Reviews of Evidence Regarding Interventions to Reduce Alcohol-Impaired Driving

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Background: Alcohol-related motor vehicle crashes are a major public health problem, resulting in 15,786 deaths and more than 300,000 injuries in 1999. This report presents the results of systematic reviews of the effectiveness and economic efficiency of selected population-based interventions to reduce alcohol-impaired driving.

Methods: The *Guide to Community Preventive Services*'s methods for systematic reviews were used to evaluate the effectiveness of five interventions to decrease alcohol-impaired driving, using changes in alcohol-related crashes as the primary outcome measure.

Results: Strong evidence was found for the effectiveness of .08 blood alcohol concentration laws, minimum legal drinking age laws, and sobriety checkpoints. Sufficient evidence was found for the effectiveness of lower blood alcohol concentration laws for young and inexperienced drivers and of intervention training programs for servers of alcoholic beverages. Additional information is provided about the applicability, other effects, and barriers to implementation of these interventions.

Conclusion: These reviews form the basis of the recommendations by the Task Force on Community Preventive Services presented elsewhere in this supplement. They can help decision makers identify and implement effective interventions that fit within an overall strategy to prevent impaired driving.

Medical Subject Headings (MeSH): community health services; decision making; evidence-based medicine; practice guidelines; preventive health services; public health practice; meta-analysis; review literature; motor vehicles; seat belts; accidents, traffic; accident prevention; automobile driving; alcohol drinking; wounds and injuries (Am J Prev Med 2001;21(4S):66–88)

Introduction

The United States has made substantial progress in reducing alcohol-related traffic fatalities in recent decades. Since the National Highway Traffic Safety Administration (NHTSA) began keeping records on alcohol involvement in fatal crashes in 1982, the proportion of all traffic fatalities that are alcohol-related has declined steadily from 57% to 38%.[1] Despite this progress, alcohol-related motor vehicle crashes continue to be a major public health problem, resulting in 15,786 deaths and more than 300,000 injuries in 1999.[1]

Since 1970, individual states and communities have implemented a broad range of strategies to reduce alcohol-impaired driving. Laws to deter alcohol-impaired driving and to control the sale or public consumption of alcohol are among the most widely used strategies. By 1987, all states had enacted a minimum legal drinking age of 21 years. As of May 1, 2001, a total of 24 states, the District of Columbia, and Puerto Rico had lowered the illegal blood alcohol concentration (BAC) for drivers aged 21 years and older from 0.10 g/dL to 0.08 g/dL. Community-based interventions, including sobriety checkpoints, enhanced enforcement of alcohol control policies, and training programs for servers of alcoholic beverages, have also been implemented in some states.

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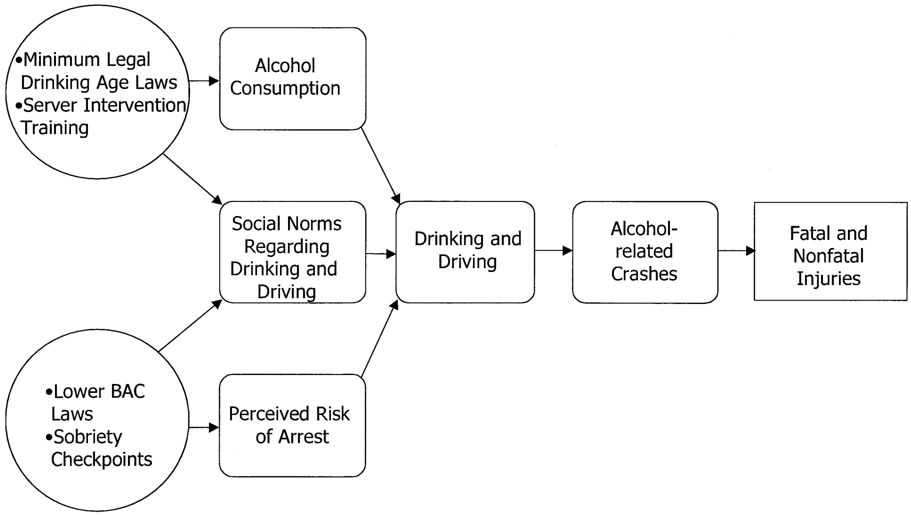

Figure 1. Logic framework for reviews of interventions to reduce alcohol-impaired driving. BAC, blood alcohol concentration.

Data provided by NHTSA and the U.S. Bureau of the Census in 1999 indicate that the United States nearly met the *Healthy People 2000* objective for alcohol-related motor vehicle deaths of no more than 5.5 deaths per 100,000 persons, with a rate of 5.8 per 100,000 persons.[1–3] The *Healthy People 2010*[4] target for alcohol-related motor vehicle fatalities is 4.0 per 100,000 persons or fewer. Meeting the 2010 objective will require a further decrease of 31% in the rate of alcohol-related motor vehicle fatalities. The recommendations of the Task Force on Community Preventive Services (the Task Force)[5] are intended to help communities, working with public health and traffic safety professionals, identify and implement effective programs and policies.

Some of the laws evaluated in the systematic review have already been widely enacted in the United States. Information about the effectiveness of these laws will be useful in evaluating any future proposals to repeal or to revise them. In addition, the systematic review highlights important unanswered questions about the effectiveness of these laws in various settings (e.g., urban vs rural).

Conceptual Approach

This systematic review was undertaken to assess the effectiveness of a number of laws and other community-based interventions in reducing alcohol-impaired driving and alcohol-related motor vehicle crash fatalities in the United States and other Established Market Economies.[a] Of the 76 studies included in the review, 55 (72%) were conducted in the United States. Other studies were conducted in Australia, Canada, New Zealand, France, and The Netherlands.

We focused on interventions for which the primary goal was to reduce alcohol-impaired driving. We did not review interventions intended primarily to restrict access to alcohol (e.g., alcohol taxation, alcohol outlet zoning restrictions) or to address health outcomes of alcohol abuse or misuse other than alcohol-impaired driving. Those topics will be included in the Task Force's review of interventions to prevent alcohol abuse and misuse as part of the *Guide to Community Preventive Services* (the *Community Guide*).

The logic framework shown in Figure 1 depicts the conceptual approach that guided the review process. This figure illustrates the hypothesized links between interventions to reduce alcohol-impaired driving and the outcomes of interest. These interventions are thought to work through three separate pathways: They may reduce alcohol-impaired driving by increasing the perceived risk of detection and punishment; They may reduce alcohol consumption in high-risk settings or among high-risk groups; and they may foster a social norm that reduces the acceptable amount of alcohol to consume before driving.

Methods

The *Community Guide*'s methods for systematic reviews and for linking evidence to recommendations have been published elsewhere.[6] An overview of the general methods used in the systematic reviews of interventions to reduce motor vehicle occupant injury appears in the supplement.[7] This discussion is limited to topics that apply specifically to interventions to reduce alcohol-impaired driving.

[a]Established Market Economies as defined by the World Bank are Andorra, Australia, Austria, Belgium, Bermuda, Canada, Channel Islands, Denmark, Faeroe Islands, Finland, France, Former Federal Republic of Germany, Germany, Gibraltar, Greece, Greenland, Holy See, Iceland, Ireland, Isle of Man, Italy, Japan, Liechtenstein, Luxembourg, Monaco, The Netherlands, New Zealand, Norway, Portugal, San Marino, Spain, St. Pierre and Miquelon, Sweden, Switzerland, the United Kingdom, and the United States.

Table 1. Priority interventions selected for review

	Interventions
Laws & Policies	**.08 blood alcohol concentration (BAC) laws**
	Lower BAC laws for young and inexperienced drivers
	Minimum legal drinking age laws
	Administrative license revocation
	Ignition interlocks
Law Enforcement	**Sobriety checkpoints**
Behavioral	**Intervention training programs for servers of alcoholic beverages**
	Alternative transportation (e.g., designated driver programs)
	Assessment and treatment for DUI offenders
Provision of Information	*Mass media campaigns*
	School-based education
Multifaceted Programs	*Multifaceted community-based programs*

Italics indicate interventions not yet reviewed.
DUI, driving under the influence of alcohol.

Selecting Interventions

The consultation team (see Acknowledgments) generated a comprehensive list of interventions to reduce alcohol-impaired driving and created a priority list of interventions to be reviewed after surveying consultants and other experts. Those individuals were asked to rank interventions as priorities for systematic review, considering whether each intervention is (1) thought to be effective but underused; (2) thought to be ineffective but overused; (3) popular, but its effectiveness is not well established; (4) costly, but its effectiveness is not well established; (5) targeted to a specific population of interest (e.g., youth); or (6) broad reaching, and could achieve large reductions in alcohol-impaired driving if found to be effective. Rankings were compiled, and the 12 interventions with the most votes were selected as priorities for this review (Table 1). Resource limitations prevented us from completing reviews of all of the priority interventions in time for this publication. Additional reviews will be published as they are completed.

Selecting Summary Effect Measures

The primary outcomes assessed in this literature are fatal and nonfatal injuries resulting from alcohol-related motor vehicle crashes. This information is primarily derived from police incident reports. In the United States, information about all fatal crashes that occur on public roads is available in electronic form through NHTSA's Fatality Analysis Reporting System (FARS).[8] There is no comparable single source of electronic information about all nonfatal crashes. Of the 69 studies that examined crash data, 35 (51%) examined only fatal crashes.

Differences in how "alcohol-relatedness" of crashes is operationally defined from study to study contribute to the variability in the effect measures in this review. Until recent decades, the BACs of drivers involved in fatal crashes were measured too sporadically to be useful in evaluating interventions to reduce alcohol-impaired driving. Objective measures of alcohol involvement in nonfatal crashes continue to be collected only sporadically. Given the limited availability of BAC data, many studies have used proxy variables for alcohol-related crashes. Commonly used proxy variables, and their estimated level of association with alcohol involvement, are listed in Table 2. Using proxy variables for alcohol involvement produces effect estimates that are biased toward the null, with the degree of bias being more pronounced for proxies with weaker association with alcohol involvement.

