Law Hum Behav
DOI 10.1007/s10979-011-9273-5

ORIGINAL ARTICLE

Intoxicated Eyewitnesses: Better than Their Reputation?

Nadja Schreiber Compo · Jacqueline R. Evans · Rolando N. Carol · Daniella Villalba · Lindsay S. Ham · Tracy Garcia · Stefan Rose

Abstract According to law enforcement, many witnesses are intoxicated either at the time of the crime, the interview, or both (Evans et al., Public Policy Law 15(3):194–221, 2009). However, no study to date has examined whether intoxicated witnesses' recall is different from sober witnesses' and whether they are more vulnerable to misinformation using an ecologically valid experimental design. Intoxicated, placebo, and sober witnesses observed a live, staged theft, overheard subsequent misinformation about the theft, and took part in an investigative interview. Participants generally believed they witnessed a real crime and experienced a real interview. Intoxicated witnesses were not different from placebo or sober witnesses in the number of accurate details, inaccurate details, or "don't know" answers reported. All the participants demonstrated a misinformation effect, but there were no differences between intoxication levels: Intoxicated participants were not more susceptible to misinformation than sober or placebo participants. Results are discussed in the light of their theoretical and applied relevance.

Keywords Intoxication · Eyewitness memory · Alcohol expectancy effects

Witnesses and victims often provide central leads in criminal investigations (Fisher, 1995; Fisher & Schreiber, 2007). According to a recent law enforcement survey many are under the influence of alcohol either at the time of the crime, the witness interview, or both (Evans, Schreiber Compo, & Russano, 2009). Further, a recent study investigating perceptions of intoxicated witnesses demonstrated that intoxicated witnesses may be perceived as more cognitively impaired than sober ones by potential jurors (Evans & Schreiber Compo, 2010; see also Benton, Ross, Bradshaw, Thomas, & Bradshaw, 2006). Similarly, a survey of eyewitness experts found that most expert witnesses expected intoxicated witnesses' memory to be impaired (Kassin, Tubb, Hosch, & Memon, 2001). Given these beliefs, surprisingly little is known about if, and how, alcohol intoxication affects witness memory under conditions that are similar to real-world eyewitness contexts (see also Soraci et al., 2007). Filling this void was the objective of this study.

A considerable amount of research has examined the effects of alcohol on memory for simple stimuli such as lists of consonants, words, word fragments, sentences, pictures, prose passages, or general information. Such research shows that alcohol typically impairs memory and cognitive processes (Bisby, Leitz, Morgan, & Curran, 2010; Brown, Brignell, Dhiman, Curran, & Kamboj, 2010; Maylor & Rabbitt, 1993), even at low intoxication levels (Breitmeier, Seeland-Schulze, Hecker, & Schneider, 2007; Clifasefi, Takarangi, & Bergman, 2006). This impairment affects both encoding and retrieval (Hartley & Birnbaum, 1978;

N. Schreiber Compo (✉) · R. N. Carol · D. Villalba · T. Garcia
Department of Psychology, Florida International University, 11200 SW 8th Street, Miami, FL 33199, USA
e-mail: schreibe@fiu.edu

J. R. Evans
Department of Psychology, University of Texas at El Paso, El Paso, TX, USA

L. S. Ham
Department of Psychology, University of Arkansas, Fayetteville, AR, USA

S. Rose
Department of Chemistry, Florida International University, Miami, FL, USA

Published online: 19 February 2011

Ray & Bates, 2006; Soraci et al., 2007), even at delays up to 1 week (Söderlund, Parker, Schwartz, & Tulving, 2005). At encoding, intoxicated participants are less able to attend to multiple cues, relate incoming information to existing knowledge, draw abstract inferences and process new semantic information (e.g., Marinkovic, Halgren, & Waltzman, 2004; Steele & Josephs, 1990). At retrieval, alcohol decreases sensitivity in recognition tasks (Maylor, Rabbitt, & Kingstone, 1987) and impairs long-term memory (Nelson, McSpadden, Fromme, & Marlatt, 1986). Overall, alcohol's impact on memory consolidation appears to be considerably greater than on recall of previously formed memories or the ability to hold new information in short-term memory (Soraci et al., 2007; White, 2003). However, alcohol's impact on retrieval is dependent on the recall format: Some findings suggest that intoxication impairs memory on explicit but not implicit tests (Lister, Gorenstein, Risher-Flowers, Weingartner, & Eckhardt, 1991; Ray, Bates, & Bly, 2004). Similarly, whether new information requires controlled/effortful or automatic processing at encoding may determine the effect of intoxication on cognitive processes and later recall (e.g., Abroms, Gottlob, & Fillmore, 2006; Kirchner & Sayette, 2003). Counter-intuitively, under certain conditions, alcohol appears to enhance memory: Moulton et al. (2005) found that alcohol's effect on memory depends on the timing of intoxication in relation to the event and retrieval. When consumed shortly after encoding, alcohol actually facilitated later retrieval when sober. The authors argue that alcohol suppresses cognitive activity, which may interfere with new memory formation. Others have confirmed this retrograde facilitation and anterograde impairment of memory (Bruce & Pihl, 1997; Garfinkel, Dienes, & Duka, 2006) with emotional stimuli and free recall instructions (Knowles & Duka, 2004).

To explain why alcohol has different effects on the same person in seemingly similar situations, Steele and Josephs (1990) proposed the concept of *alcohol myopia,* a state of disproportionate attention to immediate situational cues at the expense of weaker, possibly inhibitory cues when intoxicated. As intoxicated people are unable to attend to all relevant situational cues simultaneously, depending on the behavioral conflict in a situation, they may be more influenced by strong salient cues (e.g., someone insulting them) than weaker cues (e.g., being arrested if they start a fight). The authors argue that specific situational combinations of strong and weak cues will elicit differences between intoxicated and sober people. Intoxication limits cognitive capacity and disrupts cognitive functioning (Giancola & Corman, 2007) so that attention is allocated to salient cues at the disproportionate expense of peripheral cues (Steele & Josephs, 1990), resulting in impaired retrieval of peripheral or contextual event features. A number of studies have used this model to explain their results (e.g., MacDonald, Fong, Zanna, & Martineau, 2000; Monahan & Lanutti, 2000).

