

Challenging Hofmann's negative thoughts: A rebuttal

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Abstract

In his commentary on Longmore and Worrell [Longmore, R., & Worrell, M. (2007) Do we need to challenge thoughts in Cognitive Behavioural Therapy? *Clinical Psychology review*, 27, 173–187], Hofmann [Hofmann, S. G. (2008-this issue). Common misconceptions about cognitive mediation of treatment change: A commentary to Longmore and Worrell (2007). *Clinical Psychology Review*, doi:10.1016/j.cpr.2007.03.003] argues that the paper shows a range of errors, biases, and misconceptions that have led to incorrect interpretations of the literature and general conclusions. Hofmann discusses recent developments in the methodological criteria for mediation studies in support of his contentions. In this rebuttal it is demonstrated that Hofmann has in fact misrepresented the primary focus and arguments of Longmore and Worrell (2007). Additionally, it is shown that the arguments and research findings that Hofmann discusses do not constitute a significant challenge to the conclusions drawn as a result of the original review.

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1. Introduction

We would like to thank Dr. Hofmann for showing interest in our paper sufficient to produce his commentary. It is gratifying that our review might stimulate what we consider to be a necessary and timely debate regarding the current empirical status of Cognitive Behavioral Therapy (CBT).

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In his commentary on Longmore and Worrell (2007), Hofmann (2008-this issue) suggests that due to a biased review of the literature and incorrect interpretation of findings, the authors have questioned the validity of the CBT model. He further suggests that the authors promote what have become known as ‘third wave’ models as an alternative to CBT. Through a discussion of current developments in the methodology for establishing mediation, Hofmann seeks to demonstrate the inaccuracy of the central conclusions made in Longmore and Worrell (2007). In this brief rebuttal, we demonstrate that Hofmann has misrepresented the central arguments of our original paper and has failed to provide either robust data or reasoned argument that would constitute a challenge to the conclusions drawn therein.

In responding to Hofmann’s criticisms, it is important to restate the central aims, findings and arguments of our original paper. Longmore and Worrell (2007) sought to examine the central question: “Do we need to challenge thoughts in cognitive behavior therapy?” A fundamental postulate of the cognitive behavioral model is that cognitive change is central to treating psychological distress. Further, the modification of maladaptive cognition is held to be both the process by which therapy is effective as well as the mechanism of change more generally. As well as distinguishing CBT theory, the use of strategies explicitly designed to modify dysfunctional cognitions defines what CBT therapists spend much of their time actually doing in treatment sessions. The use of logical, “rationalist” strategies to directly challenge and modify cognition has been subject to critical reappraisal from a variety of sources. In our review of the literature we focused on three “empirical anomalies” in the CBT outcome literature identified by Hayes, Follette, and Linehan (2004). First, component analyses have not shown that cognitive interventions provide added value to the therapy. Second, CBT treatment is often associated with a rapid, early improvement in symptoms, which most likely occurs before the implementation of any distinctive cognitive techniques. Third, measured changes in cognitive mediators have not been demonstrated to precede changes in symptoms. Our paper provided an empirical review of the status of cognitive interventions in CBT through an examination of these apparent anomalies.

2. Component analysis studies

Our paper provided a thorough review of CBT component analysis studies, as these provide evidence regarding the effectiveness of specific CBT interventions. On the basis of the amount and quality of data available, we concluded that for a range of clinical problems, specifically cognitive interventions have not been shown to produce superior outcomes or indeed to provide “added value” to the behavioral components of CBT.

Hofmann’s commentary on this aspect of our paper makes two fundamental errors. First, he erroneously states that we have questioned the validity of the cognitive behavioral treatment model, instead promoting third wave models as an alternative. Our conclusion that there is a lack of research evidence supporting the specific role of cognitive interventions is not equivalent to stating that the CBT model is invalid. Indeed, in respect of component analyses, we suggested three *possible* conclusions as follows:

1. That there simply have not been a sufficient amount of studies of the required quality undertaken to allow the true potency of the specifically cognitive interventions to be shown.
2. That the frequent findings of equality of outcome between conditions in the studies may provide evidence that common, non-specific therapy factors underpin clinical improvement.
3. Consistent with the conclusions drawn by Borkovec, Newman, Pincus, and Lytle (2002), it is possible that the different components of CBT examined in component analysis studies work on different systems, such that producing change in one system (for example, the behavioral system) will produce changes in other systems (such as the cognitive system). If this last possibility was the case, we argued, then it would not be surprising that component analysis studies show equal outcomes for behavioral and cognitive interventions.

