Effectiveness of Nonresuscitative First Aid Training in Laypersons: A Systematic Review

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Study objective: This study reviewed evidence on the effects of nonresuscitative first aid training on competence and helping behavior in laypersons.

Methods: We identified randomized and nonrandomized controlled trials and interrupted time series on nonresuscitative first aid training for laypersons by using 12 databases (including MEDLINE, EMBASE, and PsycINFO), hand searching, reference checking, and author communication. Two reviewers independently evaluated selected studies with the Cochrane Effective Practice and Organisation of Care Review Group quality criteria. One reviewer extracted data with a standard form and another checked them. In anticipation of substantial heterogeneity across studies, we elected a descriptive summary of the included studies.

Results: We included 4 studies, 3 of which were randomized trials. We excluded 11 studies on quality issues. Two studies revealed that participants trained in first aid demonstrated higher written test scores than controls (poisoning first aid: relative risk 2.11, 95% confidence interval [CI] 1.64 to 2.72; various first aid cases: mean difference 4.75, 95% CI 3.02 to 6.48). Two studies evaluated helping responses during unannounced simulations. First aid training improved the quality of help for a bleeding emergency (relative risk 25.94; 95% CI 3.60 to 186.93), not the rate of helping (relative risk 1.13; 95% CI 0.88 to 1.45). Training in first aid and helping behavior increased the helping rates in a chest pain emergency compared with training in first aid only (relative risk 2.80; 95% CI 1.05 to 7.50) or controls (relative risk 3.81; 95% CI 0.98 to 14.89). Participants trained in first aid only did not help more than controls (relative risk 1.36; 95% CI 0.28 to 6.61).

Conclusion: First aid programs that also train participants to overcome inhibitors of emergency helping behavior could lead to better help and higher helping rates. [Ann Emerg Med. 2009;54:447-457.]

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INTRODUCTION

In case of an accident, injury, or sudden illness, first aid delivered by bystanders can save lives and limit damage until professional help has arrived.[1,2] Laypersons trained in first aid can also potentially reduce delays in seeking medical assistance. The latter is also potentially lifesaving because in emergencies time may constitute a critical determinant of victims' outcomes. Recently, the potential value of first aid training for laypersons has assumed a heightened importance in the context of both manmade and natural mass casualty incidents and disasters. When a sudden disaster strikes, survivors often constitute the sole source of initial help to others because professional help takes time to mobilize or is insufficient.[3,4]

Reports from the recent bombings in New York and London state that first aid by bystanders might have saved lives.[5,6] In the Madrid bombings, 67% of the injured persons arrived at the hospital in nonambulance vehicles.[7] Victim transport by bystanders occurs in many mass casualty disasters if ambulance transport is lacking.[3] First aid training can reduce the risks of private victim transport and provide laypersons with guidance about when not to move a victim.[3] After the Armenia and Kobe earthquakes, bystanders provided only minimal first aid efforts.[5,8] Officials interviewed after the disaster strongly agreed that first aid training for laypersons would likely decrease death

rates in similar events.[8] Conversely, in disaster preparedness efforts, emergency personnel and authorities often disregard the potential role of individual laypersons.[3,4]

Although laypersons rarely panic or develop psychological shock in disasters, the emotional stress of the situation can temporarily limit their reasoning ability.[9] Experts widely advocate layperson first aid training as a way of improving the initial response in such situations. Recommendations range from voluntary or mandatory first aid courses to be offered to the community at large, to courses limited to target groups such as workers, professional drivers, family members of high risk individuals, or citizens living in disaster-prone areas.[3,4,6,9-12]

Given the increased premium on layperson first aid skills under conditions of disaster and the consequent likelihood of substantial increase in resources devoted to training efforts, the evaluation of effectiveness of such training assumes paramount importance. Such evaluations need to assess whether course participants acquire appropriate attitudes, competences, and behavior for first aid provision. This implies that course participants demonstrate a positive attitude and helping reaction toward emergencies, and furthermore that they can assess the situation, ensure safety, assess the condition of the victim, get help if required, and administer first aid and provide emotional support to victims.

Published reports of effectiveness of first aid training are scattered across a large array of biomedical journals published in different languages and in journals associated with different practice specialties, making it difficult to derive a valid assessment of current knowledge in this area. The objective of this study was to systematically review primary studies on the effects of nonresuscitative first aid training with regard to acquisition and retention of competence or modification of helping behavior.

MATERIALS AND METHODS

We reported this study in accordance with the QUOROM statement for meta-analyses.[13]

Search Strategy

We developed the search strategy in consultation with an information specialist and conducted a literature search of MEDLINE (PubMed), EMBASE (EMBASE.com), the Cochrane Central Register of Controlled Trials (Wiley), C2-SPECTR (Campbell Collaboration), Cinahl (EBSCOhost), British Nursing Index and Archive (OVID), SPORTDiscus (EBSCOhost), PsycINFO (WebSPIRS), and Education Resources Information Center database (EBSCOhost). A search of the abovementioned databases took place from inception until May 2007. Appendix E1 (available online at http://www.annemergmed.com) contains the description of the actual search strategies.

We searched for grey literature reports[14] about first aid training in the Open Archives Initiative (OAIster) database and the British Library Integrated Catalogue and consulted the database of the Netherlands Institute for Health Services Research (NIVEL) for Dutch-language reports on June 19, 2007. We further searched for studies by communicating with the authors of included studies, checking reference lists of selected studies and of related systematic reviews,[15-21] and hand searching. We hand searched conference proceedings and supplement issues of the journals *Resuscitation* and *Prehospital and Disaster Medicine* from January 1997 to April 2007.

Study Selection

In anticipation of finding only a few studies with useful data, we deliberately defined broad inclusion criteria. Systematic reviews with broad questions are valid on the condition that they do not generalize findings across differing conditions.[22] We included randomized controlled trials, nonrandomized controlled trials, controlled before-and-after trials, and interrupted time series in our review.

We defined a layperson as somebody who has never received a formal health care education, and we included studies covering first aid training for laypersons of all education levels. We included studies including participants with previous knowledge of first aid but excluded studies on patient education and studies involving training for health care students and professionals. There were no restrictions on participants' age, sex, ethnicity, motivation, learning potential, learning behavior, or education level. We defined first aid as immediate help provided to a suddenly ill or injured person, until that person has recovered or medical care is available.

We defined first aid training as a formal learning activity, with learning goals defined in terms of skills and attitudes pertaining to immediate help in case of accidents, injuries, or sudden illness. We did not include studies about the effect of informal learning activities, such as mass media awareness campaigns.

We included studies that reported learning and behavior outcomes relating to first aid training for bleeding, shock, wounds, injuries, poisoning, stroke, chest pain, asthmatic attacks, epileptic seizures, or diabetic crisis. We excluded studies focusing only on basic life support or the use of automated external defibrillators. If studies covered both first aid and resuscitation training, we included only results of the effectiveness of first aid training. We placed no restrictions about training methods, materials, duration, or delivery format.

