PEER REVIEW HISTORY

BMJ Open publishes all reviews undertaken for accepted manuscripts. Reviewers are asked to complete a checklist review form (http://bmjopen.bmj.com/site/about/resources/checklist.pdf) and are provided with free text boxes to elaborate on their assessment. These free text comments are reproduced below.

ARTICLE DETAILS

| TITLE (PROVISIONAL) | Mindfulness- and acceptance-based programmes in the treatment of obsessive-compulsive disorder: A study protocol for a systematic review and meta-analysis |
|---------------------|----------------------------------------------------------------------------------------------------------------------------------|
| AUTHORS             | Bürkle, Johannes; Fendel, Johannes; Schmidt, Stefan |

VERSION 1 – REVIEW

| REVIEWER             | Anne Speckens  |
|----------------------|----------------|
| Radboudumc Instituut voor Wetenschappelijk Onderwijs en Opleidingen, Psychiatry |
| REVIEW RETURNED      | 14-Mar-2021    |

| GENERAL COMMENTS     | This is a protocol paper about an intended systematic review and meta-analysis of mindfulness- and acceptance based interventions for patients with obsessive compulsive disorder. |
|----------------------|----------------------------------------------------------------------------------------------------------------------------------|
| Abstract             | As the review concerns both effectiveness and potential moderators, I think the moderators should be pre-specified, also in the abstract. The authors do not use any language restrictions. In the paper they do not explain how they will deal with the inevitable translations. I would add that in the paper. With regard to the inclusion, the authors include both RCTs, but also non-randomised controlled trials and uncontrolled studies. I would suggest to meta-analyse both the randomized and the non-randomised trials and report on them separately. See previous meta-analyses on MBIs (Piet & Hougaard, 2011; Khoury et al. 2013). The authors only mention pre-post changes as outcomes, but they do intend to include follow-up assessments as well. I would mention this here as well. |
| Strengths and limitations | As the authors intend to provide different estimates for different MABPs, such as MBCT and ACT, they should prespecify these different kinds of MABPs. |
| Introduction         | The manuscript should be checked for both language and typing errors. The authors identify “techniques that promote tolerance towards unpleasant thoughts and emotions” as interventions that may address the limitations of classical treatment. For this reason, they should include previous CBT as a possible moderator to be able to look closer into this relationship. “Treatment seeking adults with OD report lower trait mindfulness”. It is impossible to know for the reader whether this might be a cause or a consequence. Higher severity of OCD might also result in lower |
trait mindfulness rather than the other way round. The same applies to “Higher self-reported mindfulness being associated with lower symptom severity and higher distress tolerance in OCD patients.” Please nuance this in the introduction.

Objectives
In accordance with the remarks above, please prespecify moderators of the effect.

Methods
Patients will be included who were diagnosed with OCD by means of a validated diagnostic interview, validated self-report or by an expert clinician based on DSM- or ICD-based criteria. I would have serious doubts whether the latter two are valid ways to diagnose OCD, so would advise against using them. The criteria to include interventions, i.e. “programmes explicitly based on mindfulness and/or acceptance” is too vague. Please prespecify the different interventions you would like to look at separately and carefully define them. I would recommend to only include protocolized interventions, such as MBCT, MBSR, ACT and DBT.
Outcome: please mention here that you do not only include pre-post intervention but also follow-up assessments.
The authors intend to perform a separate meta-analysis on RCTs using between-group data. As they will also collect data on non-randomised controlled studies, I would recommend to include both in the meta-analysis and report on them separately.
The authors plan to aggregate studies with ERP as control condition separately to estimate the potential increment of MABPs and MABPs in combination with ERP over ERP alone. I would recommend to separate the different control conditions in WL/TAU, non-specific control conditions, and specific/evidence-based control conditions, like previous meta-analyses on MBIs have done. You can always look at previous CBT as a possible moderator of treatment efficacy.
See previous remark about how you are going to deal with the different languages.

Data extraction
With regard to the type of control used in the study, please specify this.
With regard to the population, please specify prior therapy. Are you going to differentiate between behavioural and cognitive therapy for OCD?
With regard to intervention: are you going to look at whether the intervention was protocolized, whether the therapist were qualified (professional, mindfulness training), whether the adherence to the protocol was assessed? Whether the intervention was a group or individual treatment? Not all of them will be group treatments.
With regard to outcomes: here the authors do mention follow-ups, whereas earlier in the protocol they do not.

