

52. Naples M. Effective Frequency: The relationship between frequency and advertising effectiveness, New York, Association of National Advertisers, 1979.
53. Leckenby J and Kim H. How media directors view reach/frequency estimation: now and a decade ago, *Journal of Advertising Research*, 1994, 34, 5, 9-21.
54. Jones J.P. When ads work: New proof that advertising triggers, New York, Lexington, 1995.
55. Jones J.P. How advertising works, Thousand Oaks, Ca. Sage, (1998).
56. Ephron E. More weeks, less weight: The shelf space model of advertising. *Journal of Advertising Research*, 1995, May/June, 18-23.
57. Ephron E, Heath M. Once may not be enough. But it's the best we can do, *Admap*, 2001, 36, 10, 44-6.
58. Heath R. The hidden power of advertising: How low involvement advertising influences the way we choose brands, Henley-on-Thames, Oxfordshire, *Admap monograph No. 7*, 2001.
59. Heath R, Hyder P. Measuring the hidden power of emotive advertising, *International Journal of Market Research*, 2005, 47, 5.
60. Bargh J. The automaticity of everyday life, in R. Weyer, Jr. (Ed.) *Advances in Social Cognition*, Mahwah, NJ. 1997, 1-61.
61. Bargh J. The four horseman of automaticity: awareness, efficiency, attention and control in cognition, in R. Wyer and T. Srull (Eds.) *Handbook of Social Cognition*, 2nd. ed. Hillsdale NJ. 1994, 1-40.
62. Bargh J, Morsella E. The unconscious mind, *Perspectives on Psychological Science*, 2008, 3, 73-9.
63. Cialdini R. *Influence: Science and practice* 4th ed. 2001, Boston, Allyn and Bacon.
64. Fennis B, Stroebe W. The psychology of advertising, Ch. 7 *Beyond persuasion: Achieving consumer compliance without changing attitudes*, p.229-266, Hove, East Sussex, Psychology Press, 2010.
65. Frederick S. Automated choice heuristics, Ch. 30 in Gilovich et al (66), p.548-58.
66. Gilovich T, Griffin D, Kahneman D. (Eds.) *Heuristics and Biases: The psychology of intuitive judgment*, New York, Cambridge University Press, 2005.
67. Tversky A, Kahneman D. The framing of decisions and the psychology of choice, *Science*, 1981, 185, 453-8.

A systematic review of how anti-speeding advertisements are evaluated

by BRC Plant¹, F Reza², and JD Irwin¹

¹ Department of Psychology, Macquarie University, Sydney

² School of Psychology, The University of Sydney, Sydney

Abstract

A systematic review of the methodologies used to empirically evaluate anti-speeding advertisements was conducted, and the advantages and limitations of these methods consolidated. Of the 28 studies that met the inclusion criteria, approximately equal proportions employed experimental (57%) and observational (43%) evaluation approaches. While the majority of observational evaluations of anti-speeding advertisements (N = 8, 29% of total evaluations) examined changes to direct measures of speeding (e.g., crash statistics, speeding infringements or on-road driving speeds), the majority of experimental evaluations (N = 12, 43% of total evaluations) relied on indirect measures of speeding behaviour (e.g., self-reported anti-speeding attitudes, intentions, and behaviour). The current review presents the strengths and limitations of previous evaluation approaches, with a particular focus on study design, outcome measures, and advertisement manipulations.

Keywords

Anti-speeding, Campaign, Design, Evaluation, Review, Road safety, Road safety advertising, Speeding

Introduction

Travelling at speeds over the posted maximum speed limit has been shown to be associated with increases in the rate (for a review, see [1]) and severity of crashes [2, 3]). Although the

number of speed-related crashes has reduced considerably over the past decade, speeding remains the most significant contributor to road crashes. Consequently, there is still a great need to continue to develop effective countermeasures to reduce drivers' speeding behaviour.

Current countermeasures against speeding include a range of legislative and educational approaches, including mass media campaigns. Televised road safety advertisements constitute a large proportion of expenditure for road safety initiatives (for example [4]). For Australia's most populous state (New South Wales), the state government spent approximately \$A15.5 million on Roads and Traffic Authority media campaign advertising from 2008-2009, with speed-related media campaign expenditure reported at approximately \$A5.3 million [5].

Given that the cost of executing mass media interventions could instead be used to implement other road-safety initiatives, it is necessary to justify their use. This is particularly important for anti-speeding advertisements, given that speeding remains socially acceptable (see [6]). That is, it is essential to continue to develop and evaluate speeding countermeasures to ensure their effectiveness.

Despite the large number of mass media campaigns – and the associated costs of funding these interventions – evidence suggests that relatively few anti-speeding advertisements are evaluated [7]. Similarly, often the research methods employed to evaluate anti-speeding advertisements have methodological limitations that may significantly impact the reliability or

validity of the findings. This has important implications for road safety and behaviour change interventions because it makes it difficult to determine which strategies are the most effective, and what particular aspects of them ensure and enhance advertisement effectiveness.

While reviews of road safety advertisements have previously been conducted, many of them tend to focus on the content, or the nature of emotional appeals, utilised in road safety advertisements (for example [7]); however, some such reviews have included discussions about methodological issues with previous empirical evaluation methods (for example [8]). Similarly, while many peer-reviewed evaluations of road safety advertisements discuss the limitations of the methodologies employed in their studies, to the authors' knowledge this information has not been consolidated in the form of a systematic review.

The aim of the current paper is to review those studies that have provided an empirical evaluation of Australian and international anti-speeding advertisements, and to bring together a summary of the methodological limitations of these approaches and suggest ways to resolve these problems. Given these aims, it should be noted that the current review does not attempt to assess the extent to which anti-speeding advertisements are effective in reducing speeding (i.e., the *results* of the included studies are not reviewed). Attempting to evaluate the overall effectiveness of anti-speeding advertisements would result in restricting papers included in the review; this would be at the expense of the breadth of information gathered, especially for those studies deemed less suitable for assessing effectiveness (see [9]). The current systematic review of the methodologies used in previous evaluations of anti-speeding advertisements will allow researchers to consider each of the approaches discussed, and to make informed decisions about which evaluative method would be most suitable for their research.

Method

This systematic review intends to confirm standard methodologies used to evaluate anti-speeding advertisements. In order to identify key research designs, measures of campaign effectiveness, and advertisement manipulations, the review attempts to select a representative sample of the methods used to evaluate anti-speeding advertisements.

Inclusion criteria

Studies that evaluated anti-speeding advertisements, or aspects of such advertisements (for example, exposure to a road trauma segment), were eligible for inclusion in the current review, providing that speed-related measures were employed post-exposure. A range of campaign delivery modes was accepted, including: televised, audio or print advertisements, which could be real-world anti-speeding campaigns or designed specifically for the study. A comprehensive search was conducted for peer-

reviewed journal articles and conference papers, as well as technical reports published between 1991 and 2011. To be included in this review, the study had to be primary research published in English, and if multiple publications of the same data were identified, the most recent version was selected. Due to this review's focus on outcome evaluations, studies that used non-comparative research designs (e.g., focus groups) to examine anti-speeding advertisements were not included in the current review; a comparison of outcome measures had to be made between exposed and unexposed groups or between different advertisements. There were no restrictions based on the country or language of the anti-speeding campaign's origin.

