Individual-Level Injury Prevention Strategies in the Clinical Setting

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Abstract

Health care providers have numerous opportunities to intervene with parents and children to promote child safety practices that reduce rates of unintentional injuries. These individual-level interventions may be delivered in a variety of settings such as physician offices, clinics, emergency departments, or hospitals. This article systematically reviews 22 randomized controlled trials (RCTs) that examined the impact of interventions delivered in the clinical setting on child safety practices and unintentional injuries. The results indicate that counseling and other interventions in the clinical setting are effective at increasing the adoption of some safety practices, but not others. Specifically, motor vehicle restraint use, smoke alarm ownership, and maintenance of a safe hot tap water temperature were more likely to be adopted following interventions in the clinical setting. Clinical interventions were not proven effective at increasing a variety of safety practices designed to protect young children from injuries in the home, increasing bicycle helmet use, or reducing the occurrence of childhood injuries, though few studies examined the latter two outcomes. Clinical interventions were most effective when they combined an array of health education and behavior change strategies such as counseling, demonstrations, the provision of subsidized safety devices, and reinforcement. The article concludes with implications for research and practice.

Unintentional injuries are the leading cause of death for children and adolescents ages 1 to 19, accounting for more than 13,000 deaths each year in the United States. Effective prevention strategies are needed to reduce this toll. This article will address injury prevention strategies focused at the level of the individual. Individual-level strategies can be offered in varied settings such as homes, residential institutions, work sites, clinics, emergency departments, or hospitals. The role home visitors play in preventing childhood injuries has previously been reviewed. This article will focus on individual-level interventions delivered in clinical settings, including primary care (for example, physician offices, clinics) and acute care (for example, emergency departments, hospitals).
To evaluate the effectiveness of such interventions, this article will examine the results of a systematic review of randomized controlled trials, or RCTs (see Box 1). It will describe the evidence from this systematic review in response to two primary questions: (1) What are the effects of individual-level child and adolescent injury prevention interventions delivered in the clinical setting on safety practices? (2) What are the effects of such interventions on injuries? The synthesis of evidence from all studies reviewed will be presented first, followed by a more detailed discussion of specific safety practices and injury outcomes. Finally, this article will examine implications of these trials for practice and research.

Evaluating Individual-Level Injury Prevention Strategies

Studies were included in the systematic review if (1) the intervention was designed to prevent unintentional injuries to children or adolescents under 20 years of age; (2) the intervention was delivered in a "clinical setting," such as a physician's office, a clinic, an emergency department, or a hospital; (3) participants were assigned randomly to intervention and control groups; and (4) the study collected data on injuries, events that can cause injuries (for example, falls), or safety practices (for example, seat belt use). This systematic review was restricted to RCTs because effectiveness information from such studies is less prone to bias than is information from other study designs, and because clinical settings can be studied by using a randomized controlled design (see Box 2). The review also considered how well each trial met certain design principles that can influence study results even when random assignment is used (see Box 3). The strategies used to find relevant trials and search results, the assessment of study design issues, and analytic methods used are described in the Appendix at the end of this article.

The systematic review identified 22 relevant RCTs that met the above conditions (see Tables 1 to 5). Most trials assessed the effect of the intervention on safety behaviors rather than injuries. The outcomes assessed included motor vehicle restraint use, bicycle helmet use, safe hot tap water temperature, smoke alarm ownership, and a variety of safety practices designed to protect young children from injuries in the home (identified collectively as "childproofing" the home). The review found no relevant trials evaluating clinical interventions designed to prevent motorcycle, pedestrian, drowning, alcohol-related, or firearm injuries, despite their importance as causes of unintentional injuries among children and adolescents (see the article by Grossman in this journal issue).

The targeted population and the type and duration of intervention varied among the studies examined. Most trials enrolled pregnant women or the parents of infants or preschool-age children. Only three primarily targeted school-age children, and none specifically targeted adolescents. Most experimental interventions tested included written educational materials and verbal counseling, sometimes combined with strategies to reinforce the message delivered. About one-third of the trials evaluated the effect of offering subsidized safety devices, such as a free car seat loan program or smoke alarm discount coupons. These factors, in addition to the design of the evaluation and the outcome measures selected, were considered in the evaluation of the scientific evidence of injury prevention strategies in the clinical setting.

The Effectiveness of Interventions in the Clinical Setting

An Overview of the Impact

To assess the overall effectiveness of childhood injury prevention strategies delivered in a clinical setting, evidence was synthesized from studies that examined safety practices and studies that examined injury outcomes.

Effects on Safety Practices

For most safety practices evaluated, counseling and other interventions in the clinical setting resulted in a greater likelihood of safety practice. For example, practices including motor vehicle restraint use, smoke alarm ownership, and maintenance of a safe heating system showed increased use after intervention. The review also found that interventions targeting specific safety practices, such as bicycle helmet use, had a greater impact than those targeting broader areas. However, the effectiveness of interventions varied depending on the type of practice, the population targeted, and the specific safety strategies employed.

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Why Systematic Reviews?

Given ever-increasing amounts of scientific information, it is not surprising that digesting current with this information is difficult. Yet health professionals, policymakers, and consumers can only make informed health-related decisions if the decisions are based on the most accurate and up-to-date information available. Too often, help is sought from incomplete or even inaccurate literature reviews. Many reviews do not explain how decisions were made about what was included in the review. Hence, the reader cannot judge whether such reviews accurately reflect the totality of evidence on a given topic. A review could be based, for example, on the studies of which the author happened to have copies. Such “file drawer” reviews may be more likely to include trials that support the authors’ preconceived opinions.1

In contrast, a systematic review uses a systematic approach to comprehensively identify relevant literature, hence minimizing selection bias, and makes explicit the methods used to conduct the review.1 Systematic reviews are more likely to reflect the true state of available information on the topic of interest. Key aspects of systematic reviews include (1) a well-formulated review question, (2) explicit inclusion criteria, (3) an attempt to identify and include all relevant evidence, (4) an adequate description of the methodology used to conduct the review, and (5) an effort to link explicitly the conclusions to the evidence.

In response to the need for systematic, up-to-date reviews of all relevant trials of health care, the Cochrane Collaboration was developed. This international organization aims to help people make informed decisions about health care by preparing, maintaining, and ensuring accessibility to systematic reviews of the effects of health care interventions. Cochrane reviews, which are prepared and maintained by international collaborative review groups, are published electronically in The Cochrane Database of Systematic Reviews. These reviews are readily available to inform and improve decision making about health care and research.

Endnote:


Estimating the potential benefit from each of these interventions depends on the baseline rate of safety practice as well as on the effectiveness of the intervention (as measured by the odds ratio). That is, all else being equal, the absolute effect of interventions delivered in the clinical setting on the rate of safety practice will be greater if the baseline rate of use in the target population is low. Based on estimates from the review, clinical counseling alone could increase short-term infant motor vehicle restraint use from the current 85%24 to about 88%, and toddler restraint use from the current 60%24 to about 66%. Interventions that provided subsidized car safety seats or extensive reinforcement could have a substantially greater effect. Clinical interventions also could have an important beneficial effect on unsafe hot tap water temperatures. For example, if 90% of households currently have a safe tap water temperature, then counseling could increase that proportion to about 95%. If the baseline proportion were only 70%, however, counseling could increase the prevalence of safe hot tap water temperature to as much as 84%. Similarly, given that an estimated 93% of homes in the United States are equipped with smoke alarms,25

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clinical interventions would be likely to increase this figure to 96%. All of these estimates are based on the assumption that interventions are equally effective in populations with high and low baseline rates of safety practice. As discussed later in this article, this may not necessarily be true.