Primary outcomes of interest included measures of helping behavior in real situations, including deception experiments. In deception experiments, the study participants are unaware that an emergency is being simulated and do not know that their helping behavior is being evaluated. This concept originates from social psychology research and differs from customary simulations in which participants know that it is an imitation of an emergency. Secondary outcomes included measures of learning gains in knowledge and skills. We did not include course participant views on the training or self-assessment measures as outcome measures. There was no restriction on the outcome assessment method or timing. Our interest focused

both on the outcomes directly after completing the first aid training and outcomes in the weeks or months afterward. Because of time and resource constraints, we considered only studies reported in English, German, French, or Dutch.

Two reviewers (S.V.D.V., A.H.) independently selected studies from titles and abstracts. Interrater agreement was evaluated with a Cohen's κ. The reviewers resolved disagreements on the selection of studies by discussion and then screened full texts and excluded irrelevant studies.

Assessment of Risk of Bias in Selected Studies

Two reviewers (S.V.D.V., A.H.) independently evaluated all the selected studies for methodological quality. We used the quality criteria of the Cochrane Effective Practice and Organisation of Care Review Group[23] because they provide validated checklists for every type of study design that was included in our review. The quality criteria for randomized controlled trials included concealment of allocation, participant follow-up, blinded assessment, measurement of baseline data, reliability of outcome measures, and protection against contamination among study groups. Appendix E2 (available online at http://www.annemergmed.com) contains the actual checklist for randomized controlled trials.

We rated studies as having low risk of bias if all the quality criteria were met and as having moderate risk of bias if 2 criteria were not met, partially met, or not clear. Studies received a high risk of bias rate when 3 or more criteria were not met, partially met, or not clear. Some quality issues about eligibility arose during the assessment process in studies in which bias was explicitly clear. The group of reviewers decided in agreement to set a cut point for inclusion between studies with a risk of bias and undoubtedly biased studies. This is one of the possible approaches to limit bias in a systematic review.[22] We considered whether to include studies with fewer than 10 participants and decided not to do so because the allocation could not lead to balanced groups. We decided to exclude studies if the comparison groups had substantial differences at baseline because this made groups noncomparable. We excluded studies with clear indications of exposure to the intervention in the control group because this creates contamination bias and dilutes the effect of the intervention. We also discussed whether to include studies with limited compliance with the intervention and decided not to do so because this raises serious concerns about the representativeness of the data and might be misleading. We contacted authors of selected studies if certain data were not reported in the article, and if essential information was not forthcoming, we excluded the study from further analysis. In cases of disagreement on the quality, a third reviewer (B.A.) resolved differences.

Data Extraction and Synthesis

Because there is no single data extraction form that fits all uses, we adapted the Best Evidence Medical Education Collaboration (BEME) coding sheet[24] to the needs of our review. The extracted data related to methodology, participants, setting, educational descriptors, assessment features, outcomes, and the study's conclusions. We classified education levels according to the International Standard Classification of Education (ISCED) levels.[25]

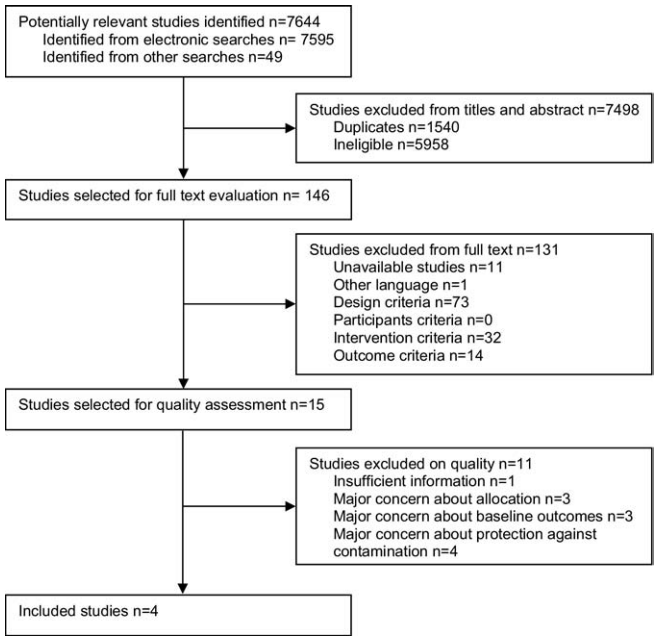

Figure 1. Flowchart of identification and selection of studies.

For the outcome measures, we entered indications of face, content, construct, concurrent, or predictive validity and reliability measures such as internal consistency, interrater reliability, or test-retest reliability in the data extraction form. One reviewer (S.V.D.V.) extracted data and another (A.H.) checked them. Disagreements were resolved by discussion.

The group of reviewers (S.V.D.V., A.H., B.A.) assessed clinical heterogeneity by evaluating the type of participants, intervention, and outcome for each study. In anticipation of finding only a few and very heterogeneous studies, there was no plan to conduct subgroup or sensitivity analysis, or meta-analysis methods. Instead, we conducted a descriptive review of the included studies. To present the outcomes uniformly across studies, we used the Review Manager software version 4.2.10 (The Nordic Cochrane Centre, The Cochrane Collaboration, Copenhagen, Denmark) to calculate mean differences for studies with continuous data and relative risks for studies with dichotomous data.

RESULTS

Trial Flow

Figure 1 provides a flowchart of the identification and selection of studies. The reviewers screened 7,644 citations, including 1,540 duplicates. Evaluation of titles and abstracts resulted in 146 citations. Agreement between the 2 reviewers was high (Cohen's κ of 0.89). After full text evaluation, 15 studies matched every selection criteria. The reviewers agreed

Table 1. Assessment of quality criteria about risk of bias among the 4 included studies meeting quality and nonquality eligibility criteria for inclusion.

Included Study by First Author	Concealment of Allocation	Participant Follow-up	Blinded or Objective Assessment	Baseline Measurement	Reliable Outcome(s)	Protection Against Contamination	Global Assessment
Kelly, 2003[37]	Partially done	Done	Done	Partially done	Unclear	Done	Included study with a high risk of bias
Moore, 1987[38]	Not done	Done	Not done	Done	Partially done	Done	Included study with a high risk of bias
Shotland, 1985[39]	Partially done	Partially done	Unclear	Not done	Done	Unclear	Included study with a high risk of bias
Hawks, 1998[40]	Not done	Done	Done	Done	Partially done	Done	Included study with a moderate risk of bias

unanimously to exclude 10 studies[26-35] after the assessment of probable bias. One poorly reported study was excluded because it did not provide sufficient information to evaluate its quality.[36] Table 1 presents the results of the quality assessment per included study; Table 2, per excluded study.