While no restrictions were made based on study quality, the suitability of each of the studies' designs to evaluate anti-speeding advertisements was assessed. The studies' designs were first classified using a standard algorithm (for example, see [9]) before they were categorised in terms of suitability for assessing the effectiveness of their anti-speeding advertisements (using the criteria in Table 1, adapted from [9]). While the attributes, or each of the levels of suitability, were based on the guidelines outlined in [9], some adaptations were made, as greater emphasis was placed on designs that reduced threats to internal validity. The analyses and discussions of research methods were conducted under these design quality judgement categories (i.e., strengths and limitations of these designs/approaches and their outcome measures). It should be noted that these categorisations refer to the studies' choice of design only; that is, for the purposes of this review, randomised trial designs were categorised as having the greatest suitability for assessing effectiveness, regardless of the execution of these studies.

Table 1. Suitability of study design for assessing effectiveness (adapted from [9])

Suitability	Attributes
Greatest	Concurrent comparison groups <i>and</i> assigned exposure with prospective measurement of outcome <i>i.e., randomised trial; group randomised trial designs</i>
Moderate	Single or multiple pre and post-measurements with concurrent comparison group <i>or</i> prospective measurement of outcome (within a cohort) but no assigned exposure <i>i.e., time series or before-after with concurrent comparison group(s); prospective cohort designs</i>
Least	Single or multiple pre and post-measurements without concurrent comparison group <i>or</i> exposure and outcome measured in a single group at the same point in time <i>i.e., time series or before-after without concurrent comparison group(s)</i>

Literature search strategy

Electronic literature searches were performed using ScienceDirect, PSYCinfo (via OvidSP), EBSCOhost, ProQuest and Google Scholar. The titles and abstracts of articles were searched using the following key terms: ‘driving’ AND ‘advertisement;’ ‘road’ AND ‘safety’ AND ‘advertisement;’ ‘speeding’ AND ‘advertisement.’ In addition, these searches were repeated by replacing the term ‘advertisement’ with ‘campaign,’ ‘message’ and ‘appeal.’ The abstracts of appropriate articles were manually reviewed to determine whether the study met the inclusion criteria. Seventeen eligible studies were identified this way. To widen the literature search, electronic literature searches were also conducted using the terms ‘speeding’ OR ‘road safety’ AND ‘review,’ and ‘speeding’ OR ‘road safety’ and ‘meta-analysis.’ Reference lists of appropriate reviews and meta-analyses were manually searched for additional primary research studies, and the abstracts of such studies were reviewed to determine whether they met the inclusion criteria. Eleven appropriate studies were identified this way. The final list of 28 studies included in this review was derived from 21 peer-reviewed publications (articles or conference proceedings papers) and five technical reports.

Results: summary of evaluation methods

Summary of study designs

Of the 28 studies identified in this review (summarised in Tables 2–4 in Appendix; [10-34]), almost half (43%, $N = 12$) measured the relationship between the presence/absence of real-world anti-speeding campaigns and changes in the outcome measure of interest; that is, observational approaches were utilised. Of these observational approaches, just under half utilised designs which were categorised as *moderate* to assess effectiveness (42%, $N = 5$), while the remainder employed designs which were categorised as *least* suitable ($N = 7$; see Figure 1). Conversely, in just over half of the studies included in this review (57%, $N = 16$), respondents were exposed to campaigns in a controlled environment (randomised trials and group randomised trials, see Figure 1). Accordingly, separate analyses of the research methodologies were conducted for observational and experimental approaches. Due to the similarities in measures of effectiveness for the study designs categorised as *moderate* and *least* suitable to assess effectiveness, these designs have been combined for the purposes of the current analyses.

Summary of outcome measures

For the reviewed studies that employed experimental approaches ($N = 16$), the vast majority utilised indirect measures of anti-speeding behaviour (75%, $N = 12$, see Figure 2), including measures of: self-reported attitudes, intentions, and awareness about speeding; results on a video speed test¹; and self-reported behaviour (see Table 2 in Appendix). In over half of the experimental evaluations (63%, $N = 10$),

respondents’ perceptions of the message were assessed, which included recall and awareness of the campaign, perceived campaign effectiveness, and emotions experienced. The least common measures for experimental evaluations were direct measures of behaviour, with only one study measuring speeding behaviour, using a driving simulator [20]; this was also the only paper to include both direct and indirect measures of anti-speeding behaviour (although the same participants were not assessed on the different outcome measures). For all of the experimental studies, the outcome variables were measured immediately following participants’ exposure to the campaign, with one quarter of these studies ($N = 4$) including follow-up measures (ranging from 1–4 weeks).

For the observational studies reviewed ($N = 12$; including five designs with *moderate* suitability to assess effectiveness), various direct and indirect outcome measures were used to evaluate real-world anti-speeding campaigns. The most common dependent variables used were direct measures of speeding ($N = 8$, see Figure 2), including crash statistics, speeding infringements, or on-road driving speeds (see Tables 3 and 4 in Appendix). Indirect measures were also common in observational designs ($N = 6$, see Figure 2), and for two of the observational studies both indirect and direct measures of behaviour were obtained (albeit from different samples). Respondents’ perceptions of the message were also common measures in observational designs ($N = 7$, see Figure 2); however these were often used to determine exposure rates (cf. advertisement effectiveness).

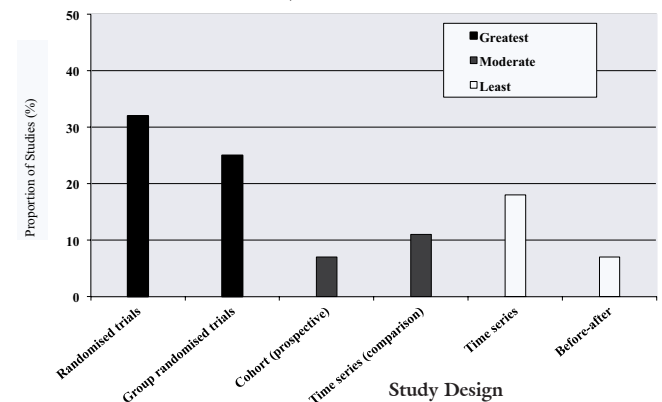


Figure 1. Proportion of studies that used each design type to evaluate anti-speeding advertisements ($N = 28$), categorised according to design suitability to assess effectiveness

When comparing approaches, observational evaluations most often relied on direct outcome measures (although the proportions are quite similar across the observational studies), while experimental evaluations most often used indirect measures. Overall, the most commonly used evaluative method was a randomised trial design (individual or group) with indirect measures of speeding behaviour ($N = 12$, 43% of all reviewed studies). Conversely, the methods least often employed to evaluate anti-speeding advertisements were randomised trial designs using direct measures of speeding behaviour (i.e., using

a driving simulator); only one study did this, and it did not evaluate a true anti-speeding campaign (see [20]).

