL program with an intermediate phase. Helmet Law is an indicator variable that is equal to one if the respondent lives in a state that had a mandatory helmet law over the previous 12 months.[14] The coefficient of interest, β_2, captures the relative effect of the mandatory helmet law on youth outcomes by comparing within-state changes in outcomes for youths in helmet-law-adopting states coinciding with the law taking effect with the associated changes in outcomes for youths in states that did not experience a policy change in that year. We include unrestricted state and year fixed effects, and we cluster standard errors at the state level.

To complement the difference-in-differences specifications in the YRBSS data, we also explicitly take advantage of the age threshold in the helmet law statutes to estimate difference-in-difference-in-differences (DDD) models (Gruber 1994). Specifically, we use changes in bicycling for youths at or above the age cutoff coincident with helmet law adoption as controls for the associated changes in outcomes for youths below the age cutoff. In practice, this amounts to estimating a variant of equation (1) on the full sample of youths, augmented by including interactions for a treatment group dummy (equal to one for all youths under the state's helmet law threshold) with each state dummy and each year dummy. Importantly, this model also supports a full set of dummies for each state-year

[12] Note that the BRFSS does not ask parents about outcomes for youths who are 16 or older.

[13] Probit models return very similar results.

[14] This time window was chosen to account for the wording of the question. Note that in theory we would want to control for the fraction of the previous year that the youth was covered by a helmet law. This is made difficult by the fact that we do not observe the actual date that the surveys were administered; we simply know that they were done in the spring. We choose to drop from the sample any observations from a state-year combination in which this fractional treatment status is uncertain. In practice, the timing of the effective dates of the laws, combined with the fact that the surveys are done only in odd-numbered years, means that this excludes very few observations. Creating such a fractional helmet law variable with the assumption that all youths are surveyed in, say, March, returned very similar results. In the BRFSS analyses, we are able to account for a handful of state laws that set the relevant helmet law threshold at 12 instead of 16 because of the inclusion of parental reports for children as young as 5. In all the BRFSS models, Helmet Law is coded as one when the referenced youth are covered and zero otherwise.

Table 1

Reduction of Bicycling Fatalities due to Bicycle Helmet Laws: Fatality Analysis Reporting System, 1991–2005

	0–15-Year-Olds	16–30-Year-Olds
Helmet Law	−.189**	−.057
	(.069)	(.078)
	[.086]	[.102]

Note. Regressions are estimated on the state-year count of fatalities among cyclists in the referenced age group. Values are marginal effects, with standard errors clustered at the state level in parentheses. Robust standard errors without state clustering are in brackets. Models include state and year fixed effects and controls for the log of the relevant age-specific population in the state, the log of vehicle miles traveled in the state, average income in the state, the state unemployment rate, and indicators for the presence of a .08 blood alcohol content law, a zero-tolerance law, a graduated driver licensing law, a primary enforcement seat belt law, a secondary enforcement seat belt law, and speed limits (65 mph and 70+ mph). $N = 765$ for each model.

** Significant at 1%.

interaction. The key advantage to this model is that the only assumption required for identification of the helmet law effect is that there are no shocks to outcomes in experimental states that affect youths under the age threshold differently than youths over the age threshold. We implement this model by estimating the following equation:

$$\begin{aligned} Y_{ist} = {} & \beta_0 + \beta_1 \boldsymbol{X}_{ist} + \beta_2(\text{Helmet Law} \times \text{Treatment Group})_{st} \\ & + \beta_3 S + \beta_4 T + \beta_5(\text{Treatment Group} \times T) \\ & + \beta_6(\text{Treatment Group} \times S) + \beta_7(S \times T) + \varepsilon_{ist}. \end{aligned} \tag{2}$$

Treatment Group is an indicator equal to one if the youth is below the state's relevant helmet law threshold. Note that in this model, $\boldsymbol{Z}_{st}$, the vector of state characteristics (such as the unemployment rate) that vary only at the state-year level, drops out of equation (2) since a full set of state-by-year interactions is included.

