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Bicycle helmet wearing is associated with closer overtaking by drivers:

A response to Olivier and Walter, 2013

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ABSTRACT

There is a body of research on how driver behaviour might change in response to bicyclists' appearance. In 2007, Walker published a study suggesting motorists drove closer on average when passing a bicyclist if the rider wore a helmet, potentially increasing the risk of a collision. Olivier and Walter re-analysed the same data in 2013 and claimed helmet wearing was not associated with close vehicle passing. Here we show how Olivier and Walter's analysis addressed a subtly, but importantly, different question than Walker's. Their conclusion was based on omitting information about variability in driver behaviour and instead dividing overtakes into two binary categories of 'close' and 'not close'; we demonstrate that they did not justify or address the implications of this choice, did not have sufficient statistical power for their approach, and moreover show that slightly adjusting their definition of 'close' would reverse their conclusions. We then present a new analysis of the original dataset, measuring directly the extent to which drivers changed their behaviour in response to helmet wearing. This analysis confirms that drivers did, overall, get closer when the rider wore a helmet. The distribution of overtaking events shifted just over one-fifth of a standard deviation closer to the rider – a potentially important behaviour if, as theoretical frameworks suggest, near-misses and collisions lie on a continuum. The paper ends by considering wider issues surrounding this topic and suggests public health research might be best served by shifting focus to risk elimination rather than harm mitigation.

INTRODUCTION

Bicycling is a generally safe activity that offers tremendous individual and population health benefits (Cavill & Davis, 2007; Woodcock, Tainio, Cheshire, O'Brien & Goodman, 2014) through improved cardiovascular function (Teschke, Reynolds, Ries, Gouge & Winters, 2012), enhanced mental health and wellbeing (Biddle, 2000; Ettema, Gärling, Olsson & Friman, 2010; Mutrie, 2000) and reductions in air pollution (Künzli et al., 2000). It also offers large economic benefits (Crawford & Lovelace, 2015; WHO, 2014). But despite all this, in many countries bicycling is not particularly well supported. Whilst nations like The Netherlands and Denmark provide safe dedicated routes (Pucher & Buehler, 2008; Pucher & Dijkstra, 2003; Teschke et al., 2012), bicyclists in many nations are required to travel in the same space as faster-moving and more massive motor vehicles in a relationship that is fundamentally asymmetrical in terms of the harm each party can cause to the other. It is often suggested that if a bicyclist wears an expanded-foam helmet, this can help ameliorate the dangers arising from collisions with motor vehicles. Whilst these helmets provide cushioning in the event of certain impacts (Cripton, Dressler, Stuart, Dennison & Richards, 2014; Karsch, Hedlund, Tison & Leaf, 2012), and whilst helmets in general do not seem to impair users' concentration (Bogerd, Walker, Brühwiler & Rossi, 2014), the issue of whether helmets are an effective solution to the danger caused by motor vehicles specifically, and whether they result in population-level health benefits when used in traffic, is the topic of intense debate (e.g., Curnow, 2005; Elvik, 2013; Fyhri, Bjørnskau & Backer-Grøndahl, 2012; Fyhri & Phillips, 2013; Hagel & Pless, 2006; Rissel, 2012; Robinson, 2007).

We suggest that a key reason this debate continues, and becomes so heated at times (Goldacre & Spiegelhalter, 2013), is because often people inadvertently conflate two separate questions: (1) do bicycle helmets absorb sufficient force to cushion the brain *in the event of a collision or fall?* and (2) is the likelihood of injury increased or decreased by wearing a helmet *on a given trip?* Critically, almost all bicycle trips do not involve a collision or fall. As such, we must consider whether wearing a helmet might push certain journeys from the large pool of non-incident trips to the small pool of incident trips. Some researchers have suggested that this might happen because riders' behaviour changes when they wear helmets (e.g., Fyhri et al., 2012; Fyhri & Phillips, 2013; Phillips, Fyhri & Sagberg, 2011). Here, however, we focus on possible changes in the behaviour of the motorists with whom the riders share the road.

Consider, purely as a thought-experiment, a hypothetical bicycle helmet with extremely high efficacy in the event of a collision, but which has the side-effect of causing a small proportion of motorists to become aggressive and attack the wearer with their vehicles. In such circumstances, it requires only for the helmet's efficacy to be less than 100% and for the proportion of aggressive motorists to be greater than 0% for some additional injuries to occur as a result of helmet-wearing. Perhaps the net benefit is still positive, given the various injuries helmets prevented in other riders, but regardless of this, additional preventable injuries would be occurring.[1]

One might object to our using deliberate aggression to make this illustration more salient[2], and we could debate the exact figures for helmet efficacy indefinitely, but all that would be besides the point. This thought experiment shows that it requires only two criteria to be met for there to be some possibility of new injuries occurring as a result of helmet-wearing: for more than 0% of motorists to change their behaviour around helmeted bicyclists sufficiently to cause occasional collisions, and for helmet efficacy to be under 100%. Nobody claims that 100% efficacious helmets exist; therefore, the key question is whether helmets ever change driver behaviour sufficiently to cause collisions.