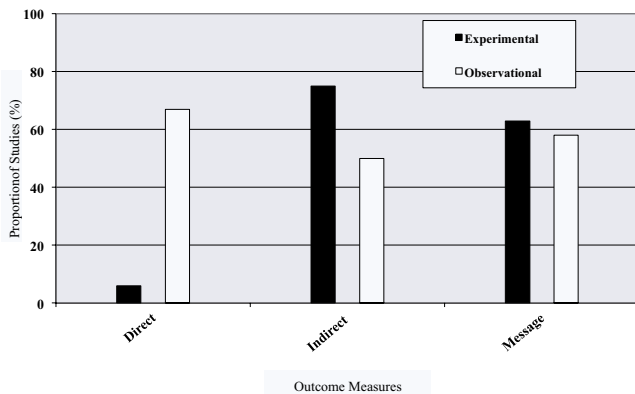


Figure 2. Proportion of experimental ($N = 16$) and observational ($N = 12$) studies that used each type of outcome measure to evaluate anti-speeding advertisements

(Note: Outcome measure categories are not mutually exclusive)

Summary of advertisement manipulations

Of the 16 studies which utilised an experimental design, just over half employed newly designed advertisements (56%, $N = 9$), while approximately one third utilised advertisements that had been aired in a different state to that in which the study was conducted (31%, $N = 5$). Thus, for the majority of experimental evaluations, previous exposure to the anti-speeding advertisement did not occur (88%, $N = 14$); for the remainder, at least some participants had prior exposure to the advertisement or campaign of interest. Respondents in almost all of the experimental studies were instructed to concentrate on the advertisement or campaign (88%, $N = 14$). Furthermore, one quarter of the experimental studies ($N = 4$) assessed the effects of repeated exposure on outcome measures.

A between-subjects design was utilised in many of the experimental studies (38%, $N = 6$), whereby participants were randomly allocated to view a single advertisement. Similarly, a large proportion of the experimental studies utilised mixed designs (44%, $N = 7$); for the majority of these cases ($N = 5$) participants each only viewed one advertisement. Finally, approximately one fifth of the reviewed studies employed a within-subjects design (19%, $N = 3$), whereby all participants were exposed to multiple advertisement conditions.

With respect to the emotional appeals of the advertisements employed for the experimental evaluations, almost all studies evaluated a campaign with a negative emotional appeal (94%, $N = 15$), such as a fear- or shock-inducing advertisement. Conversely, relatively few studies evaluated campaigns with a positive emotional appeal, such as those incorporating humour (25%, $N = 4$), and few evaluated advertisements or campaigns that evoked neutral emotions (19%, $N = 3$). Importantly, in approximately half of the evaluations reviewed (56%, $N = 9$), a manipulation check was conducted to ensure that the intended emotion was experienced by respondents.

For all of the observational studies ($N = 12$), real-world anti-speeding campaigns were evaluated and exposure to the campaign was assumed; in over half of these studies advertisement exposure was measured or sampled (58%, $N = 7$). The campaigns which were most heavily evaluated using this method were the various Transport Accident Commission campaigns which featured in Victoria.

Summary of sampling methods

The most common sampling method for experimental evaluation approaches was to recruit undergraduate university students (50%, $N = 8$). Other sampling methods included directly approaching participants (13%, $N = 2$), or posting invitations to participate (19%, $N = 3$); for two studies, members of the Israeli Defence Force were recruited to participate (see [20]). Questionnaires (paper or online) were most often utilised to measure participants' reactions to the advertisements or campaigns (81%, $N = 13$). It should be noted that possible social desirability effects were only examined in two studies (Study 1 and Study 2 in [20]).

The most common sampling method used to measure the observational studies which utilised indirect measures of behaviour ($N = 6$) was telephone surveying (33%), followed by door-to-door knocking (17%), and random location sampling (17%). In the case of observational studies examining crash statistics, data were analysed across timeframes of up to 11 years.

Discussion: strengths and limitations of evaluation methods

The current research has extended previous reviews of road safety advertisement evaluation methodologies by conducting a systematic review of the research methods used to evaluate Australian and international anti-speeding advertisements. As such, in this section, the strengths and limitations of the research methods used to evaluate anti-speeding advertisements will be discussed, and some suggestions of ways to overcome the addressed limitations will be made. Finally, the implications of the current review will be presented, and directions for future research will be suggested.

Suitability of study designs to assess effectiveness

Greatest level of suitability: strengths and limitations of experimental approaches

A large proportion of the studies reviewed here have evaluated anti-speeding advertisements using designs deemed to have *greatest* suitability to assess effectiveness – these included randomised trials and group randomised trials (i.e., experimental approaches with random allocation). The main advantage to such designs over all others is that the investigator has control over exposure to the advertisement(s) – such control does not occur in observational studies. Similarly, randomised trials evaluate the effects of the advertisement(s) in

highly controlled environments, whereby extraneous variables (i.e., possible confounds) are minimised. Thus, experimental evaluation approaches have high internal validity; the observed effects on outcome measures can clearly be attributed to the anti-speeding advertisement(s).

Given that the main strengths of experimental approaches are attributable to the controlled environment in which they're conducted, the main limitation of these approaches is that they are removed from the natural driving environment; as a result, experiments are often low in ecological validity. The external validity of experimental approaches may also be impacted due to sampling issues or biases. Given that experimental evaluations often rely on samples of undergraduate university students, their findings might not truly reflect the general population of (young) drivers. Thus, the external validity of experimental approaches could be improved upon by collecting data from drivers from the general population and collecting data in the most natural settings possible.

Other limitations of experimental procedures that should be addressed are the problems of social desirability and measurement (i.e., Hawthorne) effects. It is a real possibility that any reported effects of anti-speeding advertisements, which have been evaluated in an experimental situation, could be explained by participants responding in a socially desirable manner. Despite many researchers' awareness of this issue, only two of the reviewed studies examined, and attempted to minimise, possible social desirability effects (see [20]). It is recommended that future evaluations of anti-speeding advertisements, in which participants are aware that their responses are being measured, examine possible social desirability effects (e.g., using the Marlowe-Crowne [35] social desirability scale, as utilised in [20]). In addition, future investigations could attempt to reduce possible social desirability effects by embedding the advertisement(s) within a short cartoon or television programme (using a similar procedure to [20]); this could reduce any social desirability effects by making the intervention more subtle (and naturalistic, see later).

There are additional concerns with previous experimental procedures used to evaluate anti-speeding advertisements that should be addressed. For instance, while one third of the reviewed experimental evaluations have examined the effects of the advertisements over time, the majority have only examined the immediate effects of the advertisement(s) on their dependent variable(s). Excluding delayed measures is a limitation because relatively little is known about the persuasive effects of anti-speeding advertisements over time. In order to determine the effects of advertisements on measures of speeding behaviour, it would be beneficial to include both immediate and delayed measures (also see [8]).

Moderate and least levels of suitability: strengths and limitations of observational approaches

The strength of observational studies (and their associated designs), in general, is that they employ a global approach to

evaluating anti-speeding advertisements, which is advantageous for multiple reasons. First, drivers are often unaware that their behaviour or behavioural outcomes are being measured (excluding cohort studies); consequently, any possible Hawthorne and social desirability effects are avoided. Second, in the case of measures of on-road driving speeds, these evaluations are often conducted in the natural driving environment. In addition to the high ecological validity of such studies, measuring driving speeds in a natural setting will likely capture the driving behaviour of a representative sample of the driving population; this approach should enhance the external validity of the findings. A final strength of observational evaluation approaches is that these designs often provide an opportunity to examine large-scale societal effects (cf. experimental approaches).

Despite the aforementioned strengths, there are limitations to observational evaluation approaches. Perhaps the most significant of these – preventing some of these studies from being categorised as having *greatest* suitability for assessing effectiveness – is that the investigator has no control over advertisement exposure. That is, exposure to the campaign is either assumed or checked retrospectively (e.g., by sampling advertisement awareness and recall). Another significant limitation of some observational studies is their tendency to examine relationships between outcome measures and the presence/absence of anti-speeding advertisement waves (e.g., before-after and time series designs without concurrent comparison groups). Such designs provide no control over extraneous variables, which means that changes to the outcome measure of interest cannot, reliably, be attributed to the advertisement(s). One way to increase the internal validity of the findings is to include a concurrent comparison group that has not been exposed to the advertisement (i.e., use time series or before-after designs with concurrent comparison). Assuming that the concurrent comparison group was similar or matched to the exposed group, it could act as a suitable control for extraneous variables – thereby making any causal connections between the intervention and changes to outcome measures more reliable.

