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Comparative Effectiveness Review Disposition of Comments Report

Research Review Title: *Home-Based Primary Care Interventions*

Draft review available for public comment from April 27 to May 25, 2015.

Research Review Citation: Totten AM, White-Chu EF, Wasson N, Morgan E, Kansagara D, Davis-O'Reilly C, Goodlin S. Home-Based Primary Care Interventions. (Prepared by the Pacific Northwest Evidence-based Practice Center under Contract No. 290-2012-00014-I.) AHRQ Publication No. 15(16)-EHC036-EF. Rockville, MD: Agency for Healthcare Research and Quality; February 2016. www.effectivehealthcare.ahrq.gov/reports/final.cfm.

Comments on Research Review

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Comments on draft reviews and the authors' responses to the comments are posted for public viewing on the EHC Program Web site approximately 3 months after the final research review is published. Comments are not edited for spelling, grammar, or other content errors. Each comment is listed with the name and affiliation of the commentator, if this information is provided. Commentators are not required to provide their names or affiliations in order to submit suggestions or comments.

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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Commentator & Affiliation	Section	Comment	Response
Peer Reviewer #1	General Comments	General Comments Generally good framing of questions, although less clear as to fundamental challenges in the field. (for example, the problem of patients entering hbpc programs at periods of peak clinical instability, and the implications for pre/post analyses in that population) Two studies (one good quality, one fair quality from published methods) were omitted. Good quality study with instrumental variable analysis for ACS hospitalizations. Fair quality study with well matched controls (based on variables in PACE prognostic index, clinical and social variables from AAA intake assessment, zip code/demographics), although known from other presentations, not paper. Significant reductions in NH, hosp, cost; sig increase in community survival. (Articles attached) General tone concern is a conflation of effectiveness of targeting hbpc to frail patients to reduce costs with benefiting from hbpc. As the authors note, there are few outcome measures studied that are patient centric. Reduced utilization is almost certainly not one of them. (Until patients can save in IAH shared savings).	Thank you for your detailed review and comments. We have addressed this in the Discussion. Thank you for calling our attention to these studies. One (Edwards, 2014) was found in our updated citation search and review and included. The other publication (Yudin, 2013) was reviewed and excluded as it did not meet our inclusion criteria for study design. We have revised the text to clarify when the results are about benefit to patients and when they are about reducing costs, and we added text about the need for future studies to include patient-centered outcomes. In the summary of results we have added information about specific aspects of the studies that should be noted when considering differences in results. We cannot speculate about the Independence at Home (IAH) Demonstration results beyond what has been publically released.

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Peer Reviewer #1	General Comments (continued)	Second general issue is the handling of studies around cost, given the bias of the Hughes study, the inconsistency of the hospitalization finding with the cost finding, and what will likely be the results of the IAH demo soon to be released (which won't align well with a conclusion of insufficient data on costs). In terms of weighting and study quality, i think (with a little bit of foreknowledge) that the 2 more recent good quality cohort studies (each of which uses a method being used in the Demo evaluation) are likely to be more accurate of the Demo outcome. In terms of laying out a future research agenda, i'd suggest that the authors contact the CMS project officer, and get permission to speak with the MPR evaluation team, so that they can sort the future research agenda into those items the Demo will likely answer, and those that it won't. (some areas are noted in comments on the draft as examples)	AHRQ has been in contact with relevant individuals at the Centers for Medicare & Medicaid Services (CMS) since the beginning of this project. In addition, we included relevant individuals from CMS on our Key Informant panel and Technical Expert panel, and as reviewers for the draft report. At this time the results from the Independence at Home Demonstration are not publicly available, and we do not have access to any information that has not been released to the public. The evaluation results of the IAH Demonstration are not publicly available, and we are not able to access additional information.
Peer Reviewer #1 (continued)	General Comments (continued)	There seemed to be a general confusion about what the Edes study did--- it calibrated the HCC model against a frail, community dwelling geriatric population (so directly measured the model residual and added it back in as a frailty adjustment), then compared each patient to their own projected cost baseline. There are a number of reasons an HBPC population needs to have a directly measured HCC model residual, but once it is well calibrated it should work well as a cost baseline from which to address cost-saving questions. The flaw in the Edes study was the group against which the calibration was done (GEC NIC users), rather than a well-matched hbpc control group (which the demo is doing). A number of detailed comments are placed on the attached draft.	We have revised the description of the Edes, 2014 study. Thank you. We considered these comments and incorporated many in the revision.

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Commentator & Affiliation	Section	Comment	Response
TEP Reviewer #1	General Comments	Great review of HBPC. Unfortunately, due to the limited availability of literature on the topic, it makes it difficult to draw conclusions on aspects of HBPC that help in patient care or in reducing health care utilization.	Thank you, we agree that the lack of literature is a challenge and have emphasized the need for more research in the revised Future Research section of the final report.
Peer Reviewer #2	General Comments	I found this to be a very thorough and comprehensive review of the literature documenting and assessing empirical studies of home-based primary care interventions. Given the range of programs with various goals, populations served and organizational characteristics, I was particularly pleased to see clear definitions and inclusion criteria; this narrowing of scope and focus provides readers with confidence that they are comparing “apples with apples”, and will also help policymakers, practitioners and other researchers who intend to use this evidence base to support further model development.	Thank you for your review and comments. We have addressed your concerns and our specific responses are below for each report section referred to.
TEP Reviewer #2	General Comments	Because it covers only the past, the report is missing the most important current information we have regarding results from this field, from the IAH Demonstration. The Independence at Home results have been made available to practices, and CMS may be willing to discuss the results with you prior to public release of the information. Since the IAH results are so important to the evidence base for this field, you may want to incorporate what is known into your report to increase its relevance as a current source of information.	Only limited IAH demonstration data had been released at the time of the report completion. Systematic reviews are in nature retrospective therefore we use published studies available during the time of our citation search. We did include what was available in the Discussion section of the review. We have been in contact with CMS; however, we do not have access to information that has not been released to the public.

