Deliverable D-1.3

Effects of Road Safety Campaigns

Final version

Public

CAMPAIGNS AND AWARENESS RAISING STRATEGIES IN TRAFFIC SAFETY

Contract No TREN-05-FP6TR-S07.59641-019520-CAST

Project Co-ordinator: Institut Belge pour la Sécurité Routière (IBSR)

Authors: Truls Vaa, Ross Phillips (eds), Giannis Adamos, Alain Areal, Karin Ausserer, Patricia Delhomme, Marko Divjak, Werner de Dobbeleer, Sonja Forward, Barbara Krol, Annette Meng, Inger Synnøve Moan, Teti Nathanail, Ewald Pohlmeier, Gian Marco Sardi, Paul Schepers, Eva Sedá, Pål Ulleberg, Esther Walter

Date: 31.07.2009

Sixth framework programme

Priority sustdev-2004-3.4.1.4.5.
Sustainable surface transport priority

DOCUMENT CONTROL INFORMATION

Title	Effects of Road Safety Campaigns
Author(s)	Truls Vaa, Ross Phillips (eds), Giannis Adamos, Alain Areal, Karin Ausserer, Patricia Delhomme, Marko Divjak, Werner de Dobbeleer, Sonja Forward, Barbara Krol, Annette Meng, Inger Synnøve Moan, Teti Nathanail, Ewald Pohlmeier, Gian Marco Sardi, Paul Schepers, Eva Sedá, Pål Ulleberg, Esther Walter,
Editor(s)	Truls Vaa, Ross Phillips
Date	31.07.2009
Report Number	D1.3
Version status	Revised final draft for EU approval
Distribution	
Availability	
QA check	Final draft checked by Tamara Hoekstra and Charles Goldenbeld (SWOV)
Authorized by	
Signature	

TABLE OF CONTENTS

EXECUTIVE SUMMARY

There is a large body of research on the effect of road safety campaigns available that potentially allows formulating best practices for designing and evaluating campaigns. There is an urgent need for research focussing on the effects of road safety campaigns on the number of road traffic accidents and other key variables as accompanying measures like enforcement. This could be achieved by estimating the relative effects of contributing measures as for example the use of information mediated by television, radio, newspapers, different forms of education, police enforcement, etc.

The EU-project CAST – **C**ampaign **A**wareness raising **S**trategies in **T**raffic Safety - is in the form of a Specific Targeted Research Project aimed at meeting the European Commission's needs for improving road traffic safety by means of effective road safety campaigns.

The project was launched in February 2006 and will be completed January 2009. The consortium comprises 19 partners from 15 European countries and is lead by Insitut Belge pour la Sécurité Routière asbl (IBSR) located In Brussels. The present report is CAST deliverable D1.3 – one of two deliverables from Work Package 1.

Defining 'campaigns'

Defining road safety campaigns is, however, not a straight-forward task, as campaigns may comprise a wide range of variables, as media-activities, accompanying measures, target groups, scales, etc. In the present context, a campaign is defined as follows:

> *"Purposive attempts to inform, persuade, and motivate behaviour changes in a relatively well-defined and larger audience in order to improve road safety, typically within a given time period, by means of organized communication activities involving specific media channels often complemented by interpersonal support and/or other supportive activities as enforcement, education, legislation, commitment, rewards, etc."*

Responses from 15 partner countries give an overview of current road safety campaign traditions in each country, and an idea of the different themes and measures used. Past work attempting to determine whether campaigns work and what makes them work is summarized.

Former basis of the CAST project

The CAST project is heavily based on the EU-project GADGET completed in 1999 and the succeeding project INFOEFFEKT completed in 2004. The results from these projects formed the basis with regard to methodology, analytic tools, and knowledge of factors that contribute to the effects of

campaigns. A brief description of the results from these projects is provided as we regard these efforts as a knowledgebase and "State-of-the-art" when CAST was kick-off in February 2006. An important corner-stone of the knowledgebase is the use of meta-analysis, a formal analytic tool that facilitiates and structures analyses of large numbers of data being extracted from individual evaluation studies. Meta-analysis runs through several formal steps and a description of the method is provided in order to facilitate the subsequent presentation of results from CAST which is the core of the deliverable. Building on the GADGET and INFOEFFEKT projects, an expanded database containing 433 individual campaign effects has been developed in the CAST project.

EFFECTS OF CAMPAIGNS: CONCLUSIONS

Road safety campaigns work

Based on a conservative meta-analytical summary of an extensive database, we can say that not only do road safety campaigns work, but they work well. This assertion is based on a number of different outcome measures, including risk comprehension (16 per cent increase), yielding behaviours (37 per cent increase), speeding (16 per cent reduction), seatbelt use (25 per cent increase) and accident reduction (9 per cent decrease).

We do not find that campaigns are significantly effective at reducing drink-driving behaviour, possibly because there is a remarkably wide range of effects evident for campaigns using this measure. A summary of 39 results also shows no significant overall effect of campaigns on attitude.

Several factors associated with campaign effectiveness

Bivariate (subgroup) analyses give crude indications that effects on both accident counts and seatbelt use are better for those campaigns identifying a target group, using personal communication, combining emotional and rational content, and addressing subjective social norms. There is no evidence from bivariate analyses of a link between campaign effect and campaign scale, or the use of humour.

Results from bivariate analyses were used to help construct models for more sophisticated multivariate analyses (meta-regression). Testing of these models suggests several factors are associated with improved effect of campaigns on accident levels. Those that are amenable to manipulation by campaign designers are on-road delivery, personal communication and accompanying enforcement.

Several factors were also identified as being associated with positive campaign effects on seatbelt use. Of those amenable to manipulation by campaign designers, carrying out a campaign within a restricted area, such as an organisation or car park, appears to be most beneficial. There were also indications that on-road delivery and use of enforcement are beneficial to the effects of campaigns on seatbelt use.

Campaigns achieving intimacy and immediacy may be more effective

The suggestions from bivariate analyses that campaigns using some form of personal communication with the target audience have somewhat improved effects is consolidated by the multivariate analyses, at least in the case of

campaign effects on of accidents. The use of a personal element or the use of other people as channels for the delivery of the campaign message implies that an increased level of **intimacy** with the target could be important, possibly because it increases the likelihood that the target attends and processes the campaign message.

That on-road delivery is also positively associated with beneficial campaign effects suggests that delivery of the message to drivers at a place that is in terms of space and time proximal to the target behaviour is beneficial in terms of campaign effect. In other words, achieving **immediacy** in the delivery of a campaign message might be important. In terms of accident counts, short campaign duration is also beneficial according to multivariate analyses, something which also implies a sense of immediacy. In shorter, more intense campaigns, the message may be more likely to be received at a time that is proximal to carrying out of the target behaviour.

Taken together, intimacy and immediacy suggest that those campaigns that make the target person think carefully about the message in the context of the driving behaviour would tend to be more successful at achieving effects during or after the campaign. In this light, it is interesting to consider that the carrying out of campaigns in a limited area, such as an organisation or car park, was positively associated with improvements in seatbelt use, since such campaigns would presumably be more likely to use personal communication and deliver the message to the driver at a time that is proximal in space and time to the context of the targeted driving behaviour.

Mass media methods are still important

Lack of evidence linking mass-media methods such as television or radio to improvements in campaign effects is in line with the idea that intimacy and immediacy are beneficial to campaign outcomes. While we cannot conclude from bivariate analyses that use of mass-media has less effect at the local level than at regional or national levels, it does seem that within a small area, where a much more focused campaign is possible, those methods which are difficult to carry out on a larger scale (e.g. rewards, pledge cards) are particularly effective and therefore worth considering.

We recommend that campaign designers continue to consider mass-media methods for delivery because they have a clear advantage over personal communication in terms of audience reach, and their effects 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. In addition, there are some mass-media forms that do deliver campaign messages in an immediate, if not intimate, way. Examples are in-car radios and posters on the back of buses. National-level campaign designers more concerned with immediate, shorter term effects should try to identify methods that have the potential to reach large audiences in a context relevant to target behaviour (immediacy), or supplement mass-media methods with more immediate or intimate delivery methods.

Support for the use of enforcement
When other factors are controlled for, our results indicate that the use of measures to enforce the campaign message can lead to improved reductions in accident counts (meta-regression results), even though the use of enforcement was only near significance according to a random effects meta-regression model. Given the conservative tests of statistical significance applied in random effects meta-regression, we conclude that the use of enforcement as an accompanying measure can be beneficial.

Humour (and fear) should not detract from the central message
The indicated beneficial effect of addressing risk of harm in a campaign fits somewhat with the detrimental effect of the use of humour on outcome (according to multivariate analysis). One inference that can be drawn is that unless campaigns are taken seriously, their messages will not be attended to. An additional possibility is that the use of humour in a campaign detracts attention of the target group from the actual message. We suggest therefore that campaign planners intending to use humour should carefully check that it does reduce the likelihood that the central message is fully and seriously appreciated. Concerning fear, we were unable to find a sufficient number of studies evaluating campaigns explicitly using fear to develop and test a 'use of fear' variable. This was surprising given the high profile that fear campaigns seem to have, at least among researchers in the field.

Link between initial behaviour level and campaign outcome
The testing of meta-regression models suggests that a main determinant of campaign effectiveness is the initial level of that safety outcome. This relationship was clear and strong in the case of seatbelt use, where initial use levels were also positively linked with the year of the campaign study. We would therefore like to suggest that, at least in the case of seatbelt use, it is worth exploring the presence of a ceiling for campaign effects, and the reasons for its existence.

Consider limitations of meta-analysis when interpreting results
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.

Future work
Given the indicated benefits of roadside delivery of campaign messages, we outline ways in which this should be investigated further. Since the variable comprises billboard, variable message sign and fixed message sign use, it would be worth exploring further to see whether it is active feedback or simply delivery of a message in a driving context that is important. The use of roadside posters or posters mounted on public transport could be added to expand the variable.

We were unable to analyse treatment of social norms, combined use of emotional and rational content, and identification of target group as potential predictors in multivariate analyses, despite benefits indicated in bivariate analyses. This was because of insufficient information about the campaigns in the evaluation studies. For this reason and others, we provide detailed

recommendations about how evaluation studies might be improved in the future.

1. INTRODUCTION

There is a large body of research on the effect of road safety campaigns available that potentially allows formulating best practices for designing and evaluating campaigns (Delhomme et al, 1999). Defining road safety campaigns is, however, not a straighy-forward task, as campaigns may comprise a wide range of variables as media-activities, accompanying measures, target groups, scales, etc. A discussion on the definition of campaigns is put forward in chapter 3, but as a start, Rice and Atkin's definition (1994) addresses the core of what safety campaigns could be:

> *"[Campaigns are] …purposeful attempts to inform, persuade, or motivate attitude and (or) behaviour changes towards safety in a relatively well-defined and more or less wide audience generally for non-commercial benefits to the individuals and/or society at large;*

There is an urgent need for evaluation research focussing on the effects of road safety campaigns on the number of road traffic accidents and other key variables from the effect of accompanying measures like enforcement. This could be achieved by estimating the relative effects of contributing measures as for example the use of information mediated by television, radio, newspapers, different forms of education, police enforcement, etc.

Availability of clear scientific knowledge both on the effectiveness of different contributing factors and the cost-effectiveness of these same effects is a prerequisite to design future campaigns in the most effective and optimal way.

The EU-project CAST – **C**ampaign **A**wareness raising **S**trategies in **T**raffic Safety - is in the form of a Specific Targeted Research Project aimed at meeting the European Commission's needs for improving road traffic safety by means of effective road safety campaigns.

The project was launched in February 2006 and will be completed in January 2009. The consortium comprises 19 partners from 15 European countries and is lead by Insitut Belge pour la Sécurité Routière asbl (IBSR) located In Bruxelles.

2. CAST PROJECT WORK PACKAGE 1: ROAD USER MODEL

2.1 DESCRIPTION OF WORK PACKAGE 1

Work package 1 (WP1) comprises both a theoretical and an empirical perspective. The theoretical perspective is needed to understand and properly model the road user and road user behaviour. When the future aim is to influence the road user and road user behaviour in directions that benefit traffic safety, one must know which psychological or social factors determine road user behaviour and which factors can be influenced by road safety campaigns and persuasion techniques. A sound theoretical basis should aim at giving valid predictions about the partial effect of campaigns and valid predictions of the effect of any road safety measure are needed in order to avoid waste of money.

When one wants to reduce the number of road accidents, one must know what kinds of behaviours are associated with accidents, i.e., in the statistical sense: If one reduces the frequency of a certain kind of risky behaviour, one should experience a reduction in the frequency of accidents. Currently, associations between behaviour and accidents have been documented empirically for drink driving, speeding, red light crossing, yielding violations, and violations of driving-and-resting regulations for drivers of heavy vehicles (Elvik and Vaa 2004). An overwhelming number of studies have confirmed the link between use of personal protection equipment (seat belt wearing, child restraint systems, helmets) and reductions in the degree of personal injuries. All these types of behaviour can be proposed as themes of road safety campaigns. Furthermore, all these behaviours are also regulated by law. Hence, non-compliance with traffic law and regulation is defined as a violation, which can be enforced by the police. Police enforcement is confirmed to be a potentially effective accompanying measure to road safety campaigns (Delhomme et al 1999).

On a deeper level, factors such as motivation, attitudes, and personality traits have also been linked to accidents (Ulleberg, 2002) . Reducing driving speeds by influencing drivers' subjective perception of apprehension is documented to be an effective strategy (ETSC 1999). In sum, these factors constitute a model of driver behaviour, how drivers think and feel in their processing of information and decision-making in traffic, which in turn could serve as routes of influence from road safety campaigns to the minds of drivers.

WP1 comprise partners from 9 countries: Austria, Czech Republic, Denmark, Germany, Italy, Norway, Poland, Portugal and Sweden.

2.2 THE TASKS OF WP1

Four tasks were proposed in WP1 (Annexe 1 – "Description of work")

- Task 1.1 – Workshop on theories of behaviour and effects of campaigns: "State-of-the-art"
- Task 1.2 – Effects of campaigns on behaviour and accidents, campaigns strategies and "best practices"
- Task 1.3 – Road user behaviour and behaviour models
- Task 1.4 – Key elements for evaluation

2.2.1 Task 1.1 "Workshop on theories and models"

WP1 started with a workshop where the partners came together and established a common base of knowledge regarding partners' experiences of theories of behaviour, road user modelling, and effects of campaigns which in sum contributed to the exchange and discussion of joint knowledge and competence in this field.

Partners were especially encouraged to come up with national experiences on campaigns, evaluation of campaigns and campaigns effects. Partners specify the following activities: Results from the workshop were summarized in a Working Paper (CAST WP1 working paper: Summary of OSLO workshop: Workshop on theories of behaviour and effects of campaigns: "State-of-the-art"-October 2006)

2.2.2 Task 1.2: Effects of campaigns on behaviour and accidents, campaigns strategies and "best practices"

The GADGET project sampled a total of 265 studies (Delhomme et al, 1999). Only 30 of these were appraised
to comply with evaluation design standards that were judged necessary to be included in the subsequent meta-analyses that were done in the GADGET project. WP1 will use the same strategy as the one used in the GADGET-project: A literature search in all accessible and relevant databases and selecting studies which have a satisfactory evaluation design suitable for meta-analysis.

In the CAST-project individual campaigns are coded according to campaign characteristics that have been identified across all campaigns in the database, i.e. common characteristics. In turn, these characteristics will be used as predictors in a regression model suited for subsequent meta-regression. A search for the accumulated accession of studies since the GADGET-project to date, has been done.

In meta-regression the partial effects of the predictors are analysed and established in order to identify the key elements of campaigns, i.e. the predictors that seem to contribute to the effect, if any. The database has been further expanded by including studies from partners' countries. All CAST-

partners are, hence, contributing to the enlargement of the database and subsequently to the reliability and validity of the meta-analysis.
In addition to the quantitative approach described above, evaluation studies should also be investigated qualitatively, aimed at identifying te key elements of campaigns, and describing best practices.

Estimates of effects of campaigns on behaviour and accidents will be calculated by meta-analysis. Since the GADGET project, meta-analysis has been further developed and refined. The tool as we know it today, includes tests for publication bias, correction of estimates by “trim-and-fill-procedures” if publication bias is identified, tests for homogeneity, choosing between fixed and random-effect models, correction for systematic variance if necessary, and finally also meta-regression, i.e. the development and testing of multivariate predictor models where partial effects of predictors can be estimated. The application of these methods enhances the reliability of the effects considerably compared to previous methods of estimation of effects (GADGET only used bivariate tests). These developments were put into practice in the INFOEFFEKT-project financed by the Swedish Public Roads Administration (Vaa et al, 2004). This project can be characterized as a follow-up of the GADGET project, most of the GADGET studies are retained in the INFOEFFEKT-project, but the analytic methods are now far more sophisticated than they were in GADGET, the main improvement of standards concern the ability to correct for publication bias and the use multivariate techniques as done in meta-regression analysis of the effects of campaigns on accidents.

2.2.3 Task 1.3 Road user behaviour and behaviour models

When the ultimate goal of any road safety campaign is to reduce the number of accidents, it is essential that campaigns address behaviour that is confirmed to be validly linked to accidents. More specifically, campaigns should not be put into practice on an assumed association between behaviour and accidents. They should only be based on empirically confirmed relationships between behaviour and accidents. As mentioned, empirical relationships between behaviour and accidents are established, among others, for speeding and for drink driving. Seat belt wearing is proven to save lives and reduce the degree of injury in car crashes.

If campaigns are to be effective, it is necessary to know which individual characteristics contribute to speeding, to drink driving and to not wearing a seat belt. Today, there exists good knowledge of factors contributing to these behaviours. The challenge however is to change these behaviours by influencing the key factors and thereby reduce the number of accidents and the risk of personal injuries.

The objectives of task 1.3 is to gather information on variables that increase risk, pinpoint how behaviour can be changed by influencing these variables, and further to systematize evidence by elaborating a road user model that generate hypotheses, which can be empirically tested by campaigning against detrimental behaviour and accidents.

2.2.4 Task 1.4 - Key elements for evaluation

One main conclusion from GADGET was the frequent inadequacy of "evaluation designs" that had been used in evaluation studies. Very often an after-measurement of one single variable, as for example attention or awareness was used as a criterion variable for assessing the effect of a given campaign. This is clearly an insufficient evaluation design strategy putting the evaluators in an inconclusive position.

To do proper evaluations, the evaluators naturally have to think and plan in advance of the campaigns. Before-measurements are essential to be able to evaluate any behavioural change, and, hence, obtain valid conclusions about effects. It may seem self-evident, but this criterion is nevertheless frequently jeopardized. Key elements for evaluation should be sought at two levels: One regarding the individual campaign, the other the individual road user. In practice then, an outcome of task 1.4 should be the key psychological and social factors that influence driver behaviour, or aspects of the driver model that is expected from task 1.3.

Several candidate variables regarding campaigns have been mentioned – such as theme of campaign, type of campaign, size of target group, etc. In addition, factors as type of message, layout of message, the ability to stir up awareness, knowledge of the target group, intensity and frequency of messages, emotional aspects etc, must be elaborated qualitatively and quantitatively.

2.3 THE OBJECTIVES OF WP1

Summing up the above elaboration of the tasks of WP1, the following objectives were stated as the main objectives of (Annexe 1 "Description of work" of the CAST project):

- Describe state-of-the-art regarding applicable theories of behaviour and (partial) effects of campaigns;
- Estimate (partial) effects of campaigns on speeding, drink driving and related accidents, and estimate the (partial) effects of enhanced seat belt wearing on the reduction of personal injuries by meta-analysis;
- Develop a generic and theoretically based model for road safety campaigns by ascertaining campaign characteristics (key elements) that contribute to behaviour change and/or reduction in the number of accidents by meta-regression;
- Elaborate a model of the road user, based on applicable theories of behaviour, and empirical evidence of effective strategies for behavioural change;
- State hypotheses about predictors for behavioural change, establish key elements identified by evaluation studies, by meta-analysis, by road user models, and provide proposals for variables to be measured as integral parts of the evaluation of campaigns.

3. DEFINING CAMPAIGNS

A major finding in the GADGET and the INFOEFFEKT projects was that pure mass media campaigns, i.e. campaigns using only television, radio and/or newspapers and no other accompanying measure, did not have any effect at all on accidents (Delhomme et al, 1999; Vaa et al, 2004). As the CAST Annexe 1 very often refersto “mass media campaigns” as if this type of campaigns were the sole/main focus of the CAST project, we were concerned that the CAST project could be biased in the sense that it would focus too much on ineffective mass media campaigns and too little on other types of campaigns. There could then be a danger of excluding campaigns which have been effective, especially more local campaign types utilizing more personal communication and education than communication by mass media channels, which in the INFOEFFEKT project had been confirmed to be very effective in some cases (Vaa et al, 2004).

A special topic in this debate was whether one can – or should – distinguish between “Information” and “Education”. One could ask: When is it “Information” and when is it “Education”? “Which are the “cardinal criteria” that could separate these concepts? Another concern was that mass media communication channels as television, radio, newspapers plus leaflets, posters also can be applied locally, i.e. in counties, municipalities, companies. Is it then a “Mass media campaign”?

Based on this concern, and a discussion in CAST Steering Committee on whether “education” could or should be an element in what we would like to define as a campaign, we raised a debate on the Vienna Plenary meeting in April 2007.

With this as a starting point, IBSR as the co-ordinator picked up the glove and initiated a discussion on this issue which arrived at a conclusion which we find satisfactory. We will here put forward the main topics and conclusions from this discussion.

For a start, the CAST project adopted the definitions which had previously been applied both in the GADGET and the INFOEFFEKT project. These definitions are as follows:

Rice & Atkin, (1994: 365):

1) [Campaigns are] *….”purposeful attempts to inform, persuade, or motivate attitude and(or) behaviour changes towards safety in a relatively well-defined and more or less wide audience generally for non-commercial benefits to the individuals and/or society at large;*

> *2) campaigns are typically within a given time period by means of organised communication activities involving media often complemented by interpersonal support;*

Elliott, (1993):

> 3) *road safety campaigns are very often combined with other actions (enforcement, education, legislation, commitment, rewards, etc.),* or as Elliott (1993: 2) quote *" mass media campaigns need to be viewed as an integral support element for other countermeasures - a signposting role. ".*

Of these definitions, only definition amendment 3) explicitly uses "mass media" as a key concept. Rice and Atkin give their 1994-definition an update in their recent book (Rice and Atkin; 2000):

> "Purposive attempts to inform, persuade, and motivate a population (or sub-group of a population) using organised communication activities through specific channels, with or without other supportive community activities."

In another project, Coffman (2002) elaborated another definition – or perhaps more a description – of what a "campaign" actually is:

> "Public communication campaigns use the media, messaging, and an organized set of communication activities to generate specific outcomes in a large number of individuals and in a specified period of time. They are an attempt to shape behavior toward desirable social outcomes. To maximize their chances of success, campaigns usually coordinate media efforts with a mix of other interpersonal and community-based communication channels."

No doubt, (road safety) campaigns are indeed difficult to define in concrete terms as "campaigns" will comprise a huge range of activities that in some sense might be labelled "attempt(s) of mediating significant message(s) to an audience through a variety of communication channels". One conclusion of the discussions was that it is the ***communication*** – or the ***quality and type of communication*** that is of significant importance, not the media or the media channels as such.

Werner de Dobbelleer at BIVV/IBSR searched the literature on campaigns and campaign definitions and provided several definitions which were brought into a fruitful discussions subsequently. De Dobbeleer's final definition proposal was stated as follows:

> **"Purposive attempts to inform, persuade, and motivate a population (or sub-group of a population) to change its attitude and/or behaviour to improve road safety, using organized communication activities involving specific media channels within a given time period, often complemented by other supportive**

activities (enforcement, education, legislation, commitment, rewards, etc.).” (Werner de Dobbelleer, July 2007)

At this point of discussion it became apparent that the CAST-project and the WPs may need different definitions which means that the option of having a "working definition" is a good idea, and perhaps necessary. It also became quite obvious that the definitions may be categorized as either "**descriptive"** - i.e. what a campaign actually is or has been, and **"normative"** – i.e. "what a campaign ideally should look like".

The differences between for example WP1 and WP3, can be illustrated as follows: WP1 would like to sample all types of campaigns, "good or bad", "ineffective or effective", while WP3 - in their Manual, obviously would be much more concerned with defining the best way of performing campaigns. So, with that background, it would be apparent that WP1 and WP3 will prefer to define campaigns differently. Based on this, we decided on the following the “working definition” in WP1 in the CAST project:

“Purposive attempts to inform, persuade, and motivate behaviour changes in a relatively well-defined and larger audience in order to improve road safety, typically within a given time period, by means of organized communication activities involving specific media channels often complemented by interpersonal support and/or other supportive activities as enforcement, education, legislation, commitment, rewards, etc.”

Hence, this is the definition which we have used in WP1 to search for studies and for inclusion in the database we use for meta-analysis.

4. NATIONAL CAMPAIGN EXPERIENCES

The description of the national campaign experiences in CAST-partner countries are different. Some give a general description of campaigning in their country, some mention those campaigns included in the CAST database and some concentrates on the description of important campaigns. This might be confusing for the reader. On the other hand it underlines the fact that there are significant differences between the countries.
Based on information that was revealed in the Oslo workshop, it became apparent that partner countries were not on equal footing regarding past experience with road safety campaigns. As a consequence of these differences, we proposed to add a short description of campaign traditions in each of the partner countries. A request was sent to each CAST partner asking for a "1-page description" of each of the participating countries. The following topics were proposed as guidance regarding how to describe campaigns in each of the countries:

- Number of campaigns conducted?
- When conducted? (Time periods)
- Type(s) of campaigns: Mass media only?
- Additional measures used? Information/education/New legislation?
- Any experience with "Publicized enforcement?
- Local campaigns? (counties, cities, municipalities, companies)
- Themes: behaviour/accidents addressed
- Evaluation characteristics ("awareness after" only, before-after…..)

The national descriptions below comprise 15 European countries.

4.1 AUSTRIA[1]

When conducted?

With respect to the duration there are long term campaigns (e.g. "apple & lemon" - continuous since 1994: pupils "punish" fast drivers with lemons, and drivers with appropriate speed receive apples as a present, campaign is carried out on a national level; or fair and safe – I take part" – annual since 2001 the self-motivation and self-control regarding speeding, alcohol and seat belt wearing shall be increased by information and various activities, campaign is carried out on a regional level), and short term campaigns (e.g. "life is fast enough" – local campaign carried out in 2003; during two weeks people were informed of the introduction of 30km/h zones in the city of Graz by folders, banners and various activities).

[1] Text provided by Karin Ausserer, FACTUM

Types of campaigns

Traffic safety campaigns in Austria are common practice on various levels. The types of campaigns vary from information and awareness campaigns to educational and combined campaigns. There are international traffic safety campaigns (e.g. European Red Cross road safety campaign - the aim: is to reduce the number of children killed or injured in road traffic), national campaigns (e.g. “stay alive-stop speeding”, “seatbelts save lives”), regional campaigns (e.g. “life is fast enough” , “safe – not too fast), and local campaigns (e.g. “The traffic saving community Langenlois” – a campaign to motivate people to change from car to other traffic modes). National campaigns are usually initiated by the ministry of traffic technology and innovation or by the ministry of the interior. Regional campaigns are carried out by the responsible traffic department of the provincial government, and local campaigns by the municipalities. There are, however, also other institutions that set off traffic safety campaigns like the Traffic Club Austria, Car Touring Clubs or the different parties.

Themes of campaigns

The themes are manifold: speed, alcohol, seatbelt, visibility, mobile-phoning, traffic safety and children, traffic safety and pedestrians, traffic safety and cyclists, etc. Enforcement is mainly involved in speed or alcohol campaigns. Mass Media campaigns are common for increasing seat-belt wearing rates, reducing speeding and the number of people who drink and drive. Direct communication is primarily used in campaigns to increase the traffic safety for children.

Evaluation characteristics

Even though there are lots of traffic safety campaigns carried out in Austria, hardly any detailed evaluation is done. In average, one out of four campaigns is somehow evaluated in an after studyThe evaluation mainly concentrates on the awareness/recognition/recall and not on behavioural change. Before-after studies are the exception and are still not common. According to an employee of the ministry of transport, innovation and technology, evaluation only became a topic since 2005.

4.2 BELGIUM [2]

When conducted?

The Belgian Road Safety Institute (IBSR-BIVV) has extensive experience with road safety campaigns ever since the first campaigns in Belgium were started (1960’s). At present, IBSR-BIVV runs 6 large-scale national campaigns each year. The duration of each national campaign is approx. 4 to 6 weeks.

[2] Text provided by Werner de Dobbelleer, IBSR/BIVV

Accompanying measures

The campaigns always include billboards along the main road network, most often in combination with radio or television advertisements, and supported by information leaflets, brochures, internet websites, and/or promotional materials. If applicable, they can be combined with education (e.g. child seat campaign) and/or inform on new legislation (e.g. new priority rules).

The campaigns are always combined with enhanced police enforcement. The enhanced enforcement is always publicized at the start of the campaign. In case of the drink driving campaign in the end of the year period, the enhanced enforcement is also publicized at mid-term (before New Year's eve) and at the end of the campaign (mid-January).

The national campaigns are also advertised locally by means of small-size posters that are sent to a network of public institutions, libraries, municipalities, police stations, schools etc. In the future, the volunteer's network (Ikbenvoor – Jesuispour) will provide local support for the national campaigns as well.

Themes of campaigns

The campaign themes are defined according to the priorities set by the Federal Road Safety Commission (FCVV-CFSR) and the European Commission, with driving under the influence, speeding and the use of restraint systems as top priorities. The campaigns address attitudes as well as behaviour, and by doing so try to reduce accidents. The final aim of each campaign is always to reduce the number of accidents and victims.

Evaluation characteristics

Each campaign is evaluated by means of a post-test (after-measurement), conducted by an independent marketing research institute. The post-test measures awareness and appreciation of the campaign as well as attitudes, behavioural intentions and self-declared behaviour of the target audience. If possible, these data are complemented by observed behaviour data (pre- and post measurements), for example seat belt wearing rates.

4.3 CZECH REPUBLIC [3]

Historic background

The Czech Republic was a communistic state until November 1989, when there was a revolution and putsch. By that time everything was under state control, the state police had a big authority. The month April officially became the National Month of Safety. Every year a safety theme was chosen (mainly

[3] Text provided by Eva Sedá, CDV

Speed, Seat belts, Alcohol) and introduced on a press conference. Some safety and preventive activities were supported by the state budget (e.g. leaflets, posters, traffic contests for children, etc. One contest was even organized nationally with co-operation with Czech National Radio Station).

Until the year 1998 BESIP (in Czech language this is an abbreviation of road safety) was a department of Czech Ministry of Internal Affairs. Since 1998 BESIP is a department of Czech Ministry of Transport. In 1999 came the first offers for Road Safety Projects made by advertising agencies. Since 2000 the First Road Safety Projects started and were called „campaigns" (visibility, cycling…), but there were no evaluation of these.

Process of organizing new campaigns:

The Czech Ministry of Transport – BESIP Department (Road Safety Department) **or/with**
ÚAMK ČR – civic association supporting motor sports and road safety education **or/with**
Associations or Individuals **or/with**
Others
↓
Problem, Idea
Objectives (National Road Safety Strategy)
↓
Checking Budget
↓
Cooperation – Subcontracting (PR, Media, Newspapers...and other)
Making Campaign Elements
↓
The Campaign

Types of campaigns

There is only few mass media support, because it is very expensive. But the Czech Republic is a partner of many consortiums and is involved in pan-European Road Safety campaigns (EUCHIRES 2005, EUCHIRES 2006, EUCHIRES 2007, "Designated driver) and Road Safety Projects (Close To, Close To Module, Road Safety Web, The Action - Road Show).

Themes of campaigns

The following are the best known Czech Road Safety campaigns/projects:

- 50 km/h Speed Limit Has a Sense
- Do Not Drink and Drive
- Designated Driver
- When You Drink Do Not Open the Car
- The Death Does Not Bind
- Will You Buckle Me Up?

- Visibility
- Keep Safe Distance
- Incaution Kills
- The New Rules – I Better Control Myself
- The Christmas Campaign
- Incaution Kills
- Close To – Risk Prevention for Beginning Drivers
- The Action
- Packages for the first class pupils
- Bike Only With a Helmet
- Hold Your Steering Wheel Not The Handy

Evaluation characteristics

Evaluation of campaigns and road safety projects in the Czech Republic does not have any long history and tradition.We have a few evaluated campaigns/projects (Will you buckle me up? – EUCHIRES 2005 and 2006, Close To, The Action, Bike Only With a Helmet) and only two are evaluated by before- and after-measurements. Questionnaires and/or field observations are used as evaluation tools.

4.4 DENMARK [4]

Historic background

Denmark started Road Safety Campaign activity already in 1936. However, it was not until the 1950's that it really took off. The 1950's and 60's were the golden years for the campaigns but it was recognised that it was too expensive to sustain that level of activity so the activity level was reduced. In the 1990's campaigns at the local level were introduced which resulted in an increase in campaign activity yet again.

On a yearly basis approximately 4 national campaigns and 5 – 10 local campaigns are carried out. Many of the national campaigns are run in cooperation with the local counties and municipalities.

Types of campaigns

The national campaigns are commonly mediated through roadside posters and television spots. Sometimes leaflets are sent out to the households. The local campaigns commonly use roadside posters, radio spots and direct mail. In addition, adverts in newspapers, cinema, SMS, postcards, stickers, folders, and note pads are used - primarily locally though. In recent years the internet has increasingly been used to mediate campaigns both nationally and locally.

Themes of campaigns

It is mainly three areas of traffic behaviours that are addressed in the campaigns in Denmark: Speeding, drink driving and restraint use. Some campaigns aimed at cyclists have also been launched for example to increase

[4] Text provided by Annette Meng, DTF

helmet use or the use of lights. However, quite a few campaigns are aiming to increase the number of cyclists and thus not focussing on traffic safety per se.

Campaigns aimed at speeding

Speed campaigns are launched on a regular basis, approximately once a year, at the national level. The counties and municipalities often join in these campaigns. Speed campaigns are the type of campaigns that are most often supplemented with local police enforcement. Some counties and municipalities have launched speed campaigns especially aimed at young drivers. In addition to the campaigns some counties have arranged project days, courses and talks by young people who have been disabled from traffic accidents themselves.

Campaigns aimed at drink driving

The Council for Road Safety launched several national campaigns against drink driving in the 1990'es. However, in the period 1998 – 2003 no national campaigns were launched. The latest campaign was launched in 2004 and was run both nationally and locally and combined with police enforcement. This campaign was evaluated using phone interviews asking if people would stop a drunk person wanting to drive – no before measure was used. In addition, some local evaluations were performed using accident statistics or interviews. Since 2001 there have been several campaigns aimed at young drivers both nationally and locally that also involved the topic of drink driving. Additional measures such as teaching materials, talks by young people who have been in traffic accidents themselves, and the establishing of parent networks have been used in this connection.

Campaigns aimed at restraint use

The Council for Road Safety has launched national campaigns on restraint use yearly from 2001-2003 and again in 2005. These campaigns were also run locally in many places. In addition, they were combined with police enforcement. Furthermore, there have been some local campaigns aimed especially at young people or children.

Evaluation characteristics

As a rule all larger national campaigns are evaluated. However, the local campaigns are not very often evaluated mostly due to limited resources. In general, for the national campaigns, there has been an increase in the number of evaluations since 1990.

The Council for Road Safety, who arranges the national speed campaigns, usually evaluates these campaigns. The evaluations are commonly qualitative and based on a limited number of participants. The evaluations sometimes just evaluate if the target group have been exposed to the campaign, and if so what their opinion of the message in the campaign is. In some cases participants are asked if the campaign has influenced their behaviour. A few of the counties did also evaluate the campaigns locally. The methods used were: looking at accident statistics, interviews or questionnaires.

Especially the smaller local campaigns aimed at seat belt use are often not evaluated. But in the cases where they have been evaluated the method used was restraint use counts, analysis of accident statistics, or surveys.

4.5 FRANCE [5]

Time periods

The French campaigns mentioned in this document conducted for 5 of them for several months (between 1 and 3 months) or for 4 of them for several days (between 10 and 22 days). The oldest one was conducted in 1983 and the most recent one in 2004.

Most of them were mass media only campaigns, with only 3 exceptions: 2 were made with enforcement (in 1990 and 1993) and an other one with a new legislation (in 1994). France has had no campaign accompanied with publicized enforcement.

Themes addressed in French campaigns:

- Speed, other traffic regulations (red light)
- Speed, seat belt use, vulnerable road users (pedestrians, bicyclists), other traffic regulations, physical conditions
- Be careful during the summer
- General ability to drive
- Seat belt use, driving under influence, other traffic regulations, physical conditions, driving circumstances
- Seat belt use, speed
- Driving under influence
- Safety distances
- Driving under influence, speed and seatbelt

Evaluation characteristics

There are only after-evaluations, no before-after evaluations,

4.6 GERMANY [6]

Themes of campaigns

Germany do not conduct many campaigns on a country level, but rather on the level “Lãnder” and counties within “Länder” (Germany has 16 “länder”). However, three road safety campaigns should be mentioned here, because they comprise the whole, or nearly the whole country: 1. “Darauf fahr‘ ich ab:

[5] Text provided by Patricia Delhomme, INRETS
[6] Text provided by Ewald Pohlmeier, BASt

Trinken und Fahren könnt ihr euch sparen“ (That’s the thing for me: you can forget drinking and driving), 2. “Gelassen läuft’s“ (Keep cool - keep moving) and 3. “Rücksicht ist besser“ (Consider Consideration).

These campaigns served the purpose of intensifying the social responsibility of each individual road user and of changing the behaviour in road traffic in a safety supporting way. The aim is to behave more considerate as a principle for sharing the road space.

Three types of campaigns

Campaign 1 “Darauf fahr ich ab”: In 1998/1999 a road safety campaign against drinking and driving was carried out in 17 counties of 12 German “länder”. Young citizens (female 16-24 years, male 18-24 years) received a letter and an information brochure on the risks of drinking and driving. Different media outlets (e. g. local radio), poster advertising, personal letters and public promotion supported this road safety campaign. In some counties enforcement measures of the police had also been intensified during the road safety campaign.

