Author's response to reviews

Title: A Mediation Model of Mindfulness and Decentering: Sequential psychological constructs or one and the same?

Authors:

Judith Gecht (j.gecht@psych.rwth-aachen.de)
Ramona Kessel (rkessel@ukaachen.de)
Thomas Forkmann (tforkmann@ukaachen.de)
Siegfried Gauggel (sgauggel@ukaachen.de)
Barbara Drueke (bdrueke@ukaachen.de)
Anne Scherer (ascherer@ukaachen.de)
Verena Mainz (vmainz@ukaachen.de)

Version: 4 Date: 27 June 2014

Author's response to reviews:

Dear Editor, dear Reviewers

First we wish to thank you for giving us the opportunity to submit a revised version of our manuscript entitled "A mediation model of mindfulness and decentering: sequential psychological constructs or one and the same?" to BMC Psychology.

We believe the comments have been very constructive and helpful and elaborating on these comments has contributed to improving the manuscript.

The changes in the manuscript are highlighted by using “track changes” in MS Word. Below we have responded to each of your concerns and have outlined the changes made in the revised manuscript.

Sincerely

Judith Gecht

Editor's request:

Please update your ethics statement to include the name of the ethics committee that approved your study.

Comment to the Editor
We have updated our ethics statement now including the name of the ethics committee, i.e., local ethics committee of the Medical faculty of RWTH Aachen University, that approved our study (see the title page, lines 24/25; page 8, line 182; page 25, lines 630/631).

Reviewer: Marjan Nijkamp

Reviewer's report:

Discretionary Revisions

1. As stated in the introduction of the manuscript, the aim of the study was to clarify the relationship between mindfulness and decentering. A recent study by Hayes-Skelton and Graham (2013) published in Behavioural and Cognitive Psychotherapy aimed for the same clarification. They applied their research question to social anxiety (as compared to depressive symptoms in the current study). I suggest the authors to include and discuss this relevant publication in the manuscript: * Hayes-Skelton, S. and Graham, J. (2013). Decentering as a common link among mindfulness, cognitive reappraisal, and social anxiety. Behavioural and Cognitive Psychotherapy, 41: 317-328.

Comment to the Reviewer

We followed the reviewer's suggestion and included the study of Hayes-Skelton and Graham (2013) on page 5 line 122 ff.. We refer to this study in the context of the conflicting findings regarding the empirical support for the mediation model of mechanisms of mindfulness proposed by Shapiro, Carlson, Astin, and Freedman (2006).

2. Forty-eight participants in this study scored above 12 on the DESC, reflecting a possible depressive episode. Did the study team refer these students to a psychologist for counseling? No comments were made with respect to these vulnerable respondents.

Comment to the Reviewer

The questionnaires were administered anonymously within the context of university lectures. Therefore, it was not possible to contact students with a DESC cut-off above 12 personally to refer them to a therapist. However, the results of our study regarding sample descriptive data have been communicated to the professor who gave us the opportunity to collect data in his lectures and who is responsible for the students in question. Furthermore, all students were well informed that within the RWTH Aachen University Hospital the “Center for Mental Health for Undergraduate and Postgraduate Students (ZPG)” gives
students the possibility to ask for support when they encounter emotional crises or psychological problems (see page 13, lines 324-330).

Major Compulsory Revisions

3. No information is given about the response rate of the study (only a given n=495). This number is quite low, especially regarding the complexity of the tested model. Furthermore, one should be cautious in generalizing the results of this study among higher educated and mostly Caucasians (no information is given about ethnic background) university students. Please elaborate on this limitation in the Discussion.

Comment to the Reviewer

We agree with the reviewer to give information about the response rate and have included this aspect in the manuscript (see page 8, lines 176-180; page 13, lines 319-321). Before the start of the university lecture a questionnaire was handed out to every student entering the lecture hall. In exchange for a small token, the students returned the completed questionnaires directly after the lecture when leaving the lecture hall. By this procedure we were able to ensure that all eligible students had the possibility to participate in our study as well as to check the response rate: one student refused participation beforehand, the number of distributed questionnaires equaled the number of returned ones (N=565) and during data entry no uncompleted questionnaires were found. From the total sample 70 cases were excluded due to missing values on items of the EQ-D (Gecht et al., 2014), the KIMS-Short (Hoefling, Stroehle, Michalak, & Heidenreich, 2011), or the DESC (Forkmann et al., 2009). The subscales of these questionnaires consist of a relatively small number of items. Therefore, we did not impute missing values but eliminated the cases with missing values instead. The remaining sample consists of 495 undergraduate university students.