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TEP Reviewer #3	General Comments	This is an excellent summary of studies to date on HBPC. I think it accurately reflects the state of the literature, ie that there is pretty good evidence that HBPC is associated with reduced hospitalizations, but not great evidence for other outcomes. It also accurately describes the likely great variation in what is called "HBPC" and that there is not good evidence on program factors associated with improved outcomes.	Thank you very much for your comment and assessment of the review.
TEP Reviewer #4	General Comments	Overall, this is a thoughtful and credible evidence-based review. The Key Questions were well framed. Defining characteristics of home-based primary care models were well delineated and I predict they will become a standard for the literature. The approach to the evidence review was well considered. However, I do have some concerns and suggestions. First the language contained in the body of the report and the language contained in the abstract are not entirely consistent. The language in the body of the report is presented in a generally more favorably frame than that used in the abstract – specific examples will be provided below. In terms of the evidence base, I believe there were some studies that were included in the review that should not have been, given the definition of the model adopted. In addition, other studies that met requirements were not included. A serious gap is the lack of any qualitative data, which could have been accomplished with relative ease and would have contributed substantially to fill in some of the gaps in the literature cited by the reviewers. Finally, the discussion could have been better framed in the context of key policy issues that are now facing the U.S. health care system and in the context of the ongoing Independence at Home demonstration from CMMI. Early results from IAH will likely be reported by CMMI within the next few weeks.	Thank you very much. We have addressed your concerns and comments below: We have revised the Abstract to more accurately reflect the full text. We agree that qualitative data would add to our understanding of HBPC, but it was outside the scope of this review. We have revised the Discussion section to include the early results released about IAH and to include text about which questions additional data from IAH is likely to answer.

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Commentator & Affiliation	Section	Comment	Response
TEP Reviewer #5	General Comments	The report is an excellent assessment of the sparseness of the useful outcomes research on home-based primary care. My (perhaps overly) extensive comments below highlight the challenge of defining the intervention or innovation that is being examined – I hope my comments are useful, they are not meant as a criticism of the report, which met its goals admirably. The review pointed up to me, as an economist, what does not appear to be present in the research, or, if present, was apparently not in scope for the review: it could be helpful to pay much more attention to the definition of the intervention. Good primary care consists of many moving parts, with possibilities for substitution of some personnel for others, frequency of in person visits, telephone contact, patient education and engagement for self-care, and so on. Setting of care – office, clinic, urgent care, emergency department, home – is just one of these dimensions. It seems limiting for any research study or demonstration to hypothesize that changing the setting of care alone will have an impact, without accounting for all the other factors. For example, if care in the office is provided by a consistent primary care physician with whom the patient and family can develop a strong rapport while home visits are provided by a rotating series of home care physicians, how can the added value of HBPC be isolated and assessed? The research studies do not seem to be sufficiently clear in specifying the intervention, and the review appropriately highlights this.	Thank you very much for your comments and assessment of the review. We agree with the example, but for this review we defined the intervention as necessarily tied to a location (the patient's home). We acknowledge that there are additional questions that need to be asked and answered and we included some of these in the Future Research section of the Discussion.

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TEP Reviewer #5 (continued)	General Comments (continued)	Further, speaking as an economist, it could be useful to think systematically about the role of the setting where specific primary care services could be provided (home, clinic, office, urgent care) as a kind of input into the production process for health care overall. A primary care practice aiming to produce a given level of primary care outcomes, rather than aiming for fee-for-service revenues based on setting and personnel providing visits, might flex setting for frail older adult patients quite frequently -- e.g. home visits in the winter and office visits in the summer, home visits in an urgent situation when transport is not available, urgent care when the patient can get there, office visits for planned primary care activities for all but the most frail. The HBPC interventions that are described seem to be "all or nothing" – all primary care services delivered in the home compared to standard office-visit care– rather than providing encouragement/ permission to flex the setting as appropriate. I imagine the doctors of yesteryear didn't think of themselves as providing HBPC – they were providing primary care, and home visits were part of their armamentarium, probably used sparingly and only when most valuable, but extensively for some patients.	We agree with this idea and example, but for this review we focused on the existing model of HBPC. We added this idea to the Discussion section of the report.
TEP Reviewer #6	General Comments	Yes. The questions are good but the ability to gather data from a variety of HBPC programs and then assess the data hampers full exploration.	Thank you for your response. We agree that this is a challenge.
Peer Reviewer #3	General Comments	Home based primary care is a care delivery strategy that would seem to be effective and necessary for selected patient populations. For this review, the authors focused on studies that only included adults with chronic illnesses or disabilities--a relevant population for HBPC. The three key questions for this review were appropriate and explicitly stated.	Thank you very much for your comments and assessment of the review.

