Statistical Errors in Anti-Helmet Arguments

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Abstract

Bicycle helmets are designed to mitigate head injury during a collision. In the early 1990's, Australia and New Zealand mandated helmet wearing for cyclists in an effort to increase helmet usage. Since that time, helmets and helmet laws have been portrayed as a failure in the peer-reviewed literature, by the media and various advocacy groups. Many of these criticisms claim helmets are ineffective, helmet laws deter cycling, helmet wearing increases the risk of an accident, no evidence helmet laws reduce head injuries at a population level, and helmet laws result in a net health reduction. This paper will demonstrate the data and methods used to support these arguments are statistically flawed.

Keywords

Bicycle helmets, Bicycle helmet legislation, Statistical errors

Introduction

The helmet is the most controversial topic in all of cycling. Media discussions about cycling safety often devolve into a debate about helmets (Piper et al., 2011). To date, a substantial body of research has been published both in favour and against bicycle helmet use and mandatory helmet legislation (MHL). It is important to note there are two distinct but related debates with regards to bicycle helmets. One is centred on the helmet itself and its effectiveness in a crash. The other debate focuses on whether governments should mandate their use. It is not uncommon for an individual to favour helmet use but oppose government mandated use of helmets.

Research evidence supportive of helmet use notes a protective effect in mitigating head injuries while research opposed argues helmet use increases the likelihood of rotational head injuries, increases risky behaviour and is associated with closer motor vehicle overtaking. Research evidence supportive of MHL notes declines in bicycle related head injury coinciding with an increase in helmet wearing at the time of the law while research opposed argues declines in head injury are due to less cycling as MHL is a cycling deterrent and the absence of population-level evidence demonstrating a benefit. Those who oppose MHL further argue the combination of deterred cycling, increased risk per cyclist due to fewer cyclists and risk compensation leads to a negative health benefit. Note this final argument is dependent on the other arguments holding true.

This manuscript will demonstrate the primary arguments against helmet use and/or MHL are statistically flawed. In turn, we will discuss the arguments (1) helmets are ineffective, (2) helmet laws deter cycling, (3) helmet wearing increases the risk of an accident, (4) no evidence helmet laws reduce head injuries at a population level and (5) helmet laws result in a net health reduction. These are the core arguments found on anti-helmet advocacy websites (Bicycle Helmet Research Foundation, http://www.cyclehelmets.org/; Cyclists rights Action Group, http://crag.asn.au/; Helmet Freedom, http://helmetfreedom.org/; Freestyle Cyclists, http://www.freestylecyclists.org/; Transport and Health Study Group, http://www.transportandhealth.org.uk/) and even cycling organisations (Bicycle NSW, http://bicyclensw.org.au/advocacy/; European Cyclists' Federation, http://www.ecf.com/).

Helmets are ineffective

There is substantial biomechanical evidence using test dummies that helmet use will lessen the kinetic energy to the head when struck in a collision (for example, see McIntosh, Lai & Schilter, 2013). Randomised controlled trials are not ethically possible to assess the potential association between helmet wearing and head injury; therefore, most human subjects research on helmet efficacy comes from observational studies. There have been many case-control studies that assess the association between helmet wearing and head injury and, to date, there has been a Cochrane review (Thompson, Rivara & Thompson, 1999), a meta-analysis (Attewell, Glase & McFadden, 2001) and a re-analysis of the meta-analysis (Elvik, 2011). In each case, the odds of a head injury were significantly diminished for cyclists wearing helmets versus those that did not.

Curnow (2003) suggested helmets exacerbate rotational injuries, the more serious being diffuse axonal injury (DAI). Although Curnow only hypothesised the DAI/helmet link, some have taken this as fact (BHRF, 2003; Bicycle Australia, 2010; Gillham, 2011; Stewart, 2012; Rissel, 2012; Bicycle NSW, 2013). There is, however, no existing evidence to support the DAI hypothesis. McIntosh, Lai and Schilter (2013) found, when testing oblique impacts on dummies to simulate head rotation, helmet wearing did not increase angular acceleration, a result unsupportive of Curnow's hypothesis. Using trauma registry data from seven Sydney area hospitals over one calendar year, 110 cyclists were identified and none were diagnosed with DAI regardless of helmet wearing (Dinh, Curtis & Ivers, 2013). Walter et al. (2013), using linked police and hospitalisation data in NSW from 2001-2009, reported at most 12 possible DAI cases out of 6,745 cyclists in a motor vehicle collision. Seven of the twelve cyclists were unhelmeted. These results suggest the incidence of DAI among cyclists appears to be rare and unrelated to helmet wearing. Additionally, computer simulated studies of bicycle crashes found no evidence helmets increased the likelihood of neck injury among adults (McNally & Whitehead, 2013) nor was there evidence helmets increased the severity of brain or neck injury in children (McNally & Rosenberg, 2013).

In addition to head injuries, Elvik (2011) performed separate analyses by combining head, face and neck injuries. The results from a random effects model estimate a small, slightly significant benefit to helmet wearing to protect the head, face or neck (OR: 0.74, 95% CI: 0.64-0.86). However, due to data and analytic errors, Elvik (in press) has published a full length corrigendum to this article. The current article provides a slightly different estimate (OR: 0.79, 95% CI: 0.69-0.90). Churches (2013), on the other hand, has reported difficulty in reproducing Elvik's results from random effects models and estimates a substantially larger overall benefit of helmet wearing (OR: 0.60, 95% CI: 0.50-0.73) to protect the head, neck and face using the same data as Elvik.

