The neglected epidemic: road traffic injuries in developing countries

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Road traffic injuries are a major cause of death and disability globally, with a disproportionate number occurring in developing countries.[1 2] Road traffic injuries are currently ranked ninth globally among the leading causes of disability adjusted life years lost, and the ranking is projected to rise to third by 2020.[1] In 1998, developing countries accounted for more than 85% of all deaths due to road traffic crashes globally and for 96% of all children killed.[2] Moreover, about 90% of the disability adjusted life years lost worldwide due to road traffic injuries occur in developing countries.[1] The problem is increasing at a fast rate in developing countries due to rapid motorisation and other factors (fig 1).[3] However, public policy responses to this epidemic have been muted at national and international levels. Policy makers need to recognise this growing problem as a public health crisis and design appropriate policy responses.

Summary points

Injury and deaths due to road traffic crashes are a major public health problem in developing countries

More than 85% of all deaths and 90% of disability adjusted life years lost from road traffic injuries occur in developing countries

Among children aged 0-4 and 5-14 years, the number of fatalities per 100 000 population in low income countries was about six times greater than in high income countries in 1998

The highest burden of injuries and fatalities is borne disproportionately by poor people in developing countries, as pedestrians, passengers of buses and minibuses, and cyclists

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Vulnerable population groups

Road traffic injuries in developing countries particularly affect the productive (working) age group (15-44 years) and children. (A developing country is defined as a country that has an annual per capita gross national product (GNP) less than US$9361 (£6456), based on 1998 figures from the World Bank.[4] Most low and middle income countries fall into this category.) Globally, in 1998, 51% of fatalities and 59% of disability adjusted life years lost due to road traffic injuries occurred in the productive age group.[2] Fatality rates among children are especially high in developing countries, as shown in fig 2. In 1998 the fatality rate for children aged 0-4 years was 29.5 per 100 000 population in South East Asia and low income countries of the western Pacific region, compared with 4.5 deaths per 100 000 population in high income countries. For older children, aged 5-14 years, the fatality rate was 28.1 per 100 000 population in Africa compared with 4.8 for North America, western Pacific countries, and high income countries in Europe.

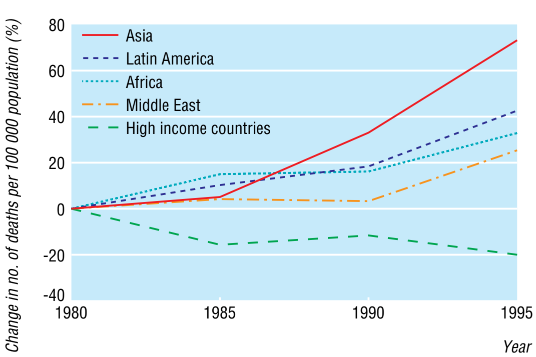

Fig 1 Trends in fatalities due to road traffic injuries for different regions of the world, 1980-95. Data from Transport Research Laboratory[3]

Road traffic injuries in developing countries mostly affect pedestrians, passengers, and cyclists—as opposed to drivers, in whom most of the deaths and disabilities in the developed world occur. In the United States, for example, more than 60% of road crash fatalities occur in drivers, whereas drivers make up less than 10% of the deaths due to road traffic injuries in the least motorised countries (shown by Kenya in fig 3). In developing countries, where most injuries occur in urban areas, pedestrians, passengers, and cyclists combined account for around 90% of deaths due to road traffic injuries.[5 6] Urban pedestrians account for 55-70% of deaths.[5 6]

The choice of mode of transport in developing countries is often influenced by socioeconomic factors, especially income.[5 7] In Kenya, for example, 27% of commuters who have no formal education were found

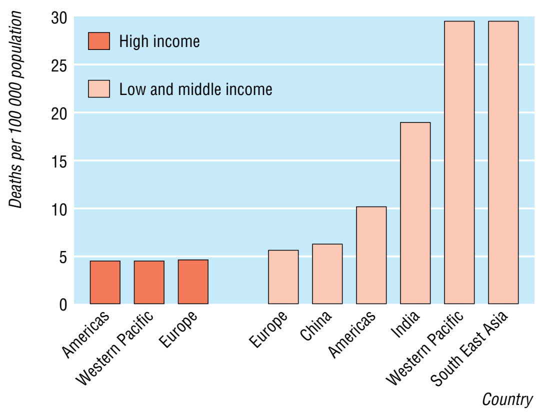

Fig 2 Fatality rates due to road traffic injuries in children aged 0-4 years. Data from World Health Organization[2]

to travel on foot, 55% usually used buses or minibuses, and 9% used private cars. By contrast, 81% of people with secondary level education or above usually travelled in private cars; 19% travelled by bus, and none walked. People with little formal education earn low incomes. For them, the affordable means of transport are walking, travelling by bus or truck, or cycling—all of which expose them to high risks for road traffic injuries.

People in developing countries are frequently aware of these risks. A regular commuter on the buses in Lagos, Nigeria—which are referred to locally as *danfos*, "flying coffins," or *molue*, "moving morgues"—said, "Many of us know most of the buses are death traps but since we can't afford the expensive taxi fares, we have no choice but to use the buses."[8]

Reasons for high burden in developing countries

Growth in motor vehicle numbers

The growth in numbers of motor vehicles is a major contributing factor in the rising toll of fatalities and injuries from road traffic crashes in poor countries. In India, the number of four-wheel motor vehicles increased by 23% to 4.5 million between 1990 and 1993, and by 2050 the number could rise to 267 million.[9] In Vietnam, deaths increased by 31%, injuries by 16%, and crashes by 12% between 2000 and 2001,[10] whereas the number of motor vehicles is estimated to have increased by 14%.[10] Motorcyclists were involved in 62% of the crashes.[10]

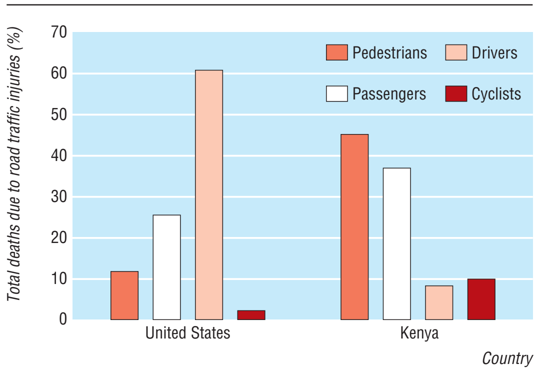

Fig 3 Deaths due to road traffic injuries by road user category in a developed and a developing country. US data for 1996[13]; Kenya data for 1996[5]

The trend of increasing numbers of injuries is likely to continue as the number of motor vehicles rises, especially in countries with low numbers at present.[11] People in developing countries, which comprise 84% of the global population, currently own around 40% of the world's motor vehicles.[12]

People killed or injured per crash

The higher number of people killed or injured per crash in countries with low income is a second reason for the high number of road traffic injuries in developing countries. Fig 4 shows the number of fatalities and injuries per 10 000 crashes for a developed country, the United States,[13] and two developing countries in Asia and Africa—Vietnam and Kenya. The number of people killed and the number of people injured per 10 000 crashes were higher for Vietnam and Kenya than for the United States. The high rates in Vietnam and Kenya (and elsewhere) are due to frequent crashes involving multi-passenger vehicles, including buses, trucks, and minibuses.[5 8]

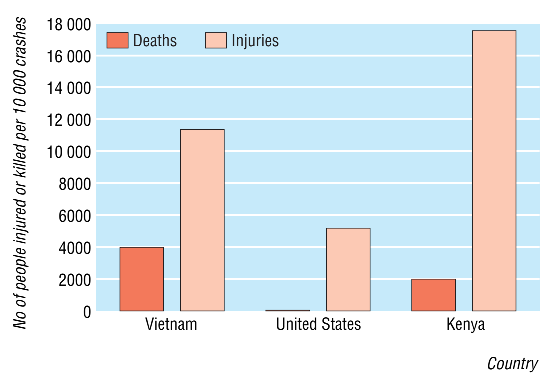

Fig 4 Injuries and deaths in a developed and developing countries. Vietnam data for 1998[10]; US data for 1996[13]; Kenya data for 1996[5]

Poor enforcement of traffic safety regulations

A third explanation for the high burden is poor enforcement of traffic safety regulations in low income countries due to inadequate resources, administrative problems, and corruption.[6] Corruption is a huge problem in some countries, often creating a circle of blame—the police blame drivers and the public, the public blames drivers and the police, and drivers blame the police.[6] Corruption also extends to vehicle and driver licensing agencies. An officer with the Lagos State Inspection Unit in Nigeria said, "You wonder how most of the buses secured road worthiness certificates in the first place. And when you ban the buses from the roads, they still find their way of returning to the roads."[8]

Inadequacy of public health infrastructure

A fourth explanation is the inadequacy of the public health infrastructure in providing treatment for traffic injuries. Only 40% of public, mission, and private hospitals in Kenya in 1999 were well prepared to treat trauma cases from traffic crashes, with 74% of the least prepared being public health facilities. All or most of the items needed for management of injuries—that is, oxygen, blood units, plaster of Paris, dressings, antiseptics, local and general anaesthetics, intravenous fluids, Boyle's anaesthetic machine, and blood pressure machine—were available at mission and private hospitals, whereas government health facilities rarely had these items in stock (VM Nantulya, F Muli-Musiime, T Omurwa, personal communications). The poor public health infrastructure means that patients often do not receive appropriate care promptly. This delay can compromise the patient's recovery, as there is a strong correlation between the time taken to receive appropriate treatment and the likelihood of adverse health outcomes and long term disability occurring.[14 15]

Poor access to health services

A fifth explanation is poor access to health services by vulnerable groups. In developing countries, pedestrians, cyclists, and passengers in minibuses and buses frequently belong to lower socioeconomic groups.[5 7] These groups cannot afford out-of-pocket payments for health care at the better equipped private health facilities. Moreover, with the introduction of user fees at public health facilities in many developing countries, these groups have lost the free health care that was previously available to them. For example, a study in Ghana showed that only 27% of people injured in road crashes used hospital services. Among patients with severe injuries, 60% of people injured in towns and cities, and 38% of people injured in the countryside received hospital care.[16] The most common reason cited for not seeking health care was lack of money.

Discussion

The injury profile for road traffic crashes in developing countries differs in important ways from the profile seen in developed countries, and it can provide guidance for making policies to improve prevention and control. Protection is needed for three main vulnerable groups—pedestrians, who in urban areas constitute up to 70% of the fatalities; passengers commuting on buses, trucks and minibuses, who constitute the next largest population group affected; and cyclists. Addressing the risks of these three groups will require multiple policy initiatives.[3]

To be effective, policies on traffic safety in developing countries must be based on local evidence and research, and designed for the particular social, political, and economic circumstances found in developing countries.[5] In particular, policies for developing countries need to protect poor people, who are predominantly affected by road traffic crashes owing to the mixture of vehicles and unprotected road users on the same roads, as well as other factors.[5 6 17] International efforts should be made to promote learning among developing countries about policies that can successfully reduce the injury burden from road traffic crashes in developing countries.

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An old friend returns in disguise

Since my graduation from medical school in 1968 I had been prescribing diphenhydramine (Benadryl) for night cramps with very satisfactory results; it had been the only drug mentioned for this condition in the *Harrison's Textbook of Medicine* of that period. When the drug was deleted from the local markets about 15 years ago and *Harrison's Textbook of Medicine* also stopped promoting it, I had to shift to an older medicine, quinine sulphate, although it was available in only a few pharmacies. Moreover, a few philosophising pharmacists would send patients back to remind me that the drug was for malaria.

Therefore, it was a pleasant surprise for me to learn that my old friend diphenhydramine had recently come back to the market, wearing a paracetamol mask under the name Panadol Night, even though muscle cramps are not mentioned as one of its indications.

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Reducing motor vehicle crash deaths and injuries in newly motorising countries

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Reducing motor vehicle crash deaths and injuries in newly motorising countries

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The United States was the first country to experience deaths and injuries from motor vehicle crashes in large numbers. As other countries motorised, they, too, experienced large numbers of crash deaths and injuries. Early efforts to address this problem were based largely on guesswork, with the principal focus on educating motorists. This simplistic and narrow approach continued for decades, even though the numbers of crash deaths and injuries continued to grow.[1]

In the 1960s a public health approach to the problem emerged in the United States and other motorised countries. Under this new approach, the available prevention options greatly expanded, and the effectiveness of countermeasures was scientifically evaluated before widespread adoption. This emphasis on evaluation was important because the earlier efforts continued for decades without evaluation, and when some of these programmes were eventually evaluated there was no evidence of effectiveness.[2]

Today motor vehicle crashes are causing substantial numbers of deaths in countries that are relatively new to motorisation.[3] A key question is how these countries can avoid the many unnecessary deaths and serious injuries that occurred in today's motorised countries during the decades that motor vehicle use was rapidly expanding, ineffective countermeasures were in place, and potentially effective countermeasures were being ignored.

Summary points

One consequence of the rapid growth in motor vehicle use in many countries is increasing numbers of crash deaths and injuries

To reduce this toll, countries need to adopt a broad array of research based measures

Despite being widely advocated as essential safety programmes, driver education or training programmes have not been found to reduce motor vehicle crashes

Almost all of the demonstrable gains produced by changing road user behaviour have resulted from properly enforced traffic safety laws

In many less motorised countries a disparate mix of road users share the roads, and so local measures will be required, such as setting safety standards for the front ends of motor vehicles to make them less hazardous for pedestrians and bicyclists

Methods and review of the evidence

This review reflects the authors' knowledge accumulated over more than 50 years combined in this subject. The published literature on highway safety is unfortunately fragmented and not always easy to retrieve. For example, many articles on various aspects of the crash performance of vehicles and human tolerance to injury are published in various specialist journals that focus on biomechanics and other aspects of vehicle crashworthiness. But the published research on behavioural issues tends to be widely scattered, with work relating to traffic laws and enforcement appearing in publications ranging from public health journals to criminology journals.

Medical journals have frequently rejected research on behavioural issues, especially those showing various road safety programmes to be ineffective. This publication bias is well known in scientific literature but is especially unfortunate in the case of safety programmes aimed at changing road user behaviour because the overwhelming bulk of programmes, when scientifically evaluated, have not been shown to be effective. This means that often only researchers who have been active in the subject for many years are aware of the many unpublished (or published in sources other than scientific journals) articles on ineffective programmes. Yet the field of road safety is constantly faced with enthusiastic newcomers who are convinced that their particular countermeasure will be effective. As a result, resources are continually squandered on ineffective programmes.

Research based countermeasures

The public health approach to road safety has resulted in a mix of countermeasures, and the choices among them are driven by research on their effectiveness. This mix includes measures aimed at improving vehicles, roads, and road user behaviour. A planning tool used to help identify the complete range of options is the Haddon matrix,[4] derived by first dividing the time sequence of a crash into three phases (precrash, crash, and postcrash) and then considering the human, vehicular, and environmental factors that can interact during each phase of a crash. The result is a nine cell matrix, each cell of which offers opportunities for intervention to reduce motor vehicle crash injuries (fig 1).

Under the old approach virtually all prevention efforts were focused in the precrash-human cell. Opportunities to prevent deaths and injuries by taking measures such as designing better vehicles or less hazardous roadsides or improving emergency medical systems were ignored.[5] The failure to identify a range of countermeasures meant, for example, that as late as the 1970s newly built highways in the United States had rigid signposts and other roadside hazards that guaranteed that the consequences of many crashes would be severe (fig 2).

Phases		Factors: Human	Factors: Vehicle and equipment	Factors: Environment
	Precrash			
	Crash			
	Postcrash			

Fig 1 The Haddon matrix, used to help identify possible countermeasures to road vehicle crashes

Changing road user behaviour

In motorised countries today most countermeasures have been shown by good research to be effective. Vestiges of the older unscientific approach still exist, however, and, for measures aimed at road user behaviour in particular, people continue to advocate programmes that have not been shown to be effective.[6]

Virtually all educational and training programmes aimed at adults that have been evaluated show no evidence of effectiveness. Driver education or training programmes have not been found to reduce motor vehicle crashes, but they still are widely advocated as essential safety programmes.[7–9] Research shows driver education programmes can increase knowledge, but this rarely results in appropriate behaviour change. Similarly, driver training programmes have not been shown to reduce crashes. They may be useful for teaching beginning drivers, and in some cases they may improve driving skills, but better skills do not automatically lead to fewer crashes.[10] Some advanced driver training programmes have even been shown to make things worse. For example, programmes that taught skid control, off-road recovery, and other emergency measures produced drivers with higher crash rates than drivers who did not take the course.[8] Comprehensive reviews of driver and motorcycle training programmes have found no studies showing any crash reductions due to the training.[11] Yet blind faith in the education and training of road users continues in many quarters.

The belief that increasing motorists' or other road users' knowledge or skills will produce fewer crashes reflects a naïve view of human behaviour. Most motorists and other road users acknowledge that serious risk taking and other behaviour problems are prevalent among drivers, but few people will admit that they may be part of this problem. Surveys of drivers' self ratings of their skills show that virtually no motorists believe their own skills are below average. So motorists agree that there are many "bad" drivers, but virtually all believe that the "bad" drivers are someone else. For example, drivers in motorised countries know that ignoring stop signs and running red lights are inappropriate behaviours, yet these obviously unsafe actions are common in the United States and are leading causes of crashes.[12] Similarly, all motorists know that driving after consuming alcohol increases the risk of crashing, but billions of trips are taken each year by alcohol impaired drivers worldwide.

Traffic laws and enforcement

Almost all of the demonstrable gains produced by changing road user behaviour in motorised countries have resulted from traffic safety laws.[13] However, laws by themselves often are not sufficient: the key factor in the effectiveness of a traffic law is motorists' perception that they run a high risk of being detected and punished for violating the law. The perception of likelihood of apprehension is a much stronger deterrent than the severity or the swiftness of the penalty.[14] Thus, laws requiring motorcyclists to wear helmets typically produce compliance close to 100% in the United States and much of Europe, largely because riders know they are easy to detect if they ride without a helmet. But in countries where motorcyclists' perceptions are that such laws are not likely to be enforced, levels of helmet use are much lower. Experience in less motorised countries like India confirms this behaviour.[15]

Laws work because it is possible to convince motorists that they face a considerable risk of punishment if they violate the laws. One reason education fails is that most motorists cannot be convinced that they are at risk of a crash, and many other road users (such as small children, teenagers, and people who are psychologically disturbed, under stress, under the influence of alcohol, or elderly) may not act according to their knowledge. Instead motorists believe it is other, "bad" drivers who are involved in crashes. For laws to work, however, effective enforcement and sanctions must be in place. This is not the case in many countries, so there is a strong tendency to revert to education as the preferred approach to changing road user behaviour.

International issues

In several countries that have been motorised for a long time, motor vehicle crash deaths are no longer increasing. This is largely because of the adoption of countermeasures shown to be effective. However, crash

Fig 2 As late as the 1970s newly built highways in the United States had rigid signposts and other roadside hazards, which meant that the consequences of a vehicle hitting one would be severe

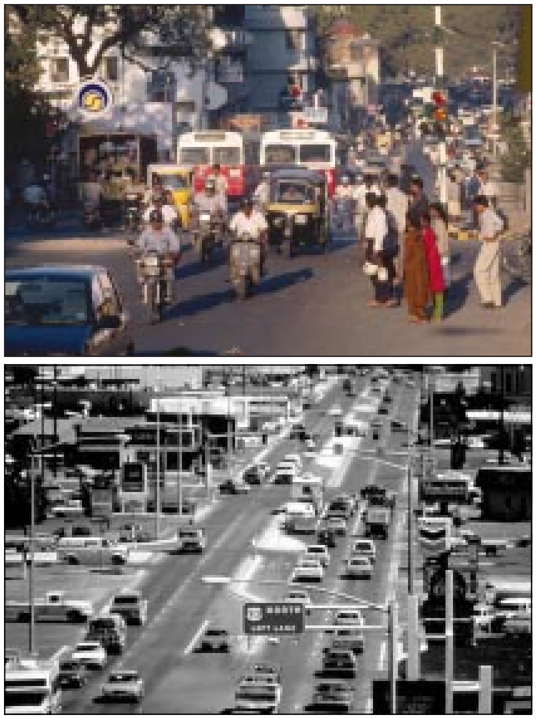

Fig 3 In many less motorised countries a disparate mix of road users share the same roads (top), so traffic engineering measures will be required that are different from those that have been successful in motorised countries, where traffic mixes are more homogeneous (bottom)

deaths and injuries are increasing dramatically in many countries where motor vehicle use on a large scale is relatively new.[3]

Some of the countermeasures that are effective in motorised countries will be applicable in others, but some will need to be adapted to local traffic conditions. For example, in many less motorised countries traffic use patterns, especially the disparate mix of road users sharing the same roads, will require traffic engineering measures that are different from those that have been successful in motorised countries, where traffic mixes are more homogeneous (fig 3).[5 6 11] Motorcycles dominate the roads in many less motorised countries, and they share the road with bicycles and other human powered vehicles, pedestrians carrying loads, and locally designed vehicles. Today's motorised countries did not experience these kinds of traffic mixes even when they were rapidly motorising, so traffic engineering solutions that work for their traffic are likely to have a much smaller effect on the roads of today's less motorised countries.

Most of the road crash deaths in less motorised countries occur among so called vulnerable road users—pedestrians, bicyclists, motorcyclists, etc. These categories of road users make up a much smaller proportion of crash deaths in motorised countries. This is not necessarily because of successful programmes aimed at such road users. For example, recent estimates from Britain suggest that the number of trips per person made on foot fell by 20% between 1985-6 and 1997-9.[16] Such trends suggest that reductions in pedestrian fatalities could be largely because of the reduced exposure of these road users rather than the road environment being "safer" for them. Similarly, pedestrian deaths have been declining for many years in the United States without major programmes to improve their safety. The most likely reason is a decline in walking. In less motorised countries many more pedestrians are killed by impacts with buses and trucks than in motorised countries, so a strong case can be made for pedestrian-friendly fronts for buses and trucks.[17] Yet such measures presently are not given any priority.

Motorised countries have safety standards for passenger vehicles, but vehicles exported to less motorised countries often do not meet these standards. It would make sense for all vehicles sold in less motorised countries to conform to some minimum international standards; there should not be a two tier system with safer vehicles sold in motorised countries and less safe ones sold elsewhere. Many less motorised countries manufacture vehicles locally (three wheeled scooter taxis, tuk-tuks, jeepneys, etc) that are not used in motorised countries. These vehicles generally are used as taxis but are not designed with any concern for crash performance. Since they are not used in motorised countries there is little pressure to improve their designs.

Future directions

The above discussion shows why the simple replication of motorised countries' policies in less motorised countries will not be sufficient to address their road safety problems. However, the body of research knowledge can be used as a foundation to develop effective programmes to reduce crash deaths and injuries in less motorised countries. This means using professionally trained practitioners to develop a broad array of appropriate measures. To accomplish this, countries in the process of motorising and experiencing substantial mortality and morbidity from motor vehicle crashes should:

- Establish national or regional road safety agencies which should be staffed with trained professionals and be responsible for accident data surveillance and analysis, funding of research activities, setting vehicle and road standards, and developing appropriate traffic engineering approaches[18]
- Promote effective speed control measures, including traffic calming suited for conditions specific to less motorised countries[19 20]
- Develop safety standards for the front ends of motor vehicles to make them less hazardous for pedestrians and bicyclists
- Promote safety measures likely to work in all locations—such as daytime running lights for motorcycles, more conspicuous bicycles and other small vehicles, compulsory helmet use for motorcyclists, effective enforcement of laws against alcohol impaired driving[21–25]
- Develop appropriate human resources. Fewer than a dozen road safety professionals presently work in less motorised countries. Training programmes should be institutionalised. This will happen only if road safety and transportation research departments are set up in selected universities and research institutions.

These recommendations reflect policies that have been shown to contribute to highway safety. Newly motorising countries cannot afford to ignore the evidence on what works, and equally important what doesn't work, to reduce deaths and injuries from motor vehicle crashes. In the United States and elsewhere, many lives were lost unnecessarily in crashes during the first 50 years of motorisation because highway safety advocates promoted ineffective measures while at the same time resisting approaches that later were shown to be effective. It will be a tragedy if the countries relatively new to motorisation repeat these mistakes.

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Designing road vehicles for pedestrian protection

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Collisions between pedestrians and road vehicles present a major challenge for public health, trauma medicine, and traffic safety professionals. More than a third of the 1.2 million people killed and the 10 million injured annually in road traffic crashes worldwide are pedestrians.[1] Compared with injured vehicle occupants, pedestrians sustain more multisystem injuries, with concomitantly higher injury severity scores and mortality.[2] Although a disproportionately large number of these crashes occur in developing and transitional countries, pedestrian casualties also represent a huge societal cost in industrialised nations. In Britain pedestrian injuries are more than twice as likely to be fatal as injuries to vehicle occupants[3] and result in an average cost to society of £57 400, nearly twice that of injuries to vehicle occupants.[4]

Despite the size of the pedestrian injury problem, research to reduce traffic related injuries has concentrated almost exclusively on increasing the survival rates for vehicle occupants. Most attempts made to reduce pedestrian injuries have focused solely on isolation techniques such as pedestrian bridges, public education, and traffic regulations and have not included changes to vehicle design. The lack of effort devoted to vehicle modifications for pedestrian safety

Summary points

Pedestrian-vehicle crashes are responsible for more than a third of all traffic related fatalities and injuries worldwide

Lower limb trauma is the commonest pedestrian injury, while head injury is responsible for most pedestrian fatalities

Standardised tests that simulate the most common pedestrian-vehicle crashes are being used to evaluate vehicle countermeasures to reduce pedestrian injury

Energy absorbing components such as compliant bumpers, dynamically raised bonnets, and windscreen airbags are being developed for improved pedestrian protection

has stemmed primarily from a societal view that the injury caused by a large, rigid vehicle hitting a small, fragile pedestrian cannot be significantly reduced by

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ORIGINAL PAPER

Speed control in developing countries: issues, challenges and opportunities in reducing road traffic injuries

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Abstract

Speed has been determined to be one of the most common contributing factors in vehicle crashes. This study explores vehicle speed as a factor in the causation of road traffic crashes, using the example of Ghana. It examines the effectiveness of various speed control measures, based on police-reported traffic crashes in Ghana and published works on speed control measures in both industrialized and developing countries. In Ghana, pedestrians were the main victims of road traffic injuries. The dominant driver error assigned by traffic police was loss of control, with the underlying factor being excessive vehicle speeds. The 'speed factor' alone accounted for more than 50% of all Ghanaian road traffic crashes between 1998 and 2000. While the enforcement of speed limits by traffic police may not be affordable for most developing countries, rumble strips and speed humps were found to be effective on Ghanaian roads. Rumble strips installed on the main Accra–Kumasi highway reduced crashes by about 35% and fatalities by about 55%. Reducing vehicle speeds may be one of the most effective interventions to stem traffic crashes in low-income countries. However, setting lower speed limits is not an effective intervention without the traffic law enforcement resources to ensure that limits are followed. Developing countries must also look to other speed reduction measures such as speed bumps and rumble strips, roads that segregate high- and low-speed users, and technological solutions such as speed governors, as well as greater public awareness of the problem.

Keywords: speed control, rumble strips, pedestrians, Ghana.

Introduction

Although the relationship between speed and road traffic crashes is a complex one, in general, the higher the speed of a vehicle, the higher the probability of becoming involved in a crash and the greater the likelihood of more severe injuries sustained. The energy dissipated during a collision of a vehicle is directly proportional to the vehicle's weight and to the square of its speed. Therefore, increased speed results in more energy dissipation, which translates into greater damage to the vehicle and more severe injuries to the occupants.

It has been established that if the mean speeds of vehicles can be reduced by 1 km/hr then, on average, injury and crashes will be reduced by about 3%[1,2]. More severe motor vehicle crash consequences (fatalities) will be reduced by a greater amount according to the "power laws" of Nilsson. Nilsson (1981) suggested that a change in mean speed is followed by a change in traffic crashes, injuries and fatalities according to the following formula:

> $(V_2/V_1)^2$ for crashes , $(V_2/V_1)^3$ for injuries, and $(V_2/V_1)^4$ for persons killed, where V_1 is the mean speed before the change and V_2 is the mean speed after the change.

It has also been estimated that the survival probability of a child pedestrian struck by a moving vehicle at 30 miles per hour (48 km/h) is approximately 50%, but at 40 miles per hour (64 km/h), it is decreased to approximately 10%. ('The slower speeds initiative' http//www.slower-speeds.org.uk).

This means that risks increase rapidly when speed is increased and lower rapidly when speed is reduced. This paper describes an exploration of the vehicle speed factor in

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the causation of road traffic crashes using the example of Ghana.

Materials and methods

The main sources of material for the report were derived from:

a) Published works on speed control measures undertaken in both the industrialized and developing countries, and
b) A recent study undertaken in Ghana to evaluate the effectiveness of a typical speed control measure using rumble strips to reduce the incidence of road traffic crashes and injuries at Suhum Junction, an accident blackspot location, on the main Accra–Kumasi highway in Ghana.

The study analyzed the police-reported crash and injury data categorized into a 'before' period from 1995 to 1999, and an 'after' period from 2000 to April 2001, using MAAP 5 accident analysis package, a microcomputer package developed by the Transport Research Laboratory (TRL), U.K. The average 'before' crash situation was then compared with the average 'after' installation situation in order to ascertain the level of effectiveness of the measure.

Results

Speed control

Speed control is one of the traffic regulatory measures that aims to ensure harmony in the interactions between vehicles and the road environment. Speed control in developed countries has been achieved through the combined application of such measures as the imposition of speed limits, police enforcement of traffic laws on speeding, and installation of speed-reducing measures like rumble strips, speed humps, raised areas, narrowings and staggerings, and speed control gadgets like speed governors in vehicles.

Speed limit imposition combined with traffic law enforcement is one of the best ways to make vehicles slow down. Studies in many countries have indicated that the introduction of speed limits often has only a short-term effect in reducing speeds unless police regularly enforce the limits. Posted speed limits alone will not guarantee compliance. It is only when backed up by strict police enforcement that speed limits reduce speed.

Most drivers in both the industrialized and developing countries, usually travel at speeds far above the speed limit. Excessive speeds are observed more often on roads with speed limits of 50 or 60 km/hr than on motorways with speed limits of 100 km/hr[3]. The speeds that most drivers think appropriate are more than 10% higher than the posted speed limits, as shown in Table 1. This means that most posted speed limits are perceived as unnatural, and most drivers will only reduce speed purely in response to perceived risk of accident or of being caught and punished[4,5].

The presence of police enforcement has also been shown to have an effect in decreasing speeds[6,7]. For many years, the police have played a prominent role in improving road safety. Active police enforcement has been found in many situations to reduce speed[7]. It is generally accepted that the use of speed enforcement, a speed-check zone, or parked patrol vehicles produce significant reductions in speeds in the vicinity of the enforcement unit. The success of police enforcement in changing human behavior depends on the ability to create a general and specific deterrence. General deterrence relies on the perception of the road user that traffic laws are enforced, and violators are prosecuted and punished. Specific deterrence concerns actual experiences of violators who are detected, prosecuted and punished.

Experiences in developing countries

The importance of vehicle speed as a factor in the high number of people killed or injured on roads in developing countries warrants a special focus on speed reduction as a priority strategy to control the rising fatalities associated with road transport. It appears, however, that the direct adoption of speed control measures proven effective in developed countries often will not produce the same safety improvement for developing countries. Several factors might account for the situation, including the failure of law enforcement agents to deter speed violators due to lack of resources for traffic police, bribery and corrupt practices, shortcomings of transport policies, weak political support for road traffic injury prevention and control, and low public awareness and participation in the adoption of speed control measures.

It has also been noted that passenger transport in most developing countries involves use of relatively old second-hand vehicles on inadequately maintained roads. Furthermore, the character of road traffic victims is different when

Table 1. Appropriate speeds (reported) for respective speed limits [3].

Speed limit	40 km/hr	50 km/hr	60 km/hr	80 km/hr	100 km/hr
Drivers' preference mean	47.8	57.5	70.3	96.5	109.1
SD	5.3	6.5	9.8	9.7	11.2

Table 2. Frequency of involvement of different vehicles in pedestrian crashes and fatalities (1998–2000) [8].

Vehicle type	% involvement in all crashes	% involvement in fatal crashes	Fatalities per 100 crashes involving the vehicle type
Cars/taxis	54.0	37.8	11.4
Bicycle	5.2	0.8	2.5
Motorcycle	2.8	2.1	12.1
Bus/minibus	23.4	31.8	22.2
Heavy goods vehicles	7.3	18.6	42.0
Pick-up trucks	6.4	7.6	19.5
Others	0.9	1.3	23.8

compared with that in the high-income countries; the most vulnerable road users in developing countries are pedestrians, cyclists and passengers in buses, minibuses and trucks. The health outcome for the pedestrian or cyclist who has been in a collision with a moving vehicle is bleak. Pedestrians are at greatest risk when mixed in traffic with high-speed motorized transport[8].

In Ghana, as shown in Table 2, fatal pedestrian crashes mostly involved pedestrians in collisions with cars or taxis (37.8%), buses and minibuses (31.8%), heavy goods vehicles (HGVs) (18.6%) and pick-up trucks (7.6%). Together they accounted for nearly 96% of the fatalities. Although buses and HGVs were involved in only 30% of all the pedestrian crashes, they gave rise to slightly over 50% of the fatal pedestrian crashes. Table 2 further shows that pedestrian crashes involving HGVs resulted in the proportionately highest number of deaths. The relative risk of death of a pedestrian when hit by a heavy goods vehicle is 3.7 times that by cars and taxis and 2 times that by buses and minibuses. The pedestrian crashes involving bicycles, however, resulted in the least number of deaths (i.e., 2.5 deaths in every 100 related crashes). In these accidents, the dominant driver error assigned by traffic police was loss of control (ran-off road), the underlying factor being excessive vehicle speeds. The ‘speed factor’ alone accounted for more than 50% of all the road traffic crashes.

As for passengers in speeding and overloaded buses and minibuses, crashes result in a low probability of survival. The buses, minibuses and trucks used for ferrying passengers have no safety belts. Moreover, the heavy metal works used to construct passenger seats for open trucks are a grave safety risk to passengers in a crash. The probability of sustaining severe injury or death in these vehicles in a crash is quite high. Unfortunately, however, there are often no transport policies to regulate the safety of passenger transport in developing countries.

In order to prevent traffic crashes and decrease their severity, effective speed controls must be enforced vigorously. The police and road administrators must check violation of maximum speed limits. Strong enforcement of speed limits is one effective way of reducing speed in built-up areas where pedestrians and cyclists are most at risk. Police presence at known accident black spots would help reduce speed at such locations. However, one main problem associated with traffic law enforcement has been with the unavailability of police on the roads to check high-speed infractions. The number of police dedicated to traffic law enforcement in Ghana, for example, is quite small, and they also lack resources to deal with the situation. The mere imposition of speed limits that are lower than the speeds that would be chosen by large number of drivers is unlikely to be effective without more intensive enforcement than is likely to be practicable in developing countries. Speed limits that are neither respected nor enforced may even erode respect for other traffic regulations that are beneficial to safety. Such speed limits must thus be reviewed.

It may be more appropriate for developing countries to utilize other, less costly tactics to reduce speeding such as physical speeding restraints. For example, rumble strips and speed humps have been found to be effective on Ghanaian roads[9]. Rumble strips installed on the main Accra–Kumasi highway at the Suhum Junction, an accident hot spot, have reduced crashes by about 35% and fatalities by about 55% over a short period of 16 months; between January 2000 and April 2001. The speed reducing measures succeeded in eliminating certain kinds of crashes as shown in Table 3.

The rumble strips were made of hot thermoplastic material. Each strip was 25 mm thick and 0.5 m wide laid across the entire carriageway at predetermined intervals to cover a distance of bout 800 m. The total cost of the rumble strips project was ¢136 million (US$20,900). In comparison, redesigning the junction area with raised central reservations and protected turning lanes would have amounted to about ¢680 million (US$104,610) while construction of a pedestrian bridge and guardrails to separate pedestrians from the motorized traffic would have been ¢1.2 billion (US$184,600). The rumble strips are simple and very cost-effective to install in developing countries. Limited funds from the central government and the general lack of public awareness of road safety measures have been major obsta-

Table 3. Crash characteristics for 'before' and 'after' traffic measures at Suhum Junction on Route N6, Ghana [9].

	No. of Crashes		Average crashes per year		
Crash type	Before 1995–1999	After Jan. 2000–Apr. 2001	Before 1995–1999	After January 2000–April 2001	% change in average no. of annual crashes
Head-on	6	0	1.2	0	−100.0
Rear-on	26	6	5.2	4.5	−13.5
Right angled	7	3	1.4	2.25	+60.7
Side swipe	9	0	1.8	0	−100.0
Over turn	4	1	0.8	0.75	−6.2
Hit object	2	0	0.4	0	−100.0
Parked vehicle	0	0	0	0	–
Hit pedestrian	23	3	4.6	2.25	−51.1
Other	21	4	4.2	3.0	−28.6
Total	**98**	**17**	**19.6**	**12.75**	**−34.9%**

cles in keeping with the pace of crashes in most developing countries. Political support for road traffic injury prevention and control is inadequate to reverse the present increasing trends in injuries and deaths from traffic crashes in developing countries. For example, road safety measures such as bike lanes and pedestrian bridges are lacking in most developing countries because the road authorities feel the inclusion of such measures will swell their budgets too much. Road network planning and development has instead favored integration of all transport modes to the disadvantage of the most vulnerable road users, pedestrians and cyclists. These vulnerable road users ought to be segregated from motorized transport in order to achieve the desired reduction in road traffic injuries and deaths[8,10,11].

One other major impediment preventing wider adoption of transport safety measures in developing countries remains lack of awareness of their importance on the part of the public and many policy-makers. Traffic education and publicity campaigns for safer speeds and information on the burden of road traffic injury are generally lacking in developing countries[11,12,13].

Discussion

There is evidence that speeds on roads in developing countries tend to be higher than is safe and that reducing these speeds would reduce the number and severity of crashes[1,3,14]. One major opportunity to control speed in developing countries is for vehicles to be fitted with devices that would warn drivers of their speed exceeding the limit applicable to the roads on which they are driving. It is possible to equip vehicles to prevent their being driven faster than is permitted by the highest speed limit on the roads of their country of registration (as is the case for government-owned buses in Ghana). It is believed that such physical limitations may offer a realistic prospect for achieving safer speeds on highways in developing countries.

Drivers choose speeds of individual vehicles according to their circumstances, and the prevailing road and traffic conditions in which they find themselves. These choices are self-motivated according to criteria which are person- and situation-dependent. One benefit from the use of currently available information technology would be the posting of speed limits that would vary according to weather, traffic conditions and time of day. This would ensure that posted speed limits were responsive to the road traffic demand and prevailing environment.

Most roads in developing countries have been built to allow different types of road users going at widely ranging speeds in the same space and at the same time. Such all-purpose roads tend to have high accident rates. Better road designs, which seek to segregate the slow-moving non-motorized transport (i.e., pedestrians and bicycles) from the fast-moving motorized transport, will go a long way to improve road safety in developing countries. Conscious road planning and design, along with safety auditing of roads, are key to road safety improvement efforts in developing countries. The application of such information technology would be most beneficial at critical locations like a school area where posted speed limits ought to vary in response to traffic conditions and time of day.

Physical speed restraint measures such as rumble strips and speed humps can be installed on roads to reduce speeds of vehicles. These have immense beneficial effects at locations with a high frequency of traffic crashes involving pedestrians[9,10].

Passenger safety can be enhanced by speed regulation through the use of speed governors and the installation and use of seat belts in buses and minibuses. Certainly, the problem of speeding is not a matter for the police alone. Involvement of civil society is essential in the creation of awareness on the importance of speed control in reducing road traffic crashes, injuries and fatalities in developing countries.

Conclusion

In conclusion, speed control offers one pragmatic way of stemming the road traffic injuries and deaths in developing countries. It must, however, be done in such a way that it becomes acceptable to most drivers through proper road design, appropriate speed limits and legal sanctions, as well as public education and information.

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Traffic calming for the prevention of road traffic injuries: systematic review and meta-analysis

F Bunn, T Collier, C Frost, K Ker, I Roberts and R Wentz

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SYSTEMATIC REVIEW

Traffic calming for the prevention of road traffic injuries: systematic review and meta-analysis

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Objective: To assess whether area-wide traffic calming schemes can reduce road crash related deaths and injuries.
Design: Systematic review and meta-analysis.
Data sources: Cochrane Injuries Group Specialised Register, Cochrane Central Register of Controlled Trials, Medline, EMBASE, Sociological Abstracts Science (and social science) citation index, National Technical Information service, Psychlit, Transport Research Information Service, International Road Research Documentation, and Transdoc, and web sites of road safety organisation were searched; experts were contacted, conference proceedings were handsearched, and relevant reference lists were checked.
Inclusion criteria: Randomised controlled trials, and controlled before/after studies of area-wide traffic calming schemes designed to discourage and slow down through traffic on residential roads.
Methods: Data were collected on road user deaths, injuries, and traffic crashes. For each study rate ratios were calculated, the ratio of event rates before and after intervention in the traffic calmed area divided by the corresponding ratio of event rates in the control area, which were pooled to give an overall estimate using a random effects model.
Findings: Sixteen controlled before/after studies met our inclusion criteria. Eight studies reported the number of road user deaths: pooled rate ratio 0.63 (95% confidence interval (CI) 0.14 to 2.59). Sixteen studies reported the number of injuries (fatal and non-fatal): pooled rate ratio 0.89 (95% CI 0.80 to 1.00). All studies were in high income countries.
Conclusion: Area-wide traffic calming in towns and cities has the potential to reduce road traffic injuries. However, further rigorous evaluations of this intervention are needed, especially in low and middle income countries.

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The worldwide epidemic of road traffic injuries is only just beginning. At present, over a million people die each year and some 10 million people sustain permanent disabilities in road traffic crashes. For people under 44 years, road traffic crashes are a leading cause of death and disablement, second only to HIV and AIDS.[1] Many developing countries are still at comparatively low levels of motorisation and the incidence of road traffic injuries in these countries is likely to increase. It is estimated that by 2020 road traffic crashes will have moved from ninth to third in the world disease burden ranking, as measured in disability adjusted life years.[2]

Most of the road deaths in developing countries involve vulnerable road users such as pedestrians and cyclists. In Ethiopia, pedestrian injuries account for 84% of all road traffic fatalities compared with 32% in Britain and 15% in the USA.[3] In the heavily motorised countries drivers and passengers account for the majority of road deaths but pedestrians account for a large proportion of road deaths involving children. The identification of effective strategies for the prevention of road traffic injuries is of global health importance.

In urban areas, road traffic crashes are scattered widely, and in such situations localised interventions for high risk sites are not appropriate. In high income countries area-wide traffic calming schemes, including the treatment of both main roads and residential roads, have been proposed as a strategy for reducing such scattered crashes. Traffic calming has been defined as the combination of mainly physical measures that reduce the negative effects of motor vehicle use, alter driver behaviour, and improve conditions for non-motorised street users.[4] It has been estimated that area-wide traffic calming schemes can reduce the number of road traffic injuries by about 15%.[5] However, this estimate was based on a review that included uncontrolled before/after studies in which the effect of traffic calming could be confounded by other factors that influence road traffic injury rates. In particular, in high income countries there is evidence that pedestrian injury rates have fallen because of a reduction in walking.[6] In this case, the inclusion of uncontrolled studies could exaggerate the apparent effect of traffic calming. We conducted a systematic review of controlled studies to assess the effect of area-wide traffic calming on road user deaths, injuries (fatal and non-fatal), and numbers of road traffic crashes.

METHODS

Inclusion criteria

We included randomised controlled trials and controlled before/after studies of area-wide traffic calming schemes. Eligible schemes included those that involved a number of specific changes to the road layout, road hierarchy or road environment, for example road narrowing, road closures, creation of one way streets, changes at junctions, mini-roundabouts, road surface treatment, or speed humps. We excluded studies describing the enforcement of legal interventions, financial incentives or disincentives, and interventions investigating alteration to road signage or traffic lights alone, or studies solely describing interventions to separate different road users (cycle lanes, bus lanes, pedestrian walkways). The outcomes of interest were all road user deaths, all road user injuries (fatal and non-fatal), and the number of traffic crashes.

Box 1: Strategy for identification of studies

Search strategy for electronic databases; searches run in 2000

- Terms describing the intervention, outcomes, and study methodology were combined.
- A: the intervention—area traffic control* or TRAFFIC RESTRAINT* or traffic calming or traffic engineering or road design or road layout or roundabout* or humps or bumps or traffic distribution or traffic redistribution or traffic flow or crosswalk* or speed cushion* or chicane* or road narrowing or refuges or road hierarchy or traffic hierarchy or four way* stop* or access only or sheltered parking or left turn lane* or wooner* or junction layout or road layout or lateral clearance.
- B: the outcome—accident* or injur* or fatalit* or death or safety.
- C: the study methodology—evaluation or assess* or stud* or evaluation or assess* or (controlled near2 stud*) or comparison or comparative or intervention near2 stud* or controls.

Web sites searched; searches conducted in 2001

- AAA Foundation for Traffic Safety (USA): www.aaafoundation.org
- ARRB, Australian Road Research Board: www.arrb.org.au
- Australian Transport Safety Bureau: www.atsb.gov.au
- CROW, Information and Technology Centres for Transport and Infrastructure (Netherlands): www.crow.nl
- Danish Council for Road Safety Research: www.trm.dk/eng/veje/rft
- Danish Transport Research Institute: www.dtf.dk
- DVR, Deutscher Verkenrssichereitsrat Road Safety Institute (Germany): www.dvr.de/
- FINNRA, Finnish National Road administration: www.tieh.fi
- INRETS, Institut National de Recherche sur les Transports et leur Sécurité (France): www.inrets.fr
- ITE, Institute of Transportation Engineers (USA): www.ite.org
- LET, Laboratoire d'economie des transports (France): www.lsh-lyon.cnrs.fr
- NHTSA, National Highway Traffic Safety Administration (USA): www.nhtsa.dot.gov
- Swedish National Roads Administration: www.vv.se/for_lang/english/
- SWOV, Institute for Road Safety Research (Netherlands): www.swov.nl
- TOI, Institute of Transport Economics (Norway): www.toi.no
- TC, Transport Canada: www.tc.gov
- TRB, Transportation Research Board: www.nas.edu/trb/
- TRL, Transport Research Laboratory (UK): www.trl.co.uk
- US Department of Transport, Federal Highway Administration (USA): www.fhwa.dot.gov
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Identification of studies

We searched the following electronic databases; Cochrane Injuries Group Specialized Register, Cochrane Central Register of Controlled Trials, Medline, EMBASE, Sociological Abstracts Science (and Social Science) Citation Index, National Technical Information Service, Psychlit, Transport Research Information Service, International Road Research Documentation, and TRANSDOC (the last three combined in the TRANSPORT database). One reviewer examined titles, abstracts, and keywords of citations, as given on electronic databases, for eligibility. Where possible the full text of all of potentially relevant citations was obtained. We also searched the web sites of road safety organisations, contacted experts, hand searched conference proceedings, and checked reference lists of relevant papers. There were no language restrictions. Further details of the search strategy can be seen in box 1.

