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Meta-analysis of the effect of road safety campaigns on accidents

Ross Owen Phillips[a,*], Pål Ulleberg[b], Truls Vaa[a,*]

[a] Institute of Transport Economics, Gaustadalléen 21, 0349 Oslo, Norway
[b] Department of Psychology, University of Oslo, PB 1094, Blindern, 0317 Oslo, Norway

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ABSTRACT

A meta-analysis of 67 studies evaluating the effect of road safety campaigns on accidents is reported. A total of 119 results were extracted from the studies, which were reported in 12 different countries between 1975 and 2007. After allowing for publication bias and heterogeneity of effects, the weighted average effect of road safety campaigns is a 9% reduction in accidents (with 95% confidence that the weighted average is between −12 and −6%). To account for the variability of effects measured across studies, data were collected to characterise aspects of the campaign and evaluation design associated with each effect, and analysed to identify a model of seven campaign factors for testing by meta-regression. The model was tested using both fixed and random effect meta-regression, and dependency among effects was accounted for by aggregation. These analyses suggest positive associations between accident reduction and the use of personal communication or roadside media as part of a campaign delivery strategy. Campaigns with a drink-driving theme were also associated with greater accident reductions, while some of the analyses suggested that accompanying enforcement and short campaign duration (less than one month) are beneficial. Overall the results are consistent with the idea that campaigns can be more effective in the short term if the message is delivered with personal communication in a way that is proximal in space and time to the behaviour targeted by the campaign.

1. Introduction

Each year more than 40,000 people are killed in accidents on European roads (Delhomme et al., 2009). Planners and policy-makers aiming to reduce this number continue to invest in road safety campaigns even though there remains little consensus about their efficacy after sixty years of study (Delaney et al., 2004; Hyman and Sheatsley, 1947; Mendelsohn, 1973). Debate among researchers has become somewhat polarised, some claiming it is also clouded by attempts to explain behavioural change in terms of attitude concepts (OECD, 1993, 1994). Lack of agreement among practitioners is rooted both in the lack of guidance from research and in differences between their own subjective experiences of campaigns with widely different properties and contexts. The situation is also exacerbated by the different accident measures and designs used by the studies evaluating campaigns (Tay, 2001).

To overcome these difficulties there have been calls for systematic syntheses of the research on road safety campaign effectiveness (Black, 2001; Morrison et al., 2003). Meta-analysis is a statistical technique used to systematically summarise the results of a group of individual studies with a common research hypothesis and a common measure of effect (Elvik, 2005). The method has been used to summarise the effects of various road safety interventions (e.g. Elvik, 1996; Erke, 2009), including road safety campaigns (Elliot, 1993; Hagenzieker et al., 1997; Delhomme et al., 1999; Elvik and Vaa, 2004; Vaa et al., 2004).

To our knowledge, three meta-analytic studies have addressed the effects of campaigns on accidents. Summarising 13 studies, Elvik and Vaa (2004) conclude that campaigns reduce accident levels by somewhere between 0 and 49%, depending on the type of campaign and accident measure used. Delhomme et al. (1999) summarise 72 effects from 35 evaluation studies, most of which are also reviewed and summarised by Vaa et al. (2004). Both these studies conclude that road safety campaigns are associated with an overall reduction of 9% while campaign activities are ongoing, increasing to 15% following campaign completion. Both studies also find a large variation in campaign effect, probably reflecting large differences in how the road safety campaigns summarised have been implemented.

By carrying out an updated and expanded meta-analysis, the present study aims to develop our current understanding of road safety campaigns. In particular, it aims to document important differences in content and delivery method between road safety campaigns, and use those differences to explain any systematic variation among the effects the campaigns have on accidents.

The content of a campaign – essentially the nature of the message(s) it delivers – will clearly influence its effect. Any message

* Corresponding authors. Tel.: +47 22 57 38 20; fax: +47 22 60 92 00.
E-mail addresses: rph@toi.no (R.O. Phillips), pal.ulleberg@psykologi.uio.no (P. Ulleberg), tva@toi.no (T. Vaa).

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seeking to effect a change in behaviour must be persuasive. However, there is little consensus among previous campaigns about what types of message are most persuasive. For example, persuasion is often attempted rationally, through the presentation of facts or figures, but research shows that the effect can be larger if an emotional message is used (Elliot, 1993; Ulleberg and Vaa, 2009). The emotion invoked in the target audience can be negative, e.g. shock or fear appeals (Lewis et al., 2008), or positive, e.g. humour appeals (Weinberger and Gulas, 1992). Research on fear appeals suggests that their effects are weak, limited by defensive responses and dependent on personal relevance (Ulleberg and Vaa, 2009). Research on humour appeals is more limited, with some studies finding weakly positive effects and others finding neutral effects (Ulleberg and Vaa, 2009). Faced by the array of strategies used by previous campaigns and ambiguous recommendations from researchers it is difficult for practitioners to conclude anything about the emotional content their message should have (see e.g. SWOV, 2008; Lewis et al., 2007; Ulleberg and Vaa, 2009). The situation is typical of other aspects of campaign content. For instance, using either factual or emotional persuasion, many campaigns aim to highlight the risks associated with certain road user behaviour, though again it is not clear whether it is advantageous to do so (Elliot, 1993; Weber et al., 2006; Lewis et al., 2007; Williams et al., 1996; Snyder, 2001).

If a campaign message is persuasive, it will only be effective in terms of behavioural change and accident reduction if it reaches the target audience. In designing a campaign, it is thus important to consider effective ways to deliver the message. Again, however, there is little guidance on which method(s) is best. Delivery via mass media channels might be expected to increase the effectiveness of the campaign on the basis of the greater exposure achieved. However, the effectiveness of mass media channels alone has been questioned in the fields of both traffic safety (see e.g. Elliot, 1993; Vaa et al., 2004) and health promotion (Wilde, 1993; Donnerstein and Linz, 1995; Snyder, 2001), partly on the grounds that the audience is likely to be exposed to mass media at a time and place that is far removed from the context in which the targeted road user behaviour occurs. In addition, exposure in and of itself is not sufficient to guarantee that the target will attend to and elaborate the message, a process thought to increase the likelihood that they will subsequently change their behaviour (see e.g. Petty et al., 2009), and one which is best achieved using interpersonal communication rather than mass media (see e.g. Berger, 2005; Rice and Atkins, 2001; Petty et al., 2009; Ulleberg and Vaa, 2009; Vaa et al., 2004).

Irrespective of message content or delivery method used, accompanying enforcement activity by the police, to increase the saliency of punitive risks, has been found to be effective in reducing the number of road accidents (Elvik and Vaa, 2004), but little has been done to describe the campaign types and contexts for which enforcement works best.

In summary, there is a need to know what makes road safety campaigns effective. Given that previous campaigns vary widely along several dimensions, an effective method is needed to determine how much of the variation in road safety campaign effect can be explained by those differences. One such method is meta-regression, which can be included as part of a meta-analysis. Several models are described for conducting meta-regression – predominantly fixed and random effect models – but there is some disagreement about which is the best to use (Higgins and Thompson, 2004; Poole and Greenland, 1999; Schultz and Altman, 1993; Shadish and Haddock, 1994; Hardy and Thompson, 1996).

To our knowledge only one study has attempted to explain the variation in effect of campaigns on accidents using meta-regression (Vaa et al., 2004). This study was, however, somewhat compromised by the large number of predictors tested, the potential problem of dependence of effect sizes extracted from the same evaluation study, and a lack of clarity about the meta-regression model used (Higgins and Thompson, 2004).

The present study uses meta-regression to identify, from available evaluations which factors, describing the nature of the campaign message and how it is delivered, are associated with significant variation in campaign effect. We attempt to base the meta-regression described on a large number of individual estimates and a limited number of predictors.

2. Method

2.1. Study design

2.1.1. Selection of studies

Evaluation studies of interest contained at least one estimate of the effect of a road safety campaign, as defined by Delhomme et al. (2009).[1] We did not limit the search to publicised evaluation studies, but rather sought to include any suitable evaluation we could access. We did not exclude non-controlled evaluation studies, but from the outset decided only to do so in the event that preliminary meta-analysis suggested that those effects derived from non-controlled evaluation designs were significantly different in size and variation from those derived from controlled evaluations. We did not limit the number of effects extracted from each single evaluation study, but instead collected all orthogonal effects reported, and checked for any signs of study-level dependence among the effects.

