

Effectiveness of Brief Interventions After Alcohol-Related Vehicular Injury: A Randomized Controlled Trial

Marilyn S. Sommers, PhD, RN, FAAN, Janice M. Dyehouse, PhD, RN, Steven R. Howe, PhD, Michael Fleming, MD, MPH, Jamison D. Fargo, PhD, and John C. Schafer, PhD

Background: Because 40% of motor vehicle fatalities in the United States are alcohol-related, interventions delivered by trauma clinicians targeted to reduce drinking are of particular importance to public health. The objective of this study was to test the effectiveness of hospital-based brief intervention strategies to reduce alcohol consumption and other health-related outcomes in the year after an alcohol-related vehicular injury. Brief interventions are clinically based strategies including assessment and direct feedback about drinking alcohol, goal setting, behavioral modification techniques, and the use of a self-help manual.

Methods: The study was a randomized controlled trial of two types of brief intervention with a 12-month follow-up.

Participants with alcohol-related vehicular injury who were admitted to Level I trauma centers were eligible for enrollment. Enrolled participants were randomized to a control, simple advice, or brief counseling condition. Primary outcome variables were alcohol consumption (standard drinks/month, binges/month), adverse driving events (driving citations, traffic crashes), and changes in health status (hospital and emergency department admissions).

Results: The study enrolled 187 participants at baseline and retained 100 across 12 months. Participants had a significant decrease in alcohol consumption and traffic citations at 12 months as compared with baseline. Mean standard drinks/month declined from 56.80 (SD 63.89) at baseline to 32.10 (SD 53.20) at 12 months. Mean binges/

month declined from 5.79 (SD 6.98) at baseline to 3.21 (SD 6.17) at 12 months. There were no differences in alcohol consumption, adverse driving events, or health status by condition.

Conclusions: Whether the reductions in alcohol consumption and traffic citations were a result of the crash, hospitalization for injury, screening for alcohol use, or combination of these factors is difficult to determine. Further work is needed to understand the mechanisms involved in reductions of health-related outcomes and the role of brief intervention in this population.

Key Words: Brief intervention, Screening and brief intervention, Alcohol, Alcohol-related injury, Motor vehicle crashes, Drinking and driving.

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During 2003, 7,013 people in the United States died in alcohol-related motor vehicle crashes (ARMVC), representing 40% of all traffic-related deaths.¹ Healthy People 2010 set goals to reduce deaths caused by injury and identified motor vehicle crashes (MVCs) as the most common cause of serious injury: “Motor vehicle crashes are often predictable and preventable. Increased use of seat belts and reductions in driving while impaired are two of the most

effective means to reduce the risk of death and serious injury of occupants in motor vehicle crashes.”²

The acute posttraumatic period represents a window of opportunity, or teachable moment, to encourage a person to change risky behaviors such as problem drinking and driving while impaired by alcohol.^{3–5} Brief intervention is a time-limited, patient-centered strategy focused on changing behaviors.^{6,7} The duration varies from a few minutes to an hour, and the intervention generally consists of four or fewer sessions. Brief intervention has shown promise in a variety of settings to reduce problem drinking in nondependent drinkers.^{8–9} The strength of brief intervention is that it is a clinical strategy appropriate for use by general practitioners such as trauma nurses and physicians. Little is known, however, about the effectiveness of brief intervention in a population composed entirely of patients seriously injured in MVCs.

This article presents the results of a randomized controlled trial (RCT) of a brief intervention that was delivered after MVCs; injured drivers met the National Highway Traffic Safety Administration (NHTSA) definition of “alcohol-related” by having a blood alcohol concentration (BAC) of at least 10 mg/dL (0.01 g/dL)¹ upon hospital admission. The trial was initiated in response to a funding request by the Centers for Disease Control and Prevention (CDC) to test interventions targeted to reduce traffic-related injuries in young adults. The purpose of this study was to determine whether,

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From the School of Nursing (M.S.S.), University of Pennsylvania, Philadelphia, Pennsylvania; College of Nursing (J.M.D., J.C.S.) and the Department of Psychology (S.R.H.), University of Cincinnati, Cincinnati, Ohio; the Department of Family Medicine (M.F.), School of Medicine, University of Wisconsin-Madison, Madison, Wisconsin; Department of Psychology (J.D.F.), Utah State University, Logan, Utah.

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Preliminary results of this study have been presented at annual conferences for the Association for the Advancement of Automotive Medicine, Research Society on Alcoholism, and Midwest Nursing Research Society. Final results are presented for the first time in this article.

Address for reprints: Marilyn S. Sommers, PhD, RN, FAAN, School of Nursing, University of Pennsylvania, 420 Guardian Drive, Room 345, Philadelphia, Pennsylvania 19104; email: ssommer@nursing.upenn.edu.

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12 months after serious injury resulting from an MVC, brief intervention strategies were effective in reducing alcohol consumption and adverse events in non-alcohol dependent, harmful drinking¹⁰ hospitalized young adults. The experimental design included a control group (C) and two experimental groups: Simple Advice (SA) and Brief Counseling (BC). The C group received a 20-minute health interview but no intervention. The SA group received the same health interview and an additional 5 minutes of advice about the importance of sensible drinking or abstinence. The BC group received the health interview, 5 minutes of advice, and an additional 15 to 20 minutes of patient-centered counseling on personal problem-solving strategies. All participants were interviewed by telephone at 3, 6, and 12 months after study entry. The study hypotheses were:

- (1) A significantly greater decrease in alcohol consumption will occur for participants in both the BC and SA conditions as compared with the C condition and in the BC condition as compared with the SA condition during the 12 months after an MVC.
- (2) A significantly greater decrease in adverse events (adverse driving events, health status changes) in the 12 months after the MVC as compared with the 12 months before the MVC will occur for participants in both the BC and SA conditions as compared with the C condition and in the BC condition as compared with the SA condition.

