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Regulatory and Road Engineering Interventions for Preventing Road Traffic Injuries and Fatalities Among Vulnerable Road Users in Low- and Middle-Income Countries: A Systematic Review

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Low- and middle-income countries have the highest proportions of road accident fatalities among vulnerable road users. This review established the effectiveness of road engineering and the enforcement of traffic laws, and regulation interventions to prevent injury (fatal and non-fatal) to vulnerable road users from low- and middle-income countries. We searched the following databases up to Jan 04, 2018: PubMed; OvidSP Medline, OvidSP Embase, OvidSP Transport, Cochrane Injuries Group Specialized Register, Cochrane Central Register of Controlled Trials, and Proquest ERIC database. In addition, road safety organizations' databases and conference proceedings were hand searched to Jan 2018. Twenty-eight studies were matched to the study inclusion criteria of which we did not analyze six studies assessed as C grade for risk-of-bias. We estimated the effect-size of 18 studies. Four of the studies presented a unique outcome or a study design; it was not possible to calculate a standardized effect-size. The risk-of-bias rating of the studies included for effect-size analysis ranged between A and B grade. There was no evidence that road engineering interventions were effective for road traffic death counts, the number of injuries, and road accident casualty outcomes. While the enforcement of mandatory helmet law was ineffective in reducing road traffic death counts, intervention efforts proved effective in decreasing injuries. Enforcement of mandatory helmet law, automated-enforcement-system (camera), and pedestrian signal interventions were effective in increasing road users' compliance with road safety laws. Daytime running-headlight intervention reduced the number of road accident casualties. The quality of evidence for outcomes was ranked very low. Further research is needed to examine the effects of road engineering interventions on injury severity outcomes. Even though the evidence was of very low quality, traffic laws, and regulation interventions when combined with enforcement initiatives or with, other approaches proved effective in changing drivers' behaviors. Research on road engineering interventions combined with automated-enforcement-systems must be explored in an Low- and Middle-Income Country (LMIC) setting. The review found evidence gaps on the effects of segregation of vulnerable road users from motorized vehicles, changes in intersections, and bicycle infrastructure interventions.

Keywords: enforcement, engineering, road safety, fatality, injury, motorcycle, pedestrian, LMIC

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INTRODUCTION

The UN Decade of Action for Road Safety 2011–2020 Sustainable Development Goals aimed to reduce road traffic deaths by 50% by 2020. While it has made some progress, the number of road traffic deaths and injuries remains high (World Health Organization, 2018). About half of the world's road traffic deaths occur among motorcyclists (23%), pedestrians (22%), and cyclists (5%) also known as "vulnerable road users" (World Health Organization, 2018). Road traffic deaths among vulnerable road users are a major public health problem in low- and middle-income countries (LMICs) where walking, cycling, and motorcycling are predominant transport modes. Pedestrians and cyclists were the largest group of victims of road fatalities in Bangladesh, at 44% (World Health Organization, 2013). In India, 78 and 53% of those killed on the roads in Mumbai and Delhi respectively were pedestrians (Mohan et al., 2009). Some studies have placed pedestrian fatalities in Mexico as high as 48% (Bartels et al., 2010). In Nairobi, Kenya, vulnerable road users accounted for 54% of all road fatalities (Kim and Dumitrescu, 2010). Pedestrians accounted for 43% of all road traffic fatalities in Ghana (World Health Organization, 2013).

Research has shown that traffic speed measures are essential to prevent road injuries among vulnerable road users (Elvik, 2001; Allsop, 2010; Wilson et al., 2010). Bicycle infrastructure reduces injury risks (Reynolds et al., 2009; Lusk et al., 2011; Harris et al., 2013), while regulatory and legislative interventions play a significant role in lessening the severity of injuries (Redelmeirier et al., 2003; Chisholm, 2008; Blanco et al., 2017). Although many LMICs have implemented road safety strategies, compliance and law enforcement are often limited. The common challenges for preventing road traffic injuries among vulnerable road users in LMICs include mixed road conditions, negative perceptions of road safety among non-motorized road users, non-use of helmets or use of non-standard helmets, alcohol, and speed (Vasconcellos, 2001; Nantulya and Reich, 2003). The new regulatory approach called the safe system approach, which focuses on the institutional level deployment of large-scale safety interventions, had a positive effect on road safety in high-income countries. Political will, leadership, active public sector participation, and full support of the private sector and civil societies are considered vital to move in the direction of a safe system approach (ITRAC, 2008; Wegman, 2017). From a public health perspective, the safe system approach framework has several benefits for examining key risk factors for vulnerable road users in LMICs. For instance, two-wheelers are used as the primary mode of transport in many LMICs. Research (International Transport Forum, 2015) shows that the mean age of motorcycle crash victims in LMICs is 25. Other studies have found that the chances of young motorcycle crash victims in the 17–19 years age group from a lower-income group is 2.5 times greater than their peers in higher socioeconomic groups (Huang and Lai, 2011; de Vasconcellos, 2013; International Transport Forum, 2015). The loss of life or disability causes a big economic burden on individuals and households from poorer backgrounds and has a direct negative effect on the national economy.

Nevertheless, institutional level reforms for road safety have received little attention in LMICs. Major barriers include under-reporting of road crash data as well as limited amount of disaggregated data on road crash fatalities, injuries and intermediate outcomes in crash databases, a lack of technical expertise, and a lack of coordination between agencies responsible for road safety management and amongst different levels of governments (International Transport Forum, 2016).

In the light of increasing road traffic deaths and low levels of public investments in road safety measures, international agencies began getting involved in improving road safety conditions in LMICs during the 1990's. 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 organizations, and academic institutions of LMICs. Because of increased sensitization of local and civic organizations, the establishment of national coordinating committees and road safety programs, and some improvements in systems for data collection and analysis, successful road safety intervention implementation practices have emerged (Bishai and Hyder, 2006; Esperato et al., 2012; Commission for Global Road Safety, 2013).

Published reviews on the effects of road infrastructure interventions have focused on interventions for road users in high-income settings (Perel et al., 2007). The lack of studies on road safety from LMICs in systematic reviews could be because of the focus on stand-alone road infrastructure interventions and inclusion criteria to include high-quality study designs only. Furthermore, many of the interventions examined in high-income settings are developed after considerable research and testing. In LMICs, the challenge is to transfer the knowledge gained in developed countries in a fraction of the time. In addition, the interventions need to be tested for cost and design effectiveness as well as the legal system for implementing road infrastructure interventions need to be developed to similar standards practiced in developed countries. LMICs should avoid repeating studies showing the effectiveness of road safety measures in high-income country settings because of contextual differences (Wegman, 2017).

In this context, a review of the evidence of road infrastructure interventions was conducted to provide a quantification of the effects of road safety interventions in LMICs. The review focused on the LMIC context, 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.

The purpose of this review was to measure the effects of road engineering and enforcement of traffic laws and regulation interventions for the prevention of injury (fatal and non-fatal) to vulnerable (non-motorized and motorized two-wheel) road users in low- and middle-income countries. This review included randomized controlled trials and non-randomized study designs and the comparator was no intervention.

METHODS

We reported this systematic review under the Preferred Reporting Items for Systematic Review and Meta-Analyses (PRISMA) Statement (Moher et al., 2009) (**Table S1**). The protocol of the review was published in the Cochrane Database of Systematic Reviews (Gupta et al., 2015).

Types of Studies

The review included randomized-controlled-trials (RCT) and non-randomized study designs: controlled before-and-after, uncontrolled before-and-after, interrupted time-series (ITS), and case-control.

Included studies compared changes in outcomes before and after the intervention implementation with or without a control group. For ITS, studies had a defined point in time when the intervention occurred and included data at least three time-points before and three time-points after the intervention. The intervention implementation was at the population level or the individual level, or both. Participants' selection was by the outcome; hence, outcome reporting was at the individual level.

This review did not include black spot identification studies and modeling studies.

Types of Participants

In this review, "vulnerable road users" included all vulnerable road users —pedestrians, those using non-motorized means of transport (NMT), and those using motorized two-wheelers (motorcycles, mopeds, and light mopeds) inclusive of all ages in LMICs. The World Bank definition of NMT (World Bank, 1993) defines "vulnerable road user" by the amount of protection they have from other motorized traffic. Pedestrians, cyclists, and riders of two-wheelers are unprotected; hence, they are referred to as "vulnerable."

The World Bank definition of LMICs was used to classify low- and middle-income countries. As of July 1, 2018, low-income economies are those with a GNI per capita of $1005 or less; middle-income economies are those with a GNI per capita of more than $1006 but <$3,995.

Types of Interventions

Three broad categories of interventions, road engineering, enforcement of traffic laws and regulation, and a combination of road engineering, and regulatory and legislative interventions were included. Alongside this, a combination of one or more of these interventions as well as with other approaches were considered.

Road engineering interventions are preventive measures involving engineering or structural changes to the road design that affect road user behavior. Road engineering interventions covered in the review comprise three sub-categories: (1) measures for reduction in vehicle-speed, (2) changes in intersections, and (3) segregation of non-motorized road users from motorized vehicles.

Enforcement of traffic laws and regulation interventions refers to setting up road safety rules and ensuring compliance from road users through legal enforcement. Regulation interventions focused on regulatory approaches that can affect large populations through law enforcement such as speed cameras, speed limits, speed zones, red-light enforcement cameras, use of daytime running-lights for two-wheelers, mandatory helmet law, traffic signal regulation, and stop signs.

A combination of road engineering and regulatory interventions includes segregated or on-road marked bicycle lanes involving specific road changes at junctions and intersections, bus rapid transit system with motorcycle lanes, pedestrian crossing, rumble strips on bus rapid transit system, and speed limit enforcement with speed reduction measures.

Comparisons: Comparison of intervention vs. no intervention.

Types of Outcome Measures

This review studies reporting data on at least one of the following primary or secondary outcomes.

Primary Outcomes

(1) Road traffic death counts; (2) Road traffic death rates expressed as rate per 100,000 inhabitants or rate per kilometer of road year; (3) Number or proportion of severe injuries (Injury Severity Score (ISS) > 17, Glasgow Coma Scale (GCS) 9 to 12) and moderate injuries (ISS 8 to 16, GCS 3 to 8); (4) Number or proportion of road accident casualty[1]; and, (5) Number or proportion of disabilities (severe [Glasgow Outcome Scale (GOS) = 4) and moderate (GOS = 3)].

Secondary Outcomes

(1) Compliance; (2) Number or proportion of head-on collisions; (3) Number or proportion of rollovers; (4) Mean-vehicular-speed; and, (5) Mean cost of road crashes.

Outcome data on adverse impacts were extracted from all included studies, if available.

Search Methods for Identification of Studies

Terms describing population and interventions were combined in the final search strategy. The final draft search strategy for bibliographic databases consisted of the following combinations: "Population and Intervention," "Population and Study type," and "Population and Outcomes." The search strategy was adapted for each database; for details, see search strategies for bibliographic databases (**Appendix 1**).

We searched the following bibliographic databases: PubMed; Ovid MEDLINE (1966–2018); EMBASE (1980–2018); ERIC (1966–2018); Cochrane Injuries Group Specialized Register; the Cochrane Central Register of Controlled Trials; TRANSPORT database [(combines records from Transport Research Information Service, International Road Research

[1] According to the Oxford dictionary, second definition of the "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." Included studies have extracted road traffic accident and injury outcome data from police reported road traffic accidents that either required medical assistance or were hospitalized. Thus, the "number or proportion of road accident casualty" defines the number or proportion of road traffic accidents.

Documentation, and TRANSDOC(1980–2018)]; World Bank Impact Evaluation Working Paper Series; Poverty Impact Evaluation database; Development Impact Evaluation (DIME) Database; JOLIS Library catalog; Latin-American and Caribbean Center on Health Sciences Information (LILACS) database; global Transport Knowledge Practice (gTKP); The Institute of Transportation Studies at the University of California; Monash University Accident Research Centre; TRANweb database; and Google Scholar.