Data synthesis
See previous comments on including the non-randomised controlled studies in the between-group meta-analysis. And on the definitions of the different programmes and pre-specification of moderators.
The authors also mentioned earlier they would like to include comorbid depression as a possible moderator. How will they define that? Also by a structured diagnostic interview?
Relevance
The authors conclude that if MABPs prove to be effective, they should be considered to be incorporated in OCD treatment and recommended as low-threshold therapeutic approach for treatment seeking individuals. Staging of MABPs is an important issue, are they more helpful as a low-threshold therapeutic approach or as a sequential treatment to prevent relapse (such as in recurrent depression), or as a supplementary treatment in those who have not sufficiently benefitted from CBT? It would be very helpful if the review/meta-analysis could shed some more light on this.

REVIEWER
Michael H. Bloch
Yale Univ
REVIEW RETURNED
03-Apr-2021

GENERAL COMMENTS
This manuscript is a protocol for a systematic review and meta-analysis examining the efficacy for mindfulness-based interventions for OCD. There has been increasing research in this area and a systematic review and meta-analysis in this area seem important. I have several suggestions to improve this protocol in revision:
1. Y-BOCS should be the primary outcome to assess OCD severity. This is a widely used scale in the field. Given that there is only one scale used to assess outcome weighted mean difference rather than standized mean difference would be the preferred outcome.
2. The justification for including uncontrolled trials is poor in the current protocol. I would recommend limiting the review to RCT.
3. There are several issues with study design that are not addressed in the current protocol ...
   a. how are RCT going to be compared across different control conditions (MAC vs waitlist)
   b. how are studies going to be compared that add mindfulness to existing CBT protocols versus those that involve only mindfulness.
   c. defining what constitutes a mindfulness-based therapy for the meta-analysis would be useful
4. English spelling should be checked throughout the manuscript. There are currently several spelling errors.

VERSION 1 – AUTHOR RESPONSE

| Element | Peer reviewer comment | Author response | Changes |
|---------|-----------------------|-----------------|---------|
| #1 Comments to the Author | This is a protocol paper about an intended systematic review and meta-analysis of mindfulness- and acceptance based interventions for patients with obsessive compulsive disorder. | | |
| #2 | Abstract | As the review concerns both effectiveness and potential moderators, I think the moderators should be pre-specified, also in the abstract. | Thank you! We agree. In addition to the already pre-specified moderator of the specific programme, we included other pre-specified moderators in the abstract. | see Abstract

“We will explore moderators and sources of heterogeneity such as the specific programme, study design, changes in depressive symptoms, hours of guided treatment, control condition, and prior therapy (e.g., CBT) using meta-regression and subgroup analyses.” |

| #3 | The authors do not use any language restrictions. In the paper they do not explain how they will deal with the inevitable translations. I would add that in the paper. | We agree and explain how we deal with translations now. | see 2.2 Search strategy

“[…] will be screened, without restrictions for language or publication date [52,53] We will translate articles in foreign languages with the help of neural machine translation.” |

| #4 | With regard to the inclusion, the authors include both RCTs, but also non-randomised controlled trials and uncontrolled studies. I would suggest to meta-analyse both the randomized and the non-randomised trials and report on them separately. See previous meta-analyses on MBIs (Piet & Hougaard, 2011; Khoury et al. 2013). | Thank you for your suggestion and we totally agree. We expect to obtain the most valid effect estimate from the between-group analysis of the RCTs. Therefore, as you suggest, we will meta-analyse RCTs separately. Moreover, we plan to assess the within-group pre-post effect of all eligible studies (including randomised and non-randomised studies) and include the study design as moderator for subgroup analysis. In this way, we will obtain separate | See Abstract

“We will include both controlled and uncontrolled trials. Randomised controlled trials (RCTs) will be meta-analysed separately assessing between-group effects. A second meta-analysis will assess the within-group effect of all eligible studies.” |
effect estimates for all kinds of study designs. Moreover, other potential moderators of the MABPs effectiveness can be investigated in the largest possible data base (see Khoury et al. 2015 for a similar approach).

| #5 | The authors only mention pre-post changes as outcomes, but they do intend to include follow-up assessments as well. I would mention this here as well. | Thank you! We agree and included follow-up assessments. | see Abstract |