Additionally, Elvik (2011, 2013) reported a diminishing estimate of helmet efficacy with a diminished estimated odds ratio of 0.45. However, it is unclear if a time trend truly exists as more recent studies have estimated substantial reductions in head injury associated with helmet wearing that do not follow this pattern. Amoros et al. (2012) report an odds ratio of 0.34 (95% CI: 0.15-0.65) for serious head injuries (AIS3+) in urban areas and Bambach et al. (2013) report an odds ratio of 0.26 (95% CI: 0.15-0.45) comparing severe versus possible minor head injury.

Helmet laws deter cycling

Using NSW and Victoria data, Robinson (1996) concluded the impact of MHL in Australia was to reduce cycling numbers and not reduce head injuries. Some recent researchers have taken MHL as a cycling deterrent as fact and present no supportive evidence (Rissel, 2012b; Rojas-Rueda, Cole-Hunter & Nieuwenhuijsen, 2013). It should be noted, however, that Robinson omits important, relevant data and other information from her analyses.

When describing cycling count data in NSW for children, Robinson (1996) states

> "*Comparable figures were not available for adults*"

and, in a related paper, Robinson (2006) states

> "*all available long and short term data show cycling is less popular than would have been expected without helmet laws.*"

Cycling count data for adults does, in fact, exist for NSW before and after MHL. Additionally, Robinson (1996) omits NSW cycling counts for children from October 1990 in her analysis.

Prior to MHL in NSW, the Roads and Traffic Authority commissioned a series of helmet wearing surveys with data collected at road intersections and recreation areas for all ages as well as school gates for children only (Walker, 1990, 1991, 1992; Smith & Milthorpe, 1993). Note counts were not taken at recreation areas in the 1990 report. The counts of adult cyclists from these reports are summarised in Table 1. MHL became effective for NSW adults on 1 January 1991.

Table 1

Counts of adult cyclists in NSW from RTA surveys (*adult recreation cycling not separated by location)

Location	Oct 90	April 91*	April 92	April 93
Road Intersections				
Sydney	2730	3332	2796	2591
Rural	2388	2146	1933	1436
Subtotal	5118	5478	4729	4027
Recreation Areas				
Sydney	n/a	n/a	911	1345
Rural	n/a	n/a	545	1293
Subtotal	835	1095	1456	2638
Total	5953	6573	6185	6665

Comparing the October 1990 and April 1991 counts, there was a 7% increase in adult cycling counts at road intersections spurred by a large increase in Sydney (+22%) but a decline in rural areas (-10%). Thereafter, counts at road intersections declined; however, counts in recreational areas increased substantially from the second to fourth surveys (+141%) and the absolute decrease in road intersection counts was smaller than the absolute increase in counts at recreation areas. As noted, adult recreation area counts were not part of the 1990 report and the value given has been extrapolated by regressing the natural log of recreational cycling counts with year (r^2=0.96). Using this extrapolated value, the total counts of adult cyclists increased 12% from the first to fourth surveys.

Time series plots of adult and child cycling counts at road intersections and recreation areas are given in Figure 1. As with adults, the 1990 count of child recreation cycling has been extrapolated. There is no evidence adult cycling counts diminished with the helmet law and there is a decline in child cycling counts before their helmet law.

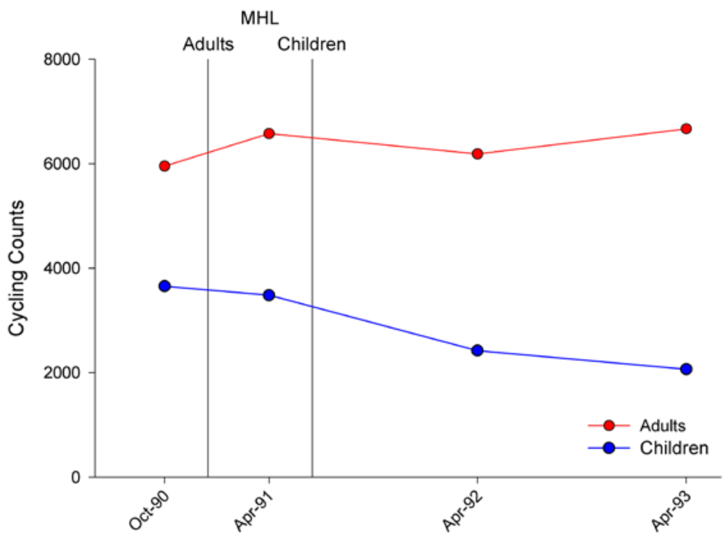

Figure 1

Counts of cyclists at NSW road intersections and recreational areas for children and adults (source: Walker, 1990, 1991, 1992; Smith & Milthorpe, 1993)

Note that although the initial survey was taken in October with subsequent ones in April, these two months have similar weather patterns for Sydney in terms of average high temperature (22.1° C vs. 22.4° C) and average number of rainy days (8.0 vs 9.0) (Bureau of Meteorology, 2013). They do differ in terms of rainfall (77.1mm vs. 127.2mm); however, this would contribute to a decline in post-MHL adult cycling since weather is often cited as a cycling deterrent. Additionally, Olivier et al., (2013) found no significant difference in cycling related head injury hospitalisations between those months in the pre-MHL period for adults.

Caution should be taken when interpreting statistical results using this survey data whether supportive or opposed to helmet legislation. Smith and Milthorpe (1993) note the surveys were designed to estimate helmet wearing in NSW and not to estimate cycling exposure. Also, over a forty-eight month period, data was only collected over four months (akin to an 8.3% response rate). However, the use of these surveys for that purpose only supports Robinson's conclusions when the adult data are ignored since those counts clearly increased from pre- to post-MHL. Additionally, Robinson's conclusions are tempered with the inclusion of cycling counts for children in 1990.

