Comparative Effectiveness Research Review Disposition of Comments Report

Research Review Title: *Transitional Care Interventions To Prevent Readmissions for People With Heart Failure*

Draft review available for public comment from October 29, 2013 to November 26, 2013.

Research Review Citation: Feltner C, Jones CD, Cené CW, Zheng Z-J, Sueta CA, Coker-Schwimmer EJL, Arvanitis M, Lohr KN, Middleton JC, Jonas DE. Transitional Care Interventions To Prevent Readmissions for People With Heart Failure. Comparative Effectiveness Review No. 133. (Prepared by the Research Triangle Institute–University of North Carolina Evidence-based Practice Center under Contract No. 290-2012-00008-I). AHRQ Publication No. 14-EHC021-EF. Rockville, MD: Agency for Healthcare Research and Quality; May 2014. www.effectivehealthcare.ahrq.gov/reports/final.cfm.

Comments to Research Review

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The tables below include the responses by the authors of the review to each comment that was submitted for this draft review. The responses to comments in this disposition report are those of the authors, who are responsible for its contents, and do not necessarily represent the views of the Agency for Healthcare Research and Quality.

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Comment Number	Commentator & Affiliation	Section	Comment	Response
1	Peer Reviewer 1	General	The literature review was comprehensive; I am unaware of any studies that were not considered for inclusion in the analyses. The manuscript is well organized and exceptionally well written. The focus of the systematic review is explicitly defined.	Thank you.
2	Peer Reviewer 1	General	The findings, however, are generally consistent with previous reviews, and it is not clear to this reviewer that the report provides novel information that significantly informs clinical practice.	Unlike most other recent reviews, this review focuses on short-term outcomes (less than 6 months). Although the conclusions are generally consistent with previous reviews, they are more applicable to patients who have been recently discharged and some conclusions differ from other reports. We have expanded the discussion to include more detail on how (and why) our results differ from other recent reviews.
3	Peer Reviewer 1	General	In general, the key questions are appropriate and clearly articulated. However, a major limitation of the report is that no attempt was made to assess cost-effectiveness of the various interventions. In the current healthcare climate, it is critically important to have information about the cost implications of transitional care interventions. While it is true that most of the published studies did not assess costs, several did, and it should have been possible to at least provide preliminary estimates of cost-effectiveness. Thus, it seems that an opportunity to inform this issue was missed by not incorporating a key question on cost-effectiveness into the study design.	Thank you, In terms of the study design, we were not conducting a cost-effectiveness analysis. We agree that such an analysis would be an important contribution to this literature, but it was beyond our scope.
4	Peer Reviewer 2	General	Yes, it is clinically meaningful. Target population and audience are defined.	Thank you.
5	Peer Reviewer 2	General	Great questions asked but unfortunately they were not able to answer most of them.	We were able to adequately address most KQs- either by synthesizing the literature or by highlighting important research gaps when literature was insufficient (such as subgroups of questions).
6	Peer Reviewer 2	General	Thank you for inviting me to review this very interesting meta-analysis of the studies conducted since 1990 in the area of transitional care for adults with heart failure (HF). I have studied the lengthy document and summarize my thoughts here.	Thank you.

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7	Peer Reviewer 2	General	In general, I was surprised at the breadth of studies included under the heading of 'transitional care'. You note that you use the term "transitional care" broadly and later specify definitions from Naylor and from Coleman. Thus, I can see the rationale for including all these studies. My surprise stems from the fact that numerous meta-analyses of these trials have been conducted over the past decade. In general, the results of this meta-analysis are consistent with those of prior studies, supporting the validity of the approach used.	Our review differs mainly in terms of outcome timing (the focus is on readmission rates < 6 months versus longer outcome timings) and by including the most recently published literature. As this reviewer notes below, some of our findings are consistent with other reviews and some are not.
8	Peer Reviewer 2	General	Some conclusions differ, however. For example, you conclude that structured telephone support is effective while others (e.g. Sochalski et al, 2009) concluded that "patients enrolled in programs using multidisciplinary teams and in programs using in-person communication had significantly fewer hospital readmissions and readmission days than routine care patients had" (p.179). Some explanation for why you reached different conclusions from prior meta-analyses would be useful. Perhaps this was included and I missed it; there was a lot of redundancy in the document making it difficult to concentrate on the detail.	Unlike most other recent reviews, this review focuses on short-term outcomes (less than 6 months). Although the conclusions are generally consistent with previous reviews, they are more applicable to patients who have been recently discharged and some conclusions differ from other reports. We have expanded the discussion to include more detail on how (and why) our results differ from other recent reviews. Regarding the publication mentioned (Sochalski 2009); this study differs significantly from our review in terms of methods and inclusion/exclusion criteria. It was not a systematic review; instead, it pooled 10 RCTs and re-analyzed data looking at specific factors. All RCTs selected were published by the authors of that review. There was no systematic search for evidence (to make sure they captured all relevant studies) and no assessment of risk of bias.

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9	Peer Reviewer 2	General	Self-care: Throughout the manuscript you use the terms “self-care”, “self-management” (page 16), “self-care management” as if they were all synonymous. They are not the same. Please pick one term and use it consistently.	We have clarified these terms; generally using “self-care” when referring to education or training on how to recognize symptoms of HF and respond appropriately (e.g., taking additional diuretics). However, these terms appear to be used synonymously in many publications. When describing intervention components, we used the terms in the way that trial authors used them.
10	Peer Reviewer 2	General	Also, you refer to “self-care burden”. I have never heard of this term and I am not sure that I understand what you are referring to. If you use this term in the final draft please provide a conceptual definition of it. I would encourage you to choose a different term.	We added a sentence to the Methods chapter that provides more detail about how we conceptualized self-care burden. This is a term that refers to the burden or stress experienced by patients undergoing treatment. For example, this asks about the burden experienced by having more appointments. This was suggested by one of our Key Informants during topic refinement.
11	Peer Reviewer 2	General	You use many, many nonstandard abbreviations throughout the text. For example on page 45: “We graded the SOE to answer KQs on the benefits and harms of the interventions in this review, using the guidance established for the EPC program.” To me this is like speaking in code. I would appreciate fewer abbreviations.	At the beginning of each chapter, all abbreviations are called out. Also, we include a glossary of terms at the end of the document to help ensure that abbreviations are clear.
12	Peer Reviewer 2	General	In several places in the document you note that one component of effective interventions is “mechanisms for postdischarge medication adjustment”. However, I did not see the evidence for this statement. Can you please clarify where this statement came from?	The evidence supporting this is detailed in KQ3 (“what are the components of effective interventions”). Effective interventions had a mechanism in place for postdischarge medication adjustment (e.g., during a home visit or during frequent outpatient clinic follow-up).
13	Peer Reviewer 2	General	Finally, perhaps the redundancy is a function of the format required for the document. But if it is not required, you may want to limit it, as less redundancy would encourage more people to read the document.	Thank you for the feedback. We intend the ES as a stand-alone document for those who want a briefer overview of this review. The main report is more technical in nature, and may be of more interest to those who want very detailed information on methods or results.
14	Peer Reviewer 2	General	No specific comments.	Thank you.

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15	TEP Reviewer 3	General	Overall this is a methodologically well done and generally clearly written report that addresses key populations, interventions and outcomes of considerable interest.	Thank you.
16	TEP Reviewer 3	General	The greatest value of this report will be to inform health care systems and policy makers in developing transitional care approaches to improve the health care of people with heart failure, in part by preventing hospital readmissions. My comments are primarily directed at: 1) improving the clarity of the message for groups/individuals/policy makers having to make decisions based on the complex/large amount of information available, 2) determining whether the findings are of sufficient strength and applicability and 3) attempt to resolve possible discrepant findings/conclusions in the EPC report from prior systematic reviews.	We appreciate this feedback and perspective.
17	TEP Reviewer 3	General	Title and main outcome: the title of the report is "Transitional care interventions to prevent readmissions for people with heart failure: Emphasis should be on readmissions though agree that other outcomes (especially mortality) are of clinical importance.	Thank you; we have noted that readmissions and mortality are our primary outcomes.
18	TEP Reviewer 3	General	Perhaps additional discussion in the intro;/discussion as to why other outcomes selected and why people should care. This is likely because:1) only 1/3 of readmissions are for CHF reasons 2) other outcomes are of interest and for some a readmission may either be a good thing, not the full picture (i.e. unavoidable or unrelated) and 3) perhaps should not be a performance measure 4) there is a potential to "game the system" when looking only at readmissions (especially CHF readmissions)…sometimes detrimental to the patient	Thank you. We have highlighted points 2, 3 and 4 in the discussion. And we have noted that other outcomes (e.g., emergency room visits) are important because an increase in these outcomes may be an unintended consequence of measures aimed at reducing readmissions. However, we don't think that it is in the scope of this review to speculate about whether or 30-day readmission rates should be a performance measure.
19	TEP Reviewer 3	General	There are several typos that need proofing and correcting A phrase notes most studies were multicenter…but it looks like it was close to 50/50 (7 vs. 7)	We have corrected this- 8 were multicenter and 7 single center.

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20	TEP Reviewer 4	General	This is a well written systematic review evaluating transitional care interventions to prevent readmissions for people with heart failure. This is an important report clinically since there are many transitional programs available, and the use of these programs is variable dependent on institution, referring physician and primary care physician. This review helps to identify the strength of available data in support of these programs, in which clinical situations each program is the most useful, and the efficacy of programs on overall outcomes.	Thank you.
21	TEP Reviewer 4	General	Although not explicit in the title, the target population focused in this review is patients with chronic heart failure, who have an increased risk for readmissions, morbidity, and mortality.	We did not specifically exclude articles based on whether investigators noted the chronicity of heart failure of included patients (this was not often described). Patients were those admitted with HF during an index hospitalization (regardless of chronicity or underlying etiology).
22	TEP Reviewer 4	General	The key questions are appropriate to this specific population and are clearly stated. The analytical framework is presented well.	Thank you.
23	TEP Reviewer 5	General	This is a well-done, comprehensive, and methodologically rigorous review of a clinically important topic.	Thank you.
24	TEP Reviewer 5	General	The rationale, inclusion criteria, key questions are well described and appropriate.	Thank you.
25	TEP Reviewer 6	General	This report covers a lot of ground and the authors are to be commended in their efforts to make sense of a diverse group of interventions and potential effectiveness.	Thank you.
26	TEP Reviewer 6	General	Overall, I think the systematic review was performed rigorously.	Thank you.
27	TEP Reviewer 6	General	I found the report difficult to digest because of the shear number of facts and permutations. That said, it is impressive how the authors have tried to make sense of this literature!	Thank you. We intend the executive summary to be a condensed version of this report. The full report does include more information for those who want to review methods or other detailed information of included studies.

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28	TEP Reviewer 6	General	I do not question the underlying conclusions of the report. Because of the number of permutations of outcomes and the relatively small number of trials, I question whether some of the fine distinctions between effectiveness can be made. At times, they came across as too certain in the report. Although going through a systematic review like this highlights to the authors all the intricacies, they are lost on the reader. I would prefer to take a step back and say: what works for patients as they leave the hospital for HF? From this report, it seems that home visits and/or MDS-HF clinics work. And, it seems that telemonitoring does not. Telephone support might help, but probably not as efficacious and may be one part of a bigger program. Education alone not too effective, but it is part of every program. I don't think readers will take more away than that (and I'm not sure they should).	This is an accurate summary of the main findings related to readmission and mortality outcomes. We revised the Executive Summary and the report to make these bottom line messages more clear. Also, to help summarize the evidence, we have combined the 3 and 6 month outcome timings, as suggested by other reviewers.
29	TEP Reviewer 6	General	Most of my comments target greater clarity in the description of the findings and interpretation. Specific comments: 1. At times, it felt like I was re-reading the same things over and over again. Some of this may be related to the fact that the outcomes are very similar (e.g., 3 month readmission or 6 month readmission), but each outcome is analyzed independently. Notably, there was not much rationale as to why each outcome would be considered independently. Specifically, it seems problematic that a given category may have 5 studies for the 3 months outcome and 2 studies for the 6 month outcome and they are meta-analyzed independently and allowed to draw different conclusions (which may be largely drawn by inclusion of different studies (with different interventions even though categorized similarly).	Thank you for this feedback. As noted above, we have collapsed the 3 and 6 -month outcome timings. We have kept the 30-day outcome timing separate.
30	TEP Reviewer 6	General	There is very little discussion of the potential effects of study inclusion criteria on the results. I suspect some of the variation in results relates more to who was included in the study rather than the actual intervention. At least, this should be mentioned.	We have added a comment to the study limitations section (Executive Summary and Discussion) regarding population heterogeneity of included trials.

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31	TEP Reviewer 6	General	Usual care is a conundrum here. Report comments on the unknown changes in usual care over time (1990's to the 2000's). But, variation in care across individual sites is probably greater than changes over the decades. A trial done in a site with poor usual care is likely to show greater benefit from these types of interventions. This also has large implications for those of us trying to implement programs. How do we compare what is 'effective' with our usual care. The authors of the report may not have enough information to address completely, but some discussion seems warranted.	We agree that this is a challenging issue to address for this literature. We include lack of description of usual care as a limitation of the evidence base. We added some additional discussion of the implication of variation in usual care to the discussion section.
32	TEP Reviewer 6	General	Categorization of interventions: the strategy seems ok to me. Did the authors query the authors of the studies to determine whether they agree with the classification? The determination of 'level of intensity' was not very well described (unless I missed it). It would be nice if you could state your criteria for low, medium, high intensity. Right now it seems to be subjective based on the reviewers. It is important for users of the report to understand the differences between low, med, high.	Criteria used to classify trials as low, medium or high intensity is included in the methods section of both the executive summary and full report. We only queried authors about intervention categorization in one or two cases (when there was disagreement among investigators on initial categorization). In the vast majority of cases, the interventions were categorized based on the authors' own (published) description of the study intervention (so there was no need to contact them). We do discuss how alternative ways of classifying interventions may lead to different conclusions (Discussion section).
33	Peer Reviewer 7	General	Overall rating: Fair – Good in terms of responding to key questions and structure of the report. Fair in terms of writing and editing of the report: The report has not been edited adequately.	Thank you for this feedback. An editor has reviewed and edited the document prior to resubmitting the final report.
34	Peer Reviewer 7	General	In addition to typos and erroneous table headings, there are many instances of inconsistent application of abbreviations. There are two sections that were incomplete with dangling sentences and repeated paragraphs. There are instances of circular statements and unnecessary repetitions.	These issues have been addressed (where they are called out specifically below).