In addition, we searched the following road safety organizations' databases: Transport Research Laboratory (TRL), Australian Road Research Board (ARRB), AMEND, African Development Bank (AfDB), Asian Development Bank (ADB), The African Community Access Program (AFCAP), Crash Modification Factors Clearing House, Central Road Research Institute (CRRI), Department for International Development (DFID), International Road Assessment Program (iRAP), International Road Federation (IRF), Global Road Safety Facility (GRSF), Global Road Safety Partnership (GRSP), EMBARQ -The World Resource Institute Center for Sustainable Transport (EMBARQ), Sub-Saharan Africa Transport Policy Program (SSATP), Transport Research and Injury Prevention Program (TRIPP), Organization of Economic Cooperation and Development (OECD), Road Engineering Association of Asia and Australia (REAAA), Road Traffic Injury Research Network (RTIRN).

Further to this, we checked reference lists of selected papers and we hand-searched conference proceedings. We have considered 1990 as a cut-off for the identification of studies analyzing road infrastructure interventions.

Data Collection and Analysis

A team of two authors have screened the titles and abstracts and identified eligible studies independently. The full text of all eligible studies was obtained. Based on the study inclusion criteria, two authors conducted a full-text review for eligibility independently, and study inclusion differences were reconciled by a discussion between the two authors.

Full text of studies was screened in detail, and data from each study were extracted by one review author and checked by a second review author. Disagreements were resolved by discussion.

Where data were missing; we attempted to obtain the data from the authors. If we were unable to obtain missing data, we analyzed only the available data.

Assessment of Risk of Bias in Included Studies

The quality of all studies (RCTs and non-randomized studies) were assessed using the Hamilton Assessment Tool (HAT) (Thomas, 1998). We proposed some modifications under the "Confounding" domain of the HAT tool to ensure that it is appropriate for studies of road infrastructure interventions. Please see the Hamilton Assessment Tool (**Appendix 2**).

Two independent review authors assessed the quality of all studies using the HAT tool. Each component was first assigned a global rating (Strong = 1, Moderate = 2, Weak = 3) indicating the overall potential for bias in each component. Each study was assigned an overall risk of bias assessment rating. The criteria for an overall risk of bias assessment rating of a study were as follows: studies with no weak ratings were assessed as A grade, studies with one or two weak ratings were assessed as B grade. And, studies having very serious (more than two weak ratings) on study design, confounders, and data collection methods used were excluded as C grade studies. Intervention integrity and analysis domain were not included in the assessment of the overall risk of bias ratings. Disagreements in the assessment of bias were resolved through discussion between the two review authors.

Based on the HAT tool, studies assessed with low risk of bias (that is two or more studies with an overall grade of A or B) were quantitatively synthesized using meta-analysis. We excluded C grade studies from meta-analysis and narrative synthesis.

Measures of Treatment Effect

For uncontrolled before-and-after studies, we presented the dichotomous outcome data[2] using Odds Ratio (OR) with 95% confidence intervals (CI). For controlled before-and-after studies, we extracted the dichotomous outcome data for treatment and control sites and calculated the ratio of the differences in event rates before and after intervention in the treatment site by a corresponding difference in the control site.

For uncontrolled before-and-after studies, we presented the continuous outcome data[3] by calculating the mean difference with 95% CI. For continuous outcome data in controlled before-and-after studies, we calculated the difference in outcome data before and after intervention in the intervention site by the corresponding difference in the control site. We used mean differences (MDs) with 95% CI when the same scale was used and standardized mean differences (SMDs) with 95% CI when a different scale was used in studies. For time-series studies, we extracted the outcome data for calculating risk ratios (RR) using R software. The studies considered in this meta-analysis have road traffic death count or accident casualties as an outcome. The outcome being a count, a Poisson regression model was used to estimate the effects of intervention after adjusting for a linear trend in time.

We assessed statistical heterogeneity using the Chi^2 and I^2 statistics. A meta-analysis of effect sizes was conducted using a random-effects model.

Unit of Analysis

Road engineering and legislative interventions were implemented to individual roads meeting pre-specified criteria or to all roads within a targeted community or a geographical

[2]In before-and-after studies with dichotomous outcome data, the individual study findings are displayed as "n/N," whereby: n = the number of outcome events (e.g., fatality or helmet use compliance count) in the after intervention period (intervention group) or before intervention period (control group), and N = the total number of outcome events in the interventions group or control group.

[3]In before-and-after studies with continuous outcome data, the individual study findings are given as "N" and "mean (SD)," whereby N = the total number of outcome events (e.g., road accident causality or red-light running count) in the after intervention period (intervention group) or before intervention period (control group) and mean SD = the arithmetic mean and standard deviation (SD) of the outcome measure in either the intervention or control group.

area. However, the reporting of outcomes was done at individual levels. In cases where a study had multiple intervention groups, data from all relevant intervention groups of the study were combined into a single group, and data from all relevant control groups were combined into a single control group.

Data Synthesis

For data synthesis, studies were grouped by intervention categories: road engineering, enforcement of traffic laws and regulations, and a combination of road engineering and enforcement of traffic laws and regulations. Under these categories, studies were grouped by randomized and non-randomized study designs. The data from randomized and non-randomized studies were presented by (1) primary outcomes: road traffic death counts, number of injuries (moderate and severe), and number of road accident casualty; and (2) secondary outcome: compliance.

We did not synthesize data from road infrastructure and enforcement of traffic laws and regulations intervention categories together. We reported meta-analysis results of randomized control trials and non-randomized studies separately.

If there was extreme heterogeneity, we presented the effect sizes of those studies graphically using forest plots, and a narrative synthesis of the data were presented according to the ESRC guidance (Popay et al., 2006). All the data to be synthesized were predominantly quantitative; 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, and (2) analyzing absolute measure of effect. Two software packages were used for meta-analysis. The RevMan was used for uncontrolled before-and-after and controlled before-and-after studies. Effect sizes for time-series studies were computed using R Statistical Software (**Appendix 3**) because RevMan is not suitable for meta-analyzing data for time-series studies.

Sensitivity Analysis

Before making decisions about which studies should be included in the final syntheses, a sensitivity analysis was conducted to examine variation in reported effects by study characteristics. The key study characteristics used for sensitivity analysis were: in between-study heterogeneity, missing data, and the overall study quality grade assigned.

We considered a sensitivity analysis about the influence of small study effects on the result of the meta-analysis when there was evidence of between-study heterogeneity. We compared the fixed and random-effects estimates of the intervention effect. We performed a sensitivity analysis by excluding studies of the lowest quality.

We examined funnel plots and cumulative meta-analysis to assess the potential for publication bias in the included studies.

GRADE and summary of findings tables

A team of two authors was involved in conducting GRADE assessments. GRADE domains, risk-of-bias, indirectness, inconsistency, imprecision of results were primarily considered for assessing the quality of evidence.

Patient and Public Involvement

The review did not involve any patients or the public.

RESULTS

Results of Search

The systematic search was conducted in January 2018. We identified 30,030 study records through database searching. Twenty-eight studies matched the study inclusion criteria; the search flow chart (**Figure 1**) details the search process.

Included Studies

Twenty-eight studies matching study inclusion criteria were identified. Twenty-two studies were included for analysis and six studies were not analyzed because the outcome data did not qualify for analysis (**Figure 1**).

Study Characteristics

Notably, 21 studies belonged to middle-income countries and one study belonged to low-income countries.

Study designs included one cluster randomized-control-trial (Sumner et al., 2014), one case-control (Quistberg et al., 2014), one controlled before-and-after (Liu et al., 2011), five time-series (Radin Umar et al., 1995a,b; Radin Umar, 2005; Espitia-Hardeman et al., 2008; Nadesan-Reddy and Knight, 2013) and 14 uncontrolled before-and-after studies (Panichaphongse et al., 1995; Chiu et al., 2000; Liberatti et al., 2001; Afukaar, 2003; Ichikawa et al., 2003; Bastos et al., 2005; Hoque et al., 2005; Bhatti et al., 2011; Antic et al., 2013; Lipovac et al., 2013; Nguyen Ha et al., 2013; Allyana et al., 2014; Van der Horst et al., 2016; Nhan et al., 2017).

There were seven studies on the impact of road engineering interventions (Radin Umar et al., 1995b; Afukaar, 2003; Hoque et al., 2005; Liu et al., 2011; Antic et al., 2013; Nadesan-Reddy and Knight, 2013; Van der Horst et al., 2016). Fourteen studies reported the impact of enforcement of traffic laws and regulation interventions (Panichaphongse et al., 1995; Radin Umar et al., 1995a; Chiu et al., 2000; Liberatti et al., 2001; Ichikawa et al., 2003; Bastos et al., 2005; Radin Umar, 2005; Espitia-Hardeman et al., 2008; Bhatti et al., 2011; Lipovac et al., 2013; Nguyen Ha et al., 2013; Allyana et al., 2014; Sumner et al., 2014; Nhan et al., 2017).

In addition, one study (Quistberg et al., 2014) included a combination of regulatory and road engineering interventions.

All studies examined one or more of the primary or secondary outcomes. Some studies reported only primary outcome measures: road traffic death counts, the number of injuries (moderate and severe), and the number of road accident casualty. Some studies reported only secondary outcome measures: compliance and mean-vehicular-speed.

One study (Allyana et al., 2014) presented outcome data for two sub-categories of enforcement of traffic laws and regulation interventions separately. Outcome data were extracted and assessed as they related to the sub-category automated-enforcement-system (camera)—red-light regulation

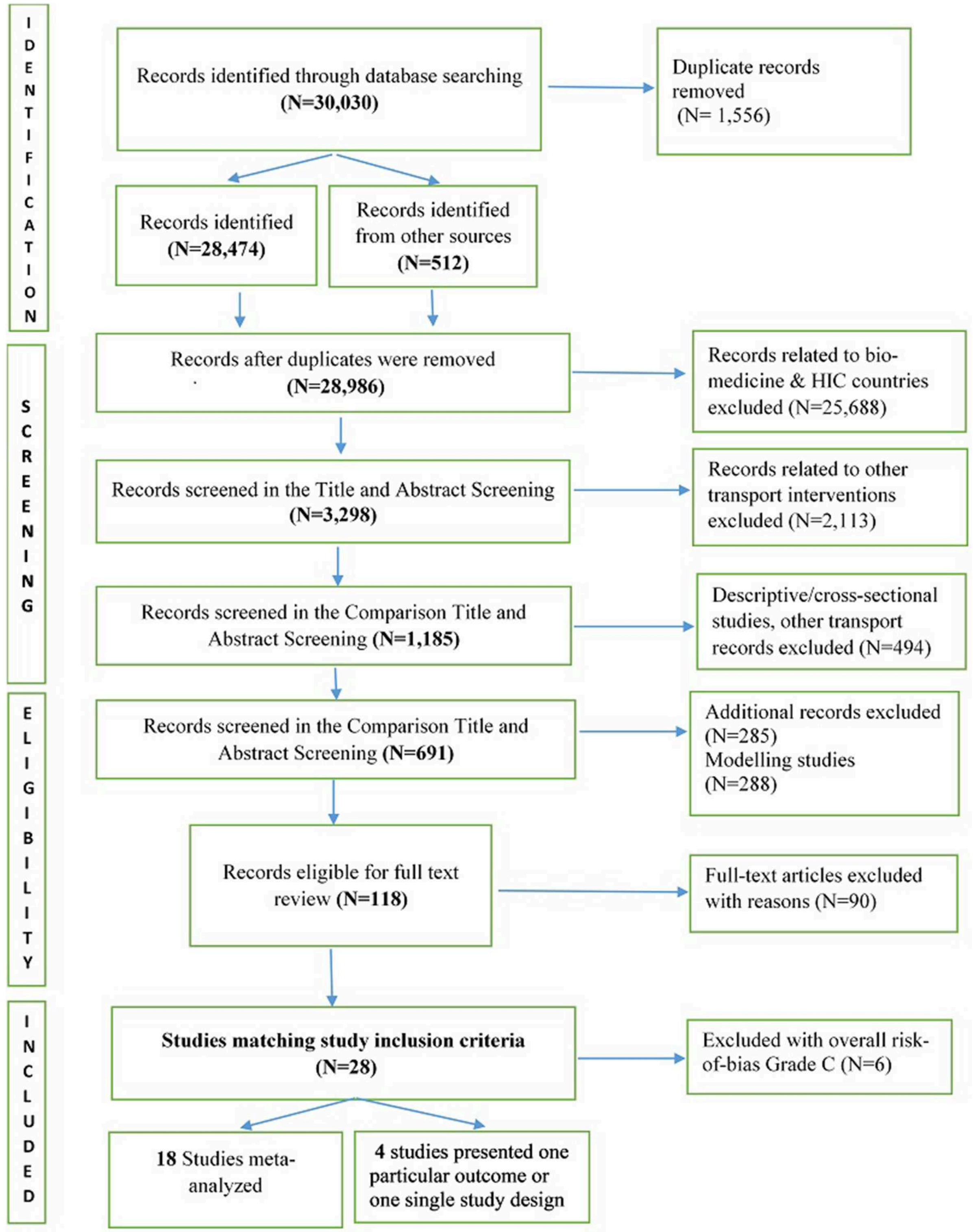


FIGURE 1 | Search flow chart.

into one group, and as they related to the sub-category automated-enforcement-system (camera)—speed limit into another group.

