

## PEER REVIEW HISTORY

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### ARTICLE DETAILS

<b>TITLE (PROVISIONAL)</b>	Does remote patient monitoring reduce acute care use? A systematic review
<b>AUTHORS</b>	Taylor, Monica; Thomas, Emma; Snoswell, Centaine; Smith, Anthony; Caffery, Liam

### VERSION 1 – REVIEW

<b>REVIEWER</b>	Annmarie Lassen, professor Emergency Medicine Emergency Medicine, Odense University Hospital and Institute of Clinical Research University of Southern Denmark, Denmark
<b>REVIEW RETURNED</b>	30-May-2020

<b>GENERAL COMMENTS</b>	<p>In this systematic review the authors describe the effect on acute care use related to use of remote patient monitoring. The question is highly relevant for clinicians in a broad - and in a specialized - context as well as for health care planners. The authors seem to use state of the art methodology for the review, but I need a few precisions to be able to evaluate this in full.</p> <p>Regarding selection: Are titles and abstracts screened independently and blinded by both of the mentioned researchers and then compared. Please state if this is the fact or not clearly for the reader .</p> <p>Regarding Analysis: Was the categorization based at a 5% significant level in the individual studies? This is relevant as the expected number of “decreased” use of acute care due to random effect have impact on the conclusion. As nearly half of the studies show decreased use of acute care; I do not think the observed effects are du to random effect, the statement is only needed as information for the reader.</p> <p>An exploratory question: at page 10 line 25 you mention a paper there finds that apps are related to a better compliance than web pages. What did you find in the present review?</p> <p>Minor point: Regarding inclusion and exclusion criteria: please state that you also included RCTs in the review. In the section of CVD non invasive: please state the method “the largest study” is based on. In the section of COPD: please state which methods “the six trials” are based on. In the study quality section :please spell the full name of JBI as you do this in the rest of the manuscript.</p>
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<b>REVIEWER</b>	Mirza Baig Orion Health, New Zealand
<b>REVIEW RETURNED</b>	11-Sep-2020

<b>GENERAL COMMENTS</b>	<ol style="list-style-type: none"> <li>1. Please define the boundary of RPM as RPM could have very broad coverage</li> <li>2. Specify the RPM technology/ tools used in this study, medical devices, apps, softwares, etc.</li> <li>3. Introduction is missing the important link on RPM - Acute admissions and more focused on conditions. Existing work in this area is also missing</li> <li>4. Why ED is added in the selection criteria, is the study's RPM scope covers ED? does ED use RPM? inclusion and exclusion criteria needs to be aligned with the objectives</li> <li>5. Apart from CVD and COPD, it would be good to have information on other conditions. Please explain figures in the text and good to have self-explanatory tables as I see tables are quite busy.</li> <li>6. A certain conclusion, learnings, new/ unknown knowledge is missing - Discussion section needs to be revised to highlight achievements, improvements, limitations, future work.</li> <li>7. Conclusion should be expanded to include RPM pros and cons learned from this study</li> <li>8. Key message is missing on why RPM is helpful for e.g. COPD or CVD and why not for other. Factors for wider adoption? use of RPM in the study's scope?</li> <li>9. References need to be checked for completeness</li> </ol>
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<b>REVIEWER</b>	Vess stamenova Women's College Hospital, Toronto, Ontario, Canada
<b>REVIEW RETURNED</b>	13-Sep-2020