3. Results

We begin by confirming and extending the result in Grant and Rutner (2004) that mandatory bicycle helmet laws reduce bicycling fatalities. Recall that Grant and Rutner found that helmet laws reduced juvenile fatalities by about 11 percent over the period 1990–2000. Table 1 presents coefficients and standard errors for the Helmet Law indicator from estimation of the fixed-effects negative binomial model of bicycle fatalities. These coefficients can be interpreted as percentage changes in bicycling fatalities associated with adoption of a helmet law. Results are shown for youths who should have been most affected by helmet laws (youths ages 0–15) and those who should have been unaffected by the policies (those

Table 2
Descriptive Statistics: Behavioral Risk Factor Surveillance System Data, 1995–2000

Variable	Weighted Mean
Bicycle Rider	.84
Continuous Helmet Use:	
Always	.38
Almost always	.11
Sometimes	.11
Rarely	.06
Never	.35
Parent demographics:	
Female	.54
Non-Hispanic white	.79
Age	37.5

ages 16–30). The findings in Table 1 confirm the key results from Grant and Rutner (2004) for the 1990s: state bicycle helmet laws were associated with statistically significant reductions (on the order of 19 percent) in youth bicycle fatalities among youths ages 0–15 but were not significantly associated with bicycle fatalities among 16–30-year-olds.[15]

What factors contributed to the reductions in bicycling fatalities associated with the helmet law adoption documented in Table 1? We investigate this issue using helmet use and bicycling behaviors from the BRFSS and YRBSS. We begin by presenting key variable means for the parental reports (BRFSS) in Table 2 and the youths' self-report data (YRBSS) in Table 3. The BRFSS data reveal that approximately 84 percent of parents report that their oldest child ages 5–15 rode a bicycle in the previous year, and about two-thirds of bicycle riders wore a helmet at least some of the time when riding. The YRBSS means in Table 3 show that 71 percent of high-school-age youths report that they rode a bicycle in the

[15] Our estimate of the effect of helmet laws on bicycling fatalities among youths is larger than that reported in Grant and Rutner (2004) (19 versus 11 percent). Personal correspondence with Darren Grant indicates that they examined deaths of all juveniles ages 0–17 as their outcome variable of interest. We choose to focus on 0–15-year-olds since the vast majority of state bicycle helmet laws explicitly apply only to those under age 16. Since we consider 16- and 17-year-olds to be unaffected by the laws, our estimate is less attenuated (that is, larger). Indeed, when we estimate the same specification on deaths among 0–17-year-olds, our estimate is 13 percent, which is considerably closer to that reported in Grant and Rutner. We revisit the magnitude of the effect below, but for comparison purposes note that Dee (2009) finds that motorcycle helmet laws reduce motorcycle fatalities by about 27 percent over roughly this same period. In unreported results (available upon request), we subjected this basic result for fatalities to a number of additional robustness tests: controlling for linear state-specific time trends; controlling for a 2-year lead and a 2-year lag of Helmet Law; dropping each of the helmet-law-adopting states individually; dropping all states in the East North Central, West North Central, and Mountain census divisions; and including only states adopting helmet laws. All of these additional robustness tests continued to return evidence that helmet laws reduced bicycling fatalities of youths under age 16 but not of older adults.

Table 3
Descriptive Statistics: Youth Risk Behavior Surveillance System Data, 1991–2005

Variable	Weighted Mean
Bicycle Rider	.71
Continuous Helmet Use:	
Always	.04
Almost always	.04
Sometimes	.04
Rarely	.06
Never	.83
State adopted helmet law during the sample period	.41
Covered by helmet law	.13
Female	.49
Race/ethnicity:	
Black	.14
Hispanic	.11
Other	.10
Grade:	
9	.27
10	.25
11	.24
12	.24
Age:	
14	.10
15	.24
16	.26
17	.25
18+	.15

previous year. In the YRBSS, however, helmet use is rare among cyclists of high school age: only about 16 percent of these youths report that they wore a helmet when riding a bicycle in the past year. The differences in helmet use across the two data sets are largely a function of the younger age distribution of children in the BRFSS, since bicycling and helmet-wearing rates strongly decline with age in this range.