This topic was first explored in a 2007 paper (Walker, 2007 – referred to here as W7). Here, Walker published an analysis of 2,355 overtaking events measured in the United Kingdom with an instrumented bicycle on-road with drivers who were unaware their behaviour was being recorded (Walker, 2010). Clearly, because they are rare, it would be unfeasible for a researcher ever to record sufficient overtaking events to show that *collisions* happen more or less often under certain circumstances. However, as described in theoretical frameworks like Hydén's (1987), near-misses and collisions lie on a continuum, such that the former – which will always be greatly more numerous – can be used as a proxy for the latter (Lu, Cheng, Kuzumaki & Mei, 2011). This is the rationale behind using reductions in overall mean passing distances as a proxy measure of increased risk in controlled studies like W7 – a potentially important point to which we will return in the Discussion.

The W7 paper generated considerable interest in the subject of driver behaviour around bicyclists (e.g., Chapman & Noyce, 2012; Chuang, Hsu, Lai, Doong & Jeng, 2013; Curnow, 2008; Kay, Savolainen, Gates & Datta, 2014; Love et al., 2012; Parkin & Meyers, 2010; Savolainen, Gates, Datta, Todd & Morena, 2012), including studies that replicated its findings on drivers changing their passing proximities as a function of rider gender (Chuang et al., 2013; Florida Department of Transportation, 2011) and vehicle type (Parkin & Meyers, 2010), and studies that applied its

1 Because almost all bicycle trips do not involve injury, even amongst the unhelmeted, once we have accepted that helmets could possibly change driver behaviour, it is easy to conceive circumstances where the net effect of wearing helmets is negative thanks to new injuries from driver-behaviour changes outweighing the number of existing injuries prevented.

2 Although if drivers deliberately hitting bicyclists with their cars sounds implausible, we recommend an online search for the term 'punishment pass', e.g., BBC (2013).

methodology to look at road design (Kay et al., 2014; Parkin & Meyers, 2010; Savolianen et al., 2012) and rider clothing (Walker et al., 2014). These replications demonstrate that the W7 methodology is sufficiently robust to reveal changes in driver behaviour when these occur. Whilst W7's finding that motorists put men in greater danger than women has passed largely without comment, various people have objected to the finding, made from the same dataset, that passing distances were on average 8.5 cm closer when the rider wore a helmet than when he did not – a potentially important outcome given the above thought experiment and the idea that near-misses are potential collisions (Hydén, 1987).

Perhaps the most notable objection to W7's conclusion about helmet wearing is a 2013 paper by Olivier and Walter (2013 – henceforth 'OW'), reporting re-analysis of the W7 data with a range of regression models to reach the strong conclusion "After re-analysis of Walker's data, helmet wearing is not associated with close motor vehicle passing" (p. 1) – an assertion repeated as the title of their paper. OW's paper can be reduced to three central claims, which we have summarized here in our own words:

- The W7 study was statistically overpowered, and thereby made a non-significant change in driver behaviour look significant
- If passing motorists leave one metre of space, this is sufficient to keep bicyclists safe from harm
- Re-analysis of the W7 data shows no effect of helmet-wearing on close passing

We now consider these three points and demonstrate that none of them provide grounds for setting aside the original study's conclusions. We finish with a new analysis of the W7 data that addresses the question of interest in a novel way, unaffected by these concerns.

(Equation 1)

'The W7 study was statistically overpowered, and thereby made a non-significant change in driver behaviour look significant'

For difficult or expensive studies (e.g., to evaluate a new medical treatment), power calculations allow researchers to make efficient use of scarce resources. Each extra data point potentially removes some doubt about the effect of the treatment, and there would be little point in carrying out an expensive study that did not gather enough data for sound conclusions to be reached. But there is a process of diminishing returns: once a certain sample size is reached, each additional data point barely changes the level of doubt and so would be a waste of valuable resources.