Data extraction and analysis

One reviewer decided whether studies met the inclusion criteria, and this was checked by a second reviewer. Using a data collection form two reviewers independently extracted data on road user deaths, injuries (fatal and non-fatal), traffic crashes, characteristics of the intervention and control area, and types of measures implemented. To assess study quality we collected information on how the intervention and control areas were matched, duration of the before and after periods,

Table 1 Table of included studies

Study ID	Methods	Participating areas	Interventions
Charlottenburg[16] (Germany 1977–84)	CBA 2 years before data 2 years after data	(I) Residential area with small businesses. Area of about 60 hectares with 15000 inhabitants (C) Similar area in the same city	Different levels of road surface, road narrowing, chicanes, staggered lanes, speed restrictions
GST Borgentreich[10–12] (Germany 1983–90)	CBA 3 years before data 3 years after data	(I) Whole town centre: mixture of residential, commercial, and farm properties (C) Similar area in different town	Road narrowing, redesigning major roads, traffic free zones, speed restrictions
GST Buxtehude[10–12] (Germany 1981–87)	CBA 2 years before data 2 years after data	(I) Mixture of shopping and residential areas. Area of about 268 hectares population of about 11000 (C) Similar area in the same city	Road narrowing, speed restrictions, and a wide range of traffic restraint measures
GST Esslingen[10–12] (Germany 1983–90)	CBA 2 years before data 2 years after data	(I) Mixture of residential, industrial, and commercial properties (C) Similar area in another town	Reconstruction of major roads, speed restrictions, and renewal of residential roads
GST Ingolstadt[10–12] (Germany 1982–90)	CBA 2 years before data 2 years after data	(I) Most of the old part of the town, 5500 inhabitants (C) Similar area in another town	A wide range of traffic restraint measures
GST Mainz[10–12] (Germany 1983–90)	CBA 2 years before data 2 years after data	(I) Rural suburb of 200 hectares with 11000 inhabitants (C) Similar area in the same city	Reconstruction of public spaces including road narrowing and narrowing of road entrances
GST Moabit[10–12] (Germany 1982–88)	CBA 2 years before data 2 years after data	(I) Residential area of about 120 hectares near the city centre (C) Similar area in the same city	Rebuilding of major traffic roads, increasing level of vegetation in streets
Rijswijk/Eindhoven[15] (Netherlands 1972–86)	CBA 6 years before data 5 years after data	(I) Road districts in Rijswijk and Eindhoven (C) Residential zones bordering on main traffic arteries within the boundaries of Rijswijk and Eindhoven	Road humps, road closures and narrowing, raised cross roads. Public spaces reclassified
Swindon[13] (UK 1975–81)	CBA 2 years before data 3 years after data	(I) 2.8 km section of an all purpose road in Swindon (C) 3 routes of similar layout and function	Roundabouts, pedestrian crossings, changes to intersections
Sydney-Canterbury[14] (Australia 1981–87)	CBA 3 years before data 2.5 years after data	(I) Predominantly residential area in city (C) Similar area in the same city	Speed humps, roundabouts, slow points, speed limits
Sydney-Willoughby[14] (Australia 1980–87)	CBA 2 years after data 2 years before data	(I) Predominantly residential area in city (C) Similar area in the same city	Entry thresholds, slow points, speed humps, T-intersection treatments, roundabouts, and road closures
USP Bradford[17] (UK 1981–88)	CBA 5 years before data 2 years after data	(I) Mainly residential area, population approximately 33000 (C) Similar area in same city	Junction redesign, closure of through roads, and installation of central refuges
USP Bristol[18] (UK 1981–88)	CBA 5 years before data 2 years after data	(I) Mainly residential area of approximately 10 square km, population was approximately 32000 in about 12000 households (C) Similar area in same city	Junction redesign, mini-roundabouts, right turn bans, improvement of pedestrian crossings, improved road signs and markings, road closures
USP Nelson[19] (UK 1980–87)	CBA 5 years before data 2 years after	(I) An area of 7 square km, population of approximately 30000 people (C) Similar area in same city	Junction redesign, road closures, and mini-roundabouts
USP Reading[20] (UK 1979–86)	CBA 5 years before data 2 years after data	(I) Approximately 8 square km, with a population of about 36000 people (C) Similar area in same city	Road closures, right turn bans, mini-roundabouts
USP Sheffield[21] (UK 1979–87)	CBA 5 years before data 5 years after data	(I) Mostly residential area covering approximately 9 square km, population approximately 50000 (C) Similar area in same city	Road closures, traffic islands, central refuges, turning restrictions

CBA, controlled before after study; I, intervention area; C, control area; GST, German six towns project; USP, UK Urban Safety Project.

and, because of the potential for contamination, we also noted the proximity of the intervention and control areas.

For each study we calculated a rate ratio: the ratio of event rates before and after intervention in the traffic calmed area divided by the corresponding ratio of event rates in the control area. This gives the reduction in the incident rate in the intervention area compared to that in the control area. For example, a rate ratio of 0.8 corresponds to a 20% reduction in events compared with that predicted from the rates in the control area. For the calculation of 95% confidence intervals, standard errors of the logarithms of the rate ratios were constructed assuming that the number of events in each area in each period followed a Poisson distribution,[7] provided there was at least one event in each period. For studies with no events in one or more periods exact confidence intervals were calculated where the rate ratio was defined. Rate ratios were combined on a logarithmic scale using a random effects meta-analysis model. The assumption of random effects means that the effect estimates and confidence intervals allow for variation in study specific rate ratios over and above that due to variability within studies.[8] In this meta-analysis such additional variability reflects both underlying heterogeneity in rate ratios across studies and any variability arising through overdispersion[9] if the assumption that events follow Poisson distributions is violated.

For studies with no events in one or more periods 1/2 was added to all counts in the pooled analysis. In the analysis of road user deaths, where the majority of studies had no events in at least one period, no test of heterogeneity was carried out, and a pooled estimate of the rate ratio was obtained from the column totals. Analyses were carried out in Stata version 7.0 (Stata corporation, College Station, Texas 77845, USA).

RESULTS

The searches identified 12 986 published and unpublished reports which were screened for eligibility. We obtained the full text of 586 reports and of these 12 reports, describing 16 controlled before/after studies, met our inclusion criteria (see table 1).[10–21] We found no randomised controlled trials. Seven studies were done in Germany,[10–12 16] six in the UK,[13 17–21] two in Australia,[14] and one in the Netherlands[15]; all were done in the 1970s and 1980s. In most studies attempts had been made to match the intervention and control sites. However, in three

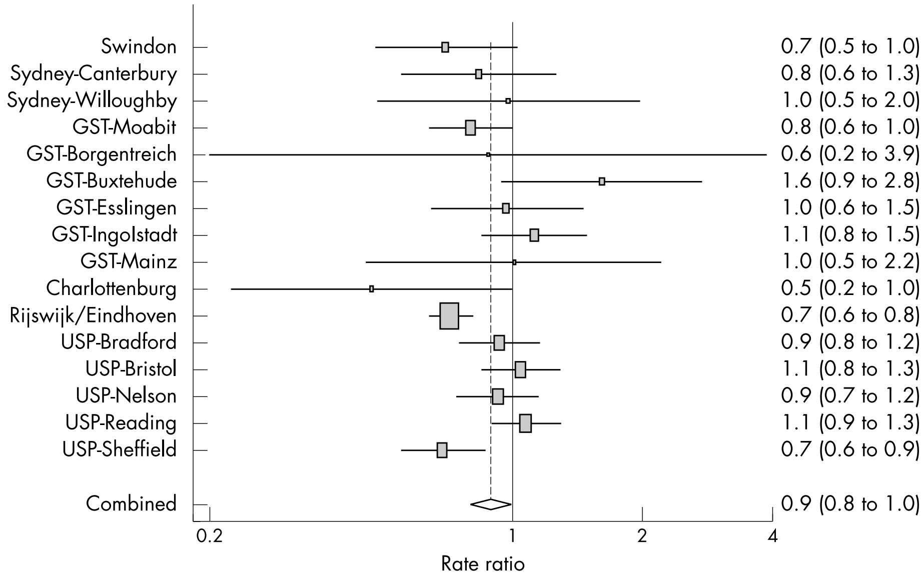

Figure 1 Number of road traffic injuries (fatal and non-fatal).

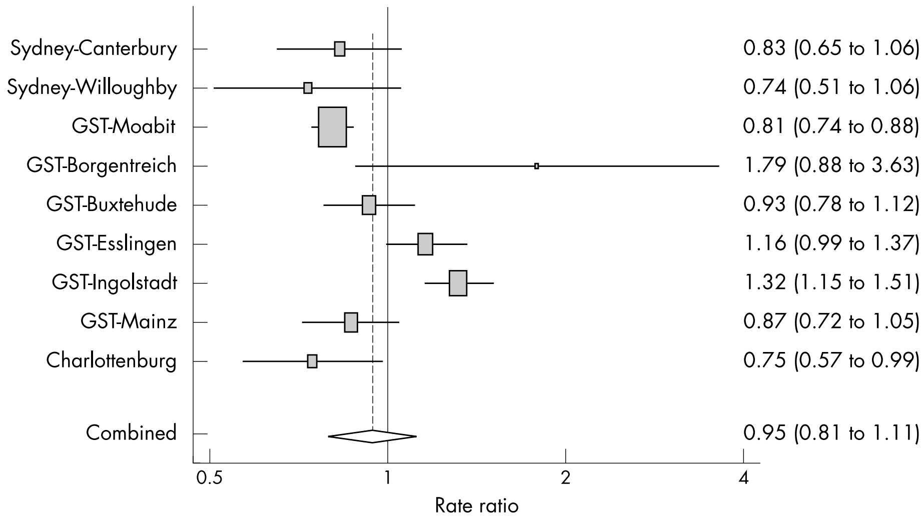

Figure 2 Number of road traffic crashes.

differences in the land use characteristics or type of district are reported,[14 15] and in one the control area was much larger than the intervention area.[15] Outcome data was collected from police or local authority records in all studies.

Eight studies reported the number of road user deaths.[10 14] The pooled rate ratio was 0.63 (95% confidence interval (CI) 0.14 to 2.59). This result should be interpreted with caution since many of the studies include at least one period in which no road user deaths were observed. Sixteen studies reported the number of road traffic injuries (fatal and non-fatal).[10–21] The pooled rate ratio was 0.89 (95% CI 0.80 to 1.00) (fig 1), with statistically significant heterogeneity between the studies ($p = 0.05$). Nine studies reported the total number of road traffic crashes.[10 19 20] The pooled rate ratio was 0.95 (95% CI 0.81 to 1.11) (fig 2), again with statistically significant heterogeneity between the studies ($p = 0.001$). Thirteen trials reported the number of pedestrian crashes.[10 14 17–21] The pooled rate ratio was 1.00 (95% CI 0.84 to 1.18) There was no significant heterogeneity ($p = 0.21$).

DISCUSSION

This systematic review of controlled before/after studies shows that area-wide traffic calming has the potential to prevent road traffic injuries. Although the effect of traffic calming on road user deaths is in the same direction as for injuries (fatal and non-fatal), because the number of road user deaths in the included studies is low the estimated rate ratio is imprecise. Indeed, the imprecision in the rate ratio may be understated by the confidence interval because the way that the confidence interval was calculated ignores the likely heterogeneity between studies. Although we found no reliable evidence that traffic calming reduces the number of road traffic crashes, because traffic calming may reduce vehicle speeds,[22] this is not inconsistent with a reduction in the occurrence of injury. Our estimates of the effectiveness of traffic calming provide a basis for future cost effectiveness analyses that would be important in informing decisions about resource allocation.

Several methodological issues may have a bearing on the validity of these results. Publication and other selection biases are a potential threat to validity in all systematic reviews, but this is a particular problem in road safety where a large proportion of the available research is published in the grey literature. In this review only two of the included studies were published in journals. There are also problems identifying published controlled studies in the road safety databases.[23] Search strategies for identifying controlled studies in medical databases can achieve high sensitivity because terms describing the study methodology are included among the indexing (descriptor) terms. Road safety databases, however, have a very limited range of indexing terms describing the study methodology. Despite our considerable efforts to identity all eligible studies, published and unpublished, irrespective of

Key points

- Injuries as a result of road traffic crashes are a global problem and are likely to increase.
- A previous meta-analysis, including uncontrolled before after studies, found area-wide traffic calming can reduce road traffic injuries by about 15%.
- This systematic review, of 16 controlled before after studies, found an 11% reduction in road traffic injuries (fatal and non-fatal).
- Traffic calming has the potential to prevent road traffic injuries but further rigorous evaluations, particularly in low and middle income countries, is required.

language of publication, we cannot exclude the possibility that some studies were missed resulting in reduced precision and the potential for bias.

Although we found no randomised controlled trials of traffic calming schemes, the inclusion of studies with well matched intervention and control areas, with adequate before and after periods, may avoid the problem of confounding by changes in the background rate of injury. All but one[15] of the included studies had attempted to match the intervention and control areas and all had collected at least two years before and two years after data, with a number collecting up to five years before or after data.

Because there was significant heterogeneity between the studies reporting the number of road traffic injuries and crashes, these results should be interpreted with caution. The observed heterogeneity may be due to differences in study design, in the types of traffic calming schemes involved, or in the way outcomes were defined and data collected.

The included studies were all conducted in the 1970s and 1980s, and, apart from two Australian studies, were all done in Europe. As a result it may make it more difficult to generalise from this systematic review and make inferences about the effectiveness of present day area-wide traffic calming schemes. In addition road traffic crashes are a major cause of death and injury in low and middle income countries where most of the casualties are pedestrians, cyclists, and riders of motorised two wheelers. Although traffic calming appears to be a promising intervention for preventing road traffic injuries because none of the included studies were conducted in low and middle income countries further rigorous evaluation is required in these settings.

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ORIGINAL PAPER

Traffic-related injury prevention interventions for low-income countries

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Abstract

Traffic-related injuries have become a major public health concern worldwide. However, unlike developed or high-income countries (HICs), many developing or low-income countries (LICs) have made very little progress towards addressing this problem. Lack of the progress in LICs is attributable, in part, to their economic situation in terms of their governments' lack of resources to invest in traffic safety, cultural beliefs regarding the fatalism of injuries, competing health problems particularly with the emergence of HIV/AIDS, distinctive traffic mixes comprising a substantial number of vulnerable road users for whom less research has been done, low literacy rates precluding motorists to read and understand road signs, and peculiar political situations occasionally predominated by dictatorship and non-democratic governments. How then can LICs tackle the challenge of traffic safety from the experiences of HICs without reinventing the wheel? This paper reviews selected interventions and strategies that have been developed to counter traffic-related injuries in HICs in terms of their effectiveness and their applicability to LICs. Proven and promising interventions or strategies such as seat belt and helmet use, legislation and enforcement of seat belt use, sidewalks, roadway barriers, selected traffic-calming designs (e.g., speed ramps/bumps), pedestrian crossing signs combined with clearly marked crosswalks, and public education and behavior modification targeted at motorists are all feasible and useable in LICs as evidenced by data from many LICs. While numerous traffic-related injury policy interventions and strategies developed largely in HICs are potentially transferable to LICs, it is important to consider country-specific factors such as costs, feasibility, sustainability, and barriers, all of which must be factored into the assessment of effectiveness in specific LIC settings. Almost all interventions and strategies that have been proven effective in HICs will need to be evaluated in LICs and particular attention paid to the effectiveness of enforcement measures. It behooves LIC governments, however, to ensure that only standard, approved safety devices like helmets are imported into their countries. Additionally, LICs may need to improvise and innovate in the traffic safety technology transfer.

Keywords: Traffic, injuries, low-income countries, injury intervention, road safety.

Introduction

While traffic-related injuries take a very large toll in almost every country around the world, particularly in low-income and/or less industrialized countries (LICs)[1,2], significant progress toward prevention and control has been limited to high-income and/or highly industrialized countries (HICs). Much of the progress in HICs is attributable to the combination of interventions, strategies, and policies that have been developed mainly in these high-income settings over the past few decades. Such factors as high health budgets, adequate numbers of researchers, high levels of health and safety awareness, and near universal literacy, have also catalyzed this progress. In many LICs, the burden of traffic-related injuries is such that as many as 40% of the adult surgical beds in some urban teaching hospitals are devoted to the treatment of motor vehicle crash victims[3]. How then can LICs benefit

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from the experiences of HICs so that they need not reinvent the wheel?

The injury control lessons to be learned from HICs were debated in the opinion-dissent section of an issue of *Injury Prevention*[4,5]. While it was argued that important lessons could be drawn by LICs from the successes of injury intervention in HICs[4], it was also acknowledged that some caution should be exercised in assuming the applicability and relevance of injury control policies given the vastly different contexts[5]. Nonetheless, the similarity of the energy exchanges causing injury in the two settings (e.g., the same *kinetic energy* causing injuries due to a vehicular crash in a LIC or in a HIC) and the fact that LICs are constrained by poor resources, lack of personnel, and low literacy rates dictates that LICs learn what they can about injury control from HICs. Despite these differences in context, there are many reasons why LICs should take advantage of relevant lessons, although it must be pointed out that most traffic-related research in HICs has focused on protecting the auto occupant and not the vulnerable road user.

In this paper, interventions and strategies that have been developed to counter traffic-related injuries in HICs are reviewed in terms of (1) their proven effectiveness and (2) their applicability to LICs. Potentially transferable interventions and strategies are assessed with respect to how well they might work in LICs. Barriers to application, the need for local evidence, and the potential for improvisation or innovation, along with other related issues impacting effective transfer of interventions or strategies from HICs to LICs are highlighted. Only a few, representing a balanced mix of active and passive interventions, are discussed in greater detail.

Materials and methods

Traffic injury definitions and strategies

The classification model adopted by the U.S. National Committee on Injury Prevention and Control in 1989 may still be applicable to the myriad of interventions and strategies developed for traffic safety and injury control[6]. This model classifies interventions and strategies regarding their effectiveness as proven, promising, unknown or ineffective.

An *intervention or strategy with proven effectiveness* is one whose application has led to obvious injury reduction or some other major discernible positive effect. Such interventions or strategies (e.g., seat belt use) need to be used and monitored routinely. An *intervention or strategy with promising effectiveness* is one that has effected some injury reduction or some other mild discernible positive effect. Promising interventions and strategies (e.g., wrong-way signs) should also be used, with their effects monitored closely and their outcomes evaluated rigorously. An *intervention or strategy with unknown effectiveness* is one that intuitively appears to have some discernible positive effect but has not been studied sufficiently. These interventions or strategies (e.g., the designated driver concept or safe ride program) should obviously be the subject of further research. An *ineffective intervention or strategy* (e.g., painted crosswalks alone) is one that has little impact or that just does not work, and may even be counterproductive and should not be used under any circumstance[6].

Table 1 groups selected potentially transferable interventions by the ‘prevention target’ to be protected and their level of effectiveness. Using the prevention target – which may be an occupant of a motor vehicle, a motorcyclist, a bicyclist or a pedestrian – provides some organization and a convenient way to assess the interventions. The definitions of these prevention target categories are derived according to the *International Classification of Diseases, Ninth Revision, Clinical Modification*[7].

An *occupant* may be the driver operating or the passenger being carried in any mechanically or electrically powered vehicle. A *motorcyclist* is any person operating or being carried on a two-wheeled mechanically or electrically powered vehicle. A *bicyclist* is a person riding or being carried on any vehicle operated solely by pedals. A *pedestrian* is a person traveling on foot. A fifth category, referred to as *cross-cutting*, is included to encompass all other interventions that cut across these four prevention target categories.

It is important to differentiate between actual *interventions* designed to ameliorate traffic-related injury (e.g., seat belts) and the *strategies* developed to promote the use of such interventions (e.g., mandatory seat belt legislation and enforcement). While the interventions have been the subject of much prior research, the strategies are still receiving increasing attention from researchers and policymakers.

In a 1999 issue of the *American Journal of Preventive Medicine*, the effectiveness of several strategies developed to promote traffic safety, along with some interventions, were evaluated systematically[8–12]. The strategies evaluated included promotion of rear seat use by children riding in motor vehicles[8], primary and secondary enforcement of motor vehicle occupant restraining laws[9] and legal statutes in reducing drunk driving recidivism[10]. In a more recent 2001 issue of the same journal, more strategies and interventions to reduce injuries to motor vehicle occupants were systematically reviewed using the more comprehensive ‘Guide to Community Preventive Services’ methods of the U.S. Department of Health and Human Services[13–15].

Results

Specific proven and promising interventions

The following is a review of selected interventions and strategies. These are classified as having proven or promising effectiveness, though evaluation is ongoing in HICs and in some LICs. It is important to realize that the full benefit of

Table 1. A matrix of traffic injury interventions using prevention target characteristics and level of effectiveness.

Prevention target	Proven	Promising	Unknown	Ineffective
Occupant	• Seat belts* • Air bags • Child safety seats • Vehicular design (e.g., integral head-rests) • Seat belt use laws • Child seat use laws	• Lowering of bumper heights • Graduated driver licensure • Licensure suspension laws	• Increasing age of driver licensure	
Motorcyclist	• Helmets*	• Motorcycle rider education • Running light conspicuity measures		
Bicyclist	• Helmets*	• Nighttime conspicuity-enhancement measures • Bike paths and lanes	• Bike safety programs	
Pedestrian	• Sidewalks • Roadway barriers* • Pedestrian crossing signs • Education on conspicuity-enhancement measures • Roadway lighting	• One-way streets • Traffic signals and pedestrian indicator lights • School-zone measures • Nighttime conspicuity-enhancement measures	• Curb parking regulations (e.g., one-side-of-street parking) • Right–turn–on–red light	• Painted crosswalks alone
Cross-cutting	• Speed limits* • Speed ramps/bumps* • Curfew laws to restrict teenage driving • Alcohol sobriety checkpoints • Lower BAC laws • Minimum drinking age laws	• Daytime running light • Ignition interlock systems • Automated enforcement devices (e.g., red light camera) • Administrative per se license suspension laws • Edge lines • Wrong-way signs	• Designated driver • Alcohol safety education • Rumble strips	• Driver education for young drivers • Skid schools

* Denotes intervention with some evaluation in LICs.

most of these interventions and strategies may not be realized when they are applied alone and must, therefore, be complemented with others. For example, the mere presence of a seat belt in an automobile may not suffice for effective intervention unless complemented with public education and enforcement by law enforcement officers. Additionally, many specific interventions and strategies require some administrative infrastructure for implementation, epidemiology for planning and prioritizing, and some fundamental programmatic requirements as discussed extensively by Trinca et al.[16] Traffic laws and enforcement are a critical intervention in their own right, cutting across nearly every behavioral intervention.

Seat belts

The seat belt is an example of an *active* intervention for occupants because it requires some action on the part of the user. Its effectiveness in preventing injury and death in motor vehicle collisions has been well established by many earlier research studies[17–19], as well as recent ones[20]. Seat belts are estimated to reduce motor vehicle fatalities by 50% and serious injury by 55%. Seat belts are useable as an intervention for traffic injury intervention in LICs because they are affordable and their implementation is feasible. In order to derive the maximum benefit from seat belts, however, several stringent strategies are required. Such strategies include *mandatory seat belt laws*, *public education on the benefits of seat belts* and *legislation on the availability of functional seat belts in vehicles*. Currently, all but one State in the United States has a mandatory seat belt law. However, with mandatory laws comes the issue of enforcement, which varies from state to state even in the United States.

Seat belt laws may be implemented through several strategies including *primary enforcement*, whereby a law enforcement officer may stop a driver based solely on a safety seat belt violation or *secondary enforcement*, whereby an officer may only address a seat belt violation after stopping the driver for some other purpose. Recent reviews of the relative efficacy of primary versus secondary enforcement suggested that primary enforcement of the seat belt law is more effective than secondary laws[9,14]. Enforcement of seat belt laws clearly affects compliance. However, whereas in HICs police departments can afford to purchase enough vehicles to conduct primary seat belt enforcement, the same cannot be said of many LICs. For instance, it was revealed recently that the Ghana Police Service, made up of 16,445 officers, has a total of only 145 vehicles for operation in a country of more than 18 million people[21]. In settings like this, the difficulty of effective enforcement puts a premium on passive protection. In the United States for a while, cars were equipped with passive seat belts which consisted of shoulder harnesses that were applied automatically to front seat passengers as the door closed. Whether these passive devices would be more effective in countries where enforcement seems impractical is debatable. Most LICs don't manufacture or assemble cars and their limited budgets may not allow them to substitute already installed active seat belts in imported cars with passive ones.

Most states in the United States also include the availability of a functional seat belt in the annual inspection of vehicles before issuing road-worthiness certificates. In many LICs, some vehicles imported from HICs may lack functional seat belts. Currently, anecdotal evidence shows that more than half of the vehicles in many LICs have no functional seat belts. LIC governments need first to find out what proportion of vehicles have functional seat belts in order to assist in devising appropriate policies. While it may not make perfect sense to mandate the use of seat belts because most cars may not be equipped with them any way, what LIC governments could do is to consider issuing policies to ban the importation of vehicles without functional seat belts into their countries. Thus, vehicle inspection could begin right at the ports of entry into LICs.

A few studies in some LICs have reported some successes and failures with seat belt use[22–24]. A study in Singapore, for example, reported that seat belt regulation did not seem to impact on traffic fatalities[22]. In Greece, however, a comprehensive intervention campaign to increase seat belt use, even in the absence of increased law enforcement, resulted in moderate gains[23]. Clearly, seat belt use is feasible and useable in LICs, but with some limitations due to the different traffic patterns. Increasing seat belt usage rates will definitely help to reduce traffic injuries among occupants, since in many LICs drivers rarely use them. In an investigation in Malaysia, for example, 60% of 60 apparently restrained taxicab drivers observed at the curbside did not fasten the latch of their seat belts[24].

Air bags

The air bag represents an example of a *passive* intervention because no action is required of the host to be protected. Air bags have been mandated in all vehicles manufactured or imported into the United States since 1994. They have also proven to be effective in increasing occupant safety[25,26], although research shows that their effectiveness is less than that provided by seat belts[27]. But the situation in LICs is different when airbags are considered without seatbelt use – an instance where there is probably little relevant research. However, the fact that there really is no barrier to use if a vehicle is already equipped with air bags makes this intervention affordable and useable although there is also the problem of airbags killing infants and children and small adults even in some low-speed crashes. Thus there is a need for more data and important to monitor the outcomes. Therefore, air bags are also useable in LICs and LIC governments should begin to mandate the importation of cars with air bags. This is feasible since many governments already restrict the age of vehicles that are imported, albeit with difficulties in enforcement.

Motorcycle helmets

Just like seat belts and air bags have proven effective in motor vehicle crash related injury reduction, motorcycle helmets have proven effective in motorcycle crash related injury reduction, making motorcycle helmet laws a strategy with proven effectiveness[28,29]. In fact, recent research findings in settings other than the United States corroborate the evidence for effectiveness of mandatory motorcycle helmet laws[30,31]. In Taiwan, motorcycle fatalities decreased by 14% following passage of a motorcycle helmet law in 1997[30]. However, like most active interventions, full benefit or protection is dependent on many parameters including use rates, whether standard or approved devices are used, and how they are used. For example, an Indonesian study reported that 45% of motorcyclists wore their helmets improperly[31]. A study in California also reported that as many as 48% of observed motorcyclists used nonstandard helmets. Not surprisingly, head injuries were found to be more frequent and of greater severity among those wearing nonstandard helmets than among those wearing standard helmets or even those wearing no helmets at all[32]. Governments of LICs should both require helmet use and set helmet standards. Motorcycle helmets and mandatory helmet laws are clearly transferable to LICs, as evidenced by studies in Taiwan[30], Indonesia[31], Thailand[33], and Greece[34]. The acquisition of motorcycle helmets is well within the budgets of the people who can afford motorcycles in these countries. In addition, promulgating motorcycle helmet laws has been associated with significant decreases in mortality and injuries sustained from motorcycle crashes. When a motorcycle is acquired, purchase of an approved helmet should be encouraged or even mandated in LICs given the feasibility and potential sustainability of this intervention.

Bicycle helmets

Notwithstanding the ongoing debate on whether mandatory bicycle helmet laws are necessary in many jurisdictions, the effectiveness of bicycle helmets in traffic safety has been proven. In fact, many studies have shown that a well-designed helmet can reduce the extent of head injuries sustained by bicyclists during falls and collisions[35]. Like other active interventions, the full benefit from helmet use increases when combined with other proven or promising interventions or strategies such as *construction of bicycle paths or lanes*, *bicycle safety programs*, *bicycle skills training programs* and *conspicuity-enhancement measures*. Likened to a vaccine, the helmet is a proven intervention that is readily useable in LICs. Currently helmet use is abysmally low in LICs. For example, none of the patients admitted for bicycle-related crashes in Wuhan, China wore a helmet at the time of injury[36]. Barriers to application in LICs may not be any different from those cited by children and their parents in the HIC setting[37]. Some of these barriers include helmets not being ‘cool,’ being uncomfortable, and being hot, as well as not having one and the high cost of helmets[37]. Again, the policy issues here are for LIC governments to ensure the importation of only standard, approved helmets and also to encourage the purchase of approved helmets in combination with the acquisition of bicycles.

Bicycle paths or lanes

Reduction of bicycle–motor vehicle crashes through the use of bicycle paths or lanes was demonstrated in a California study in 1976[38]. Subsequently, development of these paths or lanes as a means of separating motor vehicle traffic from bicycle traffic mushroomed in many towns and cities. The effectiveness of bicycle lanes was substantiated in a 1990 German study[39]. Bicycle paths or lanes represent a very promising intervention that is affordable, feasible, and sustainable in LICs. In areas of extensive bicycle use in these countries, construction of bicycle paths or lanes should be encouraged. At a minimum, bicycles should be separated from other traffic.

Roadway countermeasures for pedestrian safety

While recent research continues to demonstrate the ineffectiveness of painted crosswalks or ‘zebra crossings’ alone in encouraging drivers to slow down for pedestrians[40], several other roadway countermeasures developed to protect pedestrians such as *sidewalks*, *roadway barriers*, *pedestrian crossing signs*, *one-way street networks, school-zone measures*, *curb parking regulations, education on conspicuity-enhancement measures, provision of adequate roadway lighting* and *pedestrian indicator lights* have been proven effective[41]. However, their availability and use may differ from place to place even in the same country depending on where most pedestrians are hit (e.g., crossing the street versus walking in the street). More research on pedestrian injuries needs to be conducted to provide data to suggest specific countermeasures to be tried. It is important to note that measures to protect pedestrians have a greater importance and urgency in LICs because of the increased fatalities among vulnerable road users.

Sidewalks are an effective intervention that should be used wherever feasible[41,42]. In a recent study, the risk of a crash was found to be two times more likely at a site without a sidewalk than at a site with one[42]. Sidewalks are affordable and feasible in LICs if the general public is educated to use them instead of walking on the roadways. However, although sidewalks are an effective intervention, even in HICs they are mostly limited to urban areas. With increased pavement of roadways taking place in many LICs, the construction of more sidewalks is necessary and warranted. In HICs, sidewalks work by separating pedestrians from the motorized traffic. Other ways of separating pedestrians from the motorized traffic may also be effective, but they have not been rigorously evaluated.

Roadway barriers, used to separate the motorized traffic from pedestrians and the non-motorized traffic, have helped to reduce the carnage of traffic injury in many settings[41]. This intervention is also transferable to LICs. However, roadway barriers may be less effective in preventing pedestrian injuries if the constructed safe routes for pedestrians are not designed to be the easiest and most convenient for the specific users. An example of this scenario was provided by a pedestrian safety study in Rio de Janeiro, Brazil[43]. Although a concrete divider topped with a wire fence was constructed to separate pedestrians and bicyclists from the motorized traffic on a superhighway, many pedestrians chose to climb over the concrete divider and run across the traffic lanes instead of using the pedestrian bridge, apparently due to its long flights of stairs. Consequently, many pedestrians were struck by motor vehicles[43]. Thus, consultation with the local people to determine the exact location of roadway barriers and pedestrian bridges, along with education, is needed.

Pedestrian crossing signs in unusually hazardous locations have been proven effective in HICs[41]. This intervention can easily be transposed to many LIC settings. However, like the many safety interventions discussed above, adequate education of the general population is needed for realization of the greatest benefit. Furthermore, recent research findings that encourage measures such as location of the crossing signs and other additional measures must be considered[44,45]. Recommendations include combining crosswalks with crossing signs and locating crosswalks within two meters of an intersection in order to optimize pedestrian safety[44]. This is feasible and affordable by LICs. Additionally, it has been recommended that posting advanced signs – such as a flashing amber light, that prompts motorists to yield to pedestrians before crosswalks – has a positive influence on pedestrian safety[45]. Indeed, pedestrian crossing signs combined with clearly marked crosswalks and warning lights have the potential for a huge impact on pedestrian safety. Fortunately, this is something that is easily affordable by many LICs. With illiterate populations, it may be necessary to use graphic symbols that are universally and easily understood. Some signs will require attention by drivers and enforcement may be an important element in their effectiveness.

Speed limits and other traffic calming strategies

Speeding on highways is a major cause of traffic crashes. The effect of speed on causing traffic-related crashes, injuries and deaths has been documented in many settings[46–48]. For example, the 1995 repeal of the United States National Maximum Speed Limit, allowing states to raise interstate speed limits, resulted in a 15% increase in fatalities in 24 states that raised speed limits[46]. In Adelaide, Australia, the risk of severe crash involvement was found to increase as vehicle speed increased[47]. A study in Colombia attributed 34% of traffic-related mortalities to speeding, alcohol consumption or both[48]. Speed limits have shown proven effectiveness in reducing traffic injury and death and should be encouraged in LICs. As a crosscutting intervention, the enactment of speed limits has led to reduced pedestrian injuries, as well as reduced occupant injuries in many jurisdictions[49]. Speed limits as an intervention with proven effectiveness are useable in LICs. In fact, the over 20% reduction in traffic crashes and deaths in Brazil has been partly attributed to speed limits, which have been posted on many roads since 1998[50]. Not surprisingly, both advisory and mandatory speed limits of 20 mph in urban areas are being contemplated in Edinburgh, United Kingdom[51].

However, despite the overwhelming evidence of the effect of speed limits on traffic crashes, other factors must also be in place, as evidenced by a recent study assessing the relationships among various measures of traffic speed, flow conditions, and crash rates in the United Kingdom and Bahrain[52]. In Bahrain, for example, a significant association was demonstrated between mean traffic speed and crash rates[52]. Clearly, speed limits must go hand in hand with *strict enforcement of speed limits,* as well as other traffic-calming strategies such as *speed bumps*, *speed humps*, and *speed strips*. Afukaar et al. discuss these issues in more detail elsewhere in this journal issue[53]. Nonetheless, the effectiveness of various types of enforcement measures in LICs will need to be studied. This is particularly important given some recent research findings that question the effect of speed on crash incidence and severity[54]. According to Lonero et al., recent reductions of enforcement in some North American jurisdictions seem to have led to little speed increase, suggesting that average speed may not be very sensitive to enforcement, perhaps being more cultural[54].

Strategies targeted toward young and new drivers

The complexity of driving exposes the inexperience of the young and new driver. In all settings, young and new drivers are overrepresented in traffic crashes. There is some evidence that all beginning drivers should learn under the least hazardous conditions such as reduction of exposure and delay of driving, including a long period of only daylight driving and driving with supervision, before moving on to more complex driving conditions. Other potential interventions that could offset the preponderance of traffic crashes among young and new drivers are *curfew laws to restrict teenage driving*, *graduated driver licensure*, and *increasing the age of licensure*. It is, however, important to point out that in LICs teenage drivers probably have limited access to vehicles, shifting the emphasis on new drivers of trucks, buses, and taxis.

Curfew laws to restrict teenage driving

In a review of a dozen states in the United States with curfew laws on teenage driving as of 1984, it was found that curfew laws can be effective in reducing the high crash rates of teenage drivers which result in large numbers of injuries to themselves and others[55]. In fact, most crashes involving teens occur during nighttime hours, including nearly half that occur between the hours of 8:00 p.m. and 4:00 a.m.[56] Therefore, night-time curfew laws appear to have some proven effectiveness. It has even been recommended as an option for reducing probationary driver casualties in Australia[57]. As an intervention for traffic safety, night-time curfews for teenage drivers are feasible and affordable at virtually no cost save that of policing, and ought therefore be implemented by LICs. They appear affordable and feasible, but the effectiveness of these laws together with enforcement procedures remains to be studied in LICs.

Graduated driver licensure

The idea of graduating licensure for young drivers takes its roots from Patricia Waller, who in the 1980s recognized that existing driver training programs provided only the barest minimum of the practice that beginners require[58]. An evaluation of the graduated driver licensing system in New Zealand in the early 1990s showed that the introduction of the system was closely followed by substantial reductions in car crash injuries for all age groups, especially among 15–19 year olds who experienced a 23% reduction[59]. Recent findings attest to the promising nature of this intervention[60,61]. This is an intervention that is clearly useable and affordable in LICs. However, like most laws, enforcement is needed to realize its full effectiveness. Another study reported that many teenagers do not respect curfew laws because they perceive police enforcement of the law to be low[62]. This intervention could be beneficial to LICs, but strict policing may not be easy.

Strategies aimed at reducing alcohol-impaired driving

The impact of alcohol impaired driving on traffic collisions is well known. Some of the effective measures that have been tried include license suspension laws, ignition interlock systems, alcohol sobriety testing, implementing compulsory blood-alcohol content (BAC) testing in traffic injury cases, enforcement of minimal legal drinking age, alcohol safety education, and use of designated driver and safe ride programs. Recent findings from systematic reviews conducted for the United States Task Force on Community Preventive Services provide strong evidence that sobriety checkpoints, 0.08 g/dL Blood Alcohol Laws, and minimum drinking age laws are effective in reducing alcohol-impaired driving, alcohol-related crashes, and/or associated fatal and nonfatal injuries[15]. Unfortunately, most of these interventions and strategies have not undergone rigorous evaluation in many settings, particularly in LICs.

License suspension laws

Concerned with the immediate surrender and suspension of the driver's license if a suspicious driver refuses to undergo alcohol testing by a law enforcement officer, this intervention has been found to be associated with a statistically significant reduction in fatal traffic crashes. However, a 1999 review of the specific deterrence of this intervention in reducing drunk driving recidivism was not very convincing,[10] calling for further research. Nonetheless, this promising intervention may be feasible, affordable, and sustainable in LICs.

Ignition interlock device systems

This intervention holds great promise in preventing drunk driving. The device prevents a driver from starting a vehicle unless an alcohol-detecting test is passed. A recent review evaluated the effectiveness of ignition interlock devices in reducing recidivism of drunk drivers. A conclusion was made that alcohol ignition interlock programs appear to be effective in reducing driving while under the influence of alcohol (DWI) recidivism during the time that the device was installed in the car[63]. The feasibility and enforceability of this promising intervention in LICs is contentious at the current time given the technology, although it appears potentially affordable and sustainable. There are technical problems, such as whether it can be installed in old cars. Other issues include the possibility of tampering with the device, especially if there is no effective inspection and enforcement and what the cost would be.

Public education targeting motorists

Although the findings from a 1999 study revealed the ineffectiveness of driver education for young drivers[12], there is some evidence that general public education along with some behavioral modification that targets motorists may have some impact on traffic safety. One area is education of motorists on posted traffic signs. A recent study in three countries showed that comprehension of 28 posted traffic signs for drivers was related to years of driving experience[64]. Another area might be education about the need for vehicular testing and vision testing. For example, a Nigerian study found a third of taxi drivers to have poor vision[65]. An unpublished paper synthesized results of traffic safety issues in Africa and generated a list of probable causal factors and possible safety and crash preventive measures. These factors and measures have been summarized for incorporation into general public educational campaigns[66].

Discussion

In considering options for technology transfer to LICs, a careful evaluation of what might work in these settings is essential. What has been found to be effective in a high-income setting may not necessarily be effective in a low-income setting. In some instances, some modification or adaptation of the interventions may be required in order to maximize the likelihood of success in LICs. For example, the adoption of even such an intervention with proven effectiveness as helmet use will not be effective without cognizance of cultural sensitivities, as well as other delicate issues like feasibility, sustainability, and acceptability[67]. However, the realization that LICs could learn from the failures in injury control from HICs, which are as valuable as the many success stories, may indeed become an opportunity for innovation. It is important, however, to differentiate between mere *improvisation* and actual *innovation*. The use of local materials to substitute for some intervention device constitutes an improvisation, which is distinct from a situation where an entirely novel approach is used to enhance a transposed intervention from a high-income setting (e.g., use of bamboo instead of iron bars to separate pedestrians from traffic). Obviously, the need to search for local evidence to confirm the acceptance and effectiveness of transposed interventions cannot be overemphasized. Thus far, however, a review of the literature shows that this has not happened: there are no published evaluations of such interventions in LICs.

Clearly, there are several traffic injury policy interventions and strategies that might be transferred to low-income settings. Proven and promising interventions or strategies such as seatbelt and helmet use, legislation and enforcement of seatbelt use, sidewalks, selected traffic-calming designs (e.g., speed bumps), pedestrian crossing signs combined with clearly marked crosswalks and public education targeted at motorists are all feasible and useable in LICs as evidenced by data from many LICs[23,24,42,43,53,55]. However, there are factors that would affect their ultimate value in LICs such as costs, feasibility, and barriers to implementation.

The desire not to reinvent the wheel is probably the main motivation behind the support for transposing effective interventions and strategies from HICs to LICs. The other desire is to transfer the benefits from research and experience in HICs to LICs. Along with this motivation, however, come several barriers, including the following:

- Economic situation in many LICs, which may preclude them from paying the needed attention to traffic safety, e.g., the GNP per capita in more than half of LICs, including almost all sub-Saharan and South Asian countries, is still less than $500. The amounts available for governments to spend on safety measures is very low;
- Political situation, e.g., political leaders often do not appoint qualified people to positions of authority such as ministers of health;
- Cultural beliefs, e.g., injuries are considered to be 'acts of God;'
- Low literacy rates, often still below 60% in many LICs, which may mitigate against many of the interventions that rely on public education, e.g., adequate comprehension of posted traffic signs;
- Competing health problems, e.g., the recent scourge of the HIV/AIDS epidemic and the lack of personnel with the technical know-how in many LICs, which may prevent governments from devoting any serious attention to the problem of traffic injury intervention;
- Entirely different traffic mix in many LICs, which often involves motorized vehicles and non-motorized vehicles such as bicycles and carts, as well as pedestrians and animals in some instances, and the uncontrolled rate of urbanization.

With careful analysis and synthesis of country-specific problems, as well as proper planning, LICs can import traffic injury interventions and strategies developed largely in high-income settings. As noted by a former United States Surgeon General, safety and injury prevention must be both among the highest priorities of every nation and be based on sound scientific evidence[68]. Due to scarcity of resources, however, it behooves LICs to import only proven and promising interventions and strategies at this point in time and to collect and share information about their effectiveness in LIC settings.

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Reducing Unintentional Injuries on the Nation's Highways: A Literature Review

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Abstract: Death and injury on the nation's highways is a public health crisis, especially for youth and members of selected minority groups. The objective of this paper is to review the literature on behavioral and environmental factors that increase risk for traffic morbidity and mortality in populations at high risk. Each of the following is a risky traffic-related behavior: not wearing seat belts, not using child safety seats, not wearing bicycle or motorcycle helmets, driving after drinking, driving while fatigued or distracted, speeding, running red lights, and aggressive driving. Environmental factors that modify risk include urban sprawl, highway design, public policy, racism and economic inequality. High risk groups include youths, males, pickup truck drivers, urban dwellers, the elderly, African Americans, American Indians, and Alaska Natives. A comprehensive approach must be developed for reducing traffic-related risk of death and injury, especially in high risk populations.

Key words: Safety, traffic, injury, mortality, environment, behavior, disparities.

In 2002, an estimated 42,850 people in the United States were killed in motor vehicle crashes, an additional 2,914,000 people were injured, and a total of 4,307,000 motor vehicle crashes resulted in property damage with no injury.[1] Unintentional injury is the leading cause of death for people under age 45, and motor vehicle crashes represent the most frequent cause of unintentional injury.[2] For all these reasons, reducing traffic-related morbidity and mortality is a national priority.[3,4] In this paper, we present an overview of problems associated with motor vehicle morbidity and mortality in the United States with an emphasis on how these problems disproportionately affect minority and disadvantaged populations. Three fundamental questions are addressed: 1) What modifiable behaviors contribute to risk? 2) How do social, cultural, economic, and environmental factors influence risky behaviors? 3) What populations or groups have excess risk?