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35	Peer Reviewer 7	General	**Key Questions** I understand that the Key Questions have already been formulated and the wording shouldn't be changed at this stage, my comments about the key questions are directed more for the future and as a generic issue for the EPC program. As currently framed, key questions 1 and 2 ask "…do transitional care interventions increase or decrease…" Taking the questions literally as "do", I expect the answer to be "yes" or "no". Key questions in EPC reports search for evidence to quantify estimates of effect when possible. Thus to answer the "do" question, quantitative answers will need to be interpreted (criteria will need to be developed and operationalized) to provide this dichotomous answer which often is not what reports aim to address. While the question of "do" is often what is requested by nominators of report topics, as EPCs [the systematic review authors] we provide answers to inform their decisions.	Thank you for this feedback. At this stage, we have chosen not to change the wording of the key questions. A different formulation may have accounted for the issue of including magnitude of effect. However, we worded the questions specifically in this way because of input from KIs regarding uncertainty over the direction of effect across outcomes following an intervention. For example, there was concern that interventions that reduce readmission may increase mortality. We wanted to capture potential benefit (decrease in readmission) as well as potential harms (increase in mortality) with all health utilization and mortality outcomes.
36	Peer Reviewer 7	General	A better formulation of the key question of this nature might be to ask "what is the magnitude", "how do they compare", "how much", etc.: "…how do transitional care interventions compare with usual care in the following health care utilization…" "…what is the difference of effectiveness between transitional care interventions and usual care in the following…" The difficulties of asking "do" also spills over to Key Question 3a which ask what are the components of "effective" interventions.	Thank you for this feedback. As stated above, we have chosen not to change the wording of the key questions at this stage. We worded the questions specifically in this way because of input from KIs regarding uncertainty over the direction of effect across outcomes following an intervention.
37	Peer Reviewer 7	General	The authors of the report attempted to address the approach to effectiveness on page 14 but it was never explicit in terms of providing a minimum magnitude of difference as a definition of "effective"; the reader is left to presumed that the definition is based only on the statistical significance.	We had discussions with key informants and TEP members regarding what constitutes a meaningful change in mortality and readmission. There seemed to be consensus that any reduction in these outcomes was important. We added text to the Discussion section noting that there is uncertainty regarding what constitutes a meaningful change in readmission rates.

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38	Peer Reviewer 7	General	Similarly, for Question 3b, "…are particular components necessary?" the meaning of "necessary" was never explicitly defined. Key Question 3c (labeled incorrectly as 3d in the Executive summary) asks "do particular components add benefit?" This question could be rephrased (not that it matters in this report since there is no evidence) as what is the magnitude of contribution of individual components. Without clearly defined meanings of "effective" and "necessary", Key Question 3 was not completely answered as stated.	We fixed the labeling of 3c. We intended "necessary" to mean whether a particular component must be present for an intervention to be effective (i.e., all effective interventions must have this component). We added this definition to the Methods section. The issue of magnitude of contribution of individual components was not a question that we aimed to answer. We feel that KQ3 has been addressed in as much detail as the literature allows, but agree that we found no evidence to assess the magnitude of contribution of individual components.
39	Peer Reviewer 7	General	Similarly for Key Question 4 in asking "Does the effectiveness of interventions differ based on…" Analytic framework diagram: I found the placement of KQ5 conceptually awkward. Key questions 3, 4, and 5 all ask the question of the effect of subgroups/components/intensity/etc. on the outcomes. The direction of the KQ5 arrow does not point to any of the outcomes, just the subgroups.	We feel that the text makes it clear that we are looking for evidence in the same outcomes among subgroups of patients with HF.
40	Peer Reviewer 14	General	The authors conducted a systematic review and meta-analysis of the randomized clinical trials assessing transitional care interventions in reducing readmission and mortality for adult patients hospitalized with heart failure.	Correct.
41	Peer Reviewer 14	General	They also assessed functional status and quality of life where applicable.	Correct.
42	Peer Reviewer 14	General	They evaluated the various components of effective intervention as well as the intensity, mode of delivery and method of communication taking into consideration the effect by subgroups including age, sex, race, ethnicity, disease severity and coexisting conditions or socioeconomic status, if available. The authors performed an exhaustive search of the literature, selecting eligible studies. The risk of bias was assessed and the strength of evidence was graded.	Correct.
43	Peer Reviewer 14	General	The authors identified 47 randomized controlled trials. The authors found that home-visiting programs, structured telephone support (STS), and multidisciplinary specialty heart failure (MDS-HF) clinic intervention improved at least one of the primary outcomes.	Correct.

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44	Peer Reviewer 14	General	The strength of evidence was moderate and the NNTS ranged from 5 – 10 for home visiting programs, 10 – 25 for STS interventions, and 7 – 13 for MDS-HF clinic interventions.	This has changed some with collapsing the 3 and 6-month time points. The final report reflects the changes in NNT for specific outcomes.
45	Peer Reviewer 14	General	The authors concluded that the efficacy of telemonitoring interventions and primarily educational interventions have not been established for reducing readmission or mortality.	We concluded that these intervention types are not effective for most readmission and mortality outcomes.
46	Peer Reviewer 14	General	The report is clinically meaningful. The target population and audience are explicitly defined. The key questions are appropriate and explicitly stated. The document is timely, authoritative, and very informative.	Thank you.
47	TEP Reviewer 9	General	The data are important given the focus on readmission by CMS. In particular 30-day all cause readmission is what concerns everyone involved in hospital care given the large financial penalties now in place.	Thank you.
48	TEP Reviewer 9	General	I would have liked to see cost of the intervention included in the questions. The data may not be available.	Cost is a difficult outcome to include (and interpret) without doing a formal cost-effective analysis. Assessment of cost and cost-effectiveness were beyond the scope of our review.
49	Peer Reviewer 11	General	The report is very meaningful and highlights clearly the lack of clarity around the readmission issue.	Thank you.
50	Peer Reviewer 11	General	The key questions are quite on the mark and reviewed with care.	Thank you.
51	TEP Reviewer 12	General	This is a thorough and helpful review.	Thank you.
52	TEP Reviewer 12	General	I believe the evidence reviewed in response to the key questions was appropriate, and reasons to exclude studies were well documented.	Thank you.
53	TEP Reviewer 12	General	Frankly, given the economic imperatives currently, it is somewhat surprising that the evidence base is as small as it is.	We agree that this topic should be a focus of more research.

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54	Peer Reviewer 13	General	The authors must be commended for undertaking this monumental project to which they have risen like the Phoenix and did a superb job of selecting (very transparent inclusion exclusion criteria); quality assessment (scoring, although the methodology selected was not found by me. Perhaps I overlooked it); data extraction (superb on all levels); data synthesis [exceptional and meritorius]; presentation of the tables and the overall manuscript [simply marvelous, rigorous and cautious, keeping it close to the information provided and supported by the selected studies; conclusion [tepid!!!!!!]	Thank you. We have the detailed description and specific questions used for risk of bias assessment in the Methods section and in Appendix D. We assessed the risk of bias of studies using predefined criteria based on the AHRQ Methods Guide; including questions to assess selection bias, confounding, performance bias, detection bias, and attrition bias. Appendix D also includes a table showing the responses to these questions and risk-of-bias ratings for each study and then an explanation of the rationale for all ratings that were either high or unclear.
55	Akshay Desai, Public Reviewer	General	This document represents an important and thorough survey of the available evidence regarding transitional care interventions to improve outcomes following heart failure hospitalization. Meta-analytic approaches are always hampered by between trial heterogeneity, and in this regard, the substantial variation in the specific interventions studied in the various included trials limits the ability to draw the definitive conclusions that are adequate to set guidelines or drive policy changes.	Thank you. We disagree that no definitive conclusions adequate to inform guidelines or changes in policy. Whether this information is sufficient to set guidelines or drive policy is not without our scope to determine; other factors would be important in those decisions, such as the availability of resources. We identified intervention categories (e.g. home-visiting interventions and multidisciplinary interventions) that were effective in reducing readmission and mortality outcomes, and that should be the focus for those trying to implement interventions to prevent readmissions for people with HF.
56	Akshay Desai, Public Reviewer	General	The authors suggest that three categories of interventions have shown compelling evidence of efficacy in reducing readmissions/mortality including high-intensity home visits, structured telephone support, and multidisciplinary HF clinic interventions, while others (stand-alone telemonitoring, primary education interventions, nurse-led HF clinic interventions) have not. However, it is difficult to infer from this observation clear guidance for hospitals or healthcare providers about what specific components of these interventions are likely to be essential to translating the success observed in clinical trials into real-world clinical practice.	KQ3 gives more specific guidance regarding components of interventions for which we found efficacy. However, we agree that setting up these interventions (any intervention to reduce readmissions) is a challenge for hospitals and healthcare providers.

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57	Akshay Desai, Public Reviewer	General	Moreover, no single strategy of post-discharge care is likely to be effective for every patient; the successes achieved in clinical trials for specific interventions may be peculiar to the populations studied, and even 'ineffective' interventions may have their role in supporting post-discharge management in the right context. As an example, telemonitoring, while not effective as an adjunct to multidisciplinary HF management, may yet be beneficial for patients who would otherwise have no access to subspecialty HF follow up (for example, due to distance or paucity of local provider support in underserved areas).	While this may be possible, we did not find evidence to support the use of telemonitoring in specific subgroups.
58	Akshay Desai, Public Reviewer	General	I applaud the effort to highlight the essence of effective transitional care interventions, and the components highlighted by these authors (education for self-care, promotion of appropriate pharmacotherapy and adherence, face-face contact after discharge, streamlined mechanisms for contacting health care providers, mechanisms for post-discharge medication reconciliation) are intuitively important. As well, it does seem likely that more intensive interventions implemented early post-discharge may be more efficacious than lower intensity interventions implemented later on. However, it remains unclear which specific components are responsible for the enhanced outcomes seen in practice. Clearly, as the authors acknowledge, there is a need for additional work in this regard to provide more concrete guidance to practitioners about how to effectively improve HF outcomes after discharge. It is not enough to simply admonish providers to utilize a 'multidisciplinary HF disease management program' without detailing the aspects of that program that are critical to success.	We agree that more research is needed, and we highlight some of the relevant issues in our Evidence Gaps section. Future research may help to better clarify which specific components provide the most benefit.
59	Akshay Desai, Public Reviewer	General	The greatest strength of this document may be to highlight the evidence 'gaps' (which are plentiful) in this field. These gaps help to define a research agenda that may help guide a pathway forward into the next phase of heart failure disease management. The breadth of these gaps, however, makes it difficult to provide anything more than very general guidance to practitioners who are struggling to meet the fiscal demands imposed by the 30-day readmission metrics embedded within the Patient Protection and Affordable Care Act.	Thank you. We agree that future research will be important
60	Akshay Desai, Public Reviewer	General	Thank you for the opportunity to comment on this report.	You are welcome.

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61	Peer Reviewer 7	Abstract	Line 27: The letter "h" is missing from the last word of this line.	This has been corrected
62	Peer Reviewer 7	Abstract	Line 30: The abbreviation SOE was already defined in line 23, no need to repeat it.	Thank you. We fixed this.
63	Peer Reviewer 7	Abstract	The conclusion statement "Our results suggest . . . have the best evidence supporting their efficacy . . ." could be reworded to be more straightforward such as "Moderately strong evidence supports the use of home-visiting program, STS, . . . for reducing readmissions . . ." As currently stated, "best evidence" is relative to alternatives which could be insufficient and in that case the best evidence may well be weak, whereas "moderately strong evidence" is defined within the SOE context of the EPC methods guide.	That you for this suggestion. We have removed the "best evidence" phrases to avoid confusion.
64	TEP Reviewer 6	Executive Summary	ES-9, Table B. Can you indicate some level of statistical significance for the hazard ratios? I can't determine (easily) just from the SE. CI would be easiest.	These are not HR that we calculated; the HR and SE were reported in the Naylor 2004 trial. They did not report CI.
65	TEP Reviewer 6	Executive Summary	Throughout the report, the language gets a little imprecise when describing findings. This should be tightened throughout the report. Specifically, distinguish between 'not effective' and 'not enough evidence to decide'. For example, ES-11, line 6: shouldn't you say 'we found telemonitoring or primarily educational interventions to be NOT efficacious…" Currently, you say "did not find telemonitoring or primarily ed intervention to be efficacious…" could be interpreted as either NOT efficacious or NOT enough evidence to determine.	Thank you for this feedback. We have revised the language (as suggested) to help readers understand when we did not find evidence vs. when interventions were not efficacious.
66	TEP Reviewer 6	Executive Summary	Down to line 10: "Evidence was insufficient to support the efficacy of the following interventions in …" Isn't it more accurate to say: Evidence was insufficient to DETERMINE the efficacy? Supporting the efficacy implies vagary as to whether there was evidence that it doesn't work or there just isn't evidence. These vagaries are throughout the report and need to be precise. Down to line 31 on ES-11 you have a very clear statement in this regard. Make them all like this.	We agree that "support the efficacy" can be vague; we have replaced this with "determine" or other more precise wording when appropriate.
67	TEP Reviewer 6	Executive Summary	ES-11 to ES-12: Need to address the reach issues for home visits versus MDS-HF. I suspect most studies of MDS-HF do not enroll patients that can't make it back to the clinic. This is a big deal! Again, returning to the problem of how do you get into the study.	We address this as an issue of applicability. Thank you.

