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REGULATORY AND ROAD ENGINEERING INTERVENTIONS FOR PREVENTING ROAD TRAFFIC INJURIES AND FATALITIES AMONG VULNERABLE (NON-MOTORISED AND MOTORISED TWO-WHEEL) ROAD USERS IN LOW- AND MIDDLE-INCOME COUNTRIES

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The authors of this report are:

Manisha Gupta[1], Geetha R Menon[2], Ganesh Devkar[3], Hilary Thomson[4]

[1]Road Safety and Non-motorised Transport Consultant, New Delhi, India
[2]Division of Non-Communicable Diseases, Indian Council of Medical Research, New Delhi, India
[3]Faculty of Technology, CEPT University, Ahmedabad, India
[4]MRC/CSO Social and Public Health Sciences Unit, University of Glasgow, Glasgow, UK

Contact person: Manisha Gupta, Email: manischa.gupta@gmail.com

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Ganesh Devkar has participated in a systematic review project titled, Impact of private sector involvement on access and quality of service in electricity, telecom, and water supply sectors, funded by the Australian Agency for International Development.

Geetha R Menon has co-authored three clinical systematic reviews:

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MG is the lead review author; GM, GD and HT are co-reviewers. MG and GD screened studies for inclusion criteria; MG and GM conducted critical appraisal, data extraction and meta-analysis. MG wrote the meta-analysis and narrative synthesis for the review. HT advised on the methodology aspect of the review development.

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SYSTEMATIC REVIEW SUMMARY

The effect of traffic law enforcement and regulatory interventions generally had favourable outcomes after the intervention. Fatalities and injuries declined. Enforcement of mandatory helmet-law strongly favoured compliance among motorcyclists compared with when there was no helmet law. Red-light and speeding violations were reduced due to the impact of an automated enforcement system. Road engineering interventions had mixed results for road traffic casualties. Despite methodological differences between studies, road engineering interventions reduced fatalities. Risk of biases in included studies ranged between moderate and low quality. Studies pooled effect sizes presented statistical heterogeneity which downsized the study quality of included studies range from moderate to very low quality.

ABOUT THIS SUMMARY

Regulatory and road engineering interventions for preventing road traffic injuries and fatalities among vulnerable (non-motorised and motorised two-wheel) road users in low- and middle-income countries.

Manisha Gupta[1], Geetha R Menon[2], Ganesh Devkar[3], Hilary Thomson[4]

[1]Road Safety and Non-motorised Transport Consultant, New Delhi, India
[2]Division of Non-Communicable Diseases, Indian Council of Medical Research, New Delhi, India
[3]Faculty of Technology, CEPT University, Ahmedabad, India
[4]MRC/CSO Social and Public Health Sciences Unit, University of Glasgow, Glasgow, UK

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The purpose of this review is to establish what is known about the effects and effectiveness of enforcement of traffic laws and regulations and road engineering interventions for prevention of injury (fatal and non-fatal) to non-motorised road users and motorised two-wheel road users in low- and middle-income countries.

SUMMARY

A total of 25 studies meeting the study inclusion criteria were identified from a wide variety of low- and middle-income countries (LMIC). Three studies were conducted between 1994 and 1999; nine studies were conducted between 2000 and 2009; and, more than half (13) studies were conducted in the period 2010–14. Furthermore, 75 studies using modelling technique were identified, of which three were conducted between 1994 and 1999; 30 studies were conducted between 2000 and 2009, and more than half (42) of these were conducted in the period 2010–14. The dates of evidence in included studies and modelling studies demonstrated progressive involvement of international agencies in improving road safety conditions during the 1990s in LMIC.

Notably, of the 25 studies included for the review, 23 belonged to middle-income countries and only two studies belonged to low-income countries.

Effect sizes were computed for 18 studies. Three studies presented distinct methods to implement and measure road-infrastructure intervention outcome effects. These studies were presented as qualitative case descriptions. Four studies were excluded from meta-analysis and narrative synthesis. These studies were assessed at high risk of biases around internal validity, data collection, confounders, and intervention integrity based on HAT.

Studies were grouped by traffic law enforcement and regulatory followed by road engineering intervention categories. Then studies were sub-grouped by non-randomized study design: uncontrolled before and after and time series. We did not find any randomized study trials.

This review included four non-randomized study designs. As a result of methodological heterogeneity in study designs, risk of bias in included studies ranged between moderate and low quality. All included studies had adequate information with regard to intervention to individually meet the study inclusion criteria. However, the meta-analysis results presented extreme heterogeneity. We expected variations in between studies due to various reasons, for example study population (socio-demographic, cultural, and spatial), study sample size, data collection duration, road environment, and perceived road risk of the country. Nevertheless, studies pooled effect sizes showed inconsistency and impreciseness which downsized the study quality of included studies range from moderate to very low quality.

Traffic law enforcement and regulatory interventions

Ten uncontrolled before-and-after studies assessed the number of outcome measures before and after enforcement of traffic law and regulatory interventions. In general, effect sizes showed favourable outcomes after interventions; fatalities declined by 6% and overall injuries by 26%. The effect of intervention was strongly in favour of secondary outcome measures. In places where a mandatory helmet-law was enforced, compliance among motorcyclists was eight times greater than when there was no mandatory helmet-law enforcement. Red-light and speeding violations were reduced by 66% due to the impact of an automated enforcement system (that is, speed cameras).

Three time-series studies assessed the number of road traffic casualties and fatalities before and after enforcement of traffic laws and regulatory interventions. All studies focused on preventing road traffic injuries and fatalities among motorcycle riders. The meta-analysis showed that, as a result of interventions, the number of road traffic casualties and fatalities declined in the post-intervention period, meaning interventions were effective. The mean percentage change in road traffic casualties was a 38% decline.

Road engineering interventions

Three uncontrolled before-and-after studies assessed the effect of road engineering interventions on the number of road traffic casualties and fatalities. Contrary to expectations, the interventions resulted in a more than doubling of the road traffic casualty rate. However, fatalities declined by 48% due to the impact of interventions.

Two time-series studies assessed the number of road traffic casualties and fatalities before and after road engineering treatments. The effect of road engineering interventions strongly favoured outcomes after interventions. In terms of percent change, mean accident casualty relative to post-intervention results declined by 44%.

Qualitative case description studies using different methodologies showed a positive impact of interventions such as speed bumps/humps and transverse rumble strips. In one study, the use of transverse rumble strips reduced crashes by 25% on average. Another study showed that the use of speed bumps 5 cm and 7 cm high significantly contributed to the safety of vulnerable road users, and especially the safety of pedestrians, with mean vehicle speed reduced by 79%. However, one study found that signalisation efforts were associated with an increased risk of pedestrian–vehicle collisions.

One study reported pre-specified adverse outcome increase in collision between motor vehicles as a result of traffic calming measure speed humps. The total number of motor vehicle collisions increased by 9% as a result of speed humps installation.

APPROACH

Search methods

The systematic search was conducted between December 2014 and 13 February 2015. The following databases were searched from 1990 to current: OvidSP Medline, OvidSP Embase, OvidSP Transport, PubMed, Cochrane Injuries Group Specialised Register, Cochrane Central Register of Controlled Trials and Proquest ERIC. The databases were searched for terms describing the following: population: vulnerable road users in LMIC; interventions: road engineering, conspicuity/visibility; and purpose: safety/accident prevention in work/school-related travel.

In addition, we searched the databases of road safety organisations, contacted experts, hand-searched conference proceedings, and checked reference lists of selected papers.

Selection criteria

We considered intervention studies that reported quantitative data on evaluation of the effects of road engineering and/or enforcement of traffic laws and regulation interventions among vulnerable road users in LMIC. We included all studies that compared changes in outcomes before and after the intervention implementation, with or without a control group.

Data collection and analysis

Two authors independently screened search results for eligible articles. Two authors independently assessed full-text articles for inclusion criteria. Data were extracted by a two-author team.

A total of 25 studies meeting the study inclusion criteria were identified from a wide variety of LMIC, including 14 from East Asia and the Pacific region, four from Latin America and the Caribbean region, three from the Sub-Sahara region, and two each from the South Asia region and the Europe and Central Asia region. Notably, 23 studies belonged to middle-income countries and only two studies belonged to low-income countries, namely, Bangladesh and Uganda.

OUTLINE OF EVIDENCE

A total of 25 studies meeting the study inclusion criteria were identified. Effect sizes were computed for 18 studies, three studies were presented as qualitative case descriptions. Four studies were excluded from meta-analysis and narrative synthesis due to high risk of biases around internal validity, data collection, confounders, and intervention integrity. As a result of methodological heterogeneity is study designs, risk of bias in included studies ranged between moderate and low quality.

Traffic-law enforcement and regulatory interventions

In total, 10 uncontrolled before-and-after studies assessed the number of outcome measures before and after enforcement of traffic laws and regulatory interventions.

- Three studies assessed the number of road traffic crashes resulting in fatalities and injuries among motorcycle riders. The number of road traffic crashes resulting in deaths among motorcycle riders declined by 6% (OR 0.94; 95% CI 0.72 – 1.23; I^2=68%; P=0.64) and the number of road traffic crashes resulting in injuries declined by 26% (OR, 0.74; 95% CI 0.61 – 0.89; I2=85%; P=0.002) based on data from 16,258 people after the helmet law enforcement intervention.

- Six studies assessed secondary outcome compliance for mandatory helmet use. In places where a mandatory helmet-law was enforced, there was an eightfold increase in compliance among motorcyclists compared with when there was no mandatory helmet-law enforcement (OR 8.05; 95% CI 4.03 – 16.05; I^2=99%; P=0.0001), based on data from 10,4255 people after the helmet law enforcement intervention.

- Three studies assessed the secondary outcomes of red-light and speeding violations. There was a 66% reduction in red light and speed violations due to the impact of intervention (OR 0.34; 95% CI 0.17 – 0.70; I^2=100%; P=0.003), based on 26,388 people after the law enforcement intervention.

Three time-series studies assessed the number of road traffic casualties and fatalities before and after enforcement of traffic laws and regulatory interventions among motorcycle riders.

- Two studies assessed the number of road traffic casualties (percent change: −51%; 95% CI −228.64 – 30.56; and −68%; 95% CI −227.40 – 13.40) based on data from 3,962 people involved in accidents after the law enforcement intervention. The mean percent change in accidents was a 38% decline.
- One study assessed the number of road traffic accidents resulting in fatalities (percent change: −500%; 95% CI −1248.43 – 167.55) based on data from 748 people involved in accidents after the law enforcement intervention.

Road engineering interventions

In total, three uncontrolled before-and-after studies assessed the number of road traffic casualties and fatalities before and after the implementation of road engineering treatments.

- Contrary to expectations, the interventions resulted in over two times increase in the number of accident casualties (OR, 2.18; 95% CI 1.21 – 3.92; I^2=77%; P=0.009) based on data from 37,195 people after road engineering treatment. Due to the effects of road engineering interventions, the number of fatalities decreased (OR 0.52; 95% CI 0.12 – 2.16; I^2=66%; P=0.37) based on data from 35,868 people after road engineering treatment. The odds of occurrence of fatalities was 48% less due to the impact of interventions.

Two time-series studies assessed the number of road traffic casualties and fatalities before and after road engineering treatments.

- Two studies assessed the number of road traffic casualties (percent change: −30%; 95% CI −160.60 – 35.07; and −111%; 95% CI −492.97 – 24.34) based on data from 5,588 people after road engineering treatment. Percent change in mean accident casualties dropped by 44% in the post intervention period.
- One study reported the number of road traffic fatalities (percent change: −200%; 95% CI −1663.12 to – 48.95) based on data from 1,267 people after road engineering treatment.

RESEARCH GAPS

The included studies in this review represented methodological differences and lacked generalisability. High quality study designs are required, which use control sites or time-series, and one-year-long observational periods, at the start of the intervention and after the implementation of the interventions, are required. Most importantly, the effectiveness of road engineering interventions—in particular, interventions related to segregation of vulnerable road users from motorised vehicles, changes at intersections/junctions, bicycle infrastructure—needs to be addressed in a control trial or a time-series environment. It was astonishing to find that, in all the LMIC, to our knowledge, not a single quasi-experimental study focused on the impact of interventions for preventing road traffic injuries among bicycle riders. In several LMIC, in particular in South Asia, East Asia, and the Pacific region, bicycles and rickshaws are the main mode of transport for daily-wage earners. With increasing global awareness of climate change in relation to the rise in greenhouse-gas emissions from motorised vehicles, the use of bicycles to connect to public transport such as metros, and bus and light-rail transit systems is being encouraged in LMIC. Due to the prevalence of mixed travel conditions in LMIC, the interventions, as well as future research on the effect of interventions for preventing road traffic injuries and fatalities among bicycle riders, are needed. Finally, studies with fatalities and injuries outcome measures were limited across traffic law enforcement and regulations and road engineering interventions. It may be the case that there are limited data on injuries and fatalities primarily because of inadequate injury surveillance systems in LMIC. Improved injury-surveillance systems for data collection, particularly data driven databases linking police records and hospital records on road traffic injuries, are required.

1.0 BACKGROUND

According to the World Health Organization (WHO), road traffic crashes kill at least 1.24m people and injure 50m each year. About half of the world's road traffic deaths occur among motorcyclists (23%), pedestrians (22%), and cyclists (5%), collectively known as 'vulnerable road users' (WHO 2013). The definition of vulnerable road users includes pedestrians, cyclists, and motorcyclists of all age groups. Vulnerable road users are categorised by the amount of protection they have from other motorised traffic. Pedestrians and cyclists are referred to as 'vulnerable' because they are unprotected. Also, riders of two-wheelers—motorcycles, mopeds, and light mopeds—are largely unprotected; hence, they are also referred to as vulnerable.[1]

Low- and middle-income countries (LMIC) have the highest proportions of road deaths among vulnerable road users: pedestrians at 58%; motorised two- or three-wheelers, 40%; and cyclists, 10% (WHO 2013). On the contrary, in high-income countries (HIC), road traffic deaths are highest among car users: Europe at 49% and North America at 70% (WHO 2013). By 2030, road traffic fatalities are projected to be the fifth-greatest cause of deaths in LMIC (WHO 2011).

Road traffic injuries cause huge economic losses to victims and their families, and to nations as a whole. The cost of road traffic accidents in LMIC is estimated to be between 1% and 2% of their gross national product,[2] which is approximately US$100bn every year, exceeding the total amount received for development assistance by these developing countries (Jacobs 2000). The combined effect of population increase, rapid economic growth, and urban sprawl has increased the rate of motorisation (Commission for Global Road Safety 2013) in LMIC. For example, in the Asia region, the number of motor vehicles per 1,000 people has more than tripled in the past 30 years (Pardo 2010). In 2010, the total number of cars and trucks on the world's roads exceeded the 1bn mark. While the demand for motor vehicles in the developed world increased by between 1% and 2%, the demand for motor vehicles increased by 27% in China and by 10% in India. Brazil experienced the second-largest increase, with an additional 2.5m registrations (Commission for Global Road Safety 2009). Based on the current ratio for fatalities per 10,000 vehicles, and projections for increased vehicle registrations, by 2016, the cumulative increase in road traffic fatalities will amount to 250,000 for China; 89,000 for India; and 57,000 for Latin America (Commission for Global Road Safety 2009).

[1] In this review, the definition of 'vulnerable road users' includes pedestrians, cyclists, and motorcyclists of all age groups.

[2] According to Fletcher (2014), extrapolation from HIC estimates of crash costs indicates that the costs could be as high as 5% for LMIC, but these estimates are based on untested assumptions.

1.1 DESCRIPTION OF THE CONDITION

In HIC, owners of private vehicles account for a large majority of road users, and the traffic use pattern is more homogeneous. In low-income countries, walking, cycling, motorcycling and public transport are the predominant transport modes. The traffic use patterns in many LMIC constitute a mix of road users sharing the same roads. In metropolitan cities, non-motorised travel constituted a large modal share; for example, 44% in Chennai, India; 44% in Dakar, Senegal; and 61% in Wuhan, China (World Business Council for Sustainable Development 2004). In Indian metropolitan cities, of the total trip share, bicycles and cycle-rickshaws typically accounted for 10%, walking for 28%, and two-wheelers for 14.5% (Ministry of Urban Development, Government of India 1998). In 2010, the modal share for bicycles in Beijing was 16.7% (China Statistical Bureau 2010). In Dhaka, Bangladesh, 400,000 cycle-rickshaws and auto-rickshaws are the main modes of transport. Every day, rickshaws in Dhaka make 7m passenger trips, travelling 11m miles, which is double the distance completed daily by the London Underground (BBC News 1998).

Road traffic injuries among non-motorised road users are a major public health problem in LMIC. City-level studies have confirmed that the highest proportion of people killed in road traffic crashes are pedestrians. In India, 78% and 53% of those killed on the roads are pedestrians in Mumbai and Delhi, respectively (Mohan 2009). According to the official statistics, pedestrians accounted for 29% of the road fatalities in Mexico. Other studies have placed the figure for pedestrian fatalities in Mexico as high as 48% (Bartels 2010). Pedestrians accounted for 43% of all road traffic fatalities in Ghana (WHO 2013). Pedestrians and cyclists, collectively, are the largest group of victims of road fatalities in Bangladesh, at 44% (WHO 2013). In Nairobi, Kenya, vulnerable road users accounted for 54% of all road fatalities (United Nations Environment Programme 2010).

The figures provided on deaths and injuries involving vulnerable road users often underestimate the true extent of the problem. Many countries do not provide data on the distribution of road traffic deaths across road user categories or trend data. Although they all have some kind of mechanism for counting road-traffic deaths and injuries, moderate and severe injuries often go unreported. This is primarily because of inadequate injury-surveillance systems in LMIC. In these countries, road accidents involving pedestrians and cyclists are often poorly reported in official road traffic statistics. According to the global status report on road safety, of the 182 participating countries, only 77 LMIC reported having a national surveillance system (Commission for Global Road Safety 2013).

The underlying factors for road traffic injuries among non-motorised road users included lack of a safe road environment, mixed road conditions, high-speed motorised vehicles on the road, inadequate pedestrian crossings, as well as negative perceptions of road safety among non-motorised road users. This illustrates the lack of attention paid to the safety of vulnerable non-motorised road users by policy makers (Khayesi 1997; Nantulya 2003; Vasconcellos 2001).

Since the 1960s and 1970s, the modernisation of urban transport in HICs has demonstrated a consciousness of public health, emphasising the integration of walking, non-motorised vehicle (NMV) modes, and motorised transport. In contrast, the transport planning and investment in LMIC has largely focused on the motorised transport sector and has often ignored the needs of non-motorised transport (NMT). Road networks and traffic systems have been designed to increase motor-vehicle speeds in existing mixed-road conditions, such as freeways, built-up urban streets lacking adequate speed-control measures, and growing motorisation. As a result, there is a continual loss of street space for safe NMV use in LMIC. In the light of increasing road traffic deaths and low levels of public investment in road injury countermeasures in LMIC, the WHO and several other international agencies began increasingly getting involved in improving road safety conditions in LMIC during the 1990s. The World Bank's Global Road Safety Partnership (1999), World Bank's Global Road Safety Facility (2006), Bloomberg Philanthropies' 'Road Safety in 10 Countries Project – RS-10' (2009), and the United Nations General Assembly Resolution 'Decade of Action for Road Safety (2011 to 2020)' (2011) aimed to implement road safety measures in collaboration with national stakeholders, non-governmental organisations (NGOs) and academic institutions of LMIC. The DFID (UK) and the World Road Association (WRA) have initiated several studies on improving road-safety conditions in LMIC. Research has demonstrated that countermeasures that aim to prevent injuries appear to be cost-effective in the Road Safety in 10 Countries Project (RS-10 participating countries: Brazil, Cambodia, China, Egypt, India, Kenya, Mexico, the Russian Federation, Turkey and Vietnam (Esperato 2012).

Increased sensitisation of local and civic organisations, establishment of national coordinating committees and road safety programmes, and improved systems for data collection and analysis have led to the emergence of successful road safety intervention implementation practices in LMIC (Bishai 2006, Commission for Global Road Safety 2009, Esperato 2012). Nevertheless, it is important to document these experiences using a systematic-review approach. Scientific evidence on the effectiveness of road safety interventions will strengthen the case for informed decision-making regarding the implementation of limited public investment that targets the most common and vulnerable victims of road traffic crashes in the LMIC.

1.2 DESCRIPTION OF THE INTERVENTION

Interventions to prevent road crash injuries largely include road engineering, enforcement of traffic laws and regulations, education, information, media advocacy, vehicle design, and post-crash/trauma care. In this review, we focus on three broad categories of road infrastructure interventions: road engineering, enforcement of traffic laws and regulations, and a combination of road engineering and regulatory/legislative interventions.

Road engineering interventions are preventive measures involving physical engineering or structural changes to the road layout or road design that directly affect road-user behaviour and have the potential to prevent road traffic injuries among the most vulnerable. Road engineering interventions covered in this review comprise three sub-categories: 1) measures for reduction in vehicle speed; 2) changes at intersections; and 3) segregation of non-motorised road users from motorised vehicles.

Enforcement of traffic laws and regulation interventions refers to establishing and enforcing road safety rules and ensuring compliance from road users through legal enforcement. Within regulatory interventions, we focused on regulatory approaches that can affect large populations through law enforcement. These include, for instance, speed cameras, speed limits, speed zones, red-light enforcement cameras, use of daytime running lights for two-wheelers, mandatory use of helmets, traffic signal regulation, and give way/stop signs.

Examples of a combination of engineering and regulatory and legislative interventions include segregated or on-road marked bicycle lanes/bike paths involving specific road changes at junctions and intersections; bus rapid-transit system with motorcycle lanes; pedestrian crossings and rumble strips on bus rapid-transit system; speed limit enforcement with speed reduction measures; use of traffic lights for traffic regulation; all-time red extension; fully controlled right-turn phase; red-light cameras; and street lighting.