Effects on Childhood Injuries

Only two RCTs in this systematic review collected data on the effect of counseling in the clinical setting on childhood injuries. These studies reported little or no effect on minor injuries, and the reduction in hospitalizations, though clinically important, was not statistically significant. As an estimate of the potential benefit, there were 1% fewer first-injury hospitalizations among intervention than control children (2.2% versus 3.1%) during the two-year trial of counseling in general practices. Because hospitalizations are costly, clinical interventions that produce even modest effects would be worthwhile if the interventions could be implemented cost-effectively. Sufficiently large trials that identify beneficial effects of counseling on injuries, that also consider costs in relation to effect and benefit, are therefore needed.

Impact on Targeted Safety Practices

The previous section provided an overview of the effectiveness of interventions delivered in the clinical setting on child safety practices and injury outcomes. This section examines in greater detail the effect of such interventions on specific safety practices, including motor vehicle restraint use, smoke alarm ownership, and safe hot tap water temperatures. Previous reviews of observational studies also report beneficial effects of these and other safety practices on childhood injuries. Thus, there is indirect evidence that individual-level interventions in the clinical setting are effective measures to reduce many childhood unintentional injuries.

Randomized Controlled Trials

The randomized controlled trial (RCT) is regarded as the gold standard for evaluating interventions. This is because the challenge for any evaluator is to assemble two or more comparable groups that have equal chances of experiencing the outcomes of interest. Observed differences in outcome between the intervention and comparison groups can then be reasonably attributed to the intervention (or to chance). Comparability can be achieved if chance alone, as occurs in random allocation, dictates whether a particular person or group of persons is assigned to a particular group. If persons are allocated to a particular study group using other methods (for example, by alternation or self-selection), there is a risk that the groups might differ in ways that are not due to chance. In such a case, observed differences after intervention may be due to factors other than either the intervention or chance. Because evaluators can have the most confidence in results from RCTs, whenever possible, evaluations of injury prevention interventions should use this design. Injury prevention interventions in the clinical setting are particularly suitable for evaluation in RCTs because the decision about which persons will receive the intervention usually can be controlled by the investigator and therefore can be randomly allocated. In contrast, engineering changes, product modification, legislation, and community programs are commonly evaluated using nonrandomized trials or observational studies because the decisions about which product or area receives the intervention are often determined by factors, such as politics or economics, that are beyond the investigator's control.
vehicle restraint use, bicycle helmet use, safe hot tap water temperature, smoke alarm ownership, and measures to childproof the home.

Strategies to childproof the home include changes that are made to the home specifically to protect young children from common household risks, and that would not typically be implemented in homes where no children reside (for example, outlet covers, stair gates).

Because only two trials evaluated injury outcomes and both of these focused primarily on home safety, these trials are discussed in the section titled “Strategies to Childproof the Home.” When appropriate, results were combined from multiple RCTs using meta-analysis (see Box 4).

**Motor Vehicle Restraint Use**

Motor vehicle occupant injuries are the leading contributor to injury mortality and morbidity among children and teenagers. Observational evidence indicates that, when used, infant car seats reduce the chance of serious injury or death by about 70%, toddler safety seats by 47%, and seat belts for older children and adolescents by about 45% (see the article by Grossman in this journal issue). Ten completed trials have evaluated interventions in the clinical setting to promote motor vehicle restraint use (see Table 1). All of the interventions studied provided information and encouragement to parents, though the extent of this educational component varied substantially. One intervention, for example, provided multiple, 15-minute counseling sessions in conjunction with well-child care. Two other trials involved limited education, instead focusing on noneducational interventions. One of these trials evaluated lending car seats to
The results from multiple randomized controlled trials (RCTs) can sometimes be combined quantitatively, using a statistical method called meta-analysis.1 By combining the results of different trials, meta-analysis provides an overall estimate of the effect of the intervention being studied, as well as an estimate of how precise the effect estimate is. Quantitatively combining results from multiple trials may not always be appropriate if there is significant variation in the results of the trials. In such cases, the reasons for the variation should be explored.

In an attempt to limit the variability between studies, we restricted the trials included in this systematic review by target population (children and adolescents younger than 20 years old), interventions (those aimed at preventing unintentional injuries), setting (clinical), outcomes (safety practices and injuries), and study design (RCTs). Because intervention effects may also vary with the intervention strategy (for example, educational counseling vs. offering free devices) and/or the safety practice being promoted (for example, advice on the installation of a smoke alarm vs. advice on bicycle helmet use), we only combined quantitatively studies that promoted the same safety practices and studies that used similar intervention strategies. We tested each meta-analysis to see whether the results of the included trials differed more than would be expected from chance alone. If there was statistically significant variability (called “heterogeneity”) among the individual trials, we explored differences in interventions, populations, settings, or designs that might explain this variability.

Endnote:

In summary, interventions in the clinical setting that emphasized resources or reinforcement significantly increased short-term motor vehicle restraint use, but they had a limited effect on long-term restraint use, while education alone had only modest effects on car seat use. These modest effects were not surprising, given that the educational interventions generally lacked key program elements that are recommended to influence behavior (see Box 5). There was no evidence about the effects on motor vehicle crash-related injuries, although a large body of observational and laboratory research has linked increased motor vehicle restraint use with a substantially reduced risk of injury.26,28

The majority of these studies were conducted before infant and child motor vehicle restraint legislation was enacted in all states, and similar interventions are unlikely to have

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**Figure 1**

<table>
<thead>
<tr>
<th>Study</th>
<th>OR</th>
<th>95% CI</th>
<th>Graphic Representation</th>
</tr>
</thead>
<tbody>
<tr>
<td>Scherz (1976)</td>
<td>1.93</td>
<td>0.92, 4.02</td>
<td></td>
</tr>
<tr>
<td>Miller (1977)</td>
<td>1.12</td>
<td>0.78, 1.60</td>
<td></td>
</tr>
<tr>
<td>Moffit (1981)</td>
<td>1.69</td>
<td>0.46, 6.14</td>
<td></td>
</tr>
<tr>
<td>Greenberg (1982)</td>
<td>2.48</td>
<td>0.72, 8.57</td>
<td></td>
</tr>
<tr>
<td>Christophersen (1985)</td>
<td>0.79</td>
<td>0.20, 3.07</td>
<td></td>
</tr>
<tr>
<td><strong>Total</strong></td>
<td>1.30</td>
<td>0.98, 1.74</td>
<td></td>
</tr>
<tr>
<td>Christophersen (1982)</td>
<td>59.18</td>
<td>2.95, 1,187.78</td>
<td></td>
</tr>
<tr>
<td>Liberato (1989)</td>
<td>4.94</td>
<td>3.26, 7.50</td>
<td></td>
</tr>
</tbody>
</table>

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a Less than six months after the intervention. Note: There is no summary estimate for all trials because of statistically significant variation between the trials.

b Odds Ratio (OR) measures the strength of the association between the intervention and the outcome. An OR=1.0 indicates no association. An OR>1.0 indicates a positive effect of the intervention on the outcome, and an OR<1.0 is a negative effect.

c The 95% Confidence Interval (CI) estimates the expected range of the OR 95 out of 100 times. A “statistically significant” result in this article is defined by a 95% CI that excludes 1.0.

d The box corresponds to the OR. A box to the right of 1.0 favors the intervention group, while a box to the left of 1.0 favors the control group. The length of the line corresponds to the 95% CI.
### Table 1