Evaluation studies were retrieved from the following sources:

(i) Road safety campaign evaluation studies identified by Delhomme et al. (1999), Elvik and Vaa (2004), and Vaa et al. (2004) were retrieved and reanalysed, and in this way 43 suitable studies identified, up until 2004.
(ii) Formal requests were made in 2008 to partners in European countries to search for and retrieve campaign evaluation data from their respective home countries. Eight suitable studies were identified in this way.
(iii) New evaluation studies not previously included in meta-analysis were retrieved through normal literature searching. Evaluations published up until 2010 were searched for. Sixteen suitable studies were identified in this way.

Thus a total of 67 suitable studies were retrieved for meta-analysis.

2.1.2. Data collection and variables considered

Each effect was entered into successive cells of a column in an Excel spreadsheet, and data on variables describing how the campaign in question was delivered (delivery variables) and the type of message or material delivered (content variables) entered systematically into the row alongside each effect. Data were also entered describing the evaluation study from which each effect was derived (study variables). Effects were expressed in terms of the change in the number of accidents coinciding with the event of a road safety campaign (see Section 2.2). The delivery, content and study variables collected are detailed in Table 1.

Evaluation studies were characterised using eight variables. These included *timing*, i.e. whether accident levels were

[1] [A road safety campaign is a]. . .purposeful attempt to inform, persuade and motivate a population (or subgroup of a population) to change its attitudes and/or behaviours to improve road safety using organised communications involving specific media channels within a given time period, often supplemented by other safety-promoting activities (enforcement, education, legislation, enhancing personal commitment, rewards, etc.)."

Table 1
Coding sheet.

Category	Variable	Level
Study	Author	Single
	Date	Single (year)
	Title	Single
	Publication medium	1. Scientific (in peer-reviewed journal), 2. Report (research institution), 3. Unpublished, 4. Government report, 5. Conference, 6. Not stated, 7. Company report, 8. Popular journal, 9. Safety association report, 10. Dissertation, 11. EU report, 12. Trade journal
	Measure used	1. All accidents, 2. Property only accidents, 3. Accidents resulting in injury/casualties, 4. Accidents resulting in minor injury, 5. Accidents resulting in serious injury, 6. Accidents resulting in fatality, 7. Drink-driving accidents, 8. Child dart-and-dash accidents, 9. Accidents involving defined age group, 10. Pedestrian–motor vehicle collisions, 11. Rear-end collisions, 12. Off-road accidents, 13. Accidents involving trucks
	Timing	0. Before–during, 1. Before–after, 2. Before–during–after
	Control	0. No, 1. Yes
	Country	1. Australia, 2. Belgium, 3. Canada, 4. Denmark, 5. Finland, 6. France, 7. Germany, 8. Israel, 9. Japan, 10. Korea, 11. Kuwait, 12. The Netherlands, 13. New Zealand, 14. Norway, 15. Sweden, 16. UK, 17. USA, 18. Austria, 19. Portugal, 20. Czech Republic, 21. Poland, 22. Italy, 23. Multinational
Delivery	Campaign level	1. Local (small scale campaign in city, municipality, company), 2. Regional (e.g. one or more counties or federal states), 3. National (whole country), 4. Mixed level
	Number of groups targeted	1. Not specified, 2. One target group, 3. More than one target group
	Size of target group[a]	Single continuous variable (number of people)
	Size of catchment area[a]	Single continuous variable (number of people)
	Duration	1. 0–29 days, 2. 30–200 days, 3. Other
	Accompanying enforcement	0. No, 1. Yes
	Accompanying law change	0. No, 1. Yes
	Television	0. No, 1. Yes
	Radio	0. No, 1. Yes
	Newspapers	0. No, 1. Yes
	Posters	0. No, 1. Yes
	Billboards	0. No, 1. Yes
	Leaflets	0. No, 1. Yes
	Reward	0. No, 1. Yes
	Video/DVD/cinema	0. No, 1. Yes
	Variable message signs	0. No, 1. Yes
	Fixed message signs	0. No, 1. Yes
	Competitions	0. No, 1. Yes
	Several smaller measures[a]	0. No, 1. Yes
	Personal communication	0. No, 1. Yes
Content	Basis on prior consultation	0. No, 1. Yes
	Basis on theory	0. No, 1. Yes
	Basis on prior campaign	0. No, 1. Yes
	Campaign theme	1. General road safety campaign or campaign with several themes, 2. Speed, 3. Drink-driving, 4. Seatbelts, 5. Other
	Use of risk message	0. No, 1. Yes
	Risk of harm to self	0. No, 1. Yes
	Risk of harm to others	0. No, 1. Yes
	Risk of detection	0. No, 1. Yes
	Consequences shown	0. No, 1. Yes
	Shocking effects[a]	0. No, 1. Yes
	Humour[a]	0. No, 1. Yes
	Social norm[a]	0. No, 1. Yes
	Emotional, rational, incentive	0. Emotional, 1. Rational, 2. Both emotional and rational, 3. Incentive

[a] Not enough information available in evaluation studies to allow use in subsequent analyses.

measured before-and-after, before-and-during or before-during-and-after the campaign; and *control*, i.e. whether or not the evaluation used a control study design. Some variables were collapsed for the final analyses, e.g. the *country* of each campaign was initially recorded, and later collapsed into a continent variable. Final variable levels are given in Section 3 in Table 2.

Data on twenty delivery variables were collected describing the campaign's scope; use of specific delivery media, including use of roadside delivery of the message (*fixed* or *variable message signs*, *posters*, *billboards*); and the number and nature of any road user group targeted. Use by a campaign of a certain type of delivery medium was indicated only where it was evident from the evaluation study that a media channel was employed actively and directly as part of the campaign strategy. We also registered whether or not a campaign employed a *personal* means of *communication* as part of its delivery strategy. In line with Vaa et al. (2004), types of communication categorised as personal were lessons or seminars delivered in person, two-way discussions with a teacher, peer, safety expert or distributor of campaign media, group discussions or personally addressed letters. Accompanying *enforcement* activity was registered for campaigns that explicitly drew attention to either pre-existing levels of ongoing police enforcement activity or an increase in normal levels of activity; any accompanying *law change* was also noted. The final variable levels used in analyses are given in Section 3 in Table 3.

Table 2
Overall effect summaries for individual effects grouped according to study variables. Unless indicated otherwise, the summaries are estimated using a random effects model. Publication bias is not adjusted for.

Study variable	Variable level	No. effects	Test of heterogeneity		Proportion of statistical weight[a]	% change in accidents		
			Cochrane's Q	p		Lower 95%	Estimate	Upper 95%
Measure used	All accidents	23	38	.02	0.05	−16	−11[a]	−6
	Injury accidents	23	98	<.001	0.31	−13	−8	−2
	Serious injury accidents	4	–	–	0.08	–	–	–
	Fatal accidents	8	18	.01	0.05	−22	−11	+1
	Drink-driving accidents	26	196	<.001	0.27	−23	−17	−10
	Other/undefined	35	–	–	0.24	–	–	–
Timing	Before–during	86	430	<.001	0.73	−16	−13	−9
	Before–after	21	99	<.001	0.19	−19	−11	−3
	Before–during–after	12	41	<.001	0.09	−16	−7	+3
Control	No	27	135	<.001	0.29	−16	−10	−4
	Yes	92	446	<.001	0.71	−16	−12	−9
Location	Europe	51	181	<.001	0.26	−16	−11	−5
	North America	33	120	<.001	0.44	−14	−10	−6
	Australasia	31	252	<.001	0.29	−20	−14	−7
Publication medium	Peer-review journal	21	110	<.001	0.24	−19	−12	−5
	Institute report	76	378	<.001	0.60	−14	−10	−6
	Other	22	92	<.001	0.16	−25	−18	−10
Decade	1980s	34	281	<.001	0.40	−22	−16	−10
	1990s	63	201	<.001	0.40	−15	−11	−7
	2000s	20	50	<.001	0.20	−10	−5	+0

[a] Based on fixed effects model.

Table 3
Overall effect summaries for individual effects grouped according to delivery variables. Unless indicated otherwise, the summaries are estimated using a random effects model. Publication bias is not adjusted for.