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Commentator & Affiliation	Section	Comment	Response
Peer Reviewer #4	General Comments	In summary, I thought the report was thorough and touched on many of the key components of home-based primary care. There were a few components of home-based primary care that the authors did not emphasize (such as continued involvement to the end of life, certain aspects of interprofessional care and care coordination) that would have added depth to the report; nevertheless, given the nature of the literature, the areas of emphasis the authors chose were most well defined by the current literature. I appreciate AHRQ focusing on this important care delivery model. I anticipate that over the next several years, the evidence around this model of care will expand tremendously.	Thank you. We have expanded on these topics in the Discussion.
Peer Reviewer #1	Introduction	Good framing of question; would add some background about the difficulties with the field (in particular, how patients enter the programs and the implications for study design, such as clinical instability and patient self-selection. (for which instrumental variables would be useful)	Thank you. We have revised the Introduction to include this idea.
TEP Reviewer #1	Introduction	Page 9, Line 40 - Centers for Medicare & Medicaid Services (not "and") Page 10, Line 32 - CMS demonstration project (not "Medicare") Page 11, Lines 40-42 - "excluded" cell needs commas (should read "primary care home visits, telephone call care only, or nurse") Page 12, Line 6 - "optional" cell, consider adding social services to list of additional services	Thank you. We have made these edits.

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Peer Reviewer #2	Introduction	I have some areas of concern that I feel should be addressed in order to further strengthen the clarity and utility of this review. In the Background section, the authors refer to the growing number of people who may need HBPC. Their numbers, however, relate primarily to people over the age of 65. Yet the studies they review include all adults. It would be helpful to include some citations documenting the size of the 18 to 64 age group that might have a need for this type of program.	We have added the numbers of people 18 to 64 years old with functional difficulties to the Introduction since our inclusion criteria includes all adults.
Peer Reviewer #2	Introduction	In the Background section, the authors identify a series of potential benefits of HBPC, including reduced hospitalizations and urgent care. This translates into reduced costs to the Medicare program, a goal that is part of CMS' triple aim and should be formally articulated in this section of the report.	While we agree that reducing costs is likely important to Medicare, understanding and discussing the priorities of CMS is outside the scope of this review. We refer to the goals but do not attribute them to CMS as we do not have a source that explicitly links these under the triple aim label.
Peer Reviewer #2	Introduction	In the section on the growth of interest in HBPC, the authors should include the importance of new financing mechanisms and reimbursement policies and a particular focus on interdisciplinary team models as drivers for expansion of HBPC.	Thank you. We have incorporated these ideas into our revised Introduction.
Peer Reviewer #2	Introduction	In the discussion of challenges to synthesizing the literature, the authors should include variations in the goals of the program and the providers who are engaged in these efforts.	Thank you. We have incorporated these ideas into our revised Introduction.

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Peer Reviewer #2	Introduction	In the Scope and Key Question section, it is not clear to me from Table 1 whether studies of HBPC in assisted living were reviewed. Are people living in that setting considered institutionalized or living in the community? If these studies include that setting, the analyses could have a strong selection bias. In that table, it is also not clear whether long-term services and supports are included as a required or optional service. Many HBPC programs integrate primary and LTSS in their activities.	We defined "patient's home" broadly, and this would include assisted living. However we did not identify any studies that provided specifics about the home setting. We did not require that HBPC programs include long term services and supports (LTSS), though as you point out, many do. We have revised Table 1 to clarify these points.
TEP Reviewer #2	Introduction	No comments	NA
TEP Reviewer #3	Introduction	Excellent. Very clear definition of HBPC, which is critical given the variation in what is called HBPC. Very clear analytic framework, and corresponding key questions.	Thank you.

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TEP Reviewer #4	Introduction	In general the introduction is well organized and focused. Specific items: Line 41: Would consider being more explicit that "Medicare home health" in this context is actually Medicare home health under the Medicare skilled home health care Part A benefit. Page 2, Line 34: In addition to an increasing number of public and private health systems and plans beginning to offer HBPC, there have been venture capital-funded enterprises in the private sector that are doing the same. That might be worth including. Might also be worth noting that recently the first home-based primary care-only accountable care organization (ACO) totally focused consisting only was formed by VPA in the Michigan area and across several other states. Page 2, Line 51-54: The fact that home-based primary care services are not standardized is clearly a challenge for the field. At some level though this is a gratuitous comment in the context of complex models of care. The same can be said of any complex model – PCMH, etc. In addition there is a relatively standardized model of home-based primary care within the VA at over 170 sites around the country. That point should be highlighted. Page 3, line 12: A comment regarding the population served ranging from generally well to severely disabled patients. My guess is that the generally well population referred to in this context is that seen in the context of preventive home-based visits. I don't believe any of the articles included in the review actually focused on generally well patients. That should be clarified carefully. In terms of the scope and key questions - these are well framed.	Thank you. This has been revised. Thank you for this information. Our experience is that there is considerable variation even within the VA HBPC programs and we have noted this in the Discussion section of the report. This has been revised to be clear that we are not addressing preventive home visits or care of the well elderly. Thank you.

