Risk compensation, contradictions and confusion

D L Robinson, BSc, PhD;
R M Keatinge, MA MB BChir, MFPHM, MRCGP, MBA

(2929 words)

Abstract

Many studies have shown that children are influenced by risk compensation. For example, when children ran an obstacle course wearing a helmet and wrist guards, tripping, falling and bumping into things increased by 51% compared to without.[1] Yet a paper by published in this journal in 2006 concluded that, for children aged 8 to 18, risk compensation was "highly doubtful".[2] To see whether this problematical conclusion might have been avoided, the methods in that study are examined and discussed, in conjunction with other readily-available evidence showing increased risks/crash rates when protective equipment is used.

Introduction

There can be little doubt that children are influenced by risk compensation. When running an obstacle course in a gym, children were more reckless and finished the course quicker wearing a helmet and wrist guards (H&WG) than without. The differences were quite substantial. 10-12 year old girls tripped, fell or bumped into things on average 2.85 times without H&WG, compared to 4.55 times with H&WG, a 60% increase. Increased risk-taking averaged 49% for 7-9 year old girls and 48% for both 7-9 and 10-12 year old boys. Sensation-seeking children showed greater risk compensation.[1]

Yet a paper by Pless et al. published in this journal in 2006 concluded that, for children aged 8 to 18, risk compensation was "highly doubtful".[2] To see whether this problematical conclusion might have been avoided, we examine the methods used in that paper and evidence from other readily-available studies.

Aims of the study

The study of Pless et al. was a continuation of a pilot study of 63 children with non-severe injuries who were waiting to be seen at an emergency department.[3] Those wearing protective equipment (PE) scored lower on the thrill and adventure seeking scale, suggesting that they were,

Author Affiliations:
Bicycle Helmet Research Foundation http://www.cyclehelmets.org/mf.html?1121
(D L Robinson & R M Keatinge)

by nature, less venturesome. The responses also suggested that children wearing PE were more likely to report increased risk-taking than those who did not. Moreover, when asked hypothetical questions, the majority of children reported changes toward riskier behavior when using PE.[3]

Researchers planning the follow-up study expressed concerns about bias – even though the children said that they would take more risks when using PE, perhaps they would not actually do so. Instead of hypothetical questions about behavior with and without PE, the follow-up study asked children if they were going fast or taking chances when they were injured.[3]

Methodological problems

Focusing on the causes of injury could also lead to biases. It could be argued that, if the child's behavior caused the injury, he or she must have been "taking chances", "going fast" or doing something dangerous. Consequently, risk compensation would not necessarily lead to significantly different responses of PE-users vs non-users.

Presumably, if risk-taking equates to risk of injury, children who are injured less often take fewer risks. In this study, non-PE-users were significantly less likely to have suffered a previous injury (crude odds ratio (OR) 0.43, P<0.0015). The adjusted OR was even more significant (0.06, CI 0.02-0.18) suggesting that, after adjusting for confounders, PE-users were 17 times more likely to have been injured previously than non-users.

The strong association between PE use and likelihood of previous injury illustrates the difficulties of drawing useful conclusions from this study. Do children with greater risk of injury choose to wear PE, or do children who wear PE take more risks?

Given such problems, it is difficult to envisage how this study could have provided evidence of risk compensation – i.e. the difference in behavior of the same child with and without PE. If a child usually wears PE, "going faster than usual" would mean going faster than the normal circumstances when PE is worn. This may provide information on whether going faster than usual leads to injury. However, without knowing whether each child normally used PE, it tells us very little about risk compensation.

Moreover, a study published in 1988 found that risk compensation could not be detected from between-subject comparisons, only within-subject comparisons.[4] The pilot study of 63 children compared the same child with and without hypothetical use of PE, successfully showing that children say they will take more risks when wearing PE.

Between-subject comparisons can be an order of magnitude more variable than within-subject comparisons, so the follow-up study would have limited chances of success. The authors noted that their sample was large enough to detect a 2-fold difference in risk-compensatory behavior, but this is nowhere near adequate. In this study, helmets were the most common PE and 16.2% of PE-users and 17% of non-users had head and neck injuries. This suggests that even a 20% increase in risk-taking by PE-users would result in more ER presentations than if no PE had been worn.

It is also now also generally recognized that observational studies comparing self-selected groups (i.e. subjects who chose a safety measure or health treatment, e.g. using PE or hormone replacement therapy) with subjects who chose otherwise, are much less accurate than randomized trials (where treatments are assigned at random to subjects) or crossover trials (where, e.g. subjects receive both treatment and control in random order).