**Randomized Controlled Trials of Interventions in the Clinical Setting to Promote Motor Vehicle Restraint Use**

<table>
<thead>
<tr>
<th>Trial, Year, Country</th>
<th>Population Enrolled in Study</th>
<th>Intervention</th>
<th>Outcome Assessment</th>
<th>Allocation Concealment</th>
<th>Selected Outcomes</th>
</tr>
</thead>
<tbody>
<tr>
<td>Miller² 1977 United States</td>
<td>654 children 0 to 17 years old seen for WCC, private practice</td>
<td>I₁: Pamphlet about car restraints; I₂: Verbal instructions from pediatrician; I₃: Brief slide show on car restraints; C: No intervention</td>
<td>Self-reported usual and most recent use, two weeks later</td>
<td>Not described</td>
<td>Short-term use: 182/296 (61%); Short-term use: 130/221 (59%)</td>
</tr>
<tr>
<td>Scherz³ 1976 United States</td>
<td>500 parents of four-week-olds seen for WCC, military clinic</td>
<td>I₁: Display about car seats; I₂: Pamphlets about car seats; I₃: Nurse encouragement to read pamphlet and buy car seat; I₄: Physician encouragement to read pamphlet and buy car seat; C: No intervention</td>
<td>Self-reported usual use, collected at 8-week and 9- to 12-month WCCs</td>
<td>Not described</td>
<td>Short-term use: 64/400 (16%); Long-term use: 185/233 (79%); Short-term use: 9/100 (9%); Long-term use: 40/52 (77%)</td>
</tr>
<tr>
<td>Kelly⁴ 1987 United States</td>
<td>171 parents of six-month-olds seen for WCC</td>
<td>I: 15-minute developmentally oriented child safety education, hazard assessment, and handout by research physician at 6-, 9-, and 12-month WCCs; usual WCC; C: Usual WCC</td>
<td>Self-reported usual use 1 month after 12-month WCC</td>
<td>Not described</td>
<td>Long-term use: 18/55 (33%); Long-term use: 16/53 (30%)</td>
</tr>
<tr>
<td>Greenberg⁵ 1982 United States</td>
<td>Four prenatal classes (225 expectant parents)</td>
<td>I: Brief lecture on risk from auto crashes; car seat demonstration, postpartum use at hospital discharge; C: Postpartum car seat information</td>
<td>Blinded observed use at hospital discharge</td>
<td>Not adequate</td>
<td>Short-term use: 24/57 (42%); Short-term use: 4/18 (22%)</td>
</tr>
<tr>
<td>Moffit⁶ 1981 United States</td>
<td>50 pregnant women seen for prenatal care, public health clinic</td>
<td>I: Car seat education by health educator (pamphlet, demonstration, practice, film, lecture); low-cost car seat loan; C: Instructions for car seat use, low-cost car seat loan</td>
<td>Observed correct use at two-week WCC; self-reported use on last ride at six months; blinding not stated</td>
<td>Not described</td>
<td>Short-term use: 8/26 (31%); Long-term use: 10/17 (59%); Short-term use: 5/24 (21%); Long-term use: 8/15 (53%)</td>
</tr>
<tr>
<td>Williams⁷ 1988 United States</td>
<td>12 prenatal classes (165 pregnant women)</td>
<td>I: One-hour safety lecture, including motor vehicle safety and video; usual health education; C: One-hour lecture, handouts, and video on infant stimulation and feeding; usual safety education</td>
<td>Unblinded home inspection two to four weeks after birth</td>
<td>Adequate</td>
<td>Data not available; Data not available</td>
</tr>
<tr>
<td>Trial, Year, Country</td>
<td>Population Enrolled in Study</td>
<td>Intervention</td>
<td>Outcome Assessment</td>
<td>Allocation Concealment</td>
<td>Control</td>
</tr>
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<tr>
<td></td>
<td></td>
<td>I: One-hour safety lecture, including motor vehicle safety and video; usual safety education C: Usual safety education</td>
<td>Unblinded inspection of whether car seat installed six months after class; observed use at six-month WCCs</td>
<td>Not adequate</td>
<td>Long-term installed seat: 39/41 (95%) Long-term use: 6/6 (100%)</td>
</tr>
<tr>
<td>Barone 1988, United States</td>
<td>Five parenting classes (108 parents of toddlers)</td>
<td>I: One-hour safety lecture, including motor vehicle safety and video; usual safety education C: Usual safety education</td>
<td>Unblinded observation of correct use at d/c, 1-, 6-, and 12-month WCCs</td>
<td>Not adequate</td>
<td>Short-term use: 59/64 (92%) Long-term use: data not available</td>
</tr>
<tr>
<td>Christophersen 1985, United States</td>
<td>129 women on obstetric unit after delivery</td>
<td>I: Discharge orders for loan program, demonstration, and infant to ride home in the car seat; handouts and nurse demonstration at d/c; follow-up advice and handouts C: Car seat loan program offered at d/c, use encouraged</td>
<td>Unblinded observation of correct use at d/c, 1-, 6-, and 12-month WCCs</td>
<td>Not adequate</td>
<td>Short-term use: 10/15 (67%) Long-term use: 4/14 (29%)</td>
</tr>
<tr>
<td>Christophersen 1982, United States</td>
<td>30 women on obstetric unit after delivery</td>
<td>I: Nurse offer to demonstrate correct car seat use, place infant in seat, and fasten seat into car; free car seat loan C: No intervention</td>
<td>Blinded observation of correct use at d/c, four- to six-week WCCs</td>
<td>Not adequate</td>
<td>Short-term use: 170/450 (38%) Long-term use: 159/450 (35%)</td>
</tr>
<tr>
<td>Liberato 1989, United States</td>
<td>Six public health clinics (families of children under five years old)</td>
<td>I: 10-month car seat promotion: staff in-service; parking lot program to reward families if children properly restrained, warn and educate if not; waiting room stickers, posters, information, and mini-talks; verbal reinforcement, incentives from staff; usual advice about car seats C: Usual advice about car seats</td>
<td>Blinded observation of sample of cars entering parking lot 6 and 12 months after start of program</td>
<td>Not adequate</td>
<td>Short-term use: 170/450 (38%) Long-term use: 159/450 (35%)</td>
</tr>
</tbody>
</table>

a I=intervention group; C=control group; WCC=well-child care visit; ED=emergency department; d/c=discharge.

b See related endnotes at the end of this article.

c Unpublished methodological details provided by author.
the same impact now. The trial that showed the strongest effect on car seat use, for example, was conducted before infant restraints were required by law (see the article by Schieber, Gilchrist, and Sleet in this journal issue); very few subjects in the control group used restraints, and none used them correctly. In such a population, many families may fail to use car seats simply because they are unaware of the potential benefits. Now that use is 60% to 85% across the United States, these interventions are likely to have quite a different impact, so these results cannot be generalized to the current situation. Parents who do not use child car seats after a decade of legislation are likely to be harder to influence than were the subjects of these trials. There is a clear need for additional studies under present circumstances.

Many children in car seats are not correctly restrained, reducing the effectiveness of the restraints (see the article by Grossman in this journal issue). Clinical counseling has the potential to influence correct car seat use, particularly if the intervention includes demonstrations, practice, and reinforcement. New trials are needed to evaluate the effect of such interventions on correct and consistent car seat use. The optimal solutions for correct and consistent motor vehicle restraint use, however, are likely to come from engineering and regulatory changes that would turn child restraints into built-in safety features that are as easy to use as seat belts. To this end, the National Highway Traffic Safety Administration (NHTSA) has established a new federal motor vehicle safety standard that requires motor vehicle manufacturers to equip vehicles with uniform child restraint anchorage systems, effective September 1, 1999.

**Bicycle Helmet Use**

Bicycle crashes are a major cause of injury and death among school-age children and adolescents. Most bicycle crash-related hospitalizations and deaths are attributable to head injuries. Observational studies estimate that bicycle helmets reduce the risk of head injuries after crashes by at least two-thirds. Two trials tested the effect of counseling school-age children and their parents about bicycle helmet use (see Table 2). The brief interventions in these two trials were identical and included information, persuasion, and a...
list of stores that sold helmets. In one trial, the intervention was delivered by the emergency physician who treated the child for a bicycle injury and in the other by the child’s own pediatrician during well-child care.