In addition to the pharmacological consequences of intoxication, many studies have confirmed that alcohol expectancies (or "placebo effects'') can also account for changes in behavior (e.g., McKay & Schare, 1999), although few have confirmed expectancy effects on cognitive performance (Testa et al., 2006). Depending on the metacognitive processes involved, placebo groups may perform better or worse than sober groups. For example, according to the hypervigilance hypothesis, expectancies can lead to compensatory behavior to counteract anticipated poorer performance (Fillmore & Blackburn, 2002; Testa et al., 2006).

Despite expert witnesses' and potential jurors' beliefs, there are reasons for concern that previous findings on alcohol and memory only minimally translate into forensic contexts. First, while studies that include stimuli such as word lists inform research and theory on alcohol's effect on memory, they are of limited value when predicting alcohol's effect on memory for complex, possibly interactive autobiographical events. There is a lack of research examining alcohol's effect on episodic memory, specifically the conditions affecting episodic memory retrieval (see Mintzer, 2007). Second, how much and how accurately people remember depends on recall format—for both sober and intoxicated individuals (e.g., Curran & Hildebrandt, 1999). However, few studies on alcohol and memory have included recall instructions similar to witness recall conditions (e.g., Cunningham, Milne, & Crawford, 2007; Garfinkel et al., 2006); instead they have used recognition-test formats, despite the fact that witnesses are likely to provide a description of the observed event in a free format. Arguably, differences in response bias and metamnemonic functioning may be differentially reflected in free recall formats and recognition tests. Finally, whether possible differences among alcohol, placebo, and sober witnesses will be considered substantial and thus relevant to law enforcement, expert witnesses and policy makers will likely depend on the effect size of any given difference in quantity and quality of witness testimony.

Two studies have examined alcohol's effect on eyewitness recall (Read, Yuille, & Tollestrup, 1992; Yuille & Tollestrup, 1990). Read et al. (1992) examined suspects' memory for a simulated robbery they committed while intoxicated. Intoxicated participants recalled more incorrect details than sober participants (study 1) and more incorrect information about a bystander/intruder than placebo participants (study 2). However, in contrast to a cooperative witness scenario, this study examined how suspects recalled their own (and others') actions. These "suspect" participants may have differed from cooperative witnesses in their encoding, retrieval, and motivation to provide a complete, accurate, detailed, and non-deceptive account.

In Yuille and Tollestrup's (1990) study, intoxicated, sober, and placebo participants were told that they would witness a staged theft. This witnessed event included an emotional interaction between two people, one of whom eventually left the room. Once alone, the thief stole money from a purse, threw the purse in the trash and left. Some participants in all three groups were interviewed immediately after the event and all were interviewed 1 week later. As the authors did not find significant differences between placebo and control groups, these two groups were collapsed for all analyses. Overall, intoxicated witnesses provided less relevant information and were less accurate, although differences were small.

Unfortunately, several aspects of this study limit the relevance of its findings. Participants were warned about the upcoming event and asked not to interrupt, creating an unlikely witness-encoding context. Furthermore, the authors collapsed across placebo and sober groups although the lack of significant differences between groups could warrant description and interpretation itself. Notably, even after collapsing across placebo and control groups, the difference in accuracy rates between intoxicated and sober witnesses was small (actual effect size of 2%), raising the question of practical importance. This study was thus designed to detect a large effect within a forensically relevant experimental framework, in line with beliefs within the legal system that there are considerable and reliable differences between intoxicated and sober witnesses.

Further, little is known as to whether intoxicated witnesses may be more vulnerable to post-event misinformation (e.g., Loftus, Miller, & Burns, 1978). There is reason to assume that intoxication may influence witnesses' vulnerability to suggestion. One study (Assefi & Garry, 2003) examined whether participants who were told they drank alcohol (but did not; i.e., a placebo group) were more likely to be influenced by post-event misinformation than participants who were told they did not consume alcohol (and did not; i.e., a control group). The authors found that placebo participants showed a stronger misinformation effect than control participants. However, this study did not include intoxicated participants or test the misinformation effect via an investigative interview.

In contrast to Assefi and Garry's (2003) findings, research has examined the effects of alcohol on suggestibility (Santtila, Ekholm, & Niemi, 1999) and false memory (Garfinkel et al., 2006) and found a decrease in suggestibility/false memory when intoxicated. Santtila et al. (1999) presented participants with a story, subsequent immediate recall, then alcohol intoxication (high, medium, low or placebo), another delayed recall and suggestive questions (via the Gudjonsson Suggestibility Scale, GSS; Gudjonsson, 1984). Hence, alcohol participants in this study were sober at encoding but drunk at time of recall and suggestive questioning. Overall, higher doses of alcohol were associated with decreased suggestibility for most of the dimensions measured by the GSS. The authors argue that intoxicated participants may have been less sensitive to negative social feedback/suggestive pressure compared to their sober counterparts. Garfinkel et al. (2006) investigated the effects of intoxication on false memory via the Deese–Roediger–McDermott paradigm (Roediger & McDermott, 1995). They presented alcohol or placebo participants with associated word lists either once or repeatedly. Each list's critical item (lure) was absent at encoding. When completing both an implicit and explicit memory test the following day, in explicit tests, alcohol decreased the number of correctly and falsely recalled items for lists presented once, with no difference between intoxication levels for repeated lists. There was no difference between intoxication levels for implicit recall.

