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)	A systematic review of patients' participation in and experiences of technology-based monitoring of mental health symptoms in the community
AUTHORS	Walsh, Sophie; Golden, Eoin; Priebe, Stefan

VERSION 1 - REVIEW

REVIEWER	Maria Faurholt-Jepsen Psychiatric Center Copenhagen, Denmark
REVIEW RETURNED	06-May-2015

GENERAL COMMENTS	**1: Abstract** A limitation worth mentioning in the abstract is that the present review covers mental health as a broad definition and perhaps comparison across different mental illnesses cannot be done. **2: Introduction section** - Pg. 3 line 28: A reference to EMA is lacking. Please provide. - Pg. 4 line 19: Overall, I must say that I find the objectives too broad. It will for sure be very difficult to compare rates of participation and experiences across different mental illnesses. This is not a minor concern since it questions the entire merit of the review. **3: Methods section** - Pg. 4 line 29: A major concern: The literature search needs an update. It is now almost one year old. - Pg. 4 line 32: Perhaps you should consider eliminating the search items 1-12. My concern is that you might miss some monitoring systems where the self-monitoring was only a small part of the "intervention" and thereby not mentioned directly. Still there could be important information is the papers. - Pg. 4: Was there a review protocol prepared in advance? Was it registered anywhere or can it be retrieved by contacting the author? - Pg. 5 line 7-21: A major concern: The selection criteria should be clearer in the title of the review. As I understand this review both patients diagnosed with some DMS-IV

condition or if a person experienced any symptoms? These are two very different situations. Also you only included papers conducted in community settings. I don't understand this rationale. Further, you only included papers if the monitoring system provided feedback to somebody. Why? And if this is really the thing you would like to investigate, then it should be clearer in the title as well as in the objectives. As I see it this review should include studies that 1) included a clearly defined patient sample, 2) used some kind of technological tool for monitoring, and 3) reported on patient participation rates or experiences.

All the different inclusion criteria make the conclusion drawn from this review difficult to understand.

4: Results section

Regarding the flow diagram: You have a total of n= 3458 additional records included through other sources. Please specify in the flow diagram where these additional records were identified.

Flow diagram: Again I must highlight my concern on excluding papers not using feedback as a part of the system. You exclude 23 papers on this ground. Why are they not interesting? They could provide some useful insights to this area of research.

- Pg. 6 line 23: Please provide numbers to the studies from Australia and Japan also. Also provide number son pilot studies, RCTs and observational studies.
- Same section: please provide references to the papers each time you list or mention something in relation to the included papers (location, design, sample size etc.).
- Pg. 6 line 33-46: Please provide references to the papers throughout.
- Pg. 6 line 51- pg. 7 line 5: Please provide references to the papers throughout the section.
- Pg. 7 line 9-19: Again provide references.
- Pg. 7 line 23-42: Again provide references.
- Pg. 7 line 49- pg. 8 line 5: Provide reference throughout.
- Pg. 10 line 3-17: Provide references.

Overall, the results section lists a large amount of reported results but conclusions based on these are not made. This makes the reading quite monotonous, and after reading the paper it is difficult to capture what the take-home message really was.

- Table in supplementary file 1: Needs to be simplified. Too much information on very little space.

5: Discussion section

- As listed in the results section rates of participation and experiences using the different monitoring tool s were very different across illnesses. One of my major concerns is that

	this review's conclusion might not take this into account. I don't think that a review like this can draw any general conclusions on rates of participation and experiences of using a monitoring tool (your limitation number two). - Overall, the discussion section repeats much of the findings from the results. The discussion section should discuss the findings, not repeat them. - Please discuss potential publication bias and how this influences the results given in this review. - Please discuss the ethical issues in monitoring of this kind - Please discuss the economic issues in monitoring of this kind **6: Conclusions** I think that the conclusion support my concern that from this review there cannot be drawn any conclusions due to the heterogeneity of the studies.

REVIEWER	Alex Harris Center for Innovation to Implementation – (MPD:152) VA Palo Alto Health Care System
REVIEW RETURNED	13-Jul-2015

GENERAL COMMENTS	1) It appears that the literature relating to patients with substance use disorder were excluded for some reason. A quick search finds the following: http://www.sciencedirect.com/science/article/pii/S037687160500061X http://ajp.psychiatryonline.org/doi/abs/10.1176/appi.ajp.2010.10030463 http://psycnet.apa.org/journals/pha/11/1/73/ 2. The review mixes a lot of different kinds of studies in terms of the role of symptom monitoring. One question is whether regular symptom monitoring is an important adjunct to other treatments. To the extent possible, I would be interested in knowing more about the effect of symptom monitoring in those studies that compared it to a no-monitoring condition. 3. The limitation of these analyses are well represented in the discussion section, but the statements in the abstract do not reflect the same level of cautious. I suggest adding some caveats to the abstract that reflect the proper level of uncertainly and humility.