These trials, individually and combined, showed no effect on subsequent helmet purchases two to three weeks after counseling. In the first study, 9% in the intervention group and 8% in the control group purchased helmets; in the second study, an additional 7% of families in both groups purchased helmets. Overall, the likelihood of purchasing a helmet after intervention only increased by 10%, a difference that was not statistically significant. The study’s author states that helmet use also did not differ significantly after intervention, although numerical data were not available. No studies evaluated effects of interventions on bicycle injuries.

In a third trial, children in both groups were offered identical counseling and materials promoting bicycle helmet use. This trial did not evaluate counseling; instead, the trial compared two ways to subsidize helmet ownership in low-income families: free bicycle helmets versus a $5 copayment for a helmet (although the copayment was reduced or waived for families who could not pay). The authors speculated that families who made a copayment would perceive the helmets as more valuable, and that their helmet use might be greater. Consistent bicycle helmet use was 1.4 times more likely among children whose families were required to make a copayment than among children given a free helmet. Although not statistically significant, the results suggest that requesting copayment may increase helmet use and is unlikely to adversely affect it. It is not clear from the report, however, whether requiring a copayment led to fewer families getting a helmet. The clinics requesting copayment distributed fewer helmets than did those providing free helmets (218 versus 288), but the authors did not report how many families refused helmets.

In summary, available trials do not show that individual counseling increases bicycle helmet ownership or use, nor is there any evidence about the effects of counseling on injury occurrence. However, there were only two relevant trials, involving very limited interventions. Counseling in the clinical setting may have failed because only isolated, one-time counseling sessions were offered, and they did not address key issues such as negative peer attitudes toward helmet use. Other studies have shown positive effects on childhood bicycle helmet ownership and use from community-based educational interventions that included clinical counseling as one component of a broader effort (see the article by Klassen and colleagues in this journal issue).

Safe Hot Tap Water Temperature
The leading cause of hospitalizations for burns among children under five years of age.
<table>
<thead>
<tr>
<th>Country in Study</th>
<th>Intervention</th>
<th>Control</th>
<th>Population Size</th>
<th>Selected Outcomes</th>
</tr>
</thead>
<tbody>
<tr>
<td>Canada</td>
<td>Cushman12</td>
<td>Cushman13</td>
<td>712 children</td>
<td>Helmet purchase: 146/170 (86%)</td>
</tr>
<tr>
<td></td>
<td></td>
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<td></td>
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</tbody>
</table>

**Intervention**
- Brief physician counseling and pamphlets about bicycle helmets; unblinded
- Helmet survey three weeks later

**Control**
- Helmet survey three weeks later

**Table 2**

Randomized Controlled Trials of Interventions in the Clinical Setting to Promote Bicycle Helmet Use

- I=intervention group; C=control group; WCC=well-child care visit; ED=emergency department.
- *Unpublished methodological details provided by author.
- *Unpublished outcomes data provided by author.
- *See related endnotes at the end of this article.
- *"Helmet purchase: " indicates percentage of participants who purchased helmets.
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Scalds. Hot tap water scald burns can be prevented by setting household water heaters at or below 120°F to 130°F. The incidence of such burns has declined, possibly due to legislation mandating maximum preset temperature settings of 120°F on new hot-water heaters (see the article by Grossman in this journal issue). Because thermostats in hot-water heaters may be inaccurate, parents are advised to measure the hot-water temperature using a thermometer and, if required, lower the temperature to the safe range.

Six trials were found that evaluated the effect of interventions delivered in the clinical setting on lowering, testing, and/or maintaining a safe hot tap water temperature (see Table 3). One of these trials also evaluated the effects of the intervention on injuries from all causes combined (see “Strategies to Childproof the Home”), but it did not provide data specifically on scald burns. Five trials tested educational interventions, one of these trials also provided free hot-water thermometers. The sixth trial tested the effect of providing a free thermometer compared with no thermometer when all subjects received advice and information.

All five trials of education reported positive effects, with a more than twofold greater likelihood of lowering hot tap water temperature (or of testing it, if “lowering” was not reported) and of using a safe hot tap water temperature, after education (see Figure 2). Compared to advice alone, providing a free thermometer with advice increased the likelihood of lowering hot tap water temperature by 70%, although the result was based on only one trial and was not statistically significant.

In summary, families are more likely to test and lower their hot tap water to a safe temperature when counseled to do so. While regulatory changes requiring safe settings at the factory may have substantially reduced the risk of hot tap water burns since these studies were performed, some families remain at risk from older heaters or malfunctioning thermostats.

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**Table 3: The Effect of Clinical Interventions on Safe Hot Tap Water Temperature**

<table>
<thead>
<tr>
<th>Study</th>
<th>OR</th>
<th>95% CI</th>
<th>Graphic Representation</th>
</tr>
</thead>
<tbody>
<tr>
<td>Thomas (1984)</td>
<td>10.48</td>
<td>3.01, 36.47</td>
<td></td>
</tr>
<tr>
<td>Kelly (1987)</td>
<td>1.72</td>
<td>0.76, 3.91</td>
<td></td>
</tr>
<tr>
<td>Williams (1988)</td>
<td>2.82</td>
<td>1.09, 7.33</td>
<td></td>
</tr>
<tr>
<td>Barone (1988)</td>
<td>1.16</td>
<td>0.46, 2.91</td>
<td></td>
</tr>
<tr>
<td><strong>Total</strong></td>
<td>2.32</td>
<td>1.46, 3.68</td>
<td></td>
</tr>
</tbody>
</table>

* Odds Ratio (OR) measures the strength of the association between the intervention and the outcome. An OR=1.0 indicates no association. An OR>1.0 indicates a positive effect of the intervention on the outcome, and an OR<1.0 is a negative effect.

* The 95% Confidence Interval (CI) estimates the expected range of the OR 95 out of 100 times. A “statistically significant” result in this article is defined by a 95% CI that excludes 1.0.

* The box corresponds to the OR. A box to the right of 1.0 favors the intervention group, while a box to the left of 1.0 favors the control group. The length of the line corresponds to the 95% CI.
### Table 3

**Randomized Controlled Trials of Interventions in the Clinical Setting to Promote Safe Hot Tap Water Temperature**

<table>
<thead>
<tr>
<th>Trial, Year, Country</th>
<th>Population Enrolled in Study</th>
<th>Intervention</th>
<th>Outcome Assessment</th>
<th>Allocation Concealment</th>
<th>Selected Outcomes</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Kelly⁷ 1987 United States</td>
<td>171 parents of six-month-olds seen for WCC</td>
<td>I: 15-minute developmentally oriented child safety education, hazard assessment, and handout by research physician at 6-, 9-, and 12-month WCCs; usual WCC</td>
<td>Blinded home inspection, questionnaire, and medical chart review 1 month after 12-month WCC</td>
<td>Not described</td>
<td>Safe temperature: 41/55 (74%) Safe temperature: 34/54 (63%)</td>
</tr>
<tr>
<td>Williams⁷ 1988 United States</td>
<td>12 prenatal classes (165 pregnant women)</td>
<td>I: One-hour safety lecture, including handouts on burn prevention; usual safety education</td>
<td>Unblinded home inspection two to four weeks after birth</td>
<td>Adequate</td>
<td>Safe temperature: 22/39 (56%) Safe temperature: 11/35 (31%)</td>
</tr>
<tr>
<td>Barone⁷ 1988 United States</td>
<td>Five parenting classes (108 parents of toddlers)</td>
<td>I: One-hour safety lecture, including slides and handouts on burn prevention; hot-water gauge; tap water thermometer; usual safety education</td>
<td>Unblinded home inspection six months after class</td>
<td>Not adequate</td>
<td>Safe temperature: 17/41 (41%) Lowered temperature: 18/41 (44%) Safe temperature: 15/38 (39%) Lowered temperature: 11/38 (29%)</td>
</tr>
<tr>
<td>Shapiro¹⁵ 1987 United States</td>
<td>696 pregnant women admitted for delivery</td>
<td>I: Pamphlet about scalds; free tap water thermometer; one-minute education about pamphlet and thermometer</td>
<td>Self-report two to nine months later; blinding not stated</td>
<td>Not described</td>
<td>Tested temperature: 155/302 (51%) Tested temperature: 88/302 (29%)</td>
</tr>
</tbody>
</table>
### Table 3 (continued)