These differences in findings indicate that how suggestibility is tested can influence its effect on intoxicated witnesses. To reflect likely suggestive influence in actual investigative settings, the current study compared intoxicated, placebo, and sober witnesses' memory for a "real" crime and real misinformation in a real investigative interview. The study's two main objectives were to explore possible differences in overall event recall and to compare possible differences in misinformation effects among intoxication levels. Beliefs of lay people, law enforcement, and experts would predict that intoxicated participants will provide the least amount of information, be the least accurate, and be most susceptible to misinformation out of the three groups.[1]

Method

Undergraduate students at least 21 years of age, recruited via the psychology department's participant recruitment website, participated in exchange for research credit. A telephone screening of potential participants excluded those with medical or psychological risks contraindicating alcohol consumption. Eligible participants were told that they might consume up to 0.08 g/210 L of alcohol and

[1] Based on Yuille and Tollestrup's (1990) data, this study was designed to detect an actual effect size of at least a 5%. With our sample size ($n = 93$), this study had 89% power to detect a medium effects size ($\eta_p^2 = .06$) to find an interaction between intoxication group and misinformation in our 3 × 2 repeated measures ANOVA. Furthermore, we had a 94% power to detect a medium/large effect ($\eta_p^2 = .11$) of intoxication on any of the recall measures in a one-way ANOVA.

instructed to abstain from alcohol for 24 h before participating, abstain from eating and drinking for 4 h before participating, and bring legal photo identification.

After arriving on site, participants completed an extensive consenting procedure and medical screening. Participants were allowed to continue if they had consumed alcohol in the past year ($\geq$3 drinks in one sitting), had no medical disorders aggravated by alcohol, were not pregnant, had not taken medications (except contraceptives), had no problematic reaction to cranberry juice/alcohol or limes, and scored $\leq$6 on the Brief Michigan Alcoholism Screening Test (Pokomy, Miller, & Kaplan, 1972).

Design

The study used a 3 (intoxication level: alcohol vs. placebo vs. control) $\times$ 2 (information type: misinformation vs. control) mixed design with information type manipulated within subjects. The dependent measures of primary interest were Proportion of reported correct, false, uncertain and "don't know" informational units out of all information reported, accuracy rate (percent correct information out of all correct and incorrect information reported), and number of words. To test the misinformation effect, the dependent measures of primary interest were number of misinformation items reported, and proportion of misinformation and control information reported out of all misinformation and control information presented.

Materials

During an extensive pilot phase, a believable, staged mock crime, "misinformation" phone call and witness interview were developed. The final version of the scripted mock crime was designed to re-create a detailed "real-world" theft containing both visual and auditory stimuli that would be noticeable, and, long enough to catch participants' attention while still being sufficiently ambiguous (to avoid drunk, or sober participants chasing the mock suspect or creating excessive emotional stress). Specifically, in the final version of the mock crime, a confederate "the intruder" entered the room and removed a laptop computer. When questioned by the experimenter, the intruder indicated that the computer needed to be moved to another room. When asked why the usual person (Craig) did not come, the intruder replied that he was out of town and left quickly. The experimenter protested that she had seen Craig earlier that day and became suspicious of the intruder.

After the intruder left, the experimenter became suspicious of the intruder and appeared to call University Technology Services (UTS) to explain the situation on the lab phone. This phone call contained the misinformation manipulation. The sets of misinformation and control items for this phone call were created as follows: On the basis of the staged crime script, a total of 32 potentially memorable details were generated and presented to 47 student participants from a different University who were asked to imagine that they were in a room witnessing an ambiguous event ("Imagine that you are in a room and that there is someone else in the room with you (Person A). Both of you see someone else enter the room (Person B), take a laptop and leave. There is also a verbal exchange between Person A and Person B.") Participants were then asked to rate the centrality of each of the 32 items on a scale from 1 (not central at all) to 10 (very central). "Central" was defined as what would most likely be the focus of their gaze if they were witnesses to the event and what would be considered key to the event. Each item's average centrality rating was then used generate the misinformation material, which was administered via a staged phone call participants overheard after witnessing the crime (see "Procedure"). Specifically, using the centrality ratings generated by the 47 misinformation pilot participants, 24 forensically relevant items were selected to generate a broad misinformation item pool that contained items that were rated as both central and less central. As misinformation was manipulated within participants, the misinformation material contained 12 misinformation items and 12 control items. To control for item repetition, the 12 control items were further divided into six control items correctly mentioned/repeated during the phone call and six control items not mentioned/repeated during the phone call. Holding average centrality between misinformation and control items and between mentioned and not mentioned control items constant, the 24 items selected based on the pilot data were then randomly assigned as either a misinformation or control item. Each half (misinformation and control) contained an equal number of central and less central items (see Appendix).

After piloting the feasibility of different question formats, participants were interviewed about the main details of the crime using one of three interview formats—all of which contained a series of open questions about the actors, actions, conversation, objects, and location involved in the theft to ensure that witnesses reported information pertaining to all aspects of the crime. Participants who were interviewed with the "open-ended narrative" interview were asked a series of questions about all relevant crime details (both misinformation and control items) phrased in an open-ended narrative fashion (e.g., "Please tell me everything you remember about the person's face/about what was said/about was stolen."). In the open-ended cued interview, participants were asked about the same items, but questions were phrased in a more specific way (e.g., "What did his or her face look like? What, if anything, did the person say? What was stolen?"). In the mixed interview format the interviewer started with an open-ended narrative questions ("Please tell me everything you remember about the incident") followed by the same list of open-ended cued questions as in the open-ended cued interview (e.g., "What did his or her face look like? What, if anything, did the person say? What was stolen?").

