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Letter to the Editor in Response to The Alcohol Mixed with Energy Drinks Debate: Masking the Facts!

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In Reply

Verster et al. provided commentary and raised questions about our recent paper, “Mixing an Energy Drink with an Alcoholic Beverage Increases Motivation for More Alcohol in College Students” (Marczinski et al., 2012). The authors stated that our conclusions may be premature and that issues with design, analysis and interpretation of the data may merit different conclusions. In addressing the concerns raised by Verster et al., we will provide additional details and data analyses. These additional particulars that were not explicit in our publication will confirm our study does indeed provide laboratory evidence that alcohol mixed with energy drink (AmED) beverages may lead to greater motivation to drink versus the same amount of alcohol alone.

The primary issue raised in the letter pertained to our choice of analyses for the data. Our overall analysis of the desire for alcohol ratings utilized a 2 (Alcohol) $\times$ 2 (Energy Drink) $\times$ 2 (Gender) $\times$ 6 (Time) ANOVA with Time as a repeated measures factor, which Verster et al. conceded was appropriate. However, Verster et al. raised concerns about how we conducted follow-up comparisons on our obtained Alcohol $\times$ Energy Drink $\times$ Time interaction. We conducted paired samples *t* tests comparing each rating following dose administration to the appropriate baseline rating of desire for alcohol, since this would allow us to determine how long desire for alcohol was elevated compared to the baseline assessment under each dose condition. This approach was chosen in response to issues being raised in the literature. Specifically, it has been reported that consumption of AmED beverages leads to longer drinking episodes and drinking to higher BAC levels as compared to the same amount of alcohol alone (Jones et al., 2012; Thombs et al., 2010). However, no laboratory research, prior to our current publication, had specifically examined whether the AmED beverage itself led to longer desire to drink compared to alcohol alone. The methodological approach of using multiple assessments over time also ensures that any one observed subjective rating is not a spurious observation. Finally, our choice to perform within-subjects comparisons of subjective ratings against the subject’s own baseline was guided by the fact that social drinkers do differ in baseline ratings of desire for alcohol (Fillmore, 2001), making a within-subject comparison of change over time following a priming dose most appropriate.

As such, our findings that desire for alcohol ratings were elevated at 10, 20, 40 and 60 minutes after AmED dose administration as compared to only 10 and 20 minutes after alcohol alone provide laboratory evidence converging with what has been reported by others. In our paper, we also reported a nonsignificant trend when the ratings at 40 minutes

for the alcohol vs. AmED conditions were directly compared using an independent samples t test ($p = .075$). Verster et al. stated that the lack of a statistically significant between group difference at this time point leads to erroneous conclusions about our data. The authors were concerned that our analysis may have been influenced by chance in part because of variations in mean baseline scores in the desire for alcohol ratings.

To address Verster et al.'s concerns, we employed an alternative approach to examine our data by utilizing analysis of covariance (ANCOVA) using baseline desire for alcohol ratings as the covariate. This alternative analytic approach is appropriate given that we have baseline desire for alcohol scores and that baseline scores were correlated with post-dose scores (Tabachnick & Fidell, 2007). Use of ANCOVA addresses Verster et al.'s concern that variability in baseline ratings contributed to the alcohol vs. AmED difference by chance. We regret that we did not employ ANCOVA as a statistical approach in our original publication, in part because ANCOVA increases the power of the F test for main effects or interactions by removing predictable variance associated with the covariate from the error term. As correctly noted in the Verster et al. letter, there are individual differences in our baseline desire for alcohol ratings, although our groups were not statistically different from one another at baseline. This variance in baseline ratings is undesirable when the response under the dose is really what is of interest. As such, we repeated our analysis of our desire for alcohol ratings by conducting a 2 (Alcohol) $\times$ 2 (Energy Drink) $\times$ 2 (Gender) $\times$ 5 (Time) ANCOVA with the baseline desire ratings treated as the covariate and Time treated as a repeated measure. In this analysis, the covariate was significant, $F(1,71) = 84.66$, $p < .001$, $\eta^2 = .544$. More importantly, we observed an Alcohol $\times$ Energy Drink interaction, $F(1,71) = 4.69$, $p = .034$, $\eta^2 = .062$.

Specifically to address Verster et al.'s concern about the alcohol vs. AmED comparison at the 40 minute time point, we conducted a simple 2 (Energy Drink) $\times$ 2 (Gender) ANCOVA with the baseline desire ratings treated as the covariate and the 40 minute ratings collected in the alcohol conditions (alcohol and AmED only) as the only dependent measure. Note that this analysis is analogous to the between-groups t test comparing alcohol vs. AmED at 40 minutes reported in the paper, but we are including the baseline data as the covariate. In this analysis, the covariate was significant, $F(1,35) = 16.10$, $p < .001$, $\eta^2 = .315$. Most importantly, we observed a main effect of the Energy Drink, $F(1,35) = 4.84$, $p = .035$, $\eta^2 = .121$, with higher ratings in the AmED condition (see Table 1). Given the partial η^2 value obtained for this alcohol vs. AmED comparison at 40 minutes, convention dictates that this is a moderate to large effect (Green & Salkind, 2011). Thus, this alcohol vs. AmED group comparison was statistically significant using ANCOVA, even though we reported only a nonsignificant trend when using an independent samples t -test in our original paper. We regret not reporting a covariance analysis to adjust for individual differences in baseline ratings in our original publication, since this analytic approach underestimated the difference between alcohol and AmED in desire for alcohol ratings. In addition, it should be noted that for every time point following dose administration, desire for alcohol ratings were higher for AmED compared to alcohol alone. This is observable with both the raw means and the adjusted means from the ANCOVA (see Table 1). Therefore, based on the analyses in the original paper and the above-described new analyses, we conclude that consumption of AmED results in increased desire for alcohol ratings for a longer period of time compared to the same amount of alcohol alone.