> "[…] using meta-regression and subgroup analyses. We will perform sensitivity analyses using follow-up data."

| #6  | Strengths and limitations | We agree. Prior to the final literature search, we cannot conclusively determine for which MABPs we will have sufficient data. We are confident to perform subgroup analyses for ACT and MBCT. If possible, we will be happy to look at other MABPs individually in terms of their effectiveness. | see Strengths and limitations |

> "We separately provide effect estimates for different MABPs, namely mindfulness-based cognitive therapy (MBCT) and acceptance and commitment therapy (ACT)."

| #7  | Introduction | The manuscript should be checked for both language and typing errors. | We and an external native speaker double checked the manuscript for |

We and an external native speaker double checked the manuscript for
| #8 | The authors identify “techniques that promote tolerance towards unpleasant thoughts and emotions” as interventions that may address the limitations of classical treatment. For this reason, they should include previous CBT as a possible moderator. | Thank you very much for this suggestion. We agree and included prior therapy (including CBT) as possible moderator. | see Abstract |
| --- | --- | --- | --- |
|  | “We will explore moderators and sources of heterogeneity such as the specific programme, study design, changes in depressive symptoms, hours of guided treatment, control condition, and prior therapy (e.g., CBT) using meta-regression and subgroup analyses.” | see Abstract |
|  | “We will explore moderators and sources of heterogeneity such as the specific programme, study design, changes in depressive symptoms, hours of guided treatment, control condition, and prior therapy (e.g., CBT) using meta-regression and subgroup analyses.” | see Abstract |
|  | “We will extract information on […] (2) the population: sample size (treatment/control), dropout rate, mean age, sex ratios, mean duration of illness, prior therapy (e.g., CBT); […].” | see 2.4 Data extraction |
|  | “[…] we prespecify subgroup analyses on the influence of the specific programme, the study design, and prior therapy (e.g., CBT).” | see 2.7 Data synthesis |
|  | “Treatment seeking adults with OD report lower trait mindfulness”. It is impossible to know for the reader whether this might be a cause or a consequence. Higher severity of OCD might also result in lower trait mindfulness rather than the other way round. The same applies to “Higher….” | Thank you for your recommendation! We agree and clarified the correlational relationship between the constructs. | see 1.1 Rationale |
|  | “Consequently, mindfulness and acceptance may reduce the perceived importance of intrusive thoughts, thereby reduce anxiety and the urge for compulsive behavior.[15,27] Accordingly, correlative findings suggest that adults with OCD seeking treatment report lower trait mindfulness compared to non-clinical controls.[29] Higher self-reported mindfulness is associated with lower symptom severity and higher distress tolerance in patients suffering from OCD.[30] Acceptance of present-moment experience is associated with increased willingness to experience intrusive thoughts.[31] The ability to non-judgmentally accept thoughts and….” | see 1.1 Rationale |
self-reported mindfulness being associated with lower symptom severity and higher distress tolerance in OCD patients." Please nuance this in the introduction.

emotions predicts the reduction of OCD symptom severity after CBT.[32] Thus, mindfulness and acceptance might be considered as a prerequisite and as an amplifier of ERP and inhibitory learning.[14] However, a causal effect of increased mindfulness and acceptance on the reduction of OCD symptoms can only be shown in studies that aim to manipulate mindfulness and acceptance."

| #10 Objectives | In accordance with the remarks above, please prespecify moderators of the effect. | We agree and pre-specified other moderators of the effect. See #2 | see Abstract |
|----------------|----------------------------------------------------------------------------------|----------------------------------------------------------------------|---------------|
|                | "We will explore moderators and sources of heterogeneity such as the specific programme, study design, changes in depressive symptoms, hours of guided treatment, control condition, and prior therapy (e.g., CBT) using meta-regression and subgroup analyses." | see 1.2 Objective | |
|                | "We will explore moderators of the effect and sources of between-study heterogeneity, such as the specific programme, study design, changes in depressive symptoms, hours of guided treatment, and prior therapy (e.g., CBT)." | see 2.1.1 Population | |

| #11 Methods | Patients will be included who were diagnosed with OCD by means of a validated diagnostic interview, validated self-report or by an expert clinician based on DSM- or ICD-based criteria. I would have serious doubts whether the latter two are valid ways to diagnose OCD, so would advise | Thank you very much. We share your concerns about diagnoses based only on expert clinicians and will no longer include respective studies in the review. Furthermore, we discussed your suggestion with Dr. Anne Külz, who is a renowned expert on OCD and co- | see 2.1.1 Population |
|--------------|-----------------------------------------------------------------------------------------------|---------------------------------------------------------------------------|---------------|
|              | "Eligible study populations include patients who were validly diagnosed with OCD based on DSM- or ICD-based criteria through a validated diagnostic interview or validated self-report (e.g., Y-BOCS)." | | |
against using them.

