Published in final edited form as:

Addiction. 2013 May ; 108(5): 882–884. doi:10.1111/add.12136.

Specifying the Active Ingredients in Evidence-Based Treatments: Setting the Bar Too High?

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Keywords

substance use disorders; behavioral treatment; evidence-based treatment; active ingredients; common factors; mediation

In their thoughtful article, Magill and Longabaugh propose that the current methods for determining whether behavioral treatments for addiction are evidence-based should be modified. They argue that in addition to evidence of efficacy, there should also be evidence that the treatment works the way it is hypothesized to work. More specifically, there should be empirical evidence that the purported active ingredients of the intervention account for its beneficial effects. Here, the emphasis is on the specific interventions, exercises, and contents that are delivered as part of the treatment (i.e., teaching a coping skill, using a ruler technique to assess motivation and confidence), rather than the processes that go on in the client (i.e., changes in self-efficacy or commitment language). The authors assert that without this second requirement, we really do not know how or why an intervention exerts its effect, which makes it difficult to improve it or train clinicians to deliver it.

This argument is persuasive, and it is consistent with the recent trend in the field to move away from a primary focus on efficacy “horse race” trials to a greater emphasis on studies that attempt to increase our understanding of how behavioral treatments initiate and sustain change in substance use (1). This work is all the more important because studies of mediation have indicated that evidence based treatments often do not appear to work through their purported active ingredients (2). In an ideal world, we really should know which specific therapist actions or techniques account for the positive impact of an evidence based treatment.

Despite the appeal of this argument, I have a number of concerns. By attempting to focus on a few active ingredients, we run the risk of oversimplifying what is likely to be a very complicated and multiply determined process. A parallel example might be found in research on genetics. There was originally the hope that we would discover the gene (or at least a small number of genes) that account for particular diseases, thereby opening the door to the discovery of targeted cures. However, now it seems that the vast majority of diseases

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No conflicts of interest

are influenced by a large number of genes, as well as by differences in the ways genes exert or express their influence. Another concern with the recommendation is that it appears to assume that an effective treatment should work through the same set of active ingredients with all patients. There is no allowance for heterogeneity in what makes a treatment work—the very real possibility that different patients react in different ways to different components of an intervention.

The authors note the potential importance of nonspecific, or common factors (3,4), but do not fully consider the implication of findings in this area. What would it mean to the authors' proposal if much of the impact of a behavioral treatment is due to these factors, rather than the purported active ingredients. In addition, the distinction between “active ingredients” delivered by the therapist and within-patient processes such as changes in self-efficacy may be somewhat artificial, in that change can be the result of the interaction of the two participants in the room. For example, the degree to which directness on the part of the therapist leads to better drinking outcomes appears to depend on characteristics of the patient (5).

One issue that is not addressed in the paper concerns how large the active ingredient effect needs to be for the intervention to be designated as evidence based with regard to active ingredients and efficacy. Since small effects can be statistically significant if the sample is large enough, more information on magnitude would be useful. The attainment of statistical significance in the absence of a reasonably large effect size would not be consistent with the argument put forward by the authors.

Finally, the proposal assumes that it is possible to specify how a “talk therapy” intervention works, much like one can specify how an antibiotic stops an infection or how an antihypertensive medication reduces blood pressure. As someone who has been on the giving and receiving side of psychotherapy, my experience has been that change often comes in unexpected ways, and from attempts to intervene that did not go according to plan. The literature on sudden gains in the treatment of depression provides a good example of how treatment can go along with little sign of progress, until marked improvements are achieved following one session (6,7). Therefore, my concern is that proposal by Magill and Longabaugh sets the bar too high for the designation of evidence based, and is overly optimistic about our ability to identify the active ingredients in what is really a very complicated, unpredictable, and messy process.

Acknowledgments

This research was supported by K24 DA029062 from the National Institute on Drug Abuse and P01-AA016821 from the National Institute on Alcohol Abuse and Alcoholism. Additional support was provided by the Medical Research Service and the SUD QUERI of the Department of Veterans Affairs.

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