Evaluation characteristics

The road safety campaign has been accompanied by several scientific analyses to study the impact of the campaign. These scientific analyses comprise statistical accident analysis, time serious analysis and questionnaires. The results have been summarized in a road safety related cost/benefit-analysis.

Campaign 2: „Gelassen läuft’s“ (Keep cool - keep moving)

For improving the general atmosphere between traffic participants the German Ministry of Traffic, Building and Urban Development has created the campaign „Gelassen läuft’s“ (Keep cool - keep moving). According to some analyses weak traffic participants have been especially endangered by increasing aggression, stress and conceitedness. So a new attractive role model for the behaviour in road traffic comprising self responsibility, sovereignty and aplomb should be introduced.

The nationwide campaign started in March 2001 with posters, in newspapers and magazines over a duration of 12 months and was designed for adults. These classical media were accompanied by numerous other activities carried through by partners of all social groups. In order to achieve a broader effect this campaign was connected with the activities of the German Traffic Council (DVR) and the German Traffic Guard (DVW). An evaluation which has gone along with this traffic safety measure has shown that public feedback has been characterised by large acceptance.

Campaign 3: "Rücksicht ist besser" (Consider Consideration)

The German Ministry of Traffic, Building and Urban Development has started this traffic safety campaign in July 2004 and it was finished in March 2006. In order to achieve a broader effect this campaign was connected with the activities of the German Traffic Council (DVR) and the German Traffic Guard (DVW). The core message was that people should behave more considerate and cooperatively in road traffic.

4.7 GREECE [7]

Theme of campaigns

The following 10 campaigns are considered as the most complete in terms of design, implementation, importance of theme addressed and evaluation (even potentiality of evaluation, where evaluation was not conducted within the context of campaign):

- Bob Campaign,
- Seatbelts and Helmets Campaigns (Tsalikis),
- Seatbelt Campaign (KEPPA1),
- Infant Car Protection Equipment (KEPPA2),
- Helmet Campaign (KEPPA3),
- Railway Crossing Safety (Hellenic Railways),
- Picketing,
- Smashed Cars,
- Driving Behaviour (Larisa),
- Drinking and Driving (Psomiadis)

In the sample of 10 campaigns, 2 of them have as a theme the prevention of alcohol usage if driving, 2 the usage of seatbelts, 1 the usage of helmets, 1 the child car restraint equipment usage, while the other 4 the safe driving in generally.

Time periods

The 10 campaigns were conducted in the period from 1996 to 2007. Three of them (Bob Campaign, Seatbelts and Helmets Campaigns (Tsalikis) and Infant Car Protection Equipment) take place until today and are still on going .

Type(s) of campaigns

All of the 10 campaigns have an approach through mass media (television, Radio, Press or Magazines), while more than the half of them includes an integrated approach.

[7] Text provided by Teti Nathanail and Giannis Adamos, University of Thessaly

Additional measures used

In the case of campaigns, where integrated approaches were implemented – i.e. for 6 out of 10 campaigns, the additional measures concern mostly the law/police enforcement, the education and the driving schools.

Scale of campaigns

Among the 10 half of them were local or regional (Infant Car Protection Equipment (KEPPA2), Helmet Campaign (KEPPA3), Smashed Cars, Driving Behaviour (Larisa), Drinking and Driving (Psomiadis)) and the other half national (Bob Campaign, Seatbelts and Helmets Campaigns (Tsalikis), Seatbelt Campaign (KEPPA1), Railway Crossing Safety (Hellenic Railways), Picketing).

Evaluation characteristics

Among the 10 campaigns, an evaluation has only been conducted in 4 of them. “Bob Campaign” used a comparison of accidents before and after implementation as an evaluation method, “Seat Belt Campaign-KEPPA1” made an observational study of 2250 drivers and passengers, and “Helmet Campaign (KEPPA3) used interviews of motorcycle riders.

4.8 ITALY [8]

Who carries out road safety campaigns in Italy?

At a national level, a number of organizations, public and private, invest money on road safety campaigns:

1) Ministries (mainly Transportation, Health, Interior Affairs)
2) Insurance companies (National Association of Insurance Companies)
3) Police corp authorities
4) Foundations
5) Highway companies

Italy do have the particular situation, many decision makers do state that “we have a road safety problem! Let’s do a campaign!” and one can a usual overestimation of the power of the campaigns that leads to a number of campaigns carried out, but they are mainly seen as a good and immediate reaction to the polemics raised by the road accident toll. Overall, the number of road safety mass media campaigns in Italy is quite high, also because it is used as most immediate answer to the public attention of a problem.

[8] Text provided by Gian Marco Sardi, SIPSiVi

Scale and types of campaigns

At a national level, mainly television and radio spots are concerned. Campaigns are rarely accompanied by enhanced enforcement; and when they are, it is quite difficult to see evidence or explicit links between the initiatives. At a lower level, Regions, Provinces and Municipalities carry out road safety campaigns: One might see collaboration/co sponsorship of private organisations.

There is no clear strategy or explicit decision, at least at national level, but fear arousing appeals messages, with shocking images in television spots are mainly adopted. At a national level, television and radio spot are mainly adopted. The collaboration, either at *vertical* (same field, different level, e.g. from municipality up to province, region and ministry) or *horizontal level* (same level, different field e.g. between ministries of different types, as transport and health) is very rare, if not absent. The target group is rarely defined, even if we can see campaigns mainly and generally aiming at young drivers with speeding and drinking and driving as main issues.

Theme of campaigns

The topic is rarely defined, but recently an higher number of campaigns have been registered with alcohol related problems. Note: even if we have a specific argument chosen for the campaign (alcohol in this case) unfortunately often this specificity is not only dedicated to the road safety issue: e.g. often the organisation involved are in the health frame (public or private) and they often choose as a strategy to start from an alcohol related issues (consequences of alcohol assumption/abuse/addiction) and extending afterwards the problem to the drinking and drive issue.

Evaluation characteristics

It is difficult to obtain detailed reports of the evaluation of campaigns independently from the level (national/local) and evaluation seems to be really rare. When it is present, evaluations are only on the form of after-measurements regarding the opinion of the campaign (if the campaign was liked or not).

4.9 THE NETHERLANDS [9]

Themes and time periods

The use of road safety campaigns to increase road safety has gained prominence in the Netherlands since the eighties. Over the years the campaigns have concentrated on various themes: speeding, safety distance, drink driving, bicycle lightning, seat belts, aggression, the introduction of new legislation, etc.

[9] Text provided by Paul Schepers, The Transport Research Centre AVV (part of the Ministry of Transport, Public Works and Water Management

Scale of campaigns

Campaigns were conducted on national and regional level. In 2003 the Long term Programme on Road Safety Campaigns (LPRSC) was introduced. The LPRSC contains a framework that gives direction to the traffic safety campaigns implemented during the period 2003-2007. Both the development and execution of these campaigns are conducted in close cooperation with local government authorities, enforcement agencies and relevant social interest organisations. Concentrating the campaigns on certain key themes over a period of five years should ensure continuity of the efforts on a national and regional level.

Evaluation characteristics

In order to establish the yield of the campaigns, an extensive monitoring programme is in place around the LPRSC. A chain model consisting of input, output and outcome is generated on the basis of the monitoring data, making it possible to visualize the campaign effects. Behaviour is measured by questionnaires and roadside observations, both before and after the campaign. The chain model applied is shown in the figure below.

Figure 4.1: Chain model describing the process of linking input, output and outcome of Dutch campaigns

	A campaign is defined as the coordinated, thematic utilization of national mass media public information, local actions and enforcement throughout a specified period	
Input (ex ante)	List and description of the activities carried out on behalf of the campaign implementation	
Output (ex post)	Quantitative statement of the material resources deployed on behalf of the campaign	
Outcome	Campaign processing	Reach, valuation, recognition and recall, and dissemination of the campaign message
	Campaign processing	Knowledge, attitude, risk perception, subjective chance of getting caught
		Self-reported and observed behaviour
		Relation to development in victim data (indicative)

4.10 NORWAY [10]

The following is a short description of Norwegian road safety campaigns evaluated in the period 1984-2005.

Number of campaigns

A total of 31 Norwegian evaluation reports/articles concerning road safety campaigns were identified for the period 1984-2005. However, only reports

[10] Text provided by Inger Synnøve Moan, TØI

which tested the effect of a single campaign are included in this overview. Evaluation reports containing the effect of several studies, i.e. meta-analysis, were excluded. The remaining sample consisted of 23 road safety campaigns.

When conducted

The 23 articles/reports that were identified were all published in the period 1984-2005. Four reports were published in the period 1984-1989, 5 were published between 1990-1999, 14 reports were published in the years 2000-2005. This equals an average of 0.80, 0.60, 2.80 publications per year in the three time periods, respectively.

Additional measures used

Of the 23 Norwegian evaluation reports/articles that were included in the CAST database, 9 concerned mass media campaigns only while 14 concerned integrated approaches (mass media campaign and police enforcement/controls mostly). It is not evident from the 23 evaluation reports whether or not police enforcement/control were particularly comprehensive and/or whether they received a lot of attention in one or more media channels.

Scale of campaigns

Of the 23 campaigns, 14 were local/regional and 9 were national.

Themes of campaigns

The themes most commonly addressed in the 23 Norwegian road safety campaigns were: to reduce speed of drivers (8), make young road users responsible (cf. "Speak out!") (4), increase seatbelt use (2), make pedestrians behave safely in traffic (2), make bicyclists behave more safely (2), increase the use of safer/healthier means of transportation (2), sleepiness (making drivers stop) (1), increase the safety distance to the car in front (1), and increase and improve the use of child restraint systems (1).

Evaluation characteristics

Of the 23 campaigns, 3 studies assessed after-measures only, 1 study assessed effect measures before the campaign was implemented and during the campaign period, 16 assessed before- and after measures, and 3 studies assessed before, during, and after measures.

4.11 POLAND [11]

Themes and time periods

The first national seatbelt campaign was implemented in fall 2005. On the basis of the research results the target group is young people (18-25) and the message is addressed mainly to those sitting in the back seat. It shows that a crash may occur any time and everywhere and that not using the seatbelts has a dreadful outcome. The slogan was "the last bash" when a young man,

[11] Text provided by Barbara Krol, The National Road Safety Council, Republic of Poland

unfastened in the rear seat, bashes out through the windshield indicating that this would be the final move of his life. The campaign was repeated in 2006 and studies demonstrate need for new edition of seat belts campaign.

Type of campaigns

The purpose of the project was to ensure **high profile media coverage** to this nationwide seatbelt campaign. The campaign media included television, radio, cinemas, billboards, posters in clubs, discos, bus backs and there is intensive PR envelope. The campaign ran nationwide in September – October 2005 and was supported by step-up enforcement of the seatbelt use by the police.

A cycle of campaigns under the logo slogan “Turn on thinking” continues. Themes of this campaign cycles are Speeding, alcohol, seatbelts and road users. Smaller, but nationwide campaigns on alcohol and visibility of children have been done according safety issues that vary with time of the year.

Scale of campaigns

Most of campaigns in Poland are local campaigns, initiated by cities, municipalities or private sector. The ”Last bash” campaign was nationwide.

4.12 PORTUGAL [12]

Types and levels of campaigns

Several different road safety campaigns have been regularly implemented in Portugal for many years, some of them particularly focussing on the themes alcohol and driving, speed, seat belts and child restraint systems, pedestrians and 2 wheeler user’s safety, with different communication strategy and approach. Themes for the campaigns are mainly decided on the basis of data and analysis on accidents and road user behaviour although at local level they are also decided on the basis of county specific problems.

Road safety campaigns implemented at a national level are organized and funded by the Ministry of Interior, until 2005, through the Traffic Directorate, and nowadays through the recently created National Road Safety Authority.

Campaigns funded by the government are normally conducted by Portuguese Road Safety Association that is responsible for the developing, organization and implementation of nationwide campaigns.

At a regional and local level, campaigns are also implemented and funded by authorities and other partners in the community.

[12] Text provided by Alain Areal, PRP

Accompanying measures

To advertise campaigns different communication strategies to reach different target groups are used. Several campaign materials are used and chosen according to criteria of cost-effectiveness and the target group to be addressed. Mass media campaigns are combined with additional measures, such as live demonstrations or awareness and information actions.

Campaigns running period are normally decided according the available resources, problem analyses, statistics data, and availability of target groups that is supposed to be receptive of media messages.

Evaluation characteristics

Campaign evaluations in Portugal are not a regular practise. The Prevenção Rodoviária Portuguesa (PRP) started doing evaluations a few years ago and at present moment PRP evaluate all campaigns directed by us. It's more usual to carry out evaluations about campaign's notoriety and perception than to evaluate changes of behaviour.

PRP started to carry out pre- and post-evaluations to see if changes have been brought about. PRP also measure the attitudes and behaviours of road users in general and compare these figures with those of previous years and gather the statistics on the number of performed controls. Concerning changes in behaviour the evaluation is normally done by using pre-test and post-test observations and comparison with rates measured in both observations. Post-tests on the perception and notoriety of the campaign is normally done by a sample that is representative of the population.

4.13 SLOVENIA [13]

Types, themes and scale of campaigns

Implementing road safety campaigns is primarily the domain of the Slovene Road Safety Council, the Ministry of Transport, and the Ministry of Health in co-operation with the police and other governmental and civil institutions. Preventive efforts can generally be classified in two broad categories:

- Media campaigns (publicity, media component)
- Local/small-scale actions (e.g. activities for schools/kindergartens in the beginning of the school year; workshops for secondary schools; stands in shopping centres/local areas, etc.) – these activities are quite frequent; sometimes they coincide with the media campaigns (as additional/supportive measures) but they most often stand alone.

Speaking of media campaigns, they have been generally implemented nationally addressing a wide range of the major road safety issues such as speeding, intoxicated driving, seat belt use, the use of mobile phones while

[13] Text provided by Marko Divjak, University of Ljubljana, Faculty of Arts (ULFF)

driving, pedestrians' safety, etc. Most of them were launched on a single occasion and were thus not part of the long-term strategy (e.g. the same concept being repeated several times). Depending on the theme, most campaigns have been aimed at reducing violent behaviour (e.g. adherence to the speed limits, sober driving) and/or stimulate the use of protective measures (seat belts, child safety seats, visibility of pedestrians) with the ultimate goal being the reduction in the number of road accidents and associated fatalities (e.g. by persuading drivers to keep up with the speed limit it was intended to reduce the number of speed-related accidents).

Accompanying measures

Recently, there has been a tendency to combine media campaigns with increased police enforcement, although such an integrated approach is often still unsystematic (e.g. the timing of media activity and enforcement do not exactly match). Increased enforcement also frequently stands alone (without the media campaign) and is publicized in advance (e.g. in the newspapers, press-conferences, television-emissions). Also, in the last couple of years much more financial resources have been devoted to construct and implement each campaign, which has significant implications for the more effective positioning of campaign's messages. Previously,television and radio spots were often advertised outside prime time (because of financial constraints).

Evaluation characteristics

Although Slovenia has a lot of experiences with implementing campaigns most of them have not been systematically evaluated. Campaigns were traditionally either:

a) not evaluated at all,
b) assessed only in terms of awareness/recognition/recall (by means of telephone polls) or
c) informally evaluated – without structured methodology (in terms of campaign's outputs, e.g. the number of leaflets delivered, the number of postcards returned; or in terms of coincidental descriptive responses from the affected/interested parties – local road safety councils, teachers, experts, etc.).

A milestone towards a more scientific and systematic evaluation represents the Euchires campaign in 2005. Its effectiveness was assessed by means of different measurement variables:

reach/awareness/recognition/attractiveness of the campaign (after-only); observed behaviour (before-after) and self reported measures (before-after study with a control group that was formulated on the level of supportive action – education in a few kindergartens). Similar evaluation methodology was also used to evaluate the Euchires 2006 campaign.

4.14 SWEDEN [14]

Time periods

Campaigns from the period 1966-2005 were found in the review. Many of the more ambitious and national campaigns lasting for a year or more were carried out during the 1970's and early 1980's. Later, campaigns were often carried out on a local or a regional level.

Type of campaigns

The message could be conveyed by a number of ways in a single campaign, typically by advertisements in newspapers and magazines, leaflets, billboards and spots on television. Leaflets could for example be distributed by mail to vehicle owners together with other information from an authority or to targeted groups such as parents of children aged 5-6 years. In other campaigns, leaflets could be made available at gas stations etc or handed out directly by people engaged in the campaign placed at companies, parking places etc. New techniques as DVD/video and Internet have not been used frequently.

Level of campaigns

Both local and national campaigns have been carried out. Either a single county or 2 or more counties have been targeted in regional campaigns. Local campaigns have been carried out in cities (one or more), on streets with many accidents and at companies.

Themes

Speeding and drunk driving are the two most addressed undesired traffic behaviour in Swedish campaigns. Another common theme is to increase use of safety systems as seat belts, bicycle helmets and restraint systems for children. Also to show consideration for vulnerable road users, groups such as children, cyclists, pedestrians and elderly have been specifically addressed in several campaigns. General traffic safety has been in focus in other campaigns instead of specific types of accidents, but two examples of the latter are single accidents and game collision.

Evaluation characteristics

In about half of the campaigns that were found in the review, only an evaluation afterwards was made and usually penetration and possible change in behaviour were addressed. The other campaigns, which were scored, carried out before and after studies but seldom with control groups/areas.

[14] Text provided by Sonja Forward, VTI

4.15 SWITZERLAND [15]

bfu – Council for Accident Prevention in Switzerland is a non-governmental, private foundation (founded 1938). The bfu has a large knowledge and experience in the development, implementation and evaluation of traffic safety campaigns (during the last 20 years bfu has implemented three to four national campaigns per year). When doing road safety campaigns bfu often collaborates with other bodies as the Swiss Road Safety Fund, automobile clubs, insurances or the police. The former is (co)financing most of the road safety campaigns in Switzerland (via non-occupational accident insurance premiums).

Types and scales of campaigns

The core elements of a national mass media campaign headed by bfu are billboard advertising, television-spots and sometimes radio-spots. In Switzerland advertising along streets is not allowed with an exception for road safety. Bfu has the possibility to placard on its own 350 "Mahnwände" (about four times the F4-format) on motorway accesses and along rural roads. Furthermore bfu has – through special agreements with the local authorities – the possibility to placard on special mobile advertising column (format F4) in the communities (directly beside the roads). television-spots and radio-spots on road safety have lower charges than commercial advertising. Bfu often combines mass media campaigns with personal contacts with the target group (e.g. in discos, parking garages, on events, police controls) and distributes leaflets, give-aways etc.

Furthermore there are smaller (more regional or even local) campaigns implemented through local authorities or local associations (e.g. of residents in a quarter) – without participation of bfu.

Themes of campaigns

The topics of the campaigns sometimes have a long tradition (e.g. an annual campaign at the start of a new school year) or are the product of political interests (e.g. campaigns on speeding from local authorities) without any relation to its importance regarding accidents. But more and more campaigns – at least regarding the big national campaigns which are (co-)financed by the Swiss Road Safety Fund – are based on research. The research department of bfu strongly demands a scientific situation analysis from its partners before developing a mass media campaign.

Topics of national mass media campaigns during the last few years with bfu-participation or bfu-lead:

[15] Text provided by Esther Walter, BfU

- annual campaign at the start of the new school year
- annual campaign to promote helmet wearing when cycling
- 2000 and 2001: campaign to raise the risk awareness of young drivers
- 2001, 2002, 2003: campaign to promote daytime running lights
- 2004: campaign about alcohol and driving to inform about the new legislation (0.5 ‰ BAC when driving)
- 2005, 2006, 2007: campaign to promote seat belt wearing
- 2007: campaign to promote a correct adjustment of the head restraints

Other campaigns:

- 2006, 2007: campaign to raise the risk awareness of motorcyclists
- 2006: campaign against speeding / racers

Evaluation characteristics

We are further urging an evaluation with at least one pre-and post--measurement on different levels (outcome, impact and process evaluation) – if possible with a control group which is quite difficult in national campaigns. A very basic evaluation has become a standard for most campaigners. But to recommend a professional evaluation is still hard work.

5. EFFECTS OF CAMPAIGNS: "STATE-OF-THE-ART" PER 2006

The topic of estimating effects of campaigns has over the years been addressed in several literature reviews (Elliot, 1993; Delhomme et al, 1999; Elvik and Vaa, 2004[16]; Vaa et al, 2004, Delaney et al, 2004). These comprehensive studies differ in several ways. Of these five reviews all but Delaney et als' study use meta-analysis as an instrument to assess an estimate of the effect of campaigns. Elliot's study address awareness, knowledge, attitudes and behaviour, but not accidents. Elliot and Delaney et als' studies are explicitly confined to mass media studies only. Finally, the three studies specifically addressing effects on accidents are Delhomme et al, Elvik and Vaa (2004) and Vaa et al (2004). That strategy pursued here is the one of applying meta-analysis to assess a concrete estimate of the effect of campaigns on the number of accidents. While Delhomme et al, and Elvik and Vaa, only use meta-analysis to assess a rather crude and un-sophisticated estimate, the Vaa et al study (2004), has incorporated more sophistication of meta-analytic techniques in terms of correcting for publication bias, testing heterogeneity and applying meta-regression to assess partial effects of given predictors.

It is in this sense, that the present description of effects is labelled state-of-the-art regarding estimation of the effect of campaigns, ie. what we knew of the effects of campaigns when the CAST-project was kicked-off in February 2006. The results presented here are from the INFOEFFEKT project which TØI completed in 2004 on commission from the Swedish Public Roads Administration ("Vägverket") (Vaa et al, 2004). It builds on the GADGET project which was completed in 1999 (Delhomme et al, 1999). We also include a cost-benefit analysis and appraisals of cost-effectivenes of selected Swedish campaigns as these topics are also addressed in the CAST project. The Swedish examples may, hence, serve as "good examples" on how to perform cost-benefit analysis of effects of campaigns on accidents.

5.1 INFOEFFEKT 2004: RESULTS FROM META-ANALYSIS

The report from the INFOEFFEKT project consists of three studies of relevance for the CAST project (Vaa et al, 2004):

1) Effects of campaigns on road traffic accidents,
2) Effects of campaigns on other areas of behaviour than road traffic,
3) Swedish campaigns: Appraisal of cost effectiveness and benefit-cost aspects

[16] Chapter 7.3: Road user information and campaigns, pp 949-954.

5.1.1 Effects of campaigns on road traffic accidents,

Study 1 in INFOEFFEKT considered estimation of effects of campaigns on road traffic accidents. The basis for estimating the effects was the construction of a database, which comprised 86 results from a total of 30 evaluation studies. Of these, 72 results were used for estimating the effects of campaigns during the campaign period and 14 results for estimating the effect in the after-period. Two types of analysis were presented, the first from meta-analysis, concerning the general, overall effect of campaigns, and the second comprising results from meta-regression, concerning factors – partial effects – helping to explain the effects of campaigns.

The overall, general question, addressed, was whether campaigns can be said to have effects on road traffic accidents. The premises for answering this question were:

1. Only evaluation designs using a control group or comparison group were included in the database
2. The data were put through meta-analysis in order to calculate a best estimate of a potential effect
3. The data were tested for homogeneity and corrected for publication bias

Considering the studies that evaluated effects in the campaign period the number of campaigns against drink driving was 33. Statistical tests indicated publication bias (the tendency not to publish results from campaigns where the effect seems to have been zero or gone in the opposite direction of what would have been expected; see Chapter 6 or section 7.3 for further explanation). A number of “campaigns” was therefore added to counterbalance the effects of these biases. There was no publication bias among the studies used to estimate effects in the after-periods.

Under these premises the best estimates were calculated. (The results are summarised here but results detailed in Appendix D of this report.) The weighted average of the effect of road safety campaigns in the campaign period was found to be an 8.9 % reduction on the number of road accidents. In the after-period the effect of campaigns was estimated to a reduction of 14.8%. Both estimates were statistically significant.

Most campaigns were directed towards one single theme. The two largest subgroups in the database were campaigns against drink driving and against speeding. Single-theme campaigns appeared to do better than multi-theme campaigns. Campaigns against drink driving and other single-theme campaigns showed statistically significant reductions on the number of accidents of 14.,4 and 10.4 % respectively. Multi-theme campaigns did not have any effect at all, while campaigns against speeding showed a tendency of reducing the number of accidents (by 8.5 %), but the effect was not statistically significant.

The campaigns were classified into four different types for INFOEFFEKT: media campaigns alone, media campaigns with enforcement, media campaigns with enforcement and education, and local, personally-directed campaigns. Analyses showed that mass media campaigns alone, i.e. without any accompanying measure, did not have any effect at all. The remaining three groups, however, all had statistically significant effects on the number of accidents. This concerned campaigns with police enforcement as accompanying measure, a type of campaign which may be labelled "publicized enforcement", and campaigns using education as an element in addition to police enforcement. There was, however, no significant difference between the effects of these two types of campaigns, they were both in the range of 13-14 %. The fourth and last type of campaign, i.e. local, personally directed campaigns was the one with the largest effect of all campaigns types, with an effect of nearly 40%. This campaign type was, however, based on rather few results and the confidence interval was also quite large (Table D3, Appendix D).

5.1.2 INFOEFFEKT 2004: Results from meta-regression

A multivariate predictor model specifying campaign characteristics and their contribution to the reduction of the number of accidents was also elaborated and tested as part of INFOEFFEKT. The project discussed several theories, which could have a potential in explaining why some campaigns might reduce the number of road accidents. The main purpose of applying multivariate models was to describe factors and assess partial effects by meta-regression, which may contribute to explaining why campaigns might have an accident-reducing effect. The characteristics (predictors) included in the regression model were:

- Country,
- Theme of campaign
- Decade published
- Type of campaign, including use of accompanying measure(s) (police enforcement is one example)
- Size of target group
- Duration of campaign
- Strategy
- Theoretical framework
- (mass) media used (television, radio, newspaper, posters/billboards, leaflets)
- Personal communication/influence
- Other communication channels
- Feedback "on-the-road" (of campaign theme or of behaviour)
- Spreading of campaign message ("two-step strategy")

Two multivariate models were described, one called the ***full*** model, the other the ***reduced*** model. The full model comprised the following predictors: country, theme, year, type of campaign, size of target group, length of

campaign (4 groups), communication channels used (television, radio, newspapers, posters/billboards, leaflets/brochures), personal influence, other communication channels.

The main results from meta-regression by applying **full** model were the following (see Table D4 in Appendix D for full results):

- Australian and Dutch campaigns both gave statistically significant contributions to an explanation of the overall accident-reducing effect of campaigns. The Australian campaigns were mainly directed towards drink driving and most of the campaigns utilized Random Breath Testing (RBT) as a part of the police enforcement. The Dutch campaigns were all directed towards speeding and speed enforcement was an accompanying measure in all of the campaigns. Warning signs displaying "you are speeding" were posted at the roadside and drivers were informed that speed enforcement might take place in 6 of the 7 Dutch campaigns. Thus, both the Australian and the Dutch campaigns relied heavily on police enforcement.
- Posters/billboards seemed to reduce the effect of campaigns. It is difficult to find a reasonable explanation for this result, but one hypothesis could be that campaigns using posters/billboards might have other, common characteristics that are not comprised by the full model.
- *Personal influence* was the only kind of communication, which contributed significantly to explain why campaigns reduce the number of accidents ($p = 0.0032$). Personal influence was partly defined as two-way communication face-to-face, but two-way communication was not a precondition for that personal influence might have taken place. If, for example, letters are addressed personally to members of a given target group, it could be coded as a source of personal influence. Communications categorized as 'personal influence' were thus information or seminars delivered in person; two-way discussions with a peer, teacher, or safety expert; group discussions; and personally addressed letters. It is worth noting that campaign messages delivered through means of such personal influence probably have a greater likelihood to be processed via the central route than other types of campaign messages.
- For the rest of predictors, there were no statistical significant effects or effect tendencies. Considering all communication channels, none of them seemed to have special advantages in explaining the accident-reducing effects of campaigns, except personal influence.

The **reduced** model comprised the following predictors: country, theme, type of campaign, length of campaign (2 groups: $\leq$ 200 days and > 201 days), mass media communication channels used (television, radio, newspapers), "two-step/multi-step strategy, feedback of information (at the roadside), and personal influence.

The main results from meta-regression by applying the reduced model were the following:

- Again, both Australian and Dutch campaigns contributed significantly in explaining the accident-reducing effect of campaigns. The Australian campaigns were mainly directed towards drink driving and the Dutch campaigns were all directed towards speeding
- A categorisation of accompanying measures into three groups, as 1) campaign alone, 2) campaign + police enforcement + education, and 3) local, individually directed campaign did not give any substantial difference between these groups. This was surprising, but an explanation could be that the effect would already be explained by the variable *country*, i.e. by Australian and Dutch campaigns, which both relied heavily on police enforcement.
- The effect of length of campaigns appeared more clearly when only two groups were considered, i.e. $\leq$ 200 days and $\geq$ 201 days (full model used 4 groups). A duration of $\leq$ 200 days is significantly more efficient ($p = 0.0002$) than a duration of $\geq$ 201 days. It seemed reasonable that there existed an optimal campaign length and, further, that the reason behind an optimum could have something to do with how long it was possible, or optimal, to focus on one single theme.
- Again, as was the case in the full model, personal influence was the only communication channel that resulted in a statistically significant contribution to explain why campaigns may reduce the number of accidents.

Effects of campaigns on other areas of behaviour

The objective of study 2 was to investigate possible effects of campaigns aimed at influencing behaviour on other areas than road user behaviour. The meta-analyses were based on results from 99 studies, which had evaluated effects of information campaigns on behaviour. The majority of these studies comprised effects of campaigns directed towards promotion of health protective/preventive behaviours such as exercise, healthy eating habits, sun protection, HIV/AIDS protective behaviours etc. Studies concerning smoking, alcohol, or drug abuse were excluded from the meta-analyses as these may imply addictive behaviour and physiological processes.
This study had two objectives. The first was to investigate whether such campaigns have any effects at all on behaviour, while the second was to examine whether certain characteristics of campaigns might be more important for obtaining behavioural change than others.

The results from these meta-analyses indicate that information campaigns have an effect on behaviour. This is a significant result in itself. However, the average effect on behaviour across all studies was not very large, although there is no doubt that the effect exists. There were some differences between the campaigns regarding how effective the different campaigns are in obtaining behavioural change. Results from meta-regression analysis showed

that much of these differences in campaign effects could be explained by the following characteristics:

- Large campaigns, which are directed towards a large population, are more effective than campaigns directed towards few people. One possible explanation is that large campaigns possess more and stronger measures and resources than small campaigns. Furthermore, large campaigns might be more professionally conducted than small campaigns.
- Campaigns directed towards a specified target group tend to be the most effective ones, but the effect is not statistically significant. At first glance, this finding seems to contradict the above result, i.e. that large campaigns are more effective than smaller campaigns. However, this is not necessarily a contradiction as several of the campaigns that are directed towards a specified target group also have a considerable size.
- Campaigns lasting more than one year are less effective than campaigns of shorter duration. This could be a consequence of reaching a saturation point regarding the information efforts, among those who are campaigning as well as those in the target population.
- In particular, campaigns using some form of personal influence seem to be more effective than others. Campaigns using mass communication in combination with personal influence are also more effective than campaigns using mass communication only.
- The results also suggest that the use of video and letters may be beneficial in order to obtain behavioural change.

Several of the results are consistent with the results from study 1, especially regarding the use of personal influence, the length of campaigns, and the fact that several of predictors in the regression models do not seem to have any effect on behaviour.

Cost-effectiveness and benefit-cost analyses

Study 3 attempted to estimate both cost-effectiveness and benefit-cost relationships regarding six campaigns conducted by the Swedish Road Administration. These economic analyses are primarily based on the information given in the evaluation studies, but also on additional information from the Road Administration and some calculations done by the author.
In order to do benefit-cost and cost-effectiveness analyses, one needs the same type of information as in the case of meta-analysis (and meta-regression), i.e., primarily scientifically based measurements of the effects of the campaign. In addition, complete cost figures for the campaign are required, including valuation of the time and the equipment that is used (where values are based on opportunity cost approaches, e.g., applying wage rates or market prices). Valuation of environmental and traffic safety gains, which would be outcomes of the campaigns, must likewise be provided.

For all six campaigns benefit-cost ratios were estimated. For the traffic safety campaigns cost-effectiveness were also estimated – a comparing overview based on estimated number of saved lives per million Euro (ca SEK 9,16 mill). Table 5.1 presents the results from the economic analyses:

Table 5.1. Rate of cost-effectiveness and benefit-cost ratios according to campaign and campaign type. Results from economic analyses.

Campaign type	Campaign	Rate of cost-effectiveness	Benefit-cost ratios
Safety	*Lite lugnare tempo (Speed cameras)*	0.386	3.14
	Alkolås i min bil! (Alcolock in my car)	0.211	6.13
	ISA – Intelligent Speed Adaptation (assumed 20% reduction in accidents)[17]	0.059	0.29
	ISA – Intelligent Speed Adaptation (assumed 5% reduction in accidents)	0.015	0.07
	Säkereken (The safe oak)	-0.224	17.3
Other	*Sparsam körning – SPARK (Thrifty driving)*		0.22
	Zkona Zebrafolket (Protect the zebra people)		(negative)

Source: TØI report 727/2004

Comment: Regarding ”Alkolås i min bil” the cost per saved life is estimated to € 4,6 mill. Assuming 40 lived years as an average in one life saved, this will give approximately €116.000 per saved life year. This is less cost-effective than the studies reported in BASt (2003).

According to the result depicted in Table 5.1, only the campaign “*Lite lugnare tempo”* and “*Alkolås i min bil*”can be regarded as unambiguously effective, both in purely economical terms and in terms of “Vision Zero”[18]. “*Säkereken”* could also be regarded as economically effective, but in this case there is no documentation of lives saved compared to the rest of Sweden. Regarding the other campaigns the estimated benefit-cost ratios are well below 1 – i.e. the benefits < the costs

It is emphasised that the basic data from all six campaigns in fact are too incomplete or too unclear for performing economic analysis. In all of the analyses we have applied specific assumptions in order to come close to economic appraisals. We have also emphasised that the ISA-project potentially could be profitable if the costs of equipment per car could be somewhat reduced, and it is also probable that the project as a whole would be effective in terms of safety via reduced (top) speed and reduced levels of injury. Both of these effects are important for Vision Zero as well for the economic analysis.

It looks like SPARK is defeated in terms of an economically appraised effectiveness. This project is relatively expensive, per participating driver, while the effectiveness per car, and the value of this effectiveness, is apparently very limited. It seems that the valuation of CO_2-reduction has to be increased considerably for such a project to yield benefits larger than costs. One should consider other approaches if one wants to inspire car owners to

[17] ISA comprised an information campaign alongside several demonstration projects

[18] In short, the “Vision Zero” is a vision where none is killed or permanently impaired in road traffic accidents. The “Vision Zero” was stated and launched by the Swedish Public Roads Administration in 1996.

drive in a manner that reduces effluents and CO_2. In combination with a less expensive dissemination of such knowledge to drivers, increased fuel prices would probably provide the stronger incentive to push for a manner of driving implying less fuel consumption and, hence, less emission of CO_2.
Apparently "*Zkona Zebra-folket"* results in a net loss of time value, i.e., given the values we have applied.[19] However, this project is probably the one which is least suitable for economic analysis, since the issue addressed actually concerns the transfer of rights from one road user group – drivers – to another road user group – pedestrians.

[19] The issue here was that drivers were obliged to yield for pedestrians at pedestrian crossings. The increase in waiting time for drivers was estimated to 1,2 million hours, while the decrease in waiting time for pedestrians was estimated to 1,3 million hours.

6. CAST STRATEGY FOR ASSESSING CAMPAIGN EFFECTS

The remainder of this report describes how a sample of campaign evaluation studies was collected and analysed as part of the CAST project in order to (i) assess the overall effects of road safety campaigns and (ii) identify factors that influence those effects. This was achieved using meta-analysis to estimate the size and variation of the overall effects of campaigns, and to inform subsequent meta-regression analysis, the aim of which was to identify factors that vary with campaign effect in order to suggest important determinants of campaign success. Relevant chapters are structured as follows:

6 CAST strategy for assessing campaign effects
Describes strategy used and reasons for its choice. The processes involved in the systematic review of road safety campaigns using meta-analysis and meta-regression are outlined.

7 Effects of campaigns: analysis of the CAST database

7.1 Study retrieval and processing	Gives criteria and process for selecting (i) the road safety campaign evaluation studies themselves; and (ii) those variables describing the campaigns evaluated.
7.2 Sample description	The sample of effects and variables comprising the CAST database are described.
7.3 Meta-analysis example	Gives an example of how a best estimate of an overall campaign effect is derived from the CAST database. Serves to illustrate the theoretical explanations given in chapter 6.
7.4 Overall effect of campaigns	Summarizes overall effects of road safety campaigns on accidents, behaviours (seatbelt use, drink-driving, speeding), beliefs, knowledge and recall.
7.5 Factors associated with variation in campaign effect	Simple bivariate analyses of the overall effect of campaigns on (i) accidents and (ii) seatbelt use according to various aspects of campaign delivery, content and evaluation.

8 Model of factors influencing campaign effect

8.1 Meta-regression	How and why meta-regression is used to propose sets of factors influencing campaign effects. Outlines important theoretical limitations to be considered.
8.2 Factors associated with effectiveness	Factors associated with the size and direction of campaign effects on (i) accidents and (ii) seatbelt use

9 Conclusions from analysis of the CAST database

6.1 META-ANALYSIS

The CAST database was developed to (i) estimate the overall effects of road safety campaigns, and (ii) identify factors associated with these effects. The

database contains a set of individual campaign effects derived from studies that have evaluated road safety campaigns, where each effect is the change in an outcome measure (such as accident numbers or percentage wearing seatbelts) resulting from a campaign. For each effect, the database also contains data for a set of associated variables that describe the way the evaluated campaign was delivered, the nature of its content, and certain background factors such as the country it was carried out in or the year of its evaluation. Overall effects and associated factors were identified using a set of statistical techniques that together form the procedure known as meta-analysis[20].