Important differences exist in the operational definitions of "alcohol-relatedness" of crashes even among studies that use driver BAC data. For fatal crashes in the United States, the FARS system uses a statistical model to estimate the BAC of drivers for whom BACs were not obtained.[9] Some of the studies in this review used the estimated values for BAC

Table 2. Estimated probability of alcohol involvement for various crash types in the United States, 1999

Crash type[a]	Estimated probability of alcohol involvement[b] (%)
Proxies for alcohol-involved crashes	
Nighttime single-vehicle fatal crashes (6:00 PM to 5:59 AM)	64
Nighttime fatal crashes (6:00 PM to 5:59 AM)	60
All fatal crashes	38
Late night single-vehicle nonfatal injury crashes (12:00 AM to 5:59 AM)	41
Late night nonfatal injury crashes (12:00 AM to 5:59 AM)	37
Late night property damage only crashes (12:00 AM to 5:59 AM)	23
Comparison crash types	
Daytime fatal crashes (6:00 AM to 5:59 PM)	17
Daytime nonfatal injury crashes (6:00 AM to 5:59 PM)	4

[a]The categories of crash types are provided for descriptive purposes. In most studies reviewed, crashes that met or exceeded a given level of severity were combined.
[b]Alcohol involvement is defined by a measured or estimated blood alcohol concentration (BAC) of 0.01 g/dL or greater for fatal crashes and by police report for nonfatal crashes.
Source: National Highway Traffic Safety Administration.[1]

provided by the FARS statistical model. Other studies considered as alcohol-related only those crashes involving drivers with measured BACs above an established level. Additionally, studies defined alcohol-relatedness using various BAC cutpoints (i.e., ≥0.01 g/dL, ≥0.08 g/dL, or ≥0.10 g/dL).

We often had to select from several possible effect measures. We established and consistently applied rules for identifying the outcome measure that most adequately reflected alcohol-related crashes and addressed potential confounding variables. Briefly, we considered BAC data to be the most objective measure of alcohol-relatedness of crashes. For studies that reported results using more than one cutpoint for BAC, we chose the results based on the BAC cutpoint closest to 0.10 g/dL.

When available, we selected effect measures that compared alcohol-related fatalities with non–alcohol-related fatalities (e.g., proportion of all fatal crashes involving drivers with BACs of ≥0.10 g/dL; ratio of single-vehicle nighttime fatal crashes to multi-vehicle daytime fatal crashes) over the absolute number of alcohol-related fatalities. These effect measures help control for both the long-term downward trend in total fatal crashes and factors that influence the total number of crashes, such as weather, economic conditions, vehicle miles traveled, and safety characteristics of vehicles and highways.[10] When available, we also selected effect measures that incorporated a concurrent comparison group such as drivers in adjacent states or drivers within the same state who were unaffected by the intervention. For those studies, results were reported in the form of the net change, reflecting the difference between the percent change for the intervention group and the comparison group. Net change was calculated by using the formula

$$(I_{post} - I_{pre})/I_{pre} - (C_{post} - C_{pre})/C_{pre},$$

where:

I refers to the group exposed to the intervention,
C refers to the group not exposed to the intervention (the comparison group),
post refers to outcome measurements after implementation of the intervention, and
pre refers to outcome measurements before implementation of the intervention.

For studies using interrupted time series or other regression-based analyses, results were reported in terms of the percent change estimated from the model.

The other outcomes assessed in this review were BACs of drivers at roadside surveys, as well as measured and estimated BACs of people leaving bars or other licensed establishments. Net changes in these outcomes were calculated by using the same formula as for the crash outcomes.

Effect measures from individual studies are displayed in figures, and a median effect measure and range for each outcome of interest is reported. For median effect measures based on seven or more studies, the interquartile range is reported. For interventions with a large number of studies, we also evaluated whether the intervention's effect varied by follow-up time.

Table 3. .08 BAC laws: descriptive information about included studies

	Number of studies
Papers meeting inclusion criteria	9[10–18]
Papers excluded, limited execution quality	0
Qualifying papers	9[10–18]
Study designs	
Time series with concurrent comparison group	2[11,18]
Time series, no concurrent comparison group	2[15,16]
Before–after with concurrent comparison group	5[10,12–14,17]
Outcomes reported	
Fatal injury crashes	8[10–15,17,18]
Fatal and nonfatal injury crashes	1[16]

BAC, blood alcohol concentration.

Intervention Effectiveness and Economic Efficiency

.08 BAC Laws

In the United States, states have two basic types of alcohol-impaired driving laws. The first type prohibits a person from driving while intoxicated (DWI). Originally, these laws did not require evidence of a specific BAC. The second type of law, which came later, made it illegal "per se" to operate a motor vehicle at or above a specified BAC. These laws, referred to as per se laws, were usually enacted in addition to the existing DWI laws. Originally, most per se laws specified a BAC of 0.10 g/dL or 0.15 g/dL as being illegal. In 1983, Utah and Oregon lowered the illegal BAC from 0.10 g/dL to 0.08 g/dL. By May 1, 2001, a total of 24 states, the District of Columbia, and Puerto Rico had enacted laws lowering the illegal BAC to 0.08 g/dL. These laws, referred to as .08 BAC laws, are the subject of this review.

In the United States, per se laws apply to all drivers, but they target primarily drivers aged 21 years and older. This target is because, as of July 1998, all states had enacted per se laws for drivers aged 20 years and younger that establish BAC limits of 0.02 g/dL or less.

Reviews of evidence

Effectiveness. The evidence base for .08 BAC laws included published journal articles, technical reports, and conference papers. Our search identified nine studies, all of which were of sufficient design quality and execution to be included in the review.[10–18] Descriptive information about the quality, study design, and outcome measures from these studies is presented in Table 3. Details of the nine qualifying studies are provided in the Appendix and at the website (www.thecommunityguide.org).

All nine studies analyzed data from police incident reports of crashes occurring on public roadways. Post-law follow-up times for individual state laws ranged from 1 to 14 years (median, 5).

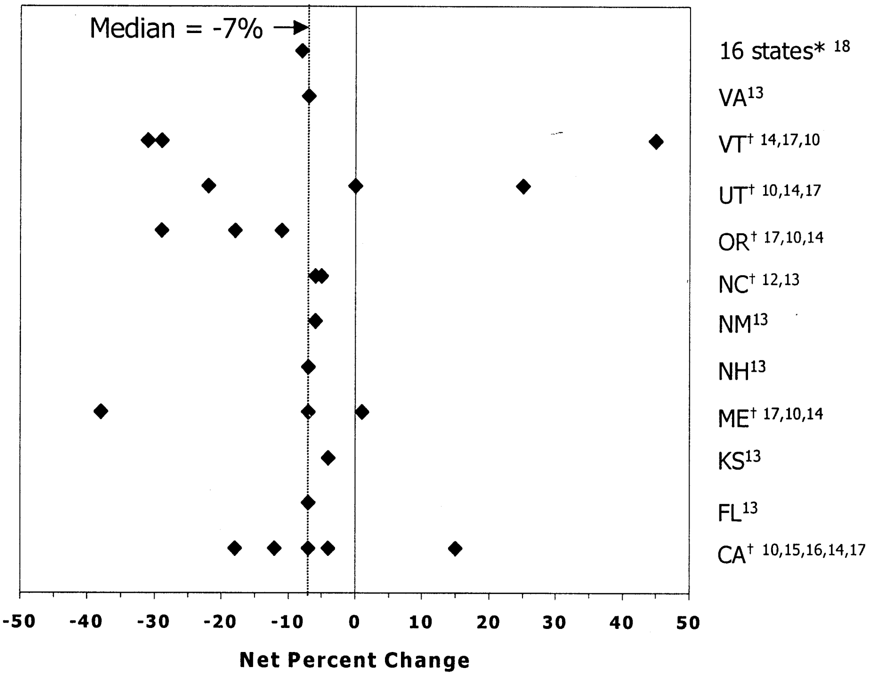

Figure 2. Percent change in measures of alcohol-related motor vehicle fatalities after .08 BAC laws were enacted, by state. *Numbers correspond to reference numbers of cited studies. †Median percent change calculated by using the median value for the state.

Eight of the nine studies reported the percent change in alcohol-related fatalities (post-law period vs pre-law period) or provided the data needed to calculate the measure.[10,12–18] Seven studies provided state-specific percent change values, and the remaining study provided a summary percent change value for all 16 states that enacted .08 BAC laws before January 1, 1998 (Figure 2). The median post-law percent change in alcohol-related motor vehicle fatalities was −7% (interquartile range, −15% to −4%). Results were generally consistent in direction and size across the studies.

One study reported a 45% net increase (95% confidence interval [CI], −13% to +144%) in fatalities after enactment of the .08 BAC law in Vermont.[10] This study compared alcohol-related fatalities in Vermont with those in New Hampshire. The result of this comparison is imprecise because there were fewer than 100 alcohol-related motor vehicle fatalities in each state during the 3-year study period.

Evaluations of .08 BAC laws in the states of California, Maine, Oregon, Utah, and Vermont[10,11,14–17] were limited in their ability to separate the effect of .08 BAC laws from that of administrative license revocation (ALR) laws, which were enacted at about the same time. ALR laws allow the arresting officer, judge, or magistrate to seize the license of a driver who refuses or fails a BAC test. Two recently published studies provide summary estimates of the effect of .08 BAC laws independent of ALR laws. In a secondary analysis, Hingson et al.[13] reported an overall post-law decline in alcohol-related fatal crashes of 5% in four states that had long-standing ALR laws. Voas et al.[18] estimated the separate effects of BAC laws and ALR laws by using multivariate regression analysis. They reported an 8% decline in fatally injured alcohol-impaired drivers attributable to .08 BAC laws.