Suitability of outcome measures to assess effectiveness

Direct measures of speeding behaviour: strengths and limitations

Only one of the experimental evaluations presented in this review included a direct measure of speeding behaviour (simulated driving speeds, see [20]); however, it should be noted that this study did not evaluate an anti-speeding campaign (rather it was a road trauma video that did not mention, nor depict, speeding). Similarly, although not captured using the inclusion criteria of the current review, the authors are aware of previous experiments that have evaluated anti-speeding advertisements using a driving simulator (see [36–40]). Together, these studies have measured a range of

speed-related dependent variables, including completion time, speeding frequencies, average driving speeds, and speed exceedance magnitudes. The main advantage of such direct measures of speeding behaviour is that they may provide a more representative account of the effects of anti-speeding advertisements on actual driver behaviour (cf. indirect measures, see next section). In addition, any erroneous findings that may emerge as a result of heuristics that drivers hold about what constitutes an effective anti-speeding advertisement (see [41, 42]) may be averted if empirical evaluations measure speeding behaviour directly. That is, if drivers do not have accurate insight into which anti-speeding advertisements will be effective, then measuring advertisement effectiveness via direct measures of their behaviour may bypass any subconscious biases that drivers hold (cf. indirect measures, such as behavioural intentions, or measures of message characteristics, such as perceived campaign effectiveness).

Despite the potential advantages of measuring anti-speeding advertisement effectiveness using simulated driving speed measures, the reliability and validity of simulated driving behaviour to model actual on-road driving behaviour should be considered. While driving simulators provide, arguably, a more ecologically valid and reliable method of measuring driving behaviour (cf. indirect measures), simulated driving behaviour should not be treated as identical to actual driving. For instance, the driving simulator provides drivers with a risk-free driving experience, where physical threat and fear factors are absent. Thus, simulated driving speeds might not be representative of actual on-road driving speeds. Despite this apparent limitation, studies have shown driving simulators to have relative [43] and absolute validity [44] with respect to on-road driving speeds. However, the authors are not aware of any empirical evaluations of anti-speeding advertisements that have included a validation procedure to determine the driving simulator's absolute validity.

For a large proportion of the observational evaluations of anti-speeding advertisements, behavioural outcomes (e.g., crash rates and speeding violations) or on-road driving speeds were the measure of interest. The main strength of these direct measures is that they are high in ecological validity and – especially in the case of crash rate measures – it could be argued that outcome-based measures are closely related to safety.

While outcome-based measures of anti-speeding advertisement effectiveness may be high in ecological validity, it is debatable how measures of speed-related crash rates and speeding violations are linked to actual speeding behaviour. For instance, it could be argued that speeding violations are really a measure of the number of times the driver was *caught* for speeding. Similarly, there may be multiple causes of crashes – this is problematic when trying to determine whether speed was the sole, main, or one of many contributing factor(s) of a crash. Thus, while speeding violations and speed-related crash rates are, presumably, indicators of speeding behaviour, this is not the same as measuring actual speeding. There are other problems

associated with using speed-related crash rates as a dependent variable, including the issue of minor crashes often going unreported (for a more detailed discussion about the limitations of using crash rates as a measure of road safety advertisement effectiveness, see [45]). Thus, when relying on behavioural outcome measures as an indicator of speeding, it can be difficult to establish whether anti-speeding advertisements have been effective because of the nature and limitations of these measures.

Indirect measures of speeding behaviour: strengths and limitations

Some of the observational evaluations and a large proportion of the experimental evaluations reported in the current review have measured anti-speeding campaign effectiveness via indirect measures of behaviour, including: reported issue awareness; attitude towards speeding; intention to speed; and self-reported speeding. The major advantage that indirect measures of behaviour have over direct behavioural measures relates to the relative ease and economic efficiency with which anti-speeding advertisements can be evaluated. For instance, in the case of experimental approaches, if anti-speeding advertisements are evaluated via indirect behavioural measures, it is possible to arrange the procedure such that several drivers participate simultaneously; for example, participants could each view a randomly-assigned advertisement and complete the post-exposure measures at the same time (using computers).

Although the findings from such research can have important implications for anti-speeding advertisements, the ecological validity of these measures is subject to criticism. For instance, investigations into the predictive validity of attitudes have indicated that while positive attitudes towards speeding may correlate positively with self-reported frequencies of speeding, significant correlations with average driving speeds in a driving simulator may not be found for all speed zones [46]. Similarly, examinations into the validity of self-reported speeding, compared with objective speed measures using GPS technology, have revealed a tendency for drivers to over report their driving speeds when travelling at low speeds and to under report their driving speeds when travelling at high speeds [47]. Importantly, an investigation into the effects of anti-speeding advertisements on the reported intentions to reduce speeding and the simulated driving speeds of young drivers has suggested that there may be a gap between drivers' reported intentions to change and their observed changes to speeding behaviour [36, 37]. These findings highlight a potential limitation of using indirect measures of behaviour to examine the effects of anti-speeding advertisements, as drivers' post-exposure anti-speeding attitudes, intentions, or self-reported behaviours might not be representative of their subsequent driving behaviour - a suggestion which has important implications for some of the anti-speeding advertisement evaluations reviewed here.

Suitability of advertisement manipulations

Advertisement characteristics: limitations

Although the purpose of many of the evaluations reviewed here was to examine the effects and perceptions of different real-world anti-speeding advertisements, using real-world advertisements presents some limitations that should be discussed. In particular, real-world anti-speeding (and control) advertisements often include different characters, settings, and vehicles, and they may present speeding behaviour (and its consequences) differently; that is, the examined components are not manipulated in a controlled manner. For this reason, caution should be made when attempting to isolate, and make conclusions about, the components of the advertisement which contribute to any observed changes in the dependent variables of interest. Similarly, very few real-world anti-speeding advertisements include specific coping strategies or instructions for drivers [7]. As a consequence, such anti-speeding advertisements do not lend themselves to an examination of the behavioural or perceived effects of response efficacy. Thus, while studies that employ real-world anti-speeding advertisements may be higher in ecological validity, using such stimuli makes it difficult to tease out the characteristic(s) or aspect(s) of the advertisement which makes it effective (or ineffective). It is recommended that future experimental evaluations of anti-speeding advertisements employ controlled stimuli (cf. real-world advertisements) which manipulate the depictions of speeding behaviour and its consequences, response efficacy, and the emotional appeal in a systematic manner.

On a similar note, almost half of the experimental evaluations presented in the current review have made a priori assumptions about the emotions evoked by the advertisements employed. This is a limitation for two reasons. First, for studies in which the advertisements' emotional appeal is the experimental manipulation, if the evoked emotions are not established before or during the study, the manipulation has not been verified – this may mean that any observed effect, or lack thereof, has been inaccurately attributed to emotional effects. Second, without measures of the emotions evoked by the advertisement(s), it makes it difficult to compare findings across studies (for a more detailed discussion about the implications of an absence of manipulation checks, see [8]). Future investigations should rectify this limitation by independently establishing the emotions evoked by anti-speeding advertisements (and the strength of these emotions), via a pilot study or by measuring participants' evoked emotions throughout the study.