<b>GENERAL COMMENTS</b>	<p>Does remote patient monitoring reduce acute care use? A systematic review</p> <p>This study is a systematic review of the remote monitoring literature between 2015-2019. The review focuses on studies that examine acute hospital use as outcome (ED admissions, hospitalizations and length of stay). The review does not focus on specific patient population(s).</p> <p>75 studies were included in total. The authors report that reductions in hospitalizations, length of stay and ED admissions was reported in just about less than half of the studies reviewed. Most studies were focused on COPD and CVD with only 15% of studies focusing on non COPD/CHF patient populations.</p> <p>Introduction:</p> <ul style="list-style-type: none"> <li>• I would suggest the authors paraphrase the first paragraph. The second sentence sounds incomplete, as it starts with “while healthcare providers...” and it feels like it lacks a finish. It seems to me the authors are using “while” as a substitute to “in spite of the fact that” and we are left include what is happening in spite of the fact.</li> <li>• The second paragraph could use more references around definitions of RPM</li> <li>• I would also recommend citing more studies that exemplify some of the methods (e.g. invasive vs. non-invasive methods).</li> <li>• I think the introduction could be strengthened by providing a more detailed summary of past RPM reviews and meta-analyses. My sense is that there are a lot of review and meta-analysis in COPD and CHF and the authors may have failed to describe what have past studies shown.</li> </ul>
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	<ul style="list-style-type: none"> <li>• In reference to what have past studies shown, the authors may benefit from strengthening their case on what have past studies shown and what they have failed to do and how their study fills that gap. My concern is that health utilization being a secondary outcome and focusing on the last 5 years only is not a sufficient justification for yet another review that is not even a meta-analysis. I am struggling a bit with figuring out what contribution this article makes over and above what is already out there, so I think the authors need to justify this a bit more in the introduction.</li> </ul> <p>Methods:</p> <ul style="list-style-type: none"> <li>• While the authors highlight focusing on the last 5 years as a benefit, I don't see it as such. I think focusing on the last 5 years would be very limiting in terms of available articles and 2019 was not a full year of publications, so the study is focused on 4.5 years. I probably would have looked at least at the last full 10 years.</li> <li>• Should the search criteria include any keywords around emergency department, emergency room, or just emergency.</li> <li>• I also question the value of focusing on any patient populations. As this is a review and not a meta-analysis, I don't think the effect of adding these extra studies is overly negative, so maybe they could stay, but at 15% and with variety of conditions in that group, the other studies may just be distracting. COPD and CHF are two very similar conditions with frequent exacerbations and if not controlled properly result in hospitalizations/ED admissions and therefore it makes sense to combine those. The other conditions may be of different nature and the health utilization outcomes may not be as sensitive to actual patient outcomes. Overall, I would consider the value of removing them, if other reviewers/editor feel the same way.</li> </ul> <p>Results:</p> <ul style="list-style-type: none"> <li>• CVD invasive: can you provide a description of the patient populations included in this group within the text?</li> <li>• A comment here that the fact that you are including invasive and non-invasive remote monitoring approaches could potentially be mentioned in your intro as a strength of your approach.</li> </ul> <p>Discussion</p> <ul style="list-style-type: none"> <li>• P.9 at the bottom: "while findings from....", sentence seems incomplete. Same problem as in the intro, my first comment.</li> <li>• P. 10 states "the one previous review of RPM in COPD". I am confident that there are multiple reviews AND meta-analyses on RPM in COPD.</li> </ul> <p>As a final general comment, I think if the authors were to exclude non-COPD/CVD studies, with the 3 outcomes that they are looking for, they could have conducted a meta-analysis, which would have been more informative and potentially reliable. This would have allowed for a quantifiable comparison between patient populations and also between invasive and non-invasive RPM, which could have been a considerable contribution to the literature. I recognize that may this is not feasible at the moment, but it could be something to be considered for the future given the substantial work put into collating all studies.</p>
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**VERSION 1 – AUTHOR RESPONSE**

Reviewer 1:

Overall	<p>In this systematic review the authors describe the effect on acute care use related to use of remote patient monitoring. The question is highly relevant for clinicians in a broad - and in a specialized - context as well as for health care planners. The authors seem to use state of the art methodology for the review, but I need a few precisions to be able to evaluate this in full.</p>	<p>Thank you for your overall positive assessment of our chosen topic and methods.</p>
Methods	<p>Regarding selection: Are titles and abstracts screened independently and blinded by both of the mentioned researchers and then compared. Please state if this is the fact or not clearly for the reader.</p>	<p>The screening was independent and blinded by the two researchers, we have added this in the "Selection" section of the Methods.</p> <p><i>"Titles and abstracts were screened independently by two researchers (MT, MB) who were also blinded to each other's selections."</i></p>
Analysis	<p>Regarding Analysis: Was the categorization based at a 5% significant</p>	<p>Individual studies did not always report their significance level, however, where this information was available it has been included in Supplementary Table 1. A</p>
	<p>level in the individual studies? This is relevant as the expected number of "decreased" use of acute care due to random effect have impact on the conclusion. As nearly half of the studies show decreased use of acute care; I do not think the observed effects are due to random effect, the statement is only needed as information for the reader.</p>	<p>statement has been added to our Results section under "Effect of remote monitoring on acute care use".</p> <p><i>"The majority of studies set a significance level of 5% for concluding that there was a difference between groups, however individual study details on this can be viewed in Supplementary Table 1."</i></p>
Discussion	<p>An exploratory question: at page 10 line 25 you mention a paper there finds that apps are related to a better compliance than web pages. What did you find in the present review?</p>	<p>As the change in acute care use was the focus of this review, compliance to RPM was not one of the variables we investigated across the individual studies. We make this comment in our manuscript to highlight that evidence exists for intervention characteristics having an effect on how patients engage with in. There were many sub-analyses we would have liked to include such as results according to RPM device type, or data collection method, however, it was outside the scope/word limits.</p>

Methods	Regarding inclusion and exclusion criteria: please state that you also included RCTs in the review.	We have added RCTs as well as the other study types included to our statement on inclusion/exclusion criteria.  <i>"We included primary, empirical studies including randomised controlled trials (RCTs), cohort studies, and case control studies that compared acute hospital use..."</i>
Results	In the section of CVD non invasive: please state the method "the largest study" is based on.	We updated the sentence to read:  <i>"The largest study, a cohort..."</i>
Results	In the section of COPD: please state which methods "the six trials" are based on.	The start of this sentence reads "Of the 12 RCTs..." and so if we interpret the reviewer's comment correctly, we feel that the methods of the six discussed trials have already been articulated as RCTs.
Results	In the study quality section: please spell the full name of JBI as you do this in the rest of the manuscript.	We have updated JBI to "Joanna Briggs Institute".

Reviewer 2:

Introduction/Methods	1. Please define the boundary of RPM as RPM could have very broad coverage	We have added to the Inclusion/exclusion criteria:  <i>"and the patient was monitored while outside of a hospital setting "</i>  Otherwise, we agree it is broad and for the purpose of this review the boundary is as stated in the Introduction: "recording and transmission of patient biometrics, vital signs, and/or disease-related data to a healthcare provider" and later specified in the Methods – Inclusion/exclusion criteria: "as long as the monitored data was sent to a clinician for review"
Introduction/Methods	2. Specify the RPM technology/ tools used in this study, medical devices, apps, softwares, etc.	We have added the following statement to Inclusion/exclusion criteria:  <i>"A variety of RPM technology was eligible for inclusion such as peripheral measurement devices, cardiac implantable electronic devices, and manual data entry using tablets, smartphones, or websites."</i>

Introduction	3. Introduction is missing the important link on RPM - Acute admissions and more focused on conditions. Existing work in this area is also missing	Thank you. To make the link between RPM and acute admissions clearer to the reader we have added:  <i>“Early detection and proactive management of chronic disease exacerbations may result in decreased costly acute hospital use. Previous RCTs have demonstrated that RPM can effectively alert a healthcare team to a decline in a persons’ condition enabling issues to be resolved out of hospital thereby reducing the need for urgent hospital admissions. (Ref)”</i>
Methods	4. Why ED is added in the selection criteria, is the study's RPM scope covers ED? does ED use RPM? inclusion and exclusion criteria needs to be aligned with the objectives	Our aim is to determine if RPM can reduce acute care use. Ideally, through early detection of disease, RPM can prevent a person from having to present to hospital at all (whether to the ED or admitted into hospital). We therefore define acute care use in the selection criteria as <i>“hospital admissions (including readmissions), length of stay, and emergency department (ED) presentations”</i> .
Results/Figures	5. Apart from CVD and COPD, it would be good to have information on other conditions. Please explain figures in the text and good to have self-explanatory tables as I see tables are quite busy.	Under ‘Disease conditions’ we briefly describe all the conditions in the included studies. CVD & COPD account for 86% of the included studies which is why we have focused on these conditions. Additional information is also provided in Supplementary Table 1.