PATIENTS AND METHODS

Drivers and passengers assigned a Diagnosis-Related Group (DRG) and cost code indicating a traumatic injury from an MVC requiring hospital admission to either of two Level I Trauma Centers in southwestern Ohio were eligible for study enrollment. Adults between 18 and 45 years of age who were hospitalized with an injury after an MVC were the target population. Other inclusion criteria included: hospital admission within 24 hours of the injury; BAC ≥ 10 mg/dL; English-speaking; intact cognition as judged by nurse clinicians upon physical assessment and chart review; and potential for discharge from hospital to home within 4 weeks after entry into the study so that drinking patterns could be assessed during injury recovery. Given our interest in ARMVC, only alcohol-positive participants were enrolled. The decision to enroll participants with relatively low BAC (10 to 80 mg/dL) as well as those who were legally intoxicated was made for two reasons. First, the NHTSA definition of alcohol-related crashes includes all those crashes involving a driver or non-occupant who had a BAC at or above 10 mg/dL. Secondly, even if participants were not legally intoxicated, the fact that they had been involved in an MVC indicated that they were experiencing the negative consequences of drinking, and thus a brief intervention was warranted for harmful drinking (a pattern of alcohol use that has resulted in health-related or other problems).¹⁰

Participants were excluded from the study if they had attended an alcohol treatment program in the past year, evi-

denced signs and symptoms of alcohol withdrawal, received advice from their health care provider in the past three months to reduce their alcohol use, drank more than 150 g of alcohol (12 standard drinks) per day,⁶ or had a score of 2 or higher on any or all of the three alcohol-dependence items on the Alcohol Use Disorders Identification Test (AUDIT).¹⁰⁻¹¹ Participants who had the possibility of dependence based on an initial assessment were referred for further evaluation; the decision was made to exclude patients who might be alcohol dependent for two reasons: (1) brief intervention has been primarily used to manage harmful, non-dependent drinkers rather than dependent drinkers^{6,7}; (2) dependent drinkers require a more intensive intervention than was provided through brief intervention, and the intervention was therefore not deemed ethically appropriate for dependent drinkers. The findings by Gentilello et al. that prior alcohol treatment indicating chronic alcohol problems was a negative predictor of brief intervention response lent further support to the exclusion of those with possible alcohol dependence.⁹

Power analysis at the time of study initiation indicated that a sample of 40 participants per group (N = 120) would be necessary to detect a moderate effect for alcohol consumption: (1) between treatment groups (power = 84%); (2) within treatment groups over time (power = 99%); and (3) for the interaction between treatment group and time (power = 99%).

Intervention Protocol

The protocol was approved by the institutional review board (IRB) of the affiliated university. Eligible participants were identified through a daily review of the emergency department (ED) or trauma service admitting logs. When the participant was alcohol free, specially trained nurse clinicians employed by the study investigators obtained informed consent and explained the concept of a "standard drink" (one standard drink contains approximately 14 g of alcohol, such as 12 ounces of beer, 5 ounces of wine, or 1.5 ounces of spirits/hard liquor).¹² Participants were told that the interview contained questions about their general health as well as alcohol and substance use. Participants were randomly assigned to the Control (C), Simple Advice (SA), or Brief Counseling (BC) condition. The nurse clinician used an interview protocol adapted from the one used in the Trial for Early Alcohol Treatment (TrEAT)⁶ to complete baseline screening that lasted approximately 20 minutes. The protocol contained embedded questions on preinjury alcohol consumption, the health-related effects of drinking, and a health history. The interview and interventions (described below) were completed as early as possible during the hospital stay depending on the patient's condition, and were usually delivered during the week after the injury. The brief intervention protocols for both the SA and BC conditions were based on the World Health Organization's model for the initial brief intervention work.¹³ The BC condition had added components from the TrEAT protocol⁶ and Miller's FRAMES Feed-

back, Responsibility, Advice to Change, Menu of Alternative Choices, Empathy, and Self-Efficacy (FRAMES) model.¹⁴

Participants in the SA condition received 5 minutes of feedback that they were drinking in ways that put their health at risk and advice about current health behaviors and adverse effects of alcohol. The most important part of the simple advice intervention was to discuss “low risk” drinking¹² and to review the menu of alternative choices: abstinence or cutting back to low risk drinking. One month after discharge, participants in the SA condition received by telephone a booster (repeat) session of the simple advice intervention.

Participants in the BC condition received two 20-minute patient-centered interventions, one at the hospital and a second telephone intervention 1 month after discharge. In the hospital-based intervention, the topic of alcohol consumption and its possible consequences was raised in a nonconfrontational manner using the FRAMES¹⁴ and reflective listening techniques. Recommended drinking limits¹² were discussed and the interviewer provided feedback about the participant’s drinking behaviors to make the connection between drinking and the injury. The participant and interviewer discussed the pros and cons of changing drinking behaviors and how the participant’s life might be different with a change in drinking. A realistic drinking agreement was negotiated based on the discussion. One month after discharge, a telephone booster session occurred. The participants reviewed their drinking agreement and discussed with the interviewer whether or not they were able to maintain the agreement goals. The outcome of the drinking agreement was used as a framework to explore risky situations in which the participant was most tempted to drink beyond the recommended drinking limits¹² and to develop strategies to cope with risky situations. The same non-confrontational approach used in the initial intervention was employed using reflective listening to support a discussion of the meaning and consequences of risky situations. The participant and interviewer then re-negotiated and agreed to realistic and constructive drinking goals, no matter how small the change in drinking and based on what was learned in the past month. The booster session was intended to emphasize the intervention and to attenuate relapse.^{4,15,16} A printed self-help manual was used to guide both the SA and BC interventions. Interviews were repeated at 3, 6, and 12 months by telephone for all participants; interviewers were blinded to participant condition during the follow-up interviews.

Nurse clinicians were hired by the study team and were not part of the hospital staff caring for the participants. All nurse clinicians delivering the interventions were trained in brief intervention procedures for at least 24 hours at study initiation by a social psychologist with advanced training from William Miller¹⁴ in brief intervention. The nurse clinicians were videotaped delivering the intervention to proxy participants every 6 months throughout the study to maintain the integrity of the intervention. The videotapes were reviewed and rated by experts who were not study team members. When indicated, retraining sessions were provided for those delivering the intervention.