One study (Hoque et al., 2005) evaluated controlled before-and-after analysis of safety improvements at three black spots, and before-and-after analysis of road engineering safety measures at two different site segments. Outcome data were extracted and assessed for the before-and-after analysis of road engineering safety measures only.

Included studies did not report the following primary and secondary outcome measures: moderate and severe disabilities (primary outcomes), the number, or proportion of rollovers, and mean cost of road crashes (secondary outcomes).

One study (Espitia-Hardeman et al., 2008) reported the outcome, the number of road traffic death rates expressed as the rate per 100,000 inhabitants. One before-and-after study (Afukaar, 2003), one case-control study (Quistberg et al., 2014), and one time-series study (Radin Umar et al., 1995b) reported the number of head-on collisions. One study (Nadesan-Reddy and Knight, 2013) reported pre-specified adverse outcome, increase in the collision between motor vehicles because of traffic calming measure speed humps. Because of an insufficient number of studies, we did not meta-analyze these outcome results.

For characteristics and the role of the funding source of studies analyzed, see the summary table of characteristics of studies included for meta-analysis (**Table 1**).

Risk-of-Bias Assessment

The majority of the studies included in this review are observational and therefore subject to risk-of-bias in terms of making a causal inference of the effect of road safety interventions.

Selection Bias

Intervention sites in almost all studies were somewhat or most likely to represent the target population. Based on HAT ratings, studies ranked between strong and moderate for selection bias.

Study Design

Based on HAT ratings for study design criteria, one cluster RCT was rated strong for study design. Seven studies having time-series, controlled before-and-after, and case-control study designs were rated moderate. Time-series studies were short period time-series studies using monthly data extending at the most 1 year before and 1 year after the intervention. The controlled before-and-after study reported controlling for confounders by matching treatment and comparison sites on geographic characteristics of roads/location and the same period. The case-control study observed a short time-period for pedestrian and vehicle-flow, but the collision dates and data collection did not happen contemporaneously. However, the study explored the impact on the time difference and found no significant changes.

Confounders

All before-and-after studies adequately controlled for some essential confounders related to road infrastructure interventions such as 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-motorized road users or having road traffic injuries only). 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 improvement interventions is influenced by the high accident rates (Elvik, 2002).

Data Collection

All studies described proper data collection methods. Overall, tools for primary and secondary outcome data were valid and reliable. All studies reported outcomes at the individual levels. Traffic laws enforcement intervention studies on motorized two-wheel road users reported outcomes for motorized two-wheel road users only.

Overall Risk-of-Bias

The overall risk-of-bias using the HAT ratings for one cluster RCT (Sumner et al., 2014), five time-series studies (Radin Umar et al., 1995a,b; Radin Umar, 2005; Espitia-Hardeman et al., 2008; Nadesan-Reddy and Knight, 2013), one controlled before-and after-study (Liu et al., 2011), and one case-control study (Quistberg et al., 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 14 studies (Panichaphongse et al., 1995; Chiu et al., 2000; Liberatti et al., 2001; Afukaar, 2003; Ichikawa et al., 2003; Bastos et al., 2005; Hoque et al., 2005; Bhatti et al., 2011; Antic et al., 2013; Lipovac et al., 2013; Nguyen Ha et al., 2013; Allyana et al., 2014; Van der Horst et al., 2016; Nhan et al., 2017) with a moderate risk-of-bias assessment. These studies used an uncontrolled before-and-after study design. In some uncontrolled before-and-after studies, the post-intervention period started soon after the intervention and included just one period before and one period after; therefore, studies controlled for general changes and change of traffic volume confounding variables.

We have excluded six studies that have overall risk-of-bias grade C because the outcome data did not qualify for analysis because of high risk-of-bias around study design, data collection, and confounders (Mutto et al., 2002; Passmore et al., 2010; Yuan et al., 2012; Wu et al., 2013; Yuan and Chen, 2013; Mousa et al., 2014).

Table 2 provides an assessment of risk-of-bias and the overall risk-of-bias grade of A, B, and C.

Effects of Interventions

Effect sizes were computed for 18 studies. Four studies (Liu et al., 2011; Antic et al., 2013; Quistberg et al., 2014; Sumner et al., 2014) presented one particular outcome or one single study design. The number of studies was not sufficient to calculate a standardized effect size.

Road Engineering Intervention Compared to No Intervention

Uncontrolled before-and-after studies

Three uncontrolled before-and-after studies assessed primary outcomes: road traffic death counts, the number of injuries (moderate and severe), and the number of road accident casualty before and after road engineering interventions.

TABLE 1 | Characteristics of studies included for meta-analysis.

Short Title	Methods	Intervention	Outcomes
1. Afukaar, 2003	**Uncontrolled before-and-after**	**Road engineering *(measures for reduction of vehicle speed)***	
This study analyzed the police-reported crash and injury data on high-risk roads vulnerable to road accidents. The annualized average before the crash situation was compared with the average after intervention installation in order to ascertain the effectiveness of the measure.		Rumble strips at Suhum Junction on the main Accra-Kumasi highway **[Sample size not available]**. **Population:** Non-motorized road users at the Suhum Junction, Ghana.	Road traffic death counts; Number of injuries; Number of road accident casualty.
Primary study for data extraction; there was no funding source for this study.			
2. Allyana, 2014	**Uncontrolled Before and After**	**Enforcement of traffic laws and regulations *(automated-enforcement-system: cameras)***	
Speed limit study: A comparison of speed limit violations before the intervention and after intervention periods was made to determine speed changes and speed limit compliance. **Red light regulation study**: The red-light violations before speed camera installation were compared to those obtained after installation along with each individual approach of RLC on a lane-by-lane basis.		The Malaysia Road Transport Rules 1997 prohibits red-signal crossing. The National Speed Limit Orders were implemented on 1 February 1989. Speed limits enforcement **[Sample size: 31,580 vehicles observed]**; Red light enforcement of all vehicles **[Sample size 331,154 vehicles observed].** **Population:** Motorized two-wheel road users on six locations in the Malaysia Peninsular.	Non-compliance–red light regulation; Non-compliance–speed limit; Mean-vehicular speed.
Primary study for data extraction; No funding source for this study. This study presented outcomes for two sub-categories of traffic laws enforcement and regulations. Outcome data were extracted and assessed as they related to the automated-enforcement-system (AES)—red-light compliance into one group, and the AES—speed-limit compliance into another group.			
3. Antic, 2013	**Uncontrolled before and after**	**Road engineering intervention *(measures for reduction of vehicle speed)***	
In this study, vehicle speed measurements before speed bumps were installed and 1-day and 1-month after the installation were taken in order to determine the effects of speed bumps on the motor-vehicle speed at three locations on roads having a large number of pedestrian and other vulnerable road users.		On each location, two-speed bumps, 50 m away from each other, were installed. **[Sample size: speed of 5,182 vehicles were observed]**. **Population:** Non-motorized road users in Belgrade, Serbia.	Mean-vehicular-speed km/h.
Primary study for data extraction; this paper was supported by the Ministry of Science and Technological Development of the Republic of Serbia.			
4. Bastos, 2005	**Uncontrolled before-and-after**	**Enforcement of traffic laws and regulations (*mandatory helmet law)***	
This study evaluated the enforcement of the Brazilian traffic code that mandates the use of helmets and seat belts of motorized road users in an urban setting. The data were obtained from the Integrated Service of Trauma and Emergency Care for the pre- and the post-intervention period.		The Brazilian traffic code was established in January 1998 specifically to the mandatory use of helmets and seat belts for motorized road users. **[Sample size for helmet use: 6,298 motorcyclists]**. **Population:** Motorcycle road users in Londrina, Brazil.	Compliance–mandatory helmet law.
Primary study for data extraction; Funding from CAPES (Coordenação de Aperfeic,oamento de Pessoal de Nível Superior).			

(Continued)

TABLE 1 | Continued

Short Title	Methods	Intervention	Outcomes
5. Bhatti, 2011	**Uncontrolled before-and-after**	**Enforcement of traffic laws and regulations *(mandatory helmet law)***	
This study evaluated the nationwide enforcement of National Highway Safety Ordinance 2000, which mandates helmet use and use of seat belts. A pre-enforcement awareness exercise was conducted between Dec. 2009 and Feb 2010. Seat belt and helmet wearing were observed on high-risk roads vulnerable to road accidents.		The National Highway Safety Ordinance established in 2000 is specific to the mandatory helmet use for motorcycle road users and the mandatory seat belt use for drivers and front-seat occupants. [**Sample size: 742 motorcyclists; 295 pillion riders]**. **Population:** Motorcycle road users on Karachi-Hala Highway, Pakistan.	Compliance–mandatory helmet law by motorcyclists and pillion riders.
Primary study for data extraction; No funding source for this study.			
6. Chiu, 2000	**Uncontrolled before-and-after**	**Enforcement of traffic laws and regulations *(mandatory helmet law)***	
This study evaluated the effect of motorcycle helmet law in an urban setting. Data on head injury were collected for the year before the intervention and after implementation of the helmet use law. The data were collected from 56 major teaching hospitals in Taiwan. Patients dead on arrival and non-hospitalized patients were excluded.		The motorcycle helmet law was implemented in Taiwan, on June 1, 1997 **[Sample size: 8,795 cases of motorcycle-related head injuries]**. **Population:** Motorcycle road users in Taiwan.	Road traffic death counts; Number of injuries; Compliance–mandatory helmet law.
Primary study for data extraction; This study was supported by grant NSC88-2314-B-038-132 from the National Science Council and grant DOH87-TD-1040 from the Department of Health, Taiwan			
7. Espitia-Hardeman, 2008	**Time-series**	**Enforcement of traffic laws and regulations (mandatory helmet law and mandatory use of reflective vests)**	
Time-series analysis was performed to assess the effects of motorcycle rider safety interventions in an urban community. The study had a defined point in time when the intervention occurred and included data at least three time points before and after the intervention.		The enforcement of the mandatory helmet law was established in August 1996; the use of helmets for both drivers and passengers of motorcycles in November 1997; and the use of reflective vests in April 2001 **[Sample size: not available].** **Population:** Motorcycle road users in Cali, Colombia.	Road traffic death counts.
Primary study for data extraction; there was no funding source for this study.			
8. Hoque, 2005	**Uncontrolled before-and-after**	**Road engineering (*changes at intersection; segregation of VRU from motorized road users)***	
This study examined the effectiveness of road engineering interventions before and after the implementation of interventions on high-risk roads. This study consists of a controlled before-and-after analysis of road engineering interventions at three black spots and uncontrolled before-and-after analysis of road engineering interventions at two different site segments. Only the later was analyzed.		The improvement measures included pavement widening, installation of a median barrier and other safety-related features, such as installation of pavement markings and signs and safety guard posts at the bridge approaches **[Sample size: not available]**. **Population:** Non-motorized road users on the Dhaka-Arica Highway, Bangladesh.	Road traffic death counts; Road accident casualty.
Primary study for data extraction; No funding source for this study.			