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Behavioral Risk Factors for Motor Vehicle Morbidity and Mortality

Seat belt use. Wearing a shoulder and lap restraint can significantly reduce the risk of severe injury and death for drivers, front seat passengers, and rear seat passengers,[5–9] particularly for children.[10] The National Highway and Traffic Safety Administration (NHTSA) estimates that shoulder and lap belt use in automobiles reduces the risk of death by 45% and the risk of severe injury by 50% while their use in light trucks lowers the risk of death by 60% and severe injury by 65%.[11] In comparison, air bags reduce the risk of death by only 12%.[11] Seat belt use has also been shown to decrease medical costs associated with accident-related injuries.[12] Over 26 billion dollars in costs could be saved each year if seat belt use were universal.[13]

Occupants of the front passenger seat have a similar or even higher risk of dying in motor vehicle crashes than drivers have,[14,15] a risk that is reduced through the use of air bags and seat belts.[16,17] (The effectiveness of airbags does not supercede the effectiveness of seat belts; using both is the safest practice.[14]) Although the National Occupant Protection Use Survey (NOPUS) data do not include observations of seat belt use for rear seat passengers, these passengers and the front seat passengers[18] are safer when back seat passengers wear restraints.[8]

Seat belt use in the United States has improved dramatically over the past 10 years, but remains below 80%.[19] In 2002, the national rate of belt use, estimated from observational surveys in all 50 states, reached 75% overall and 80% in states with primary seat belt laws (laws allowing a driver to be stopped for not wearing a seat belt).[19] Increasing seat belt use could save thousands of lives each year.[11]

Use of child restraints. Use of restraints in children should occur in 3 stages that are age and/or height dependent. Infants and children under 4 years of age should be restrained in a child car seat. Children 4–8 years old should graduate to a booster seat that allows shoulder and seat belts to be used more safely. Small children restrained only by shoulder and lap belts designed for adults are at risk for head, face, spinal, and abdominal injuries. After age 8, or when the child is taller than 145 cm (57 inches), the child should graduate to an adult seat belt.[20]

Properly installed safety seats can reduce mortality by 70% in infants and 50% in toddlers,[21] while injuries requiring hospitalization are reduced by 69% in children 4 years old and younger.[21]

A 1995 study by NHTSA checked safety seat use and installation in shopping center parking lots in 4 states and showed that the rates of safety seat use were 96% for infants (20 pounds and under), 68% for toddlers (20–40 pounds), and 6% for preschoolers (40–60 pounds). When installation and use of the seat were examined, 80% were not being used properly. There was a strong relationship between the driver's use of seat belts and child restraint: if the driver was restrained, only 5% of the children were unrestrained; if the driver was unrestrained, 47% of the children were unrestrained.[22]

Lap and shoulder belts were designed for adults and do not work well when used with young children because they cannot be pulled tight, the lap belt hits the child in the abdomen, and the shoulder harness either does not restrain the child at all or crosses the child at the head or neck.[22] A retrospective study of insurance claims showed that children ages 2–5 were 3.5 times more likely to suffer significant injury and 4.5 times more likely to suffer head injury if they were using a seat belt instead of a car seat.[23]

A case control study compared people who died while riding in the front seat with those who survived and examined the relationship of air bags to survival.[24] Restrained children in the front seat were not at increased risk (compared with children in cars with no air bag) if an airbag was present, but unrestrained children were 37% more likely than children in cars with no air bag to have been killed if an air bag was present.[24]

Motorcycle and bicycle helmets. Motorcycles are a more dangerous mode of transportation than automobiles because there is no structure to protect the driver during a crash. Per mile traveled, drivers are 21 times more likely to be killed on a motorcycle than in an automobile.[3] Helmets offer important protection from fatal and non-fatal head injury when a motorcyclist is involved in an accident. An unprotected rider is 40% more likely to die in an accident than a rider wearing a helmet.[25] In states with mandatory motorcycle helmet laws, compliance with helmet use is nearly 100%. In states without these laws, less than half of all motorcyclists wear a helmet.[26] Mandatory helmet laws have been weakened in several states in recent years with a corresponding increase in rates of motorcycle fatalities in these states.[27]

While this is rare compared with motor vehicle deaths, bicyclists are sometimes killed on the nation's roads and highways. In 2001, 728 bicyclists were killed and over 45,000 were injured in accidents with motor vehicles.[28] Each year, over 400,000 children are treated in emergency rooms for bicycle-related injuries. Bicycle helmets are estimated to reduce head injuries by 88% in bicycle accidents. Universal use of bicycle helmets among children 4–15 years old could prevent over 40,000 head injuries each year.[28]

According to a national telephone survey of 5,328 respondents, 20% of adults reported riding a bicycle during the past 20 days. Of the bicycle riders, only 18% reported wearing a bicycle helmet and only 5% of those between 18 and 24 reported wearing a helmet.[29] Rates of helmet use in children vary depending upon local helmet laws, but nationwide less than 20% of children wear bicycle helmets. With 67 million bicyclists in the United States, low rates of bicycle helmet use are placing many individuals at increased risk for debilitating head injuries or unnecessary death.[30] While young people are often responsible for bicycle wrecks they are in, most older cyclists are injured or killed because of the actions of motorists.[31] Helmet use is important to protect cyclists of all ages, despite the fact that mandatory bicycle helmet laws typically only apply to children.[28]

Drinking and driving. Driving after consuming alcohol long has been a problem in the United States and continues to be a serious public health issue.[32] In the year 2000, there were over 2 million alcohol-related vehicle crashes killing 16,792 people, injuring over 500,000, and costing over 114 billion dollars in direct and indirect costs.[13] It is estimated that alcohol use when the driver's blood alcohol level (BAL) is above 0.10% contributed to the cause of 91% of these alcohol-related crashes. Alcohol use ranks as a contributing factor in only 24% of crashes when BAL is below 0.08%.[33] According the NHTSA, "... the societal costs of alcohol-related crashes in the United States averaged $0.80 per drink consumed, with drinking drivers paying $.30 and other people paying $.50."[34]

Alcohol-related crashes are a bigger problem with younger drivers; one third of all fatal crashes involving alcohol also involved 21–24-year-old drivers. In 2001, 25% of drivers 16–20 years old involved in fatal crashes were intoxicated. Alcohol involvement in fatal and nonfatal crashes is more common among young males than young females. Alcohol use is also associated with lower rates of seat belt use in youth involved in motor vehicle crashes.[35]

The National Highway Traffic and Safety Administration conducted a nationally representative telephone survey of 6,002 individuals aged 16 and older, examining attitudes, beliefs, and behaviors associated with drinking and driving. Although 77% of respondents viewed drinking and driving as a threat to their personal safety, 23% of respondents admitted to driving within 2 hours of consuming alcohol during the previous year, with more males (32%) admitting to drinking and driving than females (13%).[36] Rates of drinking and driving are higher among young adults (21–29), with 37% of males and 20% of females admitting to drinking and driving. While 37% of males in the 20–45 age group continue to drink and drive, the rate drops to 16% for females in this age group. The National Highway Traffic Safety Administration estimates that about 11% of the time individuals who drive after drinking have blood alcohol levels above 0.08%, resulting in over 94 million episodes of driving while intoxicated per year in the United States.[36]

Approximately one of every three drivers arrested for intoxication is a repeat offender. Repeat offenders are more likely than others to be involved in motor vehicle crashes and, in particular, in fatal crashes.[37]

Driver fatigue. While it is difficult to obtain precise prevalence estimates, falling asleep while driving is the cause of as many as 50,000 motor vehicle crashes and 1,500 fatalities per year[38] representing about 3% of all fatalities.[39] Driving between midnight and 6 a.m., driving long distances, consumption of alcohol, and use of certain medications are risk-factors for falling asleep behind the wheel.[38, 40–5] Three groups are at highest risk: males between ages of 16 and 29, people working night shifts or whose work schedule varies from day to day, and people with undiagnosed sleeping disorders.[38,44,46] About 1 of every 3 drivers reports falling asleep at the wheel at least once, and about 4% of drivers report an episode of falling asleep while driving during the previous month.[47] Most drivers, especially those who have fallen asleep behind the wheel, view driving while sleepy as a major public safety hazard.[48]

Driver distraction. A recent survey by the Gallup Organization examined the prevalence of driver distractions, finding that the most common distractions were the use of electronic devices such as cell phones (60%), electronic pagers (12%), wireless internet (15%), and electronic navigation devices (5%). Of those with cell phones, about 40% reported either making or taking phone calls while driving, with phone use higher among younger drivers. About 20% of all driving trips involved either taking or making a cellular phone call. Only about 2% of drivers reported using personal digital assistants (PDA) or wireless internet devices while driving and 3% of drivers used a pager while driving.[47] Drivers are 4 times as likely to have a motor vehicle accident while using a cell phone than when they are driving without using the phone.[49] However, some authors suggest that the regulation of cell phone use may not be a cost-effective way to reduce motor vehicle crashes.[50–2]

Other driver distractions and their rate of occurrence included talking to passengers (83%), attending to children in the back seat (24%), eating or drinking while driving (49%), personal grooming (8%), looking at maps (12%), and reading (4%). About 29% of all driving trips involved eating, 18% dealing with children, 10% reading maps, 8% grooming, and 7% reading books or newspapers.[47] It is not entirely clear how each of these behaviors contributes to crash risk,[53,54] but a number of studies clearly show that distraction by cell phones, attention to electronic devices, and verbal exchanges interfere with a driver's ability to respond quickly to road hazards.[55–8]

Risky driving behaviors. At least 3 sets of driving behaviors can increase the risk of motor vehicle crashes: 1) running red lights, 2) exceeding the posted speed limit, and 3) aggressive driving. About 40% of all crashes occur at intersections.[59] Entering the intersection on the red light either inattentively or deliberately is a major risk factor for intersection crashes.[60] There are over 260,000 crashes associated with running red lights each year, resulting in 750 fatalities.[59] Red light running crashes are more dangerous than other types of urban crashes because drivers are often exceeding the speed limit as they enter the intersection. Forty-five percent of red light running crashes are associated with occupant injury.[61] Drivers involved in crashes who ran red lights were more likely to be younger, male, using alcohol, and have a record of previous traffic violations than the other drivers involved in the same crash who did not run the red light.[59] A national survey of drivers showing that 20% of drivers admitted to running red lights on occasion, also found that being in a hurry was the main reason given for running a red light, with very few drivers believing there were any negative consequences for doing so.[62]

The likelihood of injury and death in an automobile crash is associated with vehicle speed, especially when a vehicle collides with a pedestrian or bicycle.[63] The cost of motor vehicle crashes in which driver speeding was implicated exceeds 40 billion dollars per year.[13] In 1987, U.S. law was changed to allow states to increase speed limits from 55 to 65 miles per hour and several studies demonstrated a corresponding increase in motor vehicle deaths.[64–7] The problem of speed is compounded by the fact that many drivers, especially teenagers, routinely exceed the posted speed limits.[68–70] In urban areas, between 40 and 80% of drivers on expressways exceed the speed limit by 10 miles an hour or more.[71] The National Highway Traffic Safety Administration estimates that in the year 2000, speeding was associated with 30% of all fatal crashes and that speeding-related crashes, on and off interstate highways, cost over 27 billion dollars a year.[39]

Aggressive driving comprises a constellation of behaviors that includes speeding, tailgating, verbally abusive speech, obscene gestures, dangerous maneuvers, and, in rare instances, physical violence.[71] Deffenbacher, *et al.*, identified three clusters of aggressive driving behaviors: 1) verbal expressive aggression, including yelling and cursing at other drivers; 2) personal physical aggression, including getting out of the car and threatening or fighting another driver, and 3) use of the vehicle to express anger, such as flashing lights or cutting in front of another driver.[72] The base rate of some of the behaviors (such as speeding, running red lights and stop signs, and tailgating) is high in urban areas.[71] A small number of drivers engage in very risky aggressive behaviors and may do violence to other motorists when anger is combined with a desire for vengeance.[73,74] Aggressive driving and other risky driving behaviors may be associated with personality characteristics such as sensation-seeking that might motivate risk-taking behind the wheel.[75,76] More research is needed to determine the extent to which aggressive driving influences the risk of motor vehicle accidents.

Environmental Factors Associated with Traffic Morbidity and Mortality

Highway infrastructure, urban sprawl, and the design of transportation systems. Specific design features of the transportation infrastructure in the United States and elsewhere are associated with risk of motor vehicle accidents. Urban sprawl, the trend of the past 50 years for urban centers to expand out from the central city to encompass several rings of suburban development all interconnected by roads and expressways, has reshaped the American landscape in the second half of the 20th century.[77–9] Residential areas and commercial/industrial districts are separated from each other, and people typically drive individual cars and trucks to work and shopping areas.[80] Overall, motor vehicles on U.S. roads travel over 4.5 trillion miles each year.[81] The number of people commuting to work using private vehicles has increased dramatically in the past 25 years, creating tremendous rush-hour congestion in most cities.[82] In 1999, the median commute time in the United states was 21 minutes and the median distance 10 miles with almost 80% of American workers driving to work alone.[81] A recent study showed a direct association between urban sprawl and the likelihood of fatal motor vehicle crashes.[79]

Because of highway design and traffic volume, even short trips often must be made by motor vehicle[83–5] and the more urban areas sprawl, the more dependent the population becomes on driving. Such circumstances have major consequences for traffic safety, air pollution, and climate change.[86–91]

About one third of all motor vehicle deaths are associated with crashing into roadside hazards such as trees, utility poles, bridges, and other objects that are fixed in place.[92] Other aspects of roadway design such as the use of protected turn lanes, the placement and timing of red lights, and design of intersections alter the risk for motor vehicle crashes.[93–6] Coupling pedestrian cross-walks with traffic signals, keeping parked cars away from crosswalks, and clearly marking pedestrian zones can reduce the risk of pedestrian accidents.[97,98] Both the probability and the lethality of an accident are strongly associated with vehicle speed; many aspects of roadway design influence how safely vehicles can travel at high speeds.[63,66,99,100] In many areas, the need to accommodate hundreds of thousands of vehicles per day has resulted in the design of roads that can carry high volumes of rapidly moving traffic, yet also create opportunities for deadly motor vehicle crashes.[101] Other aspects of the built environment, like zoning or the density of bars and liquor stores in an area, can influence the likelihood and severity of different kinds of motor vehicle crashes.[102,103]

State Traffic Laws and Law Enforcement

The two most effective strategies for increasing seat belt use have been the passage of primary seat belt laws and campaigns to enforce these laws and publicize their enforcement.[6,104–105] Primary seat belt laws allow a law enforcement officer to stop and ticket a driver for not wearing a seat belt. Secondary seat belt laws allow drivers to be ticked for not wearing a seat belt, but only if they were stopped for some other reason. In states with primary seat belt laws, rates of seat belt use among African Americans are equal to those among non-Hispanic whites.[106,107] Several studies have documented increases in seat belt use after passage of a primary seat belt law.[108,109] The National Highway Transportation Safety Administration has incorporated these findings into their Click-It-or-Ticket program, which has been shown to increase seat belt use.[110,111]

However, the introduction of a primary seat belt law does not necessarily mean that rates of seat belt use will rise. State and local law enforcement agencies differ in the degree to which they are willing and able to allocate resources to enforcing seat belt and car safety seat laws. For example, in 2001 observed rates of seat belt use in California were 91.1% compared with 68.1% in Louisiana, although both states have primary seat belt laws.[112] While there may be a number of reasons for the differences between California and Louisiana, it is possible that the expectation that one will be stopped for a seat belt violation is higher in California than Louisiana.

Other laws governing driving also make important contributions to public safety.[113,114] Laws establishing stricter standards for drunk driving, mandating suspension of driver's license, impounding vehicles or arresting impaired drivers, and mandating jail time for repeat offenders are all strategies for reducing the impact of driving while intoxicated.[37] Enforcing traffic laws such as those prohibiting speeding and red-light running also reduce rates of motor vehicle crashes.[115–7] Systems that assign points towards license suspension for violating traffic laws may also contribute to reductions in accident rates.[50]

Public policy and highway safety. While there are many aspects of public policy that might influence traffic safety,[118] we will discuss only a few as examples: 1) automated systems for enforcing speed limit and red light laws; 2) graduated drivers licensing laws that put limits on drivers aged 16–18; 3) laws requiring daytime running lights; 4) transportation policies that encourage mass transit, walking, and cycling, and 5) motor vehicle safety standards.

Red light running is a common behavior for which drivers perceive few negative consequences,[119] even though it increases a driver's risk of intersection crashes.[120] Several states have programs for the automated enforcement of red light and speeding laws. Such programs may include cameras mounted at intersections that are triggered by a car moving through the intersection on a red light. The license plate is used to issue a ticket to the owner of the vehicle automatically.[121] Several studies have shown increased compliance with traffic laws, a reduction in crash frequency, and citizen support of the use of these automated enforcement systems when they have been installed.[121–3] Automated systems have also been used to give drivers feedback on their speed[124] and to enforce speed limits. Like police patrols, they have been effective in slowing traffic.[125–9]

Because traffic death and injury are prevalent problems among young drivers, a number of states have instituted graduated driver licensing.[130] Graduated licensing involves a three step process: 1) a learner's permit that allows driving only with a licensed adult present; 2) a provisional license that restricts the young driver to daytime driving, limits carrying of teenage passengers, and may include other restrictions and limitations on driving privileges; and 3) a full adult license. As of July 2003, 35 states and the District of Columbia had some form a graduated licensing law.[131] These laws have been effective in reducing the frequency and severity of traffic crashes involving youth,[132,133] effectiveness that would probably increase with improved implementation and enforcement.[134,135]

Daytime running lights are car and truck headlights that are illuminated whenever the vehicle engine is running. Running lights increase the visibility of vehicles by creating a greater contrast between vehicle and background. A number of developed nations, including Finland and Canada, have passed laws requiring daytime running lights on all new vehicles. Research has shown modest but significant reductions in the frequency of multi-vehicle daytime crashes in association with daytime running lights with no apparent negative side effects.[136–9]

Transportation policy concerns the laws, policies, and practices of local, state, and national governmental entities that are involved in building, maintaining, and regulating roads and public transit systems. Policy makers can emphasize different aspects of transportation, such as building wider roads to accommodate more cars or putting resources into mass transit and encouraging walking and bicycling.[84,91,140–2] Many decisions made by government agencies about transportation policy influence physical activity, air quality, and the likelihood and severity of motor vehicle crashes.[143] Nations, states, and cities can begin to design transportation and urban infrastructures that promote active transport (walking and cycling) possibly having an appreciable positive impact on population health.[144] Impact assessment methods are now available to help planners and policy makers assess the health consequences of transportation and other infrastructure design decisions.[145–51]

The U. S. government has the legislative authority to mandate, review, and enforce safety standards for motor vehicles manufactured and imported into the United States. The Federal Motor Vehicle Safety Standards establish the minimum safety and performance requirements for motor vehicles,[152] in an effort to help motorists avoid crashes, to minimize injury during crashes, and to reduce the likelihood of fires and other dangerous outcomes after a crash.[152,153]

Racism, discrimination, and economic inequality. There are many differences between racial/ethnic groups in risky behaviors and in morbidity and mortality associated with motor vehicle use. In general, identifying and describing health disparities is only the first step in a long process that includes understanding the causes of these disparities (including poverty, education, access to health insurance and health care services, education, cultural stereotypes, patient-provider interactions, racism and discrimination) and adopting strategies to reduce and eventually eliminate them.[154–9] The task is made more difficult by the fact that racial/ethnic groups are socially constructed to a great extent.[160,161] Racial categories in the United States are based on skin tone and country of origin, and also on the historical legacy of slavery and its aftermath.[158] Racial groups differ not only on the sometimes superficial characteristics that are used to identify group membership, but in many other ways that are embedded in political, economic, social, and cultural systems.[159,162,163] Among the many manifestations of race consciousness in the U.S.

is the fact that residential patterns in most major cities are racially segregated. These patterns, maintained by economic factors and by racial discrimination, contribute to health disparities through economic, environmental, interpersonal, behavioral, and cultural mechanisms.[164–7]

Economic inequalities between groups contribute to the development of health disparities by influencing access to societal resources, where one can afford to live, exposure to environmental hazards, and behavior.[163] Stress associated with low socioeconomic status, living in inner city neighborhoods, and/or racism and racial discrimination may also contribute to group differences in behavior and health outcome.[168–72]

Health disparities and their elimination may need to be approached one problem at a time. Causes and solutions related to diabetes or heart disease may be different from those needed to overcome disparities in motor vehicle death and injury. However, it is important also to focus on the fact that there are deep underlying factors such as individual and institutional racism, racial segregation, income inequalities, access to social and economic resources, and stress associated with belonging to a minority group that contribute to multiple disparities. Increasing seat belt use is important, but so is insuring equal access to education and health care, enforcement of fair housing laws, and community development efforts that address these deep underlying causes of many health disparities, including rates of traffic-related injuries.[162,173–6]

High Risk Groups for Motor Vehicle Crash Morbidity and Mortality

Youth. Even though mortality rates for young drivers are decreasing,[177] young drivers are still at higher risk for death in a motor vehicle accident than middle-aged drivers.[68,70] According to the Insurance Institute for Highway Safety, 40% of all deaths occurring among US teens are the result of motor vehicle crashes, with 4,997 teens dying in 2001.[178] Motor vehicle crashes are the leading cause of death for children 4–12 years old.[179] Traffic crash mortality rates for youth (15–20) were 26.8/100,000 in the year 2000 and were over 40/100,000 in Alaska, Alabama, and Wyoming.[180] Males, particularly during their first few years of driving, are at much higher risk than females.[178,181,182] Risk behaviors for youth include driving at night, alcohol use, failure to wear seat belts, and carrying passengers.[177,183] Observational studies have documented below average use of seat belts among younger drivers and passengers.[184]

Males. Males are 33% more likely to be involved in fatal automobile crashes than females.[185] Although the gender gap has steadily declined since the early 1970's, death rates were 15/100,000 for males and 8/100,000 for females in 2001.[186] The increased risk associated with male gender may be due to several factors. Males drive more miles per year than females, thus increasing their exposure to risk for traffic accidents. Males also consistently engage in more risky behaviors than females. Ninety% of people killed in motorcycle crashes are males.[26,186] In fatal motor vehicle crashes, males drivers were almost twice as likely to have a blood alcohol concentration above 0.08% than females.[35] Surveys also show that males are more than twice as likely as females to drive after drinking[36] and that they are less likely than females to wear seat belts.[9] In fatal accidents in the year 2000, 23% of females were unrestrained at the time of the accident compared with 43% of males.[39] Males involved in accidents were more likely to have run a red light than females.[59]

Pickup truck drivers. The most recent National Occupant Protection Use Survey (NOPUS) data and data from observational studies showed that rates of seat belt use were especially low among drivers and passengers in pickup trucks.[187–189] Although passengers in heavier vehicles like pickup trucks and sports utility vehicles are safer than passengers in lighter vehicles in head-on crashes,[15,190] this safety factor is offset if the vehicle's occupants are not wearing seat belts.[191]

Mortality rates for drivers and passengers in pickup trucks are higher than for any other type of vehicle.[192] Pickup trucks are especially dangerous if children or adults are allowed to ride unprotected in the bed of the truck.[191,193,194] Given that light trucks account for an increasing proportion of the miles driven by people in the U.S., targeting the safety of pickup truck drivers is important.[81] No information is available on racial and ethnic differences in risk for pick up truck drivers.

People living in urban areas. While mortality rates for motor vehicle crashes are generally higher in rural areas than in urban areas,[195] the total number of traffic fatalities is strongly associated with population density.[196] According to the Insurance Research Council, 81% of all crashes and 69% of all pedestrian fatalities occur in urban areas.[197] The most common types of crashes in urban areas are those in which a driver runs a red light, turns left across a lane of traffic, changes lanes, or strikes the rear end of a stopped vehicle.[197] Roadways in urban areas (characterized by a high density of stop lights, multilane highways, and dense traffic) pose dangers to drivers not posed in rural areas.[197]

Elderly. According to the Centers for Disease Control and Prevention (CDC), 7,780 Americans ages 65 and older died in motor vehicle crashes during 1999 and an estimated 209,000 in the same age group suffered nonfatal injuries in motor vehicle crashes.[198] Traffic crash rates are higher among the elderly than among the non-elderly[199] and the number of elderly injured or killed in motor vehicle crashes is expected to increase substantially in the next 15 years as the elder population expands.[200] The causes and types of motor vehicle crashes involving older drivers differ from those involving younger drivers, which may mean that different strategies are needed to reduce traffic-related mortality among the elderly.[70]

The elderly are the fastest growing segment of the population, with 40,000,000 drivers over the age of 65 expected by the year 2010.[198,200–2] Age-related declines in vision, hearing, reaction time, cognitive function, and physical ability increase the older driver's risk of being involved in a traffic crash.[203] Even though older individuals drive fewer miles than younger individuals, they have a higher risk of being involved in a crash and a higher risk of being killed in that crash.[204] The risk of death and injury from traffic accidents for older people has two components: 1) an increased risk of being involved in certain types of crashes, and 2) a higher risk of sustaining injuries because of the frailty associated with aging.[205] Some researchers have suggested that changes in automobile design might be useful in reducing injuries to elderly drivers.[206]

African Americans, American Indians, and Alaska Natives. Age-adjusted traffic accident mortality rates are higher among African Americans (17.2/100,000 in 1998) than among any other ethnic group except American Indians and Alaska Natives

(31.8/100,000).[207] An analysis of mortality data from 1993–1995 suggested that African Americans involved in fatal traffic crashes were more likely than non-Hispanic whites to be unrestrained.[15] Another recent study compared rates of seat belt use among motor vehicle crash victims and showed that differences in rates of seat belt use between African Americans and non-Hispanic whites were greater in states with secondary seat belt laws. In a secondary seat belt law state, only 21% of African Americans were wearing a seat belt at the time of the accident (compared with 42% of non-Hispanic whites).[107]

American Indians have higher mortality rates[208] and injury rates in motor vehicle crashes than do non-Hispanic whites living in the same state.[209] Similar findings have been reported for pedestrians, with death rates among American Indian pedestrians being 3 to 4 times higher than among white pedestrians in Arizona.[210] American Indian and Alaska Native youth also are at higher risk than non-Hispanic whites for unintentional injury and death from other causes besides motor vehicles.[210] Motor vehicle crashes are the 3rd leading cause of death for American Indian males (compared with 7th for non-Hispanic white males) and the 5th leading cause of death for American Indian women (compared with 10th for white women).[211]

There are racial and ethnic disparities within some of the other identified high-risk subgroups. The CDC reports that African American students are more likely than Caucasian students to report not using a seat belt,[212] with African American females reporting lowest rates of seat belt use.[213] Secondly, urban areas are where racial/ethnic minority groups, especially African Americans, live in the greatest concentrations. An analysis of the 2002 NOPUS observations of African Americans in urban areas showed that only 63% were wearing seat belts[214] compared with an overall U.S. rate of 77% for African Americans. Using parking lot observations of a convenience sample of African Americans visiting community health centers in four communities, driver rates of seat belt use were 57% and passenger rates were 52%.[215] Finally, rates of dying in motor vehicle crashes among the elderly in the U.S. are higher in minority populations.[216] Among pedestrians, death rates by ethnic group differ depending upon whether or not the population is urban or rural, with minorities at higher risk in urban areas.[210]

Little information is available about racial and ethnic differences in drinking and driving. Among college students, non-Hispanic whites are 2.1 times more likely to report drinking and driving than non-Hispanic blacks.[213] Patterns of alcohol use in the United States are a complex function of age, gender, and ethnicity.[217–19] Heavy alcohol use is more common among young non-Hispanic white males than other racial/ethnic groups, while problem drinking patterns emerge in the 25–45 age group more often among Hispanic and African American males than among non-Hispanic white males.[220–2]

Summary

Scientific research has identified a number of behaviors that increase one's risk for being injured or killed in a motor vehicle. Specific behaviors such as using seat belts and child car seats, wearing helmets, avoiding drinking and driving, minimizing driver fatigue and distraction, and obeying traffic laws and driving courteously have already been adopted by large segments of the population. If adopted by even more people, these behaviors could greatly reduce the human and economic toll of motor vehicle crashes. The risk of injury and death in vehicle crashes are not uniformly distributed across the population. Young people, pickup truck drivers, inner city residents, the elderly, African Americans, and Native Americans are groups at particularly high risk.

In some cases, the association between high-risk groups and risky behaviors (*e.g.*, inner city African Americans having lower rates of seat belt use; young males driving more aggressively) is known, while in other instances more research is needed. In addition, specific environmental factors such as urban sprawl, highway design, public policy, and laws contribute to risk. Disparities in risk between racial/ethnic groups are complex and dealing with them must involve consideration of the deeper underlying causes (including segregation, racism, discrimination, and unequal access to social and economic resources).

The field of traffic safety has an impressive infrastructure to monitor morbidity and mortality associated with seat belt use. Surveillance systems are in place tracking all traffic fatalities, sampling non-fatal traffic accidents, and monitoring seatbelt use nationwide by means of observational and self-report methodologies.[3] In addition, other large data sets such as the hospital discharge database[223] and other sources of data on injury such as the CDC's Web-based Injury Statistics Query and Reporting System (WISQARS)[224] give scientists the ability to track changes and trends. While surveillance systems could be improved by tracking additional risky behaviors such as not wearing helmets while biking and using cell phones while driving, a large amount of useful data is being regularly collected and analyzed. There is clearly a commitment at the National Highway Traffic Safety Administration and among non-governmental groups to find ways to make it safer to drive, walk, and cycle on or next to the roads in the U.S.

Advancing our understanding of risky behaviors and how psychosocial and environmental factors influence these behaviors is the most urgent priority. Better models integrating theory and data from the environmental and social sciences are needed to further our understanding of risk and of why some populations are at higher risk than others. Finally, an enhanced understanding of risky behavior must be translated into strategies for individual, group and community change. These strategies must be tested in community-based participatory research studies.

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Effectiveness of speed cameras in preventing road traffic collisions and related casualties: systematic review

Paul Pilkington and Sanjay Kinra

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Papers

Effectiveness of speed cameras in preventing road traffic collisions and related casualties: systematic review

Paul Pilkington, Sanjay Kinra

Abstract

Objectives To assess whether speed cameras reduce road traffic collisions and related casualties.
Design Systematic review.
Data sources Cochrane Injuries Group Specialised Register, Cochrane Central Register of Controlled Trials, Medline, Embase, Social Science Citation Index, TRANSPORT database, ZETOC, the internet (including websites of road safety and motoring organisations), and contact with key individuals and organisations.
Main outcome measures Road traffic collisions, injuries, and deaths.
Inclusion criteria Controlled trials and observational studies assessing the impact of fixed or mobile speed cameras on any of the selected outcomes.
Results 14 observational studies met the inclusion criteria; no randomised controlled trials were found. Most studies were before-after studies without controls (n = 8). All but one of the studies showed effectiveness of cameras up to three years or less after their introduction; one study showed sustained longer term effects (4.6 years after introduction). Reductions in outcomes across studies ranged from 5% to 69% for collisions, 12% to 65% for injuries, and 17% to 71% for deaths in the immediate vicinity of camera sites. The reductions over wider geographical areas were of a similar order of magnitude.
Conclusions Existing research consistently shows that speed cameras are an effective intervention in reducing road traffic collisions and related casualties. The level of evidence is relatively poor, however, as most studies did not have satisfactory comparison groups or adequate control for potential confounders. Controlled introduction of speed cameras with careful data collection may offer improved evidence of their effectiveness in the future.

Introduction

Road traffic collisions are an important cause of death and disability worldwide. Every year around the world 1.2 million people are killed and up to 50 million are injured or disabled as a result of road traffic collisions.[1] Morbidity from road traffic collisions is expected to increase in future years, and it is estimated that road traffic collisions will move from ninth to third place in the global burden of disease ranking, as measured in disability adjusted life years.[2 3]

Measures to reduce traffic speed are considered essential to reducing casualties on the road.[1 4 5] Speed cameras are increasingly used to help to reduce traffic speeds in the belief that this will reduce road traffic collisions and casualties, and an expansion in the use of speed cameras is under way in many countries, most notably the United Kingdom.[6] The use of speed cameras is controversial, however. Vociferous opponents, including some motoring associated organisations, oppose their use, and cameras are often criticised in the media.[7–9] The lack of readily available evidence of the effectiveness of cameras has made it difficult for road safety and health professionals to engage in an informed debate about the effectiveness of speed cameras.

A previous small non-systematic review of six studies found a 17% reduction in collisions after introduction of speed cameras.[10] Non-systematic reviews can, however, be limited by bias. We aimed, therefore, to systematically assess the evidence for the effectiveness of speed cameras in reducing road traffic collisions and related casualties.

Methods

We specified the protocol before undertaking the review, and we made no deviations from the protocol.

Study selection

Controlled trials and observational studies assessing the impact of fixed or mobile speed cameras on any or all of three outcomes (collisions, injuries, and deaths) were eligible for inclusion. We considered all published and unpublished material, with no restrictions on date or language. As the effect of co-intervention is difficult to exclude in interventions such as this, studies that did not have speed cameras as the major intervention were not eligible for inclusion.

Identification of primary studies

We searched the following electronic databases: Medline (1966 to February 2004), Embase (1988 to

An appendix, two tables and a figure are on bmj.com

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February 2004), Cochrane Controlled Trials Register (February 2004), Cochrane Injuries Group Specialised Register (February 2004), Social Science Citation Index (1981 to February 2004), TRANSPORT database (1988 to February 2004), and ZETOC British Library database (February 2004). The Medline and Embase search strategies were of the general structure "Intervention synonyms" AND "Outcome synonyms" AND "Study methodology synonyms" (see appendix on bmj.com for terms used). We translated the Medline search strategy into comparable search strategies for other databases. We then searched the bibliographies of studies identified by electronic searches to identify additional studies. We searched the internet by using the Google search engine (February 2004) (see appendix for terms used). We also searched the websites of road safety and motoring organisations (see appendix for list of websites searched). Key individuals and organisations contacted included every police force in England and Wales, the Faculty of Public Health Transport and Health e-group, several road safety organisations, and key experts in the field.

Data extraction and analysis

One reviewer (PP) searched for studies by using the search strategy outlined earlier and, together with a second reviewer (SK), selected studies to obtain for possible inclusion in the review, on the basis of titles and abstracts (where available). Both reviewers then independently extracted data from each study by using a data extraction form that was piloted before use. We extracted details of the study design, aim of the study, setting of the study and nature of the roads, study period, measurement of exposure, outcome and relevant confounders, and results. We also assessed the quality of the studies with a predefined quality scale, which, in the absence of pre-existing scales, we developed and piloted ourselves. The quality scale rated studies on the basis of representativeness of study areas to general population; control areas being representative of intervention areas; objective and valid outcome(s); results provided with estimates of uncertainty; main conclusions based on primary study hypotheses; and important confounders measured and controlled for. For each of the six quality criteria, we rated the studies on a three point scale (0-2). We rated studies scoring a total of 9-12 as good quality, 6-8 as average quality, and 0-5 as poor quality (see appendix for full data extraction form and quality scale). We combined the results from the data extraction forms of the two reviewers and made decisions on inclusion in the review. We resolved disagreements by consensus.

We also extracted data on the actual number of cases in the intervention and control areas for each time period and where appropriate combined them to produce summary statistics. We calculated risk ratios with confidence intervals for before-after and experimental-control comparisons where possible.

Meta-analysis

Owing to the differing nature of the studies, we decided that meta-analysis would not be appropriate. The camera operations evaluated in the studies differed in terms of the nature of camera operation (types of cameras used, intensity of camera usage, nature of punishments for motorists caught speeding). Studies also used a range of outcome measures to assess camera effectiveness and assessed these outcomes over varying time periods. Risk ratios could not be calculated for five out of 14 studies owing to lack of relevant data. We could not use funnel plot analysis to test for publication bias for the same reasons.

Results

We selected 92 studies to review, on the basis of the title or abstract of the report. After reviewing the full articles, we identified 21 studies that were potentially suitable for inclusion. Of these, two studies did not consider the intervention or outcome of interest,[11 12] one study reported only secondary results without details of the methods,[13] two studies did not look at the effectiveness of the introduction of cameras,[14 15] and two studies were preliminary reports that were updated in later publications.[16 17] After excluding these studies, we included 14 studies in the final review (see figure on bmj.com).

All the studies were observational studies; we found no randomised controlled trials. Five studies had control areas distinct from the areas where the cameras were introduced.[18–22] One study used the same areas at times when cameras were not operating as a control,[23] and eight studies used the same areas before introduction of cameras as the comparison group (before-after studies).[10 24–30] The studies were published between 1992 and 2003. All studies were in high income countries. Six studies assessed the effect of fixed cameras,[10 18–20 29 30] four studied the effect of mobile cameras,[21–23 26] and four studied the effect of a combination of fixed and mobile cameras.[24–28] Outcome measures in the studies were diverse and included various measures of collisions, deaths, and injuries. Three studies had a follow up period of one year following the introduction of cameras,[22 26 29] nine studies had a follow up period of one to three years,[18–21 23–25 27 28] and one study had a follow up period of four years.[10] One study stated only that follow up data of at least one year were used.[30] See table A on bmj.com for details of the studies. In terms of methodological quality, we classified no studies as being good quality, seven as average, two as average-poor, and five as poor.

All studies reported a reduction in road traffic collisions and casualties. The reduction in adverse outcomes in the immediate vicinity of camera sites varied considerably across studies, with ranges of 5-69% for collisions, 12-65% for injuries, and 17-71% for deaths at camera sites. Smaller reductions in adverse outcomes were seen over a wider area. See table B on bmj.com for full results.

Discussion

Research conducted so far consistently shows that speed cameras are an effective intervention in reducing road traffic collisions and related casualties. The level of evidence is relatively poor, however, as most studies did not have satisfactory comparison groups or adequate control for potential confounders.

Strengths and weaknesses of the review

This is the first systematic review on the effectiveness of speed cameras. The main strengths of this review are the thoroughness of the search carried out to find relevant publications and the independent extraction of data by the reviewers.

Despite our best efforts we may not have been able to identify all relevant publications, as road safety research is often published as reports and other forms of grey literature. However, owing to the highly controversial nature of the debate about speed cameras in high income countries, we would expect any published negative studies to be highly publicised.

Although it is plausible that findings could have been withheld from publication, we could not test formally for publication bias because of the varied nature of study designs and outcome measures used. Studies (positive or negative) from low income and middle income countries were notably absent. We are unclear whether this represents a lack of research from such countries or their unavailability in published form. We were unable to pool the results and arrive at a summary estimate owing to the multiplicity of interventions, study designs, and outcomes (often lacking explicit case definitions).

Road safety interventions are often multifaceted. Introduction of speed cameras may have been accompanied by other road safety initiatives such as traffic calming and education campaigns against speed and drink driving. Temporal changes such as improvements in car safety, changes in traffic volume, trends in drink driving, and changes in risk taking behaviour can also influence the frequency of road traffic collisions. Speed cameras are generally introduced at sites identified on the basis of high rates of speed related collisions. However, as a rise in traffic collisions could be due to chance, any subsequent reduction could merely be indicative of normal variation ("regression to the mean").[31] All these factors could result in an underestimate or overestimate of the effectiveness of cameras, and most studies only controlled for a few of these factors, if any.

Implications of the research

This review has highlighted the limited nature of the evidence base underpinning the large scale introduction of speed cameras and the need for further robust evidence. Two possibilities exist for improving this evidence base. Randomised controlled trials offer the highest form of evidence. In countries where a large scale introduction of speed cameras is planned and the subject is not politicised, speed cameras could be introduced in a controlled fashion, randomising the allocation of cameras within a larger sampling framework of high risk sites (with remaining sites serving as controls). However, this approach may not be feasible in most settings because of political and other local pressures. In such settings, an alternative may be to carry out any planned introduction of speed cameras in a phased manner spread over a few years with careful collection of data on collisions and injuries, hence producing a natural comparison group (wedge shaped study design). In either case, the research needs to be conducted as soon as possible, before the widespread introduction of cameras results in a permanent loss of such opportunities.

This review was limited to studying the effectiveness of the introduction of speed cameras in preventing collisions and injuries. Although some evidence exists to suggest that the effectiveness of speed cameras varies according to type of camera (visible or hidden),[14 15] questions remain about how the effectiveness of cameras is affected by location criteria (restricting cameras to collision black spots or not) and use of educational initiatives alongside enforcement. Speed cameras may also change the culture of speeding over a longer period of time. Further research is needed into how these other factors may influence the effectiveness of speed cameras.

What is already known on this topic

Speed cameras are used increasingly as a means of reducing road traffic collisions and related casualties

Opinions about the effectiveness of speed cameras in reducing road traffic collisions and related casualties are conflicting

What this study adds

Existing research consistently shows that speed cameras are an effective intervention in reducing road traffic collisions and related casualties

However, the level of evidence is relatively poor, and most studies lack adequate comparison groups

Controlled introduction of speed cameras with careful data collection is needed to improve the evidence base for the effectiveness of speed cameras

Conclusion

Published research consistently shows the effectiveness of speed cameras in preventing road traffic collisions and injuries. However, the level of evidence is relatively poor, and better data need to be collected to improve the evidence base.

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Contributors: PP and SK developed the protocol. PP searched for studies and, together with SK, selected studies to obtain. PP and SK independently extracted data from each study. PP collated the data and drafted the report, with input from SK at all stages, including the calculation of relative risks. SG, BY, and LT made comments on the draft report. PP and SK finalised the review. PP is the guarantor.

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Rectal artemether versus intravenous quinine for the treatment of cerebral malaria in children in Uganda: randomised clinical trial

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Editorial by Whitty et al and p 347

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Abstract

Objective To compare the efficacy and safety of rectal artemether with intravenous quinine in the treatment of cerebral malaria in children.

Design Randomised, single blind, clinical trial.

Setting Acute care unit at Mulago Hospital, Uganda's national referral and teaching hospital in Kampala.

Participants 103 children aged 6 months to 5 years with cerebral malaria.

Intervention Patients were randomised to either intravenous quinine or rectal artemether for seven days.

Main outcome measures Time to clearance of parasites and fever; time to regaining consciousness, starting oral intake, and sitting unaided; and adverse effects.

Results The difference in parasitological and clinical outcomes between rectal artemether and intravenous quinine did not reach significance (parasite clearance time 54.2 (SD 33.6) hours *v* 55.0 (SD 24.3) hours, P = 0.90; fever clearance time 33.2 (SD 21.9) hours *v* 24.1(SD 18.9 hours, P = 0.08; time to regaining consciousness 30.1 (SD 24.1) hours *v* 22.67 (SD 18.5) hours, P = 0.10; time to starting oral intake 37.9 (SD 27.0) hours *v* 30.3 (SD 21.1) hours, P = 0.14). Mortality was higher in the quinine group than in the artemether group (10/52 *v* 6/51; relative risk 1.29, 95% confidence interval 0.84 to 2.01). No serious immediate adverse effects occurred.

Conclusion Rectal artemether is effective and well tolerated and could be used as treatment for cerebral malaria.

Introduction

The recommended treatment of cerebral malaria is intravenous quinine, but alternative drugs are necessary where intravenous treatment is not possible. Studies comparing rectal artemether with intravenous quinine have been carried out in adults,[1 2 3] but results were variable, and information in children is limited.[3] A single dose of rectal artesunate has been found to be associated with rapid reduction in parasite density in children and adults with moderately severe malaria.[4]

We compared the efficacy and safety of rectal artemether with that of intravenous quinine in the treatment of children, aged 6 months to 5 years, with cerebral malaria.

Methods

This study was carried out in Mulago Hospital, Uganda's national referral and teaching hospital, from July 2002 to February 2003. We recruited patients from the acute care unit and followed them for seven days.

This is the abridged version; the full version is on bmj.com

Safety education of pedestrians for injury prevention (Review)

Duperrex O, Roberts I, Bunn F

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T A B L E   O F   C O N T E N T S

Safety education of pedestrians for injury prevention (Review)

Duperrex O, Roberts I, Bunn F

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ABSTRACT

Background
Each year about one million people die and about 10 million are seriously injured on the world's roads. Educational measures to teach pedestrians how to cope with the traffic environment are considered to be an essential component of any prevention strategy, and pedestrian education has been recommended in many countries. However, as resources available for road safety are limited, a key question concerns the relative effectiveness of different prevention strategies.

Objectives
To quantify the effectiveness of pedestrian safety education programmes in preventing pedestrian-motor vehicle collisions.

Search strategy
We searched the Cochrane Injuries Group Specialised Register, Cochrane Controlled Trials Register, TRANSPORT, MEDLINE, EMBASE, ERIC, PSYCHLIT, SPECTR, and the WHO database on the Internet. We checked reference lists of relevant reviews and papers and contacted experts in the field. Most database searching was conducted in 1999, and updated in May 2003.

Selection criteria
Randomised controlled trials of safety education programmes for pedestrians of all ages.

Data collection and analysis
One reviewer screened records. Two reviewers independently extracted data and assessed methodological quality of trials. Because of differences in the types of interventions and outcome measures used in the trials, meta-analyses were not carried out.

Main results
We found 15 randomised-controlled trials of pedestrian safety education programmes, conducted between 1976 and 1997. The methodological quality of the included trials was generally poor. Allocation concealment was adequate in three trials, outcome assessment was blinded in eight, and in most of the studies large numbers of participants were lost to follow up. Study participants were children in 14 studies and institutionalised adults in one. Eight studies involved direct education of participants, seven used parents as educators. No trials were conducted in a developing country and there were none of pedestrian safety training in the elderly. None of the trials assessed the effect of pedestrian safety education on the occurrence of pedestrian injury, but six assessed the effect on observed behaviour. Some trials showed evidence of behavioural change following pedestrian safety education but it is difficult to predict what effect this might have on pedestrian injury risk.

Authors' conclusions
Pedestrian safety education can result in improvement in children's knowledge and can change observed road crossing behaviour, but whether this reduces the risk of pedestrian motor vehicle collision and injury occurrence is unknown. There is evidence that changes in safety knowledge and observed behaviour decline with time, suggesting that safety education must be repeated at regular intervals.

SYNOPSIS

Pedestrian safety education for children can improve their knowledge and change their road crossing behaviour, but effects on injury are unknown.

A major proportion of the people killed or seriously injured in road traffic crashes are pedestrians, and children are particularly vulnerable. Education programmes try to teach people how to cope with the road environment. Parents are sometimes used as educators. The review of trials (mostly in children) found that pedestrian safety education can improve children's road safety knowledge and their observed road crossing behaviour. Education may need to be repeated at regular intervals, as the effect can decline with time. However, whether these changes to knowledge or behaviour can be linked to a reduction in pedestrian deaths and injuries is unknown.

BACKGROUND

Road traffic crashes are now the leading cause of death and disablement for people aged 3 to 35 years, killing each year about one million people and seriously injuring about 10 million people (Murray 1996). The global economic burden of road traffic crashes is estimated at US$500 billion (World Bank 2001). Most of the casualties are in low and middle-income countries (LMIC), and most are vulnerable road users: pedestrians, cyclists and riders of motorised two wheelers. Most of pedestrian casualties are children and elderly (Murray 1996; Rivara 1990; Barss 1998).

In the prevention of pedestrian injuries, educational measures to teach pedestrians how to cope with the traffic environment are considered to be an essential component of any prevention strategy and pedestrian education has been recommended in high, middle and low-income countries (World Bank 2001). Because the resources available for road safety are limited, a key question for road safety policy concerns the relative cost-effectiveness of different prevention strategies. Resources allocated to pedestrian education programmes become unavailable for other prevention strategies, such as environmental strategies. A New Zealand study estimated that if the same amount of resources that were allocated to pedestrian safety education were allocated to traffic calming, on the basis of estimates of the effectiveness of traffic calming there would be 18 fewer child pedestrian hospitalisations in New Zealand each year (Roberts 1994). Several reviews have been carried out on injury prevention topics including pedestrian-motor vehicle collisions (Avery 1982; Berger 1975; Dowswell 1996; Ehrlich 1982; Forjuoh 1996; Malek 1990; Munro 1995; OECD 1983; Phinney 1985; Rivara 1990; Smith 1983; Towner 1996; Tripp 1938; Wazana 1997). However, these reviews included both randomised and non-randomised trials and may have missed unpublished trials and trials reported in languages other than English. The aim of this systematic review of randomised controlled trials was to quantify the effectiveness of pedestrian education programmes in improving pedestrian knowledge, attitudes and behaviour, and most importantly, in preventing pedestrian-motor vehicle collisions.(

OBJECTIVES

- To quantify the effectiveness of pedestrian education programmes in preventing pedestrian-motor vehicle collisions.
- To quantify the effectiveness of pedestrian education programmes in changing behaviour, attitude and knowledge of pedestrians.

CRITERIA FOR CONSIDERING STUDIES FOR THIS REVIEW

Types of studies

Randomised controlled trials.