In contrast, when the collection of additional data entails little extra cost, there is no reason necessarily to be limited by the minimum sample size from a power calculation. Whilst a large sample becomes a concern if it is analysed to make tiny effects appear statistically significant in a null hypothesis test (Aberson, 2010), it is not inherently problematic when used to quantify the magnitude of an effect. To give a colourful example, strychnine is toxic to humans, and this phenomenon does not somehow become less real if it is measured in 50,000 people rather than 50.

Accordingly, whilst W7 did present null hypothesis tests, it also reported and discussed an R^2 measure of variance explained (.08) on p. 420 to quantify, in a way entirely unrelated to sample size, the extent to which helmet-wearing and riding position were associated with changes in passing proximity. Admittedly, this measure wrapped helmet-wearing and road position into a single effect size measure – the reason being that road position was the original focus of the study and helmet-wearing was at that stage a minor factor included out of curiosity. But the point is that the effect of helmet wearing cannot so easily be dismissed as an artefact of sample size. Below, we further respond to OW's concern with a novel measure of the helmet wearing effect, entirely unrelated to sample size and, for the first time, separated from the effect of riding position.

'If passing motorists leave one metre of space, this is sufficient to keep bicyclists safe from harm'

OW's strong conclusion that helmet wearing is not associated with an increase in close passing rests on their shifting the analysis to ask a subtly different question. The original W7 study looked at changes in the overall distribution of overtaking distances, and found that drivers passed around 8.5 cm closer on average when a helmet was worn – a conclusion that OW agree with. The key distinction between W7 and OW is that, after confirming that drivers on average got *closer*, OW immediately put aside this finding and focused instead on the question of whether drivers who saw the helmet got, as they term it, *close*. As such, their approach stands or falls on how they justify this change of focus from *closer* to *close* and, subsequently, how they justify their definition of *close*.

Table 1 – The effect of bicycle helmet wearing on driver behaviour in Olivier and Walter's (2013) two regression analyses. The upper row shows regression coefficients predicting change in passing distance (in metres); the lower row shows odds ratios for motorists encroaching within 1.00 m of the bicyclist when the rider wore a helmet

Univariate approach	Multivariate approach

	Univariate approach			Multivariate approach		
	Estimate	**SE or CI**	**p**	**Estimate**	**SE or CI**	**p**
Linear regression	– 0.085	0.016	< .001	– 0.058	0.015	< .001
Logistic regression	1.30	0.88 to 1.91	.182	1.13	0.76 to 1.68	.54

To make OW's shift of focus clear, Table 1 reproduces the key information from their two analyses of whether driver behaviour changed in response to a helmet. The first row of Table 1 comes from their linear regression analysis to predict passing distance and shows a significant effect of helmet wearing, agreeing with the conclusion from W7. The second row of Table 1 comes from OW's equivalent *logistic* regression analysis, predicting the binary outcome of whether each passing event left more or less than one metre of space. This analysis, unlike the first, finds the effect of helmet-wearing is non-significant. Whilst an analysis of close passing might be useful as a complementary follow-up test when studying overtaking (Walker et al., 2014), this is not what OW did. Rather, they carried out a linear regression and found a significant effect of helmet wearing on behaviour, then carried out a logistic regression that found no effect, and at that point they set aside the first analysis without further comment. If OW wish to introduce this change in focus, it is their task to convince the reader that:

1. there are good grounds for setting aside variability in driver behaviour and instead coding every event as categorically safe or dangerous, rather than as points on a continuum;

2. there is a sound basis for treating one metre as the qualitative dividing line between safe and dangerous overtakes; and

3. their analysis had sufficient power to detect a relevant (say, 10%) difference in the risk of experiencing passing events under one metre.

The theoretical issue (should events be categorized into a safe/dangerous dichotomy rather than being placed on a continuum?) is not addressed by OW. Rather, they note near the top of p. 2 that the mean passing distance in W7 was over one metre and thereafter focus on this division. This choice of a one-metre cutoff was, in OW's own words, "somewhat arbitrary" (p. 4) and seems to have been influenced particularly by several governments having passed laws specifying one metre, or three feet, as a legal minimum passing distance. However, none of those laws is based on evidence. Indeed, there are countries that have rejected one metre and instead passed laws specifying that 1.5 metres should be the minimum space left by motorists (road.cc, 2009;

SpanishNewsToday, 2014). The lack of evidence underpinning overtaking laws can be seen when we note that only one study has looked at the physical forces exerted on bicyclists by passing vehicles (Khan & Bacchus, 1997), and that Olivier (2014) himself later noted:

> "There is unfortunately scant peer-reviewed research, one way or the other, on this topic. The two citations listed [in OW][3] were the only ones I could find. I contacted Khan and Bacchus for a copy of their paper and more information about trying to establish a "safe" passing distance. They provided the paper, but would not answer my queries."