VERSION 1 – AUTHOR RESPONSE

Reviewer 1 Comments	Response	Location
1: Abstract		

A limitation worth mentioning in the abstract is that the present review covers mental health as a broad definition and perhaps comparison across different mental illnesses cannot be done.	Has been added	Final bullet point of the abstract
2: Introduction section		
- Pg. 3 line 28: A reference to EMA is lacking. Please provide.	Added	Background page 3 line 33
- Pg. 4 line 19: Overall, I must say that I find the objectives too broad. It will for sure be very difficult to compare rates of participation and experiences across different mental illnesses. This is not a minor concern since it questions the entire merit of the review.	The objectives have been amended to more closely reflect the intended aims	Title Abstract line 5-6 Intro page 3 line 50,58 Intro page 4 line 30-36
3: Methods section		
- Pg. 4 line 29: A major concern: The literature search needs an update. It is now almost one year old.	This has been completed and 5 additional studies found	Method page 4 line 56 (and results added throughout)
- Pg. 4 line 32: Perhaps you should consider eliminating the search items 1-12. My concern is that you might miss some monitoring systems where the self-monitoring was only a small part of the “intervention” and thereby not mentioned directly. Still there could be important information is the papers.	The search terms were chosen to provide a balance between specificity and sensitivity. If search terms 1-12were eliminated the database searches would bring up all papers using technology in mental health. This lack of specificity would increase numbers greatly. The authors are confident that the terms used and complementary hand searches and reference screening minimises the potential for missed papers.	
- Pg. 4: Was there a review protocol prepared in advance? Was it registered anywhere or can it be retrieved by contacting the author?	Yes a protocol was used and information has been added	Method page 4 line 44
- Pg. 5 line 7-21: A major concern: The selection criteria should be clearer in the title of the review. As I understand this review both patients diagnosed with some DMS-IV condition or if a person experienced any symptoms?	The objective of this review was so synthesis the evidence of the use of symptom monitoring in routine clinical care.	Title Abstract line 5-6 Intro page 3 line 50,58

These are two very different situations. Also you only included papers conducted in community settings. I don't understand this rationale. Further, you only included papers if the monitoring system provided feedback to somebody. Why? And if this is really the thing you would like to investigate, then it should be clearer in the title as well as in the objectives. As I see it this review should include studies that 1) included a clearly defined patient sample, 2) used some kind of technological tool for monitoring, and 3) reported on patient participation rates or experiences. All the different inclusion criteria make the conclusion drawn from this review difficult to understand.	The interventions of interest were defined as those which provide feedback to patient, clinician or carer.to distinguish these schemes from those which collect data used only for research purposes (as mentioned in the introduction page 4, lines 5-7). These schemes for research are fundamentally different both because patients are sometimes paid to provide research data, and are not doing so with a view to contributing to their own healthcare. Therefore the adherence and experience reported in these papers ae not necessarily relevant to clinical care. The use of symptom monitoring exclusively in inpatient care/during appointments with clinicians was also excluded because it is likely to be heavily supported by the clinician and again participation rates and experiences may not be relevant. The population included is purposively inclusively in order to provide an overview of the range of symptom monitoring practices across the spectrum of mental health conditions. Changes have been made to the title, introduction, objectives and inclusion criteria to address these points, to improve the clarity from the outset.	Intro page 4 line 3,4, 21,22
4: Results section		
Regarding the flow diagram: You have a total of n= 3458 additional records included through other sources. Please specify in the flow diagram	The list of grey literature sources is provided in full (methods page 4 line 58 – page 5 line 3-7). The words 'grey literature' have been	