1.3 HOW THE INTERVENTION MIGHT WORK

The main cause of human morbidity and mortality in road-crash injuries is due to sudden mechanical energy reaching people at rates that involve forces in excess of their body injury threshold (Haddon 1999). Managing the excess energy that may contribute to the occurrence of a crash and the severity of injuries during the crash, therefore, is one of the main basic principles of road traffic injury control. The Haddon approach, formalised in 1973 (Haddon 1995; also referred to as 'ten countermeasure strategies') emphasised the use of technological modifications to reduce road traffic injuries. The 10 countermeasures are as follows: 1) to prevent the generation of thermal, kinetic or electrical energy (by promoting alternative travel modes); 2) to reduce the amount of energy generated (by the use of speed limits on roads); 3) to prevent the inappropriate release of energy (use of safer intersections and roundabouts); 4) to modify the rate of spatial distribution of release of the energy from its source; 5) to separate in space or time the energy being released from susceptible structures (use of sidewalks and phasing of pedestrian and vehicular traffic); 6) to separate by interposition of a material barrier (median, segregated cycle tracks); 7) to modify appropriately the contact surface, sub-surface, or basic structure (through motor-vehicle design); 8) to strengthen the structure that might otherwise be damaged by the energy transfer (using tougher codes for motor-vehicle-impact resistance, such as helmet legislation); 9) to move rapidly in detection and evaluation of that which has occurred or is occurring (use of emergency transport); and 10) all emergency measures following the accident (provision of emergency care) (Haddon 1995).

Broadly, road engineering interventions and regulatory and law enforcement interventions affect the road environment and minimise vulnerable road user exposure to road traffic injuries in the following ways: 1) by reducing the amount of energy; 2) by preventing the inappropriate release of energy in the environment; 3) by interposing a physical barrier or separating the released energy from susceptible structures; and 4) by strengthening the legal framework on road safety rules and regulations.

Speed reduction measures consist of traffic calming techniques that discourage traffic from entering certain areas, and the installation of physical speed reducing barriers. These barriers are designed to achieve various levels of appropriate speed. A study by Brilon (1993) demonstrated that, in Germany, traffic calming measures improved traffic safety by reducing the number of injury accidents for motorbikes by 78%, for pedestrians by 25%, and for cyclists by 17%. Area-wide urban traffic calming schemes have shown a reduction in the overall number of injury accidents by about 15%. The study (Elvik 2001) presented a meta-analysis of 33 studies that have evaluated the effects on road safety of area-wide urban traffic calming schemes. According to the study, the number of accidents in residential streets was reduced by 25%, and, on main roads, the number of accidents was reduced by 10%.

Road surface treatments, such as creating raised road surfaces at cross roads (speed humps or speed bumps) or noisy road surfaces such as rumble strips, can also act as traffic calming measures by discouraging motorised vehicles from travelling at speeds that may put pedestrians and cyclists at high risk. Reviews, case studies, and evaluation reports demonstrate that the measures aimed at reducing traffic speed are considered essential to prevent road injuries (Allsop 2010; Department for Transport 2010 and 2011; Morrison 2003; Wilson 2010).

Mini-roundabouts are a type of intersection that may produce some traffic calming effect. These are an optimal alternative for safety and operational issues (excessive delays at minor approaches) at existing stop controlled or signalised intersections. Mini-roundabouts are distinguished from neighbourhood traffic circles primarily by their traversable islands and yield control on all approaches. The concept of mini-roundabouts was introduced in the UK in the 1970s. Since then, mini-roundabouts have been used successfully in the UK to improve safety at junctions. At three-arm junctions, mini-roundabouts have a mean accident rate that is 30% less than that of signalled junctions (Department for Transport 2011). The severity of accidents at three-arm mini-roundabout sites is lower than at three-arm signalled junctions and considerably lower than at 30-mph T-junctions. This intervention has reduced the crash rate by approximately 30% as compared to signalised intersections (Bodé 2006).

There are studies that have shown how bicycle infrastructure reduces injury risks and makes cycling a safe mode of transport (Garder 1998; Harris 2013; Lusk 2011; Reynolds 2009). Sidewalks, pathways, or footpaths provide several benefits, including safety, mobility, and healthier communities (Federal Highway Administration [FHWA] 1987). Walkways separated from the travel lanes may prevent up to 88% of walking-related roadway crashes (FHWA 2001).

Road safety legislation addresses road, vehicle, and user safety standards and rules, and their compliance with them. A strong legislative framework, public acceptability, and practicality are crucial determinants for effectiveness of road safety legislation. Regulatory and legislative interventions have played a significant role not only in reducing the number of motor vehicle crashes and injuries but also in lessening the severity of injuries (Chisholm 2008; Ferrando 2000; Redelmeirier 2003).

1.4 WHY THIS REVIEW IS IMPORTANT

Published reviews on the effects of road infrastructure interventions have largely focused on interventions for road drivers in high-income settings and have included only controlled-study designs, such as randomised controlled trials (RCTs) or controlled before-and-after (CBA) studies. In an assessment of the contribution of road-safety studies from LMIC in systematic reviews in the Cochrane Injury Group, we found that, of the 13 published systematic reviews, only one review, Helmets for preventing injury in motorcycle riders, included studies that were conducted in LMIC (Perel 2007). This review included 53 studies, of which 51 were cross-sectional studies. This review was able to include studies from LMIC because the review inclusion criteria included any non-randomised study that compared an intervention and a control group. Three Cochrane reviews have looked at interventions that impact vulnerable road users. One is a published protocol on cycling infrastructure, the second is on educational interventions (outside the purview of this review), and the third review only included RCTs, but not from any LMIC.

The lack of studies on road safety from LMIC in systematic reviews could be due to contextual differences in road infrastructure interventions between LMIC and non-LMIC, and inclusion criteria restricted to ideal study designs only.

Many of the interventions examined in high-income settings are costly and may not be relevant in low-income settings. Road engineering interventions in developed countries evolved in the 1960s and the 1970s in response to a growing number of road accidents caused by increasing use of motor vehicles. The implementation of effective interventions being practised in the developed world required considerable research, testing, and capital funds. In LMIC, the challenge is to transfer the knowledge gained in the developed countries in a fraction of the time. Also, the interventions need to be tested for cost and design effectiveness. According to the safe-system infrastructure implementation, cost, compliance issues, design and implementation difficulties, public acceptance, and maintenance were typical barriers in LMIC (Turner 2013). Compliance with road treatments as well as compliance towards traffic law enforcement by road users is a major issue in several LMIC. The legal system for implementing road infrastructure interventions needs to be developed to standards similar to those practised in developed countries.

From our preliminary searches, we identified some non-randomised studies in LMIC that assessed impacts on vulnerable road users, and that would be eligible for this review. One study (Donroe 2008) investigated personal and environmental risk factors for child-pedestrian road traffic injuries relevant to the setting of a developing country. Mutto undertook a before-and-after study on pedestrian road behaviour and traffic patterns one year before and one year after overpass construction (Mutto 2002). Another before-and-after study (Wu Yuan 1996) examine the effectiveness of the 'ride-bright' legislation in Singapore that was implemented in November 1995.

A review of the evidence of road infrastructure interventions with a focus on LMIC is necessary to establish what is known and what is, as yet, unknown, and will attempt to provide a quantification of the effects of these interventions. The focus of this review was LMIC (the setting). This review included the context of interventions being implemented by LMIC and may represent stages of development towards HIC status. In this review, the LMIC context is important, not the sophistication of the intervention. The findings of this review provide valuable, and contextually relevant, data for road safety policy making in LMIC settings.

We are aware that interventions shown to be effective in HIC have been implemented in LMIC, but the evidence of such interventions has not been evaluated. The findings of this review will facilitate a comparison with the results of rigorous reviews of similar interventions in non-LMIC. Examination of areas of commonality between HIC and LMIC in respect of comparative effectiveness may allow for the development and refinement of an empirically supported theory for policy-making in LMIC and non-LMIC.

The results also provide an overview of the quality of available intervention research and how future primary research could be improved within the transport field in LMIC. This review addresses a topic of particular relevance to tackling income-related health inequalities in LMIC.

2.0 METHODS

This review included intervention studies that reported quantitative data evaluating the effects of road infrastructure for preventing road traffic injury (fatal and non-fatal) among non-motorised road users and motorised two-wheel road users in LMIC. Studies that compared changes in outcomes before and after the intervention implementation, with or without a control group, were included. The intervention implementation was either at cluster level or individual level, or at both. Participants' selection was by outcome; hence, outcome reporting is at individual level.

Study designs considered for this review were RCTs, CBA, uncontrolled before and after, time series and case-control. Road safety audits were considered and included to add richness to the effectiveness data as part of a process evaluation of a qualifying intervention study, provided they met the inclusion criteria for considering studies for this review. This review did not include black-spot-identification studies.

The definition of vulnerable road users[3] included all vulnerable road user pedestrians and those using NMT[4] and motorised two-wheelers (motorcycles, mopeds and light mopeds)—inclusive of all ages in LMIC. The review used the World Bank definition of NMT. The World Bank classifies NMT as necessary modes of transport only, while excluding activities that use walking or bicycling for exercise and pleasure (World Bank 1993). The review used the World Bank definition of LMIC.

Road safety interventions broadly fall under the following categories: road engineering, enforcement of traffic laws and regulations, education, information, media advocacy, vehicle design and post-crash/trauma care. This review looked at specific population-level interventions that were implemented to prevent road traffic crashes and injuries among vulnerable road users in a LMIC setting. The broad categories of interventions considered for this review were:

I. Road engineering interventions: 1) measures for reduction of vehicle speed; 2) changes at intersections; 3) segregation of vulnerable road users from motorised vehicles.

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[3] Vulnerable road users are categorised by the amount of protection they have from other motorised traffic. Pedestrians and cyclists are referred to as vulnerable because they are unprotected. Also, riders of two-wheelers—motorcycles, mopeds, and light mopeds—are largely unprotected; hence, they are also referred to as vulnerable.

[4] A broad definition of NMT would be any mode of transport system that does not run on a motor. NMT is also known as human-powered transportation. Some examples are walking, bicycles, skateboards, snowboards, roller-skates, push scooters, hand-drawn carts, animal-drawn carts, rickshaws, wheel barrows and wheelchairs. These modes are used by 1) those who walk as part of their occupation (e.g. for example, porters and cycle-rickshaw operators transporting goods or people); and 2) those who walk as an activity (for example, for leisure or to their workplace).

II. Traffic law enforcement and regulatory interventions.
III. A combination of engineering and regulatory and legislative interventions.

Although highly sophisticated interventions on safe-vehicle designs influence road safety, they are not yet widely used in LMIC. Hence, the scope of this review does not include vehicle design. This review focused on preventive measures implemented to avoid road traffic accidents and injuries. Therefore, we did not cover post-crash-care interventions such as pre-hospital care, access to emergency hospital systems, the role of bystanders, emergency rescue services, and rehabilitation services used to avoid death and disability and to limit the severity of the suffering of the victim.

Interventions such as education, information, and advocacy programmes have been covered by past Cochrane reviews (Duperrex 2002, Kardamanidis 2010, Owen 2011). Systematic reviews or meta-analyses describing the effects of education, information, and advocacy-programme interventions have been covered in non-Cochrane reviews as well (Coleman 1996, Delhomme 1999, Elliot 1993, Struckmann Johnson 1989, Vernick 1996); hence, these interventions were not covered by this review.

The systematic search was conducted between December 2014 and 13 February 2015. The following databases were searched from 1990 to current: OvidSP Medline, OvidSP Embase, OvidSP Transport, PubMed, Cochrane Injuries Group Specialised Register, Cochrane Central Register of Controlled Trials and Proquest ERIC.

Databases were searched for terms describing the population: vulnerable road users in LMIC; interventions: road engineering, conspicuity/visibility; and purpose: safety/accident prevention in work/school-related travel. Two sets of interventions were developed for this search. The first contained road engineering interventions, and the second included conspicuity/visibility terms. Both sets of interventions were combined using the Boolean operator OR, then combined (AND) with population and purpose. The intention was to include all potentially relevant results, while simultaneously excluding the large body of literature for road safety in non-work/school-related purposes in non-LMIC. The search was limited to publication year 1990 to current.

Search strategies for bibliographic databases provide detailed notes regarding the search of each of these databases, including the keywords used and search strategy (Appendix 1). In addition, searches of grey literature (Appendix 2) and sources of conference proceedings (Appendix 3) provided notes on type of search and dates of the search strategy for other sources.

We identified 8539 study records through database searching. A preliminary screening removed 5532 study records related to bio-medicine and to other transport disciplines. In total, 3007 records were included for screening. An additional 489 study records were identified through searching other sources which included web sites of road safety organization's databases, conference proceedings and reference lists of selected papers. Thus, 3496 study records were included for screening. After removing 1004 duplicate records, 2492 study records were screened for inclusion criteria. The search flowchart (Appendix 4) details the search process.

Finally, 162 studies were identified for inclusion, of which 82 were assessed for full-text review. During the full-text assessment, 57 studies were excluded - 49 studies were excluded for several reasons plus 8 studies were supplementary publications of included studies. Several studies used modelling techniques to predict the impacts of road infrastructure interventions. These study designs represented a large body of evidence on the review topic. While screening the search results, we identified a list of 75 studies using modelling techniques (Appendix 5) that had potentially relevant evidence related to our review topic.

Multiple reports or research papers based on the same study or data were treated as a single entity. We selected the most complete reference as the primary document for coding. Other documents were used only if they provided unique information of relevance to this review.

Dealing with missing data

For those studies in which the raw data required clarity of information or where the data were missing, the corresponding authors were contacted. Almost all authors responded with regard to clarity of information and two authors provided a supplementary publication for detailed data. Raw data were incomplete in two CBA studies with regard to sample size, pre- and post-intervention period, and outcome data in treatment or control sites. Required data were drawn from the reported data-related information in the studies; however, the computed data were not used in the meta-analysis.

2.1 DESCRIPTION OF STUDIES

A total of 25 studies meeting the study-inclusion criteria were identified for the review (Appendix 6). Three studies were conducted between 1994 and 1999; nine studies were conducted between 2000 and 2009; and, more than half (13) studies were conducted in the period 2010–14. Additionally, using a modelling technique, we identified a list of 75 studies that had potentially relevant evidence related to our review topic (Appendix 5). Three studies were conducted between 1994 and 1999; 30 studies were conducted between 2000 and 2009; and more than half (42) the studies were conducted in the period 2010–14. The dates of evidence in included studies and modelling studies demonstrated progressive involvement of international agencies in improving road safety conditions during the 1990s in LMIC.

Studies included for the review were identified from a wide variety of LMIC: 14 from East-Asia and the Pacific region, four from the Latin-America and Caribbean region, three from the Sub-Sahara-Africa region, and, finally, two each from the South Asia region and Europe and Central Asia region. Notably, 23 studies belonged to middle-income countries and only two studies belonged to low-income countries (namely, Bangladesh and Uganda).

The status of Taiwan as an independent nation is currently disputed. Therefore, very few international organisations recognize Taiwan and usually cite Taiwan as a province of China. Since China is classified as a middle-income country according to the World Bank definition, we have considered Taiwan to be a province of China under the middle-income country category.

There were variations in study design. Five time-series studies, one case-control study, and one controlled before-and-after study (CBA) were identified. The rest of the remaining 18 studies, the majority used an uncontrolled before-and-after study design.

By intervention classification, 15 studies were classified under the category of traffic law enforcement and regulatory interventions. The majority focused on mandatory helmet use, a few studies focused on enforcement of pedestrian signalization, and red-light and speed violation at crossings. Across 9 road engineering studies, the majority evaluated the effect of traffic-calming measures, such as road bumps/humps, followed by changes at intersection such as pedestrian signalisation or signals at pedestrian crossings. And, one study included a combination of regulatory and road engineering intervention.

All studies examined one or more of the primary or secondary outcomes. Some studies reported only primary outcome measures: deaths, moderate and severe injuries, and road traffic casualties. Some studies reported only secondary outcome measures, compliance for traffic law enforcement and regulation and mean vehicle speed. Last, only one study reported adverse outcome measure, increase in collision between motor-vehicles as a result of traffic calming measure speed bumps.

One study (Allyana 2014) presented outcome data for two sub-categories of traffic law enforcement and regulatory intervention separately. We extracted and assessed outcome data as they related to the sub-category Automated Enforcement System – Red-Light Violation into one group, and as they related to the sub-category Automated Enforcement System – Speed Limit into another group.

One time-series study (Radin Umar 1995a) presented a before-and-after analysis of motorcycle accident casualties on enforcement of conspicuity; specifically, the use of daytime running headlight intervention in Seremban and Shah Alam, Malaysia. A second time-series study (Radin Umar 1995b) presented a preliminary analysis on the impact of a road engineering intervention—segregation of a motorcycle lane—on motorcycle accident casualties along Federal Route F02 within the Shah Alam, Malaysia.

2.2 INCLUDED STUDIES

For assessment of the overall quality of evidence for each study, we downgraded the evidence from 'high quality' (i.e. A grade studies) by one level for study limitations (risk of bias) with one or two weak ratings (i.e. B grade studies). For studies having very serious (more than two weak ratings) study limitations, confounders, lack of data collection method used, intervention integrity, outcome ascertainment, and missing dates were excluded as C grade studies.

A total of 25 studies matched study inclusion criteria. Of the included studies, we excluded four studies from meta-analysis and narrative synthesis: Passamore 2010, Wu 2013, Yuan 2010, and Yuan 2012. These studies were assessed as C grade studies due to high risk of biases around internal validity, data collection, confounders, and intervention integrity based on Hamilton Assessment Tool (HAT) ratings. For short description of included studies, see Appendix 6.

We tabulated the data from the included studies using the same format as the Cochrane Review's 'characteristics of included studies', such as methods, participants, interventions, and outcomes. We also added information on country of study, the setting/context in which intervention was implemented, data collection duration, and data collection timing. See table ''characteristics of included studies' (Appendix 7).

2.3 EXCLUDED STUDIES

A total of 57 studies (49 studies plus 8 supplementary publications) were excluded from the review for reasons related to study design, outcome or intervention. In five studies, the main findings were not clearly stated. Another seven studies did not include data on relevant outcomes. Five studies focused on intervention category, such as road traffic injuries related to type of motorcycle engine used or quality of helmet used, were not included in this review. Nine studies focused on other causes of road traffic injuries, such as quality-adjusted life years and head injuries of motorcyclists with helmet use, comparison of trauma injuries between motorcycle and cycle accidents, cross-sectional studies on head injury and helmet use, signal timing, and advocacy. In 23 studies, the study design did not compare changes in outcomes before and after intervention, with or without a control group.

2.4 RISK OF BIAS IN INCLUDED STUDIES

All of the studies included in this review are observational and, therefore, subject to risk of bias in terms of making a causal inference regarding the effect of road safety measures.

Based on the HAT ratings for study design criteria, the five studies having moderate risk of bias were: Espitia-Hardeman 2008, Nadesan-Reddy 2013, and Radin Umar 1995a, Radin Umar 1995b, and Radin Umar 2005. These studies used time-series designs; two studies defined the use of interrupted time-series methodology and three studies used time-series data, but did not discreetly define using data from the intervention period. All five, however, were short time-series studies using monthly data, extending, at the most, one year before and one year after the intervention. Hence, a serious issue across these studies was the inability to assess the long-term trend effect confounding factor.

Another set of studies having a moderate risk for bias rating for study design criteria based on the HAT ratings was: Liu 2011. This study used a controlled before-and-after design, but neither the control sites nor the treatment sites were selected randomly. The comparison group was selected by researchers based on the rule that an intervention had never been deployed at the comparison site. Nevertheless, the study reported to control for confounders by matching treatment and comparison sites on trends, exposure, and geographic characteristics of roads/location. In addition, the study matched one large comparison site for same time period. Hence, general changes and change of traffic-volume confounding factors were possibly controlled (Elvik 2002).

The last study having moderate risk of bias for study design criteria based on the HAT: Quistberg 2014. This study used a case-control design, and compared cases with road traffic injuries to people from the same population and during the same time period. This study observed a very limited time period for pedestrian and vehicle flow, however. Also, the collision dates and data collection were not contemporaneous. However, the study explored the impact for time difference and found no significant changes. It is possible that the police did not report all pedestrian collisions.

In our assessment of overall study quality using the HAT ratings, the time-series studies: Espitia-Hardeman 2008, Nadesan-Reddy 2013, Radin Umar 1995a, Radin Umar 1995b, and Radin Umar 2005; the controlled before-and after-study: Liu 2011; and one case control study: Quistberg 2014 provided the strongest assessment of the safety effects of road engineering and enforcement interventions compared to the rest of the included studies.

There were 13 studies of moderate overall quality: Afukaar 2003, Antic 2013, Allyana 2014, Bastos 2005, Bhatti 2011, Chiu 2000, Hoque 2005, Ichikawa 2003, Liberatti 2001, Lipovac 2013, Nguyen 2013, Panichaphongse 1995, and Zhang 2010. These studies used an uncontrolled before-and-after study design. To be statistically sound, a before-and-after study design should control for general changes of the number of accidents from before to after the road safety measure is introduced, changes in traffic volume, and the problem of regression-to-the-mean biases (Elvik 2002). In all uncontrolled before-and-after studies, the post-intervention period started immediately after the deployment of the intervention and included just one period before and one period after; therefore, general changes and change of traffic volume confounders were possibly controlled.

Finally, one study was rated of overall weak quality: Mutto 2002. This study used an uncontrolled before-and-after study design and two relevant confounders were not adequately controlled.

Of the included studies, we excluded four studies from meta-analysis and narrative synthesis: Passamore 2010, Wu 2013, Yuan 2010, and Yuan 2012. These studies received a couple of weak ratings for biases around internal validity, data collection, confounders, and intervention integrity, based on HAT ratings.

Withdrawal and dropout biases are not applicable in road engineering intervention studies. Based on HAT tool ratings a moderate risk of bias is assigned to all studies. Additional details of individual studies' risk-of-bias ratings based on HAT is provided in risk of bias assessment table (Appendix 8).