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68	TEP Reviewer 6	Executive Summary	ES-12: line 43 and line 45 seem to have conflicting sentences back to back.	We made some edits to these sentences to clarify the wording so that they don't seem conflicting. We now say "Within most categories, evidence was insufficient to draw definitive conclusions about whether higher- or lower-intensity interventions are more or less efficacious in reducing all-cause readmissions or mortality. The one exception was…"
69	TEP Reviewer 6	Executive Summary	ES 13 discussion: in first paragraph, STS is included in efficacious strategies, but left out down at line 40. What is the difference? I know there is one, but it becomes too subtle. Do you really have the confidence to draw these distinctions based on your study design and the available evidence?	The paragraph at line 40 is referring to strategies that were effective in reducing all-cause readmission (STS interventions did not reduce all-cause readmission). We are confident in the conclusions made.
70	TEP Reviewer 6	Executive Summary	ES 17: line 38: "does not establish the efficacy" is too vague. Is it "current evidence suggest that telemonitoring is ineffective."? Or "current evidence is inadequate to determine the efficacy of telemonitoring." (along with comment 6 above…this is throughout the report)	Thank you. As noted above, we have clarified this language.
71	Peer Reviewer 7	Executive Summary	ES-1, Line 9: United States is included in the abbreviation list on page 87 but its first use was not abbreviated. Similarly, United States was not abbreviated in line 30. However, abbreviation of U.S. appears on page ES-13, line 46. Suggest you do a global search for consistency.	This was searched and abbreviations were made consistent, following AHRQ guidance.
72	Peer Reviewer 7	Executive Summary	ES-7, line 57: STS was used before spelling out first and then on page ES-9, line 15: structured telephone support (STS) was spelled out and abbreviated	Thank you. We corrected it.
73	Peer Reviewer 7	Executive Summary	ES-9-11, Efficacy for Reducing Readmissions and Mortality: This section is hard to follow and could be streamlined. Some of the results appear to be repeated unnecessary. In Particular, the results of home visiting program reducing all-cause readmission and the combined outcomes at 30 days were restated in various forms on ES9, line7-9; ES10, line 46-47; ES-11, Line 7-9 (this paragraph starts by discussing telemonitoring); in the discussion section, and in the conclusion section. The problem appears to be that you primarily organized Table B according to Intervention category and then secondarily the outcomes, but you organized the text (subsection headings) according to outcomes and then embedded the interventions within the outcomes.	We reorganized this section. We collapsed data (e.g., combining 3 and-6 month outcome), helping to improve readability in this section.

Comment Number	Commentator & Affiliation	Section	Comment	Response
74	Peer Reviewer 7	Executive Summary	Line 38-39: “Readmissions following an index hospitalization for HF appear to be related to various conditions.” Since this sentence did not refer to specific conditions that may be uncertain, the words “appear to be” seems inappropriate since readmissions have to be related to some factors/conditions.	Thank you; we’ve removed “appear to be” and replaced it with “are”.
75	Peer Reviewer 7	Executive Summary	NNT results were reported in the results section in Table B and bottom of ES-10, but the use of NNT was never mentioned in the methods (Data Synthesis) section. That is, you calculated NNT only when the results for an intervention-outcome category was statistically significant.	We added this information to the methods. Thank you.
76	Peer Reviewer 7	Executive Summary	In using the standardized mean difference, since this is a dimensionless unit that many readers probably are not familiar with, it would be useful to provide an interpretation of the magnitude of the results and provide this in the methods section.	We’ve provided an interpretation of Cohen’s d in the methods section (new text).
77	Peer Reviewer 14	Executive Summary	Page ES-16 (line 46); page 79 (line 21): The authors state that "interventions that show efficacy in RCTs may not perform differently under diverse settings". The authors may have meant "perform differently or may not perform similarly".	We changed the text here as suggested.
78	TEP Reviewer 12	Executive Summary	I particularly appreciated Table A-which defined the categories of interventions. This would be a helpful table to promulgate henceforth in the literature.	Thank you.
79	TEP Reviewer 12	Executive Summary	The discussion on page ES-8 (and relevant sections in main text) that discuss the severity of HF and relevant HF medications omit one major issue-the type of heart failure, e.g. HF with low LVEF(HFrEF) and HF with preserved LVEF(HFpEF). Although your tables outline LVEF in each study, the various studies are lumped together. It is difficult to know whether this is a major limitation, but it must be listed as one. The reason it may be relevant is that studies have yet to define effective treatment for patients with HFpEF, despite a number of studies designed to do so. If pharmacotherapy is difficult for these patients, what suggests that they might respond equally well to non-pharmacologic interventions	Thank you. The vast majority of studies do not differentiate between those with preserved vs. reduced Ejection Fraction (EF) (only noting that inclusion criteria require that patients be diagnosed with HF). Although pharmacotherapy may differ; we did not find trials that assessed differences in readmission rates among patients with preserved vs. reduced EF. We know of no evidence that suggests patients with preserved vs. reduced EF would respond differently to non-pharmacologic transitional care interventions. We added text to the Study Limitations sections regarding this point.

Comment Number	Commentator & Affiliation	Section	Comment	Response
80	TEP Reviewer 12	Executive Summary	Page S-2(and relevant sections in main text) *"Outcomes far away from the index hospitalization probably reflect the natural history of HF or an unrelated illness, rather than a preventable readmission related to the transition of care."* There is no reason to think that readmissions within 30 days do not represent natural history of HF either. Not all readmissions within 30 days are preventable. At issue is defining preventable readmission in severe HF.	Readmission within 30-days are more often related to various conditions (other than HF) compared with those farther away from the index hospitalization (more likely to be for HF). CMS measures 30-day readmission because there is evidence that suggests a higher proportion of early readmissions are preventable. We have changed this text to read: Outcomes far away from the index hospitalization probably reflect the natural history of HF or an unrelated illness, whereas a higher proportion of early readmissions are thought to be preventable.
81	TEP Reviewer 12	Executive Summary	Page ES-13(and relevant sections in main text) *"The two categories of interventions that reduced all-cause readmissions and the composite outcome (home-visiting programs and MDS-clinic interventions) are multicomponent, complex interventions. We could not separate out individual components from the overall bundle of interventions that showed efficacy; we found no single-component intervention that reduced all cause readmissions."* As an editorial comment, for discussion, the finding that the effective interventions are complex and require either time-intensive home visits or costly office visits with a MDS team are very significant for implementation in a scalable manner. Although cost of these interventions were not considered, the real cost of these interventions and the availability of trained personnel are real issues for hospital administrators. Reimbursement for much of these types of interventions do not support the level of time and personnel needed.	We agree with the editorial statement and do note that the effective interventions were more intense. We did not measure cost in this review, but do note that this is an important consideration for those implementing the interventions.

Comment Number	Commentator & Affiliation	Section	Comment	Response
82	TEP Reviewer 12	Executive Summary	Page ES-14 (and relevant sections in main text) *"Most studies included adults with moderate to severe HF. The mean age of subjects was generally in the 70s; very few studies enrolled patients who were, on average, either younger or older. We did not find evidence to confirm or refute whether treatments are more or less efficacious for many other subgroups, including groups defined by sex, racial or ethnic minorities, people with higher severity of HF, and those with certain coexisting conditions."* Again, as indicated above, a critical part of the populations studied have not been considered: what type of HF. (see above)	We addressed this concern above: "The vast majority of studies do not differentiate between those with preserved vs. reduced Ejection Fraction (EF) (only noting that inclusion criteria require that patients be admitted with a primary diagnosis of HF). Although pharmacotherapy may differ; we did not find trials that assessed differences in readmission rates among patients with preserved vs. reduced EF. We know of no evidence that suggests patients with preserved vs. reduced EF would respond differently to non-pharmacologic transitional care interventions. We added text to the Study Limitations sections regarding this point."
83	Peer Reviewer 2	Introduction	I would like to see a stronger rationale for calling all these approaches "transitional care" early in the introduction.	We do address this in the introduction and realize that there are differences in what defines both the "transitional period" and a "transitional care intervention." We also structured our inclusion and exclusion criteria to require that the populations of included trials were transitioning from hospital to home, and that the intervention was intending to improve outcomes over this period.
84	TEP Reviewer 4	Introduction	The introduction is well written and clearly states the burden of disease, use of transitional care programs and proposed advantages, and the current uncertainties in the evidence base. There is a good summary of existing guidelines currently in use clinically including the current reimbursement status associated with prevention of readmissions.	Thank you.
85	TEP Reviewer 4	Introduction	The key questions are again clearly summarized.	Thank you.
86			Suggested edit to the Introduction section: Suggest adding couple of lines on the proposed mechanisms by which transitional care programs possibly improve clinical and resource utilization outcomes.	Thank you. We have added this information.

Comment Number	Commentator & Affiliation	Section	Comment	Response
87	TEP Reviewer 4	Introduction	Please clarify if the target population is patients with chronic heart failure.	We included trials enrolling patients with HF (regardless of chronicity) who were transitioning from hospital to home. Most trials did not report on specifics of HF diagnosis beyond a measure of disease severity. However, many trials excluded people with HF related to another acute illness (e.g., HF in the setting of an acute MI).
88	TEP Reviewer 5	Introduction	Well written and appropriate	Thank you.
89	TEP Reviewer 5	Introduction	This report is challenged to determine what prevents readmissions in 30 days. The notion is that these readmissions are 'preventable' and, I believe, NOT the natural course of illness. At some point, the challenge changes to preventing admissions (not readmissions). Interventions to prevent early readmissions (<15-30 days) may be different from interventions to prevent admissions once a patient has returned to some level of stability. I think it would be helpful to make this distinction more clearly. The interventions described in this review are targeted more toward the long term reduction in admissions (just based on the time frame of the studies (3, 6, 12 months)). In fact, you only had 1 study (I think) that looked at 30 days. This may be a different set of interventions than what you would want to answer your original 30 day question.	We agree with this. To highlight this distinction, we have added a new summary table that gives an overview of SOE grades and direction of effect by intervention category and outcome timing (in the ES and Discussion). We have also collapsed the 3 to 6 month time period but have kept the 30 day outcome timing separate in the analyses.
90	Peer Reviewer 7	Introduction	Appropriate	Thank you.
91	Peer Reviewer 14	Introduction	Minor comments: Page ES-2 (line 12); page 2 (line 41): When the authors mention the 30-day readmission, they list among stakeholders: hospitals, payers, quality improvement organizations. The authors may wish to include health care providers, or specifically physicians, since in many settings accountable care organizations (ACO) are being formed in which physicians partner with hospitals to deliver better care at lower price.	We have added health care providers to this list in the introduction. Thank you.
92	TEP Reviewer 9	Introduction	The introduction was well done.	Thank you.

Comment Number	Commentator & Affiliation	Section	Comment	Response
93	Peer Reviewer 11	Introduction	The Intro is well written.	Thank you.
94	Peer Reviewer 11	Introduction	Suggest adding a definition of what the writing group considers moderate to severe HF. Several times that phrase is mentioned but never defined. It may take a small table showing the definition (which is probably quite varied) among the studies they have reviewed.	We defined this based on NYHA classification – i.e, most studies included a population with a mean NYHA classification of III or IV. We describe this in the section on study characteristics.
95	Peer Reviewer 11	Introduction	The target audience, although well described should perhaps include "providers" rather than clinicians only. Pharmacists are not clinicians but are considered providers.	We have realized that here is variability in how the term of “clinician” is used; some consider this to be any provider (e.g., nurse, psychologist, doctor, pharmacist) who provides direct patient care and not restricted to doctors or nurses. In the introduction, we have generally replaced the word “clinician” with “health care provider”; in other places, we have used the word “clinician” as it is used in the included trials to describe intervention personnel, but try to be as descriptive as possible.
96	TEP Reviewer 12	Introduction	Comprehensive introduction.	Thank you.
97	Sandra Oliver McNeil, Public Reviewer	Introduction	The introduction was very good.	Thank you.

Comment Number	Commentator & Affiliation	Section	Comment	Response
98	Tom Denberg, Public Reviewer	Introduction	A few general comments about the entire document: First, this work is incredibly helpful. Nicely done! Would help to include a table that summarizes at the highest level the type of intervention and whether it affected all cause readmissions, mortality, composite outcomes; along with NNT for all-cause readmits. A table of this sort will be very helpful to the reader who can easily get confused understanding the difference between HF-specific readmits and all-cause readmits, and between mortality and composite outcomes (all-cause readmits plus mortality), and between 30 day and 3 and 6 month outcomes. Such a table might also help the authors go back through the text and make sure there are no contradictory statements accidentally sprinkled about. Very important to define" multidisciplinary heart failure clinic (MDS_HF clinic)" -- what is this exactly? Who staffs it, who owns it, where is it located (bricks and mortar, any component virtual?) On page 35, claim that no interventions reduced 30-day all-cause readmits, but this contradicts text elsewhere that cites the Naylor study. Pg 41 has a repeated sentence about "19% of patients in a nurse run clinic..." There should be greater elaboration about evidence gaps related to: *use of primary care to manage certain categories of HF patients (unless patient is incredibly ill, the presumption is that lower cost primary cost clinics can do a decent job) * criteria around severity of disease warranting primary management in a cardiology run HF clinic/program * use of palliative care/advanced planning - very high mortality among HF patients and readmits and better outcomes can presumably be improved by more appropriate advanced-care planning rather than intensive education and medication optimization, etc. * challenges related to multidisciplinary programs for patients who live in rural areas Why does HF-specific readmission matter at all given challenges coding admission diagnoses correctly and given the need/desire to reduce all cause readmissions?	Thank you for this feedback. We have included an additional table (Table 20) summarizing readmission and mortality outcomes by intervention type. For this table (and others) we have also collapsed the 3 and 6 month time points (e.g., combining outcomes reported at 3 and 6 months). The MDS-HF clinic interventions are defined (and described) in the results section under the “Characteristics of included studies”. We have also added some detail regarding the applicability of these interventions in the discussion section. We have clarified the statement regarding 30-day all-cause readmission outcomes. We have added the issue of primary care interventions (vs. specialty clinic interventions) to the table of research gaps (ES and Discussion). We agree that HF readmissions may be less important (from a policy perspective) compared to all-cause readmissions. However, HF readmission is a common outcome in this literature. We note in the discussion that interventions that reduce both all-cause readmission and death should receive the greatest consideration.