Types of participants

Pedestrians of all ages.

Types of intervention

Pedestrian safety education programmes. Community-based interventions such as media awareness campaigns and parental education programmes were also included. Studies of education aimed at modifying the behaviour of drivers towards pedestrians were not included.

Type of comparison of interventions in eligible studies:

- pedestrian safety education vs no intervention;
- pedestrian safety education vs intervention A;
- pedestrian safety education + intervention A vs intervention A;
- pedestrian safety education + intervention A vs intervention A + intervention B.

Intervention A and B can be an educational intervention unrelated to the prevention of pedestrian injuries; for example, home safety education or a pedestrian safety intervention that does not involve pedestrian education, such as traffic calming.

Studies where pedestrian safety education is confounded by another intervention were not included e.g. pedestrian safety education + intervention A vs intervention B.

Types of outcome measures

- Pedestrian injury (fatal and non-fatal).
- Pedestrian-motor vehicle collisions.
- Behaviour, attitude and knowledge of pedestrians.

SEARCH STRATEGY FOR IDENTIFICATION OF STUDIES

See: Injuries Group search strategy

Searches were conducted on transport, educational and medical databases.

The following general search strategy was used for all databases: (pedestrian OR synonyms) AND (education OR synonyms) AND (traffic OR road OR crossing OR safety OR injury OR accident OR synonyms). When appropriate MESH terms (or equivalent) and free text with truncation were used, and searches were restricted to title, abstract and keywords fields. Searches were also conducted with keywords translated into French, German, Italian, Spanish, Dutch, Danish. Details are in Additional Table 01.

The following electronic databases were searched:

- Cochrane Injuries Group's specialised register (see Review Group's details for more information);
- Cochrane Controlled Trials Register (2000, issue #4);
- TRANSPORT (1968-11/98) which includes 3 databases from the Transportation Research Board (Transport Research Information Services - TRIS), from the Organisation for Economic Co-operation and Development (International Road Research Documentation - IRRD) and from the European Conference of Ministers of Transport (TRANSDOC);
- MEDLINE (1966-5/99);
- EMBASE (1980-1/99);
- ERIC (1992-9/98);
- PSYCLIT (1898-12/1998);
- SPECTR (7/2000);
- WHO database available on Internet (1/2001).

The search was updated in May 2003.

Further potential trials were identified by checking the reference lists of relevant reviews, books and articles, contacting authors of relevant papers, use of the citation analysis facility of SCI and SSCI, contacting professionals, organisations and voluntary agencies with an interest in road safety. The JANCOC (Japanese informal network for the Cochrane Collaboration) mailing list and some Japanese specialists were contacted by e-mail or letter. The protocol for this review was presented at the 33 rd session of the UN Working Party on Road Traffic Safety (1/10/1999) in Geneva. The United Nations Economic Commission for Europe Working Party on Road Traffic Safety brings together the governmental road safety organisations of 55 member countries throughout Europe. Working Party officials were asked to provide information on any published or unpublished controlled trials of pedestrian safety education that were available to them.

METHODS OF THE REVIEW

Selection of trials
All records identified by searching electronic databases were screened for eligibility by one reviewer (OD) and the full text of all potentially eligible studies were obtained for assessment. Two reviewers (OD, FB) independently extracted data on injuries, pedestrian—motor vehicle collisions, behaviour, attitude and knowledge, methods of randomisation and numbers lost to follow up. Disagreement was resolved by discussion with a third reviewer (IR).

Assessment of methodological quality
Since there is evidence that the quality of allocation concealment particularly affects the results of studies (Schulz 1995), two reviewers (OD, FB) scored this quality on the scale used by Schulz (Schulz 1995) as shown below, assigning C to poorest quality and A to best quality:

- A: trials deemed to have taken adequate measures to conceal allocation (i.e. central randomisation; numbered or coded bottles or containers; drugs prepared by the pharmacy; serially numbered, opaque, sealed envelopes; or other description that contained elements convincing of concealment);
- B: trials in which the authors either did not report an allocation concealment approach at all or reported an approach that did not fall into one of the other categories;
- C: trials in which concealment was inadequate (such as alternation or reference to case record numbers or to dates of birth).

Where the method used to conceal allocation was not clearly reported, the author was contacted, if possible, for clarification. We then compared the scores allocated and resolved differences by discussion.

When assessing trial quality, the reviewers were not blinded to the names of the authors, institutions, journal of publication or results of the trials, because evidence for the value of this is inconclusive (Berlin 1997).

Wherever possible, an intention-to-treat analysis was performed. Because of the differences in the types of interventions and the types of outcomes meta-analysis was not considered appropriate. For all studies, we report the results provided by the authors in the text and used the METAVIEW facility in RevMan to show the results of each individual trial graphically. The outcomes are expressed as positive expected behaviour, attitude or knowledge. For dichotomous outcomes, relative risks (RR) and risk differences (RD) with their 95% confidence intervals (CIs) were calculated using RevMan version 4.1. For continuous outcomes, results are reported as standardised mean difference (SMD) and weighted mean difference (WMD) with their 95% confidence intervals (CI). If the variance for the change score was not presented and could not be obtained from the authors, this was imputed using a correlation factor between pre- and post-test of r=0.50 (Mulrow 1997; Follmann 1992). In the graphical presentation of results, we report the post-test data and the change between pre-test and post-test whenever possible, grouped by age groups and by type of outcomes (behaviour, attitude, knowledge).

For cluster randomised trials, an effective sample size was calculated based on the inter-cluster coefficient if this was available (Donner 1993). Studies in which there were less than five randomised clusters were excluded, because of the interpretational difficulties caused by the total confounding of two sources of variation: the variation in response due to the effect of intervention, and natural variation that exists between the clusters even in the absence of an intervention effect. Measuring and adjusting for baseline differences can help reduce such confounding, but the inherent problem that such trials can only be analysed at the level of the individual remains. To analyse at the individual level, one would have to assume that there was no clustering of individual responses within the community, which is almost always untenable (Donner 2000).

We were unable to conduct the planned examination of the impact of small study bias by conducting funnel plots and using statistical tests for funnel plot asymmetry (Egger 1997). The following hypotheses were specified a priori as factors that might explain heterogeneity between the results of the included trials but were not explored:

- participants: children versus adults;
- setting: high-income versus low and middle income countries;
- trial quality: adequately versus inadequately concealed.

DESCRIPTION OF STUDIES

Of the 13,899 published and unpublished studies identified by our original search strategies, 674 (5%) were potentially relevant, based on title or abstract. After full text review, 15 trials met our inclusion criteria (Ampofo-Boateng 1993; Bouck 1992; Cross 1988; Downing 1981; Limbourg 1981; Luria 2000; Matson 1980; Miller 1982; Nishioka 1991; Renaud 1989; Singh 1979; Thomson 1992; Thomson 1997a; Thomson 1997b; Thomson 1998).

The study participants were children in 14 studies and institutionalised adults in one (Matson 1980). Eight studies involved the direct education of study participants (Ampofo-Boateng 1993; Cross 1988; Luria 2000; Matson 1980; Nishioka 1991; Renaud 1989; Thomson 1992; Thomson 1998), seven involved the use of parents (Downing 1981; Limbourg 1981; Thomson 1997a; Thomson 1997b) or teachers (Bouck 1992; Miller 1982; Singh 1979) as educators. Pedestrian safety education was given at home in two studies (Downing 1981; Limbourg 1981), in the classroom in four studies (Cross 1988; Miller 1982; Renaud 1989; Singh 1979), in a semi-real traffic environment in one study (Nishioka 1991), in the classroom and a semi-real traffic environment in three studies (Bouck 1992; Luria 2000; Matson 1980), and in the classroom and the real traffic environment in five studies (Ampofo-Boateng 1993; Thomson 1992; Thomson 1997a; Thomson 1997b; Thomson 1998). The outcomes were measured both before and after the intervention in 12 studies (Ampofo-Boateng 1993; Cross 1988; Downing 1981; Limbourg 1981; Luria 2000; Matson 1980; Miller 1982; Singh 1979; Thomson 1992; Thomson 1997a; Thomson 1997b; Thomson 1998) and after intervention in three studies (Bouck 1992; Nishioka 1991; Renaud 1989).

We excluded one randomised controlled trial (RCT) because the number of clusters was less than five (Cross D 2000), two RCTs because they compared pedestrian safety education methods without control group (Dueker 1975; McKelvey 1978), two RCTs without a pedestrian education component (Kelly 1987; Stuy 1993) and 35 studies involving a control group but without a random allocation process (details available in Characteristics of excluded studies). Two of the excluded studies provided data on accident/injury rates (Schioldborg 1976; Ytterstad 1995) but did not use a random allocation.

METHODOLOGICAL QUALITY

The methodological quality of the included trials was generally poor. The method of allocation concealment was adequate in only three trials (Downing 1981, Miller 1982, Nishioka 1991), outcome assessment was blinded in eight (Ampofo-Boateng 1993, Cross 1988, Limbourg 1981, Luria 2000, Matson 1980, Thomson 1992, Thomson 1997a, Thomson 1998) and in most of the studies large numbers of participants were lost to follow up.

Ampofo-Boateng 1993 (some information obtained from author): Study participants were individually allocated from the class register in alphabetic order (with separate lists for boys and girls) to intervention or control groups by alternation. Outcome assessment was blind to intervention allocation. Loss to follow-up was 37.5% for the intervention groups. Inter-rater reliability was 0.89 for coding.

Bouck 1992 (some information obtained from author): Children were selected to participate in the study from their class lists by drawing lots from a basket. Then the selected children were allocated to intervention or control groups by again drawing lots from a basket . Outcome assessment was blind to intervention allocation. Loss to follow up was 20% in each group.

Cross 1988 (some information obtained from author): Children were allocated to a class by drawing lots from a hat. Then the classes were allocated to intervention or control groups by again drawing lots from a hat. No information was available on blinding of outcome assessment and loss to follow up.

Downing 1981 (some information obtained from author): Children were individually allocated to intervention or control groups by an on-site computer system. Outcome assessment was not blind to intervention allocation. Overall loss to follow-up was 44%, mostly because children had moved out of the area.

Limbourg 1981 (some information obtained from author): Schools were allocated by block randomisation within groups of four similar schools (matched for age, sex, parental social status and urban characteristics of living area). Paper cards were drawn from an envelope to allocate blocks to intervention or control groups. Outcome assessment was blind to intervention allocation. Overall loss to follow up was 15%.

Luria 2000 (some information obtained from author): Schools were allocated to intervention or control groups by identical folded pieces of paper drawn from a container. In each school, one class of 25 children was selected by randomisation to participate in the evaluation study. Outcome assessment was not blind to intervention allocation. Loss to follow-up was 26% for both groups, mostly because children were absent on post-test day or had moved out of the area.

Matson 1980: Triplets of individuals (matched for age, IQ, adaptive behaviour and deficits in pedestrian behaviour) were allocated by block randomisation. Outcome assessment was blind to intervention allocation. No information available for loss to follow-up.

Miller 1982 (some information obtained from author): Classrooms were allocated by a list of random numbers read by someone not involved in the trial (closed list). No information was available on blinding of outcome assessment. Overall loss to follow-up was 6% for knowledge test, 65% and 77% for reported behaviour (two questionnaires).

Nishioka 1991 (some information obtained from author): Triplets of individuals (matched for age, sex and class) were allocated by block randomisation using a table of random numbers. Outcome assessors were not blinded. Loss to follow up was 10%.

Renaud 1989 (some information obtained from author): Study participants were randomly allocated by alternation. Outcome assessment was not blind to intervention allocation. Author confirmed there was no loss to follow-up.

Singh 1979 (some information obtained from author): Study participants were randomly allocated to intervention or control groups by classroom. The method of randomisation and allocation concealment is not described. Outcome assessment was not carried out by the teachers who administered the intervention but by an interviewing team from the study organisers. It is not stated whether interviewers were blinded to study group. Number of classes lost to follow-up: in the intervention group two refused and seven did not complete (7/106=6.6%), and in the control group 33 refused but all the others completed.

Thomson 1992, Thomson 1997a, Thomson 1997b, Thomson 1998 (some information obtained from author): Study participants were individually allocated to intervention or control groups by alternation from the class register in alphabetic order (with separate lists for boys and girls). Outcome assessment was blind to intervention allocation. Author confirmed there was no loss to follow-up in any of the studies.

RESULTS

Results are presented in a narrative form below as well as graphically in METAVIEW. The outcomes are expressed as positive expected behaviour, attitude or knowledge. For the graphical presentation of the results, we report the post-test data for dichotomous and continuous outcomes and ,whenever possible, the change between pre-test and post-test for continuous outcomes, often after imputation of the variance of the change between pre and post-testing (Mulrow 1997, Follmann 1992).

Overall, the effect of safety education of pedestrians on behaviour varied considerably. The relative probability of trained pedestrians behaving correctly compared to non-trained ones ranged between 0.49 and 9.29 depending on the study and the outcome measured. Safety education of pedestrians improved the attitude / intentions with an effect ranging from a standardised mean difference of 0.17 to 1.48. Knowledge about road safety increased more in the trained groups than the non-trained ones when outcomes were measured before and after (standardised mean difference from 0.16 to 1.01), but for dichotomous outcomes the range of effect is wide (relative risk ranging from 0.72 to 1.66).

The observed behaviour of 3 to 4 years old children can be improved by indirect education, although the importance of the effect varies considerably depending on the outcome chosen and on the conditions of observation [Comparison Table 02/02]. With time, the potential benefit of indirect education seems to diminish [Comparison Table 02/03]. No information is available for effect on behaviour of direct education in this age group.

For children aged 5 to 7, the immediate (less than 1 month) evaluations show that their observed behaviour can be improved by direct as well as by indirect education, although the importance of the effect varies considerably depending on the outcome chosen and on the conditions of observation [Comparison Tables 01/03 & 02/03]. With time, the potential benefit of indirect education diminishes [Comparison Tables 02/05 & 02/06]. No information is available for long term effect on behaviour of direct education in this age group.

Direct and indirect education might have some positive impact on attitude of 7 to 9 years old pedestrian [Comparison Table 01/13]. No information is available for effect on behaviour of education in this age group.

Impact of educational programs on knowledge of children pedestrian is inconsistent across studies [Comparison Tables 01/22 & 01/26 & 02/20 to 02/29].

Details of indicial studies results are presented below.

Ampofo-Boateng 1993 assessed children's ability to choose a safe route for crossing the road. They reported the mean proportion of routes falling into different safety categories. Children were tested before training (PT), immediately after training (PT1), nine weeks after training (PT2) and eight months after training (PT3). For children trained in a real traffic environment, the proportion of chosen routes (standard deviations in brackets) classified as safe was: PT = 0.13 (0.20), PT1 = 0.72 (0.28), PT2 = 0.50 (0.36). For children trained with the tabletop model, the proportion of chosen routes (standard deviations in brackets) classified as safe was: PT = 0.07 (0.16), PT1 = 0.70 (0.30), PT2 = 0.54 (0.34). Children in the control group were only tested once, eight months after training. At eight months, the mean proportion of chosen routes classified as safe was 0.38 (0.23) in the trained groups (real and tabletop groups together) compared with 0.12 (0.15) in the control group – WMD 0.26 (95%CI: 0.09 to 0.43) (Table of Comparisons 01/12). In the trained group, the mean proportion of chosen routes classified as safe declined over time.

- [Table 01/12] - post-test 2:

Post-test proportion of routes categorised as Safe - (Table top & roadside training) versus No training: SMD 1.28 (0.30 to 2.26) ; WMD 0.26 (0.09 to 0.43)

Bouck 1992 reported the mean score (standard deviation) of a knowledge test conducted immediately after the intervention. The mean knowledge score was 83.3 (10.6) in the intervention group and 35.7 (25.3) in the control group.

- [Table 01/24] - Knowledge of 8 to 11 years old - post-test at less than 1 month

Post-test score of "Conspicuity, mass, speed and control" test (maximum score 100): SMD 2.39 (1.46 to 3.33) ; WMD 47.60 (34.16 to 61.04)

Cross 1988 reported the percentage of children with the correct understanding of the concept of speed before and after training for four set tasks.

- Task 1: In the intervention group (n=69), the proportion giving the wrong answer decreased from 78% (54) to 25% (17) between pre and post-testing whereas in the control group (n=69), the proportion giving the wrong answer decreased from 80% (55) to 62% (43) between pre and post-testing.
- Task 2: In the intervention group (n=69), the proportion giving the wrong answer decreased from 36% (25) to 6% (4) between pre and post-testing whereas in the control group (n=69), the proportion giving the wrong answer decreased from 29% (20) to 9% (6) following testing.
- Task 3: In the intervention group (n=69), the proportion giving the wrong answer decreased from 54% (37) to 10% (7) between pre and post-testing whereas in the control group (n=69) the proportion giving the wrong answer decreased from 47% (33) to 35% (24) following testing.
- Task 4: In the intervention group (n=69), the proportion giving the wrong answer decreased from 27% (19) to 0% (0) between pre and post-testing whereas in the control group (n=69), the proportion giving the wrong answer decreased from 20% (14) to 7% (5) following testing.

- [Table 01/13]: Apply "concept of speed": RR 1.27 (1.07 to 1.50) ; RD 0.19 (0.06 to 0.32).

Downing 1981 reported the percentage of three year old children correctly answering 18 questions relating to simple traffic knowledge. The results were stratified according to whether or not the families were aware that they were taking part in an experiment. In the groups that were aware that they were taking part in an experiment, baseline information was collected from mothers and children during interviews. In the group that were unaware that they were taking part in an experiment no baseline information was collected.

In families that were aware they were taking part in an experiment:
(a) when children were tested on simple traffic knowledge, the group that obtained the booklet showed greater improvement for 14 of the 18 items tested; the average increase (pre-post) in the percentage of children giving correct replies was 13% per item in the intervention group compared with an average improvement of 8% per item in the comparison group;
(b) when children were tested on road safety rules both groups showed improvement for six of the seven items tested, without significant difference in the amount of improvement; the average increase (pre-post) in the percentage of children giving correct replies was 11% per item in the intervention group compared with an average improvement of 13% per item in the comparison group.

In families that were unaware they were taking part in an experiment:
(a) when children were tested on simple traffic knowledge, the group that obtained the booklet performed better than the group that did not for 12 of the 18 items tested, but there was no difference on three items and the booklet group performed worse

than the control group on four items. The average advantage of the intervention group over the control group was about 3% per item;
(b) when children were tested on road safety rules, the group that received the booklet performed better than the group that did not on four of the seven items tested. The difference, however, was small, averaging about 3% per item.

- [Table 02/20] - Knowledge of 3 years old - post-test at 1 to 3 months

1 Hold hands - Road safety booklet after an interview versus Interview only: RR 0.72 (0.52 to 0.99) ; RD -0.13 (-0.26 to 0.00
2 Hold hands - Road safety booklet with a letter versus No intervention: RR 1.13 (0.90 to 1.43) ; RD 0.04 (-0.03 to 0.12
3 Walk / stay on pavement - Road safety booklet after an interview versus Interview only: RR 0.74 (0.44 to 1.24) ; RD -0.06 (-0.17 to 0.04
4 Walk / stay on pavement - Road safety booklet with a letter versus No intervention: RR 1.24 (0.87 to 1.76) ; RD 0.04 (-0.02 to 0.10
5 Look / watch out for cars - Road safety booklet after an interview versus Interview only: RR 1.66 (0.78 to 3.57) ; RD 0.06 (-0.03 to 0.14
6 Look / watch out for cars - Road safety booklet with a letter versus No intervention: RR 1.33 (0.82 to 2.18) ; RD 0.03 (-0.02 to 0.08

Limbourg 1981 reported the change in the proportion of children who were observed to stop at the kerb and to look right and left before crossing. The results were stratified according to whether or not children were deliberately distracted by the investigators at the time of road crossing. Children were divided into the following four groups: Group one (behavioural training by parent with psychologist supervision), Group two (behavioural training by parent without psychologist supervision), Group three (simple road safety information given to parents), Group four (behavioural training by parent with psychologist supervision). The percentage of children who behaved adequately was given for intervention (Groups one and two) and for control groups (Groups three and four).

The proportion of children aged 3 to 4 years old who stopped at kerb without being distracted were PT=4%, PT1=83%, PT2=20% for the intervention group and PT=5%, PT1=43%, PT2=15% for the control group.
The proportion of children aged 3 to 4 years old who stopped at kerb whilst being distracted were PT=8%, PT1=76%, PT2a=15%, PT2b=19% for the intervention group and PT=6%, PT1=8%, PT2a=8%, PT2b=9% for the control group.

- [Table 02/01] - Behaviour (observed) of 3 to 4 year olds - post-test at 1 to 3 months

1 Stop at kerb - no distraction: RR 1.96 (1.48 to 2.59) ; RD 0.41 (0.28 to 0.54)
2 Stop at kerb - distraction (competition): RR 9.29 (4.28 to 20.12) ; RD 0.68 (0.58 to 0.78)
3 Stop at kerb - distraction (alone): -
4 Stop at line of vision - no distraction: RR 2.00 (1.24 to 3.23) ; RD 0.22 (0.09 to 0.35)
5 Stop at line of vision - distraction (competition): RR 5.12 (1.89 to 13.88) ; RD 0.23 (0.13 to 0.32)
6 Stop at line of vision - distraction (alone): -
7 Stop & look at kerb - no distraction: RR 1.19 (0.82 to 1.71) ; RD 0.07 (-0.07 to 0.21)
8 Stop & look at kerb - distraction (competition): RR 3.84 (1.39 to 10.62) ; RD 0.16 (0.06 to 0.25)
9 Stop & look at kerb - distraction (alone): -
10 Stop & look at line of vision - no distraction: RR 1.44 (0.86 to 2.40) ; RD 0.10 (-0.03 to 0.22)
11 Stop & look at line of vision - distraction (competition): RR 4.70 (1.46 to 15.13) ; RD 0.15 (0.07 to 0.24)
12 Stop & look at line of vision - distraction (alone): -

- [Table 02/02] - Behaviour (observed) of 3 to 4 year olds - post-test at 4 to 6 months

1 Stop at kerb - no distraction: RR 1.27 (0.64 to 2.52) ; RD 0.04 (-0.08 to 0.16)
2 Stop at kerb - distraction (competition): RR 1.79 (0.73 to 4.43) ; RD 0.07 (-0.03 to 0.17)
3 Stop at kerb - distraction (alone): RR 2.20 (0.92 to 5.30) ; RD 0.10 (0.00 to 0.21)
4 Stop at line of vision - no distraction: RR 1.07 (0.45 to 2.52) ; RD 0.01 (-0.09 to 0.11)
5 Stop at line of vision - distraction (competition): RR 1.92 (0.38 to 9.62) ; RD 0.03 (-0.03 to 0.09)
6 Stop at line of vision - distraction (alone): RR 1.40 (0.49 to 3.99) ; RD 0.03 (0.06 to 0.12)
7 Stop & look at kerb - no distraction: RR 0.99 (0.48 to 2.04) ; RD 0.00 (-0.12 to 0.11)
8 Stop & look at kerb - distraction (competition): RR 0.90 (0.32 to 2.55) ; RD -0.01 (-0.09 to 0.08)
9 Stop & look at kerb - distraction (alone): RR 1.17 (0.44 to 3.12) ; RD 0.01 (-0.08 to 0.10)
10 Stop & look at line of vision - no distraction: RR 1.71 (0.62 to 4.70) ; RD 0.05 (-0.04 to 0.14)
11 Stop & look at line of vision - distraction (competition): RR 0.77 (0.20 to 2.97) ; RD -0.01 (-0.08 to 0.06)
12 Stop & look at line of vision - distraction (alone): RR 0.49 (0.17 to 1.42) ; RD -0.06 (-0.15 to 0.03)

The proportion of children aged 5 to 6 years old who stopped at kerb without being distracted were PT=14%, PT1=82%, PT2=31% for the intervention group and PT=9%, PT1=56%, PT2=29% for the control group.
The proportion of children aged 5 to 6 years old who stopped at kerb whilst being distracted were PT=16%, PT1=80%,

PT2a=21%, PT2b=28% for the intervention group and PT=11%, PT1=13%, PT2a=10%, PT2b=17% for the control group.

- [Table 02/05] - Behaviour (observed) of 5 to 7 year olds - post-test at 1 to 3 months

1 Stop at kerb - no distraction: RR 1.47 (1.27 to 1.70) ; RD 0.26 (0.17 to 0.35)
2 Stop at kerb - distraction (competition): RR 6.21 (4.24 to 9.09) ; RD 0.67 (0.59 to 0.75)
3 Stop at kerb - distraction (alone): -
4 Stop at line of vision - no distraction: RR 1.54 (1.26 to 1.88) ; RD 0.23 (0.13 to 0.33)
5 Stop at line of vision - distraction (competition): RR 5.10 (3.30 to 7.89) ; RD 0.44 (0.35 to 0.53)
6 Stop at line of vision - distraction (alone): -
7 Stop & look at kerb - no distraction: RR 1.04 (0.85 to 1.27) ; RD 0.02 (-0.09 to 0.12)
8 Stop & look at kerb - distraction (competition): RR 4.65 (2.99 to 7.23) ; RD 0.39 (0.30 to 0.48)
9 Stop & look at kerb - distraction (alone): -
10 Stop & look at line of vision - no distraction: RR 1.40 (1.15 to 1.70) ; RD 0.18 (0.08 to 0.28)
11 Stop & look at line of vision - distraction (competition): RR 3.98 (2.54 to 6.24) ; RD 0.32 (0.23 to 0.41)
12 Stop & look at line of vision - distraction (alone): -

- [Table 02/06] - Behaviour (observed) of 5 to 7 year olds - post-test at 4 to 6 months

1 Stop at kerb - no distraction: RR 1.09 (0.78 to 1.53) ; RD 0.03 (-0.08 to 0.13)
2 Stop at kerb - distraction (competition): RR 2.04 (1.18 to 3.54) ; RD 0.11 (0.03 to 0.19)
3 Stop at kerb - distraction (alone): RR 1.67 (1.09 to 2.55) ; RD 0.11 (0.02 to 0.20)
4 Stop at line of vision - no distraction: RR 2.84 (1.78 to 4.53) ; RD 0.22 (0.13 to 0.31)
5 Stop at line of vision - distraction (competition): RR (1.70 to 6.64) ; RD 0.14 (0.07 to 0.22)
6 Stop at line of vision - distraction (alone): RR 2.00 (1.30 to 3.10) ; RD 0.15 (0.06 to 0.24)
7 Stop & look at kerb - no distraction: RR 1.17 (0.81 to 1.67) ; RD 0.04 (-0.06 to 0.14)
8 Stop & look at kerb - distraction (competition): RR 2.41 (1.30 to 4.49) ; RD 0.11 (0.04 to 0.18)
9 Stop & look at kerb - distraction (alone): RR 1.56 (0.98 to 2.48) ; RD 0.13 (0.04 to 0.23)
10 Stop & look at line of vision - no distraction: RR 1.79 (1.18 to 2.72) ; RD 0.13 (0.04 to 0.23)
11 Stop & look at line of vision - distraction (competition): RR 2.80 (1.39 to 5.64) ; RD 0.11 (0.04 to 0.18)
12 Stop & look at line of vision - distraction (alone): RR 1.62 (0.99 to 2.66) ; RD 0.08 (0.00 to 0.16)

Luria 2000 assessed the change in children's knowledge on how to cross the street (maximum score 16 points).
The intervention group improved their mean score from 4.31 to 6.21 and control group from 4.27 to 5.63.

- [Table 01/22] - Post-test score of "Crossing the street" test (maximum score 16): SMD 0.23 (-0.07 to 0.52) ; WMD 0.58 (-0.16 to 1.32)

The mean change (standard deviation) between pre- and post-tests was 1.9 (2.7) for intervention group and 1.4 (3.5) for control group.

- [Table 01/26] - Change in score of "Crossing the street" test (maximum score 16): SMD 0.16 (-0.13 to 0.45) ; WMD 0.50 (-0.41 to 1.41)

Matson 1980 examined the effect of pedestrian skills training in 30 'mentally retarded' institutionalised adults. Study participants were randomly allocated to one of three groups. These were individualised classroom training involving the practice of correct pedestrian behaviour using movable figures on a scale model of an intersection (n=10), independence training using a mock up of an intersection on the hospital grounds (n=10) and a control group receiving a non-road safety educational intervention (n=10). Assessment was carried out at a city intersection before and after the three month treatment period. Each trial participant was asked to perform a target behaviour which was graded on the basis of whether or not the target behaviours were performed correctly and if they were in the proper sequence. Prior to the intervention, the percentage of the steps performed correctly for the classroom training, independence training and control groups were 34%, 25% and 17% respectively. Following the intervention, the percentage of the steps performed correctly for the scale figure taught, hospital intersection taught and control groups were 77%, 90% and 16%. These figures were obtained from a graph.

- [Table 01/08] - Post-test mean proportion of steps correct at city intersection (13 steps/person * 10 persons = 130 steps):

classroom versus control: RR 1.91 (1.23 to 2.98) ; RD 0.16 (0.06 to 0.27)
"independence" versus control: RR 1.43 (0.89 to 2.30) ; RD 0.08 (-0.02 to 0.18)

Miller 1982 reported changes in safety knowledge and parentally reported safety behaviour in 550 second grade students in a cluster randomised controlled trial of the Beltman traffic safety programme. Teachers were randomised to one of three groups: Beltman traffic safety training, Beltman traffic safety training with two booster lessons four months following training and control group. Children's safety knowledge was assessed in a 20-item multiple choice test. Prior to the intervention the mean test scores (standard deviation) in the three groups were 13.22 (3.06) in the Beltman group, and 13.40 (3.11) in the Beltman with Booster group and 13.74 (3.21) in the control group. Six months following the intervention the scores were 18.06 (1.92), 18.27 (1.74) and 16.31

(2.58) respectively. Children's out of school safety behaviour was assessed by parental questionnaire. The response rate to the questionnaires was only 30%. It appears from the published reports that the cluster nature of the trial was not taken into account in the analyses.

- [Table 02/07] - Behaviour (reported) of 5 to 7 years old - post-test at 4 to 6 months

Always cross in crosswalks according to parents:
- Beltman program versus No training: RR 2.26 (1.20 to 4.24) ; RD 0.29 (0.08 to 0.49)
- (Beltman+Booster) versus No training: RR 1.18 (0.59 to 2.40) ; RD 0.04 (-0.13 to 0.21)
Always look before crossing according to parents:
- Beltman program versus No training: RR 1.40 (0.87 to 2.25) ; RD 0.15 (-0.06 to 0.37)
- (Beltman+Booster) versus No training: RR 1.74 (1.15 to 2.65) ; RD 0.29 (0.09 to 0.48)

- [Table 02/21] - Knowledge of 5 to 7 years old - post-test at less than 1 month

Post-test score of "Traffic safety knowledge" test (maximum score 20):
- Beltman program versus No training: SMD 0.97 (0.75 to 1.20) ; WMD 2.36 (1.86 to 2.86)
- (Beltman+Booster) versus No training: SMD 1.05 (0.83 to 1.27) ; WMD 2.59 (2.08 to 3.10)

- [Table 02/22] - Knowledge of 5 to 7 years old - post-test at 4 to 6 months

Post-test score of "Traffic safety knowledge" test (maximum score 20):
- Beltman program versus No training: SMD 0.76 (0.54 to 0.90) ; WMD 1.75 (1.27 to 2.23)
- (Beltman+Booster) versus No training: SMD 0.89 (0.67 to 1.11) ; WMD 1.96 (1.50 to 2.42)

- [Table 02/26] - Knowledge (change) of 5 to 7 years old - post-test at less than 1 month

Change in score of "Traffic safety knowledge" test (maximum score 20):
- Beltman program versus No training: SMD 1.00 (0.78 to 1.23) ; WMD 2.88 (2.28 to 3.48)
- (Beltman+Booster) versus No training: SMD 1.01 (0.80 to 1.23) ; WMD 2.93 (2.34 to 3.52)

- [Table 02/27] - Knowledge (change) of 5 to 7 years old - post-test at 4 to 6 months

Change in score of "Traffic safety knowledge" test (maximum score 20):
- Beltman program versus No training: SMD 0.80 (0.58 to 1.02) ; WMD 2.27 (1.68 to 2.86)
- (Beltman+Booster) versus No training: SMD 0.81 (0.60 to 1.02) ; WMD 2.30 (1.73 to 2.87)

Nishioka 1991 examined the effect of verbal instructions on the safety behaviour of children in a simulated traffic environment. The children were divided into three groups: one group received a caution advising how to behave safely, one was given a simple caution and one was given no caution. The children's behaviour was classified as safe if they both changed the speed of walking or running and looked to the right and left. The children's behaviour was classified as unsafe if there was no safety response, if they only changed speed or if they only looked. The percentages of children with safe behaviour in each group was 61% (detailed caution), 46% (simple caution) and 25% (no caution).

- [Table 01/03]

Safe behaviour - Detailed caution vs No caution: RR 2.43 (1.13 to 5.24) ; RD 0.36 (0.09 to 0.62)
Safe behaviour - Simple caution vs No caution: RR 1.83 (0.81 to 4.15) ; RD 0.21 (-0.06 to 0.47)

Renaud 1989 reported attitudes to pedestrian injury risk in children allocated to one of three traffic safety simulation games or to a control group.
The transfer of children's learning from the simulation game was measured by observing children's reaction to a quasi-real life model of traffic risks set up in the gymnasium. Once again the three intervention groups achieved higher mean scores on the transfer of learning test than the control group (attitude simulation game 8.7 (3.1); behaviour simulation game 10.4 (2.1); attitude and behaviour simulation game 10.1 (2.3); control 7.9 (3.7)).

- [Table 01/04]

Post-test Transfer Score (maximum score 31) - Attitude game versus No training: SMD 0.23 (-0.26 to 0.73) ; WMD 0.80 (-0.89 to 2.49)
Post-test Transfer Score (maximum score 31) - Behaviour game versus No training: SMD 0.83 (0.31 to 1.35) ; WMD 2.50 (0.99 to 4.01)
Post-test Transfer Score (maximum score 31) - Attitude&Behaviour game versus No training: SMD 0.71 (0.20 to 1.22); WMD 2.20 (0.66 to 3.74)

Children's intended behaviour was assessed by showing children a picture of a road, asking a series of questions and the children would use stickers to answer the questions. The means of the behaviour tests were slightly higher for each of the intervention groups than the control group (attitude simulation game 4.9 (0.2); behaviour simulation game 4.3 (0.8); attitude and behaviour simulation game 4.5 (0.7); control 4.0 (1.1)).

- [Table 01/10]

Post-test Behaviour Score (maximum score 5) - Attitude game versus No training: SMD 1.13 (0.62 to 1.64) ; WMD 0.90 (0.52 to 1.28)

Post-test Behaviour Score (maximum score 5) - Behaviour game versus No training: SMD 0.31 (-0.17 to 0.79) ; WMD 0.30 (-0.16 to 0.76)
Post-test Behaviour Score (maximum score 5) - Attitude&Behaviour game versus No training: SMD 0.54 (0.05 to 1.02) ; WMD 0.50 (0.06 to 0.94)

The means (SD) of the attitude tests were similar for each of the intervention groups but different from the control group (attitude simulation game 1.9 (0.7); behaviour simulation game 1.8 (0.6); attitude and behaviour simulation game 2.0 (0.7); control 1.3 (0.7)).

- [Table 01/10]

Post-test Attitude Score (maximum score 3) - Attitude game versus No training: SMD 0.85 (0.35 to 1.35) ; WMD 0.60 (0.27 to 0.93)
Post-test Attitude Score (maximum score 3) - Behaviour game versus No training: SMD 0.76 (0.26 to 1.26) ; WMD 0.50 (0.19 to 0.81)
Post-test Attitude Score (maximum score 3) - Attitude&Behaviour game versus No training: SMD 0.99 (0.48 to 1.50) ; WMD 0.70 (0.36 to 1.04)

Singh 1979 examined the effect of the use of traffic education materials by class teachers. The outcome measure was change in knowledge assessed by a test specially developed for each book and the proportion of children achieving 'Mastery' (at least 80% correct answers on the post-test). The mean test scores in the class using book one (infants) was 28 (6) before and 34 (10) after the intervention compared with 28 (7) and 30 (6) in the control group. In the class using book two (lower juniors) the mean score was 32 (12) before and 45 (11) after the intervention, compared with 32 (11) and 33 (11) in the control group. In the class using book three (upper junior and middle) the mean score was 28 (7) before and 35 (7) after the intervention, compared with 29 (8) and 31 (8) in the control group. In the group using book one, the proportion of children achieving mastery was 27% in the intervention group compared with 9% in the control group. In the group using book two the corresponding figures were 42% and 13%, and in the group using book three 39% of the intervention group achieved mastery compared with 17% in the control group.

- [Table 02/21] - Knowledge of 5 to 7 years old - post-test at less than 1 month

Post-test score of "Cognitive" test (maximum score 54): SMD 0.41 (0.30 to 0.52) ; WMD 3.65 (2.74 to 4.56)

- [Table 02/23] - Knowledge of 7 to 9 years old - post-test at 4 to 6 months

Post-test score of "Cognitive" test (maximum score 64): SMD 1.03 (0.91 to 1.15) ; WMD 11.30 (10.08 to 12.52)

- [Table 02/25] - Knowledge of 10 to 13 years old - post-test at 4 to 6 months

Post-test score of "Cognitive" test (maximum score 64): SMD 0.49 (0.38 to 0.60) ; WMD 3.71 (2.87 to 4.55)

- [Table 02/27] - Knowledge (change) of 5 to 7 years old - post-test at 4 to 6 months

Change in score of "Cognitive" test (maximum score 54): SMD 0.47 (0.36 to 0.57) ; WMD 3.83 (2.98 to 4.68)

- [Table 02/28] - Knowledge (change) of 7 to 9 years old - post-test at 4 to 6 months

Change in score of "Cognitive" test (maximum score 64): SMD 0.96 (0.85 to 1.08) ; WMD 10.92 (9.67 to 12.17)

- [Table 02/29] - Knowledge (change) of 10 to 13 years old - post-test at 4 to 6 months

Change in score of "Cognitive" test (maximum score 64): SMD 0.57 (0.46 to 0.68) ; WMD 4.28 (3.46 to 5.10)

Thomson 1992 reported the mean proportion of routes falling into different safety categories as a function of training group and testing phase. For children trained in a real traffic environment, the proportion of routes (standard deviations in brackets) classified as safe was: PT=0.10 (0.14), PT1=0.35 (0.21), PT2=0.34 (0.18). For children trained with the tabletop model, the proportion of routes classified as safe was: PT=0.14 (0.12), PT1=0.37 (0.21), PT2=0.37 (0.16). For untrained children, the proportion of routes classified as safe was: PT=0.04 (0.05), PT1=0.12 (0.25), PT2=0.12 (0.24).

- [Table 01/10] - Post-test 1:

Post-test proportion of routes categorised as Safe - Roadside training versus No training: SMD 0.95 (0.02 to 1.89) ; WMD 0.23 (0.03 to 0.43)
Post-test proportion of routes categorised as Safe - Table top training versus No training: SMD 1.40 (0.09 to 1.98) ; WMD 0.25 (0.05 to 0.45)

- [Table 01/14] - Post-test 1:

Change in proportion of routes categorised as Safe - Roadside training versus No training: SMD 0.77 (-0.14 to 1.69) ; WMD 0.17 (-0.01 to 0.35)
Change in proportion of routes categorised as Safe - Table top training versus No training: SMD 0.70 (-0.21 to 1.60) ; WMD 0.15 (-0.03 to 0.33)

- [Table 01/11] - Post-test 2:

Post-test proportion of routes categorised as Safe - Roadside training versus No training: SMD 0.99 (0.05 to 1.93) ; WMD 0.22 (0.03 to 0.41)
Post-test proportion of routes categorised as Safe - Table top training versus No training: SMD 1.17 (0.21 to 2.14) ; WMD 0.25 (0.07 to 0.43)

- [Table 01/15] - Post-test 2:

Change in proportion of routes categorised as Safe - Roadside training versus No training: SMD 0.80 (-0.12 to 1.72) ; WMD 0.16 (-0.01 to 0.33)
Change in proportion of routes categorised as Safe - Table top training versus No training: SMD 0.78 (-0.14 to 1.70) ; WMD 0.15 (-0.01 to 0.31)

Thomson 1997a reported the mean proportion of routes falling into different safety categories as a function of training group and testing phase. For the trained children, the proportion of routes (standard deviations) classified as safe was: PT=0.07 (0.11), PT1=0.26 (0.23), PT2=0.21 (0.18). For the children who were not trained (the control group), the proportion of routes classified as safe was: PT=0.08 (0.11), PT1=0.15 (0.20), PT2=0.19 (0.21). Children's behaviour was reported when crossing between parked cars for part two, and when crossing near a junction for part three. Thomson 1997b reports similar outcomes for another group of children.

- [Table 01/03] - Behaviour (observed) of 5 to 7 years old - post-test at less than 1 month

21 Stop at kerb - Parked cars - no distraction:
Thomson 1997a: RR 1.43 (1.13 to 1.81) ; RD 0.29 (0.13 to 0.45)
Thomson 1997b: RR 1.13 (0.99 to 1.29) ; RD 0.11 (0.00 to 0.23)
22 Stop at kerb - Junctions - no distraction
Thomson 1997b: RR 1.13 (0.98 to 1.30) ; RD 0.11 (-0.01 to 0.23)
23 Stop at line of vision - Parked cars - no distraction
Thomson 1997a: RR 1.66 (1.23 to 2.25) ; RD 0.37 (0.19 to 0.54)
Thomson 1997b: RR 2.56 (1.68 to 3.90) ; RD 0.57 (0.41 to 0.74)
24 Look three times at line of vision - Parked cars - no distraction
Thomson 1997a: RR 1.70 (1.21 to 2.39) ; RD 0.35 (0.16 to 0.53)
Thomson 1997b: RR 2.65 (1.69 to 4.15) ; RD 0.56 (0.39 to 0.73)
25 Choose a position that offers a clear view - Junctions - no distraction
Thomson 1997b: RR 1.20 (0.87 to 1.66) ; RD 0.12 (-0.08 to 0.33)

- [Table 01/05] - Behaviour (observed) of 5 to 7 years old - post-test at 1 to 3 months

21 Stop at kerb - Parked cars - no distraction
Thomson 1997a: RR 1.30 (1.05 to 1.61) ; RD 0.22 (0.06 to 0.37)
Thomson 1997b: RR 1.03 (0.97 to 1.08) ; RD 0.03 (-0.04 to 0.09)
22 Stop at kerb - Junctions - no distraction
Thomson 1997b: RR 0.98 (0.95 to 1.02) ; RD -0.02 (-0.08 to 0.04)
23 Stop at line of vision - Parked cars - no distraction
Thomson 1997a: RR 1.72 (1.28 to 2.31) ; RD 0.40 (0.23 to 0.57)
Thomson 1997b: RR 1.72 (1.26 to 2.36) ; RD 0.38 (0.20 to 0.56)
24 Look three times at line of vision - Parked cars - no distraction
Thomson 1997a: RR 1.94 (1.33 to 2.83) ; RD 0.42 (0.24 to 0.60)
Thomson 1997b: RR 2.02 (1.37 to 2.98) ; RD 0.43 (0.24 to 0.61)
25 Choose a position that offers a clear view - Junctions - no distraction
Thomson 1997b: RR 1.17 (0.87 to 1.58) ; RD 0.11 (-0.09 to 0.31)

- [Table 02/10] - Attitude of 5 to 7 years old - post-test at less than 1 month

Thomson 1997a: Post-test proportion of routes categorised as Safe: SMD 0.51 (0.12 to 0.90) ; WMD 0.11 (0.03 to 0.19)

- [Table 02/11] - Attitude of 5 to 7 years old - post-test at 1 to 3 months

Thomson 1997a: Post-test proportion of routes categorised as Safe: SMD 0.10 (-0.28 to 0.48) ; WMD 0.02 (-0.05 to 0.09)

- [Table 02/14] - Attitude (change) of 5 to 7 years old - post-test at less than 1 month

Thomson 1997a: Change in proportion of routes categorised as Safe: SMD 0.65 (0.26 to 1.04) ; WMD 0.12 (0.05 to 0.19)
Thomson 1997a: Change in proportion of routes categorised as Safe or More safe: SMD 0.51 (0.12 to 0.90) ; WMD 0.11 (0.03 to 0.19)

- [Table 02/15] - Attitude (change) of 5 to 7 years old - post-test at 1 to 3 months

Thomson 1997a: Change in proportion of routes categorised as Safe: SMD 0.17 (-0.21 to 0.55) ; WMD 0.03 (-0.03 to 0.09)
Thomson 1997a: Change in proportion of routes categorised as Safe or More safe: SMD 0.10 (-0.28 to 0.48) ; WMD 0.02 (-0.05 to 0.09)

Thomson 1998 reported the mean proportion of routes falling into different safety categories as a function of training group and testing phase. For the trained children, the proportion of routes (standard deviation) classified as safe was: PT=0.15 (0.14), PT1=0.43 (0.31), PT2=0.35 (0.29). For the children who were not trained (the control group), the proportion of routes classified as safe was: PT=0.16 (0.12), PT1=0.13 (0.09), PT2=0.16 (0.19).

- [Table 01/10] - Post-test 1:

Post-test proportion of routes categorised as Safe - (Table top & roadside training) versus No training: SMD 1.30 (0.74 to 1.86) ; WMD 0.30 (0.18 to 0.42)

- [Table 01/14] - Post-test 1:

Change in proportion of routes categorised as Safe - (Table top & roadside training) versus No training: SMD 1.48 (0.91 to 2.06) ; WMD 0.31 (0.21 to 0.41)

- [Table 01/11] - Post-test 2:

Post-test proportion of routes categorised as Safe - (Table top & roadside training) versus No training: SMD 0.76 (0.24 to 1.29) ; WMD 0.19 (0.07 to 0.31)

- [Table 01/15] - Post-test 2:

Change in proportion of routes categorised as Safe - (Table top & roadside training) versus No training: SMD 0.92 (0.39 to 1.46) ; WMD 0.20 (0.09 to 0.31)

DISCUSSION

After screening close to 14,000 published and unpublished studies, we identified 15 randomised-controlled trials of pedestrian safety education programmes. The methodological quality of the included trials was generally poor. The method of allocation concealment was adequate in three trials, outcome assessment was blinded in eight, and in most of the included studies large numbers of participants were lost to follow-up. None of the trials was conducted in a developing country setting and there were no trials of pedestrian safety training in the elderly. The studies identified were conducted between 1976 and 1997. Because of differences in the types of interventions and in the outcome measures used in the 15 included trials, meta-analyses were not carried out.