Marshall and White (1994), in a report assessing the South Australia MHL, give estimated changes in cycling exposure. This work is cited by Robinson (2006); however, she does not mention survey results of cycling exposure. Using data from approximately 3000 households before (1990) and after (1993) helmet legislation, the authors found no significant declines in cycling exposure regardless of age, gender or level of urbanisation. Marshall and White (1994) also report a 2.9% increase in counts of cyclists into Adelaide following MHL. Another survey of helmet wearing among SA schoolchildren did note a 38.1% decline of cycling to school. This is inconsistent with the other SA surveys; however, the authors note only 20% of those aged 15 years of age or younger reported cycling to school.

Current opinions regarding bicycle helmets suggest it is a minor issue with more important concerns regarding cycling. Recent surveys list helmet wearing as the 10th and 13th most common barrier to cycling among current and non-cyclists respectively (Cycling Promotion Fund, 2011). In a survey of Australian women regarding encouraging women to cycle more, 4.1% gave the repeal of the helmet law as their main response (Cycling Promotion Fund, 2013). In both surveys, the lack of cycling infrastructure and safety concerns were much more common responses. Rissel and Wen (2012) report significantly more people would cycle without helmet legislation. However, Olivier et al. (2012) note the authors misinterpreted their statistical results and most Australians would not cycle more. Further, since Rissel and Wen's survey only concerned helmets as a cycling deterrent, it is unclear if those indicating they would cycle more without helmet legislation would not be further deterred due to other, more often cited factors such as lack of cycling infrastructure or concerns regarding safety.

It has been argued that increasing the number of cyclists will lower the number of cycling injuries per cyclist (Jacobsen, 2003). This is often called the *safety in numbers* (SiN) effect and is a variation of Smeed's Law. Robinson (2005), using her estimates of the deterrent effects of MHL, further hypothesised helmet legislation could increase the number of injuries per cyclist. The mathematical representation of SiN for cyclists is

$$\frac{I}{C} \propto C^{-0.6} \tag{1}$$

where I represents the number of injuries and C is the amount of cycling.

As noted above, very little cycling exposure data exists at the time of helmet legislation in the early 1990's. Yearly estimates of cycling participation does exist beginning in 2001 as part of the Participation in Exercise, Recreation and Sport (ERASS) surveys (ABS, 2001).

Equation (1) can be reformulated as

$$I = I_0\left(\frac{C}{C_0}\right)^{0.4} \tag{2}$$

where I_0 and C_0 are initial values for injuries and amount of cycling respectively. Using NSW hospitalisation data (Olivier, Walter & Grzebieta, 2013), Figure 2 gives actual and expected head and arm injuries for 2001-2010 using equation (2) and 2001 injury and cycling participants as initial values.

The results are not supportive of SiN as the observed injuries differ substantially from expected (chi-square test, $p<0.001$ in each case). Additionally, using the counts of head/arm injuries and ERASS cycling estimates, the exponent is estimated to be 0.94 (95% CI: 0.59-1.30). Therefore, this data suggest a proportional change in cycling is associated with a similar change in the proportion of cycling-related injury and is not supportive of the SiN effect for cycling.

Although the counts of observed and expected injuries diverge immediately, they seem to converge after 2006. In fact, observed head injuries are less than expected by 2010. This change coincides with increased cycling expenditures in NSW (Montoya, 2010) suggesting segregated cycling infrastructure and helmet legislation, not safety in numbers, are major causal factors in cycling safety. In other words, the safety in numbers effect may be a consequence of an existing safe cycling environment. Other authors (Bhatia & Wier, 2011) have further questioned the use of SiN in determining transportation policy due to the lack of supportive evidence.

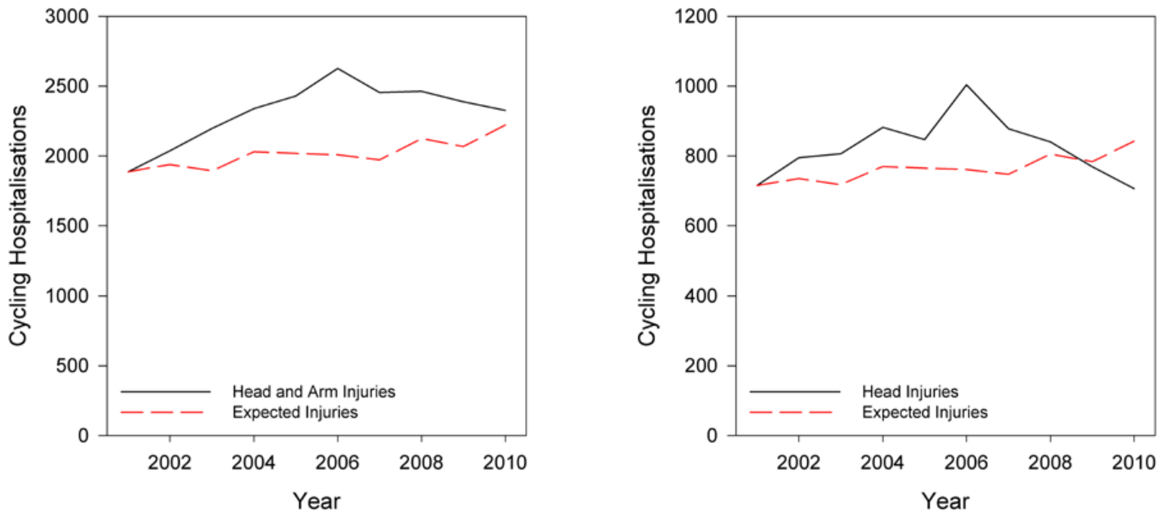

Figure 2

Actual and expected NSW cycling hospitalisations (2001-2010) for (a) head and arm injuries and (b) head only

The increase in cycling injuries is also consistent with increased cycling per person (measured in either time or distance). The ERASS surveys estimate participation rates and not actual amounts of cycling. However, this would indicate the amount of cycling (not just participation) can increase in jurisdictions with helmet legislation which runs counter to most arguments against helmet legislation. In fact, a key assumption by de Jong (2012) is the kilometres cycled per person can only decrease with helmet legislation.