Comment Number	Commentator & Affiliation	Section	Comment	Response
99	Thomas Denberg, Public Reviewer	Introduction	Additional comment: This report should also mention and address likely reasons for differences between its conclusions and those of the Cochrane report of 2012: The Cochrane report says: Amongst CHF patients who have previously been admitted to hospital for this condition there is now good evidence that case management type interventions led by a heart failure specialist nurse reduces CHF related readmissions after 12 months follow up, all cause readmissions and all cause mortality. It is not possible to say what the optimal components of these case management type interventions are, however telephone follow up by the nurse specialist was a common component. Multidisciplinary interventions may be effective in reducing both CHF and all cause readmissions. There is currently limited evidence to support interventions whose major component is follow up in a CHF clinic.	We have addressed this in the Discussion section. Briefly, this has to do with differences in inclusion/exclusion criteria regarding outcome timings (Cochrane review excluded outcome timings before 6 months). Also the Cochrane review combined home-visiting type interventions and STS, calling them both "case-management."
100	Sarah Goodlin, Public Reviewer	Introduction	The introduction, methods and discussion fail to address transitions to skilled nursing facilities, to hospice care or home health care. Although data about these sites of care are limited, they need to be acknowledged as important components in the web of care. Additionally, and related to hospice or home health care, identification of goals for care and election of hospice or palliation only might limit rehospitalization. While few data (or none?) address these components of care, they should be acknowledged as important.	Thank you- we specifically limited the scope of this review to patients who were discharged to home. Goals of care and appropriate interventions (although important) maybe be very different for patients who are in hospice care.

Comment Number	Commentator & Affiliation	Section	Comment	Response
101	Peer Reviewer 1	Methods	While the inclusion/exclusion criteria are well-defined, in my opinion they are overly restrictive. As a result, some of the largest and most important trials were excluded. For example, the 1518 patient DIAL trial (BMJ 2005), a positive study, was excluded because it only enrolled out-patients with heart failure (HF). The negative telemonitoring trial by Chaudry et al (NEJM 2010), which enrolled 1653 patients with a recent hospitalization for HF, was also excluded for unclear reasons. Similarly, several other smaller but nonetheless important studies (e.g. DeBusk) were also excluded. Unfortunately, exclusion of these studies not only compromises the power of the analyses, but also limits the scope and applicability of the study conclusions.	Those trials (although important) did not address our KQ- e.g., the prevention of early readmissions in patients recently discharged from HF. We do not see this as limiting the applicability, but improving the applicability of our review to patients who are recently discharged. The trial by Chaudry et al. recruited patients from outpatient centers who had been hospitalized at any time in the preceding 30 days, We were interested in trials recruiting patients during or within the 7 days following discharge. We excluded a trial by DeBusk and colleagues published in Annals of Internal Medicine in 2004; this trial reported outcomes at 1 year (excluded for wrong outcome timing). We agree that these trials are important, but they do not address the prevention of early (< 6 month) readmissions in patients transitioning from hospital to home.
102	Peer Reviewer 1	Methods	Another major limitation is that the authors apparently did not attempt to acquire patient-level data from transitional care trialists. Such data would be particularly valuable for examining the impact of transitional care interventions on 30-day outcomes (readmissions and mortality).	Correct- we did not attempt to acquire patient-level data from investigators. The study design of this review is a systematic review with meta-analysis, not an individual patient data meta-analysis. We do note that there are few published trials that report 30-day outcomes.
103	Peer Reviewer 1	Methods	As noted by the authors, 30-day outcomes were rarely reported in the primary papers. However, many publications included Kaplan-Meier curves, implying that time-to-event data were available, and that it would be feasible to reconstruct 30-day event rates using patient-level data. Lack of such data also somewhat weakens study power, which is relevant in situations where the number of studies available for analysis and/or sample size are modest.	Yes, few trials reported 30-day outcomes. We attempted to contact study authors to obtain this data.
104	Peer Reviewer 2	Methods	Methods are a strength. Only minor questions in the attached review.	Thank you.

Comment Number	Commentator & Affiliation	Section	Comment	Response
105	Peer Reviewer 2	Methods	Specific and minor issues: Search criteria: Data Sources included MEDLINE®, Cochrane Library, CINAHL®, ClinicalTrials.gov, and World Health Organization International Clinical Trials Registry Platform. Did you consider EMBASE, which overlaps with Medline, CINAHL, and probably Cochrane, but which also often contains some unique citations?	Yes, we considered EMBASE and consulted with two Evidence-Based Practice Center librarians regarding the search strategy. Briefly, we have found no utility in including EMBASE in recent systematic reviews unless we are focused on international literature or unless the yield from other databases is very small. There is a great degree of overlap among these data sources and including EMBASE in searches over the past years has not lead to any effect on finding studies eligible for the reviews (aside from an increase in duplicate publications).
106	Peer Reviewer 2	Methods	Following on this theme of the thoroughness of the search, you include one article featuring "individual peer support" by Riegel et al but not the one by Heisler (Circ Heart Fail 2013;6;246- 253; originally published online February 6, 2013). This was available within your time frame.	Our searches did identify the trial by Heisler et al.; however, it did not meet our inclusion criteria. Patients were included in this trial if they had any hospitalization within the past year (and were not recruited during or shortly after an index hospitalization). According to Table 1 in that study, approximately 15% had no hospitalization within the past year. We were specifically interested in studies in patients with HF who had recently been discharged.
107	Peer Reviewer 2	Methods	Coding disagreements: On pages 15 and 17 you specify: "If the reviewers disagreed, they resolved conflicts by discussion and consensus or by consulting a senior member of the team." Do you have any data on how often such disagreement this occurred? How often did you need to consult with a senior member of the team? Do you have any data (k statistic) illustrating your agreement?	No, we don't. We did not calculate a k statistic for disagreements between team members for grading the SOE. For this particular review, SOE was graded by two reviewers who were both senior team members. The approach to grading the SOE and any disagreements were discussed with the full team during weekly meetings.
108	TEP Reviewer 4	Methods	The inclusion criteria are very narrow and that could have limited the number of eligible studies for the newer technologies such as telemonitoring programs.	Yes, it could limit the number of eligible studies, but the other/excluded studies focus on different populations, outcomes, or comparisons than those of interest to this review. The specific questions were related to trials conducted among HF patients who were transitioning in care from hospital to home. Although this limits the number of included studies, it ensures that the trials are conducted in the population of interest.

Comment Number	Commentator & Affiliation	Section	Comment	Response
109	TEP Reviewer 4	Methods	The search strategies used appear appropriate and logical. The full search terms and combinations used are included in the appendix.	Thank you.
110	TEP Reviewer 4	Methods	Overall, appropriate given the current literature on this topic, but only randomized controlled trials are included. Although the non-randomized comparative studies and observational studies were deemed eligible, none met the eligibility criteria.	Correct-no non-randomized or observational studies were eligible.
111	TEP Reviewer 4	Methods	Comparators used for each of the Key Questions are clearly defined, and the outcomes of interest including definitional time points appear to be clearly defined. Statistical tests for the meta-analyses appear appropriate using a random effects model of DerSimonian and Laird, with inclusions of estimate of statistical heterogeneity and sensitivity analyses.	Thank you.
112	TEP Reviewer 4	Methods	Suggested edits to the Methods section: Page 37 of 215 line 8 “We included observational studies to ensure..” should be changed to “We considered observational studies to ensure” because no observational studies were included in the review.	Thank you- this has been changed for clarification.
113	TEP Reviewer 4	Methods	Lines 26-41 on page 37 are the same as Lines 16-30 on page 38. Delete one of those.	We have deleted the duplicate information.
114	TEP Reviewer 4	Methods	Please clarify how bundled categories of programs were categorized (for example, if a home visiting program also included structured telephone support and was followed up in a multidisciplinary clinic was categorized). Please comment if categorization was conducted in duplicate.	No trials overlapped in this manner. We categorized interventions by primarily by setting and mode of delivery because these were the primary areas of differences across interventions (e.g., all included some form of “education”, but trials that conducted home-visiting programs did not also include structured telephone support). Categorization was conducted in duplicate; we added a comment about this to the methods.
115	TEP Reviewer 4	Methods	Please add definitions for intensity of programs in Executive summary Methods section.	We have added this information to the ES.

Comment Number	Commentator & Affiliation	Section	Comment	Response
116	TEP Reviewer 4	Methods	Please add a line on how multiple comparisons from a study are handled in meta-analysis. For example, in figure 6 (meta-analyses of STS at 3 and 6 months), the usual care in Wakefield 2008 study is considered as independent comparison groups for the interventions of telephone and videophone.	Thank you for noting this. We have re-run this analysis and are considering the STS arms as one group for the mortality meta-analysis (comparing this to the usual care group). We do discuss whether there were differences in the two STS arms in KQ4. Since there were not differences based on type of STS, we felt comfortable combining data for those groups. We have added a note about this in the methods section.
117	TEP Reviewer 5	Methods	Generally sound methods, well-described inclusion/exclusion criteria.	Thank you.
118	TEP Reviewer 5	Methods	The categorization of complex interventions is difficult and will never be perfectly clean, but the approach used is generally transparent and appropriate.	Thank you.
119	TEP Reviewer 5	Methods	A few clarifications: - were hospital-at-home interventions considered or excluded? These would probably fit most closely with home-visiting interventions, but they are a bit different in that a prescribing practitioner (MD or NP) is often involved, they may have the ability to provide IV medications etc - so the emphasis is both on semi-acute med mgmt and education. Might be worth explicitly stating one way or the other if these interventions were included/excluded.	Hospital-at-home interventions were excluded. We made this clearer in the methods by adding it to the list of excluded interventions.
120	TEP Reviewer 5	Methods	- population – I assume you focused on studies including only chf patients. There are TC studies including broader populations of patients with chronic illness, including CHF – I assume these studies were excluded. Please clarify	Yes- this is noted in the inclusion/exclusion criteria. We only included data from populations admitted for HF (and not transitional care interventions studied in other populations not admitted for HF).

Comment Number	Commentator & Affiliation	Section	Comment	Response
121	TEP Reviewer 5	Methods	- risk of bias assessment - it wasn't clear to me how you assessed readmission ascertainment. You appropriately looked at the adequacy of readmission metric definition, but I wasn't sure if you also considered whether or not a study had access to all readmissions or only readmissions to their own center. This can be a hugely important issue - single-center studies not relying on something like Medicare administrative data may only capture readmissions to their own hospital but these rates may not at all be reflective of overall readmission rates since many patients will be admitted to other centers. This is a different issue from missing data/attrition/outcome definition since a study could do an excellent job of identifying all same-hospital readmissions but miss other readmissions. You may have considered this in your assessment, but its not entirely clear - would state explicitly. If not considered, then this is an important limitation.	We did consider these issues carefully in the risk of bias assessment. We have added a section to the methods that reads: "When assessing measurement bias related to readmission ascertainment, we considered whether a study had access to all potential readmission data (versus readmissions collected from only a single institution's database). When investigators used data from only a single institution to measure readmission rates (without at least collecting additional data on admissions to other institutions from patients or caregivers), we considered this a major methodological shortcoming (e.g., high risk of measurement bias). We rated studies as unclear risk of bias when information provided was inadequate for judging the validity of outcome measures (primarily readmission rates and mortality)."
122	TEP Reviewer 6	Methods	Use of risk difference (instead of relative risk) as the unit for the meta-analysis. Because overall event rates may be variable (reflecting different underlying populations), wouldn't relative risk be more comparable. I am certain that you have given this much thought. For unsophisticated readers like me, you may want to indicate why you chose an absolute rather than relative risk difference for comparison.	We have re-run analyses using risk ratios rather than risk difference and also described the variability in readmission rates in the control groups (reporting the median and IQR).
123	Peer Reviewer 7	Methods	Page 8-9: The Inclusion and Exclusion Criteria section and the Study Selection section have not been completed. There are broken sentences and duplicate paragraphs.	Paragraphs in this section were not duplicative-one was focused on abstract review and the other on full-text review. This section has been edited and condensed to avoid confusion.
124	Peer Reviewer 7	Methods	Line 24 of page 8: this is an incomplete sentence that appears to be from a cut-and-paste operation but not edited afterward.	Thank you. This has been corrected.
125	Peer Reviewer 7	Methods	Two subsequent paragraphs about "Two trained members . . ." were repeated in the Study Selection section. The word "trained" is unnecessary.	We have deleted the word "trained" and condensed this section.
126	Peer Reviewer 7	Methods	The first line on page 9 has no beginning text.	We have corrected this section.

Comment Number	Commentator & Affiliation	Section	Comment	Response
127	Peer Reviewer 7	Methods	Page 12; line 3: "We gave high risk-of-bias ratings to studies that we determined to have a fatal flaw (defined as a methodological shortcoming that leads to a very high risk of bias)." This is circular definition.	Thank you. We have revised this sentence to read: We gave high risk-of-bias ratings to studies that we determined to have a major methodological shortcoming in one or more categories based on our qualitative assessment. Common methodological shortcomings contributing to high risk-of-bias ratings were high rates of attrition or differential attrition, inadequate methods used to handle missing data, lack of ITT analysis, and unclear or invalid measures of readmission or mortality rates.
128	Peer Reviewer 7	Methods	Page 13; line 11: Another circular sentence: "We conducted quantitative synthesis using meta-analysis of outcome…." Meta-analysis is a quantitative synthesis. You can simply say we conducted metaanalyses of outcomes…"	We have edited this section, removing the "quantitative synthesis" as suggested.
129	Peer Reviewer 7	Methods	Page 13; line 17: what do you mean by "When quantitative synthesis was not appropriate (e.g. …insufficient number of studies…)"? I see that you did meta-analyses with as few as two studies. Single studies were also plotted in the same chart as meta-analyses and included in the Appendix E which was labeled as "meta-analysis".	These figures have been re-labeled to indicate that they represent meta-analyses and risk ratio calculations. We also intended this sentence to indicate that we did not combine studies that calculated total number of readmission per group with those that counted people readmitted.
130	Peer Reviewer 7	Methods	Page 13; line 21: The first sentence of this paragraph is result; it does not belong in the methods section.	We have deleted the sentence from the method section.
131	Peer Reviewer 7	Methods	Page 13; line 32: this paragraph should also say something about NNT, after description of risk difference; the discussion of SMD should also give some idea about how this metric will be interpreted in the meta-analyses of this report	We have added additional text describing how NNT was calculated. We have also added text describing how to interpret SMD.
132	Peer Reviewer 7	Methods	Page 14; line 4-6: "For most interventions, we defined intensity as the duration, frequency, or periodicity of patient contact, categorizing each intervention as low, medium, or high intensity." This would be clearer written as "We categorized the intensity of each intervention as low, medium, or high using the duration, frequency, and periodicity of patient contact."	We have edited this sentence as suggested.
133	Peer Reviewer 7	Methods	Table 1: The inclusion criteria were sufficiently inclusive thus making much of the exclusion criteria in the table is unnecessary.	Thank you, but we feel the table is helpful to some readers. Some reviewers wanted to see additional detail not described in the text.