Recent developments in the field of meta-analysis have led to recommendations that the procedure be supplemented with certain statistical methods to improve validity. The new methods account for potential biases and non-random variation in the dataset summarized. Since they did not employ such methods, those meta-analyses carried out under the EU-funded GADGET project are now considered to be relatively basic (Delhomme et al., 1999). At the time of GADGET less was known about the conditions to be met for valid application of meta-analyses, about weaknesses in the method, about tests that could identify biases in the data, or about the extent to which overall best estimates could be generalised. New knowledge in each of these areas means that the meta-analysis procedure can be supplemented with formal steps that improve the robustness of the results. Such a supplemented series of steps is employed by the CAST project, which thus represents a substantial development of the GADGET project. The modified meta-analysis process is as follows:

1. Carry out meta-analysis on the assembled set of effects using the log-odds method, in which odds ratios of effects are transformed to their natural logarithms (log-odds).
2. Test the set of effects (the set of natural logarithms of the effects) for publication bias and compensate if necessary using the trim-and-fill method to simulate any 'unpublished' effects missing from the dataset.
3. Test the set of effects for homogeneity, both before and after adjusting for publication bias; use the result of this test to decide whether to use a fixed- or random-effects model to describe the overall effect for a set of effects.
4. Assess the output of steps 2 and 3 to make a conservative best estimate of overall effect.
5. Divide the whole set of effects for a single outcome measure into sub-groups, according to variables defining aspects of campaign evaluation, campaign delivery or campaign content. Obtain overall effects for each sub-group.
6. Use differences between overall effects for different sub-groups, together with known theory and results from past analyses, to develop

[20] Here it is considered that meta-regression, which we also use, is part of the meta-analysis process.

hypotheses about the causes of systematic variation in campaign effect.

7. Test hypotheses by using them to inform a model for meta-regression analysis, and thereby accept or reject ideas about the partial effects of different campaign elements.

These new steps were followed and refined during the recent INFOEFFEKT project, which estimated the effect of campaigns on accident counts (Vaa et al, 2004). The criteria employed for study selection under INFOEFFEKT were narrow, such that the number of campaign effects selected for the final set summarized was low. CAST represents the first use of the refined method on a substantially broader dataset.

In order to fully understand the meaning of the results of meta-analysis, it is important to appreciate the process used to generate them. A more detailed description of the above steps is therefore given below. The process is also exemplified in detail using an effect taken from the CAST database in section 7.3.

Step 1. The log-odds method

Odds ratios are convenient ways of expressing effects in terms of before-and-after or before-and-during measures (see 7.3 for a fuller explanation). There are several reasons to transform odd ratios to their natural logarithms, but in simple terms such transformation helps address a statistical demand, that the set of effects should be normally distributed.

Step 2. Publication bias

Publication bias describes a tendency for authors and editors to publish only those studies demonstrating desirable, statistically significant effects. To put it another way, it is a tendency to avoid publishing those studies that fail to demonstrate desirable effect. There is documented evidence for publication bias (Light & Pillemer, 1984), and it means that any set of effects gathered from accessing a selection of available studies will be incomplete in that it will lack those undesirable or non-significant effects that were never published. The problem is that the true overall effect cannot be derived from sample of effects that does not fairly represent the whole set of effects. Fortunately, statistical tests are now available that can be used to detect publication bias for a sample of effects. Such tests are used on the set of effects collected for CAST.

Several methods have been developed to correct the overall effect of a set of effects for which publication bias has been indicated. The 'trim-and-fill' method proposed by Duval and Tweedie (2000a & b) has been developed to test and, where such bias is indicated, adjust the overall effect estimate by generating the missing, 'undesired' effects to complete the original set of effects. A new, corrected overall effect estimate is then calculated, based on the new completed set of effects. This is exemplified in section 7.3.

Step 3: Fixed- and random-effects: homogeneity vs heterogeneity.

One must decide whether to use a so-called fixed-effects or random-effects model when carrying out meta-analysis. A fixed-effects model assumes that the same intervention (in our case a road safety campaign) will have the same

effect regardless of whether it is carried out on different populations, at different times or in different countries. That is, it is assumed that the characteristics of the intervention are such that it will have the same effect in different contexts. Any variation that is observed will just be due to chance. A random-effects model, on the other hand, assumes that characteristics of the situation, as well as the intervention itself, can cause variations in effect, as indeed can characteristics of the method used to measure the effect.

Fortunately again, a statistical test can be used to inform the decision about which model will give the best estimate of overall effect for a set of effects. The test assesses whether there is significant heterogeneity among the set of effects (Everitt, 2002). Non-significant heterogeneity implies homogeneity, in which case a fixed-effects model is therefore used to describe the overall effect. Where the level of heterogeneity is significant, a random-effects model is used, and an estimator is introduced to account for possible unknown sources of systematic variation between individual effects.

The true overall effect of a heterogeneous set of effects can be inaccurately described using a fixed-effects model, whereas the true overall effect of a homogeneous set of effects will be estimated quite accurately by both fixed- and random-effects models. However, because the random-effects model reports larger confidence intervals, reflecting the greater uncertainty assumed, it is preferable to use a fixed-effects model on a homogenous dataset.

Step 4. Conservative best estimate of overall effect.

Using the preceding steps it is possible to generate estimates of overall effect in four different ways:

(1) using a fixed-effects model without adjusting for publication bias;

(2) using a fixed-effects model and adjusting for publication bias;

(3) using a random-effects model without adjusting for publication bias; or

(4) using a random-effects model and adjusting for publication bias.

The statistical output on the level of publication bias and heterogeneity in the dataset is assessed together in order to select the *best estimate* for an intervention's overall effect. In this way the statistical tests help answer questions of generalizability. They address to what extent, based on the set of effects identified, one can expect to achieve an overall effect in different situations, and therefore what level of confidence should be employed to describe the overall effect. They essentially allow us to make more useful predictions about future situations.

The following thus summarizes the strategy followed in the analysis and presentation of results:

(1) always use the random-effects model where heterogeneity is indicated;

(2) where there is heterogeneity include an estimator to account for any systematic variation in each set of effects; and

(3) calculate overall effects for corrected sets of effects where publication bias is evidenced.

This is a cautious strategy that will tend to give effect sizes associated with higher levels of uncertainty (confidence intervals). It will also, however, minimise the risk of overestimating the size of or confidence associated with campaign effects.

Step 5 & 6. Model development and choice of explanatory variables.

In the CAST project we wanted to identify those factors underlying any statistically significant overall effects that a campaign intervention is shown to have. This involves taking the meta-analysis a step further by employing meta-regression.

Meta-regression is an expansion of meta-analysis in which one models the relationships between an intervention's effect and known explanatory variables using regression (Everitt, 2002). It is preferable that meta-regression is carried out on a model, which is effectively a set of potential explanatory factors. To help identify these factors, a set of campaign effects is divided into sub-groups of effects according to simple variables that define aspects of campaign evaluation, campaign delivery and campaign content. Meta-analysis is carried out to obtain overall effects for each of these sub-groups. The differences between overall effects of different sub-groups are then used to develop hypotheses about the causes of systematic variation in campaign effect.

Without subsequent meta-regression analyses, sub-group analysis itself is quite a crude way to identify factors influencing the effect of campaigns. This is because it is simply a set of bivariate tests in which associations existing between only one variable (i.e. size of selected influencing variable) and another (i.e. size of the campaign effect) are assessed. In reality, however, several factors or variables interact to influence campaign effects. Because sub-group analysis does not account for associations between campaign effect and a selected variable *in the presence of other influencing variables on which it may be dependent*, it is only a crude approximation of the influences of variables that exist in reality. To be able to assess systems of variables together we need to use meta-regression.

Sub-analyses are useful, then, in identifying candidate variables to be refined by meta-regression. In addition, since there are less demands placed on the variables that can be analysed by bivariate analysis, sub-analysis is also useful where we wish to learn about variables which can not be entered into meta-regression.

Step 7 – Meta-regression and testing hypotheses.

The final step consists of multivariate regression analysis based on the model developed in steps 5 & 6. As stated, the advantage with multivariate analysis, in which several variables can be tested together, is that it can shed light on the effects of a variable of interest in the presence of other controlling variables in the model. This is more representative of real-world situations. Although meta-regression can be an informative method, it is important to attend to its limitations and assumptions when interpreting its findings. These

are therefore outlined in section 8.1, just before the results from meta-regression are presented.

6.2 GROUPING OF EFFECTS

Assuming that a set of effects have been identified, it will not always be desirable to analyse them using the steps above. For instance, some consider it nonsensical to group together different outcome measures for meta-analysis. We have not done this in CAST[21]. Further, there are minimal requirements placed on the size of any set of effects destined for meta-analysis. To meet these requirements, we have subjected to meta-analysis only groups of at least five individual effects, and accounted for publication bias only where there are at least 15 effects.

Due to the need to gather large numbers of effects to enable sub-group analysis and subsequent meta-regression, we also took a decision prior to data collection to lump together data from controlled and non-controlled studies, and check afterwards for systematic differences between the two types of effects. This was necessary in order to build a dataset large enough for analysis. On this basis we also grouped together self-reported with observed results, and grouped together different types of accidents in a single, general accident measure. To avoid confusion that this might have caused, we attempted to clearly delineate the effects described at every step, and, where possible, tried to show the statistical differences between any subgroups of data that some readers might think ought not to be grouped together.

6.3 SUMMARY

In this chapter we have described a strategy for the extraction and treatment of data obtained from a sample of campaign evaluation studies. The data describe either the **campaign effect** according to a given measure, or **associated factors** that might influence the size of this effect. Campaign effects are effectively changes in outcome measures resulting from campaigns (e.g. changes in accident numbers or percentage wearing seatbelts). Associated factors are those describing the way in which the campaign was evaluated, delivered or designed (i.e. its content). To estimate overall effects of campaigns we use a conservative meta-analysis process, supplemented with several statistical tests and adjustments in an attempt to improve validity. In order to generate a large enough effect set, we have made what some may see as controversial decisions in grouping of effects for analysis, but have attempted to be clear about what we have done when describing the results.

[21] For example, we do not look at the effects of campaigns on drink-driving behaviour and seatbelt use together because we consider that the two outcome measures have different influences.

7. EFFECTS OF CAMPAIGNS FROM ANALYSIS OF CAST DATABASE

The CAST database contains data describing a sample of campaign evaluation studies. This chapter describes how the studies were retrieved (section 7.1) and gives a descriptive analysis of the sample of studies (section 7.2). An example is given to show how the sample was 'meta-analysed' (section 7.3) before the overall effects generated by meta-analysis are summarised (section 7.4). Bivariate analyses of factors potentially associated with variation in the overall effect of campaigns are then given (section 7.5).

7.1 STUDY RETRIEVAL AND PROCESSING

STUDY RETRIEVAL

Studies describing the evaluation of a road safety campaign carried out over the last 40 years were considered. The definition of a road safety campaign used was quite broad, as described earlier in this deliverable. Each study was scanned for any reported campaign effects (i.e. the change in an outcome measure taken either before-and-during or before-and-after the campaign). A surprising number of studies only reported measures taken during or after the campaign. These were omitted.

The outcome measure of ultimate interest to CAST is accident counts, but we were also interested in the effects of campaigns on other road user behaviour, such as the percentage of car occupants wearing seatbelts or drivers speeding.

A total of 228 studies containing suitable campaign effects were identified. Of these studies, 44 were generated by the systematic selection and summary of road safety campaigns by CAST partners. This was achieved by inviting partner representatives to fill in a form for each campaign evaluation they were aware of from their respective home countries. The form was designed to capture each of the database variables (see 7.1.2). A further 144 studies were included as the result of re-assessment of road safety campaign evaluations used in previous meta-analyses (Delhomme et al, 1999; Vaa et al, 2004; Elliott, 1993; and Elvik and Vaa, 2004). A literature search for more recent road safety campaign evaluations, performed using accessible and relevant databases, resulted in the retrieval of a further 40 studies.

Of these 221 studies, 182 contained information that could be used in the final meta-analysis.[22].

[22] For example, speed measures reported simply as kph or mph in the absence of standard deviations or number of people surveyed could not be used. Studies reporting percentages without n numbers could not be used. Other studies contained unusual outcome measures of which there were not a sufficient number for meta-analysis.

Table 7.1. Categorisation and coding of variables used in the CAST database.

Category	Variable	Coded as…
reporting and evaluation	study author	string variable
	study date	continuous variable (year)
	study title	string variable
	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
	outcome measure	1. No. of accidents/injuries, 2. Speeding, 3. Seat-belt use, 4. Drink-driving, 5. Attitudes, 6. Risk perception, 7. Knowledge, 8. Other, 9. Recall of campaign message, 10. Child reflectors, 11. Driver behaviour towards other road user, 12. Booster cushion use, 13. Red light running, 14. Estimation that others speed, 15. Intervention behaviour, 16. Intention to behave, 17. Conservation behaviour
	time measure recorded	0. Before-during, 1. Before-after, 2. Before-during-after
	use of control group	0. No, 1. Yes
theme and country	campaign theme	1. General / multiple, 2. Speeding, 3. Drink-driving, 4.Seatbelt use adults, 5. Child restraints, 6. Helmet use, 7. Mobile phone use, 8. Fatigue, 9. Aggressive driving, 10. Distance to car in front, 11. Driver consideration towards other road users, 12. Visibility, 13. Awareness of collision risk, 14. Children in traffic, 15. Safe commuting, 16. Interaction between commercial and other road users to improve safety, 17. Improve knowledge of elderly as vulnerable road users, 18. Decrease number of car journeys, 19. Awareness of limitations of driving skills, 20. Red-light-running, 21. Safe distance, 22. Lane discipline, 23. Careless driving, 24. Truck drivers who take amphetamines, 25. Driver Inattention, 26. Pedestrian safety, 27. Speaking out about dangerous driving, 28. Mobile phone while driving, 29. Save petrol
	campaign 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
scope	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 group, 3. More than one group
	size of target group[23]	continuous variable (number of people)
	size of catchment area[21]	continuous variable (number of people)
	campaign duration	continuous variable (number of days)
delivery	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

[23] Not enough studies reported this variable for use in analysis

Category	Variable	Coded as…
	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
	use of several smaller mesures	0. No, 1. Yes
	personal communicatio n	0. No, 1. Yes
content	basis on prior consultation	0. No, 1. Yes
	basis on theory	0. No, 1. Yes
	kind of theory	1. Theory of Planned Behaviour (TPB), 2. Theory of Reasoned Action (TRA), 3. Social Learning theory (Bandura), 4.Peer-eduation method, 5. Behavioural theory, 6. Public information and education model, 7. Public Information & Enforcement and Selective Traffic Enforcement Program (STEP), 8. Self-responsibility, 9. Risk salience link to behaviour, 10. Based on previous work, 11. Organizational Behavior theory, 12 Social marketing
	basis on prior work	0. No, 1. Yes
	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	0. No, 1. Yes
	humour	0. No, 1. Yes
	social norm	0. No, 1. Yes
	emotional, rational, incentive	0. Emotional, 1. Rational, 2. Both emotional and rational, 3. Incentive

DATA EXTRACTION

Relevant data were systematically extracted from each study using a spreadsheet. The spreadsheet was designed to capture and structure variables describing the evaluation study, the delivery of the campaign, the content of the campaign, and of course the effects of campaign on the outcome measure. The spreadsheet was elaborated as the data was collected in order to capture any aspect of campaign delivery or content that might influence campaign effectiveness.

The variables used to structure the spreadsheet are given in Table 7.1, where they are also grouped into categories.

To attain the large number of results necessary for meta-regression, effects were included whether or not data for corresponding control groups were reported.

The data was coded by a single researcher i.e. there were no checks for inter-rater reliability.

PROCESSING OF STUDY DATA

For each campaign effect, data describing the campaign and its evaluation were entered into the database. Meta-analysis was used to test whether the effects reported by controlled and non-controlled designs differed significantly for each of the outcome measures. If no such difference was identified, all effects reported for the outcome measure were included in subsequent analyses. Meta-analysis was performed to determine the overall effect of campaigns according to a specific outcome measure and to inform a meta-regression analysis. This process is outlined in Chapter 6 and exemplified in some detail in section 7.3.

7.2 SAMPLE DESCRIPTION

The sample of effects entered into the CAST database is meant to be representative of all safety campaign evaluations carried out in recent times in the Western world. To enable the reader to make a judgement about this, various characteristics of the sample are described. These should be carefully considered when assessing the general applicability of conclusions from meta-analysis of this sample. Sample description is based on the variables collected during data extraction, which are detailed and categorized in Table 7.1.

REPORTING AND EVALUATION

The 182 studies passing selection criteria contained 437 different campaign effects judged as suitable for entry into the CAST database. Different types of outcome measure included in the final sample are given in Table 7.2.

Table 7.2. Different types of outcome measure reported by the 182 studies identified

outcome measure	campaign effects	observed	self-reported
seatbelt use	133[24]	119	14
accidents	119[25]	118	1
attitude	39	0	39
speeding	28	21	7
risk perception	24	0	24
drink-driving	23	4	19
recall	23	0	23
knowledge	17	0	17

[24] Includes 14 effects for child restraint use
[25] The GADGET and INFOEFFEKT projects summarized a total of 72 and 86 effects of road safety campaigns on accidents, respectively.

driver behaviour towards pedestrian	13	11	2
cycle helmet use	4	4	0
intervention behaviour	4	0	4
intention to behave	4	0	4
red light running	2	2	0
reflector use	1	1	0
booster cushion use	1	0	1
estimation that others speed	1	0	1
conservation behaviour	1	0	1
total	437	280	157

Table 7.2 also shows for each group of effects, whether outcome measures were evaluated by others (i.e. observed) or self-reported by the target group. The outcome measures 'accidents', 'seatbelt use', 'speeding' and 'driver behaviour towards pedestrians' were mostly observed, and are therefore considered more reliable. Drink-driving behaviour, attitudes, risk appreciation, knowledge, intentions and recall were mostly self-reported. While there was a sufficient number of effects in most groups for meta-analysis based on outcome measure, the only outcome measures for which there were enough effects to perform sub-group meta-analyses based on the variables in Table 7.1 were 'accidents' and 'seatbelt-use'.

The general outcome measure 'accidents' comprises mostly three main measures: 'all types' of accidents (38 effects), 'personal injury' accidents (39 effects) and 'drink-driving' accidents (32 effects). Drink-driving accidents could be based either on the number of accidents during drinking hours, relative to those during non-drinking hours, or more directly on an involved driver being over the drink-drive limit.

Of the 437 effects in the sample, 152 were based on changes in outcome measure taken before-and-during a road safety campaign, while 220 were based on before-and-after measurements. The remaining 65 were based on measures taken before-during-*and*-after a campaign intervention. In the latter case a weight-adjusted average of the during and after measurements was taken in order to keep the analyses simple while at the same time maintain power.

There were corresponding control effects for 192 out of the 437 effects in the sample. More than half of the studies used in the sample thus did not employ control groups.

As shown in Table 7.3, most of the effects derive from studies published during the last 30 years. Only 13 out of 433 campaign effects pre-date 1980.

Table 7.3. Number of effects according to decade of study publication

decade	campaign effects
1960s	4
1970s	9
1980s	213
1990s	137
2000s	74
total	437

The studies reporting effects were published via one of two main publication channels: refereed scientific articles (161 effects); or research institute reports (156 effects) Other publication sources are detailed in Table 7.4.

Table 7.4. Number of effects according to publication source

source	effects
refereed scientific article	161
institute report	160
unpublished	34
government report	33
conference paper	24
not specified	9
company report	7
popular journal article	3
safety association report	3
dissertation	1
EU report	1
trade journal article	1
total	437

THEME AND COUNTRY

Effects were reported for 20 different types of road safety campaign theme (Table 7.5). The most common themes were seat-belts, drink-driving, speeding, and general road safety. The latter encompassed multiple road safety themes.

Table 7.5. Number of effects extracted according to campaign theme

campaign theme	effects
seat-belt	149
drink-driving	105
speeding	69
general / multiple	22
child restraints	14
children in traffic	13
yielding to pedestrians	12
interaction with commercial road users	8
awareness of elderly road users	8
speaking-out about unsafe driving	5
helmet use	4
collision risk appreciation	4
safe distance to car in front	3
driver inattention	3
mobile phone use	2
fatigue	2
visibility	1
red-light running	1
lane discipline	1
unknown	11
total	437

In some cases the same outcome measure is used to evaluate different types of road safety campaign. Table 7.6 shows the different types of campaign in our sample, according to the outcome measure used to assess them.

Table 7.6. Number of effects extracted for each campaign theme according to outcome measure

outcome measure	campaign theme					
	seat-belt	drink-drive	speed	general / multiple	other	unknown
seatbelt use	118	2	-	-	12	1
accidents	5	41	29	9	25	10
attitude	6	10	7	3	12	-
speeding	-	-	21	6	1	-
risk comprehension	8	9	4	2	1	-
drink-driving	1	22	-	-	-	-
recall	5	11	3	-	4	-
knowledge	3	2	3	2	6	-
behaviour towards pedestrian	-	-	1	-	12	-
other	3	8	1	-	7	-
Total (437)	149	105	69	22	81	11

There are 175 effects from campaigns carried out in the USA, 172 from Europe, and 87 from Australasia or Japan. A breakdown by individual country is given in Table 7.7.

Table 7.7. Number of effects derived from each country carrying out road safety campaigns

country	effects
USA	175
Australia	82
Sweden	70
Canada	23
Norway	19
Netherlands	18
Denmark	13
Austria	9
Czech Republic	7
UK	7
Germany	5
New Zealand	4
multinational	3
Portugal	1
Japan	1
total	437

A tendency for particular countries to focus on certain types of campaigns was noted. Most notably, many of the effects of seatbelt campaigns in the sample derive from the USA (104 out of 149 seatbelt-themed campaign effects), while almost half of the drink-driving campaign effects are from Australia (45 out of 105 drink-drive-themed campaign effects).

SCOPE

The campaign associated with each effect was categorized according to its delivery at a local, regional or national level. A local campaign was any campaign carried out on a population in an area up to and including a single town or city, while a regional campaign was one encompassing one or more counties or federal states. The number of effects derived from campaigns of different scale is given in Table 7.8.

Table 7.8. Number of effects according to scale of associated campaign.

scale	effects
local	180
regional	168
national	67
mixed	12
missing	10
total	437

Table 7.9. Number of campaign effects according to numbers of road users targeted

number of people in target group (or catchment area)	effects
0-100	10
100-1,000	21
1,000-10,000	28
10,000-100,000	19
100,000-1,000,000	30
> 1,000,000	13
Missing	316
total	437

Most of the effects (271) were those of campaigns targeting a single, specified group of road users. 50 effects were reported for campaigns targeting more than one type of target group, while 103 effects were from campaigns that did not specify a target group.

The campaigns in the sample lasted from a single day to several years (Table 7.10). For all themes the most frequent campaign length was between 12 and 52 weeks, apart from seat-belt-themed campaigns, which most frequently lasted from one to four weeks.

Table 7.10. Number of effects according to campaign theme and length

	theme					
duration	general	speed	drink-drive	seat-belt	other	total
0 - 1 week	2	3	0	12	2	19
1 - 4 weeks	2	15	12	65	7	101
4 – 12 weeks	3	10	27	48	9	100
12 - 52 weeks	4	31	38	31	27	131
> 52 weeks	5	6	22	4	18	55
missing						31
					total	437

DELIVERY

There were 84 effects derived from campaigns that paid for the publicity needed to deliver their message, 101 for campaigns that earned it (through press releases, lobbying or publicity stunts) and 79 for campaigns that both paid for and earned their publicity.

Road safety campaigns are often delivered with increased police enforcement, which has been reported to enhance campaign effects (e.g. Delhomme et al, 1999). We therefore noted whether campaigns were accompanied by any intensified enforcement measures. Of the 433 effects reported here, 202 were for campaigns accompanied by increased enforcement. 61 out of 149 seat-belt-themed campaign effects were enforced, 34 out of 105 drink-drive campaigns enforced, and 32 out of 69 speed

campaigns enforced. We included both primary and secondary enforcement measures[26].

The delivery methods used by campaigns to convey their message are ranked in Table 7.11, according to frequency of associated campaign effects. Television, newspapers, radio, leaflets and posters are the most commonly used delivery channels in this sample.

Types of communications categorized as 'personal' were information or seminars delivered in person; two-way discussions with a peer, teacher, or safety expert; group discussions; and personally addressed letters.

The most common forms of leaflet distribution were also noted, and are shown in Table 7.12.

For the sake of analyses in this study, mass-media campaign effects are defined as those deriving from campaigns that use at least one of the three methods (television, radio and newspaper) to deliver their message. There are 314 such effects in this sample. A total of 203 effects are derived from campaigns that use all three methods.

Table 7.11. Number of effects according to method of delivering campaign message. Any one campaign often uses more than one of these methods.

method of delivery	Effects
television	285
newspapers	273
radio	232
leaflets	220
posters	205
personal	130
billboards	70
reward	51
numerous minor elements*	49
video / DVD	36
cinema	32
variable message sign (feedack)	27
website	23
fixed message sign	17
competitions	14
pledge cards	12

*describes a campaign using several minor channels for message delivery, such as stickers or streamers and, isolated information displays or 'freebies'.

[26] Primary enforcement describes situations where the primary reason for the police to stop drivers is to check and enforce a driver behaviour. Secondary enforcement describes checks and controls carried out only after the driver is stopped for another, primary reason.

Table 7.12. Number of campaign effects according to method of leaflet distribution

leaflet distribution…	effects
at school	22
through post	19
by hand to assembled target group	18
through car window	17
in a public place	11
at work	8
at police checkpoints	4
at exhibition or event	1

CONTENT

Only 60 of the 437 effects in the sample came from campaigns basing their content on prior consultation with the target group, and only 81 from those basing content on an explicitly stated psycho-social theory or model. A breakdown of the different models used by these campaigns is given in Table 7.13. 200 of the effects were of campaigns that built on previous campaigns. 133 effects were of campaigns whose content had no explicit basis.

The campaign associated with each effect was also characterised in the way it addressed risk. In the case of 224 campaign effects, risk was specifically addressed. Table 7.14 shows the different types of risk addressed by campaigns in the sample.

Table 7.13. Campaign effects according to basis in model or theory

theory / model	effects
public information and education model	26
public information and education model + selective traffic enforcement program	21
behavioural theory	15
social learning theory	5
peer-education method	3
theory of reasoned action	2
risk salience	2
theory of planned behaviour	1
self-responsibility	1

Table 7.14. Number of effects according to different types of risk addressed by campaign (a single campaign can address more than one type of risk)

type of risk addressed	effects
risk of detection	163
risk of harm to self	69
risk of harm to others	60

We also coded whether the associated campaigns showed the consequences of unsafe behaviour on the road. Of the 437 effects, 216 were associated with campaigns that dealt with consequences. Only 22 effects were associated

with studies reporting that the campaign used shocking effects in order to show the consequences. These campaigns might be considered to be 'fear campaigns'. A breakdown of the use of other emotional dimensions in content is shown in Table 7.15. A single campaign can use more than one of these dimensions (e.g. humour and emotional content). However, most campaigns in the sample use rational content to inform and persuade the road user, rather than emotion, humour, shock or incentive.

Table 7.15. Number of effects according to emotional dimension of campaign (a single campaign can use more than one of these dimensions).

content	*effects*
emotional	36
rational	197
emotional and rational	66
incentive	45
humour	42
shock	22

Finally, 93 effects were of campaigns whose content was judged to address social norms.

SUMMARY

The set of campaign effects and associated campaign variables sampled for CAST is described for two main reasons:

(1) to enable judgements about how well the CAST sample represents those campaigns of interest; and

(2) to assist in interpretation of findings from meta-analysis.

About two campaign effects have been extracted from each identified study. Of the 437 effects, over half are derived from evaluation studies not employing a control group. The most common outcome measures reported are percentage seatbelt use and number of accidents, and the most common campaign themes are seatbelts, drink-driving and speeding. Almost all studies in the sample have been published within the last 30 years, most often in peer-reviewed journals or institute reports. Most of the studies describe campaigns carried out in Europe or the USA. The campaigns in the sample target a wide range of population sizes, from less than one hundred to over one million, and their duration ranges from a single day to several years. They use a range of delivery methods, most commonly television, radio, newspapers, leaflets and posters. Several delivery methods are normally combined within a single campaign. Most campaigns do not have a theoretical basis and do not develop their content by consulting the target beforehand. The most common basis for campaign content is previous campaigns, but a third of the effects come from campaigns that have no explicit basis. Finally, half of the effects are from campaigns that attempt to raise awareness of risk (risk of being detected or risk of harming self or others), and most are for campaigns that use rational rather than emotional persuasion.

7.3 META-ANALYSIS EXAMPLE

The sample of campaign effects described above was subject to meta-analysis in order to derive knowledge about the overall effects of campaigns on different outcome measures. A meta-analysis of the effect of road safety campaigns on accidents is presented here in detail to exemplify the meta-analysis process outlined in chapter 6. Results reported subsequently were derived using the same process.

Data extraction resulted in a total of 119 effects of road safety campaigns on accidents being entered in the CAST database. Treatment of each of these effects is exemplified using one effect extracted from Diamantopolou (2002).

Step 1. The log odds method

Data describing a campaign effect were extracted from Diamantopolou and entered into the database as shown in Table 7.16. An odds ratio of the effect was calculated using the formula[27,28]:

(accident counts after campaign / accident counts before campaign) /
(accident counts after campaign [control] / accident counts before campaign [control])

Table 7.16. Treatment of effects data exemplified using an effect of a campaign on accident counts taken from Diamantopolou (2002)

Number of accidents before campaign	356
Number of accidents before campaign in control group	350
Number of accidents after campaign	1242
Number of accidents after campaign in control group	1109
Odds ratio	1,1011
Ln odds ratio	0,0963
Weight	135.630
Ln odds ratio x weight	13.057

The effect of a campaign can be easily derived from the odds ratio. By subtracting 1 from the odds ratio, and then multiplying by 100 we obtain the percentage effect. Thus in our example we can say the campaign had an undesirable effect, increasing accidents counts by 10.1% ([1.101 – 1] x 100). Odds ratios also allow the extra reliability of studies with higher sample numbers to be accounted for in the meta-analysis by allocating extra 'weight' to such effects. For example, in Diamantopolou's (2002) evaluation, the counts recorded after the campaign are higher, because the counting period was longer than that used before the campaign. Expressing each effect as an odds ratio allows direct comparison of longer and shorter periods of counts, and the extra reliability of longer counting periods (from greater n size) can therefore be maintained and accounted for in the meta-analysis.

In meta-analysis, then, the odds ratio is treated in two ways: (i) it is transformed to its natural logarithm, and (ii) it assigned a weight. A weight is calculated for each individual effect using the formula[29]:

[27] For all other outcome measures accident count data is substituted by percentage data (Christensen, 2003).
[28] The control denominator is simply omitted for non-controlled effects.

1/(1/[n before campaign] + 1/[n after campaign] + 1/[control n before campaign] + 1/[control n after campaign])

Thus the greater the number of accident counts the effect is based on, the greater the weight assigned to the effect. A final weighted effect is then obtained by simply multiplying the ln odds ratio by the weight.

Once a weighted effect has been obtained for each individual effect in a set of effects, an overall effect for the set is calculated by dividing the sum of the individual weighted effects by the sum of the individual weights, and then inversing the natural logarithm to obtain the overall effect in terms of the original odds ratio, which can then be converted to a percentage effect, as described above.

In this way, we calculated that the overall weighted effect of road safety campaigns on accident counts *according to the effects extracted from the studies retrieved* is a statistically significant reduction of 10%. The result is presented in Table 7.17.

Table 7.17 Overall effect of road safety campaigns on road accidents according to the effects extracted from the studies identified.

Group	effect (%)	95% confidence interval (%)	Significant? (p < 0.05)
All accident effects (n = 119)	- 11	(- 12 -10)	yes

Step 2 & 3. Publication bias

The overall effect reported is dependent on the effects extracted from the studies retrieved. It is a valid concern that these studies do not represent the *true* overall effect. Although we have tried to obtain unpublished studies where possible, most of the studies included in the database are published. The concern is therefore that our overall effects are influenced by publication bias, a tendency for authors to publish only favourable effects and not to publish unfavourable ones. If there is publication bias, the magnitude of the overall effects reported here may be too great.

One way to observe publication bias is to plot the weight of each effect, which as we have seen can be considered as the relative number of participants or counts in that study, against the effect size, as shown in Figure 7.1. It is reasonable to assume that the individual effects in such a plot should be normally distributed about the true effect size, which should be approached with greater proximity by those studies with greater weight. There is said to be

[29] The reader might appreciate the anomaly that where there are no control data, greater weight is assigned. However, an adjustment is made for this by doubling the weight denominators for non-controlled effects.

publication bias where this normal distribution is skewed in the direction of 'favourable' effect.

Figure 7.1 indicates that there is indeed publication bias for the set of effects based on accident counts included in the CAST database (bias can be seen in normal distribution of the clear squares). However, it is possible to correct for this bias using a statistical method called trim-and-fill. This method attempts to represent the true distribution of effects by adding in the 'missing' or unfavourable data points. A computer algorithm is used to generate two estimates of the number of studies that should be added to the 'depleted' side of the distribution in order to make it normal. The two estimators are called R and L.

R is generated by first modifying the effect set by subtracting the average of all effects from each individual effect. The mathematical sign of the modified effects is then disregarded while they are ranked in order of size. Sign is reallocated to the resulting rank values, in order to find the number of effects along an uninterrupted sequence of one mathematical sign, beginning with the largest effect value going down. The number 1 is subtracted from the number of effects in this uninterrupted sequence to obtain R.

For the accident data in Figure 7.1 it is estimated that there are no missing studies using R. This can be explained in simpler terms by referring to the extreme negative and positive ln effect values in Figure 7.1. Independent of sign, the data point with the highest value is (-)1.31, but the next highest is (+)1.11, and so the number of ranks of one particular sign in the highest valued sequence is 1. Thus according to the R estimator there are no missing studies because 1-1=0.

L estimates not the number of *extreme* values missing on the depleted side of the distribution, but the number of values missing across the depleted side of the effects. To begin with, the effects are again ranked according to size, independent of sign, and the signs of the original effects reattributed to their rank values. The rank values are then divided into two sets, according to sign. Then the following equation is used[30]:

$$L = (4T_n – n[n+1]) / (2n-1)$$

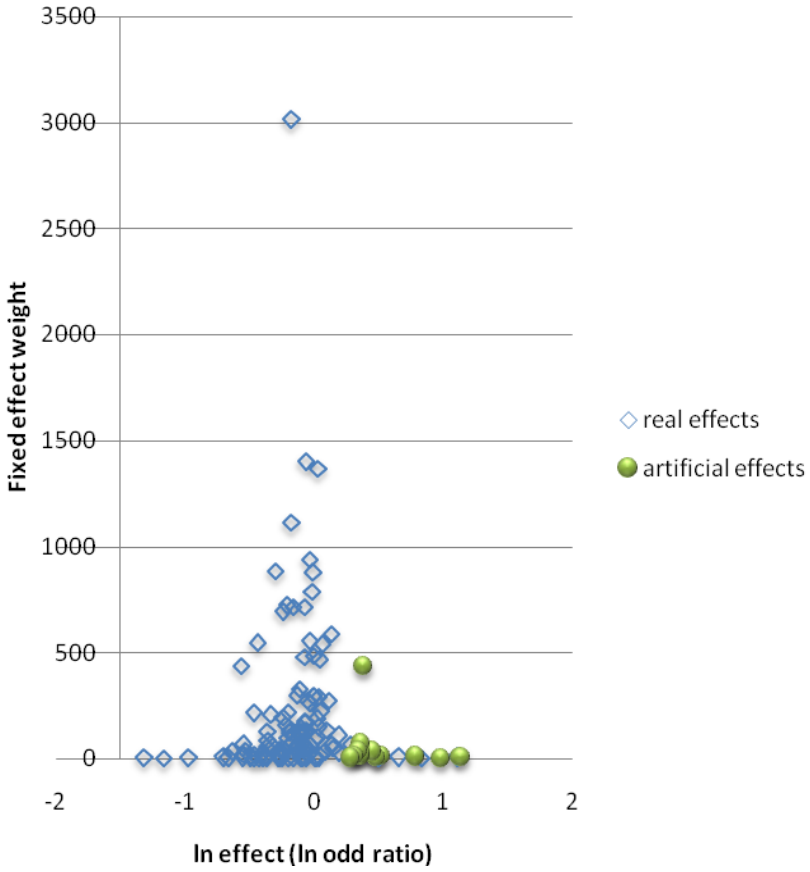
where T_n is the sum of rank values in the largest rank set, and n is the total number of effects in the group being analysed for bias . For the accident data in Figure 7.1, the estimator of missing effects L = 13. We can see by looking at Figure 7.1 that, relative to the left, a number of effects are indeed missing across the right hand side.

Consultation of the two estimators R and L together is recommended by Duval and Tweedie (2000). In our conservative approach, we simply use whichever estimator gives the largest value.

Trim-and-fill generates compensatory effects to fill the distribution by copying the most extreme values on the favoured side and 'pasting' them into the distribution with the opposite mathematical sign. This is exemplified for the accidents count effects in Figure 7.1 (solid circles added).

[30] This is actually used in reiterative rounds of testing of the data set in order to achieve an optimal solution, using a computer algorithm.

Figure 7.1 Correcting for publication bias in effect of road safety campaigns on accident numbers using trim-and-fill



A meta-analysis is then re-run on 'complete' sets of effects to give a more accurate estimate of the overall effect.