Procedure

Upon arrival sobriety was verified via a baseline breath alcohol test. To ensure accurate breath alcohol concentration (BrAC) testing, all readings were administered via an Intoxilyzer 5000®, an instrument on the Department of Transportation's (DOT) Conforming Products List of Evidential Breath Alcohol Measurement Devices. To ensure accuracy of the BrAC readings, this benchtop model was used, as opposed to handheld preliminary breath test devices (PBT). PBTs do not meet evidentiary standard for breath alcohol testing in many of the U.S. states (e.g., Florida) because of their low reliability (DOT, 2006). To further ensure accurate BrAC readings and that instruments were measuring accurately across the study's relevant BrAC range, all the instruments were calibrated using distilled water as a negative control and a 0.080 g/210 L ethanol solution as a positive control for every experiment day and using ethanol solutions of 0.050, 0.080, and 0.200 g/210 L monthly.

After the medical screening and consenting process, eligible participants proceeded to the simulated "barlab" where they watched the bartender prepare their three beverages. For the alcohol group, participants' weight and sex were used to calculate the dose required to achieve a peak BrAC of 0.060 to 0.080 g/210 L 30–40 min after drinking, assuming an average 0.015 g/210 L/h metabolism rate. This dose was computed using a modified formula based on Fisher, Simpson, and Kapur (1987), consisting of 2.35 mL 40% USP units of alcohol/kg of body weight for women and 2.82 mL 40% USP units of alcohol/kg of body weight for men, mixed 1:4 parts vodka:cranberry juice (e.g., MacDonald, Baker, Stewart, & Skinner, 2000). Placebo participants were led to believe that they would also reach a BrAC of 0.080 g/210 L. However, their three drinks contained only enough alcohol to achieve a peak BrAC of 0.010–0.200 g/210 L 30–40 min after ingestion (low-dose placebo). To increase the placebo's believability, the bartender poured a pre-calculated mixture of vodka and water from a vodka bottle into concentrated cranberry juice. Control participants saw the bartender pour cranberry juice into each glass. A slice of lime was added to all drinks (primarily to disguise taste in the placebo condition) and participants were asked to finish their beverages within 30 min. BrAC was measured at 20 and 30 min after the last drink. If the alcohol group's BrAC had not reached 0.060–.080 g/210 L at 30 min, they were tested once more after 10 min (40 min total) before continuing with the study.

Next, participants were taken to a different room, their BrAC was measured, and they were then asked to write down everything they remembered about the bar. A few minutes into the task, every participant witnessed the scripted staged crime (see "Materials"). After the intruder left, the experimenter became suspicious of the intruder and appeared to call University Technology Services (UTS) to explain the situation on the lab phone; the phone call followed a standard script. For all participants, her account included the same 12 pieces of misinformation and six pieces of correct information (control items) about the event and the perpetrator (out of a total of 12 misinformation and 12 control items). The other half of the control items (6) was not reported to control for item repetition (see "Materials").[2] The participant was then informed that a UTS representative (confederate) was coming to question both the experimenter and him/herself about the intruder to file a report with the University Police. A new confederate arrived 15 min later, introduced him/herself as a UTS employee, carried a clipboard, wore a UTS badge and presented a made-up form labeled "University Technology Services: Lost and Stolen Equipment Form." Participants were then interviewed while the experimenter waited outside. Both the mock crime and witness interview were recorded via a webcam hidden on the desk.

After the interview, participants filled out intoxication ratings, and believability ratings for the alcohol and placebo condition, the staged crime, the misinformation, and the UTS interview. All the participants were then fully debriefed. Intoxicated participants were retained in the lab until two consecutive readings of less than 0.040 g/210 L were obtained and behavioral observations (e.g., gait, coherence, hand–eye coordination, stability of mood, etc.) indicated a safe release.

All the recorded witness interviews were transcribed verbatim. Each transcript's witness statements were divided into units of information. A unit of information was defined as any new piece of information that could assist in solving the crime. Every unit was scored by a primary scorer on several dimensions. Regarding accuracy, each unit was scored as either correct, false, a "don't know" answer, subjective interpretation ("He was handsome"), or irrelevant ("I will be going to the movies tonight"). In addition, each unit could be scored as uncertain ("He was maybe six feet tall"). Each unit was also scored for misinformation or control information, that is, whether the reported information pertained to any of the misinformation or control items. If this was the case, then the scorer

[2] Owing to several experimental constraints to the misinformation manipulation within this study's "real-world" procedure, misinformation and control items were not counterbalanced across participants.

then determined whether the information was misinformation ("Black hair"), original/correct information ("Blonde hair") or other false information ("Brown hair"). Control items were scored as either correct or false. A subset of 62 transcripts was scored by a second independent scorer. Both scorers participated in extensive training and were blind to participants' experimental conditions. Across all the scoring categories inter-rater agreement reached a satisfactory level (overall ICC = .85).

Results

Participants

The study included 93 participants (alcohol = 30, placebo = 38, control = 25), with a majority of females (63%) and Hispanics (70%; 10% African-American, 18% Caucasian, and 2% Other). The mean age was 24 years ($SD = 4.6$).

Manipulation Checks

For the alcohol condition, the mean BrAC immediately before the staged crime (i.e., before encoding) was 0.07 g/210 L ($SD = .02$) (for placebo 0.00 g/210 L; $SD = .01$). The lowest BrAC before the crime was 0.04 g/210 L, and the highest BrAC was 0.14 g/210 L (for placebo 0.00 and 0.02 g/210 L, respectively).

The mean BrAC for the alcohol condition immediately after the witness interview, i.e., after retrieval was 0.07 g/210 L ($SD = .02$) (for placebo 0.00 g/210 L; $SD = .01$). In the alcohol condition the lowest BrAC immediately after the witness interview was 0.00 g/210 L and the highest BrAC was 0.12 g/210 L (for placebo 0.00 and 0.00 g/210 L, respectively).