<table>
<thead>
<tr>
<th>Trial, Year, Country</th>
<th>Population Enrolled in Study</th>
<th>Intervention</th>
<th>Outcome Assessment</th>
<th>Allocation Concealment</th>
<th>Selected Outcomes</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td></td>
<td>I: Burn prevention lecture, pamphlets, and handouts; usual safety guidance</td>
<td>Home inspection four to six weeks after class; blinding not stated</td>
<td>Not adequate</td>
<td>Safe temperature: 22/29 (76%) Lowered temperature: 19/29 (66%)</td>
</tr>
<tr>
<td></td>
<td></td>
<td>C: Usual safety guidance</td>
<td></td>
<td></td>
<td>Safe temperature: 6/26 (23%) Lowered temperature: 0/26 (0%)</td>
</tr>
<tr>
<td>Thomas16 1984 United States</td>
<td>16 well-baby classes (55 parents enrolled)</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td></td>
<td>I: Free tap water thermometer; brief advice and pamphlet about hot tap water and scalds</td>
<td>Self-report at one month; blinding not stated</td>
<td>Not adequate</td>
<td>Tested temperature: 122/263 (46%) Lowered temperature: 29/205 (14%)</td>
</tr>
<tr>
<td></td>
<td></td>
<td>C: Brief advice and pamphlet about hot tap water and scalds</td>
<td></td>
<td></td>
<td>Tested temperature: 55/240 (23%) Lowered temperature: 17/193 (9%)</td>
</tr>
<tr>
<td>Katcher27 1989 United States</td>
<td>686 children ages 0 to 18 seen at outpatient clinic</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
</tbody>
</table>

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*a* I=intervention group; C=control group; WCC=well-child care visit; ED=emergency department.

*b* See related endnotes at the end of this article.

*c* Unpublished methodological details provided by author.
or because their thermostat has been reset to an unsafe temperature setting.

**Smoke Alarm Ownership**

Residential fires are the second leading cause of injury deaths among young children (see the article by Grossman in this journal issue). Owning a smoke detector has been estimated to reduce the risk of death in a residential fire by more than two-thirds.40

Seven trials evaluated the effect of counseling interventions in the clinical setting on smoke alarm ownership (see Table 4).4,7,8,16-20 Two of these trials also collected data on all injuries combined (see “Strategies to Childproof the Home”).4,19 but neither reported data specifically about fire-related injuries. Six trials evaluated counseling for prospective parents or families of young children in primary care settings,4,7,8,16,18,19 and one advised families of children hospitalized for burns.20 All interventions included an educational component, though the intensity of the education varied. Three interventions also provided discounts for smoke alarms.16,18,19

Four trials reported increased smoke alarm ownership after clinical counseling,4,8,16,18 while two reported no significant effects on smoke alarm ownership.7,20 One of the latter trials also did not provide numerical data on smoke alarm outcomes.7 The seventh trial reported no effect of intervention on all safety practices combined, but data specifically on smoke alarms were not available.19 When the results of the five trials with data available were combined, smoke alarm ownership was 1.7 times more likely in counseled families compared with families that received usual care (see Figure 3). This estimate may be biased upward, however, by the inability to include data from two trials that found no significant intervention effects.

The results of the trials did not vary significantly, but there were differences worth noting. The trials with the largest effects offered subsidies—discount coupons16 or discounted smoke alarms18—in addition to counseling. Smoke alarm ownership was nine times more likely in families who received both counseling and discounted alarms than in the control families who received neither. The one trial that found no effect on ownership (and also reported outcome data) evaluated the impact of a teaching booklet added to routine hospital discharge teaching for children with burn injuries.20 The absence of an effect may have resulted from its emphasis on burn management, the limited nature of the intervention, or the focus on an injured population. However, this hospital-based trial also had the strongest study design among the trials. Inadequate allocation concealment and unblinded outcome assessment in the other trials may have exaggerated their interventions’ effects.

In summary, counseling families about smoke alarms increased smoke alarm ownership nearly twofold, although the effect might have been smaller if data from all seven trials had been available. Observational studies have linked smoke alarm ownership to a substantially reduced risk of death in the event of a residential fire (see the article by Grossman in this journal issue). Therefore, even a modest effect of counseling on ownership may lead to important decreases in fire-related injuries.

The effect on ownership is much stronger when discounted smoke alarms are provided, possibly because low-income families, who are least likely to own smoke alarms,41 require assistance to overcome cost barriers. Trials of community programs involving home visits to distribute free smoke alarms have reported large increases in smoke alarm ownership42 and decreases in fire-related injuries.43 Such community approaches may be more useful for targeting the highest-risk families, who are less likely to have access to injury prevention counseling and may require assistance to purchase and install alarms.

**Strategies to Childproof the Home**

Among children under age five, falls, poisoning, cuts, burns, smoke inhalation, drowning, suffocation, and choking cause at least two-thirds of all unintentional injuries.
Many of these injuries occur in the home, where young children spend most of their time. Household risk factors for injuries to young children include unprotected stairs, windows, and fireplaces; and improperly stored household cleaning agents, medications, matches, and sharp objects (see the article by Grossman in this journal issue). Strategies to childproof the home include changes that are made specifically to protect young children from these risks, and that would not typically be implemented in homes without children.

Five trials were found that evaluated counseling families to childproof their homes, all of which targeted children under five years of age (see Table 5).4,18,19,21,22 Four trials involved intensive interventions4,18,19,21 and two of these also evaluated the effect of offering free or discounted safety devices.18,19 The fifth trial offered a brief intervention in the emergency department.22 The trials assessed various safety practices, most commonly safe storage of household medications, cleaning agents, matches, and sharp objects; and use of electric outlet covers.

Overall, intervention appeared to have little effect on safety practices. Two trials reported modest effects that were not statistically significant.4,22 Two others reported no statistically significant differences between intervention and control households, but they provided no data.18,22 In contrast, one trial reported strongly positive and statistically significant beneficial effects for most outcomes.18

Combined results from the three trials that provided numerical outcome data4,18,21 show that, compared with control families, intervention families were 1.8 times more likely to store cleaning agents safely. There was also a modest effect on safe storage of matches (which was not statistically significant) among the two trials that reported this outcome.4,21 These estimates may have been biased upward, however, by the failure to include data from the two trials that found no significant differences between intervention and control households.19,22 Because of marked variation in the results for use of electric outlet covers and for safe storage of medications and knives, data from the three trials that studied these outcomes could not be combined. Most of the trials used similarly intensive interventions,
### Table 4

