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Intended and Unintended Effects of Youth Bicycle Helmet Laws

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ABSTRACT

Over the past 15 years, 21 states have adopted laws requiring youths under a certain age (generally 16) to wear a helmet when riding a bicycle. Previous research finds that these laws significantly reduced youth bicycling fatalities, and the prevailing view is that fatalities fell because helmet use increased. In this paper we confirm that helmet laws reduced fatalities by about 9 percent, but we uncover robust evidence of an alternative and unintended mechanism: helmet laws significantly reduced youth bicycling by about 5 percent. We find this result in standard two-way fixed effects models of self-reported cycling behaviors, as well as in augmented triple difference (DDD) models that explicitly account for cycling behaviors of youths just above the helmet law age threshold. The reduction in cycling also obtains using independent samples of parental reports of child bicycling behaviors. Our evidence on the effects of helmet laws on youth helmet use is mixed, though in all cases we find that the true effects on helmet use are less than one-third of the size implied by previous approaches common in the public health literature. These results illustrate the unintended consequences of a well-intentioned public health policy.

Keywords: bicycle helmet laws, bicycle helmet use, bicycling, quasi-experiment

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1. Introduction

Ever year, emergency departments in the United States treat hundreds of thousands of bicycle-related injuries, with several hundred resulting in deaths – usually due to head injuries (Rodgers 2000). Some large fraction of these deaths would likely have been preventable 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, Thompson, Rivara, and Thompson 1989). Despite this evidence, however, rates of bicycle helmet use are low, particularly among youths: over 80% of high school age youths in the United States who report that they rode a bicycle in the past 12 months also reported that they *never* wore a helmet when they rode a bike. 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 "fit" problems generally include laments that helmets are too tight and induce sweating (AAA 1995).

Because the life-saving effects of helmets are well-accepted in the medical community, state and local governments have reason to try to increase helmet use. As with many public health policies, state efforts in this area have reflected two distinct types of approaches: "carrots" and "sticks". Examples of the former include: bicycle helmet give-aways, educational outreach about the life-saving effects of helmets, and media campaigns designed to change social norms regarding bicycle helmets (e.g. to make it seem "cool" to wear a bike helmet). The most direct example of the "stick" approach has been to adopt laws that require youths to wear a helmet when riding a bicycle, a policy adopted by 21 US 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 US youth population is now covered by a state bicycle helmet law. Most of these laws apply to youths under age 16 and impose modest penalties for violations (e.g. verbal warnings, counseling, or a small fine). A recent study evaluating the effects of state helmet laws returned evidence that the laws significantly reduced youth bicycle fatalities over the 1990s by about 11 percent (Grant and Rutner 2005).

The prevailing view in the public health and medical communities is that the fatality reductions brought about by mandatory helmet laws have been generated by increases in helmet wearing. Evidence in favor of this hypothesis has taken a variety of forms, including: 1) cross-sectional observational comparisons of helmet wearing rates in communities with and without helmet laws; 2) cross-sectional telephone surveys where parents are asked about the helmet use of their children in places with and without helmet laws; and 3) pre/post observational evaluations of helmet wearing rates in communities that adopted helmet laws (for reviews of the literature see Towner et al. 2002 and Karkhaneh et al. 2006).

All of these approaches have important limitations, however. The cross-sectional studies (whether observational or telephone-based) suffer from the usual concerns about bias associated with unobserved characteristics of communities that have adopted helmet legislation, while the observational studies (cross-sectional or pre/post) are subject to criticisms about variations in the conditions of observation (e.g. different observers, weather, or road conditions, etc.) as well as the inability to account for demographic characteristics that have been shown to be correlated with bicycling and helmet use behaviors such as income, race, and sex. The pre/post evaluations also have questionable external validity given that they are typically restricted to evaluations at a handful of sites. Finally, even the most comprehensive evaluations of helmet use face the difficulty that other public health campaigns are likely to be correlated with helmet law adoption.

When states adopt bicycle helmet laws, for example, they are likely to have free helmet give-aways, media campaigns to increase awareness about the lifesaving benefits of helmets, and other helmet-related public health initiatives. Failure to account for these other campaigns that are likely to be correlated with helmet law adoptions will systematically overstate the effects of the laws at increasing helmet use and reducing cycling fatalities.

In addition to these concerns about the evidence on helmet use, there is another important limitation of the existing research on helmet legislation. Specifically, the literature has not credibly tested an alternative mechanism behind the observed fatality reductions associated with policy adoption: specifically, by increasing the costs of bicycling the policies may have reduced bicycling participation.[1] This possibility of decreased bicycling has been raised by a handful of researchers in the prevention literature, though there is a paucity of relevant peer-reviewed published evidence.[2] If such a reduction in bicycling occurred, then even in the absence of any true behavioral effect of helmet legislation on helmet use, the reduction in cycling by non-helmet wearers will result in a compositional increase in the fraction of bicycle riders who wear helmets.

We fill this gap in the literature by providing the first comprehensive quasi-experimental estimates of the effects of mandatory bicycle helmet laws in the US on bicycling fatalities, helmet use, and cycling behavior among youths. We first confirm and extend previous evaluation research on cycling fatalities by showing that helmet laws significantly reduced bicycling fatalities among 12-15 year olds by 9.4 percent but had no effects on slightly older youths age 16-19 who were not directly covered by the laws.

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[1] 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.

[2] Specifically, evidence for a reduction in cycling has come primarily from observational studies in Australia. This evidence is referred to in several commentaries by Robinson (2003, 2006, and others), but none has appeared in the peer reviewed literature; these results are instead in government reports. A peer reviewed study by Macpherson et al. (2001) did not find evidence of systematic changes in youth cycling volume associated with helmet law adoption in Ontario, Canada.

To determine the mechanisms underlying the fatality reduction we estimate similar models of self-reported cycling participation and helmet use. Our most robust finding using both youth self-reports and parental reports of child behavior is that state helmet laws significantly reduced youth cycling participation. These evaluations – like our fatality models – are based on models that condition on fixed effects for survey year and state, effectively identifying the helmet law effects from within area changes in outcomes for residents of states adopting laws compared to the associated changes for youths in places that did not adopt a law in that same year. Our analysis of high school youths goes further by showing that the reduction in youth cycling obtains in triple difference models that rely on within-state differences in cycling outcomes for youths under (treatment) and over (control) the helmet law age threshold coincident with policy adoption. In contrast, we find mixed evidence regarding the effects of helmet laws on helmet use: while there is evidence of an increase in helmet use from the parental reports of child behavior, there is no helmet law effect on helmet use in the student self-reports. In both data we mimic the most common approaches used in the public health and medical literatures, and we consistently find that these approaches dramatically overstate the effects of the laws on helmet use by failing to account for: 1) time invariant characteristics of the places that adopt helmet laws and 2) reductions in cycling associated with helmet law adoption. Overall, our results highlight that a full cost benefit analysis of the laws should take into account the previously ignored reductions in cycling associated with helmet law adoption. More generally, our results highlight the unintended consequences of a well-intended public health policy.

The paper proceeds as follows: Section 2 briefly describes the previous literature, and Section 3 outlines the data and empirical approach. Section 4 presents the results, Section 5 offers a discussion, and Section 6 concludes.

2. Previous Literature

There is a large body of research examining the effects of bicycle helmet laws on helmet use, mainly in the public health and medical literatures. In fact, multiple reviews of the literature have found that helmet laws have increased helmet use among the groups targeted by the laws (see, for example, Attwell et al. 2001, Towner et al. 2002, and Karkhaneh et al. 2006). The evidence for the effects of helmet laws on helmet use generally come from observational studies of cyclists, both across cities with and without helmet legislation as well as within cities before and after adoption of helmet laws. There are, however, no studies that evaluate the effects of multiple state bicycle helmet laws on helmet use in an explicitly quasi-experimental framework using large samples and a unified empirical framework.