		We have now included an explanatory sentence below each Figure & updated Figure 5 to be clearer. Other conditions are discussed in the Results section entitled “Other conditions”.
Discussion	6. A certain conclusion, learnings, new/ unknown knowledge is missing - Discussion section needs to be revised to highlight achievements, improvements, limitations, future work.	We agree the Discussion would benefit from better organization. We have added the following headings: <ul style="list-style-type: none"> <li>• <i>Principal findings</i></li> <li>• <i>Implications for practice</i></li> <li>• <i>Sub-populations</i></li> <li>• <i>Importance of a patient-centric approach</i></li> <li>• <i>Limitations</i></li> <li>• <i>Future research</i></li> </ul>
Conclusion	7. Conclusion should be expanded to include RPM pros and cons learned from this study	The scope of the Conclusion fits the scope of the study. Pros are included i.e. RPM can reduce acute care use in around half of studies. Cons are included: RPM does not reduce acute care in around half of studies; there is variation across subpopulations; variation across technologies.

Discussion/Conclusion	8. Key message is missing on why RPM is helpful for e.g. COPD or CVD and why not for other. Factors for wider adoption? use of RPM in the study's scope?	<p>To date the RPM literature has been dominated by COPD &amp; CVD studies. RPM in other conditions may be helpful however, the literature is currently too limited to determine. We mention this: <i>The effect of RPM for other disease condition is inconclusive due to the limited number of studies in these areas.</i></p> <p>We have added why invasive CVD is likely to be more effective than non-invasive. However, the underlying mechanisms require further investigation (we explain this in the conclusion).</p> <p><i>Invasive monitoring of CVD was more effective at reducing hospital admissions compared to other disease condition and non-invasive monitoring. This may be in part due to the ability of implantable devices to continuously monitor a person and automatically transmit data, identify arrhythmias (e.g. atrial fibrillation) and the advanced ability of these devices to directly detect abnormal cardiac issues rather than relying on physiological signs (e.g. changes in weight or blood pressure) that may or may not be due to the underlying cardiac condition. Further analysis is</i></p>
		<i>required to understand the underlying mechanisms causing such variation in RPM studies.</i>
References	9. References need to be checked for completeness	We have updated our references where required (including adding the issue numbers to all journal articles which were initially missing from each).

Reviewer 3:

Section	Comment	Response
Overall	This study is a systematic review of the remote monitoring literature between 2015-2019. The review focuses on studies that examine acute hospital use as outcome (ED admissions, hospitalizations and length of stay). The review does not focus on specific patient population(s). 75 studies were included in total. The authors report that reductions in hospitalizations, length of stay and ED admissions was reported in just about less than half of the studies reviewed. Most studies were focused on COPD and CVD with	No response required.

	only 15% of studies focusing on non COPD/CHF patient populations.	
Introduction	I would suggest the authors paraphrase the first paragraph. The second sentence sounds incomplete, as it starts with “while healthcare providers...” and it feels like it lacks a finish. It seems to me the authors are using “while” as a substitute to “in spite of the fact that” and we are left include what is happening in spite of the fact.	We have updated this statement for more impact to:  <i>“Healthcare providers often only become aware of a decline in an individual’s condition once symptoms have become severe enough to require escalation to acute care.”</i>
Introduction	The second paragraph could use more references around definitions of RPM	We have now referenced our definition of RPM - original source being The American Telemedicine Association – and reproduced on a World Health Organization (WHO) webpage.
Introduction	I would also recommend citing more studies that exemplify some of the methods (e.g. invasive vs. non-invasive methods).	We have further explained invasive vs non-invasive methods and added reference to relevant studies as shown in the additional statements in the Introduction:  <i>“Examples of implanted devices include pacemakers which are used to regulate abnormal rhythms, and implantable cardioverter defibrillators (ICDs) which are used in patients with previous cardiac arrests, congenital heart disease or ventricular arrhythmias.<sup>2</sup> Non-invasive interventions involve the transmission of data, such as bodyweight, blood pressure, or pulse oximetry<sup>3</sup> and are used commonly for patients with heart failure.<sup>4</sup>”</i>