Helmet wearing increases the risk of an accident

Robinson (1996, 2006) suggested a cyclist's perception of risk is modified when wearing a helmet and, as a consequence, will exhibit riskier behaviour when wearing a helmet. This is often termed risk compensation. In a criticism of a Cochrane Review assessing the protective effect of bicycle helmets (Thompson, Rivara & Thompson, 1999), Adams and Hillman (2001) argue in favour of risk compensation. Adams (2007) has made similar arguments around seat belts in motor vehicles. However, there is scant evidence to support this theory.

A series of Norwegian studies, in an effort to measure risk compensation for helmet wearing, recruited cyclists who either usually wear or not wear helmets. Their primary outcome was average speed while wearing or not wearing a helmet and a measure of psychological relaxation. For usual helmet wearers, Phillips, Fyhri and Sagberg (2011) report lower cycling speeds and increased heart rate variability when not wearing a helmet. No significant differences were found for non-helmet users. A plot of this relationship is given in Fyhri and Phillips (2013) which has been reproduced below in the left panel of Figure 3. The authors urge caution regarding helmet legislation in light of their results.

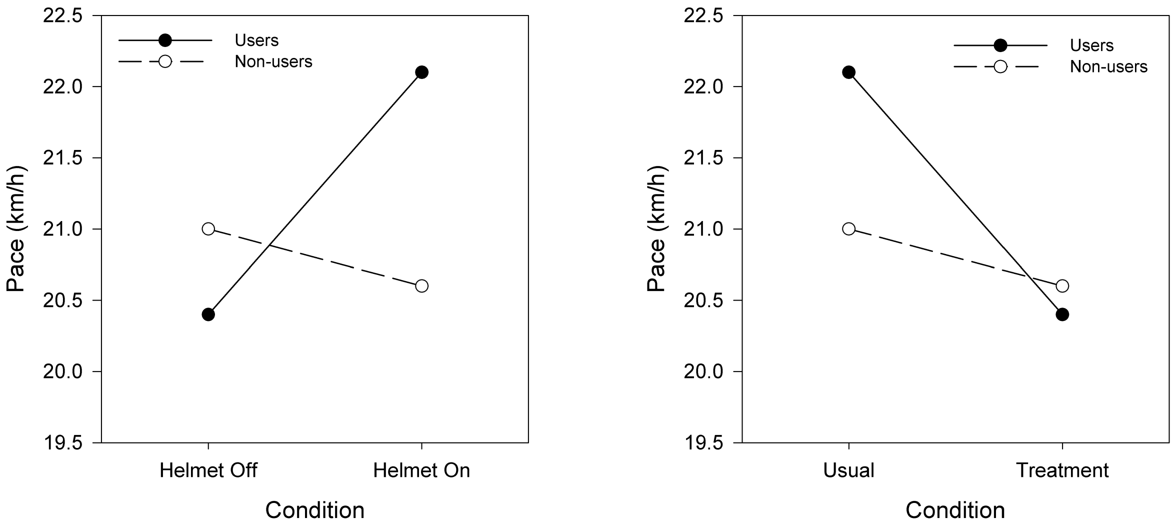

Figure 3

Cycling speed with and without helmet wearing for regular helmet users and non-users with (a) incorrect and (b) correct temporal ordering (source: Fyhri & Phillips, 2012)

These results, and particularly their figure, are misleading as it conveys a temporal ordering that does not exist. This figure gives the impression a cyclist who usually wears a helmet will increase speed when wearing a helmet. The correct temporal ordering here is the reverse for usual helmet wearers and the correct ordering is given in the right panel of Figure 3. When plotted correctly, their results demonstrate a decrease in cycling speed when a cyclists moves from their usual condition (helmet use or non-use) to the treatment condition (non-use or helmet use). This is also true for their psychological relaxation results, i.e., declines in both groups when subjected to the treatment condition. Further, it is unclear if increased speed is a valid measure of risk compensation for bicycle helmet use. Through the use of computer simulation of bicycle crashes, helmet use was found to increase in protection as cycling speed increased thereby negating any potential effect of risk compensation (McNally & Rosenberg, 2013; McNally & Whitehead, 2013).

More importantly, helmet promotion and helmet legislation have a clear temporal ordering: usual non-wearers are urged or mandated to put on a helmet. In this situation, the authors report no significant changes in speed or psychological relaxation when a non-user wears a helmet, so their results do not support risk compensation theory as it relates to helmet promotion or legislation. On the other hand, results from case-control studies give evidence non-helmet users in a crash were more likely to exhibit illegal behaviour (Lardelli-Claret, 2003; Bambach et al., 2013).

One of the NSW helmet wearing surveys (Walker, 1991) examined whether helmet legislation may have influenced levels of compliance with other regulations governing the use of bicycles on the road. The data estimated a decrease in certain illegal behaviour by NSW adults including riding on the wrong part of the road or riding on the footpath following MHL. There was also no evidence that dangerous riding behaviour, such as doubling, riding 'no hands' or 'no feet' or riding more than three abreast, increased after the law. The report concluded that "the evidence available provides no support for the risk hypothesis."

In a study of driver behaviour towards cyclists, Walker (2007) reported significantly less overtaking distance when wearing a helmet versus not. Although not an example of classical risk compensation, the implication is the cyclist's environment is riskier when wearing a helmet.