The overall mean peak BrAC throughout the experiment was 0.08 g/210 L ($SD = .02$) for the alcohol group (for placebo 0.01 g/210 L; $SD = .02$). In the alcohol group, the lowest peak BrAC was 0.04 g/210 L and the highest peak BrAC was 0.14 g/210 L (for placebo 0.00 and .02 g/210 L, respectively). For all 3 BrAC readings (before the crime, after the witness interview and peak) the alcohol and placebo group's mean BrACs were significantly different, $t(63) = 19.91$, $p < .01$, $t\ (63) = 19.31$, $p < .01$ and $t\ (54) = 16.33$, $p < .01$, respectively.

Believability ratings, on a scale from 1 to 10 (*not at all–completely*), confirmed participants in the alcohol and placebo condition felt intoxicated. When asked whether they believed they were intoxicated at the 0.080 g/210 L level, alcohol participants' mean rating was 8.0, placebos' was 3.9, and controls' was 1.2, $F(2, 80) = 36.42$, $p < .01$. When asked whether they believed they consumed alcohol, the ratings were 9.4 (alcohol), 5.5 (placebo) and 1.3 (control), $F(2, 81) = 53.38$, $p < .01$. When asked whether they were subjected to real breath tests, the ratings were 9.6 (alcohol), 8.7 (placebo), and 9.1 (control), $F(2, 81) = 1.31$, *ns*.

Across intoxication levels, on a scale from 1 to 10 (*completely disagree–completely agree*) participants' mean agreement with "I believe that everything in the experiment was real," "I believed I witnessed a real crime," "I believe that the phone call was real," and "I believed that the witness interview was real" were 5.8, 5.7, 6.5, and 5.3, respectively, $Fs(2, 81) = 1.23$, 0.59, 0.23 and 0.89, respectively, all *ns*. Given that asking for believability ratings automatically uncovered the staged nature of the crime, thus probably decreasing participants' ratings, these means likely indicate that most participants believed the study's manipulations.

Primary Analyses: Overall Recall of Crime Information

A one way analysis of variance (ANOVA) tested for differences between intoxication levels (alcohol vs. placebo vs. control). Fisher's LSD tests were used for post hoc comparisons.

No significant differences were found between intoxication groups for the percentage of correct, false and "don't know" answers out of all informational units provided, $F(2, 90) = 1.24$, 0.85 and 0.22, respectively, all *ns* (see Table 1).[3] There were also no differences among groups in accuracy rates (percentage of correctly recalled details out of all correct and false details reported), $F(2, 90) = 1.67$, *ns*, or in the total number of words reported, $F(2, 90) = .48$, *ns*.

There were also no significant differences among groups for the percentage of uncertain statements, the percentage of subjective interpretations and the percentage of irrelevant details, $F\ (2, 90) = 0.48$, 1.2, and 2.93, respectively, all *ns*.

We then tested whether BrAC at the time of the crime predicted accuracy rate and percentage of correct, false and "don't know" answers out of all informational units provided by regressing BrAC levels onto those dependent variables. BrAC did not significantly predict any of these recall measures, $\beta = -.15$, $-.14$, 0.08, and 0.06, $t(87) = -1.36$, -1.28, 0.78, and 0.59, respectively, all *ns*.

Finally, we conducted two additional analyses. One redefined the alcohol group to include only those participants who had an unlawful breath alcohol (i.e., reached a peak BrAC of 0.08). The other analysis controlled for

[3] For proportional data, we also tested each comparison using different data transformations. Neither had an impact on our findings, i.e., all comparisons remained non-significant.

Table 1 Mean numbers and percentage of information reported per intoxication level (standard deviations in parentheses)

	Intoxication level			F	η^2	$CI_{.95}$
	Alcohol ($n = 30$)	Placebo ($n = 38$)	Control ($n = 25$)			
Percentage details out of all information reported						
Correct details	58.82 (12.52)	61.69 (10.4)	63.18 (8.02)	1.24	.03	.00–.11
False details	23.90 (8.5)	21.40 (8.94)	21.45 (7.95)	.85	.02	.00–.09
Don't know	17.28 (12.9)	16.91 (11.82)	15.37 (8.31)	.22	.01	.00–.05
Accuracy rate (correct/correct and false)	70.83 (10.27)	74.61 (9.94)	74.90 (8.26)	1.67	.04	.00–.12
Number out details of all information reported						
Correct details	27.27 (13.08)	31.16 (14.14)	30.84 (10.19)	.88	.02	.00–.09
False details	10.80 (6.08)	10.29 (4.98)	10.68 (6.16)	.08	.00	.00–.03
Don't know	7.33 (4.76)	7.87 (5.56)	7.32 (3.9)	.14	.00	.00–.04
Total number of details reported	45.40 (17.31)	49.32 (18.23)	48.84 (15.78)	.48	.01	.00–.07

whether participants were likely to have believed the staged crime. When only legally drunk participants were included in the alcohol group, there was again no difference among the three intoxication levels for accuracy rate, or percentage of correct, false and "don't know" answers out of all informational units provided, $F(2, 75) = .30$, 0.72, 0.82, and 0.86, respectively, all *ns*. As believability ratings varied between 1 and 10, they were recomputed into low believability (ratings between 1 and 5) and high believability (ratings between 6 and 10). A 2 (high vs. low believability) $\times$ 3 (alcohol vs. placebo vs. control) ANOVA revealed no main effect of intoxication level $F(2, 76) = 2.07$, 0.55, 0.74, and 0.19, respectively, all *ns*, but a main effect of believability on the percentage of correct and "don't know" answers reported, $F(2, 76) =$ 6.6, and 5.1, respectively, all ps $< .05$. Post-hoc comparisons revealed that high believers reported significantly more correct and fewer "don't know" answers than low believers. However, there was no interaction between believability and intoxication level for any of the dependent variables, $F(2, 76) = 1.22$, 1.44 and 0.09, respectively, all *ns*.