Exposure methods: strengths and limitations

A small number of the experimental evaluations reviewed employed advertisements or slogans which had been previously televised within the state in which testing was conducted. As a result, often participants of such studies have seen the advertisements prior to their participation in the research, which introduces prior exposure issues. Research into

advertisement wear-out effects suggests that the effects of road safety advertisements might decline over time [25], and that people tend to habituate to the fear evoked by threatening anti-speeding advertisements with repeated exposure [21]. Accordingly, where there are high rates of prior exposure to an advertisement, reported changes to indirect or direct speeding behaviour measures could be too conservative. To better examine pre-exposure effects on persuasive outcomes, future studies should gather data on the approximate number of times participants have seen the advertisements prior to the study, which would allow pre-exposure levels to be entered into the model as a covariate. Similarly, future studies could examine the effects of repeated exposure by presenting the entire advertisement or advertisement prompts to drivers at multiple intervals.

Another limitation that has been noted as a result of the current review is that some empirical evaluation methods allow participants to view multiple anti-speeding advertisements or campaigns, via within-subjects designs. This manipulation is undesirable because there is a possibility that any reported differences between advertisements could be explained, or could have been enhanced, by interaction or contrast effects (see [48]). Information provided in previously encountered stimuli has been shown to impact upon the persuasive impact of a target message). That is, perhaps after viewing an advertisement the remaining advertisement(s) would be perceived by participants as significantly more or less influential, resulting in inflated or deflated effects. While many evaluations attempt to avoid these issues via counter-balancing the presentation of the advertisements, this would likely lead to wash-out effects; consequently, there may be a failure to detect any real differences. Accordingly, future experimental evaluations should employ between-subjects or mixed designs, such that each participant only views one anti-speeding advertisement.

Finally, most often, experimental evaluations of anti-speeding advertisements subject participants to 'forced' exposure to the advertisement, which limits the ecological validity of the research. Despite this limitation being addressed in previous reviews [8], only two of the studies reviewed in the current paper have attempted more natural exposure methods (see [20]). Although not meeting the inclusion criteria for the current review, the authors are aware of two evaluations which have employed subtle exposure methods (see [39, 40], [49]); future empirical evaluations could adopt similar strategies to enhance their ecological validity with respect to naturalistic exposure to anti-speeding advertisements.

Implications and future directions

The current review has highlighted the salient methodological limitations of previous research methods used to evaluate anti-speeding advertisements. Given that the methodological limitations outlined in this review may significantly impact the reliability or validity of the conclusions drawn from such evaluations, the findings of the current review have important implications for road safety and for behaviour change

interventions. In particular, the current findings suggest that some evaluation methods, by themselves, are unsuitable for providing reliable evidence from which to govern effective media advertisement design.

Overall, it is suggested that investigators first determine the goals of the evaluation and select the study design based on what is feasible and most suitable to meet these objectives. It is recommended that investigators with narrow research questions (e.g., what types of messages enhance effectiveness), employing experimental approaches, include direct measures of behaviour, where possible, to enhance the ecological validity of their findings. In addition, it is suggested that future research using experimental approaches considers exploring in-car technologies to measure drivers' actual on-road driving speeds to validate indirect outcome measures. For investigators who wish to examine large-scale societal effects, it is recommended that any observational studies (with direct measures) include a concurrent comparison group, where possible, to enhance the study's internal validity. While a large emphasis has been placed on the design and outcome measures in this review, considering the overall execution of the evaluation is vital. Overall, a well-executed large-scale design may be superior to a poorly-executed randomised trial (see [9]). Thus, it is recommended that researchers consider the limitations of previous approaches, as discussed in this review, and ensure that they avoid the relevant limitations where possible.

Furthermore, it is recommended that further basic research is conducted. For instance, additional research is needed in order to gain a better understanding about the effects of prior exposure and any delayed effects that advertisements have on persuasive outcomes. Similarly, more research into the persuasive effects of various emotional appeals is needed, especially given the dominance of negative emotional appeals in anti-speeding campaigns and empirical evaluations (also see [7, 8]). In addition, further research is required to establish whether the conclusions drawn about the effective characteristics of anti-speeding advertisements can be generalised to other road safety behaviours and/or target groups (see [50]); that is, if the findings are particular to the behaviour or population of interest this will influence future campaign development. Such basic research would be foundational for the future design and evaluation of anti-speeding, and other road safety, advertisements.

Acknowledgements

This work was supported by the Macquarie University Research Excellence Scholarship (MQRES).

Notes

- ¹ The video speed test involves a driving simulation test, whereby participants watch a video of someone driving in real driving situations. Participants then indicate their speed choice for the just-seen driving scenario (for example, see [21]).

References

- Aarts L, van Schagen I. Driving speed and the risk of road crashes: a review. *Accident Analysis and Prevention* 2006; 38(2):215–24.
- Cooper PJ. The Relationship Between Speeding Behaviour (as Measured by Violation Convictions) and Crash Involvement. *Journal of Safety Research* 1997; 28(2):83–95.
- Moore VM, Dolinis J, Woodward AJ. Vehicle Speed and Risk of a Severe Crash. *Epidemiology* 1995; 6(3):258–262.
- Donovan RJ, Jalleh G, Henley NC. Executing effective road safety advertising: Are big budgets necessary? *Accident Analysis and Prevention* 1999; 31(3):243–252.
- NSW Department of Finance and Services. NSW Government Advertising - Media expenditure. NSW: Department of Finance and Services; 2010. Viewed 22 May 2011. http://advertising.nsw.gov.au/sites/default/files/downloads/page/total_advertising_expenditure_july_2008_to_june_2009.pdf
- Petroulias T. Community attitudes to road safety – 2009 survey report. Canberra: Department of Infrastructure, Transport, Regional Development and Local Government; 2009. Report No.: Road Safety Report 4.
- Phillips R, Torquato R. An overview of 45 anti-speeding campaigns. Oslo, Norway: Institute of Transport Economics; 2009.
- Lewis IM, Watson B, White KM. What do we really know about designing and evaluating road safety advertising?: current knowledge and future challenges. In: *Smarter, Safer Directions*. 11-12 November, 2009, Sydney: 2009.
- Briss PA, et al. Developing an evidence-based Guide to Community Preventive Services—methods. *American Journal of Preventive Medicine* 2000; 18(1S):35–43.
- Algie J, Rossiter JR. Fear patterns: A new approach to designing road safety advertisements. *Journal of Prevention and Intervention in the Community* 2010; 38(4):264–279.
- Caubergh V, De Pelsmacker P, Janssens W, Dens, N. Fear, threat and efficacy in threat appeals: Message involvement as a key mediator to message acceptance. *Accident Analysis and Prevention* 2009; 41(2): 276–285.
- Elliott MA, Armitage CJ. Promoting drivers' compliance with speed limits: Testing an intervention based on the theory of planned behaviour. *British Journal of Psychology*. 2009; 100(1):111–132.
- Glendon AI, Lorenzo C. Young drivers' responses to anti-speeding and anti-drink-driving messages. *Transportation Research Part F: Traffic Psychology and Behaviour* 2003; 6(3):197–216.
- Goldenbeld C, Divera T, Sjoerd H. Effects of persuasive communication and group discussions on acceptability of anti-speeding policies for male and female drivers. *Transportation Research Part F: Traffic Psychology and Behaviour* 2008; 11(3):207–220.
- Lewis I, Watson B, Tay R. Examining the effectiveness of physical threats in road safety advertising: The role of the third-person effect, gender, and age. *Transportation Research Part F: Traffic Psychology and Behaviour* 2007; 10(1):48–60.
- Lewis I, Watson B, White KM. Predicting future speeding behaviour: The appeal of positive emotional appeals for high risk road users. In *Proceedings High risk road users - motivating behaviour change: what works and what doesn't work?* National Conference of the Australasian College of Road Safety and the Travelsafe Committee of the Queensland Parliament. Brisbane, 2008.
- Lewis IM, Watson BC, White KM. Response efficacy: the key to minimizing rejection and maximizing acceptance of emotion-based anti-speeding messages. *Accident Analysis and Prevention* 2010; 42:459–467.