(Continued)

TABLE 1 | Continued

Short Title	Methods	Intervention	Outcomes
9. Ichikawa, 2003	**Uncontrolled before-and-after**	**Enforcement of traffic laws and regulations *(mandatory helmet law)***	
This study investigated the effect of the helmet act on increasing mandatory helmet use and reducing motorcycle-related deaths and injuries in a rural community in Thailand. Helmet use in a motorcycle accident was compared 2-years before and after enforcement of the helmet act.		The helmet act for motorcyclists was enacted in Thailand in December 1994 and subsequently enforced at the regional level **[Sample size: 12,002 injured motorcyclists]**. **Population:** Motorcycle road users in northeast Thailand.	Road traffic death counts; Number of injuries; Compliance–mandatory helmet law.
Primary study for data extraction; No funding source for this study.			
10. Liberatti, 2001	**Uncontrolled before-and-after**	**Enforcement of traffic laws and regulations *(mandatory helmet law)***	
This study evaluated the enforcement of the Brazilian traffic code of January 1998 that mandates the use of helmets and seat belts among motorized road users in an urban setting. The data were obtained from the Integrated Service of Trauma and Emergency Care before and after periods.		The enforcement of the Brazilian traffic code enacted in January 1998 is specific to the mandatory use of helmets and seat belts for motorized road users. **[Sample size for motorcycle victims: 747 in before period; 1090 in after-intervention period].** **Population:** Motorcycle road users in Londrina, Brazil.	Compliance–mandatory helmet law.
Primary study for data extraction; No funding source for this study.			
11. Lipovac, 2013	**Uncontrolled before-and-after**	**Enforcement of traffic laws and regulations *(red-light-running)***	
This study investigated pedestrians' compliance with red-light regulation at traffic signals at two different pedestrian crossing in an urban setting. Pedestrian observance of traffic light signals before and after the pedestrian countdown intervention.		The former Yugoslavia signed an international traffic Convention (Geneva, 1947), a signatory to the Vienna Convention (Vienna, 1968). A swinging red light constitutes a stop signal for road users. **[Sample size: 20,227 pedestrians].** **Population:** Non-motorized road users in Doboj city.	Compliance–running-red-light violation
Primary study for data extraction; No funding source for this study.			
12. Liu, 2011	**Controlled before-and-after**	**Road engineering intervention *(measures for reduction of vehicle speed)***	
Controlled before-after and Empirical Bayesian (EB) methods were used to evaluate the effectiveness of transverse rumble strips at pedestrian crosswalks in a rural setting. At each treated site, point speed data were measured at 23 selected locations with different distances from pedestrian crosswalks. At each control site, speed data were measured at the pedestrian crosswalk as well as upstream and downstream control points.		Raised rumble strips deployed on both approaches of a signalized pedestrian crosswalk on a rural road **[Sample size: For road crass 366 roadway segments; For speed 15,000speed observations].** **Population:** Non-motorized road users in Guangdong, China.	Number of road accidents casualty.
Primary study for data extraction; this research was funded by China's National Science and Technology, China's National Science Foundation, and Excellent Young Faculties Program, Southeast University.			

(Continued)

TABLE 1 | Continued

Short Title	Methods	Intervention	Outcomes
13. Nadesan-Reddy, 2013	**Time-series**	**Road engineering intervention *(measures for reduction of vehicle speed)***	
This study assessed the vehicle and pedestrian-vehicle collision data on high-risk roads vulnerable to road accidents (school route) over a 5-year period following the implementation of speed humps. The study had a defined point in time when the intervention occurred and included data three-time points before and three-time points after the intervention.		Traffic calming humps in the Chatsworth and KwaMashu residential areas of the eThekwini Municipality **[Sample size: Pre−5,911, Post−6,228].** **Population:** Non-motorized road users in the eThekwini province, South Africa.	Road traffic death counts; Number of injuries; Number of road accident casualty.
Primary study for data extraction; No funding source for this study.			
14. Nhan, 2017	**Uncontrolled before-and-after**	**Enforcement of traffic laws and regulations *(mandatory helmet law)***	
This study evaluated the effects of the Vietnamese Child Helmet Action Plan to mandate helmet use among school-going children passengers and adult drivers in an urban setting. The helmet use was measured by filmed observations of drivers and passengers as they arrived or left their schools.		In Vietnam, the mandatory helmet use law for adult motorcycle drivers was introduced in 2007 **[Sample size: 124,366 motorcycle riders].** **Population:** Motorcycle road users in Ha Naoi, Da Nang, and Ho Chi Minh cities in Vietnam.	Compliance–mandatory helmet law.
Primary study for data extraction. No funding source for this study			
15. Nguyen, 2013	**Uncontrolled before-and-after**	**Enforcement of traffic laws and regulations *(mandatory helmet law)***	
This study evaluated mandatory use of helmet among motorcycle drivers and pillion riders in three urban provinces in the Socialist Republic of Viet Nam, before and after the mandatory helmet law enforcement.		The mandatory helmet law took effect in Vietnam on December 2007 **[Sample size: 665,428 motorcycle riders].** **Population:** Motorcycle road users in Yen Bai, Da Nang, and Binh Duong provinces in Vietnam.	Compliance–mandatory helmet law.
Primary study for data extraction; this study was funded by Bloomberg Philanthropies.			
16. Panichaphongse, 1995	**Uncontrolled before-and-after**	**Enforcement of traffic laws and regulations (*mandatory helmet law)***	
This study compared the death rates two years before the helmet use decree and two years after its enforcement in an urban setting. The population included persons injured from MCA who were treated at Chulalongkorn Hospital and those who died because of MCA between 1991 and 1994.		The mandatory use of helmet by motorcyclists and pillion riders law was promulgated on 16 Dec. 1992 in Bangkok Metropolitan area **[Sample size: 4,035 injured motorcycle accidents]**. **Population:** Motorcycle road users in Bangkok Metro area.	Road traffic death counts; Number of injuries; Number of road accident casualty.
Primary study for data extraction; this study was supported by the Bangkok General Hospital.			

(Continued)

TABLE 1 | Continued

Short Title	Methods	Intervention	Outcomes
17. Quistberg, 2014	**Case-control study**	**Combination of engineering and regulatory and legislative (*traffic enforcement)***	
A matched case-control design was used where the units of study were crossing locations in an urban setting in Lima. 97 control sample collisions (weighted *N* = 1,134) at intersections in Lima were randomly selected. Controls were pedestrian crossings in the proximity of matched case sites.		Visible traffic signals, pedestrian signals, and signal timing **[Sample size: 97 control-matched sample collisions]**. **Population:** Non-motorized road users in Lima, Peru.	Number of accident casualty.
Primary study for data extraction; the study was funded by the Thomas Francis, Jr. Global Health Fellowship from the Department of Global Health, University of Washington.			
18. Radin Umar, 1995a	**Time-series**	**Enforcement of traffic laws and regulations *(conspicuity -use of daytime-running-headlights)***	
This study presented a six months before-and-after analysis of the impact of running headlight. A nationwide daytime 'running-headlight' campaign was conducted, followed by an establishment of the daytime 'running-headlight' regulation. The study had a defined point in time when the intervention occurred and included monthly data at least three time points before and after the intervention.		The running headlight regulation was established in September 1992 **[Sample size: 3,662 motorcycle accidents].** **Population:** Motorcycle road users in the Seremban and Shah Alam in Malaysia.	Number of road accident casualty.
Primary study for data extraction; there was no funding source for this study.			
19. Radin Umar, 1995b	**Time-series**	**Road engineering intervention *(segregation of vulnerable road users from motorized vehicles)***	
This study presented an analysis of the impact of segregation of a motorcycle lane on high-risk roads vulnerable to road accidents. Accident data was extracted from the four-year pilot project data. The time series cumulative plot'2 of six months records before the intervention and after the intervention. The study had a defined point in time when the intervention occurred and included monthly-data at least three time points before and after the intervention.		Motorcycles segregation from other traffic using an exclusive motorcycle lane **[Sample size: 4,319 motorcycle accidents].** **Population:** Motorcycle road users along Federal Highway F02, Shah Alam, Malaysia.	Number of road accident casualty; Number of injuries.
Primary study for data extraction; No funding source for this study.			
20. Radin Umar, 2005	**Time-series**	**Enforcement of traffic laws and regulations (*conspicuity -use of daytime-running- lights)***	
This study evaluated the effect of the daytime-running-headlight regulation. A before and after evaluation of the safety intervention from the two-year accident series monthly data was conducted. The study had a defined point in time when the intervention occurred and included monthly data at least three time points before and after the intervention.		The running headlight regulation was established in September 1992 **[Sample size: 4,865 motorcycle accidents].** **Population:** Motorcycle road users in the Seremban and Shah Alam, Malaysia.	Number of road accident casualty; Number of injuries.
Primary study for data extraction; there was no funding source for this study.			

(Continued)

TABLE 1 | Continued

Short Title	Methods	Intervention	Outcomes
21. Sumner, 2014	**Cluster randomized-control-trial**	**Enforcement of traffic laws and regulations *(conspicuity measures)***	
A cluster-randomized trial was conducted among 180 motorcyclist-taxi drivers in an urban city in northern Tanzania. Participants from the intervention arm (90) received a free reflective/fluorescent vest and participants form the control arm (82) did not receive free vests.		The Transport Licensing (Motor Cycles and Tricycles) Regulations, 2010 mandates the use of reflective/fluorescent vests for motorcycle-taxi drivers in Moshi, Tanzania **[Sample size: 180 motor-cycle drivers].** **Population:** Motorcyclists in the Kilimanjaro region.	Compliance–motorcycle conspicuity.
Primary study for data extraction.			
22. Van der Horst, 2016	**Uncontrolled before-and-after**	**Road engineering intervention *(measures for reduction of vehicle speed)***	
This before-and-after study used a combination of research methods to monitor and evaluate speed humps, rumble strips, and road markings on high-risk roads vulnerable road accidents on the N2 national highway.		Speed management related road engineering interventions: speed humps, rumble strips, and road markings **[Sample size: Before: 5,966; After period: 5,964 traffic counts].** **Population:** Non-motorized road users on N2 highway in Bangladesh.	Number of road traffic accidents; Number of injuries; Road traffic death counts.
Primary study for data extraction. This study was funded by GRSF supported RTIRN small grants program.			

Primary outcomes. Three studies (Afukaar, 2003; Hoque et al., 2005; Van der Horst et al., 2016) assessed road traffic death counts before and after road engineering interventions. There was no evidence that road engineering interventions were effective (OR 1.63; 95% CI 1.01–2.63; $I^2 = 0\%$) based on data from 948 people. Results were analyzed using a random-effects model, and the outcome was downgraded to very low quality. See forest plot, primary outcome: changes in "road traffic death counts" after road engineering interventions (**Figure 2**). Three studies (Afukaar, 2003; Hoque et al., 2005; Van der Horst et al., 2016) assessed the number of injuries and found no difference in the odds of occurrence of injuries before and after road engineering interventions (OR 0.76, 95% CI 0.47–1.23; $I^2 = 36\%$) based on data from 948 people. The results were analyzed using a random-effects model, and the outcome was downgraded to very low quality. See forest plot, primary outcome: changes in the number of "injuries" after road engineering interventions (**Figure 3**).

Two studies (Afukaar, 2003; Van der Horst et al., 2016) assessed the number of road accident casualties before and after road engineering interventions. There was no evidence that road engineering interventions were effective after a road treatment (OR, 2.31; 95% CI 0.57–9.35; $I^2 = 77\%$) based on data from 718 people. Results were analyzed using a random-effects model, and the outcome was downgraded to very low quality because heterogeneity was significant. See forest plot, primary outcome: changes in the number of "road accident casualty" after road engineering interventions (**Figure 4**).