Delivery variable	Variable level	No. effects	Test of heterogeneity		Proportion of statistical weight[a]	% change in accidents		
			Cochrane's Q	p		Lower 95%	Estimate	Upper 95%
Scale	Local	23	75	<.001	0.16	−17	−9	−1
	Regional	79	442	<.001	0.72	−17	−13	−9
	National	16	37	.001	0.12	−13	−7	+1
Target group	Not specified	47	194	<.001	0.46	−13	−9	−4
	Specified	71	382	<.001	0.54	−18	−14	−9
Duration (days)	0–29	7	5	.54	0.03	−21	−15[a]	−9
	30–200	47	151	<.001	0.30	−16	−11	−6
	More than 200	64	425	<.001	0.67	−16	−12	−7
Accompanying measure	Enforced							
	Yes	80	386	<.001	0.77	−16	−13	−9
	No	34	165	<.001	0.23	−16	−10	−3
	Law change							
	Yes	9	50	<.001	0.15	−17	−9	0
	No	107	531	<.001	0.85	−16	−12	−9
Personal communication	Yes	27	135	<.001	0.18	−23	−16	−9
	No	90	446	<.001	0.82	−14	−10	−7
Media	Television							
	Yes	97	549	<.001	0.86	−15	−12	−8
	No	19	28	.06	0.14	−14	−11[a]	−8
	Radio							
	Yes	83	413	<.001	0.69	−15	−12	−8
	No	33	162	<.001	0.21	−17	−11	−4
	Newspaper							
	Yes	82	381	<.001	0.75	−15	−11	−7
	No	33	180	<.001	0.25	−18	−12	−5
	Leaflet							
	Yes	40	231	<.001	0.42	−16	−10	−5
	No	75	345	<.001	0.58	−16	−12	−8
	Vid/cinema							
	Yes	15	26	.03	0.06	−19	−15[a]	−10
	No	101	548	<.001	0.94	−15	−11	−8
	Roadside							
	Yes	32	178	<.001	0.38	−17	−12	−6
	No	84	393	<.001	0.62	−15	−12	−8

[a] Based on fixed effect model.

Table 4
Overall effect summaries for individual effects grouped according to content variables. Unless indicated otherwise, the summaries are estimated using a random effect model. Publication bias is not adjusted for.

Content variable	Variable level	No. effects	Test of heterogeneity		Proportion of statistical weight[a]	% change in accidents		
			Cochrane's Q	p		Lower 95%	Estimate	Upper 95%
Basis	Stated basis?							
	Yes	50	348	<.001	0.56	−14	−9	−4
	No	66	224	<.001	0.44	−18	−14	−9
Theme	General-mixed	9	120	<.001	0.19	−25	−14	−1
	Speeding	26	55	<.001	0.21	−10	−4	+1
	Drink-driving	41	234	<.001	0.40	−23	−18	−12
	Other	35	73	<.001	0.20	−12	−7	−1
General content	Emotional	4	–	–	0.07	–	–	–
	Rational	52	203		0.50	−14	−10	−5
	Emotional+ rational	29	282	<.001	0.35	−21	−15	−7
	Incentive	3	–	–	0.07	–	–	–
Risk (harm)	Risk of harm highlighted							
	Yes	22	64	<.001	0.17	−14	−8	−2
	No	92	493	<.001	0.83	−16	−13	−9
Risk (detection)	Risk of detection highlighted							
	Yes	52	353	<.001	0.68	−17	−13	−8
	No	62	209	<.001	0.32	−16	−11	−6

Data were collected on thirteen content variables. A broad variable was defined to capture whether the individual effect was associated with a campaign attempting *emotional* persuasion, *rational* persuasion, or persuasion by *incentive*. Attempts at rational persuasion were characterised by the dominant use of facts, statistics and information, while emotional persuasion was characterised by the use of messages that, according to the researcher, would arouse a sense of guilt, sympathy, fear or fun in the target audience. To inform about the dimension of any emotional persuasion used, variables were also collected to indicate whether *shock* or *humour* was used. Registrations were made to indicate whether the effect was for a campaign emphasising the risk associated with unsafe behaviour. The associated risk was registered either as *risk of detection* by the police or *risk of harm to self* or *other* road users. The content of the message is believed to be most effective if it is evolved through thorough preparation and pre-testing (see e.g. Kotler and Roberto, 1989; Solomon, 1989; Stead et al., 2006), so we recorded any *basis* for the road safety campaign, in the form of a *consultation* with a sample of the target group, results from *previous campaigns*, or constructs drawn from psychological or social marketing *theories*. The *theme* of the campaign was also recorded. Variable levels used in final analyses are given in Section 3 in Table 4.

2.1.3. Development of candidate variables for entry into meta-regression

Candidate predictors were selected initially on the following grounds.

- The predictor must make theoretical sense, i.e. there must be a good reason to believe that the variable could influence the way a road safety campaign affects accident levels.
- Together with the other predictors, the predictor must capture efficiently a range of possible influences on campaign effect.
- There must be information about the predictor in most of the campaign evaluation studies from which effects were derived (because meta-regression is sensitive to missing cases).

The initial list was tested and evolved by statistical analysis. Zero-order correlations among variables were inspected to reconcile any highly correlating sets, and subgroup analyses and output of an initial weighted simultaneous regression run (to examine tolerance relationships among variables) were consulted with a view to capture important influences on campaign effects in the most efficient way.

2.1.4. Special concerns

2.1.4.1. Publication bias. Publication bias denotes “a tendency not to publish a study if its findings are not statistically significant or are regarded as unwanted or difficult to explain” (Høye and Elvik, 2010). To test whether the road safety campaign evaluations retrieved suffer from publication bias, the final set of weighted effects, obtained using the numerical method below, was tested and adjusted for publication bias using the ‘trim-and-fill’ method of Duval and Tweedie (2000a,b). Two estimators of publication bias (R and L) were generated using an algorithm described in Christensen (2003). Publication bias was adjusted for if the value of either estimator indicated that there were more than five effects missing on the ‘less favourable’ side of the distribution. Checks and adjustments for publication bias were performed only for whole sample of campaign effects. They were not performed for subgroup analyses because (i) the assessment based on the whole sample of effects is considered to give the most reliable estimation of the level of bias among the smaller subgroups; (ii) we could think of no sensible reason why publication bias should be different among different subgroups of effects; and (iii) the subgroup analyses were used to inform development of the multivariate model for analysis by meta-regression, which cannot account for publication bias.

2.1.4.2. Measuring and accounting for heterogeneity. The most widely used meta-analytic models – the fixed and random effect models – differ in the way they treat heterogeneity. The fixed effect model is based on the assumption that there is no systematic variation in the set of effects considered, i.e. that the variation in estimates of campaign effect can be completely accounted for by random variation in the number of accidents. In contrast, the random effect model regards the variation in effects for a set of evaluated campaigns as systematic, and the statistical weight assigned to each result is therefore modified to include a constant reflecting the level of systematic variation present among the estimated effects. While fixed effect meta-regression gives more power to detect any relationships between explanatory variables and effect size, computer simulations indicate that the method is insufficiently conservative (Higgins and Thompson, 2004). On the other hand the random effect model reports very conservative estimates

of statistical significance to the detriment of the power to explain variance in effect size (see e.g. Lipsey and Wilson, 2001). By apportioning relatively greater weight to smaller studies, random-effect methods can also be overly susceptible to publication bias (Poole and Greenland, 1999). In our analysis heterogeneity was tested for, and a random effect model used to describe the data where the heterogeneity in the set of effects summarized was significant.

To explain heterogeneity in the sample of effects, the final list of candidate variables was analysed using fixed and random-effect meta-regression using the 'metareg.sps' SPSS macro which generates maximum likelihood random effect variance component using a method based on Eq. (7), in which Q is Q for the residual (Lipsey and Wilson, 2001). Using this variance component, random effect weights were calculated for each effect size, and the regression re-run using the new weights.

2.1.4.3. Investigating dependency among individual effects. Most of the evaluation studies included in this study report a single effect size. Some studies, however, reported more than one effect for the same campaign, either because the same campaign was run on different populations or because two or more orthogonal measures of accident counts were reported (e.g. number of light and number of heavy injuries). This may violate the assumption of independence of observations in regression analysis, and thus make both parameter estimates and their standard errors biased. Thus to account for any effects of dependency among effect sizes in meta-regression, the different effect sizes reported were aggregated into a single effect measure for each campaign, and fixed and random effect meta-regression analyses re-run using the explanatory variables (Lipsey and Wilson, 2001). Aggregation was achieved by taking the weighted mean effect in which the weight is the sum of weights of all outcomes reported by the study.

2.2. Numerical method

2.2.1. Estimates of individual effect

Changes in accident counts were expressed as odds ratios. Thus for before-and-after study designs, the basic effect was extracted as:

$$\mathrm{ES} = \frac{a/b}{a_{\mathrm{con}}/b_{\mathrm{con}}} \quad (1)$$

where ES is the effect size, a the accident counts recorded after a campaign, b the accident counts before a campaign, and a_{con} and b_{con} corresponding counts for control populations. In the case of before–during study designs, accident counts recorded during the campaigns replaced counts recorded after the campaign in Eq. (1). In the case of before–during–after study designs, before–after and before–during effect sizes were averaged. The main denominator in Eq. (1) was omitted when extracting individual effects for non-controlled studies.

