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The impact of compulsory helmet legislation on cyclist head injuries in New South Wales, Australia: A response

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ABSTRACT

This article responds to criticisms made in a rejoinder (Accident Analysis and Prevention 2012, 45: 107–109) questioning the validity of a study on the impact of mandatory helmet legislation (MHL) for cyclists in New South Wales, Australia. We systematically address the criticisms through clarification of our methods, extension of the original analysis and discussion of new evidence on the population-level effects of MHL. Extensions of our analysis confirm the original conclusions that MHL had a beneficial effect on head injury rates over and above background trends and changes in cycling participation. The ongoing debate around MHL draws attention away from important ways in which both safety and participation can be improved through investment in well-connected cycling infrastructure, fostering consideration between road users, and adequate legal protection for vulnerable road users. These are the essential elements for providing a cycling environment that encourages participation, with all its health, economic and environmental benefits, while maximising safety.

1. Introduction

A rejoinder (Rissel, 2012a) to our paper examining the impact of mandatory bicycle helmet legislation in New South Wales (NSW), Australia (Walter et al., 2011) raised questions around the data and methodology used in the study. In this paper we respond to each of the points raised, and endeavour to clarify certain aspects of our study.

Our original analysis attempted to assess the impact of mandatory helmet legislation (MHL) in NSW, using an interrupted time-series modelling approach with several sensitivity analyses to check underlying assumptions. As in any observational study that uses routinely collected data, there were inevitably data limitations, but we attempted to be as transparent as possible about their existence and our methods for dealing with them.

In the introduction and elsewhere in the rejoinder, a paper co-authored by Rissel, which was retracted by the publishing journal due to pervasive data, arithmetic and graphing errors, is cited as evidence against the conclusions of our study (Australasian College of Road Safety (ACRS), 2010; Churches, 2010; Grzebieta, 2011). Citation of and reliance on the results of a scientific paper after it has been formally retracted is an unusual practice in the peer-reviewed scientific literature, and may have negative consequences for the integrity of scientific discourse. For example, Fyhri et al. (2012) have cited the rejoinder as evidence that the decline in head injuries after MHL is similar to declines in other cycling injuries.

2. Window of analytic focus

The rejoinder suggested that we arbitrarily chose an 18-month pre- and post-MHL analysis window. The 18-month window was constrained by data availability prior to the MHL commencement date and was chosen to be symmetrical so as not to overstate the significance of post MHL trends. An imbalance in the time periods pre- and post-intervention may contribute to biased estimates (French and Heagerty, 2008). The rejoinder asserted that using a longer post-law period would "*significantly reduce any impact of helmet legislation in the regression analysis*". However, as a sensitivity analysis we also analysed the data using a longer post-MHL window and the observed benefit was maintained, as described in our paper (p. 2069). The suggestion that our paper found a reduced effect of helmet legislation from 21% to 9% with the inclusion of 3 and 5 years of post-law data is a misinterpretation of our reported results. In our paper, we state (p. 2068):

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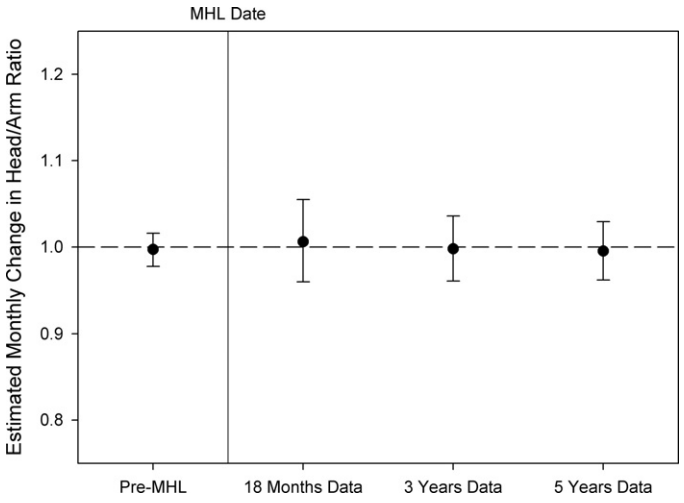

Fig. 1. Trend estimates in head to arm injury ratio for 18 months pre-law data, and 18 months, 3 years and 5 years post-law data (categorical x-axis).

> "With 18 months of post-law data, trends ranged from −7.5% to 21.2% per year, whereas with five years of data the range of trends was −0.6 to 9.2%."