Research Procedures

Nurse clinicians screened 4,618 individuals after an MVC who were admitted to two Level I trauma centers. After instituting the aforementioned inclusion/exclusion criteria, 481 screened individuals were included in the present study. The 4,137 excluded individuals were excluded for one or more of the following reasons: too old ($n = 1,227$), too young ($n = 446$), or age unknown ($n = 24$); released from the hospital in less than 24 hours ($n = 440$); or BAC at the time of the MVC was less than 10 mg/dL ($n = 3,426$) or BAC was unknown ($n = 556$). Of those who met both age and BAC criteria ($n = 583$), 95 were excluded for probable alcohol dependence. Of the 481 individuals who met the criteria for enrollment into the present study, 294 refused to participate (61%), resulting in a final sample of 187 individuals who were randomized to one of the three treatment conditions. See Figure 1 for the flow chart indicating progress of the randomized trial in terms of stage and sample size.

Outcome Variables

The primary outcome variables of interest were changes in alcohol consumption (expressed in terms of standard drinks per month, and binges per month) as measured with a Timeline Followback procedure.¹⁷ Binges were defined as the number of drinking days on which a female participant consumed ≥ 4 standard drinks or a male participant consumed ≥ 5 standard drinks. Other variables included adverse driving events (traffic crashes, driving citations) as measured with police crash records and driving records from state motor vehicle departments and health status changes. Driving event variables included a driving-related suspension, citations for driving under the influence (DUI) of alcohol, or any citation (including a DUI) anytime during the period 12 months before or after the MVC but not including it. Health status variables included responses to questions on the interview protocol pertaining to number and length of hospital stays for medical or psychiatric problems, number of ED visits for illness or injury, and number of times physical activity was limited because of illness or injury within the previous 12-month period.

Data Analyses

Statistical analyses were performed using SAS v9.0. Race/ethnicity was dichotomized as white or black/African-American. Independent-samples t tests (for continuous variables) and χ^2 tests (for categorical variables) were used to compare the enrolled and excluded samples on the following demographic and alcohol-related characteristics as measured at baseline: sex, race/ethnicity, age, BAC, and injury severity score (ISS).¹⁸ Analysis of variance (ANOVA) and χ^2 tests were conducted to determine whether the distributions of these same variables differed significantly among the treatment conditions (BC, SA, and C) at: 1) the beginning of the study and 2) the 12-month follow-up. Similar analyses were

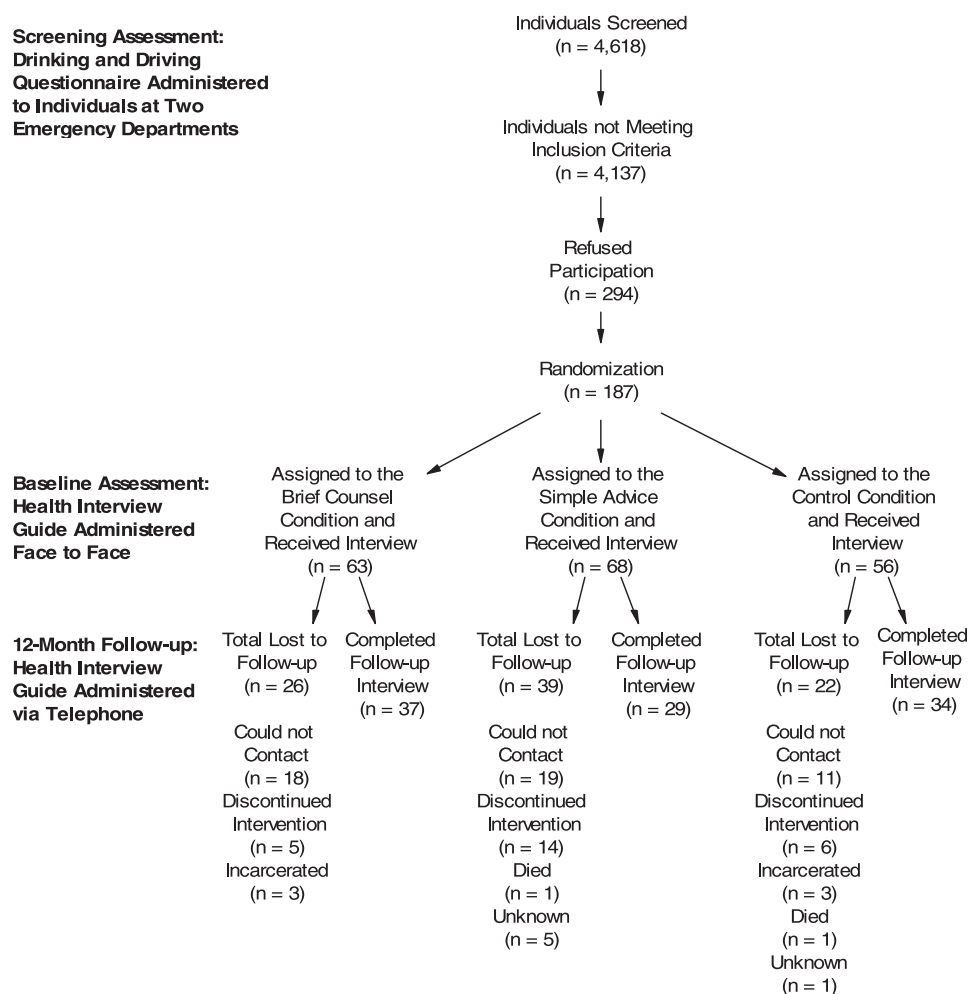


Fig. 1. Flow chart detailing sample size at each stage of the randomized trial.

performed to compare the characteristics of participants who remained at the end of the study to those who were lost to follow-up. Institutional Review Board regulations prohibited us from conducting any analysis of data from individuals who refused participation.