We provide direct evidence of the effects of helmet laws on helmet use in Table 4. Specifically, we present the coefficients for Helmet Law from two-way fixed-effects models estimated on the full sample (that is, not conditional on bicycle riding in the past year) that control for individual demographic characteristics, time-varying state characteristics, and a full set of state and year fixed effects. We present the results for parental reports of children's helmet use behaviors from the BRFSS and the associated results for high school youths' self-reports from the YRBSS. Estimates are presented from linear probability models of Frequent Helmet Use, and we complement these estimates with models of Continuous Helmet Use. Finally, we show two-way fixed-effects results for Bicycle

Table 4

Effects of Bicycle Helmet Laws on Helmet Use and Bicycling: Behavioral Risk Factor Surveillance System (BRFSS) Data, 1995–2000, and Youth Risk Behavior Surveillance System (YRBSS) Data, 1991–2005

	BRFSS (N = 115,886)	YRBSS (N = 34,014)
Frequent Helmet Use:		
Helmet Law	.107**	.006
	(.027)	(.020)
	[.010]	[.009]
Implied effect as % of prereform mean	34.9	9.7
R^2	.228	.055
Continuous Helmet Use:		
Helmet Law	.100**	.019
	(.024)	(.021)
	[.009]	[.008]
Implied effect as % of prereform mean	28.7	22.9
R^2	.259	.080
Bicycle Rider:		
Helmet Law	−.030*	−.038**
	(.015)	(.013)
	[.009]	[.014]
Implied effect as % of prereform mean	−3.6	−4.7
R^2	.087	.074

Note. Regressions are estimated using the ordinary least squares method. Robust standard errors clustered at the state level are in parentheses; those without state clustering are in brackets. All models include state and year fixed effects and time-varying state characteristics, including the state unemployment rate and dummies for the presence of a mandatory seat belt law and any state graduated driver licensing program with an intermediate phase. Other individual demographic characteristics in the BRFSS models for Frequent Helmet Use include parental age, parental age squared, six parental education dummies, parental gender, a white non-Hispanic dummy, dummies for age of oldest child under 15, and survey month dummies. Other individual demographic characteristics in the YRBSS models for Bicycle Rider include age, grade, and race dummies.

* Significant at 5%.

** Significant at 1%.

Rider. Values are the coefficients for Helmet Law, and standard errors are clustered by state.

The results in Table 4 show that helmet laws did, in fact, increase helmet use. This is particularly evident from the parental reports of children's bicycling behaviors, as reported in the BRFSS. For example, we estimate that adoption of a helmet law increases frequent helmet use by 10.7 percentage points, or approximately 35 percent relative to the prereform mean. We find a much smaller point estimate for the high school youths' self-reports, although notably these are calculated from a much smaller base, such that the estimated proportional effect is nontrivial (about 10 percent). Results for Continuous Helmet Use return more consistent evidence from the BRFSS and YRBSS that helmet laws increased helmet use relative to prereform levels.[16]

In Table 4, we also present the first evidence for our main contribution—namely, that helmet laws also reduce youth bicycling. Specifically, we find in both the parental reports and high school youths' self-reports that helmet laws reduced the probability that a youth rode a bicycle in the previous year by 3–4 percentage points, and both estimates are statistically significant at the 5 percent level.[17] Relative to the prereform means, the estimates from the BRFSS and the YRBSS for whether youths rode a bicycle in the past year suggest helmet law effects on bicycling of about 4 and 5 percent, respectively.[18]

We further investigate the robustness of the main bicycling result for high school youths in Table 5, which presents results for the two-way fixed-effects models and for the augmented triple-difference models with a full set of age-by-state, age-by-year, and state-by-year fixed effects. The models for all youths compare the associated outcomes for high school youths under the helmet law age threshold with those for youths at or above the age threshold (who were therefore unaffected) coincident with policy adoption, and we show the estimate on the coefficient of interest from equation (2) on Helmet Law × Treatment Group. The baseline difference-in-differences (DD) estimate reproduces the

[16] In unreported results (available upon request), we found that the control variables for the helmet use models were reasonable and largely consistent with previous research. In the parental reports, we found that more highly educated parents were significantly more likely than less educated parents to report that their child wore a bicycle helmet. White parents were also significantly more likely than nonwhite parents to report helmet use by their child. Older parents were significantly more likely than younger parents to report helmet use by their child. In the youths' reports, we found that white students were significantly more likely than black or Hispanic youths to report wearing a bicycle helmet, but there was no gender difference in helmet use.