None of the included trials assessed the effect of pedestrian safety education on the occurrence of pedestrian injury but six trials assessed the effect on observed behaviour. Some of these trials showed evidence of behavioural change following pedestrian safety education but for a variety of reasons it is difficult to predict what effect this might have on pedestrian injury risk. Firstly, we cannot be sure that the observed behaviour is causally related to the occurrence of pedestrian injury. For example, Nishioka 1991 examined the effect of verbal instructions on the safety behaviour of children in a simulated traffic environment. The children were shown a video of a running motorcycle in an environment in which they were playing catch. The children's behaviour was classified as safe if they changed their speed of walking or running and looked right and left. In this particular study, slowing down or stopping were considered to be the safe response. Even if the behavioural changes observed in the simulated traffic environment were also present in a real traffic situation, it is difficult to estimate what effect, if any, these behaviours would have on injury risk. For example, once a child has established that the road is clear, it may be safer to run across the street before another vehicle passes because it reduces the time of exposure to risk. Similarly, in the study by Ampofo-Boateng 1993, routes chosen by the children were coded into four safety categories, depending on the degree to which dangerous road features were avoided in the chosen route. However, the authors provide no evidence that making the 'correct' choices would lead to a reduction in injury risk. Indeed, in the example given by the authors, the routes classified as 'more safe' and 'safe' involved two crossings, whereas the routes classified as very unsafe and unsafe involved one road crossing. Whether two 'safe' road crossings would involve a lower risk of pedestrian injury than one 'unsafe' road crossing is questionable.

Secondly, assuming that the behaviours measured are causally related to pedestrian injury risk, we have no reliable information about the magnitude of this effect and so we cannot predict how much a given behavioural change will reduce a child's pedestrian injury risk. Finally, there is uncertainty about the extent to which the observed behavioural changes persist over time. For example, in the study by Ampofo-Boateng 1993, for children trained in a real traffic environment the proportion of routes classified as safe declined from 72% immediately after training to 50% nine weeks later.

There are some methodological issues that could have an important bearing on the validity of the results of this systematic review. In particular, publication and other selection biases may have resulted in the over representation of studies showing promising intervention effects. This is particularly likely in the context of road safety where a large proportion of the available research information is published in the grey literature of the road safety research organisations. Most of the statistical methods that can be used to assess the possibility of publication bias require the use of meta-analysis and so cannot be used in this systematic review. Although considerable efforts were made to identify all eligible trials, published and unpublished, irrespective of language of publication, we cannot exclude the possibility of selection bias. The validity of the inferences from any systematic review depends on the quality of the included studies and in this case many of the studies were of poor quality. It is has been shown that inadequate allocation concealment, lack of blinding of outcome assessment and large losses to follow-up can result in the overestimation of intervention effects in randomised-controlled trials, and many of these methodological weaknesses were present in the included trials (Schulz 1995).

Each year some 300,000 children die in road traffic crashes worldwide. Most of these deaths are in countries that the World Bank classes as low and middle-income countries and most involve children as pedestrians. The provision of pedestrian safety education for children in these countries is considered to be an essential part of a global road safety strategy and has been strongly recommended by the Global Road Safety Partnership (GRSP) who say "One reason why these accidents happen is that children do not have the necessary knowledge and skills that allow them to deal with the hostile traffic environment. Receiving road safety education as part of their normal school curriculum is recognised as being one of the most effective ways of providing children with this type of knowledge".

Given the lack of high-quality randomised-controlled trials of pedestrian safety education, in particular the lack of trials in low and middle-income countries, and the fact that none of the available trials have assessed injury outcomes, GRSP optimism about the potential of this intervention may be misplaced. Whilst the value of pedestrian safety education remains in doubt, environmental modification and the enforcement of appropriate speed limits may be a more effective strategy to protect children from the hostile traffic environment.

AUTHORS' CONCLUSIONS

Implications for practice

Pedestrian safety education can result in improvement in children's knowledge of the road crossing task and can change observed road crossing behaviour but whether this reduces the risk of pedestrian motor vehicle collision and injury occurrence is unknown. There is evidence that changes in safety knowledge and observed behaviour decline with time, suggesting that safety education must be repeated at regular intervals.

Implications for research

Large-scale randomised controlled trials with injury outcomes (or endpoints that are likely to predict injury outcomes such as near-miss collisions) are needed to establish the effectiveness of pedestrian safety education. Although a number of existing trials show evidence of behavioural change following pedestrian safety education, the target behaviours in these trials cannot be assumed to decrease pedestrian injury risk.

NOTES

Seaches updated in May 2003: no study included, 4 studies pending, 19 studies excluded.

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POTENTIAL CONFLICT OF INTEREST

None known.

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* *Indicates the major publication for the study*

TABLES

Characteristics of included studies

Study	Ampofo-Boateng 1993
Methods	Allocation by alternation from class register in alphabetical order with separate lists for boys and girls. Outcome assessment blinded. Loss to follow-up was 37.5% for the intervention groups.
Participants	26 children aged 5 yrs old who were randomly selected from a primary school in Edinburgh, Scotland.
Interventions	Children were divided into three groups: 1. trained using a tabletop model of the traffic environment ; 2. trained in a real traffic environment ; 3. no training. The training concentrated on two main areas: failing to recognise dangerous road crossing sites and selecting the most direct route to the destination as safest. Each group underwent six training sessions at a rate of about one per week. The training aimed to help children appreciate the danger posed by poor visibility, complex road layouts and lengthy excursions across the road. The broad aim of the training was to improve children's understanding so that they could deal flexibly with a wide range of traffic situations.
Outcomes	The outcome measure was children's perceptions about the safest place to cross the road. Children were taken to the roadside on 3 occasions, asked to imagine that they were alone and to indicate their preferred route to cross to get to a specific destination marked by a red cone. Their answers were reported on a diagram and coded into 4 safety categories (very unsafe, unsafe, more safe, safe). Results were presented as the mean proportion of routes falling into each safety category as a function of training. Children were never asked to cross the road. Intervention groups had pre-test and post-tests observations: immediately after 6th training

	session (post-test 1), 63 days after end of training (post-test 2) and 8 months (post-test 3). Control group had only post-test 3.
Notes	Study done 1989
Allocation concealment	C

Study	**Bouck 1992**
Methods	Allocation by selecting paper cards drawn from basket by "blind" person. Outcome assessment was blind to intervention allocation. Loss to follow-up was 20% for both groups.
Participants	40 children 8 to 11 years old who were randomly selected from two small primary schools of Wiltshire County, Western England.
Interventions	Children were divided in 2 groups: 1. trained in classroom and in semi-real environment; 2. no training. The aim of the programme was to improve children knowledge and behaviour by providing a road safety education package with support materials to be used by class teachers during 6 units. Teaching strategies included topic webs, lectures, class discussions and and group activities.
Outcomes	The outcome measure was a post-test on knowledge conducted immediately after the intervention. Only two units were evaluated: (a) conspicuity and (b) mass, speed and control. In each school the experimental group was given the road safety support materials and administered the post-test. The control group was also administered the post-test.
Notes	Study done 1992
Allocation concealment	C

Study	**Cross 1988**
Methods	Cluster allocation (classrooms) to control or intervention group by drawing lots from a hat, after children had been individually allocated to their class by drawing lots from a hat. No information given on blinidng of outcome assessment or loss to follow-up
Participants	138 children 7 to 8 yrs old who were randomly selected from 3 primary schools in Melbourne, Australia.
Interventions	Children were divided in 2 groups: 1. trained in the classroom during a unit on speed; 2. no training. The training aimed at improving children's understanding of the concept of speed in the hope that this would enable them to make safer road crossing decisions. The course incorporated elements of an integrated educational experience.
Outcomes	Outcome measures were observations of children's response and documentation of verbal explanation. In a play situation children were asked to perform 4 tasks before (pre-test) and after intervention (post-test). Task 1: speed and time variables are fixed; children operate on distance variable (complex case of unequal speeds). Task 2: speed and time variables are fixed; children operate on distance variable (simple case of equal speeds). Task 3: speed and distance variables are fixed; children operate on time variable (complex case of unequal speeds). Task 4: speed and distance variables are fixed; children operate on time variable (simple case of equal speeds).
Notes	Year of study not provided.
Allocation concealment	C

Characteristics of included studies (*Continued*)

Study	Downing 1981
Methods	Allocation by computer. Assessors were not blinded to intervention status and loss to follow-up was 44% mostly because children had moved out of the area.
Participants	1560 children aged 3 yrs old and their parents, selected from 2 towns and 1 rural district in England.
Interventions	Children were divided in 4 groups: 1. road safety booklet after an interview; 2. interview but no booklet. 3. road safety booklet with a letter. 4. No intervention. The aim of the study was to assess the effectiveness of the booklet in improving parental road safety education and road supervision.
Outcomes	Children were tested with pictures from the booklet on simple traffic knowledge and simple safety rules. Only the first 2 groups had an interview before the intervention (pre-test), but all groups had one at the end of the intervention (post-test at 2 months).
Notes	Study done 1979.
Allocation concealment	A

Study	Limbourg 1981
Methods	Cluster allocation (schools) by block randomisation within groups of four similar schools (matched for age, sex, parental social status and urban characteristics of living area) by selecting paper cards drawn from envelope by "blind" person. Outcome assessment was blind to intervention group. Overall loss to follow-up was 15%.
Participants	658 parents volunteered to learn how to teach safe pedestrian behaviour to their 3-6 yr old children from 26 kindergarten schools in Augsburg, Germany.
Interventions	Children were divided into 4 groups: 1. behavioural road safety training by parent with psychologist's supervision; 2. behavioural road safety training by parent without psychologists supervision;. 3. parents were shown a film and given a booklet on road safety problems in childhood. 4. No training. The main goal of the research project was to improve road safety behaviour of pre-school children on the basis of behavioural learning theories and empirical research findings. The behavioural training programme consisted of a film and an instructional booklet that set behavioural learning objectives and demonstrated how to reach them. The film showed model parents carrying out the behavioural modification programme with children in different traffic situations. The booklet included pictures from the film with training instructions.
Outcomes	Outcome measures were observation of children's behaviour in situations with and without distraction (objects to pick up on the other side of the road) in real traffic situation (2 way traffic with 2 kerbs). Pre-test: alone; bring back a ball as quickly as possible (with distraction); bring photos back without timing (without distraction). Post-test 1: +1 month; in pairs, competition to bring back red objects (with distraction); bring photos back without competition (without distraction). Post-test 2: +5 months; combination of pre-test and post-test 1 but with toys to bring back instead of red objects (with distraction); bring a bag back without competition (without distraction).
Notes	Study done 1978 25 parents from INT1 refused to be supervised by psychologists and were considered as being in INT2: not intention-to-treat analysis
Allocation concealment	C

Characteristics of included studies (*Continued*)

Study	**Luria 2000**
Methods	Cluster allocation (schools) by drawing identical pieces of paper with the name of schools. Assessors were not blinded to intervention allocation. Loss to follow-up was 26% for both groups.
Participants	246 children who were randomly selected from kindergarten and primary classes of elementary schools in Columbus, Ohio, USA.
Interventions	Children were divided into two groups: 1. trained with the Safety City programme; 2. no training. Safety City is a safety education programme which focuses on three safety issues: how to cross the street, call 911 in an emergency situation, and avoid strangers. For the road crossing section, trained volunteers used a mock intersection of the traffic environment and a lecture in classroom in a 20 minutes session. Children also received a booklet and attended a rock concert reinforcing the messages. The training concentrated on 7 main messages: (a) Cross the street at the corner, (b) Look both ways, (c) Listen for cars, trucks, and motorcycles, (d) Never run in the street, (e) Cross the street with an adult if possible, (f) Always tell the person responsible for you where you are going, (g) Traffic lights (red means stop; green means go; yellow means slow down)
Outcomes	Evaluation tool specially developed to assess change in individual knowledge by test scores. The questions were checked by a paediatric psychologist. Children were tested with a drawing of a city map and in a mock intersection. They showed how they would cross. They were also asked about the colours on a traffic light. Children were never asked to cross the road. The maximum number of points for the section on crossing the street was 16. Individual questions were weighted with regard to their importance to the Safety City curriculum. Both groups were tested before and 6 months after the intervention or after the pre-test (control group). Results were presented as the mean scores.
Notes	Study done 1996-1997.
Allocation concealment	C

Study	**Matson 1980**
Methods	Allocation by block randomisation within triplets of individuals. Outcome assessment blinded. Information on loss to follow-up not provided.
Participants	30 ”mentally retarded” institutionalised adults aged 21 to 55 years from Pittsburg, USA.
Interventions	Participants were divided in three groups: 1. individual training in classroom using a tabletop model (each participant received 30 minutes of behavioural training). 2. independence training in a semi-real traffic situation (participants were taught how to recognise common pedestrian signs and target pedestrian behaviours). 3. training in how to cook and to make the bed.
Outcomes	Outcome measures were steps performed correctly on a set of target behaviours (proper sidewalk behaviour, recognition of an intersection, crossing the street) at a city intersection. They were assessed before and after the intervention.
Notes	Year of study not provided. Data extracted from graph.
Allocation concealment	B

Study	**Miller 1982**
Methods	Cluster allocation (classrooms) using a list of random numbers read by someone not entering participants into trial. Assessor blinding not stated. Loss to follow-up was 6% for knowledge test, 65% and 77% for reported behaviour.

Characteristics of included studies (*Continued*)

Participants	Study participants were 550 second grade children in Oregon, USA.
Interventions	Children were divided into three groups: 1. Beltman programme; 2. Beltman programme with a booster course at 4 months; 3. normal safety teaching. The Beltman programme was a multi-media traffic safety programme, the main objective of which was to develop the habit of seat-belt wearing but the programme materials also focused on correct pedestrian behaviour. Teachers trained the children.
Outcomes	The two outcome measures were child's safety knowledge and behaviour. Knowledge was assessed with a 20-item multiple-choice test performed before (pre-test), immediately after (post-test 1) and after 6 months (post-test 2). Parents reported their child's safety behaviour with a postcard sized questionnaire mailed when post-tests took place.
Notes	Study done September 1981 to April 1982.
Allocation concealment	A

Study	**Nishioka 1991**
Methods	Allocation by block randomisation within triplets of individuals (matched for age, sex and class) using table of random numbers. Outcome assessment was not blinded. Lost to follow-up 10%.
Participants	79 children between 4 and 5 yrs old attending a kindergarten in Tokyo, Japan.
Interventions	Children were divided into three groups: 1. caution advising how to behave safely ("a motorcycle is running; if you come around here, stop surely and look at the right and left side, as it is dangerous"); 2. simple caution ("A motorcycle is running. Be careful as it is dangerous").; 3. No caution.
Outcomes	The children's behaviour was observed and recorded on video. The children's behaviour was classified as safe if they both changed the speed of walking or running and looked to the right and left. The children's behaviour was classified as unsafe if there was no safety response, if they only changed speed or if they only looked. Outcome was measured after the intervention (post-test only).
Notes	Study done June 1983 Paper also reports an experiment with similar setting testing audio-visual information as factors on children behaviour
Allocation concealment	A

Study	**Renaud 1989**
Methods	Allocation by alternation. Outcome assessment not blinded. No loss to follow up.
Participants	136, five-year-old children from four schools in Montreal, Canada.
Interventions	Children were divided into four groups: 1. simulation game (1) targeted attitude: the game aims to change attitudes through role play and group dynamics; 2. simulation game (2) targeted behaviour: the game aims to change behaviour through modelling and training elements. 3. sSimulation game (3) targeted attitude and behaviour: the game aims to change both attitudes and behaviour with role play, group dynamics, modelling and training; 4. control group (no simulation game).
Outcomes	The three outcomes measured after the intervention were: (1) Attitude score (range 0-3 / Day 1): children looked at 10 photos and told their perception of risk (health dimension) and how to avoid risk (perception

Characteristics of included studies (*Continued*)

	dimension). (2) Behaviour score (range 0-5 / Day 1): children answered questions by showing how they would behave on a picture of a road with the help of stickers. (3) Transfer of learning score (range 0-31 / Day 10): a trained observer scored selected behaviours of children in a quasi-real traffic environment set up in a gymnasium.
Notes	Study done 1987. Reliability coefficients: Attitude Score alpha=0.89, Behaviour Score alpha=0.41, Transfer of learning score alpha=0.85.
Allocation concealment	C

Study	**Singh 1979**
Methods	Cluster allocation (classrooms) but method not stated. Assessor blinding was not stated. Number of classes lost to follow-up: 2 refused and 7 did not complete in intervention group (7/106=6.6%), 33 refused and none did not complete in control group.
Participants	Children aged between 5 and 13 yrs old selected from schools within six education authorities in the UK.
Interventions	Children were divided into two groups: 1. intervention group: the study intervention involved the use of traffic education materials by class teachers; the materials were books designed to help children improve their knowledge and awareness of the real world of traffic, one book for infant and two for junior and middle schools. 2. control group: The control group received no road safety education.
Outcomes	Outcome measures were change in knowledge assessed by a test specially developed for each book, and proportion of children achieving “Mastery” (at least 80% correct answers on the post-test). Children were tested before training (PT), six months after training (PT1).
Notes	Study done 1976.
Allocation concealment	B

Study	**Thomson 1992**
Methods	Allocation by alternation from class register in alphabetic order with separate lists for boys and girls. Outcome assessment blinded. No loss to follow up.
Participants	30 children aged 5 years who were randomly selected from a primary school in Edinburgh, Scotland.
Interventions	Children were divided into three groups: 1. trained in a real traffic environment; 2. trained using a tabletop model of the traffic environment . 3. No training. The intervention was almost identical to the study by Ampofo-Boateng 1993 but the children were trained in small groups rather than individually.
Outcomes	The outcome measure was the mean proportion of routes classified as safe as a function of the training. Children indicated their preferred route by pointing and describing it to the investigator. They were not asked to walk across the road. All tests were conducted in the road environment. Children were tested before training (PT), immediately after training (PT1), and two months after training (PT2).
Notes	Study done 1990.
Allocation concealment	C

Study	**Thomson 1997a**
Methods	Allocation by alternation from class register in alphabetical order with separate lists for boys and girls. Outcome assessment was blind to intervention group.

Characteristics of included studies (*Continued*)

Participants	104 children aged 5 years randomly selected from 10 primary schools in Glasgow, Scotland.
Interventions	Children were divided into two groups: 1. trained in a real traffic environment; 2. no training. The training concentrated on two main areas: recognising dangerous road crossing sites and selecting the most direct route to the destination as safest. Part 1 focused on the identification of safe places to cross, Part 2 on crossing safely between parked cars and Part 3 on crossing safely near junctions. Each group underwent six training sessions for Part 1 and Part 3, four training sessions for Part 2, at a rate of about one per week.
Outcomes	For Part 1, the outcome measure was children's perceptions about the safest place to cross the road. Children were taken to the roadside, asked to imagine that they were alone and to indicate their preferred route to cross to get to a specific destination marked by a red cone. Their answers were reported on a diagram and coded into 4 safety categories (very unsafe, unsafe, more safe, safe). Results were presented as the mean proportion of routes falling into each safety category as a function of training. Children were never asked to cross the road. Both groups had pre-test and post-test observations immediately after 6th training session (post-test 1) nad two to three months after end of training (post-test 2). For Part 2, the outcome measure was children's behaviour when crossing between parked cars. For Part 3, the outcome measure was children's behaviour when crossing near a junction.
Notes	Study done 1995.
Allocation concealment	C

Study	**Thomson 1997b**
Methods	Same as Thomson 1997a but with a second "cohort" of children conducted the year after
Participants	97 children aged 5 yrs old randomly selected from 10 primary schools in Glasgow, Scotland.
Interventions	Same as Thompson 1997a.
Outcomes	Same as Thompson 1997a.
Notes	Study done 1996.
Allocation concealment	C

Study	**Thomson 1998**
Methods	Allocation by alternation from class register in alphabetical order with separate lists for boys and girls. Outcome assessment blinded. No loss to follow up.
Participants	60 children aged 5 years whose parents agreed to participate, from three primary schools in Glasgow, Scotland.
Interventions	Children were divided into 2 groups: 1. trained using a tabletop model of the traffic environment as well as in the real traffic environment; 2. no training. The training concentrated on the same areas as in Ampofo-Boateng 1993, but was provided by ten parent volunteers who received experience of training children at courses organised within the school. Volunteers were recruited from among the parents at the participating schools to assist in training other people's children. Before training the children, all volunteers took part in a one-day training course to ensure that they understood the aims and objectives of the programme. Children in the trained group received two sessions of training at the roadside followed by four sessions on a table-top model, each session lasted about 30 minutes and were conducted over a three week period.
Outcomes	The outcome measure was the mean proportion of routes classified as safe as a function of the training. All tests were conducted in the road environment. Children indicated their preferred route by pointing and

Characteristics of included studies (*Continued*)

	describing it to the investigator. They were not asked to walk across the road. Children were tested before training (PT), immediately after training (PT1), and 40 days after training (PT2).
Notes	Study done 1991.
Allocation concealment	C

Characteristics of excluded studies

Antaki 1986	Controlled before/after, allocation not randomised.
Blomberg 1975	Control group, no evidence of random allocation could be obtained.
Blomberg 1983	Controlled before/after, allocation not randomised.
Bostick 1975	Control group, no evidence of random allocation could be obtained.
Boyle 1973	Controlled before/after, allocation not randomised.
Bryan-Brown 1995	Controlled before/after, allocation not randomised.
Bryan-Brown 1999	Controlled before/after, allocation not randomised.
Clayton 1991	Controlled before/after, allocation not randomised.
Cleven 1994	Controlled before/after, allocation not randomised.
Colborne 1971B	Comparing 2 ways of education but without control group.
Cross 1991	Controlled before/after, no evidence of random allocation could be obtained.
Cross D 2000	Controlled before/after, allocation randomised but only 3 clusters for 2 interventions + 1 control.
Davidson 1994	Controlled before/after, allocation not randomised.
Demetre 1993	Controlled before/after, allocation not randomised (post-hoc selection of control group).
Downing 1981D	Controlled before/after, allocation not randomised.
Dueker 1975	RCT comparing 3 pedestrian safety education methods without control group.
Dueker 1981	Controlled before/after, allocation not randomised.
Firth 1973	Controlled before/after, allocation not randomised.
Fisk 1975	Controlled before/after, no evidence of random allocation could be obtained.
Geiler 1981	Controlled before/after, allocation not randomised.
Gregersen 1994	Controlled before/after, allocation not randomised.
Grime 1952	Controlled before/after, allocation not randomised.
Guyer 1989	Controlled before/after, allocation not randomised.
Hazinski 1995	Cohort study design.
Heinrich 1976	Controlled before/after, allocation not randomised.
Jones 1979	Controlled before/after, no evidence of random allocation could be obtained.
Kelly 1987	Randomised controlled trial but no pedestrian education.
Kromann 1976	Controlled before/after, no evidence of random allocation could be obtained.
Lahtinen 1973	Controlled before/after, allocation not randomised.
Linklater 1978	Uncontrolled before/after.
Maisey 1982	Controlled before/after, allocation not randomised.
McKelvey 1978	RCT comparing 2 ways of education (feedback vs non-feedback) but without control group.
Padgett 1975	Controlled before/after, no evidence of random allocation could be obtained.
Pease 1967	Controlled before/after, allocation not randomised.
Peterson 1988	Controlled before/after, allocation not randomised.

Characteristics of excluded studies (*Continued*)

Powney 1995	Comparing 3 ways of education but without control group.
Preusser 1988	Control group, no evidence of random allocation could be obtained.
Rothengatter 1981	Controlled before/after, no evidence of random allocation could be obtained.
Sandels 1975	Report controlled trials conducted in the sixties - no evidence of randomisation could be obtained.
Sayer 1997	The 12 schools taking part in the study were "split into two matched groups of six schools each." No details were provided about how schools were allocated. The authors were contacted for further information and on the basis of their responses it could not be confirmed that random allocation was used to form the intervention and comparison groups.
Schelp 1988	Controlled before/after, allocation not randomised.
Schioldborg 1976	Controlled study, allocation not randomised. Provides accident data.
Stikarova 1991	Controlled before/after, allocation not randomised.
Stuy 1993	RCT but no pedestrian education component (child passenger safety - use of seatbelts).
Tucker 1993	Controlled before/after, allocation not randomised.
Van Schagen 1988	Controlled before/after, no evidence of random allocation could be obtained.
Van Steenwijk 1984	Uncontrolled before/after.
Van den Herik 1981	Uncontrolled before/after.
Wiener 1968	Controlled before/after, allocation not randomised.
Young 1987	Comparing 2 ways of education but without control group.
Ytterstad 1995	Controlled before/after, no allocation. Comparison with another Norwegian city (non-equivalent control and no-equivalent variable design). Provides data on injury rates.
Zeedyk 2001	RCT comparing 3 pedestrian safety education methods without a valid control group.

ADDITIONAL TABLES

Table 01. Search strategies

Language	Concept A	Concept B	Concept C	Concept D
	pedestrian	education /safety education / prevention	road trafic accident / road crossing	injury/accident outcomes (injury, mortality, disability)
English	pedestr* walker* walkin*	educat* teach* informat* train* instruct* safe* preven* securit*	accident* road* street* traf?ic* crossin* crash* mot* near3 vehic?l* car cars automob*	injur* fatal* mortal* emergenc* ho?pital* disab* AIS ISS trauma* handicap*
French	pieton*	educat* informat* instruct* preven* securit* formation* enseigne*	rue* route* routier* collision* voiture*	mortal* urgences ho?pital* trauma* handicap* bless*
German	Fussganger Fussgänger	Unterweis* Lehr*	Unfall* Unglu*	Verletz* Schaden

Table 01. Search strategies (*Continued*)

Language	Concept A	Concept B	Concept C	Concept D
	Geher Gehen spazier* Zu Fuß	Unterricht* Inform* Train* schulen Instruieren unterweis* Instruk* Sicher* gesichert Verkehrssicher* Strassenverkehrssicher* Vorbeug* vermeid* Sicher*	Verunglu* Landstra* Strasse* Straß* Verkehr* Strassenverk* Kreuzung Zebrastreifen Uberweg* Ueberweg* Überweg* Fussgaengerueberweg* Unfall Zusammensto* Auto PKW Kraftwagen Auto Autos Auto Autos Personenkraftwagen Automobil*	Personenschaden Todlich* tödlich* Fatal* Todlich* Mortalitaet Sterblichkeit* Notfall* Notfaell* Krankenhaus* Hospital* Behinder* Korperbeh* Trauma* Beeintracht* Beeinträcht* schwer*
Italian	pedon* deambulazione cammino marcia	formazione formativo prevenzione insegnamento didattic* informazion* allievi istruzion* apprendimento sicurezza preventiv*	accident* incident* infortuni* strad* traffico incroci* autovettur* autoveicol* veicol* automobil* crocevia circolazione	traum* dann* lesion* fatal* letal* mortal* urgen* ospedal* ricover* invalid* disabil* handicap*
Spanish	peaton marcha deambulacion caminar. andar	educati* docencia docente ensenanza formacion informa* entrenamiento instruccion segur* preven*	accident* camino encrucijada vial avenidas calle* trafico transito automo*	heridas trauma* lesion* muert* letal* fatal* mortal* urgencias hospital enferm* discapaci* incapaci* invalid* minusval* impedid*
Dutch	voetg* wandel* lopen* loop*	educat* leren* onderw* informat* train* instruct* aanswijz* veilig* preven*	ongeluk* weg* straat* verkeer* zebrapad* voetgangersoversteekplaats oversteekpl* mot* auto* wagen*	ongeluk dodelijk* noodgeval hospitaal ziehenhuis handicap* invalid* trauma* gehandicap*
Danish	fodgaenger ga	uddannelse laere information tog instruktion sikker forebyggelse sikkermed	ulykke vej gade trafik krydse sammenstod koretoj bil biler automobil	skade fatal dod nodsituation kritisk hospital sygemus invalid traume handikap
Comments	a) words relating to one concept were combined by OR b) combination of concepts: A AND B AND (C OR D) c) searches in each language were run separately			

G R A P H S

Comparison 01. Direct education compared to No education

Outcome title	No. of studies	No. of participants	Statistical method	Effect size
03 Behaviour (observed) of 5 to 7 yr olds - post-test at less than 1 month			Relative Risk (Random) 95% CI	Totals not selected
04 Behaviour (observed) of 5 to 7 yr olds - post-test at less than 1 month			Standardised Mean Difference (Random) 95% CI	Totals not selected
08 Behaviour (observed) of institutionalised adults - post-test at less than 1 month			Relative Risk (Random) 95% CI	Totals not selected
10 Attitude of 5 to 7 yr olds - post-test at less than 1 month			Standardised Mean Difference (Random) 95% CI	Totals not selected
11 Attitude of 5 to 7 yr olds - post-test at 1 to 3 months			Standardised Mean Difference (Random) 95% CI	Totals not selected
12 Attitude of 5 to 7 yr olds - post-test at 7 to 9 months			Standardised Mean Difference (Random) 95% CI	Totals not selected
13 Attitude of 7 to 8 yr olds - post-test at less than 1 month			Relative Risk (Random) 95% CI	Totals not selected
14 Attitude (change) of 5 to 7 yr olds - post-test at less than 1 month			Standardised Mean Difference (Random) 95% CI	Totals not selected
15 Attitude (change) of 5 to 7 yr olds - post-test at 1 to 3 months			Standardised Mean Difference (Random) 95% CI	Totals not selected
22 Knowledge of 5 to 7 yr olds - post-test at 4 to 6 months			Standardised Mean Difference (Random) 95% CI	Totals not selected
26 Knowledge (change) of 5 to 7 yr olds - post-test at 4 to 6 months			Standardised Mean Difference (Random) 95% CI	Totals not selected

Comparison 02. Indirect education versus No education

Outcome title	No. of studies	No. of participants	Statistical method	Effect size
01 Behaviour (observed) of 3 to 4 yr olds - post-test at 1 to 3 months			Relative Risk (Random) 95% CI	Totals not selected
02 Behaviour (observed) of 3 to 4 yr olds - post-test at 4 to 6 months			Relative Risk (Random) 95% CI	Totals not selected
03 Behaviour (observed) of 5 to 7 yr olds - post-test at less than 1 month			Relative Risk (Random) 95% CI	Totals not selected
05 Behaviour (observed) of 5 to 7 yr olds - post-test at 1 to 3 months			Relative Risk (Random) 95% CI	Totals not selected

06 Behaviour (observed) of 5 to 7 yr olds - post-test at 4 to 6 months	Relative Risk (Random) 95% CI	Totals not selected
07 Behaviour (reported) of 5 to 7 yr olds - post-test at 4 to 6 months	Relative Risk (Random) 95% CI	Totals not selected
10 Attitude of 5 to 7 yr olds - post-test at less than 1 month	Standardised Mean Difference (Random) 95% CI	Totals not selected
11 Attitude of 5 to 7 yr olds - post-test at 1 to 3 months	Standardised Mean Difference (Random) 95% CI	Totals not selected
14 Attitude (change) of 5 to 7 yr olds - post-test at less than 1 month	Standardised Mean Difference (Random) 95% CI	Totals not selected
15 Attitude (change) of 5 to 7 yr olds - post-test at 1 to 3 months	Standardised Mean Difference (Random) 95% CI	Totals not selected
20 Knowledge of 3 yr olds - post-test at 1 to 3 months	Relative Risk (Random) 95% CI	Totals not selected
21 Knowledge of 5 to 7 yr olds - post-test at less than 1 month	Standardised Mean Difference (Random) 95% CI	Totals not selected
22 Knowledge of 5 to 7 yr olds - post-test at 4 to 6 months	Standardised Mean Difference (Random) 95% CI	Totals not selected
23 Knowledge of 7 to 9 yr olds - post-test at 4 to 6 months	Standardised Mean Difference (Random) 95% CI	Totals not selected
24 Knowledge of 8 to 11 yr olds - post-test at less than 1 month	Standardised Mean Difference (Random) 95% CI	Totals not selected
25 Knowledge of 10 to 13 yr olds - post-test at 4 to 6 months	Standardised Mean Difference (Random) 95% CI	Totals not selected
26 Knowledge (change) of 5 to 7 yr olds - post-test at less than 1 month	Standardised Mean Difference (Random) 95% CI	Totals not selected
27 Knowledge (change) of 5 to 7 yr olds - post-test at 4 to 6 months	Standardised Mean Difference (Random) 95% CI	Totals not selected
28 Knowledge (change) of 7 to 9 yr olds - post-test at 4 to 6 months	Standardised Mean Difference (Random) 95% CI	Totals not selected
29 Knowledge (change) of 10 to 13 yr olds - post-test at 4 to 6 months	Standardised Mean Difference (Random) 95% CI	Totals not selected

INDEX TERMS

Medical Subject Headings (MeSH)

Accidents, Traffic [prevention & control]; Program Evaluation; Randomized Controlled Trials; Safety; Walking; Wounds and Injuries [prevention & control]

Medical MeSH check words

Humans

COVER SHEET

Title	Safety education of pedestrians for injury prevention
Authors	Duperrex O, Roberts I, Bunn F
Contribution of author(s)	OD designed the protocol, searched databases, screened records, extracted data, contacted authors and wrote the review. IR helped design the protocol and write the review. FB helped design the protocol, extract data and write the review.
Issue protocol first published	1999/2
Review first published	2002/2
Date of most recent amendment	15 April 2005
Date of most recent SUBSTANTIVE amendment	27 February 2002
What's New	The search was updated in May 2003 for papers published between 1999 and 2003 (Feb). From the 1623 hits, 277 were relevant, 18 were added to the excluded section, four to the waiting assessment section and none to the included section.
Date new studies sought but none found	Information not supplied by author
Date new studies found but not yet included/excluded	Information not supplied by author
Date new studies found and included/excluded	25 May 2003
Date authors' conclusions section amended	Information not supplied by author
Contact address	Dr Olivier Duperrex Institut de Medecine Sociale et Preventive Centre medical universitaire 1, rue Michel-Servet Geneve 4 SWITZERLAND Telephone: +41 22 379 5921 E-mail: Olivier.Duperrex@imsp.unige.ch Facsimile: +41 22 379 5912
DOI	10.1002/14651858.CD001531
Cochrane Library number	CD001531
Editorial group	Cochrane Injuries Group
Editorial group code	HM-INJ

GRAPHS AND OTHER TABLES

Fig. 1. Comparison 01. Direct education compared to No education

01.03 Behaviour (observed) of 5 to 7 yr olds - post-test at less than 1 month

Review: Safety education of pedestrians for injury prevention

Comparison: 01 Direct education compared to No education

Outcome: 03 Behaviour (observed) of 5 to 7 yr olds - post-test at less than 1 month

Study	Intervention n/N	Control n/N	Relative Risk (Random) 95% CI	Relative Risk (Random) 95% CI
01 Safe behaviour - Detailed caution vs No caution				
Nishioka 1991	14/23	6/24		2.43 [1.13, 5.24]
02 Safe behaviour - Simple caution vs No caution				
Nishioka 1991	11/24	6/24		1.83 [0.81, 4.15]

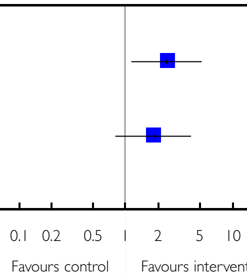


Fig. 2. Comparison 01. Direct education compared to No education

01.04 Behaviour (observed) of 5 to 7 yr olds - post-test at less than 1 month

Review: Safety education of pedestrians for injury prevention

Comparison: 01 Direct education compared to No education

Outcome: 04 Behaviour (observed) of 5 to 7 yr olds - post-test at less than 1 month

Study	Intervention N	Intervention Mean(SD)	Control N	Control Mean(SD)	Standardised Mean Difference (Random) 95% CI	Standardised Mean D 95%
01 Post-test Transfer Score (maximum score 31) - Attitude game versus No training						
Renaud 1989	33	8.70 (3.10)	30	7.90 (3.70)		0.23 [-0.26, 0.73]
02 Post-test Transfer Score (maximum score 31) - Behaviour game versus No training						
Renaud 1989	32	10.40 (2.10)	30	7.90 (3.70)		0.83 [0.31, 1.35]
03 Post-test Transfer Score (maximum score 31) - Attitude%Behaviour game versus No training						
Renaud 1989	33	10.10 (2.30)	30	7.90 (3.70)		0.71 [0.20, 1.22]

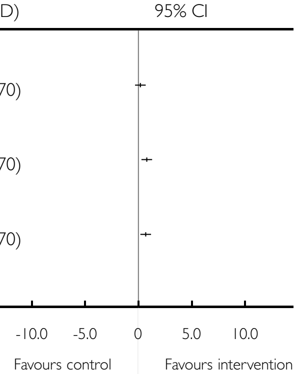


Fig. 3. Comparison 01. Direct education compared to No education

01.08 Behaviour (observed) of institutionalised adults - post-test at less than 1 month

Review: Safety education of pedestrians for injury prevention

Comparison: 01 Direct education compared to No education

Outcome: 08 Behaviour (observed) of institutionalised adults - post-test at less than 1 month

Study	Intervention n/N	Control n/N	Relative Risk (Random) 95% CI	Relative Risk (Random) 95% CI
01 Post-test mean proportion of steps correct - classroom versus control				
Matson 1980	100/130	21/130		4.76 [3.18, 7.12]
02 Post-test mean proportion of steps correct - "independence" versus control				
Matson 1980	117/130	21/130		5.57 [3.75, 8.28]

0.1 0.2 0.5 1 2 5 10

Favours control Favours intervention

Fig. 4. Comparison 01. Direct education compared to No education

01.10 Attitude of 5 to 7 yr olds - post-test at less than 1 month

Review: Safety education of pedestrians for injury prevention

Comparison: 01 Direct education compared to No education

Outcome: 10 Attitude of 5 to 7 yr olds - post-test at less than 1 month

Study	Intervention N	Intervention Mean(SD)	Control N	Control Mean(SD)	Standardised Mean Difference (Random) 95% CI	Standardised
01 Post-test Behaviour Score (maximum score 5) - Attitude game versus No training						
Renaud 1989	35	4.90 (0.20)	34	4.00 (1.10)		1.13 [0.62,
02 Post-test Behaviour Score (maximum score 5) - Behaviour game versus No training						
Renaud 1989	33	4.30 (0.80)	34	4.00 (1.10)		0.31 [-0.17,
03 Post-test Behaviour Score (maximum score 5) - Attitude%Behaviour game versus No training						
Renaud 1989	34	4.50 (0.70)	34	4.00 (1.10)		0.54 [0.05,
04 Post-test Attitude Score (maximum score 3) - Attitude game versus No training						
Renaud 1989	35	1.90 (0.70)	33	1.30 (0.70)		0.85 [0.35,
05 Post-test Attitude Score (maximum score 3) - Behaviour game versus No training						
Renaud 1989	33	1.80 (0.60)	33	1.30 (0.70)		0.76 [0.26,
06 Post-test Attitude Score (maximum score 3) - Attitude%Behaviour game versus No training						
Renaud 1989	34	2.00 (0.70)	33	1.30 (0.70)		0.99 [0.48,
07 Post-test proportion of routes categorised as Safe - Roadside training versus No training						
Thomson 1992	10	0.35 (0.21)	10	0.12 (0.25)		0.95 [0.02,
08 Post-test proportion of routes categorised as Safe - Table top training versus No training						

-10.0 -5.0 0 5.0 10.0

Favours control Favours intervention

(Continued ...)

(. . . *Continued*)

Study	Intervention N	Intervention Mean(SD)	Control N	Control Mean(SD)	Standardised Mean Difference (Random) 95% CI	Standardised
Thomson 1992	10	0.37 (0.21)	10	0.12 (0.25)		1.04 [0.09,
09 Post-test proportion of routes categorised as Safe - (Table top % roadside training) versus No training						
Thomson 1998	30	0.43 (0.31)	30	0.13 (0.09)		1.30 [0.74,

-10.0 -5.0 0 5.0 10.0

Favours control Favours intervention

Fig. 5. Comparison 01. Direct education compared to No education

01.11 Attitude of 5 to 7 yr olds - post-test at 1 to 3 months

Review: Safety education of pedestrians for injury prevention

Comparison: 01 Direct education compared to No education

Outcome: 11 Attitude of 5 to 7 yr olds - post-test at 1 to 3 months

Study	Intervention N	Intervention Mean(SD)	Control N	Control Mean(SD)	Standardised Mean Difference (Random) 95% CI	Standardised
01 Post-test proportion of routes categorised as Safe - Roadside training versus No training						
Thomson 1992	10	0.34 (0.18)	10	0.12 (0.24)		0.99 [0.05,
02 Post-test proportion of routes categorised as Safe - Table top training versus No training						
Thomson 1992	10	0.37 (0.16)	10	0.12 (0.24)		1.17 [0.21, 2
03 Post-test proportion of routes categorised as Safe - (Table top % roadside training) versus No training						
Thomson 1998	30	0.35 (0.29)	30	0.16 (0.19)		0.76 [0.24,

-10.0 -5.0 0 5.0 10.0

Favours control Favours intervention

Fig. 6. Comparison 01. Direct education compared to No education

01.12 Attitude of 5 to 7 yr olds - post-test at 7 to 9 months

Review: Safety education of pedestrians for injury prevention

Comparison: 01 Direct education compared to No education

Outcome: 12 Attitude of 5 to 7 yr olds - post-test at 7 to 9 months

Study	Intervention N	Intervention Mean(SD)	Control N	Control Mean(SD)	Standardised Mean Difference (Random) 95% CI	Standardised Mean Difference (Random) 95% CI
03 Post-test proportion of routes categorised as Safe - (Table top % roadside training) versus No training						
Ampofo-Boateng 1993	10	0.38 (0.23)	10	0.12 (0.15)		1.28 [0.30, 2.26]

-10.0 -5.0 0 5.0 10.0

Favours control Favours intervention

Fig. 7. Comparison 01. Direct education compared to No education

01.13 Attitude of 7 to 8 yr olds - post-test at less than 1 month

Review: Safety education of pedestrians for injury prevention
Comparison: 01 Direct education compared to No education
Outcome: 13 Attitude of 7 to 8 yr olds - post-test at less than 1 month

Study	Intervention n/N	Control n/N	Relative Risk (Random) 95% CI	Relative Risk (Random) 95% CI
01 Apply "concept of speed"				
Cross 1988	62/69	49/69		1.27 [1.07, 1.50]

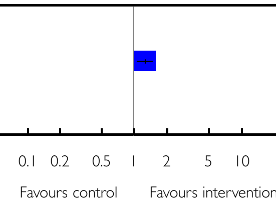


Fig. 8. Comparison 01. Direct education compared to No education

01.14 Attitude (change) of 5 to 7 yr olds - post-test at less than 1 month

Review: Safety education of pedestrians for injury prevention
Comparison: 01 Direct education compared to No education
Outcome: 14 Attitude (change) of 5 to 7 yr olds - post-test at less than 1 month

Study	Intervention N	Intervention Mean(SD)	Control N	Control Mean(SD)	Standardised Mean Difference (Random) 95% CI	Standardis
01 Change in proportion of routes categorised as Safe - Roadside training versus No training						
Thomson 1992	10	0.25 (0.19)	10	0.08 (0.23)		0.77 [-0.1
02 Change in proportion of routes categorised as Safe - (Table top % roadside training) versus No training						
Thomson 1998	30	0.28 (0.27)	30	-0.03 (0.11)		1.48 [0.91
03 Change in proportion of routes categorised as Safe - Table top training versus No training						
Thomson 1992	10	0.23 (0.18)	10	0.08 (0.23)		0.70 [-0.2

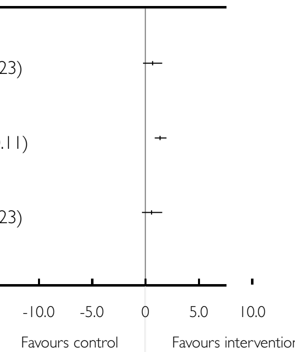


Fig. 9. Comparison 01. Direct education compared to No education

01.15 Attitude (change) of 5 to 7 yr olds - post-test at 1 to 3 months

Review: Safety education of pedestrians for injury prevention

Comparison: 01 Direct education compared to No education

Outcome: 15 Attitude (change) of 5 to 7 yr olds - post-test at 1 to 3 months

Study	Intervention N	Intervention Mean(SD)	Control N	Control Mean(SD)	Standardised Mean Difference (Random) 95% CI	Standardise
01 Change in proportion of routes categorised as Safe - Roadside training versus No training						
Thomson 1992	10	0.24 (0.16)	10	0.08 (0.22)		0.80 [-0.12
02 Change in proportion of routes categorised as Safe - (Table top % roadside training) versus No training						
Thomson 1998	30	0.20 (0.25)	30	0.00 (0.17)		0.92 [0.39,
03 Change in proportion of routes categorised as Safe - Table top training versus No training						
Thomson 1992	10	0.23 (0.14)	10	0.08 (0.22)		0.78 [-0.14

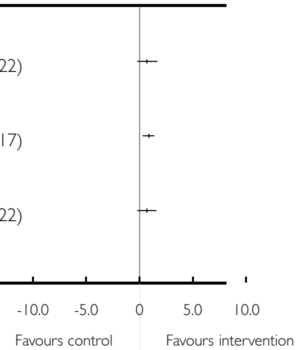


Fig. 10. Comparison 01. Direct education compared to No education

01.22 Knowledge of 5 to 7 yr olds - post-test at 4 to 6 months

Review: Safety education of pedestrians for injury prevention

Comparison: 01 Direct education compared to No education

Outcome: 22 Knowledge of 5 to 7 yr olds - post-test at 4 to 6 months

Study	Intervention N	Intervention Mean(SD)	Control N	Control Mean(SD)	Standardised Mean Difference (Random) 95% CI	Standardised Mean Difference (Random) 95% CI
01 Post-test score of "Crossing the street" test (maximum score 16)						
Luria 2000	90	6.21 (2.50)	91	5.63 (2.60)		0.23 [-0.07, 0.52]

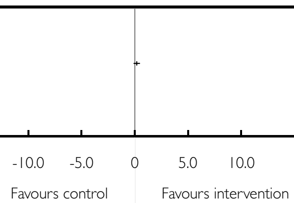


Fig. 11. Comparison 01. Direct education compared to No education

01.26 Knowledge (change) of 5 to 7 yr olds - post-test at 4 to 6 months

Review: Safety education of pedestrians for injury prevention

Comparison: 01 Direct education compared to No education

Outcome: 26 Knowledge (change) of 5 to 7 yr olds - post-test at 4 to 6 months

Study	Intervention N	Intervention Mean(SD)	Control N	Control Mean(SD)	Standardised Mean Difference (Random) 95% CI
01 Change in score of "Crossing the street" test (maximum score 16)					
Luria 2000	90	1.90 (2.70)	91	1.40 (3.50)	0.16 [-0.13, 0.45]