Secondary outcomes. We assessed no secondary outcomes for road engineering interventions in before-and-after studies.

Time-series studies

Two time-series studies were used to assess primary outcomes, road traffic death counts, and the number of road accident casualty before and after road engineering interventions.

Primary outcomes. One study (Nadesan-Reddy and Knight, 2013) reported road traffic death counts. However, because of an insufficient number of studies the outcome data for road traffic death counts were not assessed.

Two studies (Radin Umar et al., 1995b; Nadesan-Reddy and Knight, 2013) assessed the number of road accident casualty in percent change (-26.62, 95% CI—149.58 - 35.75;—111.80, 95% CI—492.97 - 24.34) based on data from 14,404 people involved in accidents after road engineering interventions. There was no difference in the occurrence of road accident casualty outcome before and after road engineering interventions. The outcome was downgraded as very low quality because of the high presence of heterogeneity. See forest plot, primary outcome: percent change in the number of "road accident casualty" after road engineering interventions (**Figure 5**).

Secondary outcomes. We assessed no secondary outcomes for road engineering interventions in time-series studies.

TABLE 2 | Assessment of risk-of-bias of studies included for analysis based on HAT.

	Short title	Selection bias	Study design	Confounder	Data collection methods	Intervention integrity	Analysis of internal validity	Overall risk-of-bias	Grade
Assessment of overall risk-of-bias									
1	Afukaar, 2003	Strong	Weak	Moderate	Strong	Moderate	Strong	Moderate	B
2	Allyana et al., 2014	Moderate	Weak	Moderate	Strong	Moderate	Strong	Moderate	B
3	Antic et al., 2013	Strong	Weak	Moderate	Strong	Strong	Strong	Moderate	B
4	Bastos et al., 2005	Moderate	Weak	Moderate	Strong	Strong	Strong	Moderate	B
5	Bhatti et al., 2011	Strong	Weak	Moderate	Strong	Moderate	Strong	Moderate	B
6	Chiu et al., 2000	Strong	Weak	Moderate	Strong	Strong	Strong	Moderate	B
7	Espitia-Hardeman et al., 2008	Moderate	Moderate	Moderate	Strong	Moderate	Strong	Strong	A
8	Hoque et al., 2005	Strong	Weak	Moderate	Strong	Moderate	Strong	Moderate	B
9	Ichikawa et al., 2003	Moderate	Weak	Moderate	Strong	Strong	Strong	Moderate	B
10	Liberatti et al., 2001	Moderate	Weak	Moderate	Strong	Moderate	Strong	Moderate	B
11	Lipovac et al., 2013	Strong	Weak	Strong	Strong	Strong	Strong	Moderate	B
12	Liu et al., 2011	Strong	Moderate	Moderate	Strong	Strong	Strong	Strong	A
13	Nadesan-Reddy and Knight, 2013	Moderate	Moderate	Moderate	Strong	Strong	Strong	Strong	A
14	Nhan et al., 2017	Strong	Weak	Moderate	Strong	Moderate	Strong	Moderate	B
15	Nguyen Ha et al., 2013	Strong	Weak	Moderate	Strong	Moderate	Moderate	Moderate	B
16	Panichaphongse et al., 1995	Strong	Weak	Moderate	Strong	Moderate	Strong	Moderate	B
17	Quistberg et al., 2014	Strong	Moderate	Strong	Strong	Moderate	Strong	Strong	A
18	Radin Umar et al., 1995a	Strong	Moderate	Moderate	Strong	Moderate	Strong	Strong	A
19	Radin Umar et al., 1995b	Strong	Moderate	Moderate	Strong	Moderate	Strong	Strong	A
20	Radin Umar, 2005	Strong	Moderate	Moderate	Strong	Strong	Strong	Strong	A
21	Sumner et al., 2014	Strong	Strong	Strong	Strong	Moderate	Strong	Strong	A
22	Van der Horst et al., 2016	Strong	Weak	Strong	Strong	Moderate	Strong	Moderate	B
Assessment of confounding variables									
	Short title	**Q1–What was the basis for the selection of intervention sites -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 transitional period following the infrastructure construction?**	**Q6–Whether the study controlled for restricted participant selection?**	**Percentage of relevant confounders controlled or adjusted?**	**Potential for risk-of-bias**
1	Afukaar, 2003	Very likely	Yes	Yes	No	Can't tell	Can't tell	60–79%	Moderate
2	Allyana et al., 2014	Very likely	Yes	Yes	No	N A	Yes	60–79%	Moderate
3	Antic et al., 2013	Not likely	Yes	Yes	No	Yes	Can't tell	60–79%	Moderate
4	Bastos et al., 2005	Not likely	Yes	Yes	Can't tell	N A	Yes	60–79%	Moderate
5	Bhatti et al., 2011	Very likely	Yes	Yes	No	N A	Yes	60–79%	Moderate

(Continued)

TABLE 2 | Continued

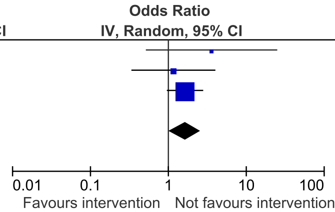
	Short title	Q1–What was the basis for the selection of intervention sites -high accident frequencies or some other traffic rule?	Q2–Were the intervention and the control sites matched for geographic characteristics.	Q3–Were the intervention and control site matched for exposure effect?	Q4–Were the intervention and control site matched for trend effect?	Q5–Was there a sufficient passage of transitional period following the infrastructure construction?	Q6–Whether the study controlled for restricted participant selection?	Percentage of relevant confounders controlled or adjusted?	Potential for risk-of-bias
6	Chiu et al., 2000	Not likely	Yes	Yes	No	N A	Yes	60–79%	Moderate
7	Espitia-Hardeman et al., 2008	Very likely	Yes	Yes	Can't tell	N A	Yes	60–79%	Moderate
8	Hoque et al., 2005	Very likely	Yes	Yes	Yes	Can't tell	No	60–79%	Moderate
9	Ichikawa et al., 2003	Not likely	Yes	Yes	No	N A	Yes	60–79%	Moderate
10	Liberatti et al., 2001	Not likely	Yes	Yes	No	N A	Yes	60–79%	Moderate
11	Lipovac et al., 2013	Not likely	Yes	Yes	No	Can't tell	Yes	80–100%	Low
12	Liu et al., 2011	Not likely	Yes	Yes	No	Can't tell	Can't tell	60–79%	Moderate
13	Nadesan-Reddy and Knight, 2013	Very likely	Yes	Yes	Yes	Can't tell	Yes	60–79%	Moderate
14	Nhan et al., 2017	Not Likely	Yes	Can't tell	Can't tell	N A	Yes	60–79% (some)	Moderate
15	Nguyen Ha et al., 2013	Not likely	Yes	Yes	Can't tell	N A	Yes	60–79%	Moderate
16	Panichaphongse et al., 1995	Not likely	Yes	Yes	Can't tell	N A	Yes	60–79%	Moderate
17	Quistberg et al., 2014	Not likely	Yes	Yes	No	Yes	Yes	80–100%	Low
18	Radin Umar et al., 1995a	Not likely	Yes	Can't tell	Can't tell	NA	Yes	60–79%	Moderate
19	Radin Umar et al., 1995b	Very likely	Yes	Yes	Can't tell	No	Yes	60–79%	Moderate
20	Radin Umar, 2005	Not Likely	Yes	Yes	Can't tell	N A	Yes	60–70%	Moderate
21	Sumner et al., 2014	NA	NA	NA	NA	NA	Yes	80–100%	Low
22	Van der Horst et al., 2016	Not likely	Yes	Yes	Yes	Yes	Yes	80–100%	Low

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 rules then Not Likely.
Q2	If the treated facility is an intersection, the comparison site should be a similar intersection with respect to area type (commercial business district, urban, rural), intersection type (three-legged or four-legged), traffic control (signalized, two-way stop-controlled, etc.).
Q3	For CBA, traffic volume and location matching, warm/cold weather months, daylight vs. dawn/night, traffic composition, enforcement level; In before and after, vehicular traffic volume and location matching, warm/cold weather months, daylight vs. dawn/night, traffic composition, enforcement level pre, and post.
Q4	If the crash trend over a multi-year period shows a continuous increasing or a decreasing trend with little fluctuations in crash frequencies; If there is a sudden drop in the crash frequency after some improvements were made at the treatment site and the trend follows the after period trend.
Q5	In Controlled Before and After, study which does not specify the time period over which outcome was 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	Outcome ascertainment, so that all groups had the same value for the confounder, for example, restricting the study to two-wheel road users
Low risk-of-bias	For studies, if they control for 80–100% of the prespecified confounders using a transparent and rigorous method
Moderate risk-of- bias	For studies, if they control for 60–79% of the prespecified confounders, but have not used any transparent and rigorous method
High risk-of-bias	For studies with inadequate control for confounders

Study or Subgroup	After Intervention Events	After Intervention Total	Before Intervention Events	Before Intervention Total	Weight	Odds Ratio IV, Random, 95% CI	A	B	C	D	E	F	G	H
Afukaar 2003	2	10	3	46	6.1%	3.58 [0.51, 24.98]	+	−	?	+		+	?	?
Van der Horst 2016	3	78	19	570	14.8%	1.16 [0.34, 4.01]	+	−	+	+	?	+		?
Hoque 2005	69	100	83	144	79.1%	1.64 [0.96, 2.80]	+	−	?	+		+	?	?
Total (95% CI)		**188**		**760**	**100.0%**	**1.63 [1.01, 2.63]**								
Total events	74		105											

Heterogeneity: Tau² = 0.00; Chi² = 0.92, df = 2 (P = 0.63); I² = 0%
Test for overall effect: Z = 2.00 (P = 0.05)

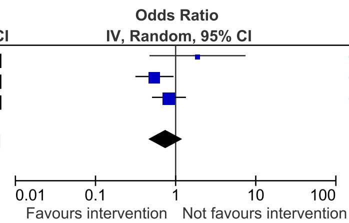


Risk of bias legend
(**A**) Selection Bias
(**B**) Study Design
(**C**) Confounders
(**D**) Data Collection Methods
(**E**) Intervention Integrity
(**F**) Analysis Internal Validity
(**G**) Other Transport Related Biases
(**H**) Overall Risk of Bias Assessment

FIGURE 2 | Primary outcome: changes in 'road traffic death counts' after road engineering interventions.

Study or Subgroup	After Intervention Events	After Intervention Total	Before Intervention Events	Before Intervention Total	Weight	Odds Ratio IV, Random, 95% CI	A	B	C	D	E	F	G	H
Afukaar 2003	5	10	16	46	10.8%	1.88 [0.47, 7.45]	+	−	?	+		+	?	?
Hoque 2005	28	100	60	144	42.0%	0.54 [0.31, 0.94]	+	−	?	+		+	?	?
Van der Horst 2016	48	78	376	570	47.2%	0.83 [0.51, 1.34]	+	−	+	+	?	+		?
Total (95% CI)		**188**		**760**	**100.0%**	**0.76 [0.47, 1.23]**								
Total events	81		452											

Heterogeneity: Tau² = 0.07; Chi² = 3.14, df = 2 (P = 0.21); I² = 36%
Test for overall effect: Z = 1.13 (P = 0.26)

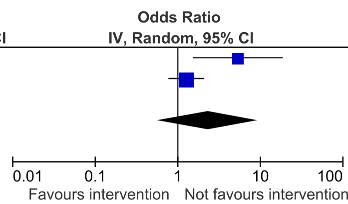


Risk of bias legend
(**A**) Selection Bias
(**B**) Study Design
(**C**) Confounders
(**D**) Data Collection Methods
(**E**) Intervention Integrity
(**F**) Analysis Internal Validity
(**G**) Other Transport Related Biases
(**H**) Overall Risk of Bias Assessment

FIGURE 3 | Primary outcome: changes in the number of 'injuries' after road engineering interventions.

