Helmet laws and cycle use

Except for one study reporting decreased cycling by children aged 12–17 in Victoria, Australia, Macpherson et al. stated: “no other study about the relationship between legislation and exposure to cycling has been published”.

Macpherson’s paper has already been cited several times as evidence that helmet laws do not discourage cycling, so it is important to examine the evidence.

In fact, several published studies of cycle use and helmet laws are cited and described in referred journals. Some were attitude surveys. The equivalent of 64% of adult cyclists in Western Australia said they’d ride more than the exception for the helmet law. In New South Wales (NSW) 51% of schoolchildren owning bikes, who hadn’t cycled in the last week, cited helmet restrictions, substantially more than other reasons, including safety (18%) and parents (20%). Street surveys by cyclists’ organisations in the Australian Capital Territory and Northern Territory found 40%–42% of cyclists would ride less, had ridden less because of helmet laws.

Large scale roadside counts were also conducted. Pre-law surveys counted 6072 child cyclists in NSW, 3121 cyclists (all aged 5–14) and over 200 000 cyclist movements on two key routes in Western Australia. Equivalent counts a year after enforced helmet laws showed declines of 36% (NSW), 36% (Victoria) and 20% (Western Australia). Sunday recreational cycling in Western Australia (24 932 cyclists pre-law) dropped by 38%. Increases in numbers wearing helmets, 1019 (NSW) and 297 (Victoria) were substantially less than declines in numbers counted (2215 and 1110). These surveys were at the same time of year, in similar weather, and used the same sites and observation periods, so can be considered reliable indicators that enforced helmet laws discourage cycling. Recreational cycling activity is associated with increased injury rates per cyclist. Australian data suggest injury rates per cyclist increased after helmet laws, the consequences of reduced cycling because of enforced helmet laws should therefore be fully investigated.

Non-enforced laws may have a lot less impact. One state (Queensland), delayed enforcement for 18 months. Helmet wearing increased 17% but 17 months after legislation, rates were little different from pre-law (primary schoolchildren 72%; secondary schoolchildren 21%; commuter cyclists 37%; primary schoolchildren 72%; secondary schoolchildren 21%; commuter cyclists 37%; schoolchildren 21%, commuter cyclists 37%, recreational cyclists 22%, compared with 59%, 21%, 21%, and 22% pre-law). Non-enforced laws can be widely flouted, conveying the undesirable message that road safety rules need not be taken seriously, perhaps encouraging dangerous riding and causing more injuries per cyclist than with no law. Enforcing laws unpopular with cyclists may require substantial effort. Queensland issued nearly 25 000 bicycle helmet offence notices annually, 6.7% of all traffic offence notices. Pre-law cyclists were three times more likely to receive a notice for not wearing a helmet than other road users for all other offences—speeding, drink-driving, failure to stop, give way, or obey other regulations. It is debatable whether this represents the best possible use of police road patrol time.

The Ontario law was not enforced and the effect on helmet wearing rates was not reported. The results should be interpreted with caution for three reasons. First, the study excluded 15–17 year olds, perhaps the most rebellious age group, despite Australian findings that teenagers were most affected by helmet laws (48% reduction for 12–17 year olds), then adults (29%) with the smallest effect on children under 12 (10%). Indeed, in Ontario, only 16–17 year olds could be given direct penalties for non-wearing.

Second, the annual sample size was much smaller than the Australian studies, only 1128 cyclists observed in 1999 (not 747 as reported, for 112 hours of observation; A K Macpherson, personal communication).

Third, the Australian surveys reported weather conditions and used the same sites, time of year and observation periods, to ensure variation due to these factors was not confused with effects of the law. The Canadian study had 111 pre-selected sites, each recorded for one hour, but weather conditions were not reported (though elsewhere 1999 was described as a particularly sunny summer; A K Macpherson, personal communication). Table 1 in the Macpherson et al paper shows that, in some years, some sites were recorded more than once. Moreover, observations were not at the same time of day and day of the week each year (A K Macpherson, personal communication). Analysis of such data requires careful scrutiny of counts and helmet wearing rates to estimate and allow for differences in observation sites and times, as well as weather conditions, to avoid confusing them with year-to-year variation and the effects of the law. This does not appear to have been done.