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TEP Reviewer #5	Introduction	The introduction is very clear. However, I wonder if some logical steps are missing. High quality primary care is defined at p. 9 lines 14-20. Then the paragraph at line 46 explains why some patients need HBPC – although I think this means, could benefit from HBPC -- and it's not fully clear that these needs can only be met in the home. Then the paragraph at p. 10 line 2 lists potential benefits of HBPC, but one of these (access to a range of services) doesn't seem to be part of the definition of primary care – or is access to therapies part of primary care? It might work better if primary care were defined and then a discussion presented of the potential benefits (and costs) of supplying these services in usual settings and in the home. (Do any of the demonstrations mix the settings, i.e. provide some services in the home and some in the outpatient setting? Is this important to their costs or outcomes?)	Thank you. The Introduction has been revised based on these suggestions. We did not identify any programs that mixed settings, though the study descriptions are not detailed and it is possible there were some.
TEP Reviewer #6	Introduction	Good	Thank you.
Peer Reviewer #3	Introduction	The introduction for this review provided a strong rationale for the importance and timeliness of HBPC interventions. The introduction provided support for the three key questions addressed in the review. They also introduced the challenges inherent, at this time, in studying the effectiveness of HBPC, specifically the variability and non-standardization in interventions, services, and resources needed/used for those interventions and services.	Thank you.
Peer Reviewer #1	Methods	Criteria are reasonable. 2 additional studies with significant results were overlooked.	Thank you for calling our attention to these studies. One (Edwards, 2014) was found in our updated citation search, reviewed, and included. The other publication (Yudin, 2013) was reviewed and excluded as it did not meet our inclusion criteria for study design.

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TEP Reviewer #1	Methods	Page 14, Line 44 - "included" cell should read "patient and caregiver experience" (experience should not be on a separate line)	Thank you. This has been corrected.
Peer Reviewer #2	Methods	In Table 2, health outcomes are identified but it is not clear whether this includes functional outcomes. I also would like to see costs of care as a specific outcome included in this summary table. In Table 3, costs are identified in 6 studies but this variable is not included in Table 2. A cross referencing of Tables 2 and 3 would help the readers better navigate the report.	Table 2 has been revised to incorporate these suggestions.
Peer Reviewer #2	Methods	In the body of the review on included studies and in the Appendix E, it would be extremely helpful to include a section on sources of data for each of the outcomes (administrative data,claims data, medical records, surveys,etc.) and where appropriate, how the data were collected(mail surveys, telephone or in-person interviews, etc.). This information would be useful to readers in their interpretation of findings and application to additional work.	This has been added to Appendix E.
Peer Reviewer #2	Methods	Given that the populations included in the various studies could be living in very different home-based settings (individual homes, congregate settings, retirement communities for veterans or other groups, etc.,), I suggest that where data are available, the authors include the setting to help readers better interpret the findings. If the data are not available, this should be identified as a limitation and further research suggested to differentiate subpopulations according to their living arrangements.	We re-examined what the studies report and have added text to the Results section stating that detail on the type of housing considered “home” was not provided. We have also added this to the Future Research topic list.
TEP Reviewer #2	Methods	No comments	NA
TEP Reviewer #3	Methods	Very clear.	Thank you.

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TEP Reviewer #4	Methods	In terms of the literature cited, there's been a recent study of home-based primary care and the risk of ambulatory care sensitive condition hospitalization among older veterans published in 2014 in JAMA Internal Medicine which should be considered for inclusion in the study. Edwards ST et al. Home-based primary care and the risk of ambulatory care-sensitive condition hospitalization among older veterans with diabetes mellitus. JAMA Intern Med 2014;174:1796	This study was identified in our update search and included.
		In addition, there was a study of place of death among patients cared for in home-based primary care for in Home-Based Primary Care that I believe also meets study inclusion criteria. Leff B, Kaffenbarger KP, Remsburg R. Prevalence, effectiveness, and predictors of planning the place of death among older persons followed in community-based long term care. J Am Geriatr Soc. 2000 Aug;48(8):943-8. PubMed PMID: 10968299.	This study was considered but excluded as there is no intervention/comparison. All patients were in HBPC and the study looked for whether records included mention of planning for location of death. Nothing was done to promote or encourage this planning so we did not consider this an addition to HBPC.
		There are several studies that perhaps should not be included in this review given the definitions established by the investigators. • Reference 26 author Aabon, the study examined whether an office-based general practitioner would make a visit or two to a dying patient and the effect that had on the patient's place of death. It was restricted to cancer patients and it's certainly possible that most patients received only one or two visits by the general practitioner. I don't believe this qualifies as home-based primary care. • Reference 37 author Neergaard is not home-based primary care. • Reference 38 last author Nichols, 2011, this study seems to be a test of the dementia intervention on top of home-based primary care and probably does not meet the definition.	These studies (Aabom, 2006 and Neergaard, 2009) were included because they met our definition in Table 1 of the full report, longitudinal and comprehensive care that included general practitioner home visits. Nichols, 2011, meets our definition as well. As all the patients received HBPC, it does not provide evidence for KQ1 (effectiveness of HBCP), but it is one approach to testing the effect of a potential component of HBPC, in this case the dementia intervention, and so it does provide evidence for KQ3.