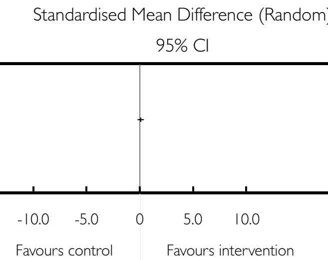


Fig. 12. Comparison 02. Indirect education versus No education

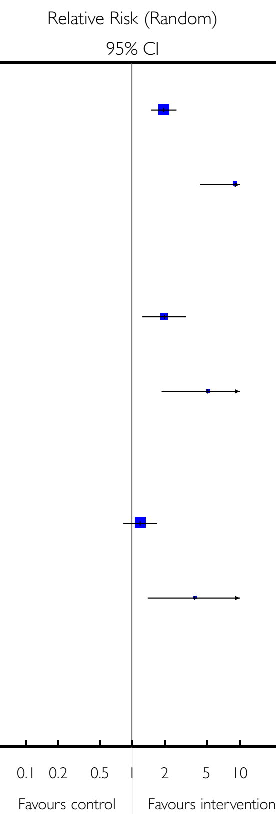
02.01 Behaviour (observed) of 3 to 4 yr olds - post-test at 1 to 3 months

Review: Safety education of pedestrians for injury prevention

Comparison: 02 Indirect education versus No education

Outcome: 01 Behaviour (observed) of 3 to 4 yr olds - post-test at 1 to 3 months

Study	Intervention n/N	Control n/N	Relative Risk (Random) 95% CI
01 Stop at kerb - no distraction			
Limbourg 1981	95/114	31/73	1.96 [1.48, 2.59]
02 Stop at kerb - distraction (competition)			
Limbourg 1981	87/114	6/73	9.29 [4.28, 20.12]
03 Stop at kerb - distraction (alone)			
04 Stop at line of vision - no distraction			
Limbourg 1981	50/114	16/73	2.00 [1.24, 3.23]
05 Stop at line of vision - distraction (competition)			
Limbourg 1981	32/114	4/73	5.12 [1.89, 13.88]
06 Stop at line of vision - distraction (alone)			
07 Stop % look at kerb - no distraction			
Limbourg 1981	50/114	27/73	1.19 [0.82, 1.71]
08 Stop % look at kerb - distraction (competition)			
Limbourg 1981	24/114	4/73	3.84 [1.39, 10.62]
09 Stop % look at kerb - distraction (alone)			
10 Stop % look at line of vision - no distraction			



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Study	Intervention n/N	Control n/N	Relative Risk (Random) 95% CI	Relative Risk (Random) 95% CI
Limbourg 1981	36/114	16/73		1.44 [0.86, 2.40]
11 Stop % look at line of vision - distraction (competition)				
Limbourg 1981	22/114	3/73		4.70 [1.46, 15.13]
12 Stop % look at line of vision - distraction (alone)				

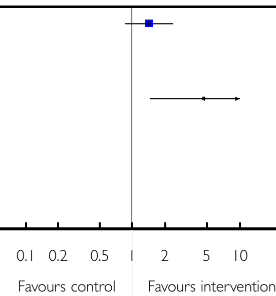


Fig. 13. Comparison 02. Indirect education versus No education

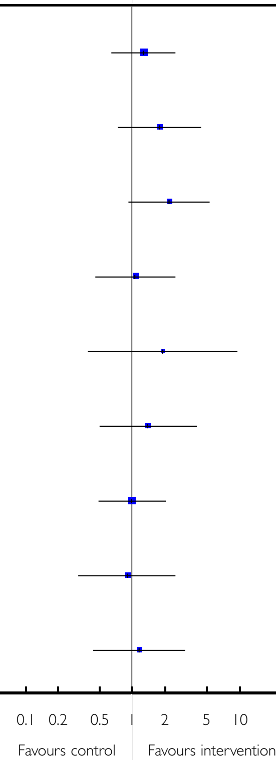
02.02 Behaviour (observed) of 3 to 4 yr olds - post-test at 4 to 6 months

Review: Safety education of pedestrians for injury prevention

Comparison: 02 Indirect education versus No education

Outcome: 02 Behaviour (observed) of 3 to 4 yr olds - post-test at 4 to 6 months

Study	Intervention n/N	Control n/N	Relative Risk (Random) 95% CI	Relative Risk (Random) 95% CI
01 Stop at kerb - no distraction				
Limbourg 1981	18/90	11/70		1.27 [0.64, 2.52]
02 Stop at kerb - distraction (competition)				
Limbourg 1981	14/91	6/70		1.79 [0.73, 4.43]
03 Stop at kerb - distraction (alone)				
Limbourg 1981	17/90	6/70		2.20 [0.92, 5.30]
04 Stop at line of vision - no distraction				
Limbourg 1981	11/90	8/70		1.07 [0.45, 2.52]
05 Stop at line of vision - distraction (competition)				
Limbourg 1981	5/91	2/70		1.92 [0.38, 9.62]
06 Stop at line of vision - distraction (alone)				
Limbourg 1981	9/90	5/70		1.40 [0.49, 3.99]
07 Stop % look at kerb - no distraction				
Limbourg 1981	14/90	11/70		0.99 [0.48, 2.04]
08 Stop % look at kerb - distraction (competition)				
Limbourg 1981	7/91	6/70		0.90 [0.32, 2.55]
09 Stop % look at kerb - distraction (alone)				
Limbourg 1981	9/90	6/70		1.17 [0.44, 3.12]



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Study	Intervention n/N	Control n/N	Relative Risk (Random) 95% CI	Relative Risk (Random) 95% CI
10 Stop % look at line of vision - no distraction				
Limbourg 1981	11/90	5/70		1.71 [0.62, 4.70]
11 Stop % look at line of vision - distraction (competition)				
Limbourg 1981	4/91	4/70		0.77 [0.20, 2.97]
12 Stop % look at line of vision - distraction (alone)				
Limbourg 1981	5/90	8/70		0.49 [0.17, 1.42]

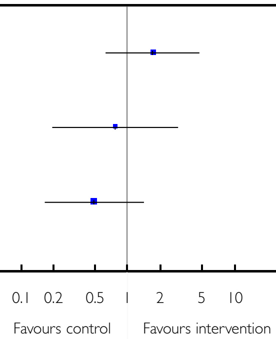


Fig. 14. Comparison 02. Indirect education versus No education

02.03 Behaviour (observed) of 5 to 7 yr olds - post-test at less than 1 month

Review: Safety education of pedestrians for injury prevention

Comparison: 02 Indirect education versus No education

Outcome: 03 Behaviour (observed) of 5 to 7 yr olds - post-test at less than 1 month

Study	Intervention n/N	Control n/N	Relative Risk (Random) 95% CI	Relative Risk (Random) 95% CI
21 Stop at kerb - Parked cars - no distraction				
Thomson 1997a	63/66	24/36		1.43 [1.13, 1.81]
Thomson 1997b	52/53	33/38		1.13 [0.99, 1.29]
22 Stop at kerb - Junctions - no distraction				
Thomson 1997b	56/57	27/31		1.13 [0.98, 1.30]
23 Stop at line of vision - Parked cars - no distraction				
Thomson 1997a	61/66	20/36		1.66 [1.23, 2.25]
Thomson 1997b	50/53	14/38		2.56 [1.68, 3.90]
24 Look three times at line of vision - Parked cars - no distraction				
Thomson 1997a	56/66	18/36		1.70 [1.21, 2.39]
Thomson 1997b	48/53	13/38		2.65 [1.69, 4.15]
25 Choose a position that offers a clear view - Junctions - no distraction				
Thomson 1997b	42/57	19/31		1.20 [0.87, 1.66]

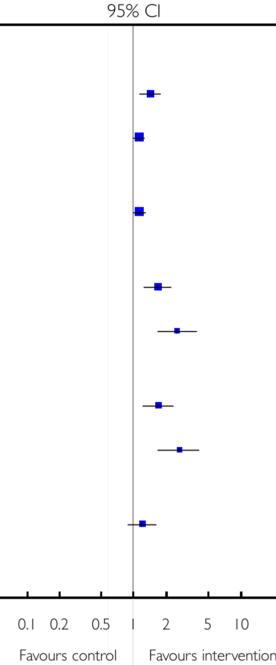


Fig. 15. Comparison 02. Indirect education versus No education

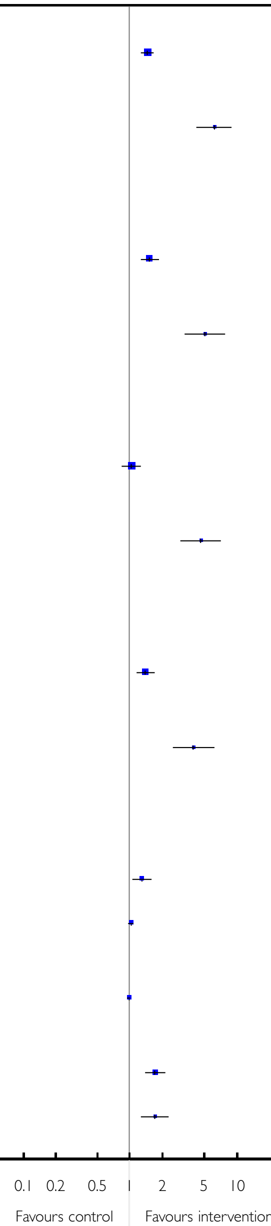
02.05 Behaviour (observed) of 5 to 7 yr olds - post-test at 1 to 3 months

Review: Safety education of pedestrians for injury prevention

Comparison: 02 Indirect education versus No education

Outcome: 05 Behaviour (observed) of 5 to 7 yr olds - post-test at 1 to 3 months

Study	Intervention n/N	Control n/N	Relative Risk (Random) 95% CI
01 Stop at kerb - no distraction			
Limbourg 1981	136/166	104/186	1.47 [1.27, 1.70]
02 Stop at kerb - distraction (competition)			
Limbourg 1981	133/166	24/186	6.21 [4.24, 9.09]
03 Stop at kerb - distraction (alone)			
04 Stop at line of vision - no distraction			
Limbourg 1981	110/166	80/186	1.54 [1.26, 1.88]
05 Stop at line of vision - distraction (competition)			
Limbourg 1981	91/166	20/186	5.10 [3.30, 7.89]
06 Stop at line of vision - distraction (alone)			
07 Stop % look at kerb - no distraction			
Limbourg 1981	88/166	95/186	1.04 [0.85, 1.27]
08 Stop % look at kerb - distraction (competition)			
Limbourg 1981	83/166	20/186	4.65 [2.99, 7.23]
09 Stop % look at kerb - distraction (alone)			
10 Stop % look at line of vision - no distraction			
Limbourg 1981	105/166	84/186	1.40 [1.15, 1.70]
11 Stop % look at line of vision - distraction (competition)			
Limbourg 1981	71/166	20/186	3.98 [2.54, 6.24]
12 Stop % look at line of vision - distraction (alone)			
21 Stop at kerb - Parked cars - no distraction			
Thomson 1997a	62/66	26/36	1.30 [1.05, 1.61]
Thomson 1997b	53/53	37/38	1.03 [0.97, 1.08]
22 Stop at kerb - Junctions - no distraction			
Thomson 1997b	56/57	31/31	0.98 [0.95, 1.02]
23 Stop at line of vision - Parked cars - no distraction			
Ampofo-Boateng 1993	111/119	40/74	1.73 [1.39, 2.14]
Thomson 1997a	63/66	20/36	1.72 [1.28, 2.31]



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Study	Intervention n/N	Control n/N	Relative Risk (Random) 95% CI	Relative Risk (Random) 95% CI
Thomson 1997b	48/53	20/38		1.72 [1.26, 2.36]
24 Look three times at line of vision - Parked cars - no distraction				
Thomson 1997a	57/66	16/36		1.94 [1.33, 2.83]
Thomson 1997b	45/53	16/38		2.02 [1.37, 2.98]
25 Choose a position that offers a clear view - Junctions - no distraction				
Thomson 1997b	43/57	20/31		1.17 [0.87, 1.58]

0.1 0.2 0.5 1 2 5 10

Favours control Favours intervention

Fig. 16. Comparison 02. Indirect education versus No education

02.06 Behaviour (observed) of 5 to 7 yr olds - post-test at 4 to 6 months

Review: Safety education of pedestrians for injury prevention

Comparison: 02 Indirect education versus No education

Outcome: 06 Behaviour (observed) of 5 to 7 yr olds - post-test at 4 to 6 months

Study	Intervention n/N	Control n/N	Relative Risk (Random) 95% CI	Relative Risk (Random) 95% CI
01 Stop at kerb - no distraction				
Limbourg 1981	47/150	48/167		1.09 [0.78, 1.53]
02 Stop at kerb - distraction (competition)				
Limbourg 1981	31/149	17/167		2.04 [1.18, 3.54]
03 Stop at kerb - distraction (alone)				
Limbourg 1981	42/150	28/167		1.67 [1.09, 2.55]
04 Stop at line of vision - no distraction				
Limbourg 1981	51/150	20/167		2.84 [1.78, 4.53]
05 Stop at line of vision - distraction (competition)				
Limbourg 1981	30/149	10/167		3.36 [1.70, 6.64]
06 Stop at line of vision - distraction (alone)				
Limbourg 1981	45/150	25/167		2.00 [1.30, 3.10]
07 Stop % look at kerb - no distraction				
Limbourg 1981	44/150	42/167		1.17 [0.81, 1.67]
08 Stop % look at kerb - distraction (competition)				
Limbourg 1981	28/149	13/167		2.41 [1.30, 4.49]
09 Stop % look at kerb - distraction (alone)				

0.1 0.2 0.5 1 2 5 10

Favours control Favours intervention

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Study	Intervention n/N	Control n/N	Relative Risk (Random) 95% CI	Relative Risk (Random) 95% CI
Limbourg 1981	35/150	25/167		1.56 [0.98, 2.48]
10 Stop % look at line of vision - no distraction				
Limbourg 1981	45/150	28/167		1.79 [1.18, 2.72]
11 Stop % look at line of vision - distraction (competition)				
Limbourg 1981	25/149	10/167		2.80 [1.39, 5.64]
12 Stop % look at line of vision - distraction (alone)				
Limbourg 1981	32/150	22/167		1.62 [0.99, 2.66]

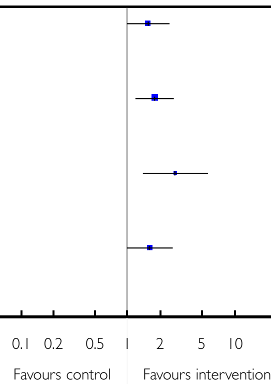


Fig. 17. Comparison 02. Indirect education versus No education

02.07 Behaviour (reported) of 5 to 7 yr olds - post-test at 4 to 6 months

Review: Safety education of pedestrians for injury prevention

Comparison: 02 Indirect education versus No education

Outcome: 07 Behaviour (reported) of 5 to 7 yr olds - post-test at 4 to 6 months

Study	Intervention n/N	Control n/N	Relative Risk (Random) 95% CI	Relative Risk (Random) 95% CI
01 "Always cross in crosswalks" according to parents - Beltman program versus No training				
Miller 1982	19/37	10/44		2.26 [1.20, 4.24]
02 "Always cross in crosswalks" according to parents - (Beltman+Booster) versus No training				
Miller 1982	14/52	10/44		1.18 [0.59, 2.40]
03 "Always look before crossing" according to parents - Beltman program versus No training				
Miller 1982	20/37	17/44		1.40 [0.87, 2.25]
04 "Always look before crossing" according to parents - (Beltman+Booster) versus No training				
Miller 1982	35/52	17/44		1.74 [1.15, 2.65]

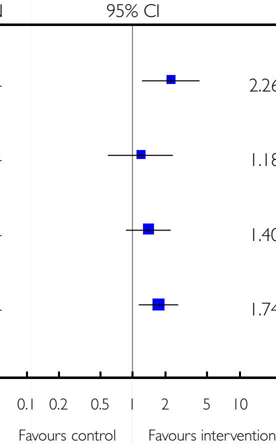


Fig. 18. Comparison 02. Indirect education versus No education

02.10 Attitude of 5 to 7 yr olds - post-test at less than 1 month

Review: Safety education of pedestrians for injury prevention

Comparison: 02 Indirect education versus No education

Outcome: 10 Attitude of 5 to 7 yr olds - post-test at less than 1 month

Study	Intervention N	Intervention Mean(SD)	Control N	Control Mean(SD)	Standardised Mean Difference (Random) 95% CI
01 Post-test proportion of routes categorised as Safe - Roadside training versus No training					
Thomson 1997a	50	0.26 (0.23)	57	0.15 (0.20)	0.51 [0.12, 0.90]

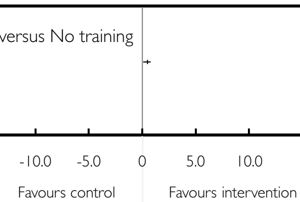


Fig. 19. Comparison 02. Indirect education versus No education

02.11 Attitude of 5 to 7 yr olds - post-test at 1 to 3 months

Review: Safety education of pedestrians for injury prevention

Comparison: 02 Indirect education versus No education

Outcome: 11 Attitude of 5 to 7 yr olds - post-test at 1 to 3 months

Study	Intervention N	Intervention Mean(SD)	Control N	Control Mean(SD)	Standardised Mean Difference (Random) 95% CI
01 Post-test proportion of routes categorised as Safe - Roadside training versus No training					
Thomson 1997a	50	0.21 (0.18)	57	0.19 (0.21)	0.10 [-0.28, 0.48]

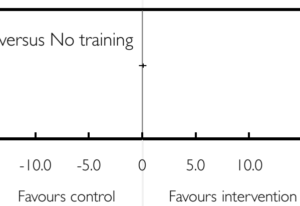


Fig. 20. Comparison 02. Indirect education versus No education

02.14 Attitude (change) of 5 to 7 yr olds - post-test at less than 1 month

Review: Safety education of pedestrians for injury prevention

Comparison: 02 Indirect education versus No education

Outcome: 14 Attitude (change) of 5 to 7 yr olds - post-test at less than 1 month

Study	Intervention N	Intervention Mean(SD)	Control N	Control Mean(SD)	Standardised Mean Difference (Random) 95% CI	Standardise
01 Change in proportion of routes categorised as Safe - Roadside training versus No training						
Thomson 1997a	50	0.19 (0.20)	57	0.07 (0.17)		0.65 [0.26,
02 Change in proportion of routes categorised as Safe or More safe - Roadside training versus No training						
Thomson 1997a	50	0.26 (0.23)	57	0.15 (0.20)		0.51 [0.12,

-10.0 -5.0 0 5.0 10.0

Favours control Favours intervention

Fig. 21. Comparison 02. Indirect education versus No education

02.15 Attitude (change) of 5 to 7 yr olds - post-test at 1 to 3 months

Review: Safety education of pedestrians for injury prevention

Comparison: 02 Indirect education versus No education

Outcome: 15 Attitude (change) of 5 to 7 yr olds - post-test at 1 to 3 months

Study	Intervention N	Intervention Mean(SD)	Control N	Control Mean(SD)	Standardised Mean Difference (Random) 95% CI	Standardise
01 Change in proportion of routes categorised as Safe - Roadside training versus No training						
Thomson 1997a	50	0.14 (0.16)	57	0.11 (0.18)		0.17 [-0.21
02 Change in proportion of routes categorised as Safe or More safe - Roadside training versus No training						
Thomson 1997a	50	0.21 (0.18)	57	0.19 (0.21)		0.10 [-0.28

-10.0 -5.0 0 5.0 10.0

Favours control Favours intervention

Fig. 22. Comparison 02. Indirect education versus No education

02.20 Knowledge of 3 yr olds - post-test at 1 to 3 months

Review: Safety education of pedestrians for injury prevention

Comparison: 02 Indirect education versus No education

Outcome: 20 Knowledge of 3 yr olds - post-test at 1 to 3 months

Study	Intervention n/N	Control n/N	Relative Risk (Random) 95% CI
01 "Hold hands" - Road safety booklet after an interview versus Interview only			
Downing 1981	40/118	49/104	0.72 [0.52, 0.99]
02 "Hold hands" - Road safety booklet with a letter versus No intervention			
Downing 1981	157/435	71/223	1.13 [0.90, 1.43]
03 "Walk / stay on pavement" - Road safety booklet after an interview versus Interview only			
Downing 1981	21/118	25/104	0.74 [0.44, 1.24]
04 "Walk / stay on pavement" - Road safety booklet with a letter versus No intervention			
Downing 1981	87/435	36/223	1.24 [0.87, 1.76]
05 "Look / watch out for cars" - Road safety booklet after an interview versus Interview only			
Downing 1981	17/118	9/104	1.66 [0.78, 3.57]
06 "Look / watch out for cars" - Road safety booklet with a letter versus No intervention			
Downing 1981	52/435	20/223	1.33 [0.82, 2.18]

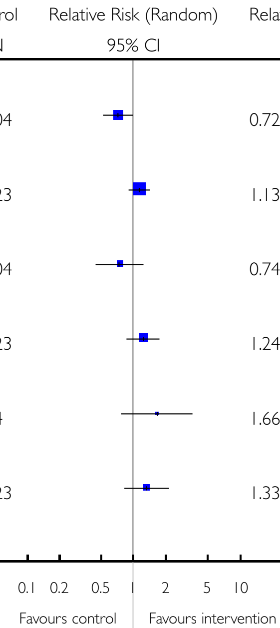


Fig. 23. Comparison 02. Indirect education versus No education

02.21 Knowledge of 5 to 7 yr olds - post-test at less than 1 month

Review: Safety education of pedestrians for injury prevention

Comparison: 02 Indirect education versus No education

Outcome: 21 Knowledge of 5 to 7 yr olds - post-test at less than 1 month

Study	Intervention N	Intervention Mean(SD)	Control N	Control Mean(SD)
01 Post-test score of "Traffic safety knowledge" test (maximum score 20) - Beltman program versus No training				
Miller 1982	169	17.99 (1.88)	181	15.63 (2.83)
02 Post-test score of "Traffic safety knowledge" test (maximum score 20) - (Beltman+Booster) versus No training				
Miller 1982	188	18.22 (2.05)	181	15.63 (2.83)

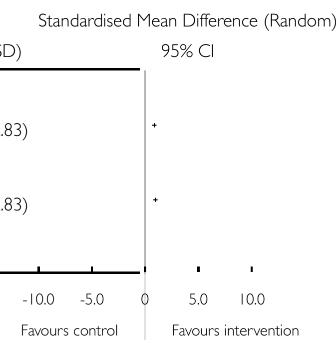


Fig. 24. Comparison 02. Indirect education versus No education

02.22 Knowledge of 5 to 7 yr olds - post-test at 4 to 6 months

Review: Safety education of pedestrians for injury prevention

Comparison: 02 Indirect education versus No education

Outcome: 22 Knowledge of 5 to 7 yr olds - post-test at 4 to 6 months

Study	Intervention N	Intervention Mean(SD)	Control N	Control Mean(SD)	Standardised Mean Difference (Random) 95% CI
01 Post-test score of "Traffic safety knowledge" test (maximum score 20) - Beltman program versus No training					
Miller 1982	158	18.06 (1.92)	179	16.31 (2.58)	
02 Post-test score of "Traffic safety knowledge" test (maximum score 20) - (Beltman+Booster) versus No training					
Miller 1982	181	18.27 (1.74)	179	16.31 (2.58)	
03 Post-test score of "Cognitive" test (maximum score 54)					
Singh 1979	748	34.10 (10.50)	615	30.45 (6.45)	

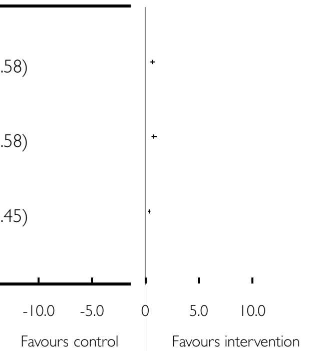


Fig. 25. Comparison 02. Indirect education versus No education

02.23 Knowledge of 7 to 9 yr olds - post-test at 4 to 6 months

Review: Safety education of pedestrians for injury prevention

Comparison: 02 Indirect education versus No education

Outcome: 23 Knowledge of 7 to 9 yr olds - post-test at 4 to 6 months

Study	Intervention N	Intervention Mean(SD)	Control N	Control Mean(SD)	Standardised Mean Difference (Random) 95% CI	Standardised Mean Difference (Random) 95% CI
01 Post-test score of "Cognitive" test (maximum score 64)						
Singh 1979	768	44.86 (10.85)	528	33.56 (11.12)		1.03 [0.91, 1.15]

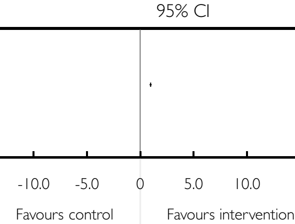


Fig. 26. Comparison 02. Indirect education versus No education

02.24 Knowledge of 8 to 11 yr olds - post-test at less than 1 month

Review: Safety education of pedestrians for injury prevention

Comparison: 02 Indirect education versus No education

Outcome: 24 Knowledge of 8 to 11 yr olds - post-test at less than 1 month

Study	Intervention N	Intervention Mean(SD)	Control N	Control Mean(SD)	Standardised Mean Difference (Random) 95% CI	Standardised Mean Difference (Random) 95% CI
01 Post-test score of "Conspicuity, mass, speed and control" test (maximum score 100)						
Bouck 1992	16	83.30 (10.60)	16	35.70 (25.30)		2.39 [1.46, 3.33]

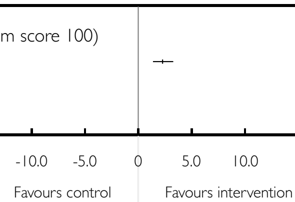


Fig. 27. Comparison 02. Indirect education versus No education

02.25 Knowledge of 10 to 13 yr olds - post-test at 4 to 6 months

Review: Safety education of pedestrians for injury prevention

Comparison: 02 Indirect education versus No education

Outcome: 25 Knowledge of 10 to 13 yr olds - post-test at 4 to 6 months

Study	Intervention N	Intervention Mean(SD)	Control N	Control Mean(SD)	Standardised Mean Difference (Random) 95% CI	Standardised Mean Difference (Random) 95% CI
01 Post-test score of "Cognitive" test (maximum score 64)						
Singh 1979	784	35.46 (7.09)	581	31.75 (8.26)		0.49 [0.38, 0.60]

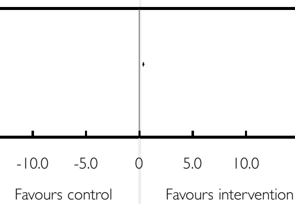


Fig. 28. Comparison 02. Indirect education versus No education

02.26 Knowledge (change) of 5 to 7 yr olds - post-test at less than 1 month

Review: Safety education of pedestrians for injury prevention

Comparison: 02 Indirect education versus No education

Outcome: 26 Knowledge (change) of 5 to 7 yr olds - post-test at less than 1 month

Study	Intervention N	Intervention Mean(SD)	Control N	Control Mean(SD)
01 Change in score of "Traffic safety knowledge" test (maximum score 20) - Beltman program versus No training				
Miller 1982	170	4.77 (2.67)	178	1.89 (3.04)
02 Change in score of "Traffic safety knowledge" test (maximum score 20) - (Beltman+Booster) versus No training				
Miller 1982	202	4.82 (2.74)	178	1.89 (3.04)

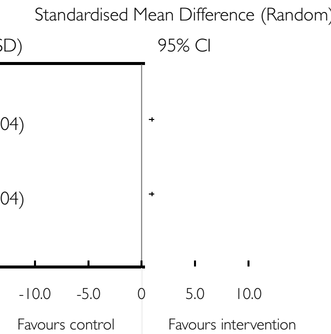


Fig. 29. Comparison 02. Indirect education versus No education

02.27 Knowledge (change) of 5 to 7 yr olds - post-test at 4 to 6 months

Review: Safety education of pedestrians for injury prevention

Comparison: 02 Indirect education versus No education

Outcome: 27 Knowledge (change) of 5 to 7 yr olds - post-test at 4 to 6 months

Study	Intervention N	Intervention Mean(SD)	Control N	Control Mean(SD)
01 Change in score of "Traffic safety knowledge" test (maximum score 20) - Beltman program versus No training				
Miller 1982	170	4.84 (2.68)	178	2.57 (2.95)
02 Change in score of "Traffic safety knowledge" test (maximum score 20) - (Beltman+Booster) versus No training				
Miller 1982	202	4.87 (2.70)	178	2.57 (2.95)
03 Change in score of "Cognitive" test (maximum score 54)				
Singh 1979	748	6.18 (9.18)	615	2.35 (6.85)

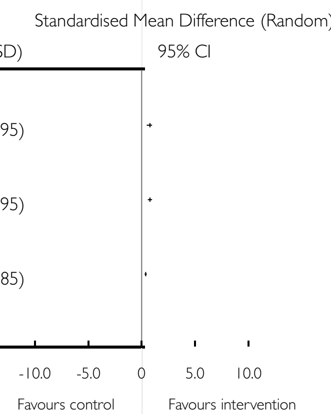


Fig. 30. Comparison 02. Indirect education versus No education

02.28 Knowledge (change) of 7 to 9 yr olds - post-test at 4 to 6 months

Review: Safety education of pedestrians for injury prevention
Comparison: 02 Indirect education versus No education
Outcome: 28 Knowledge (change) of 7 to 9 yr olds - post-test at 4 to 6 months

Study	Intervention N	Intervention Mean(SD)	Control N	Control Mean(SD)	Standardised Mean Difference (Random) 95% CI	Standardised Mean Difference (Random) 95% CI
01 Change in score of "Cognitive" test (maximum score 64)						
Singh 1979	768	12.66 (11.46)	528	1.74 (11.12)		0.96 [0.85, 1.08]

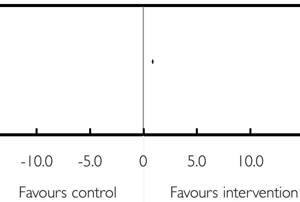
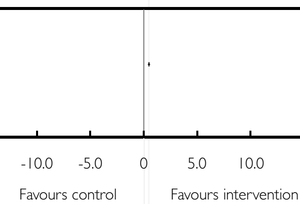


Fig. 31. Comparison 02. Indirect education versus No education

02.29 Knowledge (change) of 10 to 13 yr olds - post-test at 4 to 6 months

Review: Safety education of pedestrians for injury prevention
Comparison: 02 Indirect education versus No education
Outcome: 29 Knowledge (change) of 10 to 13 yr olds - post-test at 4 to 6 months

Study	Intervention N	Intervention Mean(SD)	Control N	Control Mean(SD)	Standardised Mean Difference (Random) 95% CI	Standardised Mean Difference (Random) 95% CI
01 Change in score of "Cognitive" test (maximum score 64)						
Singh 1979	784	6.70 (7.14)	581	2.42 (7.96)		0.57 [0.46, 0.68]



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Post-licence driver education for the prevention of road traffic crashes: a systematic review of randomised controlled trials

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Abstract

The effectiveness of post-licence driver education for preventing road traffic crashes was quantified using a systematic review and meta-analyses of randomised controlled trials. Searches of appropriate electronic databases, the Internet and reference lists of relevant papers were conducted. The searches were not restricted by language or publication status. Data were pooled from 21 randomised controlled trials, including over 300,000 full licence-holding drivers of all ages. Nineteen trials reported subsequent traffic offences, with a pooled relative risk of 0.96 (95% confidence interval 0.94, 0.98). Fifteen trials reported traffic crashes with a pooled relative risk of 0.98 (0.96, 1.01). Four trials reported injury crashes with a pooled relative risk of 1.12 (0.88, 1.41). The results provide no evidence that post-licence driver education is effective in preventing road injuries or crashes. Although the results are compatible with a small reduction in the occurrence of traffic crashes, this may be due to selection biases or bias in the included trials.

Keywords: Driver education; Injury prevention; Meta-analysis; Motor vehicle; Systematic review; Road traffic crash

1. Introduction

Each year over a million people are killed and some 10 million people are permanently disabled in road traffic crashes, worldwide (Murray and Lopez, 1996). For people under 44 years, road traffic crashes are second only to HIV and AIDS as a cause of death. Furthermore, the evidence suggests that the global epidemic of road traffic injuries is only beginning.

Because driver error is considered to be an important factor in the causation of road traffic crashes, great emphasis has been placed in road safety strategies on efforts to reduce driver error through driver education programmes. Promoting post-licence driver education, enhancing the status of advanced driving qualifications and encouraging extra training for professional drivers are key components of the UK government's road safety strategy (DETR, 2000). Many people drive as part of their job, and traffic crashes are now a leading cause of occupational injury with an estimated 92,000 work-related road traffic deaths each year worldwide (Takala, 1999). In response, some companies invest, at significant cost in driver education programmes for their employees. Driver education programmes have also been offered to drivers who have committed traffic offences. In some cases, drivers attending such programmes are offered lower insurance premiums or can have citations removed from their driving records, on the assumption that they would be 'safer' drivers after the programme.

In the past few years, however, a different perspective on road safety has emerged that emphasises a systems approach

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to improving road safety and that questions an over-reliance on education. It is well established in industrial safety that the operator is only part of a dynamic system with many components and has inherent limitations and predictable error rates. The systems approach accepts driver limitations and aims to reduce traffic crashes by designing the traffic environment with these limitations in mind. From a systems perspective, it is those that build and operate the road system that have the greatest impact on road safety and attempting to eliminate driver error is considered unlikely to have any significant impact in reducing traffic crashes (Mackay and Tiwari, 2001).

The debate about the value of driver education programmes can best be resolved by a consideration of the empirical evidence for their effectiveness. Indeed, the preparation of systematic reviews of the evidence for the effectiveness of road safety interventions has been given a high priority by the World Health Organization in its strategy to reduce traffic injuries. This systematic review was commissioned by the Head of Occupational Safety of AstraZeneca, a large pharmaceutical company, in response to a BMJ editorial (Roberts et al., 2002) calling for road safety policy to be based on evidence. In 2002, the 26,000 AstraZeneca sales representatives drove approximately 514 million km in total and there were 111 traffic crashes, of which 11 were serious. Road traffic injuries account for 59% of all lost time injuries and are the most common cause of injury within the company. The company had responded by providing driver education programmes. This review was conducted in order to establish whether this approach was effective.

2. Methods

2.1. Searching

We searched the following electronic databases: Cochrane Injuries Group's specialised register, Cochrane Central Register of Controlled Trials, MEDLINE, EMBASE, TRANSPORT (NTIS, TRIS, TRANSDOC, IRRD), Road Res. (ARRB), ATRI, National Research Register, PsycInfo, ERIC, SPECTR, Zetoc, SIGLE and Science (and Social Science) Citation Index. We searched the internet, checked reference lists of relevant papers and contacted appropriate organisations. The search was not restricted by language or publication status.

2.2. Selection

We included randomised controlled trials comparing post-licence driver education with no education or one form of post-licence driver education with another (i.e., correspondence, group or individual education). Eligible participants were drivers (including motorcyclists) of all ages and driving experience who held a valid driving licence. Trials of programmes were eligible if they incorporated an education component aimed at the motor vehicle driver. The outcomes of interest were traffic offences, traffic crashes and injury crashes (fatal and non-fatal injuries caused by a crash).

2.3. Validity assessment

We assessed the quality of allocation concealment using the method proposed by Schulz et al. (1995) assigning C to poorest quality and A to best quality.

2.4. Data abstraction

Two reviewers independently extracted data on the type of study, characteristics of intervention and control groups, the types of intervention, duration of follow-up and the outcomes evaluated.

2.5. Quantitative data synthesis

The relative risks and 95% confidence intervals were calculated for each trial and were pooled using a random effects model. Heterogeneity between trials was assessed using a chi-square test, where $p \leq 0.05$ was taken to indicate significant heterogeneity.

The studies included in the meta-analysis reported either the risk of drivers having one or more crashes/offences or the rate of crashes/offences in the follow-up period. In order to pool the results across all studies it was necessary to translate the rate statistic into a risk of having one or more failures. Assuming that the number of crashes/offences follows a negative binomial (overdispersed Poisson) distribution, explicit formulae (McCullagh and Nelder, 1989) link the expected rate with the expected risk provided that the degree of overdispersion is known. For crashes, analysis of data from those studies reporting distributions of the numbers of events, suggested that there was little or no overdispersion, whilst for offences, analysis suggested an overdispersion parameter of 0.37. These estimates of overdispersion were used to estimate expected risks, where only rates were given. The variance of the log risk ratio was calculated via the standard formula.

When the results from more than one intervention group were reported, data were combined from all educational intervention groups as appropriate.

The presence of small study effects was investigated using Egger's weighted regression method.

3. Results

3.1. Quantitative data synthesis

The combined search strategy identified approximately 1300 published and unpublished studies, of which 87 were deemed to be potentially relevant based on the title or abstract. After a full text review 24 trials were judged to meet the inclusion criteria (Table 1) including more than 300,000 randomised participants.

Table 1
Table of included studies

	Methods	Participants	Interventions	Outcomes
Coppin, 1962	Allocation concealment: B Loss to follow-up[a] Blinding of outcome assessment[a]	Drivers eligible for remedial driver education ($n = 440$).	Two groups: group education ($n = 244$). No education ($n = 196$).	Follow-up of 24 months. Subsequent traffic offences and crashes.
Coppin et al., 1965	Allocation concealment: B Loss to follow-up[a] Blinding of outcome assessment: adequate	Drivers eligible for remedial driver education remedial driver education ($n = 2050$).	Two groups: group education ($n = 1440$). No contact control ($n = 610$).	Follow-up of 12 months. Subsequent traffic offences and crashes
Fuchs, 1980	Allocation concealment: C Loss to follow-up[a] Blinding of outcome assessment[a]	Drivers eligible for remedial driver education ($n = 95{,}068$).	Two groups: individual education ($n = 84{,}300$). No education ($n = 10{,}768$).	Follow-up of 12 months. Subsequent traffic offences.
Harano, 1972	Allocation concealment: B Loss to follow-up[a] Blinding of outcome assessment: adequate	Drivers eligible for remedial driver education ($n = 3544$).	Two groups: group education ($n = 1776$). No education, regular court disposition ($n = 1768$).	Follow-up of 12 months. Subsequent traffic offences and crashes.
Helander, 1984a	Allocation concealment: B Loss to follow-up[a] Blinding of outcome assessment[a]	Drivers eligible for remedial driver education ($n = 6867$).	Four groups: individual education ($n = 1712$). Correspondence education ($n = 1709$). Re-examination group ($n = 1734$)§. No contact control ($n = 1712$).	Follow-up of 12 months. Subsequent traffic offences and crashes.
Helander, 1984b	Allocation concealment: B Loss to follow-up[a] Blinding of outcome assessment[a]	Drivers eligible for remedial driver education ($n = 24{,}156$).	Three groups: individual education ($n = 8043$). Correspondence education ($n = 8061$). No contact control ($n = 8052$).	Follow-up of 12 months. Subsequent traffic offences and crashes.
Kadell, 1987a	Allocation concealment: C Loss to follow-up[a] Blinding of outcome assessment: adequate	Drivers eligible for remedial driver education ($n = \sim 19{,}600$).	Four groups: group education ($n = \sim 4900$). Group education ($n = \sim 4900$). Correspondence education ($n = 4900$). No contact control ($n = 4900$).	Follow-up of 12 months. Subsequent traffic offences, traffic crashes and injury crashes.
Kadell, 1987b	Allocation concealment: C Loss to follow-up[a] Blinding of outcome assessment: adequate	Drivers eligible for remedial driver education ($n = \sim 24{,}000$).	Two groups: correspondence education ($n = \sim 12{,}000$). No contact control ($n = \sim 12{,}000$).	Follow-up of 12 months. Subsequent traffic offences, traffic crashes and injury crashes.
Kaestner and Syring, 1967	Allocation concealment: C Loss to follow-up: ∼9%. Blinding of outcome assessment[a]	Drivers eligible for remedial driver education ($n = 1320$).	Two groups: individual education ($n = 660$). No education ($n = 660$).	Follow-up of 12 months. Subsequent traffic offences and crashes.
Kaestner and Speight, 1975	Allocation concealment: B Loss to follow-up: ∼3%. Blinding of outcome assessment: unclear.	Drivers eligible for remedial driver education ($n = 960$).	Five groups: group education ($n = 97$). Correspondence education ($n = 206$). Probationary (restrictive) licence ($n = 222$)§. 30-day driving suspension ($n = 208$). No contact control ($n = 199$).	Follow-up of 12 months.
Kaestner, 1980	Allocation concealment: C Loss to follow-up[a] Blinding of outcome assessment[a]	Drivers eligible for remedial driver education ($n = 1377$).	Three groups: group driver education ($n = 469$). Group driver education ($n = 449$). No contact control ($n = 459$).	Follow-up of 12 months.
Lynn, 1982a	Allocation concealment: B Loss to follow-up[a] Blinding of outcome assessment[a]	Drivers eligible for remedial driver education ($n = 9783$).	Two groups: correspondence education ($n = 4899$). No education ($n = 4884$).	Follow-up of 12 months. Subsequent traffic offences.
Lynn, 1982b	Allocation concealment: B Loss to follow-up[a] Blinding of outcome assessment[a]	Drivers eligible for remedial driver education ($n = 9266$).	Two groups: group education ($n = 4649$). No education ($n = 4617$).	Follow-up of 12 months. Subsequent traffic offences.

Table 1 (*Continued*)

	Methods	Participants	Interventions	Outcomes
Lynn, 1982c	Allocation concealment: B Loss to follow-up[a] Blinding of outcome assessment[a]	Drivers eligible for remedial driver education ($n = 3388$).	Two groups: individual education ($n = 1738$). No education ($n = 1650$).	Follow-up of 12 months. Subsequent traffic offences.
Marsh, 1971	Allocation concealment: C Loss to follow-up[a] Blinding of outcome assessment[a]	Drivers eligible for remedial driver education ($n = 15,290$).	Nine groups: group education (involving five treatment groups) ($n = 9114$). Individual education (two types) ($n = 3174$). No education ($n = 1530$).	Follow-up of 12 months. Subsequent traffic offences and crashes.
Marsh, 1978	Allocation concealment: B Loss to follow-up: ∼0.7%. Blinding of outcome assessment: adequate	Drivers eligible for remedial driver education ($n = 17,780$).	Seven groups: group education (involving four treatment groups) ($n = 10150$). Correspondence education groups (involving two treatment groups) ($n = 5086$). No contact control ($n = 2539$).	Follow-up of 12 months. Subsequent traffic offences and crashes
McCoy et al., 1993	Allocation concealment: B Loss to follow-up[a] Blinding of outcome assessment[a]	Drivers eligible for advanced driver education ($n = 105$).	Six groups: group driver education ($n = 15$). The physical therapy ($n = 18$)§. The perceptual therapy ($n = 10$) The physical therapy and driver education ($n = 15$). The perceptual therapy and driver education ($n = 19$). No education ($n = 17$).	Driving performance on a test route as measured by a driver performance measurement (DPM) developed at Michigan State University.
Nolén et al., 2002	Allocation concealment: B Loss to follow-up: 29%. Blinding of outcome assessment: adequate	Drivers eligible for advanced driver education ($n = 2305$).	Two groups: group driver education ($n = 1502$). No education ($n = 803$).	Follow-up of 12 months. Subsequent injury crashes.
Peck et al., 1980	Allocation concealment: C Loss to follow-up[a] Blinding of outcome assessment: adequate	Drivers eligible for remedial driver education ($n = 14,278$).	Two groups: group education ($n = 9318$). No education ($n = 4960$).	Follow-up of 12 months. Subsequent traffic offences and crashes.
Prothero, 1978	Allocation concealment: C Loss to follow-up: 7% Blinding of outcome assessment[a]	Drivers eligible for remedial driver education ($n = 432$).	Three groups: group driver education (involving two treatment groups ($n = 246$). No education ($n = 112$).	Follow-up of 12 months. Subsequent traffic offences and crashes.
Ratz, 1978	Allocation concealment: C Loss to follow-up[a] Blinding of outcome assessment: adequate	Drivers eligible for remedial driver education ($n = 18,749$).	Three groups: group education ($n = 6270$). Individual education ($n = 6116$). No education ($n = 6363$).	Follow-up of 12 months. Subsequent traffic offences, crashes and injury crashes.
Raub et al., 1999	Allocation concealment: A Loss to follow-up[a] Blinding of outcome assessment: adequate	Drivers eligible for remedial driver education ($n = 940$).	Two groups: group driver education ($n = 452$). No education ($n = 488$).	Follow-up of 12 months. Subsequent traffic offences.
Schuman et al., 1971	Allocation concealment: B Loss to follow-up[a] Blinding of outcome assessment: adequate	Drivers eligible for advanced driver education ($n = 133$).	Two groups: group driver education ($n = 19$). No education ($n = 114$).	Follow-up of 24 months. Subsequent traffic crashes.
Stoke, 1980	Allocation concealment: C Loss to follow-up[a] Blinding of outcome assessment: adequate	Drivers eligible for advanced driver education ($n = 47,628$).	Four groups: correspondence education (involving three treatment groups ($n = 35520$). No education ($n = 12,108$).	Follow-up of 24 months. Subsequent traffic offences and crashes.

Three distinct varieties of driver education were identified for inclusion into the analysis. *Correspondence education*: Programmes were considered to be 'correspondence education' if they did not involve direct contact with an instructor. Educational information was usually contained within a manual or letter sent to drivers. *Group education*: Programmes were classed as 'group education, if information was delivered to a group of drivers, hence the content was not designed around the requirement of an individual participant. These generally took the form of one or more pre-organised sessions led by an instructor. *Individual education*: Programmes were classed as 'individual education' if they consisted of a session in which the participating driver received education delivered on a one-on-one basis with an instructor. The content of the information given could often be geared to suit the specific requirements of the driver attending. DDC: Defensive Driving Course; DDSL: Division of Driver Safety and Licensing; DMV: Department of Motor Vehicles; DUI: Driving under the influence; HI/PRI: Home Instruction/Point Reduction Incentive; MSL: Maximum Speed Law; NSC: National Safety Council; NTSI: National Traffic Safety Institute's Traffic Violator Workshop; TSS: Traffic Safety School; UDIS: Uniform Driver Improvement School. Further characteristics of the included trials are given in the Cochrane Library.

[a] Data not available; C: trials in which concealment was inadequate (such as alternation or reference to case record numbers or to dates of birth); B: trials in which the authors either did not report an allocation concealment approach at all or reported an approach that did not fall into one of the other categories; A: trials deemed to have taken adequate measures to conceal allocation (i.e., central randomisation, serially numbered, opaque, sealed envelopes, or other description that contained elements convincing of concealment).

The methodological quality of included trials was generally poor. Using predefined criteria (Schulz et al., 1995) the quality of allocation concealment was adequate in one, unclear in 13 and inadequate in 10 trials. Blinding of outcome assessment was adequate in 11 trials.