Another limitation of the existing research on helmet laws is that there is very little evidence evaluating the effects of helmet legislation in the US, despite its fairly wide coverage of laws. Instead, the large public health literature has been focused mainly on laws in Australia and Canada. Only one study to our knowledge uses data on youths living in multiple US states to evaluate the relationship between state helmet laws and youth helmet use.[3] Rodgers (2002) used a telephone survey of approximately 900 young cyclists across several states that did and did not have helmet laws. That study found that, after controlling for individual demographic

[3] There are some notable US studies that have focused on single-state evaluations in states such as Maryland, Georgia, and Oregon. Ni et al. (1997), for example, provide a comprehensive assessment of Oregon's law using observational studies, classroom surveys, and telephone surveys of parents regarding their children's behaviors.

characteristics, state helmet laws were associated with statistically significant increases in the fraction of youths reporting that they "always" or "almost always" use a helmet when riding a bike, with effect sizes on the order of 18 percent. Notably, this study did not include non-cyclists, and as such could not address whether cycling rates were different in states that had helmet laws compared to states that did not have these laws. Also, because the Rodgers (2002) study used a cross-sectional design, it could not address concerns about omitted variables bias. That is, there may be unobserved characteristics about youths living in states with helmet laws (e.g. how "cool" it is to wear a helmet) that are correlated with both the presence of a helmet law and higher rates of helmet use.

One standard method to address this type of concern is a quasi-experimental research design, in which within-area changes in helmet use and cycling behaviors coincident with adoption of a helmet law are compared with the associated changes in the same behaviors for individuals living in areas where the helmet policy did not change. When states adopt helmet laws in different years, it is possible to use a two-way fixed effects framework, or a difference-in-differences type model, in which unrestricted controls for state and year effects are included. If the confounding factors referred to above are time invariant within states, the two-way fixed effects estimator will return unbiased estimates of the effects of helmet laws.

Notably, this type of research design has been employed to evaluate the effects of helmet laws on bicycle fatalities among youths. Grant and Rutner (2004) use data from the Fatality Analysis Reporting System and a two-way fixed effects framework. They estimate that state helmet laws reduce youth bicycling fatalities by about 11 percent over the 1990-2000 period net

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Across all measures, they found that Oregon's helmet law increased helmet use, though they did not consider effects on bicycling.

of state and year dummies and a handful of other state characteristics such as the unemployment rate and the presence of a mandatory seatbelt law.

3. Data Description and Empirical Framework

We use several different data sources to estimate the effects of mandatory helmet laws on youth outcomes. We first revisit the relationship between helmet laws and cycling fatalities by updating Grant and Rutner's estimates with data from 1991-2005 from Fatality Analysis Reporting System (FARS).[4] (FARS) contains data on a census of fatal bicycle-related crashes within the 50 States and the District of Columbia. Using these data, we create counts of the number of fatalities of bicycle riders among youths age 12-15 in each state and year from 1991 through 2005.[5] Recall that most state helmet laws required riders under age 16 to wear a helmet.

To model the count nature of the outcome variables we estimate negative binomial models on annual state-specific fatality counts (Cameron and Trivedi 1998).[6] Specifically, we estimate models of the form:

$$(1)\qquad Y_{st} = \beta_0 + \beta_1 X_{st} + \beta_2(\text{Mandatory Bicycle Helmet Law})_{st} + \beta_3 Z_{st} + \beta_4 S + \beta_5 T + \varepsilon_{st}$$

where X is a vector of state-specific demographic characteristics including average per capita income, the state unemployment rate, and the log of vehicle miles traveled per year in each state.[7] To account for exposure, X also includes the log of the relevant state/year population (12-15 year olds). Mandatory Bicycle Helmet Law is equal to one in states and years when a helmet

[4] From 2000-2005, four additional states adopted youth bicycle helmet laws (Appendix B).

[5] Ideally, we would use fatalities of those aged 14-15 since this is the age group that is "treated" by the helmet laws and that appears in our student reported helmet use data. We expanded down to age 12 because models with just 14 and 15 year olds did not converge.

[6] Although both poisson and negative binomial models are appropriate for count data, we select a negative binomial model because tests reject that the mean and variance of the dependent variable are the same, a key assumption of the poisson model.

[7] Vehicle miles traveled come from the 1991-2005 issues of the US Department of Transportation publication *Highway Statistics*.

law applying to 12-15 year olds is in place. To control for other state policies that may have affected crash risk and driving and cycling opportunities, we include in the vector Z controls for: the presence of a state graduated driver licensing program with an intermediate phase, the presence of a primary enforcement seatbelt law, the presence of a secondary enforcement seatbelt law, the presence of a Zero Tolerance drunk driving law, the presence of a .08 BAC per se drunk driving law, and dummy variables for speed limits (65mph, and 70mph or greater) pertaining to cars on rural interstates. For all policy variables – including the helmet law indicator – we use fractional values for policies that were implemented mid-year. The models also include a full vector of state and year dummies (S and T vectors, respectively), and we cluster standard errors at the state level (Bertrand, Duflo, and Mullainathan 2004). Because the coefficient estimates from the negative binomial model are not easily interpretable, we present the associated marginal effects.

Our primary data on cycling behaviors and helmet use are from 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 Centers for Disease Control and are administered by paper and pencil to high school students at school 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 a confidential request). For the YRBSS analysis we restrict attention to youths with no missing data on demographic characteristics or helmet use.[8]

Specifically, 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

[8] Appendix Table 3 shows which states are represented in the national YRBSS data in each year, as well as when helmet laws were adopted. Notably, the YRBSS survey was not explicitly designed to produce estimates that are representative at the state level. Despite this, previous researchers have used the YRBSS in state policy evaluations

wear a helmet?"[9] Response options include: "Did not ride a bicycle", "Never", "Rarely", "Sometimes", "Most of the time", and "Always". We use responses to this question to create a variable called Bicycle Rider that equals one if the youth chose any of the latter five responses. To measure helmet use, we create a measure called Frequent Helmet Use that equals one if the youth reports she wore a helmet "Most of the time" or "Always".[10]

To estimate the effect of the mandatory helmet laws in the YRBSS data, we take several approaches. First, we follow the basic set-up in the fatality model described above to estimate two-way fixed effects models on the group targeted by most helmet laws – those under age 16. The two-way fixed effects model for 15 year olds and younger amounts to a linear probability model estimation of the following:

(2) $Y_{ist} = \beta_0 + \beta_1 X_{ist} + \beta_2(\text{Mandatory Bicycle Helmet Law})_{st} + \beta_3 Z_{st} + \beta_4 S + \beta_5 T + \varepsilon_{ist}$

where Y_{ist} are the outcomes of interest (Bicycle Rider, Infrequent Helmet Use, Always Wears Helmet).[11] X_{ist} is a vector of individual student characteristics that includes: age dummies, grade dummies, and race dummies. Z is a vector of other potentially relevant state policies and characteristics, including: the state unemployment rate, a dummy for the presence of a mandatory seatbelt law, and a dummy for any state graduated driver licensing program with an intermediate phase. As with the fatality models, we include unrestricted state and year fixed effects, and we again cluster standard errors at the state level.

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such as ours (see, for example, Gruber and Zinman 2001, Carpenter and Cook 2007, Carpenter and Stehr 2007, and others).