Applicability. The states represented in the evidence base are geographically diverse with varying population densities. Because all of the studies analyzed data from statewide police incident reports of fatal crashes, the evidence of effectiveness should be applicable to all drivers affected by .08 BAC laws. None of the studies, however, provided data to assess differences in effectiveness for various subgroups of the driving population.

In support of .08 BAC laws, the U.S. Congress included a provision in the Fiscal Year 2001 Department of Transportation and Related Agencies Appropriations Act[19] that requires states to implement .08 BAC laws by October 2003 or risk losing federal highway construction funds.

Other positive or negative effects. Three studies measured outcomes other than motor vehicle crashes, including public knowledge and perception of impaired driving laws, self-reported impaired driving, and impaired driving arrests.[12,15,16] Information about these other potential effects was not summarized in this review.

Table 4. Lower BAC laws for young or inexperienced drivers: descriptive information about included studies

	Number of studies
Papers meeting inclusion criteria	11[22–32]
Papers reporting on more than one study	2[25,27]
Actual number of studies meeting inclusion criteria	16
Papers excluded, limited execution quality	3[25,27,28]
Actual number of studies excluded	8
Qualifying papers	8[22–24,26,29–32]
Papers reporting additional information on already-included studies	2[31,32]
Actual number of qualifying studies	6
Study designs	
Time series with concurrent comparison group	3[24,29,30]
Time series, no concurrent comparison group	1[23]
Before–after with concurrent comparison group	2[22,26]
Outcomes reported	
Fatal injury crashes	3[22,29,30]
Fatal and nonfatal injury crashes	2[24,26]
"Had been drinking" crashes	1[23]

BAC, blood alcohol concentration.

Economic. No studies were found that met the requirements for inclusion in a *Community Guide* review.[7]

Barriers to intervention implementation. One potential barrier to implementation of .08 BAC laws is the view that the laws discourage "social drinkers" from driving after drinking small amounts of alcohol but do not deter "hard-core" drinking drivers. Results of the systematic review provide some evidence to counter this view. Five of the nine studies measured fatalities involving drivers with BACs of 0.10 g/dL or higher, and these studies reported post-law reductions for most states.[11–14,18]

Conclusion. According to the *Community Guide*'s rules of evidence, available studies provide strong evidence that .08 BAC laws are effective in reducing alcohol-related crash fatalities.

Lower BAC Laws for Young or Inexperienced Drivers

Lower BAC laws for young or inexperienced drivers establish a lower illegal BAC for these drivers than for older or more experienced drivers. Although these laws are commonly referred to as "zero tolerance" laws, in many jurisdictions the BAC limit for affected drivers is slightly above zero (e.g., 0.02 g/dL).

Young people who drive after drinking alcohol pose an inordinate risk to themselves, their passengers, and other road users. A recent U.S. study[20] estimated that male drivers aged 16 to 20 years with BACs in the range of 0.08 to <0.10 g/dL were 24 times more likely to die in a motor vehicle crash than those with BACs of zero.

In the United States, lower BAC laws have typically applied to all drivers younger than the minimum legal drinking age of 21 years. In Austria, Australia, New Zealand, and some Canadian provinces, lower BAC laws apply either to all newly licensed drivers or to newly licensed drivers younger than a specified age.[21] The first U.S. laws lowering the illegal BAC for underage drivers were enacted in 1983 in Maine and North Carolina. By December 1994, 27 states and the District of Columbia had enacted lower BAC laws, with BAC limits ranging from any detectable level of BAC to 0.07 g/dL.[22] In support of lower BAC laws, the U.S. Congress included a provision in the National Highway Systems Designation Act of 1995 that required states to implement a BAC limit of 0.02 g/dL or less for all drivers younger than the age of 21 years by October 1998 or risk losing federal highway construction funds. By July 1998, all 50 states had enacted lower BAC laws.

Reviews of evidence

Effectiveness. The evidence base for this intervention included published journal articles, technical reports, and conference proceedings. We found nine publications[22–30] that reported on 14 separate studies of the effectiveness of lower BAC laws. Two additional papers provided more information about an already-included study.[31,32] Descriptive information about the quality, study design, and outcome measures from these studies is presented in Table 4. Details of the six qualifying studies are provided at the website (www.thecommunityguide.org).

Four of the six studies were conducted in the United States,[22,23,29,30] and the remaining two were conducted in Australia.[24,26] Two of the U.S. studies evaluated lower BAC laws in multiple states.[22,30] All six studies analyzed data from police incident reports of motor vehicle crashes occurring on public roadways. Post-law follow-up times for individual state laws ranged from less than 1 year to 15 years. The median post-law follow-up time for the six studies was 22 months.

Each of the six studies reported a post-law reduction in crashes. The three studies that examined fatal crash outcomes reported declines of 24%,[30] 17%,[22] and 9%.[29] The two studies that examined fatal and nonfatal injury crashes reported declines of 17%[26] and 3.8%.[24] The study that examined crashes in which the investigating police officer believed that the driver had been drinking alcohol reported a decline of 11%.[23]

Applicability. The same body of evidence used to assess effectiveness was used to assess the applicability of these interventions in various settings. The states studied are geographically diverse and have both urban and rural populations. Because all of the studies analyzed data from the statewide files of police-reported crashes, the evidence of effectiveness should be applicable to all drivers affected by these lower BAC laws. None of the studies, however, provided data to assess differences in effectiveness for various subgroups of the affected population.

Lower BAC laws have been enacted for other defined populations not addressed in this review, including commercial truck drivers and people convicted of driving while impaired. The Centers for Disease Control and Prevention has recommended that states consider enacting lower BAC laws for all drivers who transport children.[33]

Other positive or negative effects. It is possible that drivers younger than the age of 21 years with high BACs could receive "zero tolerance" citations for violating the lower BAC law, whereas adults with the same BAC would be arrested for the more serious offense of driving under the influence of alcohol (DUI). Voas et al.[29] explored this potential negative effect in an evaluation of California's 1994 lower BAC law. They reported that the combined rate of alcohol-related license suspensions for zero tolerance citations and DUI arrests among underage drivers increased only slightly after enactment of the 1994 lower BAC law. Furthermore, 57% of underage drivers who received zero tolerance citations had BACs above 0.08 g/dL. The investigators concluded that California's 1994 lower BAC law resulted in about half of the potential DUI arrests among underage drivers being converted to less serious zero tolerance citations.

Economic. One study[34] met the criteria for inclusion[6,7] in the review of lower BAC laws for young or inexperienced drivers. The study applied previously published crash costs and used effectiveness data from other previously published studies to illustrate how these costs could be applied to lower BAC laws in the United States. The benefits from a reduction in alcohol-related crashes were estimated on the basis of the assumption that lower BAC laws reduce young drivers' alcohol-related crashes by 20%. Monetary benefits and costs were reported in dollars per mile driven.

The study conducted a cost–benefit analysis. The estimated benefit-to-cost ratio[b] for lower BAC laws was $11 per dollar invested when violators receive a 6-month license suspension. Costs included the cost of trials and sanctions imposed and compliance costs to young drivers (i.e., cost of the loss of mobility).

The study was classified as satisfactory, based on the quality assessment criteria for economic evaluations used in the *Community Guide*.[7,35] Study details, adjusted results, and quality scoring are provided in the economic evaluation summary tables at the website (www.thecommunityguide.org).

Barriers to intervention implementation. All U.S. states currently have lower BAC laws for drivers younger than age 21 years. Voas et al.[29] discussed several potential barriers to full enforcement of these laws. Because young people are less likely than adults to drink in bars, police patrols that target bar neighborhoods are likely to miss underage drinking drivers. Also, officers may have difficulty identifying underage drinking drivers with low BACs who do not show signs of impairment. Finally, because of ambiguities, some state laws do not authorize officers to test the BAC of an underage driver unless the officer has probable cause to believe that the driver's BAC is above the legal limit for adults.

Conclusion. According to the *Community Guide*'s rules of evidence, there is sufficient evidence that lower BAC laws are effective in reducing alcohol-related crashes among young or inexperienced drivers.

Minimum Legal Drinking Age Laws

Minimum legal drinking age (MLDA) laws specify an age below which the purchase or public consumption of alcoholic beverages is illegal. Studies included in this review assessed the effect of raising or lowering the MLDA on crashes and related fatal and nonfatal injury outcomes.

In the United States, several states lowered their MLDA during the early 1970s. Shortly thereafter, in response to an increase in motor vehicle fatalities among young people, some of these states raised their MLDA. To address continuing concerns about youth drinking and driving, federal legislation requiring states to adopt a minimum drinking age of 21 years or lose highway funds was passed in 1984. By 1987, all U.S. states had adopted an MLDA of 21.

Reviews of evidence

Effectiveness. The evidence base for this intervention included only published journal articles. We reviewed three bodies of evidence that evaluated the effect of MLDA changes: studies of the effect of raising the

[b]A benefit-to-cost ratio is provided as a stand-alone piece of information and should not be used to rank interventions unless (1) there is a known budget constraint, (2) the interventions are mutually independent, or (3) interventions exhibit constant returns to scale.

Table 5. Minumum legal drinking age laws: descriptive information about included studies

	Number of studies
Papers meeting inclusion criteria	46[30,36–80]
Papers excluded, limited execution quality	13[38,49,57,59,62–64,69–72,76,77]
Qualifying papers	33[30,36,37,39–48,50–56,58,60,61,65–68,73–75,78–80]
Papers reporting additional information on already-included studies	3[78–80]
Actual number of qualifying studies[a]	33[30,36,37,39–48,50–56,58,60,61,65–68,73–75]
Study designs	
Time series with concurrent comparison group	16[30,36,40,42,48,52,53,55,61,65–68,73–75]
Time series, no concurrent comparison group	2[39,50]
Before–after with concurrent comparison group	15[37,41,43–47,51,54,56,58,60]
Outcomes reported	
Fatal injury crashes or crash fatalities	22[30,36,37,39,41,43–47,50,53–55,61,65–68,73–75]
Fatal and nonfatal injury crashes	8[42,48,51,52,60]
Other crash types	4[40,52,56,58]

[a]Four studies from a single paper[60] qualified for review.