The problems of inaccuracy and erroneous conclusions from comparisons of self-selected groups were highlighted by a systematic review of recent highly-cited research, published in 2005.[5] Included were 39 studies assigning subjects randomly to treatments, and six using self-selected groups. Four of the six studies using self-selected groups were later found to be totally incorrect (subsequent randomized trials found no evidence of the claimed benefits). In another, the benefit was much lower than originally claimed. Thus five out of six were either totally incorrect or had highly exaggerated estimates of benefit. In contrast, only 2 out of 39 randomized studies (5%) were totally incorrect, with later studies finding no evidence of benefit, and only 6 (15%) showing smaller benefits less than originally claimed.[5]

These results imply that great care is required to correctly interpret data from studies comparing self-selected groups. All possible contractions and confounding factors should be examined as carefully as possible.[6]

Inconsistencies

Unfortunately, inconsistencies in this paper make it very difficult to understand the confounding factors in the self-selected groups of this study. Table 2, for example, presents odds ratios (OR) for PE use, whereas Table 3 has identical labels, but presents OR for non-use. Without careful scrutiny readers could mistakenly assume the OR in Table 3 are the same as Table 2.

Also confusing is the inadequate explanation about how the OR were calculated. Some are easy to understand, e.g. in Table 2, the reported OR of 2.43 (identical to the crude odds ratio) clearly shows that boys were more likely to use PE than girls. Reported OR for age (also identical to the crude OR) show that children under 11 years are twice were likely to wear PE as older children.

But, later in Table 2, the crude OR for bicycling (relative to hockey or skating) is 0.58, but the reported OR is 4.80. Thus the percentage of cyclists actually wearing PE was lower than for hockey players, yet we are supposed to believe that cyclists were almost 5 times more likely to wear PE. This 8-fold change in the OR implies that there is substantial confounding that could lead to considerable difficulties interpreting the data, especially if readers are left to guess what terms were fitted in the model(s) used to calculate OR. Table 4 reports both adjusted and unadjusted OR, stating the adjustments were for age, sex, risk tolerance and type of activity, but Table 2 does not even state that any statistical adjustment was done.

Even the comment "Initial information from the CHIRPP form revealed that 325 children used PE, but at the interview only 234 claimed to do so (55% used a helmet, 23% used knee pads, <1% used wrist guards and <1% used other PE)" is misleading. Only 188 of the 325 children recorded as PE-users by CHIRPP were interviewed (Table 1), so the 234 who claimed to do so (or possibly 235, as reported in the tables) include some classed by CHIRPP as non-users/unknown. Reporting this as a percent of the original 325 is nonsensical. As well as being a different sample, it incorrectly implies nil PE use by the 137 who were not interviewed. Presumably 76% (=53%*325/234) of the 235 PE-users in Table 1 wore helmets.

Another serious inconsistency is that, when discussing the OR for past injury in the same activity (0.06, CI 0.02-0.18) and for previous helmet use (0.06, CI 0.02-0.17) the text states: "in neither case did the odds ratio include the null". Either this statement is wrong or the CI are wrong, but it is impossible to tell which.

The difficulties interpreting Table 4 are exacerbated by the lack of information on whether the first column contains the number who were, or were not, taking chances. The presence of confounding is demonstrated by the fact that after adjusting for age, sex, risk tolerance and type of activity, all OR change direction. For example, the unadjusted OR for "more chances than usual" is 0.70, the adjusted OR is 1.45. More importantly, as noted above, even though the authors claim that Table 4 directly tests the risk homeostasis hypothesis, if most children injured wearing PE normally wear PE, no difference would be expected between PE-users and non-users irrespective of the amount of risk compensation.

Another difficulty is that Table 5 is described as examining the relationship between the severity of non-PE related injuries and PE-use. However, the table lists only 49 PE-users and 269 non-PE-users. This is completely different from the 235 PE-users and 159 non-users in the other tables. Until these inconsistencies are explained, no conclusions can be drawn from Table 5. Did only 49 PE-users have an injury to an unprotected body part? If so, the remainder presumably had injuries to *protected* body parts, suggesting that the PE was of limited efficacy. If true, this might be a more important conclusion that the inability of this study to demonstrate risk compensation.

Never turn a blind eye to contradiction

When a systematic review of what were considered the best quality observational studies (11 case-control studies, 16 prospective studies, 3 cross-sectional studies of hormone replacement therapy and heart disease) produced totally misleading results, epidemiologists reconsidered their methods and learned important lessons to guard against future failures. These were: 1) do not turn a blind eye to contradiction, 2) do not be seduced by mechanism, 3) suspend belief and 4) maintain skepticism.[6]

A notable contradiction in the study of Pless et al. is the significantly lower OR (crude: 0.43, adjusted 0.06), suggesting that PE-users were 17 times more likely to have suffered a past injury in the same activity. If 76% of the 235 PE-users in this study wore helmets, the over-representation of PE-users in injury data, suggests that helmet wearers are more likely to have accidents than non-wearers. This phenomenon deserves further study.