There are several points that should be borne in mind when considering data corrected for publication bias:

- An assumption is made that a skewed distribution is caused by publication bias and not by other systematic variation. Other explanations for systematic variation are, however, possible. Smaller studies could actually be more effective. Bias could also be explained by campaigns having a stronger impact on accident fatalities than other types of accidents (fatal accidents relatively infrequent and therefore less weight).
- Sometimes correcting for publication bias simply doesn't make sense. For instance, some seatbelt studies with very low initial use rates report over 30-fold increases in percentage of people using seatbelts. Filling in 'missing' values by generating 30-fold *decreases* in seatbelt use is not sensible.
- Caution should be taken when comparing corrected overall effects generated from unequal sized sets of effects, especially where one group of effects has n < 15. This is because the effects of detecting and

compensating for publication bias are more extreme for smaller groups of effects.

Table 7.18 shows overall effects of road safety campaigns on accident counts or seatbelt usage rates, before and after correcting for publication bias. Although the effects remain significant, they are somewhat reduced because 13 campaigns have been added to compensate for assumed publication bias. The resulting conservative estimate suggests that road safety campaigns reduce accident numbers by 9 % (Table 7.18).

Table 7.18 Overall effect of road safety campaigns on accidents: fixed effects before and after publication bias

Outcome measure	Account for publication bias?	effect (%)	95% confidence interval	significant? (p < 0.05)
Accidents (n = 119)	No	- 11	(- 12 -10)	yes
	Yes, generate 13 effects to compensate	- 9	(- 11, -8)	yes

Step 3 & 4 Fixed- and random-effects: homogeneity and heterogeneity

The derivation of overall effect described in step 1 is based on a fixed-effects model, which accounts only for chance variation in effect, and not any unknown systematic variation. Overall effects derived using a fixed-effects model predict smaller variations than those derived using a random-effects model, because the latter accounts for unknown systematic variations. To decide which model to use we look at the amount of variation among a set of effects. The higher the variation, the greater the chance of systematic causes of variation, and the stronger the random-effects model is recommended.

Variation is examined using a test for homogeneity that outputs a test statistic called Cochrane's Q (Christensen, 2003). Where Q is significantly larger than expected for a homogenous data set, homogeneity is rejected, and a random-effects model employed.

For our set of accident effects, Q is significantly larger than expected for homogenous data set and a random effects model is therefore used to describe the overall effect. The random effects model derives an overall effect of campaigns on accidents of -12%. As shown in Table 7.19, although the apparent effect is greater, we can be considerably less confident about this estimate compared with that generated using a fixed-effect model. Table 7.19 also shows an estimate of overall effect based on a random-effects model after accounting for publication bias. It is this final estimate, - 9 %, that we would present in this report, not least because it is the most conservative in terms of both the effect size and confidence with which we can apply this effect to campaigns of the future. Most of the effects given in this report were generated using a random effects model after accounting for publication bias.

Table 7.19 Four possible estimates of the overall campaign effect on accidents. Each overall effect is based on n individual effects (excluding the artificial effects generated by trim-and-fill).

Outcome measure	Model	Account for publication bias?	effect (%)	95% confidence interval	Significant? (p < 0.05)
accidents (n = 119)	fixed effects	no	- 11	(- 12 -10)	yes
	random effects		- 12	(-15; -9)	yes
	fixed effect	yes, generate 13 effects to compensate	-9	(- 11, -8)	yes
	random effects		-9	(-12;-6)	yes

SUMMARY

An example of the use of meta-analysis to generate en estimate of the overall effects of campaigns on road accidents is given. An effect is a change in accident counts (or percentage behaviour) occurring before and during / after a campaign. Each effect is expressed as an odds ratio from which the natural logarithm (ln) is taken. The ln odds ratio is then weighted according to the number of people or counts observed in the study. Individual weighted ln odds ratios are then summed and the sum divided by the sum of individual weights to give an overall effect. The population of effects is checked for publication bias and homogeneity in order to ensure that the overall effect is not overestimated.

7.4 OVERALL EFFECT OF CAMPAIGNS

The meta-analysis procedure described in section 7.3 was used to derive conservative estimates of overall campaign effects for each of the outcome measures listed in Table 7.2 for which there were at least five individual effects. The results are shown in Table 7.20.

As reported in section 7.3, according to our sample road safety campaigns result in an overall significant reduction in accidents of nine per cent. Campaigns evaluated in the sample also produced favourable overall changes in road user behaviour, according to several different measures (Table 7.20). We calculated a substantial overall increase of 25 per cent in the use of seatbelts, a decrease of 16 per cent and 17 per cent, respectively, in speeding and drink-driving behaviour and an increase in 37 per cent of drivers yielding to pedestrians. Apart from the drink-driving result, each of these results was statistically significant.

There were also favourable overall outcomes on some non-behavioural measures. Not surprisingly, campaigns resulted in an overall substantial, if somewhat variable, increase (120 per cent) in the recall of the campaign, but they also had a significant ability to increase understanding of risk (16 per cent). The effect of road safety campaigns on attitudes was low and non-significant, and the effect on relevant road safety knowledge inconsistent.

The original studies from which these overall effects are derived are given in Appendix B.

Table 7.20. Overall effect of road safety campaigns on different outcome measures, after accounting for publication bias. Each overall effect is based on n individual effects.

Outcome measure	effect (%)	95% confidence interval (%)	Significant? (p < 0.05)
Number of accidents (n = 119)	- 9	(-12, -6)	yes
% using seatbelts (n = 133)	+25	(+18, +31)	yes
% speeding (n = 28)	-16	(- 25, -6)	yes
% drink-driving (n = 23)	- 17	(- 46, +28)	no
% yielding to pedestrians (n = 13)	+37	(+14, +65)	yes
% expressing favourable attitude (n = 39)	+5	(-0, +11)	no
% risk comprehension (n = 24)	+16	(+4, +30)	yes
% correct knowledge (n = 17)	+44	(-10, +132)	no
% recalling campaign (n = 23)	+120	(+30, +273)	yes

7.5 FACTORS ASSOCIATED WITH VARIATION IN CAMPAIGN EFFECT

There was a sufficient number of both accident and seatbelt use effects in the CAST database to enable meta-analysis of sub-groups of each of these outcome measures. Sub-groups were formed based on the variables listed in Table 7.1. For example, the group of 119 campaign effects on accidents were sub-grouped according to campaign theme and a meta-analysis run on each of the sub-groups to see whether certain campaign themes were more effective at reducing accidents than others. Each subgroup required at least five individual effects. The summaries given here have been adjusted for publication bias to give conservative estimates of effect. They should be used to get an idea of the size of effects of campaigns that have a certain property in common (e.g. have a drink-drive theme or use television) while it is remembered that each of the campaigns on which each overall effect is based will also vary according to a number of other factors, which we have not controlled for.

REPORTING AND EVALUATION

In developing a model of factors influencing the effect of campaigns, it is important to account for any variations caused by the different ways in which individual campaign evaluations are carried out and reported. Being able to control for these sources of variation increases our chance of isolating dimensions of campaign scope, content and delivery responsible for campaign effectiveness.

Our consideration of publication bias in chapter 6 suggests that the size and significance of a reported campaign effect might vary depending on the publication medium. Table 7.21 presents the overall effects of campaigns according to whether they were published in refereed journals or institute reports. In this case the results are given before accounting for publication bias.

Table 7.21. Overall effect of safety campaigns according to publication source of study, before accounting for publication bias. Each overall effect is based on n individual effects.

	Accidents				Seatbelt use			
Theme	effect (%)	95% confidence interval (%)	n	Sig.? (p < 0.05)	effect (%)	95% confidence interval (%)	n	Sig.? (p < 0.05)
refereed journal	-12	(-19;-5)	21	yes	+53	(+46;+60)	95	yes
institute report	-10	(-14;-6)	76	yes	+37	(+11;+70)	27	yes

As can be seen, before adjusting for publication bias there is little difference in the effects presented by institute reports and those presented in refereed

journals. The results are presented in Table 7.22 after accounting for publication bias.

Table 7.22. Overall effect of safety campaigns according to publication source of study, after accounting for publication bias. Each overall effect is based on n individual effects (excluding any artificial effects generated by trim-and-fill).

	Accidents				Seatbelt use			
publication route	effect (%)	95% confidence interval (%)	n	Sig.? (p < 0.05)	effect (%)	95% confidence interval (%)	n	Sig.? (p < 0.05)
refereed journal	-8	(-15;0)	21	yes	+22	(+16;+29)	95	yes
institute report	-10	(-14;-6)	76	yes	+11	(-14;+43)	27	no

Publication bias was evident in the case of the 21 effects reported by refereed journals, according to testing by trim-and-fill. No publication bias was detected for the accident effects reported by research institutes.

Bias was also detected for those seatbelt use effects published in refereed journal articles, with the overall effect reduced from a 53 per cent increase in seatbelt use to just 22 per cent after accounting for bias. However, in this case bias was also detected among those effects published in accessed institute reports, with campaigns coinciding with an overall increase in belt use of just 11 per cent after accounting for bias, compared with 37 per cent before. It is not clear why seatbelt effects published in institute reports should be biased. One explanation is that the accessibility of institute reports is influenced by the effect sizes they report. Another is that there is another explanation for skewness. In our conservative approach we simply make an assumption that there is publication bias.

It is conceivable that campaign effectiveness varies over time. For example, the campaigns carried out today might be more effective because they are based on greater knowledge, or there might be professional or technological advances that make television campaigns or posters more effective. In order to test for any change in overall campaign effectiveness over time, the effects were grouped according to the decade in which the studies that reported them were published, and the overall effects in each group presented in Table 7.23.

Table 7.23. Overall effect of road safety campaigns on road accidents and seatbelt use according to decade of publication of study. Publication bias accounted for. Each overall effect is based on n individual effects (excluding any artificial effects generated by trim-and-fill).

	Accidents				Seatbelt use			
decade	effect (%)	95% confidence interval (%)	n	Sig.? (p < 0.05)	effect (%)	95% confidence interval (%)	n	Sig.? (p < 0.05)
1980s	-10	(-16;-2)	34	yes	+52	(+38;+67)	78	yes
1990s	- 8	(-13;-6)	63	yes	+19	(+15;+24)	27	yes
2000s	- 11	(-17;+5)	20	no	+14	(+8;+19)	23	yes

The results suggest a decrease in the effect of campaigns on seatbelt use over time. It is difficult to draw conclusions about the effect of campaigns on accidents over time while the variability in effects increases with fewer number of effects. There are two possible explanations for the seatbelt result. First, it is apparent from the database that a series of effective small-scale seatbelt campaigns were carried out in the 1980s, which may contribute to the apparent reduction in campaign effect over time. Second, Elliot (1993) reported a tendency for a reduced campaign effect for higher levels of pre-campaign belt use. It is reasonable to extrapolate from this that campaign effects would reduce as seatbelt use rates improve over time. A bivariate correlation analysis of seatbelt use effects confirms that percentage seatbelt use before the campaign is strongly and negatively correlated to campaign effect ($r = -0.631$; $p<0.001$). It is clear that initial seatbelt use levels should be controlled for in subsequent meta-regression analysis.

In developing the CAST database we decided to include effects from studies that both did and did not employ control groups. By using meta-analysis to compare overall effects derived from controlled studies versus those derived from non-controlled studies, we could determine whether those confounding factors not accounted for in non-controlled studies significantly influence overall effects. This analysis is presented in Table 7.24.

Table 7.24. Overall effect of road safety campaigns on road accidents and seatbelt use according to use of control group, after accounting for publication bias. Each overall effect is based on n individual effects (excluding any artificial effects generated by trim-and-fill).

	Accidents				Seatbelt use			
design	effect (%)	95% confidence interval (%)	n	Sig.? (p < 0.05)	effect (%)	95% confidence interval (%)	n	Sig.? (p < 0.05)
control	- 9	(-13;-5)	91	yes	+17	(+9;+25)	48	yes
no control	- 5	(-12;+4)	28	no	+31	(+21;+41)	84	yes

There is no significant difference between those accident effects reported by controlled studies (- 9 %) and those reported by non-controlled studies (- 5 %) according to a random effects model after accounting for publication bias, even if the non-controlled effects are slightly lower. It is also informative to compare these results before adjusting for publication bias. In this case the overall summary for controlled effects is a 12 % reduction in accidents (with -16;-9% confidence interval) and for non-controlled effects, a 10 % reduction in accidents (with -17; -4% confidence interval).

At 31 per cent, the overall seatbelt use effect for non-controlled studies is higher than that for controlled studies (17 per cent), but this difference is only apparent after accounting for publication bias. The actual overall effect reported by controlled studies before adjustment was 51 per cent (with a 95 per cent confidence interval of 43 to 60 per cent), compared with 48 per cent for non-controlled studies (with a 95 per cent confidence interval of 38 to 58 per cent).

In other words, before accounting for publication bias, overall campaign effects were similar in size and variability whether they were based on changes in accident counts or seatbelt use. This gives us confidence to use the effects derived from non-controlled studies.

A further aspect of evaluation design that may influence the apparent effect of campaigns is whether the outcome measure is recorded before and during or before and after the road safety campaign. Some studies took outcome measurements before, during and after the campaign, in which case an average of during and after measurements was taken for the database, and the effect allocated greater weight accordingly. An analysis was carried out to see if there are differences for effects derived from these three different types of study. This analysis informs whether the campaigns in this sample are more effective while they are ongoing or after they are complete.

Table 7.25. Overall effect of road safety campaigns on road accidents and seatbelt use according to time at which outcome measure taken. The results are given after accounting for publication bias. Each overall effect is based on n individual effects (excluding any artificial effects generated by trim-and-fill).

	Accidents				Seatbelt use			
	effect (%)	95% confidence interval (%)	n	Sig.? (p < 0.05)	effect (%)	95% confidence interval (%)	n	Sig.? (p < 0.05)
before-during	-12	(-15;-8)	86	yes	+6	(-3;+17)	26	no
before-after	-6	(-16;+5)	21	no	+29	(+17;+42)	54	yes
before-during-after	-6	(-15;+5)	12	no	+32	(+21;+43)	53	yes

Table 7.25 shows no significant difference between accident reduction effects during the campaigns (-12 per cent) and after they have been carried out (-6 per cent). However, the number of effects in the latter group led to greater variation and less certainty about the result. There were too few effects in the before-during-after group to enable solid conclusions to be made.

Seatbelt usage rates are significantly greater after campaign completion, with post-campaign rates increasing by 29 per cent compared with 6 per cent during the campaign. The overall effect of before-during-after studies was comparable to that of the before-after studies.

THEME AND COUNTRY

The overall effect of campaigns might be expected to vary according to the theme of the campaign. Table 7.26 shows that the campaigns against drink-driving included in our sample appear to be very effective, resulting in an overall significant 18 per cent decrease in accident counts. This is not surprising since drink-driving is known to increase the risk of accidents more than other behaviours addressed by campaigns. The effect of drink-driving campaigns on accidents cannot be aligned with their effect on drink-driving behaviour while evaluations of the latter are so variable. In any case drink driving campaigns may raise awareness among road users about road safety in general, and in doing so decrease the likelihood of other behaviour that might lead to accidents. Such a mechanism might indeed explain why seatbelt-themed campaigns reduce accident counts significantly by 8 per cent (Table 7.26), although this finding must be treated with caution while it is based on only 5 individual effects.

The poor overall effects of campaigns aimed at increasing driver awareness of pedestrians are also based on only five individual effects. Finally, the overall effect of speeding campaigns on accident counts appears to be poor (+3 per cent), and is not statistically significant.

Table 7.26. Overall effect of road safety campaigns on road accidents and seatbelt use according to campaign theme, and after accounting for publication bias. Each overall effect is based on n individual effects (excluding any artificial effects generated by trim-and-fill).

	Accidents				Seatbelt use			
Theme	effect (%)	95% confidence interval (%)	n	Sig.? (p < 0.05)	effect (%)	95% confidence interval (%)	n	Sig.? (p < 0.05)
General / multiple themes	- 11	(-22, +3)	9	no	--	--	--	--
Drink-driving	- 18	(-23, -12)	41	yes	--	--	--	--
Speeding	+ 3	(- 7, +14)	29	no	--	--	--	--
Seatbelts	- 8	(-12, -4)	5	yes	+25	(+18;+31)	118	yes
Awareness of pedestrians	+ 1	(-7, +9)	5	no	--	--	--	--

Analysis of overall effect by the six countries most represented in the database reveals some differences in campaign success (Table 7.27). The USA, Australia and Norway have carried out campaigns which have had statistically significant overall accident-reducing effects. The campaigns that have been included from The Netherlands, Sweden and Denmark appear to be effective overall, but there might be too few effects for the overall effect to be statistically significant. The USA and The Netherlands were the only two countries included that carried out campaigns that gave significant overall seatbelt-use increases, of 28 per cent and 20 per cent, respectively.

Table 7.27. Overall effect of road safety campaigns on road accidents and seatbelt use according to country carrying out campaign. After accounting for publication bias. Each overall effect is based on n individual effects (excluding any artificial effects generated by trim-and-fill).

	Accidents				Seatbelt use			
country	effect (%)	95% confidence interval (%)	n	Sig.? (p < 0.05)	effect (%)	95% confidence interval (%)	n	Sig.? (p < 0.05)
USA	-10	(-14;-6)	33	yes	+28	(+21;+36)	91	yes
Australia	-14	(-19;-7)	30	yes	+15	(-5;+40)	8	no
Sweden	-16	(-33;+5)	9	no	+70	(-1;+172)	9	no
The Netherlands	-13	(-39;+25)	7	no	+20	(+10;+30)	6	yes
Norway	-8	(-15;+1)	19	yes	--	--	0	--
Denmark	-25	(-56;+29)	5	no	--	--	0	--

SCOPE

Dimensions of campaign scope considered were duration, scale and specificity. To examine any link between the length of a campaign and its effect, we grouped the effects according to duration of their corresponding campaigns, and looked for any differences in overall effects between these groups (Table 7.28).

Table 7.28. Overall effect of road safety campaigns on road accidents and seatbelt use according to length of campaign. Results are given after accounting for publication bias. Each overall effect is based on n individual effects (excluding any artificial effects generated by trim-and-fill).

	Accidents				Seatbelt use			
campaign duration	effect (%)	95% confidence interval (%)	n	Sig.? (p < 0.05)	effect (%)	95% confidence interval (%)	n	Sig.? (p < 0.05)
<1 week	--	--	0	--	+78	(+42;+124)	11	yes
1-4 weeks	-15	(-22;-9)	7	yes	+21	(+9;+35)	55	yes
4-12 weeks	- 5	(-11;+1)	28	no	+25	(+13;+48)	37	yes
12-52 weeks	-10	(-14;-5)	51	yes	+14	(+4;+25)	23	yes
>52 weeks	-8	(-16;0)	29	no	+57	(+28;+94)	5	yes

As can be seen, in this sample there is no clear link between duration and effect, whether this is measured in accident counts or in percentage seatbelt use. An apparent pattern of decreasing improvements in seatbelt use with campaign duration, from +78 per cent for campaigns lasting less than a week to +14 per cent for those lasting between 12 and 52 weeks, is confounded by the strong overall effect (+52 per cent) of campaigns lasting over a year, although the latter is only based on five effects.

Likewise there is no clear link between the scale of the campaigns in this sample – as defined by whether they are carried out at a local, regional or national level – and effect (Table 7.29).

Table 7.29. Overall effect of road safety campaigns on road accidents and seatbelt use according to scale of campaign. Results are given after accounting for publication bias. Each overall effect is based on n individual effects (excluding any artificial effects generated by trim-and-fill).

	Accidents				Seatbelt use			
scale	effect (%)	95% confidence interval (%)	n	Sig.? (p < 0.05)	effect (%)	95% confidence interval (%)	n	Sig.? (p < 0.05)
local	- 3	(-11;+5)	25	no	+27	(+20;+35)	95	yes
regional	-13	(-17;-9)	70	yes	+34	(+19;+51)	20	yes
national	-2	(-9;+6)	14	yes	+17	(+3;+33)	13	yes

The greatest overall effect is for regional campaigns, whether the outcome measure is accidents or seatbelt use, but there are no clear significant differences between any levels of scale.

A trend is suggested between effect and the extent to which a campaign explicitly targets a group (Table 7.30). The effects are stronger for those campaigns that attempt to target a single group, whether the outcome measure used to evaluate them is accident counts or seatbelt use. Those campaigns in this sample explicitly targeting a single group reduced accidents by 16 per cent and increased seatbelt use by 31 per cent. These overall effects were at least 50 per cent stronger than those for campaigns not specifying a target group.

Table 7.30 Overall effect of road safety campaigns on road accidents and seatbelt use according to campaign specificity. Results are given after accounting for publication bias. Each overall effect is based on n individual effects (excluding any artificial effects generated by trim-and-fill).

	Accidents				Seatbelt use			
target group defined	effect (%)	95% confidence interval (%)	n	Sig.? (p < 0.05)	effect (%)	95% confidence interval (%)	n	Sig.? (p < 0.05)
1 target group	-14	(-18;-10)	62	yes	+31	(+20;+43)	98	yes
>1 group	-4	(-18;+13)	8	no	+21	(+12;+32)	11	yes
not specified	-8	(-14;-1)	39	yes	+19	(+14;+25)	24	yes

DELIVERY

To be effective, a campaign message must reach its target audience successfully. There are many different ways in which a campaign can be delivered and the exact media used will undoubtedly play a part in determining how effective a campaign is. After we consider different media, we present sub-group analyses according to whether the media were paid for or earned, and whether they were accompanied by changes in law and enforcement.

Overall effects on accidents based on use or non-use of different types of delivery medium is shown in Table 7.31. Because several different types of delivery media can be used by a campaign, a comparison was made with those campaigns not using each medium. Corresponding results for seatbelt use are given separately in Table 7.32.

There are no significant differences for the different delivery media. The biggest advantage conveyed by any single medium, in terms of the difference in overall effect for campaigns that use it compared with those that do not, is for billboards. However, the result is not significant and overall seatbelt use

effects are comparable for those campaigns that do and do not use billboards (Table 7.32).

Table 7.31. Overall effects of road safety campaigns on road accidents according to delivery media used in campaign. Results are given after accounting for publication bias. Each overall effect is based on n individual effects (excluding any artificial effects generated by trim-and-fill). (The variable 'posters' is omitted on grounds of highly variable contexts in which posters were used.)

	campaigns using medium				campaigns not using medium			
medium	effect (%)	95% confidence interval (%)	n	Sig.? (p < 0.05)	effect (%)	95% confidence interval (%)	n	Sig.? (p < 0.05)
television	-9	(-13;-5)	97	yes	-9	(-15; -3)	21	yes
radio	-10	(-13;-6)	83	yes	-11	(-17;-4)	35	yes
newspapers	-10	(-13;-6)	83	yes	-11	(-17;-6)	34	yes
billboards	-14	(-19; -8)	27	yes	-6	(-10;-1)	89	yes
leaflets	-10	(-15;-5)	41	yes	-9	(-15;-3)	73	yes
video, dvd	-14	(-23; -5)	11	yes	-9	(-12; -5)	103	yes
cinema	-9	(-18; +2)	9	no	-9	(-12;-6)	105	yes
personal	-15	(-23;-7)	28	yes	-7	(-11;-3)	90	yes

Surprisingly, campaigns using certain mass media tend, if anything, to have lower overall accident-reducing effects than those that do not. However, most of the campaigns for which accident counts were measured used television and radio and their overall effects (9 and 10 per cent accident reduction, respectively) were similar to the overall 9 per cent accident reduction by all campaigns in the sample. Overall seatbelt use effects are comparable for those campaigns that do and do not use television or radio (Table 7.32).

Personal communication as a type of delivery channel was included after an hypothesis developed after INFOEFFEKT (Vaa et al., 2004), that messages delivered in a personal way have greater impact on individuals because it makes them more inclined to attend to the message. Types of communications categorized 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; and personally addressed letters. Those campaigns using such personal communication to help channel the campaign message coincided with an overall reduction in accident levels of 15 per cent, compared with a reduction of 7 per cent for those campaigns not using personal communication. The beneficial effect of personal communication as a delivery medium is also apparent in the seatbelt usage results, with those campaigns using personal medium causing a 31 per cent increase in use, compared with 20 per cent for those that did not.

Data describing whether campaigns paid for the delivery media, using television or radio ads for example, or earned it, using methods such as lobbying or appealing for free air time, were also used to subgroup the effects. Table 7.33 shows that there are no significant differences in overall effects according to this variable, and again the pattern for accidents and seatbelt use is not consistent. A near-significant difference between those seatbelt effects produced by paid campaigns (41 per cent increase in seatbelt use) and earned campaigns (12 per cent increase) is notable but this must be considered alongside a stronger effect for 'earned' campaign effects on accidents.

Table 7.32 Overall effects of road safety campaigns on seatbelt use according to delivery media used in campaign. Results are given after accounting for publication bias. Blank cells indicate that adjustment was not possible. Each overall effect is based on n individual effects (excluding any artificial effects generated by trim-and-fill).

	campaigns using medium				campaigns not using medium			
medium	effect (%)	95% confidence interval (%)	n	Sig.? (p < 0.05)	effect (%)	95% confidence interval (%)	n	Sig.? (p < 0.05)
television	+18	(+10;+26)	48	yes	--	--	--	--
radio	+20	(+14;+27)	45	yes	+28	(+17;+40)	88	yes
newspaper	+21	(+14;+29)	61	yes	+29	(+16;+44)	72	yes
variable message sign	+23	(+7;+42)	10	yes	+28	(+21;+36)	123	yes
posters	+17	(+10;+25)	64	yes	+33	(+22;+45)	69	yes
billboards	+23	(+12;+34)	19	yes	+25	(+27;+33)	114	yes
leaflets	+17	(+8;+26)	75	yes	+30	(+21;+40)	58	yes
video, dvd	+76	(+22;+151)	11	yes	+26	(+18;+33)	121	yes
cinema	+10	(+2; +17)	8	yes	+29	(+22;+37)	125	yes
personal	+31	(+17;+47)	57	yes	+20	(+13;+27)	76	yes
web	+17	(+9;+25)	11	yes	+28	(+21;+35)	122	yes
reward	+67	(+57; +78)	36	yes	--	--	--	--
pledge	+62	(+30; +102)	7	yes	+24	(+17;+31)	126	yes

Table 7.33. Overall effect of road safety campaigns on road accidents and seatbelt use according to whether media paid or earned. After accounting for publication bias. Each overall effect is based on n individual effects (excluding any artificial effects generated by trim-and-fill).

	Accidents				Seatbelt use			
media paid for or earned?	effect (%)	95% confidence interval (%)	n	Sig.? (p < 0.05)	effect (%)	95% confidence interval (%)	n	Sig.? (p < 0.05)
paid	-1	(-13;+12)	8	no	+41	(+26;+57)	51	yes
earned	-13	(-18; -8)	40	yes	+12	(-4;+30)	15	no
paid and earned	-13	(-21;-5)	24	yes	+21	(+1;+43)	19	yes

Table 7.34. Overall effect of road safety campaigns on road accidents and seatbelt use according to accompanying legal measures. After accounting for publication bias. Each overall effect is based on n individual effects (excluding any artificial effects generated by trim-and-fill).

	Accidents				Seatbelt use			
campaign	effect (%)	95% confidence interval (%)	n	Sig.? (p < 0.05)	effect (%)	95% confidence interval (%)	n	Sig.? (p < 0.05)
not enforced	-9	(-16;-2)	33	yes	+29	(+18;+40)	89	yes
enforced	-12	(-15;-8)	84	yes	+19	(+15; +25)	44	yes
enforced + change in law	-12	(-20; -3)	9	yes	+26	(+12; +42)	11	yes
enforced, no change in law	-12	(-16;-8)	74	yes	+16	(+11;+21)	33	yes

Many campaigns are accompanied by the implementation or increase of enforcement measures in order to increase the visibility of risks to drivers, and hopefully enhance campaign effects. Enforcement may or may not be accompanied by a change in law, such as mandatory seatbelt use. A consistent observation from previous meta-analyses is that campaigns that are enforced are more successful at reducing accidents (the EU GADGET project, Delhomme et al.; 1999). This tendency is seen among the effects in the expanded CAST database (Table 7.34). However, the lack of a significant difference between enforced and non-enforced campaigns in the present study contrasts somewhat with the findings of previous studies. Attempts are made to explain the reasons for this in a separate section in Chapter 9.

Whether the enforcement is accompanied by a change in the law or not also has little effect on the overall effects in this sample. There is no significant effect of enforcement on the effects of campaigns on seatbelt use.

CONTENT

Successful delivery of a campaign message can only result in a successful campaign if the message contains information that persuades individuals to change their behaviour. To assess varying impacts of different types of campaign content, the effects were also grouped and analysed on the basis of content variables.

Campaigns can be based on psychological or social theories and models, on previously successful campaigns, or on consultation with a target group sample using focus groups or surveys. Again, because campaigns can be based on several of these factors, comparisons were made with those campaigns not based in each factor to see whether the basis of campaign content influences final campaign effects. Analyses were carried out according to these groupings, as shown in Table 7.35 for accidents and Table 7.36 for seatbelt use.

Table 7.35. Overall effects of road safety campaigns on accident counts according to explicit basis of campaign. Results are given after accounting for publication bias. Each overall effect is based on n individual effects (excluding any artificial effects generated by trim-and-fill).

	campaigns with basis				campaigns without basis			
basis	effect (%)	95% confidence interval (%)	n	Sig.? (p < 0.05)	effect (%)	95% confidence interval (%)	n	Sig.? (p < 0.05)
theory or model	-8	(-14;-1)	16	yes	-12	(-17;-8)	85	yes
previous campaigns	-11	(-17;-4)	47	yes	-6	(-14;+3)	18	no
consultation with target	-12	(-21; -1)	15	yes	-11	(-17; -4)	47	yes

There were no large or significant differences in overall accident-reducing effects of campaigns according to their basis. Analyses for seatbelt effects, however, showed that the overall effect of campaigns based on consultation with target group was significantly greater than that of campaigns that did not consult the target group.

Table 7.36. Overall effects of road safety campaigns on seatbelt use according to explicit basis of campaign. Results are given after accounting for publication bias. Each overall effect is based on n individual effects (excluding any artificial effects generated by trim-and-fill).

	campaigns with basis				campaigns without basis			
basis	effect (%)	95% confidence interval (%)	n	Sig.? (p < 0.05)	effect (%)	95% confidence interval (%)	n	Sig.? (p < 0.05)
theory or model	+31	(+6;+61)	27	yes	+24	(+17;+30)	104	yes
previous campaigns	+33	(+22;+44)	77	yes	+52	(+29;+80)	24	yes
consultation with target	+50	(+35;+67)	13	yes	+28	(+20;+35)	89	yes

It is important for any campaign designer to consider how the appeal will be made to the target group. According to attitude theory one can use emotional content to address the feeling component of an attitude, and more rational content to try and address the cognitive component (see Road User Model, Work Package D1.4). This theory predicts that those campaigns that manage to use both emotional and rational content together would tend to be more effective.

Therefore, where enough information was available, we made a judgement as to whether the content of the campaign was emotional, rational or both (Table 7.1). Emotional content is that which tends not to persuade through facts and figures, but uses appealing images or stories of individuals, for example. Rational content is information that tends to present logical reasons why the target should change their behaviour. Table 7.37 reports overall effects for both accidents and seatbelt use.

Table 7.37. Overall effects of road safety campaigns on road accidents and seatbelt use according to nature of appeal used in campaign. Results are given after accounting for publication bias. Each overall effect is based on n individual effects (excluding any artificial effects generated by trim-and-fill).

	Accidents				Seatbelt use			
medium	effect (%)	95% confidence interval (%)	n	Sig.? (p < 0.05)	effect (%)	95% confidence interval (%)	n	Sig.? (p < 0.05)
emotional appeal			4		+26	(+14;+39)	12	yes
rational appeal	-8	(-12;-3)	54	yes	+13	(+3;+24)	47	yes
emotional and rational appeal	-13	(-20; -6)	31	yes	+66	(+35; +104)	7	yes

Campaigns with combined content had an overall accident-reducing effect of 13 per cent, and increased seatbelt use by 66 per cent. The latter effect was significantly better than the overall effect of campaigns that used exclusively

rational appeals. These results somewhat support the hypothesis that campaigns combining a rational and emotional appeal are more effective than those focusing on one or the other.

Along with feelings and cognitions, perception of perceived social norms is also thought to be an important component of attitude that any attempts to change attitude should consider (see Road User Model, Deliverable 1.4). Where there was sufficient information about the campaign content, a subjective assessment was therefore made as to whether the campaign addressed the social norm or not. In most cases the subjective social norm was actively dealt with, rather than the descriptive social norm. A campaign addressing subjective social norms attempts to convey to the target individual the accepted and unwritten rules valued by significant others. For example, a campaign that attempts to convey to male youths that attractive girls find speeding a turn-off is one that addresses social norms. The analysis based on social norms is shown in Table 7.38. Although there are no significant differences, the trend suggests that campaigns benefit from attempts to address the social norm.

Table 7.38. Overall effects of road safety campaigns on road accidents and seatbelt use according to whether campaign addresses social norm. After accounting for publication bias. Each overall effect is based on n individual effects (excluding any artificial effects generated by trim-and-fill).

	Accidents				Seatbelt use			
Social norm addressed?	effect (%)	95% confidence interval (%)	n	Sig.? (p < 0.05)	effect (%)	95% confidence interval (%)	n	Sig.? (p < 0.05)
yes	-12	(-19;-5)	30	yes	+33	(+16;+52)	26	yes
no	-9	(-13;-5)	56	yes	+24	(+16;+32)	77	yes

Another dimension on which campaign content can vary is in the consequences of unsafe behaviour conveyed. For those campaigns that do show the consequences, they can show them in a shocking or non-shocking way. This aspect of content has relevance to the debate about fear campaigns dealt with in Deliverable 1.4 of this Work Package.

Table 7.39. Overall effects of road safety campaigns on road accidents and seatbelt use according to way campaign addresses the consequences of unsafe action. Results are given after accounting for publication bias. Each overall effect is based on n individual effects (excluding any artificial effects generated by trim-and-fill).

	Accidents				Seatbelt use			
medium	effect (%)	95% confidence interval (%)	n	Sig.? (p < 0.05)	effect (%)	95% confidence interval (%)	n	Sig.? (p < 0.05)
Shock used	-4	(-10;+1)	9	no	--	--	4	--
Shock not used	-10	(-13;-6)	84	yes	+14	(+5;+24)	29	yes

Overall effects according to use of shock are shown in Table 7.39. Those campaigns showing non-shocking consequences give the largest overall accident-reducing effect, but differences are not significant. There were too few shocking campaigns in the sample to allow for solid conclusions about their effects.

Campaign effects were also categorized subjectively according to whether they incorporated an element of humour. Examples of humour are a comical campaign mascot or the use of humour in a television campaign. Only one accident effect was for a campaign judged to use humour and so this analysis was omitted. Results for seatbelt use effects are given in Table 7.40. The use of humour has no clear influence on overall campaign effect in our sample.

Table 7.40. Overall effects of road safety campaigns on seatbelt use according to use of humour. Results are given after accounting for publication bias. Each overall effect is based on n individual effects (excluding any artificial effects generated by trim-and-fill).

	Seatbelt use			
Humour?	effect (%)	95% confidence interval (%)	n	Sig.? (p < 0.05)
Yes	+23	(+15;+31)	23	yes
No	+30	(+16;+45)	107	yes

Campaign effects were further categorized and analysed according to whether the evaluated campaign attempted to persuade by highlighting the risk involved in unsafe behaviour. Any risk addressed was further categorized according to whether it was risk of detection (by police), risk of harm to self or risk of harm to others that was addressed. It was possible for any one campaign to address one, two or all three types of risk. Analyses carried out on this basis are shown in Table 7.41 for accident effects, and in Table 7.42 for seatbelt use effects.

In terms of effects on accident counts, the results indicate that addressing risk is beneficial, but that it is more beneficial if risk of detection is addressed, rather than risk of harm to self or others. The seatbelt use results suggest that addressing risk is not beneficial.

Table 7.41 Overall effects of road safety campaigns on road accidents according to treatment of risk in campaign. Results are given after accounting for publication bias. Each overall effect is based on n individual effects (excluding any artificial effects generated by trim-and-fill).

	yes				no			
category	effect (%)	95% confidence interval (%)	n	Sig.? (p < 0.05)	effect (%)	95% confidence interval (%)	n	Sig.? (p < 0.05)
risk of detection addressed?	-7	(-12;-3)	53	yes	-7	(-13;-1)	39	no
risk of harm to self addressed?	-3	(-6;+0)	22	no	-10	(-14;-6)	71	yes
risk of harm to others addressed?	-5	(-12;+3)	17	no	-11	(-14;-7)	76	yes

Table 7.42. Overall effects of road safety campaigns on seatbelt use according to treatment of risk in campaign. Results are given after accounting for publication bias. Each overall effect is based on n individual effects (excluding any artificial effects generated by trim-and-fill).

	yes				no			
category	effect (%)	95% confidence interval (%)	n	Sig.? (p < 0.05)	effect (%)	95% confidence interval (%)	n	Sig.? (p < 0.05)
risk of detection addressed?	+17	(+12; +22)	36	yes	+29	(+17;+42)	75	yes
risk of harm to self addressed?	+15	(-8;+42)	22	no	+27	(+20;+34)	102	yes
risk of harm to others addressed?	+12	(-14;+47)	16	no	+29	(+23;+36)	108	yes

SUMMARY

The overall effects of road safety campaigns on either accidents or seatbelt use have been presented according to variables describing the background, delivery or content of the campaigns, to get an idea about the size of effects that campaigns sharing a certain property have. It should be remembered that each of these campaigns will also vary according to a number of other factors not controlled for in the bivariate analysis presented in this section.

Throughout subgroup analyses we have seen some consistencies between accident and seatbelt effects. There is some evidence according to both measures that campaign effects improve where a campaign:

- identifies a target group;
- uses personal communication;
- combines emotional and rational content in its appeal; or
- addresses the social norm.

According to both measures we found no evidence of a link between campaign effect and

- campaign scale;
- accompanying enforcement; or
- humour (not assessed for accident counts)

However, inconsistencies are also apparent. Some, such as the link between value of the pre-campaign measure and campaign effect, can be easily explained. Others are less obvious. For instance, it is not as clear why seatbelt use and not accident counts would be better after a road safety campaign rather than during a campaign. There are several possible explanations however.