Introduction	I think the introduction could be strengthened by providing a more detailed summary of past RPM reviews and metaanalyses. My sense is that there are a lot of review and meta-analysis in COPD and CHF and the authors may have failed to describe what have past studies shown.	We feel we have adequately referenced previous reviews in the Introduction as there are not many focused on acute care use, which is focus of and rationale for this study.
Introduction	In reference to what have past studies shown, the authors may benefit from strengthening their case on what have past studies shown and what they have failed to do and how their study fills that gap. My concern is that health utilization being a secondary outcome and focusing on the last 5 years only is not a sufficient justification for yet another review that is not even a metaanalysis. I am struggling a bit with figuring out what contribution this article makes over and above what is already out there, so I think the authors need to justify this a bit more in the introduction.	We feel the “ <i>Why read on?</i> ” section provides the information requested by the reviewer. “ <i>Previous studies of RPM and their impact on acute health services have largely focused on heart failure populations and manual collection of biometric data. Remote monitoring technologies have improved to now include automatic data collection using implanted devices and the use of RPM for other disease conditions. We present a contemporary review of the effectiveness of RPM in the context of hospital admissions, length of stay and emergency department presentations.</i> ”
Methods	While the authors highlight focusing on the last 5 years as a benefit, I don’t see it as such. I think focusing on the last 5 years would be	We have added justification in our Methods to focusing on the last five years: “ <i>Supporting our decision investigate research from the last five years was a recent systematic review reporting 43% of remote monitoring studies were</i>
	very limiting in terms of available articles and 2019 was not a full year of publications, so the study is focused on 4.5 years. I probably would have looked at least at the last full 10 years	<p><i>published from 2015 on, and over 60% of Oxford Level of Evidence 1 papers were published post-2015.[Farias, 2019]”</i></p> <p>We have also updated our searches to now include all of 2019 and most of 2020.</p>

Methods	Should the search criteria include any keywords around emergency department, emergency room, or just emergency.	We enlisted the assistance of a Senior Librarian with expertise in systematic review searches and found including any further terms resulted in an unmanageable amount of returned titles and abstracts for screening, of which did not yield any other studies.
Methods	I also question the value of focusing on any patient populations. As this is a review and not a meta-analysis, I don't think the effect of adding these extra studies is overly negative, so maybe they could stay, but at 15% and with variety of conditions in that group, the other studies may just be distracting. COPD and CHF are two very similar conditions with frequent exacerbations and if not controlled properly result in hospitalizations/ED admissions and therefore it makes sense to combine those. The other conditions may be of different nature and the health utilization outcomes may not be as sensitive to actual patient outcomes. Overall, I would consider the value of removing them, if other reviewers/editor feel the same way.	We feel it is better to report results stratified by disease condition which is justified by our finding that different disease conditions result in different outcomes in terms of acute care usage. For example, <i>"RPM of COPD was more effective at reducing ED presentation."</i>  We also believe that while COPD and CHF are similar in terms of exacerbations of these diseases can lead to hospitalization, they are different disease conditions with sufficient numbers of studies in each to report separately.
Results	CVD invasive: can you provide a description of the patient populations included in this group within the text?	We have included the following text to provide a description of the patient population: <i>"Invasive interventions involve direct measurement of biometric data, such as heart rate and pulmonary artery pressures, by an implanted device which is then transmitted to the healthcare provider. For example, devices such as pacemakers are used in people with arrhythmias to regulate abnormal rhythms, and implantable cardioverter defibrillators (ICDs) are used in patients with previous cardiac arrests, congenital heart disease or ventricular arrhythmias. Non-</i>