Another Canadian survey found “large variations in the number of cyclists per unit time, depending on the time of day, the day of the week, or the month”. Perhaps all we should conclude is that the Ontario survey was relatively small, with substantial year-to-year variation, possibly related to year-to-year differences in sites, observation times or weather conditions, these differences swamping any effects of the non-enforced helmet law.

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Reply to Robinson

Our study suggested that the enactment of bicycle helmet legislation is not necessarily followed by a reduction in bicycling. Robinson cites six observational studies from Australia published largely in reports that do not appear in the peer reviewed literature contesting our conclusion. Establishing the association between helmet laws and cycling is important, not only in relation to “the consequences of reduced cycling because of enforced helmet laws”, but also because of the possibly substantial effect of helmet laws (enforced and non-enforced) on helmet use rates and head injuries.

The evidence depicting an association between helmet laws and helmet use is strong. All 15 studies that examine this relationship report a positive association.1 One study, conducted in Ontario, suggests that the legislative effect on helmet use was greatest among children living in low income areas.4 Evidence related to the association between helmet laws and reductions in head injury rates is also consistent. All six studies that examine this relationship found a reduction in head injuries post-law.1 15 22–24

The results of our study on children’s exposure to cycling demonstrated that helmet legislation is not necessarily followed by a reduction in bicycling. Robinson suggests that all one can conclude from our six years of observations is that the “Ontario survey was relatively small, with substantial year-to-year variation, possibly related to year-to-year differences in sites, observation time or weather conditions, these differences swamping any effects of the non-enforced helmet law”. However, our observations were conducted at the same sites, at the same time of the year, and in similar weather conditions. Even if only data from the school observations are included (gathered at the same time of day and the same month each year), there was no reduction in the number of children bicycling to school post-legisla-
The effectiveness and effects of enforced evidence is lacking in the following areas:

1. The effectiveness and effects of enforced non-enforced laws.
2. The effectiveness of legislation related to children only vs all ages.
3. The relationship between helmet laws and bicycling among children who have grown up with helmet laws.
4. Social and cultural differences in the reaction to helmet laws between communities and between countries.
5. The effect of other trends (for example, the popularity of in-line skating, skateboarding, and scooters) on the use of bicycles.
6. Characteristics (including bicycling frequency) of cyclists who may be deterred from bicycling for other reasons are deterred from bicycling for non-violent causes.
7. The effectiveness of countermeasures (for example, bicycle friendly environments) to increase bicycling.

Given the benefits of bicycling and the importance of head injuries as a public health problem, it is essential that public policy and legislation is guided by the best available evidence. We need to understand whether, as suggested in our study, bicycling increases after a law because of the positive publicity related to bicycling. If bicyclists in Australia or elsewhere are deterred from bicycling for substantial periods, we need to explore other measures to encourage them to continue. It would help to know which cyclists change their behaviour after the enactment of a helmet law, and if they reduce their bicycling, whether they adopt other forms of activity in place. Finally, the most effective way to enforce helmet laws has yet to be determined.


Bias when using dead controls to study handgun purchase as a risk factor for violent death

In a recent, excellent study that found handgun purchase to be a risk factor for violent death, Grassel and colleagues gave explanations for why their findings may have inaccurately characterized the impact of handgun purchase on violent death, leading to results that if anything underestimate the magnitude of the danger associated with buying a handgun. “The healthy handgun purchaser effect”, which we suggest is akin to the healthy worker effect, may be a source of bias as well. Bias from this source could produce results that either underestimate or overestimate the impact of a handgun purchase on violent death. When dead controls are used in a case-control study, the death rate due to the comparison cause(s) should be equal in the exposed and non-exposed populations.” If this assumption is met, the odds ratio (OR) can be used as an estimate of the mortality density (or rate) ratio:

\[
\text{OR} \approx \frac{\text{MDR}_1}{\text{MDR}_2}
\]

where \(a\) and \(c\) are exposed and unexposed cases and \(b\) and \(d\) are exposed and unexposed controls, respectively, and \(T_E\) and \(T_T\) are the person-time contributed by exposed and unexposed subjects, respectively. We follow Morgenstern and use his notation, but notation used by others is relatively consistent. In the present context, the OR represents the rate of violent death in handgun purchasers versus non-purchasers (mortality density ratio, MDR) relative to the rate of non-violent death in handgun purchasers versus non-purchasers (MDR2). If MDR equals 1, which occurs if the exposed and unexposed populations do have equal rates of death from the comparison (non-violent) cause(s), the rate numerator-denoiminator cancels out, and the formula collapses to the familiar attributable (that is, the formula to calculate an OR from a 2 $\times$ 2 table), and the OR provides an unbiased estimate of MDR. However, if the exposed and unexposed populations do not have equal rates of death from the comparison (non-violent) cause(s), MDR may not equal 1. In this instance, the OR will provide a biased measure of the parameter being estimated (that is, MDR). Even though the authors excluded from the comparison group causes of death that had clear associations with the purchase of a handgun, bias could still exist if handgun purchasers were “healthier” or less healthy than non-purchasers. For instance, consider the fact that heart disease in the United States is the most common cause of death in California in 1998, accounting for 31% of all deaths. Therefore, a proportion of subjects who comprised the comparison group in this study could reasonably have been expected to die from heart disease. In addition, the rate of death among California residents was 1.4 times higher for men than for women in 1998. Consider this alongside the fact that men accounted for 90% of handgun purchases in California in 1998 and that fewer than 1% of adult Californians purchased a handgun in 1998. Therefore, the gender distribution of adults in California who did not purchase a handgun was about 50% male and 50% female. This information provides reasonable evidence to expect that the death rate in the comparison subjects (that is, those who died by non-violent causes) was higher in handgun purchasers than in non-handgun purchasers. This is, \(b > a\) and therefore MDR’ > 1. If MDR’ was actually >1 in the study by Grassel and colleagues, the OR

References


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In a recent, excellent study that found handgun purchase to be a risk factor for violent death, Grassel and colleagues gave explanations for why their findings may have inaccurately characterized the impact of handgun purchase on violent death, leading to results that if anything underestimate the
would provide an underestimate of the true impact of handgun purchase on violent death.

However, it is especially difficult to assess the direction and magnitude of this source of bias in the study by Grassel and colleagues because many causes of death were included in the comparison group, each of which may have occurred at different, and potentially offsetting, rates among handgun purchasers and non-purchasers. Moreover, even if the comparison group had been comprised solely of heart disease decedents, the result of our short example above does not mean that this source of bias would necessarily serve to underestimate the impact of handgun purchase on the risk of violent death. Numerous factors could lead to variation in the direction and magnitude of bias stemming from an MDR ≠ 1. For instance, if we consider victim age, the relative rate of heart disease mortality among young (age 20–24 years) Californians in 1998 was actually lower in males than in females (relative rate = 0.8). If the true value of MDR was <1 in the study by Grassel and colleagues, the resulting OR would be an overestimate of the true impact of handgun purchase on violent death among this subgroup of young Californians. The magnitude of the bias would be equal to the reciprocal of MDR. For example, if MDR was found to be 0.8 and a logistic regression model including only the subset of 20–24 year old subjects yielded an OR of 3.0, the effect estimate that is adjusted for the bias we are concerned with is actually MDR = 3.0 x 0.8 = 2.4. That is to say, the initial OR of 3.0 was an overestimate, being 1.25 times (1/0.8= 1.25) larger than expected. Including age as a covariate would not specifically control for this source of bias, nor would it obviate the need to test effect modification by age.

The major challenge in all case-control studies, including those in which injury cases are the center piece, is to identify control subjects that will yield unbiased risk factor estimates. This note illustrates one mechanism of bias that injury epidemiologists must be wary of when designing and interpreting case-control studies that incorporate dead control subjects.