In the face of such uncertainty, it is worth remembering that the standard global approach to risk management (e.g., European Union, 2011) is to follow the Precautionary Principle. This states that in the absence of clear scientific evidence, the less risky option should be preferred, and that the burden of proof falls to the party advocating a more risky option. Given OW's admission that one metre was effectively an arbitrary choice, application of the Principle should make us cautious. If we want to use existing legislation as a guide to separating close from not-close events, we should at least use the 1.5 metre rule mandated in Spain and Germany (road.cc, 2009; SpanishNewsToday, 2014) and place the burden on proof on those who would suggest a closer distance to define safety.

If we were to follow this approach, OW's conclusions look problematic. We have reproduced their logistic regression in Table 2 (this comes from Table 5 in their original paper, leaving out two rows for which there are insufficient data for any meaningful analysis). We can see that, if OW had instead defined 'close' as a motorist passing within 2 metres of the rider – or within 1.5 metres, in accordance with European laws – then they *would* have concluded that bicycle helmets are associated with 'close passing'. Basically, any cut-off other than their "somewhat arbitrary" (p. 4) one-metre choice would qualitatively change their conclusion. We should also note here that the data OW analysed were collected in the United Kingdom, where there is no legally mandated passing distance. As such, their analysis did not even assess adherence to their so-called 'one-metre rule' in a legal sense of the term.

Table 2 – Olivier and Walter's (2013) estimates of the odds ratios for motorists passing close to the bicyclist, when that rider wore a helmet, for three cut-point distances for which there were sufficient data for meaningful analysis

Cut-point	**Odds ratio**	**95% CI**	**p**
1.0 m	1.13	0.76 to 1.68	.54
1.5 m	1.21	1.02 to 1.44	.028
2.0 m	1.46	1.13 to 1.89	.004

3 The second citation was Love et al. (2012), which has no actual data – it just mentions the Khan and Bacchus study.

The final task we set for OW's paper was to demonstrate their new analysis had sufficient power. Ironically, after their invoking power as a key criticism of W7, we find that, in choosing to shift from a continuous measure of driver behaviour to a categorical one, OW left themselves with an underpowered analysis for rejecting the hypothesis that there is no effect of helmet wearing on the proportion of passes under one metre. Their power analysis for their own logistic regression suggested that, even with a sample size of 14,023, their multivariate analysis would have a 13% chance of failing to detect a 22% increase in passing events under one metre. Although close passing might be quite common across bicyclists collectively, it is, thankfully, a relatively rare event for each individual rider. As noted earlier, collecting sufficient data is a challenge when studying rare events. This all means the W7 dataset, which OW use for their analysis, is not sufficient for them to reject the idea that motorists are more likely to encroach within one metre in response to helmet wearing (this lack of power is also why our Table 2 omits two rows from their original table – there were insufficient data to make any useful analysis of 0.75 and 0.50 m cut-offs). This further demonstrates that OW's title was misleading: absence of evidence is not evidence of absence, and OW have failed to show that helmet wearing was not associated with close passing; they have simply shown that the study was so underpowered, owing to the rare nature of extremely close encroachments, that the best estimate (OR = 1.16, 95% CI 0.78 to 1.74, based on a revised model including street and vehicle type and the two cases where the experimenter was struck by a passing vehicle) has too high a standard error for us really to say one way or the other.

In summary, the OW paper rests entirely on the assumptions that (1) rather than study changes in the location of passing distributions, we should qualitatively divide the entire universe of overtaking events into two types: those above and below a certain proximity; and (2) one metre is a useful estimate of this dividing line. Neither of these assumptions was justified by OW, and both were revealed as problematic when we tested them. Without the first assumption (the first row of Table 1), OW came to a totally different conclusion and they provide no argument for setting aside that analysis in favour of the analysis in the second row of Table 1; if we relax their "somewhat arbitrary" second criterion to 1.5 m or 2.0 m, their conclusions reverse, and their analysis agrees with the conclusions in W7 that helmet-wearing was associated with changes in driver behaviour. Finally, OW's preferred analysis was too underpowered to support their desired conclusion that helmet-wearing did not affect drivers, given that close passing, within one metre, is a relatively rare event within the dataset they analysed.

'Re-analysis of the W7 data shows no effect of helmet-wearing on close passing'

As we have seen, this claim is based on OW's decision to move from using tests of location to a logistic regression analysis to predict close versus non-close passing events. As we have also seen, this approach is problematic. Given OW's interest in statistical power, it is also surprising that their multivariariate analysis arbitrarily grouped vehicle type into small or large, despite a reduction of 5.6 in the AIC by including the different vehicle types in the logistic regression model. Taxis passed closer than cars, and heavy goods vehicles passed closer than light goods vehicles.