One is that most accident effects are for campaigns whose themes are most often not seatbelt use, but drink-driving and speeding. Those aspects of campaigns that are more effective at increasing seatbelt use are not necessarily equally effective at influencing speeding and drink-driving behaviours.

Another explanation is that the effectiveness of different campaign factors might also vary according to scale or other complicating factors. If, for example, substantially more seatbelt use effects than accident effects are for small scale campaigns, and those factors making small-scale campaigns effective are different from those making large-scale campaigns effective, then we will have apparent discrepancies among accident and seatbelt trends.

Multi-variate analysis (meta-regression) should help clarify such discrepancies, and pick out which of the apparently beneficial factors listed above remain important when other factors are controlled for.

8. MODEL OF FACTORS INFLUENCING CAMPAIGN EFFECT

8.1 META-REGRESSION

Up to now in this report we have described the use of meta-analysis to generate a summary of the overall effects of road safety campaigns on beliefs, knowledge, different types of behaviour and accident counts. Since there were sufficient numbers of effects in the database, we divided each group of accident count and seatbelt use effects into sub-groups according to variables describing study background, and campaign delivery and content. We then used meta-analyses to summarize the effects in each of these subgroups, after adjusting for publication bias.

The aim of the sub-group analysis is to give a realistic idea about the overall size of campaign outcomes for campaigns using certain delivery methods or using certain content. However, we cannot be certain that a specific delivery method or type of campaign content is responsible for larger effects, where these are apparent. This is because subgroup analysis looks only at how one variable tends to vary with campaign effect. Although it enables several different levels of that variable to be examined (for example, several countries in the variable *country*) we are still only looking at the accompanying variation of one variable in isolation.

The problem is that in the real world the value of a single variable often varies to some extent in line with those of other variables, and that this covariance prevents clear conclusions being drawn from subgroup analyses based on one variable. Take for example the effect of campaigns on seatbelt use. We have seen that those studies in the database using a seatbelt use measure to evaluate campaigns tend to include several early, small-scale, seat-belt campaigns carried out in the USA. The studies report that such campaigns are actually quite effective, but because of covariance it is difficult to conclude from subgroup analyses which of the factors (time, scale, country or even campaign theme) vary most in line with campaign effect. To better understand the variables and their relative importance, we need to be able to (i) measure their influence on seatbelt use as a group of variables together; (ii) account for the covariance they may have with each other and with the outcome variable, in order to (iii) understand the unique variance each variable shares with the outcome effect. Only then will we be able to make conclusions about the importance of each individual variable in a group of variables. Meta-regression allows us to do this.

META-REGRESSION AND STANDARD WEIGHTED REGRESSION

Meta-regression is an extension of standard multiple regression, a set of statistical techniques that allows the relationship between several explanatory variables (i.e. independent variables) and one outcome variable (effect size) to be examined simultaneously (Lipsey & Wilsey, 2001). In our case, the

meta-regression model determines how much of the variation in campaign effect size (i.e. changes in accidents or seatbelt usage) can be explained by the different campaign characteristics we have coded, as well as determining which of these are related to the effect size of the campaigns. As in standard weighted regression, in meta-regression each effect size is given a weight according to its relative statistical importance. However, because meta-regression attempts to summarise a number of individual effect sizes, it differs from standard weighted regression in the way it standard errors are treated (Lipsey & Wilson, 2001). There are two main forms of meta-regression:

- Fixed effects meta-regression, which does not allow for between-effect heterogeneity *after* the effect of the explanatory variables are taken into account. In practice this means that the weights of each effect size are calculated without accounting for the possibility that there are true differences between the various campaign effects estimated by different evaluation studies. This analysis will give the same parameter estimates as an ordinary weighted regression analysis, but the standard errors will be corrected when using fixed effects meta-regression.
- Random effects meta-regression, where the random effects represent between-effect heterogeneity that remains *after* the effect of the explanatory variables are taken into account. The remaining random effect is used to calculate new weights for each effect size, and the analysis is rerun using these new weights. In practice, this means that larger studies receives less weight in random effects regression models than in fixed effects models.

The main difference between these two methods is that fixed effects meta-regression has more statistical power to detect relationships between the explanatory variables and the effect size, but the cost is a high Type I error rate (i.e. the tests of statistical significance may be too liberal). The random effects model is on the other hand very conservative in terms of the test of statistical significance (i.e. low Type I error rate), but has low statistical power to detect relationships between explanatory variables and effect size (see e.g. Lipsey & Wilson, 2001).

There is no consensus of whether the one method should be preferred over the other. Simulations such as those run by Higgins and Thompson (2004) warn against conclusions based on fixed-effects meta-regression alone on the basis that the method is insufficiently conservative. However, others point out that random-effects methods, by giving larger weight to small studies, are more susceptible to publication bias, a phenomenon which cannot be accounted for in meta-regression. Some go as far as to consider the use of random-effects methods ‘wholly wrong’ (Poole and Greenland, 1999). A reasonable way to progress in the midst of such disagreement is to include results generated by both methods for consideration as the field develops in the future. Therefore, results from the application of both methods will be presented here.

PROBLEM OF STATISTICALLY DEPENDENT EFFECT SIZES

In the present dataset, a single evaluation study may report more than one effect size based on the same type of outcome measure. One reason for this is that campaigns with different properties can be evaluated in a single evaluation study. Alternatively, it is possible for two orthogonal outcome measures to be reported assessing the same campaign (e.g. changes in injury accidents and property damage accidents are both classified as accident outcomes in our study). A third possibility is that the evaluation reports effect sizes based on the same outcome measure for the same campaign run at a different time or in a different place. In the last two instances in particular, there is potential for the assumption of independence of observations required for regression analysis to be violated. In other words, the source of dependency is not taken into account there is a danger that the statistical significance of the explanatory variables as predictors for the effect of campaigns may be too liberal. We do not see this as a major problem because most of the evaluations retrieved in this study report a single effect size. Despite this, we have tried to take dependency among effect sizes into account by comparing results from the following different approaches to meta-regression:

1. Run both fixed and random effects meta-regression analyses where the dependent variable is made of each individual effect size reported, and explanatory variables are the campaign characteristics associated with each of these effect sizes. This has the above mentioned disadvantage of not taking dependency of effect sizes within campaigns into account.
2. Aggregate the different outcome measures reported into a single effect measure for each campaign, and thereafter conduct fixed and random effects meta-regression analyses with the different campaign characteristics as explanatory variables. Using this strategy, the effect sizes will be statistically independent (Lipsey & Wilson, 2001). The disadvantage of this method is that aggregation will restrict both the number of and the variation in effect sizes. Fewer effect sizes will reduce the statistical power of the model to detect significant relationship between the explanatory variables and effect sizes. Restricting the variation in effect sizes through aggregation may also lead to the so-called “ecological fallacy”, which holds that results based upon aggregated effect sizes are not necessarily descriptive of single effect sizes.

3. Use multi-level meta-analysis[31], where each of the extracted effect sizes are defined as level 1 units and each campaign as level 2 units. Using this approach, one campaign is allowed to have more than one associated effect size. The different explanatory variables (campaign characteristics) are entered as "level 2 predictors". The advantage of this method is that it considers dependency in campaign effect sizes, but at the same time takes the variation in effect sizes into account. The disadvantage is that the correlation among the effect sizes within each campaign ideally should be known. However, this is not believed to compromise a major problem due to that most campaigns report only one outcome.

These different approaches will together provide a description of how a set of given variables influence an outcome variable (effect size), which in our case is either accident counts or seatbelt use. The set of given variables is called a model. In CAST models are developed using a number of criteria. The variable must:

1. Make theoretical sense i.e. there must be a reason to believe that the variable may influence campaign effect
2. Together with other variables in the model capture a full range of possible influences on campaign effects
3. Be fully described by most of the campaign evaluation studies in the CAST database

The variable may summarize other variables and can be included based on the results of previous meta-analyses or the subgroup analyses presented in chapter 7 before and after adjusting for publication bias.

The results of analysis of a model can only be properly understood after considering certain theoretical issues, some of which we have already discussed. Further important considerations are outlined below.

OTHER IMPORTANT THEORETICAL CONSIDERATIONS

The following analyses will tell us about relationships among a selected given set of variables. This statement reveals two important limitations, namely (i) that it tells us nothing about cause and effect between variables, either between the explanatory and outcome variable or between two explanatory

[31] Multilevel meta-analysis is similar to meta-regression, but corrects the standard errors for the nesting of data (i.e. several effect sizes reported from the same campaign) making the test for statistical significance for all parameters in the model more reliable. The greater the between-campaign variation in effect sizes, the more advantageous it is to conduct multilevel meta-analysis. It starts with an "intercept only" model, which incorporates both the outcome level and the campaign level, but does not include any predictors. This shows the overall average effect size of the campaigns, taking both random effects and the nesting of data into account. Then the model includes predictors at level 2 to explain variance between the campaign effect sizes. Weights of each study are then recalculated in a similar manner as in random effect meta- regression. This means that multilevel meta-analysis is basically the same as a random effect meta-regression analysis. For a further description of the use of multilevel modelling in meta-analysis, see e.g. Hox (2002).

variables; and (ii) that the output of the analyses depends entirely on the set of variables *given* by the researcher.

These two points are often overlooked by the reader who tries to interpret the results of these analyses without fully appreciating its limitations. It must always be remembered that regression and multilevel analysis only provides information about the way variables vary together, regardless of causal direction, and can only describe the variables under consideration. In other words it relies heavily on the theoretical input of the researcher, which itself is limited by existing knowledge and the information available in campaign evaluation studies.

Meta-regression and multilevel meta-analysis are also very sensitive to missing values on the explanatory variables, which means that information about a variable needs to be widely reported if it is to be included in a meta-regression model.

Both types of analyses assume that the variables used in the model are measured without error and the data describing a variable represents that variable validly. Let us consider the outcome variables, for example. In the case of accidents, while it is reasonable to assert that accident counts over a defined period are a good representation of accident levels, when interpreting regression models we must remember that often only officially reported accident figures are used (a source of error) and that in CAST different accident types are lumped together. Similarly, with seatbelt use measurements we should remember that the variable comprises both observed and self-reported measures, which will also contain different amounts of error. We should also consider how each of the explanatory variables in the models has been measured.

It should further be considered that the final regression solution is sensitive to the combination of variables included in it. The importance of a particular variable can depend highly on the other variables in the set. In CAST we attempt to deal with this by testing various combinations of variables around the final model.

In the final model an important explanatory variable will be the only one *included in the model* that assesses some important facet of the outcome variable. For variables that appear less important there are two possibilities: (i) that it is not as important with regards to the outcome variable; or (ii) it assesses some important facet of the outcome variable but shares this property with two or more other explanatory variables included in the model.

It should also be noted that neither meta-regression nor multilevel meta-analysis can account for publication bias. Even if it were to make theoretical sense, this would involve generation not only of artificial data for each of the effect sizes but also for their associated campaign variables, and we do not know of a method that is available to do this.

8.2 FACTORS ASSOCIATED WITH CAMPAIGN EFFECT ON ACCIDENTS

A preliminary model of factors for meta-analysis was evolved through various considerations by the project team. Available explanatory variables were considered with respect to overall effects of subgroups based on them (both before and after adjustment for publication bias). Results from past meta-analyses were also considered, along with theoretical reasoning. In this way the variables below were chosen as candidates for the explanation of variance in accident count effects. The number of explanatory variables that could be used from this model was primarily limited by the information available in the study evaluations, as explained below.

Table 8.1. Candidate variables for the explanation of campaign effects on accidents. Each is categorical. Variables with more than two levels were coded as dummy variables, so 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 coded as yes = 1 and no = 0. Scale was coded as national campaign= 1; other level of campaign = 0.

Background	
[decade]	before / after 2000
[theme]	drink-drive / speeding / general / others
[scale]	national / others
[duration]	0-29 days / 30-200 days / others
Delivery	
[combined mass-media]	Did the campaign use television+radio+newspaper? yes /no
[personal influence]	Did the campaign attempt to influence personally? yes / no
[on-road]	Did campaign use billboards, variable/fixed message signs on the road? yes / no
[dvd-video-cinema]	Did campaign use either DVD, video or cinema? yes / no
[target identification]	Was a particular group defined and targeted? yes / no
[enforcement]	Was the campaign accompanied by enforcement? yes / no
Content	
[emotional and rational]	Did campaign have emotional and rational content? yes / no
[non-shocking consequences]	Did the campaign show non-shocking consequences? yes /no
[social norm]	Did the campaign address the social norm? yes / no
[previous campaign]	Was the content based on previous campaigns? 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
[target group consulted]	Did the campaign consult a sample of the target group in developing the content? yes / no

The above model was then further developed by considering the statistical properties of each variable. Inspection of the bivariate correlations among the explanatory variables revealed a strong correlation between [risk of detection] and [enforcement] ($r = 0.73$; $p < 0.001$)[32]. We chose to exclude the former

[32] A strong correlation between explanatory variables represents a problem called collinearity. In practice, this means that it is difficult to separate the effect of one explanatory variable for the other and this makes is difficult to trust the parameter values and tests of statistical

variable from the model because we wished to compare associations between enforcement and accident counts with those found in previous projects (Delhomme et al, 1999; Vaa et al., 2004).

At this stage the model comprised 19 variables[33]. To improve the efficiency of the model, zero-order correlations, subgroup analyses and output of an initial simultaneous regression run (to examine tolerance relationships among explanatory variables) were again consulted and discussed by the project team, with a view to capture important influences on campaign effects in the most efficient way. The following variables were excluded at this stage: [duration 29 to 200 days], [theme-speeding], [theme-general], [dvd-video-cinema], [target group consulted] and [previous campaign].

The remaining 13 variables were then checked for missing cases. A further five variables were excluded on the basis that they contained too many missing cases. These were [emotional and rational], [non-shocking consequences], [social norm], [target group identified] and [risk of harm]. Although we considered these important, there simply was not enough information across the evaluation studies to score these campaign variables sufficiently.

The eight remaining variables met statistical recommendations for the ratio of variables studied to outcome effects in the sample (Tabachnikk & Fidell, 2007).

Both fixed effects and random effects meta-regression (the later with maximum likelihood estimation) were performed using the explanatory variables [duration-0 to 29 days], [decade-after 2000], [theme-drink-driving], [scale-national], [onroad], [personal influence], [enforcement] and [combined mass-media], using ln odds ratios calculated using accident effects (see chapter 7). A SPSS macro for meta-regression developed by Wilson (2005) was used to estimate the two models. An inspection of the contributions to outcome variance then led to the exclusion of the variable [scale-national] to give a final model of seven variables (Table 8.2).

In Table 8.2-8.4 different numbers are presented for each explanatory variable. These can be interpreted in the following way:

- *b* (the regression coefficient) represents the expected difference in ln odds accident ratio between two campaigns, one having the campaign characteristic in question and the other not, controlled for all other explanatory variables in the model. A negative sign means a reduction in accident rate if the campaign has this characteristic, a positive sign the opposite. For instance, On-road has a *b* of -.10, meaning that campaigns using On-road measures have larger accident reductions compared to those not using this measure.

significance of the explanatory variables. A common solution is to drop one of the highly correlated explanatory variables from the final model.

[33] Variables containing more than two levels were dummy coded so are actually counted as more than one variable.

- *% 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. The Mean Effect Size is the average of all effect sizes included in the model, i.e. the average change in accident counts. This effect is, however, not corrected for publication bias.

Table 8.2. Results from fixed effects and random effects 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.

	Fixed effects model			**Random effects model**		
	b (% change)		*p-value*[34]	*b (% change)*		*p-value*
(Constant)	-.05	(-5%)	<.01	-.03	(-3%)	.413
[Duration – 0 to 29 days]	-.16	(-15%)	<.001	-.10	(-10%)	.170
[After 2000]	.14	(15%)	<.001	.09	(10%)	.068
[Theme-drink-driving]	-.09	(-8%)	<.001	-.08	(-8%)	.040
[Personal communication]	-.07	(-6%)	<.001	-.12	(-11%)	.006
[On-road]	-.10	(-10%)	<.001	-.06	(-6%)	.108
[Enforcement]	-.09	(-8%)	<.001	-.07	(-6%)	.127
[Combined mass-media]	.11	(11%)	<.001	.04	(4%)	.351
R^2	.28		<.001	.14		.031
Q	430.8		<.001	133.2		.074
Mean Effect Size	-.11	(-11%)		-.12	(-12%)	

The results from Table 8.2 show that in the fixed effect model, all of the seven explanatory variables have a significant effect and together explain 28 % of the variation in change in accidents coinciding with campaigns. In the random effects model, the total amount of explained variance is lower (14 %), and only two of the campaign characteristics (*theme-drink-driving* and use of *personal communication*) are significantly related to a change in accidents. According to both models, however, the direction of each coefficient is consistent, and the sizes of the effects alike.

[34] Note: Probabilities output for fixed effects models in earlier versions of this report differed in two ways: (i) they were based on an unrevised set of effect sizes and associated campaign variables containing four fewer effect sizes; and (ii) they were based on weighted least squares regression run in SPSS, which reports standard errors that some consider inaccurate for meta-regression (Lipsey & Wilson, 2001).

The 119 reported outcomes are nested within 74 campaigns[35]. To overcome potential problems with dependency, all outcome measures reported from the same campaign were aggregated into a single outcome[36]. The same meta-regression analyses were then re-run using 74 effect sizes as the dependent variable (Table 8.3). These analyses give results similar to those in Table 8.2, but the models run using the aggregated effect sizes explain more variance in in campaign effect on accidents (25 % compared with 14 % for random effects models). Importantly, independent of aggregation of effect sizes there are five characteristics are significantly related to reduction in accidents in both models; short duration, starting before the year 2001, having a drink-driving theme, using personal communication and using on-road delivery.

Table 8.3 Results from fixed effects and random effects meta- regression of the final accident model. Aggregated effect size from 74 campaigns is the dependent variable. Variables, their coefficients, percentage change in accidents and probabilities are shown.

	Fixed effects model			**Random effects model**		
	b (% change)		*p-value*	*b (% change)*		*p-value*
(Constant)	-.05	(-5 %)	.006	-.04	(-4%)	.343
[Duration – 0 to 29 days]	-.16	(-15%)	<.001	-.13	(-12%)	.058
[After 2000]	.14	(+15%)	<.001	.12	(+13%)	.018
[Theme-drink-driving]	-.09	(-9%)	<.001	-.09	(-9%)	.023
[Personal influence]	-.07	(-7%)	<.001	-.09	(-9%)	.027
[On-road]	-.10	(-10%)	<.001	-.10	(-10%)	.007
[Enforcement]	-.09	(-9%)	<.001	-.07	(-7%)	.105
[Combined mass-media]	.11	(+11%)	<.001	.06	(+6%)	.080
R^2	.38		<.001	.25		<.001
Q	274.2		<.001	83.5		.071
Mean Effect Size	-.11	(-11%)		-.13	(-12%)	

Table 8.4 presents the results from the multilevel meta-analysis with campaign characteristics as level 2 predictors[37]. The results obtained are

[35] Information from the 74 campaigns are retrieved from 65 evaluation studies (some studies describe more than one campaign). Of the 74 campaigns measuring the effect in terms of change in accidents, 51 reported only one effect and the remaining 23 campaigns reporting more than one accident outcome effect. Of these, 12 reported 2 effects, 5 reported 3 effects, 4 reported 4 effects, 1 reported 6 effects and 1 reported 7 different effects. The mean correlation between the effects reported from each of the 23 campaigns were estimated to be -.07, suggesting that there is a very low degree of dependency present among the effects reported in the dataset.

[36] The aggregation into a single effect was done in the following way: if a campaign reported several accident outcomes, a weighted mean effect was constructed by the same manner as in ordinary meta-analysis: $\frac{\sum \ln odds \cdot weight}{\sum weight}$. The weight for each aggregated effect is the sum of weights for all outcomes reported from the campaign.

[37] The statistical program Hierarchical Linear and Nonlinear Modelling (HLM6.0) was applied to estimate the multilevel meta-analysis model. The model was estimated using Restricted Maximum Likelihood Method and using a “variance-known” model. This means that the

similar to those output by the previous models, in terms of both the amount of explained variance (27 %) and the size and direction of the relationship between the different campaign characteristics and campaign outcome. The tests of statistical significance are more conservative in the multilevel model than in the random effect model presented in Table 8.3. Despite this, four of the seven characteristics remain significantly related to accident reduction (starting before the year 2001, having a drink-driving theme, use of personal communication and use of on-road delivery).

Table 8.4. Results from multilevel meta-analysis. 119 effect sizes (level 1) nested within 74 campaigns (level 2). Level 2 predictors, their coefficients, percentage change in accidents and probabilities are shown.

	Model 1 (Intercept only)		**Model 2 (With predictors)**		
	b (% change in accidents)	*p-value*	*b (% change in accidents)*		*p-value*
Intercept (Grand mean)	-.13 (12%)	.001	-.04	(-4%)	.395
[Duration – 0 to 29 days]			-.13	(-12%)	.083
[After 2000]			.12	(11%)	.033
[Theme-drink-driving]			-.09	(-9%)	.034
[Personal influence]			-.09	(-9%)	.035
[On-road]			-.10	(-10%)	.016
[Enforcement]			-.07	(-7%)	.142
[Combined mass-media]			.06	(6%)	.112
Random effects σ^2_u	.0168	.001	.0123		.001
Chi-square	447.1		291.9		.001*
R square (level 2)			.27		.001

*p-value for difference in Chi-square value (model1 - model2).

To sum up, the total variance in effect of campaigns on accidents explained by the predictors after accounting for dependence and assuming there is variance between effect sizes evaluated by different evaluation studies (using a random effects model) is about 30 per cent. The main difference between the fixed and random effects models is the level of statistical significance for each of the explanatory variables. This is as expected, bearing in mind the difference between the models described earlier. Despite this, the coefficients for the explanatory variables in the different models are similar in both size and direction.

Of the explanatory factors in the model, on-road delivery of the campaign message appears to make a significantly beneficial contribution to accident

variance of each effect size is used instead of the weight of each effect size, but gives the same results as other statistical programs which use the weight.

reduction (about a 10% reduction). In contrast, combined mass-media delivery is found to be detrimental to changes in accidents relative to the other factors in the model, but this effect is not statistically significant in all models. The use of personal influence to deliver the message is beneficial, and the estimates from the models suggest a 7-9% decrease in accidents. A drink-drive theme was beneficial in terms of accident reduction, alone contributing to around 9 per cent reduction. The beneficial effect of enforcement on accidents was close to significance in the models (6-9% reduction). Finally, short campaign duration was beneficial and recent campaigns (carried out after 2000) were associated with detrimental effects. The final model can be summarized thus:

Figure 8.1 Final model of relative influence of campaign factors on reducing accident counts. Factors in italics are included even though they do not appear to be significant in all of the random effects models, because they approach significance, have theoretical grounds to be included and /or were found to be significant in a previous meta-regression analysis (Vaa et al., 2004).

+	▪ drink-drive theme ▪ short campaign duration ▪ on-road delivery ▪ personal influence ▪ *enforcement*
—	▪ recent campaigns, since 2000 ▪ *use of combined mass-media*

CHECKS ON THE FINAL ACCIDENT MODEL

1. Checks were made for collinearity and outliers. There were five outliers (maximum Mahalanobis distance = 54), but these were not omitted since statistical tests (maximum Cook's Distance = 0.44) and trial exclusion of affected cases from the model indicated that they did not influence the final model description unduly. It was assumed that homoscedasticity was minimised by weighting the regression by the fixed effects weights.
2. The variable [**stripped mass-media**] was created to identify those study effects associated with campaigns using at least one of the media television, radio *or* newspaper *and no other delivery channel*. Whereas combined mass-media -- which scored effects for campaigns using television and radio and newspaper with or without other channels -- can be considered as representing 'mass delivery' campaigns, stripped mass-media can be considered as a test for those campaigns using only a single mass media channel. A model was tested in which [stripped mass-media] was used in place of [combined mass-media]. Whereas the statistics for the other six variables were very similar, [stripped mass-media] was not significant, indicating that a single mass-media channel is neither beneficial nor detrimental to campaign effects on accidents.
3. Initially a decision was made to exclude **country** variables because we were not sure what it would tell us about campaign effects. We wished rather to identify what caused the variance between countries that was responsible for variance in campaigns effects. From the subgroup analyses country was dummy coded as [USA] and [Australia]. In neither case were there significant contributions from the country variables[38], either when they were added together or alone as variables to supplement the final model.
4. In CAST we made a decision to use effects reported by both **controlled and non-controlled studies**, and exclude non-controlled studies only if as a group they were significantly different from controlled effects. Subgroup

[38] According to a fixed effects model run using SPSS weighted least squares regression

analyses suggested that the two groups were not significantly different. This enabled us to include more effects in meta-regression analyses, and thereby include more explanatory variables in the final model. To further test differences between effects from controlled and non-controlled studies, two further checks were made in meta-regression: (a) [control] was added as a variable to the final model; and (b) the final model was used to test those effects arising from controlled studies alone.

a. All meta-regression and multilevel models were rerun using [control] as a supplementary variable to the final model variables. The results showed that controlled studies were slightly more effective than non-controlled (*b* = -.07), but not significantly (p=.07)[39]. The other explanatory variables had the same properties as in Table 8.2-8.4, both in terms of direction and significance of relationships.

b. All meta-regression and multilevel models were rerun using the final model variables on only controlled cases. This gave none or only trivial differences from the estimates in analyses based upon the total sample. However, reduction of number of campaigns resulted in fewer significant effects of the explanatory variables. This could be expected from the reduced power resulting from the reduction in number of effects to be analysed. Since the other estimates in the models were practically unaffected by excluding the non-controlled studies, this gave us confidence that the use of a control group did not affect the qualitative conclusions drawn in Figure 8.1.

5. A split-half analysis of the dataset was attempted based on whether the study came before or after 1993 (chosen because it gives a roughly 50:50 split in study number). This was done to address concerns about the **relevance of the final model to contemporary campaigns**. There were fewer significant relationships in either the pre-1993 or post-1993 output model, but there are probably too few effects present in each half to enable conclusions to be made about this. However, the sign of the relationships were the same in both models.

SUMMARY OF FACTORS INFLUENCING CAMPAIGN EFFECTS ON ACCIDENTS

During model development some important variables had to be excluded, ultimately because there was not enough information about the campaigns in the study evaluations. This prevented us from examining the way potentially important aspects of campaign content are associated with campaign outcome. Specifically, we could not consider how the use of emotion, addressing the social norm, or identifying and consulting a target group affected campaign outcome on accidents. Neither could we consider the full range of delivery channels, although we consider that the most important are captured in some way in the final model. Some delivery channels, such as posters, were not captured because we they were simply too difficult to classify. In summary then, there are important aspects about the delivery and content of the campaign that are not accounted for in the final accident model.

[39] Results reported from the multilevel meta-regression model.

Notwithstanding these reservations, and making the assumptions described in section 8.1, we conclude the following. Campaigns with a drink-drive theme tend to have larger reduction in accident counts. Campaigns of duration no longer than one month are also associated with greater reductions, though this result should be treated with caution since it is based on seven effects. Using a Delivery of the campaign message at the roadside, using a billboard, fixed or variable message sign, was also associated with accident reduction. The previous finding that personal communication would also be associated with increased accident reduction was confirmed. The effect of enforcement was only significant in the fixed effects models, although associations were in the expected direction and close to significance in the random-effects models. Recent campaigns appear to have been less effective than earlier ones, and use of combined mass-media channels is associated with less favourable accident outcomes in our model. Checks of the model for various potential problems indicate that it is robust, although a split-half analysis of the data was inconclusive.

We now turn to consider what influences the effect of campaigns on seatbelt use.

8.3 FACTORS ASSOCIATED WITH CAMPAIGN EFFECT ON SEATBELT USE

Using the process described in section 8.2, 22 variables were chosen as candidates for the explanation of variance in seatbelt effects (Table 8.3).

The variable [limited area] was introduced to control for the findings in chapter 7, that there appeared to be several effective small-scale seatbelt campaigns in the database, along with observations that many campaigns targeted a ‘captive audiences’, by targeting the campaigns within very limited areas, such as car parks, drive-in, hospital or other organisations. This variable was classified as a deliverable variable that was distinct from [scale-local], to which it did not correlate strongly.

The model was developed by considering statistical properties of each variable, in the following way. Variables with too many missing values were excluded such that all remaining variables had a maximum of seven missing values. [Social norm], [target consulted], [risk of detection] and [emotional and rational] were excluded on this basis. [Shocking consequences] was excluded because there were only five seatbelt use effects reported for campaigns explicitly using shocking content. Again important variables had to be excluded that could have important influences on campaign outcomes.

Table 8.5. Candidate variables for the explanation of campaign effects on seatbelt use. Each is categorical, apart from [initial seatbelt use]. The variable [duration] was recoded as [duration 0-29 days] (yes/no) and [duration 30-200 days] (yes/no). All yes/no variables were then coded as yes = 1 and no = 0. Scale was coded as national = 1; all other scales = 0

Background	
[Initial seatbelt use]	seatbelt usage (%) before campaign, a continuous variable
[duration]	0-29 days / 30-200 days / others
[paid media]	media coverage / ads paid for by campaign? yes; no
[scale]	national/ others
Delivery	
[combined mass-media]	Did the campaign use television+radio+newspaper? yes /no
[personal influence]	Did the campaign attempt to influence personally? yes / no
[on-road]	Did campaign use billboards, variable/fixed message signs on the road? yes / no
[dvd-video-cinema]	Did campaign delivery use either DVD, video or cinema? yes / no
[target identification]	Was a particular group defined and targeted? yes / no
[enforcement]	Was the campaign accompanied by enforcement? yes / no
[reward]	Did the campaign use a reward incentive? yes / no
[limited area]	Were the campaigns delivered to a limited or restricted population? yes / no
Content	
[emotional and rational]	Did campaign have emotional and rational content? yes / no
[humour]	Did the campaign have humorous content? yes/no
[shocking consequences]	Did the campaign show shocking consequences? yes/no
[non-shocking consequences]	Did the campaign show non-shocking consequences? yes /no
[social norm]	Did the campaign address the social norm? yes / no
[previous campaign]	Was the content based on previous campaigns? 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
[target group consulted]	Did the campaign consult a sample of the target group in developing the content? yes / no

Looking at the simple bivariate correlations among the independent variables, there was a strong relationship between [combined mass-media] and [target identification] ($r = -0.75$; $p>0.001$). The latter variable was excluded for consistency with the accident model.

There were 16 variables remaining in the model at this stage. The efficiency of the model was improved as described for accidents. The following variables were consequently excluded: [duration – 30 to 200 days], [dvd-video-cinema], [target consultation], [scale – national], [previous campaign] and [paid media].

The remaining ten variables met statistical recommendations for the ratio of variables studied to outcome effects in the sample. Meta- regression analyses were therefore run using the following explanatory variables [duration - 0 to 29 days], [initial seatbelt use], [personal influence], [on-road], [showing non-shocking consequences], [risk of harm], [enforcement], [humour], [limited area] and [combined mass-media] as explanatory variables. Ln odds ratios calculated from seatbelt use effects were used as the dependent variable.

Analyses were performed using the procedure described for accidents, i.e. both fixed and random effects meta-regression were performed, first on all reported effects, then on aggregated effects from each campaign and finally a multilevel-meta regression model. The results are shown in Table 8.6-8.9.

The effects of the explanatory variables in the models can be interpreted in the same way as for the accident model[40], apart from the variable [initial seatbelt use]. Initial seatbelt use is measured as the percentage of car occupants using seatbelt before the campaign started, and is hence a continuous variable (the others are categorical). This means that the *b* describes the expected change in seatbelt usage for one percentage change in initial seatbelt usage. A negative sign means that campaigns are less effective when the initial seatbelt usage percentage is high and hence most effective when the initial seatbelt usage rate is low. Because we have mixed continous and categorical variables, we report in addition to the *b* values the *beta*-coefficient for each explanatory variable. This describes the expected change in *standard deviation* for ln odds ratio for seatbelt usage when the explanatory variables increase with one standard deviation. The beta-coefficient is most meaningful for continuous explanatory variables, in our case initial seatbelt use, but can also be seen as an indication of the relative importance of categorical explanatory variables.

Table 8.6 Results from fixed effect and random effect meta-regression of the final seatbelt model. Effect sizes from all outcomes (N=122) is the dependent variable. Variables, their coefficients, expected percentage change in seatbelt usage and probabilities are shown.

	Fixed effect model			**Random effect model**		
	b (% change in seatbelt use)	*Beta*	*p-value*	*b (% change in seatbelt use)*	*Beta*	*p-value*
Constant	.42 (52%)		<.001	.60 (82%)		<.001
Initial seatbelt use	-.005 (-0.5%)	-.52	<.001	-.005 (-0.5%)	-.53	<.001
Duration - 0 to 29 days	.06 (6%)	.10	<.001	-.02 (-2%)	-.02	.800
Personal influence	.07 (7%)	.12	<.001	.05 (5%)	.05	.500
On-road	.10 (11%)	.19	<.001	.14 (14%)	.12	.085
Non-shock. consequences	-.10 (-10%)	-.15	<.001	-.05 (-5%)	-.05	.555
Risk of harm	.14 (15%)	.17	<.001	.19 (20%)	.17	.030
Enforcement	.03 (3%)	.06	<.001	.07 (8%)	.07	.381
Humour	-.18 (-16%)	-.32	<.001	-.16 (-14%)	-.13	.060
Limited area	.36 (43%)	.48	<.001	.33 (39%)	.35	<.001
Combined mass-media	.03 (3%)	.06	<.001	-.05 (-5%)	-.05	.539
R^2	.62		<.001	.52		<.001
Q	5146.3		<.001	122.1		.222
Mean Effect Size	0.26 (30%)		<.001	0.42 (52%)		<.001

[40] One difference is that a positive sign of the coefficients is interpreted as a beneficial effect of the explanatory variable in question, since a positive coefficient means an increase in seatbelt usage rate.

The results of the fixed effects model in Table 8.6 show that all of the ten explanatory variables are significantly related to seatbelt usage rate, and together account for 62% of the total variance in differences in campaign effects. In the random effect model, the sign and size of the coefficients are very similar to the fixed effects model, but only five if the explanatory variables have significant or near-significant effects. Of these, the following three are found to increase the effect of seatbelt usage campaigns; using *on-road measures,* addressing *risk of harm* and carrying out the campaign within a *limited area*. On the other hand, the use of *humour* is not found to be beneficial and is related to decreased effect. Finally, *initial seatbelt use* is quite strongly related to campaign effectiveness[41]; the higher the initial usage, the lower the effect of the campaign.

Since the 122 reported effects were nested within 84 campaigns[42], all outcome measures reported from the same campaign were aggregated into one single effect per campaign, and fixed and random effects meta-regression analyses re-run using the same explanatory variables. The results, presented in Table 8.7, are similar to those in Table 8.6, in terms of the size and direction of the relationships. However, having fewer effect sizes to analyse reduces (in particular the random effects model's) power to detect statistical significant relationships. Hence, fewer effects are significant at a 5% level or near-significant in the random effects model (this did not affect the fixed effects model). In particular, initial seatbelt usage and conducting the campaign within a limited area seem to have the strongest[43] and most significant effects. Addressing risk of harm and using humour have both near significant relationships with campaign effects in the random effects model.

Finally, Table 8.8 presents the results from the multilevel meta-regression analysis with the explanatory variables as level 2 predictors. The results are nearly identical to the random effect model presented in Table 8.7, exempt for the tests of statistical significance being (even) more conservative.

[41] The size of the beta-coefficient (-.53) is an indication of the strength of the relationship. The closet this coefficient is to either -1 or +1, the stronger the relationship. Removing initial seatbelt usage from the model resulted in a 17% decrease in explained variance in both models.

[42] 64 campaigns reported only one outcome (change in seatbelt usage). Of the other 20 campaigns, 11 campaigns reported 2 outcomes, 3 reported 3 outcomes, 4 reported 3 outcomes, 5 reported 2 outcomes and 1 reported 6 outcomes. The aggregation into one single effect per campaign was done in the same manner as described for aggregation of campaign effects upon accidents.

[43] Removing [initial seatbelt use] reduced the amount of explained variance in the random effect model from 50% to 34%, and further removing [limited area] from the model reduced the amount of explained variance to 22%. This suggested that the former factor explained 16% of the variance in campaign effects, and the latter 12%. The effect of removing these two variables was the same in the fixed effects model.

Table 8.7. Results from fixed effect and random effect meta- regression of the final seatbelt model. Aggregate effect sizes from all campaigns (N=84) is the dependent variable. Variables, their coefficients, expected percentage change in seatbelt usage and probabilities are shown.

	Fixed effect model			**Random effect model**		
	b (% change in seatbelt use)	*Beta*	*p-value*	*b (% change in seatbelt use)*	*Beta*	*p-value*
Constant	.42 (52%)		<.001	.64 (90%)		<.001
Initial seatbelt use	-.005 (-0.5%)	-.54	<.001	-.005 (-0.5%)	-.54	<.001
Duration – 0 to 29 days	.06 (6%)	.11	<.001	.04 (4%)	.04	.659
Personal influence	.08 (8%)	.13	<.001	.02 (2%)	.02	.797
On-road	.11 (12%)	.21	<.001	.11 (12%)	.10	.264
Non-shock. Consequences	-.10 (-10%)	-.15	<.001	-.09 (-9%)	-.08	.349
Risk of harm	.13 (14%)	.17	<.001	.18 (20%)	.15	.085
Enforcement	.04 (4%)	.08	<.001	.11 (12%)	.11	.363
Humour	-.19 (-17%)	-.33	<.001	-.18 (-16%)	-.15	.077
Limited area	.36 (43%)	.49	<.001	.31 (36%)	.33	<.001
Combined mass-media	.03 (3%)	.06	<.001	-.04 (-4%)	-.03	.739
R^2	.625		<.001	.504		<.001
Q	4841.4		<.001	83.5		.187
Mean Effect Size (average change in seatbelt usage)	.26 (30%)		<.001	0.44 (55%)		<.001

Table 8.8. Results from multilevel meta-analysis. 122 effect sizes (level 1) nested within 84 campaigns (level 2). Level 2 predictors, their coefficients, percentage change in seatbelt usage and probabilities are shown.