Study Characteristics

Table 3 summarizes the characteristics of 4 included studies. Three studies[37-39] were randomized controlled trials, and one[40] was a nonrandomized controlled trial.

Kelly et al[37] tested a 9-minute video clip about first aid for poisoning, combined with the distribution of pamphlets and stickers. Participants were parents of young children who visited a women, infant, and children clinic. Outcome measures included knowledge about first aid for poisoning. The authors carried out the assessment by means of written and oral interviews.

Moore[38] evaluated the effects of assertion training and first aid instruction for primary school children. This program was intended to increase the children's autonomy and ability to take care of their own health. The training consisted of a noncompulsory course within the school setting, with random allocation of participants to one of 4 comparison groups. The groups received either 6 hours of assertion training, 6 hours of first aid training, 3 hours of assertion training, and 3 hours of first aid training or received no training but watched 6 hours of science films without health education content. The first aid training covered bleeding, injuries, burns, shock, poisoning, and other topics outside the review. Assertiveness training focused on developing assertive behavior so that participants could become more autonomous health care consumers. This consisted of learning how to express feelings, ask for information, and make requests or refusals. The authors used a written test to assess first aid knowledge and skills.

Shotland and Heinold[39] evaluated the change in first aid practice among university students after first aid training. The first aid training was part of a program organized by the health education department. The duration of the training course is unclear. Passing the final examination would lead to an American Red Cross certificate in advanced first aid and emergency care. This study evaluated the helping responses of participants during unannounced simulations. The participants were deceived and the simulations carefully staged so that they actually believed it was a real accident.

The simulated accident was that of a fallen worker who was bleeding severely. The influence of training was tested against the presence of bystanders and level of ambiguity. The number of bystanders was either none or 2. In ambiguous conditions, there was only the sound of the falling worker; in unambiguous conditions, there was also a moan for help. Participant responses were categorized as providing no help, calling for help, or providing direct help.

Hawks and Egan[40] evaluated the effect of training at a university during a health and wellness education course open to all university students. The majority of students were from the faculty of education. Students took part in the course for reasons of personal interest or because it was a compulsory lesson or an elective. The trainers randomly allocated participants to one of 4 comparison groups: first aid training complemented with training in overcoming the inhibitors of emergency helping behavior, training that mentioned the inhibitors, training without focus on inhibitors, or no training. The training in the inhibitors of emergency helping behavior stressed during the lectures that bystanders often are held back from helping when there are other bystanders or when there is ambiguity about the gravity of the emergency. The participants discussed how to deal with these barriers to providing help and exercised this in simulations with situational ambiguity and presence of bystanders. The only difference between the 3 training programs was the amount of time spent on the inhibitors of emergency helping behavior. The exact duration of the training courses is unclear.

As in the previously described study, the participants were deceived and unexpectedly confronted with a simulated emergency. The simulation was chest pain in a middle-aged and overweight worker. Four actors played bystanders in the room with the casualty who was urgently in need of help. The study recorded whether the participants provided help. No data were available on the type of help provided. This study also carried out a preliminary test in the intervention groups of first aid knowledge and skills by means of a written and a practical test.

Table 2. Assessment of quality criteria about risk of bias among 11 excluded studies meeting nonquality eligibility criteria for inclusion.

Excluded Study by First Author	Concealment of Allocation	Participant Follow-up	Blinded or Objective Assessment	Baseline Measurement	Reliable Outcome(s)	Protection Against Contamination	Global Assessment
Marchand-Martella, 1992[26]	Not done	Done	Not done	Unclear	Done	Unclear	Study excluded because the sample size was less than 10 and the allocation could not lead to balanced groups
Peterson, 1984[27]	Not done	Done	Unclear	Done	Done	Unclear	Study excluded because the sample size was less than 10 and the allocation could not lead to balanced groups
Timko, 1999[28]	Not applicable	Done	Unclear	Not done	Done	Not done	Study excluded because the sample size was less than 10 and the allocation could not lead to balanced groups, and the controls were exposed to the intervention
McKenna, 1982[29]	Not done	Partially done	Unclear	Not done	Unclear	Unclear	Study excluded because the comparison groups had substantial differences at baseline, which made groups noncomparable
Stern, 1999[30]	Not done	Unclear	Unclear	Not done	Unclear	Done	Study excluded because the comparison groups had substantial differences at baseline, which made groups noncomparable
Frederick, 2000[31]	Partially done	Done	Partially done	Not done	Partially done	Unclear	Study excluded because the comparison groups had substantial differences at baseline, which made groups noncomparable
Campbell, 2001[32]	Partially done	Partially done	Done	Done	Partially done	Not done	Study excluded because there was insufficient compliance with the intervention (overall 57% attendance at the sessions), and the controls were exposed to the intervention
Engeland, 2002[33]	Partially done	Not done	Not done	Done	Partially done	Done	Study excluded because only 26% of the participating schools complied with the intervention, which raises serious concerns about the representativeness of the data
Capone, 2000[34]	Partially done	Done	Done	Done	Partially done	Not done	Study excluded because the controls were exposed to the intervention
Raynal, 1991[35]	Not done	Done	Done	Done	Unclear	Not done	Study excluded because the controls were exposed to the intervention
Breivik, 1980[36]	Unclear	Unclear	Unclear	Unclear	Unclear	Unclear	Study excluded because of insufficient information to evaluate the quality

Table 3. Characteristics of included studies.

Study by First Author	Education Level	Setting	Participants	Intervention	Outcome
Kelly, 2003[37]	Higher (nonuniversity)	Training at 2 women, infant, and children clinics.	Parents of young children who visited the clinic. N: 323 Age: u	I=video (9 min), pamphlets, and stickers about first aid for poisoning C=educational class on immunizations and healthy snacks	Assessment of knowledge about first aid for poisoning with written and oral interviews
Moore, 1987[38]	Primary	Training in primary school as a noncompulsory course.	Pupils from 2 primary schools. N: 92 A: 10–11 y	I(1)=training to develop assertive behavior to become a more autonomous health care consumer (6 h) I(2)=first aid training on bleeding, injuries, burns, shock, poisoning, and other topics outside the review (6 h) I(3)=assertion training (3 h) and first aid training on abovementioned topics (3 h) C=viewing science films without health education content (6 h)	Assessment of first aid knowledge and skills on first aid for bleeding, broken bones, burns, head injuries, poisoning, and heat stroke with a written test
Shotland, 1985[39]	Higher (university)	Training at university during a first aid program organized by the health education department.	Participants from the usual enrollments for the training program. Passing the examination led to an American Red Cross certificate. N: 209 A: u	I=first aid training on bleeding, wounds, and other undefined topics (duration=u) C=receiving same training as I but took tests before lesson on bleeding control (duration=u)	Assessment of helping behavior during a deception experiment with a simulated arterial bleeding emergency
Hawks, 1998[40]	Higher (university)	Training at university during a health and wellness education course open to all university students.	Recruitment through the normal university registration process. Reasons for participation in the training included personal interest, as a compulsory lesson, or as an elective. N: 98 A: 18-53 y	I(1)=first aid training that also addressed 2 inhibitors of emergency helping behavior, ie, situational ambiguity and bystander effect (duration=u) I(2)=first aid training that mentioned inhibitors of helping behavior (duration=u) I(3)=first aid training without focus on inhibitors of helping behavior (duration=u) C=no first aid training	Assessment of first aid knowledge and skills with a written and a practical test (no data reported in study) Assessment of helping rates during a deception experiment with a simulated chest pain emergency

u, Unclear; *I,* intervention group; *C,* control group.