where these additional records were identified	added to make this clear on the flow diagram.	
Flow diagram: Again I must highlight my concern on excluding papers not using feedback as a part of the system. You exclude 23 papers on this ground. Why are they not interesting? They could provide some useful insights to this area of research	This comment is addressed above (method section point 4)	
- Pg. 6 line 23: Please provide numbers to the studies from Australia and Japan also. Also provide numbers on pilot studies, RCTs and observational studies. - Same section: please provide references to the papers each time you list or mention something in relation to the included papers (location, design, sample size etc.). - Pg. 6 line 33-46: Please provide references to the papers throughout. - Pg. 6 line 51- pg. 7 line 5: Please provide references to the papers throughout the section. - Pg. 7 line 9-19: Again provide references. - Pg. 7 line 23-42: Again provide references. - Pg. 7 line 49- pg. 8 line 5: Provide reference throughout. - Pg. 10 line 3-17: Provide references.	References numbers have been provided throughout	Results page 6 line 40 through to page 8 line 23
Overall, the results section lists a large amount of reported results but conclusions based on these are not made. This makes the reading quite monotonous, and after reading the paper it is difficult to capture what the take-home message really was.	To make the results less monotonous two sections have been re-written: summary of symptom monitoring practices and summary of participation rates.	Results page 6 line 40 through to page 8 line 23. Results page 10 line 3-17
- Table in supplementary file 1: Needs to be simplified. Too much information on very little space.	This has been simplified by the removal of two columns and reduced information presented in the final column There remains detail in this table	Online supplementary file 2

	which is likely to be of interest to those wishing to develop a symptom monitoring scheme	
5: Discussion section		
- As listed in the results section rates of participation and experiences using the different monitoring tool s were very different across illnesses. One of my major concerns is that this review's conclusion might not take this into account. I don't think that a review like this can draw any general conclusions on rates of participation and experiences of using a monitoring tool (your limitation number two).	More information has been added to the discussion about the role of diagnoses	Discussion page 13 line 25-30
- Overall, the discussion section repeats much of the findings from the results. The discussion section should discuss the findings, not repeat them.	The main findings of the discussion has been reworded to draw out some conclusions, where possible from the results	Discussion page 13 line 19-38
- Please discuss potential publication bias and how this influences the results given in this review.	A sentence has been added to the method about minimising publication bias via grey literature searches	Method page 4 line 56
- Please discuss the ethical issues in monitoring of this kind	Owing to the space allocated in the journal it is not possible to discuss the ethical issues in detail but two sentence have been added	Discussion page 16 line 3-7
- Please discuss the economic issues in monitoring of this kind	Owing to the space allocated in the journal it is not possible to discuss the economic issues in detail but a sentence has been added	Discussion page 16 line 7-13
6: Conclusions		
I think that the conclusion support my concern that from this review there cannot be drawn any conclusions due to the heterogeneity of the studies.	The terms of the conclusion have been modified to make it clear that although the findings are limited the review has some merit	Discussion page 16 line 19-23
Reviewer 2 comments		
It appears that the literature relating to patients with substance use disorder were excluded for some reason. A quick search finds the following:	Justification of exclusion of this population has been added	Method page 5 line 29-30

http://www.sciencedirect.com/science/article/pii/S037687160500061X http://ajp.psychiatryonline.org/doi/abs/10.1176/appi.ajp.2010.10030463 http://psycnet.apa.org/journals/pha/11/1/73/		
The review mixes a lot of different kinds of studies in terms of the role of symptom monitoring. One question is whether regular symptom monitoring is an important adjunct to other treatments. To the extent possible, I would be interested in knowing more about the effect of symptom monitoring in those studies that compared it to a no-monitoring condition.	The sentence about studies investigating effectiveness has been moved to the beginning of the results sections. This illustrates that only a small number of studies were designed with control arms and it is therefore not possible to address this question in the present review.	
The limitation of these analyses are well represented in the discussion section, but the statements in the abstract do not reflect the same level of cautious. I suggest adding some caveats to the abstract that reflect the proper level of uncertainly and humility.	Words have been added to this effect	Abstract page 2 lines , 36

VERSION 2 – REVIEW

REVIEWER	Maria Faurholt-Jepsen Psychiatric Center Copenhagen, Denmark
REVIEW RETURNED	19-Aug-2015

GENERAL COMMENTS	**Title:** The title is too broad. Psychiatry should be mentioned as a minimum. The review is submitted to a broad journal and this should be kept in mind. **Abstract:** How is symptom monitoring defined? Monitoring of what and only subjective self-reported monitoring? What about studies on objective monitoring? Please provide reasons for excluding objective