Study or Subgroup	After Treatment Events	After Treatment Total	Before Treatment Events	Before Treatment Total	Weight	Odds Ratio IV, Random, 95% CI	A	B	C	D	E	F	G	H
Afukaar 2003	13	17	20	53	41.8%	5.36 [1.54, 18.73]	+	−	?	+		+	?	?
Van der Horst 2016	28	78	175	570	58.2%	1.26 [0.77, 2.08]	+	−	+	+	?	+		?
Total (95% CI)		**95**		**623**	**100.0%**	**2.31 [0.57, 9.35]**								
Total events	41		195											

Heterogeneity: Tau² = 0.81; Chi² = 4.43, df = 1 (P = 0.04); I² = 77%
Test for overall effect: Z = 1.18 (P = 0.24)



Risk of bias legend
(**A**) Selection Bias
(**B**) Study Design
(**C**) Confounders
(**D**) Data Collection Methods
(**E**) Intervention Integrity
(**F**) Analysis Internal Validity
(**G**) Other Transport Related Biases
(**H**) Overall Risk of Bias Assessment

FIGURE 4 | Primary outcome: changes in the number of 'road accident casualty' after road engineering interventions.

Vote counting and common rubric. We could not develop the vote counting and common rubric for time-series studies.

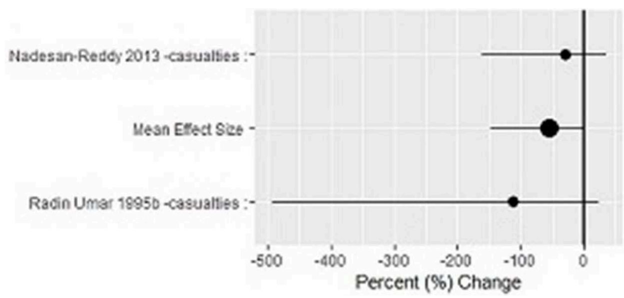


FIGURE 5 | Primary outcome: percent change in the number of 'road accident casualty' after road engineering interventions.

Enforcement of Traffic Laws and Regulation Interventions Compared to no Intervention

Uncontrolled before-and-after studies

Ten uncontrolled before-and-after studies assessed primary outcomes: road traffic death counts and the number of injuries (moderate and severe) and, secondary outcome, compliance before and after enforcement of traffic laws and regulation interventions.

Primary outcomes. Three studies (Panichaphongse et al., 1995; Chiu et al., 2000; Ichikawa et al., 2003) assessed road traffic death counts of motorcycle riders' road accidents before and after mandatory helmet law enforcement. There was no evidence that the mandatory helmet law enforcement was effective (OR 0.94, 95% CI 0.72–1.23; $I^2 = 68\%$) based on data from 35,710 people. Results were analyzed using a random-effects model, and the outcome was downgraded to very low quality. Moderate heterogeneity was present. See forest plot, primary outcome:

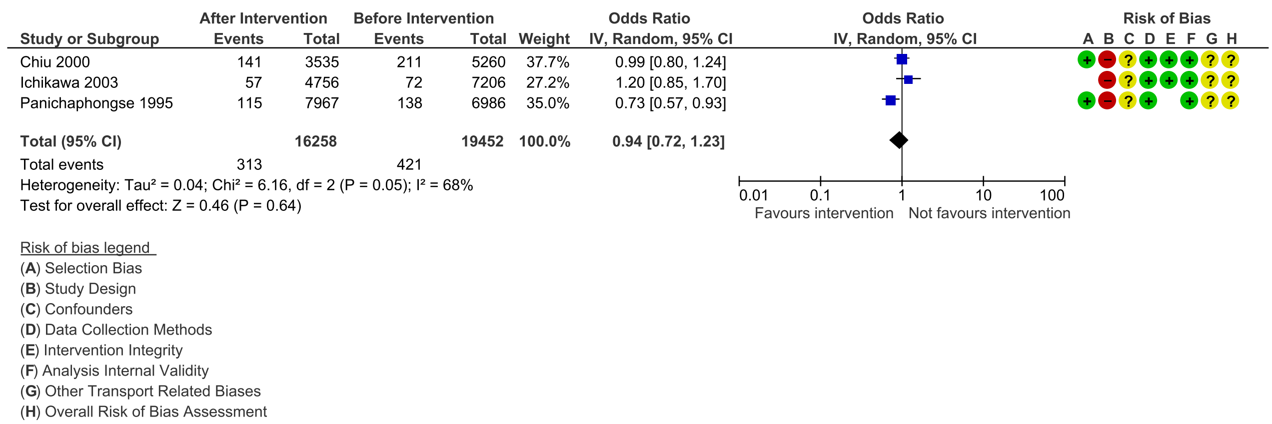


FIGURE 6 | Primary outcome: changes in 'road traffic death counts' after mandatory helmet law enforcement.

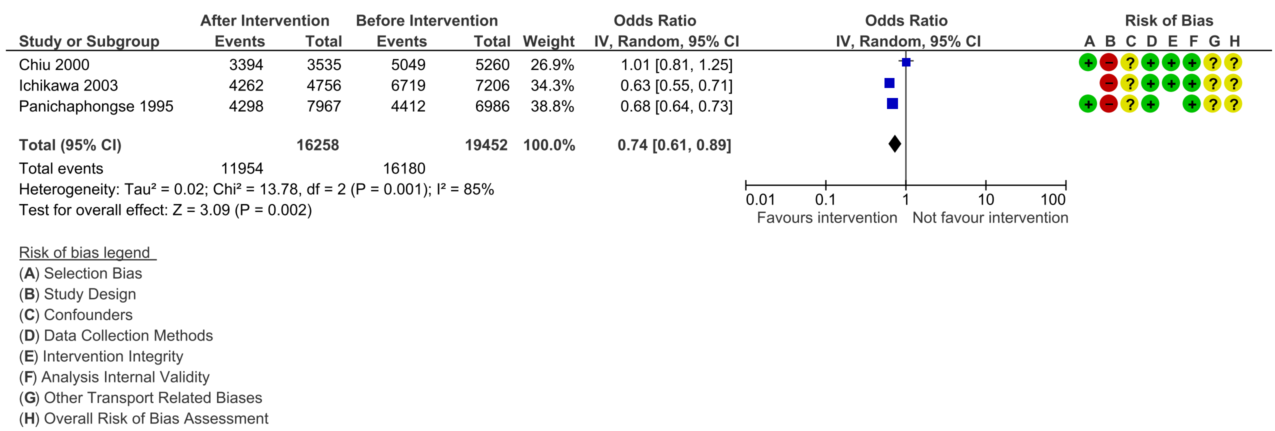


FIGURE 7 | Primary outcome: changes in the number of 'injuries' after mandatory helmet law enforcement.

TABLE 3 | Vote counting and the common rubric of enforcement of traffic laws and regulations.

Uncontrolled before-and-after studies	Study name	Odds ratio (OR)	CI	Risk ratio	CI	Risk difference	*P*-value	Z-score	Voting count (OR)	The direction of intervention effect based on variations from the mean-effect- size
Primary outcomes										
Road traffic death counts	Chiu et al., 2000	0.99	0.80,1.24	0.99	0.81, 1.23	0%	$P = 0.96$	0.05		Not favor intervention
	Ichikawa et al., 2003	1.20	0.85,1.70	1.20	0.85, 1.69	0%	$P = 0.30$	1.03		Not favor intervention
	Panichaphongse et al., 1995	0.73	0.57,0.93	0.73	0.57, 0.93	0%	$P = 0.01$	2.51	√√	Favors intervention
	Mean effect size	**0.94**	**0.72,1.23**	**0.96**	**0.83, 1.11**					
Number of injuries	Chiu et al., 2000	1.01	0.81,1.25	1.00	0.99, 1.01	0%	$P = 0.96$	0.05		Not favor intervention
	Ichikawa et al., 2003	0.63	0.55,0.71	0.96	0.95, 0.97	−4%	$P < 0.00001$	7.03	√√	Favors intervention
	Panichaphongse et al., 1995	0.68	0.64,0.73	0.85	0.83, 0.88	−9%	$P < 000001$	11.37	√√	Favors intervention
	Mean effect size	**0.74**	**0.61,0.89**	**0.94**	**0.88, 1.00**					
Number of moderate injuries	Chiu et al., 2000	0.96	0.83,1.10	0.96	0.84, 1.09	0%	$P = 0.54$	0.01		Not favor intervention
	Panichaphongse et al., 1995	0.73	0.69,0.78	0.84	0.81, 0.87	−8%	$P < 0.00001$	0.62	√√	Favors intervention
	Mean effect size	**0.83**	**0.64,1.07**	**0.89**	**0.78, 1.00**					
Secondary outcomes										
Helmet law - compliance among motorcycle road users	Bastos et al., 2005	10.31	8.65, 12.28	2.29	2.15, 2.45	49%	$P < 0.00001$	26.13	√√	Favors intervention
	Bhatti et al., 2011	1.49	1.11,1.99	1.19	1.05, 1.35	10%	$P = 0.008$	2.67	√√	Favors intervention
	Chiu et al., 2000	28.56	24.00, 33.98	15.85	13.51, 18.61	43%	$P < 0.00001$	33.82	√√	Favors intervention
	Ichikawa et al., 2003	6.17	5.42,7.03	5.01	4.45, 5.64	18%	$P < 0.00001$		√√	Favors intervention
	Liberatti et al., 2001	4.31	3.44,5.40	2.12	1.88, 2.39	35%	$P < 0.00001$	12.67	√√	Favors intervention
	Nguyen Ha et al., 2013	20.89	20.27, 21.53	2.42	2.22, 2.25	52%	$P < 0.00001$	197.63	√√	Favors intervention
	Nhan et al., 2017	0.67	0.63,0.70	0.95	0.94, 0.96	−5%	$P < 0.00001$	15.11	√√	Not favor intervention
	Mean effect size	**5.55**	**1.21, 25.55**	**2.36**	**1.69, 4.21**					
Red-light and speed limit violations	Allyana et al., 2014-speed limit violation	0.15	0.14,0.16	0.24	0.22, 0.25	−32%	<0.00001	60.11	√√	Favors intervention
	Allyana et al., 2014-red light-running	0.50	0.48,0.52	0.51	0.49, 0.53	−2%	<0.00001	33.48	√√	Favors intervention
	Lipovac et al., 2013	0.67	0.62,.73	0.75	0.71, 0.79	−8%	<0.00001	9.94	√√	Favors intervention
	Mean effect size	**0.34**	**0.17,0.70**	**0.45**	**0.24, 0.83**					

Two ticks indicate (√√) statistically significant and No tick (blank) indicates–Not significant.

changes in "road traffic death counts" after mandatory helmet law enforcement (**Figure 6**).

Three studies (Panichaphongse et al., 1995; Chiu et al., 2000; Ichikawa et al., 2003) assessed the number of injuries before and after motorcycle riders' road accidents. The number of injuries decreased after the mandatory helmet law enforcement (OR, 0.74; 95% CI 0.61–0.89; $I^2 = 85\%$) based on data from 35,710 people. Results were analyzed using a random-effects model, and the outcome was downgraded to very low quality. The presence of statistically significant heterogeneity was observed. See forest plot, primary outcome: changes in the number of "injuries" after mandatory helmet law enforcement (**Figure 7**).

Two studies (Panichaphongse et al., 1995; Chiu et al., 2000) assessed the number of severe and moderate injuries of motorcycle riders before and after motorcycle road accidents. There was no evidence that the helmet law enforcement was effective for severe injuries (OR, 0.91; 95% CI 0.82–1.01; $I^2 = 32\%$) and moderate injuries (OR 0.83, 95% CI 0.64–1.07; $I^2 = 91\%$) based on data from 23,748 people.

The presence of heterogeneity for overall and moderate injuries was significant, and we developed the vote counting and common rubric.