[17] In unreported results (available upon request), we found that the control variables for the bicycle-riding models were reasonable and largely consistent with previous research. In the parental reports, we found that more highly educated parents were significantly more likely than less educated parents to report that their child rode a bicycle. White parents were also significantly more likely than nonwhite parents to report bicycle riding by their child. Older parents were significantly more likely than younger parents to report bicycle riding by their child. In the youths' reports, we found that white students were significantly more likely than nonwhite students to report riding a bicycle, and males were significantly more likely than females to be bicycle riders.

[18] In unreported results (available upon request), we found a small and statistically insignificant association between state graduated driver licensing (GDL) programs and youth bicycling in the YRBSS. This null finding is important since it is inconsistent with a plausible confounding story—namely, that changes in driving availability (which are directly regulated by GDL program adoptions) might induce a substitution across modes of transportation that would bias our main bicycling result. See, for example, Dee, Grabowski, and Morrisey (2005) for a description of GDL programs. In addition to the null empirical finding on GDL in the bicycling regression, it is helpful to note that GDL programs were much more widespread than helmet laws over this time period. Moreover, GDL programs do not strictly adhere to the distinction of being older or younger than 16 in the same way that helmet laws do. This is because although GDL programs have common features (such as requiring intermediate phases for obtaining a driver's license), states are still free to set the minimum age at which one can obtain a learner's permit. This varies substantially across states, which further casts doubt on the ability of driving availability to explain our main bicycling result. Because we do not observe the respondent's exact age, controlling for when the state adopted a GDL program is the best we can do to control for driving availability. Unfortunately, the YRBSS data do not contain consistent questions about driving behaviors or license status.

Table 5

Effect of Bicycle Helmet Laws on Youth Bicycling: Youth Risk Behavior Surveillance System Data, 1991–2005

					Excluded States		
	Baseline	15- and 16-Year-Olds	Baseline, with 2-Year Lead	Baseline, Adopter States Only	East North Central, West North Central, and Mountain Census Divisions	California	Florida
Youths 15 or younger (DD):							
Helmet Law: 2-year lead			.016				
			(.019)				
Helmet Law	−.038**	−.040**	−.046**	−.039*	−.046**	−.041**	−.033*
	(.013)	(.014)	(.010)	(.015)	(.015)	(.015)	(.013)
	[.014]	[.017]	[.018]	[.020]	[.015]	[.015]	[.015]
R^2	.074	.075	.074	.080	.075	.074	.074
N	34,014	24,194	34,014	14,678	25,794	29,001	31,979
All youths (DDD):							
Helmet Law × below helmet law threshold	−.031*	−.047*		−.055*	−.033*	−.038*	−.037*
	(.015)	(.018)		(.019)	(.016)	(.016)	(.017)
	[.018]	[.027]		[.028]	[.020]	[.020]	[.020]
R^2	.099	.092		.101	.104	.102	.100
N	109,804	52,394		45,707	84,388	94,818	103,645

Note. Regressions are estimated using the ordinary least squares method. Robust standard errors clustered at the state level are in parentheses; those without state clustering are in brackets. All models include state and year fixed effects; age, grade, and race dummies; and time-varying state characteristics, including the state unemployment rate and dummies for the presence of a mandatory seat belt law and any state graduated driver licensing program with an intermediate phase. The baseline estimate corresponds to the YRBSS estimate for Bicycle Rider in Table 4. DD = difference in differences; DDD = difference in difference in differences.

* Significant at 5%.

** Significant at 1%.

YRBSS estimate for Bicycle Rider in Table 4, showing that helmet laws reduced bicycling among youths ages 15 and younger by a statistically significant 3.8 percentage points. The associated difference-in-difference-in-differences (DDD) estimate for the full YRBSS sample confirms that the DD finding on bicycling in Table 4 is robust to incorporating the bicycling behaviors of slightly older youths. Specifically, our DDD estimate suggests that helmet laws reduced bicycling by a statistically significant 3.1 percentage points.

Table 5 also confirms that the bicycling reduction noted in the YRBSS is robust to other reasonable changes in specification and sample. When we restrict attention to 15- and 16-year-olds, who should be the most directly comparable, we continue to find that helmet laws significantly reduce youth bicycling. We estimate a DD model that, in addition to the usual helmet law indicator, includes a 2-year lead variable that captures the possibility that helmet laws were adopted in response to some systematic change in bicycling behaviors within adopting states, potentially affecting the interpretation of our helmet law effects. Despite this possibility, we find that including the 2-year lead variable does not change our main result that helmet laws reduced bicycling, and the coefficient on the lead variable is small and statistically insignificant. We also restrict attention to youths living in states that ever adopted a helmet law, another standard robustness check. This approach returns DD and DDD estimates similar to the baseline, and they are both statistically significant. This confirms that our bicycling estimates are not driven by offsetting changes in behavior in control states coincident with helmet law adoptions.