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LETTER

The new traffic law and reduction of alcohol related fatal crashes in Japan

The National Police Agency reported in June 2003 that the number of fatal crashes caused by drunken drivers had decreased by 30% in the 12 month period after the change of the traffic law. The number of alcohol related fatal crashes in the period from June 2001 to May 2002 and that in the same period of 2002–2003 were 1187 and 830, respectively. In addition, the number of drivers found guilty of driving under the influence of alcohol was reduced by 7% (from 218 377 to 202 985) during the same period.

In Japan, the Traffic Act was changed and the new law took effect in June 2002. The legal limit for alcohol concentration while driving was lowered and penalties became tougher under the new law. Moreover, it became much harsher on drivers who committed the most serious offenses.

Drunken drivers have been defined in Japan by the level of alcohol measured by breath test. The new act lowered the breath alcohol concentration at which it is illegal to drive a motor vehicle from 0.25 mg/ml breath to 0.15 mg/ml breath. Given the fact that in the United Kingdom the legal limit for drink-driving is 0.35 mg/ml breath, which is equivalent to the legal limit of blood alcohol concentration (BAC) at 0.08%, this limit of 0.15 mg/ml breath seems stricter than other industrialized countries and actually requires “zero tolerance”.

Motorists convicted of drinking and driving can be sentenced to up to three years in prison and fined up to Yen 500 000 (US$4250). They can have administrative testing. BMJ 2002;325:1403.


The stated aim of the publication is to “provide students, researchers and practitioners with the tools and concepts required to conduct systematic evaluations of injury prevention initiatives and safety programs”. This review contends that the authors have fulfilled their aim and commends the publication as an excellent, easily followed introduction to evaluation in the area of injury prevention.

Chapter 1 introduces effectiveness evaluation of safety interventions. Chapter 2 argues for planning right from the start, and covers models of evaluation, including qualitative and quantitative methods, choosing evaluation design, and practical tips. Chapter 3 deals with before and after design, including some of the threats to validity. Chapter 4 covers quasiexperimental and experimental design. Chapter 5 deals with study samples and who should be included in the intervention and evaluation process; chapter 6 with measuring outcomes; chapter 7 with qualitative methods; and chapter 8 with assessing whether the results are significant. Chapter 9 is a summary of recommended practices. There are three sets of appendices on models to assist in planning: examples of statistical analysis; and examples of reporting results.
Evaluation

been extensively canvassed in the journal
decisions would have been of value. This has
the level at which the threshold of signifi-
t be published. Some discussion of
they can
of great practical significance even though
significance. The changes, however, may be
small and meaningless in terms of output
small changes will reach the threshold of
health interventions. When workshops
of experts considered these issues they
concluded that established methods of evi-
dence appraisal provide a useful starting
point for evaluating evidence of all types of
health interventions but recommended that
the methods should be expanded or adapted
to take account of the specific issues in public
health.

From this came the development of a draft
schema (stage one), resulting in a discussion
document in May 2000, based on literature
reviews and consultation with senior public
health researchers and practitioners. Then
came a series of trials (stage two), resulting
in the development of the document under
review.

The schema covers two stages of apprai-
sal—firstly the appraisal of individual papers
or reports to determine whether they provide
credible and useful information about an
intervention(s) and, secondly, the formul-
ation of conclusions about the value of the
available evidence. The process of reviewing
published and other literature on public
health interventions and of using these
findings is seen to involve at least six steps.
These are: identifying the purpose of the
literature review and formulating the review
question(s); finding and collating the studies
to be reviewed; appraisal of each article or
report; formulation of a statement on the
body of evidence; publication of the review
findings; and, application of the review
findings to policy and practice.

That said, we return to the starting point of
the review, that the guide is an excellent tool
to improve evidenced based decision making
in injury prevention.

The work leading to this publication arose out
of discussion in the leading Australian
committee for allocating health related
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