For road engineering interventions, almost all studies were coded as at risk for regression-to-the-mean confounder, given that the selection of treatment sites for road safety measures are largely influenced by the high accident rates (Elvik 2002). However, most studies adequately controlled for some of the essential confounders related to road infrastructure interventions, including similarity between control and intervention sites, exposure effect (in traffic volume, area type, geometric design, and type of intersection) before and after the intervention implementation, and outcome ascertainment (outcome reporting for non-motorised road users or having road traffic injuries only). Appendix 9 presents an assessment of confounders based on HAT in individual studies included for meta-analysis and narrative synthesis.

However, all studies in this synthesis are subject to some risk of bias. Three potential confounding variables across all the studies are 1) the possibility of another intervention or policy change that is confounded with the start of the road engineering or enforcement intervention, 2) lack of long-term observational periods before and after the interventions, and 3) the regression-to-the mean confounder in studies with road engineering intervention.

3.0 RESULTS

Meta-analysis of quantitative data was restricted to only higher quality studies (that is, studies having primarily low and moderate risk for bias, or up to two weak ratings, based on the HAT tool). For short description of included studies, see Appendix 6.

Effect sizes were computed for 18 studies. Three studies presented distinct methods to implement and measure road-infrastructure intervention outcome effects. The number of studies was not sufficient to calculate a standardized effect size, these studies were presented as qualitative case descriptions. Four studies were excluded from meta-analysis and narrative synthesis. Effects were coded for vulnerable road users of all ages during all hours. Effect sizes for time-series studies were adjusted for time trend.

The analysis of uncontrolled before-and-after studies used odds ratio (OR) with 95% confidence intervals (CIs) for the dichotomous outcome results and standardized mean difference with 95% CI for the continuous outcome results. The analysis of time-series studies used log risk ratio (RR). Data were presented graphically in a forest plot to show the OR/RR and 95% CI for each study. Statistical heterogeneity was assessed using Chi^2, I^2 and Tau^2 statistics. Due to the expected high level of heterogeneity across studies, the random effect model was used.

The meta-analysis results presented extreme heterogeneity; hence, a narrative synthesis of the data was presented according to the Economic and Social Research Council guidance (Popay 2006). All the data to be synthesized were predominantly quantitative. With this in mind, we found vote counting and developing a common rubric the most relevant for the synthesis at hand. The vote counting and a common rubric were developed by using two approaches: 1) using a tick mark where the effect of the intervention was positive and overall statistical significance based on effect size; and 2) analysing absolute measure of effect.

3.1 TRAFFIC LAW ENFORCEMENT AND REGULATORY INTERVENTIONS

3.1.1 UNCONTROLLED BEFORE-AND-AFTER STUDIES

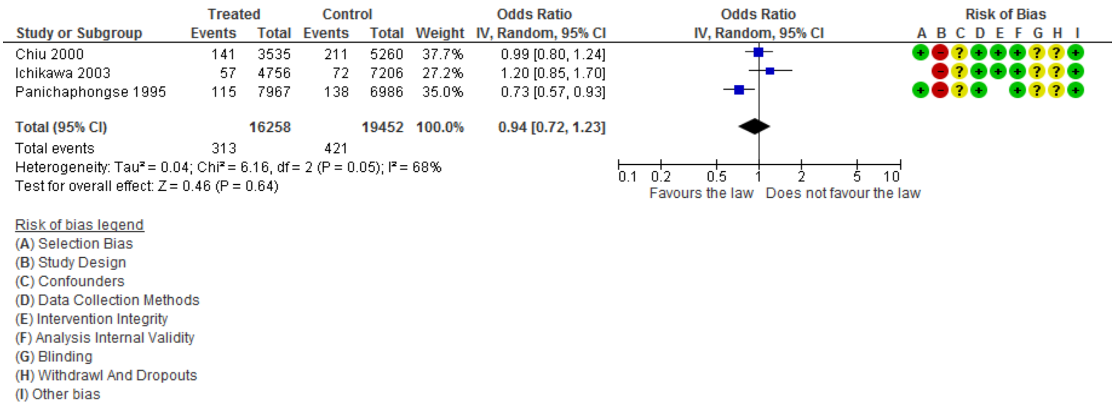
Ten uncontrolled before-and-after studies (Allyana 2014; Bastos 2005; Bhatti 2011; Chiu 2000; Ichikawa 2003; Liberatti 2001; Lipovac 2013; Nguyen 2013; Panichaphongse 1995; Zhang 2010) assessed the number of primary and secondary outcomes before and after enforcement of traffic laws and regulatory interventions. In general, effect sizes showed favourable outcomes after intervention. Fatalities declined by 6% and overall injuries declined by 26%. Although the effect sizes were not statistically significant for fatalities and severe injuries, effect sizes for moderate injuries were statistically significant.

The interventions favoured secondary outcomes, as determined by effect size. Mandatory helmet-law enforcement resulted in an eightfold increase in compliance among motorcyclists compared with when there was no mandatory helmet-law enforcement. One reason for this could be that interventions being implemented by LMIC perhaps represented the initial stages of positive development towards zero road traffic injuries and fatalities after implementing interventions.

PRIMARY OUTCOMES

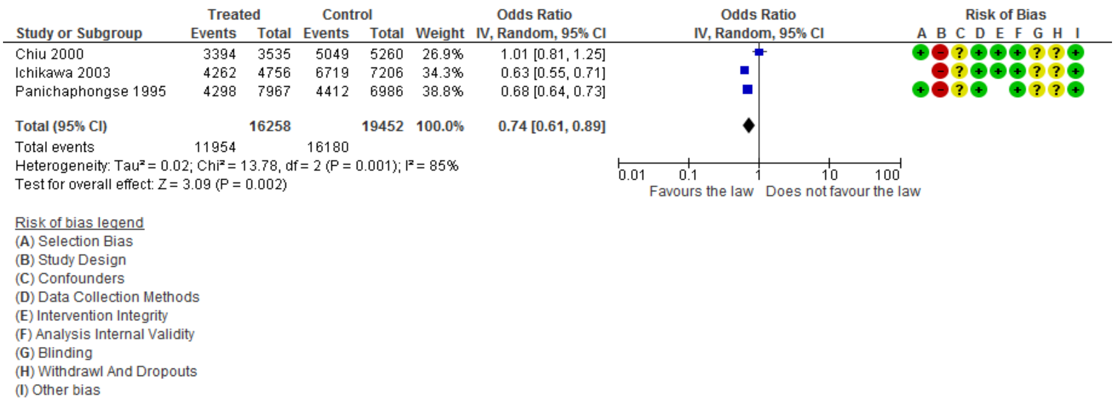
Three studies (Chiu 2000; Ichikawa 2003; Panichaphongse 1995) assessed the number of road traffic crashes resulting in fatalities among motorcycle riders (OR 0.94; 95% CI 0.72–1.23; I^2=68%; P=0.64), based on data from 16,258 people after the helmet law enforcement intervention. The Chi^2 test for heterogeneity, however, was not significant for fatalities. The I^2 value was 68% (medium heterogeneity) and the Tau^2 value of less than 1 also confirmed absence of substantial heterogeneity. For forest plot, see figure 1:

Figure 1. Traffic law enforcement and regulatory intervention – mandatory helmet use: fatalities among motorcycle riders.



Three studies (Chiu 2000; Ichikawa 2003; Panichaphongse 1995) assessed the number of road traffic crashes resulting in injuries (OR, 0.74; 95% CI 0.61– 0.89; I^2=85%; P=0.002), based on data from 16,258 people after the helmet law enforcement intervention. For forest plot, see figure 2:

Figure 2. Traffic law enforcement and regulatory intervention – mandatory helmet use: injuries among motorcycle riders.



Two studies (Chiu 2000; Panichaphongse 1995) assessed the number of road traffic crashes resulting in severe injuries (OR, 0.91; 95% CI 0.82 – 1.01; I^2=32%; P=0.22), based on data from 11,502 people after the helmet law enforcement intervention. Interestingly, there was no heterogeneity in the effect measures for severe injuries in the meta-analysis of the two studies. Two studies (Chiu 2000; Panichaphongse 1995) assessed the number of road traffic crashes resulting in moderate injuries (OR 0.77; 95% CI 0.72 – 0.81; I^2=91%; P=0.00001), based on data from 11,502 people after the helmet law enforcement intervention. For severe injuries forest plot, see figure 3 and for moderate injuries forest plot, see figure 4:

Figure 3. Traffic law enforcement and regulatory intervention – mandatory helmet use: severe injuries among motorcycle riders.

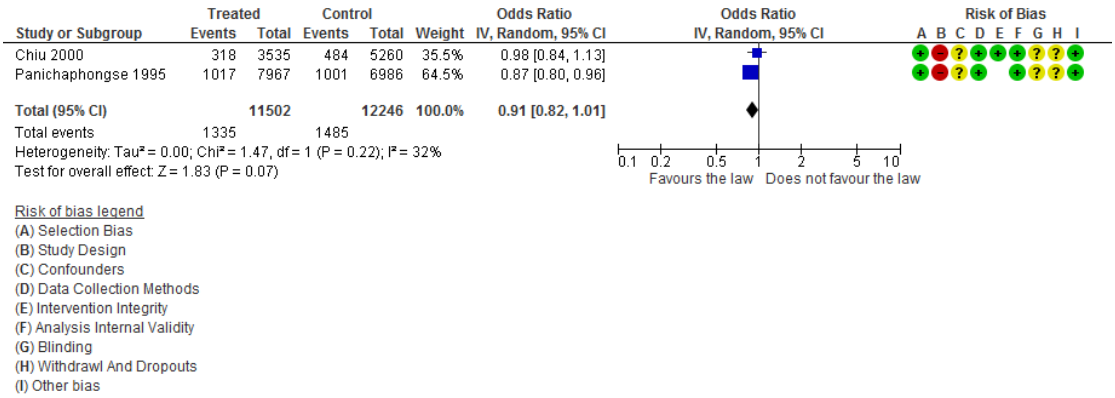
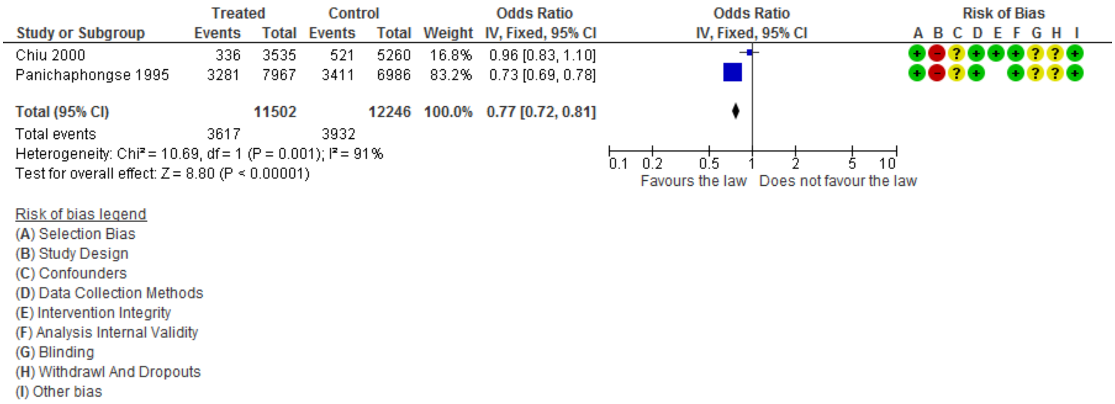


Figure 4. Traffic law enforcement and regulatory intervention – mandatory helmet use: moderate injuries among motorcycle riders.



SECONDARY OUTCOMES

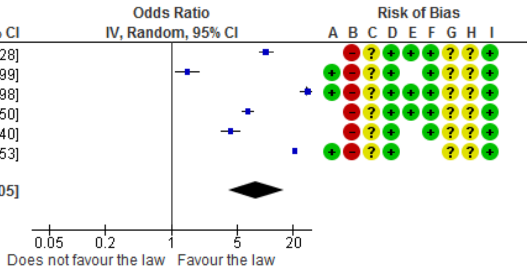
Six studies (Bastos 2005; Bhatti 2011; Chiu 2000; Liberatti 2001; Ichikawa 2003; Nguyen 2013) assessed compliance with mandatory helmet use (OR 8.05; 95% CI 4.03 – 16.05; I^2=99% P=0.0001), based on data from 10,4255 people after the helmet law enforcement intervention. In places where a mandatory helmet law was enforced, there was eight times more compliance among motorcyclists compared with when there was no mandatory helmet law enforcement. The wide confidence interval shows low precision of the odds ratio estimate. The statistical test of heterogeneity between the effect sizes for the six studies included in the meta-analysis was statistically significant (P < .00001).

Two studies (Bhatti 2011; Nguyen 2013) assessed compliance among pillion riders (OR 6.41; 95% CI 2.65 – 15.54; I^2=80%; P=0.0001), based on data from 37,552 people after the helmet law enforcement intervention. The forest plots are given in figure 5 and figure 6:

Figure 5. Traffic law enforcement and regulatory intervention – mandatory helmet use: compliance among motorcycle riders.

Study or Subgroup	Treated Events	Treated Total	Control Events	Control Total	Weight	Odds Ratio IV, Random, 95% CI
Bastos 2005	1380	1603	580	1546	16.7%	10.31 [8.65, 12.28]
Bhatti 2011	224	369	190	373	16.4%	1.49 [1.11, 1.99]
Chiu 2000	1630	3535	153	5260	16.7%	28.56 [24.00, 33.98]
Ichikawa 2003	1082	4347	325	6776	16.8%	6.58 [5.77, 7.50]
Liberatti 2001	434	656	224	718	16.6%	4.31 [3.44, 5.40]
Nguyen 2013	87921	93745	32262	76898	16.9%	20.89 [20.27, 21.53]
Total (95% CI)		**104255**		**91571**	**100.0%**	**8.05 [4.03, 16.05]**
Total events	92671		33734			

Heterogeneity: Tau² = 0.74; Chi² = 815.58, df = 5 (P < 0.00001); I² = 99%
Test for overall effect: Z = 5.92 (P < 0.00001)

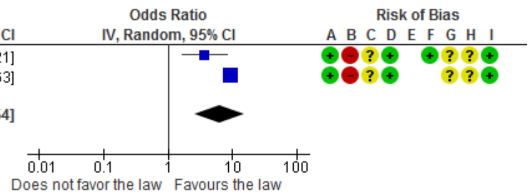


Risk of bias legend
(A) Selection Bias
(B) Study Design
(C) Confounders
(D) Data Collection Methods
(E) Intervention Integrity
(F) Analysis Internal Validity
(G) Blinding
(H) Withdrawl And Dropouts
(I) Other bias

Figure 6. Traffic law enforcement and regulatory intervention – mandatory helmet use: compliance among pillion riders.

Study or Subgroup	Treated Events	Treated Total	Control Events	Control Total	Weight	Odds Ratio IV, Random, 95% CI
Bhatti 2011	26	140	9	155	40.3%	3.70 [1.67, 8.21]
Nguyen 2013	31405	37412	12161	33779	59.7%	9.29 [8.97, 9.63]
Total (95% CI)		**37552**		**33934**	**100.0%**	**6.41 [2.65, 15.54]**
Total events	31431		12170			

Heterogeneity: Tau² = 0.34; Chi² = 5.13, df = 1 (P = 0.02); I² = 80%
Test for overall effect: Z = 4.11 (P < 0.0001)



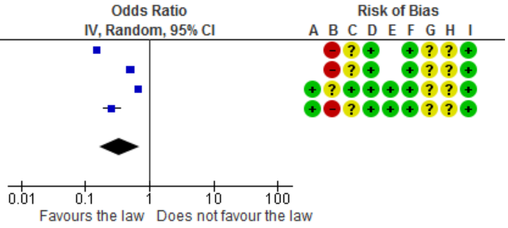
Risk of bias legend
(A) Selection Bias
(B) Study Design
(C) Confounders
(D) Data Collection Methods
(E) Intervention Integrity
(F) Analysis Internal Validity
(G) Blinding
(H) Withdrawl And Dropouts
(I) Other bias

Three studies (Allyana 2014; Lipovac 2013; Zhang 2010) reported red-light and speed violations (OR 0.34; 95% CI 0.17 – 0.70; I^2=100%; P=0.003), based on 26,388 people after the law enforcement intervention. The enforcement of traffic laws thus yielded a 66% less occurrence of red-light and speed violations. The test of heterogeneity indicated statistical significance. The forest plot is given in figure 7:

Figure 7. Traffic law enforcement and regulatory intervention – traffic law enforcement: non-compliance with red lights and speed limits.

Study or Subgroup	Treated Events	Treated Total	Control Events	Control Total	Weight	Odds Ratio IV, Random, 95% CI
Allyana 2014	1538	15679	6624	15905	25.3%	0.15 [0.14, 0.16]
Allyana 2014	3778	171378	6870	159976	25.3%	0.50 [0.48, 0.52]
Lipovac 2013	1442	6385	2127	7045	25.2%	0.67 [0.62, 0.73]
Zhang 2010	50	69946	188	67874	24.2%	0.26 [0.19, 0.35]
Total (95% CI)		**263388**		**250800**	**100.0%**	**0.34 [0.17, 0.70]**
Total events	6808		15809			

Heterogeneity: Tau² = 0.54; Chi² = 1250.94, df = 3 (P < 0.00001); I² = 100%
Test for overall effect: Z = 2.92 (P = 0.003)



Risk of bias legend
(A) Selection Bias
(B) Study Design
(C) Confounders
(D) Data Collection Methods
(E) Intervention Integrity
(F) Analysis Internal Validity
(G) Blinding
(H) Withdrawl And Dropouts
(I) Other bias

3.1.2 TIME-SERIES STUDIES

Three time-series studies (Espitia-Hardeman 2008; Radin Umar 1995a and 2005) assessed the number of road traffic accidents and fatalities before and after enforcement of traffic laws and regulatory interventions. All studies focused on preventing road traffic injuries among motorcycle riders. The meta-analysis showed that as a result of interventions, the number of road traffic casualties and fatalities declined in the post intervention period, meaning interventions were effective.

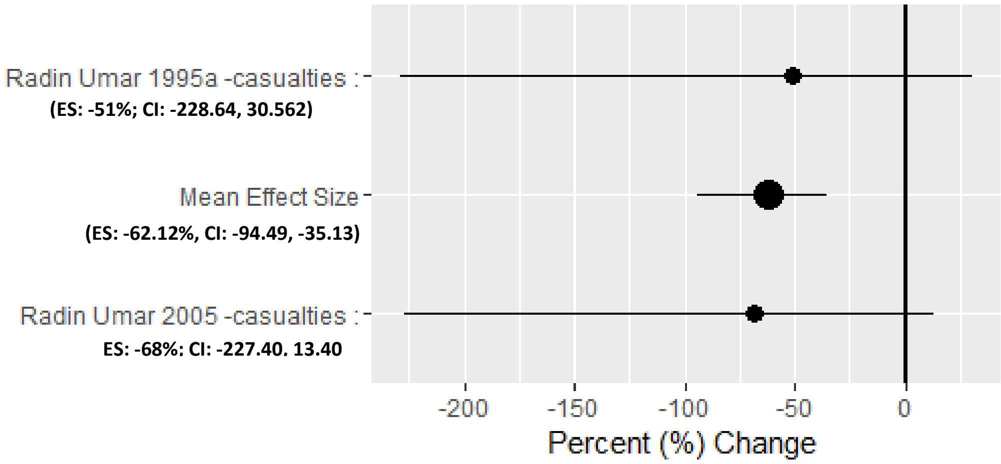
PRIMARY OUTCOMES

Two studies (Radin Umar 1995a and Radin Umar 2005) reported the number of road traffic casualties (Percentage change -51.06; 95% CI -228.641 – 30.562; Percentage change -68.37; 95% CI -227.402 – 13.406), based on data from 3,962 people involved in accidents after the law enforcement intervention. In percent change, road traffic casualties declined between 32% and 49% after intervention enforcement. The mean percent change in road traffic casualties after intervention declined by 38%.

One study (Espitia-Hardeman 2008) reported the number road traffic accidents resulting in fatalities (Percentage change -500.65; 95% CI -1248.434 – 167.555), based on data from 748 people involved in accidents after law enforcement.

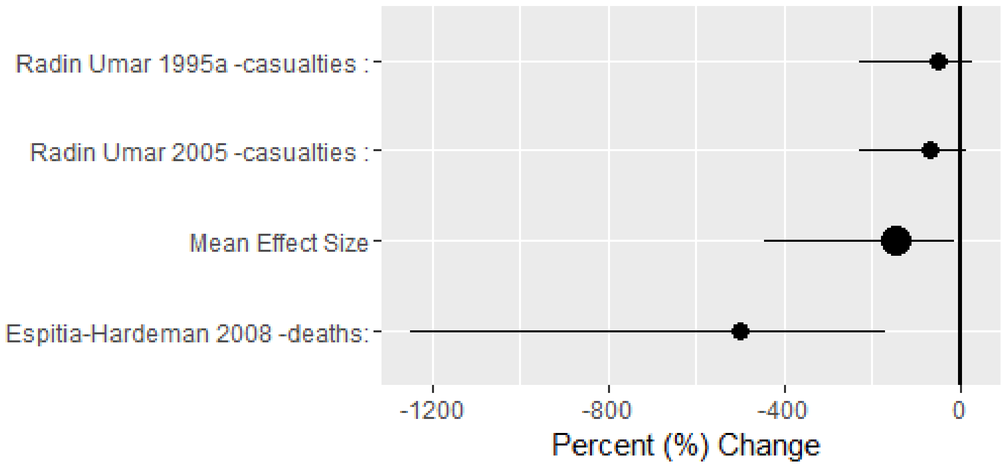
For forest plots, see figure 8 and 9.

Figure 8. Traffic law enforcement and regulatory interventions in time series studies: percent change in road traffic casualties.