In this paper, we bypass any concerns that might arise with complex multivariate models and focus exclusively on the main question of interest. Figure 1A shows the full W7 dataset presented as two histograms – the helmet-wearing data in red and the no-helmet data in blue. Figures 1B to 1F present the W7 data separately for each riding position (distance from the road edge) that was tested in the original study. Moreover, given OW's comments about statistical power, the measure of helmet effect given next to each plot in Figure 1 is a dimensionless standardized Cohen's (1988) *d* statistic, quantifying the magnitude of the difference between the helmet and no-helmet data in standard deviations, without any reference to W7's sample size.

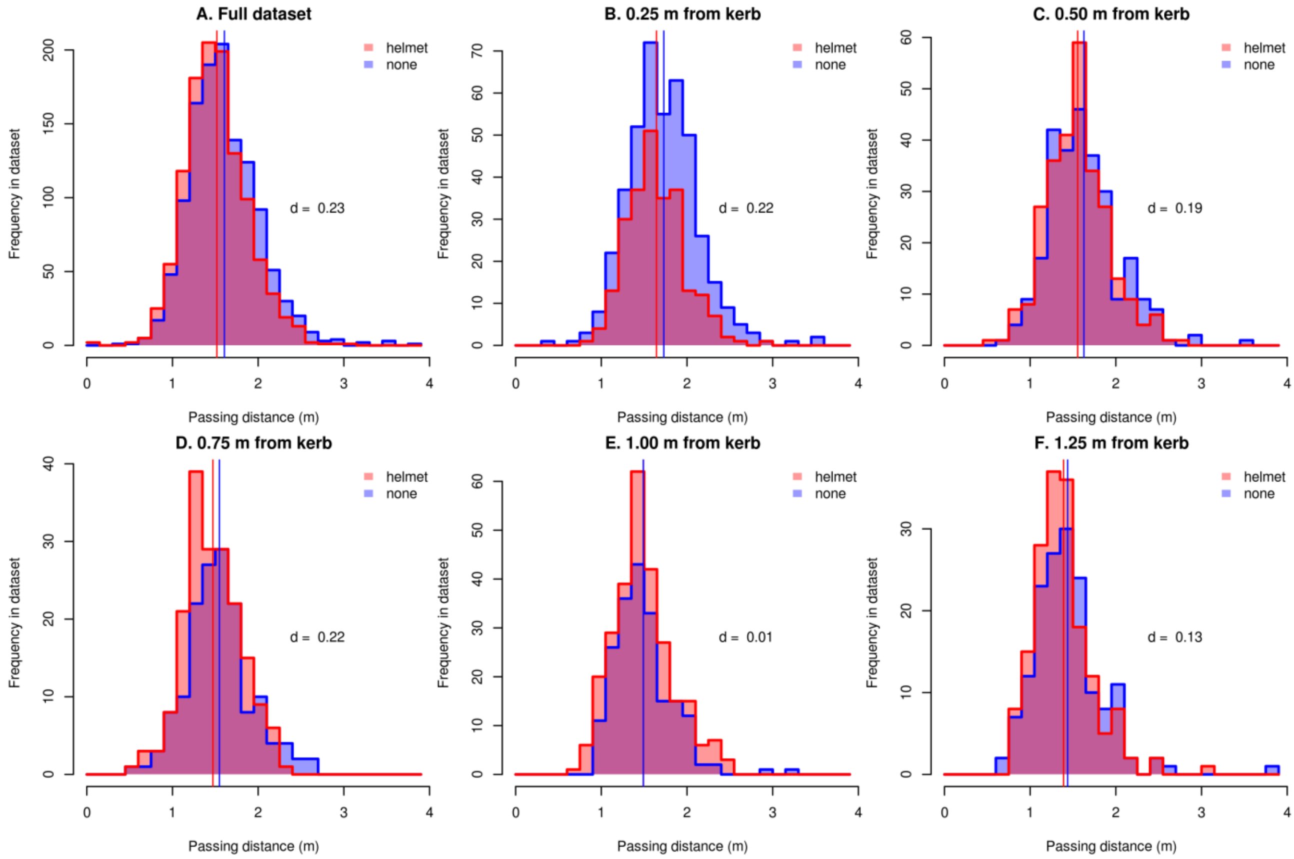

Figure 1: Distributions of passing proximities from the W7 data for instances when the rider wore a helmet (red) and no helmet (blue). Vertical lines represent distribution means