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TEP Reviewer #4 (continued)	Methods (continued)	• Finally, the GRACE study by Counsell, et. al. likely does not quality as home-based primary care. GRACE is not home-based primary care but a co-management / care management model provided by a team of a GRACE nurse practitioner and social worker that support an office-based primary care physician. Physicians under the GRACE model do not enter the home. It is care management rather than home-based primary care. At best it is something of a hybrid model. I conferred with Dr. Counsell on this issue. This study is one of the few RCT's included in the review. If it is included, this distinction should be called out in the text very clearly. The issue of GRACE comes into play on page 13 under key question 1c.	We have added the following text in the Results section: "We did include models where an additional physician may be involved or responsible for care but does not make home visits. For example, in the GRACE model a nurse practitioner (NP) and a social worker make regular home visits to conduct assessments and provide care, but another primary care provider and a consulting geriatrician may be involved in care planning without making home visits". While we agree that this could be viewed as a hybrid, we allowed for a range of model variation as long as the model met the definition we established for the review. Early in scoping, with input from stakeholders we broadened our definition to include models that included home visits by a primary care provider, which could be an MD, NP or physicians assistant. With the broadened definition, the GRACE study met the inclusion criteria.
TEP Reviewer #5	Methods	The inclusion criteria are well-justified. The search strategies are well documented. The authors have done everything they could but are not well supported by the literature itself, which lacks full definitions of the implementation of intervention being examined. Costs of the intervention are not discussed -- perhaps they are not reported by the research. Or are they out of scope of the review?	We did not exclude cost of the intervention as long as it was included in an outcome (e.g., the cost to provide care minus the payment received). However almost none of the studies reported this.
TEP Reviewer #6	Methods	Yes	Thank you for your comment.

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Peer Reviewer #3	Methods	The authors described the defining characteristics of HBPC models included and excluded in the review. The inclusion and exclusion criteria followed the PICOTS format resulting in a defendable set of studies selected for the review. Eighteen studies met the criteria for this review. Appendix B included the details about the methods used for triaging the literature, construction of the evidence tables, and grading the strength of the evidence. Statistical methods were used and briefly described in Appendix A, but the results were not provided (meta-analysis to provide estimates on outcomes; meta-regression to explore statistical heterogeneity).	Upon fully reviewing the diverse data reported in the studies we were unable to conduct a meta-analysis. Appendix A describes in detail the methods of this systematic review, and was revised to reflect our actual approach.
Peer Reviewer #1	Results	see attached comments. key outcomes/severity metrics to include for all reported studies would be mortality (low mortality is reflective of a different population, e.g., the Chang and Counsel studies, although Chang might have been due to design bias) and for RCTs, duration of exposure (the major bias in the Hughes study) for a number of studies, i couldn't figure out the exposure duration. The finding from Hughes on caregiver function should probably be placed in the more recent context of health effects of caregiving. A 1-major disease impact on the SF-36 is significant, and unaffected by the fidelity bias in the study; since the Hughes study the health impacts of caregiving have received significant attention. All of that context is missing. Cost issues w Edes study-- see above would have a summary of the pre/post issues, rather than the blow-by-blow on individual outcomes.	Thank you for your comments; we considered them as we made our revisions. We agree but these outcomes were not reported in all of the studies. For the studies that reported mortality we included the data. We established a specific time period for the duration of the studies and all studies that met the criteria were included. Length of followup is described for all of the studies in Table 3 of the report and in the evidence tables. We revised the description of the Edes, 2014 study. Not every study was pre-post and given the small number of studies we summarized the individual outcomes.
TEP Reviewer #1	Results	Page 21, Line 46 - should be "while a poor-quality article" (not an poor-quality) Page 27, Lines 36-37 - need $ (HBPC $58,689 vs. usual care $76,827)	Thank you we have corrected these.

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Peer Reviewer #2	Results	In the results section, key question 2a, it would be helpful for readers to know the ages included within each study. In the Key Question 3 section, how was effectiveness measured? The authors indicate that there is no apparent pattern of services associated with effectiveness (p. 20), but it is not clear to me what that statement means. This needs clarification.	What the studies reported about age is included in the Evidence Tables in Appendix E. We also added text in the Results section that discusses the age criteria used in individual studies and the age of study subjects. This has been revised to be more clearly stated: "including, but not limited to, use of teams, composition of teams, use of technology, frequency of visits, and types of visits/services".
Peer Reviewer #3	Results	To the knowledge of this reviewer, the authors did not overlook any studies that should have been included in the review.	Thank you.
TEP Reviewer #3	Results	Very clear, easy to follow. I was disappointed to not see Edwards et al. JAMA IM 2014 included, as it was one of three quality observational studies on outcomes in HBPC that came out last year (along with Edes JAGS 2014, and DeJonge JAGS 2014), all of which used different methodology. While they did only look at patients with diabetes, they examined the effect of VA HBPC which is not a diabetes specific intervention, and measured ACSC hospitalizations, which is not a diabetes specific outcome.	Edwards, 2014 was added in our update to our search during peer review. While it was published in Nov. 2014; the indexing was completed in March 2015.