	measures in the methods section. What was the definition of feedback? Please provide. **Introduction:** Overall, I must say that I find the objectives of the review too broad and inclusive. The authors probably discovered during the process that it was difficult to conclude the findings of the very heterogeneous studies included. It will for sure be very difficult to compare rates of participation and experiences across different mental illnesses. This is not a minor concern since it questions the entire merit of the review. What do you mean with the term 'feedback'? Please define. I do not understand why the research question/ the aim are interesting? Why compare across very different disorders? There must be different thing and concerns in every disorder that need to be taken into consideration in every study, and this complicates the comparison of studies. **Methods:** Since it is not specific mental illnesses that are in focus with the present review I think other databases should have been considered for the literature search as well. Other more technical oriented journals providing feedback on usability etc. could help give a more complete picture of the area. Papers from technical proceedings could provide relevant information of this topic. I find the selection criteria limiting to the usefulness of the review. Why should it be easier to compare DSM-IV axis 1 conditions than to include other disorders such as personality disorders? All conditions are provided with complex multifaceted treatments. This is a major concern. Why is it important that the monitoring tools provided feedback options and not only symptom monitoring? As mentioned in my first review of the paper: was a study protocol prepared in advance? Please provide information on this and potential changes during the review process to this protocol. **Results:** As mentioned in my first review of the paper reasons for exclusion of studies should be mentioned… The reduction of papers from 9521 to 207 should somehow be justified and explained in more details. And why does it state 9521 in the text but 11073 in the flow

	diagram? The numbers does not match. In the first section of the results: Results on protocols, tools etc. are provided. I think it would be better to provide the clinical relevant information first. Please remember that BMJ Open is a general journal for clinicians, and thus this information should be presented first. Also, the very last part of the results is about the adherence rates. As this is one of the aims this should be presented earlier in the results section. Again I must highlight my concern on excluding papers not using feedback as a part of the system. You exclude 26 papers on this ground. Why are they not interesting? They could provide some useful insights to this area of research. **Discussion:** As mentioned in my earlier feedback I have the following comment to the discussion section: As listed in the results section rates of participation and experiences using the different monitoring tool s were very different across illnesses. One of my major concerns is that this review's conclusion might not take this into account. I don't think that a review like this can draw any general conclusions on rates of participation and experiences of using a monitoring tool (your limitation number two). As mentioned in my first review of the paper: Please discuss potential publication bias and how this influences the results given in this review. **Conclusions** I think that the conclusion supports my concern that from this review there cannot be drawn any conclusions due to the heterogeneity of the studies. **Figure 1:** Flow diagram: names of the databases should be mentioned. **Overall comments to the review:** The review compares/reviews symptom monitoring tools across various psychiatric disorders. The review aims are too broad to be of clinical usefulness. For example for sure the requirements, views

	and potential harmful side effects across disorders are different and this complicates the review and its aims. This limits the warrant for this review. Furthermore, considerations on the validity of the monitoring tools need to be taken into consideration and discussed. Overall, I find the review of some use, but to my opinion it needs extensive revision that is beyond what can be done for a second submission. I think the authors should reconsider their aims and methods and submit the review as a new paper.

VERSION 2 – AUTHOR RESPONSE

Title:
"The title is too broad. Psychiatry should be mentioned as a minimum. The review is submitted to a broad journal and this should be kept in mind."

The title has been amended to include a reference to mental health.

Abstract:
"How is symptom monitoring defined? Monitoring of what and only subjective self-reported monitoring? What was the definition of feedback? Please provide."

The definitions of symptom monitoring and feedback have been added to the objectives.

"What about studies on objective monitoring? Please provide reasons for excluding objective measures in the methods section."
As we have clarified the definition of symptom monitoring, we believe the aims of the review are clear to the reader. Further, owing to the limitations of space in the abstract and the fact that no other exclusion criteria are mentioned here, we do not feel it is appropriate to provide the rationale for excluding objective monitoring here. Instead, it is mentioned in the methods section proper [methods – selection criteria – 5th sentence].

Introduction:
"Overall I must say that I find the objective of the review too broad and inclusive. I do not understand why the research question / the aim are interesting? Why compare across very different disorders? There must be different thing and concerns in every disorder that need to be taken into consideration in every study, and this complicates the comparison of studies."

We have previously responded to the reviewer's comment stating the objectives are too broad and inclusive. It is our view is that the broad and inclusive nature of the review is one of its strengths. As this is the first systematic review of patients' experience of and engagement with technology-based symptom monitoring in routine community mental health care, it is a benefit that we have looked across the spectrum of diagnoses, as the review paints a picture of how this practice is used. Further, we found a number of studies that did not target specific diagnoses (e.g. 52,53,62,67, 70,72,73], suggesting that symptom monitoring may not necessarily be diagnostic-specific. Further, we could not draw any specific conclusions about the role of diagnoses, perhaps because of the heterogeneity of studies but also perhaps because the evidence does not yet conclusively indicate differences. As such, it is perhaps more fruitful for symptom monitoring practices to consider general design

principles, as we conclude in this article, rather than symptom monitoring practices being developed in diagnostic-specific silos, which do not reflect the reality of community mental health care practice where patients can have multiple diagnoses.