It is clear from Figure 1 that, with the exception of the one-metre-from-the-kerb riding position (which was explained in Walker (2007) as a likely influence of the roads' centre-lines), the mean passing distance is consistently reduced in the helmet condition. The magnitude of this effect is generally around one-fifth of a standard deviation – small, but potentially important given the number of overtakes that happen every day. The nature of the effect is that the distribution shifts to the left when a helmet was worn; the number of above-average overtakes reduces, and the number of below-average overtakes increases. The answer to the question 'Were there changes in driver overtaking associated with helmet-wearing?' is most easily seen in Figure 1A, where the numbers of helmeted (1151) and unhelmeted (1206) overtakes are similar, making comparison straightforward. Figure 1A clearly shows a leftward shift in the distribution, indicating that there was a continuous change in driver behaviour and thus supporting the idea that the appropriate statistical approach for this topic is to use tests of location rather than tests predicting category membership.

Discussion

Olivier and Walter (2013 – 'OW') concluded their re-analysis of Walker's (2007 – 'W7') data with the strong statement "helmet wearing is associated with a small difference in passing distance and is not associated with close passing" (p. 7). This report has shown that the second part of their conclusion, which they presented as the sole conclusion in their abstract and on which they titled their paper, is not supported by the data. That conclusion was based on changing the nature of the question from whether helmet wearing is associated with *closer* passing (as a proxy for increased danger of collision) to whether it is associated with the relatively rare event *close* passing – with 'close' defined specifically as within one metre. OW agree with the original W7 conclusion that wearing a helmet was associated with a reduction of 8.5 cm in mean passing distance, and there is also agreement that helmet-wearing is associated with significantly more close passes if 'close' is defined as two metres, or, like the laws in Spain and Germany suggest, 1.5 metres. But the one-metre distinction is a sticking-point. We have shown that, despite the importance OW attach to this one-metre cut-off, the data they used lacked the necessary power properly to test it. We have also shown that one metre was an arbitrary choice and had OW slightly changed it – as, we believe, they ought to have done under the Precautionary Principle – their conclusions would have been qualitatively different. Given these issues, even if we were to say that OW had chosen to ask a fundamentally separate question to W7 – "Were helmets associated with motorists getting 'close'?" – their paper still does not provide a defensible basis for saying no. Overall, the data we have at present suggest that motorists, on average, passed closer when the rider wore a helmet. This is a notable finding if we accept that closer passing might be a proxy for increased probability of collision.

This raises an important point. Perhaps without being sufficiently explicit, we and OW are both making claims about the *mechanisms* by which drivers' perceptions of bicyclists might (or might not) translate into collisions during overtaking manoeuvres. Our suggestion is that, across the infinite population of all possible bicycle overtakes, there is a roughly normally distributed set of distances. The model we favour is one in which, owing to beliefs and stereotypes held by some motorists (Basford, Reid, Lester, Thomson & Tolmie, 2002; Walker et al., 2014), this distribution shifts to the left or right under certain conditions: to the right when the bicyclist is a woman rather than a man, for example (Walker, 2007), and to the left when the bicyclist wears a helmet (Figure 1A). The implication of a left-shift in the distribution would be that a small number of events, in which motorists just miss hitting bicyclists, or clip them on the elbow when overtaking (something that has happened to the first author more than once), might be translated from near-misses to more serious collisions. This approach would fit models of collision causation like Hydén's (1987), which specifically place collisions and near-misses on a continuum and which say that we can therefore learn about relatively rare collisions by studying far more numerous near-misses.

OW, on the other hand, seem to be suggesting an overall shift does not happen like this. Whilst we doubt they could look at Figure 1A and suggest there is no reduction in the more generous overtakes when the helmet was worn, their use of binary cut-offs implies that they envisage a limit to this shift. In other words, OW seem to suggest that drivers get closer, but not at the lower end of the distribution, such that the number of collisions remains the same. Such a position would be a perfectly valid claim, and the two competing models would thereby provide a testable hypothesis – perhaps best explored in a driving simulator in a country where motorists are used to seeing a mixture of helmeted and unhelmeted riders. But the key point for the time being is this: OW do not provide grounds for rejecting the original claim of a leftward shift in the distribution of passing distances in response to helmet wearing, and the data presentation in Figure 1 indeed bolsters this position. OW's strong conclusion is not supported by evidence at this point.