Comment Number	Commentator & Affiliation	Section	Comment	Response
134	Peer Reviewer 7	Methods	Page 12, line 33: “Trained reviewers”; I believe the word “Trained” is unnecessary. I hope no untrained reviewers participated in the report. This is scattered in the Exec summary and methods section.	We have omitted the word “trained” from this section in the methods, and in the executive summary.
135	Peer Reviewer 14	Methods	The inclusion and exclusion criteria are justifiable. The search strategies are explicitly stated and logical. The definitions or diagnostic criteria are appropriate for the outcome measures. Cannot comment on the appropriate use of statistical methods.	Thank you.
136	Peer Reviewer 14	Methods	Page 88 (line 46), glossary of terms: The authors identify heart failure as a chronic, progressive condition. Since the authors discuss primarily hospitalization for heart failure, which is traditionally defined as acute decompensated heart failure, the authors may which to delete "chronic" from the definition. Also, this definition is outdated since it does not address preserved dystolic function.	We have edited this definition so that it includes both chronic and acute decompensated heart failure. Since included trials generally did not distinguish between preserved and reduced EF, we are not adding this level of detail to the glossary.
137	Peer Reviewer 14	Methods	Page 88 (line 24), definition of New York Heart Association: The authors include "angina" in the limitation. The words "angina pain" can be deleted.	We have edited this definition to read: “Classification system for heart failure disease severity. Patients are classified in one of four categories based on the degree to which they are limited during physical activity by cardiac symptoms (e.g., fatigue, palpitation, dyspnea or anginal pain).”
138	Peer Reviewer 14	Methods	Page 88 (line 25): The scores range listed from II-IV should be changed to "I-IV".	This has been changed.
139	TEP Reviewer 9	Methods	The search strategies and classification of studies is appropriate. The statistical methods were appropriate for the analyses. The outcome measures chosen are the relevant ones for this population.	Thank you.
140	Peer Reviewer 11	Methods	The authors have clearly defined how they chose to include or exclude some pieces of work. With the growing number of implantable monitors, I believe some mention of these papers should be included. Although the PA monitors are not all approved, the field is moving rapidly and in fact, these are "monitors" of physiologic parameters. For example the Medtronic Optivol is still widely used. FDA is currently considering approval for the Cardiomems device.	We only included interventions that could be considered “transitional care” and could be widely applicable. These emerging technologies were not included in the scope of our review.

Comment Number	Commentator & Affiliation	Section	Comment	Response
141	TEP Reviewer 12	Methods	See above. The major issue not addressed is the potential difference between patients with HFrEF and HFpEF	This comment is addressed above: "The vast majority of studies do not differentiate between those with preserved vs. reduced Ejection Fraction (EF) (only noting that inclusion criteria require that patients be admitted with a primary diagnosis of HF). Although pharmacotherapy may differ; we did not find trials that assessed differences in readmission rates among patients with preserved vs. reduced EF. We know of no evidence that suggests patients with preserved vs. reduced EF would respond differently to non-pharmacologic transitional care interventions. We added text to the Study Limitations sections regarding this point."
142	Peer Reviewer 13	Methods	The authors may have mentioned it or outlined it but I had trouble finding the quality asssessment narrative in the "Methods" section. Could the authors make this more transparent and specify the methodology chosen (JADAD or WHO or...etc!).	We used predefined criteria based on the AHRQ Methods Guide; there is a section that describes the assessment in the methods, and further detailed description in Appendix D. Briefly, we assessed selection bias, confounding, performance bias, detection bias, and attrition bias; we included questions about adequacy of randomization, allocation concealment, similarity of groups at baseline, masking, attrition, whether intention-to-treat (ITT) analysis was used, methods of handling missing data, reliability and validity of outcome measures, and treatment fidelity. Individual responses to each of these criteria are provided in the Appendix.
143	Peer Reviewer 13	Methods	Exclusion and Inclusion Criteria: very transparent inclusion exclusion criteria	Thank you.
144	Peer Reviewer 13	Methods	Search Strategy: not clearly and systematically documented.	We provide our full search strategy in Appendix A (in addition to the description in the Methods). Other reviewers have found the search strategy well done.

Comment Number	Commentator & Affiliation	Section	Comment	Response
145	Peer Reviewer 13	Methods	Could the authors give specific acknowledgement to the Librarian (s) who assisted with the search strategy; submitting request for reprints pertaining to difficult to locate paper articles!	The librarian who assisted with the search and retrieval of articles is listed as an author in the final report. Requests on where/how to locate included articles can be sent to the corresponding author.
146	Peer Reviewer 13	Methods	Outcome Measures: the outcomes have been selected based on the contemporary focus of "readmission rates" "all-cause mortality" and "quality of life" etc. ...these paralleled the outcomes proposed and evaluated in the primary studies!	Thank you.
147	Peer Reviewer 13	Methods	Statistical Methods and measure of association: VERY PROBLEMATIC! 1. Risk Difference is not intuitive nor cognitively convey the magnitude of the effect of these HF Diesease management programs.	This is an issue that could be debated. Because of heterogeneity in baseline readmission rates, however, we have re-run analyses using risk ratios and have reported these results rather than risk differences.
148	Peer Reviewer 13	Methods	Could the authors revisit the measure of association and change that for readmission to Relative Risk or Risk Ratio [95% CI]?	As noted above, we are now reported results as risk ratios.
149	Peer Reviewer 13	Methods	Could the authors do the same for the other dichotomous outcomes and use RR [95%CI]?	Yes, we have used RR for readmission and mortality rate outcomes.
150	Peer Reviewer 13	Methods	Could the authors use "percent change relative to baseline for "Quality of Life" and other continuous outcomes? eg % Change = final - baseline/baseline	This was done when trials reported this information; there was variability in which measure was reported across included studies.
151	Peer Reviewer 13	Methods	I believe that we laid out this type of methodology with transparency in the JAMA article!	If this refers to the comment above, we do not question the methodology. However, there is variability across included trials regarding how QOL outcomes are reported.
152	Anonymous Public Reviewer	Methods	Dear Gentleperson, If you ask the wrong question, you will get the wrong answer. You ask "Which kinds of transitional care reduce readmissions?" you further define "kind" as in-person, telephonic, etc... With all due respect, I suggest you define "kind" by the issues addressed by each effort at transitional care. I would politely suggest that a telephone call wherein one asks the right questions, based clearly on the patient's individual medical history, is more likely to be effective than a "home visit" program in which every patient gets the same, standard treatment, regardless of condition and co-existing disorders.	Thank you for this comment. Our questions were based, in part, on the needs of stakeholders and the nominator of this topic. We understand that interventions could be categorized in different ways. We attempted to categorize interventions in a way that would be most helpful for those planning to implement a new intervention (e.g., by setting and method of delivery).
153	Sandra Oliver McNeil, Public Reviewer	Methods	The methods were very good for this type of project.	Thank you.

Comment Number	Commentator & Affiliation	Section	Comment	Response
154	Sandra Oliver McNeil, Public Reviewer	Methods	Definitions should be provided of Homecare, home-visit, and clinic. I felt as though there was bias in this area.	We do not use the word Homecare in this report (and have searched to make sure). In the Study Characteristic section, we define home-visit (i.e., a visit to the patient's home by a provider, usually a nurse or pharmacist). What is meant by home-visit and clinic-based is defined in the Table of Intervention Categories (Executive Summary and methods) and the intervention categories are also described in the Characteristics of included studies sections.
155	Sandra Oliver McNeil, Public Reviewer	Methods	I did not see the role of nursing defined, or the role nurses have on patient education.	This was not part of our scope; the specific role of nursing was not usually defined in included trials (aside from the components of interventions as they are described- e.g., telephone follow-up, "nurse education" etc.).
156	Sandra Oliver McNeil, Public Reviewer	Methods	The quality of discharge teaching was not included in the literature review.	We did not attempt to assess quality of discharge teaching. Few trials focused on discharge education as the primary intervention component.
157	Sandra Oliver McNeil, Public Reviewer	Methods	HF needs a team approach, and the focus of this report focused on the medical model approach to patient care.	We agree that HF needs a team approach. Interventions that showed efficacy tended to include a multidisciplinary team (e.g., multidisciplinary clinic interventions).
158	Sandra Oliver McNeil, Public Reviewer	Methods	There was no literature to support the importance of the right type of visit and whether medications could be adjusted based on the data.	The literature supported home-visiting interventions and multidisciplinary clinic visits.
159	Sandra Oliver McNeil, Public Reviewer	Methods	Socioeconomic factors, Age and ethnicity was not disclosed	Age and ethnicity of patients enrolled in included trials is presented in the tables of study characteristics.
160	Akshay Desai, Public Reviewer	Methods	A final comment regarding methodology: Though interventions were classified into separate categories (MDS-HF clinic, STS, HF education, etc.), many of these were multi-component interventions in practice that had elements of multiple categories. Most interventions were layered on 'usual care' that is in general poorly characterized and may have varied widely across trials. This considerable heterogeneity in the comparator groups also hampers the ability to draw general messages across the trials.	Thank you. This is a limitation of this literature. We added a section to the discussion on heterogeneity in usual care.
161	Akshay Desai, Public Reviewer	Methods	Nonetheless, this synthesis, very transparently done, provides an honest and complete attempt to draw useful clinical inferences from the accumulated data.	Thank you.

Comment Number	Commentator & Affiliation	Section	Comment	Response
162	Peer Reviewer 1	Results	Presentation of the results is superb. The tables, figures, and text are all clear and complementary. The main findings are well described in the Executive Summary, and specific details are elaborated in the main report.	Thank you.
163	Peer Reviewer 1	Results	To my knowledge no important studies were overlooked (although I would point out that the study by Rich et al, NEJM 1995, also included data on quality of life and caregiver burden).	This is an included study. We have reviewed the study and it does not report outcomes relevant to caregiver burden. It does report the cost of the home-visiting program but not a measure of caregiver burden. The QOL scale used in this study was not eligible.
164	Peer Reviewer 1	Results	However, as discussed above I believe that the report would have been more informative had the inclusion/exclusion criteria been less narrowly focused.	We attempted to match the inclusion/exclusion criteria to match our specific key questions and target population (HF patients who are transitioning in care). We realize that there is an important body of literature (and similar interventions) that are conducted among populations that have not been recently hospitalized or which measure readmission rates >12 months.
165	Peer Reviewer 2	Results	As noted in my formal comments, I did question the validity of some of the results. One conclusion that I found particularly problematic can be found on page 19: "Despite having only a single trial of home visiting that reported rates at 30 days, this intervention category also consistently reduced readmission rates over 3 and 6 months; therefore, we considered homevisiting programs efficacious in reducing both all-cause readmissions and the combined outcome all-cause readmission or death at 30 days." I had difficulty with your decision to extrapolate from a single study. Please reconsider this decision.	Thank you for this comment. We have considered this carefully. When assessing the SOE, we considered the consistency across outcome timings, not just for this one trial, but also other trials assessing a similar intervention that showed efficacy at 3 and 6 months. We do note that the SOE is low and not moderate or high.
166	TEP Reviewer 3	Results	Improving Main message clarity: decision makers will ultimately want to know if and how to implement transitional care interventions (admittedly there will be some outcomes that may be more important than others in these decisions-in part driven by pay for performance measures.) The report would benefit from better summarizing the main outcomes by interventions. I have the following comments and suggestions:	Thank you. We have rewritten much of the Discussion and main conclusions to make the summary more clear
167	TEP Reviewer 3	Results	The "check box tables" of intervention components are very useful-they provide an overview of tested components that may allow decision makers to implement strategies or researchers to test new/old approaches	Thank you.

Comment Number	Commentator & Affiliation	Section	Comment	Response
168	TEP Reviewer 3	Results	Clarity of outcomes: Table B is the summary of findings (there are other similar ones). While valuable it remains confusing and too "listy/wordy" making it difficult to get a "bottom line":	Thank you. We have incorporated the feedback described below and have edited this table, and added a new summary table.
169	TEP Reviewer 3	Results	Consider the following: An additional set of check box tables for outcomes of interest stratified by intervention. This would give a good "lay of the land" Construct two "matrix tables" (one for utilization [all-cause, HF readmissions, Combined…] and one for health outcomes [mortality,QOL, Caregiver]). Across the top place outcomes of interest (at 1, 3, 6 months) and along the side the intervention categories. The cells would then be filled with a "+" (benefit), "-" (no benefit or harm), "NR" not reported. Perhaps the # studies included and possibly "SOE" for this outcome by intervention (though that might get a bit busy). This would allow the reader to more easily see the totality/consistency/inconsistency of information (and the gaps). The current summary key message bullets, forest plots etc…while useful do not fully capture that, only provide data where outcomes are reported (hence only highlighting "available" data) or make for LOTS of lists that are confusing when trying to get the bottom line.	Thank you for this very helpful suggestion. We have created a table that meets these goals and now include it in the ES and Discussion sections.
170	TEP Reviewer 3	Results	Strongly consider collapsing any outcome at different time points into a single time point of "<= 6months". Report that as the main outcome in text/table and forest plot form using the longest followup data. E.g. if a study provided data for 1, 3 and 6 month…only use the 6 month data. You could include appendix tables with the additional data and you might emphasize the very limited data on 1 month outcomes as that is a "pay for performance measure"…but as a decision maker or guideline developer one really cares if the outcomes are better or not sometime within the time frame you have selected (in this case <= 6months). This would greatly clarify the messages/report and would likely allow a little more of the information to be pooled.	Thank you, we have collapsed the 3 and 6-month outcome timings. We have kept the 30-day outcome separate.
171	TEP Reviewer 3	Results	All the outcomes are "short term". There would be a little "alteration" in data results if pooling a 3 month outcome from one study with a 6 month outcome from another study the # of events is likely greater in a 6 month vs. a 3 month study and there are other issues but I believe the value would greatly exceed any harm	Thank you. As above, we have combined the 3 and 6-month outcome timings.