Vote counting and common rubric. The mandatory helmet law enforcement did not have any effect on the absolute risk for road traffic death counts among motorcyclists. In two studies (Panichaphongse et al., 1995; Ichikawa et al., 2003), the absolute risk for the number of injuries among motorcyclists wearing helmets reduced by 4% and 9%, which was statistically significant. The number of moderate injury outcome for one study (Panichaphongse et al., 1995) stands out from other studies. This study showed a positive impact of the intervention; the absolute risk for the number of moderate injuries among motorcyclists wearing a helmet reduced by 8% and was statistically significant. For details, see vote counting and the common rubric of enforcement of traffic laws and regulations (**Table 3**).

Secondary outcomes. Seven studies (Chiu et al., 2000; Liberatti et al., 2001; Ichikawa et al., 2003; Bastos et al., 2005; Bhatti et al., 2011; Nguyen Ha et al., 2013; Nhan et al., 2017) assessed motorcycle road users' compliance with mandatory helmet law, and two studies assessed effects of automated-enforcement-system (camera) and pedestrian signal (Lipovac et al., 2013; Allyana et al., 2014).

Seven studies (Chiu et al., 2000; Liberatti et al., 2001; Ichikawa et al., 2003; Bastos et al., 2005; Bhatti et al., 2011; Nguyen Ha et al., 2013; Nhan et al., 2017) assessed motorcycle drivers compliance with helmet use after the mandatory helmet law enforcement (OR 5.55, 95% CI 1.21–25.55; $I^2 = 100\%$) based on data from 2,47,599 people. In places where a mandatory helmet law was enforced, there was five times more compliance among motorcyclists compared with when there was no mandatory helmet law enforcement. Results were analyzed using a random-effects model, and the outcome was downgraded to very low quality. The presence of a considerable heterogeneity between the effect sizes for the seven studies included in the meta-analysis was statistically significant (P=0.00001). See forest plot, secondary outcome: changes in helmet use "compliance" after mandatory helmet law enforcement (**Figure 8**).

Two studies (Lipovac et al., 2013; Allyana et al., 2014) reported speed limit and red-light-running non-compliance (OR 0.37, 95% CI 0.16–0.86; $I^2 = 100\%$) based on 3,76,368 traffic volume and found that speed limit and red-light-running violations reduced by 63%. Results were analyzed using a random-effects model, and the outcome was downgraded to very low quality. See forest plot, secondary outcome: changes in "red light and speed limit non-compliance" after enforcement of traffic laws and regulations (**Figure 9**). Due to the presence of statistically significant heterogeneity ($I^2 = 100\%$), the vote counting and common rubric were also developed.

Vote counting and common rubric. Mandatory helmet law compliance among motorcyclists after the intervention period showed a statistically significant increase across individual studies. It was observed that compliance for helmet use among motorcyclists increased between 10 and 52%, in absolute terms. In one study (Nhan et al., 2017) however, compliance among motorcycle drivers decreased by 5% in absolute terms. The impact of traffic law enforcement on the speed limit and red-light-running violations showed positive and statistically significant effects across individual studies ($p < 0.00001$). The absolute risk for violations against speed limit decreased by 32% (Allyana et al., 2014), and red-light-running decreased by 2% (Allyana et al., 2014), and 8% (Lipovac et al., 2013). For details, see vote counting and the common rubric of enforcement of traffic laws and regulations (**Table 3**).

Time-series studies

Three time-series studies assessed primary outcomes, road traffic death counts and the number of road accident casualty before and after enforcement of traffic laws and regulation interventions.

Primary outcomes. One study (Espitia-Hardeman et al., 2008) reported road traffic death-counts. Due to an insufficient number of studies, the outcome data for road traffic death counts were not assessed.

Two studies (Radin Umar et al., 1995a; Radin Umar, 2005) reported the number of road accident casualties in percent change (percent-change−51.06; 95% CI−228.64–30.56; percent-change −68.37; 95% CI−227.40–13.40) based on data from 5,083 people involved in accidents after the daytime running-headlight law enforcement. The mean effect size in percent change for road accident casualties declined by 38% after the law enforcement. The outcome was downgraded to low quality. See forest plot, primary outcome: percent change in the number of "road accident casualty" after daytime running-headlight regulation (**Figure 10**).

Secondary outcomes. We assessed no secondary outcomes for enforcement of traffic laws and regulation interventions in time-series studies.

Vote counting and common rubric . We could not develop the vote counting and common rubric for time-series studies.

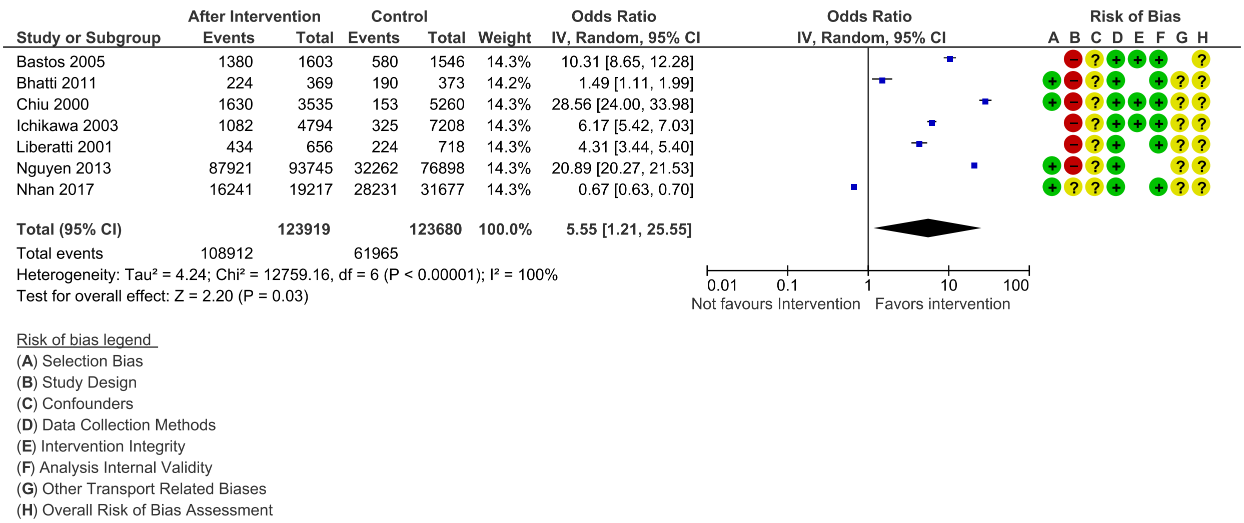


FIGURE 8 | Secondary outcome: changes in helmet use 'compliance' after mandatory helmet law enforcement.

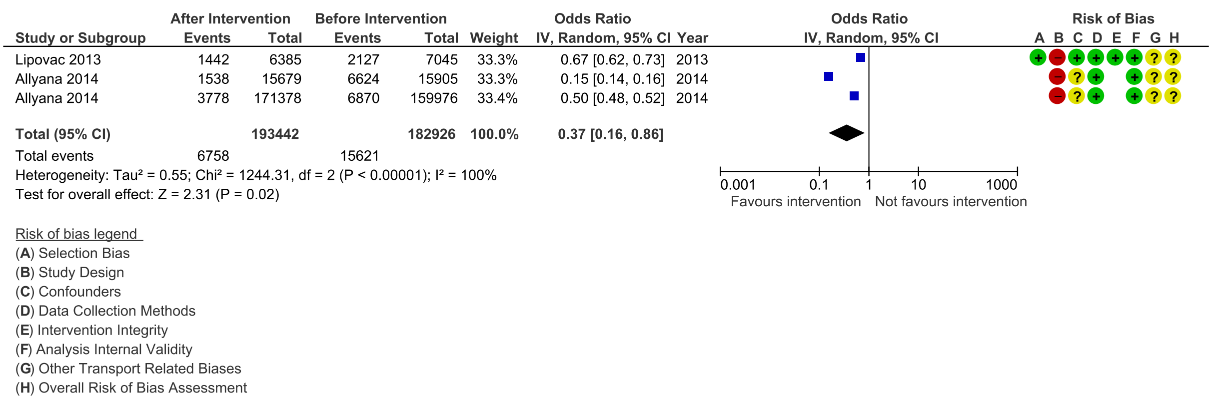


FIGURE 9 | Secondary outcome: changes in 'red light and speed limit non-compliance' after enforcement of traffic laws and regulations.

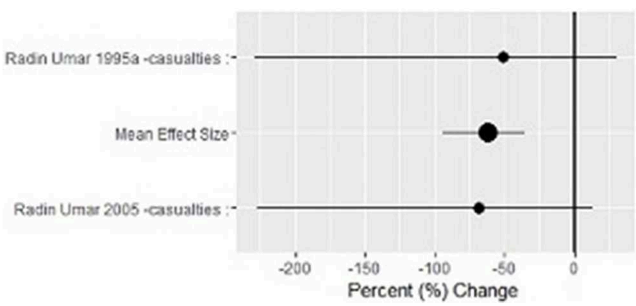


FIGURE 10 | Primary outcome: percent change in the number of 'road accident casualty' after daytime running-headlight regulation.

Sensitivity Analysis

We compared the fixed and random effects' estimates of interventions after which we found that the overall estimates were slightly larger with the random-effects model, but the confidence intervals were identical for fixed and random effects. The overall effect was calculated using a random-effects model. The heterogeneity of studies was assessed by inspection of the forest plot, the confidence intervals of studies, and Chi² and I² statistics. The analysis under each intervention category was grouped by study design. It was not possible to separate very low-quality studies from high-quality studies due to a lack of sufficient studies or outcome data.

The publication bias was analyzed by examining funnel plots and cumulative meta-analysis. No publication bias was observed.

TABLE 4 | Summary of findings (GRADE) for road engineering interventions.

Population: Vulnerable (non-motorized and motorized two-wheel) road users; **Settings:** Low- and middle-income countries
Intervention: Road engineering interventions.

Outcomes	Illustrative comparative risks* (95% CI)		Relative effect (95% CI)	No of Participants (studies)	Quality of the evidence (GRADE)
	Assumed risk	Corresponding risk			
	Control	Road engineering			
Primary outcome–"road traffic death counts"	**Study population**		**OR 1.63** (1.01–2.63)	948 (3 studies)	⊕⊖⊖⊖ **very low**[a,b,c]
	138 per 1,000	**207 per 1,000** (139–297)			
	Moderate				
	65 per 1,000	**102 per 1,000** (66–155)			
Primary outcome–"road accident injuries"	**Study population**		**OR 0.76** (0.47–1.23)	948 (3 studies)	⊕⊖⊖⊖ **very low**[a,d,e]
	595 per 1,000	**527 per 1,000** (408–644)			
	Moderate: Not available				
Primary outcome–"road accident casualty"	**Study population**		**OR 2.31** (0.57–9.35)	718 (2 studies)	⊕⊖⊖⊖ **very low**[a,f,g]
	313 per 1,000	**513 per 1,000** (206–810)			
	Moderate				
	180 per 1,000	**336 per 1,000** (111–672)			
Primary outcome–percent-change in the number of "road accident casualty"[h] Follow-up: mean 1 years	**Study population**		**Mean effect −56.02** (−147.68 to 1.716)[i]	14,404 (2 studies)	⊕⊖⊖⊖ **very low**[j,k]
	213 per 1,000	**−11,925 per 1,000** (-31,438–365)			
	Moderate: Not available				
Primary outcome–Percent-change in "road traffic death counts"[h] Follow-up: mean 1 years	**Study population**		**Percent-change −200.00** (1663.12–48.95)[l]	12,139 (1 study)	⊕⊖⊖⊖ **very low**[m]
	121 per 1,000	**−24,226 per 1,000** (1,000–1,000)			
	Moderate: Not available				

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% CI) is based on the assumed risk in the comparison group and the **relative effect** of the intervention (and its 95% CI). **CI,** confidence intervals; **OR,** odds ratio.*

[a] *Downgraded by one level. All included uncontrolled before-and-after studies were rated at high risk-of-bias for study design. The regression-to-the-mean confounding variable was not controlled in most studies.*

[b] *For all studies, effect estimates and CI overlap.*

[c] *Serious imprecision. The sample size and number of events does not meet "rule of thumb" (>76 events, >188 sample size). The 95% CI of the summary estimate includes both line-of-no-effect and potential appreciable benefits.*

[d] *There was an unexplained inconsistency that was supported by studies on either side of line-of-no-effect.*

[e] *Serious imprecision. Certainty in evidence lowered because of a small number of events (>149) leading to a wide CI in one study. The 95% CI of the summary estimate includes no effect.*

[f] *Serious inconsistency. Only two studies contributed effect estimates, the CI of individual studies was wide, presence of considerable heterogeneity (I^2 =77%, $p < 0.05$). Some heterogeneity was due to differences in country-specific population, road-environment, study size, and duration.*

[g] *Serious imprecision. The 95% CI includes no-effect and appreciable harm. The number of studies was small.*

[h] *For time-series data, the calculations of effect sizes were done using the statistical software R [1] (version 3.1.2).*

[i] *Two studies assessed the number of road accident casualty in percent-change (26.62, 95% CI −149.58 to 35.75; −111.80, 95% CI −492.97 to 24.34).The mean effect of road accident casualties declined by 44% in percent-change after road engineering intervention.*

[j] *Serious inconsistency. Unexplained inconsistency with effect estimates widely different and CI not overlapping. Some of the variations may be due to country-specific population, intervention sub-categories, road-environment, and study duration.*

[k] *Serious imprecision. The 95% CI includes both no effect and appreciable benefits.*

[l] *One study assessed road traffic death counts percent-change (−200.00, 95% CI −1663.12 to 48.95), based on 10,085 people involved in accidents after road engineering intervention.*

[m] *Wide confidence intervals.*

TABLE 5 | Summary of findings (GRADE) for traffic laws and regulation interventions.

