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Educational interventions for the prevention of eye injuries (Protocol)

Patel D, Blackhall K, Ker K, Shah A

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ABSTRACT
This is the protocol for a review and there is no abstract. The objectives are as follows:
To assess the evidence for the effectiveness of educational interventions for the prevention of eye injuries.

BACKGROUND
Ocular injury is a preventable cause of blindness, and yet it remains a significant disabling health problem that affects all age groups. Injuries may occur in the home, in the workplace, during recreational activities or as a result of road crashes. Types of injuries vary from closed globe (contusion or laceration) to an open globe injury, which includes penetration and even perforation of the globe. To date, the main strategy to prevent these injuries has been to educate people to identify high risk situations and to take correct action to avoid danger.

Information from the World Health Organization (WHO) suggests that over 55 million eye injuries occur each year (Negrel 1998). They report that about 1.6 million people go blind from these injuries, 2.3 million suffer from bilateral low vision and 19 million remain with unilateral blindness or low vision. Data from the US eye injury register (USEIR) have identified that over 57% of the injuries occur in people under 30 years of age. In children under three years old, injury remains the main cause of eye-ball loss (enucleation).

A population-based survey (Krisnainah 2006) and the USEIR have established that males are more likely to sustain eye injury. Data from the USEIR indicate that over 40% of injuries occur within the home environment. In contrast, data from studies conducted in some developing countries, specifically India and Nepal, identify the mainly-agricultural work environment (Khatry 2004; Krisnainah 2006) as a higher risk situation. The Birmingham Eye Trauma Terminology (BETT) (Kuhn 2004) has been endorsed as a comprehensive and standardised classification that attempts to address ambiguity in the description of eye injuries. The BETT classification defines all eye injuries as open or closed globe injuries. In the US it has been found that 31% of injuries occur from blunt objects and result in varying degrees of contusion (from 'black eye' to severe intraocular tearing) and, occasionally, rupture of the globe, or blow out fracture of the floor of the orbit. Sharp objects contribute to about 18% of injuries (USEIR). Penetrating injuries and perforating injuries often carry a poor prognosis. The management of each case varies, and is dependant on how far and where the sharp object entered the eye. Intraocular foreign bodies can cause structural damage as well as be toxic to the tissues. These materials vary from plastics and glass to metal, and even plant matter, which can induce an infection. Burns to the eye remain an ophthalmic emergency where the outcome depends upon the concentration and pH of the burning agent. Alkalis are a particularly challenge as they penetrate the ocular tissues and cause irreversible damage.

In the UK almost half of all eye emergencies are due to trauma (MacEwen 1999), though injuries within the UK workplace have decreased significantly due to better awareness and the use of eye protection in industrial settings (Canavan 1980). Similarly, eye injuries due to road traffic crashes have decreased in countries where there is widespread use of seat belts and laminated windscreen (Vernon 1984).

Behavioural change may prevent many ocular injuries. Educational interventions have been proposed as a potentially effective approach to assist in bringing about such a change; these
may be implemented in conjunction with new or existing legisla-
tion/regulations. Such interventions can range from simple printed 
information to intensive safety workshops. Lipscomb 2000 has
identified that the interventions to prevent ocular injuries have
been effective when used in conjunction with policy changes in
both industrial and agricultural settings. As far as we are aware,
the evidence for the effectiveness of educational interventions for
the prevention of eye injuries has not been reviewed systematically,
and there is uncertainty surrounding how effective such an ap-
proach is in bringing about the required behavioural change - with
or without legislation. Evidence on the effectiveness of interven-
tions will provide vital information in identifying the educational
method(s) that can bring about a ‘safety cultural’ shift to prevent
ocular injuries.

OBJECTIVES

To assess the evidence for the effectiveness of educational inter-
tentions for the prevention of eye injuries.

CRITERIA FOR CONSIDERING
STUDIES FOR THIS REVIEW

Types of studies

We will include the following study designs (definitions from Hig-
gins 2005):
• Randomised controlled trials (RCTs) (defined as an experiment
  in which investigators randomly allocate eligible participants
  into an intervention group (arm), each of which receives one or
  more of the interventions that are being compared. The results
  are assessed by comparing outcomes between the arms).
• Cluster randomised controlled trials (defined as a trial in which
  clusters of individuals (for example, clinics, families, geographical
  areas), rather than individuals themselves, are randomised
to different arms).
• Controlled before-and-after studies (CBAs) (a non-randomised
  study design where a control population of similar character-
  istics and performance as the intervention group is identified.
  Data are collected before and after the intervention in both the
  control and intervention groups).

Types of participants

People of any age.

Types of intervention

Any educational intervention specifically aimed at reducing the
occurrence and/or risk of eye injuries. These may be based on
written materials, video or audiotapes delivered on an individual
basis (for example, one-to-one counselling), in a group setting
(for example, classes) and/or population-wide (for example, mass
marketing campaigns).

We will include education interventions irrespective of the setting
in which they are delivered. For example, we anticipate that eligible
interventions may be home-based, school-based, work-based or
recreation-based.

Studies of educational interventions that have been administered
in a multi-component programme involving other ineligible in-
terventions, will only be considered if outcomes attributable to
the education intervention can be distinguished and/or the edu-
cational component is the primary intervention (we will refer to the
trial’s reported objectives to determine if education is the primary
intervention of interest).

Types of outcome measures

Primary outcome
• Eye injuries of any severity.

Secondary outcomes
• Change in behaviour.
• Change in knowledge.

SEARCH METHODS FOR
IDENTIFICATION OF STUDIES

See: Cochrane Injuries Group methods used in reviews.