Methods:
“Since it is not specific mental illnesses that are in focus with the present review I think other databases should have been considered for the literature search as well. Other more technical oriented journals providing feedback on usability etc. could help give a more complete picture of the area. Papers from technical proceedings could provide relevant information of this topic.”

The databases chosen are those most often used in systematic reviews in the field of mental health (as seen by other reviews published in BMJ open e.g. doi:10.1136/bmjopen-2014-006108 , doi:10.1136/bmjopen-2014-006586, doi:10.1136/bmjopen-2015-007575). The authors are confident that the searches and grey literature sources utilised minimised the potential for missed papers.

“I find the selection criteria limiting to the usefulness of the review. Why should it be easier to compare DSM-IV axis 1 conditions than to include other disorders such as personality disorders? All conditions are provided with complex multifaceted treatments. This is a major concern.”

Perhaps the way we worded this inclusion criterion leads the reader to focus on what we did not include, rather than what we did which, as outlined earlier, was very inclusive of the mental health conditions routinely treated in community mental health care. As such, we have amended this section to include a list of disorders that we did include [methods – selection criteria - first sentence].
The rationale for excluding substance misuse and personality disorder remains that these are particularly complex conditions often treated by specialist services. The aim of the review was to understand symptom monitoring in the context of the most familiar and widely seen mental health conditions in practice. All reviews must make decisions about inclusion and exclusion and it is our view that extending the criteria to include these conditions would further complicate the findings.

“Why is it important that the monitoring tools provided feedback options and not only symptom monitoring?”

In our previous response, we added to the manuscript that this criterion was used to distinguish relevant studies from those not employing symptom monitoring as part of treatment. The ‘feedback’ component suggests that the patient, their clinicians or someone else involved in the patient’s care review the information in some way, indicating that symptom monitoring is being used in the management of the patient’s health. It is also mentioned in the introduction that symptom monitoring data are sometimes used for research purposes only, to analyse, at an aggregate level, patterns in psychopathology. Although these studies would contain information about rates of participation, participants are sometimes paid to complete this symptom monitoring. As such, we expect that patient participation rates and experiences would be different in those studies. Words to this effect have been added to the introduction [introduction –penultimate paragraph – sixth sentence].

“As mentioned in my first review of the paper: was a study protocol prepared in advance? Please provide information on this and potential changes during the review process to this protocol.”

We previously responded to the reviewer’s comment about the protocol and it is stated in the methods that it is available upon request.

Results
“As mentioned in my first review of the paper reasons for exclusion of studies should be mentioned... The reduction of papers from 9521 to 207 should somehow be justified and explained in more details.

And why does it state 9521 in the text but 11073 in the flow diagram? The numbers does not match."

The reasons for excluding papers were previously listed only in Figure 1, but are now also provided in the text [Results - first sentence]. The error in the text has also been corrected to match the numbers in Figure 1.

"In the first section of the results: Results on protocols, tools etc. are provided. I think it would be better to provide the clinical relevant information first. Please remember that BMJ Open is a general journal for clinicians, and thus this information should be presented first. Also, the very last part of the results is about the adherence rates. As this is one of the aims this should be presented earlier in the results section."

The authors have discussed this point and tried to rearrange the results section. However, we feel it is necessary to provide the reader with context on the studies, prior to the results; otherwise, the results section may be confusing for the reader. We notice that comparable papers published in BMJ Open utilise a similar structure (e.g. BMJ Open 2015;5:e008708 doi:10.1136/bmjopen-2015-008708). However, we understand that the editor/reviewer may still wish for us to change this.

Discussion:
"As listed in the results section rates of participation and experiences using the different monitoring tool s were very different across illnesses. One of my major concerns is that this review's conclusion might not take this into account. I don't think that a review like this can draw any general conclusions on rates of participation and experiences of using a monitoring tool (your limitation number two)."

Again, we feel that it is a strength of this paper that we looked inclusively across diagnoses. In our previous response to the reviewer, we amended the discussion section's main findings to consider specifically the role of diagnosis. We have now specifically touched on the point about diagnoses in the strengths and limitations section [Discussion – Strengths and limitations – second, third, sixth sentences].

"As mentioned in my first review of the paper: Please discuss potential publication bias and how this influences the results given in this review."

The reviewer's comments about publication bias were previously addressed by adding a sentence to the methods section about this. However, to address this further in the discussion, we have added another sentence [Discussion – Strengths and limitations – tenth sentence]