Reviewer's report

Title: Planning and problem-solving training for patients with schizophrenia: A randomized controlled trial

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Reviewer: Matthew Kurtz

Reviewer's report:

Thank you for the opportunity to review this interesting paper by Rodewald et al. entitled “Planning and problem-solving training for patients with schizophrenia: A randomized controlled trial.”

This is a well-conducted behavioral intervention study investigating the effects of a program of planning and problem-solving training to the effects of a condition in which clients received training in elementary neurocognition. The sample size is substantial for this type of study, the interventions as described are innovative (but see comments below on descriptions), the outcome measures, while somewhat limited in scope, are relevant. In particular, as noted by the authors, the comparison of two alternative strategies for remediating cognitive impairments is unique and important. I have comments designed to improve the flow of the paper, I propose some modifications of the organization of hypotheses to increase the focus of the paper on treatment mechanism, and suggest ways to place the results in a broader context with respect to the larger literature.

Abstract

Remove “a” after “whether” in sentence 1. Methods: I think the two hypotheses should be of equal status—I don’t think the planning outcome measures should be part of a “secondary hypothesis”—you can’t (or shouldn’t) have improvement in one without the other. Results: I don’t think it is accurate to say improvement on one outcome measure does not “generalize” to another, similar outcome measures. Rather the training in the intervention does or does not generalize to outcome measures different from the training itself.

Introduction

1. To this reader it is unfair to describe what the authors label “the first-generation” of cognitive remediation studies as lacking theoretical motivation. The Benedict et al. study was motivated by data-driven theories about information processing difficulties in schizophrenia as measured by the degraded stimulus CPT. Also, the authors are encouraged to look at and cite even earlier cognitive training studies by Meichenbaum, Koh and others for a richer view of the history of this work.

2. Could the authors describe the problem-solving training in Medalia et al (2000) in more detail so that it can be compared to the current intervention? I believe the two methods are quite different in approach.
3. Almost as many neurocognitive deficits as have been identified in scz have also been linked to different aspects of outcome in schizophrenia. The authors need to make a stronger argument that there is something unique about planning and problem-solving, relative to other cognitive functions, as a predictor of outcome in scz.

4. There is almost no detail on the elementary cognitive training package. Was task-difficulty manipulated? Can the authors be more explicit on the details of the domains trained in the Introduction? Is there any data suggesting that clients got better on the tasks themselves (if so, these data should be presented in the results)?

5. I think the two hypotheses should be of equal stature, with the effects on the more proximal outcome stated first (planning ability) and the effects on functional capacity second.

Discussion

1. The results of this study do not support the idea that cognitive training enhances work therapy programs –it is just as possible that work therapy would have produced similar improvements in the absence of any cognitive training.

2. Also, can the authors consider the possibility that the work therapy may be producing some of the gains in cognitive function evident in both groups in this study?

3. I think the findings that the planning training produced changes in the planning outcome measure should be emphasized as it suggests that the intervention was effective at improving planning abilities. This finding is of crucial importance. If available it would be nice to see a similar effect for the cognitive training package to show that it worked.

4. The authors should also note that generalization of training effects to distal outcome measures, like work function, has not been documented in recent large-scale studies (see Dickinson et al., 2010).

5. In comparing their results to those of Medalia could the authors compare the training packages? To what degree do they rely on compensa

Level of interest: An article of importance in its field

Quality of written English: Needs some language corrections before being published

Statistical review: No, the manuscript does not need to be seen by a statistician.

Declaration of competing interests:
I declare I have no competing interests