Percent change in effect size after intervention

Figure 9. Traffic law enforcement and regulatory interventions in time series studies: percent change in road traffic casualties and fatalities.



Percent change in effect size after intervention

3.1.3 VOTE COUNTING AND COMMON RUBRIC

It may seem that overall effect sizes showed that the impact of mandatory helmet use was favorable; however, the summary effect sizes for fatalities and severe injuries were not statistically significant. The impact of mandatory helmet use on fatalities, severe, and moderate injury outcomes for two studies (Chiu 2000; Panichaphongse 1995) stand out from the majority, both studies individually demonstrated positive impact of intervention. In those two studies, the absolute risk for fatalities among motorcyclists wearing helmets decreased by 0.02% and 0.53%, respectively; severe injuries among motorcyclists wearing helmets were reduced by 0.21% and 1.56%, respectively; and moderate injuries were reduced by 0.40% and 7.64%, respectively.

The compliance for mandatory helmet use among motorcyclists after the intervention period had improved, as shown by the overall positive effect size, although statistically, this measure was not significant across individual studies. The confidence interval for compliance for mandatory helmet use among motorcyclists was found to be wide in all individual studies, indicating low precision for this measure. In absolute terms, in places where mandatory helmet law enforcement was implemented, compliance for helmet use among motorcyclists increased between 9.8% and 48% compared with when there was no mandatory helmet law enforcement.

The impact of traffic law enforcement on red-light violation and speeding outcome measures showed positive and overall effect statistically significant ($p = 0.003$). The absolute risk for compliance with red lights and speed limits ranged from .21% to a significant 32% among those studies that reported this measure. Appendix 10 presents information on the common rubric and vote count.

We were not able to evaluate the vote counting of time-series studies.

3.2 ROAD ENGINEERING INTERVENTIONS

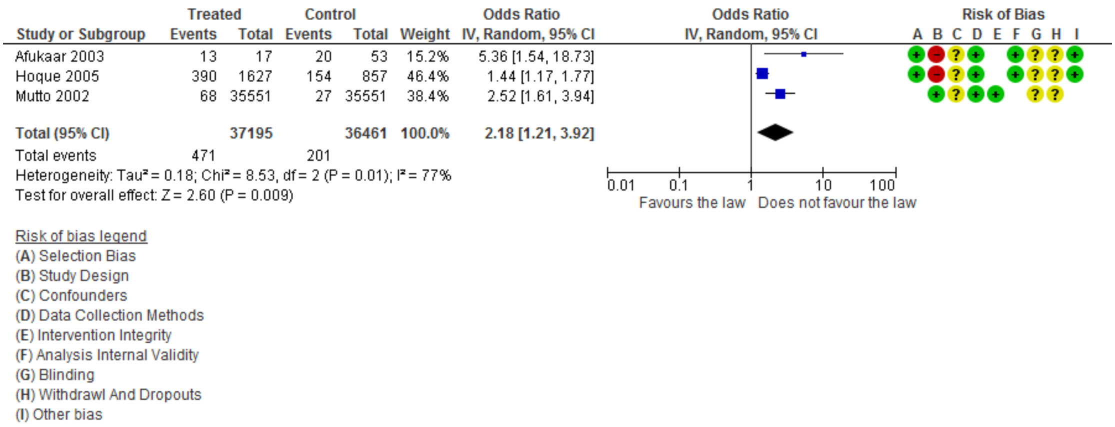
3.2.1 UNCONTROLLED BEFORE-AND-AFTER STUDIES

Three studies (Afukaar 2003; Hoque 2005; Mutto 2002) assessed the effect of road engineering interventions. Contrary to expectations, the interventions resulted in an increase in the road traffic casualty rate of over 200%. However, fatalities declined by 48% due to the impact of interventions.

PRIMARY OUTCOMES

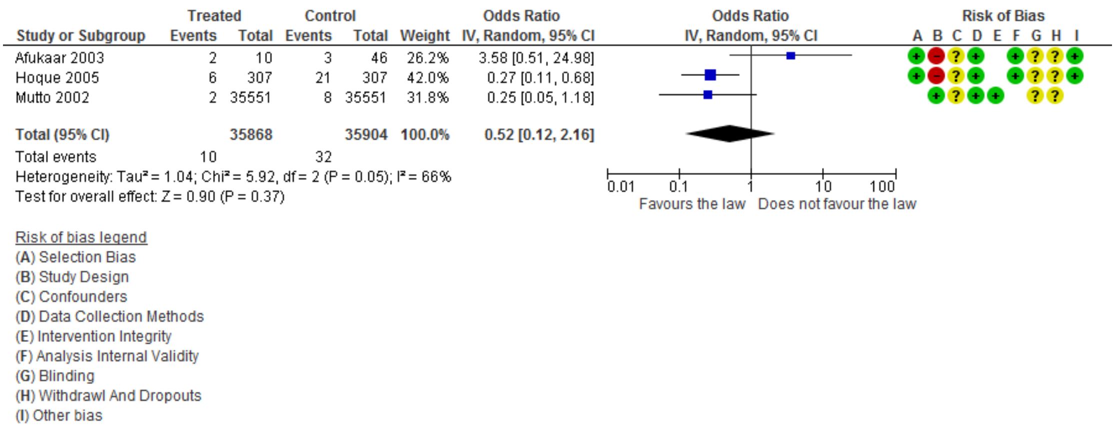
Three studies (Afukaar 2003; Hoque 2005; Mutto 2002) assessed the number of road traffic casualties before and after road engineering treatments. Contrary to expectations, the interventions resulted in over two times increase in the number of casualties (OR, 2.18; 95% CI 1.21 – 3.92; I^2=77%; P=0.009), based on data from 37,195 people after road engineering treatment. The statistical test of heterogeneity was significant among studies ($I^2 = 77\%$; $p < .05$). For forest plot, see figure 10:

Figure 10. Road engineering interventions: road traffic casualties



Three studies (Afukaar 2003; Hoque 2005; Mutto 2002) assessed the number of fatalities before and after road engineering treatments. Due to the effects of road engineering interventions, the number of fatalities decreased (OR 0.52; 95% CI 0.12 – 2.16; I^2=66%; P=0.37), based on data from 35,868 people after road engineering treatment. The odds of occurrence of fatalities was 48% less due to the impact of interventions. However, these studies were not statistically significantly heterogeneous, with a medium value 66% for I^2. For forest plot, see figure 11:

Figure 11. Road engineering intervention – road traffic fatalities.



3.2.2 TIME-SERIES STUDIES

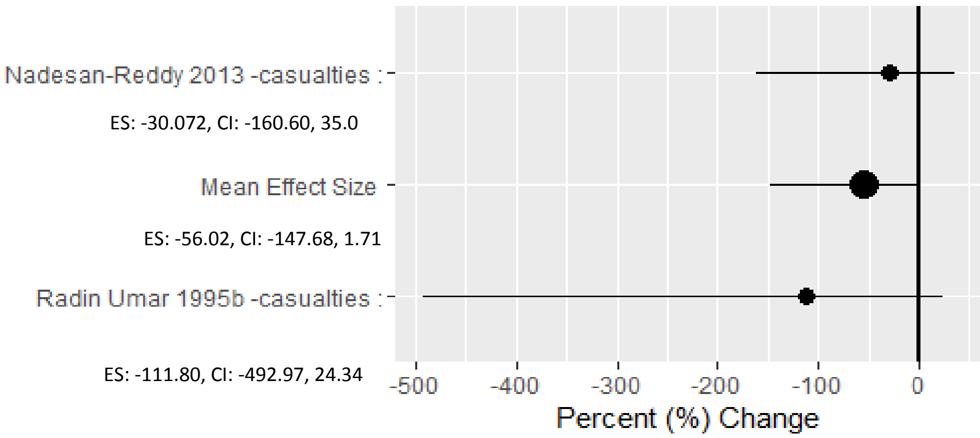
Two time-series studies (Nadesan-Reddy 2013; Radin Umar 1995b) assessed the number of road traffic casualties and fatalities before and after road engineering treatments. The effect of road engineering interventions strongly favoured interventions. In percent change, mean road traffic casualties declined by 44% after road engineering treatment. The evidence of statistical heterogeneity and effect size showed less variation towards road engineering intervention.

PRIMARY OUTCOMES

Two studies (Nadesan-Reddy 2013; Radin Umar 1995b) assessed the number of road traffic casualties (Percentage change -30.072; 95% CI – 160.60 – 35.079; Percentage change -111.800; 95% CI -492.97 – 24.34), based on data from 5,588 people involved in accidents after road engineering treatment. The mean number of road traffic casualties declined by 44% after road engineering intervention.

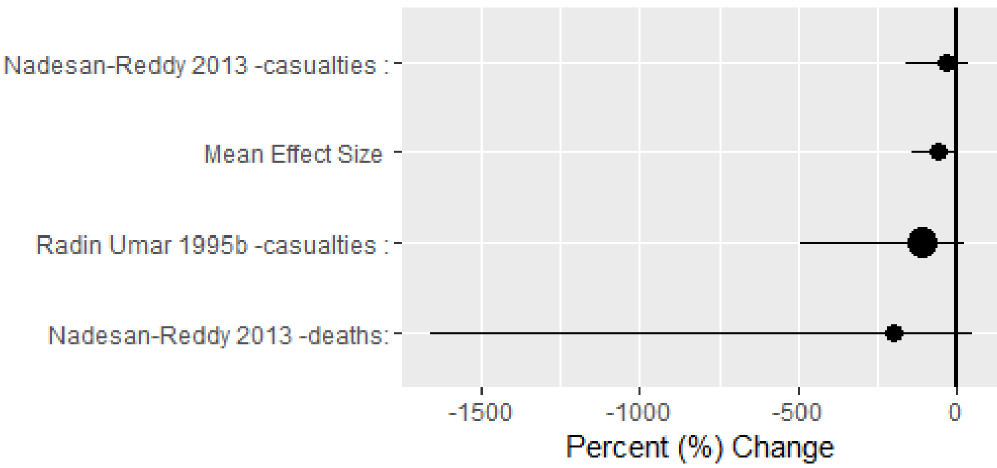
And, one study (Nadesan-Reddy 2013) assessed the number of road traffic fatalities (Percentage change -200.00; 95% CI -1663.123 – 48.954), based on 1,267 people involved in accidents after road engineering treatment. For forest plots, see Figure 12 and 13:

Figure 12. Road engineering interventions in time series studies: percent change in road traffic casualties.



Percent change in effect size after intervention

Figure 13. Road engineering interventions in time series studies: percent change in road traffic casualties and fatalities.



Percent change in effect size after intervention

3.2.3 VOTE COUNTING AND COMMON RUBRIC

The evidence of statistical heterogeneity and effect size showed less variation towards road engineering intervention. Road engineering interventions have shown a positive impact on reducing fatalities. The absolute risk for fatalities in the Hoque 2005 study was reduced by 5% and by 0.02% in the Mutto 2002 study after a road engineering intervention. However, Afukaar 2003 reported an increase in the fatalities by 13.4% as a result of road engineering interventions. Road engineering interventions did not show a positive impact on accidents in three studies (Afukaar 2003; Hoque 2005; Mutto 2002). The overall effect was found statistically significant ($p < .05$). In absolute difference, the percentage of accidents increased from 0.12% to 38.7% after the road engineering intervention. Appendix 10 presents information on the common rubric and vote count.

We were not able to evaluate the vote counting and common rubric of time-series studies.

3.3 EXPLORING RELATIONSHIPS WITHIN AND BETWEEN STUDIES

Two main tools are generally used for exploring relationships within and between studies: sub-group analysis and moderator variables.

3.3.1 SUBGROUP ANALYSIS

Because of insufficient data, the sub-group analysis was not considered appropriate to look at variations by equity indicators such as macroeconomic environment, socioeconomic status, and education.

3.3.2 MODERATOR VARIABLES

We constructed various components of the evaluated interventions table to help investigate whether there were any clear moderators of effect. The moderators of effects table (Appendix 11) shows components that were specific to the intervention implementation in each study and the overlap between different interventions and studies in terms of these components. Two components appeared to have a moderator effect on variations from mean effect sizes: setting/context, and duration of data collection in studies of traffic law enforcement and regulatory interventions.

However, for road engineering interventions, there were not enough studies to evaluate the moderator effect. We were not able to evaluate the moderator effect of time-series studies.

Traffic law enforcement and regulatory interventions

The impact of mandatory helmet use on severe and moderate injuries was reported by two studies: Chiu 2000 and Panichaphongse 1995. Data for both studies were collected over 12-months. The individual effect sizes for severe and moderate injuries as a result of mandatory helmet use showed less variance from mean effect sizes (OR, 0.92 and 0.83, respectively) in both studies.

The odds ratios for compliance for mandatory helmet use among motorcycle riders ranged from 1.49 to 28.56 in the included studies, which were highly heterogeneous. The odds ratio for overall mean effect size was 7.67. Subgroup analysis by duration of data collection showed that, the longer the period of data collection, the greater the impact of the intervention on the outcome. In studies in which data were collected over one month before and after the intervention, the overall OR was 5.71 (that is, the overall compliance was 5.7 times greater than in cases where there was no intervention) (Bhatti 2011, Nguyen 2013). The overall odds ratio was 10.84 if the duration of data collection was 12 months (that is, an increase to almost 11 times the amount of compliance when the intervention was studied over a longer period). The impact of the intervention, however, was observed to lose sustainability after 12 months, as in the study by Ichikawa 2003, which reports the effect after 24 months (OR, 6.58).

For compliance with red lights and speeding, the average effect sizes were 0.19 points away from the mean effect size of the odds ratio of 0.34; all studies consistently followed a 1- to 14-day data-collection period (Allyana 2014; Lipovac 2013; Zhang 2014).

For individual effect sizes of all studies, see the common rubric and vote count table (Appendix 10).

There were a few studies that used distinct methods to implement and measure road infrastructure intervention outcome effects. Because of a lack of sufficient studies, the data from these studies were not included in the meta-analysis. It was decided that writing a short summary of each study would provide an opportunity to describe and explore the study data. These summaries were structured such that they provided details of the setting, population, intervention, methods, and outcomes, along with any other factors of interest.

ANTIC 2013	
INTERVENTION & SETTING	This study demonstrates the influence of speed bumps of different heights (3, 5 and 7 cm) on the motor-vehicle speed at three locations in Belgrade, Serbia. These locations were selected as a result of residents' requests to install speed bumps because of the increased presence of pedestrians and schoolchildren.
METHODS	Motor-vehicle speeds were compared at three locations in Belgrade, before and after speed-bump installation. Speed measurements were taken before speed bumps were installed, and one day and one month after installation.
RESULTS	Speed bumps 5 cm and 7 cm high significantly contributed to the safety of vulnerable road users, especially safety of pedestrians. At Location B, after the installation of speed bumps, the 85th percentile speed was 40 km/h. At Location C, the 85th percentile of speed was 35 km/h after the installation of a 7-cm-high speed bump. However, at location A, the 85th percentile of speed was approximately 50 km/h after the installation of a 3-cm-high speed bump.
CONTROL FOR CONFOUNDING VARIABLES	The three locations had similar geometric and functional characteristics.
CONCLUSION	The study found that there was a significant speed decrease where speed bumps were set, compared to the period before they were installed.

LIU 2011	
INTERVENTION & SETTING	This study evaluated the impacts of transverse rumble strips in terms of reducing crashes and vehicle speeds at pedestrian crosswalks on rural roads in China. The study also looked at the influence area of transverse rumble strips on rural roads. Raised rumble strips were deployed on both approaches to municipal pedestrian crosswalks on sections of rural roads in China.
METHOD	This was an observational CBA study using a comparison site. Control sites were similar road segments located on the same highway as the treatment group, where the transverse rumble strips had never been deployed. Crash data were collected at 366 road segments on four neighbouring rural highways of Guangdong Province. Speed data were measured at 12 sites in Jiangsu and Guangdong Provinces in China. At each treated site, point-speed data were measured at 23 selected locations, with different distances from pedestrian crosswalks.
RESULTS	The 85th-percentile speed declined 9.1 km/h on roads with a speed limit of 60 km/h; and 12.0 km/h on roads with a speed limit of 80 km/h. However, the speed-reduction impacts were not found to be statistically significant for the pedestrian crosswalk on the road with a speed limit of 40 km/h. Speed profiles developed in this study show that the influence area of transverse rumble strips is generally less than 0.3 km. The crash-data analysis showed an effectiveness of 0.75 as a result of the implementation of transverse rumble strips.
CONTROL FOR CONFOUNDING VARIABLES	In this study, confounding factors were controlled for by using a comparison group that shared some properties with the treated pedestrian crosswalks. The regression to mean was addressed using the Empirical Bayes before-and-after study method.
CONCLUSION:	The speed-data-analysis results showed that transverse rumble strips significantly reduced vehicle speeds in the vicinity of pedestrian crosswalks on rural roads with posted speed limits of 60 km/h and 80 km/h. The use of transverse rumble strips, on average, reduced the number of crashes by 25%.
QUISTBERG 2014	
INTERVENTION & SETTING	This study examined the relationship between pedestrian-motor vehicle collisions and the presence of visible traffic signals, pedestrian signals, and signal timing, to determine whether these countermeasures improved pedestrian safety. The setting was an urban community in the Municipality of Lima, Peru.
METHOD	A matched case-control design was used where the units of study were crossing locations. Cases were pedestrian crossings at road intersections where the Policía Nacional del Perú (National Police of Peru) reported that a pedestrian collision had occurred between 1 October 2010 and 15 January 2011. Controls were pedestrian crossings in the proximity of case sites that matched the case site's road classification and number of lanes. Each case-control pair was matched for proximity, street classification and number of lanes.
RESULTS	Collisions were more common where a phased pedestrian signal (green- or red-light signal) was present compared with no signalisation (OR, 8.88; 95% CI, 1.32–59.6). A longer pedestrian-specific signal duration was associated with collision risk (OR, 5.31; 95% CI, 1.02–9.60 per 15-second interval). Collisions occurred more commonly in the presence of any signalisation visible to pedestrians or pedestrian-specific signalisation, although these associations were not statistically significant. Although signalisation did not demonstrate an association with safety, the presence of transit police to regulate traffic appeared to be strongly associated with lower risk for a collision compared with sites with no regulation (OR, 0.05; 95% CI, 0.004–0.60).
CONCLUSION	The study found that the signalization efforts were not associated with lower risk for pedestrians; rather, they were associated with an increased risk for pedestrian–vehicle collisions.

3.3.3 REFLECTING CRITICALLY ON THE SYNTHESIS PROCESS

This review included four non-randomized study designs. As a result of methodological heterogeneity in study designs, risk of bias in included studies ranged between moderate and low quality. All included studies had adequate information with regard to intervention to individually meet the study inclusion criteria. However, the meta-analysis results presented extreme heterogeneity. Some of the heterogeneity was due to variations in the study population (socio-demographic, cultural, and spatial). Nevertheless, studies pooled effect sizes showed inconsistency and impreciseness which downsized the study quality of included studies range from moderate to very low quality. For details, see Summary of findings table (Appendix 12).

Secondly, there were some limitations to the approach taken in the selection of moderator variables. The selection of intervention components included in the moderator table was limited, to some extent, to those components for which data were available, as opposed to choosing moderators identified through extensive examination of included studies. This, perhaps, precluded the use of some of the techniques used in the guidance.

As a result, the trustworthiness of the narrative synthesis of included studies is affected. We are uncertain about the estimates, further research is very likely to change the estimates.

3.3.4 SENSITIVITY ANALYSIS

We found extreme variation in the study design used, size of study population (socio-demographic, cultural, and spatial), intervention implementation and follow-up period, and the range of morbidity outcome scale. Hence, we considered a sensitivity analysis about the influence of small study effects on the result of meta-analysis. We compared the fixed and the random effect estimates of the intervention effect. For studies that had used different choices of outcome measures using the Injury Severity Score or Glasgow Coma Scale, we compared the overall morbidity effect estimates with effect estimates of moderate and severe injuries. We did not include studies of poor quality in the meta-analysis or narrative synthesis.

3.3.5 INVESTIGATION OF EQUITY AND DIFFERENTIAL IMPACTS ACROSS GEOGRAPHIC SUBGROUPS

The included studies in this review focused on LMIC. Unfortunately, we were not able to assess the impact of interventions across different socio-economic groups because of a lack of sufficient studies having data for specific population subgroups, such as sex, socio-economic status, education status or religion.

4.0 DISCUSSION

4.1 SUMMARY OF MAIN RESULTS

The effect of traffic law enforcement and regulatory interventions generally had favourable outcomes after the intervention. Fatalities and injuries declined. Enforcement of mandatory helmet-law strongly favoured compliance among motorcyclists compared with when there was no helmet law enforcement. Red-light and speeding violations were reduced due to the impact of an automated enforcement system. Road engineering interventions had mixed results for road traffic casualties. Despite methodological differences between studies, road engineering interventions reduced fatalities. Risk of biases in included studies ranged between moderate and low quality. Studies pooled effect sizes presented statistical heterogeneity which downsized the study quality of included studies range from moderate to very low quality.

4.1.1 TRAFFIC LAW ENFORCEMENT AND REGULATORY INTERVENTIONS

Ten uncontrolled before-and-after studies assessed the number of outcome measures before and after enforcement of traffic laws and regulatory interventions. In general, effect sizes showed favourable outcomes after interventions; fatalities declined by 6% and injuries by 26%. The effect of intervention was strongly in favour of the implementation of secondary outcome measures after intervention. In places where a mandatory helmet-law was enforced, there was an eightfold greater compliance among motorcyclists compared with when there was no helmet-law enforcement. Red-light and speeding violations were reduced by 66% due to the impact of an automated enforcement system (that is, cameras).

Three time-series studies assessed the number of road traffic casualties and fatalities before and after enforcement of traffic laws and regulatory interventions. All studies focused on preventing road traffic injuries among motorcycle riders. The meta-analysis showed that, as a result of interventions, the number of road traffic casualties and fatalities declined in the post-intervention period, meaning interventions were effective. The mean percent change in casualties was a 38% decline.