All studies measured the final outcomes within 1 month of the intervention.[37-40]

Risk of Bias

Table 1 summarizes the methodological quality of the 4 included studies.

Kelly et al[37] allocated the participants at random to the comparison groups but did not report on procedures for concealment. Eighty-nine percent of the enrolled participants completed the study. Researchers, blind to group status, recorded the outcomes. The study obtained baseline measurements of first aid knowledge. There was no reporting

of reliability data. Protection against contamination was adequate. The study did not provide indications of face validity and did not report whether content experts had reviewed the test instrument before the trial. However, we could derive content validity indications from the provided test descriptions. We assessed the study to have a high risk of bias.

Moore[38] did not use concealment of randomization. All the enrolled participants completed the study. The study failed to blind the researchers. Baseline measurements recorded first aid knowledge and skills with a written test. The test instrument had a satisfactory internal consistency (Crohnbach's α of 0.67). Protection against contamination was adequate. Face validity was evaluated by asking the children for feedback on the test instruments in a pilot study with another primary school. Content experts had reviewed the test instrument before the trial, and the provided test descriptions showed content validity. Participants who received more training also performed better on the test, which indicates construct validity. We assessed the study to have a high risk of bias.

Shotland and Heinold[39] allocated the participants at random to the comparison groups but did not describe procedures for concealment. Seventy-eight percent of the enrolled participants completed the study. The study failed to report on blinding and protection against contamination. The study did not record baseline measurements of outcomes. Video recordings allowed verifying the helping behavior data of participants obtained by the researcher's observations. No data collection errors were detected. Demonstrating a high credibility with suspicion scores about the induced deception provided an indirect indication of face validity. It was unclear whether content experts had reviewed the test instrument before the trial, but the provided test descriptions showed content validity. We assessed the study to have a high risk of bias.

Hawks and Egan[40] allocated the participants to the comparison groups through the normal university registration process. All the enrolled participants completed the study. Researchers, blind to group status, recorded the outcomes. Baseline measurements recorded first aid knowledge and skills. Video recordings allowed verifying the helping behavior data of participants obtained by the researcher's observations. No data collection errors were detected. Protection against contamination was adequate. The study did not report whether content experts had reviewed the test instrument before the trial, but the test descriptions provided in the article showed content validity. It was unclear whether the study obtained suspicion scores, but this study did report high credibility of the test in a preceding experiment. Participants who received more training also performed better on the test, which indicates construct validity. In relation to concurrent and predictive validity, the study compared scores on a written and practical test with scores during the helping behavior test. The 3 groups trained in first aid had comparable scores on the written knowledge test and practical skills test. The control group did not take the written and practical test. Therefore, no actual comparison between scores on both tests was possible. We assessed the study to have a moderate risk of bias.

Table 4. Summary of main findings.

Study by First Author	Outcome Measure	Main Results
Kelly, 2003[37]	Written and oral test on first aid knowledge for poisoning	Trained persons have a significantly better knowledge of Poison Control Center telephone number (RR 3.35; 95% CI 2.33 to 4.81) and of first aid in case of household bleach ingestion (RR 2.11; 95% CI 1.64 to 2.72)
Moore, 1987[38]	Written test on first aid knowledge and skills for multiple cases	Significantly better knowledge and skills among those with 6 h first aid training vs controls (MD 4.75; 95% CI 3.02 to 6.48) and among those with 3 h first aid training vs controls (MD 2.56; 95% CI 0.99 to 4.13)
Shotland, 1985[39]	Deception experiment on helping behavior	Trained participants provided significantly better first aid for an arterial bleeding emergency (RR 25.94; 95% CI 3.60 to 186.93). Helping response for an arterial bleeding emergency did not occur significantly more in trained participants (RR 1.13; 95% CI 0.88 to 1.45)
Hawks, 1998[40]	Deception experiment on helping behavior	Helping response for a chest pain emergency did not significantly differ between people trained in first aid only and controls (RR 1.36; 95% CI 0.28 to 6.61). Helping response occurred significantly more among those trained in first aid and helping behavior than among those trained in first aid only (RR 2.80; 95% CI 1.05 to 7.50) or than in controls (RR 3.81; 95% CI 0.98 to 14.89)

RR, relative risk; *MD,* mean difference.

Effect of Intervention

Table 4 gives a summary of the most important study results. Without statistical pooling, forest plots graphically represent the