There are also wider issues that must be addressed here. Even if we accepted the idea that there is a discontinuous effect on driver behaviour (such that drivers' mean passing distance decreases in response to seeing helmets, but without the number of collisions increasing), these are insufficient grounds for saying that wearing a helmet is definitely innocuous given our earlier point about how almost all bicycle trips involve no collision. The data available do not include measures of speed (it is plausible that passing speed might change even where distance does not – Walker et al., 2014). Moreover, the subjective experience of motor traffic passing close to a bicyclist can induce instability in the rider (Chuang et al., 2013) and can be a disincentive to healthy active travel (Aldred, 2016; Aldred & Crosweller, 2015; Sanders, 2015). So even if the sight of a helmet encourages motorists to encroach up to, say, the one-metre point and no closer, this would still be a serious problem if such close passes discouraged people from bicycling (as Aldred, 2016; Aldred & Crosweller, 2015; Joshi, Senior and Smith, 2001; and Sanders, 2015, suggest they do). It is imperative we remember that the number of premature deaths from physical inactivity outweighs the number of deaths from bicycling collisions by several orders of magnitude (Australian Institute of Health and Welfare, 2015; Cavill & Davis, 2007). Moreover, the 'Safety in Numbers' hypothesis (Jacobsen, 2003; Robinson 2005) suggests that reductions in cycling increase the risk of injury. Consequently, if we are motivated to improve public health we are far better seeking to prevent collisions from occurring in the first place rather than seeking to mitigate their effects.

At this point, it is possible to argue, as Olivier, Wang, Walter and Grzebieta (2014) have done, that promoting bicycle helmets does not preclude this focus on wider public health that we advocate; rather, Olivier et al. suggest we should wear both belt and braces by following a "safe system approach … where personal protection is seen as a critical component of the whole system to reducing vulnerable road user … injuries" (p. 19). However, this is an unusual definition of a safe system approach. In almost every other sphere of endeavour, the dominant safe systems approach to risk management is the *Hierarchy of Controls* illustrated in Figure 2 (Canadian Centre for Occupational Health and Safety, 2014; Centres for Disease Control and Prevention, 2015; Health and Safety Executive, 2011; New South Wales Government, 2011). This framework explicitly states that personal protection is the least-effective option for dealing with a potential hazard, to be employed only once efforts to neutralize the hazard through elimination, substitution, engineering and administrative controls have failed. Within this framework, protective equipment is not an approach to be used in tandem with these other measures since, if employed properly, these other measures will have rendered it redundant. In the case of protecting bicyclists from motor vehicles, hazard elimination – the first-choice solution under the Hierarchy – can be achieved through engineering (keeping motor vehicles in separate space), and further hazard reduction can be achieved through administration (for example by placing a strong legal obligation on motorists to keep non-motorists safe from harm). Both of these approaches are used extensively in, say, the Netherlands (Pucher & Buehler, 2008; Pucher & Dijkstra, 2003; Teschke et al., 2012), where the use of personal protective equipment is correspondingly unusual outside of sports riding. We leave it to the reader to decide which is the true safe system approach, and to reflect on whether encouraging sustainable active travel is better approached under Australia's philosophy – where personal protective equipment is apparently a "critical component" of bicyclist safety (Olivier et al., 2014, p. 19) – or in the Netherlands where risk is managed through engineering and legal frameworks intended to protect the vulnerable.