4.1.2 ROAD ENGINEERING INTERVENTIONS

Three uncontrolled before-and-after studies assessed the effect of road engineering interventions on the number of road traffic casualties and fatalities. Contrary to expectations, the interventions resulted in an increase of over 200% in the number of casualties, however fatalities declined by 48% due to the impact of interventions.

Two time-series studies assessed the number of road traffic casualties and fatalities before and after road engineering treatments. The effect of road engineering interventions strongly favoured outcomes after interventions. In terms of percentage change, mean road traffic casualties relative to post-intervention results declined by 44%.

Three studies used distinct methods to measure traffic law enforcement and regulatory and road engineering interventions outcome effects. Overall, studies showed a positive impact of interventions such as speed bumps/humps and transverse rumble strips. In one study, the use of transverse rumble strips could reduce the incidence of crashes by 25%, on average. Another study showed that the use of speed bumps of 5 cm and 7 cm high significantly contributed to the safety of vulnerable road users, especially the safety of pedestrians; mean vehicle speed was reduced by 79%. However, one study found that the signalisation efforts were associated with an increased risk of pedestrian-vehicle collisions.

One study reported pre-specified adverse outcome increase in collision between motor vehicles as a result of traffic calming measure speed humps. The total number of motor vehicle collisions increased by 9% as a result of speed humps installation.

This review included four non-randomized study designs. As a result of methodological heterogeneity in study designs, risk of bias in included studies ranged between moderate and low quality. Consequently, the meta-analysis results presented extreme heterogeneity. We expected variations in between studies due to various reasons, for example study population (socio-demographic, cultural, and spatial), study sample size, data collection duration, road environment, and the perceived road risk of the country. Nevertheless, studies pooled effect sizes showed inconsistency and impreciseness which downsized the study quality of included studies range from moderate to very low quality.

4.2 OVERALL COMPLETENESS AND APPLICABILITY OF EVIDENCE

The studies in this review focused on LMIC. Knowledge of impacts on low-income populations is important in respect of socio-economic status, sex, educational status and religion. However, assessments of data on variations in impact across different socio-economic groups, sex, educational status and religion were not available to quantify the results. Hence, the evidence presented in this review is too incomplete to provide any sufficient suggestions on whether an intervention is likely to impact the gap in health status between LMIC.

4.3 QUALITY OF THE EVIDENCE

Included studies presented methodological weaknesses. To a large extent, traffic law enforcement and regulations, and road engineering interventions in included studies were implemented at population level. The nature of interventions does not allow for randomisation of control sites, at least not for road engineering treatments. Neither the control sites nor the treatment sites were selected randomly. The control sites were predominantly selected by researchers based on the condition that an intervention had never been deployed at the control site. Nevertheless, researchers attempted to match treatment and comparison sites for trends, exposure and geographic characteristics of roads/locations.

One of the biases of this review was that we relied on observational data. A variety of different, non-randomised study designs were included, provided the study compared changes in outcomes before and after the intervention implementation, with or without a control group. Uncontrolled before-and-after studies were the predominant type identified. To be statistically sound, a before-and-after study design must control for environmental changes from before to after the road safety measure is introduced, such as general changes, changes in traffic volume, and the problem of regression-to-the-mean biases (Elvik 2002). In almost all studies, the post-intervention period started immediately after the deployment of the intervention, and all studies included just one period before and one period after; therefore, general changes and change of traffic volume confounders were possibly controlled. In one study, regression-to-the-mean was addressed using the Empirical Bayes before-and-after study method.

Most importantly, the most credible study design (time-series) and the least credible study design (uncontrolled before and after) in included studies produced roughly similar outcome results, but time-series studies produced stronger results for traffic law enforcement and regulatory interventions. For road engineering interventions, these study designs produced opposing results for road traffic casualties. In uncontrolled before-and-after studies, the number of road traffic casualties increased after the intervention period and, in time-series accidents, they declined. We would have expected the lower-quality studies to produce positive and over-estimated accident effects. This demonstrated, perhaps, that the findings reflect the effect of an intervention, rather than methodological biases across studies.

We found the quality of evidence rank from moderate or low primarily due to risk of bias and imprecise results because of limited outcome data on moderate and severe injuries. Since the events of road traffic injuries are rising in low and middle-income countries and that a high presence of heterogeneity across studies was found, additional research is required to determine the effects of road engineering interventions. Blinding, and withdrawal and dropouts of participants is not applicable in transport studies, thus leaving a moderate chances of performance bias and detection bias. The evidence could be therefore assessed as low quality.

5.0 AUTHORS' CONCLUSIONS

5.1 IMPLICATIONS FOR POLICY

Overall, the interventions being implemented by LMIC may represent the initial stages of positive development. Perhaps increased sensitisation of local and civic organisations, establishment of road safety programmes, and improved data collection methods have had a positive impact on building safe road environments in LMIC. Nevertheless, the number of road traffic deaths and injuries among vulnerable road users are increasing dramatically in LMIC as a result of increased motorisation.

The evidence that interventions for preventing road traffic injuries and fatalities among motorcycle riders are effective in LMIC has been well established. The enforcement of helmet acts have had an immediate impact in terms of increasing helmet use among motorcyclists. This could be because there are heavier penalties for non-compliance. As a consequence of the helmet law enforcement, there is increased police surveillance. There is a fear of being caught by the police and of being penalized which leads to a higher proportion of use of protective devices. It is also likely that motorcyclists have become more aware of traffic safety after enforcement of helmet act. Therefore, for significant effects, stringent enforcement of traffic laws and regulatory interventions for motorcycle riders, such as, increased fines, not allowing vehicles on roads without helmets, practical education for motorcyclists are required.

The use of automated enforcement systems and signalisation at pedestrian crossings yielded 66% fewer occurrences of red-light and speeding violations among all road users. The results indicate that automated enforcement systems have been successful in changing drivers' behaviour, not only in HIC, but also in LMIC. For significant impact, implementation of advanced automated intervention systems is needed. Some examples include sensory traffic lights or smart lights (intelligent traffic lights) that would stop a road user from crossing the road on a red light or would cause a motor vehicle to slow when the traffic light is about to or has turned red; installation of innovative technologies in the manufacturing of motor vehicles, such as intelligent speed adaptation; and speed-camera corridors.

Due to the impact of road engineering interventions, fatalities were reduced by 48%. Three studies using different methodologies also showed a positive impact for interventions such as speed bumps/humps and transverse rumble strips. In one study, speed bumps reduced pedestrian–vehicle accidents by 74%; in another study, the use of transverse rumble strips reduced the incidence of crashes by 25%, on average. Another study showed that the use of speed bumps 5 cm and 7 cm high significantly contributed to the safety of vulnerable road users, especially the safety of pedestrians; mean vehicle speed was reduced by 79%. Installation of speed bumps/humps and rumble strips at pedestrian crossings and school zones is, therefore, strongly recommended.

5.2 IMPLICATIONS FOR RESEARCH

Included studies in this review represented methodological differences. High-quality study designs are required, which use control sites or time series, and one-year-long observational periods, at the start of the intervention and after the implementation of the interventions, are required. Most importantly, the effectiveness of road engineering interventions, in particular interventions related to segregation of vulnerable road users from motorised vehicles, changes at intersections/junctions, bicycle infrastructure, need to be addressed in a controlled trial or a time-series environment. It was astonishing to find that, in all the LMIC, to our knowledge, not a single quasi-experimental study focused on the impact of interventions for preventing road traffic injuries among bicycle riders. This was particularly surprising because, in several LMIC, in the South Asia, East Asia and Pacific regions, especially, bicycles and rickshaws are the main mode of transport for daily-wage earners. With the increasing global awareness of climate change in relation to the rise in greenhouse-gas emissions from motorised vehicles, the use of bicycles as a way to reach public transport such as metros, and bus and light-rail transit systems, is being encouraged in LMIC. Because of the prevalence of mixed travel conditions in LMIC, the interventions, as well as future research into the effect of interventions, for preventing road traffic injuries and fatalities among bicycle riders, are needed. Finally, studies with fatality and injury outcome measures were limited across traffic law enforcement and regulations and road engineering interventions. Perhaps there are limited data on injuries and fatalities primarily due to inadequate injury surveillance systems in LMIC. Improved injury surveillance systems for data collection, particularly data-driven databases linking police records and hospital records on road traffic injuries, are required

5.3 CONCLUSION

The effect of traffic law enforcement and regulatory interventions generally showed favourable outcomes after interventions. The effect of road engineering interventions showed mixed results for road traffic casualties. Despite methodological differences between studies, road engineering interventions reduced fatalities. For significant effects, stringent enforcement of traffic laws and regulatory interventions, and intelligent transport system interventions are recommended. Because of the prevalence of mixed travel conditions in LMIC, the interventions, as well as future research into the effect of interventions for preventing road traffic injuries and fatalities among bicycle riders, are required. Improved injury surveillance systems for data collection on road traffic injuries are also needed.

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7.0 APPENDICES

APPENDIX 1 SEARCH STRATEGY

***MESH TERMS:** (MM "Accidents, Traffic/LJ/MO/PC/SN/PC&quote ;) OR & quote; road traffic injuries & quote;*

P - Population terms:

ITRD KEY TERMS: developing countries, pedestrian, cyclist, motorcyclist

TRT KEY TERMS: vulnerable road user

Note: Could consider listing targeted countries using the World Bank list of LMIC

1. (vulnerable* or non-motorized* (road user*)) or vulnerable* road user*;
2. ("transitioning econom*" or "emerging countr*" or "develop* count*")
3. (pedest* or motor-cycl* or motorcycle* or motorbike* or scooter* or moped* or motocycl* or bicycl* or bike* or powered two wheeler* or rickshaw* or three-wheele* or non-motorized)

4. or/1-3

I – Interventions:

Powered two or three-wheel road-user protection and visibility terms

TRT KEY TERMS: conspicuity

ITRD KEY TREMS: visibility

5. (conspic* or visib* or illum* or protect*)
6. (visual or perception)
7. (headlight* or light* or daytime running*)
8. (colo?r* or contrast* or reflect* or retro-reflect* or retro reflect* or fluoresce*)
9. (helmet* or head protective device*)
10. (cloth* or protective clothing*)
11. (wheel* and reflect*)
12. or/5-11
13. (motor-cycl* or motorcycle* or motor-bik* or motorbike* or scooter* or moped* or motocycl* or powered two wheeler* or rickshaw* or three-wheele*))

14. 12 and 13

Road- engineering terms

ITRD KEY TERMS: geometric design, road safety

TRT KEY TERMS: highway design, highway safety

15. road safety or road design or geometric design or traffic engineering

Segregation

16. bicycle lane*or bicycle facilit* or bikeway
17. bicycle or cyclis* or cyclin* (boulevards or track* or path* or route*)
18. intersectional and ("bicycle box*" or "advanced cycle stop*" or "advanced stop lines)
19. pedestrian facilit* or pedestrian safety
20. (pedestrian (walkway or path* or foot* or sidewalk or pavement or platform overpass or underpass))
21. "pedestrian crossing" or "raised pedestrian crossing" or "zebra crossing" or Cross$walks or "school crossing"
"traffic control" or "traffic signal" or "pedestrian signal")
22. pedestrian ("refuge island" or "traffic island" or "splitter island" or median* or raised or "grade separation")
23. pedestrian ("actuated controllers" or signal* or timing or countdown or phase)
24. pedestrian ("safety fence" or "safety barrier")
25. motorcycl* or moped* ("traffic lane" or shoulder or "safety fence" or "grade separation" or "roadside hazards")
26. motorcycle* or "moped* lane*"

Intersection design

27. roundabout* or intersection or junction
28. "turning traffic" or "right turn" or "left turn"
29. kerb or curb radius

Reduction of vehicle speed
ITRD KEY TERMS: speed control, signalization,
TRT Key TERMS: traffic control devices, speed control, signalization, road markings
30. speed (control* or reduction or limits or countermeasure or enforcement or policing or police)
31. traffic (calming or restraint or "local area traffic management")
32. speed (cushion* or table* or hump* or bump* or "raised tables" or platform* or plateau or chicane or deflection or "raised stop lines")
33. speed ("road narrowing" or "lane width" or gateway or divertor)
34. speed (advisory sign* or warning sign* or stop sign or signal* or "signal timing" or "traffic control device*")
35. "speed camera" or "red light camera"
36. delineation or "pavement mark*" or "line mark*" or "rumble strips" or reflector
37. (cycling AND (school* or work or workplace or commut* or travel* or walk)) and safety
38. bike AND (school* or work or workplace or commut* or travel* or walk)) and safety
39. bicycle AND (school* or work or workplace or commut* or travel* or walk)) and safety
40. pedestrians AND (school* or work or workplace or commut* or travel* or walk)) and safety
41. (motor-cycl* or motorcycle* or motorbike* or scooter* or moped* or motocycl*) AND (school* or work or workplace or commut* or travel* or walk)) and safety
42. or/15-36
43. 37 OR 38 OR 39 OR 40 OR 41
44. 42 AND 43
S - Study type:
45. Controlled AND (trial or trials or study or studies or experiment)
46. Randomized controlled trials.
47. (Non-randomi* AND (trial or trials or study or studies or experiment)
48. Uncontrolled (random allocation / or clinical trial/ or single-blind method/ or double-blind method/ or control groups/)
49. Retrospective study/
50. Evaluation studies/
51. Prospective study/
52. Comparative studies/
53. Cross-sectional study/
54. Case-control study/
55. Intervention studies/
56. quasi-experiment*
57. ((pre test or pretest or (posttest or post test))
58. trial.ti.
59. (time adj series)
60. ((evaluat* or intervention or interventional) adj8 (control or controlled or study or program* or comparison or "before and after" or comparative))
61. ((intervention or interventional) adj8 (effect* or evaluat* or outcome*))
62. (controlled before or 'before and after stud$" or follow up assessment)
63. interrupted time series/
64. intervention adj1 group$
65. or/45-64
O- Outcome
66. accidents or crash
67. bycycl* or motorcycl* or pedestrian or cyclist
68. (bike or bicycl*)
69. (motor-cycl* or motorcycl* or motor-bik* or motorbike* or scooter* or moped* or motorcycl*)
70. (trauma* or injur* or fatal* or accident* or crash* or prevent* or collide* or collision*)
71. or/66-70
72. 4 and (14 or 44)
73. limit yr="1990-current"

APPENDIX 2 SEARCHES OF GREY LITERATURE

Database	Date	Strategy	Hits	Abstract view
ADB (ASIAN DEVELOPMENT BANK)	5 Dec. 2014	Hand-searched papers, briefs, conference proceedings, reports, evaluation studies		4
AFCAP (THE AFRICAN COMMUNITY ACCESS PROGRAM)	8 Dec. 2014	Hand-searched publications by country		4
AfDB (AFRICAN DEVELOPMENT BANK)	8 Dec. 2014	Publications: working papers, evaluation reports, policy reports, project-appraisal reports		10
AMEND	9 Dec. 2014	Hand-searched AMEND website		5
ARRB (AUSTRALIAN ROAD RESEARCH BOARD)	17 Dec. 2014	Hand-searched ARRB reports, ARRB *Road and Transport Research Journal*, ARRB Knowledge Base - ARRB conference and journals, ARRB *Road Research Board Journal*		7
CRASH MODIFICATION FACTORS CLEARING HOUSE	9 Dec. 2014	Hand-searched publications, international resources, resources for countermeasures selection, resources for behavioural countermeasures	25	2
CRRI (CENTRAL ROAD RESEARCH INSTITUTE)	4 Dec. 2014	Hand-searched research papers published in seminars and conference proceedings, research papers published in journals	119	0
COCHRANE INJURY SPECIALIZED REGISTER	29 Dec. 2014	Hand-searched publications	200	18
DFID (DEVELOPMENT FUND FOR INTERNATIONAL DEVELOPMENT)	6 Jan. 2015	Hand-searched R4D – transport, disability, non-communicable diseases, health miscellaneous -public health, systematic reviews, education, research	101 9	18
EMBARQ	9 Dec. 2014	Hand-searched publications	14	3
GLOBAL TRANSPORT KNOWLEDGE CENTER	13 Jan. 2015	Hand searched Knowledge Centre - road safety	350	3
GRSF (GLOBAL ROAD SAFETY FACILITY)	5 Jan. 2015	Hand-searched research and analysis		
GRSP (GLOBAL ROAD SAFETY PARTNERSHIP)	9 Jan. 2015	Hand-searched		
iRAP (INTERNATIONAL ROAD ASSESSMENT PROGRAM)	16 Dec. 2014	Hand-searched publications - research and technical papers		
IRF (INTERNATIONAL ROAD FEDERATION)	12 Dec. 2014	Hand-searched publications		28
REAAA (ROAD ENGINEERING ASSOCATION OF ASIA	14 Jan. 2015	Hand-searched knowledge hub - research and publications, REAAA journals, technical reports		

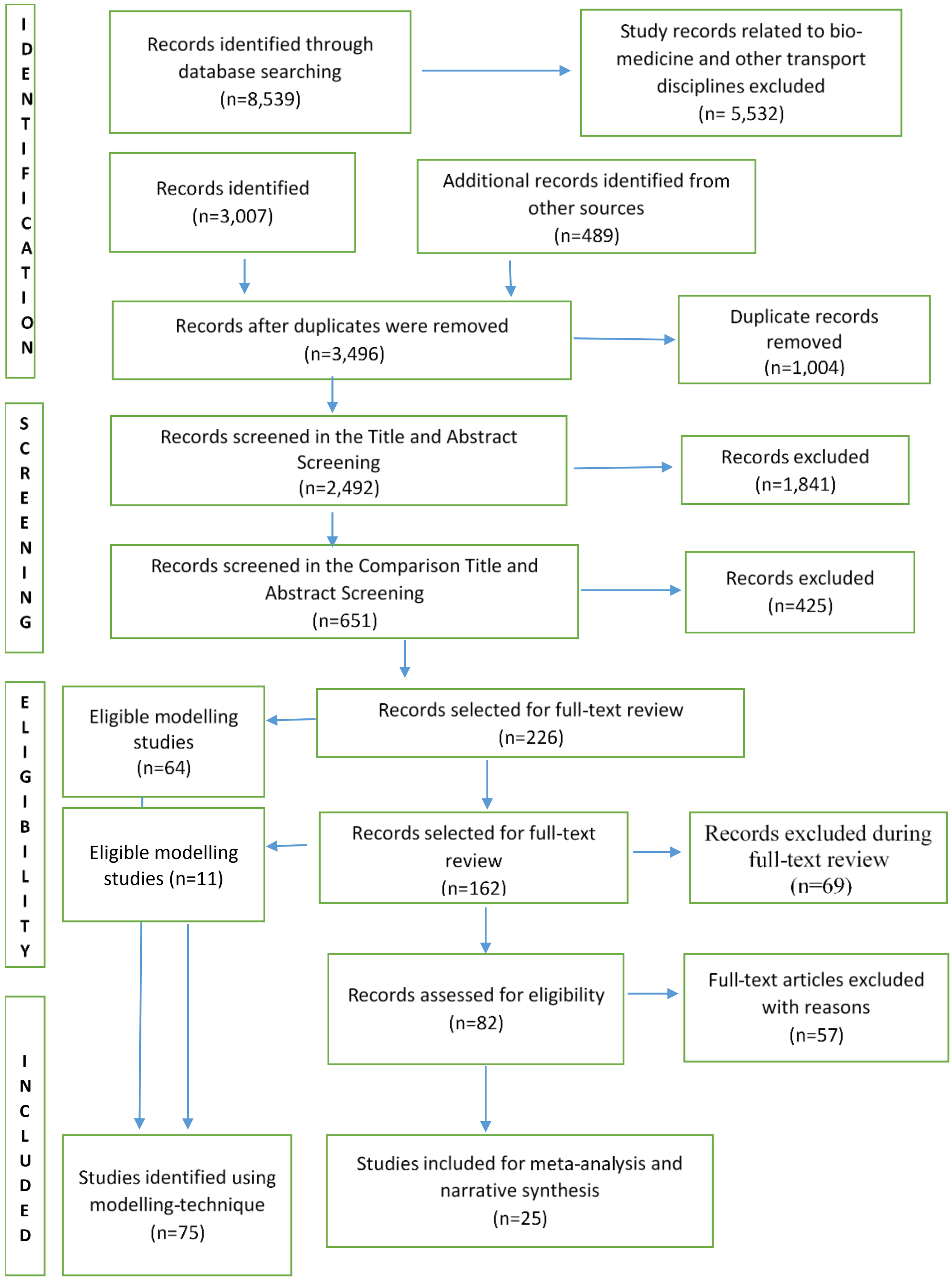
AND AUSTRALIA)				
RTIRN (ROAD TRAFFIC INJURY RESEARCH PREVENTION)	10 Dec. 2014	Hand-searched publications of members, technical reports and tools	48	21
SSATP (SUB-SAHARA AFRICA TRANSPORT POLICY PROGRAM)	10 Dec. 2014	Hand-searched publications, SSATP working papers, technical papers, discussion papers		8
TRIPP (TRANSPORT RESEARCH AND INJURY PREVENTION PROGRAM)	12 Dec. 2014	Hand-searched publications - traffic safety and injury control	66	1
TRL (TRANSPORT RESEARCH LABORATORY)	12 Dec. 2014	Hand-searched reports and publications, transport for development publications		26
MALAYSIAN INSTITUTE OF ROAD SAFETY RESEARCH	16 Dec. 2014	Hand-searched reports and publications		4
MONASH UNIVERSITY ACCIDENT RESEARCH CENTER	1 Dec. 2014	Hand-searched topics: bicycles, motorcycles, pedestrians, road users, road-safety enforcement, speed, traffic engineering and road environment, non-series reports and contract reports, peer-review publications of last 3 years, Australasian road-safety research policing and education conference, and, bibliographies or relevant papers	24	7
THE INSTITUTE OF TRANSPORT STUDIES, UNIVERSITY OF CALIFORNIA	2 Dec. 2014	Hand-searched University of California Safe Transportation Research and Education Centre, Pavement Research Centre, Transportation Centre	0	0
UNIVERSITY OF BIRMINGHAM, BIRMINGHAM CENTRE FOR RAILWAY RESEARCH AND EDUCATION	2 Dec. 2014	Hand-searched publications	1	0
UNIVERSITY OF DURHAM, DURHAM RESEARCH ONLINE-WOLFSON RESEARCH INSTITUTE	2 Dec. 2014	Hand-searched Durham Research Online -Wolfson Research Institute, Public Policy and Health, Durham Energy Institute		0
TRANweb NORTHWESTERN UNIVERSITY	28 Jan. 2015	Searched TRANweb database for traffic and road safety	62	7
AUSTRALASIAN ROAD SAFETY RESEARCH, POLICING AND EDUCATION (ARSRPE) DATABASE, AUSTRALIAN COLLEGE OF ROAD SAFETY	14 Jan. 2014	Hand-searched	10	2
AUSTRALIAN COLLEGE OF ROAD	14 Jan. 2014	Hand-searched		0