Table 1 summarizes other well-known examples of over-representation of helmet wearers in accident and injury statistics. Perhaps the best-known is for child cyclists in Seattle in 1987. Only 3.2%, of 4356 children observed cycling in Seattle wore helmets in that year, but when children whose families were members of a health co-operative were surveyed, 21.1% those who reported falling off their bikes wore helmets when they fell. Helmet wearers (HW) were also significantly over-represented, if not to the same extent, in children receiving emergency room treatment for non-head injuries (5.9% HW).

There are several possible reasons why helmet wearers might be over-represented in minor-injury data. Cyclists might choose to wear helmets when they think they might be injured, e.g. when first learning to cycle, or when enjoying the thrills of mountain biking (where PE use is often expected or mandated). Both activities (learning to cycle and mountain biking) carry a high risk of minor injury, but a much lower risk of encountering high-impact forces associated with bike/motor vehicle collisions that cause the majority of fatal or seriously debilitating head injuries to cyclists.[7]

Another interesting and related phenomenon is that some studies show helmet wearers have less severe non-head injuries, e.g. a study of injuries from bike/motor-vehicle collisions, reported that (even for patients without major head injuries), helmet wearers had a much lower mean injury severity score (3.6 vs 12.9, $P < 0.001$).[8] As well as being more likely to wear PE, cautious cyclists may be more likely to get minor injuries checked out at hospital. PE-users have higher income and socio-economic status,[9] so might be less concerned about the cost of medical treatment.

Thus decision to use PE is influenced by a person's tolerance and perception of risk, factors that also influence the choice of activity (e.g. cycling or football), and how that activity is conducted (e.g. cycling on major roads or quiet streets). The above examples indicate the difficulties researching risk-taking and PE use – do children choose to wear PE when they (or their parents) think they might be injured; would a child behave differently (e.g. ride a route with less traffic) if the helmet had been inadvertently left at home? The last scenario is an obvious example of risk compensation, which probably would not be detected in the responses to questions in the study of Pless et al. Similarly, if children who choose to wear PE tend to be more cautious, but the greater sense of security from PE leads to increased risk-taking, which ends up similar to non-PE-users, would this be detected by the questions in this study?

If case-control studies are to provide accurate results, researchers must maintain skepticism, suspend belief and consider all possible ways in which confounders, especially differences between self-selected groups (such as those who chose to wear PE vs those who did not) could affect the hypothesis being tested. Whenever possible, alternative approaches should also be considered to see if they might produce more reliable results.

Alternative approaches

For risk compensation, alternative approaches are possible. The study published in 1998 was unable to detect risk compensation from between-subject comparisons, but detected significant risk compensation using within-subject comparisons.[4] Risk compensation was also demonstrated from within-subject comparisons of drivers using anti-lock brakes,[10] children running an obstacle course with and without a helmet and wrist guards[1] and the amount of risk parents would allow their children to take.[11] It seems somewhat naïve to believe that children aged 8-18 would not understand they were allowed to take more risks when using PE and adopt similar behavior themselves.

As well as within-subject comparisons, another useful approach is to examine the effect of increased PE use on injury rates. A review in 1995 noted: *"The introduction of mandatory head and facial protection has been effective in virtually eliminating ocular, facial, and dental injuries in youth hockey, but it has also been problematically linked with an increase in catastrophic spinal injuries. Players adopt a false sense of security when donning the equipment, leading them to take excessive and unwarranted risks because of the protection they are supposedly afforded."*[12]

Increased "catastrophic spinal injuries" and "excessive and unwarranted risks" implies risk compensation.

The change in injury rates when laws are passed requiring people to use PE is another relevant indicator. Many road safety initiatives not subject to risk compensation, e.g. random breath testing, produce a clear response coinciding with the start of the initiative.[13] Mandating use of highly effective PE should also produce a significant response, gains from increased PE use outweighing the effects of risk compensation; but less effective PE could generate a false sense of security and increase injury rates. Table 2 summarizes data on cycling injury rates before and after helmet laws, suggesting that injury rates probably increased. Both risk compensation and reduced safety in numbers should be considered as possible explanations.