	Model 1 (Intercept only)		**Model 2 (With predictors)**		
	b (% change)	*p-value*	*b (% change)*		*p-value*
Intercept (Grand mean)	.44 (55%)	<.001	.64	(90%)	<.001
[Initial seatbelt use]			-.005	(-0.5%)	<.001
[Duration - 0 to 29 days]			.04	(4%)	.868
[Personal influence]			.02	(2%)	.815
[On-road]			.11	(12%)	.301
[Non-shock. consequences]			-.09	(-9%)	.386
[Risk of harm]			.18	(20%)	.111
[Enforcement]			.11	(12%)	.400
[Humour]			-.18	(-16%)	.103
[Limited area]			.31	(36%)	<.005
[combined mass-media]			-.04	(-4%)	.755
Random effect σ^2_u	0.216	<.001	.120		<.001
Chi-square	19565.4		6308.6		<.001*
R square (level 2)			0.44		

*Change in Chi-square Model1-Model2

To sum up, the results from the different models are quite similar in terms of the direction and strength of the relationship between the explanatory factors and variation in campaign seatbelt usage effect. The set of ten factors in the final seatbelt model together accounted for about 50 per cent of the variance in seatbelt use changes accompanying campaigns.

Of the individual explanatory factors, there was a large contribution from [initial seatbelt use]. The lower the initial usage rate, the higher the effect of the campaigns tended to be. This result was very robust, and analyses suggested that about 16 per cent of the variance in campaign effect on seatbelt use was explained by initial seatbelt use alone.

Carrying out campaigns within a [limited area] was also beneficial; analyses suggesting it explained 12 per cent of effect variance in campaign seatbelt usage change.

Other significant/near-significant contributions were made by addressing the risk of harm to self or others and using humour. These were, respectively, beneficial and detrimental to seatbelt use.

Of the other factors in the model use of [personal communication] and [on-road] delivery was again beneficial but not significant (in the random effect models). Enforcement too was not a significant factor, possibly due to the presence in the database of many seatbelt studies with low usage rates carried out in a limited-area, which could well have been effective without enforcement. The final seatbelt model can be summarized thus:

Figure 8.2. Final model of relative influence of campaign factors on increasing seatbelt use. Factors in italics are included even though they do not appear to be significant according to the model, because they approach significance and have theoretical grounds to be included.

+	▪ Low initial seatbelt use ▪ Limited area ▪ *On-road delivery* ▪ *Use of enforcement*
—	▪ Using humour ▪ *Non-shocking consequences*
neutral	▪ Short duration ▪ Use of combined mass media ▪ Personal influence

CHECKS ON THE FINAL SEATBELT MODEL

Several attempts were made during development of the CAST seatbelt model to improve its statistical properties (identify outliers, improve distribution normality and so on). While reservations remain about the properties of the model, as explained below, it still is considered to be a fair representation of the data in the CAST database.

1. Checks were made for **collinearity and outliers**. There were indications of outliers (maximum Mahalanobis distance = 67), but statistical tests indicated that they did not influence the final model description unduly (maximum Cook's Distance = 0.45). It was assumed that homoscedasticity was minimised by the weighting applied in the meta-regression models.

2. As discussed previously, a plot of fixed effects weights vs. ln odds ratios for seatbelt effects shows **skew**. To improve the normality of distribution those campaign effects with odds ratios above 1.4 (6 studies) were excluded. and the final model run on the reduced data set. The main conclusions of the model (Figure 8.2) were the same. Concerns do remain, however, about the skewness in the normal distribution of seatbelt effects caused by the presence of quite a few effective campaigns based on low n numbers. Attempts to treat the effects as two groups (highly effective, small-scale campaigns versus the rest) did not identify factors that were significantly influential, and so we reported the final model based on all effects.

3. To further test the final model, the variable set was used to analyse the data file (based upon the aggregation into 84 campaign effects) after it had been split on the basis of whether initial seatbelt use was above (n = 38) or below (n = 46) 40 per cent. We did this because we were also concerned about the applicability of explanatory factors drawn from studies with very low **initial seatbelt use** rates, when seat belt wearing rates in the EU are quite high, varying between 45 and 95 per cent for front seat occupants (ETSC, 2003). Further, because there was a positive link between year and initial seatbelt use ($r = +.53$; $p < .001$), the analysis also addresses to some extent the **relevance of our analysis to contemporary campaigns**. The model generated similar results for each split, with some differences (Figure 8.3). Based on this analysis we can conclude that the findings about initial seatbelt use, limited area, humour and addressing risk of harm are quite robust.

Figure 8.3. Meta-regression of final seatbelt model after aggregated data split according to whether initial seatbelt use was above or below 40 per cent.

Link with seatbelt use	*All effects (n = 84)*	***Initial seatbelt use < 40%** (n = 46)*	***initial seatbelt use > 40%** (n = 38)*
+	Low initial seatbelt use Limited area Addressing risk of harm On-road delivery Enforcement	Low initial seatbelt use Limited area Addressing risk of harm On-road delivery Enforcement	Low initial seatbelt use Limited area Addressing risk of harm
-	Using humour Showing non-shocking consequences	Using humour Showing non-shocking consequences	Using humour Combined mass media
neutral	Short duration Personal influence Combined mass media	Short duration Personal influence Combined mass media	Short duration Personal influence Enforcement On-road delivery Showing non-shocking consequences

4. The variable [**stripped mass-media**] was tested in place of [combined mass-media] in the final model, as described for the accident model checks in 8.2.1. There was very little difference in results.

5. As explained for the accident checks, we wanted to check for any differences in effects derived from **controlled and non-controlled** studies. Added as a variable to the final seatbelt model, [control] has no influence on variance in seatbelt effect (beta = 0.019; p = 0.83 according to fixed effects using uncorrected standard errors). Neither does it affect any of the conclusions drawn from the final model variables. A split file analysis based on control was not performed since there were only 35 controlled seatbelt effects making the ratio of variables to cases far too high.

SUMMARY OF FACTORS INFLUENCING CAMPAIGN EFFECTS ON SEATBELT USE

We were unable to account for all of the important potential influences on seatbelt use effects. We were limited ultimately by the information available in evaluation studies. Important considerations omitted encompass both the content and delivery of the campaigns. In particular, accounting for social norms, use of emotion or shock, and the advantage of consulting a sample of the target group before the campaign was not possible. For statistical reasons we were also unable to compare risk of detection, as a factor addressed in campaign content. together with the use of enforcement to assist delivery of

that content. The limitations of delivery considerations discussed for accidents also apply to the final seatbelt model.

With these reservations in mind, along with the assumptions highlighted in 8.1, we conclude the following.

The lower the *initial level of seatbelt use*, the more likely a campaign is to increase seatbelt use. This result is robust and the relationship is strong. Campaigns carried out within a *limited area*, where the target of the campaign can be captured very well, are associated with an increase in seatbelt use.

Addressing the *risk of harm* appears to be beneficial, and this may well be related to the fact that using *humour* in a campaign appears to be relatively detrimental to seatbelt use levels.

Delivery of messages at the *roadside*, attempts to use *personal influence* in a campaign and *enforcement* were not associated with significant variation in seatbelt usage change (in the random effect models). However the sign of the relationships were in the expected direction. One reason for the lack of significant results might be explained in part by the strength of the limited area variable in this model – and also the possibility that campaigns carried out in limited areas actually employed roadside delivery and some form of personal influence without this being registered as a campaign characteristic. Another reason could be that all these explanatory variables are coded as yes/no, and does thus not give any information about the amount and intensity of the campaigns' use of these measures.

Campaign *duration* and use of combined *mass-media* had rather weak effects in the models.

Checks on the robustness of the model included a split-half analysis based on an initial seatbelt use level of 40 per cent. Associations between campaign effect and initial seatbelt use, limited area, addressing the risk of harm and humour remained for both halves of the analysis, despite the reduced number of effects on which each half is based.

9. CONCLUSIONS FROM ANALYSIS OF THE CAST DATABASE

Road safety campaigns work
Based on a conservative meta-analytical summary of an extensive database, we can say that not only do road safety campaigns work, but they work well. This assertion is based on a number of different outcome measures, including risk comprehension (16 per cent increase), yielding behaviours (37 per cent increase), speeding (16 per cent reduction), seatbelt use (25 per cent increase) and accident reduction (9 per cent decrease).

We do not find that campaigns are significantly effective at reducing drink-driving behaviour, possibly because there is a remarkably wide range of effects evident for campaigns using this measure. A summary of 39 results also shows no significant overall effect of campaigns on attitude.

Several factors can be associated with campaign effectiveness
Bivariate (subgroup) analyses were reported mainly to provide the campaign designer with an idea of the size of overall effect that can be expected for certain types of campaigns. Such analyses give crude indications that effects on both accident counts and seatbelt use are on the whole better for those campaigns identifying a target group, using personal communication, combining emotional and rational content, addressing subjective social norm. There is no evidence from bivariate analyses of a link between campaign effect and campaign scale, or the use of humour. The influence of enforcement on campaign effect is discussed in a separate section below.

Results from bivariate analyses were used to help construct models for more sophisticated multivariate analyses (meta-regression). Testing of these models suggests several factors are associated with improved effect of campaigns on accident levels. Those that are amenable to manipulation by campaign designers are on-road delivery, personal communication and accompanying enforcement.

Several factors were also identified as being associated with positive campaign effects on seatbelt use. Of those amenable to manipulation by campaign designers, carrying out a campaign in a limited area appears to be most beneficial. There were also indications that on-road delivery and use of enforcement are beneficial to seatbelt use outcomes.

Campaigns achieving intimacy and immediacy could be more effective
Bivariate analyses suggest that campaigns using some form of personal communication with the target audience have somewhat improved effects. This finding is consolidated by the multivariate analyses, primarily for campaign effects on of accidents. The variable personal communication, according to our definition, encompassed the use of lesson, seminars or other sessions delivered in person, two-way or group discussions with a peer, teacher, safety expert or deliverer of campaign media, or personally addressed letters. The use of a personal element or the use of other people as channels for the delivery of the campaign message implies that an increased level of **intimacy** with the target could be important, possibly

because it increases the likelihood that the target attends and processes the campaign message (Vaa et al., 2004; also see Work Package Deliverable 1.4).

According to multivariate models, on-road delivery is also positively associated with beneficial campaign effects, especially if the campaign targets a reduction in accidents. On-road delivery involves the use of billboards, or other fixed or variable message signs placed at the side of the road. This suggests that delivery of the message to drivers at a place that is in terms of space and time proximal to the target behaviour is beneficial in terms of campaign effect. In other words, achieving **immediacy** in the delivery of a campaign message might be important.

In terms of accident counts, short campaign duration is also beneficial according to multivariate analyses, and this also implies a sense of immediacy. In shorter, more intense campaigns, the message may be more likely to be received at a time that is proximal to carrying out of the target behaviour.

Taken together, intimacy and immediacy suggest that those campaigns that make the target person think carefully about the message in the context of the driving behaviour would tend to be more successful at achieving effects during or after the campaign. In this light, it is interesting to consider that the carrying out of campaigns in a limited area, such as an organisation or car park, was positively associated with improvements in seatbelt use, since such campaigns would presumably be more likely to use personal communication and deliver the message to the driver at a time that is proximal in space and time to the context of the targeted driving behaviour.

But mass media methods are still important

Lack of evidence linking mass-media methods such as television or radio to improvements in campaign effects (whether accident counts or seatbelt use is the outcome measured) is in line with the idea that intimacy and immediacy are beneficial to campaign outcomes. To this we can add those findings from bivariate analysis (not reported here) that selection for campaigns using mass-media results in either the same or reduced overall effects, across several behavioural outcome measures. While we cannot conclude from bivariate analyses that use of mass-media has less effect at the local level, than at regional or national levels, it does seem that within a small area, where a much more focused campaign is possible, those methods which are difficult to carry out on a larger scale (e.g. rewards, pledge cards) are particularly effective and therefore worth considering.

Targets receive campaign messages delivered by television or newspapers in ways that are often removed in space and time from the actual (on-road) target behaviour. These targets would also often be in situations in which it would be easier to avoid attending to the campaign message. Despite this, we recommend that campaign designers continue to consider mass-media for the following reasons.

Firstly, mass-media methods have a clear advantage over personal communication in terms of audience reach.

Secondly, the effects of mass media methods 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. The vast majority of campaign evaluations in the CAST database do not measure this sort of social change but rather tend to deal with more immediate change, occurring either during or following soon after a campaign. In this sense it is important to distinguish between short-term change of an on-road behaviour – change well represented in the CAST database – and the longer term broader change of a society's collective mindset. Notwithstanding psychometric deficiencies in attitude measures in evaluation studies, the lack of evidence for overall campaign effects on attitudes supports the idea that the evaluation studies included in the CAST database are geared towards measuring a more a short term, possibly temporary, change in behaviour.

Thirdly, there are some mass-media forms that do deliver campaign messages in an immediate, if not intimate, way. Examples are in-car radios and posters on the back of buses. Some would add on-road methods to this list, as they consider these too to be a form of mass-media.

A reasonable conclusion then is that national-level campaign designers more concerned with immediate, shorter term effects, should try to identify methods that have the potential to reach large audiences in a context relevant to target behaviour (immediacy), or supplement mass-media methods with more immediate or intimate delivery methods.

The results support the use of enforcement

When other factors are controlled for, our results indicate that the use of measures to enforce the campaign message can lead to improved reductions in accident counts (meta-regression results), even though the use of enforcement was not significant according to the random effects meta-regression model. The results still approached significance in these models, and given the very conservative tests of statistical significance applied in random effects meta-regression, we conclude that the use of enforcement as an accompanying measure can be beneficial. This result is also supported by other meta-analyses (e.g. Erke, Goldenbeld and Vaa, 2009).

The weaker effects of enforcement on campaign outcomes measured using seatbelt usage rates is a little surprising, but may be partly explained by the presence of a large number of small-scale seatbelt campaigns in the sample, which use 'limited-area' delivery methods that may be highly effective without the need for accompanying enforcement measures. We should also bear in mind the broad definition of 'campaigns' used to gather material for our meta-analysis and realise that enforcement may still be important for achieving optimal effectiveness of those seatbelt campaigns conducted on larger populations, in which mass media must be used to deliver the message. It is also important to stress that no conclusions about how well enforcement can assist campaigns in reducing speeding, drink-driving or other behaviours can be drawn from a disparate sample of seatbelt campaigns.

To further explore differences between effects of enforcement according to the CAST bivariate analyses and previous bivariate analyses, we conducted a meta-analysis using only those studies included in CAST originating from the GADGET project (Delhomme et al., 1999) and INFOEFFEKT projects (Vaa et

al., 2004). As detailed in section 7.3, it is possible to describe an overall effect for a large number of campaign effects using four possible methods. In this respect it is important to note that CAST uses a different method than GADGET. If one compares the results from all four methods across the three groups of studies used as the basis for GADGET, INFOEFFEKT and CAST, they are actually very similar (Appendix A). The weaker effects of enforcement seen in the CAST bivariate analyses appear at least in part, then, to be due to the vagaries of the particular method used to derive an overall effect.

In drawing conclusions about enforcement from CAST, it is also important to remember that we counted secondary enforcement activities as 'enforcement' effects, and did not attempt to distinguish between different types of enforcement activity. Furthermore, the use of enforcement as an accompanying measure was coded as an "either or" characteristic, meaning that neither the amount nor intensity of enforcement was registered. The lack of this kind of grading could be an explanation of the lack of significant results.

In summary, our results can not be used as evidence that enforcement, and especially primary enforcement activities, do not improve the effects of media campaigns. On the contrary, they provide perhaps the strongest evidence from meta-analysis to date that enforcement improves accident reduction figures when other factors are controlled for. Accompanying enforcement is suggested by a meta-regression model, in which a number of other influential factors are controlled for, as a factor enhancing campaign effects on accidents. At the same time, our results also suggest that the effectiveness of those small scale campaign activities carried out in limited areas might be worth considering, where it is possible to carry out such activity.

Humour (and fear) should not detract from the central message

The indicated beneficial effect of addressing risk of harm in a campaign fits somewhat with the detrimental effect of the use of humour on outcome (according to multivariate analysis). One inference that can be drawn is that unless campaigns are taken seriously, their messages will not be attended to. An additional possibility is that the use of humour in a campaign detracts attention of the target group from the actual message. We suggest therefore that campaign planners intending to use humour should carefully check that it does reduce the likelihood that the central message is fully and seriously appreciated. Humour, like fear, may play a positive role in campaigns, but only when other conditions of campaign effectiveness are not undermined by its use.

Concerning fear, we were unable to find a sufficient number of studies evaluating campaigns explicitly using fear to develop and test a 'use of fear' variable. This was surprising given the high profile that fear campaigns seem to have, at least among researchers in the field. Conclusions about fear should not be drawn directly from the 'use of harm' variable developed here since addressing the use of harm does not necessarily invoke fear.

Explore link between initial behaviour level and campaign outcome
The testing of meta-regression models suggests that a main determinant of campaign effectiveness is the initial level of that safety outcome. This relationship was clear and strong in the case of seatbelt use, where initial use levels were also positively linked with the year of the campaign study. We would therefore like to suggest that, at least in the case of seatbelt use, it is worth exploring the presence of a ceiling for campaign effects, and the reasons for its existence. Do target populations become more resistant to safety campaigns over time (campaign fatigue?) or do other cultural or social effects come into play as initial levels of desired behaviour increase in the target population?

Links between campaign effects on accidents and behaviour
One might have expected that meta-regression would identify a single set of campaign factors that are responsible both for successful changes in road user behaviour and accidents. One might go further and predict that that same set of factors should be more strongly related to behavioural outcomes. We contend, however, that the two models presented in CAST should not be considered in this way for several reasons.

Firstly, the accident effects used to generate the accident model are of campaigns whose themes are not only seatbelt use, but drink-driving, speeding, general road safety behaviours and so on. Change in each of these behaviours may be more or less susceptible to influence by different campaign factors. For instance, on-road messages might be less likely to discourage drink-driving, which begins off-road (presumably), than speeding or seatbelt wearing.

The situation is further complicated by the fact that the same percentage and intensity of change in each of these behaviours will have different effects on accident counts.

Secondly, as we have noted, the models are generated from groups of studies that differ systematically. For example, studies using seatbelt effects tend, more than those using accident counts, to evaluate campaigns carried out on restricted populations. Restricted populations are amenable to different measures, and can be influenced in different ways.

Further explore on-road delivery methods
As we have noted, on-road delivery was consistently effective according to multivariate analysis of accident outcomes. Since the variable comprises billboard, variable message sign and fixed message sign use, it would be worth exploring further to see whether it is active feedback or simply delivery of a message in a driving context that is important. The use of roadside posters or posters mounted on public transport could be added to expand the variable. This also applies for campaigns addressing seatbelt usage. The effect of on-road delivery was in the expected direction (i.e. increasing seatbelt usage), but this effect was rather weak and non-significant in the random effects meta-regression models.

Addressing social norms, emotional-rational content, and clear identification of target group could also be important

Subgroup analyses in this report give insight into some factors that we were unable to enter into meta-regression analysis, due to the lack of descriptive data available in the evaluation studies. As we have said, those campaigns in our sample that identify target groups or combine emotional and rational information, are associated with better accident and seatbelt use outcomes. Bivariate analyses also indicate that addressing subjective social norms could be linked to beneficial campaign outcomes. This is a variable that merits further investigation as a predictor of campaign effect. In particular, little is known about the role of *descriptive* social norms in campaigns (Jens Schade, personal communication), a factor which is almost never considered by campaign designers.

The use of a reward, or a pledge system in which people 'pledge' to change behaviour, were also associated with better seatbelt use outcomes, as was consultation with a sample of the target for the development of campaign content. Although it should be remembered that the above associations were found when other factors are not controlled for, they remain potentially important. Just because they could not be included as candidate models for meta-regression analysis does not mean they are not meaningful influences on campaign effect.

Consider limitations of meta-analysis when interpreting results

A further reason for including subgroup analyses in this report is as a complement to the meta-regression data, which is generated on the basis of several important assumptions. We have attempted to delineate theoretical and methodological limitations throughout this report and while it is not our aim to repeat them here, 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.

In this vein, it is worth outlining the breadth of the database of road safety campaign evaluations itself. It contains 433 effects, including 133 seatbelt use effects and 119 accident effects. Most of the studies are drawn from scientific journals and institute reports, and they summarize campaigns carried out across the last 30 years, mainly in Europe and USA. The most common campaign themes in the database are seatbelts (34 per cent of effects for seatbelt campaigns), drink-driving (24 per cent) and speeding (15 per cent). The types of campaign tending to use accidents as a measure of campaign effect are drink-driving (36 per cent of accident effects are for drink-drive campaigns) and speeding (22 per cent) campaigns. There is some evidence that different countries tend to focus on certain themes; in particular many seatbelt-themed campaigns are American (69 per cent) and many drink-drive campaigns Australian (43 per cent). The campaigns cover a wide range scope and duration, from less than one hundred to over a million people and from 1 day to several years. In accepting the conclusions drawn from this data, one must assume that we can learn from the regularities apparent amongst such disparate information.

Evaluation studies should include more information

Finally, we would like to recommend improvements to reduce the number of those limitations that have their roots in the original campaign evaluation studies.

First, we are unable to say anything about the effects of campaigns on the *intensity* of safety outcomes such as speed, because such measures were few and far between. It would be interesting to evaluate known speeds for a known number of observations, but studies either state average recorded speed in the absence of other information, or give percentage speeding (i.e. frequency not *intensity* of behaviour). In this study we decided to focus on the more available percentage speeding measure, which does not tell us anything about how much speeds have slowed.

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

Only a minority (31 per cent) of the campaigns had an explicit basis, and less than one in five were based on a psychosocial theory or model. This hindered our ability to assess the influence of a particular model on campaign effects.

The factors ‘emotional content’ and ‘social norms’ remained necessarily vague while links to detailed campaign content was not available in the evaluation studies. We would particularly like to encourage the explicit documentation of the emotional content and treatment of social norms by a campaign. Cross-referencing campaign documentation would help.

We were particularly surprised at the lack of campaigns evaluated that were explicitly shocking. Very few conclusions could be drawn about the ‘use of fear’ in campaigns, and we would encourage more evaluation of these types of campaigns.

Evaluation studies often fail to report exactly when, in relation to the campaign period, measurements are taken. This prevented an analysis of possible deterioration of campaign effects over time. Furthermore, the reporting by many studies of measurements in simple terms (often as ‘before’ or ‘after’ values), encouraged the comparison of all before-after effects, even though the after effect may be taken 1 day or several months following campaign completion. We strongly recommend that after measures be reported as levels x days following campaign initiation and/or completion, not only in the results sections but in all Figures and Abstracts.

Finally, attempts are made in Deliverable 1.4 of this Work Package to consider different models of road user behaviour, and how they can be used to predict behaviour, and in turn influence accidents. For instance, the Theory of Planned Behaviour would predict that campaigns should influence behaviour through intentions to behave and, in turn, through attitudes, perceived outcome and social norms. In our experience it is difficult to evaluate these models using the current evaluation studies. Although a poor

relationship was found between attitudes and campaigns, the psychometric qualities of the attitude measures used by evaluation studies are on the whole very poor. In addition, study evaluations simply do not provide enough information about affect or perceived social norms to enable their evaluation. These findings would seem to strengthen the call for standardisation of campaign evaluations based on constructs representing a wide range of the most feasible models.

10. REFERENCES

10.1 GENERAL REFERENCES

Christensen P. (2003) Topics in meta-analysis. TOI Report. 692/2003, Institute of Transport Economics. Oslo.

Delhomme P., Vaa, T., Meyer, T., Harland, G., Goldenbeld, C., Järmark, S., Christie, N. & Rehnova, V. (1999) Evaluated road safety media campaigns: an overview of 265 evaluated campaigns and some meta-analysis on accidents. European Commission RDT programme of the 4th Framework Programme. Contract No.RP-97-SC.2235

Duval S. & Tweedie R. (2000) A non-parametric 'trim-and-fill' method of assessing publication bias in meta-analysis. Journal of the American Statistical Association 95**,**

Duval S. & Tweedie R. (2000) Trim and fill: a simple funnel-plot-based method of testing and adjusting for publication bias in meta-analysis. Biometrics 56**,** pp. 455-463.

Elliott, B. (1993) Road safety campaigns: a meta-analysis. Report CR118, Department of Transport and Communications, Federal Office of Road Safety. ISBN 0642512523.

Elvik, R. & Vaa, T. (2004) The handbook of road safety measures, Elsevier. London.

Erke, A., Goldenbeld, C. & Vaa, T. (2009) *Good practice in the selected key areas:*
Speeding, drink driving and seat belt wearing: Results from meta-analysis. PEPPER (Police Enforcement Policy and Programmes on European Roads) Deliverable 9, http://www.pepper-eu.org/docs/pepper_documents/PEPPER_D9_WP4_20090305.pdf

ETSC (2003). European Transport Safety Council. Cost-effective EU Transport safety Measures, Brussles, 2003. Accseed on-line at http://www.etsc.be/documents/costeff.pdf on 1 November, 2008.

Everitt, B.S. (2003) Modern medical statistics. Arnold Pubs, London.

Hox, J. (2002) Multilevel analysis. Techniques and Applications. Lawrence Erlbaum Associates, Inc.

Lipsey, M.W. & Wilson, D.B (2001) *Practical Meta-analysis*. Applied Social Research Methods Series, Volume 49. Sage Publications.

Tabachnikk, B.G. & Fidell, L.S. (2007). Using multivariate statistics. 5th Edition. Pearson Education Inc.

Vaa T., Assum, T., Ulleberg, P. & Veisten, K. (2004) Effekter av informasjonskampanjer på atferd og trafikkulykker - forutsetninger, evaluering og kostnadseffektivitet. TØI Rapport. 727/2004 Institute of Transport Economics. Oslo.

Wilson, D. B. (2005). *SPSS macro for meta-analytic regression analysis* (MetaReg.sps). http://mason.gmu.edu/~dwilsonb/ma.html
Light, R.J. & Pillemer, D.B. (1984). Summing up. The science of reviewing research. Cambridge, MA: Harvard University Press.

10.2 REFERENCES FROM WHICH EFFECTS WERE TAKEN

Agent K.R., Green E.R. & Langley R.E. (2002) Evaluation of Kentucky's "You Drink and Drive. You Lose." campaign. KTC-02-28/KSP1-02-2F University of Kentucky College of Engineering, Kentucky Transportation Center. Kentucky, USA.

Agent K.R., Green E.R. & Langley R.E. (2003) Evaluation of Kentucky's "Buckle up Kentucky: it's the law and it's enforced" campaign. KTC-03-26/KSP1-03-1I Kentucky Transportation Center, University of Kentucky. USA.

Amundsen A.H., Elvik R. & Fridstrøm L. (1999) Virkninger av "Sei ifrå" kampanjen i Sogn og Fjordane på antall skadde og drepte i trafikken. TØI-rapport. 425/1999 Institute of Transport Economics. Oslo.

Anund, A. (1996). Zon-96. Accessed via CAST partner Per Henriksson using structured questionnaire.

Armour M., Monk K., South, D. & Chomiak, G. (1985) Evaluation of the 1983 Melbourne random breath testing campaign - casualty accident analysis. 8/85 Road Traffic Authority, Victoria Transport. Victoria, Australia.

Austin J., Hackett S., Gravina N. & Lebbon A., (2006) The effects of prompting and feedback on driver's stopping at stop signs. Journal of Applied Behavior Analysis 39**,** pp. 117-121.

Baldcock M.R.J.&.Bailey T.J. (1997) Random breath testing in South Australia: operation and effectiveness, 1996. 3/97 Office of Road Safety, Adelaïde.

Behrensdorff I. & Arndal L. (1994) Det er ikke nok at være hårdkogt. Husk cykelhjelm. Accessed via CAST partner M. Møller using questionnaire.

Behrensdorff I. & Johansen H.J. (1992) Kampagnen "Stop spritkørsel - osse de andres" Spørgeskemaundersørgelse blant 17-21 årige. Arbeidsnotat nr. 3 Rådet for Trafiksikkerhedsforskning. Denmark.

Bill N. (1992) Safety-belt use and moto-vehicle-related injuries -- Navajo Nation, 1988-1991. Morbidity and Mortality Weekly Report 41**,** pp. 705-708.

Blomberg R.D., Preusser D.F. & Ulmer R.G. (1987) Deterrent effects of mandatory license suspension for DWI conviction. National Highway Traffic Safety Administration. DOT HS 807 138 US Department of Transportation. US.

Blomberg R.D., Preusser D.F, Hale, A. & Leaf, W.A. (1983) Experimental field test of proposed pedestrian safety messages. DOT HS 4 00952 U.S. Department of Transportation, National Highway Traffic Safety Administration.

Boots K. & Midford R., (1999) "Pick-a-skipper": an evaluation of a designated driver program to prevent alcohol-related injury in a regional Australia city. Health Promotion International 14**,** pp. 337-345.

Boughton C.J., South D.R., (1985) Evaluation of a drink driving publicity campaign. In Proceedings, 9th International Conference on Drugs and Traffic Safety, San Juan, Puerto Rico, 1983.

Bowler M.A., Torpey S. (1988) Community road safety program (Latrobe Valley): first 18 months of operation. 1988/03 Road Traffic Authority, Victoria.

Britt J.W., Bergman A.B., Moffat J., (1995) Law enforcement, pedestrian safety, and driver compliance with crosswalk laws: Evaluation of a four-year campaign in Seattle. Washington. Transportation Research Record 148**,** p. 5.

Cameron M., Newstead, S., Diamantopoulou, K. & Oxley, P. (2003) The interaction between speed camera enforcement and speed-related mass media publicity in Victoria. Report 201 Monash University Accident Research Centre. Victoria, Australia.

Cameron M., Cavallo A. & Gilbert A. (1992) *Crash-based evaluation of the speed camera program in victoria 1990-1991*. 1992/42 Australian Road Research Board, Department of Justice, Vic Roads.

Cameron M., Diamantopoulou, K., Mullan, N., Dyte, D. & Gantzer, S. (1997) Evaluation of the country random breath testing and publicity program in Victoria, 1993-1994. Report 126, Monash University Accident Research Centre. Victoria, Australia.

Cameron M.H. (1993) Evaluation of transport accident commission road safety television advertising. Report 52, Monash University Accident Research Centre. Melbourne, Victoria, Australia.

Christie N. & Downing C.S. (1989) The effectiveness of the 1988 police national motorway safety campaign. Report 268 Transport and Road Research Laboratory.

Clayton M. & Helms B., (2006) Active prompting to decrease cell phone use and increase seat belt use while driving. Journal of Applied Behavior Analysis 39**,** pp. 341-349.

Cope J.G., Grossnickle W.F. (1986) An evaluation of three corporate strategies for safety belt use promotion. Accident Analysis and Prevention 18**,** pp. 243-251.

Cope J.G., Moy S.S. & Grossnickle W.F. (1988) The behavioural impact of an advertising campaign to promote safety belt use. Journal of Applied Behavior Analysis 21**,** pp. 277-280.

Cope J.G., Smith G.A., Grossnickle W.F. (1986) The effect of variable-rate cash incentives on safety belt use. Journal of Safety Research 17**,** pp. 95-99.

Cox B.S., Cox A.B. & Cox D.J. (2000) Motivating signage prompts safety belt use among drivers exiting senior communities. Journal of Applied Behavior Analysis 33**,** pp. 635-638.

Decina L.E., Temple M.G. & Dorer H.S. (1994) Increasing child safety-seat use and proper use among toddlers: Evaluation of and enforcement and education program. Accident Analysis and Prevention 26**,** pp. 667-673.

Diamantopolou K. & Cameron M. (2002) An evaluation of the effectiveness of overt and covert speed enforcement achieved through mobile radar operations. Report 187, Monash University Accident Research Centre. Victoria, Australia.

Dowling A.M. (1986) STOP-DWI: the first four years: an evaluation update. 980630 Institute of Traffic Safety Management and Research, conducted for The New York State Department of Motor Vehicles.

Drummond A.E., Sullivan G. & CAVALLO A. (1992) An evaluation of the random breath testing initiative in Victoria 1989-1990. Report 37, September 1992 Monash University Accident Research Centre. Victoria, Australia.

Ebel B., Koepsell T.D., Bennett E.E. & Rivara F.P. (2003) Use of child booster seats in motor vehicles following a community campaign. Journal of the American Medical Association 289**,** pp. 879-884.

Eddy J.M., Marotto M.S. & Beltz S.M. (1988) Analysis and effectiveness of a safety belt incentive program at CIGNA Corporation. American Journal of Health Promotion 2**,** pp. 131-138.

Elliott B. & South D.R. (1983) The development and assessment of a drink-driving campaign: a case study. CR26 Australian Office of Road Safety.

Elman D. & Killebrew T.J. (1978) Incentives and seat belts: changing a resistant behaviour through extrinsic motivation. Journal of Applied Social Psychology 8**,** pp. 72-83.

Fischer A.J. & Lewis R.D. (1984) Tunnel vision road safety campaign: Evaluation and research report. Vol 1 and 2. Department of Transport, South Australia.

Foss R.D., (1989) Evaluation of a community-wide incentive program to promote safety restraint use. American Journal of Public Health 79, pp. 304-306.

Fosser S. Christensen P. & Ragnøy A. (1992) Effekten av kampanjen "Hold avstand - 92". TØI-rapport 139 Institute of Transport Economics. Oslo.

Fosser S. (1984) Effektmåling av forsikringsbransjens relfeksaksjon i 1983. Resultater fra førmåling i 1982 og ettermåling i 1983. Arbeidsdokument. Arbeidsdokument av 4.1.1984, prosjekt O-717 Transport Økonomisk Institut. Oslo.

Geller E.S. Paterson L. & Talbott E. (1982a) A behavioral analysis of incentive prompts for motivating seat belt use. Journal of Applied Behavior Analysis 15, pp. 403-415.

Geller E.S. (1984) A delayed reward strategy for large-scale motivation of safety belt use: a test of long-term impact. Accident Analysis and Prevention 16, pp. 457-463.

Geller E.S. Johnson R.P., Pelton S.L., (1982b) Community-based interventions for encouraging safety belt use. American Journal of Community Psychology 10, pp. 183-195.

Geller E.S. (1989) Intervening to increase children's use of safety belts. Alcohol, Drugs and Driving 5, pp. 37-58.

Geller E.S. & Hahn H.A. (1984) Promoting safety belt use at industrial sites: an effective program for blue collar employees. Professional Psychology: Research and Practice 15, pp. 553-564.

Geller E.S., Kalsher M.J., Rudd J.R. & Lehman G.R. (1989) Promoting safety belt use on a university campus: an integration of commitment and incentive strategies. Journal of Applied Social Psychology 19, pp. 3-19.

Geller E.S. (1983) Rewarding safety belt usage at an industrial setting: tests of treatment generality and response maintenance. Journal of Applied Behavior Analysis 16, pp. 189-202.

Gemming M.G., Runyan C.W., Hunter W.W., Campbell B.J., (1984) A community health education approach to occupant protection. Health Education Quarterly 11, pp. 147-158.

Gibb,D.G. (1984) DWI countermeasures – DWI update. *The Police Chief, July,* 60-61.

Glad A. (1986) "Bedrebilist - 85". Resultater fra en evaluering av av aksjonen. TØI-rapport. Insititute of Transport Economics. Oslo.

Gordon P. (1989) Evaluation of an intervention to increase seat belt use. Family Medicine 21, pp. 458-460.

Graham S. (1996) State campaigns nab drunk drivers. *Traffic Safety, January-February,* 16-19.

Grant B.A. (1990) Effectiveness of feedback and education in an employer based seat belt program. Health Education Research 5**,** pp. 197-205.

Gundy,C.M. (1988). The effectiveness of a combination of police enforcement and public information for improving seat belt use. In V.G.Assen (Ed.), Road user behaviour: theory and research.

Hagenzieker M.P. (1991) Enforcement or incentives? Promoting safety belt use among military personnel in the Netherlands. Journal of Applied Behavior Analysis 24**,** pp. 23-30.

Harrison W. (1989) Evaluation of a publicity and enforcement campaign to breath test any drivers detected speeding at night. GR/89/06 Road Traffic Authority, Victoria.

Harrison W.A. (1988) Evaluation of a drink-drive publicity and enforcement campaign. 1988/02 Road Traffic Authority, Victoria.

Harte D.S. & Hurst P.M. (1984) Evaluation of operation checkpoint accident data. Road Traffic Safety Seminar 1984. Seminar Papers Volume 2 Road Traffic Safety Research Council. Wellington, New Zealand.

Haynes R.S., Pine R.C., & Fitch H.G. (1982) Reducing accident rates with organizational behaviour modification. Academy of Management Journal 25**,** pp. 407-416.

Heathington K.W., Philpot J.W. & Perry R.L. (1982) Impact of legislation and public information and education on child passenger safety. Transportation Research Record 844**,** pp. 62-70.

Herslund, M. (1994) Effektmåling at rådet for større færdselssikkerhets unodekampagne - videobaseret adfærdsstudie efteråret 1993. Accessed via CAST partner M. Møller using structured questionnaire.

Homel R., Carseldine D. & Kearns I. (1988) Drink-driving countermeasures in Australia. Alcohol, Drugs and Driving 4**,** pp. 113-144.

Horne,T.D., & Terry,C.D. (1983) GM's great seatbelt sweepstakes. *Traffic Safety, July/August,* 6-9; 28-30.

Hunter W.W. & Woodward A.R. (1985) Seat belt incentive programs: an extension to three North Carolina high schools. HSRC-PR142 University of North Carolina, Highway Safety Research Centre.

Höök, P. (1994) Stressa av trafiken. Trafiksäkerhetskampenjen sommaren 1992 i Boden, Luleå och Piteå. Accessed via structured partner Per Henriksson using questionnaire.