Comment Number	Commentator & Affiliation	Section	Comment	Response
172	TEP Reviewer 3	Results	Consider making that the outcome of <= 6 months as a matrix table and that time point your highlighted main message table (deleting all the stuff on 3 vs. 6 months)…	As suggested, we have now combined the 3 and 6 month time points and included a matrix table highlighting our main results in the ES and in the Discussion section.
173	TEP Reviewer 4	Results	The results for each of the key questions are nicely presented in the key points with adequate descriptions of each of the studies used in the analyses.	Thank you.
174	TEP Reviewer 4	Results	There are a few inconsistencies noted. RCTs, studies, trials are interchangeably used.	We've clarified in the ES and results that although nonrandomized trials and observational studies were eligible for some outcomes, only RCTs were included. In the results section, we refer to included RCTs as "trials." We do use the term "studies" in the methods section and in the discussion when we are referring to future research needs.
175	TEP Reviewer 4	Results	In Table 4, please change last column to "Risk of bias" instead of setting.	This has been corrected.
176	TEP Reviewer 4	Results	Timing abbreviation varies across tables (m, ms, months).	We've changed this to "m" to be consistent.
177	TEP Reviewer 4	Results	Eligibility criteria states inclusion of studies that recruited subjects during or within 1 week of the index hospitalization. It is unclear how this criterion was enforced during study selection. For example, trial by Kimmelstiel 2004 enrolled subjects until 2 weeks of hospitalization.	For this trial, we contacted the authors for additional information regarding timing of enrollment. The methods section of the trial states "patients were enrolled during an index HF hospitalization or within 2 weeks of discharge." No specific information could be provided by authors on the exact number of participants who were not enrolled during the index hospitalization, but the number was felt to be small. We have made a note regarding this in the methods.
178	TEP Reviewer 4	Results	Population section on page 50 do not match well with Table 4. For example, mean age in table 4 starts at 63 not 72.	Thank you. This has been updated.
179	TEP Reviewer 4	Results	Lines 45-46, for the range of ischemic heart disease, 10 trials are cited and please match it with Table 4.	This has been corrected.

Comment Number	Commentator & Affiliation	Section	Comment	Response
180	TEP Reviewer 4	Results	Page 51 Line 19-21 add one trial in Spain (Ref 52)	This trial has been added.
181	TEP Reviewer 4	Results	Page 51, under Structured Telephone Support, only 11 are cited when 13 RCTs in 15 publications are eligible.	The missing citations have been added
182	TEP Reviewer 4	Results	Trial sample size does not match with Table 5, should be 32 to 715.	This has been corrected.
183	TEP Reviewer 4	Results	Per Table 5, Page 53 Line 40, the text should read "all other trials included 20 percent to 59 percent";	Thank you. This typo has been corrected.
184	TEP Reviewer 4	Results	Line 41-42, should be 7 trials (missing reference 59);	This has been added
185	TEP Reviewer 4	Results	line 45-46 should be two trials not three studies did not report HF disease severity (Ref 63 provides HF severity).	Thank you; this was changed to "two trails did not report HF disease severity. Reference 63 has been deleted here.
186	TEP Reviewer 4	Results	Page 54 Line 12 lists 6 trials when the text states five trials did not include predischarge educational component.	This has been corrected.
187	TEP Reviewer 4	Results	Line 31 "two in multicenter settings" cites 3 trials.	This has been corrected.
188	TEP Reviewer 4	Results	Page 59, comparing with Table 7, Lines 34-35 should be 12 percent instead of 30 percent for beta-blockers.	This has been corrected.
189	TEP Reviewer 4	Results	Line 38-39, should be 2 trials reported no information on co-occurring conditions.	This has been corrected.
190	TEP Reviewer 4	Results	Page 60 Line 17 should be sample size ranged from 76 to 230 (comparing with table 8).	This has been changed- the correct number is 75-230.
191	TEP Reviewer 4	Results	Page 66, please change "home visits" to STS in line 30.	This has been changed.
192	TEP Reviewer 4	Results	Page 76, Line 15 under telemonitoring section, please change "receiving STS" to receiving telemonitoring.	This has been changed.

Comment Number	Commentator & Affiliation	Section	Comment	Response
193	TEP Reviewer 5	Results	As far as I'm aware, no studies were overlooked or inappropriately included. The amount of detail is generally appropriate.	Thank you.
194	TEP Reviewer 5	Results	There are a few pieces of information that would have been additionally useful if available: - would be useful to know whether or not studies used a risk stratification scheme to determine eligibility for inclusion and/or for intervention dosing (as an example see Amarasingham R, BMJ Qual Saf 2013 - though this would not have met your inclusion criteria). The effectiveness of TC interventions may depend on patient selection. From a policy standpoint, people might want to know from which population the study population was drawn - was it all patients with CHF at a given center, or patients with CHF at higher risk by virtue of having had multiple past admissions or some other factor? If this information is not reported (I imagine this is the case), then it is an important research gap.	Few trials used a risk stratification scheme to determine eligibility. We have highlighted this issue in the Applicability section and noted which trials formally considered whether patients were at "high risk" of early readmission.
195	TEP Reviewer 5	Results	- it would be useful to show readmission rates - the data is all there in the forest plots, but I think when people try to think about applying the results to their own populations they will want to quickly see what the control group readmission rates are in percentage terms. This could be presented in the forest plots, or could be described in a sentence narratively (i.e. rates ranged __ to ____).	We have added readmission and mortality rates of control groups in the text.
196	TEP Reviewer 6	Results	p34 line 26: should this be 'home-visiting programs in general were efficacious….'	No. The "in general" is not necessary. We are summarizing the results of our evidence synthesis (including meta-analysis) and not results of individual trials.
197	TEP Reviewer 6	Results	p34, line 36: seems to conflict with first key point (line 21). Also, on this bullet, get rid of the wording 'support the efficacy'. Say whether you don't have enough evidence or you have evidence that it doesn't work.	This section has been edited after combining the 3 and 6-month time points.
198	TEP Reviewer 6	Results	p35 line 16: this bullet has good wording on the insufficient evidence piece	Thank you.
199	TEP Reviewer 6	Results	figure 3. I am grateful in general for these figures. But, the notations needs to be defined: T_N, T_event, etc. Font is quite small….	We have changed the headers in the forest plots in the main report to make the notation easier to read. We have also added footnotes to explicitly say what the notations are (e.g., N= sample size). Thanks for this feedback.

Comment Number	Commentator & Affiliation	Section	Comment	Response
200	TEP Reviewer 6	Results	page 40, line 3. I would say this sensitivity analysis of adding another study showed 'attenuated effect that is no longer statistically significant' rather than saying you found 'no difference'.	We have reworded this as suggested.
201	TEP Reviewer 6	Results	. p 50, Table 12: I find this difficulty to interpret. Can you plot effect sizes or something like that? Hard to tell directionality and magnitude. Not very helpful. Same with table 13.	We have added a new summary table that gives the direction of effect (and SOE) for each intervention category and outcome timing.
202	TEP Reviewer 6	Results	Tables 14, 15 do not help me at all. I don't know what I can do with this information. Frankly, I suspect many of the non-efficacious studies have the same components….	Thank you for this feedback. Some have felt that these tables are useful. We do include similar tables for all interventions.
203	TEP Reviewer 6	Results	. p 61. Line 50: In this report you equate the nurse led clinic with low intensity and not efficacious. I think many of the MDS-HF clinics are nurse led or at least nurse practitioner led. I suggest disentangling the intensity from a given specialty. I doubt it is because a nurse was leading that it was inefficacious.	Trials included and labeled as MDS-HF included regular visits with physicians and other multi-disciplinary staff (e.g., nutritionists). This, in part, increases intensity (i.e., more use of resources). With few included trials, it is not possible to separate delivery personnel from intensity for this category.
204	Peer Reviewer 7	Results	Page 17; line 17-18: relative risk and hazard ratios are mentioned but this metric was not mentioned in the methods section.	RRs are mentioned in the methods section. The HR here is not something we calculated- this was the measure reported in the primary trial.
205	Peer Reviewer 7	Results	Page 42,line 49: ". . . the number of patient seen in the ER did not differ . . ." should be the percentage of patient (you gave 38 percent of patients versus 33 percent at the end of the sentence, not counts);	We have changed this to read percentage of patients (rather than number).
206	Peer Reviewer 7	Results	you may also want to check the wording of the previous paragraph in reporting the HR; is it counts or rate?	We have ensured that this is correct.
207	Peer Reviewer 7	Results	Page 44, line 44: typo "telmonitoring" should be "telemonitoring"	This typo has been corrected.
208	Peer Reviewer 7	Results	Page 46, line 54: a space is missing between the number "5" and "to" "-0.05to 0.02"	This typo has been corrected.
209	Peer Reviewer 7	Results	Table 4: I believe there is an error in the heading of the last column "Setting", it should be "risk of Bias" instead.	This has been corrected.
210	Peer Reviewer 7	Results	Inconsistent abbreviations used for months in column heading: (ms) is used in this Table 4, (m) in Tables 5, 6, 7, 8 and 9; (months) on Table 16	We have changed all abbreviations to "m" for consistency.

Comment Number	Commentator & Affiliation	Section	Comment	Response
211	Peer Reviewer 14	Results	The amount of detail presented in the results section is appropriate.	Thank you.
212	Peer Reviewer 14	Results	The characteristics of the studies are clearly described.	Thank you.
213	Peer Reviewer 14	Results	The key messages are explicit and applicable.	Thank you.
214	Peer Reviewer 14	Results	Figures, tables and appendices are adequate and descriptives.	Thank you.
215	TEP Reviewer 9	Results	It was somewhat difficult to quickly determine the absolute reduction in hospitalization rates from the data presented. The data appear to be in the graphs though it would have been helpful to have data presented with units of readmissions or days for those outcomes.	Data on readmissions is presented both in forest plots (displaying meta-analyses and risk-rations of individual trials). We also discuss results in the text. We are now reporting the relative risk of readmission (people readmitted) within a specified outcome timing.
216	TEP Reviewer 9	Results	The included studies are appropriate though it was unclear how some of the data were obtained. For example, my reading of the 2004 Naylor article identified the combined outcome of deaths or readmissions at multiple time points (estimated from survival curves). It wasn't clear why this trial was not included in the death or readmission group, or if the authors were able to extract the readmission data from the combined endpoint.	Data from the 2004 Naylor article was obtained from the author (in the original publication, it was given only in HRs for the composite outcome. We only had data for the composite outcome in a HR.

Comment Number	Commentator & Affiliation	Section	Comment	Response
217	TEP Reviewer 9	Results	I would have liked to see a comparison by publication date. It seems there are surprisingly few recent studies published even though the search went into the Spring of 2013. For example, of the Home visit intervention, where the authors find the greatest effect on readmission, none of the 12 sources of data were published after 2008, Given that the publication date was likely well after the conduct of the trial it is likely that the average study included is started well over 10 years ago. This would be fine if usual care did not change over time. However, this is not likely to be the case and a transition of care intervention applied 15 years ago is unlikely to have the same effect today since "usual care" has improved.	In meta-analyses, we have ordered trials by publication date. There does not appear to be an effect by date, but we did not include this as a formal sensitivity analysis (and there are relatively few studies to allow for such an analysis to be meaningful). We have given more consideration to changes in usual care in the discussion section, and specifically did not include studies published prior to 1990 because of advances in pharmacotherapy for patients with HF appear to have been concentrated around that time (e.g., increased use of beta-blockers and ACEIs). There is also controversy regarding the extent to which usual care has improved. There has been no significant improvement in the readmission rate among patients hospitalized with HF in recent years (according to Medicare claims data).
218	Peer Reviewer 11	Results	The group did not mention the Hospital to Home initiative of the ACC and IHI which resulted in a survey of hospitals that had enrolled and ended in a paper by Bradley et al in Circulation Quality and Outcomes early this year. Due to the large amount of institutions, hospitals, systems and practices that enrolled, it is worth of mention. Circ Cardiovasc Qual Outcomes. 2013 Jul;6(4):444-50	We have reviewed this publication. It does not meet inclusion criteria and we have not included it in this report.
219	TEP Reviewer 12	Results	No studies were overlooked.	Thank you.
220	TEP Reviewer 12	Results	Detail was appropriate	Thank you.
221	Peer Reviewer 13	Results	Yes and Yes, and yes and yes!!	Thank you.
222	Sandra Oliver McNeil, Public Reviewer	Results	The results were slanted toward a medical care model.	We disagree with the reviewer. We cast a fairly broad net for this review when searching for interventions to reduce readmission rates for people with HF, and we did not limit eligibility to studies that followed a pure biomedical model.