author of the new German national guideline on the treatment of OCD. According to Dr. Külz, validated self-report is widely used to diagnose OCD. E.g. the Y-BOCS is by far the best validated diagnostic procedure, recognised benchmark and gold standard used in most studies in the diagnosis of OCD symptoms. Therefore, we decided to also include studies in which patients were diagnosed with OCD by means of a recognised self-report scale.

**The criteria to include interventions, i.e. “programmes explicitly based on mindfulness and/or acceptance” is too vague. Please prespecify the different interventions you would like to look at separately and carefully define them. I would recommend to only include protocolized interventions, such as MBCT, MBSR,**

**Thank you very much. You raise a difficult and very relevant issue that we have been discussing and thinking about for quite a while now.**

There is a plethora of understandings of mindfulness and acceptance. The purpose of this review is to provide a summary of see 2.1.2 Interventions

"Mindfulness can be described as moment-to-moment awareness, cultivated by paying attention to the present moment in a non-reactive and non-judgmental way.[54] Acceptance can be characterised as the willingness to experience unwanted or unpleasant thoughts or emotions without attempting to avoid, escape, change, or terminate them.[28] Mindfulness and acceptance are closely related concepts[17] that are often taught jointly in MABPs. Although these programmes are rooted in different traditions, they follow a coherent conceptual foundation[17] and are often jointly investigated in systematic reviews and meta-analyses.[18–23,55] As we seek to provide a summary of the effectiveness of MABPs in their practical application, we must
| ACT and DBT. | MABPs effectiveness in their practical application and we must not neglect the extensive use of various forms of MABPs. Hence, we do not restrict our analyses to protocolled programmes such as MBCT and ACT, but include all eligible references that explicitly base their intervention on mindfulness or acceptance. Nevertheless, we will perform pre-specified subgroup analyses with interventions based on manuals to compare their effect with other MABPs. |
| not neglect the extensive use of various forms of MABPs beyond established programmes based on manuals such as MBRS, MBCT, DBT and ACT. Therefore, we include all eligible references that evaluate manualised MABPs for OCD patients but also include references that explicitly state that their programme is based on mindfulness and/or acceptance. However, we will exclude interventions that do not explicitly state that they are based on mindfulness or acceptance, and those that are only informed by mindfulness or acceptance. |

**#13 Outcome**

Please mention here that you do not only include pre-post intervention but also follow-up assessments.

We agree and mention that we also include follow-up assessments.

see Abstract

“We will perform sensitivity analyses using follow-up data.”

see 2.1.4 Outcome measures

“The primary outcome will be changes in OCD symptom severity from pre to post intervention, assessed using validated measures. We will also examine whether changes are maintained when taking follow-up data.”
#14 The authors intend to perform a separate meta-analysis on RCTs using between-group data. As they will also collect data on non-randomised controlled studies, I would recommend to include both in the meta-analysis and report on them separately. Thank you for your suggestion. (see #4)

#15 The authors plan to aggregate studies with ERP as control condition separately to estimate the potential increment of MABP and MABPs in combination with ERP over ERP alone. I would recommend to separate the different control conditions in WL/TAU, non-specific control conditions, and specific/evidence-based control conditions, like previous meta-analyses on MBIs have done. Thank you for your suggestion. We agree and included the type of control condition as potential moderator in order to provide separate effect estimates. see Abstract

“We will explore moderators and sources of heterogeneity such as the specific programme, study design, changes in depressive symptoms, hours of guided treatment, control condition, and prior therapy (e.g., CBT) using meta-regression and subgroup analyses.”

see 2.1.3 Study design and comparators

“We will aggregate RCTs according to the control condition used, if at least two studies have chosen the same control condition. ERP is the standard treatment for OCD. Therefore, studies with ERP as the control condition will be aggregated separately to estimate the potential increment of MABP and MABPs in combination with ERP over ERP alone.”