Generalized linear mixed modeling (GLMM) was used to test the hypothesis that, during the 12-month period, a significantly greater decrease in alcohol consumption would occur for participants in the BC and SA conditions as compared with the C condition and the BC condition as compared with the SA condition. Time was partitioned into two segments in the GLMMs after it was observed that alcohol consumption sharply declined between baseline and the 3 month follow-up (time segment 1), but then gradually increased between 3 and 12 months (time segment 2) (Figs. 2 and 3).¹⁹ Covariates initially tested in both GLMMs included participant's baseline age, sex, race/ethnicity, ISS, BAC, position in the vehicle (driver versus passenger), and time (time segments 1 and 2). Baseline measurements of alcohol consumption were not included as a covariate in the GLMMs as it was of interest to model change in drinking behavior from baseline, rather than to control for it statistically. Covariates

were iteratively dropped from the models if the likelihood ratio test (LRT) statistic was nonsignificant after their removal and if they did not form part of a statistically signifi-

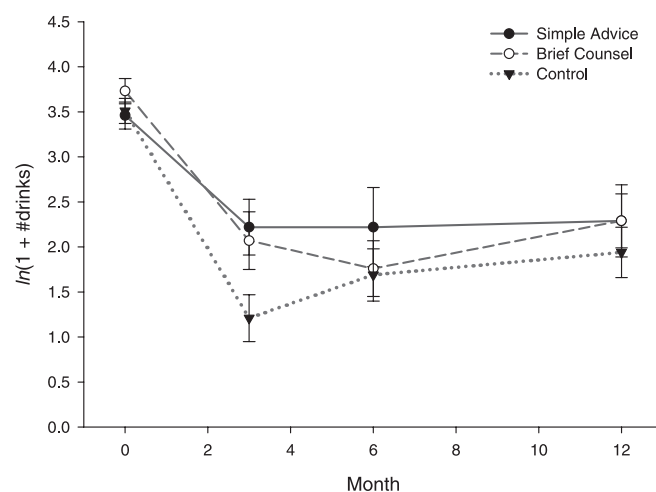


Fig. 2. Average self-reported number of standard drinks ($\ln(1+x)$) consumed during the month before baseline, 3-, 6-, and 12-month assessments by treatment condition. Error bars represent ± 1 SE.

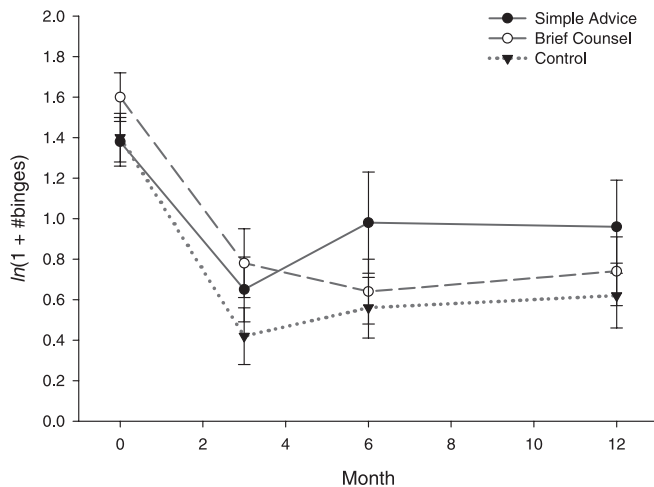


Fig. 3. Average self-reported number of binges ($\ln[1+x]$) during the month before baseline, 3-, 6-, and 12-month assessments by treatment condition. Error bars represent ± 1 SE.

cant interaction term ($\alpha = 0.05$ was used for all statistical procedures except where specifically noted).

McNemar tests for correlated proportions were conducted to evaluate the hypothesis that significantly fewer adverse events (i.e., driving or health related) would occur in the 12 months after the MVC versus the 12 months before the MVC for participants in the BC and SA conditions as compared with the C condition and the BC condition as compared with the SA condition.²⁰ Analyses were conducted for the entire sample as well as stratified by treatment condition. The driving citation or suspension received from the MVC that led to enrollment in the present study was not included in the analyses. Health status was ascertained from participant responses on the interview protocol, which were dichotomized as 0 or ≥ 1 (the latter denoting event occurrence) because of the infrequency of events.

RESULTS

Table 1 summarizes the demographic and crash-related characteristics of the enrolled sample, stratified by treatment

condition and time. The modal time of day for the MVC was between 2:30 AM and 3:30 AM (15%, $n = 28$) and 59% ($n = 110$) of the crashes occurred between 10:30 PM and 4:30 AM.

Comparison of Enrolled and Excluded Samples

Participant race/ethnic identity and age did not vary significantly between the enrolled and excluded samples. However, significantly more screened men (5%) than women (2%) were ultimately enrolled in the study ($X^2 [1, N = 4,562] = 19.06, p < 0.0001$). Additionally, after individuals with a $BAC \leq 10$ were removed from the excluded sample, BAC was significantly higher among excluded ($M = 177.95, SD = 85.66$) than enrolled participants ($M = 165.18, SD = 65.65$), $t(321.19) = 2.30, p = 0.02$, Cohen's $d = 0.15$.

Distribution of Enrolled Sample Among Treatment Conditions: Baseline

Of the 187 participants enrolled, 63 were randomly assigned to the BC, 68 to the SA, and 56 to the C condition. Participant sex, race/ethnic identity, age, BAC, or ISS did not vary significantly across treatment conditions. However, at the time of the MVC, significantly fewer participants assigned to the SA condition were drivers (71%), as compared with the BC (85%) or C (81%) conditions, $X^2 (2, n = 177) = 6.41, p = 0.04$.