2.2.2. Fixed effect weights for individual effect estimates

According to Lipsey and Wilson (2001), optimal weights for meta-analysis are the inverse variances:

$$w = \frac{1}{se^2} \quad (2)$$

where w is the weight of an individual effect and se is the standard error. The standard error is itself a direct index of effect size precision. In the case of an individual effect derived from a before–after controlled study design it was derived as follows (Lipsey and Wilson, 2001):

$$se^2_{\mathrm{con}} = \frac{1}{a} + \frac{1}{b} + \frac{1}{a_{\mathrm{con}}} + \frac{1}{b_{\mathrm{con}}} \quad (3)$$

where se_{con} is the standard error for a controlled study design, and a, b, a_{con} and b_{con} are defined in (1). The assumption here is that road safety campaign effects based on more accident counts should receive greater weight because they tend to have lower variance and are therefore more likely to estimate the 'true' effect accurately. The standard error for those effects extracted from studies lacking control groups was doubled so that a fair comparison could be made with weighted effects derived from controlled studies. Thus for a before–after study:

$$se^2_{\mathrm{noncon}} = \frac{2}{a} + \frac{2}{b} \quad (4)$$

where se_{noncon} is the standard error for a non-controlled study design.

Weights of effects from before–during studies were calculated as indicated for those from before–after studies, with accident counts during the campaign replacing accident counts after the campaign. For those single effects derived from before–during–after studies, extra weight was accorded by adding together the before–after and before–during weights.

2.2.3. Random effect weights for individual effect estimates

Statistical weights for the random effects model, w_{RE}, were generated from the fixed effect weights by including an additional estimator of between study variance, υ_{BS}, as follows:

$$w_{\mathrm{RE}} = \frac{1}{se^2 + \upsilon_{\mathrm{BS}}} \quad (5)$$

Between-study variance is in turn generated using Q, which according to Christensen (2003) can be considered as a function of the weighted squared differences between the natural logarithm of study effect estimates, ln ES, and the natural logarithm of the fixed effects summary effect, $\ln \overline{\mathrm{ES}}$ (see below for the explanation of ES).

$$Q = \Sigma w(\ln \mathrm{ES} - \ln \overline{\mathrm{ES}})^2 \quad (6)$$

Thus:

$$\upsilon_{\mathrm{BS}} = \frac{Q - k - 1}{\Sigma w - (\Sigma w^2/\Sigma w)} \quad (7)$$

where k is the number of studies on which Q is based (Lipsey and Wilson, 2001).

2.2.4. Grouping and summarising weighted individual effect estimates

The true overall mean was first estimated using the log odds method and a fixed effect model, as follows (Christensen, 2003):

$$\overline{\mathrm{ES}} = e^{(\Sigma \ln \mathrm{ES}\, w)/\Sigma w} \quad (8)$$

where $\overline{\mathrm{ES}}$ is the weighted mean effect size; e denotes the exponential function; ln ES, the natural logarithm of each individual effect estimate; and w the weight of each individual effect estimate (Eq. (2)). Eq. (8) was used to generate overall estimates for the whole sample of individual effects and to compare overall effects for subgroups of individual effects, where each subgroup of effects was defined according to their common values on delivery, content or study variables.

To help interpret the overall mean of the whole sample of accident effects, a funnel diagram was generated by plotting each individual road safety campaign effect against its corresponding weight (see Fig. 1 in Section 3). The resulting scatterplot was analysed to check (a) that the sample was suitable for summary by meta-analysis, and (b) whether there were visual signs of publication bias (Light and Pillemer, 1984; Elvik, 1998).

The above procedure was used to summarise the data using a fixed effect model. Subsequently, the chi-squared distributed statistic, Q, was generated to test the null hypothesis that there is no

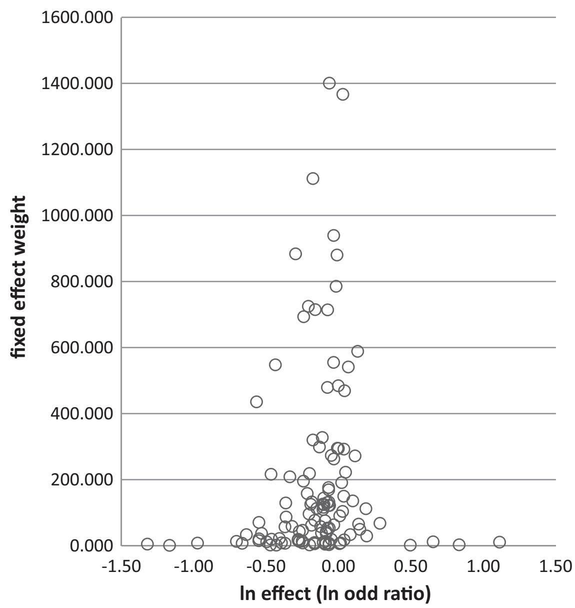

Fig. 1. Scatterplot showing the natural logarithm of each individual effect plotted against its statistical weight (w).

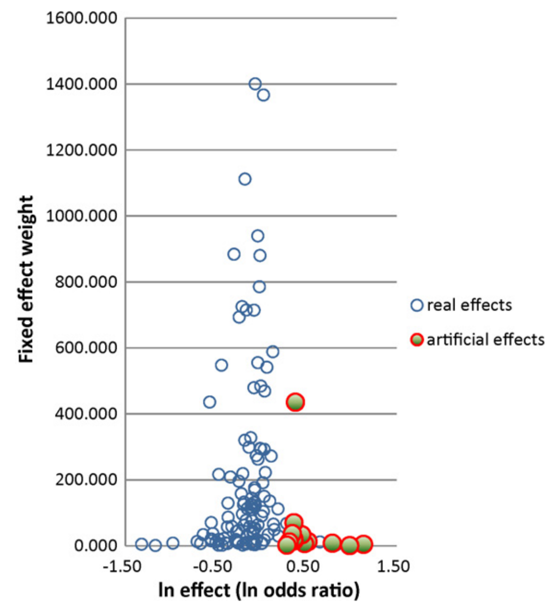

Fig. 2. Correcting for publication bias using the trim-and-fill method (Duval and Tweedie, 2000a,b). The scatterplot in Fig. 1 has been supplemented with 13 artificial effects, generated to adjust for publication bias.

heterogeneity above that due to within-study error (Eq. (6)). Where Q was significant ($\alpha = .01$), heterogeneity was assumed and an alternative random effect model employed to describe the effects. w_{RE} was used in place of w in Eq. (8) for calculating weighted means using a random effect model.

3. Results

3.1. Overall effects of road safety campaigns on accidents

A meta-analysis of 119 individual estimates of the effect of campaigns on accidents using a fixed-effect model gave a combined estimate that road safety campaigns coincide with a 10% reduction in accidents, with 95% confidence that the population mean effect is between −11% and −9%. Statistical tests indicated the presence of systematic heterogeneity in the data set ($Q = 584$; $p < .01$). Allowing for this heterogeneity using a random effect model led to a similar overall estimate of the level reduction in accidents (now −12%), but as expected the 95% confidence interval was larger, between −15% and −9%.

A visual inspection of the scatterplot (Fig. 1) showed that the data were consistent with our assumption that the effects are distributed either side of a true mean whose value is better approximated by those effects with greater statistical weight. The tails of the plot were also asymmetrical, consistent with a hypothesis of publication bias, and confirmed by statistical estimators ($R = 0$; $L = 13$).

Thirteen ‘artificial’ effects were generated to adjust for this bias (Fig. 2).

Allowing for heterogeneity, meta-analysis on the supplemented set of effects gave an overall estimate that road safety campaigns coincide with a 9% reduction in accidents (95% confidence interval, −12%; −6%).

3.2. Subgroup analyses

To characterise the campaigns and evaluation studies on which these overall effect estimates are based, summary estimates were obtained for sets of effects grouped on each of the collapsed study, deliverable and content variables.

3.2.1. Study method

Table 2 shows that most of the statistical weight in the overall meta-analysis result is assigned to effects based on changes in injury accidents and drink–drive accidents, and that those campaign evaluations using drink–drive accidents as measures of effect tend to report greater effects.

A greater part of the statistical weight, some 73%, is assigned to those effects based on accident levels recorded before and during a campaign. Most (71%) weight was from effects derived from evaluation studies using control designs. There is no significant difference between an overall summary of campaign effects based exclusively on these controlled effects (12% reduction; 95% confidence interval −9 to −16%) and a summary based solely on non-controlled effects (10% reduction; 95% confidence interval −4 to −16%). A slight tendency for those campaign effects published in institute reports to be weaker than those published in reviewed journals is consistent with an assumption of publication bias, with other sources (e.g. professional or trade journals, personal communications) possibly containing more bias. Finally, there appears to be a tendency for campaigns to have become less successful over time, especially if one compares the effects of those campaigns carried out in the 1980s (overall accident reduction effect of 16%) to those conducted in the 2000s (just 5%).