		<i>invasive interventions involve the transmission of data, such as bodyweight, blood pressure, or pulse oximetry and have most frequently been used in the management of patients with heart failure .<sup>2</sup></i>
Introduction/Methods	A comment here that the fact that you are including invasive and non-invasive remote monitoring approaches could potentially be mentioned in your intro as a strength of your approach.	We have already mentioned in the introduction that RPM interventions can be invasive or non-invasive, but have used this suggestion to improve our methods by adding under Inclusion/exclusion criteria: <i>“A variety of RPM technology was eligible for inclusion such as non-invasive peripheral measurement devices, invasive cardiac implantable electronic devices, and manual data entry using tablets, smartphones, or websites.”</i>
Discussion	P.9 at the bottom: “while findings from....”, sentence seems incomplete. Same problem as in the intro, my first comment.	Updated the statement to read: <i>“This study found no significant reduction in admissions, however, a large scale cohort study found...”</i>
Discussion	P. 10 states “the one previous review of RPM in COPD”. I am confident that there are multiple reviews AND meta-analyses on RPM in COPD.	We agree that there are multiple reviews on RPM in COPD but not as many that report specifically on acute care use outcomes. We have updated our statement from “The one” to simply “A previous review”
Overall	As a final general comment, I think if the authors were to exclude non-COPD/CVD studies, with the 3 outcomes that they are looking for, they could have conducted a meta-analysis, which would have been more informative and potentially reliable. This would have allowed for a quantifiable comparison between patient populations and also between invasive and non-invasive RPM, which could have been a considerable contribution to the literature. I recognize that may this is not feasible at the moment, but it could be something to be considered for the	A good thought for the future, thank you for your comment. In the present study, we did not want to restrict to certain chronic diseases as COPD and CVD are often accompanied by other comorbidities. While there may be other disease specific reviews already published we feel the fact that ours does not discriminate is what sets it apart.

	future given the substantial work put into collating all studies.	
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**VERSION 2 – REVIEW**

<b>REVIEWER</b>	Vess Stamenova Women's College Hospital, Toronto, Ontario, Canada
<b>REVIEW RETURNED</b>	25-Jan-2021