[9] In 1991-1997, 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?," with response options including: 0 times, 1 to 10 times, 11 to 20 times, 21 to 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 meaningful helmet law effects. Models that were specified in a similar fashion to our main estimates returned results that were qualitatively similar to those we present below (i.e. state helmet laws were associated with reductions in youth bicycling intensity).

[10] Alternative measures of helmet use all yielded very similar results (e.g. "never" wore a helmet, a continuous helmet use variable, and others).

Mandatory Bicycle Helmet Law is an indicator variable equal to one if the respondent lives in a state that had a mandatory helmet law over the previous 12 months.[12] 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 adopting states coinciding with the law taking effect to the associated changes in outcomes for youths in states that did not experience a policy change in that year. We estimate equation (1) on the sample of youths age 15 and younger – the "treatment" group who was targeted by the laws; importantly, we also estimate equation (1) on the sample of youths age 16 and older to evaluate the possibility that helmet laws are associated with broad changes in bicycling behaviors that are not attributable to direct effects of the laws per se. Since youths aged 16 and older were not legally subject to the helmet laws, we would expect β_2 to be smaller for this sample than for the younger youths. Put differently, a finding that β_2 for the older youths was similar in sign and magnitude to the associated estimate for youths under the helmet law threshold would suggest the presence of other broad trends associated with adoption of helmet laws instead of direct effects of the laws on outcomes.

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 models (Gruber 1994). Specifically, we use changes in outcomes for youths just above the age cutoff coincident with helmet law adoption as controls for the

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[11] Models estimated using probit returned very similar results.

[12] This time window is 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 the youth was covered by a helmet law. This is made difficult by the fact that we do not observe the actual date the surveys were administered; we simply know they were done in the spring. We choose to drop from the sample any observations from a state/year combination where 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 only done 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.

associated changes in outcomes for youths just below the age cutoff. In practice, this amounts to estimating a variant of equation (2) 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 interaction. The key advantage to this model is that the only assumption required for clean identification of the helmet law effect is that there are no shocks to outcomes in experimental states that affect kids under the threshold differently than kids over the threshold. We implement this model by estimating the following equation:

(3) $Y_{ist} = \beta_0 + \beta_1 X_{ist} + \beta_2(\text{Mandatory Bicycle Helmet Law*Treatment Group})_{st} + \beta_3 S + \beta_4 T + \beta_5(\text{Treatment Group * T}) + \beta_6(\text{Treatment Group * S}) + \beta_7(S*T) + \varepsilon_{ist}$

where all variables are as defined above. Treatment Group is an indicator equal to one if the youth is below her state's relevant helmet law threshold. Note that state characteristics (such as the unemployment rate) that vary only at the state/year level fall out of the above specification since a full set of state by year interactions are included.

Finally, we complement the youth self-reports with independently collected data on parental reports of child bicycling and helmet use behaviors from the CDC's Behavioral Risk Factor Surveillance System (BRFSS) over the period 1995-2000. These data include reports of child bicycling behaviors and helmet use for youths as young as age 5. The BRFSS 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 included: "Never wears a helmet", "Rarely wears a helmet", "Sometimes wears a helmet", "Almost always wears a helmet", and "Always wears a helmet".[13] Importantly, adults can also indicate that the youth never rides a bicycle. We exclude a small number of adults who report that they don't know or refused a response to the bicycle helmet question, and we restrict attention to adults with no missing demographic information. We consider the same outcomes in the parental reports as indicated in the youth self-reports: Bicycle Rider and Frequent Helmet Use (always or almost always wore a helmet). Because we do not observe youths over most states' helmet law thresholds (i.e. age 16 and older), we restrict attention to difference in differences models.[14] We also have a more limited set of years covered in the BRFSS, so the DD models are more appropriate.

4. Results

We begin by confirming and extending the result in Grant and Rutner (2000) that mandatory bicycle helmet laws reduced cycling fatalities. Recall that Grant and Rutner found that helmet laws reduced juvenile fatalities by about 11 percent over the period 1990-2000. We restrict attention to youths age 12-15 and 16-19 over the period 1991-2005 (to match the age range and time period of our individual level survey data from YRBSS and BRFSS).

We present the main results on bicycle fatalities in Table 1. Each entry in the table shows the coefficient and standard error on the Mandatory Bicycle Helmet Law indicator from negative binomial estimation of equation (1), a model that includes a full set of state and year fixed effects and the other time-varying state policies and characteristics included in the Z vector. We present the associated marginal effect below in brackets. Column 1 presents the results for the treatment

[13] Note that these response options are worded in a slightly different fashion than those on the YRBSS questionnaire, though we treat them in a similar fashion. A limitation to the BRFSS data is that the question about helmet use was in an Injury Control topical module that was only asked in a small 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.

group (age 12-15) while Column 2 presents the results for youths who should have been unaffected by the policies (age 16-19). The findings in Table 1 confirm the key results from Grant and Rutner (2000) for the 1990s: adoption of state bicycle helmet laws was associated with statistically significant reductions in youth bicycle fatalities among 12-15 year olds on the order of 9.4 percent [.0176/1.87] but was not significantly associated with bicycle fatalities among 16-19 year olds.

What factors contributed to the cycling fatality reductions in Table 1? We investigate this issue using self-reported cycling and helmet use from the YRBSS and BRFSS. We begin by presenting key variable means for the youth self report data (YRBSS) in Table 2. The YRBSS means show that 71 percent of high school age youths report that they rode a bike in the previous year. Among past year cyclists, however, helmet use is rare: fully 83 percent of high school age youths report that they never wore a helmet when riding a bike, while only 8 percent indicate that they always or almost always wore a helmet.

We provide direct evaluation evidence on the effects of bicycle helmet laws on self-reported helmet use and youth cycling in Table 3. The top panel of Table 3 presents estimates from difference in differences models (equation 2), while the bottom panel presents estimates from the triple difference models (equation 3) that explicitly account for contemporaneous behaviors of youths over the age threshold who were statutorily unaffected by the laws. The first three columns present results for frequent helmet use (i.e. always or almost always wearing a helmet when cycling), while the last three columns present the results for cycling participation. Within each panel (helmet use or cycling) we first present the coefficient on the helmet law indicator when it is defined to be "on" for all laws that set the age threshold at 16 or higher (i.e.

[14] We are, however, able to account for a handful of state laws that set the relevant helmet law threshold at age 12 instead of age 16 due to the inclusion of parental reports for children as young as age 5. In all the BRFSS models

all 15 year olds and younger are treated when the policy is adopted) for the sample of high school youths age 15 and younger. We then present the associated results for the sample of youths age 16 and older.[15] Finally, for each outcome we present the relevant coefficient on the interaction term (helmet law * treated age group) in a model that includes a full set of state fixed effects, year fixed effects, treatment group interactions with the state and year fixed effects, and a full set of state by year dummies.

The results in Table 3 provide no evidence that mandatory helmet laws affected population helmet use (recall here the sample includes all youths, including non-cyclists). The coefficient estimate on the two way fixed effects model for the treatment group is very small and statistically insignificant. While the associated estimate for the slightly older control group is also small and statistically insignificant, the triple difference estimate is wrong-signed (negative) and also very small in magnitude. Overall, these results are not consistent with the idea that mandatory helmet laws induced large increases in helmet wearing among youths.

In contrast, the results in Table 3 provide strong evidence that helmet laws reduced youth cycling. The estimate in Column 4 of Table 3 for the treatment group youths, for example, suggests that adoption of a mandatory helmet law significantly reduced cycling by about 3.8 percentage points, or about 5 percent relative to the sample mean. Appendix E shows a more complete set of coefficient estimates from this baseline DD model, and among other things reveals a small and statistically insignificant association between state graduated driver licensing (GDL) programs and youth cycling. 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

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the Helmet Law variable is coded as "one" when the referenced youth is covered and "zero" otherwise.