3.2.2. Method of delivery

Table 3 summarises groups of effects defined according to values on each of the delivery variables.

It shows that most of the statistical weight in the sample comes from regional campaigns (72%); long-term campaigns of duration over 200 days (67%); and campaigns accompanied by enforcement (77%). There is a notable discrepancy between those 27 effects derived from campaigns employing some form of personal communication (weighted average −16%) and those that do not (weighted average −10%). Those campaigns using television, radio or newspaper, which we consider to be mass media campaigns, also dominate in this sample, while campaigns using roadside delivery contribute just over one third of the statistical weight. Overall there is little difference in the size of summary effects grouped on the basis of their relation to use of media in the corresponding campaign.

Table 5
Candidate variables for the explanation of campaign effects on accidents. Each is categorical. Variables with more than two levels were coded as dummy variables, and therefore count as more than one variable. For example, [theme] was recoded as [drink–drive] (yes/no), [speeding] (yes/no) and [general] (yes/no). All yes/no variables were then coded as yes = 1 and no = 0. Level or scale was coded as national campaign = 1; other level of campaign = 0.

Evaluation	
[Decade]	Before/after 2000
Delivery	
[Duration]	0–29 days/30–200 days/others
[Level]	National/others
[Combined mass-media]	Did the campaign combine use of television and radio and newspaper? yes/no
[Personal communication]	Did the campaign attempt to communicate personally? yes/no
[Roadside]	Did campaign use billboards, variable/fixed message signs on the road? yes/no
[Video-cinema]	Did campaign use either DVD, video or cinema? yes/no
[Target group identified]	Was a particular group defined and targeted? yes/no
[Enforcement]	Was the campaign accompanied by enforcement? yes/no
Content	
[Basis]	Was the content based on previous campaigns? yes/no
[Theme]	Drink-driving/speeding/general-mixed/others
[Emotional and rational]	Did campaign have emotional and rational content? yes/no
[Risk of detection]	Did the campaign attempt to point out the risks of being detected? yes/no
[Risk of harm]	Did the campaign attempt to point out the risks of being harmed? yes/no

3.2.3. Content of message

Table 4 summarises effects grouped according to values on each of the content variables.

Few of the effects were for campaigns with content that could be described as purely emotional, preventing meta-analysis of their effects. However, there are suggestions that effects associated with campaigns described as having both emotional and rational elements in their approach are stronger than those associated with campaigns with purely rational elements.

Basis of established theory or practice does not appear to be advantageous, according to these analyses. Of the different campaign themes, drink-driving is linked to substantial accident reduction effects (weighted average −18%), significantly larger than the non-significant overall effect of those campaigns with speeding themes (weighted average −4%).

3.3. Campaign factors associated with variation in effect

Meta-regression was carried out on the full set of 119 effects to learn about the relative importance of delivery and content in terms of the ultimate effects of campaigns on accidents. An initial list of fourteen candidate predictors (Table 5) was reduced to seven according to the procedure described in the method (for more detail see Section 4).

Fixed and random effect meta-regressions were run on the dependent variable, ln ES, using as predictors those variables denoting whether or not the campaigns lasted less than a month (*duration – 0 to 29 days*); were carried out *after 2000*; had a drink driving theme (*theme – drink-driving*); used *roadside* delivery channels; used combined *mass-media* (tv and radio and newspaper with or without other channels); used *personal communication*; or were accompanied by *enforcement*.

According to a fixed effect model, the total variance in effect explained by all seven variables in the model was 28% ($p < .001$). Of these, *after 2000* and *combined mass media* were uniquely associated with a detrimental effect of campaigns on accident levels, whereas *theme – drink-driving*, *roadside*, *enforcement* and *duration – 0–29 days* were each uniquely associated with reductions in accident levels. The sum of the variance in accident effect explained uniquely by each variable was greater than the total variance explained by the seven variables together, indicating the presence of cooperative suppression among the variables (Cohen and Cohen, 2003).

To account for the heterogeneity among individual effects, meta-regression was performed using a random effect model in which the between-effect variance component was estimated to be .016. The selected model of campaign factors account for a significant but lower amount of variance (14%) in campaign effect, and this time only *theme — drink-driving* and use of *personal communication* are uniquely and significantly related to a reduction in accident counts at α level = .05. However, both the direction of the relationship of each coefficient with effect and the size of the relationships are consistent between random and fixed effect models. The random effect analysis suggests that most of the between study variance is accounted for by the model (*Q*-for-the-residual is not significant).[2]

3.3.1. Accounting for dependency among individual effects

The 119 reported outcomes were nested within 74 campaigns retrieved from 67 evaluation studies (some studies describe more than one campaign). Only one effect is reported for 51 of these campaigns, and more than one effect is reported for the remaining 23.[3] The mean correlation between the effects reported from each of the 23 nested campaigns was estimated at −.07, suggesting a low degree of dependency. The meta-regression analyses run on 74 aggregated effect sizes (Table 7) gave results similar to those obtained before aggregation (Table 6), and more of the variance in campaign effect on accidents is explained by the aggregated model (25% compared with 14% for random effects models). Importantly, independent of aggregation of effect sizes, three campaign factors are significantly related to reduction in accidents in both models at $\alpha = .05$. These are whether the campaigns had a *drink-driving* theme, used *personal communication* or *roadside* delivery of the message. In addition, campaigns carried out *after 2000* were associated with a detrimental effect on accident levels.

4. Discussion

4.1. Overall effect of road safety campaigns on accidents

Based on a meta-analysis of 119 effects extracted from 67 studies, road safety campaigns reduce the numbers of road accidents by 9%. This estimate is in line with those of Delhomme et al. (1999) and Vaa et al. (2004), despite the addition of 33 individual campaign effects in this study. Given the level of variability among the set of individual effects, we would caution against the use of any single estimate to predict the effect of a planned road safety campaign. It is rather a quantitative statement about the overall effects of all types of campaigns in all types of context.

4.2. Characteristics of evaluations and campaigns used to derive the overall effect

Bivariate or subgroup analysis was conducted to characterise the campaigns and evaluation studies on which the overall effect is based. The evaluations mostly used injury accidents or drink–drive

[2] At this stage, further tests were made to check for systematic differences between the controlled and non-controlled effects in the sample (see Appendix B).

[3] Of these, 12 reported two effects, five reported three effects, four reported four effects, one reported six effects and one reported seven different effects.

Table 6
Results from fixed effect and random effect meta-regression of the final accident model. The dependent variable ($n = 119$) is made of each individual effect size extracted regardless of dependence on campaign or evaluation study. Variables, their coefficients, percentage change in accidents and probabilities are shown. *b* (the regression coefficient) represents the expected difference in ln ES between two campaigns, one having the campaign characteristic in question and the other not, controlling for all other explanatory variables in the model. For instance, *roadside* has a *b* of −.10, meaning that campaigns using roadside measures have larger accident reductions compared to those not using this measure. Percentage change is the *b* converted to the expected percentage change in accidents counts. *p-value* is the probability of this relationship occurring by chance (i.e. due to sampling error). The conventional 5% level of significance is applied, meaning that if the *p*-value is lower than 5% we believe that this relationship is most probably not due to chance. In addition, the R^2 represents how much of the variance in campaign effects that all of the explanatory variables account for together.

	Fixed effect model		Random effect model	
	b	*p-value*	*b*	*p-value*
(Constant)	−.04	.04	−.04	.38
[Duration—0–29 days]	−.15	<.001	−.10	.23
[After 2000]	.12	<.001	.09	.11
[Theme—drink-driving]	−.10	<.001	−.09	.04
[Personal communication]	−.07	<.001	−.11	.01
[Roadside]	−.10	<.001	−.06	.16
[Enforcement]	−.08	<.001	−.06	.22
[Combined mass-media]	.09	<.001	.03	.39
R^2	.28	<.001	.14	.03
Q (model) ($df = 7$)	160	<.001	18.8	.01
Q (residual) ($df = 108$)	415	<.001	114	.32

Table 7
Results from fixed effect and random effect meta-regression of the final accident model. The dependent variable ($n = 74$) is the aggregated effect size from 74 campaign evaluation studies. Variables, their coefficients, percentage change in accidents and probabilities are shown. See Table 6 legend for further explanation.