Table 5 also addresses concerns about potential outliers and the geographic distribution of helmet law states. Figure 1 shows the set of states that adopted helmet laws over our sample period. Notably, the states are largely concentrated on the coasts, with no states in the Midwest or mountain areas that ever adopted statewide helmet laws. In Table 5, we estimate models on a sample that excludes states in divisions that contain no helmet law adopters (East North Central, West North Central, and Mountain). The intuition here is that the set of control states is arguably more like the set of treatment states when we restrict attention to states within the same census divisions; doing so has little effect on the bicycling estimates, and they remain statistically significant. We also exclude California and Florida, in turn, from the sample. These states adopted a helmet law over this time period, and each contributes more than 1,500 observations to the full sample. Ensuring that these large states are not driving the bicycling estimates, as confirmed by excluding them, is an important robustness check. In results not reported here, we found that excluding each other helmet law adopter state individually did not materially alter our main finding that helmet laws reduced youth bicycle riding in the YRBSS data.[19]

[19] In unreported results (available upon request), we also examined a number of other placebo outcomes that should not have been directly affected by helmet laws (for example, seat belt use, motorcycle riding, smoking, drinking, and sexual activity) to ensure that our observed effects are not simply proxying for contemporaneous changes in (un)healthy behaviors that, while unlikely in

4. Discussion and Conclusion

The results presented above show that state laws that require youths to wear helmets when riding a bicycle have reduced youth bicycling fatalities by about 19 percent, increased helmet use by 20–34 percent, and (unintentionally) reduced bicycling by 4–5 percent. Given that the forgone utility of riding a bicycle would seem relatively substantial, why do helmet laws lead to reduced cycling? There are several possibilities. First, the costs of helmet use are likely nontrivial for some youths. In addition to the direct monetary costs of bicycle helmets (usually between \$10 and \$40), there are likely to be substantial social costs (recall that survey evidence shows that youths do not like wearing helmets primarily because they are "uncool"). These costs are likely to be magnified if there are significant peer effects. Second, there is evidence that youths have suboptimally high discount rates (Gruber 2001), such that some youths might place too little weight on the expected gain in future utility from the prevention of injury or death relative to the costs of wearing helmets today. Finally, note that there are several reasonably close alternatives to bicycling (such as skateboarding and in-line skating) that are not regulated in the same way with respect to mandated helmet use. Helmet laws change the relative prices among these activities, such that the robust reduction in bicycling that we observe may be at least partially offset by increases in other related activities.[20]

Are the magnitudes of our results plausible given what we know about the technological efficacy of bicycle helmets and the changes in helmet use induced by helmet laws? To understand what reduction in fatalities would be inferred from our estimated increases in helmet use, we use Evans's (1987) basic calculation for seat belt effectiveness that uses information on the technological efficacy of helmets. This calculation shows that $F = (E\Delta h) / (1 - Eh)$, where F is the proportional fatality reduction, E is the technological efficacy of bicycle helmets, and h is the helmet use rate. Note that about 53.25 percent of our sample of 5–15-year-olds from the BRFSS wear helmets (using the continuous

our empirical framework, could bias our estimated helmet law effects. We found no consistent evidence that bicycle helmet law adoption was associated with economically or statistically significant effects on any of the other risky behaviors. These null findings further reinforce our main result that helmet laws reduce bicycling. Finally, we also examined the possibility that helmet laws—by reducing bicycling—could have resulted in other negative health spillovers, such as lack of exercise and increases in obesity. Unfortunately, we do not observe consistent measures of body weight or sedentary activity (such as hours of television viewing) over the sample period of 1991–2005. Using the same empirical approach as in Table 5, however, we find no evidence that helmet laws reduced physical activity as measured by (1) participation in hard physical exercise for at least 20 minutes on any day in the previous week, (2) participation in muscle-strengthening activities (for example, weight lifting or push-ups) on any days in the previous week, (3) exercising for the purpose of losing weight or maintaining current weight, and (4) participation in team sports. These results are available upon request and suggest that although helmet laws reduce bicycling, they do not appear to have large spillovers to other measures of physical activity for youths.