[15] The models for older youths exclude California because its law set the age threshold at 18, not 16.

that would bias our main cycling result.[16] In Column 5 we estimate the same DD model on the sample of 16 year olds and older. These youths were not treated by the helmet law, so if helmet laws also reduced cycling among these slightly older youths this would cast doubt on the idea that the helmet laws per se reduced cycling. We find that helmet laws had no statistically or economically significant effect on the cycling behaviors of youths just above the helmet law threshold. Finally, in the bottom panel of Column 6 of Table 3 we present the associated triple difference estimates. Not surprisingly (given the patterns in Columns 4 and 5), we find that the relevant helmet law * under helmet law threshold interaction term is negative and statistically significant at the five percent level, suggesting that helmet laws reduced cycling by 3.1 percentage points – an estimate similar to the baseline difference in differences estimate.[17]

In Table 4 we investigate the robustness of our main result that helmet laws reduced youth cycling in the YRBSS. In Column 1 we reprint the baseline DD and DDD estimates. In Column 2 we restrict attention to youths age 15 (treatments) and 16 (controls). Both the DD and the DDD estimates are larger than the baseline and are statistically significant. In Column 3 we estimate a DD model that in addition to the usual helmet law indicator also includes a variable that "turns on" two years prior to each state's actual helmet law. The use of this lead variable

[16] See, for example, Dee et al. (2006) for a description of graduated driver licensing programs. In addition to the null empirical finding on GDL in the bicycling regression, it is also 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 over/under 16 distinction 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 her learner's permit. This varies substantially across states, which further casts doubt on the ability of driving availability to explain our main cycling 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.

[17] These regression estimates are also supported visually in Figure 2, which shows the percent of youths who rode a bicycle in each survey year, separately by respondent age and by whether the respondent lives in a state that adopted a law. Although the figure does not show the exact timing of helmet law adoption, the overall pattern is apparent: youths under the helmet law age threshold had very similar rates of cycling in the period before helmet law adoption and generally followed the same pattern. However, youths in helmet law adopting states had cycling rates that were

captures the possibility that helmet laws were adopted in response to some systematic change in cycling behaviors within adopting states, potentially affecting the interpretation of our helmet law effects. Despite this possibility, we find that including the two year lead variable does not change our main result that helmet laws reduced cycling, and the coefficient on the lead variable is small and statistically insignificant. Column 4 restricts attention to youths living in states that ever adopted a helmet law, another standard robustness check. This approach returns similar DD and DDD estimates to the baseline, both of which are statistically significant. This confirms that our cycling estimates are not driven by offsetting changes in behavior in control states coincident with helmet law adoptions.

Columns 5-7 of Table 4 address 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 Column 5 we estimate models on a sample that drops all 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 cycling estimates, and they remain statistically significant. In Columns 6 and 7 we drop California and Florida, respectively, from the sample. Each of these states adopted a helmet law over this time period and contributes over 1,500 observations to the full sample. Ensuring that these large states are not driving the cycling estimates – as confirmed in Columns 6 and 7 – is an important robustness check. In results not reported, we also found that excluding each other helmet law adopter

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modestly lower after helmet law adoption. In contrast, there was no clear difference for youths above most states' helmet law age thresholds. These visual patterns are consistent with the regression evidence in Table 3.

individually did not materially alter our main finding that helmet laws reduced youth bicycle riding.[18]

The results in Tables 3-4 provided strong evidence that high school youths in the YRBSS experienced reduced cycling when states adopted helmet laws, and these results were robust to several alternative samples and explicit controls for the associated behaviors of youths just above the helmet law threshold. In Tables 5 and 6 we investigate whether these cycling reductions are also observed in parental reports of their children's behavior from the BRFSS. Recall that in these data the responding parent was asked about the helmet use and bicycling behavior of the oldest child in the household between the ages of 5 and 15. We use these parental reports to estimate difference in differences models of bicycle riding for these youths over the period 1995-2000.[19]

Mean characteristics for the BRFSS data are presented in Table 5. These data reveal slightly higher rates of cycling and substantially higher rates of helmet wearing, though this is in part a function of the younger age distribution of children in the BRFSS (since cycling and helmet wearing rates decline with age even among youths age 5-15). The evaluation results from the parental reports are presented in Table 6 and confirm that bicycle helmet laws reduced the

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[18] We also performed these same robustness checks on the fatality models presented in Table 2. Each of the fatality results for 12-15 year olds was also robust to the various approaches taken in Table 4. Given that Grant and Rutner (2005) also address robustness, we do not present these results here, though they are available upon request. We also estimated fatality models on the same set of state/year combinations for which we observe YRBSS data, and these models also produced similar reductions in fatalities. We also considered fatality and cycling models with linear state trends, which require an alternative but not necessarily preferred set of identification assumptions. The helmet law estimates for both cycling and fatalities were smaller in magnitude and not statistically significant in these specifications, though the point estimates continued to indicate economically meaningful proportional reductions in fatalities and cycling. In results not reported but available upon request we also examined a number of other "placebo" outcomes that should not have been directly affected by helmet laws (e.g. seatbelt use, motorcycle riding, smoking, drinking, sexual activity) to ensure that our observed effects are not simply proxying for contemporaneous changes in (un)healthy behaviors that – while unlikely in 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 reduced cycling.

[19] Recall that because we do not observe parental reports for youths over age 16 we do not estimate DDD models.

likelihood that a parent reported her child rode a bicycle in the past year by about three percentage points, statistically significant at the five percent level.[20]

5. Discussion

Our evaluation results using both fatality data and self-reported youth helmet use and cycling data confirm that state bicycle helmet laws have reduced youth cycling fatalities, but we uncover an alternative and robust mechanism behind the fatality reduction: helmet laws have significantly reduced youth cycling. These cycling reductions were evident in youth self-reports and parental reports of children's cycling behaviors and were robust to the inclusion of cycling activity for slightly older youths who were statutorily unaffected by the laws because they were over the age threshold. Our most preferred estimates suggest that helmet laws reduced youth cycling by about 5 percent.

Two important remaining questions with respect to interpretation merit further discussion. First, how and why do our results differ from previous literature in public health which has found very large effects on helmet use? And second, do the reductions in cycling translate to overall negative health consequences for youths? We begin the discussion by providing evidence on the first question: what would we conclude about helmet laws and helmet use if – instead of our standard policy evaluation approach – we used the most common approaches in the public health literature (which generally do not test for cycling reductions)? We present these results in Table 7, which shows results for youths in the treatment group in the YRBSS (top panel), as well as BRFSS parental reports (bottom panel) In Columns 1-3 we present results for the sample of bicycle riders, while in Column 4 we present results for the full

[20] Small sample sizes of BRFSS parents with an oldest child age 14-15 prohibited us from estimating meaningful differences by age of oldest child.

sample, including non-cyclists. Again, because helmet laws may change the composition of cyclists, focusing attention only on bicycle riders may overstate the effects of helmet laws on helmet use by combining true behavioral effects on helmet use with compositional changes from reduced cycling. Each entry in the table reflects the coefficient on the helmet law indicator, and reported standard errors have been clustered by state.

