Commentary

The lack of robust evidence for the
effects of alcohol on false memory

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We comment on Kluft et al.'s (2021) review of the effects of alcohol and other drugs on false memory reporting. Across studies, problems of internal and external validity and methodological consistency preclude any blanket conclusions and recommendations regarding alcohol's effects on false memory reporting and suggestibility in witnesses. We argue that any policy and practice conclusions drawn from this limited literature are premature and would be unfairly prejudicial to witnesses and confusing to triers of fact at this time.

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We comment on Kloft et al.'s (2021) review of the effects of alcohol and other drugs on false memory reporting. Across studies, problems of internal and external validity and methodological consistency preclude any blanket conclusions and recommendations regarding alcohol's effects on false memory reporting and suggestibility in witnesses. We argue that any policy and practice conclusions drawn from this limited literature are premature and would be unfairly prejudicial to witnesses and confusing to triers of fact at this time.

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In their narrative review, "Hazy memories in the courtroom: A review of alcohol and other drug effects on false memory and suggestibility", Kloft et al. (2021) stated that "both alcohol and cannabis can impact the susceptibility to spontaneous and suggestion-based false memory or suggestibility, often exerting effects of medium or large size" (p.302). They thus concluded that alcohol-intoxicated witnesses are "vulnerable" and offered the recommendation that a "nuanced approach" is required in handling these witnesses (p.303).

While it is useful to take stock of the research conducted to date, we believe there are substantial internal and external validity concerns arising from the studies that make it impossible to draw conclusions about drugs and false witness memory at this time. In what follows, we focus on problems with alcohol research, but similar critiques can be made regarding other drugs.

First, the conclusions that can be drawn in narrative compared to systematic and meta-analytic reviews are relatively weak. Kloft et al. conducted a narrative review. While they report that alcohol effects are "often medium or large", most of the research they reviewed found no alcohol-related differences, with 7 out of 10 papers reporting null results (see Table 2). If the review had been a meta-analysis, sample sizes would have been taken into account in assessing effect sizes. However, the authors state effect sizes without any discussion of sample size. Further, if this had been a systematic rather than a narrative review, the conclusions would be stronger because research quality, and research biases would have been considered for each paper reviewed.

Second, two out of the three papers with significant alcohol-related differences did not experimentally vary dose (i.e., van Oorsouw et al., 2015; van Oorsouw et al., 2019), with the former further testing mock suspects' memory (as opposed to mock witnesses' memory). Participants in these studies were self-intoxicating, and therefore, owing to a lack of random assignment, alcohol consumption is confounded with individual differences that may be tied to general memory ability. As such, despite the studies having included some people with relatively high BACs, their inherent lack of internal validity needs to be positioned within the larger context of experimental research.

Third, the papers vary in a multitude of ways, such as in dose, delay, encoding specificity, and especially in how suggestibility and false memory were operationalized. However, Kloft

et al. suggested dose and delay are key factors that control whether intoxicated witnesses are “more vulnerable to suggestion”. We do not think this inference is warranted based on our reading of the literature. It is not clear that dose distinguishes the studies reporting significant differences. Table 2 reports BAC ranges rather than averages. However, van Oorsouw et al. (2019) report in their paper the average BAC level was .06, which is comparable to studies with null findings. Further, the results of this study conflict with van Oorsouw et al. (2015), who did not find that the reporting of misleading details increased with BAC level (i.e., the moderate and high groups did not differ from each other in this regard). Furthermore, Evans and colleagues found alcohol-induced suggestibility to be more apparent after a delay, but only for participants who were in the same state at encoding and retrieval – an important qualification.

Fourth, we are concerned about the lack of external validity of many of the studies referenced by the authors, and this concern extends to those that report null effects as well as those reporting statistically significant effects. One of the main lessons learned from the recent body of work investigating the effects of alcohol intoxication on witness memory using eyewitness paradigms is that many findings derived from the research on alcohol’s effects on basic memory processes suggesting overall impairment do not easily generalise to episodic and witness scenarios. As other examples, in van Oorsouw et al. (2019), the to-be-remembered-event was a conversation between the participant and a research assistant, not a crime. Further, again, unlike real world witnesses, participants were not interviewed face-to-face, but took a written test, which had misleading questions. In van Oorsouw et al.’s (2015) study, again unlike the witness context, the interviewer asked two-alternative forced choice questions that included misleading details. Although we certainly appreciate the need for a diversity of experimental approaches, as well as the difficulties of conducting studies that involve both alcohol-intoxication and externally valid witness interviewing paradigms, we do take issue if these translational problems are not highlighted when drawing conclusions and attempting policy recommendations.

Relatedly, for the majority of studies reviewed, the authors provide little rationale for why they highlight certain results over others. To illustrate, in Flowe et al. (2019), one of the studies reviewed, the researchers experimentally investigated memory reporting in hypothetical rape scenarios as a function of alcohol intoxication and expectancy. They found no effect of alcohol consumption on the reporting of misinformation or extra-scenario details (i.e., imagined information, which was operationalised in a manner fundamentally different from Kloft et al.’s definition of confabulations). Among participants who drank alcohol, those who knew they drank alcohol reported fewer extra-scenario details compared to those who thought they drank tonic water. People in the real world, including rape victims, who are consuming alcohol usually know that they are, so this is the key condition of the experiment. Under these conditions, where people drink alcohol, and know it, memory accuracy is higher compared to people who do not know they are drinking alcohol. Nevertheless, Kloft et al. chose to instead highlight the greater number “confabulations” reported by participants who did not know they drank alcohol compared to those who did know. In our view, positioning the results in this way, without any argument about which condition is the most forensically-relevant, can lead to unfair prejudice against rape victims if presented out of context. This is especially concerning considering how few rape cases are prosecuted.

In summary, we believe blanket conclusions and recommendations regarding drugs and false witness memory drawn from the extant literature in a non-systematic fashion have little to no probative value and potentially cause unfair prejudice and confuse triers of fact.

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