[20] Unfortunately, we do not observe measures of participation in these activities in our data. We also cannot credibly identify in the Fatality Analysis Reporting System fatalities resulting from these activities to directly test this risk compensation hypothesis.

helmet use measure), and Table 4 shows that helmet laws increase helmet use by about .10. We are not aware of estimates from the public health or medical literatures that directly estimate the technological efficacy of bicycle helmets in reducing fatalities, although Thompson et al. (1989) use case-control methods to find that bicycle helmets reduce serious head and brain injuries by more than 80 percent; they suggest that the efficacy is likely even greater for youths. Perhaps the closest comparison regarding helmet efficacy in the context of fatalities comes from Dee's (2009) motorcycle helmet study, which uses Evans's double-pairs comparisons to find that motorcycle helmets reduce motorcycle fatalities by 34 percent. If bicycle helmets are as effective as motorcycle helmets, we would expect helmet laws to reduce bicycling fatalities by about 4.2 percent ($[.34 \times .1]/[1 - .34 \times .5325]$) in the absence of any reduction in risk exposure.

Our fatality estimate of 19 percent is substantially larger than this, although several factors may explain the difference. First, our fatality estimate has a relatively large standard error, such that we cannot rule out that the true fatality effect is substantially smaller than our reported estimate. Second, the 4.2 percent implied reduction in fatalities does not take into account the direct reduction in risk exposure associated with reduced bicycling that should independently reduce fatalities.[21] Third, if bicycle helmet laws increase police attention to bicycling routes, then we would expect an additional fatality reduction associated with more careful driving along those enforced routes that has nothing to do with helmet technology or helmet use per se. Fourth, parents may purchase other safety equipment, such as blinking lights or reflective clothing, for their child when they buy him or her a helmet to comply with a newly passed helmet law. If such equipment prevents accidents or reduces their severity, then its purchase could substantially augment the effectiveness of helmet laws. Finally, our estimated fatality effect is comparable in magnitude to credible estimates from public policy in related settings; multiple recent evaluations of motorcycle helmet laws find that they reduce motorcycling fatalities by 27–33 percent (see, for example, Dee 2009; Houston and Richardson 2008). Moreover, as in our setting for bicycle helmet laws, Dee shows that motorcycle helmet laws induce larger changes in fatalities than can be directly implied by reasonable changes in helmet use and estimates of helmet efficacy.

The current study has several important limitations. First, we do not control

[21] Although we estimate that bicycling participation decreased by about 5 percent, it is likely that the overall number of bicycling miles travelled decreased even more. Unfortunately, neither the YRBSS nor the BRFSS asked consistent questions about bicycling intensity over the full sample period. As noted previously, however, the YRBSS did ask about the number of instances of bicycle riding from 1991 to 1997. We estimated equation (1) on this outcome (using the midpoints of the ranges and coding the top category as 50 instances) and found that helmet laws reduced bicycling among high school youths 15 and under by 2.34 instances, or approximately 11 percent relative to the prereform mean of 21.32 instances. This suggests that the true overall reduction in bicycling miles travelled—and thus exposure to potential bicycling accidents—is larger than our bicycling participation estimates in Tables 3 and 4. These estimates are of course based on fewer state changes and, as such, are less precise than the results for bicycle riding, which we observe over the entire sample period from 1991 to 2005 in the YRBSS.

for the arguably important effects of enforcement, outreach, or media campaigns because we are not aware of a source that consistently tracks these efforts at the state level over our sample period. Efforts of this type, such as helmet giveaways, are of concern if they are correlated with adoption of a mandatory helmet law. Notably, however, this type of bias is not likely to be the main determinant of our estimated effects on bicycling behavior, since helmet giveaways should, if anything, increase bicycling. Given that we find significant decreases in bicycling participation, this suggests that our estimates for bicycling are likely understated with respect to outreach campaigns. Second, all of the bicycling data are self-reported, either by parents (in the BRFSS) or students (in the YRBSS). Although we find it plausible that youths and parents might be more likely to falsely report helmet use in the presence of a helmet law (perhaps because stigma associated with not wearing a helmet has increased), this type of bias is unlikely to affect our estimates of bicycling participation. Third, we lack important data that would allow us to provide a more comprehensive assessment of helmet laws. Information on helmet ownership, for example, would be extremely useful since research has shown a strong concordance between ownership and use (Schieber et al. 1996). This information could be important for understanding the likely relative effectiveness of helmet giveaways versus media campaigns to reduce the stigma of wearing a helmet (that is, if the main effect is driven by helmet availability, giveaways could be particularly effective). Finally, information on other adverse outcomes (such as head injuries, emergency room visits, and hospitalizations) is necessary for a complete accounting of the costs and benefits of helmet laws.