SAFETY CONFERENCE AND JOURNAL PAPERS				
DEVELOPMENT IMPACT EVALUATION INITITATIVE (DIME)	3 Dec. 2014	Hand-searched working-paper series, DIME impact evaluation, data catalogue	208	
WORLD BANK JOLIS LIBRARY CATALOGUE	5 Jan. 2015	Hand-searched Open Knowledge Repository -transport and environment, transportation, transport economics, policy and planning	110 0	6
LATIN AMERICAN AND CARIBBEAN CENTER ON HEALTH SCIENCES INFORMATION (LILACS)	5 Jan. 2015	Searched LILACS database	63	10
OECD (ORGANIZATION OF ECONOMIC COOPERATION AND DEVELOPMENT)	4 Dec. 2014	Hand-searched working papers, international transport forum discussion papers, International Transport Forum roundtables, Commonwealth library		2

APPENDIX 3 SOURCES OF CONFERENCE PROCEEDINGS

16th International Conference Road Safety on Four Continents
26th ARRB Conference 2014
2014 Australasian Road Safety Research, Policing and Education Conference Papers
2014 RoSPA Road Safety Conference
2014 International Cycling Safety Conference
14th International Symposium of Safe Science and Technology
Better Safety Data for Better Road Safety Outcomes, IRTAD/OISEVI Conference
ARRB Low Volume Road Symposium 2013
13th Conference of the Road Engineering Association of Asia and Australasia
VTI - Road Safety on Five Continents (RS5C) 2013
25th ARRB Conference 2012
Safety 2012 World Conference
4th IRTAD Conference, Seoul Korea
24th ARRB Conference 2010
8th International Forum of Automotive Traffic Safety 2010
Meeting Transportation's 21st Century Challenges. Compendium of Technical Papers, ITE 2010
23rd ARRB Conference 2008
Safe Roads 2008
African Road Safety Conference, Accra, Ghana 2007
13th International Conference on Safe Communities: Czech Communities 2007
14th International Conference Road Safety on Four Continents, Bangkok, Thailand, 2007
Proceedings of World Congress on Engineering 2007
22nd ARRB Conference 2006
13th International Conference on Road Safety in Four Continents, Warsaw, Poland, 2005
21st ARRB Conference 2003
20th ARRB Conference 2001
Road Safety on Three Continents International Conference, Moscow, Russia, 2001
11th International Conference on Road Safety in Three Continents in Pretoria, South Africa, 2000
Proceedings of the 'On Safe Roads Into the 21st Century' Congress, 24–26 October 2000, Budapest, Hungary
Proceedings: Conference on Transportation in Developing Countries, 17–18 April 1998, University of California
Reducing Childhood Pedestrian Injuries: Proceedings of a Multidisciplinary Conference, 27–28 September 1998, Atlanta, Georgia
New Achievements in Road Safety Research. FERSI Workshop for Young Researchers. Proceedings. Prague, Czech Republic, 8–9 October 1998
41st Annual Proceedings of the Association for the Advancement of Automotive Medicine, 10–11 November 1997, Orlando, Florida,
Canadian Multidisciplinary Road Safety Conference VIII: 14–16 June 1993
Safety Evaluations of Traffic Systems: Traffic Conflicts and Other Measures. Proceedings. 6th ICTCT workshop, October 1993, Salzburg, Austria
Transport and Communications Bulletin for Asia and the Pacific – no. 74: Road Safety
17th IRF World Meeting and Exhibition

APPENDIX 4 SEARCH FLOW CHART



APPENDIX 5 STUDIES USING MODELLING TECHNIQUES

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APPENDIX 6 SHORT DESCRIPTION OF STUDIES

Short Title	Country	Study Design	Intervention	Quality Assessment
1) Afukaar 2003	Ghana	**Uncontrolled before and after** **Sample size**: Not available	**Road engineering intervention** Rumble strips at Suhum Junction	Moderate
2) Allyana 2014	Malaysia	**Uncontrolled before and after** Speed Study **Sample size**: 31,580 vehicles observed running red lights **Sample size:** 331,154 vehicles observed	**Enforcement of traffic laws and regulations** Automated enforcement cameras to capture red-light running and speed	Moderate
3) Antic 2013	Serbia	**Uncontrolled before and after**. **Sample size:** speeds of 5182 vehicles were observed	**Road engineering intervention** Speed bumps of different heights (3, 5, and 7 cm).	Moderate
4) Bastos 2005	Brazil	**Uncontrolled before and after** **Sample size for helmet use**: 6298 motorcyclists	**Enforcement of traffic laws and regulations** Mandatory helmet use	Moderate
5) Bhatti 2011	Pakistan	**Uncontrolled before and after** **Sample size for helmet wearing**: 742 motorcyclists and 295 pillion riders	**Enforcement of traffic laws and regulations** Mandatory helmet use	Moderate
6) Chiu 2000	Taiwan	**Uncontrolled before and after** **Sample size:** 8795 cases of motorcycle-related head injuries	**Enforcement of traffic laws and regulations** Mandatory helmet use	Moderate
7) Espitia-Hardeman 2008	Colombia	**Time-Series before and after** **Sample size**: not available	**Enforcement of traffic laws & regulations** Mandatory helmet use and reflective vests	Moderate
8) Hoque 2005	Bangladesh	**Uncontrolled before and after**	**Road engineering intervention** Changes at intersection	Moderate
9) Ichikawa 2002	Thailand	**Uncontrolled before and after** **Sample size:** 12,002 injured motorcyclists	**Enforcement of traffic laws and regulations** Mandatory helmet use	Moderate
10) Liberatti 2001	Brazil	**Uncontrolled before and after** **Sample size of**	**Enforcement of traffic laws and regulations**	Moderate

		motorcycle victims: 1837	Mandatory helmet use	
11) Lipovac 2013	Serbia	**Uncontrolled before and after** **Sample size:** 20,227 pedestrians	**Enforcement of traffic laws and regulations** Red-light running	Moderate
12) Liu 2011	China	**Controlled before and after** **Sample size:** Crash data reported at 366 sites, 15,000 vehicle speed observations.	**Road engineering intervention** Transverse rumble strips	Strong
13) Mutto 2002	Uganda	**Uncontrolled before and after** **Sample size**: traffic volume of 35,551 vehicles	**Road engineering intervention** Pedestrian overpass	Weak
14) Nadesan-Reddy 2013	South Africa	**Time-Series before and after** **Sample size:** 1267 pedestrian–vehicle collisions	**Road engineering intervention** Traffic-calming humps	Strong
15) Nguyen 2013	Vietnam	**Uncontrolled before and after** **Sample size**: 665,428 motorcycle riders	**Enforcement of traffic laws and regulations** Mandatory helmet use	Moderate
16) Panichaphongse 1995	Thailand	**Uncontrolled before and after** **Sample size**: 4035 injured motorcycle accidents	**Enforcement of traffic laws and regulations** Mandatory helmet use	Moderate
17) Quistberg 2014	Peru	**Case–Control study** **Sample size:** randomly sampled 97 control-matched collisions at intersections	**Enforcement of traffic laws and regulations:** Enforcement of pedestrian signalisation	Strong
18) Radin Umar 1995a	Malaysia	**Time-Series before and after** **Sample size:** 3662 motorcycle accident casualty	**Enforcement of traffic laws and regulations** Conspicuity -Use of daytime running lights	Strong
19) Radin Umar 1995b	Malaysia	**Time-Series before and after** **Sample size**: 4319 motorcycle accident casualty	**Road engineering intervention** Exclusive motorcycle lane	Strong
20) Radin Umar 2005	Malaysia	**Time-Series before and after** **Sample size:** 4865	**Enforcement of traffic laws and regulations** Conspicuity -Use of	Strong

		motorcycle accident casualty	daytime running lights	
21) Zhang 2010	China	**Uncontrolled before and after** **Sample size:** 137,820 vehicles	**Enforcement of traffic laws and regulations** Red-light running	Moderate

APPENDIX 7 CHARACTERISTICS OF INCLUDED STUDIES

Short Title	Country	Intervention	Population	Methods	Outcomes	Control Events/Total	Treated Events/ Total
1) Afukaar 2003	Ghana	Road engineering intervention (measures for reduction of vehicle speed)	Non-motorised road users	Uncontrolled before and after	N/A	N/A	N/A
		Rumble strips at Suhum Junction, an accident black spot location, on the main Accra-Kumasi highway in Ghana. Sample size not available		The study analysed the police-reported crash and injury data categorized into a before and period from 1995 to 1999, and an after period from 2000 to April 2001, using MAAP 5 accident analysis package.	Road accident fatalities	3/46	2/10
		Duration: Before 1995–1999 and after 01/2000–04/2001			Road accident: injuries	43/46	8/10
		Setting/Context: High-risk road/vulnerability to road accidents			Road traffic casualty	20/53	13/17
2) Allyana 2014	Malaysia	Enforcement of traffic laws and regulations *(speed cameras)*	Motorised two-wheel road users	Uncontrolled before and after	N/A	N/A	N/A
		Speed limit: Automated enforcement cameras to capture speed limits of all vehicles. Sample size: 31,580 vehicles observed Red-light running (RLR): Automated enforcement cameras to capture RLR violations of all vehicles. Sample size: 331,154 vehicles observed		**Speed Study:** A comparison of speed data before and after the pprogramme was made to determine any significant speed changes on the speed profile. **Red Light Running:** A before and after study was designed to evaluate the impact of installing RLC on RLR.	Compliance: RLR violation	6,870/159,976	3,778/17,1378
		Duration: Speed: Before 08/2012 and after 03/2012 (off-peak period on weekdays and weekends); RLR: Before 09/2012 and after 09/2013 (2 days for two 2-hour periods on weekdays and weekends)			Compliance: speed violation	6,624/15,905	1,538/15,679
		Setting/Context: High-risk road/vulnerability to road accidents			Mean vehicle speed (km/h)	Mean = 87.7; SD = 15.48; N = 15,905	Mean = 76.8; SD = 14.61; N = 15,679
3) Antic 2013	Serbia	Road engineering intervention (measures for reduction of vehicle speed)	Non-motorised road users	Uncontrolled before and after	N/A	N/A	N/A
		Speed bumps of different heights (3, 5, and 7 cm) at each location, two speed bumps, 50 m away from each other, were installed. Sample size: speed of 5182 vehicles were		Speed measurements had been done before speed bumps were installed, 1 day and 1 month after	Mean vehicle speed (km/h)	Mean = 47.58; SD = 3.41; N = 2782	Mean = 43.96; SD = 4.84; N = 2676

Short Title	Country	Intervention	Population	Methods	Outcomes	Control Events/Total	Treated Events/ Total
		observed **Duration**: Before: Before speed bumps were installed and after: 1 day and 30 days after the speed bumps were installed Tuesday and Wednesday between 12 noon to 14:00 at 15-minute intervals **Setting/Context:** High-risk road/vulnerability to road accidents		the installation.			
4) Bastos 2005	Brazil	Enforcement of traffic laws and regulations (mandatory helmet use)	Motorised two-wheel road users	Uncontrolled before and after	N/A	N/A	N/A
		Use of safety devices (helmets and seat belts) among victims of motorcycle and car accidents. Sample size for helmet use: 6298 motorcyclists **Duration:** Before 1997 and after 2000 **Setting/Context:** Urban community		The study analysed the use of safety devices (helmets and seat belts) among victims of motorcycle and car accidents who were seen during the pre-hospital phase between 1 Jan. 1997 and 31 Dec. 2000 in Londrina.	Compliance: helmet use by motor-cyclists	580/1546	1,380/1,603
5) Bhatti 2011	Pakistan	Enforcement of traffic laws and regulations (mandatory helmet use)	Motorised two-wheel road users	Uncontrolled before and after	N/A	N/A	N/A
		The study setting was the Karachi-Hala highway, where a traffic enforcement campaign was conducted from Dec. 2009 to Feb. 2010. Sample size for helmet wearing: 742 motorcyclists and 295 pillion riders **Duration:** Before November 2009 and after April 2010 (7-day survey on randomly selected hours from 8:00 to 20:00) **Setting/Context:** High-risk road/vulnerability to road accidents		In this study, an uncontrolled before-and-after design was used to assess the influence of an enforcement campaign on seat-belt wearing in drivers and front-seat occupants, and helmet wearing in motorcyclists and pillion riders.	Compliance: helmet use by motorcyclists	190/373	224/369
					Compliance: helmet use by pillion riders	9/155	26/140
6) Chiu 2000	Taiwan	Enforcement of traffic laws and regulations (mandatory helmet use)	Motorised two-wheel road users	Uncontrolled before and after	N/A	N/A	N/A
		Effect of the motorcycle helmet law. Sample size: 8795 cases of motorcycle-related head injuries Duration: Before 06/1996–05/1997 and after 06/1997–05/1998 **Setting/Context:** Urban community		Data on head injury were collected for the year before (1 June 1996–31 May 1997) and after (1 June 1997–31 May 1998) implementation of the helmet use law.	Compliance: helmet use by motorcyclists	153/5260	1,630/3,535
					Motorcyclists: fatalities	211/5260	141/3,535
					Motorcyclists: injuries	5049/5260	3,394/3,535
					Motorcyclists: severe	484/5260	3,18/3,535

Short Title	Country	Intervention	Population	Methods	Outcomes	Control Events/Total	Treated Events/ Total
					injuries		
					Motorcyclists: moderate injuries	521/5260	3,36/3,535
7) Espitia-Hardeman 2008	Colombia	Enforcement of traffic laws and regulations (mandatory helmet use and mandatory use of reflective vests)	Motorised two-wheel road users	Interrupted time-series before and after	N/A	N/A	N/A
		1) Decree #1231 issued in August 1996 enforcing the use of helmets among drivers of motorcycles; 2) Decree #1867 issued in November 1997 enforcing the use of helmets for both drivers and passengers of motorcycles. 3) Decree April 2001 enforcing the use of reflective vests for motorcyclists 24 h/day, forbidding the use of motorcycles during the December holidays, as well as enforcing the mandatory attendance of a road safety course for motorcyclists violating the law. Sample size: not available		Time-series analysis was performed to assess the effects of the various interventions, and to determine whether post-intervention data were significantly different from pre-intervention data.	Motorcycle accident fatalities		
		Duration: Before: 01/1993–04/1994, 05/1994–06/1996; After: 01/2000–12/2000 01/2001–12/2001					
		Setting/Context: Urban community					
8) Hoque 2005	Bangladesh	Road engineering intervention (changes at intersection and segregation of vulnerable road users from motorised road users)	Non-motorised road users	Uncontrolled before and after	N/A	N/A	N/A
		Changes at intersection Segregation of vulnerable road users from motorised vehicles		This study evaluated the effectiveness of the safety improvement measures undertaken in three black-spot areas and two sections of the highway during the period 1995–2002. Data for such evaluations were collected both before and after the implementation of the safety improvement works.	Road accident fatalities	21/307	6/307
		Duration: Before 1996–1998 and after 06/2002–07/2003			Road traffic casualty	154/857	390/1,627
		Setting/Context: High-risk road/vulnerability to road accidents. The study area envisaged a total length of 75.4 km along the Dhaka-Aricha highway, starting from the 11.9 km reference point at Aminbazar Bridge to 87.3 km at Aricha Ferry Ghat.					
9) Ichikawa 2002	Thailand	Enforcement of traffic laws and regulations (mandatory helmet use)	Motorised two-wheel road users	Uncontrolled before and after	N/A	N/A	N/A
		Effect of the helmet act for motorcyclists. Sample size: 12,002 injured motorcyclists		Helmet use and outcome in motorcycle crashes were compared 2 years before (1994–1995) and after (1996–1997) enforcement of	Compliance: helmet use by motorcyclists	325/6776	1,082/4,347
		Duration: Before 01/1994–12/1995 and after 01/1996–12/1997			Motorcyclists: fatalities	72/7,206	57/4,756

Short Title	Country	Intervention	Population	Methods	Outcomes	Control Events/Total	Treated Events/ Total
		Setting/Context: Rural community		the helmet act.	Motorcyclists: injuries	6,719/7,206	4,262/4,756
10) Liberatti 2001	Brazil	Enforcement of traffic laws and regulations (mandatory helmet use)	Motorised two-wheel road users	Uncontrolled before and after			
		Sample size for motorcycle victims: 1837 **Duration:** Before 01/1997–07/1997 and after 01/1998–07/1998 **Setting/Context:** Urban community		The study population was car and motorcycle occupants seen in a pre-hospital care service in Londrina (Brazil) before the introduction of the new Brazilian traffic code from Jan. to July 1997, and after its implementation during the same period in 1998.	Compliance: helmet use by motorcyclists	224/718	434/656
11) Lipovac 2013	Serbia	Enforcement of traffic laws and regulations (RLR)	Non-motorised road users	Uncontrolled before and after	N/A	N/A	N/A
		RLR violation: signalised pedestrian crossing PC1 is 9-m wide, and the PC2 crossing is 8-m wide. Sample size: 20,227 pedestrians **Duration:** Before 10/2011 and after 11/2011 (14 days -2× 7 working days. Recordings at each crossing were carried out twice a day – in the morning, from 09:00 to 11:00, and in the afternoon, from 14:00 to 16:00 h (in 7-day periods) **Setting/Context:** Urban community		The analysis included the observance of light signals by pedestrians in situations before and after the pedestrian countdown displays were installed at two crossings in the city of Doboj (BIH, Republic of Srpska).	Compliance: RLR violation	2127/7045	1442/6385
12) Liu 2011	China	Road engineering intervention (measures for reduction of vehicle speed)	Non-motorised road users	Controlled before and after	N/A	N/A	N/A
		Transverse rumble strips considered in this study are raised rumble strips deployed on both approaches to a signalised pedestrian crosswalk on a rural road. Sample size: 15,000 vehicle speed observations **Duration:** Before 02/2007– 03/2008 and after 04/2008–2/2009 **Setting/Context:** Rural community **Setting/Context**: Urban community		An observational before-and-after study using a comparison group and the empirical Bayesian method to evaluate the effectiveness of transverse rumble strips in reducing crashes at pedestrian crosswalks.	Road traffic casualty	15/223	14/74
13) Mutto 2002	Uganda	Road engineering intervention (segregation of vulnerable road users from motorised vehicles)	Non-motorised road users	Uncontrolled before and after	N/A	N/A	N/A
		Effect of an overpass on the rates of pedestrian crashes		The study was conducted in	Road accident	8/35,551	2/35,551

Short Title	Country	Intervention	Population	Methods	Outcomes	Control Events/Total	Treated Events/ Total
		and injuries. Sample size: 35,551 traffic volume		Nakawa, approx. 6 km on the Kampala-Jinja highway between 16 and 22 Dec. 1999 using cross-sectional and retrospective designs. Police traffic crash records covering 1 year before and 1 year after the intervention were reviewed for traffic injuries in this trading centre.	fatalities		
		Duration: Before 12/1998 and after 12/1999 (Pedestrian road behaviours and traffic patterns were observed at two peak hours: 07:30–08:30 and 17:30–18:30; and 1 non-peak hour: 10:30–11:30			Road traffic casualty	27/35,551	68/35,551
		Setting/Context: High-risk road/vulnerability to road accidents					
14) Nadesan-Reddy 2013	South Africa	Road engineering intervention (measures for reduction of vehicle speed)	Non-motorised road users	Interrupted time-series before and after	N/A	N/A	N/A
		Traffic-calming humps in the Chatsworth and KwaMashu residential areas of the eThekwini Municipality. Sample size: 1267 pedestrian–vehicle collisions		This population-based study used an observational, interrupted time-series before-and-after study design that assessed vehicle and pedestrian–vehicle collision data on school route roads over a 5-year period: 2 consecutive years prior to and 2 consecutive years following the implementation of speed humps (the year the speed humps were installed was excluded from the analysis).	Pedestrian–vehicle casualty		
		Duration: Before 2003–2005 and after 2006–2008					
		Setting/Context: High-risk road/vulnerability to road accidents					
15) Nguyen 2013	Vietnam	Enforcement of traffic laws and regulations (mandatory helmet use)	Motorised two-wheel road users	Uncontrolled before and after	N/A	N/A	N/A
		Helmet wearing among motorcycle riders and passengers. Sample size: 665,428 motorcycle riders		This study observed helmet wearing among motorcycle riders and passengers in three provinces (Yen Bai, Da Nang, and Binh Duong) in the Socialist Republic of Viet Nam, before and after a mandatory helmet law took effect on 15 Dec. 2007.	Motorcycle riders: ccompliance	32,262/76,898	87,921/93,745
		Duration: Before 11/2007 and after 02/2011 (Observations were conducted during four time periods at each observation site [07:00– 09:00; 10:00–12:00; 16:00–18:00; and 19:00–21:00] on two non-consecutive weekdays [Monday–Friday] and one weekend day [Saturday or Sunday]. No observations were taken during bad weather [rain, storm, etc.])					
		Setting/Context: Urban community			Motorcycle pillion riders: compliance	12,161/33,779	31,465/37,412
16)	Thailand	Enforcement of traffic laws and regulations **(**mandatory	Motorised	Uncontrolled before and after	N/A	N/A	N/A