Håkansson, E. & Rydgren, H. (1981) För -och eftermätning av kunskaper och attityder hos personbilsförare i kampanjen "Jobbet bakom ratten". Trafiksäkerhetsverket.

Illinois Department of Transportation (2007). Evaluation of the 2006 Labor Day You Drink & Drive. You Lose. Campaign. July 24 – September 17, 2006. Technical Report, February 2007.

Johnson, R.P. & Geller E.S., (1984) Contingent versus noncontingent rewards for promoting seat belt usage. Journal of Community Psychology 13**,** pp. 113-122.

Jonah, B.A., Dawson, N.E:, MacGregor, C.G., Wilde, G.J.S. (1982a). Promoting seat belt use: a comparison of three approaches. 26th Annual Proceedings, American Association for Automotive Medicine, October 4-6, 1982, Ottawa, Canada.

Jonah B.A., Dawson N.E., Smith G.A. (1982b) Effects of a selective traffic enforcement program on seat belt usage. Journal of Applied Psychology 67**,** pp. 89-96.

Jonah B.A., Grant B.A., (1985) Long-term effectiveness of selective traffic enforcement programs for increasing seat belt use. Journal of Applied Psychology 70**,** pp. 257-263.

Kalfus G.R., Ferrari, J.R., Arean, P., Balser, D., Cotronea, R., Franco, M. & Hill, W. (1987) An examination of the New York mandatory seat belt law on a university campus. Law and Human Behaviour 11**,** pp. 63-67.

Kalsher M.J., Geller E.S., Clarke S.W. & Lehman G.R. (1989) Safety belt promotion on a naval base: a comparison of incentives vs. disincentives. Journal of Safety Research 20**,** pp. 103-113.

Kaye B.K., Sapolsky B.S. & Montgomery D.J. (1995) Increasing seat belt use through PI&E and enforcement: the thumbs up campaign. Journal of Safety Research 26**,** pp. 235-245.

Kello J.E., Geller E.S., Rice J.C. & Byant S.L. (1988) Motivating auto safety belt wearing in industrial settings: from awareness to behavior change. Journal of Organizational Behavior Management 9**,** pp. 7-21.

King M. (1987) Random breath testing operation and effectiveness in 1986 / Random breath testing operation and effectiveness in 1988. 2/87. Road Safety Division, Department of Transport. Australia.

Kirby,K.W. (2002). The effect of positive reinforcement on adolescent seat belt use. The Graduate College, University of Wisconsin.

Kjellström T., Henderson, C., Heffernan, J.-M., Rands, D., Dyer, R., Crowe, D., Johanson, L. & Sollitt-Morris, L. (1984) The planning, execution and evaluation of the child safety in cars campaign. pp. 185-203 Road Traffic Safety Research Council, Wellington New Zealand.

Kritz, L.B. & Widén, S. (1968) Säkerhetsbältens använding i personbilar våren 1968. Studier rörande effekten på användningsfrekvens av ökad information. Trafiksäkerhetsverket. Accessed via structured partner Per Henriksson using questionnaire.

LaForest, L. (1987) A short drinking/driving prevention campaign and its actual effects on the alcohol-induced accident rate: a target population study. Alcohol, Drugs and Traffic Safety – T86 pp. 475-478

Lane J.M., Milne P.W. & Wood H.T. (1984) Evaluation of a successful rear seat belt publicity campaign. Proceedings of the Australian Road Research Board 12(7), pp. 13-21

Lehman G.R. & Geller E.S. (1990) Educational strategies to increase children's use of safety belts: are extrinsic rewards necessary? Health Education Research 5**,** pp. 187-196.

Lewis R.J. & Hamm J.T. (1985) Seattle area HOV lanes: Innovations, enforcement and eligibility. Washington D.C. Transportation Research Record 1047**,** pp. 102-110.

Machemer, E. Runde, B. Wolf, U. Büttner, D. & Tücke, M. (1994) Delegierte belohnung und intensivierte verkehrsüberwachung im vergleich. Heft M48 Berichte der Bundesanstalt für Strassenwessen, Mensch und Sicherheit.

Maisey G.E. & Saunders C.M. (1981) An evaluation of the 1980/1981 Christmas/New Year traffic enforcement blitz. Research and Statistics Report No. 16 Research and Statistics Division, Road Traffic Authority. Perth, Western Australia.

Malenfant J.E., Van Houten, R. (1989) Increasing the percentage of drivers yielding to pedestrians in three canadian cities with a multifaceted safety program. Health Education Research 5**,** pp. 275-279.

Malenfant J.E., Van Houten, R. (1988) The effects of night-time seat belt enforcement on seat belt use by tavern patrons: a preliminary analysis. Journal of Applied Behavior Analysis 21**,** pp. 271-276.

Malenfant L., Wells J.K., Van Houten, R. & Williams A.F. (1996) The use of feedback signs to increase observed daytime seat belt use in two cities in North Carolina. Accident Analysis and Prevention 28**,** pp. 771-777.

Mann, A.-C. (2002) Lokalt hastighetsprojekt på Huddingevägen. Utvärdering av kampanjaktiviteter, attityd och beteenden, hösten 2001. Accessed via partner 'PER HENRIKSSON' using structured questionnaire.

Marosi K. & Nielsen H.S. (1997) 10 = 44. Evaluering af en hastighedskampagne fra forsommeren 1997. Rådet for Større Færdselssikkerhed & De lokale Færdselssikkerhedsudvalg.

McCaul K.A. & McLean A.J. (1990) Publicity, police resources and the effectiveness of random breath testing. Medical Journal of Australia 152**,** pp. 284-286.

McGregor, I. (1988) Evaluation of adult and child restraint programs (1988): a research report. 12/88 Division of Road Safety, Department of Transport.

McKnight A.J., McPherson K. & Hilburn B.G. (1988) Use of safety restraints by law enforcement officers following safety belt training and passage of a state-wide law. DOT HS 807 260 US Department of Transportation, National Highway Traffic Safety Administration.

McLean A.J., Kloeden C.N., McCaul K.A., (1991) Drink-driving in the general night-time driving population, Adelaide 1989. Australian Journal of Public Health 15**,** pp. 190-193.

Mercer G.W., Cooper P.J. & Kristiansen L. (1996) A cost/benefit analysis of a 5 month intensive alcohol-impaired driving road check campaign. 40th Annual Proceedings of the Association for the Advancement of Automotive Medicine, October 7-9, 1996, Vancouver, B.C: pp. 283-292

Mercer G.W. (1985) The relationships among driving while impaired charges, police drinking-driving road-check activity, media coverage and alcohol-related casualty traffic accidents. Accident Analysis and Prevention 17 **,** pp. 467-474.

Moe D., Sakshaug K. & Stene T.M. (1987) Evaluering av utforkjøringskampanjen 1986. SINTEF-rapport. STF63 A87006 SINTEF Samferdselseteknikk. Trondhiem, Norway.

Moe D. & Stene T.M. (1990) Evaluering av utforkjøringskampanjen 1989. SINTEF-rapport. STF63 A90003 SINTEF Samferdselsteknikk. Trondheim, Norway.

Morrow R. (1989) A school based program to increase seatbelt use. The Journal of Family Practice 29**,** pp. 517-520.

Mortimer R.G., Goldsteen R.W., Armstrong R.W. & Macrina D. (1990) Effects of incentives and enforcement on the use of seat belts by drivers. Journal of Safety Research 21**,** pp. 25-37.

Mortimer R.G. (1992) Extra enforcement and the use of seat belts by drivers in Illinois. Accident Analysis and Prevention 24**,** pp. 661-666.

Mulholland, E., Tierney, P. & Healye, D. (2005). Wipe-off 5: A Victorian social marketing campaign. Australian Road Safety Conference Research, Policing and Education Conference Proceedings, 2005..

Murry J.P., Stam A. & Lastovicka J.L. (1993) Evaluating an anti-drinking and driving advertising campaign with a sample survey and time series intervention analysis. American Statistical Association 88**,** pp. 50-56.

Møller, M. (2005) Respekt for fart - en spørgekskemaundersøgelse. Accessed via structured parnter M. Møller using questionnaire.

Nagatsuka Y. (1991) Effectiveness of model driving with a safety campaign sticker on the rear outside of the body of buses, trucks and taxis: an action research. Transportation Research Institute, Haifa. Proceedings of the 2nd international conference on new ways to improve road safety and quality of life (Tel Aviv, Israel, October 7-10) pp. 9-10

Nassipour M. (2004) Evaluation of the "Click It or Ticket" campaign during April-June 2004 mobilization in Illinois. Technical Report December 2004 Illinois Department of Transportation, Division of Traffic Safety, Evaluation Unit. Springfield, IL, USA.

Nimmer J.G. & Geller E.S, (1988) Motivating seat belt use at a community hospital: an effective integration of incentive and commitment strategies. American Journal of Community Psychology 16**,** pp. 381-394.

Nolen, S. & Johansen, R. (1993) Utvärdering av lokal hastighetskampanj i Mjölby, Sala och Sandviken. Accessed via CAST partner Per Henriksson using structured questionnaire.

Oei H. & Polak P.H. (1992) Effect van automatische waarschuwing en toezicht op snelheid en ongevallen. 92-93 Stichting Wetenschappelijk Onderzoek Verkeersveiligheid (SWOV). Netherlands.

Oei H. & Goldenbeld C. (1996) Snelheidscampagne 1995 in Eindhoven. R-96-61 Stichting Wetenschappelijk Onderzoek Verkeersveiligheid (SWOV). Netherlands.

Olsson (2003) Lokalt hastighetsprojekt på Nynäsvägen. Utvärdering av kampanjaktiviteter, attityd och beteenden, Våren 2003. Accessed via CAST partner Per Henriksson using structured questionnaire.

Preusser D.F. & Blomberg R.D. (1984) Reducing child pedestrian accidents through public education. Journal of Safety Research 15**,** pp. 47-56.

Provenção Rodoviária Portuguesa (2006) Euchires project 2004/2005 - Final report. Accessed via CAST partner Alain Areal using structured questionnaire.

Reznik R., Best J.B. & Morey S. (1984) Evaluation of the Australian Medical Association drink-driving campaign in Wollongong. Medical Journal of Australia 141**,** pp. 818-821.

Richtig gut ankommen. (2005). Accessed via CAST partner using structured questionnaire.

Roberts M.C. & Fanurik D. (1986) Rewarding elementary schoolchildren for use of safety belts. Health Psychology 5**,** pp. 185-196.

Roberts M.C., Fanurik D. & Wilson D.R. (1988) The community program to reward children's use of seat belts. American Journal of Community Psychology 16**,** pp. 395-407.

Robsertson L.S., Kelley A.B., O'Neill B., Wixom C.W., Eiswirth R.S. & Haddon W. (1974) A controlled study of the effect of television messages on safety belt use. American Journal of Public Health 64**,** pp. 1071-1080.

Rogers R.W., Rogers J.S. & Bailey J.S. (1988) Promoting safety belt use among state employees: the effects of prompting and a stimulus-control intervention. Journal of Applied Behavior Analysis 21**,** pp. 263-269.

Rood D.H., Kraichy P.P., Carman J.A., (1987) The effects of a selective traffic enforcement program on occupant restraint use in two upstate New York communities. Proceedings of the 31st Annual Conference for the American Association for Automotive Medicine, pp. 269-282.

Rooijers,T. (1988). Effects of different public information techniques in reducing driving speed. In Anonymous, *Road User Behaviour: Theory and Practice.* Second International Conference of Road Safety, Groningen, The Netherlands, August 1987.

Ross.H.L. (1987) Britain's crusade against drinking and driving. Journal of Studies on Alcohol 48**,** pp. 476-482.

Rudd J.R. & Geller E.S., (1985) A university-based incentive program to increase safety belt use: towards cost-effective institutionalization. Journal of Applied Behavior Analysis 18**,** pp. 215-226.

Rydgren, H. (1984a) För -och eftermätning bland äldre i kampanjen "Äldre oskyddade trafikanter". Accessed via structured partner Per Henriksson using questionnaire.

Rydgren, H. (1984b) För -och eftermätning bland körkotshavare i kampanjen "Äldre oskyddade trafikanter". Accessed via partner Per Henriksson using structured questionnaire.

Rydgren, H. (1981) Resultat från fö- och eftermätning i samband med TSVs trafiknykterhetskampanj 1979. Accessed via structured partner Per Henriksson using questionnaire.

Rydgren, H. (1982) Resultat från för- och eftermätning i samband med 1979/1980 information campaign on speed adaptation. Accessed via CAST partner Per Henriksson using structured questionnaire.

Rydgren, H. (1981a) Resultat från för -och eftermätning i samband med information i Örebro län om barns säkerhet i bil. Accessed via CAST partner Per Henriksson using structured questionnaire.

Rydgren, H. (1981b) Resultat från för -och eftermätning i samband med information i Örebro län om användning av baksätesbälten. Accessed via CAST partner Per Henriksson using structured questionnaire.

Sakshaug (2001). Evaluering av "Senk farten" - aksjonen sommeren 2000. Trondheim : SINTEF Bygg og miljøteknikk, 2000, ISBN 82-14-01453-0.

Sali G.J. (1983) Evaluation of Biose Selective Traffic Enforcement Project. Transportation Research Record 910**,** pp. 69-74.

Samdahl D.R. & Daff M.R. (1986) Effectiveness of a neighbourhood road safety campaign. 13th Proceedings of the Australian Road Research board, 1986, pp. 25-45

Saunders S. & Pine J. (1986) Seat belt education program - a model for public health settings. Health Education Quarterly 13**,** pp. 243-247.

Schlabbach, K. (1990) Improvement of traffic safety by local public relations campaigns. VTI rapport. 365A Proceedings of Road Safety and Traffic Environment in Europe in Gothernburg, Sweden, September 26-28, 1990 Swedish Road and Traffic Research Institute, Linköping, Sweden.

Simmonds A.G. (1981) The effects on vehicle speeds of roadside safety posters: Phase III. Traffic Engineering and Control 22**,** pp. 480-485.

Simons-Morton B.G., Brink S. & Bates D. (1987) Effectiveness and cost effectiveness of persuasive communications and incentives in increasing safety belt use. Health Education Quarterly 14**,** pp. 167-179.

Smith D.I., Maisey G.E. & McLaughlin K.L. (1990) Evaluation of the first year of random breath testing in Western Australia. pp. 93-106 Australian Road Research Board.

Spoerer E. (1989) Einflus von informationen zur verkehrssicherheit auf unfallbeteiligte kraftfahrer. Forschunsberichte der Bundesanstalt Strassenwesen, Bereich Unfallforschung. BAST.

Spolander, K. (1985) Effekter av kampanjer, debatt och opinionsbildning? Förändringar i föräldrarnas omsorg om barnens trafiksäkerhet. Resultat från två rikstäckande undersökningar om barn i åldrarna 4-12. Accessed via CAST partner Per Henriksson using questionnaire.

Spolander, K. (1988) Har trafikanterna nåtts av Trafiksäkerhetsverkets information om de andrade reglerna våren 1986. Accessed via partner Per Henriksson using structured questionnaire.

Statens trafiksäkerhetsverk (1977) Utvärdering av kampenjen Barn i trafiken. Accessed via CAST partner Per Henriksson using structured questionnaire.

Statens trafiksäkerhetsverk (1978) Vilt och trafik. Undersökning av kampanjeffekter. Accessed via CAST partner Per Henriksson using structured questionnaire.

Stead M., Tagg S., MacKintosh A.M., Eadie D., (2005) Development and evaluation of a mass media Theory of Planned Behaviour intervention to reduce speeding. Health Education Research 20**,** pp. 36-50.

Stene T.M. (1987) Evaluering av fotgjengerkampanjen 1987. SINTEF-rapport. STF63 A88006 SINTEF Samferdselseteknikk. Trondheim, Norway.

Stockholms trafiksäkerhetsförening (1995) Cykelhjälmsinformation på arbetsplatser. Accessed via CAST partner Per Henriksson using structured questionnaire.

Stockholms trafiksäkerhetsförening (1994) Resultat av kampanj för att öka bältesanvändningen bland förare av lätta lastbilar-firmabilar. Accessed via CAST partner 'PER HENRIKSSON' using structured questionnaire.

Strand, L.-G. (2000) Alkohol och bilkörning. Undersökning om ungdomar i Skåne 1994 och 1999. Accessed via CAST partner Per Henriksson using structured questionnaire.

Streff F.M., Molnar L.J. & Christoff C. (1992) Increasing safety belt use in a secondary enforcement state: evaluation of a three-county special enforcement program. Accident Analysis and Prevention 24**,** pp. 369-383.

Studsholt, P. (1990) Campaign against drunken driving among young drivers. 365A Proceedings of Road Safety and Traffic Environment in Europe in Gothenburg, Sweden, September 26-28, 1990 Swedish Road and Traffic Research Institute, Linköping, Sweden.

Stuster J.W. (1995) Experimental evaluation of municipal speed enforcement programs. DOT HS 808 325 National Highway Traffic Safety Administration, US Department of Transportation. USA.

Syme G.J., Seligman C., Kantola S.J. & MacPherson D.K. (1987) Evaluating a Television Campaign to Promote Petrol Conservation. Environment and Behavior 19**,** pp. 444-461.

Sävenhed, H., Brüde, U., Nygaard, B., Pettersson, E. & Thulin, H. (1996) Heja Halland. Utvärdering av en trafiksäkerhetskampanj. Accessed via CAST partner 'PER HENRIKSSON' using structured questionnaire.

Sørensen, K. (2005) Speedbusters. Trafikal holdningsændring til fart blant unge bilister i Ejby kommune. Accessed via CAST partner M. Møller using structured questionnaire.

Šedá, E. (2007) EUCHIRES 2006 – Final Report Czech Republic. Accessed via CAST partner E. Šedá using a structured questionnaire.

Taylor W.C. & Ahmed A.R. (1995) Evaluation of the "share-the-road" campaign. GLCTTR 71-95/01 Great Lakes Centre for Truck and Transit Research. MI, USA.

Taylor R.K. & Torpey S.E. (1984) Evaluation of a trial bassinet restraint loan scheme. Report 1981/22 Road Traffic Authority, Australia.

Thompson C. (1998) Results from the development and evaluation of the 1998 speed mass media community education campaign for Western Australia. Office of Road Safety, Transport Western Australia. pp. 84-87

Thyer B.A., Geller E.S., Williams M., Purcell E., (1987) Community-based "Flashing" to increase safety belt use. The Journal of Experimental Education 55**,** pp. 155-159.

Tuohey,M.H. 1982. Restraints program clicks at Kodak. *Traffic Safety, July-August,* 6-8.

Törnos, J. (1995) Intensifierad övervakning med utandningsprov. Försök i södra Sverige. 746 - 1995 Väg- och transport-forsikningsinstitutet. Linköping, Sweden.

Ulleberg P. Elvik R. & Christensen P. (2004) Evaluering av "Sei ifrå!" kampanjen i Telemark. TØI rapport 722/2004, Institute of Transport Economics, Oslo, Norway.

Ulleberg, P. & Christensen, P. (2007). Virker 'Sei ifrå!' filosofien? TØI rapport 881/2007. Institute of Transport Economics, Oslo, Norway. ISBN 82-480-0734-0.

Van Houten, R. and Nau, P.A. (1983). Feedback interventions and driving speed: a parametric and comparative analysis. *Journal of Applied Behavior Analysis*, **16**, 253-281.

Various (2006) CLOSE TO. Report on initiative to increase road safety through peer education in Czech Republic, accessed via CAST partner E S using a structured questionnaire.

Voas R.B., Holder H.D. & Gruenewald P.J. (1997) The effect of drinking and driving interventions on alcohol-involved traffic crashes within a comprehensive community trial. Addiction 92**,** p. S221-S236.

VTI rapport (1990). Rapport 365A, Proceedings of Road safety and traffic environment in Europe in Gothenburg, Sweden, September 26-28, 1990.

Vägverket (1997) Gående i trafiken 1994-96. Slutrapport. Accessed via CAST partner Per Henriksson using structured questionnaire.

Webster K., & Schnerring F., (1987) An evaluation of 40 Km/h speed limits. Forum Papers, Australian Transport Research Forum pp. 807-827. Queensland Department of Transport.

Weinstein N.D., Grubb P.D. & Vautier J.S. (1986) Increasing automobile seat belt use: an intervention emphasizing risk susceptibility. Journal of Applied Psychology 71**,** pp. 285-290.

Wells J.K., Preusser D.F. & Williams A.F. (1992) Enforcing alcohol-impaired driving and seat belt use laws, Binghamton, NY. Journal of Safety Research 23, pp. 63-71.

Wells J.K., Malenfant J.E., Williams A.F.,& Van Houten, R. (2000) Use of a community program to increase seat belt use among shopping center patrons in Charlotte, North Carolina. Journal of Safety Research 31, pp. 93-99.

Werner, K., Eveline, B., Erwin, W. & Martin, P. (2006) Erhebung der Verwendungsquoten von Sicherheitsgurten und Kindersitzen in Pkw: Evaluering der BMVIT-Kampagne. Accessed via CAST partner Karin Ausserer using structured questionnaire.

Whittam K.P., Dwyer W.O., Simpson P.W. & Leeming F.C. (2006) Effectiveness of a media campaign to reduce traffic crashes involving young drivers. Journal of Applied Social Psychology 36, pp. 614-628.

Williams A.F., Wells J.K., McCartt A.T. & Preusser D.F. (2000) "Buckle Up NOW!" An enforcement program to achieve high belt use. Journal of Safety Research 31, pp. 195-201.

Williams A.F., Hall W.L., Tolbert W.G. & Wells J.K. (1994) Development and evaluation of pilot programs to increase seat belt use in North Carolina. Journal of Safety Research 25, pp. 167-175.

Williams A.F., Reinfurt D. & Wells J.K., (1996) Increasing seat belt use in North Carolina. Journal of Safety Research 27, pp. 33-41.

Williams A.F., Preusser D.F., Blomberg R.D. & Lund A.K., (1987) Seat belt use law enforcement and publicity in Elmira, New York: a reminder campaign. American Journal of Public Health 77, pp. 1450-1451.

Williams M., Thyer B.A., Bailey J.S. & Harrison D.F. (1989) Promoting safety belt use with traffic signs and prompters. Journal of Applied Behavior Analysis 22, pp. 71-76.

Wise A. & Healey D. (1990) Evaluation of the 1989 "There's No Excuse, So Belt Up" restraint wearing campaign. GR/90-15 Roads Safety Division, VIC ROADS.

Wolfe A.C. (1983) Interim (two-year) evaluation report on the Oakland County alcohol enforcement / education project. UMTRI-63-12 University of Michigan Transport Research Institute. MI, USA.

Worden J.K., Waller J.A. & Riley T.J. (1987) The Vermont public education campaign in alcohol and highway safety: a final review and evaluation. Accident Analysis and Prevention 19, pp. 285-303.

Ytterstad B. & Wasmuth H.H., (1995) The Harstad Injury Prevention Study: evaluation of hospital-based injury recording and community-based intervention for traffic injury prevention. Accident Analysis and Prevention 27, pp. 111-123.

10.3 REFERENCES FROM WHICH EFFECTS WERE NOT ULTIMATELY USED

Cavallo A., Cameron M. (1992) Evaluation of a random breath testing initiative in Victoria 1990 & 1991. 39 Monash University Accident Research Centre. Victoria, Australia.

Caverson R.J.E., Douglas R.R., Gliksman L., Chuipka L., (1990) Community receptivity to a countermeasure designed to reward sober drivers. Health Promotion International 5**,** pp. 119-125.

Coffey M., Christie N., (1992) An evaluation of the 1991 police national motorway campaign. Barry rose law periodicals, east row, little London, Chichester. Police journal 10/12 *65***,** pp. 329-335.

Dussault C., (1990) Effectiveness of a selective traffic enforcement program combined with incentives for seat belt use in Quebec. Health education and research, theory & practice 5**,** pp. 217-233.

Edvardsson (1975) Bilbältesanvändningen i Sverige 1971-1975. Effekter av kampanjer och lagstiftning. Accessed via CAST partner.

Elder R.W., Shults R.A., Sleet D.A., Nichols J.L., Thompson R.S., Wajab W., (2004) Effectiveness of mass media campaigns for reducing drinking and driving and alcohol-involved crashes - A systematic review. American Journal of Preventitive Medicine 27**,** pp. 57-65.

Fox,L., & Holmlund,J. (1993). The effect of the Bucklebear character on promoting the use of safety seats and seat belts among children in selected day care centers in Albuquerque, New Mexico.

Freedman K., Lukin J., (1981) Increasing child restraint use in New South West Australia: the development of an effective mass media campaign.

Geller E.S., (1982) "Flash for life": Community-based prompting for safety belt promotion. Journal of Applied Behavior Analysis 18**,** pp. 309-314.

Geller E.S., Rudd J.R., Kalsher M.J., Streff F.M. & Lehman G.R. (1987) Employer-based programs to motivate safety-belt use: a review of short-term and long-term effects. Journal of Safety Research 18**,** pp. 1-17.

Geller E.S., Davis L., Spicer K., (1983) Industry-based incentives for promoting seat belt use: differential impact on white-collar versus blue-collar employees. Journal of Organizational Behavior Management 5**,** pp. 17-28.

Gosling R. (2007) THINK! Road safety campaign evaluation. Post drink drive campaign report. BMRB/RG/KB/45105228 British Market Resarch Bureau. UK.

Granström (2001) Lite lugnare tempo' Lokala hastighetsprojekt på Huddingevägen och Valhallavägen. Accessed via CAST partner.

Gregersen (1990) Trafiksäkerhetsåtgärder i Televerket. Ett jämförande forsök att minska skador och kostnader genom körträning, gruppsamtal, bonus och kampanj. Accessed via CAST partner.

Gregersen N.P., Brehmer B. & Moren B., (1996) Road safety improvement in large companies. An experimental comparison of different measures. Accident Analysis and Prevention 28**,** pp. 297-306.

Hansen (1996) Hold afstand - erfaringer med kampagner i Aalborg. Accessed via CAST partner.

Helander C.J. (1984) Intervention strategies for accident-involved drivers: an experimental evaluation of current California policy and alternatives. Journal of Safety Research 15**,** pp. 23-40.

Hunter W.W., Campbell B.J., Stewart J.R., (1986) " Seat Belts Pay Off": The Evaluation of a Community Wide Incentive Program. Journal of Safety Research 17**,** p. 31.

Johnsen (1992) Slå farten ned i Sønderjylland. Accessed via CAST partner.

Jonah B.A., Dawson N.E., MacGregor C.G., Wilde G.J.S., (1983) Promoting seat belt use: a comparison of three approaches. pp. 167-181 American Association for Automative Medicine.

Kjærgaard (1992) Rødkørselskampagne i Odense. Accessed via CAST partner.

Kossman I., (1995) A sociological perspective on different strategies of reward in urban traffic law enforcement. pp. 256-270

Mortimer R.G., (1991) Motivating seat belt use by incentives and enforcement. pp. 116-124

Nassipour M. (2006) Evaluation of the 2006 Labor Day *You Drink and Drive, You Lose.* campaign. Technical report February 2007 Illinois Department of Transportation, Division of Traffic Safety Evaluation Unit. Springfield, Illinois, USA.

National Road Safety Council (2005) Kampania pasów bezpiecze?stwa 'Ostatni wyskok' z serii "W??cz my?lenie".

Nilsson (1990) Annonserad hastighetsövervakning i Jönköpings och Hallands län.

Pigman J., Agent K.R., (1984) Impact evaluation of Lexington - Fayette County traffic alcohol program. Transportation Research Record 974**,** pp. 4-7.

Roberts M.C., Alexander K., Knapp L.G., (1990) Motivating children to use safety belts: a program combining rewards and "Flash for Life". Journal of Community Psychology 18**,** pp. 111-119.

Roberts M.C., Turner D.S., (1986) Rewarding parents for their children's use of safety seats. Journal of Pediatric Psychology 11**,** pp. 25-36.

Rolfe J., (1994) Speeding down the highway - long distance truck drivers and drug use. pp. 127-134.

Rydgren (1978) Mätning av effekter hos målgruppen huvudskyddsombud av kampanjen 'Til och från arbetet'. Accessed via CAST partner.

Schuster (2002) Projektevaluation 'Verkehrssparen Langenlois'. Accessed via CAST partner.

Scottish Executive Social Research (2006) Road safety drink-drive 2006 - post-campaign evaluation. Scottish Executive. UK.

Scottish Executive Social Research (2004) Scottish Executive evaluation on the 2003-2004 festive drink drive campaign. Scottish Executive. UK.

Solomon M.G., Nissen W.J. & PREUSSER D.F. (1999) Occupant Protection Special Traffic Enforcement Program Evaluation. DOT HS 808 884 U.S. Department of Transportation, National Highway Traffic Safety Administration.

Spolander K., (1989) How to reduce speeding? The results of combined measures. Nationalfoereningen foer trafiksaekerhetens fraemjande, Kista, Sweden. Accessed via CAST partner.

Stockholms trafiksäkerhetsförening (1993) Kepsprojektet 1993. Rapport från arbetsgruppen. Accessed via CAST partner.

System Three Scotland (1996) Evaluation of Speedwatch: a study of the 1995 speedwatch campaign. The Scottish Office Central Research Unit.

Systeme Three Social Research Unit. (1996) Evaluation of young driver cinema advert. The Central Research Unit, Edinburgh.

Systeme Three Social Research Unit. (1997) The effectiveness of leaflets in road safety. The Scottish Office.

Tarawneh T.M., Singh V.A., McCoy P.T., (1996) Investigation of effectiveness of media advertsising and police enforcement in reducing red-light violations. Transportation Research Record 1693**,** pp. 37-45.

Trivector (1988) Feedbackskyltars hastighetsdämpende effekt - analys av en kampanj i Göteborg. Accessed via CAST partner.

Van Houten R., Nau P.A., Marini Z., (1980) An analysis of public posting in reducing speeding behaviour on an urban highway. Journal of Applied Behavior Analysis 13**,** pp. 383-395.

Van Houten R., Nau P.A., (1983) Feedback interventions and driving speed: A parametric and comparative analysis. Journal of Applied Behavior Analysis 16**,** pp. 253-281.

Wojtowicz G.G., Peveler L.A., Eddy J.M., Waggle S.B., Fitzhugh E.C., (1992) The Midfield High School safety belt incentive program. Journal of School Health 62**,** pp. 407-410.

ZUTSHI I., CAVALLO A. (1989) *Evaluation of the " keep left unless overtaking " regulation*. 1989/02 Road Safety Division, Vic Roads, Victoria.

Richtig gut ankommen (2005). Accessed via CAST partner.

The Eurobob camaign 2006/2007 (draft) (2007). Accessed via CAST partner.

APPENDIX A –ENFORCEMENT AND CAMPAIGN EFFECT

Comparison of bivariate analyses of effect of enforcement accompanying campaigns on accident counts, for those studies included in CAST that derive from the GADGET and INFOEFFEKT projects. (Small differences between these results and those reported in GADGET and INFOEFFEKT are present because (i) not all studies were included in CAST, due to reconsideration of evaluation studies after CAST criteria, and (ii) there are possibly differences in interpretation by researchers on respective projects as to what constitutes enforcement activity.)

Table A1. Results from meta-analysis of studies based on accident counts in CAST database.

		Model used to describe effect			
Description	Degrees freedom	Random effects	Fixed effects	Random effects + publication bias	Fixed effects + publication bias
CAST, all (accident counts)	119	**-12** (-15; -9)	**-11** (-12; -9)	**-9** (-12;-6)	**-9** (-11;-8)
CAST, enforcement	78	**-12** (-16; -9)	**-12** (-13; -11)	**-12** (-15;-8)	**-12** (-13;-11)
CAST, no enforcement	31	**-11** (-17; -4)	**-6** (-8; -3)	**-10** (-16;-2)	**-5** (-8;-3)
GADGET, all	76	**-13** (-17; -10)	**-13** (-14; -11)	**-11** (-14;-6)	**-10** (-12;-9)
GADGET, enforcement	53	**-15** (-19; -11)	**-14** (-15; -12)	**-12** (-16;-7)	**-11** (-13;-11)
GADGET, no enforcement	20	**-8** (-15; -1)	**-7** (-10; -4)	**-7** (-14; 0)	**-6** (-8;-3)
INFOEFFEKT, all	83	**-14** (-18; -10)	**-13** (-14; -12)	**-14** (-18;-10)	**-13** (-14;-12)
INFOEFFEKT, enforcement	60	**-17** (-21; -12)	**-14** (-16; -13)	**-15** (-20;-10)	**-14** (-15;-12)
INFOEFFEKT, no enforcement	20	**-8** (-15; -1)	**-7** (-10; -4)	**-7** (-14;0)	**-6** (-10;-5)

Table A1 shows that the lack of a significant difference between those campaign effects accompanied or not accompanied by enforcement activities in the CAST bivariate analyses is due to choice of the random effects model. In GADGET, a fixed effects model was used. Most of the studies used in GADGET are present in the CAST database, and re-analysis of these studies using a fixed-effects model (without adjusting for publication bias) confirms a significant benefit for accompanying enforcement using the GADGET studies. If we had relied on the same model in CAST, we would have drawn the same conclusion. In other words, according to the fixed effects model, whether or not we adjust for publication bias, there is a significant difference in overall effect for those campaigns accompanied (12% reduction in accidents) and not accompanied (6% reduction) by enforcement.