**Randomized Controlled Trials of Interventions in the Clinical Setting to Promote Smoke Alarms**

<table>
<thead>
<tr>
<th>Trial, Year, Country</th>
<th>Population Enrolled in Study</th>
<th>Intervention</th>
<th>Outcome Assessment</th>
<th>Allocation Concealment</th>
<th>Selected Outcomes</th>
</tr>
</thead>
<tbody>
<tr>
<td><strong>Kelly</strong>&lt;sup&gt;4&lt;/sup&gt; 1987 United States</td>
<td>171 parents of 6-month-olds seen for WCC</td>
<td>I: 15-minute developmentally oriented child safety education, hazard assessment, and handout by research physician at 6-, 9-, and 12-month WCCs; usual WCC</td>
<td>Blinded home inspection, questionnaire, and medical chart review 1 month after 12-month WCC</td>
<td>Not described</td>
<td>Own alarm: 8/55 (15%)</td>
</tr>
<tr>
<td><strong>Williams</strong>&lt;sup&gt;7&lt;/sup&gt; 1988 United States</td>
<td>12 prenatal classes (165 pregnant women)</td>
<td>I: One-hour lecture and handouts on burn prevention; usual safety education</td>
<td>Unblinded home inspection two to four weeks after birth</td>
<td>Adequate</td>
<td>Data not available</td>
</tr>
<tr>
<td><strong>Barone</strong>&lt;sup&gt;8&lt;/sup&gt; 1988 United States</td>
<td>Five parenting classes (108 parents of toddlers)</td>
<td>I: Slides and handouts on burn prevention; usual safety education</td>
<td>Unblinded home inspection six months after class</td>
<td>Not adequate</td>
<td>Own alarm: 39/41 (95%)</td>
</tr>
<tr>
<td><strong>Thomas</strong>&lt;sup&gt;9&lt;/sup&gt; 1984 United States</td>
<td>16 well-baby classes (55 parents enrolled)</td>
<td>I: Burn prevention lecture, pamphlets, and handouts; coupon for smoke alarm; usual safety guidance</td>
<td>Home inspection four to six weeks after class; blinding not stated</td>
<td>Not adequate</td>
<td>Own alarm: 28/29 (97%)</td>
</tr>
</tbody>
</table>
Table 4 (continued)

<table>
<thead>
<tr>
<th>Trial, Year, Country</th>
<th>Population Enrolled in Study</th>
<th>Intervention</th>
<th>Outcome Assessment</th>
<th>Allocation Concealment</th>
<th>Selected Outcomes</th>
</tr>
</thead>
</table>
| Clamp18 1998 United Kingdom | 165 families of children under age 5 in general practice | I: Injury prevention advice from physician; leaflets; discounted smoke alarms  
C: Usual care | Unblinded self-report survey six weeks after visit | Adequate | Own alarm:  
82/83 (99%)  
Own alarm:  
71/82 (87%) |
| Kendrick19 1999 United Kingdom | 36 general practices (2,052 children ages 3 to 13 months) | I: Safety advice by health visitors and nurses; discounted smoke alarms; home safety checks; first aid training  
C: Usual care | Unblinded record review for injuries; self-reported safety practices | Adequate | Data not available  
Data not available |
| Jenkins20 1996 Canada | 141 families of children under 17 years of age in burn unit | I: Discharge teaching book on burn follow-up management and burn prevention; routine d/c teaching  
C: Routine d/c teaching | Self-report at first follow-up clinic visit by blinded interviewer | Adequate | Own alarm:  
45/62 (73%)  
Own alarm:  
46/61 (75%) |

* I=intervention group; C=control group; WCC=well-child care visit; ED=emergency department; d/c=discharge.

* See related endnotes at the end of this article.

* Unpublished methodological details provided by author.

* Unpublished outcomes data provided by author.
### Table 5

#### Randomized Controlled Trials of Interventions in the Clinical Setting to Promote Childproofing the Home

<table>
<thead>
<tr>
<th>Trial, Year, Country</th>
<th>Population Enrolled in Study</th>
<th>Intervention</th>
<th>Outcome Assessment</th>
<th>Allocation Concealment</th>
<th>Selected Outcomes</th>
</tr>
</thead>
<tbody>
<tr>
<td><strong>I: 15-minute developmentally oriented child safety education, hazard assessment, and handout by research physician at 6-, 9-, and 12-month WCCs; usual WCC</strong></td>
<td>171 parents of 6-month-olds seen for WCC</td>
<td>Blinded home inspection, questionnaire, and medical chart review 1 month after 12-month WCC</td>
<td>Not described</td>
<td>Outlet covers: 28/55 (51%)</td>
<td>Parent-reported “accidents”: OR=0.78 (0.35, 1.74) Parent-reported ED visits: OR=1.60 (0.53, 4.84) Medically attended injuries: OR=1.47 (0.60, 3.60) Hospitalizations: OR=0.98 (0.06, 16.11)</td>
</tr>
<tr>
<td><strong>C: Usual WCC</strong></td>
<td>165 families of children under age five in general practice</td>
<td></td>
<td></td>
<td></td>
<td>Outlet covers: 23/54 (43%) Safe storage of Medicines: 54/54 (100%) Cleaners: 43/54 (80%) Sharp objects: 41/54 (76%) Matches: 49/54 (91%)</td>
</tr>
<tr>
<td>Kelly^4 1987 United States</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Clamp^1^ 1998 United Kingdom</td>
<td>165 families of children under age five in general practice</td>
<td>Unblinded self-report six weeks after visit Adequate</td>
<td>Outlet covers: 68/83 (82%) Safe storage of Medicines: 79/83 (95%) Cleaners: 59/83 (71%) Sharp objects: 52/83 (63%)</td>
<td>Outlet covers: 38/82 (46%) Safe storage of Medicines: 68/82 (83%) Cleaners: 49/82 (60%) Sharp objects: 26/82 (32%)</td>
<td></td>
</tr>
</tbody>
</table>
### Table 5 (continued)

<table>
<thead>
<tr>
<th>Trial, Year, Country</th>
<th>Population Enrolled in Study</th>
<th>Intervention</th>
<th>Outcome Assessment</th>
<th>Allocation Concealment</th>
<th>Selected Outcomes</th>
</tr>
</thead>
</table>
| **Kendrick**<sup>19, 1999</sup>, United Kingdom | 36 general practices (2,052 registered children ages 3 to 13 months) | I: Safety advice by health visitors and nurses; discounted safety devices; home safety checks; first aid training  
C: Usual care | Unblinded record review for injuries; self-reported safety practices | Adequate | Medically attended injury:  
OR=0.97 (0.72, 1.30)  
ED visit for injury:  
OR=1.02 (0.76, 1.37)  
GP attendance for injury:  
OR=0.75 (0.48, 1.17)  
Hospitalization for injury:  
OR=0.69 (0.42, 1.12) | |
| **Dershewitz**<sup>21</sup>, 1977, United States | 330 families with children ages 0 to 4 seen for WCC or minor illness, prepaid health plan | I: Booklet and 20-minute discussion on home safety by research assistant; telephone reinforcement 4 weeks later; free safety devices  
C: Free safety devices | Blinded inspection, questionnaire at home visit one month later | Not described | Outlet covers:  
89/101 (88%)  
Safe storage of Medicines:  
22/101 (22%)  
Cleaners:  
1/101 (1%)  
Sharp objects:  
32/101 (32%)  
Matches:  
47/101 (47%) | Outlet covers:  
88/104 (85%)  
Safe storage of Medicines:  
20/104 (19%)  
Cleaners:  
0/104 (0%)  
Sharp objects:  
38/104 (36%)  
Matches:  
43/104 (41%) |
| **Woolf**<sup>22</sup>, 1987, United States | 343 families of children 5 years of age or younger seen in ED for nonurgent care | I: Brief counseling and handout on poisoning prevention; telephone sticker; free ipecac; questionnaire about poisoning  
C: Questionnaire about poisoning | Self-report by blinded interview four to six months later | Not described | Data not available | Data not available |

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I=intervention group; C=control group; WCC=well-child care visit; ED=emergency department.

See related endnotes at the end of this article.

Odds Ratio (OR) measures the strength of the association between the intervention and the outcome. An OR=1.0 indicates no association. An OR>1.0 indicates a positive effect of the intervention on the outcome, and an OR<1.0 indicates a negative effect. The 95% confidence interval (in parentheses) estimates the expected range of the OR 95 out of 100 times. A “statistically significant” result in this article is defined by a 95% confidence interval that excludes 1.0.

Unpublished methodological details provided by author.