<b>GENERAL COMMENTS</b>	<p>Thanks for responding to my comments. Overall, I am relatively satisfied with the responses and I think the decision is the editor's at this point.</p> <p>I think for me what is lacking still is the question around why is this review needed. I am not saying that it is not, but I feel the authors need a better justification than:  "Previous studies of RPM and their impact on acute health services have largely focused on heart failure populations and manual collection of biometric data. Remote monitoring technologies have improved to now include automatic data collection using implanted devices and the use of RPM for other disease conditions. We present a contemporary review of the effectiveness of RPM in the context of hospital admissions, length of stay and emergency department presentations."</p> <ol style="list-style-type: none"> <li>1. Hospital/acute care outcomes is not a novel component</li> <li>2. Acute health services have not focused only on CHF, there is lots of COPD literature on the matter</li> <li>3. The newer technologies point seems like a good point</li> <li>4. I'd also maybe point out that this is an update...maybe state that the RPM technologies and world is rapidly evolving and thus we need more regular reviews.</li> <li>5. Maybe strengthen the point that the past literature is a bit segregated by conditions.</li> </ol> <p>While I feel like the authors have done a comprehensive summary of past literature for inclusion of studies in their review, I am still a bit concerned that they have chosen to omit describing past reviews on the same topic and how their review contributes over others.</p> <p>The authors state: "We feel we have adequately referenced previous reviews in the Introduction as there are not many focused on acute care use, which is focus of and rationale for this study."</p> <p>I am more familiar with the COPD literature and even my review is outdated by about a year. Yet in COPD I know there have been some reviews from about a year ago that include acute care as outcomes (ED, hospitalizations) as these outcomes are common in this type of research. Some examples are listed below. None of these reviews are mentioned in this paper. This is only in COPD and I imagine the CHF literature may have even more.</p> <p>McBain, Hayley, Michael Shipley, and Stanton Newman. "The Impact of Self-Monitoring in Chronic Illness on Healthcare Utilisation: A Systematic Review of Reviews." BMC Health</p>
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	<p>Services Research 15, no. 1 (June 2015).  <a href="https://doi.org/10.1186/s12913-015-1221-5">https://doi.org/10.1186/s12913-015-1221-5</a>.          Kruse, Clemens, Brandon Pesek, Megan Anderson, Kacey Brennan, and Hilary Comfort. "Telemonitoring to Manage Chronic Obstructive Pulmonary Disease: Systematic Literature Review." JMIR Medical Informatics 7, no. 1 (2019): e11496.  <a href="https://doi.org/10.2196/11496">https://doi.org/10.2196/11496</a>.          Hong, Youna, and Seon Heui Lee. "Effectiveness of Tele-Monitoring by Patient Severity and Intervention Type in Chronic Obstructive Pulmonary Disease Patients: A Systematic Review and Meta-Analysis." International Journal of Nursing Studies 92 (April 1, 2019): 1–15. <a href="https://doi.org/10.1016/j.ijnurstu.2018.12.006">https://doi.org/10.1016/j.ijnurstu.2018.12.006</a>.          Janjua, Sadia, Christopher JD Threapleton, Samantha Prigmore, and Rebecca T. Disler. "Telehealthcare for Remote Monitoring and Consultations for People with Chronic Obstructive Pulmonary Disease (COPD)." Cochrane Database of Systematic Reviews, no. 11 (2018). <a href="https://doi.org/10.1002/14651858.CD013196">https://doi.org/10.1002/14651858.CD013196</a>.</p> <p>Ultimately, the decision is left to the editor. If they feel that this is not as important, then the paper can proceed as is.</p>
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### VERSION 2 – AUTHOR RESPONSE

Section	Comment	Response
<b>Overall</b>	Thanks for responding to my comments. Overall, I am relatively satisfied with the responses and I think the decision is the editor's at this point.	Thank you for reading our responses.
<b>Introduction</b>	<p>I think for me what is lacking still is the question around why is this review needed. I am not saying that it is not, but I feel the authors need a better justification than:</p> <p>"Previous studies of RPM and their impact on acute health services have largely focused on heart failure populations and manual collection of biometric data. Remote monitoring technologies have improved to now include automatic data collection using implanted devices and the use of RPM for other disease conditions. We present a contemporary review of the effectiveness of RPM in the context of hospital admissions, length of stay and emergency department presentations."</p> <ol style="list-style-type: none"> <li>1. Hospital/acute care outcomes is not a novel component</li> <li>2. Acute health services have not focused only on CHF, there is lots of COPD literature on the matter</li> <li>3. The newer technologies point seems like a good point</li> <li>4. I'd also maybe point out that this is an update...maybe state that the RPM technologies and world is rapidly evolving and thus we need more regular reviews.</li> </ol>	<p>We have updated our Introduction to strengthen the rationale for the review:</p> <p><b>(Pg3, L 113-120)</b>  <i>"There have been a number of disease specific reviews (such as for heart failure and COPD) that have reported effect of RPM on acute hospital use, however this is often a secondary outcome.5, 10-14 Furthermore, these reviews were largely published more than five years ago. Hence, there is limited evidence for the effect of RPM using newer technologies such as implanted devices and for other disease conditions.15 With numbers of new RPM technologies</i></p>