MLDA, studies of the effect of lowering the MLDA, and studies that used multiple regression to evaluate the effect of MLDA changes. The regression-based studies are reported separately because they cover all U.S. states during overlapping time periods, and their results are not independent of each other. Most studies in the review assessed the effect of changes in the MLDA from 18 to 21 years or vice versa. Outcomes were typically assessed in the age groups affected by the law change.

Forty-nine studies reported in 46 papers met the inclusion criteria for this review: 17 studies of the effect of raising the MLDA,[36–52] 11 studies of the effect of lowering the MLDA contained in eight reports,[53–60] and 18 regression-based studies of the effect of changing the MLDA.[30,61–77] Three papers provided additional information about already-included studies.[78–80] Descriptive information about the quality, study design, and outcome measures from all MLDA studies is presented in Table 5. The effects of changes in the MLDA on crash outcomes likely to involve alcohol are summarized in Table 6. Details of the 33 qualifying studies are provided at the website (www.thecommunityguide.org).

Figure 3 presents the findings from the evidence base, aggregated across all crash outcomes. These results suggest that changes in the MLDA result in changes of roughly 10% to 16% in alcohol-related crash outcomes for the targeted age groups, decreasing when the MLDA is raised, and increasing when it is lowered. These effects were consistent over follow-up times ranging from 7 to 108 months.

In some studies, the age group directly affected by the change in the MLDA was not identical to the age group on which outcomes were evaluated. These discrepancies usually arose when crash data pertaining only to the affected age group were not available, or when young people who were of legal drinking age before the law change were allowed to continue to purchase or consume alcohol (i.e., were grandfathered). For 15 studies with perfect overlap between the age group targeted by the law and the age group analyzed,[30,40–43,45,46,48,51–55,58,67] the median change in crashes was 19%. For 17 studies (in 14 reports) with misclassification into the outcome group of subjects not affected by the MLDA change,[36,37,39,44,50,56,60,61,65,66,68,73–75] the median change was only 12%.

Table 6. Effects of changing the mimiumum legal drinking age: summary effects from the body of evidence on crash outcomes likely to involve alcohol

Outcome	Number of studies	Median change	Range[a]
Raising the MLDA			
Fatal injury crashes[b]	9[36,37,39,41,43–46,50]	17% decrease	30%–7% decrease
Fatal and nonfatal injury crashes	4[42,48,51,52]	15% decrease	33%–6% decrease
Other crashes	2[40,52]	NA	21% and 18% decrease
Lowering the MLDA			
Fatal injury crashes	3[53,54,55]	8% increase	2%–38% increase
Fatal and nonfatal injury crashes	4[60]	5% increase	2% decrease to 22% increase
Other crashes	2[56,58]	NA	22% and 186% increase
Estimated effect of raising the MLDA by 3 years (from 18 to 21) from regression-based studies[29,64–80]			
Fatalities and fatal crashes	9[30,61,65–68,73–75]	12% decrease	17%–8% decrease

[a]When 7 or more studies were available, an interquartile range is presented.
[b]A study evaluating fatal crashes among 16- and 17-year-olds[47] was not included in the summary effect measures.
MLDA, minimum legal drinking age; NA, not applicable.

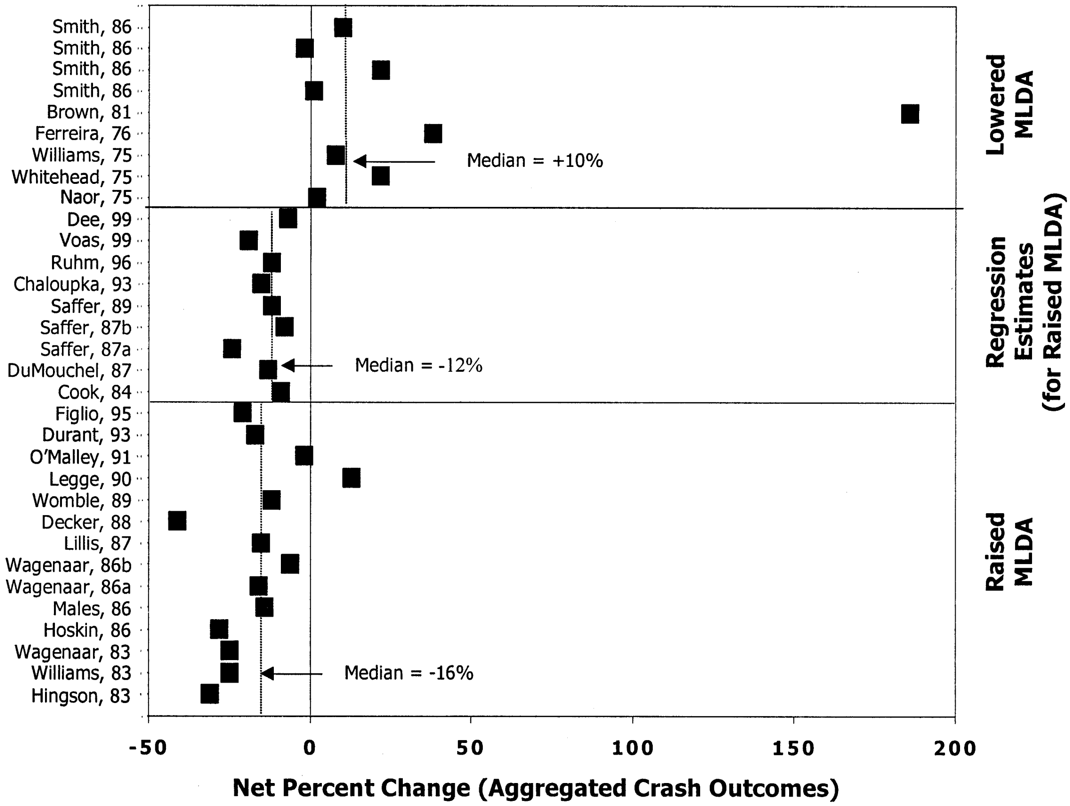

Figure 3. Percent change in aggregated crash outcomes after changes in the MLDA. The "a" and "b" in study names refer to the first or second study by the same author in that year, included in this review. Listed studies for which the author name and year are identical come from a single paper.[60]

Applicability. All of the changes in MLDA assessed in this review affected drivers aged 18 to 20 years. All studies assessed changes in state or provincial laws. Of the 33 studies included in the review, 27 were conducted in the United States,[30,36,37,39–48,50–52,54–56,61,65–68,73–75] one was conducted in the United States and Canada,[53] and the remaining studies were conducted in Australia (4 studies in one report)[60] or Canada.[58] The generalizability of these findings to other countries may be limited by differences in patterns of alcohol consumption and driving among 18- to 20-year-olds.

Other positive or negative effects. Several studies reported that raising the MLDA was associated with decreased alcohol consumption.[36,45,47,51] We did not review this literature systematically but present relevant findings in the evidence tables (available at the website www.thecommunityguide.org). Nine studies also investigated the effect of raising the MLDA on crashes involving adolescent drivers who were younger than the MLDA both before and after it was raised.[37,42,44,46,47,51,66,68,74] Although these studies indicated that raising the MLDA was associated with a median decline in crashes of 6% (interquartile range, −18% to 5%), the size of this effect was inconsistent across studies, with several showing no effect.

Some investigators have postulated that when drivers who have not been legally allowed to drink reach the MLDA, their risk of alcohol-related crash involvement will dramatically increase because of their inexperience in drinking, thus partly or completely offsetting the benefits of MLDA laws.[44,72] Studies that attempt to directly estimate the "drinking experience" effect have produced inconsistent results because of problems in statistically disentangling it from the effect of the MLDA itself.[44,67,72,81] In one study of a cohort that would be affected by both the MLDA and drinking experience effects, raising the MLDA from 18 to 21 years was estimated to decrease nighttime fatal crashes by 15% (95% CI, 3% to 27%).[67] This result is similar to the median effect estimate for MLDA alone, suggesting that if the putative drinking experience effect exists, it does not substantially diminish the benefits of raising the MLDA.

Economic. No studies were found that met the requirements for inclusion in a *Community Guide* review.[7]

Barriers to intervention implementation. Currently, the MLDA is 21 years throughout the United States and 18 years in many other countries (e.g., Australia). The belief among some opponents of MLDA laws that

Table 7. Selective breath testing sobriety checkpoints: descriptive information about included studies

	Number of studies
Papers meeting inclusion criteria	17[85–101]
Papers excluded, limited execution quality	4[88,89,91,96]
Qualifying papers	13[85–87,90,92–95,97–101]
Papers reporting additional information on already-included studies	2[100,101]
Actual number of qualifying studies	11[85–87,90,92–95,97–99]
Study designs	
Time series with concurrent comparison group	6[85,87,90,94,97,99]
Time series, no concurrent comparison group	4[86,92,93,98]
Nonrandomized group trial	1[95]
Outcomes reported	
Fatal injury crashes	2[85,90]
Fatal and nonfatal injury crashes	6[86,90,93–95,98]
Other crash types	6[86,87,92,97–99]

prohibition of drinking among young adults unjustly punishes them for the irresponsible behavior of the subgroup that drives after drinking poses a potential barrier to the strengthening or maintenance of MLDA laws.[82]

Conclusion. According to the *Community Guide*'s rules of evidence, there is strong evidence that MLDA laws, particularly those that set the MLDA at age 21, are effective in preventing alcohol-related crashes and associated injuries.