In Column 1 we mimic the most basic observational studies used in the public health literature: cross-sectional research designs where comparisons of observed helmet use by cyclists in areas with and without legislation are used to identify helmet law effects. In Column 2 we add the detailed covariates available to us in the respective data sources to account for structural differences in helmet use behaviors that are related to demographic characteristics but that are not attributable to helmet laws per se. In the YRBSS these characteristics include dummies for grade, race, and sex. In the BRFSS these characteristics include dummies for the respondent's (parent's) education, race, sex, and age. The specifications in Column 2 are most closely related to cross-sectional telephone surveys of helmet use behaviors among cyclists, which allow the researcher to ask questions about demographic characteristics of the respondents and to include these controls in the helmet use regressions. In Column 3 we present the associated "evaluation" results that are based on the standard approaches used in policy evaluation and economics research. Specifically, these models include all the demographic covariates as in Column 2 but also include unrestricted dummies for each state and year, effectively identifying the helmet law effects from within-area variation in helmet use coincident with helmet law adoption, controlling for the associated changes in helmet use for respondents in areas that did not adopt a helmet law in that year. In Column 3, however, we are still restricting attention to bicycle riders, thus still potentially confounding the true behavioral helmet use effects with compositional changes in cycling. Finally, in Column 4 we include non-cyclists in the sample but estimate the same specification as in Column 3.

The clear pattern in Table 7 is that approaches used by all previous research consistently overstate the effects of helmet laws on helmet use. This is evident from two main patterns: first, the coefficient estimates on the helmet law indicator become uniformly smaller in magnitude when moving from Columns 1 and 2 to Column 3. Although these specifications all ignore potential effects on bicycling, they are analogous to moving from a cross-sectional framework to a pre/post evaluative setting. Although controlling for demographic characteristics (moving from Column 1 to Column 2) has little effect on the estimated helmet law coefficient, we find that accounting for time-invariant state characteristics has a dramatic effect at reducing the estimated effect of helmet laws on frequent helmet use.

The second clear pattern that suggests previous research has overstated the effects of helmet laws on helmet use can be seen by comparing the “bicycle rider only” specification in Column 3 with its associated “full sample” variant in Column 4. Recall that by considering the entire sample (including non-cyclists), this ensures that any effects of helmet laws at reducing cycling are not confounded in the estimate of helmet laws. Put differently, if the goal of helmet laws is to increase the unconditional rate of helmet use in the population, then the rates of helmet wearing in the full sample are relevant. Importantly, the estimates in Columns 4 are uniformly smaller in magnitude than the parallel estimates in Columns 3, suggesting the importance of cycling reductions in overstating the effects of helmet laws on helmet use using conventional approaches.

Finally, we note that there are important differences across the different data sources regarding the effects of helmet laws on helmet use in Table 7. If we take the specifications in

Column 4 as the "preferred" models, the YRBSS data return no evidence that helmet laws increased frequent helmet use by youths. Interestingly, the parental reports from the BRFSS return very large effects on frequent helmet use. These could reflect "real" effects or falsely "positive" reporting by parents that may be correlated with adoption of helmet laws. There is some discrepancy, then, regarding the effects of helmet laws on helmet use, though in all cases we show that previous approaches dramatically overstate the effects of helmet laws on helmet use.

We conclude the results section by investigating in Table 8 whether the reductions in cycling from helmet laws had effects on overall exercise among youths. Unfortunately, we do not observe consistent measures of body weight or sedentary activity (such as hours of television viewing) over the entire sample period 1991-2005. We do, however, observe a handful of related measures that may shed light on whether helmet laws reduced overall physical activity or simply bicycling. These include: whether the youth reported participating in hard physical exercise for at least 20 minutes in any day in the previous week[21]; whether the youth participated in muscle strengthening activities (e.g. weight lifting or push-ups) on any days in the previous week; whether the youth reported exercising for the purpose of losing weight or maintaining current weight; and whether the youth participated in any sports teams. We note that it would not be implausible for reductions in cycling to have reduced overall physical activity: simple regressions showed that youths who rode bicycles were, for example, 7 percentage points more likely to engage in hard physical exercise compared to similarly situated youths who did not ride

[21] The wording of this question varies slightly over our sample period. In 1991, this question reads: "On how many of the past 7 days did you exercise or participate in sports activities that made you sweat and breathe hard, such as basketball, jogging, fast dancing, swimming laps, tennis, fast bicycling, or similar aerobic activities?" This question has the added advantage that "fast bicycling" is directly referred to in the list of activities, although this was not the case in every year.

a bicycle in the past year. Associated differentials for the other physical activity variables were similarly large when comparing cyclists and non-cyclists.

Despite these possibilities, we find little evidence of effects on overall physical activity in Table 8. Column 1 shows that helmet laws had no statistically significant effect on the probability that a youth engaged in hard physical exercise in the DD model, and the associated point estimate is substantively unimportant once we incorporate youths above the helmet law threshold in a DDD specification.[22] Column 2 shows that helmet laws similarly had no effect on the probability a youth reported any days of strengthening activities; Column 3 shows no effect of helmet laws on the likelihood a youth reported that she exercised in order to maintain weight or lose weight; and Column 4 shows the same null finding for the likelihood of participating in any sports teams. These results suggest that although helmet laws reduced cycling, they did not appear to have large spillovers to other measures of physical activity for youths.

6. Conclusion

The results above show that state laws requiring youths to wear helmets when riding a bicycle reduce cycling. For youths in the US, we estimate that such laws reduce cycling among high school age students by about five percent, with slightly larger effects for younger children of elementary and middle school age. With respect to helmet use, we find mixed evidence. Although the youth self reports provide no evidence that the helmet laws increased helmet use, the parental reports in the BRFSS suggest significant increases in use. In all cases, we find that commonly used approaches in the public health literature dramatically overstate the effects of

[22] For all outcomes in Table 8 the helmet law variable of interest is coded as the one in place at the time of the interview to account for the fact that these questions refer to more recent behavior than the cycling questions (which refer to past year behavior). We also estimated "intensity" variables measuring the number of days of hard exercise (or strengthening activities), and these results similarly indicated no substantive effect of helmet laws.

helmet laws on helmet use by failing to account for: 1) the time invariant characteristics of places that adopt helmet laws; and 2) the reductions in cycling.

There are some important limitations to the current study that should be noted. First, we did not control for the arguably important effects of enforcement, outreach, or media campaigns. The concern with these types of efforts is that adoption of a mandatory helmet law could be correlated with other state efforts to increase helmet use such as “helmet giveaways”. Although this is a limitation shared by nearly all the other papers in this literature as well, we cannot rule out that the effects of helmet laws on helmet use and fatalities are upward biased by failing to control for outreach efforts.[23] Notably, however, this type of bias is not likely to change our estimated effects on cycling behavior, since helmet giveaways should, if anything, increase cycling behavior. Given that we find significant *decreases* in cycling participation, this suggests that our estimates on cycling are likely understated with respect to outreach campaigns.

Second, all of the data are self-reported, either by parents (in the BRFSS) or students (in the YRBSS). Indeed, there were some interesting patterns regarding comparisons of reports of helmet use: parents consistently reported more helmet use than did students.[24] While 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

[23] Our triple difference models are intended to account for this possibility since it is likely that these unobserved outreach campaigns affected youths above the helmet law threshold in a manner similar to those under the age threshold. Despite this, our DD estimates on helmet use did not return strong evidence that youth helmet use increased in the YRBSS. As expected, the DDD estimates in the YRBSS returned similarly small and statistically insignificant helmet use effects. With respect to an outcome indicating that the youth “rarely” or “never” wore a helmet, DD models on the sample of bicycle riders returned a helmet law estimate indicating a reduction in infrequent use of about 4.5 percentage points, while the associated DDD estimate was -.001. Although neither estimate was statistically significant at standard confidence levels, the sensitivity of the helmet use estimate to inclusion of older youths (particularly when compared to the lack of sensitivity of the cycling estimates) does suggest that these other unobserved programs and policies may be important.