Distribution of Enrolled Sample Among Treatment Conditions: 12-Month Follow-up

Forty-seven percent ($n = 87$) of enrolled participants were lost to follow-up during the course of the study (see Fig. 1 for reasons for participant attrition). The attrition rate did not vary significantly among treatment conditions. Additionally, participant sex, age, driver/passenger status, BAC, and ISS did not vary significantly between the retained or lost to follow-up samples. However, there was a significant difference between the 44% of white and 67% of black/African American participants who were lost to follow-up, $\chi^2 (1, n = 187) = 3.86, p = 0.05$. At 12-month follow-up, there were no

Table 1 Summary of Sample Characteristics at Baseline ($n = 187$) and 12-Month Follow-up ($n = 100$) by Treatment Condition

Treatment Group	Brief Counseling		Simple Advice		Control		Total	
	Baseline	12 months	Baseline	12 Months	Baseline	12 Months	Baseline	12 Months
N (%)	63 (34)	37 (37)	68 (36)	29 (29)	56 (30)	34 (34)	187	100
Age (years)	29.46 (8.11)	29.35 (7.98)	28.82 (8.09)	27.97 (8.19)	28.87 (7.80)	28.70 (8.14)	29.03 (7.96)	28.71 (8.04)
BAC	166.02 (70.23)	179.21 (73.49)	165.34 (66.25)	154.83 (71.28)	164.29 (61.80)	152.62 (65.37)	165.18 (65.65)	162.13 (70.24)
ISS	10.02 (8.01)	9.48 (6.39)	10.45 (6.87)	9.66 (6.63)	10.97 (8.72)	11.27 (9.41)	10.50 (7.84)	10.20 (7.69)
Sex	77	79	78	79	76	70	77	76
(% male)								
Race	88	91	87	90	92	97	89	93
(% white)								
Position	88	94	71	64	85	71	77	75
(% driver)								

All data were collected at baseline, but the 12-month results are based on baseline data for only those participants who were followed for 12 months. Unless noted, values represent means (SD).

Table 2 Standard Drinks and Binges from Baseline to 12-Month Follow-up

Data Collection Time Outcome	Baseline (n = 187)		3-Month (n = 102)		6-Month (n = 87)		12-Month (n = 100)	
	Drinks	Binges	Drinks	Binges	Drinks	Binges	Drinks	Binges
Mean	56.80	5.79	23.58	2.13	26.91	2.66	32.10	3.21
SD	63.89	6.98	43.06	4.36	62.82	5.45	53.20	6.17
Median	36.00	3.00	4.00	0.00	6.00	0.00	12.00	0.00

Outcomes refer to standard drinks or binges in the month prior to the specified data collection period.

significant differences among the treatment conditions in terms of any participant variables.

Alcohol Consumption

The number of standard drinks consumed in the previous month was transformed ($\ln[1+x]$) to normalize its distribution for the GLMM. However, descriptive results are presented in untransformed terms (Table 2). Results of the GLMM indicated that the following covariates were related to the outcome variable ($\alpha = 0.20$): treatment condition, time segment 1, time segment 2, sex, race/ethnic identity, and driver/passenger status. Additionally, LRT tests indicated that three two-way interaction terms significantly improved the fit of the model to the data: (1) treatment condition $\times$ driver/passenger status, (2) time segment 1 $\times$ driver/passenger status, and (3) time segment 2 $\times$ race/ethnic identity (Table 3). The fit

of the final model combining all variables, including the three interaction terms was significantly better than both the null (LRT = 239.1, $p < 0.0001$) and the main effects only model (LRT = 19.8, $p = 0.0002$). Table 3 presents parameter estimates and tests of significance for the final GLMM. Standardized residuals were normally distributed ($<1\%$ were beyond ± 2 SD) and the correlation between the observed and fitted values of ($\ln[1+x]$) standard drinks was 0.75, $p < 0.0001$ ($R^2 = 0.56$).

The significance of the predictors may be interpreted as follows. After a significant decline in standard drink consumption subsequent to the MVC (main effect for time segment: (1) a steady, but nonsignificant increase in consumption began at the 3-month follow-up period and continued until the end of the study (main effect for time segment; but (2) never returned to baseline levels (see Fig. 2). The significant main effect for sex suggested that men consumed significantly more standard drinks ($M = 41.89$, $SD = 46.85$) than women ($M = 21.63$, $SD = 19.41$), collapsing across all time points. The significant interaction between treatment condition and driver/passenger status revealed that drivers consumed similar levels of standard drinks across treatment conditions (BC: $M = 42.45$, $SD = 44.04$; SA: $M = 40.75$, $SD = 48.30$; C: $M = 31.27$, $SD = 35.32$), whereas passenger consumption varied significantly across treatment conditions (BC: $M = 17.49$ drinks, $SD = 18.07$; SA: $M = 55.99$, $SD = 62.01$; C: $M = 75.47$, $SD = 90.86$). The interaction between time segment 1 and driver/passenger status indicated a significantly greater decrease in standard drink consumption from baseline to the 3 month follow-up among drivers (M change = -46.35 drinks) as compared with passengers (M change = -23.76 drinks). The significant interaction between time segment 2 and race/ethnic identity reflected the finding that whereas consumption of standard drinks between the 3 and 6 month follow-up periods was similar for white ($M = 25.00$, $Mdn = 4.50$) and black/African American ($M = 26.45$, $Mdn = 5.50$) participants, black/African American participants increased their drinking consumption substantially ($M = 79.40$, $Mdn = 54$) over white ($M = 28.50$, $Mdn = 7$) participants by the 12-month follow-up. However, the accuracy and generalizability of the results of these interaction terms may be limited by the small number of black/African American and passenger participants enrolled in the study (see Table 1).

The number of binges in the previous month was also transformed to normalize the distribution of this variable

Table 3 Results of the Final GLMM for Predictors of Standard Drink Consumption: 1-Month Prior through 12-Months Following the MVC ($N = 187$)

Variable	B	SE	t	p Value
Intercept	0.94	0.27	3.51	0.0006
Treatment condition				
SA vs. C	0.13	0.24	0.54	0.5897
BC vs. C	0.56	0.23	2.38	0.0181
BC vs. SA*	1.22	0.56	2.19	0.0291
Time segment				
1. First 3 months	-2.02	0.16	-12.88	<0.0001
2. Last 9 months	0.14	0.10	1.44	0.1502
Male (1)/female (0)	0.44	0.22	1.98	0.0482
White (1)/black (0)	-0.18	0.31	-0.57	0.5698
Driver (1)/passenger (0)	1.46	0.45	3.22	0.0015
Treatment condition $\times$ driver/passenger				
SA vs. C	-0.30	0.53	-0.55	0.5802
BC vs. C	-1.95	0.67	-2.91	0.0039
BC vs. SA*	1.65	0.61	2.70	0.0073
Time segment 1 $\times$ driver/passenger	0.67	0.31	2.15	0.0321
Time segment 2 $\times$ white/black	0.65	0.29	2.29	0.0230

* The model parameters were first estimated by using the first two treatment condition dummy variables listed. Subsequently, in order to test the third hypothesis about group differences, an additional test was made of the third treatment condition dummy variable. It should be noted that the treatment condition main effects are adjusted for other variables in the model and are only statistically significant in the presence of the treatment condition $\times$ driver/passenger status interaction terms.