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Commentator & Affiliation	Section	Comment	Response
TEP Reviewer #4	Results	In terms of the overview of the findings, one additional point that could be highlighted under the overview is that despite some challenges with study designs, there is a remarkable consistency in the direction of effects and that does lend some sense of strength to the evidence overall. The second sub-bullet under utilization of services that's in parenthesis at the end of that bullet it says "low strength of the evidence." Yet on page 13 lines 5 through 11 all those results are positive include two RCT's. Please reconcile that.	The text has been revised to clarify the basis for the ratings.
		Page 12, line 44: The notion that few studies focused on health outcomes is probably correct as stated if taken in a literal sense. However it is important to recognize that avoiding hospitalization in an extremely frail group of patients actually is a health outcome. The hospital is a toxic environment for those frail older patients. There is a vast literature on the harmful effects of the hospital environment for older frail adults. Avoiding hospitalization should be considered a health outcome in this population.	We have revised the text to include an expanded discussion of hospitalizations and how they may have different value to patients, providers, and payers.
		Page 12, line 48: Issues related to programs reporting mortality. It's true that most programs did not, however, some of the best and most recent studies did, including the study by DeJonge. That point should be highlighted. Further it is well known in the field that practices commonly experience mortality rates of 20-25% on average per year. On page 11, in terms of outcomes would consider adding study of Leff and Kaffenbarger under place of death.	We have revised the text to clarify that De Jonge (2014) and Counsell (2007) were the included studies that reported mortality. This article (Leff, B, Kaffenbarger, KP , et al. Prevalence, effectiveness, and predictors of planning the place of death among older persons followed in community-based long term care. J Am Geriatr Soc. 2000 48(8): 943-948) was excluded because it was the wrong study type.
		Line 36: Reference 46 is referred to in terms increasing hospitalizations. This is another study that should not be included in this review. This is a study of a post-discharge transitional model-only that included a GP and a dietician versus not including the dietician. This is not a home-based primary care intervention.	This was an error in inserting a citation, and has been corrected. The included study should be Beck, 2009. Beck, 2013 was excluded. We have corrected this in the text and study lists.

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TEP Reviewer #4 (continued)	Results (continued)	Page 20: Overview of findings for key question 3. The authors suggest that there is no apparent pattern of services associated with effectiveness. The authors may wish to review the study by Stall et al Journal of the American Geriatrics Society. This was a recent systematic review of home-based primary care interventions. (Stall N, Nowaczynski M, Sinha SK. Systematic review of outcomes from home-based primary care programs for homebound older adults. J Am Geriatr Soc. 2014 Dec;62(12):2243-51. doi: 10.1111/jgs.13088. Epub 2014 Nov 4. Review. PubMed PMID: 25371236). The authors did come to some conclusions about the core components of programs that were affected including the presence of inter-professional care teams, regular inter-professional care team meetings, and as well as other factors. It would be worth including that in the review and stating whether you agree or disagree with their conclusions, and why. In terms of thinking about the studies that were included in the review, it is notable that the more recent studies which tended to have better methodology, tended to show positive results. The authors included the study by Hughes et al from the VA. This study has been long disputed in the literature. There are many who believed that this really was not home-based primary care. The intervention lasted approximately 5 months on average, which is not a primary care interval. It's well acknowledged in the field that most providers believe that they don't start to see the true effects of the intervention until after 6 months of providing care to the patient. The authors should consider stratifying the studies by date. A reasonable cutoff date would be somewhere around the area of 2005. The Hughes study also suffered from tremendous lack of fidelity.	Thank you. We have revised our summary of these results and the Discussion section to clarify this. We agree with the Stall, 2014 review that certain characteristics are common across HBPC programs. We were looking for combinations of characteristics that were associated with more successful programs and we were not able to identify these. Many of the issues with the Hughes study were considered, and thus it was given a fair rather than a good quality rating. However, length of followup was not a quality criterion and so did not affect the quality rating. Length of followup also was not a reason for either including or excluding a study from our review, so the study was not considered for exclusion on that basis. The issue of fidelity to the model is one that can be raised with several studies, including the recent nationwide VA studies. HBPC differs across VA medical centers, and some may not adhere to VA policy related to staff and services. We incorporated this issue into our discussion, but based our assessment on what was reported. The literature search cutoff date is always an important decision. HBPC has existed for more than 10 years and we decided that a longer search period was warranted. While context and interventions may change over time, we do not believe that recent studies are inherently more valid or that stratifying by date would change the findings.

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Commentator & Affiliation	Section	Comment	Response
TEP Reviewer #5	Results	The results are presented clearly. The tables are very helpful.	Thank you.
TEP Reviewer #6	Results	Yes, there aren't enough studies which impacts the conclusions.	Yes, we agree and have emphasized the need for additional future research.
Peer Reviewer #1	Discussion	Would be a little more clear about what a core outcome set should be (e.g., mortality [as severity measure primarily], exposure, patient-centric outcomes in a guyatt framework , caregiver outcomes] to help ensure the field collects a similar set of measures going forward. Would also lay out a suggested outcome set for the various private entities that are moving ahead with hbpc programs (e.g., the managed care world); one would be to use an hbpc-residual specific hcc model for a cost-baseline for comparison to actual costs. Would be clear about what the IAH demo will and will not answer, rather than a vague assertion that it won't answer all questions.	We agree that that it would be useful for all HBPC programs, regardless of how they are financed, to measure common outcomes. We have included the need for common outcomes in the Discussion section. Deciding on the core outcomes is important, but is beyond the scope of this systematic review. We have revised the Discussion section, and added more information about the IAH demo results and what we believe they will and will not answer in the “Future Research” section.
TEP Reviewer #1	Discussion	Page 32, Lines 16-17 - Need commas ("home care to office-based primary care, that conducted short-term preventive assessment home visits, or models...") Page 32, Line 17 - remove "but that" (should read "examples that we excluded in earlier reviews" Page 36, Line 12 - explicitly, not explicit Page 37, Line 45 and Page 38, Line 24 - CMS IAH Demonstration (not Medicare Demonstration) Page 36, Line 50 - remove 18 (do not include the number of IAH demonstration sites, as this may change before the completion of the demonstration)	Thank you. We have made these revisions to the text.
Peer Reviewer #2	Discussion	In the discussion of challenges to synthesizing the literature, the authors should include variations in the goals of the program and the providers who are engaged in these efforts.	Thank you. We have revised the text to include this suggestion.