[24] Mode effects my be important here, since the parental reports are done by telephone to an interviewer, while the youth self-reports are done by pencil/paper in schools with no parents present. Stigma from not wearing a helmet may be stronger when responding to an interviewer. It may also be stronger for parents who believe their youths “should” be wearing a helmet.

increased), this type of bias is unlikely to affect our estimates on cycling participation. That is, it is difficult to believe that social stigma associated with helmet laws makes parents or youths more likely to report that they did not ride a bike at all in the previous year (since cycling itself was never an intended social "target" of helmet laws).[25]

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 "give-aways" versus media campaigns to reduce the stigma of wearing a helmet (if the main effect is driven by helmet availability, giveaways could be particularly effective). It would also clearly be useful to have information on youth cycling intensity and the nature of youth cycling. Although we found robust effects on cycling participation, the effect on intensity among users could also be quantitatively important with respect to total cycling volume and accident exposure. Finally, we do not observe youth participation in related activities such as skateboarding or in-line skating. Most state helmet laws apply only to bicycle riding (although a handful also cover related activities); if youths simply substitute away from helmet-less cycling toward helmet-less in-line skating, this may offset any fatality reductions associated with cycling per se.[26]

Despite these limitations, our research has provided the first comprehensive quasi-experimental evaluation of the effects of helmet laws on cycling and helmet use behaviors among youths. In so doing, our results offer a complementary set of evidence to the large body of public health and medical studies, and in many ways our data and methods improve on

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[25] This is an important advantage of the observational studies of helmet use.

previous research. As other states consider helmet laws as a way to reduce cycling related injuries and fatalities, they should keep in mind that the laws are likely to reduce cycling among the targeted group. While it is possible the public health benefits from mandating helmet laws may outweigh the reductions in utility associated with less cycling, future research evaluating the full costs and benefits of these policies should acknowledge these effects.

[26] We cannot reasonably identify these fatalities in the FARS. Deaths of "other cyclists" (the most plausible group) are extremely rare.

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Figure 1.
States with Bicycle Helmet Laws, as of 2005

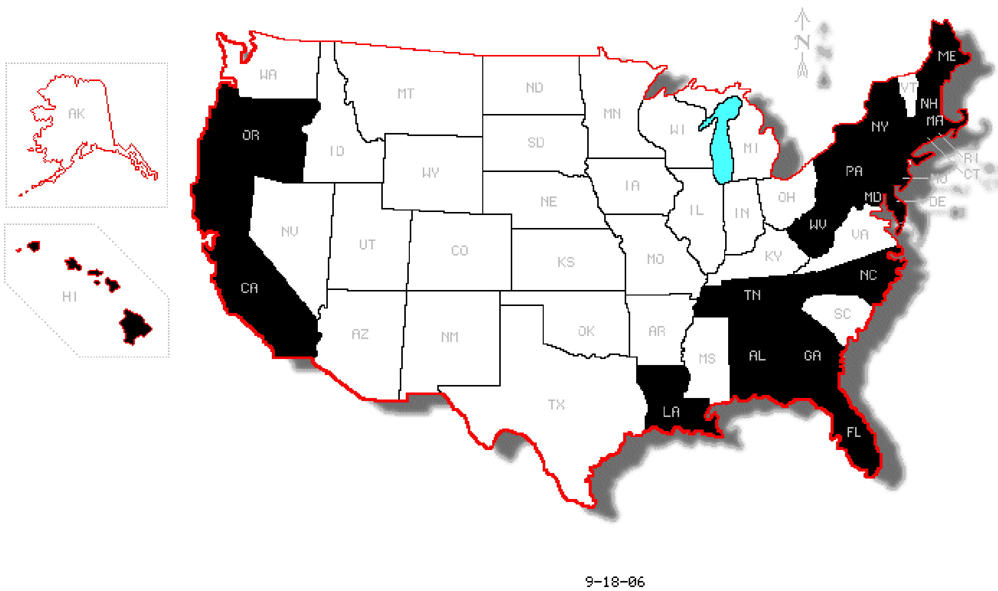

Figure 2: Percent of Youths who Bicycle, by Age & Whether State Adopted a Helmet Law

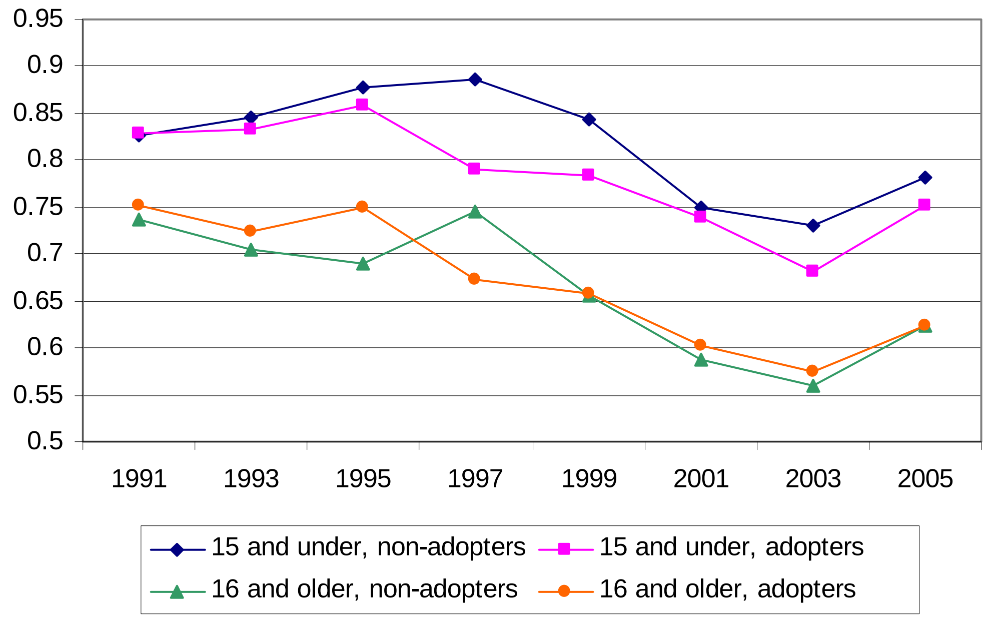

Table 1:
Mandatory Bicycle Helmet Laws Reduced Bicycling Fatalities
FARS 1991-2005

	(1) Bicycle fatalities among 12-15 year olds	(2) Bicycle fatalities among 16-19 year olds
Mean bicycle fatalities in the age group	1.87	0.97
% of state/year observations with no bicycle fatalities	36.7%	56.1%
Coefficient on Mandatory Bicycle Helmet Law Covering Youths Under 16	-.302*** (.096)	.120 (.207)
Implied marginal effect	[-.176]	[.024]
N	765	750
Controls For:		
Year dummies?	Yes	Yes
State dummies?	Yes	Yes

Each column represents a separate negative binomial regression estimated on the state/year count of fatalities of cyclists in different age groups. We present marginal effects and associated standard errors clustered at the state level. Models also include controls for: the state unemployment rate, an indicator for the presence of a .08 BAC law, a Zero Tolerance law, a graduated driver licensing law, a primary enforcement seatbelt law, a secondary enforcement seatbelt law, and speed limits (65mph, 70+ mph). * significant at 10%; ** significant at 5%; *** significant at 1%. The fatality model in Column 2 excludes California because its helmet law threshold is 18, not 16. A sample that included California teens age 18 and 19 produced very similar results.