We stratified the trials according to two distinct forms of education. The first of these was remedial driver education, aimed at drivers who had poor previous driving records in terms of the number of prior crashes and/or offences. The second form was advanced driver education, aimed at the 'general' driver, which would build upon the initial education given to drivers before obtaining a licence. Of the included trials, four investigated the effectiveness of advanced driver education (McCoy et al., 1993; Nolén et al., 2002; Schuman et al., 1971; Stoke, 1980). The remaining 20 trials studied remedial driver education (Coppin, 1962; Coppin et al., 1965; Fuchs, 1980; Harano, 1972; Helander, 1984; Kadell, 1987; Kaestner and Syring, 1967; Kaestner and Speight, 1975; Kaestner, 1980; Lynn, 1982; Marsh, 1971, 1978; Peck et al., 1980; Prothero, 1978; Ratz, 1978; Raub et al., 1999).

Three of the 24 eligible trials reported outcome data unsuitable for this meta-analysis and were not considered further (Harano, 1972; Kaestner, 1980; McCoy et al., 1993). The analysis was therefore based on 21 trials, providing data on 309,624 participants. Of these, three involved advanced driver education (Nolén et al., 2002; Schuman et al., 1971; Stoke, 1980) and the remaining 18 investigated remedial driver education, (Coppin, 1962; Coppin et al., 1965; Fuchs, 1980; Harano, 1972; Helander, 1984; Kadell, 1987; Kaestner and Syring, 1967; Kaestner and Speight, 1975; Kaestner, 1980; Lynn, 1982; Marsh, 1971, 1978; Peck et al., 1980; Prothero, 1978; Ratz, 1978; Raub et al., 1999) providing data on 49,401 and 260,223 drivers, respectively.

Eight trials compared a no-education control with more than one form (correspondence, group, individual) of driver education and have been included in more than one comparison group.

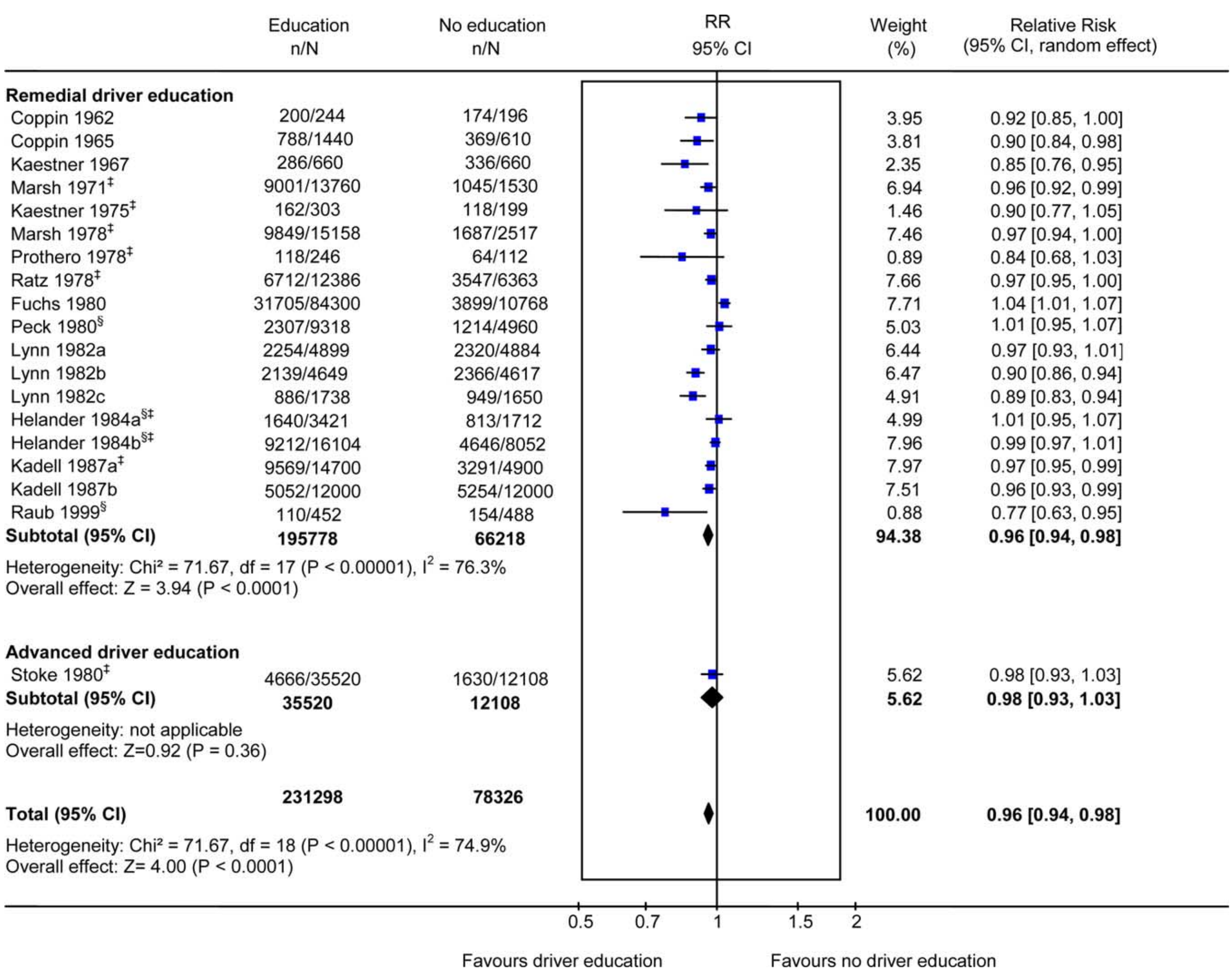

Fig. 1. Pooled relative risk of drivers receiving post-licence driver education committing a traffic offence compared to drivers receiving no education.

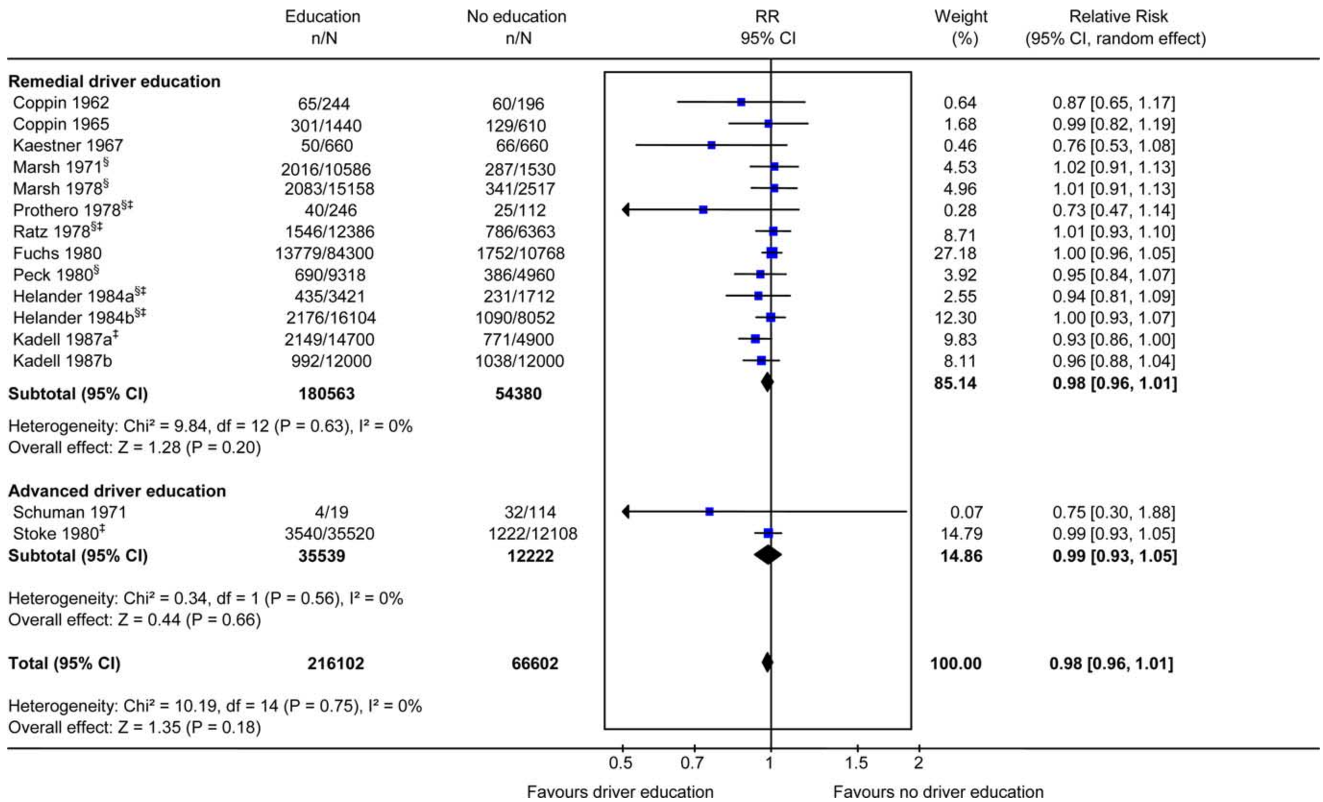

Fig. 2. Pooled relative risk of drivers receiving post-licence driver education being involved in a traffic crash compared to drivers receiving no education.

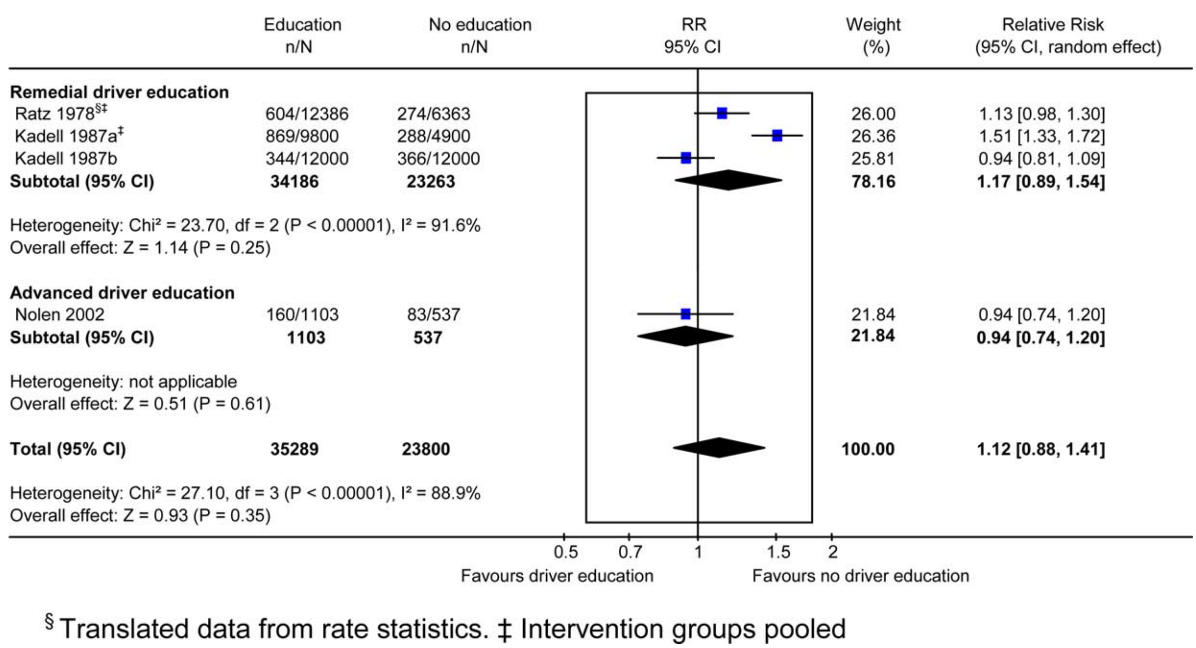

Fig. 3. Pooled relative risk of drivers receiving post-licence driver education being involved in an injury crash compared to drivers receiving no education.

3.2. Traffic offences

Nineteen trials compared the effectiveness of driver education with no education in reducing traffic offences (Fig. 1). The pooled relative risk was 0.96 (95% CI, 0.94–0.98). There was significant heterogeneity between trials (chi-square = 71.67, d.f. = 18, $p \leq 0.00001$). For the 18 trials of remedial driver education the pooled relative risk was 0.96 (95% CI, 0.94–0.98). There was significant heterogeneity between trials (chi-square = 71.67, d.f. = 17, $p \leq 0.00001$). For the one trial of advanced driver education the relative risk was 0.98 (95% CI, 0.93–1.03).

Nine trials compared correspondence driver education with no education. The pooled relative risk was 0.98 (95% CI, 0.97–0.99). There was no significant heterogeneity between trials (chi-square = 7.71, d.f. = 8, $p = 0.46$).

Eleven trials compared group driver education with no education. The pooled relative risk was 0.95 (95% CI, 0.92–0.97). There was significant heterogeneity between trials (chi-square = 23.26, d.f. = 10, $p = 0.0098$).

Seven trials compared individual driver education with no education. The pooled relative risk was 0.95 (95% CI, 0.91–1.00). There was significant heterogeneity between trials (chi-square = 46.09, d.f. = 6, $p \leq 0.00001$).

3.3. Traffic crashes

Fifteen trials compared driver education with no education for preventing crashes (Fig. 2). The pooled relative risk was 0.98 (95% CI, 0.96–1.01). There was no significant heterogeneity between trials (chi-square = 10.19, d.f. = 14, $p = 0.75$). For the 13 trials of remedial driver education the pooled relative risk was 0.98 (95% CI, 0.96–1.01). There was no significant heterogeneity between trials (chi-square = 9.84, d.f. = 12, $p = 0.63$). For the two trials of advanced driver education the pooled relative risk was 0.99 (95% CI, 0.93–1.05). There was no significant heterogeneity between trials (chi-square = 0.34, d.f. =1, $p = 0.56$).

Seven trials compared correspondence driver education with no education. The pooled relative risk was 0.98 (95% CI, 0.95–1.01). There was no significant heterogeneity between trials (chi-square = 4.14, d.f. = 6, $p = 0.66$).

Ten trials compared group driver education with no education. The pooled relative risk was 0.97 (95% CI, 0.93–1.02). There was no significant heterogeneity between trials (chi-square = 5.14, d.f. = 9, $p = 0.82$).

Six trials compared individual driver education with no education. The pooled relative risk was 0.99 (95% CI, 0.96–1.03). There was no significant heterogeneity between trials (chi-square = 5.07, d.f. = 5, $p = 0.41$).

3.4. Injury crashes

Four trials reporting injury crashes presented data suitable for this meta-analysis and compared the effectiveness of driver education with no education (Fig. 3). The pooled relative risk was 1.12 (95% CI, 0.88–1.41). There was significant heterogeneity between trials (chi-square = 27.10, d.f. = 3, $p \leq 0.00001$). For the three trials of remedial driver education the pooled relative risk was 1.17 (95% CI, 0.89–1.54). There was significant heterogeneity between trials (chi-square = 23.70, d.f. = 2, $p \leq 0.00001$). For the one trial of advanced driver education the relative risk was 0.94 (95% CI, 0.74–1.20).

One trial compared correspondence remedial driver education with no education. The relative risk was 0.94 (95% CI, 0.81–1.09).

Three trials compared group driver education with no education. The pooled relative risk was 1.02 (95% CI, 0.93–1.13). There was no significant heterogeneity between trials (chi-square = 0.11, d.f. = 2, $p = 0.57$).

One trial compared individual remedial driver education with no education. The relative risk was 1.18 (95% CI, 1.00–1.38).

The presence of small study effects was assessed using Egger's weighted regression method. There was evidence of small study effects for the outcomes traffic offences (Egger's test bias coefficient = −1.69 (95% CI, −3.14 to −0.23) $p = 0.03$) and traffic crashes (Egger's test bias coefficient = −0.99 (95% CI, −1.81 to −0.17), $p = 0.02$), but not for the injury crashes outcome (Egger's test bias coefficient = −0.53 (95% CI, −11.26–10.19), $p = 0.88$).

4. Discussion

This systematic review of randomised controlled trials provides no evidence that driver education programmes are effective in preventing road traffic injuries or crashes. Although the results are compatible with a small reduction in the occurrence of traffic offences, this may be due to publication or other selection biases, or else to bias in the included trials. Because of the large number of randomised participants included in the meta-analysis, close to 300,000 for some outcomes, we can exclude, with reasonable precision, the possibility of even modest benefits.

Publication and other selection biases are a potential threat to validity in all systematic reviews and in this review there was evidence of funnel plot asymmetry using both graphical and statistical methods. From the graphical presentations it would appear that any such bias would lead to an overestimation of the beneficial effect of driver education. Inadequate allocation concealment, lack of blinding of outcome assessment and large losses to follow-up in many of the include trials also call into question the validity of the observed reduction in traffic offences following driver education.

We pooled the available data in a random effects meta-analysis. In several meta-analyses there was significant heterogeneity and these results should be interpreted cautiously.

We had anticipated that the intervention effect would depend on characteristics of trial participants (remedial or advanced drivers), on the mode of delivery of the educational intervention (correspondence, group and individual) and on the outcome measure used (traffic offences, crashes and injuries) and we stratified the meta-analyses according to these factors. However, in several of the meta-analyses, there was significant heterogeneity even within these strata. Some of this may be explained by trial quality. However, for many of the included trials there was not enough information about quality in the trial report to investigate this further. The residual heterogeneity may also be due to more subtle differences in the study populations, the types of education programmes, or in the way that the outcome data were defined and collected.

The included trials were conducted over a 40-year publication time span, all but four were of remedial driver education and only one was conducted outside the USA. As a result, it may not be appropriate to generalise from this systematic review and make inferences about the effectiveness of present day driver education programmes. On the other hand, we can reasonably conclude that the effectiveness of current driver education programmes is as yet unproven, an observation that casts doubt on the wisdom of placing undue emphasis on this approach in current road safety policy.

Our review was commissioned by AstraZeneca, a company that provides driver education programmes for most of its driving employees in an effort to reduce the occurrence of occupational road traffic injuries and it is important to consider the implications of our results for this policy. Whilst we cannot claim that our results show that this policy is ineffective, we would argue that in the light of the evidence from previous trials, unrealistic expectations about the effectiveness of driver education must be avoided. We would recommend that the company pursue other approaches to reduce road traffic injuries amongst its employees, such as reducing exposure to the risk of road traffic crashes by eliminating all unnecessary car journeys, and by using safer modes of travel, such as train travel (HSE, 2003), whenever possible.

Acknowledgements

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Helmets for preventing injury in motorcycle riders (Review)

Liu B, Ivers R, Norton R, Blows S, Lo SK

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ABSTRACT

Background
Motorcycle crash victims form a high proportion of those killed or injured in road traffic accidents. Injuries to the head, following motorcycle crashes, are a common cause of severe morbidity and mortality. It seems intuitive that helmets should protect against head injuries but it has been argued that motorcycle helmet use decreases rider vision and increases neck injuries. This review will collate the current available evidence on helmets and their impact on mortality, and head, face and neck injuries following motorcycle crashes.

Objectives
To quantify the effectiveness of wearing a motorcycle helmet in reducing mortality and head and neck injury following motorcycle crashes.

Search strategy
Databases including the Cochrane Injuries Group Specialised Register, Cochrane Central Register of Controlled Trials (*The Cochrane Library* issue 1, 2003), MEDLINE (January 1966 to February 2003), EMBASE (January 1985 to February 2003), CINAHL (January 1982 to February 2003), IRRD (International Road Research Documentation), TRANSDOC, TRIS (Transport Research Information Service), ATRI (Australian Transport Index) (1976 to Feb 2003), Science Citation Index were searched for relevant articles. Web sites of traffic and road accident research bodies including government agencies were also searched. Reference lists from topic reviews, identified studies and bibliographies were examined for relevant articles.

Selection criteria
We considered for inclusion studies that investigated a population of motorcycle riders who had crashed, examining helmet use as an intervention and with outcomes that included one or more of the following: death, head, neck or facial injury. Studies included any that compared an intervention and control group and, therefore, included any randomised controlled trials, non-randomised controlled trials, cohort, case-control and cross-sectional studies. Ecological and case series studies were excluded.

Data collection and analysis
Two reviewers independently screened reference lists for eligible articles. Two reviewers independently assessed articles for inclusion criteria. Data were abstracted by two independent reviewers using a standard abstraction form.

Main results
Fifty-three observational studies were identified of varying quality. Despite methodological differences there was a remarkable consistency in results, particularly for mortality and head injury outcomes. Motorcycle helmets appear to reduce the risk of mortality although, due to heterogeneity in study design, an overall estimate of effect was not calculated. There was some evidence that the effect of helmets on mortality is modified by speed. Motorcycle helmets were found to reduce the risk of head injury and from five well-conducted studies the risk reduction is estimated to be 72% (OR 0.28, 95%CI 0.23, 0.35). Insufficient evidence was found to estimate the effect of motorcycle helmets compared with no helmet on facial or neck injuries. However, studies of poorer quality suggest that helmets have no effect on the risk of neck injuries and are protective for facial injury. There was insufficient evidence to demonstrate whether differences in helmet type confer more or less advantage in injury reduction.

Authors' conclusions

Motorcycle helmets reduce the risk of mortality and head injury in motorcycle riders who crash, although the former effect may be modified by other crash factors such as speed. Further well-conducted research is required to determine the effects of helmets and different helmet types on mortality, head, neck and facial injuries. However, the findings suggest that global efforts to reduce road traffic injuries may be facilitated by increasing helmet use by motorcyclists.

SYNOPSIS

Helmets shown to reduce motorcyclist head injury and death

Motorcyclists are at high risk in traffic crashes, particularly for head injury. A review of trials concluded that helmets reduce the risk of head injury by around 72%. The risk of death is also reduced, although it is not possible to estimate a percentage figure for this reduction from the available evidence. It is likely that the protective effect of the helmet may depend on other factors, such as speed. There is, so far, insufficient evidence to compare the effectiveness of different types of helmet. Some studies have suggested that helmets may protect against facial injury and that they have no effect on neck injury, but more research is required for a conclusive answer. The review supports the view that helmet use should be actively encouraged worldwide for rider safety.

BACKGROUND

Road traffic injuries contribute significantly to mortality and the burden of disease throughout the world, but particularly in developing countries (Mohan 2002; Nantulya 2002). In many developing countries, the majority of those injured in road traffic accidents are pedestrians, cyclists and motorised two-wheel riders (i.e. motorcycles, motor scooters etc.) For instance, in 1994 in Malaysia, 57% of all road deaths were riders of motorised two-wheelers (Mohan 2002). The number of road fatalities attributed to motorised two-wheelers in industrialised countries, where four-wheeled private vehicles are more prevalent, is also disproportionately high (Mohan 2002). In 1998, in Britain, motorcycle riders accounted for less than 1% of total road users but contributed to 15% of those killed or seriously injured on the roads (DFT 1998). With increasing modernisation in many developing countries, road traffic deaths are increasing (Odero 1997), and traffic deaths are projected to become the third most important health problem by 2020 (Murray 1996). Interventions to address this rising epidemic should, therefore, be assessed.

Injuries to the head, following motorcycle crashes, are a common cause of severe morbidity and mortality (Sosin 1990; Bachulis 1988). Intuitively, wearing of motorcycle helmets should reduce the number of such head injuries. Results from large scale ecological type studies have suggested that when helmet use rates increase with implementation of a law, injury and mortality rates decrease (Branas 2000; Sosin 1990; McSwain 1990). However, in both developing and developed countries, resistance to legislation on motorcycle helmets still coexists with debate on the effectiveness of motorcycle helmets in reducing morbidity and mortality.

Arguments against helmets for motorcycle riders include the possibility that they increase the risk of neck injuries in crashes (Krantz 1985) and could decrease rider visibility. Questions also surround the effectiveness of helmets in reducing mortality, given the severity of other body injuries sustained by riders in motorcycle crashes. The type of helmet worn, correct fastening of helmets and cost are secondary issues that are particularly relevant to motorcycle helmet usage in developing countries.

A recent review of the effectiveness of bicycle helmets, compared with 'no helmet', found they had significant advantage in reducing head and facial injuries (Thompson 2002). Motorcycles, like bicycles, are a convenient and popular form of transport. However, motorcycles travel at far higher speeds than bicycles, with the potential for greater impact in accidents and hence greater injury. This review collates the current available evidence on helmets and their impact on mortality, and head, face and neck injuries following motorcycle crashes. A reliable estimate of the effectiveness of helmets will assist in road safety research, particularly in assessing the likely cost-effectiveness of introducing of helmet legislation and enforcement in countries where motorcycle injuries are common and legislation does not currently exist.

OBJECTIVES

To quantify the effectiveness of wearing a motorcycle helmet in reducing mortality and head, face and neck injury following motorcycle crashes.

CRITERIA FOR CONSIDERING STUDIES FOR THIS REVIEW

Types of studies

Studies comparing an intervention and control group were considered. This included any randomised controlled trials, controlled trials, cohort and retrospective cohort studies and case-control studies. Ecological-type studies and case (or case series) studies were excluded. For ethical reasons, randomised controlled trials on interventions such as motorcycle helmets are rarely, if ever, conducted. Evidence for motorcycle helmets in injury prevention, therefore, often comes from non-randomised trials. Control of confounders in non-randomised study design is particularly important to achieve a valid estimate of effect.

Types of participants

Motorcycle riders of all ages who have been involved in any type of crash.

Types of intervention

Helmets, both full and partial coverage worn on the head.
Type of helmet (full with face-shield and chin-bar, full without face shield, partial without face shield etc.), whether the helmet is fastened and whether the helmet meets relevant safety standards was recorded if possible.

Types of outcome measures

Motorcycle rider death.
Motorcycle rider head injury including brain, skull and facial injury or concussion.
Motorcycle rider neck injury or cervical spine injury.

SEARCH STRATEGY FOR IDENTIFICATION OF STUDIES

See: Injuries Group search strategy

The following databases were searched.

Health
Cochrane Injuries Group specialised register.
Cochrane Central Register of Controlled Trials (The Cochrane Library issue 1, 2003).
MEDLINE (January 1966 to February 2003).
EMBASE (January 1985 to February 2003).
CINAHL.

Transport
IRRD (International Road Research Documentation).
TRANSDOC.
TRIS (Transport Research Information Service).
ATRI (Australian Transport Index).

General
Science Citation Index.

Reference lists of identified studies and topic reviews were searched for relevant articles, as well as road safety organisation web sites and conference proceedings. Road safety organisations were contacted for published and unpublished material, including relevant pilot projects and demonstration projects.

The following keywords and medical subject headings (exploded) were used.

Intervention
Head protective devices.
Helmets.

Outcomes
Death.
Mortality.
Craniocerebral trauma.
Head injury.
Brain injuries.
Facial injuries.
Neck injuries.
Spinal injuries.
Spinal cord injuries.

The search strategy combined all intervention terms with outcomes and was limited by the Medical Subject Heading 'motorcycles' and the truncated keywords 'motorcycl*', 'motorbi*', 'motor-cycl*' and 'motor-bi*' to eliminate articles related to other helmet types.

METHODS OF THE REVIEW

Two reviewers examined the titles and abstracts obtained through the search strategy and identified potentially eligible studies. A more inclusive strategy was employed at this stage. The full text of all potentially eligible articles was obtained. Authors were contacted for clarification if necessary. Full text articles were independently examined by two reviewers for eligibility, based on inclusion criteria. Duplicate studies were excluded. Any disagreements were resolved by discussion.

Data extraction
Two reviewers independently extracted data from each study on the study type, interventions and outcome measures. Additional information on intervention sub-groups (helmet type), confounding factors, number of participants, loss to follow up and blinding of outcome assessors were collected if appropriate. For studies where raw data was provided but the authors had not calculated an estimate of effect, two reviewers independently extracted the raw data and calculated the estimate of effect using RevMan software.

Quality assessment

Quality was assessed by taking into consideration whether the following factors were addressed: non-participants were described; potential confounders (such as gender, age, alcohol use, other injuries, motorcycle speed and environmental factors) were adjusted for; the authors took steps in case-control studies to minimise recall bias. Quality was assessed independently during data extraction and then compared between two reviewers. Differences were resolved by discussion with a third reviewer.

Analysis

The effect of the interventions on the outcome measures was analysed. Studies were classified according to study type. For outcomes with a similar measure of effect, a combined estimate of effect was calculated. The outcome measure used for analysis was the odds ratio (OR). Graphical presentation was done by means of a Forest plot, to show the odds ratio and 95% confidence interval for each study. The RevMan statistical package was used for data analysis. The generic inverse variance method for adjusted odds ratios was employed for those studies providing confounder adjusted effect measures. Unadjusted data was also analysed in RevMan to give unadjusted odds ratios. Subgroup analysis by study type was conducted for the outcome of head injury.

DESCRIPTION OF STUDIES

For additional details of individual studies see table of included studies.

A total of 53 eligible studies were identified. No randomised controlled trials or other controlled trials were found. There was great variation in study designs and quality. However, the majority of identified studies were retrospective cross-sectional designs that examined one or more of the outcomes (head injury, mortality, facial injury or neck injury) in relation to helmet use. There were four studies utilising a 'matched-pair cohort' design and all these examined the outcome of mortality in relation to helmet use. Three case-control studies and one cohort study were also identified.

Twenty-four studies examined the outcome of death in relation to helmet use, 33 examined head injury, 13 examined neck injury and eight examined facial injury. Seven studies looked at the combined outcome of head and/or neck injury in relation to helmet use and six studies examined different types of helmets in relation to a variety of outcomes of head injury, neck injury and facial injury.

The observational data were obtained from a wide variety of settings, including some developing countries (Conrad 1996; Phuenpathom 2001; Sood 1988). However, the majority of studies were based on populations from developed countries. The study participants were identified by a variety of means including using motorcycle crash presentations to hospital, linking data from police reported crashes to hospital data, routinely collected databases such as the Fatal Accident Reporting System (FARS) and trauma databases. Some investigators only examined the outcome of interest in a dead population (Krantz 1985; O'Connor 2002; Romano 1991; Sarkar 1995). The only cohort study (Lin 2001) recruited college students as participants.

Notably some studies used the same study population or overlapping periods of data for their study population. Both Weiss 1992 and Goldstein 1986 used different statistical models on data collected by Hurt 1981 to estimate helmet effectiveness and the four matched cohort studies used overlapping time periods from the FARS database. For this reason, these studies could not be included in a meta-analysis.

METHODOLOGICAL QUALITY

For additional details of individual studies see table of included studies.

While there was great variation in the quality of the 53 included studies, in general the methodological quality was poor. Only 10 studies made any attempt to measure and control for confounders in their estimate of effect and a further three studies presented their results stratified by motorcycle speed. In addition to this, most studies were either affected by selection bias or had the potential for this to influence their results. While some investigators made attempts to include all motorcycle crash victims in their defined geographic area (Gabella 1995; Rowland 1996), many studies simply examined patients at a single 'level one' trauma centre or a few non-randomly selected hospitals. Others were only able to capture a convenience sample or a small percentage of crash victims in their area (for instance only 20%, Hurt 1981) or had to exclude large proportions of crash victims due to missing data, non-linkage of data or loss to follow up. The potential for selection bias to occur in these situations is a real possibility but difficult to quantify. Few studies (Norvell 2002; Orsay 1995; Kraus 1995; Carr 1981) were able to provide any data to demonstrate that participants excluded from their study due to selection issues were not significantly different from those included.

As most studies relied on retrospectively obtained data, measurement of outcome and exposure generally had consistent methodology. Outcomes were measured by medical records or death certificates. Similarly, exposure measurement relied mostly on medical or police records although some investigators relied on direct on-the-scene measurement (Hurt 1981; O'Connor 2002) or crash victim self-report (Lin 2001). Due to the fact that no studies were controlled trials, and most relied on retrospective data, blinding of outcome and exposure assessors did not occur.

Quality ranking scales can be unreliable and may introduce bias into the review process (Greenland 1994, Clarke 2003). As there were no randomised controlled trials identified, the only objective criteria to subgroup studies was found to be study design and whether potential confounders had been controlled for. Those studies that attempted to control for confounders were ranked as higher quality. This resulted in a subgroup of 13 higher quality studies; six examined the outcome of death in relation to helmet use and nine examined the outcome of head injury. Of those examining the outcome of death, three were matched pair studies using overlapping periods of the FARS database (Norvell 2002; Evans 1988; Anderson 1996), two were cross-sectional design (Goldstein 1986; Rowland 1996) and one was a cross-sectional design that gave an estimate of death in relation to helmet use compared with no helmet use stratified by speed (Shibata 1994). Of those that investigated the outcome of head injury in relation to helmet use, two used a case-control design (Gabella 1995; Tsai 1995), five a cross-sectional study design (Christian 2003; Rowland 1996; Romano 1991; Goldstein 1986; Weiss 1992) and two were cross-sectional studies that stratified their estimate of effect by speed (Kraus 1975, Chang 1981).

RESULTS

The reported odds ratios/relative risks (ORs/RRs) from some studies were inversed to correlate with the *Cochrane Database of Systematic Reviews* (CDSR) convention for expressing outcomes.

Mortality

Studies controlling for confounders

Due to heterogeneity in study design and overlap of study participants, no attempt was made to procure a combined estimate of effect for helmet use in relation to death. The three matched pair cohort studies on overlapping populations showed helmet-wearing was protective against death. These studies gave estimates of: adjusted RR 0.61 (95%CI 0.54, 0.70), Norvell 2002; adjusted RR 0.65 (95% CI 0.58, 0.72), Anderson 1996; effectiveness 28% (+ or - 8%), Evans 1988. The study by Rowland (Rowland 1996) also showed helmets to be protective in preventing death; adjusted RR 0.61 (95% CI 0.38, 0.99). Shibata suggested that speed may be an effect modifier on the odds of death for helmeted riders and therefore estimated for those travelling 30-50km/h adjusted OR 0.03 (95% CI 0.002, 0.42) and those traveling over 50km/hr adjusted OR 0.47 (95% CI 0.086, 2.32). Goldstein's (1986) maximum likelihood probit model found that helmet-wearing resulted in no change in the probability of survival after accounting for kinetic energy of the rider and alcohol use.

Studies not controlling for confounders

Of the 18 studies, three found helmets compared with no helmet significantly protective against death (OR 0.56 (95%CI 0.32, 0.99), Copes 1991; OR 0.32 (95% CI 0.14 - 0.68), Heilman 1982; OR 0.64 (95%CI 0.51 - 0.81), Petridou 1998), three studies found helmets protective against death but provided no estimate of statistical significance (Wilson 1989: effectiveness 29%; Carr 1981: OR 0.16; Johnson 1996: OR 0.64), and 13 found a non-significant effect of helmet-wearing on death (range: OR 0.14 (95%CI 0.02 - 1.01) Ding 1994 to OR 1.21 (95% CI 0.60 - 2.44) Offner 1992). Petridou's stated measure of effect only compared the odds of death with the odds of injury and was described as being 'adjusted'. However, the reviewers have been unable to contact the author to clarify what this implied. Wilson (1989) used a 'matched pair' study design but made no attempt to adjust for potential confounders such as rider age and gender. Fifteen of these studies could be combined to give an overall unadjusted estimate of helmet effectiveness for reducing mortality as OR 0.64 (95% CI 0.52, 0.80).

Head injury

Studies controlling for confounders

Seven studies found motorcycle helmets compared with no helmets significantly protect against head injury in motorcyclists who crash. Only five studies gave estimates that could be combined in a meta-analysis: adjusted OR 0.41 (95% CI 0.21, 0.81), Gabella 1995; adjusted OR 0.26 (95% CI 0.14, 0.47), Tsai 1995; adjusted OR 0.26 (95% CI 0.18, 0.40), Romano 1991; adjusted OR 0.32 (95% CI 0.21, 0.50), Rowland 1996; adjusted OR 0.23 (95% CI 0.14 - 0.53), Christian 2003. The estimate quoted by Tsai was that for 'full-face' helmets only compared with no helmet. The combined adjusted estimate of effect for any head injury for all five studies is OR 0.28 (95% CI 0.23, 0.35). There was no significant heterogeneity (p=0.65). When subgrouping of studies by study type was undertaken, the combined adjusted estimate from the two case-control studies gave a slightly more conservative estimate, OR 0.32 (95% CI 0.20, 0.51) than the combined adjusted estimate from the cross-sectional studies, OR 0.27 (95% CI 0.21, 0.35). Using the same data but different mathematical models to control for confounders, Goldstein 1986 and Weiss 1992 both found helmets significantly reduce the probability of head injuries in motorcycle crashes.

For the two cross-sectional studies that stratified head injury estimates by speed, Kraus 1975 compared the outcome of serious head injury versus non-serious head injury and found a non-significant effect of helmets, i.e. for those travelling less than 50km/hour OR 0.59 (95% CI 0.09, 3.70) and those travelling 50-113km/hr, OR 0.31 (95% CI 0.07, 1.44). Chang 1981 compared an outcome of head injury with no head injury and found helmets to be similarly protective at different speeds, i.e. those travelling less than or equal to 35mph OR 0.38 (95% CI 0.22, 0.65) and those travelling greater than or equal to 36mph OR 0.35 (95% CI 0.24, 0.50).

Studies not controlling for confounders

The 24 remaining studies that did not adjust for confounders in their estimate of effect were remarkably consistent and, overall, found helmets to be significantly protective compared with no helmets for head injuries. The overall combined estimate from 17 studies (see meta-analysis) was OR 0.38 (0.35, 0.41), range 0.26 (95% CI 0.19, 0.36) Hurt 1981 to 0.83 (95% CI 0.25, 2.69) Krantz 1985 and there was no evidence of heterogeneity (p=0.73). The only cohort study (Lin 2001) found head injuries occurred significantly more often in unhelmeted crash victims than helmeted (4.7% compared with 1.9%, p=0.004). Johnson 1996 and LaTorre 2002 also provided estimates of helmet effectiveness but did not give raw data that could be combined in the meta-analysis. Johnson found helmets to be 65% effective (no CI given) and LaTorre OR 0.23 (95% CI 0.03, 0.48). Similarly, Johnson 1995, Lloyd 1987, May 1989 and Van Camp 1998 gave data that could not be used in the meta-analysis but these studies demonstrated that alternate measures of head injury such as average nervous system score and incidence of skull fracture were lower in helmeted riders compared with non-helmeted riders who crashed.

Neck injury

Only Goldstein's (1986) model attempted to control for confounders and predicted that beyond a critical impact speed (13mph) the average weighted helmet increases the probability of neck injury. One of the 13 studies found that motorcycle helmets compared with no helmet significantly protects against neck injury (Sarkar 1995 OR 0.11 [95% CI 0.01, 0.91]). All other studies found a non-significant relationship between helmets and neck injury. From the eleven studies providing data that could be combined, there was a non-significant effect of helmets on neck injuries OR 0.86 (95% CI 0.64, 1.15) with no heterogeneity (p=0.60).

Facial injury

None of the eight identified studies provided confounder adjusted estimates. Four studies found helmets compared with no helmet significantly protective against facial injury following a crash (Lin 2001; Rowland 1996; Gopalakrishna 1998; Johnson 1995) and the other four found a non-significant effect of helmet wearing on facial injury. The combined estimate from seven eligible studies found helmets compared with no helmets significantly protect against facial injury OR 0.42 (95% CI 0.25, 0.69). There was significant heterogeneity (p=0.01). Lin found 5.3% of unhelmeted crash victims compared with 2.6% of helmeted crash victims sustained facial injuries (p=0.007).

Helmet type

Of the six studies that examined different helmet types, only one adjusted for confounders. Tsai found full-face helmets compared with no helmet significantly protective against head injury, adjusted OR 0.26 (95% CI 0.14, 0.47). However, helmets without a chin-bar and less head coverage (defined as full helmet or partial coverage helmet) compared with no helmet were not significantly protective against head injury, adjusted OR 0.72 (0.38, 1.37). Hurt 1981 found that full-face helmets and non-full-face helmets compared with no helmet were both significantly protective against head injury (OR 0.29 (95 %CI 0.17, 0.49) and OR 0.24 (95% CI 0.16, 0.36) respectively). Both Cannell 1982 and Vaughan 1977 found full-face helmets compared with open-faced helmets (or 'jet helmet') provided no significant advantage in relation to head injury (OR 1.13 (95% CI 0.34, 3.76) and OR 0.88 (95% CI 0.58, 1.32) respectively).

Vaughan 1977, Krantz 1985 and O'Connor 2002 found that full-face helmets compared with open-faced helmets (or 'jet helmet') had no significant effect on neck injuries (OR 0.85 (95% CI 0.26,2.80), OR 0.84 (95% CI 0.07, 9.56) and OR 0.76 (95% CI 0.15, 3.81) respectively). Similarly Cannell found that full-face helmets compared with open-face helmets did not have a significant effect on facial injuries.

DISCUSSION

As no randomised controlled trials were found, we relied on observational data for this review. Although we identified many studies that addressed the study question, on the whole the methodological quality was poor. A variety of different study designs were included, as long as the design allowed for a control or comparison group. The predominant study type identified was retrospective cross-sectional studies. Although cross-sectional studies are frequently criticised as the outcome is prone to 'length-biased sampling' (Rothman 1998), for this review, investigators have measured only new events (incident injuries or death after a motorcycle crash) over the study period rather than the prevalence of these conditions. Hence, in this case, this criticism does not apply and the cross-sectional studies included are in fact similar to a case-control design.

Besides study design, the only objective quality ranking criteria applicable to studies included in this review was measurement and adjustment for confounding. Factors such as motorcycle speed, alcohol consumption, rider age and gender are often associated with motorcycle crash fatalities and injuries (Braddock 1992; Kelly 1991; Offner 1992; Wick 1998; Lin 2003) and there is good reason to suspect these factors may differ between those who wear motorcycle helmets and those who do not (Shankar 1992, Hurt 1981; Johnson 1995; Skalkidou 1999). Hence in non-randomised studies, control of these potential confounders is essential for a valid estimate of effect and therefore this criterion was used as the main quality item to differentiate higher and lower quality studies.

A number of studies had potential to be affected by selection bias as a result of a large percentage of missing data or non-random selection of a crash population (i.e. trauma hospital victims only or convenience samples from police reported crashes). However, the effect of this potential selection bias is difficult to quantify and could not objectively be examined. Therefore, rather than using a quality ranking scale to conduct a sensitivity analysis, a post-hoc sensitivity analysis was conducted for outcomes based on quality items. For outcomes where there were an adequate number of studies to make this possible (studies providing unadjusted estimates for head injury, death, neck injury), estimates of effects based on quality items were not found to be significantly different. This post-hoc analysis is available from the authors.

Despite using observational studies and the difficulties with poor quality, there is no doubt that motorcycle helmets compared with no helmets reduce the likelihood of head injuries. The estimate of effectiveness ranges from OR 0.23 to 0.35. Notably, among the confounder adjusted estimates, the case-control studies provided a similar estimate to that from the cross-sectional studies confirming the argument that a difference in the study design of included papers in this review is unlikely to bias results. Studies that made no attempt to adjust for confounding gave a more conservative estimate of effect and the study by Chang 1981 that stratified helmet effectiveness by speed further supports this finding (i.e. the overall unadjusted estimate of helmet effectiveness , OR 0.43 (95% CI 0.33, 0.57) is more conservative than the estimates obtained after stratification). Given the variability among the types of confounders adjusted for in individual studies, it is difficult to postulate reasons for this observed difference between adjusted and unadjusted estimates. However, the overall consistency among the results irrespective of study design and quality issues confirms the effectiveness of helmets in protecting against head injury.

Studies estimating the effect of helmets on mortality are less consistent in their results although overall suggest a protective effect. All three matched pair cohort studies found a significantly decreased risk of death among helmet wearers. However, these studies were essentially on a similar population (overlapped time periods for use of data from FARS) and only involved those riding as a pair. Shibata 1994 found that when motorcyclists crashed at lower speeds helmets significantly decreased the risk of death but at speeds greater than 50km/hr there was no significant benefit from wearing a helmet. This finding is plausible given that motorcycle crashes at higher speeds may result in overall body injuries not compatible with life regardless of how well the head is protected, or that the energy transfer on crashing above a certain speed overcomes any protective effect of a helmet. Rowland 1996 found helmets decreased the risk of death. However, the RR quoted was not adjusted for speed and thereby the effect of this variable in Rowland's estimate is unknown. One can postulate that the protective effect of helmets demonstrated in the 'matched pair' studies compared to the less protective estimates from Rowland and Shibata may be related to an extrinsic difference in those who ride as pairs on motorcycles. For instance paired riders may on average travel at lower speeds than single motorcycle riders or when crashing paired riders may have different dynamics that afford more protection against injury. Overall, the evidence shows helmets reduce mortality compared with no helmets but this should be further investigated in relation to their interaction with speed.

There is insufficient good quality evidence to make conclusions about helmet effects on neck and facial injuries, although findings are not inconsistent with a protective effect on facial injuries. Only one study made any attempt to adjust for confounders for the outcome of neck injury (Goldstein 1986) and this has been criticised for flawed statistical methodology (Weiss 1992; Bedi 1987). Similarly, there is insufficient evidence to make firm conclusions on the effectiveness of different helmet types. Only one study adjusted for confounders when providing an estimate of effect comparing full-face helmets and non-full-face helmets with no helmet and the author (Tsai 1995) suggested the study result may be biased by measurement error due to the fact that the quality of 'full' and 'partial coverage' helmets in Taiwan are suboptimal as many do not have an impact absorbing liner.

The findings from this review, particularly in relation to helmet effectiveness for head injury, are consistent with the conclusions drawn from other literature. Before-after studies conclude that following the implementation of a helmet law, a reduction in motorcycle-related head injuries occurs (Kraus 1995a; Chiu 2000) while the repeal of a law results in increased death and injury (McSwain 1984). Ecological-type studies also suggest that motorcycle helmet laws result in a reduction in motorcycle head injury-related deaths (Sosin 1990) and that helmet laws result in a reduction in motorcycle related death rates (Branas 2000).

Given the significant impact head injuries have on the burden of disease worldwide (McKenzie 2000) the results of this review should be contemplated widely. However, care must be taken in generalising the findings. Of note, all higher quality studies were conducted in developed countries where more technologically advanced emergency services exist and some studies used only dead populations (O'Connor 2002; Romano 1991) or paired motorcycle riders (Anderson 1996; Evans 1988; Norvell 2002). Head injury definitions mostly did not include minor injuries such as soft-tissue or scalp injuries so the results of this review relate primarily to more serious head injuries such as brain injury and skull fractures. Also, Shibata (1994) noted that in the relevant study period, Japan had no emergency on the scene medical treatment which may affect the estimates of mortality given in this study.

Few studies discussed the issue of helmet quality and measured whether helmets worn by riders met safety standards. Tsai 1995 commented on the quality of helmets in Taiwan but only a few authors actually examined helmets worn by study participants to ensure they complied with safety standards. It has been noted in both high and middle/low income countries that 'counterfeit helmets' are available (Thompson 2003; Peek-Asa 1999) and one study has suggested that such helmets may result in more injury in crashes (Peek-Asa 1999). Most of the studies in this review came from developed countries, where this is unlikely to be a major issue, but the results from this review should be viewed with this potential misclassification in mind. Further to this, the enforcement of helmet safety standards must go hand in hand with motorcycle helmet policy-making.