Despite these limitations, our research provides the first comprehensive quasi-experimental evaluation of the effects of helmet laws on bicycling and helmet use among youths. As other states consider helmet laws as a way to reduce bicycling-related injuries and fatalities, they should keep in mind that although the laws increase helmet use and reduce fatalities, they are also likely to reduce bicycling among the targeted group. Although it is possible that the public health benefits from mandating helmet laws may outweigh the reductions in utility associated with less bicycling, future research evaluating the full costs and benefits of these policies should acknowledge these effects.[22]

[22] Here we provide a very rough back-of-the-envelope calculation of these effects. In 2005, there were 103 bicycle fatalities among 0–15-year-olds in the 36 states without a helmet law applying to youths younger than 16. We estimate that helmet laws would reduce these fatalities by 19 percent, or 19.57 bicycling fatalities per year. Using a value of a statistical life of approximately $8 million in 2005 dollars (Viscusi and Aldy 2003), the annual benefit from saving these additional lives would be roughly $157 million ($8 million × 19.57 lives). There are approximately 41.9 million youths ages 0–15 in these states without helmet laws. Given that 84 percent of youths ages 5–15 ride bicycles and 35 percent of these youths do not wear helmets, we estimate that a helmet law would potentially constrain about 12.3 million youths (41.9 million × .84 × .35). Alternatively, if we consider only those youths whose helmet use increases as having been actually constrained, then we estimate that helmet laws constrain approximately 3.5 million youths (41.9 million × .84 × .10). Assuming a real discount rate of 4 percent and a 30-year time horizon, we estimate the present discounted value of the benefit to be roughly $800 for each cyclist whose helmet use increases in response to the law. Note that this calculation ignores other benefits from the laws (such as injury reductions) as well as other costs (such as the value of forgone cycling).

Appendix

Effective Dates of State Bicycle Helmet Laws

Table A1

State Bicycle Helmet Laws for Youths

State	Threshold Age	Effective Date
Alabama	16	September 19, 1995
California	5	1987
California	18	October 8, 1993
Connecticut	15	October 1, 1993
Connecticut	16	May 14, 1997
Delaware	16	April 1, 1996
District of Columbia	16	May 23, 2000
Florida	16	January 1, 1997
Georgia	16	July 1, 1993
Hawaii	16	January 1, 2001
Louisiana	12	March 1, 2002
Maine	16	September 18, 1999
Maryland	16	October 1, 1995
Massachusetts	5	1990
Massachusetts	13	March 8, 1994
Massachusetts	17	November 25, 2004
New Hampshire	16	January 1, 2006
New Jersey	14	July 1, 1992
New Jersey	17	March 1, 2006
New York	5	1989
New York	14	June 1, 1994
North Carolina	16	October 1, 2001
Oregon	16	July 1, 1994
Pennsylvania	5	1991
Pennsylvania	12	February 1, 1994
Rhode Island	9	July 1, 1996
Rhode Island	16	1998
Tennessee	12	January 1, 1994
Tennessee	16	2000
West Virginia	15	July 1, 1996

Note. Information on helmet laws was determined through comparisons of multiple sources, including the Bicycle Helmet Safety Institute, the Snell Memorial Foundation, Safe Kids USA, the Insurance Institute for Highway Safety, and various Lexis-Nexis and legislative document searches. Note that we do not use all of these helmet laws in all of the empirical work. For the Youth Risk Behavior Surveillance System analyses, we use laws applying to youths of high school age (14–18 years). For the Behavioral Risk Factor Surveillance System analysis, we use laws applying to youths ages 5–15. In cases where we could not identify the exact effective date and where such a choice would affect our definition of treatment and control groups, we excluded the small fraction of youths for whom treatment status was uncertain.

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