Regarding additional concerns raised in the Verster et al. letter, subjects in our study did not drink alcohol after the initial priming dose as this was beyond the scope of our study. Subsequent research is needed to demonstrate that elevated ratings of desire for more alcohol following a priming dose of AmED actually correspond with subsequent self-administration of alcohol. However, previous work in the priming literature suggests that

social drinkers' self-reported ratings of desire for alcohol are positively associated with actual consumption of alcohol in the laboratory (Corbin et al., 2008; de Wit, 2000). Additionally, there have been several reports using animal subjects that caffeine promotes alcohol consumption (Dietze & Kulkosky, 1991; Kunin et al., 2000; Palmatier et al., 2012).

In addition, Verster et al. commented that differences in our vehicle versus energy drink may have led to different expectancies depending on participants' experiences with Red Bull and Squirt. While we cannot specifically address expectancies given our study design, we wish to reiterate that our subjects were never informed about the types or brands of alcohol or energy drinks in this study and dose administration was double-blind. Moreover, the use of a between-subjects design should mitigate concerns that participants attempted to guess what type of beverage they were receiving. Again, future research is needed to better elucidate the role that expectancies play, particularly since manufacturers do promote these products with certain images consistent with risk taking.

Finally, we wish to address Verster et al.'s criticisms that grant awarding agencies, particularly the National Institute on Alcohol Abuse and Alcoholism (NIAAA), should not fund this type of research. There has been a dramatic escalation in the concern regarding alcoholic energy drinks worldwide. We still suffer from serious gaps in our understanding of the transition from social drinking to abusive drinking in society. Our research findings provide an empirical basis justifying further investigation of whether AmEDs may be contributing to the abuse potential of alcohol. If the consumption of AmED increases the motivation to drink more alcohol, these drinks are not merely a low-risk new fad among young people. At the very least, systematic laboratory investigations should provide greater understanding as to whether AmED differs from alcohol alone. Moreover, the mission of the overall grant funding program of the National Institutes of Health is to develop and apply knowledge that will enhance health, lengthen life, and reduce the burdens of illness and disability (see <http://nih.gov/about/mission.htm>). Thus, the research described in our paper directly fits with the mission of NIH and the information gained can be communicated to the taxpayers of the United States who fund this research. Verster et al. report their funding is derived from entities having a significant financial interest in the safe consumption of energy drinks. Considering that, we assume such entities would desire greater transparency regarding potential risks to their lucrative franchises. Our hope is that stewardship concerns prompt Verster et al. to more actively participate in this research, as opposed to casually dismissing it on the basis of spurious arguments. While we welcome the opportunity to address legitimate questions and concerns regarding methodology and analyses, we emphatically reject the authors' conclusion that our research was not pursued with appropriate scientific rigor, or that it would not be of interest to the larger alcohol research community.

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Table 1

Self-reported desire for alcohol mean ratings from the original ANOVA reported in Marczinski et al. (2012) and adjusted means from the ANCOVA using baseline desire for alcohol ratings as the covariate. The covariate in the model was evaluated at the following value: baseline desire rating = 8.03.

	Dose Condition					
	Vehicle		Energy Drink		Alcohol	
	M	(SD)	M	(SD)	M	(SD)
<i>Desire for Alcohol (ANOVA)</i>						
Baseline	7.80	(15.58)	13.00	(17.66)	6.65	(16.25)
10 min.	13.35	(19.30)	14.00	(16.76)	19.20	(25.51)
20 min.	10.00	(17.58)	11.70	(18.16)	18.45	(27.93)
40 min.	11.80	(18.64)	10.15	(17.71)	13.40	(18.73)
60 min.	8.60	(17.73)	10.00	(19.01)	8.40	(16.00)
80 min.	6.90	(14.58)	6.50	(12.73)	3.35	(7.36)
<i>Desire for Alcohol (ANCOVA)</i>						
10 min.	13.55	(18.91)	9.59	(19.17)	20.42	(18.93)
20 min.	10.23	(20.22)	6.71	(20.50)	19.83	(20.25)
40 min.	11.99	(17.61)	5.97	(17.84)	14.56	(17.62)
60 min.	8.76	(13.66)	6.47	(13.85)	9.38	(13.68)
80 min.	7.01	(8.05)	4.02	(8.15)	4.04	(8.05)
					6.93	(8.09)