Second, Hofmann (2008-this issue) states that our review of component analysis studies is designed to “answer the question of treatment mediation.” This is not the case. Our paper distinguishes between cognitive intervention as *procedure* in CBT and cognitive modification as the proposed *mechanism* of change. We attempt to examine both of these postulates of the CBT model. The purpose of our review of component studies was to examine the status of current empirical evidence regarding the effectiveness of cognitive interventions as *procedure* in the practice of CBT. Insofar as Dr. Hofmann interprets our paper as solely concerned with the matter of mediation, he reduces and distorts our arguments and conclusions.

3. Rapid early onset

Our paper considered the research evidence bearing upon the rapid early onset debate. This debate has arisen from findings suggesting that symptomatic improvement in CBT often occurs before the key specific cognitive interventions have been adequately implemented. In our discussion of this literature we concluded that, on balance, the evidence for the early rapid response phenomenon is not compelling. [Hofmann \(2008-this issue\)](#) states that it is possible to assume, as a number of authors have, that sudden gains may be the result of cognitive interventions. As our conclusion was that the evidence for the phenomena of sudden gains was not compelling, it is difficult to see how Hofmann's objections constitute a challenge to our conclusions. Rather than assumptions of the role of cognitive interventions in producing rapid gains, what is needed is robust research evidence demonstrating both sudden gains and that this can be shown to be the result of specific cognitive interventions. [Hofmann's \(2008-this issue\)](#) commentary fails to provide such evidence.

4. Cognitive mediation

In his commentary, Hofmann has not addressed our central concern: the status of research evidence supporting the specific role of therapeutic strategies to directly challenge cognitions. Rather his commentary focuses on recent developments in the methodological requirements to establish mediation in psychotherapy research. However, rather than constituting a challenge to our findings or arguments, Hofmann's suggestions are in fact consistent with our own call for further and more rigorous analyses of the different components of cognitive behavioral interventions.

[Hofmann \(2008-this issue\)](#) provides a brief discussion of the criteria for establishing mediation, proposed by [Kraemer, Wilson, Fairburn, and Agras \(2002\)](#). Further, he discusses the study reported by [Hofmann \(2004\)](#) as promising, due to it's meeting a number (although not all) of these criteria. [Johansson and Høglend \(2007\)](#) have also provided a detailed and informative discussion of models for establishing mediation in psychotherapy research. When discussing the extent to which [Hofmann's \(2004\)](#) study follows the criteria set by [Kraemer et al. \(2002\)](#), [Johansson and Høglend \(2007\)](#) conclude that this study failed to provide definitive evidence of mediation. In brief, while the correlational analyses showed that an early increase in the proposed mediator ("estimated social cost") predicted later changes in the outcome variable, the possibility remained that the outcome measure in fact changed *before* the mediator. In our own review, we identified methodological issues with the [Hofmann \(2004\)](#) study, specifically a sub-optimal use of exposure, which in our analysis introduced significant weaknesses into its design and the conclusions that could be drawn from the results obtained.

In our review of the available literature we concluded that the evidence that cognitive variables mediate therapeutic change in CBT is somewhat limited. As such, we also concluded that an important element of the rationale for specific cognitive interventions – that changes in cognition mediate change in CBT – currently lacks empirical support.

[Johansson and Høglend \(2007\)](#), in their review of the research on mediation across different forms of psychotherapy, have concluded that, as yet, no causal mediator has been satisfactorily demonstrated. From our own review of the literature this is also the case for CBT. Contrary to Hofmann's misrepresentation of our paper, we have not argued that this indicates that the cognitive behavioral model, or rather the range of different models that belong to the family of cognitive behavioral therapies, have been found to be invalid. Thus, in our view, Hoffman has failed to demonstrate that the conclusions we have drawn are expressive of the effects of bias or the result of a lack of education about mediation. Additionally, Hoffman has failed to provide rigorous data that would prompt a reconsideration of our conclusions based upon the research findings to date.

Further research on mediation, following the recommendations of [Kraemer et al. \(2002\)](#), may well provide good evidence for the mediating role of cognitive change. It is also possible that such evidence will remain elusive. Rather than suggesting that the CBT model be abandoned however, we have called for renewed interest, debate and research on these fundamental questions. [Hofmann \(2008-this issue\)](#) concludes that the precise mechanisms of cognitive change are often not known and that innovative ideas are needed and with this we would certainly agree. Additional research on mediation in CBT is certainly needed. Hofmann, however cautions against the adoption of what he refers to as "trendy approaches" such as those that have been categorized as 'third wave CBT.' While we would agree with him that such approaches need to be subject to empirical test, it is equally important to keep an open and critical mind to the novel hypotheses and strategies being proposed by these theorists and others within the family of cognitive behavioral therapies.

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