Regarding the reviewer’s comment on sample size, we regard a sample of 495 participants large enough to conduct structural equation modeling (SEM). Former studies have conducted SEM with samples of comparable size (Mercer, Neumann, Wirtz, Fitzpatrick, & Vojt, 2008; Vauth, Kleim, Wirtz, & Corrigan, 2007; Zwingmann, Wirtz, Müller, Körber, & Murken, 2006). A rule of the thumb is to have at least a sample size of 100 participants for simple models. More precisely, a difference is made regarding the cut-off values for the fit-indices for sample sizes lower or higher than 250 (Hair, Black, Babin, & Anderson, 2010).

We agree with the reviewer’s concern that it would be good to give information on the ethnic background of the participants in the paper. Regarding the ethnic background of the study sample, we inquired the participants’ first language and for how long they live in Germany. No further information on this issue was collected. From the 495 students, there were eight students whose first language was not German and who lived in Germany for less than 5 years (see page 13/14, lines 330-334). Based on the Mahalanobis distance statistic
(Kline, 2011), none of these persons was identified as an outlier on any of the variables in the mediation model. However, regarding the restricted nature of the present study sample, in the limitations section, we address this issue and recommend replicating the present study within a different, more heterogeneous sample.

4. Concerning the statistical analysis, the analyses seem to be correct. However, it is not clear why besides Mplus, AMOS and SPSS are used additionally to estimate the paths in the model and calculate the effect sizes. In my knowledge Mplus could be sufficient in reporting the necessary data. It could simplify the analyses and improve the readability of the text.

Comment to the Reviewer

The reviewer is right to address the question why Mplus as well as AMOS have been used. Actually, we have performed all the SEM-analyses with Mplus. However, to get a deeper understanding of the data and to gain quick insight into the relationships the more “user-friendly” AMOS was used in addition. Please note that because Mplus is the more advanced statistic package we performed all analysis that are reported in the manuscript with Mplus. Thus, in the manuscript we have deleted reference AMOS.

However, whereas Mplus gives estimates about the absolute effect-sizes and paths between indirect and direct effects etc. in the model, the procedure recommended by Fairchild, MacKinnon, Taborga, and Taylor (2009) provides information about the relative magnitude of the effect-sizes, indicating information about the practical importance of the respective mediator besides its statistical significance. This is not possible with Mplus so that the respective SPSS Macro provided by Fairchild, MacKinnon, Taborga, and Taylor (2009) was used instead.

5. The methods as well as the results sections of the study are presented in great detail (total word count over 6000), the authors could limit their text (and improve the readability) by referring to the revealing tables and Figure 1. With regard to Figure 1 I suggest to present Mindfulness and Decentering as the concepts in relation to Depressive symptoms, instead of only presenting the subscales in relation to depressive symptoms. In this way it is easier to compare the analytic results with for example the former mentioned study by Hayes-Skelton and Graham (2013).

Comment to the Reviewer

We agree with the reviewer that the results are presented in a very detailed manner and we share the concerns about the length of the manuscript. However, SEM and the theoretical background to the estimation of the different $R^2$-measures is a quite advanced method not every reader might be familiar with. Therefore we have decided to present the results in a rather detailed fashion in order to provide all readers with the necessary information to understand the
analyses and results.

We considered the reviewer’s suggestion to present the latent constructs “depression” and “mindfulness” rather than the respective subscales in Figure 1. However, we decided not to collapse the subscales in this figure. Detailed psychometric analyses for both the KIMS and the EQ-D have identified separate subscales representing each a unique amount of variance and showing sufficient factor and indicator reliabilities for the subscales to refer to them as single constructs (Gecht et al., 2014; Hoefling et al., 2011). These earlier studies have clearly underlined the multidimensional nature of the concepts mindfulness (Baer, Smith, & Allen, 2004; Baer, Smith, Hopkins, Krietenmeyer, & Toney, 2006) and decentering (Gecht et al., 2014). Thus, it is psychometrically not advised to pool the mean/sum scores of the different subscales into a single variable.

Minor Essential Revisions

6. A review of the linguistic style by a native speaker is recommended before (re)submission.

Comment to the Reviewer

A native speaker who is a post-doctoral research associate at the University of Cambridge, UK, has reviewed the manuscript for language problems and has performed some language corrections to improve the quality of the linguistic style of the manuscript. With regard to the readability of the manuscript these changes are not highlighted in the text.

Reviewer: Roger Hagen

Reviewer’s report:

The paper: “A mediation model of mindfulness and decentering: Sequential psychological constructs or one of the same?” tries to explore how the concepts of mindfulness and decentering is related to each other. This is an important research question since mindfulness approaches in psychological treatments have been widely used, but there seem to be less research to the question why mindfulness works, and therefore is mediation analyses warranted in order to gain more knowledge related to this important issue. The manuscript is well written and both the method and statistical analyses in order to explore the research question are well described and adequately. Although the paper is interesting to read I would suggest some suggestions for some minor revisions, which are:

1. There are some problems related to the definition of mindfulness in the literature. The concept of detached mindfulness should be mentioned in the introduction since this concept seems to include both decentering and
mindfulness.