see 2.4 Data extraction

“We will extract information on […] (1) the study: authors, publication date, country, experimental design, type of control (e.g., waitlist, treatment-as-usual, psychoeducation/evidence-based control treatment)”
| #16 | You can always look at previous CBT as a possible moderator of treatment efficacy. | Thank you. We agree and included prior therapy (including CBT) as possible moderator. See also #8 | see 2.7 Data synthesis
“Furthermore, we will aggregate RCTs according to the control condition used, if at least two studies have chosen the same control condition. For example, we will perform a subgroup analysis […].” |
| #17 | See previous remark about how you are going to deal with the different languages. | Thank you! See #3 |
| #18 | With regard to the type of control used in the study, please specify this. | We agree and specified the type of control used. See also #15. | see 2.4 Data extraction
“We will extract information on […] (1) the study: authors, publication date, country, experimental design, type of control (e.g., waitlist, treatment-as-usual, psychoeducation/evidence-based control condition); […].” |
| #19 | With regard to the population, please specify prior therapy. Are you going to differentiate between behavioural and cognitive therapy for OCD? | Yes, we will do so. Within the moderator analyses, we will differentiate between different kinds of therapies, including behavioural and cognitive therapy for OCD. | see 2.4 Data extraction
“We will extract information on […] (2) the population: sample size (treatment/control), dropout rate, mean age, sex ratios, mean duration of illness, prior therapy (e.g., CBT); […].” |
| #   | With regard to intervention: are you going to look at whether the intervention was protocolized, ... | We added the explicit extraction of data on treatment standardisation. | see 2.4 Data extraction |
|-----|-------------------------------------------------------------------------------------------------|------------------------------------------------------------------|------------------------|
| #20 | ... whether the therapist were qualified (professional, mindfulness training), ... | Yes. We rate therapists’ clinical experience (0/1) and programme experience (0/1). We supplement the protocol accordingly. | “We will extract information on […] (3) the intervention: duration of an average single session, number of sessions, implemented programme (MBCT, ACT, etc.), treatment standardisation (yes/no), […].” |
| #21 | ... whether the adherence to the protocol was assessed? | Yes, we will do so within the risk of bias and quality assessment in individual studies. | see 2.4 Data extraction |
| #22 | ... whether the adherence to the protocol was assessed? | Yes, we will do so within the risk of bias and quality assessment in individual studies. | see 2.5 Risk of bias and quality assessment in individual studies |
| #23 | Whether the intervention was a group or individual treatment? Not all of them will be group treatments. | Thank you for this valuable remark. Yes, we will do so and specified the respective section within the data extraction paragraph. | see 2.4 Data extraction |
| #24 | With regard to outcomes: here the authors do mention follow-ups, whereas earlier in the protocol they do not. | Thank you for your careful reading! We mention the follow-ups earlier now. See #5 and #13 | see Abstract |

"We will perform sensitivity analyses using follow-up data."

see 2.1.4 Outcome measures

"The primary outcome will be changes in OCD symptom severity from pre to post intervention, assessed using validated..."
See previous comments on including the non-randomised controlled studies in the between-group meta-analysis. And on the definitions of the different programmes and pre-specification of moderators.

Thank you for your suggestions! See answers above (#2, #3, #8, #12).

The authors also mentioned earlier they would like to include comorbid depression as a possible moderator. How will they define that? Also by a structured diagnostic interview?

Thank you for highlighting this relevant issue and we are happy to clarify. We will include depressive symptoms as a continuous moderator and not in the sense of a categorical diagnosis. Although some studies may use a structured diagnostic interview to detect comorbid depression, we assume that many other studies may record depressive symptoms without deriving a diagnosis. E.g., many studies might use the BDI-II to assess depressive symptoms on a continuous scale. Due to your see Strengths and limitations of this study

“Insufficient data might prevent the analysis of potential moderators such as comorbid depressive symptoms.”