Seatbelt laws are perhaps the most controversial example. A review of the UK law, published in 2007 by the Royal Statistical Society Journal, Significance, noted that deaths from late-night crashes (10pm to 4am) fell by 23%, compared to a 3% decline (in line with the prevailing trend) for other times of day. The fall in late-night fatalities was attributed to an unprecedented number of breath tests, leading to a 31% increase in prosecutions for drink-driving and a dramatic reduction in and deaths of alcohol-affected drivers. Fatalities to drivers who had not been drinking were virtually unchanged. This suggests that benefits previously attributed to seatbelt laws were probably caused reduced drink-driving. Few people doubt that seatbelts reduce injuries when crashes occur, so is seems likely their protection was offset by risk compensation. Future laws mandating PE use should therefore be fully evaluated, and all effects considered, including risk compensation.

Conclusions

The review of highly-cited research (showing that 5 of the 6 case-control studies using self-selected groups were totally incorrect or had much lower benefits than claimed)[14] demonstrates that great care must be taken if studies of self-selected groups are to provide useful results. Perhaps the most important lesson is not to turn a blind eye to contradiction.

This study of Pless et al. is confusing and has many contradictions, e.g. the OR for a past injury in the same activity is reported as 0.06 (CI 0.02-0.17, Table 3, suggesting that PE-users were 17 times more likely to have suffered a previous injury in the same activity – a possible indicator of risk compensation), but described in the text as "not significantly different from the null". The difficulties of understanding the data are exacerbated by other inconsistencies, e.g. presenting OR for PE use in Table 2 and OR for non-use in Table 3, a lack of labels in Table 4, making it unclear whether e.g. the 33 non-PE-using children said they were going faster than usual (with 126 saying they were not) or vice versa. The confusion culminates in Table 5 which states that 269 non-PE-users had an injury to an unprotected body part, even though only 159 children actually wore PE.

However, perhaps the greatest contradiction of all is why the authors attempted to study risk compensation by between-subject comparisons of self-selected groups, given the published studies highlighting their unreliability, instead of the more accurate method of directly observing the behavior of children with and without PE, that successfully detected increases of 48-60% in risky behavior when children used PE.

References

1. Morrongiello BA, Walpole B, Lasenby J. Understanding children's injury-risk behavior: Wearing safety gear can lead to increased risk taking. *Accid Anal Prev* 2007;39(3):618-23.
2. Pless IB, Magdalinos H, Hagel B. Risk-Compensation Behavior in Children: Myth or Reality? *Arch Pediatr Adolesc Med* 2006;160(6):610-614.
3. Mok D, Gore G, Hagel B, Mok E, Magdalinos H, Pless B. Risk compensation in children’s activities: A pilot study. *Paediatrics and Child Health* 2004;9(5):327-330.
4. Streff FM, Geller ES. An experimental test of risk compensation: Between-subject versus within-subject analyses. *Accident Analysis & Prevention* 1988;20(4):277-287.
5. Ioannidis J. Contradicted and initially stronger effects in highly cited clinical research. *JAMA* 2005;294:218-228.
6. Petitti D. Commentary: hormone replacement therapy and coronary heart disease: four lessons. *Int J Epidemiol* 2004;33(3):461-3.
7. Kraus JF, Fife D, Conroy C. Incidence, severity, and outcomes of brain injuries involving bicycles. *Am J Public Health* 1987;77(1):76-8.
8. Spaite DW, Murphy M, Criss EA, Valenzuela TD, Meislin HW. A prospective analysis of injury severity among helmeted and non helmeted bicyclists involved in collisions with motor vehicles. *Journal of Trauma* 1991;31:1510-1516.
9. DiGuisseppi CG, Rivara FP, Koepsell TD. Bicycle helmet use by children. Evaluation of a community-wide helmet campaign. *JAMA* 1989;262:2256-61.
10. Grant BA, Smiley A. Driver response to antilock brakes: a demonstration of behavioural adaptation. Proceedings of the Canadian Multidisciplinary Road Safety Conference VIII; Saskatoon, Saskatchewan, June 14-16, p211-220; 1993.
11. Morrongiello BA, Major K. Influence of safety gear on parental perceptions of injury risk and tolerance for children's risk taking. *Inj Prev* 2002;8(1):27-31.
12. Murray TM, Livingston LA. Hockey Helmets, Face Masks, and Injurious Behavior. *Pediatrics* 1995;95(3):419-421.
13. Robinson DL. No clear evidence from countries that have enforced the wearing of helmets. *BMJ* 2006;332:722-725.

14. Ioannidis JPA. Contradicted and initially stronger effects in highly cited clinical research. *JAMA* 2005;294(2):218-228.