The values quoted in the rejoinder are the upper limits of a range of post-law trend estimates for models based on three and five years of post-law data, respectively. The correct interpretation is that when longer post-law periods were modelled, the post-law trends in head and arm injuries both approached the horizontal (and thus no trend). This provides evidence that the observed reduction in head injuries was maintained over a longer period. Fig. 1 shows trend estimates for the head to arm injury ratio in the 18 month pre-law period and also in the 18 month, 3 year and 5 year post-law periods. With increasing duration of post-law data the confidence intervals narrow while remaining centred around one, which is indicative of no diminution of the helmet effect over time (Bell et al., in press). This has been corroborated in a subsequent study of long-term head and limb injury trends in cyclists in NSW following MHL which found that the drop in head injury rates over and above limb injuries has not only been maintained, but has increased over time (Olivier et al., 2013).

It was also suggested that because we used monthly counts of cyclist hospitalisations it would therefore be appropriate to use more stringent significance tests for the model coefficients than the customary $p = 0.05$ criterion. Stricter tests of significance are appropriate when multiple comparisons are being made, such as in models with large numbers of terms in which stepwise model selection techniques are used. However, our analysis was entirely hypothesis-driven and, as such, all seven predetermined terms remained in the model throughout the analysis, as opposed to a model building approach where non-significant terms are eliminated in an effort to obtain the most parsimonious model possible. As a comparison we also performed a backwards elimination analysis, which resulted in a similar untransformed beta estimate for the LAW × INJURY term (−0.2932) but a much more significant p-value ($p < 0.0001$) for this term. This interaction term estimates the change in head injuries in addition to any changes in arm injuries hence providing an indication of the effect of MHL independent of changes in ridership. The results from the original analysis and the most parsimonious model are given in Table 1. Because our analysis was hypothesis-driven, we did not report this more significant result in the original paper. It is well known that the power to detect significant interaction terms is lower than for main effects (Marshall, 2007). Therefore, it has also been suggested that the significance criterion for interaction terms should be less strict than for first order terms and rely more on the unstandardised effect size when interaction p-values are near the nominal 5% significance level. This further reinforces the observed significance of the head injury reduction in our original model.

3. Data pre-law in NSW

It was suggested in the rejoinder that lack of data prior to 1988 prevents examination of the true effect of MHL. While the availability of pre-MHL data was limited, Fig. 2A and B in our original paper show observed helmet wearing before and after MHL commencement, and indicate that the 3-year analytic window included the most dramatic change in helmet wearing rates. The assertion in the rejoinder that general trends in road safety adequately explain observed trends in cyclist injuries ignores the fact that our methodology accounted for such background trends, as mentioned in the preceding section. The rejoinder also included a plot of annual proportions of head injuries for various road users in Western Australia as evidence that long-term trends explain the observed drop in head injuries around the time of MHL. That plot was taken from a study by Hendrie et al. (1999) that fitted statistical models to both annual pooled counts of cyclist hospitalisations and individual subject data. After adjusting for possible changes in cyclist numbers, the authors of that study concluded that "*. . .the helmet wearing legislation was shown to have reduced the number of head-injured bicyclists in the post-law period*".

4. Phases of helmet legislation and uptake of helmet wearing

It was pointed out in the rejoinder that the MHL for children came into effect in NSW 6 months after the law for adults, and our analysis was criticised for having only a single pivot point for both adults and children. We clarify this by reference to our original paper (p. 2066):

> "Eighteen months of pre- and post-law data was therefore included for both age groups resulting in a 36 month analysis period centred on the date that the legislation came into effect and taking into account the different dates for adults and children. Thus for adults the period was from July 1989 to June 1992 and for children from January 1990 to December 1992."

Table 1
Comparison of untransformed negative binomial model estimates of cyclist head and arm injuries using the full and most parsimonious models.

Variable	Original analysis			Most parsimonious model		
	Estimate	95% CI	p-Value	Estimate	95% CI	p-Value
TIME	−0.005	−0.019, 0.009	0.454	−0.007	−0.017, 0.003	0.157
INJURY	0.072	−0.128, 0.272	0.482	0.101	0.002, 0.200	0.045
LAW	−0.112	−0.318, 0.093	0.285	−0.124	−0.288, 0.040	0.138
TIME × LAW	0.015	−0.005, 0.034	0.137	0.020	0.005, 0.034	0.007
INJURY × LAW	−0.322	−0.618, −0.027	0.033	−0.293	−0.439, −0.148	<0.001
TIME × INJURY	−0.003	−0.022, 0.016	0.740			
TIME × INJURY × LAW	0.010	−0.018, 0.038	0.504			