Primary Analyses: Misinformation

As there were no differences in results between mentioned and not mentioned control items, all misinformation analyses were collapsed across these two types of control items. Overall, out of all false information reported, 24% was misinformation, with no significant differences among intoxication levels, $F(2, 89) = .89$, *ns*. A 3 (alcohol vs. placebo vs. control) by 2 (misinformation vs. control information) mixed model ANOVA with repeated measures on the second factor confirmed the misinformation effect: across all intoxication levels participants reported a higher percentage of false information for those items they had received misinformation for (the 12 misinformation items) than those items they had not (control items), $F(1, 90) = 171.78$, $p < .01$. However, there was no interaction between intoxication level and type of information, that is, there were no significant differences in the size of the misinformation effect between intoxication levels, $F(2, 90) = 1.36$, *ns*. A second ANOVA found that there were no differences between misinformation and control items in the percentage of correctly recalled items, $F(1, 90) = .82$, *ns,* and again no significant interaction between intoxication level and type of information, that is, no significant differences among intoxication levels for misinformation and control items in the percentage of correctly recalled items, $F(2, 90) = 1.23$, *ns*.

We also tested for differences in the misinformation effect among intoxication levels in several other ways. First, out of all misinformation items possible, we compared the percentage of all false answers across intoxication levels and found no differences between the groups, both $F(2, 90) = .97$, *ns*. Second, to test whether misinformation had an effect on recall of original/correct information we then compared the percentage of correct answers for all possible misinformation items and found no differences among the groups, $F(2, 90) = 1.56$, *ns*. Finally, to test whether BrAC at the time of the crime predicted false answers for misinformation items, we regressed BrAC levels onto the percentage of false answers for misinformation items out of all misinformation items possible. BrAC levels did not predict percentage of false answers reported for misinformation items, $\beta = -0.03$, $t(87) = -0.28$, *ns*.

We conducted two additional analyses, one including only those participants with an unlawful breath alcohol concentration in the alcohol group (i.e., BrAC $\geq$ .080 g/210 L) and the other one controlling for the degree to which participants believed the staged crime. When only legally drunk participants were included in the alcohol group, there was again no difference among the three intoxication levels for percentage of misinformation answers out of all misinformation possible, $F(2, 75) = .43$, *ns*.

A 2 (high vs. low believability) × 3 (alcohol vs. placebo vs. control), ANOVA revealed no main effect of intoxication level on percentage of misinformation reported, $F(2, 76) = 1.93$, $p > .05$. There was also no interaction between believability and intoxication level for percentage of misinformation reported out of all misinformation items possible, $F(2, 76) = 1.15$, *ns*.

Finally, we examined differences among intoxication levels for control items only and compared the percentage of correct details reported for alcohol, placebo, and control participants. Again, there were no differences among groups $F(2, 90) = .13$, *ns*.

Secondary Analyses: Witness Interview Format

Although the study initially explored different witness interview formats (see "Materials"), investigating interview formats was not the primary focus of this study. Furthermore, the distribution of participant data across the different interview formats resulted in notably unequal cell sizes due to unforeseen medical screen-outs after randomization. Data for all primary analyses (for all overall recall and misinformation analyses) were thus collapsed across the different interview formats. The following set of secondary analyses explored potential differences among the interview formats for all major outcome variables.

A series of intoxication level (alcohol vs. placebo vs. control) by interview format (open-ended narrative vs. cued vs. mixed) ANOVAs were carried out on the percentage of correct, false, and "don't know" answers out of all informational units provided. There was a significant interaction between interview format and intoxication level for percentage of false answers out of all informational units provided $F(4, 79) = 2.85$, $p < .05$, $\eta^2_p = .13$. Post-hoc comparisons revealed that in the cued interview format only, intoxicated and placebo participants reported more false answers than sober participants, $ps < .01$ and 0.05, respectively. Similarly, when only those participants with an unlawful breath alcohol concentration in the alcohol group were included, there was the same interaction between witness interview format and intoxication level for the percentage of false answers out of all informational units provided across intoxication levels, $F(4, 62) = 2.89$, $p < .05$, $\eta^2_p = .16$. There were no main effects of interview format or interactions between interview format and intoxication level for any of the other dependent variables, that is, for accuracy rates (percentage of correctly recalled details out of all correct and false details reported), the total number of words reported, the percentage of uncertain statements, the percentage of subjective interpretations, and the percentage of irrelevant details, all $ps > .05$. There were also no additional effects of intoxication or interactions when interview format was added to the analysis that divided participants into high and low believers, all $ps > .05$.

Finally, as part of these secondary analyses, we added witness interview format to the "misinformation' analyses and found no additional main effects or interactions, all $ps > .05$. That is, interview format neither influenced the misinformation effect nor explained any of the misinformation null results.

Discussion

This article tested the forensically important effects of intoxication on memory for a real crime and on memory suggestibility using a simulated witness interviewing and misinformation scenario. Although our manipulation checks indicated that participants reached and believed their intended intoxication levels and the witness scenario, our findings do not suggest that intoxicated witnesses in general show noticeably impaired memory compared to sober witnesses or witnesses who believe themselves to be intoxicated. Relatedly, alcohol did not increase the vulnerability to misinformation compared to sober and placebo participants.

Our lack of differences in overall witness recall among intoxication levels is surprising in light of research indicating that both potential jurors and expert witnesses believe that intoxicated witnesses will be more cognitively impaired than sober ones (Evans & Schreiber Compo, 2010; Kassin et al., 2001). An earlier study investigating alcohol and witness recall (Yuille & Tollestrup, 1990) did find a small but significant difference between their alcohol and control group (which consisted of both placebo and sober participants) in the proportion of correctly recalled details (91 and 93%, respectively). Interestingly, differences in accuracy rates among intoxication levels in our study are similar, if not greater (see Table 1), but our effect sizes were at best small with standard deviations much larger, hence differences were not significant. In contrast to those studies examining alcohol's effect on memory for simple material, we purposely designed our study to be able to detect large effect sizes, that would imply forensically important differences in autobiographical memory that are both noticeable and relevant in investigative settings. Our set of findings does not warrant inferences with real-world applicability—similar to Yuille and Tollestrup's findings (1990).