see 2.4 Data extraction

“We will extract information on […] (4) the outcomes: means and standard deviations for OCD symptom severity and depressive symptoms for all conditions and measurement points (pre, post, and follow-ups).”
| Element | Peer reviewer comment | Author response | Changes |
|---------|-----------------------|-----------------|---------|
| #27 Relevance | The authors conclude that if MABPs prove to be effective, they should be considered to be incorporated in OCD treatment and recommended as low-threshold therapeutic approach for treatment seeking individuals. Staging of MABPs is an important issue, are they more helpful as a low-threshold therapeutic approach or as a sequential treatment to prevent relapse (such as in recurrent depression), or as a supplementary treatment in those who have not sufficiently benefitted from CBT? It would be very helpful if the review/meta-analysis could shed some more light on this. | Thank you very much for your valuable comments! | |

Dr. Michael H. Bloch, Yale University

Element | Comments | Changes |
|---------|----------|---------|
| #28 | This manuscript is a protocol for a systematic review | Thank you for the suggestions to improve this |
to the Author

and meta-analysis examining the efficacy for mindfulness-based interventions for OCD. There has been increasing research in this area and a systematic review and meta-analysis in this area seem important.

I have several suggestions to improve this protocol in revision:

| #29 | (1) Y-BOCS should be the primary outcome to assess OCD severity. This is a widely used scale in the field. Given that there is only one scale used to assess outcome weighted mean difference rather than standized mean difference would be the prefered outcome. |
| --- | --- |
|  | Thank you for this valuable remark. We agree and we will calculate a weighted mean for those studies assessing OCD severity with Y-BOCS. Due to your suggestion, we changed the ‘data synthesis’ section accordingly. |
|  | “In addition, for those studies assessing OCD symptom severity with Y-BOCS, we will calculate a weighted mean difference. We will calculate two separate meta-analyses: […].” |
|  | However, we also plan to calculate a standaredized mean difference due to following reasons. First, in order to be able to include studies in the improbable case that a study is not using Y-BOCS and second, in order to gauge the size |

see 2.7 Data synthesis
of the effect and compare it with acknowledged thresholds (i.e., Cohen).

(2) The justification for including uncontrolled trials is poor in the current protocol. I would recommend limiting the review to RCT.

Thank you for your suggestion. We agree that RCTs provide the most valid effect estimate. Therefore, we will perform a separate meta-analysis on RCTs only.

In addition, however, we want to provide a comprehensive overview of the whole body of the current knowledge in the growing field of mindfulness intervention research. Therefore, instead of limiting our synthesis to RCTs, we will include other study designs too and will perform subgroup analyses with the study design as moderator. In this way, we will obtain separate effect estimates for different kinds of study designs including RCTs. Moreover, we will provide a narrative synthesis of all

see 2.1.3 Study design and comparators

“In the RCTs allow the most accurate effect estimate,[56] the exclusion of NRTs may lead to neglecting evidence,[57] whereas inclusion may enable us to identify moderators within a broader data base. We want to provide healthcare policy makers, practitioners, and researchers with a comprehensive quantitative and qualitative overview of the current knowledge. To this end, all available evidence will be considered. A second meta-analysis will summarize pre-post data of all eligible studies including RCTs and NRTs.”
| #31   | (3) There are several issues with study design that are not addressed in the current protocol ... (a) how are RCT going to be compared across different control conditions (MAC vs waitlist) | Thank you for this suggestion. To address this issue, we will include the kind of control condition as a moderator in the subgroup analyses. | see Abstract |
|-------|-------------------------------------------------------------------------------------------------------------|--------------------------------------------------------------------------------------------------------------------------|--------------------------------------------------|
|       | “We will identify moderators and sources of heterogeneity such as the specific programme, study design, changes in depressive symptoms, hours of guided treatment, control condition, and prior therapy (e.g., CBT) using meta-regression and subgroup analyses.” | see 2.1.3 Study design and comparators | |
|       | “We will aggregate RCTs according to the control condition used, if at least two studies have chosen the same control condition. ERP is the standard treatment for OCD. Therefore, studies with ERP as the control condition will be aggregated separately to estimate the potential increment of MABPs and MABPs in combination with ERP over ERP alone.” | see 2.4 Data extraction | |
|       | “We will extract information on […] (1) the study: authors, publication date, country, experimental design, type of control (e.g., wait list, treatment-as-usual, psychoeducation/evidence-based control condition); […]” | | |
“Furthermore, we will aggregate RCTs according to the control condition used, if at least two studies have chosen the same control condition. For example, we will perform a subgroup analysis to estimate the potential increment of MABPs and MABPs combined with ERP over ERP alone.”