Comment Number	Commentator & Affiliation	Section	Comment	Response
223	Sandra Oliver McNeil, Public Reviewer	Results	Patients with HF need the right type of follow up, with anticipation of needing medications adjusted, labs drawn to avoid over diuresis, and the right medications are being prescribed.	We agree. This comment is not a suggestion to change anything in the report.
224	Sandra Oliver McNeil, Public Reviewer	Results	The results only focused on where the care was delivered, no description of the patient except for age. Cognitive function declines with HF, age is not the only factor.	Trials did not commonly report whether readmission rates differed based on patient factors such as age or comorbidity. We do report the patient characteristics for trials (e.g, heart failure severity). One included trial focused specifically on patients with HF and cognitive dysfunction.
225	Peer Reviewer 1	Discussion	The implications of the findings are clearly stated, and some of the limitations of the analyses are discussed.	Thank you.
226	Peer Reviewer 1	Discussion	However, as noted above, the study findings are not particularly novel, and the narrow inclusion criteria, the lack of a key question on cost-effectiveness, and the lack of patient-level data, especially with respect to evaluating 30-day outcomes, are important limitations.	We did not set out to perform a cost-effectiveness analysis or analyze patient-level data. As stated in a previous comment: "In terms of the study design, we were not conducting a cost-effectiveness analysis. We agree that such an analysis would be an important contribution to this literature, but it was beyond our scope."
227	Peer Reviewer 1	Discussion	Regarding the recommendations for future research, these are generally well described and appropriate.	Thank you.
228	Peer Reviewer 1	Discussion	The impact of transitional care interventions on 30-day outcomes can likely be ascertained using patient-level data from existing trials; until this has been done, I do not believe that additional studies focusing specifically on this arbitrary and somewhat controversial outcome are justified.	Thank you. We have added a sentence to the table of future research needs suggesting that individual patient data meta-analysis may address this question. However, we feel that a Systematic Review can offer some guidance to inform current practice and quality improvement efforts.
229	Peer Reviewer 1	Discussion	I am also of the opinion that the over-emphasis on 30-day outcomes is misplaced and not patient-centered (do patients care if they are readmitted at 29 days vs. 31 days? should we be incentivizing providers to keep the patient out of the hospital for that extra couple of days?). If anything, it seems to me that we should be looking at longer term outcomes -- how is the patient faring over a period of 6 months, or 12 months, or 2 years?	We understand that there are controversies surrounding the use of 30-day readmissions as a quality metric. However, given the current CMS policy, we feel that this is an appropriate outcome to evaluate in a systematic review. This review may help to inform whether or not it is an appropriate metric for policy.

Comment Number	Commentator & Affiliation	Section	Comment	Response
230	Peer Reviewer 1	Discussion	More specifically, what types of interventions, initiated at the time of HF diagnosis or hospitalization, lead to sustained improvements in clinically relevant outcomes, including quality of life, functional status, healthcare utilization, and mortality?	These are important questions, although not in the scope of this review.
231	Peer Reviewer 1	Discussion	An additional area for future research concerns assessment and impact of health literacy on outcomes in HF patients receiving transitional care interventions.	Thank you. This is an important topic- though out of scope for our current review.
232	Peer Reviewer 2	Discussion	I provided suggestions for further discussion in my review.	Thank you.
233	Peer Reviewer 2	Discussion	The second major issue I had was in relation to your key questions. I was as disappointed as I'm sure you were that you were unable to address many of the important questions you set out to address.	We disagree with this statement. We were able to address the KQs; lack of evidence is not a disappointment, but an opportunity to highlight important research gaps.
234	Peer Reviewer 2	Discussion	You were able to make conclusions regarding readmissions and mortality at 3- and 6-months but not at 30 days.	We were able to conclude that home-visiting programs reduce all-cause readmission at 30-days (low SOE) and to conclude that there was insufficient evidence regarding the efficacy of other types of interventions at 30-days.
235	Peer Reviewer 2	Discussion	The data were insufficient to allow you to make clear statements about functional status, quality of life, caregiver burden, or self-care burden (which I will come back to, because I have never heard of "self-care burden").	Yes, in general, there was insufficient data to determine how transitional care interventions influence these outcomes.
236	Peer Reviewer 2	Discussion	You were unable to discern which components of interventions were most effective and which added benefit. Your questions about intensity and delivery personnel were unanswered. Effectiveness or harms for subgroups of patients also remain unanswered.	We were able to discern components of effective interventions that were common; however, it was not possible to isolate single components and determine which added benefit and which did not . This was primarily due to the fact that there were few trials in each intervention category and insufficient variation within categories to determine the added benefit of a single component.
237	Peer Reviewer 2	Discussion	These are important questions and the fact that the data were insufficient to answer them should, I believe, be a major emphasis of this report.	We have added an additional summary table that helps readers quickly see when we found evidence to support a particular intervention category (and the SOE) and when we did not (i.e., when evidence was insufficient). We have also emphasized this issue in the table of future research needs.

Comment Number	Commentator & Affiliation	Section	Comment	Response
238	Peer Reviewer 2	Discussion	You note that potential harms or unintended consequences of interventions do not appear to have been widely considered in previous reviews. But then you note that there is little evidence on the potential harms and you make no conclusions from the literature on this topic. Can you criticize prior reviews for something that you also are unable to accomplish?	We believe there is an important difference between not considering the harms (i.e., not searching systematically for them; not even looking for them in a review) and finding that harms are not widely reported (i.e., searching for them but finding no evidence). We have added some additional text to the discussion regarding potential harms of transitional care interventions and note that this is an important research gap.
239	TEP Reviewer 3	Discussion	Refine applicability discussion: Please make clearer results from studies conducted: a) in the United States and b) multicenter vs. single center studies.	Thank you. We have added some additional text to the applicability section that has to do with study setting.
240	TEP Reviewer 3	Discussion	Given that these are multicomponent interventions that are likely very sensitive to the health care systems/home care personnel and populations studied and specifics of a single individual (or small group of individuals) carrying out the intervention applicability for practice and policy implementation across the United States is important.	Thank you. We note some of these issues in the applicability section.
241	TEP Reviewer 3	Discussion	Strength of evidence may be even lower if evaluating information most likely to be applicable to the US (i.e. conducted in the U.S. and studied at multiple centers).	This is unclear. We did not have a compelling reason to exclude studies performed in other (developed) countries.
242	TEP Reviewer 3	Discussion	Speculate or provide information on the likelihood of applicability of findings from other countries/health care systems/home settings or single center sites to a broader dissemination effort in the U.S.	As above, we have added some additional information on setting and applicability. Dissemination is difficult to speculate on. Our goal was to highlight a broad range of intervention types so that end-users could determine which types of interventions may be most feasible (of those that were found to be effective).
243	TEP Reviewer 3	Discussion	Describe QOL findings more fully: Please comment on the clinical significance of changes in scale scores used.	Thank you. The heterogeneity of measures used across studies makes it difficult to make a general statement about QOL. We have given more attention to summarizing these results in the discussion section, as well as significance of these outcomes.
244	TEP Reviewer 3	Discussion	Were any data provided on functional status (e.g. IADL's)?	No additional data was provided on functional status (aside from what is already reported in the review).

Comment Number	Commentator & Affiliation	Section	Comment	Response
245	TEP Reviewer 3	Discussion	You stated there was low strength of evidence for Heart Failure specific QOL but this is a single study that may not be representative and thus the results may be insufficient or at least low SOE and poor applicability.	We have not made a SOE grade other than "insufficient" for a single study that reports QOL. Due to heterogeneity in scales and reporting of the measures in general, it was difficult to assess consistency of results. We will make sure this is clear in the Discussion.
246	TEP Reviewer 3	Discussion	I think the number and size of studies could be included and then as you walk through the data let the reader know if these are from the same or different studies and any concern you might have regarding outcome reporting bias.	The number and size of studies are included in each meta-analyses and also in summary tables. In the text, when we refer to only one study, we note the sample size.
247	TEP Reviewer 3	Discussion	Summarize totality of evidence: The majority of the data is related to health care utilization as defined by <= 6month all-cause or heart failure readmissions and composite outcomes.	As above, we have attempted to summarize the evidence in a more succinct manner (e.g., adding an additional summary table and combining the 3 and 6 month outcome timings).
248	TEP Reviewer 3	Discussion	A clearer statement that little data were available on ER or all-cause visits, and hospitalizations would be useful for policy makers in understanding what evidence is likely "driving" decisions for implementation…and where the key outcome gaps are.	Thank you. We have added a statement about this in the "research gaps" section.
249	TEP Reviewer 3	Discussion	Focus comments on primary outcome and performance measurement: Given the lack of evidence at 30 days should one gap be that reconsideration of whether this as an evidence-based performance measure.	Thank you. We have edited the report to highlight primary outcomes and when we did and did not find evidence on 30-day outcome timings.
250	TEP Reviewer 3	Discussion	Clarify and speculate on discrepancies with past reviews:	We have added additional information to the discussion describing the results of our review in context of other, past similar reviews.
251	TEP Reviewer 3	Discussion	please comment and clarify why findings in this report may contrast with those in the Cochrane review. Particularly related to telephone or telemonitoring and nurse run clinics.	As above, we have added an additional section to the results contrasting the results (and scope) of our review with other recent reviews, including the Cochrane review on HF disease management.
252	TEP Reviewer 3	Discussion	Were there differences in how interventions were categorized, outcomes assessed or studies included	Yes. This is addressed in the Discussion and related to differences in scope.
253	TEP Reviewer 3	Discussion	High intensity home visits (but not low intensity)? best evidence	Yes. High intensity home-visiting programs were effective at 30-days (low SOE) but lower intensity interventions were not.
254	TEP Reviewer 3	Discussion	Some of the wording is a bit "boggy" and could be tightened	Thank you for this feedback. We have attempted to be more concise.

Comment Number	Commentator & Affiliation	Section	Comment	Response
255	TEP Reviewer 3	Discussion	The above includes multiple acronyms that make for difficulty in following the interventions	We followed AHRQ guidelines for which acronyms to use. In addition, we have added a summary table to the discussion that makes it easier to follow our conclusions by intervention category.
256	TEP Reviewer 4	Discussion	There is a nice summary of the key findings in this review which is also presented in tabular format.	Thank you.
257	TEP Reviewer 4	Discussion	There is a comparison with recently published systematic reviews and comparison of current review with these prior reviews, including differences in the methodologies that led to differences in the studies included in the review.	As above, we have expanded this section and highlight how our results differ from other, recent similar reviews.
258	TEP Reviewer 4	Discussion	In particular, limitations in the current literature with regard to patient populations, clinical scenarios, contemporary data, and lack of data on comparative effectiveness of transition care programs are summarized.	Thank you.
259	TEP Reviewer 4	Discussion	In particular, the section on evidence gaps nicely summarizes the key elements for which there are missing data and for which future research should be directed.	Thank you.
260	TEP Reviewer 4	Discussion	Suggested edits to the Discussion section: Please discuss if statistically significant findings for home-visiting and STS (specifically for mortality) were probably because more number of trials with larger sample sizes were included.	We address this issue in the “limitations of the evidence base” section of the Discussion. Specifically, we note that in many cases evidence was insufficient to draw conclusions. In addition, our SOE grades takes into consideration the consistency of evidence across trials.
261	TEP Reviewer 4	Discussion	Under limitation, please address if the eligibility criteria restricted inclusion of trials in certain categories, for example telemonitoring.	The eligibility criteria determine the inclusion of trials. We have noted that certain interventions (i.e., telemonitoring) may or may not be beneficial to patients with HF in long-term disease management.
262	TEP Reviewer 4	Discussion	Please add a line regarding lack of evidence on the potential harms of transitional care interventions in the conclusion paragraph, both in abstract and Executive summary.	Thank you. This has been added.
263	TEP Reviewer 5	Discussion	Generally the discussion and conclusions are appropriate.	Thank you.

Comment Number	Commentator & Affiliation	Section	Comment	Response
264	TEP Reviewer 5	Discussion	A couple of the take-home points are a bit confusing, however. You state that higher-intensity interventions are generally more effective than lower-intensity interventions, but lump STS with MD clinic and home visit programs (ES-12). While I understand that categorizing intervention intensity is difficult, from a health system standpoint telephone-based interventions are quite different in resource intensity than those requiring home visits or a clinic visit. A health system could likely reach a larger number of patients over a greater geographic distance with telephone f/u than with face to face post-discharge f/u.	We have only mentioned intensity for the home-visiting programs due to the fact that our sub-group analysis found a difference (within the homevisiting category) due to higher vs. lower intensity. We did not find this at other time points or for other intervention categories. In KQ3 and 4 (as well as in the Discussion), we do note that intervention categories (as a whole) that were effective are resource intensive. STS programs were not effective in reducing all-cause readmission; even though a health system may reach a larger number of patients with this intervention, it may not meet the health system's (or patients) needs.
265	TEP Reviewer 5	Discussion	- I'm not sure I agree with the conclusion that MD-clinic or home visit interventions deserve the greatest consideration. The review supports this conclusion from the perspective of readmission reduction and this is what the review is focused on. However, you suggest moderate SOE that STS reduced long-term mortality - many would argue this is actually the more distal and important outcome from patient standpoint. I worry that policy makers quickly reading the report would conclude that they invest considerable resources in the highest-intensity resources and discount promising data for STS. Again, some of this comes down to which outcomes are most important - readmission rates are important for health system in part because of the financial penalties, but mortality would arguably be as or more important from patient standpoint.	Thank you. We have since combined the 3 and 6-month time points and have revised the wording of the sections about interventions that deserve greatest consideration. STS and home-visiting programs have similar effects on mortality over 6 months; however, home-visiting programs both reduce mortality and also short-term all-cause readmissions. For this reason, they may deserve the greatest consideration given that they are associated with an improvement in mortality and a reduction in health-care utilization.
266	TEP Reviewer 5	Discussion	Also, not sure I agree that you found no data suggesting a disconnect between readmissions and mortality outcomes - yes, they didn't move in opposite directions, but the STS data suggest that mortality effects may be achieved without doing anything to readmission rates.	We have clarified this to highlight that we did not find evidence to suggest that for interventions that decrease readmission (or health care utilization in general), and that mortality increased.
267	TEP Reviewer 5	Discussion	- applicability - again, would be useful to include something here about control group readmission rates so people get a sense of who this applies to - in general, these are patients with very high 30-180 day readmission rates (though admittedly not too different from what is known about medicare readmission figures for CHF patients generally)	We have included this data in the results and also note in the discussion that control group readmissions are similar to readmission rates in Medicare populations.

