PEER REVIEW HISTORY

ARTICLE DETAILS

TITLE (PROVISIONAL)	The clinical, humanistic, and economic outcomes, including experiencing of patient safety events, associated with admitting patients to single rooms compared with shared accommodation for acute hospital admissions. A systematic review and narrative synthesis
AUTHORS	Bertuzzi, Andrea; Martin, Alison; Clarke, Nicola; Springate, Cassandra; Ashton, Rachel; Smith, Wayne; Orlowski, Andi; McPherson, Duncan

VERSION 1 – REVIEW

REVIEWER	Stephen Senn The University of Sheffield, School of Health and Related Research
REVIEW RETURNED	05-Dec-2022

GENERAL COMMENTS	My main comment is that greater clarity and care is needed in the narrative describing the results. I list some concerns below in the order in which the occur to me and not in order of importance. <P> 1) It is at least conceivable that where patients shared accommodation that their outcomes are correlated in a way that they would not be in a completely randomised design comparing two pharmaceuticals. A careful statistical analysis would allow for this by allowing for a random room effect. Where there is only one subject per room such an effect would be irrelevant but for shared accommodation it would be not be. The consequence of such a random effect would be to reduce the 'effective n' in the shared accommodation arm. I found no discussion of this in your methods section but it may be that you took this into account in judging the quality of studies and their claims for statistical significance. More generally, it raises the issue as to whether you simply took any such claims at face value from the studies you reported. 2) You state that you included "comparative clinical trials, observational studies, and systematic literature reviews". This raises that possibility that studies were included twice, once as stand-alone and again in systematic reviews. (See (1) for a discussion.) Did you make sure that this did not happen? 3) On p6 you state "nine studies did not report baseline characteristics and of those that did only three reported no significant difference between age, sex, and comorbidity or health status of patients at baseline". This statement is very difficult to parse. Suppose that there were 60 studies for whom such comparisons might have been meaningful. This leaves 60-9=51 who reported such characteristics. It seems that these can be split into 48 versus 3. Now, what makes the difference between these two groups? That at least one of age, sex, and comorbidity was different for the 48 but that none were for the 3 or that at least

	one was different for the 3? In that case what was the claim about the 48? As regards these and other detail you need to be clearer. 4) On P 6 you state "The numbers of deaths were low, meaning that the studies might not have had enough patient-years of follow-up to detect small but statistically significant differences in mortality. " This is an incorrect statement. It makes statistical significance a property that exists independently of the sample size, since low sample size is to be blamed for not detecting it as small. " I presume you mean something like "the studies may have been underpowered to detect moderate effects" 5) I disagree with your reasons for not conducting formal analysis. However, I accept that there are others who would find your arguments reasonable and that there may have been practical reasons that would have made a formal analysis difficult. <P> <B>Reference</B> 1. Senn SJ. Overstating the evidence: double counting in meta-analysis and related problems. BMC Med Res Methodol. 2009;9:10.

REVIEWER	Ralf Kuhlen Helios Health , Helios Health Institute
REVIEW RETURNED	13-Dec-2022

GENERAL COMMENTS	The paper addresses the most relevant question whether single rooms versus shared accommodation for acute hospital patients provides benefit or harm. The authors applied appropriate methods of systematic review, the paper is clearly structured and well written and limitations of the study are clearly addressed by acknowledging that the underlying publications were methodologically not suitable for stronger evidence generation by i.e a meta-analysis. The review suggests that only minimal differences for any of the mentioned outcomes are found for routine care, whereas a tendency towards better outcomes was found for single rooms in intensive care. The review adds a systematic overview to our knowledge of highly debated topic and therefore, I appreciate the data based approach to generate systematic evidence from the existing publications on the controversy. I have only one suggestions for the authors to consider for the analysis of the underlying papers as well as for the discussion: There was a weak signal for intensive care in favor of single room accommodation. Are we sure that this is the effect of the accommodation per se, or could it be that the underlying severity of disease might exert a bias on this observation. The nursing efforts to take care of single rooms are without any question higher in single rooms as compared to shared accommodation. In my experience from many intensive care units, this leads to the policy that more severe patients are accommodate near to the central nursing stations and preferably in larger rooms, where continuous monitoring is easier to facilitate. This might result into the bias that less severely affected patients are more often accommodated to single rooms. The tendency towards better outcomes could therefore result from the underlying severity of disease itself…. I do suggest to re-analyse data from the underlying publication with regard to that point and if not feasible to at least address the potential confounder in the discussion.