Unpublished outcomes data provided by author.
so it is not clear whether these marked variations were due to differences in the quality of the interventions or the study designs, or to other unknown factors.

The two trials that measured injury outcomes (all types of injuries combined) showed that, with intervention, the likelihood of injury hospitalization was reduced by 27%. One trial also reported that the likelihood of injury resulting in attendance by a general practitioner was reduced by 25%, and the other found that the risk of parent-reported “accidents” was reduced by 22% in the intervention group. However, none of these differences were statistically significant. Moreover, no intervention effects were associated with the risk of emergency visits for injury, medically attended injuries, injury severity scores, or length of hospital stay.

In summary, evidence from this systematic review suggests that clinical counseling has little effect on most home safety practices designed to childproof the home. It is possible that the scope and intensity of the interventions were not adequate to reduce household hazards, or that study families were overwhelmed or confused by the number and variety of safety practices recommended. High cost, difficulty obtaining recommended safety devices, the need for technical skills and tools to install some devices, and residence in rental accommodation should be recognized as important barriers to implementing advice about home safety, particularly for low-income households.

Trials in this review also do not establish that clinical interventions aimed at making homes safer for children will reduce injuries. Such interventions may fail to reduce injuries if they do not change safety practices, or if changes in these practices do not affect injuries. This review provides some support for the first explanation. However, there also is a lack of well-conducted observational studies demonstrating that the general home safety measures advocated in these trials are associated with a reduction in childhood injuries. For example, although the widespread use of child-resistant containers for medications and household chemicals has reduced childhood poisonings, any added effect of locking away such containers has not been evaluated. Lack of evidence about the impact of these strategies on injury outcomes makes it difficult to ascertain the extent children would benefit from clinical interventions that encourage their parents to adopt measures to childproof the home.

Implications for Practice and Research
This systematic review provides evidence that interventions delivered in the clinical setting are likely to be successful at increasing the adoption of some safety practices aimed at reducing childhood injuries, but not others. Elements of these trials that may have influenced their effectiveness have important practice and policy implications. This section examines characteristics of these studies in relation to designing and implementing clinical strategies and setting future research agendas for childhood injury prevention.
Implications for Practice

In general, the trials reviewed support evidence from research that describes three types of factors that health education can modify: predisposing, enabling, and reinforcing (see Box 5).30 Trials that had the greatest effect on safety practices used a combination of these strategies, combining education with reinforcement or with free or subsidized safety devices, for example. As future clinical interventions to improve child safety are developed, practitioners should be aware that counseling alone may not be sufficient to alter behavior.

The clinical interventions that appeared most effective—those involving advice, demonstrations, subsidized safety equipment, and reinforcement through repeated messages and visits, incentives, and rewards—are time consuming. Yet, the average clinician has only limited time to devote to injury prevention. The busy clinician should consider focusing injury prevention counseling on practices where a beneficial effect is most likely to result. It would be reasonable to emphasize correct and consistent motor vehicle restraint use, installation and maintenance of smoke alarms, and testing and maintaining a safe hot tap water temperature.

Careful consideration of the target population also is important to maximize the impact of interventions in the clinical setting on child safety practices. For example, this review did not find evidence to support injury prevention counseling that targeted bicycle helmet ownership. However, these were the only trials reviewed that targeted school-age children.12,13 The lack of impact may reflect differences in the responsiveness to safety counseling of families of school-age children compared with pregnant women or parents of newborns.

Similarly, study populations also varied in terms of the health of the children targeted. Most trials enrolled parents or parents-to-be in well-care settings, including prenatal and parenting classes. However, two trials—one promoting bicycle helmet purchase12 and the other smoke alarm ownership20—targeted injured children. Neither of these trials found differences in safety practices comparing intervention and control groups. Parents of injured children may be less amenable to safety messages, or perhaps such parents receive safety messages from other sources that outweigh any potential impact from the experimental intervention.

Study populations also differed according to the baseline level of safety practice. For most safety practices, there were greater effects in trials where baseline use was low. These trials were usually implemented before legislation mandating safety practice was enacted. It is reasonable to assume that when only a small proportion of the population has adopted a safety practice, others can be influenced simply by providing information, advice, and persuasion. When 90% of the population has already adopted a practice, however, it is likely that the remaining families are prevented from doing so not by lack of awareness, but by more substantial barriers such as cost, access, language, or culture. In these instances, low-cost availability of safety devices, and education sensitive to the local culture and available in the local language, may be critical to increase use.

The clinical setting may not be suitable for implementing the entire range of information, modeling, resources, and reinforcement required to change safety practices, particularly for some populations, including parents of school-age or injured children. Clinical interventions may be more effective if delivered in the context of broader community, legislative, engineering, and regulatory changes. In many communities, physicians and other health professionals have provided leadership for effective injury prevention programs and legislation.26,27 Clinicians may wish to consider playing an advocacy role as a means of preventing injuries, while continuing to support behavior change with clinical interventions.

Implications for Research

Many of the trials identified in this review were conducted more than a decade ago,
before legislation and regulations regarding infant safety seats, bicycle helmets, smoke alarm installation, and preset hot-water heater temperatures were implemented (see the article by Schieber, Gilchrist, and Sleet in this journal issue). It is doubtful that the brief, simple interventions evaluated in the past would have the same impact today. The identification of efficient and cost-effective clinical interventions that improve safety practices in the current regulatory and legislative environment is needed. In developing such trials, careful attention to evaluation design is essential to reduce the possibility of biased results.

This review supports previous research showing that studies with weaker evaluation methods, on average, report larger effect estimates than do studies with stronger methods. For example, for safe hot tap water temperature, smaller effects were observed among the trials that used adequate study methods. Hence, while the best available evidence supports counseling to promote certain safety practices, the benefit may be less than that reported here because many of the trials reviewed did not use adequate study methods. Additional RCTs that adhere to rigorous design principles would be useful to supplement the results reported in this systematic review. Also, studies are needed to examine the effects of interventions in the clinical setting on safety practices aimed at reducing other causes of childhood injuries, including pedestrian, firearm, drowning, recreational, and alcohol-related injuries.

The evidence about the impact of counseling in the clinical setting on childhood injuries is limited. Although injuries—and resulting hospitalization, disability, and death—are of primary interest in evaluations of injury prevention interventions, these outcomes are relatively rare in the general population. Consequently, most studies examine effects on intermediate outcomes such as safety practices, which are more easily measured. Counseling interventions that target safety behaviors can be considered effective if evidence shows that changing the safety behavior can reduce injury risk and that clinicians can influence this behavior through counseling.

Evidence that changing a safety behavior will reduce injury risk usually comes from studies that observe people who have and have not chosen to implement a safety practice, to see whether the two groups’ injury rates differ. Such observational studies are more subject to systematic bias than are RCTs because other differences may exist between people who have and have not chosen to initiate that practice, and those differences may also affect injury...
risk. For example, fire fatalities occur less commonly among households that have smoke alarms, but these households may differ from households without alarms in other ways that also affect fire fatalities, such as the occupants' ages, disabilities, or use of alcohol, or the condition of the property. Therefore, it is important to identify such differences and correct for them in the analysis. A number of well-conducted observational studies examining the association of safety behaviors and injury outcomes have made substantial efforts to identify and control for such "confounding variables." For other areas, such as linking measures to childproof the home with reduced childhood injuries, well-conducted observational studies are still needed. Nevertheless, it is important to recognize that even the best observational studies can only correct for identified, measured differences and that other differences (unidentified or unmeasured) might also influence outcomes. The evidence that is least subject to bias comes from RCTs that report injury outcomes. Large trials of good methodological quality would be valuable for quantifying the benefit of counseling in the clinical setting on injuries among children.