Table 2:
Descriptive Statistics, 1991-2005 YRBSS Data

Variable	Mean
Rode a bike last year	.71
How often wore a helmet, among riders:	
Always	.04
Almost Always	.04
Sometimes	.04
Rarely	.06
Never	.83
Lives in a state that adopted a helmet law over sample period	.41
Individual is covered by a mandatory state helmet law	.13
Any seatbelt law	.96
Any graduated driver licensing law	.41
Female	.49
Black	.14
Other race	.10
Hispanic	.11
Grade 9	.27
Grade 10	.25
Grade 11	.24
Grade 12	.24
Age 14	.10
Age 15	.24
Age 16	.26
Age 17	.25
Age 18+	.15

Weighted means.

Table 3:
State Helmet Laws Had No Effects on Helmet Use But Significantly Reduced Bicycling, Youth Self Reports
Regression Adjusted Difference in Differences and Triple Difference Models, 1991-2005 YRBSS

	Always or Almost Always Wears Helmet			Rode a Bike		
	(1) 15 & younger	(2) 16 & older	(3) All youths	(4) 15 & younger	(5) 16 & older	(6) All youths
Difference in Differences						
Helmet Law	.006 (.020)	.005 (.012)	--	-.038*** (.013)	.001 (.024)	--
R-squared	.055	.023		.074	.074	
N	35014	65817		34014	65817	
Difference in Difference in Differences						
Helmet Law * Under Helmet Law Threshold	--	--	-.012 (.011)	--	--	-.031** (.015)
R-squared			.062			.099
N			109804			109804
State dummies?	Y	Y	Y	Y	Y	Y
Year dummies?	Y	Y	Y	Y	Y	Y
Treatment group * state dummies?			Y			Y
Treatment group * year dummies?			Y			Y
State * year dummies?			Y			Y

Each entry represents a separate regression estimated using a linear probability model. Robust standard errors clustered at the state level are shown in parentheses. All models include state and year fixed effects. The model in Columns 2 and 5 exclude California because its helmet law threshold is 18, not 16. Other variables in the models but not reported here include: age dummies, grade dummies, race dummies, the state unemployment rate, a dummy for the presence of a mandatory seatbelt law, and a dummy for any state graduated driver licensing (GDL) program with an intermediate phase. The triple difference models do not include the state unemployment rate or the seatbelt law and GDL program indicators. * significant at 10%; ** significant at 5%; *** significant at 1%.

Table 4:
Robustness of Main Bicycle Riding Result, YRBSS 1991-2005

	(1) Baseline	(2) Only 15 & 16 year olds	(3) Baseline, with a control for 2 year lead of helmet law	(4) Baseline, adopter states only	(5) Drop all states in East North Central, West North Central, and Mountain Divisions	(6) Drop California	(7) Drop Florida
Difference in Differences, 15 and Younger							
Helmet Law – 2 year lead			.016				
			(.019)				
Helmet Law	-.038***	-.040***	-.046***	-.039**	-.046***	-.041**	-.033**
	(.013)	(.014)	(.010)	(.015)	(.015)	(.015)	(.013)
R squared	.074	.075	.074	.080	.075	.074	.074
N	34014	24194	34014	14678	25794	29001	31979
Difference in Difference in Differences, All Youths							
Helmet Law * Under Helmet Law Threshold	-.031**	-.047**	--	-.055**	-.033**	-.030	-.037**
	(.015)	(.018)		(.019)	(.016)	(.019)	(.017)
R squared	.099	.092		.101	.104	.102	.100
N	109804	52394		45707	84388	94818	103645

See notes to Table 3.

Table 5:
Descriptive Statistics, 1995-2000 BRFSS Data

Variable	Mean
Oldest child age 5-15 in household rode a bike last year	.84
How often child wore helmet, among riders:	
Always	.38
Almost Always	.11
Sometimes	.11
Rarely	.06
Never	.35
Demographic Characteristics of the Responding Parent:	
Female	.54
White, non-Hispanic	.79
Age	37.5

Weighted means.

Table 6:
State Helmet Laws Reduced Youth Bicycle Riding, Parental Reports
Regression Adjusted Difference in Differences Models
Outcome is: Rode Bike in Past 12 Months (0/1)
1995-2000 BRFSS, Adults with an Oldest Child age 5-15

	(1) Child Rode a Bike in Past Year
Helmet Law	-.030** (.015)
R squared	.087
N	115886

Each entry represents a separate regression estimated using a linear probability model. Robust standard errors clustered at the state level are shown in parentheses. All models include state and year fixed effects. Other variables in the models but not reported here include: parental age, parental age squared, six parental education dummies, parental gender, a white non-Hispanic dummy, dummies for each age of oldest child under 15, survey month dummies, the state unemployment rate, a dummy for the presence of a mandatory seatbelt law, and a dummy for any state graduated driver licensing (GDL) program with an intermediate phase. * significant at 10%; ** significant at 5%; *** significant at 1%.

Table 7:
Previous Approaches Greatly Overstate Effects of Helmet Laws on Helmet Use
Outcome is Always or Almost Always Wears Helmet

	(1) **Only Bike Riders**	(2) **Only Bike Riders**	(3) **Only Bike Riders**	(4) **Full Sample**
Model ➔	No covariates	With covariates	With state & year fixed effects	With state & year fixed effects
Similar to:	Simple observational studies of helmet use in places with/without laws	Telephone surveys that collect demographic characteristics	Pre/post observational studies, bicycle riders only	Population based evaluation, full sample
YRBSS high school youths 15 and under				
Helmet Law	.062*	.055*	.016	.006
	(.033)	(.031)	(.025)	(.020)
R squared	.008	.025	.068	.055
N	26051	26051	26051	34014
BRFSS parents whose oldest child is 5-15				
Helmet Law	.327***	.281***	.121***	.107***
	(.027)	(.030)	(.038)	(.027)
R squared	.099	.197	.241	.228
N	98927	98927	98927	115886

See notes to Tables 3 (youth reports) and 6 (parental reports).

Table 8:
No Evidence of Negative Health Spillovers
YRBSS 1991-2005

	(1) Any hard exercise days (>20 mins) in past week	(2) Any days of strengthening activities (weights, push-ups, etc.) in past week	(3) Any exercising for the purpose of losing weight or maintaining current weight	(4) Participate in any sports teams
	Difference in Differences, 15 and Younger			
Mean of DV	.86	.77	.56	.63
Helmet Law	-.051	-.003	.009	-.022
	(.040)	(.022)	(.015)	(.032)
R squared	.037	.040	.072	.039
N	34014	29805	34014	34014
	Difference in Difference in Differences, All Youths			
Mean of DV	.83	.71	.52	.58
Helmet Law * Under	-.007	-.006	.006	.001
Helmet Law Threshold	(.014)	(.026)	(.020)	(.019)
R squared	.059	.054	.075	.059
N	109804	96343	109804	109804

See notes to Table 3. Laws coded to reflect current helmet law, not past year helmet law. The question about strengthening activities was not asked in the 2005 wave.

Appendix A:
Relevant Text from Sample Helmet Law (Pennsylvania)

With regard to any bicycle used on a public roadway, public bicycle path, or other public right-of-way:

(a) — It shall be unlawful for any parent or legal guardian of a person below the age of 16 to knowingly permit that person to operate or be a passenger on a bicycle unless at all times when the person is so engaged he or she wears a protective bicycle helmet of good fit fastened securely upon the head with the straps of the helmet.