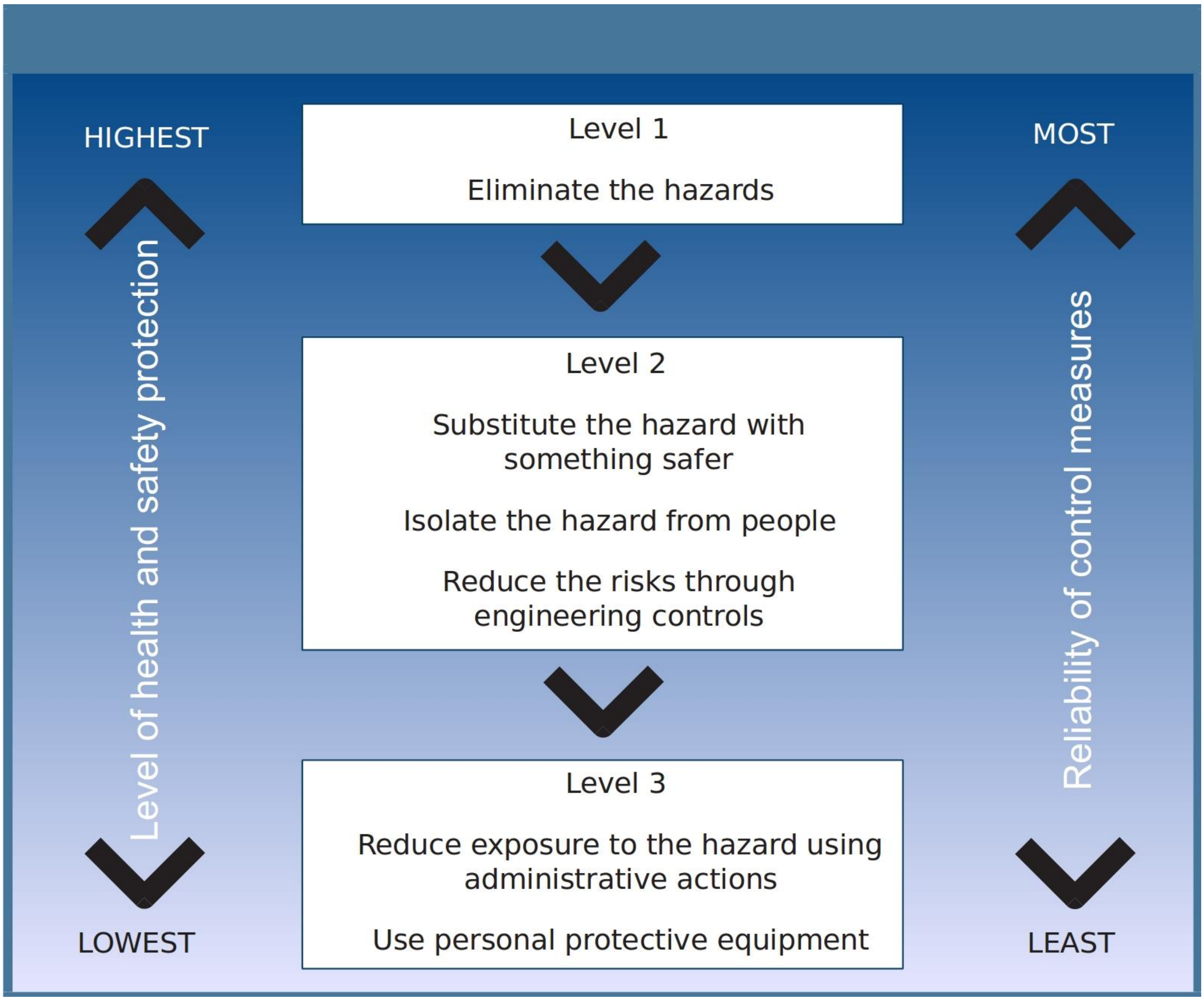

Figure 2: The Hierarchy of Controls for hazard management, ranging from the most effective to the least effective approach. Image Creative Commons from New South Wales Government (2011)

Finally, for the avoidance of doubt, we are not insisting here that bicycle helmets can have no role in making bicyclists safer; and we do not make the arguments in this paper to be contrarians. We understand it can seem counter-intuitive for researchers interested in public health to question the utility of equipment intended to promote safety, and obviously, if a head is going to receive a blow, it is generally going to be better if that blow is cushioned to some extent. But – and this is the critical point – to a first approximation, making a trip by bicycle does not involve receiving a blow to the head. The W7 dataset at least hints that the probability of a trip transitioning from the large pool of uneventful journeys to the smaller pool of journeys with a collision might increase owing to changes in driver behaviour in response to seeing a helmet. This merits caution until further data can be obtained.

Above all, and tied back to our preference for the genuinely safe-system approach embodied in the Hierarchy of Controls, our reservations about the recommendations of authors such as Olivier et al. (2014) concern the moral issues that arise with bicycle helmets being promoted – and especially mandated – as a solution to dangers that would not be present if it weren't for motoring. Even if bicycle helmets offered 100% protection from the impacts of motor vehicles, this is, critically, a risk imposed upon bicyclists without their consent. To paraphrase Voeckler (2007), suggesting that bicyclists must buy and wear protective devices to remain safe is no different from suggesting non-smokers must buy and wear gas-masks as a solution to passive smoking. In both cases, these are solutions that technically 'work', but they place all the responsibility for action – and a financial burden – on the non-consenting injured party. In the case of bicycle helmets, it is, moreover, a 'solution' that serves to maintain a status quo in which people choosing a healthy, clean and socially responsible mode of travel are systematically marginalised (Aldred, 2014) in their competition for limited public space with those who have chosen to use motor vehicles.

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Figure legends

Figure 1 – Distributions of passing proximities from the W7 data for instances when the rider wore a helmet (red) and no helmet (blue). Vertical lines represent distribution means

Figure 2 – The Hierarchy of Controls for hazard management, ranging from the most effective to the least effective approach. Image Creative Commons from New South Wales Government (2011)