Comment Number	Commentator & Affiliation	Section	Comment	Response
268	TEP Reviewer 6	Discussion	p79, line 51: 'current evidence does not establish the efficacy of ….' Again, vague wording. Please fix this. I have not identified every incidence of this in my comments, so it will require some editing on the part of the report writers.	We have edited this report as suggested here and above so that these statements are more precise.
269	Peer Reviewer 7	Discussion	Page 70: lines 26-30; the first sentence is unnecessary, this sentence is repeated verbatim from a previous usage.	Thank you. We have shortened this section.
270	Peer Reviewer 7	Discussion	Page 70, Lines 36, 40, 42: the terms "types" and "category" seems to have been used interchangeably; I would suggest using only one, I think you used category more often elsewhere in the report.	We have edited this section to be more consistent.
271	Peer Reviewer 7	Discussion	Page 72; line 12: footnote "h" should be in a separate paragraph.	This typo has been corrected.
272	Peer Reviewer 14	Discussion	The implications of the major findings are clearly stated.	Thank you.
273	Peer Reviewer 14	Discussion	The limitations of the review/studies are described adequately.	Thank you.
274	Peer Reviewer 14	Discussion	The future research section is clear, however, that does not automatically mean that is easy to translate into new research.	Thank you, we agree.
275	Peer Reviewer 14	Discussion	Although not included in prior RCT, the authors may consider mentioning biomarkers or devices that measure intracardiac, pulmonary pressures or lung volumes to be considered in future research.	These issues are outside the scope of this review; we prefer to focus on transitional care interventions that were within our scope in the future research gaps section.

Comment Number	Commentator & Affiliation	Section	Comment	Response
276	TEP Reviewer 9	Discussion	In general the conclusions are reasonable; however, I am concerned with the conclusion regarding efficacy of home visiting programs for the 30 day all cause readmission outcome. There is the very strong (though small study size) effect from the Naylor study. The 2nd study (Jaarsma) had no clear effect and the combination (Figure 3) did not reach statistical significance. There was also significant heterogeneity. Yet, the conclusion seems to be that since the overall effect size was consistent with 3 and 6 months estimates then it the 30 day non-significant result is probably real. This would make sense if all the studies were independent. However, the same Naylor study was used for the 30 day 3 month and six month outcomes and contributed to each summary effect. Thus there is a bias in saying the results are consistent across time frames.	We do not mean that the one study is consistent across time frames, but that other home-visiting interventions also show similar results across 3 and 6-month time-frames. As noted in the SOE table (footnote): "For home-visiting programs, reduction in 30-day all-cause readmission differed by intervention intensity. The one trial assessing a higher intensity intervention showed efficacy1 while the one trial assessing a lower intensity intervention did not show efficacy.2 In grading the SOE, we considered results of similar interventions at other time-points. The low SOE refers to the overall assessment that the higher intensity home-visiting program reduced all-cause readmission while the lower intensity intervention did not. " We note that the SOE for this conclusion is "low."
277	Peer Reviewer 11	Discussion	The implications are clear and attest to the confusion in this field.	Thank you.
278	Peer Reviewer 11	Discussion	Would like to see further suggestions to translate what they have found into the CE sphere for PCORI.	Thank you. We have highlighted research needs and noted which outcomes are patient centered (e.g., caregiver and self-care burden). However, we did not make specific recommendations regarding what research gaps PCORI should address.
279	Peer Reviewer 11	Discussion	What needs to be compared prospectively? The paper mentions the qualities that have been most associated with improved outcomes, but perhaps needs a bit more.	We have clarified this statement and suggested specific components to be compared prospectively.
280	Peer Reviewer 11	Discussion	Given the emphasis on patient reported outcomes, would dedicate a small paragraph to what the writing group found within those programs that seemed to be beneficial, even if small or trivial.	Thank you. We consider most outcomes included to be "patient centered"- particularly the health and social outcomes. In this case, the utilization outcomes (specifically readmission rates) are likely of great importance to patients as well.

Comment Number	Commentator & Affiliation	Section	Comment	Response
281	TEP Reviewer 12	Discussion	The findings of the study are cleared stated.	Thank you.
282	TEP Reviewer 12	Discussion	The implications of the study are not very expansive, particularly with respect to the cost involved with the complex, highly skilled interventions that were found to be effective.	We have highlighted (in the applicability section) that interventions found to be effective are complex and resource intensive.
283	Peer Reviewer 13	Discussion	NO...the authors backed away from a great opportunity to be bold in the recommendations overall and in particular they failed to specify which strategy and which patient population were congruent. This data is not hard to synthesize. Could the authors provide it?	We have highlighted which strategies are effective; we do not make recommendations on which should be implemented. Rather, we hope to provide information on efficacy that helps stakeholders determine which strategy is most appropriate in a given setting and with given resources. As to particular strategies for patient populations, we did not feel there was sufficient data to make detailed recommendations based on specific subpopulations (e.g., age, severity of HF).
284	Peer Reviewer 13	Discussion	The stratification based on time of followup is not meaningful and it is not clear how this interpretation could be unified given that there was lots of heterogeneity in follow-up across the studies.	We have combined the 3 and 6 month outcome timings but are keeping the 30-day outcome separate.
285	Peer Reviewer 13	Discussion	What would have been innovative would be a type of hierarchical modeling base on the number of components contained in each of the listed interventions as per the printed/published study protocol! [I have a table that I would like to share with the authors for guidance with this...perhaps you could take a look at it and let me know what is the appropriate course of action!	This strategy has potential bias, given that some studies may not list components in as much detail as others. Also, we are not sure that number of listed components should be emphasized over what the components provide, and over things such as intensity. For example, some components may carry more "weight" than others.
286	Peer Reviewer 13	Discussion	Could the authors revisit this strategy and provide the reader with more information pertaining to the complexity of the programs and the relative change or incremental chance in benefit, if any?	We have attempted to accomplish this by looking at "intensity" of interventions (within categories). There are few number of trials in each category (with limited heterogeneity between studies); this prohibits the isolation of incremental value of any one component.

Comment Number	Commentator & Affiliation	Section	Comment	Response
287	Peer Reviewer 13	Discussion	Finally in sensitivity analysis could the authors then explore the associated benefits in the context of different follow up; different patient ages (over >/=65years versus <65 years; US studies versus non-US studies; quality score (high versus others!	There is insufficient variation within included trials (within each category) to perform these analyses. We do include sensitivity analyses in the Appendix which include trials rated as high risk of bias and discuss these results when they differ from our primary analyses.
288	Sandra Oliver McNeil, Public Reviewer	Discussion	Discussion was good and I appreciated the focus on the need for future research in this area.	Thank you.
289	Sandra Oliver McNeil, Public Reviewer	References	Complete	Thank you.
290	Sandra Oliver McNeil, Public Reviewer	Abbreviations and Acronyms	Accurate	Thank you.
291	Sandra Oliver McNeil, Public Reviewer	Glossary of Terms	Well done	Thank you.
292	Sandra Oliver McNeil, Public Reviewer	Figures	Very good	Thank you.
293	Peer Reviewer 7	Appendixes	Appendix E is labeled as “Meta-Analysis”. However, single studies were also included in this appendix along with meta-analyses. Thus, this appendix is more appropriate to be named summary of results or summary of meta-analyses and individual studies.	We have re-named the appendix to “Meta-Analysis and Forest Plots of Individual Trials."
294	Peer Reviewer 7	Appendixes	It would be good to have the metric of the meta-analyses listed in each graft. Currently only the abbreviations such as RD, SMD are listed as column heading along with 95% CI.	Thank you. We also spell these out in the methods and in the text and provide footnotes in the tables (ES and discussion) that explain the abbreviations.
295	Peer Reviewer 7	Appendixes	Appendix G. Table G-1: The meaning of the column heading “Study Limitations” was not defined. It is unclear what does “low”, “medium” study limitations mean.	We have added a footnote defining this.
296	Peer Reviewer 7	Appendixes	Table G-1: The entry under Strength of Evidence, first row (Home-visiting; 30 days), “ILow” should be “Low”	This typo has been corrected.
297	Sandra Oliver McNeil, Public Reviewer	Clarity/Usability	The report is appropriately structured and very well organized.	Thank you.

Comment Number	Commentator & Affiliation	Section	Comment	Response
298	Thomas Denberg, Public Reviewer	Clarity/Usability	The data and main points are clearly articulated.	Thank you.
299	Michele Blair, Public Reviewer	Clarity/Usability	The conclusions are appropriate based on the analyses performed, but in the absence of further analyses, I suspect that the report will have limited impact on policy, practice decisions, or future research.	Thank you.
300	Sarah Goodlin, Public Reviewer	Clarity/Usability	The redundancy makes this report laborious to read. I am sorry to say that I did not see major conclusions that build further on prior meta-analyses of the same body of literature.	Thank you. We have tried to make the report as succinct as possible but also provide enough information to be transparent. The ES is intended to be a brief overview for those who do not wish to read the detailed report.
301	TEP Reviewer 4	Clarity/Usability	As summarized in the earlier points, this is a well written, well structured and organized report on transitional care interventions to prevent readmissions for people with heart failure.	Thank you.
302	TEP Reviewer 4	Clarity/Usability	The key questions are focused and clearly delineated with detailed and summarized presentation of the key findings in this review.	Thank you.
303	TEP Reviewer 4	Clarity/Usability	The conclusions are succinctly summarized and may help inform clinical practice decisions and inform policy, although as pointed out in the review, with the lack of evidence on which components are beneficial, additional studies are necessary to determine implications of these components on selection of interventions in clinical decision making context.	Thank you.
304	TEP Reviewer 5	Clarity/Usability	Yes - generally well organized. See points above.	Thank you.
305	TEP Reviewer 5	Clarity/Usability	Re: policy/practice decisions, issue as raised above - as currently written, there is a risk that the highest intensity interventions could be prioritized while discounting telephone based interventions even though there is promising mortality data for STS.	As above, we have revised the wording of the main conclusions, and we have included the point about STS reducing HF-specific readmissions and mortality among the discussion of interventions that should receive greatest consideration. We ultimately leave that to the reader based on which outcomes are of most interest to decide whether this makes it sufficient to focus on when home-visiting programs and MDS-HF clinic interventions reduced all-cause readmission and mortality.

Comment Number	Commentator & Affiliation	Section	Comment	Response
306	TEP Reviewer 6	Clarity/Usability	I would try to make the conclusions of each section stand alone better.	Thank you. We have edited the conclusions in each section after collapsing the 3 and 6-month time points. This has led to greater clarity in the report.
307	TEP Reviewer 6	Clarity/Usability	Too much detail in the report for anyone to read straight through.	We hope the Executive Summary serves as an overall guide to those who want a brief overview of the report and main findings.
308	TEP Reviewer 6	Clarity/Usability	Better concise and usable summaries of each section.	Sections have been collapsed and better summarized.
309	Peer Reviewer 7	Clarity/Usability	sections need to be edited to correct for incomplete writing and for clarity	These have been addressed.
310	Peer Reviewer 14	Clarity/Usability	Yes, the report is well structured and organized.	Thank you.
311	Peer Reviewer 14	Clarity/Usability	The main points are clearly presented.	Thank you.
312	Peer Reviewer 14	Clarity/Usability	The conclusion can be used to inform policy and/or practice decision.	Thank you.
313	TEP Reviewer 9	Clarity/Usability	The report is well structured though these evidence reports are difficult to read without hyperlinks to different parts of the manuscript and to different studies.	Thank you for this feedback.
314	TEP Reviewer 9	Clarity/Usability	I am concerned that most hospitals will look to this report to find interventions to improve their 30 day all-cause readmission rates. I feel the key point on page 34, that higher intensity home visiting programs were efficacious in reducing readmissions, is misleading. There is really just one study (Naylor 2004) on which this statement is based. Thus, the word programs should not be plural.	We have reworded the SOE statement here and in other places to make clear there was one study at this time point for which we found low SOE for efficacy- that that this was the intervention assess to be “high intensity.” We also note that the SOE for this conclusion is low (not medium or high). As discussed in the report (and in prior comments), we included consistency at time points following 30 days of similar interventions as one domain when grading the SOE.
315	TEP Reviewer 9	Clarity/Usability	Similarly I believe there is only a single study looking at low intensity home visiting.	That is correct.

Comment Number	Commentator & Affiliation	Section	Comment	Response
316	TEP Reviewer 9	Clarity/Usability	The limited amount of data at 30 days should be made clear in the Key Points section.	We have highlighted this point with subsequent revisions.
317	Peer Reviewer 11	Clarity/Usability	Structure is excellent and clear.	Thank you.
318	Peer Reviewer 11	Clarity/Usability	Note my comments under Discussion/conclusion to make the informing of policy and practice more direct and firm.	Thank you for those comments.
319	Peer Reviewer 11	Clarity/Usability	In addition, a piece on whether patient reported outcomes have been "flushed out" in any of the reviewed works would be of great interest and inform future research.	Thank you.
320	TEP Reviewer 12	Clarity/Usability	Clearly written.	Thank you.
321	TEP Reviewer 12	Clarity/Usability	Implications of research may be beyond the expertise of the authors. However, given the evidence, do the governmental financial penalties for preventable readmissions seem reasonable?	This is a compelling policy question; making recommendations as to whether governmental financial penalties are reasonable is not within the scope of this review.
322	Peer Reviewer 13	Clarity/Usability	Yes!	Thank you.
323	Peer Reviewer 13	Clarity/Usability	NO!	Thank you.
324	Peer Reviewer 13	Clarity/Usability	Yes but not as informative, specific and data driven based on the available published evidence.	Thank you. Given the included studies, we feel our conclusions are as specific as the data allows.
325	Peer Reviewer 13	Clarity/Usability	The authors took a short cut and CUT the importance of HF Disease management programs in general and in particular failed to provide guidance as to which stratefy was congruent with a specific patient population stata defined by Age, country of origin; and	We disagree with this statement; rather than cutting the importance of HF disease management, we feel we have highlighted the efficacy of different program types following an index hospitalization.