Table 4 Results of the Final GLMM for Predictors of Binges: 1-Month Prior through 12-Months Following the MVC ($N = 187$)

Variable	B	SE	t	p Value
Intercept	0.98	0.18	5.43	<0.0001
Treatment condition				
SA vs. C	0.27	0.15	1.87	0.0619
BC vs. C	0.04	0.30	0.13	0.8927
BC vs. SA*	-0.68	0.35	-1.96	0.0514
Time segment				
1. First 3 months	-0.88	0.10	-9.16	<0.0001
2. Last 9 months	0.05	0.06	0.86	0.3908
White (1)/black (0)	-0.34	0.19	-1.75	0.0807
Driver (1)/passenger (0)	-0.51	0.20	-2.54	0.0116
Treatment condition $\times$ driver/passenger				
SA vs. C	-0.99	0.42	-2.38	0.0179
BC vs. C	-0.08	0.33	-0.25	0.8043
BC vs. SA*	0.91	0.38	2.39	0.0173
Time Segment 2 $\times$ white/black	0.42	0.18	2.39	0.0173

* The model parameters were first estimated by using the first two treatment condition dummy variables listed. Subsequently, in order to test the third hypothesis about group differences, an additional test was made of the third treatment condition dummy variable. It should be noted that the treatment condition main effects are adjusted for other variables in the model and are only statistically significant in the presence of the treatment condition $\times$ driver/passenger status interaction terms.

($\ln[1+x]$). Again, descriptive results are given in untransformed form. The GLMM indicated that the following covariates had a significant univariate relationship with binge frequency ($\alpha = 0.20$): treatment condition, time segment 1, time segment 2, race/ethnic identity, and driver/passenger status. Results of the *LRT* suggested that the following interaction terms improved the fit of the model: (1) time segment 2 $\times$ race/ethnic identity and (2) treatment condition $\times$ driver/passenger status. The final GLMM for binges with all variables combined, including the two interaction terms, demonstrated a better fit than both the null ($LRT = 159.5$, $p < 0.0001$) and the main effects only model ($LRT = 20.2$, $p < 0.001$). See Table 4 for parameter estimates and tests of significance for the final GLMM. Standardized residuals were normally distributed ($<1\%$ beyond ± 2 SD) and the correlation between the observed and fitted values of ($\ln[1+x]$) binges was 0.74, $p < 0.0001$ ($R^2 = 0.55$).

Similar to the pattern for standard drinks, the number of binges dropped precipitously between baseline and the follow-up assessment at 3 months, as indicated by the significant main effect for time segment 1 (see Fig. 3). Again, a nonsignificant main effect for time segment 2 indicated that there was no significant change in binge drinking between the 3 and 12 month assessment for the sample as a whole. However, the significant interaction between segment 2 and race/ethnic identity reflected the finding that between 3 and 12 months, binge drinking was fairly constant among white/

Caucasian participants (3 month: $M = 0.82$, $SD = 1.46$; 6 month: $M = 1.05$, $SD = 1.66$; 12 month: $M = 1.01$, $SD = 1.69$), but after an initial decrease between 3 and 6 months (3 month: $M = 0.93$, $SD = 1.61$; 6 month: $M = 0.55$, $SD = 1.46$), levels of bingeing ultimately exceeded baseline levels ($M = 1.97$, $SD = 1.95$) at 12 months ($M = 4.00$, $SD = 3.76$) among black/African American participants. The significant interaction between treatment condition and driver/passenger status may be interpreted in the same way as in the analysis for standard drinks. For instance, although similar levels of bingeing were reported for drivers across treatment conditions (BC: $M = 1.83$, $SD = 1.72$; SA: $M = 1.51$, $SD = 1.86$; C: $M = 1.16$, $SD = 1.69$), passenger bingeing varied by treatment condition (BC: $M = 0.79$, $SD = 1.05$; SA: $M = 2.90$, $SD = 1.97$; C: $M = 2.98$, $SD = 2.13$). Again, the generalizability of the results of these interaction terms may be limited by the small number of participants identified as black/African American or passengers.

Driving Events and Health Status Changes

Driving records 1-year before and 1-year after the MVC were available for 96% ($n = 179$) of the enrolled sample. Before the crash, 19% of the sample ($n = 34$) received ≥ 1 motor vehicle suspension, 35% ($n = 63$) received ≥ 1 motor vehicle citation, and 6% ($n = 10$) were cited for ≥ 1 DUI. After the MVC, 26% ($n = 47$) of the sample ($n = 179$) received ≥ 1 motor vehicle suspension, 24% ($n = 43$) received ≥ 1 motor vehicle citation, and 8% ($n = 15$) were cited for ≥ 1 DUI. Only the percent of ≥ 1 motor vehicle citations significantly decreased from pre- to post-MVC (9% decrease), exact $Q_M(1, n = 179) = 6.06$, $p = 0.019$. No significant changes were found for any of the driving event variables when the analysis was stratified by treatment condition. Although 124 enrolled participants (from the sample of 179) were eligible to receive a DUI citation for the focal MVC according to the parameters of the DUI laws existing at the time of data collection (driver had a BAC $> .09$ at the time of the MVC), only 44 (35%) were actually issued for the focal event.