(b) — It shall be unlawful for any parent or legal guardian of a person below the age of 16 to knowingly permit that person to be a passenger on a bicycle unless all of the following conditions are met:

(1) — The person is able to maintain an erect, seated position on the bicycle.

(2) — Except as provided in subdivision (3) of this subsection, the person is properly seated alone on a saddle seat (as on a tandem bicycle).

(3) — With respect to any person who weighs less than 40 pounds, or is less than 40 inches in height, the person can be and is properly seated in and adequately secured to a restraining seat.

(c) — No negligence or liability shall be assessed on or imputed to any party on account of a violation of subsection (a) or (b) of this section.

(d) — Violation of this section shall be an infraction. Except as provided in subsection (e) of this section, any parent or guardian found responsible for violation of this section may be ordered to pay a civil fine of up to ten dollars ($10.00), inclusive of all penalty assessments and court costs.

(e) — In the case of a first conviction of this section, the court may waive the fine upon receipt of satisfactory proof that the person responsible for the infraction has purchased or otherwise obtained, as appropriate, a protective bicycle helmet or a restraining seat, and uses and intends to use it whenever required under this section.

Appendix B:
State Bicycle Helmet Laws for Youths

State	Ages Covered	Effective Date
Alabama	under 16	9/19/1995
California	under 5	1987
California	under 18	10/8/1993
Connecticut	under 15	10/1/1993
Connecticut	under 16	5/14/1997
Delaware	under 16	4/1/1996
District of Columbia	under 16	5/23/2000
Florida	under 16	1/1/1997
Georgia	under 16	7/1/1993
Hawaii	under 16	1/1/2001
Louisiana	under 12	3/1/2002
Maine	under 16	9/18/1999
Maryland	under 16	10/1/1995
Massachusetts	under 5	1990
Massachusetts	under 13	3/8/1994
Massachusetts	under 17	11/25/2004
New Hampshire	under 16	1/1/2006
New Jersey	under 14	7/1/1992
New Jersey	under 17	3/1/2006
New York	under 5	1989
New York	under 14	6/1/1994
North Carolina	under 16	10/1/2001
Oregon	under 16	7/1/1994
Pennsylvania	under 5	1991
Pennsylvania	under 12	2/1/1994
Rhode Island	under 9	7/1/1996
Rhode Island	under 16	1998
Tennessee	under 12	1/1/1994
Tennessee	under 16	2000
West Virginia	under 15	7/1/1996

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 YRBSS analyses, we use laws applying to high school age youths (14-18). For the BRFSS analysis, we use laws applying to youths age 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.

Appendix C:
Data Coverage of the YRBSS
N indicates YRBSS data for that state in that year
Bicycle helmet law for high school age youths indicated by dark shaded area

Location	1991	1993	1995	1997	1999	2001	2003	2005
Alabama		N	N	N	N	N	N	
Arizona		N		N	N	N	N	N
Arkansas		N	N	N			N	
California	N	N	N	N	N	N	N	N
Colorado	N	N	N	N		N		
Connecticut				N				N
Delaware			N				N	
DC			N					
Florida	N	N	N	N	N	N	N	N
Georgia	N	N	N	N	N	N	N	N
Hawaii					N			
Idaho						N		N
Illinois	N	N	N		N	N	N	N
Indiana	N					N	N	N
Iowa			N	N				N
Kansas		N		N			N	N
Kentucky								N
Louisiana			N	N	N		N	N
Maine		N	N	N	N	N	N	
Maryland	N	N		N			N	
Massachusetts		N	N	N		N	N	N
Michigan	N	N	N	N	N	N	N	N
Minnesota		N						N
Mississippi	N	N	N	N	N	N		
Missouri	N	N	N		N	N	N	N
Montana						N		
Nebraska		N						
Nevada						N		
New Jersey	N			N	N	N	N	N
New Mexico	N	N		N		N	N	
New York	N	N	N	N	N	N	N	N
North Carolina		N	N	N	N	N		N
Ohio	N	N	N	N	N	N	N	N
Oklahoma				N		N		N
Oregon		N				N		N
Pennsylvania	N	N	N	N	N		N	N
Rhode Island					N			
South Carolina	N	N		N		N	N	N
South Dakota	N						N	
Tennessee		N	N	N	N	N		N
Texas	N	N	N	N	N	N	N	N
Utah							N	N

Vermont	N						N	
Virginia	N		N		N		N	N
Washington	N	N	N	N		N		N
West Virginia		N				N		N
Wisconsin				N	N	N	N	N

The shading does not exactly correspond to the dates of the laws in all cases. This is because we take into account that all YRBSS surveys are administered in the spring. As such, Georgia's law, for example, which took effect 7/1/1993 only gets shaded from 1995 onward (i.e. no youth in Georgia in the 1993 survey was "treated" by the law). Most laws apply to youths under age 16, with the following exceptions: West Virginia's law only applies to youths under age 15, Massachusetts law applies to all youths under age 17, and California's law applies to all youths under age 18.

Appendix D:
Data Coverage of the BRFSS
B indicates BRFSS data for that state in that year
Any bicycle helmet law for youths between age 5-15 indicated by dark shaded area

Location	1995	1996	1997	1998	1999	2000
Alabama	B		B		B	
Arizona	B	B	B	B	B	
Arkansas	B		B		B	
California	B		B		B	
Colorado	B		B		B	
Connecticut	B		B		B	
Delaware	B		B		B	
DC	B		B		B	
Florida	B		B		B	
Georgia	B		B		B	
Hawaii	B		B		B	
Idaho	B		B		B	
Illinois	B	B	B	B	B	B
Indiana	B		B		B	
Iowa	B		B		B	
Kansas	B		B		B	
Kentucky	B	B	B		B	
Louisiana	B	B	B	B	B	
Maine	B		B		B	
Maryland	B		B		B	
Massachusetts	B		B		B	
Michigan	B		B		B	
Minnesota	B		B		B	
Mississippi	B		B		B	B
Missouri	B		B		B	
Montana	B		B		B	
Nebraska	B		B		B	
Nevada	B		B		B	
New Jersey	B	B	B	B	B	
New Mexico	B		B		B	
New York	B		B		B	
North Carolina	B		B		B	
Ohio	B		B		B	B
Oklahoma	B	B	B	B	B	B
Oregon	B		B		B	
Pennsylvania	B	B	B	B	B	
Rhode Island	B		B		B	
South Carolina	B	B	B		B	
South Dakota	B		B		B	
Tennessee	B		B	B	B	
Texas	B		B		B	
Utah	B		B		B	

Vermont	B		B		B	
Virginia	B	B	B		B	
Washington	B		B		B	
West Virginia	B	B	B		B	
Wisconsin	B		B		B	

See notes to Appendix C. Again, note that there are more changes captured in the analysis than illustrated by the shading, since some states changed their laws after initially adopting them.

Appendix E:
Expanded Set of Coefficient Estimates, Baseline YRBSS Results
1991-2005 YRBSS Age 15 & Younger

	(1) Rode a Bike	(2) Almost Always Wears Helmet
Helmet Law	-.038***	.006
	(.013)	(.020)
Graduated Driver Licensing Program	.010	-.001
	(.015)	(.017)
Seatbelt Law	-.016	-.027
	(.020)	(.020)
State unemployment rate	-.006	-.006
	(.005)	(.008)
Black	-.037**	-.050***
	(.016)	(.010)
Hispanic	-.067***	-.062***
	(.014)	(.013)
Other race	-.066***	-.001
	(.017)	(.011)
Female	-.139***	.001
	(.010)	(.006)
R squared	.074	.055
N	34014	34014

See notes to Table 3. Coefficients on age, grade, year, and state dummies not reported.