It is known that lateral forces are increased when vehicles get nearer a cyclist. This is often the basis for the one metre rule, or similar three foot rule in the US, for safe overtaking (Love et al., 2012). Further, on his website, Walker (2012) supports the categorisation of his data using the one metre rule stating "this is perhaps the clearest way to illustrate the effect of helmet wearing." However, using data available on his website, Olivier and Walter (2013) demonstrated the association between helmet wearing and unsafe passing distances (< 1m) is non-significant (OR=1.3, p=0.182) and this effect is reduced when adjusted for vehicle size, city of occurrence and distance to the kerb (aOR=1.1, p=0.540). This result is not due to lack of statistical power since the sample size of the original study was based on 98% power.

No evidence helmet laws reduce head injuries at a population level

Although helmet use has been shown to be beneficial in a cycling crash, Robinson (2006, 2007) and Rissel (2012a) argue a population level effect has not been detected for jurisdictions with helmet legislation. Both authors cite a study by Hendrie et al. (1999) using WA data to support their arguments, yet each fail to note the paper found a significant decline in the ratio of cycling to pedestrian head injury at the time of the WA helmet law.

Comparing head and arm injury hospitalisations in NSW, Voukelatos and Rissel (2010) concluded helmet legislation did not lead to a greater reduction in head injuries beyond an overall declining trend in cycling injuries. However, serious data issues were identified in this study (Churches, 2010) and the article was later retracted by the journal (Grzebieta, 2011). Subsequently, however, the results from the retracted paper have been used as evidence against helmet legislation (Rissel, 2012). Additionally, Gillham (2011) uses the incorrect data reported by Voukelatos and Rissel (2010) as the basis for arguing against conclusions drawn from subsequent analyses by Walter et al. (2011) using the same source data while also hosting the original, retracted article (http://www.cycle-helmets.com/rissel.pdf).

Mindell, Wardlaw and Franklin (2011) combined figures found in Walter et al. (2011) and state "it is difficult to discern any particular reduction in head injuries to cyclists (red) compared with pedestrians (blue), although the data are rather "noisy"." Their plot is given in Figure 3. Note that these plots do not correspond to the actual data and is not a "like for like" comparison. In fact, the time series of head/arm and head/leg ratios for cyclists and pedestrians respectively do not overlap at all and exhibit differing amounts of variability or "noise".

The correct plots are given in Figure 4. To reproduce the plots in Mindell, Wardlaw and Franklin (2011), the height and variability of each time series would need to be adjusted producing time series that are ultimately no longer comparable. This is a clear case of manipulating the presentation of data to produce a desired result: trends in NSW hospitalisation data for cyclists are not supportive of helmet legislation.

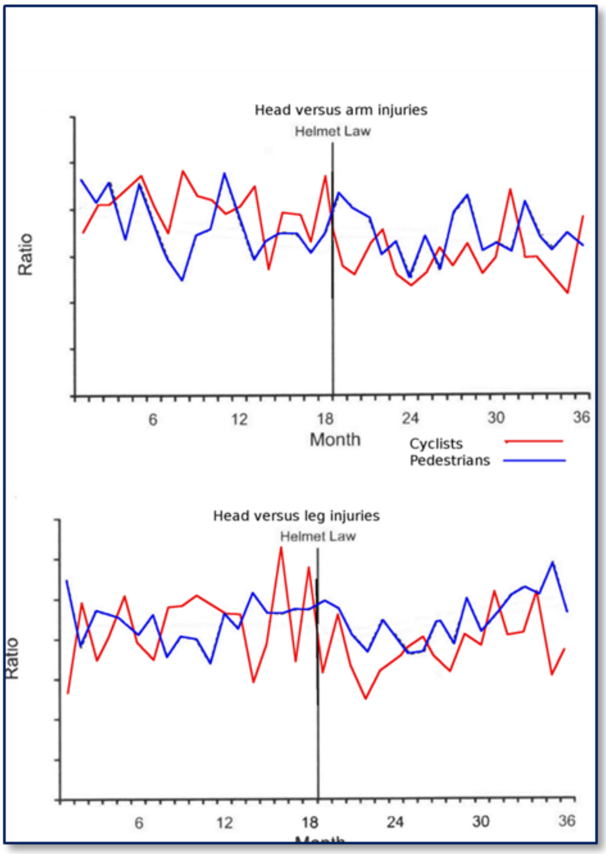

Figure 3

Time series of the ratio of head to limb injuries for bicycle and pedestrian related hospitalisation in NSW (source: Mindell et al., 2011)

Relative to the other time series plots, there would appear to be less variability (i.e., “noise”) in the head/arm ratio for cyclists and the head/leg ratio for pedestrians. By contrast, there is more “noise” in the comparison between cycling head and leg injuries. This suggests cycling arm and pedestrian leg injuries are better comparators with their respective primary outcomes (i.e., head injury). With regards to cycling injury, this is supported numerically as the within-month correlation is higher comparing cycling head injuries to arm injuries as opposed to leg injuries (Walter et al., 2013). Further, Figure 5 gives a plot of the head/arm injury ratio and the estimated counterfactual, i.e., the trend without the effect of the helmet law. This plot demonstrates a clear level shift in the head/arm ratio for cyclists after the helmet law as 89% (16/18) of monthly ratios are below the counterfactual.