18. Parker D, Stradling SG, Manstead ASR. Modifying beliefs and attitudes to exceeding the speed limit: an intervention study based on the theory of planned behavior. *Journal of Applied Social Psychology* 1996; 26(1):1–19.
 19. Rossiter JR, Thornton J. Fear-pattern analysis supports the fear-drive model for antispeeding road-safety TV ads. *Psychological Marketing* 2004; 21(11):945-960.
 20. Taubman Ben-Ari O, Florian V, Mikulincer M. Does a threat appeal moderate reckless driving?: A terror management theory perspective. *Accident Analysis and Prevention* 2000; 32(1):1–10.
 21. Thornton J, Rossiter JR. Advertising wearout of shock-value anti-speeding ads. In *Proceedings ANZMAC2001*. Albany, Auckland: Massey University, 2001.
 22. Walton D, McKeown PC. Drivers' biased perceptions of speed and safety campaign messages. *Accident Analysis and Prevention* 2001; 33(5):629-640.
 23. Brown SL. Changes in risk perceptions prospectively predict changes in self-reported speeding. *Risk Analysis* 2010; 30(7):1092-1098.
 24. Cameron M, Newstead S, Diamantopoulou K, Oxley P. The interaction between speed camera enforcement and speed-related mass media publicity in Victoria. Clayton, Victoria: Monash University Accident Research Centre, 2003.
 25. Fry TR. Advertising wearout in the Transport Accident Commission road safety campaigns. *Accident Analysis and Prevention* 1996; 28(1):123–129.
 26. Stead M, Tagg S, MacKintosh AM, Eadie D. Development and evaluation of a mass media Theory of Planned Behaviour intervention to reduce speeding. *Health Education Research* 2005; 20(1):36-50.
 27. Whittam KP, Dwyer WO, Simpson PW, Leeming FC. Effectiveness of a media campaign to reduce traffic crashes involving young drivers. *Journal of Applied Social Psychology* 2006; 36(3): 614–628.
 28. Angle H, Greggs J, Goddard E. Think! Road safety campaign evaluation post stage: 'live with it' speed campaign and motor cycle campaign. London: Department for Transport, 2009. Viewed 15 May 2010. <http://think.dft.gov.uk/pdf/332982/3329861/0904-mc-speed.pdf>
 29. Divjak M, Zabukovec V. Evaluation of an anti-speeding campaign in Slovenia. Campaigns and awareness raising strategies in traffic safety — Deliverable D-4.1. Slovenia: University of Ljubljana, 2009. Viewed May 18 2011. http://www.cast-eu.org/docs/CAST_WP4_Deliverable%204.2.pdf
 30. Guria J, Leung J. An evaluation of a supplementary road safety package. *Accident Analysis and Prevention* 2004; 36(5):893-904.
 31. Mulholland E, Tierney P, Healye D. Wipe-off 5: A Victorian Social Marketing Campaign. In *Proceedings Australian Road Safety Conference Research, Policing and Education Conference 2005*. Wellington Central, Wellington: Australian Road Safety Conference Research, Policing and Education Conference, 2005.
 32. Tay R. The effectiveness of enforcement and publicity campaigns on serious crashes involving young male drivers: Are drink driving and speeding similar? *Accident Analysis and Prevention* 2005; 37(5):922-929.
 33. Tay R, Ozanne L. Who are we scaring with high fear road safety campaigns? *Asia Pacific Journal of Transport* 2002; 4(1):1-12.
 34. Taylor MAP, Woolley JE, Dyson CB. Outcomes from the South Australian road safety media evaluation study. In *Proceedings of the National Speed and Road Safety Conference*. North Terrace, Adelaide: Adelaide Convention Centre, 2001.
 35. Crowne D, Marlowe D. The approval motive: Studies in evaluative method. New York: Wiley; 1964.
 36. Plant BRC. Combating youth speeding: A televised intervention approach. Master's thesis. Sydney, Australia: Macquarie University, 2009.
 37. Plant BRC, Irwin JD, Chekaluk E. When threat appeals: exploring the anti-speeding intention-behaviour relationship of young drivers. Manuscript in preparation 2011.
 38. Plant BRC, Irwin JD, Chekaluk E. The effects of anti-speeding road safety advertisements on the simulated speeding behaviour of young drivers. Manuscript in preparation 2011.
 39. Reza F. Responsible driving - everyone thinks big of you. Can positive emotional appeals be utilised to influence male driver behaviour? Honour's thesis. Sydney, Australia: Macquarie University, 2010.
 40. Reza F, Irwin JD. The influence of social-based and physical-based positive emotional appeals on male drivers. Manuscript in preparation 2011.
 41. Lewis I, Watson B, White K, Tay R. Promoting Public Health Messages: Should We Move Beyond Fear-Evoking Appeals in Road Safety? *Qualitative Health Research* 2007; 17(1):61–74.
 42. Sofoulis Z, Noble G, Redshaw S. Youth, Media and Driving Messages: Report of the Transforming Drivers Media Study. Sydney: National Roads and Motorists Association Limited, 2005. Viewed 16 May 2011. http://www.uws.edu.au/__data/assets/pdf_file/0017/46322/Transforming_Drivers_Report_1_-_Youth,_Media_and_Driving_Messages.pdf
 43. Törnros J. Driving behavior in a real and a simulated road tunnel - a validation study. *Accident Analysis and Prevention* 1998; 30(4):497–503.
 44. Bella F. Driving simulator for speed research on two-lane rural roads. *Accident Analysis and Prevention* 2008; 40(3):1078–87.
 45. Hutchinson TP, Wundersitz LN. Road safety mass media campaigns: Why are results inconclusive, and what can be done? *International Journal of Injury Control and Safety Promotion* 2011; (iFirst article):1–7.
 46. Hatfield J, Fernandes R, Faunce G, Job SRF. An implicit non-self-report measure of attitudes to speeding: Development and validation. *Accident Analysis and Prevention* 2008; 40(2):616–627.
 47. Greaves SP, Ellison AB. Personality, risk aversion and speeding: An empirical investigation. *Accident Analysis and Prevention* 2008; 43(5):1828–1836.
 48. Tormala Z, Petty R. Contextual contrast and perceived knowledge: Exploring the implications for persuasion. *Journal of Experimental Social Psychology* 2007; 43(1):17–30.
 49. Pani K, Cauberghe V, De Pelsmacker P. Impact of an Interactive Anti-Speeding Threat Appeal: How Much Threat Is Too Much? *Cyberpsychology, Behavior, and Social Networking* 2011; 14(5):281–9.
 50. Fernandes R, Job R, Hatfield J. A challenge to the assumed generalizability of prediction and countermeasure for risky driving: Different factors predict different risky driving behaviors. *Journal of Safety Research* 2007; 38(1):59–70.
- * Denotes that the publication was included in the systematic review. An Appendix to this article, containing tables which summarise the methodological details for each of the anti-speeding advertisement evaluations included in this review, can be viewed in the online version of this issue of the journal at www.acrs.org.au/publications/journalscurrent.