We will search the following electronic databases:
• Cochrane Eyes & Vision Group’s trials register;
• Cochrane Injuries Group’s specialised register;
• CENTRAL (The Cochrane Library (latest issue));
• MEDLINE (1966 to date);
• EMBASE (1980 to date);
• Science Citation Index (1981 to date);
• Social Science Citation Index (1981 to date);
• ISI Proceedings (1990 to date);
• National Research Register (latest issue);
• ERIC;
• Zetoc.

We will base the electronic database searches on the following
MEDLINE strategy which will be adapted, as appropriate, for
each database:
1. exp eye/
2. exp accident prevention/
3. accidents occupational/pc [Prevention & Control]
METHODS OF THE REVIEW

Selection of studies
At least two authors will independently assess the titles and abstracts of citations identified from the electronic database searches for eligibility. We will obtain full copies of those studies that definitely, or possibly, meet the pre-defined inclusion criteria. The reasons for excluding any study for which a full text copy was obtained will be documented in the 'Characteristics of excluded studies table'. Any disagreements will be resolved by discussion, or by consultation with a third author.

Data extraction and management
Two authors will extract the following information independently on to a standardised form developed beforehand. The following data will be extracted:

- study design;
- study setting;
- details of participants;
- details of interventions;
- outcomes.

One author will enter data into RevMan and the other author will use the double-data-entry facility to check for errors and inconsistencies. Where information is missing or unclear, we will contact the trial authors.

Assessment of methodological quality of included studies
We will assess the methodological quality of the included studies independently; disagreements will be resolved by discussion. We will use an adapted version of the tool developed by the Effective Public Health Practice Project (Thomas 2003) to assess methodological quality of both randomised and non-randomised study designs.

We will not use the tool to derive an overall score for each study; Higgins 2005 discusses how the relationship between a quality score and the extent to which a study is free from bias is not clear. Also, deriving one summary score by adding the scores for each item is not supported by the evidence, therefore it is suggested that it is preferable to report fully how each trial performed on each criterion.

A modified framework of the Thomas 2003 quality tool will be used to describe each of the included studies against the following criteria:

- Allocation bias (for example, was allocation to the experimental and control groups random and adequately concealed?).
- Confounders (for example, did the groups under study differ in terms of distribution of potential confounders?).
- Masking (for example, were the outcome assessors aware of the allocation status of the participants?).
- Data collection methods (for example, were outcome data collected through self-report methods or more objective methods such as researcher observation or extracted from official records?).
- Withdrawals and dropouts (for example, how many participants failed to complete the study and/or were lost to follow-up?).
- Intervention compliance (for example, what proportion of participants received the allocation intervention?).
- Duration of follow-up (for example, how long was/were the data collection period(s)?).

In addition, since there is evidence that the quality of allocation concealment particularly affects the results of RCTs (Schulz 1995), two authors will score this quality on the scale used by Schulz 1995 as shown below, assigning C to poorest quality and A to highest quality.

A: trials deemed to have taken adequate measures to conceal allocation (that is, using central randomisation; serially numbered, opaque, sealed envelopes; or containing a description of elements convincing of concealment).
B: trials in which the authors either did not report an allocation concealment approach at all or reported an approach that did not fall into one of the other categories.

C: trials in which concealment was inadequate (such as alternation or reference to case record numbers or to dates of birth).

Measures of treatment effect
For dichotomous outcomes we will calculate a summary risk ratio (RR). For continuous outcomes we will calculate the (weighted) mean difference (MD) or the standardised mean difference.

Dealing with missing data
We will contact the trial investigators for any missing data.

Assessment of heterogeneity
We will use the chi-squared test to check for statistical heterogeneity. We will use the I-squared value to assess the impact of heterogeneity (a value greater than 50% may be considered substantial heterogeneity). We will look for clinical heterogeneity by examination of the study details. We will also examine the funnel plot for other sources of heterogeneity.

Data synthesis (meta-analysis)
If no substantial statistical or clinical heterogeneity is detected we will combine the results in a meta-analysis using a random-effects model. If there are fewer than three trials and no heterogeneity has been detected, we will use the fixed-effect model. In case of substantial statistical or clinical heterogeneity we will not combine the study results but will summarise the information.

Sensitivity analysis
We will conduct sensitivity analyses to determine the impact of exclusion of studies with lower methodological quality (randomised versus non-randomised studies), unpublished data and industry-funded studies.

Subgroup analyses
If there are enough data, we will perform subgroup analysis. Potential subgroups would be:

- age (children < 18 years of age, adults > 18 years of age);
- severity of injury;
- setting (work based injury, home based injury, recreation based injury, school based);
- education only versus education combined with supportive legislation.

NOTES
This review is a collaborative project between the Cochrane Eyes & Vision and Injuries Review Groups.

POTENTIAL CONFLICT OF INTEREST
None known.

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External sources of support
- No sources of support supplied

Internal sources of support
- London School of Hygiene & Tropical Medicine UK

REFERENCES

Additional references

Canavan 1980

Higgins 2005

Khatry 2004

Krisnainah 2006

Kuhn 2004

Lipscomb 2000

MacEwen 1999
Negrel 1998

Schulz 1995

Thomas 2003

USEIR

Vernon 1984

COVER SHEET

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What's New  Information not supplied by author
Contact address  Daksha Patel
International Centre for Eye Health
London School of Hygiene & Tropical Medicine
Keppel Street
London
WC1E 7HT
UK
E-mail: daksha.patel@lshtm.ac.uk
Tel: +44 20 7958 8318
Fax: +44 20 7958 8317
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