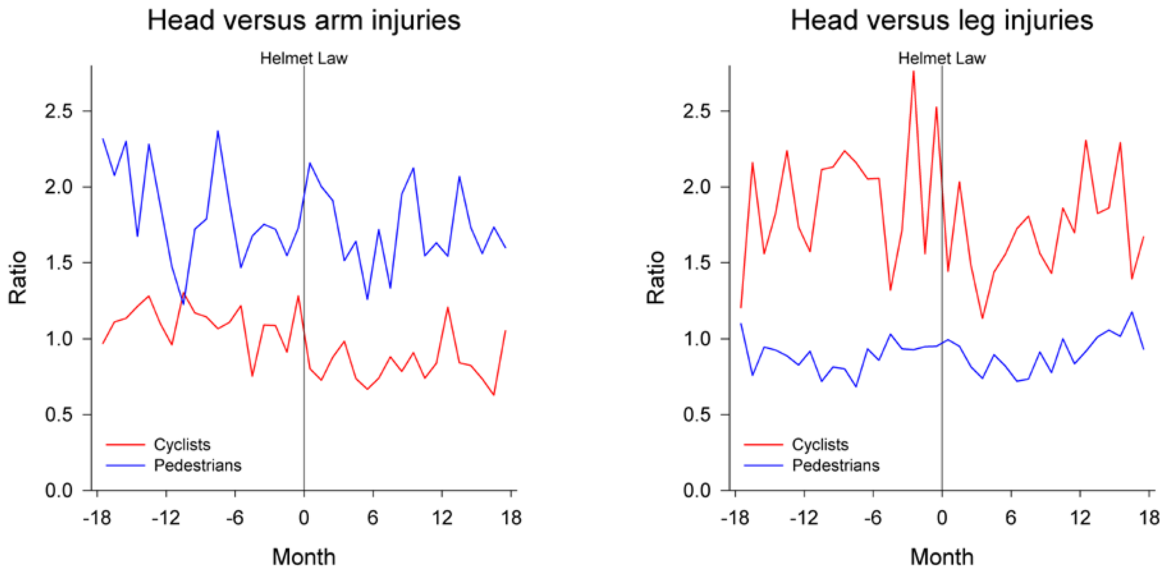

Figure 4
Time series of the ratio of head to limb injuries for bicycle and pedestrian related hospitalisation in NSW (source: NSW Department of Health)

Although graphical displays of data are an efficient method for presenting a study's results, they can also be misleading as demonstrated above. Additionally, a determination that data is "noisy" should be assessed objectively by comparing an observed effect to an estimate of variance, sometimes called the "signal" to "noise" ratio. Importantly, Ramsay et al. (2003), in a systematic review of studies using interrupted time series designs, found over 40% of studies in which the data was not analysed or analysed inappropriately, the original conclusions were reversed when appropriate statistical methods were used. A numerical analysis of the NSW hospitalisation data for cycling and pedestrian head injuries is given in Table 2.

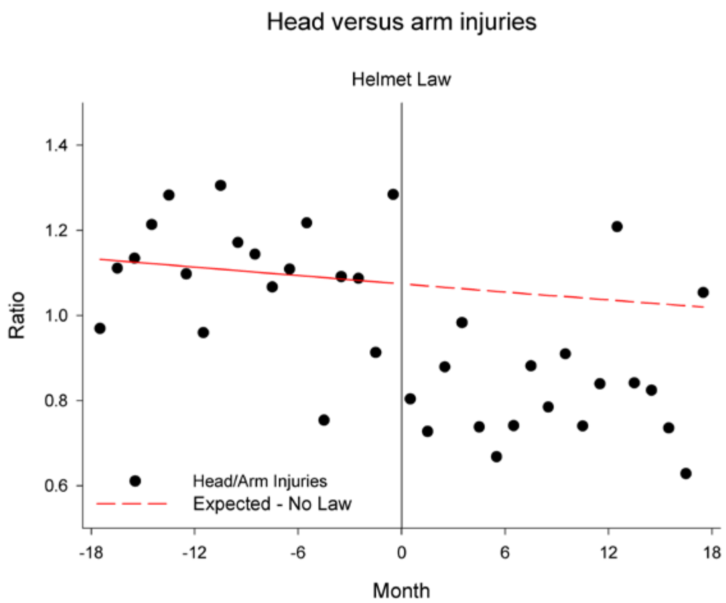

Figure 5
Time series of the ratio of head to arm bicycle injury hospitalisations in NSW and the expected ratio without the helmet law (source: NSW Department of Health)

Table 2
Ratio of head to limb injury hospitalisations in NSW for cyclists and pedestrians immediately before and after mandatory helmet legislation (source: Walter et al., 2011)

	Pre-Law	Post-Law	% Change	p-value
Head/Arm				
Cyclists	1.075	0.779	-27.5	0.03
Pedestrians	1.579	1.756	+11.2	0.41
Head/Leg				
Cyclists	2.164	1.493	-31.0	0.03
Pedestrians	0.896	0.804	-10.2	0.38

Note that the p-values given are substantially lower when the within-month correlation between head and limb injuries is part of the model or the most parsimonious model is chosen (Walter et al., 2013). For each type of ratio, there is a significant change with the helmet law for cyclists but not for pedestrians. In fact, there is an estimated increase in the head/arm ratio for pedestrians while there is a substantial decrease for cyclists. These results point to a small amount of "noise" relative to "signal" in the NSW hospitalisation data for cycling head injuries around the helmet law.

There is a drawback of strictly analysing the ratio of one injury to another. Specifically, the ratio between them may vary over time, yet it will be unclear whether it is due to changes in one or both. A more appropriate analysis, and perhaps time series plot, would be to estimate them as part of a joint model. Separate time series plots of cycling head and arm injury hospitalisations in NSW for the eighteen month period around the helmet law and the following two decades are given in Figure 6.