Although this study was not primarily concerned with the influence of interview format on intoxicated witness memory, our exploratory analyses of witness interview format together with prior research on alcohol and memory in general cautiously suggest that alcohol's effect on memory may depend on recall format (e.g., Ray et al., 2004). It should be pointed out however, that the number of comparisons involved in these analyses does not allow for a strict interpretation of this preliminary finding. It suggests, however, that future research should further explore whether using open-ended retrieval strategies (in a context that emphasizes accuracy) may assist intoxicated witnesses in applying more stringent retrieval strategies compared to recognition tests, thus eliminating possible differences among intoxication levels.

In addition, our findings may have also been due to the type of to-be-remembered event. In line with alcohol myopia theory (Steele & Josephs, 1990), the staged crime may have been perceived as an important stimulus, allowing for intoxicated participants to allocate their attention similarly to sober participants. According to Steele and Josephs, whether cognitive-behavioral differences exist between intoxication levels depends on the specific combination of cues in a given situation. It may have been exactly this combination of salient encoding situation and open-ended recall that allowed intoxicated witnesses to compensate for any possible impairment. In other words, whether recall differences exist between intoxicated and sober witnesses may be a function of both the type of material presented (cohesive autobiographical event vs. list of words) and the recall format. Specifically, salient events, such as the crime in this study, may not elicit mnemonic differences between intoxicated and sober participants, because both witness groups are equally able to allocate their full attention to the to-be-remembered material. In addition, a subsequent official, free recall interviewing situation may further downplay potential differences such that the demand characteristics of the recall situation (accuracy is of utmost importance) are readily apparent. Thus, under these conditions, even the intoxicated may be able to notice the need for metamnemonic strategies and apply those accordingly. Future research should disentangle the respective influences of type of event, recall format/situation and level/state of intoxication. For example, based on the presented data, it appears particularly promising to focus on investigating the effects of more specific or leading question formats on witness recall when intoxicated.

Several alternative explanations need to be considered when interpreting the current set of findings. Although we purposely designed the event to reflect memory processes in actual witnesses, we only tested one type of event: A short, unanticipated, ambiguously staged theft. This may have influenced attention, and in turn how well the event was encoded and later remembered. The effects of intoxication on autobiographical memory may only become apparent when people have the opportunity to dedicate all resources to a longer, unambiguous event that contains both peripheral and central information, and possibly also reduces the variance in details remembered across participants. As mentioned above, Steele and Josephs' (1990) alcohol myopia framework suggests that alcohol can affect cognitive functioning via restricting the range of perceived cues. However, detecting this reduced attentional capacity may require a minimum length and level of detail of stimulus material.

A second alternative explanation is that the BrAC levels in our alcohol group were not high enough to reflect/induce cognitive impairment. According to law enforcement experience, on average, real-world witnesses are more intoxicated than 0.080 g/210 L when witnessing a crime (e.g., Evans et al., 2009), possibly resulting in more impaired cognitive processes than reflected in this study. Although differences in cognitive performance have been found in previous studies between 0.080 g/210 L BrAC levels and sober/placebo groups (e.g., Cunningham et al., 2007), none examined memory for autobiographical events. However, BrAC levels of more than 0.08 will be difficult to include under controlled experimental conditions without significantly increasing participants' risk level.

Finally, it was interesting to find that despite an overall misinformation effect, under the conditions tested, the effect did not vary with intoxication level. Others have reported either an increased suggestibility for placebo compared to sober participants (Assefi & Garry, 2003) or a decrease in suggestibility when intoxicated compared to sober participants (Santtila et al., 1999; Garfinkel et al., 2006). However, all the studies including this one used different methodologies when examining suggestibility, that is, how suggestibility was manipulated (misinformation vs. exposure to suggestive question), how it was tested (multiple choice questions vs. investigative interview format vs. suggestive questions) and whether participants were intoxicated at encoding, after encoding and/or retrieval. Although these multi-dimensional differences render it difficult to pinpoint under which circumstances the intoxicated may or may not be more suggestible, they do suggest that how and when suggestibility is tested may determine the effects of alcohol on suggestibility. Specifically, whether suggestibility is manipulated via exerting (social) pressure within an interview situation (see Santtila et al., 1999) or via presenting outside/post-event misinformation creating source-monitoring decisions at time of recall may be important. Santtila et al. (1999) have further argued that the intoxicated may show impaired processing

Table 2 Mean numbers and percentage of Information reported for misinformation and control items per intoxication level (standard deviations in parentheses)

	Intoxication level			F	η^2	$CI_{.95}$
	Alcohol ($n = 30$)	Placebo ($n = 38$)	Control ($n = 25$)			
Information reported for misinformation items						
Number of misinformation items reported	2.27 (1.53)	2.50 (1.71)	2.12 (1.81)	.41	.01	.00–.06
Percentage of misinformation items reported[a]	19.31 (13.14)	21.10 (14.37)	17.82 (14.08)	.42	.01	.00–.06
Number of correct items reported	2.87 (1.31)	3.13 (1.73)	3.68 (1.8)	1.76	.04	.00–.13
Percentage of correct items reported[a]	24.52 (11.58)	26.50 (14.72)	31.00 (14.79)	1.56	.03	.00–.12
Number of total false items reported	3.47 (1.83)	3.95 (1.86)	3.32 (2.04)	.97	.02	.00–.10
Percentage of total false information reported[a]	29.41 (15.48)	33.38 (15.91)	28.03 (16.96)	.97	.02	.00–.10
Information reported for control items						
Number of control items reported	2.93 (1.57)	3.13 (1.73)	3.04 (1.49)	.13	.00	.00–.04
Percentage of control items reported[a]	24.44 (13.12)	26.10 (14.39)	25.33 (12.38)	.13	.00	.00–.04
Number of false items reported	.90 (.66)	.84 (.82)	1.00 (1.15)	.24	.01	.00–.05
Percentage of false information reported[a]	7.50 (5.52)	7.02 (6.86)	8.33 (9.62)	.24	.01	.00–.05

