

Revisiting our review of SBIRT: Meta-analytic results still point to no efficacy in increasing the use of substance use disorder services

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We appreciate the response to our meta-analysis (1) by Simioni and colleagues (2). They published two systematic reviews this year in other peer-reviewed addiction journals on this topic (3,4). The major differences between our review and theirs is that our study meta-analyzed the data whereas they were focused on providing an overview of the literature, and each research team chose different eligibility criteria leading to slightly different samples of trials. We determined through a meta-analysis of RCTs that there was a lack of evidence to support the efficacy of brief interventions, as currently implemented, in increasing the utilization of alcohol treatment (1), whereas they determined, based on qualitative summaries of RCTs and non-RCTs, that there is no firm evidence or limited evidence (3,4).

Their commentary raises two issues with our meta-analysis. First, they suggest that the two published (5,6) and one unpublished (7) studies found in their literature reviews may have altered our meta-analytic results had we identified and analyzed them. Herein we put the two published studies in context by providing a supplemental meta-analysis that used the same data extraction and analytic techniques as described in our original report (1). Our published report meta-analyzed 1,930 participants in the 9 studies that had an acceptable risk of bias and available data (1). We added the two additional published studies (5,6), yielding 2,380 participants in 11 studies. The revised random effects pooled risk ratio (RR) was 1.16 (95% CI=0.96-1.40). The heterogeneity statistic ( $I^2$ ) was 6.8% ( $X^2=10.72$ ,  $p = 0.379$ ), indicating no evidence of heterogeneity. Our findings and conclusions have not changed. Brief interventions as currently implemented do not appear to have efficacy in increasing the utilization of alcohol treatment.

It is not surprising that two different research teams conducting systematic reviews would differ slightly in their identification of studies due to their search and/or screening processes, or in their findings due to their different eligibility criteria and analytic techniques. These are known issues in systematic reviews and meta-analyses (8–10). None of the studies Simioni and colleagues mentioned (2) were identified in our database search, expert query, or hand search. Two of the studies were pertinent. The endpoint for the third, unpublished study was an assessment to see if

treatment was needed (7), whereas we were interested in treatment utilization. There were also issues of bias in how the outcome was assessed that would have led to its exclusion had we identified it.

Simioni and colleagues posed the question, “Is there really no evidence of the efficacy of brief alcohol interventions for increasing subsequent utilization of alcohol-related services?”. We believe the answer is still “yes”, but there is much room for innovation. One of the published RCTs identified by their team was a pilot study of a bibliotherapy intervention among emergency department patients, which was not designed to detect statistically significant effects and did not detect them (6). However, it demonstrated the feasibility and acceptability of a bibliotherapy intervention to engage patients following a time-limited emergency department visit to help inspire them to obtain treatment. The other study showed that a multiple-session brief alcohol intervention had a statistically significant effect on the utilization of alcohol treatment in medical inpatients (5). This study by Liu and colleagues offers important insights. In particular, their post-hoc analysis showed that the association between the number of brief intervention sessions attended (there were up to three) and alcohol treatment utilization was positive and statistically significant. Interestingly, the two commentaries published in this journal in response to our meta-analysis, as well as our response to them (11–13), argued that more intensive interventions may be needed to facilitate linkage from medical settings to addiction treatment. Liu and colleagues’ study supports this hypothesis. There is an emerging consensus that our field needs to study more intensive referral to treatment efforts (11–14).

The second point in their commentary argues that our research question necessitates the exclusion of trials that included lower-severity individuals. Our response does not address their second issue; rather, we refer readers back to our meta-analysis, which addressed this issue both analytically in severity-specific subgroup analyses and conceptually within the text (1). Overall, we share Simioni and colleagues’ perspective that brief interventions hold promise (3,4) as methods to link individuals with higher severity to alcohol treatment, but we feel that conclusive

demonstration of these benefits will require additional work as well as potential new modifications to existing brief intervention approaches.

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