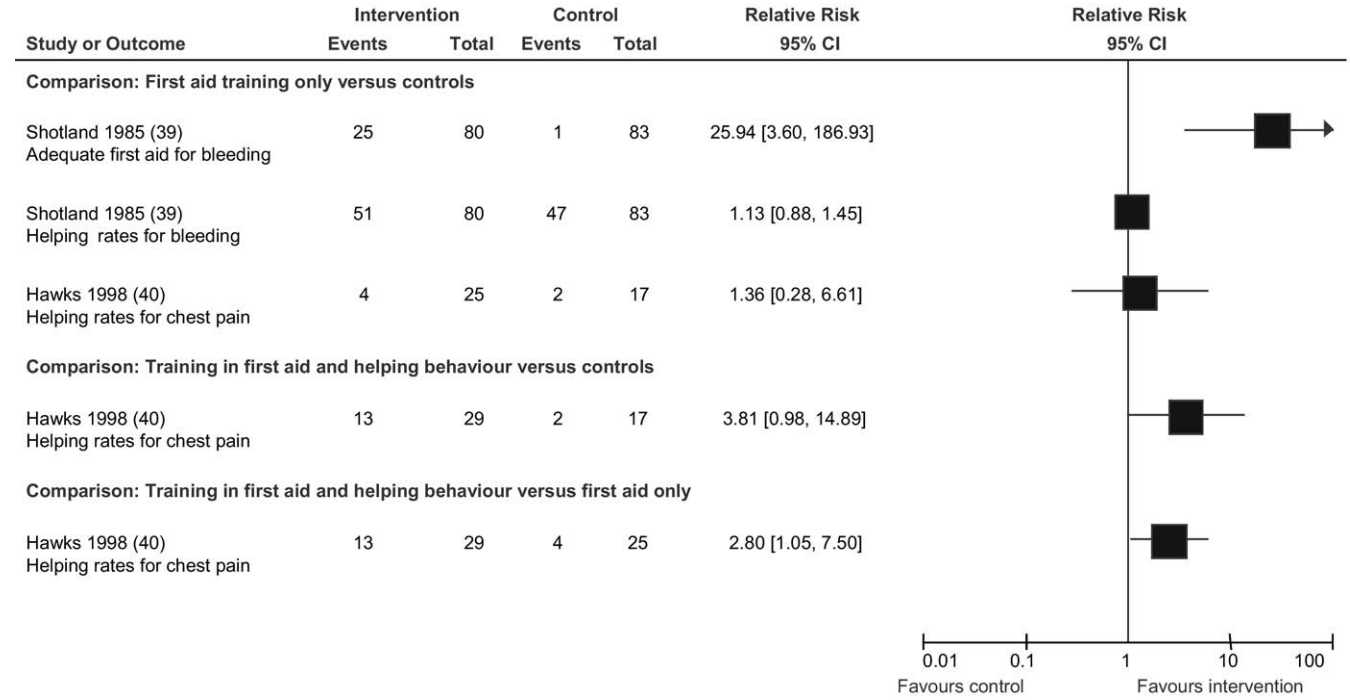

Figure 2. Forest plots without statistical pooling for the main findings of the 2 deception experiments first sorted by comparison and then by outcome. The squares display the effect sizes and the horizontal lines represent the CIs. If the CI crosses the vertical line, it means there is no statistically significant difference.

main findings of the 2 deception experiments (Figure 2). Appendix E3 contains the data used to calculate relative risk and mean difference for each comparison (available at http://www.annemergmed.com).

The study by Kelly et al[37] on first aid for poisoning revealed significantly better knowledge of the Poison Control Center work and its telephone number in trained participants (relative risk 3.35; 95% confidence interval [CI] 2.33 to 4.81). In the case of household bleach ingestion, participants receiving the intervention responded twice more that they would call the Poison Control Center (relative risk 2.11; 95% CI 1.64 to 2.72).

The study by Moore[38] with first aid training for primary education children revealed significantly greater knowledge and skills in the groups receiving first aid instruction. The highest difference in scores occurred between the group receiving 6 hours of first aid training and the control group (mean difference 4.75; 95% CI 3.02 to 6.48; range of scores possible 0 to 20).

The study by Shotland and Heinold[39] that unexpectedly confronted university students with an arterial bleeding emergency revealed that some type of help was provided by 64% of participants in the intervention group and by 57% of the controls. The chances of helping were not significantly different when comparing the 2 groups (relative risk 1.13; 95% CI 0.88 to 1.45). The number of respondents was significantly higher in unambiguous situations (relative risk 1.86; 95% CI 1.40 to 2.46) and when individuals were alone with the casualty (relative risk 1.29; 95% CI 0.99 to 1.66). Untrained participants limit their helping response significantly more to a call for help only. Trained participants applied significantly more direct pressure to control the bleeding than untrained and performed much better in the combination of calling for help and applying direct pressure (relative risk 25.94; 95% CI 3.60 to 186.93).

The study by Hawks and Egan[40] about university students' helping response to a chest pain emergency found a 45% helping rate in the group trained in first aid and helping behavior. Some type of help was provided by 26% of the participants in the group addressing helping behavior in small measure, 16% in the group receiving first aid training without helping behavior, and 12% among controls. Levels of significance were reached when helping behavior in the group being trained in first aid and helping behavior with those trained in first aid only were compared (relative risk 2.80; 95% CI 1.05 to 7.50) or with controls (relative risk 3.81; 95% CI 0.98 to 14.89). The group addressing helping behavior in small measure did not help significantly more than controls (relative risk 2.20; 95% CI 0.52 to 9.39), nor did the group receiving first aid training without focus on helping behavior (relative risk 1.36; 95% CI 0.28 to 6.61).

LIMITATIONS

Analyzing the references obtained per source shows that the search strategy can be improved. Searching MEDLINE (PubMed), EMBASE (EMBASE.com), PsycINFO

(WebSPIRS), and Education Resources Information Center database (EBSCOhost) would have been sufficient to retrieve all relevant studies. Dropping key words such as "emergency medicine" and "emergency nursing" that did not add any relevant studies for laypersons can increase the specificity of the search formula. Inserting a set of terms describing the criteria for study designs is recommended. Adding search terms on specific first aid topics to the intervention terms and adding the key words "helping behavior" with OR to the outcome terms could be meaningful. As a result of checking the reference lists of all relevant studies and related systematic reviews and communication with authors of included studies, it is highly unlikely that any studies were missed. As a safeguard, we performed the search again after completion of the review, adding the key words "helping behavior," but this did not reveal any new relevant studies.

A limitation in our study is that we excluded studies from our review if they were not reported in English, German, French, or Dutch. Because emergency and disaster preparedness is a topic of major interest in Europe and elsewhere, it is possible that important studies were not included in our review because of their choice of language.

Another potential source of bias is incomplete reporting of results in the published reports of the studies included in the review. We contacted the authors of the 4 included studies with a request for more information, and 2 replied with more details.

The research question in this systematic review was broadly defined and included 4 studies that vary by either participants or outcomes, making it difficult to make generalizations.

DISCUSSION

We know of 3 previous reviews on the effectiveness of cardiopulmonary resuscitation training for cardiac arrest.[15-17] We believe that ours is the first systematic review of effectiveness of nonresuscitative first aid training in laypersons.

All 4 of the studies we included found statistically significant effects of first aid training on either competence or helping behavior of laypersons.[37-40] The identification of 2 studies that measured the change in practice[39,40] is an important finding of this review.

Although first aid training appeared to improve the quality of first aid procedures for a bleeding emergency, it did not lead to an increased helping rate.[39] The presence of bystanders and ambiguity of situations emerged as barriers to providing help.[39] A training program that focused both on first aid procedures and on inhibitors of emergency helping behavior was closely associated with higher helping rates in a chest pain emergency.[40] Although it is difficult to generalize from the 4 included studies, it nonetheless appears likely that traditional first aid training improves objectively measured skill competences more than it develops a positive attitude and a likely helping reaction toward emergencies on the part of trainees.

The evidence available to answer our question about the effects of first aid training is of low quality and incomplete. The studies provided little evidence to demonstrate validity and reliability of the outcome measures. Data are available only for single-victim emergencies. The findings are also limited to training at a few educational levels and to observation of effectiveness of training within 1 month of the intervention. Furthermore, the evidence is too limited to take into account either the variability across cases or across components of competence.