Sobriety Checkpoints

At sobriety checkpoints, law enforcement officers systematically stop drivers to assess their degree of alcohol impairment. There are two types of sobriety checkpoints. At random breath testing (RBT) checkpoints, all drivers stopped are given breath tests for BACs. RBT checkpoints are used in Australia and several European countries. Issues about the violation of constitutional protections against unreasonable search and seizure[83] have prevented the use of RBT checkpoints in the United States. At selective breath testing (SBT) checkpoints, used in many U.S. states, police must have reason to suspect the driver stopped at a checkpoint has been drinking before a breath test can be demanded. Both types of sobriety checkpoint programs generally include media efforts to publicize the enforcement activity and the consequences of driving with a BAC above the legal limit.

The rationale for the use of checkpoints is based on deterrence theory. Although checkpoints may remove some drinking drivers from the road, their primary goal is to reduce driving after drinking by increasing the perceived risk of arrest. This perceived risk may be influenced by the level of publicity accompanying the enforcement effort, visibility of the checkpoint operations themselves, the likelihood of detection, and drivers' beliefs about their ability to avoid detection.[84]

Reviews of evidence

Effectiveness. The evidence base for this intervention included published journal articles, technical reports,

Table 8. Random breath testing sobriety checkpoints: descriptive information about included studies

	Number of studies
Papers meeting inclusion criteria	16[84,102–116]
Papers excluded, limited execution quality[a]	3[103,110,113]
Papers excluded, least suitable design quality	2[107,109]
Qualifying papers	12[84,102,104–106,108,110–112,114–116]
Papers reporting additional information on already-included studies	2[115,116]
Actual number of qualifying studies[b]	12[84,102,104–106,108,110–112,114]
Study designs	
Time series with concurrent comparison group	2[105,106]
Time series, no concurrent comparison group[b]	7[84,108,110,111,114]
Before–after with concurrent comparison group	3[102,104,112]
Outcomes reported	
Fatal injury crashes	6[84,104,110,114]
Fatal and nonfatal injury crashes (or injuries)	10[84,102,105,106,108,110,112,114]
Other crash types	1[110]
Drivers with BAC >.08	1[111]

[a]One of four studies in one paper[110] did not meet quality criteria.
[b]Three studies from one paper[110] qualified for review.
BAC, blood alcohol concentration.

Table 9. Effects of selective breath testing checkpoints on crash outcomes likely to involve alcohol: summary effects from the body of evidence

Outcome	Number of studies	Median change	Range[a]
Fatal injury crashes	2[85,90]	NA	26% and 20% decrease
Fatal and nonfatal injury crashes	6[86,90,93–95,98]	20% decrease	23%–5% decrease
Other crashes	6[86,87,92,97–99]	24% decrease	35%–13% decrease[b]
Aggregated crashes	11[85–87,90,92–95,97–99]	20% decrease	27%–13% decrease[b]

[a]When 7 or more studies were available, an interquartile range is presented.
[b]One study[99] reported data in a form that could not be converted to our summary effect measures.
NA, not applicable.

and Association for the Advancement of Automotive Medicine proceedings. We found 15 studies of the effectiveness of SBT checkpoints.[85–99] Two additional papers provided more information about an already-included study,[100,101] and one presented data in a form that could not be converted to our summary effect measure.[99] Descriptive information about the quality, study design, and outcome measures from these studies is presented in Table 7. Our search identified 17 studies of the effectiveness of RBT checkpoints (four of these studies were reported in one paper).[84,102–114] Two additional papers provided information about an already-included study.[115,116] Descriptive information about the quality, study design, and outcome measures from these studies is presented in Table 8.

Details of the 23 qualifying studies are provided at the website (www.thecommunityguide.org). Summaries of study outcomes are reported in Tables 9 (SBT) and 10 (RBT). Outcomes from studies reporting crash-related outcomes are also provided in Figures 4 (SBT) and 5 (RBT). Both SBT and RBT checkpoints consistently resulted in decreased crashes. Length of follow-up time ranged from 1 to 120 months (median, 14) and did not appear to influence the size of the declines.

One study assessed the effect of RBT checkpoints on the observed incidence of drinking and driving. This study found that during an RBT checkpoint program, the proportion of drivers with any detectable BAC level declined 13% and the proportion of drivers with BACs above 0.08 g/dL declined 24% from prior levels.[111]

Although RBT checkpoints have greater sensitivity in detecting drinking drivers than SBT checkpoints, this review found no evidence that their effectiveness for reducing alcohol-related crashes differed. None of the studies reviewed was designed to directly compare the effectiveness of RBT and SBT checkpoints, however, so these results should be interpreted with caution. Sensors that allow police to passively sample air in the car for alcohol vapors (passive alcohol sensors) can improve the detection rate at SBT checkpoints by approximately 50%.[99] If such technology becomes more widely used, the sensitivity in detecting drinking drivers at SBT checkpoints may approach that of RBT checkpoints.

Applicability. The same body of evidence used to assess effectiveness was used to assess the applicability of these interventions. Studies that met our quality criteria involved a somewhat larger scale of enforcement and publicity activity than studies that were excluded because of quality limitations. Thus, the reported results may be most generalizable to these larger-scale interventions. The studies were conducted on interventions implemented at the city,[86,87,95,97,98,102,111] county,[91] state,[84,85,90,93,94,104,105,108,110,112] and national level[114] and were evaluated in rural areas,[106,112] in urban areas,[86,87,95,97,98,102,111,112] and in mixed rural and urban areas.[84,85,90,93,94,104,105,108,110]

Other positive or negative effects. Several studies report the arrest of drivers stopped at sobriety checkpoints for other offenses, such as driving with a suspended license or carrying weapons, as an added benefit.[90,94,95,97]

One negative effect of stopping drivers at checkpoints is the resulting inconvenience and intrusion on driver privacy. According to the U.S. Supreme Court, the brief intrusion of a properly conducted sobriety checkpoint is justified in the interest of reducing alcohol-impaired driving.[117] Some civil libertarian groups have also endorsed this position.[84]

In the United States, checkpoints use established protocols to ensure that they are conducted properly.[83]

Table 10. Effects of random breath testing checkpoints on various outcomes: summary effects from the body of evidence

Outcome	Number of studies	Median change	Range[a]
Fatal injury crashes	6[84,104,110,114]	22% decrease	36%–13% decrease
Fatal and nonfatal injury crashes	10[84,102,106,107,108,110,112,114]	16% decrease	20%–11% decrease
Other crashes	2[110]	NA	26% and 15% decrease
Aggregated crashes	11[84,102,104–106,108,110,112,114]	18% decrease	22%–13% decrease
Drivers with BAC >.08%	1[111]	NA	24% decrease

[a]When 7 or more studies were available, an interquartile range is presented.
NA, not applicable; BAC, blood alcohol concentration.

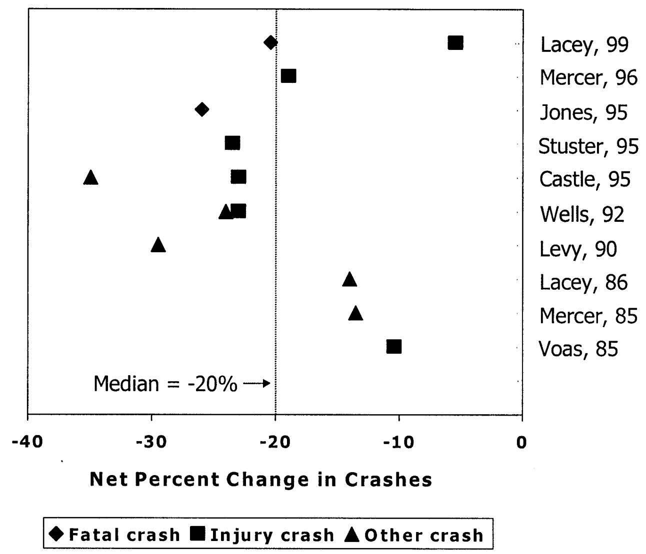

Figure 4. Percent change in crashes likely to involve alcohol after implementing selective breath testing checkpoint programs.

Common components of the protocols include selecting checkpoint locations on the basis of objective criteria (e.g., the incidence of alcohol-related crashes in the area) and stopping cars according to a predetermined system (e.g., every third car that approaches the checkpoint).[90,94,97]

Economic. Four studies[95,104,118,119] were included in the review of sobriety checkpoints. Two studies[95,118] evaluated SBT checkpoints and two studies[104,119] evaluated RBT checkpoints. All studies conducted cost–benefit analyses. Three studies[104,118,119] reported annual net

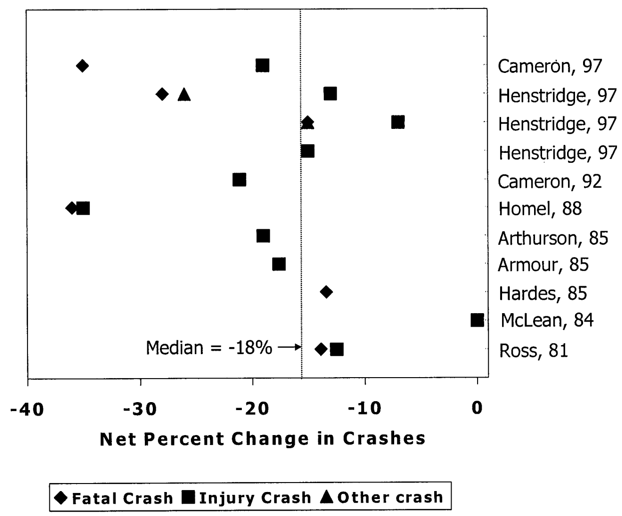

Figure 5. Percent change in crashes likely to involve alcohol after implementing random breath testing checkpoint programs. Listed studies for which the author name and year are identical come from a single paper.[110]

benefits (the stream of benefits minus the stream of costs incurred in 1 year), and the fourth study[95] reported net benefits for the length of the intervention (9 months). Study details, adjusted results, and quality scoring for all four studies are provided in the economic evaluation summary tables at the website (www.thecommunityguide.org).