Short Title	Country	Intervention	Population	Methods	Outcomes	Control Events/Total	Treated Events/ Total
Panichaphongse 1995		helmet use)	two-wheel road users				
		Motorcycle accidents following the decree promulgated in 1992 for compulsory use of safety helmets by motorcyclists and pillion riders. Sample size: 4035 injured motorcycle accidents		This study compared the death rates 2 years before the decree was enforced (1991–1992) and 2 years after its enforcement (1993–1994). The sample included persons injured in a motorcycle accident who were treated at Chulalongkorn Hospital and those who died as the result of motorcycle accident between 1991 and 1994.	Motorcyclists: fatalities	1,38/6986	115/7,967
		Duration: Before 1991–1992 and after 1993–1994			Motorcyclists: injuries	4412/6986	4,298/7,967
		Setting/Context: Urban community			Motorcyclists: severe injuries	1,001/6,986	1017/7,967
17) Quistberg 2014	Peru	Combination of engineering and regulatory and legislative (traffic enforcement)	Non-motorised road users	Case–ccontrol study	N/A	N/A	N/A
		This study examined the relationship between pedestrian–motor vehicle collisions and the presence of visible traffic signals, pedestrian signals, and signal timing to determine whether these countermeasures improved pedestrian safety.		A matched case–control design was used where the units of study were crossing locations. Cases were pedestrian crossings at road intersections where the Policy Nacional del Peru (National Police of Peru) reported that a pedestrian collision had occurred between 1 Oct. 2010 and 15 Jan. 2011. Controls were pedestrian crossings in the proximity of case sites that matched the case site's road classification and number of lanes.		No signal: 56/97	No signal: 52/97
		Duration: 01/2011–02/2011 (collection time 01/2011–02/2011)				Phased signal: 26/97	Phased signal: 31/97
		Setting/Context: Urban community				Countdown signal: 16/97	Countdown signal: 13/97
18) Radin Umar 1995a	Malaysia	Enforcement of traffic laws and regulations (conspicuity: use of daytime running lights)	Motorised two-wheel road users	Interrupted time-sseries before and after	N/A	N/A	N/A
		Running headlights campaign and regulation. Sample size: 3,662 motorcycle accidents		This paper presents the analysis of motorcycle accidents and the impact of the recent running headlight campaign and regulation in the pilot project areas, Seremban	Conspicuity-related road traffic casualty		
		Duration: Before 01/1992–06/1992 and after 07/1992–12/1992					
		Setting/Context: Urban community					

Short Title	Country	Intervention	Population	Methods	Outcomes	Control Events/Total	Treated Events/ Total
				and Shah Alam, Malaysia.			
19) Radin Umar 1995b	Malaysia	Road engineering intervention (segregation of vulnerable road users from motorised vehicles)	Motorised two-wheel road users	Interrupted time-series before and after	N/A	N/A	N/A
		Motorcycles segregation from other traffic using an exclusive motorcycle lane. Sample size: 4,319 motorcycle accidents		The study presents a preliminary analysis of the impact of the motorcycle lane on motorcycle accidents along Federal Route F02 within the Shah Alam, Malaysia. Accident data were extracted from the 4-year pilot project data, the time-series cumulative plot'2 of monthly records and the traditional Chi^2 before and after analysis'.	Motorcyclists' road traffic casualty		
		Duration: Before 03/1993–11/1993 and after 12/1993–08/1994					
		Setting/Context: High-risk road/vulnerability to road accidents					
20) Radin Umar 2005	Malaysia	Enforcement of traffic laws and regulations (conspicuity: use of daytime running lights)	Motorised two-wheel road users	Interrupted time-series before and after	N/A	N/A	N/A
		Frontal conspicuity intervention as a low-cost safety policy: running headlights during the day. Sample size: 4865 motorcycle accidents		To analyse the potential benefit of the intervention, a before-and-after analysis was carried out. Data were classified according to daytime and night-time accidents involving conspicuity-related, single-motorcycle accidents and non-conspicuity related accidents.	Motorcyclists' conspicuity-related road traffic casualty		
		Duration: Before 06/1991–07/1992 and after 07/1992–06/1993					
		Setting/Context: Urban community					
21) Zhang 2010	China	Enforcement of traffic laws and regulations (red-light running)	Motorised two-wheel road users	Uncontrolled before and after	N/A	N/A	N/A
		RLR driver behaviour at intersections. Sample size: 137,820	Non-motorised road users	Data were collected from video recorders at an intersection with four legs in the Beijing Fangzhuang district. Video cameras recorded drivers' crossing and stopping behaviours at every approach in the 7 days before and after installing countdown signals.	N/A	N/A	N/A
		Duration: Before: 7 days before the installation of signal in March 2010. After: 7 days after the installation of signal in March 2010 (Drivers' crossing and stopping behaviours at every approach 7 days before and after installing countdown signals. Observing time was from 11:30 to 13:30, from 17:00 to 19:00, and from 20:00 to 22:00 daily)			Compliance: RLR violation	188/67,874	50/69,946
		Setting/Context: Urban community					

APPENDIX 8 RISK OF BIAS ASSESSMENT

Author	SELECTION BIAS	STUDY DESIGN	CONFOUNDERS	DATACOLLECTION METHODS	INTERVENTION INTEGRITY	ANALYSIS INTERNAL VALIDITY	BLINDING	WITHDRAWLS AND DROPOUTS	GLOBAL RATING FOR THIS PAPER
1)Afukaar 2003	STRONG	WEAK	MODERATE	STRONG	MODERATE	STRONG	MODERATE	MODERATE	MODERATE
2) Allyana 2014	MODERATE	WEAK	MODERATE	STRONG	MODERATE	STRONG	MODERATE	MODERATE	MODERATE
3) Antic 2013	STRONG	WEAK	MODERATE	STRONG	STRONG	STRONG	MODERATE	MODERATE	MODERATE
4) Bastos 2005	MODERATE	MODERATE	MODERATE	STRONG	STRONG	STRONG	MODERATE	MODERATE	MODERATE
5) Bhatti 2011	STRONG	WEAK	MODERATE	STRONG	MODERATE	STRONG	MODERATE	MODERATE	MODERATE
6) Chiu 2000	STRONG	WEAK	MODERATE	STRONG	STRONG	STRONG	MODERATE	MODERATE	MODERATE
7) Espitia-Hardeman 2008	MODERATE	MODERATE	STRONG	STRONG	MODERATE	STRONG	MODERATE	MODERATE	STRONG
8) Hoque 2005	STRONG	WEAK	MODERATE	STRONG	MODERATE	STRONG	MODERATE	MODERATE	MODERATE
9) Ichikawa 2002	MODERATE	WEAK	MODERATE	STRONG	STRONG	STRONG	MODERATE	MODERATE	MODERATE
10) Liberatti 2001	MODERATE	WEAK	MODERATE	STRONG	MODERATE	STRONG	MODERATE	MODERATE	MODERATE
11) Lipovac 2013	STRONG	WEAK	STRONG	STRONG	STRONG	STRONG	MODERATE	MODERATE	MODERATE

Author	SELECTION BIAS	STUDY DESIGN	CONFOUNDERS	DATACOLLECTION METHODS	INTERVENTION INTEGRITY	ANALYSIS INTERNAL VALIDITY	BLINDING	WITHDRAWLS AND DROPOUTS	GLOBAL RATING FOR THIS PAPER
12) Liu 2011	STRONG	MODERATE	MODERATE	STRONG	STRONG	STRONG	MODERATE	MODERATE	STRONG
13) Mutto 2002	MODERATE	WEAK	MODERATE	STRONG	STRONG	MODERATE	MODERATE	MODERATE	MODERATE
14) Nadesan-Reddy 2013	MODERATE	MODERATE	MODERATE	STRONG	STRONG	STRONG	MODERATE	MODERATE	STRONG
15) Nguyen 2013	STRONG	WEAK	MODERATE	STRONG	MODERATE	MODERATE	MODERATE	MODERATE	MODERATE
16) Panichaphongse 1995	STRONG	WEAK	MODERATE	STRONG	MODERATE	STRONG	MODERATE	MODERATE	MODERATE
17) Passmore 2010	MODERATE	WEAK	WEAK	WEAK	WEAK	WEAK	MODERATE	MODERATE	WEAK
18) Quistberg 2014	STRONG	MODERATE	STRONG	STRONG	MODERATE	STRONG	MODERATE	MODERATE	STRONG
19) Radin Umar 1995a	STRONG	MODERATE	MODERATE	STRONG	MODERATE	STRONG	MODERATE	MODERATE	STRONG
20) Radin Umar 1995b	STRONG	MODERATE	MODERATE	STRONG	MODERATE	STRONG	MODERATE	MODERATE	STRONG
21)Radin Umar 2005	STRONG	MODERATE	MODERATE	STRONG	STRONG	STRONG	MODERATE	MODERATE	STRONG
22) Wu 2013	MODERATE	WEAK	WEAK	MODERATE	MODERATE	STRONG	MODERATE	MODERATE	WEAK
23) Yuan 2010	MODERATE	WEAK	WEAK	MODERATE	WEAK	WEAK	MODERATE	MODERATE	WEAK

Author	SELECTION BIAS	STUDY DESIGN	CONFOUNDERS	DATACOLLECTION METHODS	INTERVENTION INTEGRITY	ANALYSIS INTERNAL VALIDITY	BLINDING	WITHDRAWLS AND DROPOUTS	GLOBAL RATING FOR THIS PAPER
24) Yuan 2012	MODERATE	WEAK	WEAK	WEAK	WEAK	WEAK	MODERATE	MODERATE	WEAK
25) Zhang 2010	STRONG	WEAK	MODERATE	STRONG	STRONG	STRONG	MODERATE	MODERATE	MODERATE

Author	Q1- What was the basis for selection of intervention site -high accident frequencies or some other traffic rule?	Q2- Were the intervention and the control sites matched for geographic characteristics?	Q3 - Were the intervention and control site matched for exposure effect?	Q4- Were the intervention and control site matched for trend effect?	Q5 - Was there a sufficient passage of transition-al period following the infrastructure construction?	Q6- Did the study control for restricted participant selection?	Indicate the percentage of relevant confounders controlled or adjusted?	Rate this section
1)Afukaar 2003	Very likely	Yes	Yes	No	Can't tell	Can't tell	60-79% (some)	MODERATE
2)Allyana 2014	Very likely	Yes	Can't tell	No	N A	Yes	60-79% (some)	MODERATE
3)Antic 2013	Not likely	Yes	Yes	No	Yes	Can't tell	80-100% (most)	MODERATE
4)Bastos 2005	Not likely	N A	N A	Yes	N A	Yes	60-79% (some)	MODERATE
5)Bhatti 2011	Very likely	Yes	N A	No	N A	Yes	60-79% (some)	MODERATE
6)Chiu 2000	Not likely	Yes	N A	Yes	N A	Yes	60-79% (some)	MODERATE
7)Espitia-Hardeman 2008	Not likely	Yes	N A	Yes	N A	Yes	80-100% (most)	STRONG
8)Hoque 2005	Very likely	Yes	Yes	Yes	Can't tell	No	60-79% (some)	MODERATE

9)Ichikawa 2002	Not likely	Yes	N A	Yes	N A	Yes	60-79% (some)	MODERATE
10)Liberatti 2001	Not likely	Yes	N A	Yes	N A	Yes	60-79% (some)	MODERATE
11)Lipovac 2013	Not likely	Yes	Yes	No	Can't tell	Can’t tell	60-79% (some)	MODERATE
12)Liu 2011	Not likely	Yes	Yes	No	Can't tell	Can't tell	60-79% (some)	MODERATE
13)Mutto 2002	Very likely	Yes	Can't tell	No	Can't tell	Can't tell	Less than 60% (few or more)	WEAK
14)Nadesan-Reddy 2013	Very likely	Yes	Yes	Yes	Can't tell	Yes	60-79% (some)	MODERATE
15)Nguyen 2013	Not likely	Yes	N A	Yes	N A	Yes	60-79% (some)	MODERATE
16)Panichaphongse 1995	Not likely	Yes	N A	Yes	N A	Yes	60-79% (some)	MODERATE
17)Quistberg 2014	Not likely	Yes	Yes	No	Yes	Yes	80-100% (most)	STRONG
18)Radin Umar 1995a	Not likely	Yes	N A	No	N A	Yes	60-79% (some)	MODERATE
19)Radin Umar 1995b	Very likely	Yes	Yes	No	No	Yes	60-79% (some)	MODERATE
20)Radin Umar 2005	Not likely	Yes	N A	No	N A	Yes	80-100% (most)	MODERATE
21)Zhang 2010	Not likely	Yes	Yes	No	N A	Can't tell	60-79% (some)	MODERATE

Explanations	
Q1	Regression-to-the-mean is typically observed at sites with high values for crash frequencies: If high accident frequencies, then Very Likely; If other general traffic rule, then Not Likely.
Q2	If the treated facility is an intersection, the comparison site should be a similar intersection in respect of area type (commercial business district, urban, rural), intersection type (three-legged or four-legged), traffic control (signalised, two-way stop-controlled, etc.).
Q3	For controlled before-and-after studies, traffic volume and location matching, warm/cold weather months, daylight versus dawn/night, traffic composition, enforcement level; In before and after, vehicular traffic volume and location matching, warm/cold weather months, daylight versus dawn/night, traffic composition, enforcement level, pre and post.
Q4	If the crash figures over a multi-year period show a continuous increasing or a decreasing trend with little fluctuation in crash frequencies; If there is a sudden drop in the crash frequency after some improvements were made at treatment site and the figures follow the after-period trend.
Q5	In controlled before and after, study which does not specify the time period over which outcomes were reported, the question should be answered as Can't tell; In before and after studies, if the intervention site was not given a 'sufficient' passage of transitional period following the infrastructure construction, the answer is No. In case-control studies, if the period between the intervention and outcomes is not the same for cases and controls, the answer is No.
Q6	So that all groups had the same value for the confounder, for example, restricting the study to two-wheel road users.

APPENDIX 10 COMMON RUBRIC AND VOTE COUNT

Uncontrolled before and after studies	**Study**	**Odds ratio**	**CI**	**Risk ratio**	**CI**	**Risk Difference**	**Direction of Intervention**	**Voting count OR**	**Voting count RR**
Mandatory helmet use - fatalities	Chiu 2000b	0.99	0.80, 1.24	0.99	0.81, 1.23	-0.02%	Trend towards intervention	√	√
	Ichikawa 2002b	1.20	0.85, 1.70	1.20	0.85, 1.69	0.20%	Does not favour intervention		
	Panichaphongse 1995a	0.73	0.57, 0.93	0.73	0.57, 0.93	-0.53%	Favours intervention	√√	√√
	Mean effect size	**0.94**	**0.69, 1.28**	**0.93**	**0.79, 1.09**				
Mandatory helmet use - injuries	Chiu 2000c	1.01	0.81, 1.25	1.00	0.99, 1.01	0.02%	Does not favour intervention		
	Ichikawa 2002c	0.63	0.55, 0.71	0.96	0.95, 0.97	-3.63%	Trend towards intervention	√	√
	Panichaphongse 1995b	0.68	0.64, 0.73	0.85	0.83, 0.88	-9.21%	Favours intervention	√√	√√
	Mean effect size	**0.87**	**0.68, 1.11**	**0.97**	**0.91, 1.03**				
Mandatory helmet use - severe injuries	Chiu 2000d	0.98	0.84, 1.13	0.98	0.85, 1.12	-0.21%	Trend towards intervention	√	√
	Panichaphongse 1995c	0.87	0.80, 0.96	0.89	0.82, 0.97	-1.56%	Favours intervention	√√	√√
	Mean effect size	**0.92**	**0.66, 1.29**	**0.92**	**0.83, 1.03**				
Mandatory helmet use - moderate injuries	Chiu 2000e	0.96	0.83, 1.10	0.96	0.84, 1.09	-0.40%	Trend towards intervention	√	√
	Panichaphongse 1995d	0.73	0.69, 0.78	0.84	0.81, 0.87	-7.64%	Favours intervention	√√	√√
	Mean effect size	**0.83**	**0.60, 1.16**	**0.88**	**0.80, 0.97**				
Secondary Outcomes									
Mandatory helmet use - compliance pillion riders	Bhatti 2011b	3.70	1.67, 8.21	3.20	1.55, 6.59	12.76%	Trend towards intervention	√	√
	Nyugen 2013b	9.29	8.97, 9.63	2.33	2.30, 2.37	47.94%	Favours intervention	√√	√√
	Mean effect size	5.99	3.33, 10.77	2.21	1.89, 2.58				

Uncontrolled before and after studies	Study	Odds ratio	CI	Risk ratio	CI	Risk Difference	Direction of Intervention	Voting count OR	Voting count RR
Mandatory helmet use - compliance motorcycle riders									
	Nyugen 2013a	20.89	20.27, 21.53	2.24	2.22, 2.25	51.8%	Favours intervention	√	√
	Bhatti 2011a	1.49	1.11, 1.99	1.19	1.05, 1.36	9.8%	Trend towards intervention	√√	√√
	Mean effect size	**5.71**	**1.53, 21.27**	**2.16**	**0.98, 4.73**				
	Liberatti 2001	4.31	3.44, 5.40	2.12	1.88, 2.39	35.0%	Favours intervention	√√	√√
	Bastos 2005	10.31	8.65, 12.28	2.29	2.15, 2.45	48.6%	Favours intervention	√√	√√
	Chiu 2000a	28.56	24.00, 33.98	15.85	13.51, 18.61	43.2%	Favours intervention	√√	√√
	Mean effect size	**10.84**	**2.91, 40.38**	**4.24**	**1.93, 9.30**				
	Ichikawa 2002a	6.58	5.77, 7.50	5.19	4.61, 5.84	20.1%	Favours intervention	√√	√√
	Overall mean effect size	**7.67**	**3.24, 18.14**	**3.34**	**1.75, 6.37**				
Red-Light Running/violations	Allyana 2014b	0.15	0.14, 0.16	0.24	0.22, 0.25	-31.84%	Favours intervention	√√	√√
	Zhang 2010	0.26	0.19, 0.35	0.26	0.19, 0.35	-0.21%	Trend towards intervention	√	√
	Allyana 2014	0.50	0.48, 0.52	0.51	0.49, 0.53	-2.09%	Favours intervention	√√	√√
	Lipovac 2013	0.67	0.62, 0.73	0.75	0.71, 0.79	-7.61%	Favours intervention	√√	√√
	Mean effect size	0.34	0.17, 0.70	0.39	0.23, 0.67				
Measures for speed reductions - injuries	Afukaar 2003b	0.28	0.04, 1.95	0.86	0.62, 1.18	-13.48%			
	Mean effect size	**0.28**	**0.03, 2.45**	**0.86**	**0.36, 2.02**				
Measures for speed reduction -fatalities	Mutto 2002a	0.25	0.05, 1.18	0.25	0.05, 1.18	-0.02%	Trend towards intervention	√	√
	Hoque 2005a	0.27	0.11, 0.67	0.29	0.12, 0.69	-5.00%	Trend towards intervention	√	√
	Afukaar 2003a	3.58	0.51, 24.98	3.07	0.59, 16.02	13.48%	Does not favour intervention		

Uncontrolled before and after studies	**Study**	**Odds ratio**	**CI**	**Risk ratio**	**CI**	**Risk Difference**	**Direction of Intervention**	**Voting count OR**	**Voting count RR**
	Mean effect size	**0.44**	**0.17, 1.16**	**0.47**	**0.20, 1.12**				
Measures for speed reduction - casualty	Mutto 2002b	2.52	1.61, 3.94	2.52	1.61, 3.93	0.12%	Does not favour intervention		
	Afukaar 2003c	5.36	1.54, 18.73	2.03	1.31, 3.13	38.73%	Does not favour intervention		
	Hoque 2005b	1.44	1.17, 1.77	1.33	1.13, 1.58	6.00%	Does not favour intervention		
	Mean effect size	**2.43**	**1.35, 4.37**	**2.00**	**1.27, 3.15**				
Measures for speed reduction - Mean vehicle speed (km/h)	Antic 2013	0.21	0.19, 0.23				Trend towards intervention	√	√
	Mean effect size	**0.21**	**0.19, 0.23**						
Controlled before and after	**Study**	**Odds ratio**	**CI**	**Risk ratio**	**CI**				
Measures for speed reduction - mean vehicle speed	Liu 2011	3.24	1.48, 7.08	2.81	1.43, 5.55	12.19%	Does not favour intervention		
	Mean effect size	**3.24**	**1.48, 7.08**	**2.81**	**1.43, 5.55**				
Time series studies	**Study**	**Effect Size**							
Measures for speed reduction - casualty	Nadesan Reddy 2013b	-1.10							
	Nadesan Reddy 2013a	-0.24							
Segregation of VRU - casualty	Radin Umar RS 1995b	-0.75							
Mandatory helmet use - fatalities	Espitia-Hardeman 2008	-1.79							
Daytime running headlight - casualty	Radin Umar RS 1995a	-0.41							
Daytime running headlight - casualty	Radin Umar RS 2005	-0.52							

Note: Two ticks (√√) indicate positive and highly significant based on effect size; a single tick (√) indicates positive and favorable based on effect size; and no tick (blank) indicates a negative effect