New rigorous randomized controlled trials on the acquisition and retention of competence and the modification of helping behavior are needed to explore the provisional findings of our systematic review. The key challenge is to gain a better understanding of actual helping behavior. The study by Hawks and Egan[40] found that 45% of the participants trained in first aid and helping behavior demonstrated helping behavior. Participants in this group helped almost 3 times more than those trained in first aid only. Nevertheless, 55% of trainees did not provide any type of help. Many social psychology experiments have studied the influence of different contextual, personal, group, and victim factors in medical emergencies. A systematic review of these studies might help to determine which factors are likely to be important for enhancement of the effectiveness of first aid training.

Deception experiments seem potentially useful means of testing behavioral effectiveness. However, there are some ethical considerations. Researchers may only use deception as part of a study if the research question is important and deception is the only way to obtain valid data. Participants should be informed about the risks involved in the research and should not be exposed to harmful stress levels. At the end of the study, participants should be fully debriefed.[41] Caution is also recommended to prevent harm to simulation participants.

First aid covers assistance for a variety of situations. It is important for the validity of the outcome measures to gather a broad sample of performance when possible. When new randomized controlled trials are designed, it is essential to carefully consider the reliability and validity of the outcomes that will be measured. Apart from the recommendation above, we refer readers to the *International Handbook of Research in Medical Education*, by Norman et al,[42] for an elaborate discussion on obtaining valid and reliable outcome measurements.

In conclusion, on the basis of 2 studies with university students, programs for first aid that also train participants to overcome inhibitors of emergency helping behavior could lead to better help and higher helping rates for single-victim emergencies. Because the overall completeness and quality of the evidence is low, full confidence in this conclusion should be considered to be contingent on further research.

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APPENDIX E1. Search formulas.

This file contains further details of methods to accompany the paper Effectiveness of non-resuscitative first aid training for increasing competence and helping behaviour in laypersons: A systematic review by Stijn Van de Velde, Annemie Heselmans, Ann Roex, Philippe Vandekerckhove, Dirk Ramaekers and Bert Aertgeerts.

Below we describe the search formula per consulted database. We consulted the databases on 4 June 2007.

MEDLINE (Pubmed)

1. "First Aid"[mh] OR "Emergency Treatment"[mh:NoExp] OR "Emergency Medicine"[mh] OR "Emergency Nursing"[mh] OR "first aid"[tw] OR "first response"[tw] OR "prehospital care"[tw] OR "prehospital management"[tw] OR lifesupport*[tw] OR "life support*"[tw] OR lifesaving[tw] OR "life saving"[tw] OR "wilderness medicine"[tw] OR "mountain rescue*"[tw]
2. "Education"[mh] OR "Learning"[mh] OR educat*[tw] OR train*[tw] OR teach*[tw] OR instruct*[tw] OR learn*[tw]
3. 1 AND 2
4. "Educational Measurement"[mh] OR "Health Knowledge, Attitudes, Practice"[mh] OR "Knowledge"[mh] OR "Psychomotor Performance"[mh] OR "Competency-Based Education"[mh] OR knowledge[tw] OR attitude*[tw] OR skill*[tw] OR competenc*[tw] OR perform*[tw] OR abilit*[tw]
5. "Retention (psychology)"[mh] OR retention[tw] OR retain*[tw] OR recall*[tw] OR maintenance[tw] OR maintain*[tw] OR remember*[tw]
6. 4 OR 5
7. 3 AND 6

EMBASE (EMBASE.com)

1. ('first aid'/de OR 'emergency treatment'/de OR 'emergency medicine'/de OR 'emergency nursing'/de OR 'first aid':ab,ti OR 'first response':ab,ti OR 'prehospital care':ab,ti OR 'prehospital management':ab,ti OR lifesupport*:ab,ti OR 'life support*':ab,ti OR lifesaving:ab,ti OR 'life saving':ab,ti OR 'wilderness medicine':ab,ti OR 'mountain rescue*':ab,ti)
2. ('Education'/exp OR 'Learning'/exp OR educat*:ab,ti OR train*:ab,ti OR teach*:ab,ti OR instruct*:ab,ti OR learn*:ab,ti)
3. 1 AND 2
4. ('attitude to health'/de OR 'knowledge'/exp OR 'psychomotor performance'/exp OR knowledge:ab,ti OR attitude*:ab,ti OR skill*:ab,ti OR competenc*:ab,ti OR perform*:ab,ti OR abilit*:ab,ti)
5. long term memory'de OR retention:ab,ti OR retain*:ab,ti OR recall*:ab,ti OR maintenance:ab,ti OR maintain*:ab,ti OR remember*:ab,ti
6. 4 OR 5
7. 3 AND 6

COCHRANE CENTRAL (Wiley)

1. MeSH descriptor Emergency Treatment, this term only
2. MeSH descriptor First Aid, this term only
3. MeSH descriptor Emergency Medicine explode all trees
4. MeSH descriptor Emergency Nursing explode all trees
5. first aid OR "first response" OR "prehospital care" OR "prehospital management" OR lifesupport* OR "life support*" OR lifesaving OR "life saving" OR "wilderness medicine" OR "mountain rescue*"
6. OR 1-5
7. MeSH descriptor Education explode all trees
8. MeSH descriptor Learning explode all trees
9. educat* OR train* OR teach* OR instruct* OR learn*
10. OR 7-9
11. 6 AND 10
12. MeSH descriptor Educational Measurement explode all trees
13. MeSH descriptor Health Knowledge, Attitudes, Practice explode all trees
14. MeSH descriptor Knowledge explode all trees
15. MeSH descriptor Psychomotor Performance explode all trees
16. MeSH descriptor Competency-Based Education explode all trees
17. MeSH descriptor Retention (Psychology) explode all trees
18. knowledge OR attitude* OR skill* OR competenc* OR perform* OR abilit* OR retention OR retain* OR recall* OR maintenance OR maintain* OR remember*
19. OR 12-18
20. 11 AND 19

C2-SPECTR (Campbell Collaboration)

1. {first aid} OR {first response} OR {prehospital care} OR {prehospital management} OR {lifesupport} OR {life support} OR {lifesaving} OR {life saving} OR {wilderness medicine} OR {mountain rescue} OR {Emergency Treatment} OR {Emergency Medicine} OR {Emergency Nursing} [SEARCH ALL INDEXED FIELDS]
2. {first aid} OR {first response} OR {prehospital care} OR {prehospital management} OR {lifesupport} OR {life support} OR {lifesaving} OR {life saving} OR {wilderness medicine} OR {mountain rescue} OR {Emergency Treatment} OR {Emergency Medicine} OR {Emergency Nursing} [SEARCH ALL NON-INDEXED FIELDS]
3. 1 OR 2

CINAHL (EBSCOhost)

