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Original article

Helmet Laws, Helmet Use, and Bicycle Ridership

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A B S T R A C T

Purpose: To assess bicycle helmet laws' effect on helmet and bicycle use among U.S. high school students in urban jurisdictions.
Methods: Log-binomial models were fit to Youth Risk Behavior Survey data from five jurisdictions. Adjusted helmet and bicycle use proportions were calculated with post-regression marginal effects. Difference-in-differences were estimated, comparing intervention to concurrent controls. A placebo outcome was used to falsify possible confounding or selection effects.
Results: In San Diego and Dallas, helmet use increase increased 10.6 (95% confidence interval [CI] 6.5 to 14.7, $p < .001$) and 8.1 (95% CI 4.3 to 12.0, $p < .001$) percentage points more than out-of-jurisdiction controls. Increases in Florida counties were 5.0 (95% CI 1.8 to 8.2, $p = .003$) and 4.0 (95% CI −.7 to 8.8, $p = .098$) points against age-based and out-of-jurisdiction controls, respectively. Bicycle use fell 5.5 points in both San Diego (95% CI −9.8 to −1.1, $p = .015$) and the Florida counties (95% CI −11.5 to .5, $p = .075$) against out-of-jurisdiction controls, but other comparisons had no significant changes. The placebo outcome never changed significantly.
Conclusions: Laws increased helmet use in all jurisdictions, with limited evidence of reduced cycling. Although sound health policy, laws should be coupled with physical activity promotion.

IMPLICATIONS AND CONTRIBUTION

Using multiple approaches to identify causal effects, this study provides evidence that bicycle helmet laws produce sustained increases in helmet use among urban high school students. The study finds limited evidence that helmet laws may slightly reduce cycling.

Bicycle helmets substantially protect against death and serious head and facial injury when cyclists are in crashes [1–3]. Nevertheless, under 15% of American high school students consistently wear a helmet when cycling [4], a rate considerably lower than for younger children [5]. Simultaneously, youth aged 14–17 years endure approximately 50,000 nonfatal cycling injuries each year [6] and currently comprise nearly half of all cycling fatalities among minors [7].

A 2008 Cochrane review concluded that bicycle helmet laws significantly increase helmet use based on six controlled studies that existed at the time [8]. In two studies that used direct observation of youth cyclists, helmet use increased by more than 40 percentage points among youth, with no corresponding increase among adults [9,10]. Two additional studies assessed California's youth helmet law using trauma registry. One found significant post-law reductions in head injuries [11]. The other found a significant increase in helmet use but did not detect a change in head injury [12]. Finally, two studies found that Canadian provincial helmet laws reduced pediatric cycling head injuries [13] and reduced deaths by youths subject to helmet requirements by more than half [14].

Since the Cochrane review, additional controlled studies have found beneficial effects from helmet laws. A Swedish interrupted time series study identified an 8% reduction in head injuries among male but not female youths [15]. A Canadian study found sustained helmet increases among Alberta youth through 4 years of follow-up [16]. A number of noncontrolled before-after studies have also found substantial helmet use increases coincident with helmet laws [17–19].

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Nonetheless, 29 U.S. states have no bicycle helmet laws, and only California, Delaware, and New Mexico require all minors to wear helmets [20]. Three enduring empirical debates contribute to jurisdictions' reluctance to enact helmet laws. First, several recent studies have challenged helmet laws' effectiveness. A Canadian study found that, provinces with helmet laws did not have lower head injury rates than provinces without them [21]. Another Canadian study found that, reductions cycling-related head injury reductions in provinces with helmet laws resulted from pre-existing trends [22]. Finally, an American study found evidence that head injuries decreased because of helmet laws, but it also found evidence, albeit limited, that the reduction may result from decreased cycling [23].

Second, opponents often claim that helmet laws reduce cycling and, therefore, physical activity, a claim with mixed evidence. Two Canadian studies found no evidence of reduced cycling due to helmet laws [17,24]. An American study, however, found that cycling declined by 5% in jurisdictions with helmet laws [25]. From a physical health perspective, cycling reductions would matter only if the health consequences of less activity exceed the injury benefits from helmet uptake (although, of course, cycling may also bear nonhealth dividends for transportation systems and promote social independence for adolescents). Two modeling studies sought to examine this tradeoff. One concluded that cycling helmet laws would rarely have a net public health benefit [26], whereas the other, focused on US contexts, estimated that helmet laws would generally have net health benefits [27]. Both models were, however, sensitive to uncertain helmet and bicycle use assumptions [27].