15. Thompson RS, Rivara FP, Thompson DC. A case-control study of the effectiveness of bicycle safety helmets. *N Engl J Med* 1989;320(21):1361-7.

16. Wasserman RC, Waller JA, Monty MJ, Emery AB, Robinson DR. Bicyclists, helmets and head injuries: a rider based study of helmet use and effectiveness. *AJPH* 1988;78:1220-1.

17. Finch C, Newstead S, Cameron M, Vulcan A. Head Injury Reductions in Victoria Two Years After Introduction of Mandatory Bicycle Helmet Use. Melbourne: Monash University Accident Research Centre - Report 51, 1993.

18. Dorsh MM, Woodward AJ, Somers RL. Do bicycle safety helmets reduce severity of head injury in real crashes? *Accid. Annal. Prev.* 1987;19:183-90.

19. Wasserman RC, Buccini RV. Helmet protection from head injuries among recreational bicyclists. *Amer. J. Sports. Med.* 1990;18:96-7.

20. Robinson DL. Head injuries and bicycle helmet laws. *Accid Anal Prevent* 1996;28:463-475.

21. Robinson DL. Safety in Numbers in Australia: more walkers and bicyclists, safer walking and bicycling. *Health Promotion Journal of Australia* 2005;16(1):47-51.

22. Curnow WJ. The Cochrane collaboration and bicycle helmets. *Acc Anal Prevent* 2005;37(3):569-73.

23. Adams J. Seat Belt Laws - Repeal them? *Significance* 2007;4(2):86-89.

24. Harvey A, Durbin J. The Effects of Seat Belt Legislation on British Road Casualties: A Case Study in Structural Time Series Modelling. *Journal of the Royal Statistical Society. Series A (General)* 1986;149(3):187-227.

Table 1. Evidence of greater helmet wearing rates in accident/injury victims than the general population

%HW of non-injured participants	%HW of accident or injury victims (P-value for difference[1])
3.2% - child cyclists < 15 years in Seattle, 1987 (sample size 4501)[9]	With head injury - 2.1%[15] (3/143;P = 0.45) With other injuries – 5.9%[15] (12/202; P = 0.35) Who reported falling off bikes 21.1%[15] (101/478; $P<10^{-66}$)
7.8% (40/516 cyclists in roadside interview) [16]	38% (8/21; P<0.000002) wearing a helmet when they hit their heads[16]
No data on %HW for SA but 3% of cyclist in Melbourne, Vic wore helmets in 1983[17].	Questionnaires sent to all cycling club members in South Australia (SA) c 1984. 62% of cyclists who reported hitting their heads in their most recent crash wore a helmet at the time.[18]
USA – population %HW unknown	60% HW (191 responses from readers of 4 US bicycle magazines to a request for information on cycling mishaps that involved hitting the head)[19]

[1] ?2 test for %HW of accident/injury victims different to %HW of non-injured participants.

Table 2. Evidence of increased (or no decrease in)injury rates, after laws mandating PE-use

Law mandating PE-use	Evidence of increased injury rates, or no decrease in injury rates
Bicycle helmet law (BHL) for children < 16 in NSW, Australia	a) Increased injury rate relative to the amount of cycling (head injuries from 388 to 488; non-head injuries from 926 to 1595);[20] b) 51% increase in deaths and serious injuries of cyclists aged 0-16 compared to child pedestrians, adjusting for the amount of cycling[20]
BHL for children < 15 in Victoria, Australia	Total injuries (relative to the amount of cycling) increased from 809 to 944, head injuries from 88 to 91[20]
BHL, Victoria	After adjusting for a 30% reduction in cycling, deaths and serious head injuries to cyclists from bike/motor vehicle crashes increased by 8% relative to pedestrians, other serious injuries increased by 9%[21]
BHL, all of Australia	After adjusting for a 30% reduction in cycling, fatal head injuries increased by 45% and all cycling fatalities by 29% relative to other road users[22]
UK seatbelt laws	No change in overall fatalities for crashes from 4 am to 10 pm (3% reduction, in line with the prevailing trend); 23% reduction for crashes from 4am to 10pm, possibly due to reduced drink-driving and not increased seatbelt use.[23] An earlier analyses ignoring time of day reported reductions of 18% and 25% in fatalities to drivers and front seat passengers, offset by increases of 27%, 13% and 8% in fatalities to rear seat passengers, cyclists and pedestrians respectively.[24] For numbers killed or seriously injured (KSI), the earlier analyses (ignoring time of day) found reductions for drivers and front seat passengers but no increases for other road users.[24] Under-reporting of serious injuries especially for cyclists and pedestrians increases the difficulty in interpreting KSI statistics.