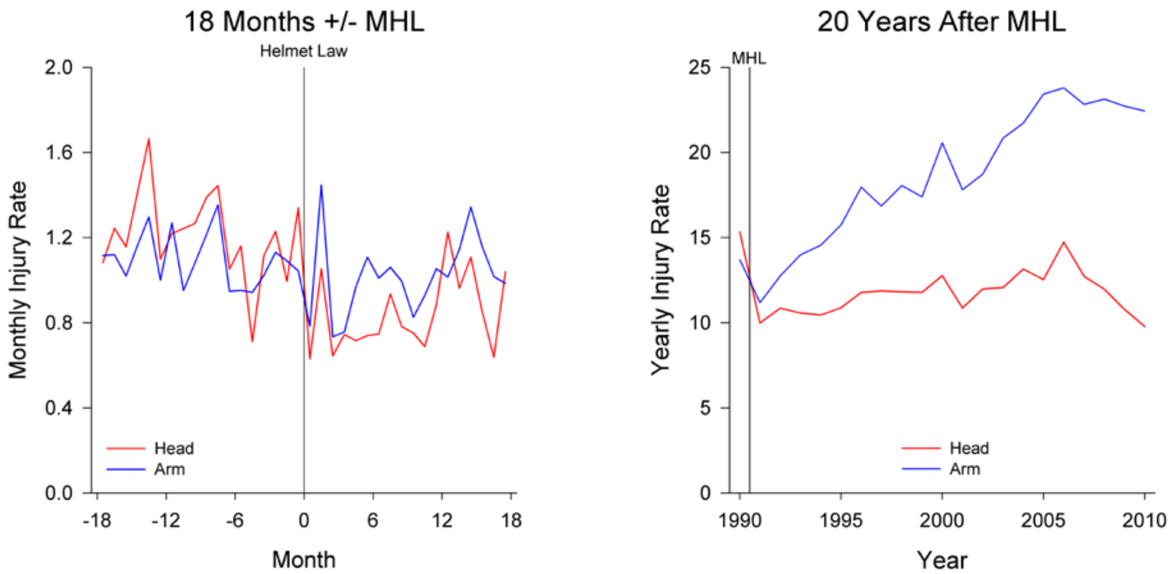

Figure 6
Cyclist head and arm injury hospitalisations in NSW during (a) the 36 month period around the helmet law and (b) 20 years post-MHL (source: NSW Department of Health)

In the eighteen month period before the helmet law, the head injury rate is consistently higher than the arm injury rate while the opposite holds in the subsequent eighteen month period. There is a clear divergence between these injury rates over the next twenty years using yearly aggregated data.

In a review of New Zealand data found in Tin Tin, Woodward and Ameratunga (2010), Clarke (2012) argues the NZ helmet law is associated with an increased injury risk of 20-32%. This conclusion comes from comparing serious injuries per million hours cycling in the periods 1988-1991 and 2003-2007. The NZ helmet law was effective 1 January 1994 and Clarke's comparison ignores intermediate injury data for 1996-1999 and estimates of helmet wearing. There is a 17% decline in serious cycling injury comparing 1988-1991 with 1996-1999 data. This time period also corresponds to an increase in helmet wearing (see Figure 7).

Although helmet use is a targeted intervention (i.e., a helmet will only protect the head), Clarke did not analyse head injuries separately and instead combined all cycling related injury. Missing from Clarke's study was a 67% decline in serious traumatic brain injury (TBI) comparing 1988-1991 and 1996-1999 data. Further, when contrasted with increases in helmet wearing, there is a decline in both serious injuries overall and serious TBI alone. While there is an increase in serious cycling injury comparing 1996-1999 and 2003-2007 data, there is only a slight increase in TBI. During this period, estimates of helmet wearing in NZ have remained steady indicating any changes in the injury trends are unrelated to helmet wearing.

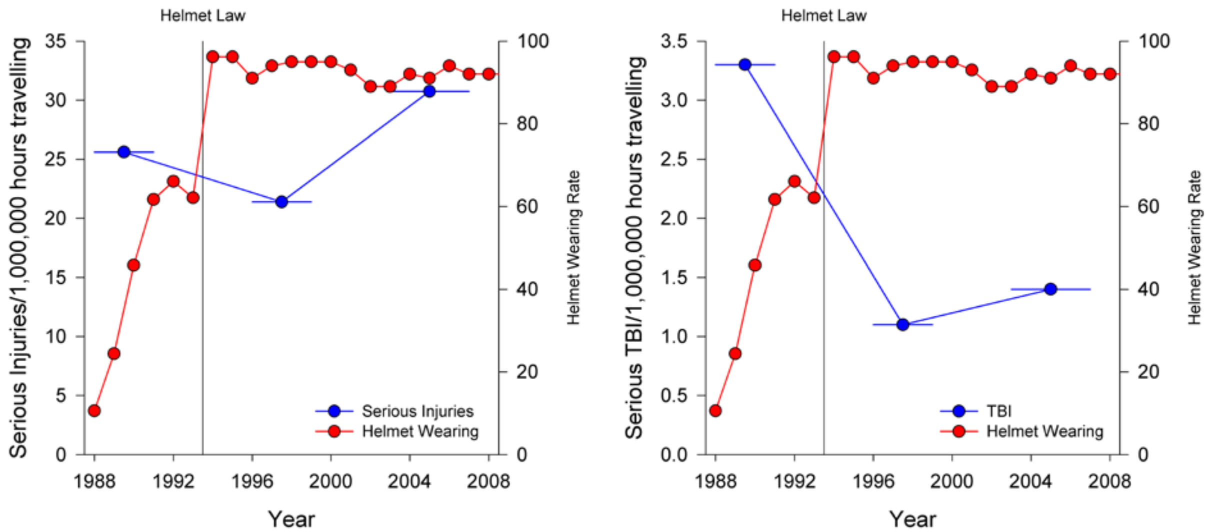

Figure 7

Serious cycling-related injuries and traumatic brain injury (TBI) per one million hours travelling and estimated helmet wearing rates in New Zealand (source: Tin Tin et al., 2010; New Zealand Ministry of Health)

Helmet laws result in a net health reduction

It is often argued the deterrent effects of MHL, and subsequent increase in injury risk per cyclist through safety in numbers, leads to a net reduction in health. In a study regarding the health impact of MHL, de Jong (2012) concludes MHL is only overall beneficial under "relatively extreme assumptions".