Selective breath testing checkpoints. The first study[118] modeled a 1-year campaign conducted in a hypothetical community of 100,000 licensed drivers in the United States. The modeled campaign consisted of 156 checkpoints (4 hours each) per year. The effect size assumed in the analysis was a 15% reduction in alcohol-related crashes. Program costs included in the analysis were personnel, equipment, travel delay, trial, punishment, and mobility loss associated with sanctioning (e.g., loss of driver's license). The estimated annual total benefit from alcohol-related crashes averted was $9.2 million (in 1997 U.S. dollars). Benefits were estimated by accounting for medical care, property damage, and public costs averted plus future earnings and quality of life gained. Estimated annual total costs of the intervention were $1.6 million. The estimated annual net benefit was $7.6 million (in 1997 U.S. dollars), resulting in a benefit-to-cost ratio[b] of $6 per dollar invested. This study was classified as very good, based on the quality assessment criteria used in the *Community Guide.*

The second study[95] evaluated a 9-month campaign conducted in four communities in California with checkpoint sites (a fifth community was a comparison group and a sixth implemented roving DWI patrols). The program consisted of 18 checkpoints per community plus publicity campaigns and education programs. Net reductions in alcohol-related crash injuries and deaths ranged from 17.5% to 31.6%. Total aggregated benefits of $3.86 million (in 1997 U.S. dollars) came from societal savings realized through injuries and fatalities avoided. Costs included personnel and equipment. Cost of the publicity campaigns and education programs were not included in the analysis. Total costs of the intervention (aggregated for four communities) were $164,552. The aggregated net benefit was $3.7 million, resulting in a benefit-to-cost ratio of $23 per dollar invested. This study was classified as satisfactory, based on the quality assessment criteria used in the *Community Guide.*

In summary, both studies show positive net benefits and, therefore, from a societal viewpoint, economic benefits of these interventions exceed costs. The hypothetical study reported net benefits almost double those reported by the California study although greater disparity was observed in the benefit-to-cost ratio ($6 in the hypothetical study vs $23 in the California study). The high benefit-to-cost ratio reported by the California study is due, in part, to the underestimation of total costs because the analysis did not include the cost of publicity and education.

Random breath testing checkpoints. The first study[104] was conducted 3 years after statewide RBT checkpoints were introduced in New South Wales, Australia. The program included police operations, media publicity, and revised drunk-driving penalties. Annual total benefits were $228 million (in 1997 U.S. dollars) and were calculated on the basis of the assumption that 75% of the reduction in fatal crashes, serious injuries, minor injuries, and tow-away crashes was attributable to the checkpoints. Annual total program costs were $4 million (in 1997 U.S. dollars) and included personnel, equipment, publicity, and transportation. The net annual benefit reported in the study was $224 million. This study was classified as satisfactory, based on the quality assessment criteria used in the *Community Guide*.

The second study[119] evaluated a proposed nationwide RBT checkpoint intervention in The Netherlands. The proposed intervention included a publicity component and incorporated a more efficient method of transporting offenders to police stations. The checkpoint program was assumed to result in a 25% reduction in alcohol-related injury or property damage on weekend nights. Annual total benefits from cost-savings in the reduction in alcohol-related injury and property damage were estimated at $31.4 million (in 1997 U.S. dollars). The investigators did not specify the value of statistical life used to calculate the cost-savings from averted death or the procedure and assumptions to calculate cost-savings from averted injury. Annual total costs, including materials and publicity, were estimated at $15.6 million. The annual net benefit of the intervention was estimated to be $15.8 million, resulting in a benefit-to-cost ratio of $2 per dollar invested. This study was classified as good, based on the quality assessment criteria used in the *Community Guide*.

In summary, both studies showed positive net benefits (i.e., the economic benefits of the interventions are greater than the economic costs). The Australian intervention, which was more intensive and reached one in three drivers, showed larger net benefits than the modeled Netherlands intervention, which was designed to reach one in nine drivers.

Barriers to intervention implementation. Although the U.S. Supreme Court has determined that SBT checkpoints are permissible,[117] some state courts prohibit them. Where checkpoints are permitted, police concern about low arrest rates can be an important barrier.[89] Informing police officers about the general deterrence benefit of their efforts and providing them with regular feedback that links these efforts to crash prevention may decrease this frustration.[85,109]

Conclusion. According to the *Community Guide*'s rules of evidence, available studies provide strong evidence that both SBT and RBT sobriety checkpoints are effective in preventing alcohol-impaired driving, alcohol-related crashes, and associated fatal and nonfatal injuries.

Intervention Training Programs for Servers of Alcoholic Beverages

Server intervention training programs provide education and training to servers of alcoholic beverages with the goal of altering their serving practices to prevent patron intoxication and alcohol-impaired driving. These practices may include offering patrons food with drinks, delaying service to rapid drinkers, refusing service to intoxicated or underage patrons, and discouraging intoxicated patrons from driving.

People often drive after consuming alcohol in bars, clubs, and restaurants. Two analyses found that about 40% to 60% of intoxicated drivers had recently departed from a licensed drinking establishment.[120,121] Thus, altering server practices to prevent intoxication at drinking establishments may be an effective means of reducing alcohol-impaired driving. As of January 1, 2000, 11 states had established mandatory server training programs for all licensed establishments, and 10 states provided liability protection to establishments that voluntarily implemented server training.[122] Local governments can also mandate server training.

There are currently no standards for server training programs, and their implementation varies widely in terms of the content covered, instructional time, and mode of delivery (e.g., face-to-face vs videotaped). Some programs are offered in classroom settings by professional trainers, and others consist only of a video or written material that employees are encouraged to look at on their own.[123] Generally, the programs involve education about alcohol beverage control (ABC) laws and training in identifying signs of intoxication. They frequently include training in specific intervention techniques such as offering food, delaying service, or refusing service. This training may be supplemented by role-playing of intervention scenarios. Some programs also evaluate the alcohol serving policies of a drinking establishment and recommend changes to reduce intoxication such as eliminating drink promotions, serving a variety of nonalcoholic beverages, or increasing the availability of food.[124]

Factors other than server training influence serving practices in licensed establishments. These factors include enforcement of existing ABC laws,[125] server liability (or dram shop) laws and high-profile server liability cases,[126] and community coalitions to encourage responsible serving practices.[127] These factors may also influence the degree of management support that servers receive for participating in server training and for improving serving practices. Such management support is thought to be an essential prerequisite for changes in server behavior.[123,128–130]

Reviews of evidence

Effectiveness. The evidence base for this intervention included published journal articles and technical reports. Our search identified eight studies of the effectiveness of server training.[129,131–137] Four reports provided additional information about already-included studies.[138–141] Three studies had limited execution quality and were not included in the review.[133,134,136] Details of the five included studies are provided at the website (www.thecommunityguide.org).

Two studies assessed observed server behaviors, and both studies found significant improvements after relatively intensive (4.5- to 6-hour) training programs.[132,135] One study[132] found that servers in four bars at which training was provided showed improved scores on a rating scale, reflecting both appropriate and inappropriate server behaviors relative to those in comparable bars. Another study,[135] in which interested servers in two bars received training, found an increase in appropriate interventions by trained servers in response to rapid drinking by "pseudopatrons," research assistants pretending to be patrons.

Three studies evaluating drinkers' BACs found that server training was associated with a decrease in patron intoxication.[129,135,137] One study discussed above[135] found that none of the pseudopatrons served by trained servers reached BAC levels of 0.10 g/dL, whereas 45% of those served by untrained servers did. A second study[137] that involved less-intensive server training (1 to 2 hours) at 14 drinking establishments assessed the proportion of patrons leaving the premises with BACs above 0.08 g/dL. This study found that the rate of intoxication in participating premises relative to matched comparisons decreased by 17% at a 2-week follow-up and by 28% after 3 months. The investigators noted that much of this success was attributed to a single establishment with an unusually supportive manager. In the third study,[129] conducted at a Navy enlisted club, an intensive 18-hour training course was supplemented by other policy changes such as eliminating the sale of pitchers of drinks. These changes were associated with a 33% net decrease in the percentage of patrons with estimated BACs of 0.10 g/dL or greater relative to a comparable club. Although overall alcohol consumption did not substantially decrease (−0.1 drinks, $p>.05$), there was a nonsignificant decrease in the rate of consumption (−0.8 drinks/hour, $p>.05$), suggesting that patrons drank more slowly but stayed in the establishment longer.

Finally, one study[131] evaluated the effect of a statewide 1-day mandatory server training program. On the basis of a time series analysis that included single-vehicle nighttime *fatal* crashes in other states as a covariate, server training resulted in an estimated net decrease of 23% in single-vehicle nighttime *injury* crashes.

Applicability. Of the five studies evaluated in this review, three[129,131,135] were conducted in the United States, one in Canada,[132] and one in Australia.[137] With the exception of one study,[131] all of the participating drinking establishments volunteered to have their servers attend the training. Thus, managers who chose to participate in the evaluated server training programs may have been unusually supportive of the goals of the programs. Three of the five programs evaluated[129,132,135] were also implemented on a very limited scale, in a small number of drinking establishments. These training programs were relatively time intensive (longer than 4 hours), involved face-to-face training, and covered a broad curriculum, including specific intervention practices in contrast with training programs generally in use, which vary widely in intensity, mode of delivery, and content.[123] Thus, the studies we reviewed may reflect the efficacy of server training under near-optimal conditions. It is not clear to what extent these findings might generalize to larger-scale community-wide programs, to programs with substantially different training methods or content, or to programs that do not recruit well-motivated managers. Finally, only one study[131] evaluated outcomes beyond a 3-month follow-up period, leaving the long-term effect of this intervention open to question.