APPENDIX B – INDIVIDUAL EFFECTS FROM CAMPAIGN EVALUATION STUDIES

First author / organisation / project title	year	project identity number	ln odds ratio	weight (fixed effects)	ln odds x weight	outcome measure
Behrensdorff	1994	1	0,55	148,35	81,9	other
Behrensdorff	1994	1	0,37	120,95	44,48	other
not stated	2005	2	-0,16	714,89	-116,24	accidents
not stated	2005	2	-0,33	13,38	-4,42	accidents
Provenção Rodoviária Portuguesa	2006	3	-0,02	632,47	-14,61	seatbelts
Stockholms trafiksäkerhetsförening	1995	4	0,16	54	8,87	other
Stockholms trafiksäkerhetsförening	1994	6	0,42	236,1	98,86	seatbelts
Statens trafiksäkerhetsverk	1978	7	-0,58	57,91	-33,49	risk
Statens trafiksäkerhetsverk	1978	7	-0,28	57,91	-16,26	attitudes
Statens trafiksäkerhetsverk	1978	7	0,2	57,91	11,79	speed
Statens trafiksäkerhetsverk	1978	7	0,07	57,91	4,17	knowledge
Statens trafiksäkerhetsverk	1977	8	-0,08	130,79	-10,49	accidents
Statens trafiksäkerhetsverk	1977	8	0,13	569,32	75,66	attitudes
Spolander	1988	9	0,05	125	6,19	knowledge
Spolander	1988	9	-0,64	125	-80,09	knowledge
Vägverket	1997	10	-0,09	5,49	-0,5	accidents
Rydgren	1981	11	0,23	94,04	21,59	seatbelts
Rydgren	1981a	11	-0,04	49,87	-2,17	attitudes
Rydgren	1981a	11	0,08	49,87	4,14	knowledge
Rydgren	1981a	12	0,46	75	34,46	seatbelts
Rydgren	1981a	12	2,83	203,75	577,27	seatbelts
Rydgren	1981a	12	0,1	75	7,48	attitudes
Rydgren	1981a	12	0,32	75	23,83	risk
Kritz	1968	13	0,02	438,75	9,08	seatbelts
Kritz	1968	13	-0,1	207,3	-21,3	seatbelts
Kritz	1968	13	0,02	443,34	10,68	seatbelts
Kritz	1968	13	0,19	119,94	22,76	seatbelts
Sävenhed	1996	16	0,13	588,49	76,78	accidents
Sävenhed	1996	16	-0,47	216,44	-101,26	accidents
Sävenhed	1996	16	-0,64	33,7	-21,51	accidents
Nolen	1993	17	-0,05	96,49	-4,72	attitudes
Nolen	1993	17	0	96,49	0	knowledge
Rydgren	1981b	18	-0,09	60,34	-5,25	drink-driving
Rydgren	1981b	18	-0,34	42,67	-14,36	drink-driving
Rydgren	1981b	18	0,23	66,63	15,11	attitudes
Rydgren	1981b	18	0,1	58,6	5,82	attitudes
Rydgren	1981b	18	0,09	60,34	5,55	risk
Rydgren	1981b	18	0,06	42,67	2,52	risk
Rydgren	1981b	18	0,38	60,34	23,11	attitudes
Rydgren	1981b	18	0,36	42,67	15,56	attitudes
Mann	2002	21	0,06	76,95	4,57	attitudes
Höök	1994	24	-0,2	2,48	-0,5	accidents
Höök	1994	24	-0,05	19,49	-1	accidents
Höök	1994	24	-0,09	43,29	-3,75	accidents
Rydgren	1982	25	0,01	151,72	1,58	attitudes
Spolander	1985	26	0,7	664,38	463,64	other

First author / organisation / project title	year	project identity number	ln odds ratio	weight (fixed effects)	ln odds x weight	outcome measure
Spolander	1985	26	0,69	664,38	458,01	seatbelts
Spolander	1985	26	-0,03	551,24	-14,7	other
Spolander	1985	26	0,04	509,8	20,23	attitudes
Håkansson	1981	27	-0,02	69,12	-1,57	attitudes
Håkansson	1981	27	-0,07	44,05	-3,04	attitudes
Håkansson	1981	27	0,07	69,12	4,63	knowledge
Håkansson	1981	27	0,05	44,05	2,18	knowledge
Anund	1996	28	0	10,5	0	speed
Anund	1996	28	0	8,75	0	speed
Anund	1996	28	0	7,75	0	speed
Rydgren	1984a	29	-0,43	31,36	-13,44	attitudes
Rydgren	1984a	29	-0,06	28,08	-1,71	attitudes
Rydgren	1984a	29	-0,01	31,36	-0,32	attitudes
Rydgren	1984a	29	0,06	28,08	1,61	attitudes
Rydgren	1984b	30	0,11	56,12	6	other
Rydgren	1984b	30	0	43,62	0,17	other
Rydgren	1984b	30	0,22	55,5	12,24	attitudes
Rydgren	1984b	30	0,28	43,62	12,23	attitudes
Olsson	2003	31	-0,1	50,33	-5,12	attitudes
Olsson	2003	31	-0,06	50,33	-2,93	speed
Törnros	1995	32	-0,1	75,44	-7,17	accidents
Törnros	1995	32	-0,03	299,07	-7,75	attitudes
Törnros	1995	32	0,1	300,09	28,97	attitudes
Törnros	1995	32	0,1	192,38	18,56	risk
Törnros	1995	32	0,33	299,36	99,73	drink-driving
Strand	2000	33	-0,18	132	-24,07	drink-driving
Strand	2000	33	0	136,83	0	drink-driving
Strand	2000	33	-0,22	270,96	-59,35	attitudes
Strand	2000	33	-0,04	270,96	-10,84	attitudes
Various	2006	34	0,06	38,96	2,22	attitudes
Various	2006	34	0,06	38,96	2,16	attitudes
Various	2006	34	0,21	38,96	8,19	attitudes
Šedá	2007	35	0,26	259,95	66,82	seatbelts
Šedá	2007	35	0,15	259,95	38,11	seatbelts
Šedá	2007	35	0,46	259,95	120,26	seatbelts
Šedá	2007	35	0,5	259,95	130,17	seatbelts
Šedá	2007	35	0,28	259,95	71,67	seatbelts
Šedá	2007	35	0,03	259,95	7,32	recall
Šedá	2007	35	-0,52	259,95	-134,86	recall
Werner	2006	37	0,04	3949,74	147,82	seatbelts
Werner	2006	37	0,05	1744,69	78,66	seatbelts
Werner	2006	37	-0,05	95,42	-4,32	seatbelts
Werner	2006	37	0,14	313,78	45,13	seatbelts
Werner	2006	37	0,03	83,33	2,7	seatbelts
Werner	2006	37	0,06	83,33	4,93	risk
Werner	2006	37	0,03	83,33	2,39	attitudes
Sørensen	2005	39	-1,16	1,01	-1,18	accidents
Studsholt	1990	40	-0,71	13,06	-9,23	accidents
Studsholt	1990	40	-0,55	15,13	-8,35	accidents
Møller	2005	41	0,28	221,75	62,16	risk
Møller	2005	41	0,93	221,75	207,18	risk
Herslund	1994	44	-0,04	1710,43	-60,02	other
Hunter	1985	48	2	1015,87	2035,04	seatbelts

First author / organisation / project title	year	project identity number	ln odds ratio	weight (fixed effects)	ln odds x weight	outcome measure
McKnight	1988	49	0,97	46,93	45,43	seatbelts
McKnight	1988	49	0,33	75	25	knowledge
McKnight	1988	49	0,12	75	8,7	attitudes
Kjellström	1985	50	0,21	216	45,83	seatbelts
Kjellström	1985	50	0,83	161,5	133,83	seatbelts
Webster	1987	51	-0,16	90,48	-14,63	speed
Webster	1987	51	0,1	93,62	9,35	attitudes
Webster	1987	51	0,44	93,62	41,37	other
Blomberg	1983	52	-0,69	75,94	-52,64	recall
Blomberg	1983	52	0,1	191,77	18,28	recall
Blomberg	1983	52	1,97	268,64	528,17	knowledge
Blomberg	1983	52	0,23	2227,35	523,07	other
Blomberg	1983	52	-0,2	218,86	-44,18	accidents
Boughton	1985	53	-0,29	61,66	-17,79	drink-driving
Boughton	1985	53	0,05	115,51	5,62	other
Boughton	1985	53	0,11	172,17	19,7	other
Mercer	1996	54	0,14	113,1	16,16	attitudes
Mercer	1996	54	-1,07	259,17	-276,75	drink-driving
Jonah	1982a	55	0	207,56	-0,43	other
Laforest	1987	57	-0,28	19,37	-5,43	accidents
Gibb	1984	60	-0,08	714,44	-54,95	accidents
Grant	1990	62	0,18	79,26	14,38	seatbelts
Malenfant	1989	63	2,02	52,5	106,17	other
Malenfant	1989	63	0,48	162,5	77,75	other
Malenfant	1989	63	0,41	175	70,96	other
Rood	1987	65	-0,01	65	-0,81	seatbelts
Rood	1987	65	-0,09	65	-6,05	seatbelts
Christie	1989	67	0,15	100,27	14,7	other
Christie	1990	67	0	230,6	0	risk
McGregor	1987	68	-0,05	75	-3,56	attitudes
McGregor	1987	68	0,3	75	22,2	seatbelts
McGregor	1988	68	-0,21	75,12	-15,86	seatbelts
Baldcock	1997	69	-2,04	1000	-2043,13	drink-driving
Baldcock	1997	69	1,28	1000	1281,21	drink-driving
Baldcock	1997	69	-0,41	21,94	-9	accidents
Harrison	1988	70	0,81	100	81,17	recall
Harrison	1988	70	0,15	100	14,69	risk
Harrison	1988	70	-0,13	100	-13,45	seatbelts
Bowler	1988	71	0,06	49,05	2,75	seatbelts
Bowler	1988	71	0,75	382,3	288,15	recall
Bowler	1988	71	0,1	82,49	8,46	other
Taylor	1984	72	0,59	22,5	13,29	seatbelts
Harrison	1989	73	0,18	51	8,96	recall
Harrison	1989	73	-0,37	51	-18,95	drink-driving
Elliott	1983	75	4,21	127,15	535,28	recall
Elliott	1983	75	4,4	132,99	584,73	recall
Elliott	1983	75	0,07	129,69	8,57	other
Elliott	1983	75	0,07	113,16	8,18	other
Elliott	1983	75	0,05	115,51	6,27	other
Elliott	1983	75	-0,24	60,95	-14,81	drink-driving
Fischer	1984	76	1	25,3	25,2	recall
Fischer	1984	76	-0,05	31,43	-1,45	attitudes

First author / organisation / project title	year	project identity number	ln odds ratio	weight (fixed effects)	ln odds x weight	outcome measure
Fischer	1984	76	0,3	31,1	9,23	knowledge
Fischer	1984	76	1,11	10,91	12,11	accidents
Homel	1988	78	-0,44	547,84	-239,77	accidents
Homel	1988	78	-0,04	361,11	-16,05	drink-driving
Homel	1988	78	-0,47	333,33	-156,67	drink-driving
Ross	1987	79	-0,17	76,94	-12,8	accidents
McCaul	1990	80	-0,16	532,87	-84,87	drink-driving
Reznik	1984	81	-0,04	53,63	-2,15	other
Reznik	1984	81	-0,21	96,17	-19,75	accidents
Malenfant	1988	82	0	266,67	0	seatbelts
Malenfant	1988	82	0,11	800	84,93	seatbelts
Bill	1992	83	-0,22	158,24	-34,48	accidents
Foss	1989	84	0,21	1474,06	310,87	seatbelts
Williams	1987	85	0,19	100	19,24	seatbelts
Houten	1983	86	-0,32	746,67	-239,95	speed
Houten	1983	86	-0,17	646,15	-109,93	speed
Houten	1983	86	-0,14	700	-100,04	speed
Houten	1983	86	-0,5	595,83	-297,32	speed
Houten	1983	86	-0,45	674,07	-304,67	speed
Houten	1983	86	-0,47	444,44	-209,72	speed
Houten	1983	86	-1,01	631,58	-637,53	speed
Houten	1983	86	-0,38	661,76	-253,94	speed
Marosi	1997	88	0,29	466,38	134,17	attitudes
Marosi	1997	88	-0,01	466,38	-5,59	speed
Marosi	1997	88	2,81	75,51	212,01	knowledge
McLean	1991	89	-0,36	718,87	-262,13	drink-driving
Syme	1987	90	4,62	16,57	76,51	other
Syme	1987	90	3,6	75,51	271,55	recall
Syme	1987	90	0,14	75,51	10,23	attitudes
Lane	1984	92	0,53	2460	1299,84	seatbelts
Lane	1984	92	-0,08	479,3	-37,67	accidents
Jonah	1982	94	0,33	1595,66	523,03	seatbelts
Jonah	1982	94	0,86	43,21	37,09	risk
Jonah	1982	94	0,04	41,18	1,67	attitudes
Jonah	1985	95	0,02	1025,23	18,1	seatbelts
Jonah	1985	95	0,11	649,73	71,82	seatbelts
Graham	1996	96	-0,19	62,07	-11,68	accidents
Samdahl	1986	97	-0,02	227,36	-3,62	speed
Samdahl	1986	97	-0,03	257,59	-7,28	speed
Samdahl	1986	97	-0,04	357,47	-13,54	speed
Cameron	1992	98	-0,18	3015,73	-539,71	accidents
Malenfant	1996	102	0,1	9633,03	996,61	seatbelts
Williams	1989	103	0,5	732,9	366,66	seatbelts
Williams	1989	103	0,31	983,44	309,68	seatbelts
Rogers	1988	104	1,36	125,75	171,21	seatbelts
Rogers	1988	104	1,61	60	96,57	seatbelts
Rogers	1988	104	0,7	37,5	26,22	seatbelts
Decina	1994	105	0,11	403,33	43,94	seatbelts
Decina	1994	105	0,22	168,06	37,05	seatbelts
Decina	1994	105	0,29	335,39	98,85	risk
Decina	1994	105	0	504,4	-0,96	knowledge
Streff	1992	106	0,12	1127,43	133,01	seatbelts
Wise	1990	107	0,01	2647,92	35,19	seatbelts

First author / organisation / project title	year	project identity number	ln odds ratio	weight (fixed effects)	ln odds x weight	outcome measure
Wise	1990	107	0,09	4484,02	391,56	seatbelts
Mercer	1985	108	-0,11	109,51	-11,97	accidents
Tuohey	1982	109	0,26	1333,33	350,56	seatbelts
Mortimer	1992	110	0,31	31666,67	9735,81	seatbelts
Simmonds	1981	111	-0,25	8,29	-2,07	accidents
Simmonds	1981	111	-0,37	7,09	-2,63	accidents
Simmonds	1981	111	0,83	2,6	2,16	accidents
Horne	1982	112	0,56	1800	1014,44	seatbelts
Geller	1984	114	0,84	3036,33	2560,66	seatbelts
Cope	1986	115	0,34	2217,51	751,07	seatbelts
Ytterstad	1995	116	-0,34	208,5	-70,37	accidents
Britt	1995	118	0	484,37	-1,75	accidents
Britt	1995	118	0,24	300	70,92	other
Britt	1995	118	0,49	300	147,74	other
Britt	1995	118	0,24	300	70,92	other
Britt	1995	118	0,11	50	5,27	other
Lewis	1985	119	0,32	1018,4	325,89	other
Heathington	1982	120	0,55	2556,19	1403,52	seatbelts
Williams	1994	122	0,16	250	41,19	seatbelts
Williams	1994	122	0,66	250	164,19	seatbelts
Williams	1994	122	0,2	250	49,46	seatbelts
Williams	1996	123	-0,07	1400,89	-91,19	accidents
Williams	1996	123	0,22	5000	1115,72	seatbelts
Williams	1996	123	0,12	2592,59	301,41	seatbelts
Kalsher	1989	124	0,2	14948,18	3003,66	seatbelts
Kalsher	1989	124	0,29	6557,49	1919,18	seatbelts
Kaye	1995	126	-0,03	1028,57	-29,2	seatbelts
Mortimer	1990	127	0,28	10863,1	3050,97	seatbelts
Mortimer	1990	127	0,26	10863,1	2861,5	seatbelts
Mortimer	1990	127	0,15	10863,1	1599,07	seatbelts
Mortimer	1990	127	0,24	10863,1	2631,22	seatbelts
Cope	1986	128	1,51	175,53	265,78	seatbelts
Cope	1986	128	0,8	190,19	151,61	seatbelts
Cope	1986	128	0,92	140,8	129,23	seatbelts
Cope	1986	128	1,03	224,4	231,18	seatbelts
Cope	1986	128	1,14	149,86	171,05	seatbelts
Cope	1986	128	0,91	214,83	195,45	seatbelts
Kalfus	1987	129	-0,01	619,82	-7,19	seatbelts
Geller	1982	130	0,43	361,26	156,38	seatbelts
Gordon	1989	131	0,49	26,86	13,1	seatbelts
Morrow	1989	132	0,29	128,12	37,54	seatbelts
Geller	1983	133	0,51	886,18	456,31	seatbelts
Geller	1984	134	0,54	1424,77	765,08	seatbelts
Geller	1984	134	1,63	764,3	1247,01	seatbelts
Elman	1978	135	1,22	135	164,43	seatbelts
Geller	1989	136	0,19	368,63	70,86	seatbelts
Geller	1983	137	0,12	341,61	40,24	seatbelts
Geller	1983	137	0,38	99,69	37,69	seatbelts
Eddy	1988	138	0,43	253,19	108,29	seatbelts
Eddy	1988	138	0,01	17,5	0,1	attitudes
Hagenzieker	1991	139	0,18	1893,6	341,52	seatbelts
Hagenzieker	1991	139	0,06	1447,67	79,81	seatbelts
Hagenzieker	1991	139	0,08	232,58	18,3	seatbelts

First author / organisation / project title	year	project identity number	ln odds ratio	weight (fixed effects)	ln odds x weight	outcome measure
Hagenzieker	1991	139	0,31	695,1	215,97	seatbelts
Roberts	1988	142	1,05	905,9	947,23	seatbelts
Geller	1982	143	0,74	211,55	156,38	seatbelts
Geller	1982	143	0,78	71,81	55,84	seatbelts
Thompson	1998	144	0,08	83,58	6,69	speed
Thompson	1998	144	-1,8	29,27	-52,6	knowledge
Thompson	1998	144	-0,05	29,27	-1,43	attitudes
Thompson	1998	144	0,41	29,27	11,87	risk
Roberts	1986	146	0,35	270,86	94,67	seatbelts
Roberts	1986	146	1,34	142,67	191,14	seatbelts
Geller	1989	148	0,68	1811,32	1225,38	seatbelts
Geller	1989	148	0,35	130,42	46,08	seatbelts
Geller	1989	148	0,34	290,01	97,58	seatbelts
Rooijers	1987	149	0	8,88	0,01	speed
Rooijers	1987	149	-0,02	8,88	-0,18	speed
Rooijers	1987	149	-0,26	8,5	-2,21	speed
Rooijers	1987	149	-0,06	8,5	-0,49	speed
Gundy	1987	150	0,28	2000	558,56	seatbelts
Gundy	1987	150	0,15	466,67	68,65	seatbelts
Gundy	1987	150	0,32	175	56,2	recall
Robertson	1987	151	-0,04	376,82	-16,14	seatbelts
Cope	1988	152	0	184,55	0	seatbelts
Cope	1988	152	0,82	324,25	266,04	seatbelts
Preusser	1984	153	0,25	4454,69	1129,3	other
Preusser	1984	153	1,9	268,64	511,14	knowledge
Thyer	1987	154	0,62	262,5	161,49	seatbelts
Rudd	1985	156	0,28	10472,34	2968,24	seatbelts
Johnson	1984	157	0,32	483,05	153,83	seatbelts
Johnson	1984	157	0,44	303,28	132,03	seatbelts
Weinstein	1986	158	0,23	47,14	10,81	seatbelts
Weinstein	1986	158	-0,04	12,5	-0,56	knowledge
Simons-Morton	1987	159	0,03	19,89	0,6	seatbelts
Simons-Morton	1987	159	0,01	17,42	0,12	seatbelts
Simons-Morton	1987	159	0,67	19,54	13,18	seatbelts
Simons-Morton	1987	159	0,91	35,9	32,6	seatbelts
Saunders	1986	160	0,94	55,76	52,66	seatbelts
Gemming	1984	161	0,46	250	114,88	seatbelts
Nimmer	1988	162	0,69	786,6	541,44	seatbelts
Lehman	1990	163	0,27	113,68	31,11	seatbelts
Lehman	1990	163	0,34	113,68	38,67	seatbelts
Lehman	1990	163	0,5	113,68	56,33	seatbelts
Kello	1988	165	1,4	35,25	49,45	seatbelts
Nagatsuka	1991	167	-1,32	4,73	-6,25	accidents
Schlabbach	1990	168	0,11	271,9	30,44	accidents
Fosser	1984	169	-0,27	13,02	-3,55	accidents
Fosser	1984	169	-0,12	57,92	-7,22	accidents
Moe	1987	169	-0,24	195,58	-47,76	accidents
Glad	1986	169	0,03	1367,02	37,82	accidents
Stene	1988	170	0,28	67,66	19,23	accidents
Moe	1990	171	-0,1	116,27	-12,2	accidents
Fosser	1992	172	-0,06	121,86	-7,76	accidents
Fosser	1992	172	-0,11	125,88	-13,86	accidents
Fosser	1992	172	-0,07	121,81	-8,55	accidents

First author / organisation / project title	year	project identity number	ln odds ratio	weight (fixed effects)	ln odds x weight	outcome measure
Amundsen	1999	175	0,08	33,6	2,66	accidents
Amundsen	1999	175	-0,09	37,19	-3,25	accidents
Amundsen	1999	175	0,19	29,44	5,67	accidents
Amundsen	1999	175	-0,55	20,74	-11,42	accidents
Maisey	1981	176	-0,32	58,46	-18,88	accidents
Maisey	1981	176	-0,46	19,61	-9,11	accidents
Cameron	1997	177	-0,07	169,19	-11,82	accidents
Smith	1990	178	-0,13	299,06	-40,06	accidents
Smith	1990	178	-0,24	693,46	-168,26	accidents
Smith	1990	178	0,02	466,96	7,85	recall
Smith	1990	178	-0,06	465,14	-29,33	risk
Sali	1983	179	-0,18	1112,01	-198,51	accidents
Voas	1997	180	0,02	191,11	3,8	accidents
Voas	1997	180	-0,08	53,16	-4,13	accidents
Voas	1997	180	-0,07	53,23	-3,52	accidents
Dowling	1986	180	-0,21	725,08	-152,28	accidents
Blomberg	1987	181	0,4	69,17	27,38	recall
Blomberg	1987	181	0,55	345,72	191,62	knowledge
Blomberg	1987	181	0,36	64,96	23,08	risk
Blomberg	1987	181	0,12	69,4	8,6	drink-driving
Blomberg	1987	181	-0,37	129,32	-47,34	accidents
Blomberg	1987	181	-0,3	883,96	-264,08	accidents
Taylor	1995	183	-0,04	555,23	-19,99	accidents
Taylor	1995	183	-0,02	785,65	-14,66	accidents
Taylor	1995	183	-0,07	132,18	-9,07	accidents
Taylor	1995	183	0,19	111,95	20,92	accidents
Cameron	1993	184	-0,11	124,93	-13,79	accidents
Cameron	1993	184	-0,1	144,18	-14,95	accidents
Wolfe	1983	185	-0,04	939,55	-33,17	accidents
Wolfe	1983	185	1,2	175,92	211,8	recall
Wolfe	1983	185	0,4	125,93	50,09	knowledge
Wolfe	1983	185	0,05	125,93	6,89	drink-driving
Wolfe	1983	185	0,11	236,02	25,62	risk
King	1987	186	-0,01	295,21	-2,98	accidents
King	1987	186	0,03	149,86	5,12	accidents
King	1989	186	-0,57	435,62	-247,39	accidents
King	1989	186	-0,1	124,73	-12,76	accidents
Wells	1992	187	-0,39	336,31	-132,73	drink-driving
Wells	1992	187	0,17	520,8	90,21	seatbelts
Wells	1992	187	-0,19	125,09	-24,25	accidents
Harte	1984	188	-0,36	86,76	-31,57	accidents
Harte	1984	188	-0,1	4,55	-0,43	accidents
Armour	1985	189	-0,27	43,19	-11,75	accidents
Armour	1985	189	-0,55	70,43	-38,81	accidents
Drummond	1992	191	-0,25	46,66	-11,67	accidents
Drummond	1992	191	0,04	469,35	18,7	accidents
Drummond	1992	191	-0,19	132,39	-24,72	accidents
Drummond	1992	191	0,04	17,59	0,64	accidents
Stuster	1995	192	0,59	78,43	46,52	recall
Stuster	1995	192	0,44	78,43	34,27	recall
Stuster	1995	192	-0,01	67,18	-0,46	risk
Stuster	1995	192	0,09	66,59	6,28	risk
Stuster	1995	192	0,03	67,67	1,79	speed

First author / organisation / project title	year	project identity number	ln odds ratio	weight (fixed effects)	ln odds x weight	outcome measure
Stuster	1995	192	-0,02	66,67	-1,17	speed
Stuster	1995	192	-0,14	257,14	-36,99	speed
Stuster	1995	192	0,02	257,14	4,95	speed
Stuster	1995	192	-0,09	13,17	-1,22	accidents
Stuster	1995	192	-0,37	57,16	-21,26	accidents
Stuster	1995	192	-0,11	10,04	-1,08	accidents
Stuster	1995	192	-0,03	63,32	-2,16	accidents
Stuster	1995	192	-0,25	13,79	-3,48	accidents
Stuster	1995	192	0,14	49,45	7,17	accidents
Spoerer	1989	193	0,01	90,4	0,62	accidents
Behrensdorff	1992	194	-0,5	11,89	-5,92	accidents
Behrensdorff	1992	194	0,65	11,72	7,63	accidents
Machemer	1995	195	-0,17	9,66	-1,62	accidents
Machemer	1995	195	0	6,32	0,03	accidents
Machemer	1995	195	-0,17	5,95	-0,99	accidents
Oei	1995	196	-0,07	176,16	-12,5	accidents
Oei	1992	196	0,49	1,6	0,79	accidents
Oei	1992	196	-0,67	7,22	-4,82	accidents
Oei	1992	196	-0,43	1,6	-0,69	accidents
Oei	1992	196	-0,97	8,09	-7,89	accidents
Oei	1992	196	-0,47	2,18	-1,03	accidents
Oei	1992	196	-0,4	8,79	-3,48	accidents
Haynes	1982	198	-0,28	16,08	-4,52	accidents
Clayton	2006	199	0,28	536,25	148,14	seatbelts
Clayton	2006	199	0,23	586,67	133,04	seatbelts
Austin	2006	200	-0,09	30,27	-2,72	other
Wells	2000	201	0,11	2614,79	300,11	seatbelts
Wells	2000	201	0,11	2153,97	247,22	seatbelts
Wells	2000	201	0,04	796,42	30,4	seatbelts
Wells	2000	201	0,02	612,11	15,03	seatbelts
Cox	2000	202	0,25	62,5	15,71	seatbelts
Kirby	2002	203	0,63	13,87	8,77	seatbelts
Agent	2003	204	0,01	8,3	0,1	accidents
Agent	2003	204	-0,11	327,68	-37,42	accidents
Agent	2003	204	-0,05	190,98	-9,83	seatbelts
Agent	2003	204	-0,06	190,98	-11,76	risk
Agent	2003	204	-0,12	190,98	-23,1	recall
Nassirpour	2004	205	0,02	16098,78	398,49	seatbelts
Nassirpour	2004	205	0,01	216,59	1,3	risk
Nassirpour	2004	205	0,14	216,59	30,75	risk
Nassirpour	2004	205	0,11	216,59	24,43	risk
Cameron	2003	206	0	294,45	-0,9	accidents
Cameron	2003	206	0,03	292,55	10,18	accidents
Cameron	2003	206	-0,05	273,52	-14,15	accidents
Cameron	2003	206	0,05	222,42	10,47	accidents
Cameron	2003	206	0,42	75,42	31,52	recall
Diamantopoulou	2002	207	0,1	135,63	13,06	accidents
Diamantopoulou	2002	207	-0,12	38,42	-4,58	accidents
Diamantopoulou	2002	207	-0,53	37,32	-19,89	accidents
Boots	1999	208	0,25	36,92	9,19	other
Boots	1999	208	0,2	36,92	7,45	recall
Stead	2005	210	-0,29	179,99	-51,78	speed
Worden	1975	210	-0,11	14,15	-1,49	drink-driving

First author / organisation / project title	year	project identity number	ln odds ratio	weight (fixed effects)	ln odds x weight	outcome measure
Worden	1975	210	-0,07	2,95	-0,2	accidents
Whittam	2006	211	0,07	541,35	35,46	accidents
Agent	2002	212	-0,15	112,97	-17,18	accidents
Agent	2002	212	0,18	126,5	22,8	drink-driving
Agent	2002	212	-0,17	126,5	-21,17	risk
Agent	2002	212	0,04	126,5	4,77	recall
Illinois Department of Transportation	2006	213	0,06	140,87	8,05	drink-driving
Illinois Department of Transportation	2006	213	0,15	140,87	21,72	risk
Illinois Department of Transportation	2006	213	0,13	140,87	18,81	recall
Williams	2000	214	0,27	271,25	73,37	seatbelts
Williams	2000	214	0,65	100	64,97	recall
Mulholland	2005	215	-0,01	880,34	-10,75	accidents
Ebel	2003	216	0,52	205,1	106,49	seatbelts
Murry	1993	220	-0,37	47,93	-17,88	drink-driving
Murry	1993	220	-0,07	7,42	-0,5	accidents
Williams	1987	221	0,31	179,19	56,06	drink-driving
Ulleberg	2004	222	-0,03	262,61	-8,96	accidents
Sakshaug	2001	226	0,03	103,78	2,73	accidents
Sakshaug	2001	226	0,14	65,47	8,97	accidents
Ulleberg	2007	227	-0,36	92,23	-32,28	accidents
Ulleberg	2007	228	-0,11	146,18	-16,08	accidents

APPENDIX C – 'INFOEFFEKT' ORIGINAL REFERENCES [VAA ET AL. (2004)]

Amundsen, A.H.; Elvik; R & Fridstrøm, L. (1999) *Evaluering av "Sei i frå" kampanjen i Sogn og Fjordane*. TØI rapport 425/1999, Transportøkonomisk institutt, Oslo.

Armour, M., Monk, K., South. D., & Chomiac, G. (1985). *Evaluation of the 1983 Melbourne random breath testing campaign: casualty accident analysis*. Road Traffic Authority (report (8/85). 18p.Assum, T. (2003a) *INFOEFFEKT – aktivitet 5: Evaluering av informasjon*. Arbeidsdokument 1451/2003

Behrensdorff, I; Johansen, H.J. (1991). *Kampagnen "Stop spritkørsel – osse de andres". Spørgeskemaundersøgelse blant unge på vej til "Grøn koncert".* Gentofte, Rådet for Trafiksikkerhedsforskning. Arbeidsnotat, 1.

Blomberg, R.D., Preusser, D.F; Ulmer R.G. (1987). Deterrent effects of mandatory license suspension for DWI conviction. Final report.U.S. Department of Transportation, National Highway Traffic Safety Administration. (Report DOT HS 807 138).

Cameron, M., Haworth, N., Oxley, J., Newstead, S., & Le, T. (1993). *Evaluation of transport accident commission road safety television advertising.* Monash University Accident Research Centre (report N°52).

Cameron, M., Diamantoupoulou, K., Mullan, N., Dyte, D., & Gantzer, S. (1997). *Evaluation of the country random breath testing and publicity program in Victoria 1993-1994*. Monash University Accident Research Centre (report N°126). 60p.

Delhomme, P; Vaa, T; Meyer, T; Harland, G; Goldenbeld, C; Järmark, S; Christie, N; Rehnova, V (1999): Evaluated Road Safety Media Campaigns: An Overview of 265 Evaluated Campaigns and Some Meta-Analysis on Accidents. INRETS. Project funded by EC and partners under EU 4th Framework Programme. Paris March 1999.

Dowling, A.M.(1986). *Stop-dwi: the first four years - an evaluation update*. Conducted for the New York state department of motor vehicules by the Institute for Traffic Safety Management and research.

Drummond, A.E., Sullivan, G., Cavallo A. (1992). *An evaluation of the random breath testing initiative in Victoria 1989-1990: quasi-experimental time series approach.* Monash University Accident Research Centre (report N°37). 105p.

Harte, D.S., & Hurst, P.M. (1984). Evaluation of operation checkpoint - Accident data. *Road traffic safety seminar 15-17 august 1984.* Road traffic safety research council, Wellington, New Zealand. *Seminar papers, Vol. 2*, pp.153-167.

Haynes, R.S., Pine, R.C.; Fitch, H.G. (1982). Reducing accidents with organizational behaviour modification. *Academy of Management Journal, 25*(2), 407-416.

Machemer, E., Runde, B., Wolf, U., Büttner, D., Tucke, M., Kley, H., Pieper, M., Schopuis, R., Timmer, H., Trecksel, H., & Wundram, A. (1994). *Delegierte belohnung und intensivierte Verkehrs-überwachung im Vergleich*. BAST (Berichte der Bundesanstalt für Strassenwesen), Mensch und Sicherheit (Heft M 48).

Maisey, G.E., & Saunders, C.M. (1981). An evaluation of the 1980/81 christmas/new year traffic enforcement blitz. Road Traffic Authority, Research and statistics division, Western Australia (report N°16).

Moe, D., Sakshaug, K.; Stene, T.M. (1987). *Evaluering av utforkjøringskampanjen 1986*. Trondheim, SINTEF Samferdselsteknikk. (SINTEF-Rapport STF63 A87006).

Nagatsuka, Y. (1991). Effectiveness of model driving with a safety campaign sticker on the rear outside of the body of buses, trucks and taxis: an action research. Transportation research institute, Haifa. *Proceedings of the second international conference on new ways for improved road safety and quality of life (Tel Aviv, Israel, october 7-10),* pp. 9-10.

Oei, H.L., & Polak, P.H. (1992). *Effect van automatische waarschuwing en toezicht op snelheid en ongevallen. Resultaten van een evaluatie-onderzoek in vier provincies*. SWOV, Leidschendam (Rapport N° 92-23).

Oei, H.L; Goldenbeld, C. (1996). *Snelheidscampagne 1995 in Eindhoven. Leidschendam: Stichting Wetenschap- pelijk Onderzoek Verkeersveiligheid*. SWOV (Rapport N° R-96-31).

Sali, G.J. (1983). Evaluation of Boise traffic enforcement project. Washington D.C, *Transportation Research Record 910*.

Smith, D.I., Maisey, G.E., & McLaughlin, K.L. (1990). Evaluation of the first year of random breathtesting in Western Australia. *15th arrb conference, Darwin, northern territory (26-31 august, Victoria).* Australian Road Research Board Proceedings, 15(7), pp. 93-106.

Spoerer, E. (1989). *Einflus von Informationen zur Verkehrssicherheit auf unfallbeteiligte Kraftfahrer*. BAST (Berichte der Bundesanstalt für Strassenwesen), Bergisch Gladbach.

Studsholt, P (1990). Campaigns against drunken driving among young drivers. *Proceedings of road safety and traffic in Europe (Conference, Gothenburg, Sweden september 26-28)*. In National Swedish Road & Traffic Research Institute, Linköping, Sweden (report N°HS-041 279, VTI-365A), pp. 26-33.

Stuster, J.W (1995): *Experimental evaluation of municipal enforcement programs.* Washington D.C, National Highway Traffic Safety Administration (Report No DOT HS 808 325).

Sävenhed, H., Brüde, U., Nygaard, B., Petterson, H-E., & Thulin, H. (1996). *Heja Halland: An evaluation of a traffic campaign.* Statens vaeg- och transportforskningsinstitut, Linkoeping, Sweden (VTI Meddelande, 779).

Taylor, W.C., & Ahmed, A.R. (1995). *Evaluation of the " Share-the-road " campaign.* Final Report. Michigan State University, USA (Report GLCTTR 71-95/01). 75p.

Törnros, J. (1995). *Intensified random breath testing in southern Sweden*. Statens vaeg-och transportforskningsinstiut, Linkoeping, Sweden (VTI Meddelande, 746).

Voas, R.B; Holder, H.D; Gruenewald, P.J (1997): The effect of drinking and driving interventions on alcohol-involved traffic crashes within a comprehensive community trial. *Addiction,* 92 (Supplement 2), S221 – S236.

Wells, J.K., Preusser, D.F., & Williams, A.F. (1992). Enforcing alcohol-impaired driving and seat belt use laws, Binghamton, NY. *Journal of Safety Research, 23,* 63-71.

Wolfe, A.C. (1983). *Interim (two-year) evaluation report on the Oakland county alcohol enforcement/education project*. University of Michigan Transport Research Institute, USA (report N°83-12).

Appendix D –Results FROM META-ANALYSIS OF INFOEFFEKT PROJECT (VaA et al., 2004)

Table D1 General effect of road safety campaigns on road accident in the campaign period and in the after-period. Percentage and confidence interval.(n = number of results)

Group:	***Effect***	***95% -confidence interval***	***P < 0.05 ?***
Effect in campaign period (n = 72)	- 8.9	(- 12.7; - 4.6)	yes
Effect in after-period (n =14)	- 14.8	(- 23.0; - 0.5)	yes

Source: TØI report 727/2004

Table D2. Effects (%) of road safety campaigns on road accidents. Based on random-effect models with generated data from "trim-and-fill". Percentages and confidence intervals.

Theme of campaigns	***Effect***	***95% CI***	***p < 0.05?***
All campaigns (n=72)	- 8.9	(-12.7; - 4.6)	Yes
Campaigns against drink driving (n=33)	-14.4	(- 21.1; - 8.3)	Yes
Australian RBT-campaigns *) (n=9)	-13.5	(- 22.2; - 3.8)	Yes
Campaigns against speeding (n=18)	-8.5	(- 19.9; + 3.4)	No
Other single-theme campaigns (n=6)	-10.4	(- 18.9; - 1.1)	yes
Multi-theme campaigns (n=15)	1.0	(- 6.7; + 9.3)	No

*) RBT = Random Breath Testing. Source: TØI report 727/2004

Table D3: Effect (%) of road safety campaigns on road accident distributed according to type of campaigns. Based on random-effect models with generated data from "trim-and-fill". Percentages and confidence intervals.

Campaigns according to type	Effect	95% - CI	p < 0.05?
Mass media campaigns alone	0.9	(- 8.6; + 11.7)	No
Mass media campaigns + enforcement	-12.7	(- 18.9; - 6.2)	Yes
Mass media campaigns + enforcement + education	-14.2	(- 22.0; - 4.9)	Yes
Local, personally directed campaigns	-39.3	(- 56.0; - 17.4)	Yes

Source: TØI report 727/2004

*Table D.4. Results from linear, multivariate regression (meta-regression) – **FULL** model. Predictors, coefficients, t-values, p-values.*

Predictors	Coefficients	*t-value*	*p-value*
Constant	- 0,509	- 0,401	0.3584
Country: (Reference: Other countries (UK,D,JPN,NZ,S,DK,N)			
USA	- 0,411	- 0.206	0.4530
Australia	- 0,271	- 2,356	0.0227 *
The Netherlands	- 0,485	- 2,602	0.0123 *
Theme of campaigns (Reference: Speed campaigns)			
Alcohol	- 0,047	- 0,289	0.7770
Other single-theme campaigns	0,168	0,668	0.5075
Multi-theme campaigns	0,058	0,224	0.8241
Decade (Reference: 1980-1989)	- 0,874	- 0,689	0.3863
Type of campaign (Reference: Campaign + politce enforcement- "Publicized enforcement")			
Campaign alone	- 0,132	0,814	0.4198
Campaign + police enforcement + education	- 0,240	- 1,426	0.1606
Local, individually directed campaign	0,291	0,580	0.5648
Size of target group (Reference: N < 35.000)			
2) 35.000 < N < 1.080.000	0,017	0,071	0.9441
3) N > 1.080.000	- 0,201	1.087	0.2827
Length of campaign (Reference: 30-100 days)			
2) 101 – 200 days	- 0,495	- 0,130	0.1983
3) 201 – 540 days	0,214	0,200	0.5171
4) > 540 days	0,285	0,172	0.9202
"Two-step"	0,530	1,871	0.0675
"Feedback on the rad"	0,094	0,416	0.6792
Television?	0.063	0,508	0.6139
Radio ?	- 0,016	- 0,050	0.9600
Newspaper ?	0,000	- 0,000	0.9998
Posters ?	0.298	2,626	0.0116 *
Leaflets ?	- 0,010	- 0,068	0.9464
Personal influence?	- 0,477	- 3,220	0.0023 *
Other communication channels ?	0,016	0,061	0.9513

Number of observations: 72 Number of parameters: 25 Degrees of freedom: 47 $R^2 = 0.512$
Modell-test: F[24;47] = 2,06 p = 0.0172. *) Statistical significance p < 0.05

Source: TØI report 727/2004

*Table D.5. Results from linear, multivariate regression (meta-regression) – **REDUCED** model. Predictors, coefficients, t-values, p-values.*

Predictors	*Coefficients*	*t-value*	*p-value*
Constant	0,232	1,558	0.1249
Country: (Reference: Other countries (UK,D,JPN,NZ,S,DK,N)			
USA	- 0,142	- 1,767	0.0828
Australia	- 0,199	- 2,293	0.0257 *
The Netherlands	- 0,374	- 2,650	0.0105 *
Theme of campaigns (Reference: Speed campaigns)			
Alcohol	- 0,118	- 1,426	0.1594
Other single-theme campaigns	- 0,061	- 0,546	0.5874
Multi-theme campaigns	- 0,053	- 0,490	0.6257
Type of campaign (Reference: Campaign + police enforcement- "Publicized enforcement")			
Campaign alone	- 0,085	- 0,944	0.3493
Campaign + police enforcement + education	- 0,025	- 0,348	0.7293
Local, individually directed campaign	0,188	- 1.016	0.3139
Length of campaign (Reference: ≥ 201 days)			
< 201 days	- 0,232	- 3,983	0.0002 *
TV ?	0,036	0,612	0,5429
Radio ?	- 0,015	- 0,174	0.8624
Newspapers ?	- 0,083	- 1,128	0.2640
"Two-step" ?	0,068	0,828	0.4114
"Feedback on the road" ?	0,026	0,405	0.6870
Personal influence ?	- 0,220	- 3,084	0.0032 *

Number of observations: 72 Number of parameters: 17 Degrees of freedom: 55 $R^2 = 0.399$
Model-test: F[16;55] = 2.28 p = 0.0122 *) p < 0.05

Source: TØI report 727/2004