[a] As all 12 misinformation and 12 control items (collapsed across 6 mentioned and 6 not mentioned control items in the misinformation phone call) were incorporated into a live, staged phone call, occasionally experimenters failed to include a misinformation or control item. Thus, the different percentages of information reported for each participant were based on the number of misinformation and control items possible, i.e., mentioned in the phone call (12 for most participants)

of suggested information and may therefore be less suggestible than sober groups. It is possible that intoxicated witnesses in our study did not pay attention to or process the misinformation well, thus counteracting any suggestibility effect it could have had. Alternatively, possible differences amongst the groups may have been difficult to detect because the overall number of original and misinformation items recalled (correctly or incorrectly) was low (see Table 2)—possibly due to the open recall format.

Irrespective of intoxication, asking witnesses to freely recall crime items may have both increased the reporting threshold and thus decreased overall recall of misinformation compared to forced-response multiple choice tests oftentimes used in misinformation paradigms. It is also possible that participants became suspicious of the misinformation due to the sheer number of falsely mentioned items in the phone call and may have become particularly critical of the information and discarded any/most information given on the phone. Thus, the overall low recall of misinformation in this study may be the result of how it was administered.

Alternatively, it should be pointed out that the (size of the) misinformation effect in this study could have been the result of the experimental approach chosen. Namely, as misinformation and control items were not counterbalanced across participants, our misinformation effect may have been the result of the specific material presented as misinformation or control information, respectively. It is also possible that a different set of misinformation items may have magnified possible differences between intoxication levels. However, the rather high number of misinformation items presented and the random assignment of control versus misinformation items, reflected by similar percentages of correctly remembered control and misinformation, decrease this possibility. Given that the experimental procedure chosen did not allow for the typical counterbalancing of misinformation items without a significant loss in experimental standardization and manageability, future studies should allow for better experimental control over possible misinformation item effects, possibly at the expense of ecological validity.

It remains clear, however, that a "blanket" assumption that intoxicated witnesses will be more vulnerable to suggestion is at odds with this study and with most of the few studies conducted so far. The fact that our findings appear to be in contrast to Assefi and Garry (2003), who found an increased vulnerability to misinformation in placebo compared to control participants, is further evidence that how misinformation is tested may be an important feature in determining the size of the effect. Relatedly, others have reported a more conservative response bias when intoxicated (Mintzer, 2007), suggesting that under conditions that promote accuracy and more stringent recall criteria, such as a witness interview, accuracy rates may not be affected by alcohol.

Conclusion

This study examined intoxicated witness memory using an experiment designed to closely reflect real-world conditions. Interestingly, findings suggest that research on alcohol and memory in general may not be easily applied to the legal context: when testing memory for a surprise autobiographical event, intoxication did not result in noticeable cognitive impairment in a subsequent interview and did not render intoxicated witnesses more vulnerable to freely recalling post-event misinformation. Although null findings have traditionally been met with skepticism, we argue that in the eyewitness context, these null results are important given that hey are at odds with opinions about and possibly practices with intoxicated witnesses. Our findings should cast doubt on the uncritical adoption of findings on alcohol's effects on basic mnemonic processes and address the inclusion of different and relevant stimulus and testing conditions when examining legally relevant questions in psychology research.

Appendix The 24 items selected based on the pilot data were then randomly assigned as either a misinformation or control item

Original information	Misinformation	%Not mentioned	%Correct	%Misinformation	%Other false
Misinformation items					
E: "Laptop won't be picked up for at least ½ h"	E: "Laptop won't be picked up for at least another h"	79	5	15	1
Laptop was on desk	Laptop was on table	28	52	19	1
S. ethnicity (e.g., Hispanic)	S. ethnicity (e.g., White)	16	37	42	5
S. build (e.g., medium build)	S. build (e.g., skinny)	20	44	26	10
S. hair color (e.g., brown)	S. hair color (e.g., blond)	22	43	24	12
S. shirt (t-shirt)	S. shirt (polo shirt)	27	44	17	12
S. height	S. height (6 inch difference)	12	8	20	60
Laptop type (Gateway)	Laptop type (Dell)	79	21	0	
S. nose (small)	S. nose (big)	94	0	3	3
S. eye color (e.g., brown)	S. eye color (e.g., blue)	62	25	8	5
S. necklace (yes)	S. necklace (no)	76	4	19	
S. ring (no)	S. ring (yes)	76	16	8	
Control Items					
S. said Craig is out of town	S. said Craig is out of town	69	31		
S. ran off	S. ran off	44	55		1
S. was not wearing glasses	S. was not wearing glasses	91	7		2
S. opened door with card	S. opened door with card	54	46		
S. was wearing strong cologne	S. was wearing strong cologne	97	3		
S. was not wearing earring	S. was not wearing earring	80	20		
S. pants		36	54		10
S. age		23	21		56
S. Shoes		84	3		13
S. hairlength		68	28		4
S. had no accent		85	12		3
S. had a watch		88	10		2

Note: Based on pilot test data, a set of 24 forensically relevant and diverse items was chosen containing items that were rated as both central and less central. Those 24 items were then randomly assigned as misinformation or control items holding average centrality rating constant across misinformation and control items. Out of the 12 items listed here, the first six experimental and control items were on average rated as central in the pilot test (in descending centrality), and the second half as peripheral. The percentage mentioned, not mentioned, and misinformation depicts the *actual* data, that is, average answers for these items after incorporation in the study

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