Conclusions
The clinical setting offers numerous opportunities for interventions to promote childhood safety practices because most young children are seen regularly for well-child care, immunizations, and minor illnesses. Pregnant women, through participation in regular prenatal visits and classes, also form a ready target for strategies designed to protect their infants. Emergency departments and hospitals are potential settings for injury prevention strategies for those for whom an emergency visit may provide the only opportunity to receive preventive services, such as older adolescents or families with limited access to care.

Physicians caring for children have long recognized the importance of injury prevention counseling. Offering guidance to prevent injuries to infants, children, and adolescents has been recommended by professional societies, government agencies, and national task forces. Although many regard injury prevention guidance as beneficial, this benefit had not been adequately quantified prior to this systematic review. The costs of injury prevention may compare favorably with those of injury treatment and rehabilitation (see the article by Miller, Romano, and Spicer in this journal issue), yet funding for injury prevention interventions in the clinical setting is not always provided by third-party payers. Clinical injury prevention interventions have important resource implications, including time and effort devoted to the intervention and the costs of pamphlets, equipment, and other materials. Estimates of the likely effects of clinical interventions on safety practices and injuries provide important input for cost-effectiveness analyses and for policy decisions about implementation and funding.

This systematic review found that individual-level interventions delivered in a clinical setting are a promising way to promote improvements in certain safety practices, including motor vehicle restraint use, smoke alarm ownership, and safe hot tap water temperature, and, consequently, to reduce injuries. Further research may expose additional opportunities for clinical interventions that are as yet unproven or untested; for example, counseling adolescents about safety practices. For some types of injuries, strategies aimed at the school, community, state, or nation, or clinical interventions delivered in the context of such strategies, may be more effective at reducing childhood injuries.

The assistance of Catherine Godward (data collection, screening citations) and Drs. David Dunn and Julian Higgins (statistical advice) is gratefully acknowledged. The authors are solely responsible for the results reported.

http://www.futureofchildren.org
Methodology and Search Results of the Systematic Review

This review was developed using the methods of the Cochrane Collaboration (see Box 1) and will be published and periodically updated in the Cochrane Database of Systematic Reviews.

Methods

Data Sources

We searched the Cochrane Controlled Trials Register (CCTR) (The Cochrane Library 1998, issue #3), Cochrane Injuries Review Group database (searched August 1998), MEDLINE EXPRESS (1966-August 1998), EMBASE (1980-August 1998), Cumulative Index to Nursing and Allied Health (1982-August 1998), and Dissertation Abstracts (1861-August 1998). The CCTR was searched using injury- and age-related content terms (available from author on request). MEDLINE was searched by combining these content terms with a search strategy for identifying controlled trials. Similar search strategies were developed for the other databases. There were no language restrictions.

We searched the bibliographies of published reviews and a review on the World Wide Web (http://depts.washington.edu/hiprc/childinjury). We reviewed presentation abstracts from four World Conferences on Injury Prevention and Control. We contacted authors of relevant trials, the membership of the International Society for Child and Adolescent Injury Prevention, and more than 50 organizations and institutions in Europe, North America, Australia, and New Zealand.

Inclusion Criteria

We included studies if: (1) the intervention was designed to prevent unintentional injuries to persons ages 0 to 19; (2) the intervention was delivered in a clinical setting, such as physician offices, clinics, or hospitals; (3) participants were assigned randomly to intervention and control groups; and (4) the study collected data on injuries, events that can cause injuries (for example, falls), or safety practices.

Study Selection

Two researchers independently screened citations to exclude studies that clearly failed to meet our first three inclusion criteria; differences were resolved by discussion. The full texts of remaining citations were reviewed and additional studies were excluded using the same criteria. We contacted the corresponding authors or other coauthors of all remaining studies to determine eligibility, request study details, and identify additional trials.

Data extraction

Two researchers independently extracted data on study design, participants, and interventions. To assess study design, we extracted information on methods of allocation concealment, blinding of outcomes assessment, and the withdrawal or exclusion of subjects after randomization. We rated allocation concealment as adequate if the methods contained elements indicative of concealment (for example, allocation by independent statistician) and inadequate if the method was not convincing of concealment (for example, coin toss at enrollment) or was not described.

One researcher extracted outcome data. For two trials, data as reported was not usable in our analyses. Christophersen and colleagues reported outcomes for each group as percentages, but reported only total numbers of subjects randomized (n=129) and lost to follow-up at 12 months (n=63). To calculate denominators, we assumed that equal numbers were randomized to each group and that there was no loss to follow-up at first observation (which occurred immediately after intervention); numerators were calculated from published percentages. We excluded later assessments. Miller and Pless reported a mean car seat use score by combining self-reported usual use and use when traveling to the doctor’s office that day. We estimated the proportion of subjects who reported both “always used car seat” and “used car seat that day” from the mean postintervention scores for each group and the published postintervention distributions of responses to “always used” and “use on that day” for the total sample. We calculated numerators from these proportions using published group denominators. The sum of participants in each group did not equal the total number of participants reported. As the authors were unable to provide correct data, we made conservative assumptions in our calculations.
Analysis

Primary outcome measures included injuries and safety practices. We compared the number and rate of injuries during follow-up. For safety practices, we compared postintervention prevalences between intervention and control groups. For car restraint use, we assessed short- and long-term follow-up results, defining the earliest measurement in a given trial as “short term” and the last measurement as “long term.” Where only one measurement was reported, we arbitrarily assigned follow-up periods up to six months as short term and six months or more as long term.

We explored whether variations in study results were related to the study population, intervention characteristics (duration, use of reinforcement techniques, subsidization of safety devices), or study design (allocation concealment, blinding of outcomes assessment).

Using Review Manager 3.1, meta-analyses were performed to combine odds ratios between intervention and control groups, using fixed-effects models. We used a significance level of 10% to test for heterogeneity. When the results from more than one intervention group were reported, we combined data from all intervention groups, except when exploring heterogeneity according to intervention characteristics. Kim and colleagues reported odds ratios and confidence intervals that were adjusted for nonindependence of data; from these, we calculated adjusted numerators and denominators for use in our meta-analyses using standard techniques.

To combine the data on injury outcomes, which in one trial was reported only as adjusted odds ratios and confidence intervals, a pooled odds ratio for each injury outcome was calculated as an inverse variance weighted average of the study-specific odds ratios.

For cluster randomized trials, we reduced subject numbers to an “effective sample size” to take into account the cluster randomization, using an estimate of the intraclass correlation coefficient (0.0027) from a published trial involving randomized clinical practices. All meta-analyses include effective sample sizes. The unadjusted numerators and denominators are shown in the tables.

Search Results

We identified 10,330 unduplicated citations, of which 103 were potentially eligible. We reviewed the full texts of 101 studies; two were available only in abstract. Twenty-one trials (reported in 20 papers and two abstracts) met all inclusion criteria, 79 studies were excluded after full text review, and 2 trials were excluded based on information provided by investigators. We received responses from the investigators of 18 (86%) of 21 eligible trials, from which we identified one additional trial.

Endnotes:

2 See note no. 36 at the end of this article.
3 See note no. 27 at the end of this article.
12 See note no. 38 at the end of this article.
13 See note no. 39 at the end of this article.
14 See note no. 40 at the end of this article.
15 See note no. 41 at the end of this article.
16 See note no. 42 at the end of this article.
17 See note no. 43 at the end of this article.
18 See note no. 44 at the end of this article.

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23. One trial is evaluating the provision of five hours of special training in injury prevention counseling to pediatricians in training, compared to the usual training provided. Safety practices will be assessed in families whose children ages birth to six months are seen for well-child care by these physicians. This study is still in progress and outcome measures are not yet available. It will not be discussed further in this report. Gielen, A.C., McDonald, E., Wilson, M., et al. Assessing exposure to an intervention with self-report and audio tapes of medical


34. Personal communication with Dr. Robert Cushman, University of Ottawa, October 21, 1998.


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