APPENDIX 11 MODERATORS OF EFFECTS

	Short Title	Region					Low- or middle-income		Setting/Context			Data-collection period			
		East Asia & Pacific	Europe & Central Asia	Latin America &Caribbean	South Asia	**Sub-Sahara Africa**	**Middle-income**	**Low-income**	High-risk road/vulnerability to road accidents	Urban community	Rural community	1-14 days	1-6 months	6-12 months	24+months
1	Afukaar 2003					√	√		√					√	
2	Allyana 2014	√					√		√			√			
3	Antic 2013		√				√		√				√		
4	Bastos 2005			√			√			√				√	
5	Bhatti 2011				√		√		√				√		
6	Chiu 2000	√					√			√				√	
7	Espitia-Hardeman 2008			√			√			√					√
8	Hoque 2005				√			√	√					√	
9	Ichikawa 2002	√					√				√				√
10	Liberatti 2001			√			√			√				√	
11	Lipovac 2013		√				√			√		√			
12	Liu 2011	√					√				√				
13	Mutto 2002					√		√	√			√			
14	Nadesan-Reddy 2013					√	√		√						√
15	Nguyen 2013	√					√			√			√		
16	Panichaphongse 1995	√					√			√					√
17	Quistberg 2014			√			√			√			√		
18	Radin Umar 1995a	√					√			√				√	
19	Radin Umar 1995b	√					√		√						√
20	Radin Umar 2005	√					√			√					√
21	Zhang 2010	√					√			√		√			

APPENDIX 12 SUMMARY OF FINDINGS

Traffic Law Enfrocement & Regulatory Interventions

Patient or population: Vulnerable (non-motorised and motorised two-wheel) road users
Settings: Low- and middle-income countries
Intervention: Traffic law enforcement & regulatory versus no intervention

Outcomes	Illustrative comparative risks* (95% CI)		Relative effect (95% CI)	No of Participants (studies)	Quality of the evidence (GRADE)	Comments
	Assumed risk **Control**	Corresponding risk **Traffic law enforcement & regulatory versus no intervention**				
Fatalities among motorcycle riders	**Study population**		**OR 0.94** (0.72 to 1.23)	35710 (3 studies)	⊕⊖⊖⊖ **very low**[1,2,3]	
	22 per 1000	**20 per 1000** (16 to 26)				
	Moderate					
	20 per 1000	**19 per 1000** (14 to 24)				
Injuries among motorcycle riders	**Study population**		**OR 0.74** (0.61 to 0.89)	35710 (3 studies)	⊕⊖⊖⊖ **very low**[4,5]	
	832 per 1000	**785 per 1000** (751 to 815)				
	Moderate					
	932 per 1000	**910 per 1000** (893 to 924)				
Severe Injuries among motorcycle riders	**Study population**		**OR 0.91** (0.82 to 1.01)	23748 (2 studies)	⊕⊕⊕⊖ **moderate**[6]	
	121 per 1000	**112 per 1000** (102 to 122)				
	Moderate					
	118 per 1000	**109 per 1000** (99 to 119)				
Moderate Injuries among motorcycle riders	**Study population**		**OR 0.77**	23748	⊕⊖⊖⊖	

	321 per 1000	**267 per 1000** (254 to 277)	(0.72 to 0.81)	(2 studies)	**very low**[7,8,9]
	Moderate				
	294 per 1000	**243 per 1000** (231 to 252)			
Fatalities among motorcycle riders - percent change	**Study population**		Not estimable	0 (1 study)	⊕⊕⊝⊝ **low**[10,11]
	Moderate				
Casualties among motorcycle riders - percent change	**Study population**		Not estimable	5083 (2 studies)	⊕⊕⊕⊝ **moderate**[12]
	191 per 1000	**0 per 1000** (0 to 0)			
	Moderate				
Secondary Outcome - Mandatory helmet use compliance among motorcyclists	**Study population**		**OR 8.05** (4.03 to 16.05)	195827 (6 studies)	⊕⊝⊝⊝ **very low**[13,14,15,16]
	368 per 1000	**824 per 1000** (702 to 903)			
	Moderate				
	344 per 1000	**808 per 1000** (679 to 894)			
Secondary Outcome - Mandatory helmet use compliance among pillion riders	**Study population**		**OR 6.41** (2.65 to 15.54)	71486 (2 studies)	⊕⊝⊝⊝ **very low**[17,18,19]
	359 per 1000	**782 per 1000** (597 to 897)			
	Moderate				
	209 per 1000	**629 per 1000** (412 to 804)			
Secondary Outcome - Non-compliance to red light and speed traffic laws	**Study population**		**OR 0.34** (0.17 to 0.7)	514188 (3 studies)	⊕⊝⊝⊝ **very low**[20,21,22,23]
	63 per 1000	**22 per 1000** (11 to 45)			
	Moderate				
	172 per 1000	**66 per 1000** (34 to 127)			

*The basis for the **assumed risk** (e.g. the median control group risk across studies) is provided in footnotes. The **corresponding risk** (and its 95% confidence interval) is based on the assumed risk in the comparison group and the **relative effect** of the intervention (and its 95% CI).

CI: Confidence interval; **RR:** Risk ratio; **OR:** Odds ratio;

GRADE Working Group grades of evidence
High quality: Further research is very unlikely to change our confidence in the estimate of effect.
Moderate quality: Further research is likely to have an important impact on our confidence in the estimate of effect and may change the estimate.
Low quality: Further research is very likely to have an important impact on our confidence in the estimate of effect and is likely to change the estimate.
Very low quality: We are very uncertain about the estimate.

[1] Although, included studies are uncontrolled before and after, however the post-intervention period started immediately after the deployment of the intervention and included just one period before and one period after; therefore, general changes and change of traffic volume confounders were possibly controlled.
[2] Effect sizes are closely aligned and confidence interval overlap, Even though, I2=68%, but, tau-square is less than 1 and chi-square p-value = .05. Perhaps, some heterogeneity is due to differences in population.
[3] 95% confidence interval around the pooled estimate of effect of two out of three studies include no effect and confidence interval cross the line of no effect.
[4] Although, included studies are uncontrolled before and after, however the post-intervention period started immediately after the deployment of the intervention and included just one period before and one period after; therefore, general changes and change of traffic volume confounders were possibly controlled.
[5] The effect estimates are not variant and confidence intervals overlap. Presence of statistically significant heterogeneity is noted, I2=85%, tau-square is less than 1 and chi-square p-value is less than.05. Perhaps, some heterogeneity is due to differences in populations in included studies
[6] Although, included studies are uncontrolled before and after, however the post-intervention period started immediately after the deployment of the intervention and included just one period before and one period after; therefore, general changes and change of traffic volume confounders were possibly controlled.
[7] Although, included studies are uncontrolled before and after, however the post-intervention period started immediately after the deployment of the intervention and included just one period before and one period after; therefore, general changes and change of traffic volume confounders were possibly controlled.
[8] Presence of high level of statistical heterogeneity, I2 = 91%, chi-square of 10.69 with low p-value = 0.001. Confidence intervals overlap. Perhaps, some heterogeneity is due to differences in populations.
[9] Although, the pooled effect size confidence interval is away from the line of no effect, however, of the two studies, one study effect size is close to no effect line and the confidence interval crosses the no effect line.
[10] The total for population was inferred from other studies
[11] Plausible confounding variable: there is a possibility of another intervention or policy change that might be confounded during the enforcement intervention period
[12] Plausible confounding variable, lack of long-term observational periods before and after the interventions.
[13] The post-intervention period started immediately after the deployment of the intervention and included just one period before and one period after; therefore, general changes and change of traffic volume confounders were possibly controlled.
[14] Presence of high level of statistical heterogeneity, I2 =99%. Although, some of the heterogeneity is due to differences in populations in included studies.
[15] The point estimate of the odds ratio is 8.05, the upper and the lower boundary of the estimate (16.05 and 4.03) indicate an appreciable benefit after the helmet-law enforcement. Even though, effect points are closely aligned, the confidence interval overlap in a curvilinear manner. The evidence can be downgraded by one level due to wide confidence interval.

[16] Some relevant confounders were controlled, however, there is a possibility of another intervention or policy change that is confounded with the start of the enforcement intervention.

[17] Presence of high level of statistical heterogeneity, I2 =80%, chi-square is less than .05 and Tau square is less than 1. Some of the heterogeneity is due to differences in populations in included studies.

[18] The point estimate of the odds ratio is 6.41, the upper and the lower boundaries of the estimate (2.65- 15.54) indicate an appreciable benefit after the helmet-law enforcement. Even though, effect points are closely aligned and confidence interval overlap, the evidence can be downgraded by one level due to wide confidence interval.

[19] Some relevant confounders were controlled, however, there is a possibility of another intervention or policy change that is confounded with the start of the enforcement intervention.

[20] The post-intervention period started immediately after the deployment of the intervention and included just one period before and one period after; therefore, general changes and change of traffic volume confounders were possibly controlled.

[21] Although, the overall effect is favourable and effect sizes are in close alignment. Also, confidence intervals do not overlap in two studies and overlap in other two studies. Confidence intervals are to the right of the line of no effect meaning there is a statistical difference between the before and the after intervention periods. Nevertheless, presence of high level of statistical heterogeneity, I2 = 100%.

[22] In this case, the sample size is large (n= 263388), the number of events high (n= 6808), and the confidence intervals clearly do not cross the line of appreciable benefit and no effect. The overall result is considered to be precise.

[23] Confounding variables lack of long-term observational periods before and after the interventions and regression-to the mean were not controlled.

Road Engineering Interventions

Patient or population: Vulnerable (non-motorised and motorised two-wheel) road users
Settings: Low- and middle-income countries
Intervention: Road engineering intervention versus no intervention

Outcomes	**Illustrative comparative risks* (95% CI)** Assumed risk **Control**	 Corresponding risk **Road engineering intervention versus no intervention**	**Relative effect (95% CI)**	**No of Participants (studies)**	**Quality of the evidence (GRADE)**	**Comments**
Road accident fatalities	**Study population**		**OR 0.52** (0.12 to 2.16)	71772 (3 studies)	⊕⊖⊖⊖ **very low**[1,2]	
	1 per 1000	**0 per 1000** (0 to 2)				
	Moderate					
	65 per 1000	**35 per 1000** (8 to 131)				
Road accident casualties	**Study population**		**OR 2.18** (1.21 to 3.92)	73656 (3 studies)	⊕⊖⊖⊖ **very low**[3,4,5,6]	
	6 per 1000	**12 per 1000** (7 to 21)				
	Moderate					

	180 per 1000	**324 per 1000** (210 to 463)				
Road accident casualties - percent change	**Study population**		**Percent Change 0** (0 to 5.53)	14404 (2 studies)	⊕⊕⊝⊝ **low**[7,8]	
	213 per 1000	**0 per 1000** (0 to 1000)				
	Moderate					
Road accident fatalities - percent change	**Study population**		Not estimable	10085 (1 study)	⊕⊕⊕⊝ **moderate**[9]	
	5 per 1000	**0 per 1000** (0 to 0)				
	Moderate					

*The basis for the **assumed risk** (e.g. the median control group risk across studies) is provided in footnotes. The **corresponding risk** (and its 95% confidence interval) is based on the assumed risk in the comparison group and the **relative effect** of the intervention (and its 95% CI).

CI: Confidence interval; **RR:** Risk ratio; **OR:** Odds ratio;

GRADE Working Group grades of evidence
High quality: Further research is very unlikely to change our confidence in the estimate of effect.
Moderate quality: Further research is likely to have an important impact on our confidence in the estimate of effect and may change the estimate.
Low quality: Further research is very likely to have an important impact on our confidence in the estimate of effect and is likely to change the estimate.
Very low quality: We are very uncertain about the estimate.

[1] Effect sizes are closely aligned and confidence interval overlap, except for one study. Even though, I2=66%, but, tau-square is greater than 1 and chi-square p-value=.05. Perhaps, some heterogeneity is due to differences in population
[2] Results are imprecise as studies included few events (less than 300). Although, the pooled effect confidence interval overlap, but cross the line of no effect. Confidence intervals of three studies cross the line of no effect.
[3] Some of the confounding variables were not controlled in both studies: regression-to-the mean confounding variables was not controlled; both studies were short term time series using monthly or annual data up to two years before and two years after. A serious issue was the inability to assess the long-term trend-effect confounding factor.
[4] Effect sizes are closely aligned and confidence interval overlap, except for one study. Even though, I2=77%, but, tau-square is less than 1 and chi-square is less than .05. Perhaps, presence of some heterogeneity is due to differences in population.
[5] Although, the health care question was clearly stated, however the outcome of interest was not absolute. Studies have extracted road accident injury outcome data from police reported road traffic crashes and injuries. Often, only fatal and serious road traffic accidents involving hospitalisation are reported in police records.
[6] Results are perhaps imprecise as studies included few events (less than 300).

[7] Statistically, there is a wide variance in effect sizes between two studies, although both studies are on one side of the line having an effect. The confidence intervals overlap. The quality of the evidence would be downgraded for inconsistency based on the fact there is a large difference in the effects between studies, even though there are differences in populations (one study is from Malaysia and the other study is from South Africa).

[8] One study is a short time-series study using monthly data extending at the most 1 year before and 1 year after the intervention. Hence, a serious issue was the inability to assess the long-term trend-effect confounding factor. The second study, regression-to-the-mean confounding factor.

[9] Regression-to-the-mean bias confounding variable was not controlled.

WORLD BANK LIST OF LOW- AND MIDDLE-INCOME COUNTRIES

Afghanistan	Colombia	Hungary	Moldova	South Africa
Albania	Comoros	India	Mongolia	South Sudan
Algeria	Congo, Dem. Rep.	Indonesia	Montenegro	Sri Lanka
American Samoa	Congo, Rep.	Iran, Islamic Rep.	Morocco	St. Lucia
Angola	Costa Rica	Iraq	Mozambique	St. Vincent, Grenadines
Argentina	Côte d'Ivoire	Jamaica	Myanmar	Sudan
Armenia	Cuba	Jordan	Namibia	Suriname
Azerbaijan	Dominica	Kazakhstan	Nepal	Swaziland
Bangladesh	Djibouti	Kenya	Nicaragua	Syrian Arab Republic
Belarus	Dominican Republic	Kiribati	Niger	Tajikistan
Belize	Ecuador	Korea, Dem. Rep.	Nigeria	Tanzania
Benin	Egypt, Arab Rep.	Kosovo	Pakistan	Thailand
Bhutan	El Salvador	Kyrgyz Republic	Palau	Timor-Leste
Bolivia	Eritrea	Lao PDR	Panama	Togo
Bosnia and Herzegovina	Ethiopia	Lebanon	Papua New Guinea	Tonga
Botswana	Fiji	Libya	Paraguay	Tunisia
Brazil	Gabon	Macedonia, FYR	Peru	Turkey
Bulgaria	Gambia, The	Madagascar	Philippines	Turkmenista n
Burkina Faso	Georgia	Malawi	Romania	Tuvalu
Burundi	Ghana	Malaysia	Rwanda	Uganda
Cabo Verde	Grenada	Maldives	Samoa	Ukraine
Cambodia	Guatemala	Mali	Sao Tome, Principe	Uzbekistan
Cameroon	Guinea	Marshall Islands	Senegal	Vanuatu
Central African Rep	Guinea-Bissau	Mauritania	Lesotho	Venezuela, RB
Chad	Guyana	Mauritius	Serbia	Vietnam
China & Province of Taiwan	Haiti	Mexico	Seychelles	West Bank and Gaza
	Honduras	Micronesia, Fed.	Sierra Leone	Yemen, Rep.

			Solomon Islands	Zambia
			Somalia	Zimbabwe

LIST OF ABBREVIATIONS

AfDB	African Development Bank
AFCAP	The African Community Access Program
ADB	Asian Development Bank
AIS	Abbreviated Injury Scale
ARRB	Australian Road Research Board
BRT	Bus Rapid Transit
CAM	Chevron Alignment Markers
CI	Confidence Interval
CMA	Comprehensive-Meta-analysis
CRRI	Central Road Research Institute
DFID	Department for International Development
DIME	Development Impact Evaluation
EMBARQ	The World Resource Institute (WRI) Ross Centre for Sustainable Transport
EPPI-Centre	Evidence for Policy and Practice Information and Co-ordinating Centre
EPOC	Effective Practice and Organization of Care
ESRC	Economic and Social Research Council
gTKP	Global Transport Knowledge Practice
GCS	Glasgow Coma Scale
GOS	Glasgow Outcome Scale
GRSF	Global Road Safety Facility
GRSP	Global Road Safety Partnership

HAT	Hamilton Assessment Tool
ICC	Intra-class correlation coefficient
IRF	International Road Federation
iRAP	International Road Assessment Program
HIC	High-income country
ISS	Injury Severity Score
ITS	Interrupted Time Series
JOLIS	Library catalogue
LILACS	Latin-American and Caribbean Centre on Health Sciences Information
LMIC	Low- and Middle-Income Countries
MCA	Motorcycle accident
NMT	Non-motorised Transport
NMV	Non-motorised vehicle
OECD	Organization of Economic Cooperation and Development
OR	Odd Ratio
SSATP	Sub-Saharan Africa Transport Policy Program
RCT	Randomised Control Trials
REAAA	Road Engineering Association of Asia and Australia
RLR	Red-light Running
RRPM	Raised Reflective Pavement Markers
RR	Risk Ratio
RS10	Road Safety in 10 Countries Project
RTIRN	Road Traffic Injury Research Network
RTS	Revised Trauma Score
RTS	Revised Trauma Score
TRIPP	Transport Research and Injury Prevention Program

TRL	Transport Research Laboratory
TRISS	Trauma and Injury Severity Score
WHO	World Health Organization

DIFFERENCES BETWEEN PROTOCOL AND REVIEW

A few changes from the protocol were necessary, given the complexities of this review. However, these changes were consistent with the objectives of the review.

Firstly, the search criteria were made more sensitive by the addition of thesaurus terms and increased use of synonyms and truncations for keywords. Secondly, the concepts were grouped together to increase the specificity of the search and better to reflect the research question. Thirdly, the search grouped the concepts simply to make it clearer and easier to follow. However, the changes made did not change the structure of the original search:

Population AND Interventions AND Purpose, where: Population = vulnerable road users AND LMIC; Interventions = road engineering OR conspicuity/visibility; Purpose = safety/accident prevention in work/school-related travel.

Secondly, the protocol stated that data will be extracted and input in an excel spreadsheet by one reviewer and checked by a second review author. However, a more inclusive strategy was employed in the selection of studies using the EPPI Reviewer 4.0. EPPI reviewer 4.0 includes a comparative review component in which multiple reviewers double screen and review studies independently and the software shows live comparison of similarities and discrepancies of results. First, two authors screened the titles and abstracts obtained through the search strategy and identified potentially eligible studies independently. Next, using the comparison screening component, results of screened studies were compared and differences in study inclusion was reconciled after a discussion.

Thirdly, the protocol stated that data on secondary outcomes will be extracted from all included studies, wherever available. This review focused on preventive measures implemented to avoid road-traffic accidents and injuries in LMIC (the setting). Secondly, the purpose of this review is to establish the best available and contextually relevant evidence about the effectiveness of enforcement of traffic laws & regulatory and road engineering interventions. The interventions that are being implemented by LMIC may represent stages of development towards HIC status. Several included regulatory and law-enforcement interventions studies reported compliance as an outcome to measure the impact of law enforcement and regulatory interventions for preventing road-traffic injuries among vulnerable road users. Hence, we have extracted, reported, and synthesised data on compliance as an additional secondary outcome in studies that compared changes in compliance outcome before and after a traffic law enforcement & regulatory or a road engineering intervention implemented for prevention of injury (fatal or non-fatal) among vulnerable road users was included.

Additionally, we have amended the wording of the second morbidity outcome "number or proportion of hospitalizations rate'' to ''number or proportion of road traffic casualty'' for clarity purpose. The term '"number or proportion of hospitalization rate" as mentioned in the protocol could refer to reporting of hospitalization from any accident rather than reporting of hospitalization from road traffic accidents. The Oxford dictionaries define casualty as: 1) casualty noun (injured) ''a person injured or killed in a serious accident or war"; 2) casualty noun (hospital) "emergency room, the part of a hospital where people who are hurt in accident or suddenly become ill are taken for treatment";

and, a road traffic accident "when a motor vehicle colloids with another motor vehicle, a stationary object, or person resulting in injuries, deaths, and/or loss of property''. Therefore, while writing the protocol we mentioned "number or proportion of hospitalizations rate'' based on causality definition number 2. However, while reviewing the studies, it was clear that studies have extracted road traffic accident and injury outcome data from police reported road traffic accidents. And, often road traffic accidents requiring medical assistance and hospitalization (excluding damage/loss of property) are reported in police records. Thus, the amended morbidity outcome "number of proportion or road traffic casualty" clarifies the number or proportion of road traffic accidents requiring hospital treatment.

Finally, a new software was added to compute effect sizes for time-series studies using R Statistical Software. The computation effect sizes is drawn from the methodology developed by David B Wilson for his review *Juvenile curfew effects on criminal behaviour and victimization: a systematic review* (2015*).* The calculations for all of the effect sizes were conducted using the statistical software R1 (version 3.1.2). There is a subset for each study indexed by a reference identifier and by the first author and year. The studies considered in this meta-analysis have either accidents or deaths as the outcome. The outcome being count, a Poisson regression model was used to estimate the effects of intervention after adjusting for a linear trend in time. Specifically, we fitted a model of the form:

Where,

·: The logarithm of the expected count prior to intervention (pre-intervention period)

·: The additional effect of intervention in the logarithmic scale (that is, the difference in the log number of counts for the post- versus pre-intervention period. Therefore, it is the change relative to pre-intervention and is the effect size. A negative effect size ( ) signifies that the post-intervention period has a reduced number of counts from the pre-intervention period. The percentage change relative to post-intervention can also be computed as

·: The additional effect of time

Finally, an additional question was added under the Confounding domain (C) - Q6. It is important to identify whether studies are implicitly controlled for selection bias (that is, any design features used for this purpose, for example, matching or restriction to a particular subgroup of participants or outcome. The added question reported whether studies restricted participants or outcome selection so that all groups had the same value for the confounder; for example, restricting the study to motorcycle riders or pedestrians only, or, if the study reported outcomes for non-motorised road users and motorised road users, restricting the study to road-traffic injuries only.