PEER REVIEW HISTORY

BMJ Open publishes all reviews undertaken for accepted manuscripts. Reviewers are asked to complete a checklist review form (see an example) and are provided with free text boxes to elaborate on their assessment. These free text comments are reproduced below. Some articles will have been accepted based in part or entirely on reviews undertaken for other BMJ Group journals. These will be reproduced where possible.

ARTICLE DETAILS

TITLE (PROVISIONAL)	A cross-sectional survey and service evaluation of simple telehealth in primary care: what do patients think?
AUTHORS	Cottrell, Elizabeth; Chambers, Ruth; McMillan, Kate

VERSION 1 - REVIEW

REVIEWER	Bonnie J Wakefield, PhD, RN, FAAN Associate Research Professor University of Missouri School of Nursing Columbia, MO USA
REVIEW RETURNED	14-Jun-2012

THE STUDY	The "effectiveness of reducing BP" is not operationalized - reduced by a certain amount? reduced relative to non-intervention patients?The design introduces bias because it is a convenience sample. There were two inclusion criteria, but it seemed that others who did not meet the criteria were included (it was a bit confusing). Later, in the discussion it specifies differing results when "results were analyzed according to inclusion criteria" and later they acknowledge the inclusion criteria were not always adhered to; this makes the reader suspicious about what else happened during the data collection period. Were the younger patients those with CKD? That was hard to tell from Table 1. Data were differentially collected from control and intervention, ie every 2 months for intervention, every 3 for control. Unclear whether the data collection period was 3 or 6 months - if the latter, only 19 were still using the system. The drop out rate seemed high, approximately 32% indicating problems with the system or user satisfaction/perceptions. Table 2 shows mean BP which are not informative given the varying enrollment cut points, the variablity across patients, and the drop out rate. Table 3 appears to use changes in BP but why were linear models not used to analyze the data? In the discussion, the authors note that changes in BP were likely due to changes in medications, but there is not data to support that - could it be greater patient awareness of BP or better adherence? The quick reductions could be regression to the mean.
RESULTS & CONCLUSIONS	because inclusion criteria weren't adhered to, the credibiity of the data can be questioned. The statistical analysis is unsophisticated, and the drop out rate was high. Conclusions are drawn that are not supported by the data.
REPORTING & ETHICS	the study flow was very difficult to follow and so I am unsure whether it meets any checklist.

REVIEWER	Dr Janet Hanley Principal Research fellow

	Edinburgh Napier University School of Nursing, Midwifery and Social Care Sighthill Campus Sighthill Court Edinburgh EH11 4BN
REVIEW RETURNED	25-Jun-2012

THE STUDY	This The methods section of this report is inadequate. The title / introduction refers to a qualitative study, but the method section is short and mainly describes the lickert scales used in electronic questionnaires. There is no description of the discussion groups (which are only mentioned in the results) or how the resulting qualitative data were analysed.
REPORTING & ETHICS	This was treated as a service evaluation so research ethics not required
GENERAL COMMENTS	This looks like an interesting study, but it cannot be recommended for publication without much more detail in the methods section. It is only when that is available that a judgement about how well the question is answered can be made. The theme of 'companionship' resulting from telehealth is a novel interpretation of patients' response to telemetry surveillance which the patients often find difficult to describe. It would be worth putting quite a lot of work into revising this paper

VERSION 1 – AUTHOR RESPONSE

Many thanks for your constructive and important comments and suggestions. We have improved the methods section as you requested with particular focus on adding detail of each of the methods used to obtain feedback and clarifying how the results were analysed. This was a service evaluation, therefore recruitment did not continue until we had ensured data saturation but rather as many patients as possible were recruited during the allocated programme period and we have had to pragmatically work with the results this has yielded. This detail has been added to the discussion for clarity to and transparency for the reader.
Given the novelty of the results about simple telehealth becoming a companion of some of the patients it serves, we have emphasised this result in the concluding statements.
Finally, we have made changes to the reporting of the use of Florence over the programme and data collection period to align this paper with its partner paper.

VERSION 2 – REVIEW

REVIEWER	Janet Hanley Edinburgh Napier University UK Competing interests: I am involved in publicly funded clinical trials/ qualitative studies of similar technologies in hypertension
REVIEW RETURNED	31-Jul-2012

THE STUDY	This was not presented as a systematic evaluation so some of these questions are difficult to answer. I could not open the last two documents despite having updated adobe reader so cannot comment on the supplemental material
RESULTS & CONCLUSIONS	The data collection methods included questionnaires with ordinal scales and comments and discussion groups. The data are all described as qualitative, although ordinal scales are usually considered to be quantative data. The questions used were all in the same direction which may have predisposed to a positive response, but this was not addressed in the discussion. In addition, averages were quoted with no measures of spread or distribution. There was still no evidence of systematic qualitative analysis of discussion group data or other free comments – these appeared as a series of anecdotes. It would appear that this was not done
GENERAL COMMENTS	Thank you letting me see the resubmission of this paper. The authors had been asked for more methodological detail which they have provided. Unfortunately I do not think that this has been enough to convince the reader that there was a rigorous approach to the evaluation and that the results are as free of bias as possible

VERSION 2 – AUTHOR RESPONSE

Many thanks for providing us with another opportunity to further improve our paper in attempt to share this work with a broader audience. We feel this is important as it will enrich the results of our other paper, recently submitted, giving the clinical results of this service evaluation.

We completely take on board the concerns raised by the reviewer and apologise for apparent use of incorrect terms when reporting our results. Therefore, we have removed all reference to qualitative work and have ensured that all relevant elements of the STROBE checklist for cross-sectional surveys have been met. Thus you will see changes in parts of the paper not directly referred to by the reviewer.

We have added median values to each of the Likert score averages to demonstrate the spread of the data and a table detailing the number of responses for each category for the questions reported. we hope that this further describes the results in an acceptable way.

We have expanded the limitations section of the paper with regards to the way in which attitude statements were posed and also in response to fulfilling the requirements of the STROBE statements.

We hope you find these changes agreeable.