1. (MH "First Aid") or (MH "First Aid (Iowa NIC)") or "first aid" or "first response" or lifesupport* or "life support*" or lifesaving or "life saving" or "wilderness medicine" or "mountain rescue*"

2. (MH "Education+") or (MH "Learning+") or educat* OR train* OR teach* OR instruct* OR learn*
3. 1 AND 2
4. (MH "Educational Measurement+") or (MH "Knowledge+") or (MH "Health Knowledge and Behavior (Iowa NOC) (Non-Cinahl)+") or (MH "Education, Competency-Based") or (MH "Psychomotor Performance+") or (MH "Student Performance Appraisal+") OR knowledge OR attitude* OR skill* OR competenc* OR perform* OR abilit*
5. (MH "Skill Retention") OR retention OR retain* OR recall* OR maintenance OR maintain* OR remember*
6. 4 0R 5
7. 3 AND 6

BRITISH NURSING INDEX AND ARCHIVE (OVID)

1. first aid/ or "accident and emergency nursing"/ or ("first aid" or "first response" or "prehospital care" or "prehospital management" or lifesupport$ or life support$ or lifesaving or life saving or wilderness medicine or mountain rescue$).mp. [mp=title, abstract, heading words]
2. (educat$ or train$ or teach$ or instruct$ or learn$).mp. [mp=title, abstract, heading words]
3. 1 AND 2
4. knowledge OR attitude$ OR skill$ OR competenc$ OR perform$ OR abilit$
5. retention OR retain$ OR recall$ OR maintenance OR maintain$ OR remember$
6. 4 OR 5
7. 3 AND 6

SPORTSDISCUS (EBSCOhost)

1. (DE "FIRST aid in illness & injury") or (DE "EMERGENCY medicine") or "first aid" OR "first response" OR "prehospital care" OR "prehospital management" OR lifesupport* OR "life support*" OR lifesaving OR "life saving" OR "wilderness medicine" OR "mountain rescue*"
2. DE "EDUCATION" OR DE "HEALTH education" OR DE "OUTDOOR education" OR DE "PHYSICAL education & training" OR DE "SPECIAL education" or DE "TRAINING" or DE "TEACHING" OR educat* OR train* OR teach* OR instruct* OR learn*
3. 1 AND 2
4. DE "PERFORMANCE" OR DE "JOB performance" OR knowledge OR attitude* OR skill* OR competenc* OR perform* OR abilit*
5. retention OR retain* OR recall* OR maintenance OR maintain* OR remember*
6. 4 OR 5
7. 3 AND 6

PsycINFO (WebSPIRS)

1. "first aid" or "first response" or "prehospital care" or "prehospital management" or lifesupport* or "life support*" or lifesaving or "life saving" or "wilderness medicine" or "mountain rescue*"
2. (explode "Teaching-" in MJ,MN) or (explode "Training-" in MJ,MN) or (explode "Learning-" in MJ,MN) or (explode "Education-" in MJ,MN) or (educat* or train* or teach* or instruct* or learn*)
3. 1 AND 2
4. (knowledge or attitude* or skill* or competenc* or perform* or abilit*) or ((explode "Ability-+" in MJ,MN) or (explode "Competence-" in MJ,MN) or (explode "Performance-+" in MJ,MN)) or (explode "Knowledge-Level" in MJ,MN) or (("Health-Attitudes" in MJ,MN) or ("Health-Behavior" in MJ,MN) or ("Health-Knowledge" in MJ,MN)) or (explode "Educational-Measurement" in MJ,MN)
5. "Retention (psychology)"[mh] OR retention[tw] OR retain*[tw] OR recall*[tw] OR maintenance[tw] OR maintain[tw] OR remember*[tw]
6. 4 OR 5
7. 3 AND 6

ERIC (EBSCOhost)

1. DE "First Aid" OR "first aid" OR "first response" OR lifesupport* OR "life support*" OR lifesaving OR "life saving" OR "wilderness medicine" OR "mountain rescue*"
2. (DE "Education" OR DE "Academic Education" OR DE "Adult Education" OR DE "After School Education" OR DE "Aging Education" OR DE "Allied Health Occupations Education" OR DE "Citizenship Education" OR DE "Coeducation" OR DE "Community Education" OR DE "Comparative Education" OR DE "Compensatory Education" OR DE "Competency Based Education" OR DE "Compulsory Education" OR DE "Corporate Education" OR DE "Correctional Education" OR DE "Distance Education" OR DE "Driver Education" OR DE "Early Childhood Education" OR DE "Elementary Secondary Education" OR DE "Family Life Education" OR DE "Health Education" OR DE "Industrial Education" OR DE "Informal Education" OR DE "Inservice Education" OR DE "Intergroup Education" OR DE "Leisure Education" OR DE "Migrant Education" OR DE "Noncategorical Education" OR DE "Nondiscriminatory Education" OR DE "Nonformal Education" OR DE "Nontraditional Education" OR DE "Open Education" OR DE "Outcome Based Education" OR DE "Outdoor Education" OR DE "Patient Education" OR DE "Physical Education" OR DE "Police Education" OR DE "Popular Education" OR DE "Population Education" OR DE "Postsecondary Education" OR DE "Process Education" OR DE "Professional Education" OR DE "Progressive Education" OR DE "Public Education" OR DE "Rural

Education" OR DE "Safety Education" OR DE "Special Education" OR DE "Supplementary Education" OR DE "Urban Education" OR DE "Values Education" OR DE "Vocational Education" OR DE "Womens Education")

3. (DE "Learning" OR DE "Active Learning" OR DE "Adult Learning" OR DE "Associative Learning" OR DE "Aural Learning" OR DE "Cooperative Learning" OR DE "Discovery Learning" OR DE "Discrimination Learning" OR DE "Experiential Learning" OR DE "Incidental Learning" OR DE "Intentional Learning" OR DE "Lifelong Learning" OR DE "Mastery Learning" OR DE "Multisensory Learning" OR DE "Nonverbal Learning" OR DE "Observational Learning" OR DE "Prior Learning" OR DE "Problem Based Learning" OR DE "Rote Learning" OR DE "Sequential Learning" OR DE "Serial Learning" OR DE "Transfer of Training" OR DE "Transformative Learning" OR DE "Verbal Learning" OR DE "Visual Learning")
4. (DE "Instruction" or DE "Assignments" OR DE "College Instruction" OR DE "Concept Teaching" OR DE "Group Instruction" OR DE "Home Instruction" OR DE "Individual Instruction" OR DE "Mass Instruction")
5. (DE "Training" OR DE "Caregiver Training" OR DE "Industrial Training" OR DE "Job Training" OR DE "Laboratory Training" OR DE "Military Training" OR DE "Professional Training" OR DE "Retraining" OR DE "Volunteer Training")
6. OR 2-5
7. 1 AND 6
8. DE "Knowledge Level" or DE "Ability" OR DE "Cognitive Ability" OR DE "Competence" OR DE "Skills" or DE "Performance" OR DE "Job Performance" OR DE "Attitudes" OR knowledge OR attitude* OR skill* OR competenc* OR perform* OR abilit*
9. DE "Recall (Psychology)" or DE "Retention (Psychology)" OR retention OR retain* OR recall* OR maintenance OR maintain* OR remember*
10. 8 OR 9
11. 7 AND 10

APPENDIX E2. EPOC quality criteria for randomized controlled trials.