Other positive and negative effects. None of the studies reviewed examined consequences of intoxication other than those associated with drinking and driving. It is plausible, however, that the benefits of decreased levels of intoxication resulting from improved server practices would extend to other forms of alcohol-related injury, violence, and crime. In one study, there was also a trend toward servers receiving increased gratuities after training.[135] No negative effects of server training programs were noted.

Economic. No studies were found that met the requirements for inclusion in a *Community Guide* review.[7]

Barriers to intervention implementation. Resistance to server training by managers of drinking establishments is a potential barrier to effective implementation of this intervention. Although many managers of drinking establishments are supportive of the concept of server training,[136] concerns about the effect on profits can seriously erode their support for improved server practices.[141] One study that addressed this issue by examining gross receipts found no noticeable reduction after server training.[129] That study was conducted at a Navy base enlisted club, however, and the finding may not generalize to other types of drinking establishments. In addition to profitability concerns, some managers also react negatively to the concept of "policing" their customers.[141] Management support for server training programs could be increased by offering positive incentives (e.g., insurance discounts) to establishments that improve serving practices,[142] by strengthening or highlighting disincentives for irresponsible practices (e.g., stronger enforcement of ABC laws),[125] and by building broad community support for such programs.[136]

Maintaining the consistency of server training programs is essential for effective implementation. Given the high employee turnover rate for servers, going beyond a "demonstration" training program requires that training sessions be offered on a continuing basis and that their quality be consistent across time and locations. Problems in staffing and in scheduling training sessions can result in decreased quality of implementation.[137] Although less-intensive server training programs (e.g., video-based) are easier and less expensive to implement, their effectiveness is not known.

Conclusion. According to the *Community Guide*'s rules of evidence, there is sufficient evidence that intensive, high-quality, face-to-face server training, when accompanied by strong and active management support, is effective in reducing the level of intoxication in patrons. This type of training is likely to have a desirable effect on alcohol-impaired driving if the affected patrons cease drinking or continue drinking in relatively safe environments after leaving the drinking establishment.[128] The optimal conditions for this situation would exist if server training were established at all drinking establishments within a community. In this review, only two studies that met the quality criteria evaluated community-wide server training programs. Thus, further research is needed about the fundamental question of whether server intervention training programs delivered community-wide are effective at decreasing intoxication and, ultimately, alcohol-impaired driving.

Research Issues

Effectiveness

Sufficient or strong evidence exists that the effectiveness of the five interventions reviewed reduces alcohol-impaired driving. However, important issues related to the effectiveness of these interventions require further research.

General questions

- How do interventions to reduce alcohol-impaired driving interact with each other (e.g., .08 BAC laws and administrative license revocation)?
- What effects do these interventions have on long-term changes in social norms about drinking and driving?

Laws

- How do variations in enforcement levels influence the effectiveness of laws to reduce alcohol-impaired driving?
- What are the independent effects of publicity on the effectiveness of laws to reduce alcohol-impaired driving?
- Does public compliance with new laws change in a predictable manner over time?

Sobriety checkpoints

- Does the use of passive alcohol sensors at sobriety checkpoints improve their deterrent effects?
- Are the deterrent effects of sobriety checkpoints diminished if warning signs are posted that allow drivers to avoid the checkpoints?
- How do various configurations of sobriety checkpoints (e.g., intermittent blitzes vs continuous, weekend nights vs random time periods, number of officers per checkpoint) affect deterrence?
- What level of enforcement and publicity about sobriety checkpoints is necessary to maintain effectiveness over time?

Server intervention training

- Are server intervention training programs delivered community-wide effective at decreasing alcohol-impaired driving and alcohol-related crashes?
- What essential content areas should be included in all server intervention training programs?
- What effect does the method by which training is delivered (e.g., videotapes, lectures, role-playing) have on the effectiveness of server training programs?
- How do mandatory versus voluntary server training programs differ with respect to:
  —management support for program goals?
  —level of participation in training programs?
  —overall effectiveness for decreasing patron BACs and drinking and driving?

- What specific management policies and practices are necessary to get the maximum benefits from server intervention training?
- What is the long-term effect of server intervention training programs? Are "booster sessions" required to maintain effectiveness?
- What effect does server intervention training have on alcohol sales, overall revenues, and tips?

Applicability

These five interventions should be applicable in most target populations and settings. However, questions remain about possible differences in the effectiveness of each intervention for specific settings and subgroups. For example:

- Are these interventions equally effective in rural and urban settings?
- Are these interventions equally effective when applied to populations with different baseline levels of alcohol-impaired driving?

- Does targeting publicity efforts to specific subpopulations (e.g., young drivers, ethnic minorities, men) improve the effectiveness of interventions to reduce alcohol-impaired driving?

Other Positive or Negative Effects

Few other positive and negative effects were reported in this body of literature. Further research about the following questions would be useful:

- What proportion of youths charged with violating zero tolerance laws had BAC levels elevated enough to warrant a more serious drinking-driving offense?
- Do interventions to reduce alcohol-impaired driving reduce other forms of alcohol-related injury?

Economic Evaluations

Little economic evaluation information was available. Research is warranted to answer the basic economic questions: What are the cost-benefit, cost utility, and cost-effectiveness of interventions to reduce alcohol-impaired driving?

Barriers to Implementation

Several of the interventions reviewed face barriers to effective implementation. Research into the following areas may help to overcome these barriers:

- What role can community coalitions play in removing barriers to implementing interventions designed to prevent alcohol-impaired driving?
- What are the most effective means of disseminating research findings about effectiveness to groups that want to implement interventions?
- What forms of incentives (e.g., insurance discounts) are most helpful for increasing management and owner support for server intervention training?
- How can the costs of interventions to prevent alcohol-impaired driving be shared or subsidized?
- What situational and environmental influences help or hinder the implementation of server intervention training?

Discussion

Interventions to prevent alcohol-impaired driving are implemented within the social and legal context of a community. Although these reviews evaluate each intervention as an independent activity, effective prevention of impaired driving requires a comprehensive and systematic approach that addresses various individual and ecologic influences on drinking and driving behavior.[143–145] These reviews can help decision makers identify and implement effective interventions that fit within an overall prevention strategy.

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Appendix: Studies Measuring the Effectiveness of 0.08% Blood Alcohol Concentration (BAC) Laws

Author & year (study period) Design suitability: design Quality of execution Evaluation setting	Study description[a] and other information	Results				
		Effect measure	Reported baseline	Reported effect	Value used in summary	Follow-up time
Research and Evaluation Associates 1991 (1986-1990)[1] Moderate: Time series Fair California	Law went into effect: Jan. 1, 1990 Comparison: Pre-law fatalities from crashes involving alcohol in California Note: Because .08 BAC and administrative license revocation laws were implemented within 6 months of each other, separate effects of each cannot be isolated	Fatalities from crashes involving drivers with BAC ≥0.01%	Pre-law mean = 225 fatalities/mo	-12% (p < .01)	- 12%	1 year
Rogers 1995 (1985-1993)[2] Moderate: Time series Fair California	Law went into effect: Jan. 1, 1990 Comparison: Pre-law fatal and severe injury crashes in California Note: Because .08 BAC and administrative license revocation laws were implemented within 6 months of each other, separate effects of each cannot be isolated	Single vehicle nighttime (8:00 PM-3:59 AM) fatal and severe injury crashes involving male drivers	Pre-law mean estimated from graph = ~190 crashes/mo	~ -7% (NS)	-7%	4 years
Foss 1998 (1991-1996)[3] Greatest: Before-after with concurrent comparison Fair North Carolina	Law went into effect: Oct. 3, 1993 Comparison: 1) Pre-law fatal crashes involving alcohol in North Carolina; 2) Fatal crashes involving alcohol in 37 states without 0.08% BAC laws Note: Some of post-law effect in NC may be attributable to the Booze-It-and-Lose-It sobriety checkpoint program (Nov. 1994 – July 1995)	Percent change in proportion of all fatal crashes involving drivers ≥21 years with BAC ≥0.10%	Pre-law proportion: NC = .228 37 comparison states = .238	Post-law proportion: NC = .183 37 comparison states = .207 NC vs. 37 comparison states: -6% (95% CI, -13%, +3%)	-6%	39 mos
Apsler 1999 (1982-1995)[4] Greatest: Time series with concurrent comparison Fair States: CA, FL, KS, ME, NH, NM, NC, OR UT, VT, VA	Laws went into effect: Aug. 1, 1983 to July 1, 1994 Comparison: Pre-law crashes involving alcohol	Ratio of fatal crashes involving a driver with BAC ≥0.10% vs. 0.00%	N/A	Coefficient of change in ratio of fatal crashes involving a driver with BAC ≥0.10% vs. 0.00% CA: -0.10, p < .05 FL: -0.15, p < .05 KS: -0.15, p < .05 ME: +0.09, NS NH: +0.16, NS NM: -0.36, p < .05 NC: -0.08, p < .05 OR: +0.60, NS UT: -0.09, NS VT: -0.48, p < .05 VA: -0.13, p < .05	Not used because coefficient cannot be transformed to percent change	1.5–12.4 years