Among de Jong's assumptions is helmet legislation can only lead to declines in cycling. As support for this assumption, de Jong notes, without citation, motorcyclists do not like helmets, so it is "safe to assume the same is true for bicyclists". He also points to Robinson (1996, 2006, 2007) as the "main statistical studies" on the subject. As demonstrated above, there is no evidence adult cycling diminished with helmet legislation in NSW and the safety in numbers hypothesis is not supported using available NSW data. There is also little evidence helmet use increases the risk of DAI or an increase in riskier behaviour. Therefore, the belief that helmet legislation will not lead to less cycling or helmet use will not increase the risk of injury are reasonable assumptions. Under those conditions, de Jong's model will always demonstrate a net benefit to helmet legislation.

With regards to Australia, de Jong used model parameters based on data from other nations. So, it is unclear if any of his results are applicable to cycling in Australia. Additionally, Newbold (2012) found a benefit to helmet legislation using de Jong's model using parameters relevant to the United States.

Discussion

In this paper, we discuss common arguments against the use of bicycle helmets and government mandated helmet use. As demonstrated, these arguments are not supported by available data (DAI hypothesis, safety in numbers), rely on the omission of key data (deterrent effects of legislation, lack of population level effects) or the misrepresentation of data (risk compensation, lack of population level effects). The hypothesis helmet legislation leads to a net health disbenefit, or the related obesity link (for example, see Rissel, 2012a), is dependent on these arguments and is therefore not supported by available evidence.

This is not the first paper critical of methods used in anti-helmet arguments. Other work not cited above has pointed to common fallacies in the literature portraying bicycle helmets or helmet laws negatively (Hagel & Pless, 2006; Hagel et al., 2006; Rechnitzer, McIntosh & Grzebieta, 2012; Olivier et al., 2012; Biegler & Johnson, in press; Trégouët, in press).

Many of the authors arguing against helmets cited in this paper belong to anti-helmet advocacy groups. Adams, Curnow, Franklin, Gillham, Hillman, Robinson and Wardlaw are members of the Bicycle Helmet Research Foundation (BHRF, 2013). Curnow and Gillham also maintain their own websites dedicated to anti-helmet advocacy (CRAG, 2013; Gillham, 2011). Mindell is vice-chair of the Transport and Health Study Group whose objectives include "To promote a more balanced approach to cycle safety and oppose cycle helmet legislation" (THSG, 2013). The THSG is affiliated with a new Elsevier journal with Mindell as editor-in-chief with Rissel and Wardlaw as members of the editorial board (Journal of Transport & Health, 2013). Additionally, Rissel has participated in anti-helmet protests (Chadwick, 2012).

Quite often arguments against helmet legislation are framed as an all-or-nothing safety intervention strategy that is in direct competition with creating segregated cycling infrastructure. In other words, it is believed a government will support one but not both. To wit, Ian Walker in a recent New York Times article states "Any solution to bicyclist safety should focus on preventing collisions from taking place, not seeking to minimize the damage after a collision has occurred" (Egan, 2013). This strategy runs counter to the safe management system (SMS) approach supported by government and safety advocacy groups. There is also little support for focussing on injury avoidance alone in the injury record. In

NSW from 1991 to 2010, only 12% and 23% of bicycle related head injury hospitalisations for children and adults respectively involve a motor vehicle. The goal of SMS, on the other hand, would be to minimise the risk of a crash and to minimise the risk of injury when a crash occurs.

There are other anti-helmet arguments we have not addressed. A Straw Man is often posited that helmet use is not mandated for pedestrians, so it should not be applied to cyclists. This argument has appeal on the surface; however, a similar argument could be made regarding seat belt legislation. A similarly structured argument might be “seat belts are not required for cyclists who are often injured falling of a bicycle, so it should not apply to drivers or passengers.” Another argument is that helmet legislation impedes personal freedoms. In a democratic society, this is a valid argument for an individual. However, helmet legislation would be valid for a democratic society with support from the majority. An estimated 94% of Australians support helmet legislation (Essential Report, 2012).

This paper does not suggest research in favour of helmets is not without flaws. For example, Robinson (2001) was critical of Povey et al. (1999) for not fitting time trends in their assessment of the New Zealand helmet law. Povey et al. fit the log of the ratio of head injuries to limb fractures with estimates of helmet wearing for years 1990-1996. Observations taken over time can exhibit serial dependence and failure to account for this interdependence can lead to invalid inferences. The model used by Povey et al. assumes independence, serial or otherwise. Fitting time trends is an indirect method for accounting for serial dependence and there are more direct statistical methods for this purpose, for example, autocorrelated regression or autoregressive integrated moving average models. At issue with the Povey et al. analysis is whether their model assumptions were justified, specifically serially independent observations. Neither Povey et al. (1999) nor Robinson assessed serial dependence in the New Zealand data and there are other methodological issues in much of the research assessing the New Zealand law (Wang et al., submitted). Importantly, the Durbin-Watson statistic for this data is 1.8 indicating an independence assumption is reasonable and, therefore, the results of the Povey et al. (1999) analysis are valid. So, Robinson’s concerns were reasonable, although her specific criticism was not.

Conclusion

While there is much conflicting evidence related to helmets and MHL efficacy, when brought under statistical scrutiny the majority of evidence against helmets or MHL appears overstated, misleading or invalid. Moreover, much of it has been conducted by people with known affiliations with anti-helmet or anti-MHL organisations. Ultimately, this body of work distorts our understanding of the mechanisms by which helmet wearing protects the heads of cyclists and the factors related to the success or failure of helmet legislation. Future research should exercise caution regarding the validity of the anti-helmet arguments discussed in this paper unless, of course, they are supported by robust data and analyses from the peer-reviewed literature. We further caution against the use of advocacy groups, such as those listed above, as a resource for shaping road safety policy.

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