The only significant change observed in injury or health status during the course of the study was the frequency of illness limiting physical activity during ≥ 1 days during the previous month; decreasing from 37% of participants at baseline to 20% at 12-month follow-up (exact $Q_M(1, n = 89) = 7.26$, $p = 0.011$). Additionally, when stratified by treatment condition, only the BC group demonstrated significant changes over time in the proportion reporting ≥ 1 day of limited physical activity because of illness, exact $Q_M(1, n = 33) = 9.31$, $p = 0.003$. For instance, 34% fewer reported that an injury had limited physical activity ≥ 1 days at 12-month follow-up than at baseline (a decrease from 46% to 12%).

DISCUSSION

All participants regardless of condition demonstrated a significant and dramatic decrease in drinking during the 12

months after the MVC. If the marked reduction in drinking we observed is to be attributed to something other than the combined effects of the intervention and the natural response to trauma, then it is necessary to assume that participants became more likely to underreport during the course of the study. This assumption strikes us as unlikely, given that the legal and financial pressures to underreport were probably greatest in the immediate aftermath of the MVC. The ancillary evidence of a decrease in the number of multiple citations in the year after the crash supports our contention that the decline in reported drinking was probably real.

But if we are correct that drinking declines, a second objection might be that we have misstated the true intervention. That is, perhaps neither the brief intervention nor the natural effect of trauma that we have postulated was responsible for the drop in drinking. Participants across all three groups might have curtailed their drinking because it was being monitored. This effect is a conundrum for not just our study, but many studies of the long-term effects of clinical interventions. However, we would point to a piece of ancillary evidence, which was that the decline in standard drinks was steeper for drivers than passengers. We think this suggests that the clinical intervention was effective in helping drivers connect the health consequences to their drinking behaviors.

It also appears unlikely that these results were a result of a bias in the attrition rates such that people who began drinking heavily were more likely to drop out of the study. Instead, it appears most plausible that serious trauma associated with alcohol use has a previously undocumented effect that promotes radical changes in drinking. There are three important qualifications. First, likely dependent drinkers were screened out of the sample, so enrolled participants did not have to struggle with an addiction. Second, bear in mind that all of these participants were *so* seriously injured that they had to be hospitalized. The medical treatment needed to manage the injury may have served as a deterrent to drinking as participants were hospitalized or involved with rehabilitation for up to 4 weeks after the MVC. Finally, the in-depth screening that all participants underwent regardless of condition may have boosted the strength of the interaction with the interviewer so that it actually served as an intervention. The intensity of the screening might help to explain why there was decreased drinking across all groups, largely eliminating the possibility that there might have been significant differences by condition.

Although the number of standard drinks consumed by participants began to increase in the third month after the MVC, the number of binges per month remained constant. The decline of binge drinking from baseline may partially explain the decrease in motor vehicle citations as well, because heavy, episodic drinking is associated with MVCs, risky driving practices, and injury.^{21–23} Taken together, these findings point to a “natural effect” of trauma on non-dependent drinkers whereby people are able to relate their actions (e.g., the responsibility of having been

the driver) to an injury outcome. This effect is worth noting. The focus of intervention in this population, therefore, should be to equip people to maintain healthier behaviors through clinical follow-up that stresses the avoidance of binge drinking.

The low rate of legal prosecution for DUI is an important finding. Although 124 enrolled participants were eligible to receive a DUI citation for the focal MVC, only 44 (35%) citations were actually issued. Biffi and colleagues,²⁴ in a review of trauma registry data, found of 137 intoxicated drivers admitted to a trauma center for injury, only 19% were charged with an offense related to the MVC. Of 91 participants in the Biffi study not charged for the index event, 31 had a prior or subsequent charge. Our findings reinforce the concern that the infrequency of prosecution for DUI despite personal injury is a significant social concern and calls for revision of DUI legislation.

Several limitations exist with the study. We experienced a 61% refusal rate to participate in the study. Because of institutional review board constraints, we were not able to ask eligible participants who refused why they were unwilling to participate, nor were we permitted to report demographic data on those participants who refused. In addition to concerns over legal jeopardy, potential participants were often in pain, uncomfortable, or tired because of their injury, which may have limited their willingness to participate. Our findings might have differed had those who refused participated, but it is difficult to speculate on the direction of any bias, or even if there was bias resulting from the refusal rate.

Only patients who had a positive BAC were eligible for study enrollment. Although up to 40% of MVC patients who abuse or are dependent on alcohol have a negative BAC at the time of trauma center admission,^{25,26} these individuals were beyond the scope of the study.

Participants who were excluded had a significantly higher BAC than those who were enrolled ($M = 177.95$ and $M = 165.18$ respectively), but this finding is most likely explained by the fact that excluded participants (as compared with refusers, for whom we had no data) included participants who were most severely injured or who were alcohol dependent. The sample size was also a limitation of the study. Although the power analysis indicated that a sample of 120 participants was needed, the investigators were only able to enroll and retain 100 participants throughout the study. Even if all 120 participants were enrolled, the sample size may not have been sufficient to detect significant differences for the size of effects we observed. During protocol development, we may have failed to appreciate the apparent natural effect of trauma on subsequent drinking. In addition, although the 53% retention rate was similar to that of the Gentilello et al.⁹ study of brief intervention in a Level I Trauma Center, the level of attrition may have biased study findings in either direction. Of particular note is that whites had an attrition rate of 44%, whereas blacks had a rate of 67%. In future trials more cultural sensitivity is necessary, and investigators should consider matching participants and interveners by race/ethnicity;

in this particular trial, all interveners were white. The reasons for the attrition rate are difficult to assess, but trauma patients are notoriously difficult to follow longitudinally.⁹ In some brief intervention trials,¹³ investigators limited loss to follow up by exclusion criteria such as social/residential instability, but those criteria then lead to the lack of a representative sample of the trauma population.

Measures of alcohol consumption were based on self-reported data. Collateral informants such as other occupants of the vehicle at the time of the MVC were not asked to provide comparison data. Although no single biological measure is available as the “gold standard” to assess the validity of self-reported drinking, investigators have reported that individuals involved in MVCs have self-reports that are significantly and positively related to measured BAC.^{27,28} This previous work supported the validity of self-reported drinking in the vehicular injury population.