Patient or population: Vulnerable (non-motorized and motorized two-wheel) road users; **Settings:** Low- and middle-income countries

Intervention: Traffic law enforcement & regulation interventions

Outcomes	Illustrative comparative risks* (95% CI)		Relative effect (95% CI)	No of Participants (studies)	Quality of the evidence (GRADE)
	Assumed risk	**Corresponding risk**			
	Control	**Traffic laws and regulations**			
Primary outcome–"road traffic death counts" among motorcycle road users	**Study population**		**OR 0.94** (0.72–1.23)	35,710 (3 studies)	⊕⊖⊖⊖ **very low**[a,b,c]
	22 per 1,000	**20 per 1,000** (16–26)			
	Moderate				
	20 per 1,000	**19 per 1,000** (14–24)			
Primary Outcome–"number of injuries (moderate and severe)" among motorcycle road users	**Study population**		**OR 0.74** (0.61–0.89)	35,710 (3 studies)	⊕⊖⊖⊖ **very low**[a,d]
	832 per 1,000	**785 per 1,000** (751–815)			
	Moderate				
	932 per 1,000	**910 per 1,000** (893–924)			
Primary outcome–percent-change in "road traffic death counts" among motorcycle road users[e] Follow-up: mean 1 years	**Study population: One study only**		**Percent-change–500.65** (−1248.43 to 167.55)[f]	0 (1 study)	⊕⊖⊖⊖ **very low**[g]
	Moderate: Not available				
Primary outcome–percent-change in the number of "road accident casualty" among motorcycle road users[e] Follow-up: mean 6 months	**Study population**		**Mean percent-change–62.122** (−94.495 to 35.138)[h]	5,083 (2 studies)	⊕⊖⊖⊖ **low**[i]
	191 per 1,000	**−11867 per 1,000** (−18051–1,000)			
	Moderate: Not available				
Secondary Outcome–"compliance" to mandatory helmet law among motorcycle road users	**Study population**		**OR 5.55** (1.21–25.55)	24,7599 (7 studies)	⊕⊖⊖⊖ **very low**[a,j,k]
	501 per 1,000	**848 per 1,000** (549–962)			
	Moderate: Not available				
Secondary Outcome–"non-compliance" to red-light and speed limit	**Study population**		**OR 0.37** (0.16–0.86)	37,6368 (3 studies)	⊕⊖⊖⊖ **very low**[a,l,m]
	86 per 1,000	**34 per 1,000** (15–75)			
	Moderate				
	172 per 1,000	**71 per 1,000** (32–152)			

*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 intervals; **OR**, Odds ratio.

[a]Downgraded by one level. Included uncontrolled before-and-after studies were rated at high risk-of-bias for study design.

[b]Effect estimates of individual studies are closely aligned; CI is narrow enough for decision-making based on statistical analysis.

[c]Large sample size (>2,000 people in each arm) and narrow CI of the summary effect estimate.

[d]Serious inconsistency. I^2 of 85% ($p < 0.05$) indicate the presence of substantial heterogeneity. Some heterogeneity is due to differences in populations, data-collection duration, and study size.

[e]For time-series studies, the calculations of effect sizes were done using the statistical software R [1] (version 3.1.2).

[f]One study reported road traffic death counts based on data from 1,496 people involved in accidents after law enforcement.

[g]Serious indirectness. The population total sample size was inferred from other studies. The study included multiple interventions for the road safety of motorcycle road users.

[h]Two studies reported the number of road accident casualty (Percent-change −51.06, 95% CI −228.64 to 30.56; Percent-change −68.37, 95% CI −227.40 to 13.40).

[i]The 95% CI around the summary estimate for both studies include both (1) no-effect and (2) potential large effect.

[j]Serious inconsistency. Unexplained inconsistency supported by non-overlapping CI, a considerable level of statistically significant heterogeneity of effect estimates (I^2 =100%, $p < 0.05$). Some of the heterogeneity is due to differences in data collection duration and study size.

[k]Serious imprecision. The 95% CI is consistent with the possibility of appreciable benefits.

[l]Very serious unexplained inconsistency supported by statistically significant heterogeneity of effect estimates (I^2 = 100%). Some of the heterogeneity is due to differences in populations, intervention sub-categories, and data collection size in included studies.

[m]Serious indirectness. Interventions included in the studies were of different sub-categories and used different population segments (motorcyclists and pedestrians).

GRADE quality of evidence

We assessed the methodological quality of the included studies according to the GRADE system for rating the quality of evidence. We ranked the overall quality of evidence across all studies as very low quality. Please see, the summary of findings (GRADE) for road engineering interventions (**Table 4**) and the summary of findings (GRADE) for enforcement of traffic laws and regulations (**Table 5**).

DISCUSSION

Road engineering interventions were not effective in reducing road traffic death counts, the number of injuries, and road accident casualty outcomes after road improvements. While the enforcement of mandatory helmet law was ineffective in reducing road traffic death counts, the intervention efforts proved effective in decreasing overall injuries and increasing helmet use compliance. Automated-enforcement-system (camera) and pedestrian signal interventions were effective in increasing road users' compliance to road safety measures. The daytime-running headlight intervention was effective in reducing road accident casualties.

Four studies presented either a unique outcome or a unique study design to measure enforcement of traffic laws and regulations and road engineering intervention outcome effects. In one study (Liu et al., 2011), the use of transverse rumble strips could reduce the incidence of crashes by 25%, on average. Another study (Antic et al., 2013) showed that the use of speed bumps of 5 and 7 cm height significantly contributed to the safety of vulnerable road users, especially the safety of pedestrians because mean-vehicle-speed was reduced by 79%. The findings of one study (Quistberg et al., 2014) showed that the signalization efforts were associated with an increased risk for pedestrian-vehicle collisions. One RCT (Sumner et al., 2014) found that the free distribution of reflective vests led to a statistically significant increase in vest usage among motorcycle riders; however, the absolute increase was modest.

This review included four non-randomized study designs. Because of methodological differences in study designs, the risk of bias in included studies ranged between A and B grade. Overall, all included studies had adequate information to individually meet the study inclusion criteria. The meta-analysis results assessed the presence of substantial heterogeneity for enforcement of traffic laws and regulation intervention studies. We expected some variations in between studies because of differences in sample size, data collection duration, and road environment. The summary effect estimates of studies showed inconsistency and impreciseness, which downsized the quality evidence of included studies as very low quality. The majority of the studies included in this review were uncontrolled before-and-after. It is difficult to perform controlled trials in the transport environment especially those meant for road improvements. In most studies, the post-intervention period started soon after the deployment of the intervention, and all studies included just one period before and one period after, therefore general changes and traffic volume confounders were most likely controlled. In addition, we attempted to minimize bias in the review process by searching a range of databases and not limiting the search by language. We also ensured that study identification and inclusion, data extraction, and risk-of-bias assessment were carried out by two review authors working independently or together.

This review has some limitations, which must be taken into account when interpreting our results. Given our broad approach to study inclusion criteria, we ended up with multiple types of study design, interventions, and outcomes. In addition, there were variations in between studies due to differences in sample size, duration of data collection, road environment, and macro-economic conditions in which the study was conducted. Almost all road engineering studies were found at the risk of regression-to-the-mean confounder given the fact that the selection of treatment sites for road safety intervention is largely influenced by the high frequency of accident rates. For time-series studies, the effect sizes were computed as the percent change. A drawback of this method is that it does not address the problem of autocorrelation and subsequently may result in reduced standard errors. Using ARIMA models may help evade the problem but fitting the ARIMA models requires larger datasets.

The findings of this review mirror the findings of the first systematic review of road traffic injury initiatives' effectiveness in LMICs (Staton et al., 2016). In this review, the first finding was that legislation interventions evaluated with the best outcomes when combined with strong enforcement initiatives or as part of a multifaceted approach. Second, road improvements may increase road traffic injuries. In our review, the evidence showed that enforcement of legislative interventions when combined with increased police surveillance, penalization for non-compliance, advocacy campaigns, and automated-enforcement-systems were more effective in changing drivers' behaviors when compared to stand-alone road engineering interventions. Because of police surveillance, the likelihood of being caught by the police and of being penalized force compliance behavior among road users in LMICs. It is also likely that road users become more aware of traffic safety because legislative enforcements are generally accompanied by awareness campaigns, as seen in some studies (Radin Umar et al., 1995a; Radin Umar, 2005; Bhatti et al., 2011; Nhan et al., 2017). For road engineering interventions, the results were uncertain on reducing the number of road traffic injuries and deaths. In one study (Mutto et al., 2002), because of the construction of an overpass (footbridge), pedestrian injuries increased, although there was a slight decline in fatalities. Since there was no police surveillance or a financial penalization for using roads to cross or any attempts to channel pedestrian traffic to the overpass, most of the pedestrians crossed by creating their own path at "convenient" points through traffic while risking life. Another research (Hijar et al., 2003) also found similar findings. Conversely, the findings of this review did not mirror the findings of reviews conducted in a high-income country setting. For instance, one review (Bunn et al., 2013) suggested that area-wide traffic calming measures may be a promising intervention for reducing the number of road traffic injuries and deaths. In another review (Liu et al., 2008), researchers found that the motorcycle helmet reduced the risks of death and head injury in motorcyclists who crashed.

CONCLUSION

Further research is needed to examine the effects of road engineering interventions on injury severity outcomes. Even though the evidence was of very low quality, traffic laws, and regulation interventions when combined with enforcement initiatives or with, other approaches proved effective in changing drivers' behaviors.

Future research on road engineering interventions combined with automated-enforcement-system must be explored in an LMIC setting. There is a need for high-quality study designs that use long-term observational periods at the start of the intervention and after the intervention implementation. This review found evidence gaps on the effects of segregation of vulnerable road users from motorized vehicles, bicycle infrastructure interventions, traffic-calming measures on pedestrian accidents.

DATA AVAILABILITY STATEMENT

All datasets generated for this study are included in the article/**Supplementary Material**.

AUTHOR CONTRIBUTIONS

MG prepared the review article. SB read and approved the review article.

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SUPPLEMENTARY MATERIAL

The Supplementary Material for this article can be found online at: https://www.frontiersin.org/articles/10.3389/frsc.2020.00010/full#supplementary-material

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