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Commentator & Affiliation	Section	Comment	Response
TEP Reviewer #2	Discussion	No comments	Thank you.
TEP Reviewer #3	Discussion	Yes. I think this does an excellent job of laying out what are the important next steps for research in understanding this care model.	Thank you.
TEP Reviewer #4	Discussion	In the discussion the authors should also make a strong argument and highlight the fact that there is value in home-based primary care simply by providing access to primary care for people who would absolutely have no other means to access it. To the extent that this model does anything above and beyond that and does not cause should be highlighted in bold letters Limitations Line 29 and 30: Authors note there's a lack of detailed information on implementation and content of interventions. This is not unexpected given short length of journal articles and the complexity of the interventions. The investigators should do qualitative work by interviewing investigators about this very important issue. This serious flaw can be addressed. In the discussion the author should acknowledge the remarkable consistency and direction of change and the effect on a very sick population even if the programs are not entirely consistent in their components. Page 27, Under Limitations: Again the issue related to in Lines 55 – 57, the issue that utilization is not just utilization avoiding hospitalizations in a very frail population is actually is a quality of life issue.	Thank you. We have incorporated this point in the revision of the Discussion. We agree that this would be useful though it is outside the scope of this review, and we have included it in our discussion of future research needs. We have revised the text as suggested about the consistency, direction, and effect on more frail populations. We have added text discussing the different meanings of hospitalizations.

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Commentator & Affiliation	Section	Comment	Response
TEP Reviewer #4 (continued)	Discussion (continued)	Page 29, line 15-16: The line that reads "we still lack evidence about the value of home-based primary care." Based on the review this is not at all consistent with what is presented to this point. You have a model that is targeting the most frail patients meeting the triple aim in reducing hospitalizations and costs. The obsession with RCTs in a population for whom that is quite difficult to accomplish, this again is another example of a gratuitously negative comment. It is certainly appropriate to say that we may lack the highest quality evidence. But to say we lack any evidence, which is the implication of that phrase is, at some level, inappropriate. Such comments do not serve the health care community or policymakers. Further, the authors should consider that their review will be coming out probably just ahead of or just behind the release of results of the Independence at Home Demonstration. Such a demonstration will be regarded as gold standard evidence given its provenance as a CMMI innovation.	Thank you. We have revised the text in the Future Research section significantly to clarify what is and is not known and what knowledge we think would advance the field. In addition, we have added a section on study designs, which outlines challenges in studying HBPC and potential ways to study this model of care and advance our knowledge.

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Commentator & Affiliation	Section	Comment	Response
TEP Reviewer #5	Discussion	The authors do a good job with very scant material to pull out generalizable findings about targeting HBPC to the most frail patients. The authors' distinction concerning patient goals is extremely important -- desired outcomes may differ depending on patient goals. One takeaway for future research could be to suggest separate studies to focus on value of HBPC for patients receiving hospice care at home, in contrast to the value for patients with multiple chronic health and functional conditions who are not in a terminal phase of life. The overall message of the report, that existing research is difficult to use as policy evidence and that better research is needed, comes through loud and clear.	Thank you. We have revised the Discussion to include the idea that HBPC can serve different types of patients and that the different types of patients may have different goals.
		Omitted from the discussion of future research is consideration of the variation and evolution of Medicare payment approaches. Future research should consider the impact of financing differences (PACE, Medicare Advantage, Accountable Care Organizations, the dual-eligible demonstrations, VA, fee-for-service Medicare) on the provision and impact of HBPC. When a physician group is at least partially responsible for all medical care costs and outcomes for a patient population, they could find it cost-effective to flex benefits and provide some services in the home. The variation in incentives between accountable care organizations and fee-for-service Medicare could provide the basis for research on who receives home visits under what circumstances with what overall outcomes. (Medicare Advantage plans apparently use home visits, sometimes to uncover undiagnosed, and unrecorded, conditions in new members; rather than being lauded as an effective intervention, as suggested by the review, this is controversial because of the impact on plans' risk adjustment.)	Thank you. While the larger context and the relationship between different programs are beyond the scope of the review, we have added these ideas in our revision of the Discussion section.

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Commentator & Affiliation	Section	Comment	Response
TEP Reviewer #5 (continued)	Discussion (continued)	Another way to frame and spark future research might build on the targeting suggested by the current review and consider the variation in provision of primary care for older adults in long-term care settings. Persons with functional disabilities are found in nursing homes, in private homes receiving informal and formal services, in assisted living and other group residences, and in adult day health. How does primary care differ in effectiveness for older adults in these settings, and could bringing better services to the patient, rather than expecting him or her to go out for ambulatory visits, be valuable across all these settings, as well as in the home? For example, adult day health also might be a promising setting for community-based (as opposed to outpatient-provided) primary care for older adults receiving long-term services and supports. Bringing together this LTC- setting dimension with the financing dimension, a thought-provoking comparison might be made between HBPC and the primary care provided to nursing home residents under new Medicare payment approaches. The usual practice is for nursing home residents to switch to physicians associated with the nursing home they enter. But as accountable care organizations begin to take responsibility for hospitalizations and other Medicare utilization for their patient populations, we are observing more physicians following their patients into long-stay nursing home settings – making “house calls” in the nursing home for the first time, either in person or through nurse practitioners. To the extent that the home is also a long-term care setting for some patients, it will not be surprising if very frail ACO-affiliated patients start receiving house calls for urgent situations that don’t really require a costly ambulance trip to the ER.	We agree this is interesting and while it is outside the scope of the review, we have added this idea to the Discussion where appropriate. We agree that there may be many changes and different variations on HBPC in the future as new models of service delivery and finance are developed. Whether a care provider following patients into a long-term care facility will be for long term or post acute care and whether they will be the same care provider who provided the office based care remains to be seen. These suggest interesting though different models than those we considered in this review.
TEP Reviewer #6	Discussion	Yes. The limitations are indicated. This is particularly shown in the multiple "low strength" conclusions which from a practice point of view on the ground we see many of these categories in a clearly stronger way.	Thank you. We agree that this is challenging and hope that future research will improve the ability to draw conclusions.