Finally, as noted in the 2008 Cochrane review, most bicycle helmet studies were conducted shortly after laws' implementation, and it is uncertain how long their effects last [8]. More recently, one Canadian study, which did not find evidence of helmet laws' effectiveness, has assessed 12 years of follow-up [22]. An uncontrolled observational study found long-term benefits from helmet laws in high-income parts of Toronto but not in lower-income areas [28]. There are no long-term American studies.

This study uses Youth Risk Behavior Survey (YRBS) data collected at the school district level to assess the effect of helmet laws on (1) bicycle helmet usage—including long-term trends—and (2) cycling prevalence among high school students. District-level data, which are collected for a set of large, urban school districts, have not previously been used to assess the effect of bicycle helmet laws. Unlike state-level data, in which respondents may be subject to municipal laws but municipalities cannot be identified, using district-level data enables crisper classification of exposure to helmet laws.

Methods

Setting and participants

YRBS is a biannual, Centers for Disease Control and Prevention (CDC)-funded survey of student health risk behaviors, fully described elsewhere [4,29,30]. Relevant to this study, CDC has supported surveys of public high school (and, to a limited degree, middle school) students in large, urban school districts since 1991. Data are collected via two-stage cluster sampling, with school selected with probability proportionate to size at the first stage and classes selected randomly at the second. Surveys use a standard questionnaire, with jurisdictions able to add or delete questions. CDC considers response rates at or above 60% to be sufficient for population representativeness. Sampling weights are applied to adjust for nonresponse and oversampling of some subpopulations, with weights summed to the overall student population size [4,29]. Anonymized data for all jurisdictions used in this study are available from CDC.

Helmet laws were initially identified from lists maintained by the Bicycle Helmet Safety Institute [31] and Governors Highway Safety Administration [20] and further characterized using the WestlawNext legal database. Jurisdictions were included if they (1) had a helmet law at the municipal or state level; (2) had population-representative YRBS data from immediately before and after the helmet law's effective date; and (3) surveys ascertained bicycle helmet use in at least those years. Broward and Miami-Dade Counties, Florida; Dallas, Texas; and San Diego, California, met these requirements [32]. One additional jurisdiction, Boston, had reliable data on both sides of Massachusetts' helmet law, but it temporarily discontinued collecting helmet data for the period immediately after the law. Control jurisdictions were selected if they had population-representative data for same years as the intervention jurisdictions and ascertained helmet use. Only Chicago met these requirements. All jurisdictions had population-representative data for the complete period from 1991 to 2013 except for Dallas (1991–2011) [32]. Laws were summarized with respect to their effective date, target population, penalties, and pre-existing legal requirements. A summary of the included laws is in Table 1.

Causal identification

This study used two approaches to identify a potential causal relationship between helmet laws and cyclists' helmet use. First, it used a difference-in-differences approach to determine whether the before-to-after change in confounder-adjusted proportions of cyclists' helmet use is significantly different in intervention than control populations [33]. For Dallas (all ages) and San Diego (<18 years), controls were students of the laws' target age in Chicago. In Miami-Dade and Broward Counties, two control populations were available: Chicago students <16 years old and Miami-Dade/Broward students 17 years and over.

Second, it used a placebo outcome approach to exclude the possibility of selection effects (e.g., if helmet laws cause only cyclists predisposed to greater safety equipment use continue riding) or confounding by a general trend toward improved youth safety in intervention but not control populations [33,34]. To do so, the confounder-adjusted proportion of cyclists who used seatbelts when riding in automobiles was analyzed using difference-in-differences; a significant seatbelt use increase in intervention compared to control populations would provide evidence of selection or unmeasured confounding. Because there is no suitable placebo outcome for bicycle use, its relationship with helmet laws was assessed using only difference-in-differences.

Multiple methods of causal identification were used because none are individually perfect. The use of out-of-jurisdiction controls assumes that changes over time in Chicago adequately represent counterfactual changes in intervention jurisdictions (at least once adjusted for potential confounders). This is plausible but impossible to definitively establish, and YRBS data include few social and demographic variables on which estimates can be adjusted. The age-based controls used for the Florida jurisdictions are likely less susceptible to different time-variant confounders but introduce their own challenges. For example, older