“[…] we will perform a subgroup analysis to compare MABPs with MABPs combined with ERP and with ERP alone. Furthermore, we prespecify that if we find at least two studies based on mindfulness or acceptance but added to existing protocols (e.g., CBT), we perform a subgroup analysis to compare their effect to those that involve only mindfulness or acceptance.”

“A particularly promising approaches for the reduction of symptom severity in OCD are mindfulness- and acceptance-based programmes (MABPs) such as mindfulness-based stress reduction (MBSR), mindfulness-based cognitive therapy (MBCT), dialectical behavioral therapy (DBT), and acceptance and commitment therapy (ACT).[15–17] MABPs aim to promote the observation of thoughts, emotions, and bodily sensations in a non-reactive, open, and non-judgemental way.[17,18]”

“Mindfulness can be described as moment-to-
moment awareness, cultivated by paying attention to the present moment in a non-reactive and non-judgmental way.[54] Acceptance can be characterised as the willingness to experience unwanted or unpleasant thoughts or emotions without attempting to avoid, escape, change, or terminate them.[28] Mindfulness and acceptance are closely related concepts[17] that are often taught jointly in MABPs. Although these programmes are rooted in different traditions, they follow a coherent conceptual foundation[17] and are often jointly investigated in systematic reviews and meta-analyses.[18–23,55] As we seek to provide a summary of the effectiveness of MABPs in their practical application, we must not neglect the extensive use of various forms of MABPs beyond established programmes based on manuals such as MBRS, MBCT, DBT and ACT. Therefore, we include all eligible references that evaluate manualised MABPs in OCD patients but also include references that explicitly state that their programme is based on mindfulness and/or acceptance.”

| #34 | (4) English spelling should be checked throughout the manuscript. There are currently several spelling errors. | Thank you very much. We and an external native speaker double checked the manuscript for language and typing errors. |

**VERSION 2 – REVIEW**

| REVIEWER | Anne Speckens  
Radboudumc Instituut voor Wetenschappelijk Onderwijs en Opleidingen, Psychiatry |
| REVIEW RETURNED | 28-May-2021 |

| GENERAL COMMENTS | Overall, I am very happy with how the authors adapted the manuscript following my comments and suggestions. I have a few remaining if minor concerns: |
| | I thought the professional and programme experience of the therapist was not very well defined. I would recommend to use the internationally agreed recommendations for qualifications of, for instance, mindfulness teachers to assess this. Although I suspect not many papers will have mentioned or assessed this, it is important to report on it so it can be improved in the future. |
The same applies to the adherence to the protocol. The authors refer to “deviations from intended intervention” in the risk of bias and quality assessment of individual studies. I was referring more specifically to the use of instruments such as the Mindfulness Based Interventions Teachers Assessment Criteria (MBI-TAC). Again, I suspect not many studies have done so. But I do think this should be noted and change in the future.

### VERSION 2 – AUTHOR RESPONSE

| Prof. Anne Speckens, Radboud University |
|-----------------------------------------|
| **Element** | **Peer reviewer comment** | **Author response** | **Changes** |
| **#3 Comments to the Author** | Overall, I am very happy with how the authors adapted the manuscript following my comments and suggestions. I have a few remaining if minor concerns: | Thank you! We were happy to implement your good suggestions. |  |
| **#4** | I thought the professional and programme experience of the therapist was not very well defined. I would recommend to use the internationally agreed recommendations for qualifications of, for instance, mindfulness teachers to assess this. Although I suspect not many papers will have mentioned or assessed this, it is important to report on it so it can be improved in the future. | We agree. This is an important topic we will discuss in the final publication. We have made appropriate changes. | see 2.4 Data extraction |
| **#5** | The same applies to the adherence to the protocol. The authors refer to “deviations from intended intervention” in the risk of bias and quality assessment of individual studies. I was referring more specifically to the use of instruments such as the Mindfulness Based Interventions Teachers Assessment Criteria (MBI-TAC). Again, I suspect not | We agree. Thank you for clarifying your concern. We have made appropriate changes and will critically discuss this issue in the final publication. | see 2.4 Data extraction |
many studies have done so. But I do think this should be noted and change in the future.

| setting (yes/no) | group size, use of measurement tools to assess the clinical and programme experience of the therapist; […] |