	Fixed effect model		Random effect model	
	b	*p-value*	*b*	*p-value*
(Constant)	−.04	.054	−.04	.358
[Duration—0-–29 days]	−.15	<.001	−.13	.062
[After 2000]	.12	<.001	.12	.019
[Theme—drink-driving]	−.10	<.001	−.09	.022
[Personal communication]	−.07	<.001	−.09	.026
[Roadside]	−.10	<.001	−.10	.007
[Enforcement]	−.08	<.001	−.07	.113
[Combined mass-media]	.09	<.001	.06	.088
R^2	.38	<.001	.25	<.001
Q (model) (df = 7)	160.4	<.001	28.1	<.001
Q (residual) (df = 66)	267.5	<.001	83.3	.074

accidents as measures, which are often taken only before and during the campaign, i.e. most of the effects in the sample can be regarded as short term effects. Most evaluations included in the sample are controlled (71% of statistical weight). The evaluations were published across the last thirty years, mostly between 1980 and 2000 (80% of statistical weight). It was more difficult to find studies from later years, and a thorough search failed to find any campaign evaluations based on accident effects that were published between 2008 and 2010. This observation implies that less campaigns are now being carried out and/or evaluated.

The campaigns themselves were mostly mass media campaigns, conducted exclusively in industrialised countries, often on a regional basis and often for extensive periods (over 200 days). In terms of statistical weight, the greatest contribution is from speeding and drink–drive campaigns. Most campaigns in the sample were accompanied by enforcement, and used rational persuasion, possibly supplemented by emotional content. Campaigns relying solely on emotional persuasion or incentives are poorly represented, limiting the extent to which we were able to analyse the effect of emotional appeals and the effect of invoking negative vs. positive emotions. Campaigns highlighting the risk of detection are well represented, but those highlighting the risk of harm are not.

4.3. Comparing effects of different groups of campaigns

Bivariate or subgroup analysis was also conducted as a first step towards explaining the significant between-study variability observed. Because the variance shared by different campaign factors is not accounted for in these analyses, they serve only as indications of the effects associated with these factors. While campaign designers may use these "first look" results (Tables 2–4) to get an idea about the relative size of overall effects for campaigns of a certain scope or using a certain media strategy, for example, they should remember that large differences remain among the campaigns within each subgroup, and that the confidence intervals between overall effect estimates for different subgroups often overlap. The level of publication bias evident for the whole sample of effects should also be considered when interpreting the results of the bivariate analyses.

Bearing in mind these limitations, the subgroup results provide preliminary indications that effects on accident counts are on the whole better for drink–drive campaigns (see Table 4), for campaigns identifying a target group or using personal communication (Table 3), combining emotional and rational content (Table 4), or focusing on the risk of detection rather than risk of harm (Table 4). Notably, no advantage is apparent for campaigns having a stated basis (Table 4) or using any particular form of media (Table 3). These indications merit further consideration through further meta-analysis or other forms of systematic literature review.

4.4. Methodological issues and meta-regression: choosing candidate predictors and choice of model

One of the drawbacks of any form of multivariate analysis is its demand for a large number of results. Where a medium effect size is anticipated it is recommended for normal multiple regression that the number of effects should be larger than $50 + 8m$ (where m is

the number of influencing variables) or 104 + m, depending on the purpose of the analysis (Tabachnick and Fidell, 2007). While corresponding guidelines are yet to be established for meta-regression, it is reasonable to consider as a starting point that the demands on effect numbers will be similar. To meet these demands it was necessary to (i) include effects from both controlled and non-controlled studies; (ii) to summarise different types of accident measure together; and (iii) to include more than one effect from the same evaluation study as long as those effects were orthogonal. The use of non-controlled results was justified by the lack of evidence of a systematic difference in the size and variation of overall effect on comparing subgroups based on the variable 'control' (Table 2). That most of the effects (74%) in the sample did come from controlled studies is nevertheless important concerning possible influences from regression to the mean effects on our overall estimate (Elvik, 2005).

During model development, some of the variables considered potentially influential in terms of campaign effect on accidents had to be excluded due to lack of information about them in the evaluation studies retrieved. In other words, the initial candidate variable list in Table 5 was limited to those variables for which sufficient data were available. In assessing our results it should therefore be remembered that important influences on campaign effect may not be accounted for in the final model of variables assessed, e.g. the amount of publicity achieved by a campaign; subjective evaluation of the campaign by the target; whether a campaign has addressed the social norm; and actual behavioural change achieved by the campaign.

It should also be noted that accident type was not included as a candidate variable. This was because (i) we did not consider that the variable would inform about what makes campaigns more or less effective; (ii) we expected and found strong correlation between effects of those campaigns with a drink-driving theme and those campaigns using drink-driving accidents as a measure; and (iii) we considered the aggregation analyses would to some extent account for variations according to accident measure.

In considering our findings, it is important to be aware of the decisions made during selection of the final candidate predictors in Table 6. The variables *emotional* and *rational* and *risk of harm* were excluded because data describing the campaign content was not sufficient enough for objective assessment of the material. *Risk of detection* was excluded after inspection of bivariate correlations among the candidate variables revealed a strong correlation between *risk of detection* and *enforcement* ($r = 0.73$; $p > 0.001$). The *enforcement* variable was retained in order compare associations between enforcement and accident counts with those found in previous studies (Delhomme et al., 1999; Vaa et al., 2004). Zero-order correlations, subgroup analyses and output of an initial weighted simultaneous regression run (to examine tolerance relationships among variables) were consulted with a view to capture important influences on campaign effects in the most efficient way, resulting in the exclusion of the variables *duration—30–200 days*, *theme–speeding*, *theme–general*, *video/cinema* and campaign *basis*. Finally *scale–national* was excluded because explained little outcome variance in the final model. This strategy influenced on the final model of variables, which to a certain extent is thus determined by the subjective experience and knowledge on which our decisions were based.

4.5. Campaign factors uniquely associated with accident effects

Having described methodological limitations, we now turn to the findings of meta-regression. As described, we developed a set of campaign factors for testing by meta-regression, describing campaign duration, recency and theme, and whether or not roadside delivery, enforcement, personal communication or combined mass-media was used to help deliver the message. Depending on the meta-regression method used, between 14 and 38% of the variance of campaign effects on accident levels was accounted for by this group of factors.

According to a fixed effects model, a drink–drive theme, shorter campaign duration (<30 d), use of personal communication, roadside delivery, and enforcement are each uniquely associated with greater accident reductions, while those campaigns conducted after 2000 and those combining mass media each uniquely associated with poorer effects in the context of the model. Analysis using a stricter (random effects) model showed that only a drink–drive theme and use of personal communication were uniquely associated with variance in campaign effect, both again beneficial in terms of accident levels. After accounting for dependency using aggregation in random effect meta-regression, a drink–drive theme, use of personal communication or roadside communication was seen as beneficial in terms of accident effects. A further check for dependence using multi-level meta-analysis (see e.g. Hox, 2002; Raudenbush and Bryk, 2002) gave identical parameter estimates and only trivial differences in p-values (data not shown).

The finding that drink–drive campaigns are relatively successful could reflect the disproportionately large effect on accidents that a small reduction in drink-driving behaviour might achieve (Elvik et al., 2009), or that drinking before driving often occurs in a social context and may therefore be particularly well suited for targeting by social marketing campaigns (Ulleberg and Vaa, 2009). Although interesting, this result is similar to the finding that recent campaigns tend to be less effective in that it says little to practitioners about how they might improve short-term effects of their campaigns. What may be of more practical use are our findings that personal communication and roadside delivery of a message are associated with greater reductions in accidents.

The beneficial effect of using personal communication by road safety campaigns was also found by Vaa et al. (2004), who proposed that the effect may be due to more effective processing of the campaign message by the target audience (Berger, 2005). The beneficial effect of roadside delivery is supported by various driving behaviour theories emphasising the importance of situational influences on driving behaviour (see e.g. Fuller, 2005; Näätänen and Summala, 1974; Vaa, 2007). According to these theories, delivery of a persuasive message in the context of the road user behaviour (i.e. at the roadside) can create cues that activate desirable attitudes within the immediate context of the target behaviour. Making attitudes accessible in a context-relevant way has been found to increase correspondence between attitudes and behaviour (e.g. Fazio, 1986), and is thus expected to increase effectiveness of campaigns. This idea of "immediacy" of delivery being beneficial is given some support by our finding that shorter campaigns are associated with improved accident reductions, assuming that shorter, more intense campaigns are more likely to be delivered at a time proximal to performance of the target behaviour. Lack of evidence linking mass-media methods such as television or radio to short-term improvements in campaign effects is also in line with the notion that "immediacy" is beneficial to campaign effects on accidents.