Comment to the Reviewer

We agree with the reviewer that the concept of detached mindfulness (Wells & Matthews, 1994; Wells, 2005) is an important construct in the discussion of the relationship between mindfulness and decentering. We have extended our description of conflicting studies regarding whether it should be referred to these concepts as two different ones or to one and the same and have included the topic of detached mindfulness within this discussion (see page 6, lines 134-142). Additionally, we have referred to Well’s “Metacognitive Therapy” (MCT; Wells, 2000) in the context of clinical interventions including mindfulness-related techniques (see page 3, lines 59).

2. The use of a healthy sample should me mentioned as a major limitation in the manuscript. It could be that the processes explored in the manuscript would be different in a sample of depressive patients compared to a healthy sample. This should be mentioned under limitations.

Comment to the Reviewer

We followed your suggestion to include the possibility that different results could be obtained within a sample of depressive patients as compared to a healthy sample and have added this point in the limitations section (see page 24, lines 588-589).

3. The concept of detached mindfulness should also be mentioned in the discussion part of the paper.

Comment to the Reviewer

We agree with the reviewer regarding this concern. When discussing in which way the identified relationship between mindfulness and decentering fits into the theoretical background we state that our findings are supportive for the concept of detached mindfulness (see page 19, lines 478-480).

In addition (see page 24, lines 601-606), we recommend to explicitly investigate the relationship between detached mindfulness (Wells, 2005) and the different subdomains of the multidimensional construct mindfulness (i.e., Baer et al., 2006) in future research.

When this is said, I found the paper interesting to read, the research questions are well defined and the conclusions drawn from the results are well balanced. It could be of interest for the reader to know how the findings of the study could be translated into clinical advices. The paper should be published after a minor revision.
Reviewer: Stefan Sütterlin

Reviewer’s report:

This manuscript investigates the actual meaning and interrelationship of psychological constructs which have become popular in psychological science over recent years. Despite the growing number of studies investigating the effects of mindfulness, the definition and boundaries of this construct has not been sufficiently investigated, leading to sometimes fuzzy or unclear discussions and theoretical interpretations of study results.

In this manuscript the authors address a fundamental question in this context, the relationship between two concepts that have been both considered to be distinct or identical in controversial discussions.

The authors define the problem clearly and accurately, apply an appropriately chosen method on a decent sample size. I consider the strengths of this manuscript to be the thorough theoretical introduction and the carefully conducted statistical analysis and interpretation. From my opinion, the authors do a very good job on explaining the SEM analysis both precise and understandable also for readers less familiar with this advanced method.

What I am missing in this manuscript and would thus recommend as being part of a (compulsory) revision is a more thorough explanation/discussion of previously conflicting research results on this matter. As a reader of the introductory part of this manuscript I learn about the (seemingly?) contradictory findings of Shapiro et al and Carmody et al. However, at this stage it doesn't become clear enough yet why "one more study" can contribute to a preliminary or final answer to this question. The authors are careful suggesting no specific predictions/hypotheses for their study. I would like to see a clearer rationale why this study contributes more than just "one more result", i.e., what is the unique and superior contribution of THIS particular study - the methodological approach or other design-related advantages? The authors might reflect a bit more on the specifics of the conflicting studies and state which other studies besides the two mentioned contribute to this conflict. I consider these theoretical conflicts between researchers or research groups as potentially fruitful and which to be given a clearer overview and how the present manuscripts fits into this landscape. I can see such an attempt in the discussion section on pp. 22f, lines 544ff (multidimensionality), but think that the considerations that have been made prior to the study should be made clearer and will help the reader to assess the relevance and individual contribution of this paper better.

This being said, I think more manuscripts of this kind, applying advanced and appropriate statistical methods for further validation of frequently used and seemingly understood constructs are needed.
Comment to the Reviewer

We agree with the reviewer’s suggestion to describe more explicitly the previously conflicting research giving the objective to conduct our study (see pages 5/6, lines 119-142). In addition to the articles of Shapiro, Carlson, Astin, and Freedman (2006) and Carmody, Baer, Lykins, and Olendzki (2009), we have included the studies of Hayes-Skelton and Graham (2013) and refer to the work of Wells and others (e.g., Wells & Matthews, 1994; Wells, 2005). The study of Hayes-Skelton and Graham (2013) regards decentering as a working mechanism of mindfulness. However, they analyzed the relationship based on one-dimensional concepts. Wells and others (e.g., Wells & Matthews, 1994; Wells, 2005) introduced yet another approach to decentering and mindfulness, i.e., “detached mindfulness”, which is a specific form of mindfulness that includes aspects of mindfulness as well as decentering.

Furthermore, we have pointed out methodological advances of the present study compared to former research, i.e., the use of different questionnaires to measure decentering and mindfulness multidimensionally as well as the application of advanced methods to estimate the importance of the specific effect-sizes of the different paths within the mediation model (see page 7, lines 156-164).

Reference List