REVIEWER	Ilaria Marcomini University of Milan
REVIEW RETURNED	15-Dec-2022

GENERAL COMMENTS	Thank you to the authors for taking on the challenging task of doing this review. The manuscript is well-written in many sections. The abstract is a good reflection of the manuscript, but it could be strengthened with some adjustments; Including the implications for future research would make the abstract more informative. Introduction: There is room for improvement. The current state of knowledge and its uncertainties on the topic should be better described and articulated; there would be more current references to be cited. Method: This section is well described. The authors specify study characteristics used to decide whether a study was eligible for inclusion in the review. There is a full report on the search strategy and the selection process. The authors Listed and defined the outcome domains. Results This section is well-reported. The authors well-reported and summarized the number of records identified and each outcome. Discussion There is room for improvement. The authors did not provide a general interpretation of the results in the context of other evidence; previous literature was not cited. In addition, they did not explicit recommendations for future research.

VERSION 1 – AUTHOR RESPONSE

We thank Professor Senn for his comments and in response to each point:

1. We agree that outcomes of patients in a shared room might not be independent of each other, and that the analysis he suggests would be helpful to elucidate this. However, we did not undertake a meta-analysis and the underlying studies do not report the data that would be necessary to do this analysis. We cover this further in point 5. We have added his point to the list of limitations of both our study and the underlying data, and in our recommendations for future research suggest that studies with stronger methods that would allow for this analysis would be useful.
2. We did make sure we did not double count studies in systematic reviews and have clarified the methods section to make this clearer.
3. We have rewritten the section on page 6 to make it easier to parse.
4. We have altered to wording in the mortality subsection of the results section to be more accurate with our language.
5. There are reasons that formal meta-analysis was not undertaken, which Professor Senn acknowledges. We have added a paragraph in the discussion to expand on these. We note that of the ten systematic reviews we reviewed, only one undertook meta-analysis. Further we agree this is a significant limitation of the field, and have added to our suggestions for future research that primary studies should use stronger methods that would eventually allow meta-analysis.

We thank Professor Kuhlen for his comments and respond to his points:

. We note that he is generally supportive of our methods and reporting so will not expand on that directly. To his one suggestion for consideration:

· We agree it is common practice to locate more sick patients closer to the nurses station, however we do not think this practice is limited to intensive care wards, rather it is common in all wards, so do not believe this practice would explain why we found a weak effect only for intensive care wards. However, it is certainly possible.

· We agree that if the proximity to the nurses station was correlated with the type of room, e.g. more shared rooms closer to the nursing station, then proximity would be a significant bias in our analysis.

· We have already covered the quality of reporting of baseline comorbidity between groups and why we cannot do a more detailed formal analysis of that.

· We looked at the studies to check whether we could find useful analysis of proximity to the nurses station. None of the studies reported all three of comorbidity, room type and proximity to nurses station. Three studies did include some data on proximity to nursing stations. All three were before-after designs where proportion of single rooms, proximity to nursing station and several other factors all changed at the same time. One was in a neonatal intensive care (Harris et al, 2006), one a cardiology unit with intensive, acute and progressive care all together (Real et al, 2018) and one a stroke unit with acute and rehabilitation sub-units (Rosbergen et al, 2020).
· Overall, we have not added this to our current, already very large review. We have noted it as another potential confounder and another area for future research.

We thank Doctor Marcomini for her comments. Our repsonse to each point is:
· We have restructured the abstract according to the editors requirements
· We have revised the introduction to set the scene more clearly and have added some relevant recent references.
· We have expanded on the opportunities for future research in the discussion.
· We have added a paragraph to the discussion focussing on the previous literature, in particular the previous systematic reviews that we found.

VERSION 2 – REVIEW

REVIEWER	Ralf Kuhlen Helios Health , Helios Health Institute
REVIEW RETURNED	20-Feb-2023

GENERAL COMMENTS	I thank the authors for carefully address all points raised by the reviewers.