Despite the indicated benefits of immediacy of delivery to short-term campaign effects, it would be unwise to caution against the use of mass media. Mass-media have obvious advantages in terms of audience reach. Secondly, the effects of mass-media delivery may be more telling in terms of broader social change over the long term, especially when they are employed continually in campaigns that form part of a road safety programme (Delhomme et al., 2009). It is important to remember that most of the campaign evaluations on which our conclusions are based do not measure longer term change, but rather tend to deal with the more immediate change that occurs either during or following soon after a campaign (see Table 2).

Our model indicates that the use of measures to enforce the campaign message is associated with improved reductions in accident counts, a finding reported by previous meta-analyses (e.g. Vaa et al., 2004; Delhomme et al., 1999). It is reasonable to consider that enforcement is important in consolidating the effects of large scale campaigns, possibly by providing a channel of 'immediacy' for those mass-media messages delivered in less immediate ways. If this is the case, our results, which are based on effects for campaigns of different scales, may detract from the importance of enforcement for large scale campaigns.

The meta-regression suggests that today's campaigns are less effective than earlier ones. This could be because road user behaviour has grown safer (less drink–driving; increased seatbelt use), and that there are therefore less 'easy wins' for road safety campaigns (Elliot, 1993; Elvik et al., 2009; Hagenzieker et al., 1997). Alternatively, recent campaigns may differ systematically to earlier ones in ways not identified in this study.

4.6. Future work

The focus of our study has been the effect of campaigns on accidents, which we see as the measure of ultimate interest for planners and politicians. Regarding the size of effect on accident campaigns are found to have, it must be remembered that to accept our findings one must assume that the evaluation studies on which they are based report valid effect estimates. We are aware of criticisms in the field that campaign evaluations tend to be methodologically weak, and that they tend to overestimate campaign effects (although we cannot find clear empirical basis for this). To address these criticisms a study is needed to see whether there is a link between the quality of method used by an evaluation and the effect size reported. Although the present meta-analysis weight evaluations according to sample size and looks for differences between controlled and non-controlled studies, it does not attempt to control for study quality in terms of measurement accuracy, specificity of effect, regression to the mean or accident migration (Elvik et al., 2009).

Greater support for the beneficial influence of personal communication and roadside delivery on the short-term effects of campaigns on accidents would be found if explicit links could be drawn between their use in road safety campaigns and relevant road user behaviours, and then between those behaviours and accidents (Delhomme et al., 2009). In attempting to draw such links careful consideration should be given to the differential effects that changes in different road user behaviours would have on accident levels (Elvik et al., 2009).

Concerning roadside delivery in particular, it would be worth investigating whether it is *active* feedback or simple delivery of a message in a driving context that is important (the variable we use comprises use of roadside billboards and both variable and fixed message road signs). Variable message signs have been found to be particularly effective for route guidance, and it would be interesting to isolate the effect of their use in safety campaigns (Erke et al., 2007). The use of roadside posters or posters mounted on public transport could be added to expand this variable.

We were unable to gain any insight into the efficacy of some campaign factors, because of a lack of descriptive data available in the evaluation studies. We would encourage both the further consideration of these factors as predictors of campaign effect and their improved reporting by evaluation studies. Most notably, these were whether social norms were accounted for by a campaign, which may be particularly important (e.g. Cialdini, 2007), the publicity strategy of a campaign (whether it pays for or earns media attention), and size of the target group related to catchment area.

We would also like to encourage the explicit documentation of the emotional content of a campaign. We were particularly surprised at the lack of campaigns *evaluated* that used explicitly positive or negative emotion. Thus, very few conclusions could be drawn about the 'use of fear' in campaigns, for instance, and we would encourage more evaluation of these types of campaigns.

The longer term effects of campaigns remain largely unknown. In many cases it was not clear whether a campaign is part of an ongoing program of campaigns or not. While we tried to avoid including campaigns that followed on from recent campaigns that could still be having effects, this was not always easy to ascertain from the available information. Clearer information about the history of campaigns carried out in the study area would help.

Finally, we would like to emphasise that the meta-regression data are generated on the basis of several important assumptions. We have attempted to delineate theoretical and methodological limitations throughout this report and willingness to accept the conclusions we draw should depend on the level of agreement with decisions we have made and assumptions we have taken during treatment of the data.

5. Conclusion

A systematic summary of 119 individual road safety campaign effects suggests that road safety campaigns have an overall significant accident-reducing effect of 9%. Meta-regression analysis suggests that those campaigns using personal communication, roadside and/or enforcement strategies to deliver their message are associated with greater accident reductions. In interpreting our analyses we conclude that achieving immediacy in the delivery of a campaign message, in terms of proximity to the target behaviour, might tend to increase campaign effect in the shorter term, and complement any long-term campaign effects achieved using mass-media delivery. The method use to derive these conclusions should be carefully borne in mind by any practitioner wishing to apply these results.

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Appendix A.

See Table A.1.

Table A.1
List of 119 effects and associated variables extracted from 67 studies, where OR = odds ratio of effect, and where V1–7 are variables entered into the final regression model, respectively duration 0–29 days (V1), publication since 2000 (V2), drink–drive theme (V3), use of personal communication (V4), roadside delivery (V5), accompanying enforcement (V6) and combined use of TV, radio and newspaper (V7); con = study uses control design.

Project ID	Author	Year	OR	ln OR	w_j	V1	V2	V3	V4	V5	V6	V7	con
2	Unknown	2005	0.72	−0.33	13.4	0	1	0	1	1	1	0	0
2	Unknown	2005	0.85	−0.16	715	0	1	0	1	1	1	0	0
8	STF[a]	1977	0.92	−0.08	131	0	0	0	1	0	0	0	0
10	Vägverket	1997	0.91	−0.09	5.49	0	0	0	1	1	1	1	0
16	Sävenhed	1996	0.53	−0.64	33.7	0	0	0	1	0	0	1	1
16	Sävenhed	1996	0.63	−0.47	216	0	0	0	1	0	0	1	1
16	Sävenhed	1996	1.14	0.13	588	0	0	0	1	0	0	1	1
24	Höök	1994	0.82	−0.2	2.48	0	0	0	0	1	1	1	0
24	Höök	1994	0.95	−0.05	19.5	0	0	0	0	1	1	1	0
24	Höök	1994	0.92	−0.09	43.3	0	0	0	0	1	1	1	0
32	Törnos	1995	0.91	−0.1	75.4	0	0	1	0	0	1	1	1
39	Sørensen	2005	0.31	−1.16	1.01	0	1	0	1	0	0	0	0
40	Studsholt	1990	0.49	−0.71	13.1	0	0	1	1	0	0	1	0
40	Studsholt	1990	0.58	−0.55	15.1	0	0	1	1	0	0	1	1
52	Blomberg	1983	0.82	−0.2	219	0	0	0	1	0	0	0	1
57	Laforest	1987	0.76	−0.28	19.4	0	0	1	0	0	1	1	0
60	Gibb	1984	0.93	−0.08	714	0	0	1	0	0	1	1	0
69	Baldcock	1997	0.66	−0.41	21.9	0	0	1	0	1	1	1	0
76	Fischer	1984	3.03	1.11	10.9	0	0	0	0	0	0	1	0
78	Homel	1988	0.65	−0.44	548	0	0	1	0	1	1	1	0
79	Ross	1987	0.85	−0.17	76.9	0	0	1	0	1	1	1	0
81	Reznik	1984	0.81	−0.21	96.2	1	0	1	0	0	0	1	1
83	Bill	1992	0.8	−0.22	158	0	0	0	0	1	1	1	0
92	Lane	1984	0.92	−0.08	479	0	0	0	0	0	1	1	1
96	Graham	1996	0.83	−0.19	62.1	0	0	1	0	0	1	0	0
98	Cameron	1992	0.84	−0.18	320	0	0	0	0	0	1	0	1
108	Mercer	1985	0.9	−0.11	110	0	0	1	0	0	1	0	0
111	Simmonds	1981	2.3	0.83	2.6	0	0	0	0	1	0	0	1
111	Simmonds	1981	0.69	−0.37	7.09	0	0	0	0	1	0	0	1
111	Simmonds	1981	0.78	−0.25	8.29	0	0	0	0	1	0	0	1
116	Ytterstad	1995	0.71	−0.34	209	0	0	0	0	1	0	1	1
118	Britt	1995	1	0	484	0	0	0	0	0	1	0	0
123	Williams	1996	0.94	−0.07	1401	0	0	0	0	1	1	1	0
167	Nagatsuk	1991	0.27	−1.32	4.73	0	0	0	1	0	0	0	1
168	Schlabbach	1990	1.12	0.11	272	0	0	0	1	0	0	0	0
169	Fosser	1984	1.03	0.03	1367	0	0	0	0	0	1	1	1
169	Fosser	1984	0.76	−0.27	13	1	0	0	0	0	0	1	1
1691	Glad	1986	0.78	−0.24	196	0	0	0	1	0	0	0	1
1692	Moe	1987	0.88	−0.12	57.9	1	0	0	0	0	0	1	1
170	Stene	1988	1.33	0.28	67.7	0	0	0	0	0	1	1	1
171	Moe	1990	0.9	−0.1	116	0	0	0	0	0	1	1	1
172	Fosser	1992	0.93	−0.07	122	0	0	0	1	0	0	0	1
172	Fosser	1992	0.94	−0.06	122	0	0	0	1	0	0	0	1
172	Fosser	1992	0.9	−0.11	126	0	0	0	1	0	0	0	1
175	Amundsen	1999	0.58	−0.55	20.7	0	0	0	1	0	1	1	1
175	Amundsen	1999	1.21	0.19	29.4	0	0	0	1	0	1	1	1
175	Amundsen	1999	1.08	0.08	33.6	0	0	0	1	0	1	1	1
175	Amundsen	1999	0.92	−0.09	37.2	0	0	0	1	0	1	1	1
176	Maisey	1981	0.63	−0.46	19.6	0	0	1	0	0	1	0	1
176	Maisey	1981	0.72	−0.32	58.5	0	0	1	0	0	1	0	1
177	Cameron	1997	0.93	−0.07	169	0	0	1	0	1	1	1	1
178	Smith	1990	0.87	−0.13	299	0	0	1	0	0	1	1	1
178	Smith	1990	0.78	−0.24	693	0	0	1	0	0	1	1	1
179	Sali	1983	0.84	−0.18	1112	0	0	0	0	0	1	0	1
180	Voas	1997	0.81	−0.21	725	0	0	1	1	1	1	1	1
180	Voas	1997	0.93	−0.08	53.2	0	0	1	0	0	1	0	1
180	Voas	1997	0.94	−0.07	53.2	0	0	1	0	0	1	0	1
181	Blomberg	1987	1.02	0.02	191	0	0	1	0	0	1	0	1
181	Blomberg	1987	0.69	−0.37	129	0	0	1	0	1	1	1	1
182	Dowling	1986	0.74	−0.3	884	0	0	1	0	1	1	1	1
183	Taylor	1995	1.21	0.19	112	0	0	0	0	1	0	1	1
183	Taylor	1995	0.93	−0.07	132	0	0	0	0	1	0	1	1
183	Taylor	1995	0.96	−0.04	555	0	0	0	0	1	0	1	1
183	Taylor	1995	0.98	−0.02	786	0	0	0	0	1	0	1	1
184	Cameron	1993	0.9	−0.11	125	0	0	0	0	0	0	0	1
184	Cameron	1993	0.9	−0.1	144	0	0	0	0	0	0	0	1
185	Wolfe	1983	0.97	−0.04	940	0	0	1	0	0	1	1	1
1861	King	1987	0.9	−0.1	125	0	0	1	0	0	1	0	1
1861	King	1987	1.03	0.03	150	0	0	1	0	0	1	0	1
1862	King	1989	0.99	−0.01	295	0	0	1	0	0	1	0	1
1862	King	1989	0.57	−0.57	436	0	0	1	0	0	1	0	1
187	Wells	1992	0.82	−0.19	125	0	0	1	0	1	1	1	0