CONCLUSIONS

This trial tested the effectiveness of two types of brief intervention to reduce alcohol consumption and adverse events in the year after an alcohol-related vehicular injury. As compared with baseline, all participants decreased their alcohol consumption and had fewer traffic citations regardless of condition. Whether these findings were the result of the hospitalization, the injury, the screening procedures, or all three is unclear. Further work is needed in the vehicular crash population to understand the mechanisms underlying these changes in behavior and to find clinical strategies that capitalize on the opportunity to reduce death and disability from alcohol-related vehicular injury.

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EDITORIAL COMMENT

Injured alcohol-impaired drivers concern clinicians for legal and medical reasons. The alcohol brief intervention trial reported by Sommers and colleagues draws attention to both areas of concern.

The authors documented that 35% of patients who met age and length of stay criteria had a positive blood alcohol concentration (BAC) test. Further, the mean BAC of enrolled subjects was 165 mg/dL, a level well above the current national standard of 80 mg/dL for impaired driving. Of the 179 patients for whom driving records were available, 69% were eligible to receive a driving under the influence citation for the index event. Overall, only 35% were issued a citation. Combined data from this study and sixteen other studies,¹⁻³ indicate that only 22% of legally impaired drivers admitted to trauma centers or emergency departments were issued citations for the crash in which they were injured.

The clinical problem associated with alcohol, injury and driving in the current study is highlighted by data indicating that a BAC-positive status has a sensitivity of 0.52 to 0.66 for detecting patients with an alcohol use problem.¹ Hence, using BAC+ as a screening test, at least half of the patients included in the intervention trial probably had a diagnosable alcohol use problem. Further, one in six of BAC+ patients who were potentially eligible for study were excluded as being possibly alcohol dependent based on AUDIT responses. Finally, the authors point out that about 40% of patients with alcohol use problems will be BAC- at the time of injury. As noted, BAC- patients were not assessed for such problems or included in their clinical trial.

It is important to note that this study also excluded patients over 45 years of age. Approximately 15% or more of trauma victims ≥ 60 years of age have an alcohol use problem.^{4,5} Although only 3.8% of BAC-tested drivers ≥ 65 years of age admitted to Maryland trauma center for fiscal years 1995 to 2001 were BAC+, their mean BAC was 208 mg/dL.⁶ These data clearly indicate that elderly trauma patients, many of whom are drivers, should be considered as potentially at-risk drinkers.

Although the current study and other recent studies^{7,8} indicate that querying about risking drinking, or providing advice or formal intervention for such drinking, are a means clinicians can use to reduce risking drinking in legally impaired drivers, many are left with an ethical dilemma of what to do about the illegal behavior. Should they report them to the police? The results of a survey of clinicians posed with that problem provide an alternative solution that addresses both clinical and driving concerns. Mello and colleagues⁹ found that given a choice, physicians were much more comfortable referring an impaired injured driver to a Department of Motor Vehicle medical advisory board than to the police. That option notifies the driver licensing agency of the problem. That notification should result in a request for a clinical

assessment of a substance use problem and a recommendation for treatment if one is identified.

Carl A. Soderstrom, MD

National Study Center for Trauma and EMS
University of Maryland School of Medicine
Baltimore, MD

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EDITORIAL COMMENT

Sommers and colleagues present a well designed study with intriguing findings. Alcohol-related motor vehicle collision (MVC) patients were randomized to one of three treatment conditions: standard care, simple advice, and brief counseling. All participants received a 20-minute health interview that included questions about alcohol consumption and its consequences. Although many readers would interpret this as a "negative study" in that there were no differences between treatment conditions, the participants in all three arms decreased their drinking and some drinking-related consequences. In particular, they decreased their binge drinking episodes, an important predictor of harm. One natural history study by Dunn and colleagues¹ showed that for the first few months after injury patients decrease their drinking, which was again seen in this population. However in Dunn's study drinking patterns returned to baseline by 4 months. All three groups in the present study did not return to baseline drinking by 12 months, implying some effect of study participation. It is unfortunate that we do not have information on the large

number of patients (61%) who refused study participation to compare them to the enrolled patients to determine whether the control group truly functioned as a control.

Driving events and health-related outcomes are relatively infrequent and hence may not have occurred at high enough rates to detect in the follow-up time frame. Moreover, Gentilello² showed between group differences in outcomes by controlling for injury severity and alcohol screening score. Prior drinking is also a known large predictor of future drinking. If the injury itself has an impact on outcome, perhaps including injury severity and prior drinking would have made a difference between groups. However, a more important question to ask is how different were the three treatments. The control group underwent a 20-minute health interview, with subsequent follow-up interviews throughout the year. One must ask what minimum amount of treatment is necessary to function as a booster intervention (BI). Simple advice as short as 5-minute has been demonstrated effective.³ Perhaps simply asking drinking and alcohol related consequences questions in the context of the health interview after an MVC is sufficient. Inquiries about drinking throughout the year after injury may also have functioned as booster interventions. Another argument for lack of differences between groups could be that the control group, knowing they did not receive a counseling session, sought more treatment after injury. Treatment seeking data would have been helpful in the year after discharge.

It is also important to note that only patients with a positive BAC were enrolled but the cutoff was only 10 mg/dL. A positive BAC per se is not indicative of harmful

drinking, but those participants were enrolled as a result of the MVC. Gentilello's pivotal article also showed that subjects with a positive BAC but a low screening score did not respond to BI. When trying to allocate resources for BI programs in trauma centers, one needs to consider whether patients with a low BAC without a positive screen would need an intervention at all.

The nicely performed study demonstrates that some combination of care provided in the hospital that includes talking about alcohol use, has an impact on consumption. I applaud the authors for completing this study in a difficult population and for publishing it despite no demonstration of differential effect between treatment groups. Works like this keep future meta-analyses from being too limited by publication bias and inform us how to better tailor and study alcohol interventions.

Carol R. Schermer, MD, MPH
Loyola University Medical Center
Maywood, IL

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