Appendix Continued

Author & year (study period) Design suitability: design Quality of execution Evaluation setting	Study description[a] and other information	Effect measure	Results: Reported baseline	Results: Reported effect	Results: Value used in summary	Results: Follow-up time
Hingson 2000 (1988-1998; varied by state, range 8 to 12 years)[5] Greatest: Before-after with concurrent comparison Fair States: FL, KS, NH, NM, NC, VA	Laws went into effect: July 1, 1993 to July 1, 1994 Comparisons: 1) Pre-law fatal crashes involving alcohol in study states 2) Fatal crashes involving alcohol in matched comparison states	Percent change in the proportion of all drivers in fatal crashes with BAC ≥0.10%	Pre-law proportion FL: .21 GA: .21 KS: .24 OK: .23 NH: .23 CT: .28 NM: .31 CO: .25 NC: .20 TN: .25 VA: .22 MD: .14	95% CI FL vs GA: - 7% (-14%, ±0%) KS vs OK: - 4% (-15%, +10%) NH vs CT: - 7% (-25%, +14%) NM vs CO: - 6% (-17%, +6%) NC vs TN: - 5% (-22%, +4%) VA vs MD: - 7% (-19%, +7%) Overall: - 6% (-10% , -2%)	FL: -7% KS: -4% NH: -7% NM: -6% NC: -5% VA: -7%	4–5 years
Hingson 1996 (1976-1991; varied, range from 3 to 15 years)[6] Greatest: Before-after with concurrent comparison Fair States: CA, ME, OR, UT, VT	Laws went into effect: Aug. 1, 1983 to July 1, 1990 Comparisons: 1) Pre-law fatalities among drivers with BAC ≥0.08% in study states 2) Fatalities among drivers with BAC ≥0.08% in matched comparison states Note: All study states and only one comparison state had administrative license revocation laws (authors estimate these accounted for ~5% of observed decreases in fatal crashes)	Percent change in the proportion of fatally injured drivers with BAC ≥0.08%	Pre-law proportion CA: 0.22 TX: 0.20 ME: 0.26 MA: 0.22 OR: 0.29 WA: 0.28 UT: 0.14 ID: 0.15 VT: 0.25 NH: 0.22	95% CI CA vs TX: -18% (-33%, -12%) ME vs MA: - 7% (-33%, +12%) OR vs WA: -18% (-25%, -11%) UT vs ID: -22% (-36%, - 5%) VT vs NH: +45% (-13%,+144%) Overall: -16% (-22%, -10%)	CA: -18% ME: -7% OR: -18% UT: -22% VT: +45%	2–8 years

Appendix Continued

Author & year (study period) Design suitability: design Quality of execution Evaluation setting	Study description[a] and other information	Effect measure	Results Reported baseline	 Reported effect	 Value used in summary	 Follow-up time
Scopatz 1998 (1976-1991; varied, range from 3 to 15 years)[7] Greatest: Before-after with concurrent comparison Fair States: CA, ME, OR, UT, VT	Laws went into effect: Aug. 1, 1983 to July 1, 1990 Comparisons: 1) Pre-law fatalities among drivers with BAC ≥0.08% in study states 2) Fatalities among drivers with BAC ≥0.08% in matched comparison states Note: This study is a re-analysis of Hingson 1996 study using different comparison states	Percent change in the proportion of fatally injured drivers with BAC ≥0.08%	Pre-law proportion CA: 0.22 AZ: 0.16 ME: 0.26 RI: 0.21 OR: 0.29 WY: 0.29 UT: 0.14 CO: 0.30 VT: 0.33 NY: 0.25	(CIs were not provided) CA vs AZ: +15% ME vs RI: -38% OR vs WY: -29% UT vs CO: +25% VT vs NY: -29% Overall: - 5%	CA: +15% ME: -38% OR: -29% UT: +25% VT: -29%	2–8 years
Johnson 1995 (1982-1992; varied) range from 3 to 4 years[8] Greatest: Before-after with concurrent comparison Fair States: CA, ME, OR, UT, VT	Laws went into effect: Aug. 1, 1983 to July 1, 1990 Comparison: Pre-law fatal crashes involving alcohol in study states	Percent change in proportion of all fatal crashes involving drivers ≥21 years with BAC ≥0.10%	Pre-law proportion CA: 0.246 ME: 0.219 OR: 0.341 UT: 0.198 VT: 0.336	CA: - 4%, p = .09 ME: + 1%, p = .10 OR: -11%, p = .06 UT: 0%, p = .90 VT: -31%, p = .04	CA: -4% ME: +1% OR: -11% UT: 0% VT: -31%	18–24 mos
Voas 2000 (1982-1997)[9] Greatest: Time series with concurrent comparison Fair 50 U.S. states and Washington, DC	Laws went into effect: Aug. 1, 1983 to July 2, 1997 Location: All 50 states and DC; 16 states had enacted 0.08% BAC laws by Dec. 1997 Comparisons: 1) Pre-law fatal crashes involving alcohol in 16 states that enacted 0.08% BAC laws by Dec. 1997 2) Fatal crashes involving alcohol in 34 states that did not enact 0.08% BAC laws by Dec. 1997	Change in the ratio of fatal crashes involving drivers ≥21 years with BACs ≥0.10% vs. 0.00%	N/A	-8.0% (95% CI, -3.4%, -12.4%)	-8.0%	0.5–14.4 years

Appendix Continued

[a] Each study analyzed information from police incident reports of motor vehicle crashes that occurred on public roads.

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Reviews of evidence regarding interventions to reduce alcohol-impaired driving. Shults RA, Elder RW, Sleet DA, Nichols JL, Alao MO, Carande-Kulis VG, Zaza S, Sosin D.M., Thompson RS, Task Force on Community Preventive Services, American Journal of Preventive Medicine, Vol 21 No 4S, pp 66-88.

Erratum. On page 77 of this paper, some of the author names in Figures 4 and 5 were transposed. Figures 4 and 5 below are corrected. The authors regret any inconvenience this may have caused.

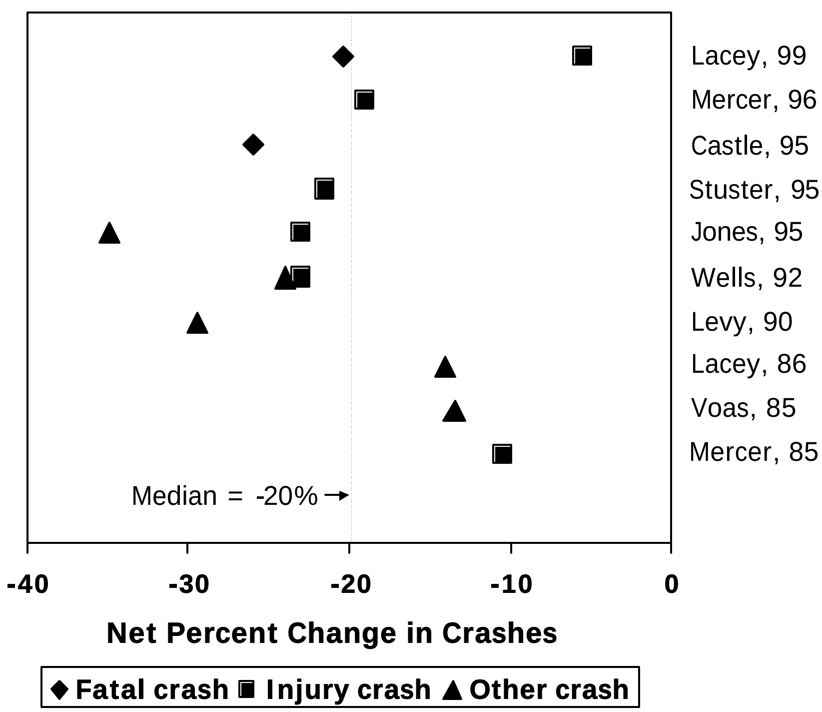

Figure 4. Percent change in crashes likely to involve alcohol after implementing selective breath testing checkpoint programs

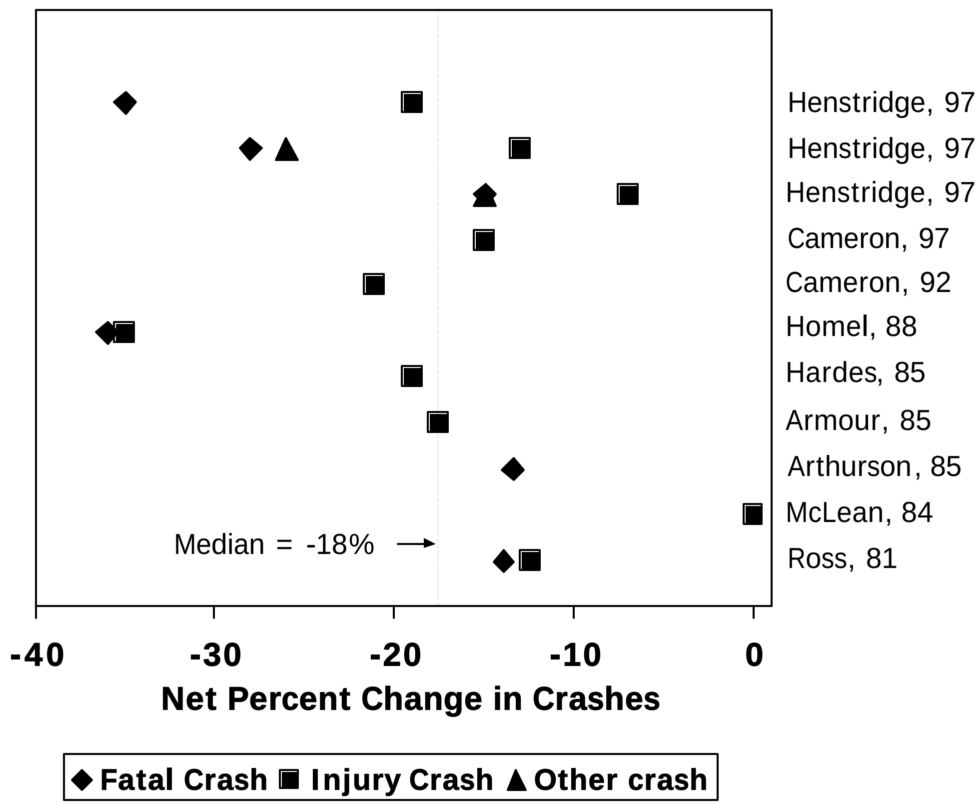

Figure 5. Percent change in crashes likely to involve alcohol after implementing random breath testing checkpoint programs. (Listed studies for which the author name and year are identical come from a single paper.)