	<p>5. Maybe strengthen the point that the past literature is a bit segregated by conditions.</p>	<p><i>substantially increasing in research trials and in the marketplace, more regular reviews of the literature are warranted. The aim of this study is to provide a contemporary evidence synthesis that will determine if the latest RPM tools being used across condition types are reducing acute hospital use.</i></p>
<p><b>Introduction</b></p>	<p>While I feel like the authors have done a comprehensive summary of past literature for inclusion of studies in their review, I am still a bit concerned that they have chosen to omit describing past reviews on the same topic and how their review contributes over others. The authors state: “We feel we have adequately referenced previous reviews in the Introduction as there are not many focused on acute care use, which is focus of and rationale for this study.”</p> <p>I am more familiar with the COPD literature and even my review is outdated by about a year. Yet in COPD I know there have been some reviews from about a year ago that include acute care as outcomes (ED, hospitalizations) as these outcomes are common in this type of research. Some examples are listed below. None of these reviews are mentioned in this paper. This is only in COPD and I imagine the CHF literature may have even more.</p> <ul style="list-style-type: none"> <li>• McBain, Hayley, Michael Shipley, and Stanton Newman. “The Impact of Self-Monitoring in Chronic Illness on Healthcare Utilisation: A Systematic Review of Reviews.” <i>BMC Health Services Research</i> 15, no. 1 (June 2015). <a href="https://doi.org/10.1186/s12913-015-1221-5">https://doi.org/10.1186/s12913-015-1221-5</a>.</li> <li>• Kruse, Clemens, Brandon Pesek, Megan Anderson, Kacey Brennan, and Hilary Comfort. “Telemonitoring to Manage Chronic Obstructive Pulmonary Disease: Systematic Literature Review.” <i>JMIR Medical Informatics</i> 7, no. 1 (2019): e11496. <a href="https://doi.org/10.2196/11496">https://doi.org/10.2196/11496</a>.</li> <li>• Hong, Youna, and Seon Heui Lee. “Effectiveness of Tele-Monitoring by Patient Severity and Intervention Type in Chronic Obstructive Pulmonary Disease Patients: A Systematic Review and Meta-Analysis.” <i>International Journal of Nursing Studies</i> 92</li> </ul>	<p>Thank you to the reviewer for highlighting a number of important papers in this area, two of which we have now added to our Introduction and Discussion section as references.</p> <p>Update to Introduction: <b>(Pg3, L 113-120)</b> <i>“There have been a number of disease specific reviews (such as for heart failure and COPD) that have reported effect of RPM on acute hospital use, however this is often a secondary outcome.”</i> [added Kruse et al. 2019 &amp; Hong and Lee 2019 to existing 4 references]</p> <p>Update to Discussion: <b>(Pg10, L 362)</b> <i>“There has been a number of previous reviews of RPM for COPD populations.”</i> [changed wording and added Hong and Lee 2019 to existing reference]</p> <p><b>(Pg10, L 385-387)</b> <i>“Further to this, the severity of disease can also be a determining factor of how effective an RPM intervention will be in reducing acute care use.”</i> [cited Hong and Lee 2019]</p> <p>Note: We did not feel McBain 2015 was appropriate to cite in this instance as this paper summarised self-monitoring which was different to our focus on remote patient monitoring. We emphasize in our review a</p>

	<p>(April 1, 2019): 1–15.  <a href="https://doi.org/10.1016/j.ijnurstu.2018.12.006">https://doi.org/10.1016/j.ijnurstu.2018.12.006</a>.</p> <ul style="list-style-type: none"> <li>Janjua, Sadia, Christopher JD Threapleton, Samantha Prigmore, and Rebecca T. Disler. “Telehealthcare for Remote Monitoring and Consultations for People with Chronic Obstructive Pulmonary Disease (COPD).” <i>Cochrane Database of Systematic Reviews</i>, no. 11 (2018).  <a href="https://doi.org/10.1002/14651858.CD013196">https://doi.org/10.1002/14651858.CD013196</a>.</li> </ul>	<p>clinician actively reviewing biometric patient data, as opposed to self-interpretation of data or self-adjustment of treatment as was included in the McBain article.</p> <p>We also did not include the Janjua 2018 paper as it was a protocol for a Cochrane review without outcomes reported.</p>
	<p>Ultimately, the decision is left to the editor. If they feel that this is not as important, then the paper can proceed as is.</p>	<p>Thank you for your comment.</p>