Finally, various authors suggest that protective measures such as helmets and seat belts may decrease an individual's perception of risk and thereby increase their propensity to engage in risk-taking behaviour (Wilde 2002; Adams 1999). Although this review did not aim to investigate the effect of wearing motorcycle helmets on the likelihood of increasing risk-taking behaviour such as speeding, this issue of 'risk compensation' deserves mention. No doubt the arguments supporting and refuting this theory need to be considered when applying the findings of this review to policy.

In terms of reporting risk reduction, the odds ratio, the primary measure of effect in most of the included studies, provides an estimate of the relative risk provided by helmets in the population, that is, motorcycle riders who crash (Rothman 1998; Schlesselman 1982; Kahn 1989; Hennekens 1987). Therefore, the relative risk of head injury in those who crash and wear helmets compared with those not wearing helmets can be estimated as 0.28 and the relative risk reduction as 0.72. It is thus appropriate to estimate that motorcycle helmets reduce the risk of head injury by 72%.

AUTHORS' CONCLUSIONS

Implications for practice

Well-conducted observational studies demonstrate that helmets are effective in reducing head injuries in motorcyclists who crash by 72%. Motorcycle helmets are effective in preventing death in a crash. However, there is some evidence to suggest that this may be modified by other crash factors such as speed at impact. Currently no conclusive evidence exists on the effect of motorcycle helmets on neck or facial injuries.

Implications for research

Further research is required to address the issue of whether motorcycle helmets influence neck injury, facial injury and the effects of motorcycle speed on the risk of death for motorcycle riders wearing helmets. In addition, the effectiveness of different helmet types needs to be addressed in a well conducted controlled trial. Issues of cost-effectiveness and enforcement of industry approved helmets are further issues that need to be considered

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POTENTIAL CONFLICT OF INTEREST

None known

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* *Indicates the major publication for the study*

TABLES

Characteristics of included studies

Study	**Anderson 1996**
Methods	Matched pair cohort study
Participants	Motorcycle crash driver/passenger pairs, identified by FARS (entire USA) from 1976-1989, where both riders 14 years or older where one or both died. (N=8,816 pairs)
Interventions	Motorcycle helmet use compared with no helmet use
Outcomes	Death within 30 days of crash
Notes	25% of eligible pairs excluded due to missing data on potential confounders or helmet use. Confounders measured incl: age, gender, seating position, 'police reported BAL' from FARS. Study design assumed participant pairs matched for environmental factors including speed, road conditions etc. FARS validity dependent upon police reporting - differential misclassification of exposure and confounders unlikely

	Provided a fatality risk ratio adjusted for age, gender and seating position (N=8816 pairs) and another adjusted for 'police reported BAL' (N=4265 pairs). Authors note that when results stratified by year, effectiveness increases. Helmet effectiveness decreased in crashes involving collisions with other vehicles compared with non-collision crashes and helmets appeared more effective in less severe crashes.
Allocation concealment	D

Study	**Anonymous 1994**
Methods	Retrospective cross-sectional study
Participants	Police reported motorcycle crash victims where participants were able to be linked with medical record via probabilistic linkage in state of Wisconsin for 1991. (N=3009)
Interventions	Motorcycle helmet use compared with no helmet use
Outcomes	Death and head injury as recorded in medical record
Notes	No potential confounders measured. Approx 6% missing helmet status excluded. Also states approx 7% linking matches made by computer incorrect
Allocation concealment	D

Study	**Bachulis 1988**
Methods	Retrospective cross-sectional study
Participants	Motorcycle crash victims presenting to one hospital in the USA from Jan 1, 1983 to May 31 1987. (N=367)
Interventions	Motorcycle helmet use compared with no helmet use
Outcomes	Death, brain injury, neck injuries and maxillofacial injuries as defined from medical record
Notes	No potential confounders measured
Allocation concealment	D

Study	**Brandt 2002**
Methods	Retrospective cross-sectional study
Participants	Motorcycle crash victims over 15 years of age presenting to a level 1 trauma centre from July 1996 to Oct 2000. (N=216)
Interventions	Motorcycle helmet use compared with no helmet use
Outcomes	Mortality and head and/or neck injury AIS as recorded on trauma registry
Notes	Potential confounders measured but none adjusted for. Raw numbers only given for mortality. Head, neck and facial injuries results recorded as average AIS compared between helmeted and unhelmeted riders
Allocation concealment	D

Study	**Cannell 1982**
Methods	Prospective cross-sectional study
Participants	Selection of motorcycle crashes identified from police and ambulance radio links and by hospital casualty officers over 4 month period from 1978-1979. (N=45)
Interventions	Full-face helmets compared with open-face helmets.
Outcomes	Head injury and maxillofacial injury as recorded on medical records
Notes	Besides age, no potential confounders measured and none adjusted for.

Characteristics of included studies (*Continued*)

	11 deaths excluded. No indication of comparability of those selected for inclusion compared with general motorcycle riders in area.
Allocation concealment	D

Study	**Carr 1981**
Methods	Case-control study.
Participants	Participants were motorcycle crash injured patients recruited from 7 hospitals in the area selected because they were more likely include patients with major trauma. Cases were those who had head injury (N=96) and controls were were non-head injured participants (N=177).
Interventions	Motorcycle helmet use compared with no helmet use
Outcomes	Head trauma (and severity) as defined by medical records
Notes	Potential confounders measured but not adjusted for. 31% participants had unknown helmet status. Quotes OR for death with helmet use as intervention factor but no CI given (OR 0.16)
Allocation concealment	D

Study	**Chang 1981**
Methods	Retrospective cross-sectional study
Participants	Systematic sampling of motorcycle accident cases from Wisconsin state accident records from 1977 to 1979. (N=888)
Interventions	Motorcycle helmet use compared with no helmet use
Outcomes	Head injury as classified on scene - not verified by medical records
Notes	Potential confounders such as speed and 'manner of collision' measured and results stratifed by these factors. Found for all strata of speeds greater than 25mph, there was a significant difference in head injury incidence between helmeted and non-helmeted. 3% missing helmet data.
Allocation concealment	D

Study	**Christian 2003**
Methods	Retrospective cross-sectional study
Participants	Motorcycle drivers involved in a crash identifed from one level 1 trauma centre trauma registry from 1995 to 2000. (N=311)
Interventions	Motorcycle helmet use compared with no helmet use.
Outcomes	Head injury and serious head head injury defined from ICD9 and AIS codes of medical record
Notes	Measured potential confounders such as age, gender, riding season, type and time of accident, drug screen, blood alcohol from trauma registry and adjusted for this in estimate of effect. Only small loss of participants due to unknown helmet use.
Allocation concealment	D

Study	**Conrad 1996**
Methods	Prospective cross-sectional study
Participants	Motorcycle riders injured and admitted to any of the 4 hospital EDs in the region. (N=475)
Interventions	Motorcycle helmet use compared with no helmet use
Outcomes	Head injury and serious head injury based on medical records.

Characteristics of included studies (*Continued*)

Notes	Potential confounders measured but not adjusted for. 9% excluded due to unknown helmet use.
Allocation concealment	D

Study	**Copes 1991**
Methods	Retrospective cross-sectional study
Participants	Injured motorcycle riders who were treated at participating Level 1 & 2 trauma centres across the USA from 1982-1988 and identified on the trauma registry. (N=1066)
Interventions	Motorcycle helmet use compared with no helmet use
Outcomes	Mortality, head injury and/or spinal cord injury as recorded on medical records (trauma registry)
Notes	Potential confounders measured but not adjusted for. 87% of selected participants had missing helmet data and were excluded from analysis. Found average severity of head/brain/spinal injury significantly less for helmeted versus unhelmeted riders.
Allocation concealment	D

Study	**Diemath 1989**
Methods	Retrospective cross-sectional study
Participants	Patients (ages 16 to 24 years) that sustained a head injury following a motorcycle or moped accident. Selection of participants not described. (N=192)
Interventions	Motorcycle helmet use compared with no helmet use
Outcomes	Mortality and head injury severity
Notes	Potential confounders measured but not adjusted for. No description of method of selection of participants and all from a subgroup of those alread with a head injury.
Allocation concealment	D

Study	**Ding 1994**
Methods	Retrospective cross-sectional study
Participants	Motorcycle crash presenting to hospital ED in 1990. (N=2498)
Interventions	Motorcycle helmet use compared with no helmet use
Outcomes	Death (or survival) up to 4 months after discharge from hospital. Head injury (as per AIS score)
Notes	Measured confounders but none adjusted for. <20% missing data due to either unknown helmet use or injury status.
Allocation concealment	D

Study	**Evans 1988**
Methods	Matched pair cohort study.
Participants	Motorcycle crash driver/passenger pairs identified by FARS (entire USA) during 1975-1986 where both riders were 16 years or older and one or both riders died. Pairs had to be matched for age (driver and passenger ages within 3 years of one another) and only males included (N=4714 fatalities)
Interventions	Motorcycle helmet use compared with no helmet use
Outcomes	Death within 30 days of crash

Characteristics of included studies (*Continued*)

Notes	Study design matched for age, gender (by excluding females as too few all female pairs). This resulted in loss of 42% fatality data. Authors found driver seating position had greater risk of fatality.
Allocation concealment	D

Study	**Fledkamp 1977**
Methods	Prospective cross-sectional study
Participants	Consecutive motorcycle drivers presenting as trauma victims to one hospital from 1972-1974. (N=124)
Interventions	Motorcycle helmet use compared with no helmet use
Outcomes	Death because of head trauma. Head trauma - defined as a "contusion". Facial injuries.
Notes	Potential confounders not measured nor controlled for. No loss to follow up or missing information data provided. Only outcome of 'death because of head trauma' used because inadequate definitions given for other outcomes.
Allocation concealment	D

Study	**Gabella 1995**
Methods	Case-control study
Participants	Cases and controls identified from traffic accident reports of motorcycle crashes investigated by Division of Motor Vehicles during Jan1, 1989 to Dec 31, 1990 in El Paso County (Colorado, USA) ie: all motorcycle crashes where there was personal injury or property damage. Cases were those who crashed and sustained a traumatic brain injury or skull fracture identified thought the Colorado dept of health severe head injury surveillance system (based on death certificates, discharge ICD-9 codes, text diagnoses). (N=71) Controls were those who crashed and did not sustain a head injury (ie: were not identified by the head injury surveillance system) (N=417)
Interventions	Motorcycle helmet use compared with no helmet use
Outcomes	Head injury: traumatic brain injury or skull fracture as defined by ICD-9 codes or comparable medical record diagnoses
Notes	Confounders such as DUI, age, passenger status, crash time and type, motorcycle speed, citatation for various motorcycle offences measured and adjusted for. Misclassification of minor head injury cases (ie: superficial lacerations or concussions) as controls is possible and if helmets are protective, this will result in underestimate of effect.
Allocation concealment	D

Study	**Goldstein 1986**
Methods	Prospective cross-sectional study
Participants	Used participants from Hurt 1981 study. See description of this study. (N=644)
Interventions	Motorcycle helmet use compared with no helmet use
Outcomes	Fatality, head and neck injury
Notes	Uses econometric model to take account of confounders such as age, alcohol consumption, rider on-road experience and speed in predicting effect of motorcycle helmets on outcomes. Model used has been criticised. Excluded some data (28%) due to missing values and for some models assigned a mean value to missing data.
Allocation concealment	D

Characteristics of included studies (*Continued*)

Study	Goodnow 1990
Methods	Retrospective cross-sectional study
Participants	Identified initially from Motor Vehicle Accident files for motorcycle crashes occuring in 4 counties during Sept 1, 1986 to Dec 31, 1987 where at least one crash victim was transported to hospital. (N=742)
Interventions	Motorcycle helmet use compared with no helmet use
Outcomes	Head injury as defined by a medical record
Notes	Potential confounders measured but not adjusted for. 21% loss of participants due to missing injury data or unknown helmet data.
Allocation concealment	D

Study	Gopalakrishna 1998
Methods	Retrospective cross-sectional study
Participants	Non-fatally injured motorcyclists admitted to any of 28 non-randomly selected hospitals across 10 Californian counties from1991 to 1993. (N=4895)
Interventions	Motorcycle helmet use compared with no helmet use
Outcomes	Facial injuries defined from medical records.
Notes	Potential confounders measured but not adjusted for. 15% of particpants excluded due to unknown helmet status.
Allocation concealment	D

Study	Heilman 1982
Methods	Retrospective cross-sectional study
Participants	Included by linking databases including death certificates, hospital data, highway patrol motor vehicle crash report over 1977 to 1980 for one US state. (N=2874)
Interventions	Motorcycle helmet use compared with no helmet use
Outcomes	Head/neck/facial injury and deaths defined from medical records and death certificates.
Notes	Potential confounders measured but not adjusted for. Unknown proportion of participants lost in linkage process, 11% unknown helmet status.
Allocation concealment	D

Study	Hurt 1981
Methods	Prospective cross-sectional study
Participants	Non-random selection of reported motorcycle crash victims that investigators were notified of by emergency services and able to investigate on-scene. (N=878)
Interventions	Motorcycle helmet use compared with no helmet use. Different helmet types.
Outcomes	Head and neck injuries in relation to helmet or no helmet use and type of helmet use.
Notes	Potential confounders measured but not adjusted for.
Allocation concealment	D

Study	Johnson 1995
Methods	Retrospective cross-sectional study
Participants	All injured motorcycle crash victims admitted to a regional level 1 trauma centre over 4 years. (N=331)

Characteristics of included studies (*Continued*)

Interventions	Motorcycle helmet use compared with no helmet use.
Outcomes	Injuries including skull fracture, facial fracture and cervical spine injury as recorded in medical records
Notes	Potential confounders measured but not adjusted for. No mention of any lost data or participants. Incidence of skull fracture found to be significantly less in those wearing helmets (p<0.01)
Allocation concealment	D

Study	**Johnson 1996**
Methods	Retrospective cross-sectional study
Participants	All drivers of motorcycles involved in police reported crashes in 7 US states that were able to be linked to injury databases (EMS, hospital). (N=10353)
Interventions	Motorcycle helmet use compared with no helmet use
Outcomes	Head injury and death as confirmed through linkage with medical records and death certificates.
Notes	Potential confounders measured but not adjusted for. Unclear as to lost data through non-linkage. Also 38% unknown helmet use in NY state data and one state, Utah, excluded due to inability to distinguish between helmeted and non-helmeted riders. No raw data or confidence intervals provided with estimates of effect. Also provided information on seat belt effectiveness.
Allocation concealment	D

Study	**Kelly 1991**
Methods	Prospective cross-sectional study
Participants	Motorcycle rdiers involved in a crash presenting less than 24 hours after the crash to one of 8 hospitals in 4 counties. Engine size must be 150cc or greater and have known helmet status. (N=398)
Interventions	Motorcycle helmet use compared with no helmet use
Outcomes	Death and injuries including head and/or neck injury, facial injury and neck injury as recorded from medical records.
Notes	Potential confounders measured but not adjusted for with injury as outcome (confounders controlled for in outcome of overall injury severity).
Allocation concealment	D

Study	**Krantz 1985**
Methods	Retrospective cross-sectional study
Participants	All motorcycle and moped riders killed in traffic accident identified through autopsy reports from 1977-1983. (N=132)
Interventions	Motorcycle helmet use compared with no helmet use. Full-face and open-face helmet types.
Outcomes	Head injuries and neck injuries as defined on autopsy report
Notes	Potential confounders not measured. Authors stated that autopsies are conducted on all deaths in traffic accidents in Sweden and therefore likely to have included all deaths in region.
Allocation concealment	D

Study	**Kraus 1975**
Methods	Retrospective cross-sectional study

Characteristics of included studies (*Continued*)

Participants	All motorcycle riders who crashed and required medical treatment as identified from police reports, death certificates, hospital records in county. Non-county residents and females excluded (N=626)
Interventions	Motorcycle helmet use compared with no helmet use.
Outcomes	Serious and non-serious head injury. No clear indication of definition of head injury although serious head injury defined as that resulting in death, hospitalisation, boney fracture and requiring continuous medical care beyond 2 visits
Notes	Potential selection bias as only 628 male drivers responded to questionnaire of 1273 injured persons. Furthermore, only 268 of the 628 male drivers had speed and helmet use data for the stratified analysis.
Allocation concealment	D

Study	**Kraus 1995**
Methods	Retrospective cross-sectional study
Participants	Drivers from fatal or severe injury motorcycle crashes reported to police in LA county from July 1988 to Oct 1989 where drivers records could be linked to coroner or hospital records. (N=477)
Interventions	Motorcycle helmet use compared with no helmet use
Outcomes	Head Injury from medical records. Fatality from medical record or coroner
Notes	Potential confounders measured but no adjusted for. 60% data missing due principally to non-linkage of reported crashes.
Allocation concealment	D

Study	**Kraus 1995a**
Methods	Retrospective cross-sectional study
Participants	Non-fatally injured motorcycle crash victims presenting to 18 non-randomly selected hospitals in 10 California counties over a period Jan 1, 1991 to Dec 31, 1993.
Interventions	Motorcycle helmet use compared with no helmet use
Outcomes	Head injury and severe head injury as recorded on medical records
Notes	No potential confounders measured. <20% participants excluded due to missing helmet or injury data.
Allocation concealment	D

Study	**LaTorre 2002**
Methods	Prospective cross-sectional study
Participants	injured motorcycle riders following a crash aged 14-35 years presenting to 2 selected hospitals in Italy during Jan to June 1999. (N=736).
Interventions	Motorcycle helmet use compared with no helmet use
Outcomes	Head Injury based on data collected by investigators or those recruited by investigators.
Notes	Potential confounders measured by none adjusted for. No apparent missing data.
Allocation concealment	D

Study	**Lin 2001**
Methods	Cohort study

Characteristics of included studies (*Continued*)

Participants	Junior college students from 3 randomly selected colleges in a rural and urban area of Taiwan. Participants followed for 18 months from Nov 1994 to June 1996. (N=1889 crashes)
Interventions	Motorcycle helmet use compared with no helmet use
Outcomes	Head, neck and facial injury reported by participants on a questionnaire and supplemented by school records.
Notes	Potential confounders measured and although states a multivariate analysis conducted, this is not shown and attempts to contact authors have been unsuccessful. Average response rate to questionnaire 92%. 20% participants lost to follow up due to graduation of one year. Participants could be included more than once in this study as investigators collected relevant injury data for each crash sustained by the participant and there were more crashes (N=1889) than individual riders involved (N=1284) and therefore despite having raw numbers, no RR were extrapolated. Reliability of questionnaire responses assessed through re-test of 150 randomly selected questionnaires.
Allocation concealment	D

Study	**Lloyd 1987**
Methods	Retrospective cross-sectional study
Participants	Injured motorcycle riders presenting to one hospital in Texas during Feb 1985 to Jan 1986 (N=88)
Interventions	Motorcycle helmet use compared with no helmet use
Outcomes	Head injury as recorded on trauma registry
Notes	No potential confounders measured. Only reported a difference in average nervous system score between helmeted and nonhelmeted riders. No estimate of statistical significance provided. 45% participants excluded due to unknown helmet use status.
Allocation concealment	D

Study	**Luna 1981**
Methods	Retrospective cross-sectional study
Participants	Motorcycle accident victims presenting to a US trauma centre from July 1, 1978 to Nov 30, 1979. (N=263)
Interventions	Motorcycle helmet use compared with no helmet use
Outcomes	Death within first week following admission to hospital. Major head injury from medical records
Notes	Potential confounder not measured. 15% participants with unknown helmet use.
Allocation concealment	D

Study	**May 1989**
Methods	Retrospective cross-sectional study
Participants	Victims of motorcycle crashes requiring transport according to county triage criteria to one trauma centre (N=213)
Interventions	Motorcycle helmet use compared with no helmet use
Outcomes	Head injury as recorded on medical record
Notes	Potential confouders measured but not adjusted for. 5% participants unknown helmet use. Found significant head injuries accounted for 9% of injuries in helmeted patients compared with 37% in unhelmeted.

Characteristics of included studies (*Continued*)

Allocation concealment	D

Study	**Murdock 1991**
Methods	Retrospective cross-sectional study
Participants	Motorcycle crash victims seen a one level 1 trauma centre over 45 months (N=347)
Interventions	Motorcycle helmet use compared with no helmet use
Outcomes	Head and/or neck injury and neck injury alone as described in medical records. Death as recorded from medicla record.
Notes	No potential confounders measured. 28% of participants had unknown helmet status.
Allocation concealment	D

Study	**Norvell 2002**
Methods	Matched pair cohort study
Participants	Motorcycle crash driver/passenger pairs, identified by FARS (entire USA) during 1980-1998, where riders were 16 years or older and one or both riders in the pair died. (N=9,222 pairs)
Interventions	Motorcycle helmet use compared with no helmet use
Outcomes	Death within 30 days of crash
Notes	Study design matches for motorcycle characteristics such as type, speed and environmental factors. 20% pairs excluded due to missing data. Those with missing helmet data had similar age and gender distribution as those with helmet data. Confounders measured and adjusted for included gender, age, rider position.
Allocation concealment	D

Study	**O'Connor 2002**
Methods	Prospective cross-sectional study
Participants	Motorcyclists who died in a crash in the Adelaide (Australia) metropolitan area between 1983-1991 (N=159)
Interventions	Full face motorcycle helmet compared with open-faced motorcycle helmet
Outcomes	Cervical spine injury verifed by autopsy examination (i.e. only in motorcyclists who died)
Notes	Authors comment on subgroups with head impact cases and helmet retention. Study measured confounders such as age, head impact crash type, BAL but did not find any significant predictor of cervical spine injury and therefore did not control for these in final OR. Study base includes all crashes in the area but selects from this a subset of all those who died. Presents evidence to suggest there is no systematic difference between those motorcycle riders who live or die and the type of helmet worn. 8% missing autopsy data
Allocation concealment	D

Study	**Offner 1992**
Methods	Retrospective cross-sectional study
Participants	Motorcycle crash victims admitted to a level 1 trauma centre between Jan 1, 1985 to Jan 1 1990. (N=425)
Interventions	Motorcycle helmet use compared with no helmet use
Outcomes	Death, head injury and neck injury as recorded in medical record
Notes	Potential confounders measured. Gives an estimate effectiveness of helmets for mortality and head injury weighted by a non-head Injury severity score.

Characteristics of included studies (*Continued*)

	14% participants have no helmet data
Allocation concealment	D

Study	**Orsay 1994**
Methods	Retrospective cross-sectional study
Participants	Motorcycle crash victims identified from 28 hospital databases across 4 US states. (N=1056)
Interventions	Motorcycle helmet use compared with no helmet use
Outcomes	Head Injury according to AIS from medical records. Mortality from medical records and some on-the-scene ambulance and police data. Cervical spine injury as recorded in medical records
Notes	Potential confounders measured by none adjusted for. <20% participants excluded due to lack of helmet use data.
Allocation concealment	D

Study	**Orsay 1995**
Methods	Retrospective cross-sectional study
Participants	Motorcycle crash victims identified via a state public health trauma registry including all level 1 & 2 trauma centres in state from July 1, 1991 to Dec 31, 1992. (N=819)
Interventions	Motorcycle helmet use compared with no helmet use
Outcomes	Head Injury according to AIS
Notes	Potential confounders measured but none adjusted for. 26% of those identified had missing helmet status but investigators noted no significant difference in demographics of those with missing helmet status.
Allocation concealment	D

Study	**Petridou 1998**
Methods	Retrospective cross-sectional study
Participants	Identified by traffic police as any motorcycle riders involved in a motor vehicle accident where at least one person was killed or injured in 1985 and 1994 in Greece.
Interventions	Motorcycle helmet use compared with no helmet use
Outcomes	Death rather than injury according to traffic police statistical department
Notes	Masured age and gender and states final estimate of effect is adjusted for confounders but does not state what these are. Attempts to contact authors to clarify this have been unsuccessful. Authors state that approximately 20% of information was missing due to incomplete returns.
Allocation concealment	D

Study	**Phuenpathom 2001**
Methods	Prospective cross-sectional study
Participants	Injury motorcycle riders directly transferred to one of two selected hospital emergency departments where the accident occurred in the Hadyai municipality. from April to Sept 1997 (N=581)
Interventions	Motorcycle helmet use compared with no helmet use
Outcomes	Head and/or neck injury according to AIS
Notes	Potential confounders measured but not adjusted for

Characteristics of included studies (*Continued*)

Allocation concealment	D

Study	**Romano 1991**
Methods	Retrospective cross-sectional study
Participants	All fatally injured motorcyclist, moped, motorscooter and minibike riders as identified by California FARS during 1987-1988 and able to be linked with California MCOD and SMD files with known helmet status. (N=1025)
Interventions	Motorcycle helmet use compared with no helmet use
Outcomes	Head injury in those who died defined by ICD-9 codes 800-803, 850-854 inclusive.
Notes	Reports adjusted OR for odds of head injury with helmet use adjusted for gender, seating position, cycle damage and crash type. Authors report limitation of high proportion (40%) of deaths have unspecified injuries thereby potential misclassification of those with head injury. Authors recalculated OR re-classifying those with unspecified injuries as non-head injuries and found that OR still showed helmets protective against death. Confounders such as speed, BAL not considered.
Allocation concealment	D

Study	**Rowland 1996**
Methods	Retrospective cross-sectional study.
Participants	Motorcycle drivers only who crashed in Washington state in 1989 as identified by State patrol records and linked to hospital and death records
Interventions	Motorcycle helmet use compared with no helmet use
Outcomes	Head Injury defined by ICD-9 codes and then mapped to AIS scores. Death defined by death certificate. Facial injury defined as AIS>0.
Notes	Reports adjusted RR for risk of death with helmet use (Rivara 2003) and adjusted OR for odds of head injury with helmet use. Confounders measured included age, gender, locality of crash, environmental conditions. 23% participants missing from head injury data because of non-linkage.
Allocation concealment	D

Study	**Rutledge 1993**
Methods	Retrospective cross-sectional study
Participants	All motorcycle riders involved in a crash hospitalised in any of 8 level 1 or 2 trauma centres in state during Oct 1, 1987 to Jan 1, 1991. (N=460)
Interventions	Motorcycle helmet use compared with no helmet use
Outcomes	Head injury defined by AIS from medical record.
Notes	Potential confounders measured but none adjusted for. 48% of participants excluded due to unknown helmet use.
Allocation concealment	D

Study	**Sarkar 1995**
Methods	Retrospective cross-sectional study
Participants	Dead motorcycle crash victims identified from police and coroner reports in one county from July 1, 1988 to Oct 31, 1989. (N=164)

Characteristics of included studies (*Continued*)

Interventions	Motorcycle helmet use compared with no helmet use
Outcomes	Head and/or neck injury, facial fracture or neck injury as recorded in medical record or autopsy
Notes	Measured other injuries as potential confounders and stratified findings according to those with equally severe non-head injuries.
Allocation concealment	D

Study	**Shankar 1992**
Methods	Retrospective cross-sectional study
Participants	All motorcycle drivers involved in a crash that was reported to police and transported to hospital in Maryland USA during July 1987 to June 1988. (N=721)
Interventions	Motorcycle helmet use compared with no helmet use
Outcomes	Head injury defined by medical records
Notes	Potential confounders measured but none adjusted for. 25% participants had missing data and were excluded.
Allocation concealment	D

Study	**Shibata 1994**
Methods	Retrospective cross-sectional study.
Participants	Traffic accidents reported by police in Fukuoka Prefecture (Japan) in 1990 catagorised into motorcycle crashed and motorcar accidents. (N=1077)
Interventions	Motorcycle helmet use compared with no helmet use. Seat belt use compared with no seat belt use.
Outcomes	Death within 24 hours of accident compared to no injury for both motorcyclists and motorcar occupants. Only outcome for motorcyclists examined in review.
Notes	OR stratified by gender and only given for male riders. Compared population who died with those with no injuries. OR adjusted for age and alcohol use. Speed found to be an effect-modifier, therefore at speeds between 30-50km/h helmets have protective effect against death but at speeds >50km/hr the protective effect is not significant. Authors also note that at the time of the study, Japan had no on-scene emergency management of injuries which may affect generalisability of results.
Allocation concealment	D

Study	**Sood 1988**
Methods	Prospective cross-sectional study.
Participants	Injured motorcycle riders seen by author in one hospital during May to Dec 1985. (N=302)
Interventions	Motorcycle helmet use compared with no helmet use
Outcomes	Head Injury measured by author according to AIS
Notes	Unclear description of methodology including selection of participants and blinding of assessor.
Allocation concealment	D

Study	**Tsai 1995**
Methods	Case-control study.

Characteristics of included studies (*Continued*)

Participants	Motorcycle riders receiving care for crash injuries in the ED of one of 16 hospitals in Taipei (Taiwan) from August 1 to Oct 15 1990. Cases were those recieving care for head injuries. (N=562) ED Controls were randomly selected individuals seeking care for injuries other than head injuries. (N=789) Street Controls: Were photographs of uninjured, non-crash motorcycle riders matched for time and place of daytime cases. (N=1094)
Interventions	Motorcycle helmet use compared with no helmet use. Full-face motorcycle helmets compared to no helmet. Non-full face helmets (full helmet or partial coverage helmet) compared to no helmet
Outcomes	Head injury defined as brain injury, cerebral concussion, skull fracture, clinically proven unconsciousness, amnesia or neurologic sequalae on a re-visit to the ED. Soft-tissue/scalp injuries are not included Head injury severity as measured by GCS scores.
Notes	Reported comparative estimates for ED and street controls. One ED excluded because suspected bias in selection of participants (5% excluded). Confounders including gender, age, rider position, motorcycle type, weather, place of accident measured. Quasi-random sampling of participants and unable to guarantee completeness of sample but odds of helmet use for street controls found to be similar in ED cases and controls.
Allocation concealment	D

Study	**Van Camp 1998**
Methods	Prospective cross-sectional study
Participants	A consecutive sample of motorcycle and moped accident victims admitted to university hospitals in one town from May 1, 1992 to April 30, 1994. (N=221)
Interventions	Motorcycle helmet use compared with no helmet use
Outcomes	Head injury, head injury severity and cervical spine injury as recorded from a medical record
Notes	Stratified results according to non-head injuries (defined as a surrogate for kinetic energy) and found the ratio of head and facial injuries per a patient was more than double in non-helmeted patients compared with helmeted.
Allocation concealment	D

Study	**Vaughan 1977**
Methods	Retrospective cross-sectional study
Participants	Motorcycle accident victims as identified by routine crash data in Sydney during a three month period. (N=1552)
Interventions	Full face motorcycle helmets compared with jet-style motorcycle helmets
Outcomes	Head injury, facial injury and neck injury from police reports and supplemented by medical records
Notes	Older study may mean different helmet standards and manufacturing practices mean comparisions not generalisable. No confounders measured. Methodology brief and not always clear.
Allocation concealment	D

Study	**Wagle 1993**
Methods	Retrospective cross-sectional study
Participants	Motorcycle accident victims transferred to a major trauma centre on helicopter ambulane (Lifestar) over a 5 year period. (N=80)

Characteristics of included studies (*Continued*)

Interventions	Motorcycle helmet use compared with no helmet use
Outcomes	Cervical spine injury and fatality from medical records
Notes	Potential confounders measured but none adjusted for
Allocation concealment	D

Study	**Weiss 1992**
Methods	Prospective cross-sectional study
Participants	Used participants from Hurt 1981 study. See description of this study.
Interventions	Motorcycle helmet use compared with no helmet use
Outcomes	Head injury
Notes	Statistical model controlled for alcohol and speed of rider in estimating predicted effect of helmets. Found that helmets lead to 42% increase in riders with no head injury
Allocation concealment	D

Study	**Wilson 1989**
Methods	Matched pair cohort study
Participants	Motorcycle crash driver/passenger pairs, identified by FARS (entire USA) from 1982-1987, where both riders 14 years or older where one or both died. (N=5292 riders)
Interventions	Motorcycle helmet use compared with no helmet use
Outcomes	Death within 30 days of crash
Notes	Confounders including rider gender and age not measured nor adjusted for. Helmet effectiveness 29% (no CI given). Effectiveness stratified by passenger (30%) and driver (27%)
Allocation concealment	D

Characteristics of excluded studies

Ankarath 2002	Only outcome reported in relation to helmet use is Glasgow Coma Scale (GCS) which is not specifically a measure of head injury.
Asogwa 1982	Inadequate exposure measurement (helmet wearing). Author stated helmet use could only be defined as those “possessing” a helmet and not necessarily wearing one and no attempt was made to distinguish between those actually wearing a helmet.
Balcerak 1978	Descriptive study that does not report outcomes in relation to helmet use.
Braddock 1992	No individual participant exposure data presented.
Byrd 1978	Intervention measured is “helmet contact” and not helmet use.
Chinn 1999	Examines mechanisms of head injury in motorcycle accidents and not effectiveness of helmet.
Dowdell 1988	Does not examine outcomes of injury in relation to helmet use.
Hell 1993	Case series
Hitosugi 1999	Does not separate bicycle riders from motorcycle riders for intervention of helmet use.
Hoffman 1977	Case series
Konrad 1996	Case series of autopsy cases.
Thom 1993	Case series. No control group.

GRAPHS

Comparison 01. Motorcycle helmet versus no helmet

Outcome title	No. of studies	No. of participants	Statistical method	Effect size
01 Death (not adjusted)	15	13260	Odds Ratio (Random) 95% CI	0.64 [0.52, 0.80]
02 Head Injury (adjusted)	5	10	Adjusted Odds Ratio (Random) 95% CI	0.28 [0.23, 0.35]
03 Head Injury (not adjusted)	17	16859	Odds Ratio (Random) 95% CI	0.38 [0.35, 0.41]
04 Neck Injury (not adjusted)	11	4334	Odds Ratio (Random) 95% CI	0.86 [0.64, 1.15]
05 Facial Injury (not adjusted)	7	8570	Odds Ratio (Random) 95% CI	0.42 [0.25, 0.69]

INDEX TERMS

Medical Subject Headings (MeSH)

Accidents, Traffic; Craniocerebral Trauma [prevention & control]; Facial Injuries [prevention & control]; Head Protective Devices; Motorcycles; Neck Injuries [prevention & control]; Skull Fractures [prevention & control]

COVER SHEET

Title	Helmets for preventing injury in motorcycle riders
Authors	Liu B, Ivers R, Norton R, Blows S, Lo SK
Contribution of author(s)	BL: Wrote drafts of the protocol and review, performed searches, reviewed titles and abstracts, reviewed full text of studies for inclusion, extracted data, performed analyses. RI: Edited drafts of the protocol and review, reviewed titles and abstracts, reviewed full text of studies for inclusion, extracted data, provided epidemiological advice on methodology and interpretations. RN: Edited drafts of the protocol and review, provided epidemiological advice on methodology and interpretations. SB: Edited drafts of the protocol and review, reviewed full text of studies for inclusion, extracted data. SL: Edited drafts of the protocol and review, provided statistical advice.
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What's New	Information not supplied by author
Date new studies sought but none found	Information not supplied by author
Date new studies found but not yet included/excluded	Information not supplied by author
Date new studies found and included/excluded	Information not supplied by author
Date authors' conclusions section amended	Information not supplied by author

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GRAPHS AND OTHER TABLES

Fig. 1. Comparison 01. Motorcycle helmet versus no helmet

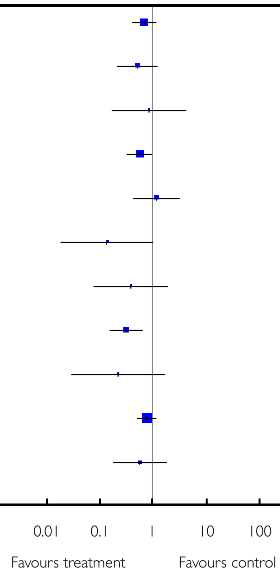
01.01 Death (not adjusted)

Review: Helmets for preventing injury in motorcycle riders

Comparison: 01 Motorcycle helmet versus no helmet

Outcome: 01 Death (not adjusted)

Study	Helmet n/N	No helmet n/N	Odds Ratio (Random) 95% CI	Weight (%)	Odds Ratio (Random) 95% CI
Anonymous 1994	19/994	55/2015		14.8	0.69 [0.41, 1.18]
Bachulis 1988	7/132	23/235		5.9	0.52 [0.22, 1.24]
Brandt 2002	7/174	2/42		1.8	0.84 [0.17, 4.19]
Copes 1991	37/810	20/256		13.2	0.56 [0.32, 0.99]
Diemath 1989	6/52	14/140		4.5	1.17 [0.43, 3.24]
Ding 1994	1/350	41/2016		1.2	0.14 [0.02, 1.01]
Fledkamp 1977	2/51	7/73		1.8	0.38 [0.08, 1.93]
Heilman 1982	9/1113	44/1761		8.5	0.32 [0.15, 0.65]
Kelly 1991	1/58	25/340		1.2	0.22 [0.03, 1.66]
Kraus 1995	41/134	124/343		20.8	0.78 [0.51, 1.20]
Luna 1981	4/101	11/162		3.4	0.57 [0.18, 1.83]



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Study	Helmet n/N	No helmet n/N	Odds Ratio (Random) 95% CI	Weight (%)	Odds Ratio (Random) 95% CI
Murdock 1991	6/111	14/236		4.7	0.91 [0.34, 2.42]
Offner 1992	15/164	20/261		8.9	1.21 [0.60, 2.44]
Orsay 1994	9/252	50/804		8.4	0.56 [0.27, 1.15]
Wagle 1993	1/22	9/58		1.0	0.26 [0.03, 2.18]
Total (95% CI)	4518	8742		100.0	0.64 [0.52, 0.80]

Total events: 165 (Helmet), 459 (No helmet)
Test for heterogeneity chi-square=14.96 df=14 p=0.38 I² =6.4%
Test for overall effect z=3.95 p=0.00008

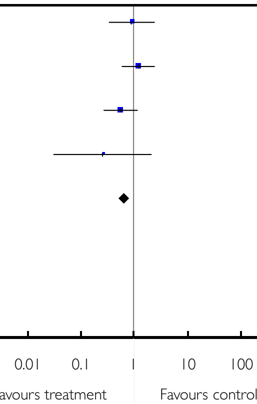

Fig. 2. Comparison 01. Motorcycle helmet versus no helmet

01.02 Head Injury (adjusted)

Review: Helmets for preventing injury in motorcycle riders
Comparison: 01 Motorcycle helmet versus no helmet
Outcome: 02 Head Injury (adjusted)

Study	log [Adjusted Odds Ratio] (SE)	Adjusted Odds Ratio (Random) 95% CI	Weight (%)	Adjusted Odds Ratio (Random) 95% CI
01 Case-control studies				
Gabella 1995	-0.88 (0.34)		10.9	0.41 [0.21, 0.81]
Tsai 1995	-1.35 (0.31)		13.4	0.26 [0.14, 0.48]
Subtotal (95% CI)			24.4	0.32 [0.20, 0.51]
Test for heterogeneity chi-square=1.03 df=1 p=0.31 I² =2.8%				
Test for overall effect z=4.89 p<0.00001				
02 Cross-sectional studies				
Christian 2003	-1.47 (0.25)		19.7	0.23 [0.14, 0.38]
Romano 1991	-1.34 (0.21)		30.2	0.26 [0.18, 0.39]
Rowland 1996	-1.13 (0.22)		25.7	0.32 [0.21, 0.50]
Subtotal (95% CI)			75.6	0.27 [0.21, 0.35]
Test for heterogeneity chi-square=1.04 df=2 p=0.59 I² =0.0%				
Test for overall effect z=10.00 p<0.00001				
Total (95% CI)			100.0	0.28 [0.23, 0.35]
Test for heterogeneity chi-square=2.46 df=4 p=0.65 I² =0.0%				
Test for overall effect z=11.15 p<0.00001				

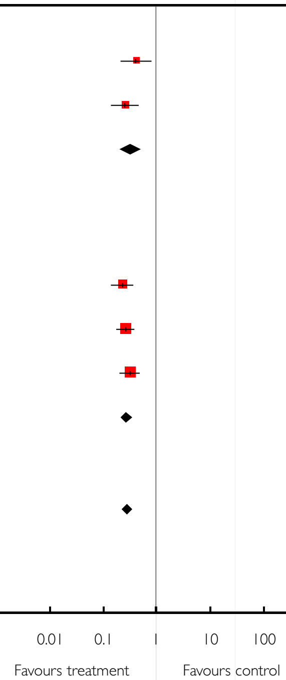

Fig. 3. Comparison 01. Motorcycle helmet versus no helmet

01.03 Head Injury (not adjusted)

Review: Helmets for preventing injury in motorcycle riders

Comparison: 01 Motorcycle helmet versus no helmet

Outcome: 03 Head Injury (not adjusted)

Study	Helmet n/N	No helmet n/N	Weight (%)	Odds Ratio (Random) 95% CI
Anonymous 1994	17/994	97/2015	2.4	0.34 [0.20, 0.58]
Bachulis 1988	32/132	105/235	2.9	0.40 [0.25, 0.64]
Carr 1981	39/96	115/177	2.5	0.37 [0.22, 0.61]
Conrad 1996	102/318	82/157	4.2	0.43 [0.29, 0.64]
Ding 1994	140/363	1252/2135	12.5	0.44 [0.35, 0.56]
Goodnow 1990	31/247	120/495	3.5	0.45 [0.29, 0.69]
Hurt 1981	55/342	228/536	5.8	0.26 [0.19, 0.36]
Krantz 1985	86/102	26/30	0.5	0.83 [0.25, 2.69]
Kraus 1995	49/134	205/343	3.8	0.39 [0.26, 0.59]
Kraus 1995a	614/2408	745/1554	35.6	0.37 [0.32, 0.43]
Luna 1981	11/101	51/162	1.3	0.27 [0.13, 0.54]
Offner 1992	63/164	172/261	4.0	0.32 [0.22, 0.48]
Orsay 1994	28/252	210/804	3.6	0.35 [0.23, 0.54]
Orsay 1995	61/202	315/617	5.7	0.41 [0.30, 0.58]
Rutledge 1993	88/314	77/146	3.9	0.35 [0.23, 0.52]
Shankar 1992	68/330	156/391	5.8	0.39 [0.28, 0.55]
Sood 1988	52/233	33/69	2.0	0.31 [0.18, 0.55]
Total (95% CI)	6732	10127	100.0	0.38 [0.35, 0.41]

Total events: 1536 (Helmet), 3989 (No helmet)

Test for heterogeneity chi-square=12.23 df=16 p=0.73 I² =0.0%

Test for overall effect z=23.79 p<0.00001

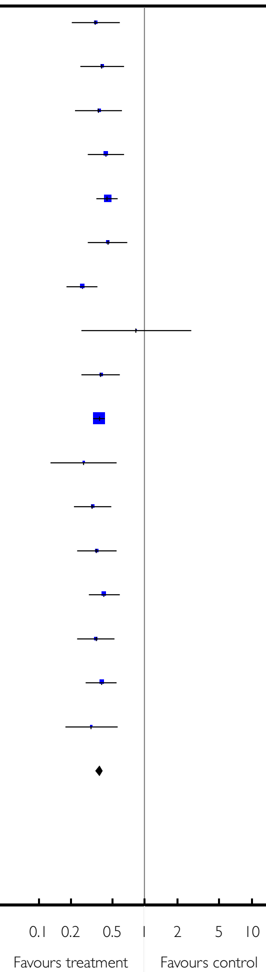

Fig. 4. Comparison 01. Motorcycle helmet versus no helmet

01.04 Neck Injury (not adjusted)

Review: Helmets for preventing injury in motorcycle riders

Comparison: 01 Motorcycle helmet versus no helmet

Outcome: 04 Neck Injury (not adjusted)

Study	Helmet n/N	No helmet n/N	Odds Ratio (Random) 95% CI	Weight (%)	Odds Ratio (Random) 95% CI
Bachulis 1988	9/132	11/235		10.6	1.49 [0.60, 3.69]
Hurt 1981	27/342	60/536		38.4	0.68 [0.42, 1.09]
Johnson 1995	5/77	11/254		7.3	1.53 [0.52, 4.56]
Kelly 1991	6/58	46/340		10.7	0.74 [0.30, 1.81]
Krantz 1985	3/102	1/30		1.6	0.88 [0.09, 8.77]
Murdock 1991	3/111	8/236		4.8	0.79 [0.21, 3.04]
Offner 1992	4/164	8/261		5.9	0.79 [0.23, 2.67]
Orsay 1994	6/252	15/804		9.5	1.28 [0.49, 3.34]
Sarkar 1995	1/30	16/69		2.0	0.11 [0.01, 0.91]
Van Camp 1998	13/174	4/47		6.4	0.87 [0.27, 2.80]
Wagle 1993	2/22	4/58		2.8	1.35 [0.23, 7.95]
Total (95% CI)	1464	2870		100.0	0.86 [0.64, 1.15]

Total events: 79 (Helmet), 184 (No helmet)

Test for heterogeneity chi-square=8.27 df=10 p=0.60 I^2 =0.0%

Test for overall effect z=1.03 p=0.3

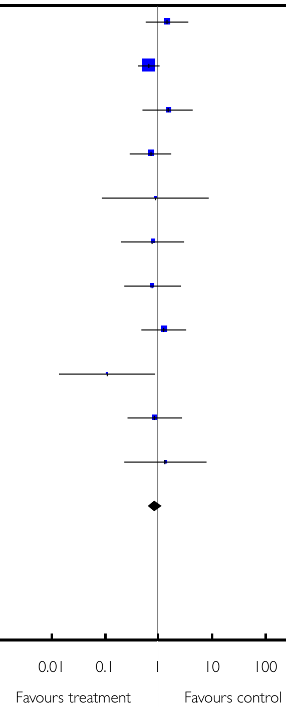

Fig. 5. Comparison 01. Motorcycle helmet versus no helmet

01.05 Facial Injury (not adjusted)

Review: Helmets for preventing injury in motorcycle riders

Comparison: 01 Motorcycle helmet versus no helmet

Outcome: 05 Facial Injury (not adjusted)

Study	Helmet n/N	No helmet n/N	Odds Ratio (Random) 95% CI	Weight (%)	Odds Ratio (Random) 95% CI
Bachulis 1988	9/132	30/235		16.5	0.50 [0.23, 1.09]
Gopalakrishna 1998	518/2874	799/2021		27.0	0.34 [0.30, 0.38]
Johnson 1995	4/77	41/254		12.3	0.28 [0.10, 0.82]
Kelly 1991	4/58	39/340		12.2	0.57 [0.20, 1.67]
Phuenpathom 2001	13/223	16/355		16.9	1.31 [0.62, 2.78]
Rowland 1996	3/945	20/957		10.4	0.15 [0.04, 0.50]
Sarkar 1995	1/30	12/69		4.7	0.16 [0.02, 1.32]
Total (95% CI)	4339	4231		100.0	0.42 [0.25, 0.69]

Total events: 552 (Helmet), 957 (No helmet)

Test for heterogeneity chi-square=16.38 df=6 p=0.01 I² =63.4%

Test for overall effect z=3.41 p=0.0006