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Commentator & Affiliation	Section	Comment	Response
Peer Reviewer #3	Discussion	The discussion section included a particularly helpful table (table 6) that outlined each of the outcomes for each key question, a summary of the findings and the strength of the evidence. Study limitations were comprehensively addressed. The authors identify a number of significant recommendations for the research that is needed on this topic as well as study design and method recommendations. They noted that the Medicare Independence at Home demonstration project may provide important and relevant data and findings to contribute to the body of knowledge that is lacking for HBPC research.	Thank you.
Peer Reviewer #2	Discussion	In the Applicability section, the authors indicate that while the most common patients may be the homebound elderly, age is not the predominate factor and this model could service patients of various ages. As I noted above, it would be good to see the ages included in each of the studies to better understand the applicability across populations. The authors do note that many of the studies were conducted with a primarily male veteran population, very different from the average elderly female population. But it is also likely that many vets who could benefit from these programs are under age 65. So differentiating these age groups is important.	The information on age reported in the studies is included in the evidence table (Appendix E), in the inclusion criteria column and in demographics. We have also added a paragraph to the Results section. While it is a possible and interesting question, none of the studies in this review included many non-elderly adults and none of the studies presented results by age subgroups. This seems like an area that needs future study. .
Peer Reviewer #2	Discussion	also think the paper goes a bit far in speculating the best organizational structures for HBPC (p.28). Given the lack of evidence in the review, the need for further research in this area should simply be highlighted.	Thank you. We have revised the Discussion section to add more detail about the need for future research and explained in greater depth the variations in the structure of HBPC programs.

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Commentator & Affiliation	Section	Comment	Response
Peer Reviewer #2	Discussion	On a very specific note, the De Jonge study should identify the Medstar health care system (formerly the Washington Hospital Center) as the auspice for the HBPC program.	Thank you. This has been added to the results section for KQ 1c: "in a Washington, D.C., HBPC program that is part of the Medstar Health Care System ($44,455 vs. $50,977, p=0.001) De Jonge, 2014."
TEP Reviewer #4	Conclusion	Lines 8-12: Should be the type of language that appears in the abstract to this paper. The abstract could use some language that is more closely aligned with the results and the text of the document.	Thank you. We incorporated this suggestion into our revision of the Abstract.
Peer Reviewer #1	Clarity and Usability	as is, some of the conclusions may mis-inform policy decisions, and will likely be at odds with more compelling evidence that will be released soon. Those relate primarily around the cost question, and how the evidence was weighted and combined.	While we cannot include evidence that has not yet been made public we have tried to address this by both adding more detail to the Results section (so it is clearer what can be said) and by discussing areas where the IAH results may answer question in the Discussion section.
TEP Reviewer #1	Clarity and Usability	The authors did address the importance of HBPC from patients' perspectives. It is also important to consider other stakeholders, such as insurance providers like CMS, and attaining the goal of improving patient care while reducing health care utilization (and cost).	We agree that the different perspectives are important and we have revised the text in several places to emphasize this.
Peer Reviewer #2	Clarity and Usability	NR	NA
TEP Reviewer #2	Clarity and Usability	See above--without the IAH results the relevance to policy and practice decisions will be limited.	While we cannot include evidence that has not yet been made public we have tried to address this by both adding more detail to the Results section (so it is clearer what can be said) and by discussing areas where the IAH results may answer question in the Discussion section.
TEP Reviewer #3	Clarity and Usability	The report is well structured and organized, and the main points are clearly presented.	Thank you.
TEP Reviewer #4	Clarity and Usability	No additional comments.	NA

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TEP Reviewer #5	Clarity and Usability	The report is clear and well organized. The authors have done an excellent job in summarizing the state of current research. The literature is too weak to support practice decisions. It is my opinion, as indicated by my comments, that an expanded framing of HBPC, considering more deeply the limitations and value of the role of setting (home, office, clinic, etc.) in the provision of primary care, might be more fruitful. But I believe this was beyond the scope of what the authors were asked to do.	Thank you. We agree. Yes, though outside the scope of the current review, this is an interesting idea, which we now mention in the Future Research section.
TEP Reviewer #6	Clarity and Usability	I would see this as a starting point to ask more questions and get more similar data from a variety of programs. I would be wary of basing policy on this study, as it is not reflective but based on limited data, and some data that does not compare.	Thank you. We considered this as we revised the text and make this point in the Discussion section.
TEP Reviewer #9	Clarity and Usability	The report was extremely easy to follow; well organized. The appendices and tables were clearly presented. Main points were consistently reiterated in the abstract, results, discussion and conclusions.	Thank you.