

Brief communication

Bicycle helmets: Lack of efficacy against brain injury

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Abstract

This paper is a rebuttal of the criticism by Hagel and Pless of my 2005 article in which I dispute the conclusion of a Cochrane Collaboration review that all types of standard bicycle helmet protect against injury to the brain. The main ground of rebuttal is that my critics take the relevant efficacy of helmets as given and argue from there.

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I reply here to the paper by Hagel and Pless (2006) which criticises an article of mine (Curnow, 2005). In my article, I dispute a conclusion of the Cochrane Collaboration review *Helmets for preventing head and facial injuries in bicyclists* (Thompson et al., 2004), that all types of standard helmet protect against injury to the brain. I argue that, at best, the review provides evidence that hard-shell helmets, now rare, protect the brain from injury consequent upon damage to the skull.

The abstract to Hagel and Pless states that I begin by criticising scientific evidence that helmets protect against head and brain injuries. This is wrong; my paper deals only with brain injury and the lack of scientific evidence that all types of helmet protect against it. The abstract continues that the crux of my argument is that “in theory helmets should not protect all mechanisms of brain injury (sic) and, therefore, all epidemiological research showing helmets are beneficial is invalid”. This is not the crux of my argument, which is that the review’s conclusion is not in accord with scientific theory of brain injury which is supported by experimental evidence (Curnow, 2003) and that it is a result of misinterpreting data. The suggestion that my paper attempts to invalidate research other than that concerning the brain is a misrepresentation.

The main argument of Hagel and Pless begs the question in contention, which is that helmets of all types protect against brain injuries. That this is assumed is evident from the reference in its abstract to “the scientific evidence” and the statement in its

fourth paragraph that helmets have been shown to be effective at reducing severe brain injury. The suggestion that support is lacking for the theory of brain injury which I expound is a corollary of the assumption. I argue that data from case–control studies need to be interpreted by that theory, but Hagel and Pless would question or dismiss it according to the simplistic theory implicit in their authors’ interpretation, thus attributing more reliability to case–control studies than to experiment. This is contrary to the aim of Cochrane reviews, which is to use evidence obtained by randomised controlled trial, the nearest approximation to experiment with human subjects.

In an early version of the review, the argument for use of case–control studies states that randomised controlled trials would be “neither feasible nor ethical” for determining the efficacy of helmets (Thompson and Rivara, 1999). As the authors had already recommended legislation to increase use of helmets (Thompson et al., 1996), presumably their concern is that it would be unethical to require randomly chosen participants in a trial *not* to wear them. Yet legislation compels whole populations of cyclists to wear helmets. And, where the efficacy of helmets against brain injury had not first been verified, as in Australia (Curnow, 2003), compulsory wearing would, in effect, be a trial. Would that not be more unethical? Moreover, it has been an uncontrolled trial in Australia because its effects have not been properly measured. Using what data are available, population studies have attempted to do this, but the assertion of Hagel and Pless that I prefer them to case-control studies is wrong; I accord primacy to scientific experiment.

Hagel and Pless devotes most of its attention to matters bearing on the value of data as evidence for showing efficacy against

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head injury in general, such as risk compensation, control for confounding factors such as involvement with a motor vehicle, and type of study. Its first paragraph states that I “largely dismiss” studies by Thompson et al. (1989, 1996) for the reason that cyclists who wore helmets may have been more careful and were over-represented in the control groups. That is not the reason; it is just one example in my argument that the studies included in the review are vulnerable to bias. Thus, the 1989 study admits to not being able to rule out the possibility that more cautious cyclists may have chosen to wear helmets and also had less severe accidents, but the 1996 one dismisses it arbitrarily. Hagel and Pless are wrong in referring to this possibility as my contention and in stating that the 1996 study fully eliminates it.

Other, unmeasured, factors could result in error in assessing protection against brain injury. Two are subjection to oblique impulse and a declining proportion of hard-shell helmets. Also, unmeasured factors could result in controls being less likely to strike their heads, violating a requirement of the design of both Thompson et al. (1989, 1996). To investigate this, the 1989 study makes a sub-analysis which focuses on brain injury and concludes that there was no significant error from unmeasured factors. The larger study in 1996 implicitly assumes the same, but my similar sub-analysis of its data suggests that unmeasured factors result in error.

Finally, I repudiate the criticism that my article would not comply with the criteria required of a Cochrane review because it “fails to present all relevant evidence for the effect of bike helmet use and legislation in a balanced way”. My article does not purport to be a review. In any case, the review itself, apart from not complying with the normal criterion of using randomised con-

trolled trials, does not present all relevant evidence; for example, it takes no account of findings of scientific research on mechanisms of brain injury of various types and those of Corner et al. (1987) on effects of helmets upon it.

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