Appendix: Summary of the methodological details for each of the anti-speeding advertisement evaluations included in this systematic review

Table 2. Studies employing designs with greatest suitability for assessing effectiveness

First author (year) ^[Ref] Design Evaluation Setting Description Peer reviewed status (PR)	Participant details: Sampling (driver status) Gender Age	Intervention details: Mode (real-world status) Emotions Target audience	Intervention Checks: Manipulation Social desirability	Exposure to Intervention: Prior to study During study Nature or exposure	Evaluation Measure(s) Collection Method	Duration of data collection
Algie (2010) ^[10] Randomised trial (mixed) Australia Fear-relief patterns PR	First-year undergraduate university students (drivers) M and F 18–25y	TV ads (real-world) Strong negative and positive (control) emotions Targeted all drivers	Manipulation checks (fear- relief patterns)	Previous exposure Forced exposure	Continuous response measurement (CRM) of fear from <i>very tense</i> to <i>very relieved</i> CRM device	Immediate
Cauberghe (2009) ^[11] Group randomised trial (between) Australia Effect of high and low threat ads and context PR	Undergraduate university students M and F 18–27y	TV ads (designed for the study) Weak and strong negative emotions Targeted young drivers	Manipulation checks (threat severity and fear)	Forced exposure	Message involvement; anti-speeding attitude; anti-speeding intentions Questionnaires	Immediate
Donovan (1999) ^[4] Australia Randomised trial (between) The relationship between production cost and ad effectiveness PR	General public, approached directly at shopping centres and in the city centre (drivers) M and F 17–29y	TV ads (real-world) Weak and strong negative emotions Ads 1 and 2 targeted drivers 24–39y; Ads 3 and 4 targeted drivers 17–29y	Manipulation checks (various emotions and cognitions)	Repeated exposure (x2) Forced exposure	Likelihood of complying with the recommended behaviour; as a passenger, the likelihood of influencing the driver to comply with the recommended behaviour; emotions experienced, and their intensity; perception of the main messages Questionnaires	Immediate

Table 2. Continued

<p>Elliott (2009)^[12] United Kingdom Randomised trial (mixed) Evaluating a print ad based on the TPB PR</p>	<p>Random sampling from Transport Research Laboratory database (UK) M and F M = 49.2y</p>	<p>8-page booklet (designed for the study) Neutral emotions Targeted all drivers</p>	<p>Manipulation check (belief targets)</p>	<p>Forced exposure</p>	<p>Theory of Planned Behaviour (TPB) constructs; intention to comply with speed limits; self reported speeding behaviour Questionnaires</p>	<p>Immediate Follow-up (1 month)</p>
<p>Glendon (2003)^[13] Australia Randomised trial (within) Effectiveness of billboard slogans PR</p>	<p>Undergraduate university students (drivers) M and F 17-25y</p>	<p>Road sign messages (designed for the study) Negative and neutral emotions Targeted all drivers</p>	<p>Manipulation checks (categories; target behaviours)</p>	<p>Forced exposure</p>	<p>Perceived effectiveness; perceived personal influence; agreement with message; self-reported speeding behaviour; perceived crash vulnerability Questionnaires</p>	<p>Immediate</p>
<p>Goldenheld (2008)^[14] The Netherlands Group randomised trial (mixed) Effects of fear evoking ad and neutrally toned information leaflet PR</p>	<p>Invitation letters (drivers) M and F M = 51y</p>	<p>Communication leaflet (designed for the study) Neutral emotions Targeted all drivers TV ad (real-world) Negative emotions Targeted all drivers</p>	<p>Manipulation checks (intervention clarity)</p>	<p>Forced exposure</p>	<p>Perceptions about speeding; attitudes towards introduction of 60 km/h zones; intentions to speed; self-reported speeding Questionnaires</p>	<p>Immediate (only; TV ad) Delayed (only; leaflet)</p>
<p>Lewis (2007)^[15] Australia Group randomised trial (within) Threatening ads and the third person effect PR</p>	<p>University students and acquaintances of the researcher (drivers) M and F Drivers of all age groups</p>	<p>TV ad (real-world) Strong negative emotions Targeted all drivers</p>	<p>Manipulation checks (threat level)</p>	<p>Forced exposure</p>	<p>Attitudes towards speeding; self-reported speeding; future speeding intentions; perceived influence (self; other) Questionnaires</p>	<p>Immediate</p>

Table 2. Continued

Lewis (2008)^[16] Australia Randomised trial (mixed) Comparison of positive and negative emotional ads PR	Invitations mailed to students/staff at a large organisation (drivers) M and F 17–59y	Audio ads (designed for the study) Positive and negative emotions Targeted all drivers	Manipulation checks (evoked emotions)	Forced exposure	Past speeding behaviour; intentions to obey the speed limit; self-reported speeding Online questionnaires	Immediate Follow-up (4 weeks)
Lewis (2010)^[17] Australia Randomised trial (between) Role of response efficacy PR	Invitations mailed to students/staff at a university and a large organisation (drivers) M and F Drivers of all age groups	Audio ads (designed for the study) Positive and negative emotions Targeted all drivers	Manipulation checks (evoked emotions)	Forced exposure	Intentions to monitor and obey the speed limit; self-reported maladaptive responses to the advertisement (e.g. ignoring the ad if it were to appear on TV; perceived response efficacy Online questionnaire	Immediate
Parker (1996)^[18] England Group randomised trial (between) Evaluation of a campaign based on the TPB PR	General public approached directly in the city centre (drivers) M and F 17–40y	TV ads (designed for the study) Neutral, positive and negative emotions Targeted all drivers	No checks	Repeated exposure (x2) Forced exposure	TPB constructs; intentions to speed; self-reported driving behaviour; attitudes towards committing violations Questionnaires	Immediate
Rositer (2004)^[19] Study 1: Group randomised trial (between); Study 2: Randomised trial	First-year undergraduate university students M and F 18–25y	TV ads (real-world) Negative emotions Targeted all drivers	Manipulation checks (fear-relief patterns)	Forced exposure	Continuous response measurement (CRM) of fear from <i>very tense to very relieved</i> ; post-exposure static measures of fear and relief CRM device; questionnaire	Immediate