This file contains further details of methods to accompany the paper Effectiveness of non-resuscitative first aid training for increasing competence and helping behaviour in laypersons: A systematic review by Stijn Van de Velde, Annemie Heselmans, Ann Roex, Philippe Vandekerckhove, Dirk Ramaekers and Bert Aertgeerts.

Below we list the EPOC Quality criteria for randomised controlled trials. (Extract from the following reference : Cochrane Effective Practice and Organisation of Care Group. The data collection checklist. Available at: http://www.epoc.uottawa.ca/checklist2002.doc.)

Seven standard criteria are used for randomised controlled trials and controlled clinical trials included in EPOC reviews:

a) Concealment of allocation (protection against selection bias)

Score DONE if the unit of allocation was by institution, team or professional and any random process is described explicitly, e.g. the use of random number tables or coin flips; the unit of allocation was by patient or episode of care and there was some form of centralised randomisation scheme, an on-site computer system or sealed opaque envelopes were used.

Score NOT CLEAR if the unit of allocation is not described explicitly; the unit of allocation was by patient or episode of care and the authors report using a 'list' or 'table', 'envelopes' or 'sealed envelopes' for allocation.

Score NOT DONE if the authors report using alternation such as reference to case record numbers, dates of birth, day of the week or any other such approach (as in CCTs);

the unit of allocation was by patient or episode of care and the authors report using any allocation process that is entirely transparent before assignment such as an open list of random numbers or assignments; allocation was altered (by investigators, professionals or patients).

b) Follow-up of professionals (protection against exclusion bias)

Score DONE if outcome measures obtained for 80-100% of subjects randomised. (Do not assume 100% follow up unless stated explicitly.); Score NOT CLEAR if not specified in the paper; Score NOT DONE if outcome measures obtained for less than 80% of subjects randomised.

c) Follow-up of patients or episodes of care

Score DONE if outcome measures obtained for 80-100% of subjects randomised or for patients who entered the trial. (Do not assume 100% follow up unless stated explicitly.) Score DONE if there is an objective data collection system; Score NOT CLEAR if not specified in the paper; Score NOT DONE if outcome measures obtained for less than 80% of subjects randomised or for less than 80% of patients who entered the trial.

d) Blinded assessment of primary outcome(s)* (protection against detection bias)

Score DONE if the authors state explicitly that the primary outcome variables were assessed blindly OR the outcome variables are objective, e.g. length of hospital stay, drug levels as assessed by a standardised test; Score NOT CLEAR if not specified in the paper; Score NOT DONE if the outcome(s) were not assessed blindly.

* Primary outcome(s) are those variables that correspond to the primary hypothesis or question as defined by the authors. In the event that some of the primary outcome variables were assessed in a blind fashion and others were not, score each separately and label each outcome variable clearly.

e) Baseline measurement

Score DONE if performance or patient outcomes were measured prior to the intervention, and no substantial differences

were present across study groups; Score NOT CLEAR if baseline measures are not reported, or if it is unclear whether baseline measures are substantially different across study groups; Score NOT DONE if there are differences at baseline in main outcome measures likely to undermine the post intervention differences (e.g. are differences between the groups before the intervention similar to those found post intervention).

f) Reliable primary outcome measure(s)*

Score DONE if two or more raters with at least 90% agreement or kappa greater than or equal to 0.8 OR the outcome is obtained from some automated system e.g. length of hospital stay, drug levels as assessed by a standardised test; Score NOT CLEAR if reliability is not reported for outcome measures that are obtained by chart extraction or collected by an individual; Score NOT DONE if agreement is less than 90% or kappa is less than 0.8.

* In the event that some outcome variables were assessed in a reliable fashion and others were not, score each separately on the back of the form and label each outcome variable clearly.

g) Protection against contamination

Score DONE if allocation was by community, institution or practice and it is unlikely that the control received the intervention; Score NOT CLEAR if professionals were allocated within a clinic or practice and it is possible that communication between experimental and group professionals could have occurred;

Score NOT DONE if it is likely that the control group received the intervention (e.g. cross-over trials or if patients rather than professionals were randomised).

APPENDIX E3. Data and comparisons.

This file contains further details of methods to accompany the paper Effectiveness of non-resuscitative first aid training for increasing competence and helping behaviour in laypersons: A systematic review by Stijn Van de Velde, Annemie Heselmans, Ann Roex, Philippe Vandekerckhove, Dirk Ramaekers and Bert Aertgeerts.

Below we describe the comparisons and data used to calculate RR or MD.

Kelly 2003

Knowledge of the Poison Control Center telephone number (First aid training versus Controls)

Intervention n	Intervention N	Control n	Control N
91	145	27	144

Knowledge of first aid for ingestion of household bleach (First aid training versus Controls)

Intervention n	Intervention N	Control n	Control N
102	145	48	144

Moore 1987

Knowledge of first aid for multiple cases (First aid training versus Controls)

Intervention N	Intervention Mean	Intervention SD	Control N	Control Mean	Control SD
22	11.92	3.43	23	7.17	2.35

Shotland 1985

Helping rates for bleeding (First aid training versus Controls)

Intervention n	Intervention N	Control n	Control N
51	80	47	83

Helping rates for bleeding (Unambiguous versus Ambiguous situation)

Unambiguous n	Unambiguous N	Ambiguous n	Ambiguous N
64	82	34	81

Helping rates for bleeding (Alone versus Group situation)

Alone n	Alone N	Group n	Group N
56	83	42	80

Adequate first aid for bleeding (First aid training versus Controls)

Intervention n	Intervention N	Control n	Control N
25	80	1	83

Hawks 1998

Helping rates for chest pain (Training in first aid and helping behaviour versus Controls)

Intervention n	Intervention N	Control n	Control N
13	29	2	17

Helping rates for chest pain (Training in first aid and helping behaviour (small measure) versus Controls)

Intervention n	Intervention N	Control n	Control N
7	27	2	17

Helping rates for chest pain (First aid training only versus Controls)

Intervention n	Intervention N	Control n	Control N
4	25	2	17

Helping rates for chest pain (Training in first aid and helping behaviour versus First aid training only)

Intervention n	Intervention N	Intervention (3) n	Intervention (3) N
13	29	4	25