Table A.1 (*Continued*)

Project ID	Author	Year	OR	ln OR	w_j	V1	V2	V3	V4	V5	V6	V7	con
188	Harte	1984	0.91	−0.1	4.55	0	0	1	1	0	1	1	1
188	Harte	1984	0.7	−0.36	86.8	1	0	1	1	0	1	1	1
189	Armour	1985	0.76	−0.27	43.2	0	0	1	0	0	1	1	1
189	Armour	1985	0.58	−0.55	70.4	0	0	1	0	0	1	0	1
191	Drummond	1992	1.04	0.04	17.6	0	0	1	0	0	1	1	1
191	Drummond	1992	0.78	−0.25	46.7	0	0	1	0	0	1	1	1
191	Drummond	1992	0.83	−0.19	132	0	0	1	0	0	1	1	1
191	Drummond	1992	1.04	0.04	469	0	0	1	0	0	1	1	1
192	Stuster	1995	0.9	−0.11	10	0	0	0	0	0	1	1	1
192	Stuster	1995	0.91	−0.09	13.2	0	0	0	0	0	1	1	1
192	Stuster	1995	0.78	−0.25	13.8	0	0	0	0	0	1	1	1
192	Stuster	1995	1.16	0.14	49.5	0	0	0	0	0	1	1	1
192	Stuster	1995	0.69	−0.37	57.2	0	0	0	0	0	1	1	1
192	Stuster	1995	0.97	−0.03	63.3	0	0	0	0	0	1	1	1
193	Spoerer	1989	1.01	0.01	90.4	0	0	0	0	0	0	0	1
194	Behrensdorff	1992	0.61	−0.5	11.9	0	0	1	0	0	0	0	1
194	Behrensdorff	1992	1.92	0.65	11.7	0	0	1	0	0	0	0	1
195	Machemer	1995	0.85	−0.17	9.66	0	0	0	0	1	1	1	1
195	Machemer	1995	0.85	−0.17	5.95	0	0	0	0	1	1	0	1
195	Machemer	1995	1	0	6.32	0	0	0	0	1	1	0	1
196	Oei	1992	0.65	−0.43	1.6	0	0	0	0	0	1	1	1
196	Oei	1992	1.64	0.49	1.6	0	0	0	0	0	1	1	1
196	Oei	1992	0.62	−0.47	2.18	0	0	0	0	0	1	1	1
196	Oei	1992	0.51	−0.67	7.22	0	0	0	0	0	1	1	1
196	Oei	1992	0.38	−0.97	8.09	0	0	0	0	0	1	1	1
196	Oei	1992	0.67	−0.4	8.79	0	0	0	0	0	1	1	1
196	Oei	1992	0.93	−0.07	176	0	0	0	0	0	1	1	1
198	Haynes	1982	0.75	−0.28	16.1	0	0	0	1	0	0	0	1
204	Agent	2003	1.01	0.01	8.3	1	1	0	0	0	1	1	0
204	Agent	2003	0.89	−0.11	328	1	1	0	0	0	1	1	0
206	Cameron	2003	0.95	−0.05	274	0	1	0	0	0	1	0	1
206	Cameron	2003	1.04	0.03	293	0	1	0	0	0	1	0	1
206	Cameron	2003	1	0	294	0	1	0	0	0	1	0	1
206	Cameron	2003	1.05	0.05	222	0	1	0	0	0	1	0	1
207	Diamantopolou	2002	0.59	−0.53	37.3	0	1	0	0	0	1	0	1
207	Diamantopolou	2002	0.89	−0.12	38.4	0	1	0	0	0	1	0	1
207	Diamantopolou	2002	1.1	0.1	136	0	1	0	0	0	1	0	1
210	Worden	1975	0.94	−0.07	2.95	0	0	1	0	0	1	1	0
211	Whittam	2006	1.07	0.07	541	0	1	0	0	1	0	0	0
212	Agent	2002	0.86	−0.15	113	1	1	1	0	1	1	0	0
215	Mulholland	2005	0.99	−0.01	880	0	1	0	0	1	1	1	0
220	Murry	1993	0.93	−0.07	7.42	0	0	1	0	1	0	1	1
222	Ulleberg	2004	0.97	−0.03	263	0	1	0	1	0	1	1	1
226	Sakshaug	2001	1.03	0.03	104	0	1	0	0	1	0	1	1
226	Sakshaug	2001	1.15	0.14	65.5	0	1	0	0	1	0	1	1
227	Ulleberg	2007	0.69	−0.36	92.2	0	1	0	1	0	1	1	1
228	Ulleberg	2007	0.90	−0.11	146.2	0	1	0	1	0	1	0	1

[a] Statens trafiksäkerhets-forening.

Appendix B.

To further test differences between controlled and non-controlled effects, two further checks were made in the fixed effects meta-regression: (a) *control* (see Table 1) was added as a variable in the final model; and (b) the final model variables were used to test the group of effects arising from controlled studies alone. Addition of *control* to the final model variables did not affect their unique relations with accident effect, in terms of the direction and significance of the variable associations. Furthermore, *control* itself was not significantly associated with the outcome variable (beta = −0.15; p = 0.12). When a weighted, standard multiple regression was performed using the final model variables only on controlled cases (n = 91), the results were similar to those output for pooled controlled and non-controlled cases (n = 119). Significance of the associations between accident effect (ln ES) and either *duration—0–29 days* or *enforcement* was lost, but the direction of the associations remained the same. These results are consistent with loss of power resulting from decreasing the number of cases in the analysis.

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