Table 2. Continued

<p>(mixed) Influence of efficacy of fear appeals PR</p>	<p>Undergraduate university students (drivers) M and F 18–25y</p>	<p>TV ads (real-world) Negative emotions Targeted all drivers</p>	<p>Manipulation checks (fear- relief patterns)</p>	<p>Repeated exposure (x3) Forced exposure</p>	<p>Continuous response measurement (CRM) of fear from <i>very tense to very relieved</i> CRM device (each week); Video Speed Test (VST) (after final exposure)</p>	<p>Weekly (3 consecutive weeks)</p>
<p>Taubman Ben-Ari (2000)^[20] Israel Randomised trials (Study 1: between; Study 2: mixed) Effect of a road trauma video PR</p>	<p>Soldiers in the Israeli Defence Forces M only 18–21y</p>	<p>TV road trauma segment (designed for the study) Strong negative emotions Targeted young male drivers</p>	<p>Social desirability questionnaire</p>	<p>No forced exposure (segment embedded between commercials)</p>	<p>Self-reported intentions to drive recklessly Questionnaire</p>	<p>Immediate</p>
<p>Thornton (2001)^[21] Australia Group randomised trial (mixed) Wear out effects PR</p>	<p>Soldiers in the Israeli Defence Forces M only 18–21y</p>	<p>TV road trauma segment (designed for the study) Strong negative emotions Targeted young male drivers</p>	<p>Social desirability questionnaire</p>	<p>No forced exposure (segment embedded between commercials)</p>	<p>Average driving speed (pre- and post- exposure) Driving simulator</p>	<p>Immediate</p>
<p>Walton (2001)^[22] New Zealand Group randomised trial (within) Perceived influence of ads PR</p>	<p>First-year undergraduate university students (drivers) M and F Age = unknown</p>	<p>TV ads (real-world) Negative emotions Targeted all drivers</p>	<p>Manipulation checks (fear- relief patterns)</p>	<p>Repeated exposure (x3) Forced exposure</p>	<p>Attention paid to the ad; expected effect on speeding behaviour; emotions evoked Questionnaires; VST (after final exposure)</p>	<p>Weekly (3 consecutive weeks)</p>
<p>Walton (2001)^[22] New Zealand Group randomised trial (within) Perceived influence of ads PR</p>	<p>Undergraduate university students (drivers) M and F M = 21y</p>	<p>Slogans (real-world) Negative emotions Targeted all drivers</p>	<p>No checks</p>	<p>Previous exposure (some slogans) Forced exposure</p>	<p>The extent to which the slogans were intended for them and others Questionnaire</p>	<p>Immediate</p>

Table 3. Studies employing designs with moderate suitability for assessing effectiveness

First author (year) ^[Ref] Design Evaluation Setting Description Peer reviewed status (PR)	Participant details: Sampling (driver status) Gender Age	Intervention details: Mode (real-world status) Emotions Target audience	Evaluation Measure(s) Collection Method	Duration of data collection
Brown (2010) ^[23] Australia Cohort study (prospective) Effectiveness of an anti-speeding campaign PR	Randomly selected from an electronic telephone directory (drivers and non-drivers) M and F M = 42y	TV, radio and billboard ads (real-world) Negative emotions Targeted all drivers	Risk perception; self reported speeding behaviour; campaign exposure Telephone interviews	14 weeks
Cameron (2003) ^[24] Australia Time series (with comparison – districts) Interaction between ads and enforcement	Data obtained from VicRoads M and F Age = unknown	Transport Accident Commission (TAC) ads (real-world) Negative emotions Targeted all drivers	Casualty crashes; serious crashes; fatal crashes; awareness of advertising and enforcement; self-reported intention to speed Telephone interview (awareness and intentions)	5 years
Fry (1996) ^[25] Australia Time series (with comparison – regions; low/high alcohol hours) Evaluation of the TAC campaigns PR	Data obtained from the Australian Bureau of Statistics, Victorian Police, VicRoads M and F Age = unknown	TV, radio, billboard ads (TAC; real-world) Strong negative emotions Targeted all drivers (particularly young males)	Serious casualty crashes; number of speed camera infringement notices issued	11 years
Stead (2005) ^[26] Scotland Cohort study (prospective) Evaluation of the <i>Footspeed</i> campaign PR	Door-to-door knocking (drivers) M and F 17–54y	TV ads (real-world) Negative and neutral emotions Targeted all drivers	TPB constructs; awareness, recall, involvement and perceptions of key messages; intentions to speed; self-reported speeding behaviour Face-to-face interviews	4 years
Whittam (2006) ^[27] USA Time series (comparison – regions) Evaluation of the <i>What's the Hurry?</i> Campaign PR	Young driver crash statistics (State of Tennessee) M and F 16–19y	TV, radio and billboard ads (real-world) Strong negative emotions Targeted young drivers and their parents	Crash rates of at-fault drivers; awareness and recall of campaign; parental reports of changes to driving privileges or restrictions Telephone interview (awareness and self-reported changes to privileges)	6 years (monthly)

Table 4. Studies employing designs with least suitability for assessing effectiveness

First author (year) ^[Ref] Design Evaluation Setting Description Peer reviewed status (PR)	Participant details: Sampling (driver status) Gender Age	Intervention details: Mode (real-world status) Emotions Target audience	Evaluation Measure(s) Collection Method	Duration of data collection
Angle (2009) ^[28] Great Britain Before-after (no comparison) Evaluation of <i>Live With It</i> campaign	Random location sampling (drivers and non- drivers) M and F 15y+	TV, radio, poster and online ads (real- world) Strong negative emotions Targeted all drivers	Attitudes towards speeding (acceptance of speed limits, personal responsibility, risks and acceptability of speeding); recall, prompted recognition; opinions about campaign Interviews	4 months (pre- and post- attitudes) 1 week (post- recall)
Divjak (2009) ^[29] Slovenia Before-after (no comparison) Effectiveness of the <i>Speeding is Worth Regretting!</i> Campaign	Random sampling via phone directory (drivers and non-drivers) M and F Ms = 34.2y (pre-) and 36.8y (post-)	TV, radio and billboard ads (real- world) Strong negative emotions Targeted all drivers	TPB constructs; perceived crash risk; reported intentions to speed; self-reported speeding; campaign recall Online questionnaires; telephone interviews (campaign recall)	2 weeks pre exposure – 2 weeks post exposure
Guria (2005) ^[30] New Zealand Time series (no comparison) Evaluation of shock ads to supplement enforcement PR	Data obtained from the Traffic Crash Report database of police-reported injury crashes (LTSA) M and F Age = unknown	TV ads (real-world) Strong negative emotions Targeted all drivers	Number of: fatal crashes; fatalities; non- motorcycle fatalities; serious casualties; serious non-motorcycle casualties	10 years
Mulholland (2005) ^[31] Australia Time series (no comparison) Evaluation of the <i>Wipe Off</i> 5 campaign	Data obtained from VicRoads M and F Age = unknown	TV, radio and billboard ads (TAC; real-world) Negative (unspecified) emotions Targeted all drivers	Number of: fatalities; serious injuries; self reported speeding behaviour; on-road travelling speeds; recall of ad; attitudes toward the ad and speeding	4 years

Table 4. Continued

<p>Tay (2005)^[32] Australia Time series (no comparison) Interaction between TAC ads and enforcement PR</p>	<p>Data obtained from VicRoads M only Age < 25y</p>	<p>TV, radio and billboard ads (TAC; real-world) Negative emotions Targeted all drivers</p>	<p>Number of serious crashes involving at least one young male driver; number of speeding tickets issued</p>	<p>6.5 years (monthly)</p>
<p>Tay (2002)^[33] New Zealand Time series (no comparison) Effectiveness of a TV ad campaign PR</p>	<p>Data obtained from LTSA's Accident Investigation System M and F Age = unknown</p>	<p>TV ad (real-world) Strong negative emotions Targeted all drivers (mainly males, 18–24y)</p>	<p>Number of speed-related fatal crashes</p>	<p>9.5 years (monthly)</p>
<p>Taylor (2001)^[34] Australia Time series (no comparison) Evaluation of an anti-speeding campaign</p>	<p>On-road speed data collected at six locations on the Adelaide metropolitan arterial road network M and F Age = unknown</p>	<p>TV ads (real-world) Various negative emotions Targeted all drivers (mainly young males)</p>	<p>On-road free driving speeds (km/h)</p>	<p>3.5 years (with 6 test weeks per year)</p>