

## PEER REVIEW HISTORY

BMJ Open publishes all reviews undertaken for accepted manuscripts. Reviewers are asked to complete a checklist review form (<http://bmjopen.bmj.com/site/about/resources/checklist.pdf>) and are provided with free text boxes to elaborate on their assessment. These free text comments are reproduced below.

### ARTICLE DETAILS

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| <b>TITLE (PROVISIONAL)</b> | Emergency medicine physician supervision and morality among patients receiving care from non-physician clinicians in a task-sharing model of emergency care in rural Uganda: a retrospective analysis of a single-centre training programme |
| <b>AUTHORS</b>             | Rice, Brian; Pickering, Ashley; Laurence, Colleen; Kizito, Prisca; Leff, Rebecca; Kisingiri, Steven; Ndyamwijuka, Charles; Nakato, Serena; Adriko, Lema; Bisanzo, Mark; Investigators, Global Emergency Care Collaborative                  |

### VERSION 1 – REVIEW

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| <b>REVIEWER</b>        | Chen, Tse-Hao<br>Mackay Memorial Hospital |
| <b>REVIEW RETURNED</b> | 04-Feb-2022                               |

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| <b>GENERAL COMMENTS</b> | <p>This article described the difference of mortality outcomes in emergency care Task-sharing in rural Uganda. However, some points could be clarified as below. I encourage the author to modify the manuscript to reach the standard of this journal.</p> <p># Although this study collected a relative large cohort, the assumption of normal vital signs fill in for the missing data had made this study unreliable. An analysis excluding pediatric patients may be conducted since the majority of missing data were pediatric patients.</p> <p># Analysis of severity was examined by the number of abnormal vital signs. However, this method may inherited massive heterogeneity since different disease may composed of completely different presentation. Ex: Trauma patient with hypotension and tachycardia may represent a severe hypovolemic shock with only two vital signs abnormality. A high mortality rate in trauma patient with hypovolemic shock in rural area is inevitable since prehospital time were long. While patient suffered from septic shock could present with 4 vital signs such as high body temperature, low blood pressure, tachycardia and tachypnea. The administration of antibiotics may successfully reduce mortality of sepsis patients. Comparison of disease severity could be switch to ICD-9 / ICD-10 for a better insight of severity comparison.</p> <p># Please explained why data missing occurred in 10 months of the 2009, and 3 months of the 2010.</p> <p># A paired matching comparison of the high risk mortality patient in two groups: supervision (direct and consultation) group and independent group may be more suitable to the conclusion point 4.</p> |
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| <b>REVIEWER</b>        | McGee, Blake<br>Georgia State University |
| <b>REVIEW RETURNED</b> | 10-Feb-2022                              |

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| <b>GENERAL COMMENTS</b> | <p>Thank you for this informative and well written manuscript. It should be an important contribution to the evidence base on task-sharing in sub-Saharan Africa, especially with respect to emergency care.</p> <p>I appreciate the attempts to address the underlying changes in health indicators in the Ugandan population generally during the study period, as well as the role of patient acuity in the mortality effects of physician supervision. I have two overarching concerns that could be addressed with revisions:</p> <p>1) Throughout the manuscript, the assumption is that the results show a mortality benefit for high-acuity patients from direct physician supervision. However, the physician described in the study site description section was a US-trained emergency medicine physician. It seems like testing direct supervision from Uganda-trained physicians should be recommended to see whether the effects observed were attributable to supervision from a clinician with an MD, with US training, or with emergency medicine credentials.</p> <p>2) Because all eligible records were used and no power analysis was done, I'm a bit concerned about an over-reliance on arbitrary p-value thresholds to determine whether an effect exists. I would at least address the trend in magnitude and direction of the odds ratio estimates for the 0-1 and 2 abnormal vital signs groups (especially for Independent vs. Direct).</p> <p>Minor comments:</p> <ul style="list-style-type: none"> <li>- I don't love the term "narrowly" trained to refer to nurses in the Abstract and Introduction.</li> <li>- Kindly remember to spell out abbreviations before using them (NCP) and to place the abbreviation in parenthesis next to the full term at first mention (LMICs, SSA).</li> <li>- First paragraph -- why are there two estimates (45 and 90) for years of training needed?</li> <li>- The order in the 3rd paragraph of the Intro. is a bit confusing. It seems like the documentation of the "few short courses" for teaching NPCs in SSA should be mentioned earlier.</li> <li>- There's redundant language at the end of the 1st paragraph of the Methods section.</li> <li>- Under Data Collection or Limitations, can you quantify the scope of the issue of patients not having a phone?</li> <li>- An explanation of how these 11 variables were selected for the final model (2nd paragraph of Data Analysis) would be valuable.</li> <li>- A quick, parenthetical explanation of why 3 months of data were missing for 2010 is warranted.</li> <li>- I would add the % to the number of patients meeting inclusion criteria (1st sentence of Results).</li> <li>- Sentence with reference to Figure 3: cut "was"</li> <li>- Thank you for performing diagnostics on your logistic regression model.</li> <li>- At the end of the 2nd paragraph of the Discussion, isn't it also possible that vital signs were preferentially taken on sicker patients when the rate of complete vitals was lower early in the study period? That could also explain why vitals appeared to improve.</li> <li>- 3rd paragraph of Discussion -- I think you mean to refer to acuity</li> </ul> |
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|  | <p>groups rather than mortality groups.</p> <ul style="list-style-type: none"> <li>- Under Limitations -- need to specify that the mortality rate of 0.08% in those with complete follow-up was among discharged patients (if I understand correctly).</li> <li>- I recommend adding something like "...for the vast majority of patient encounters" to the end of the 1st sentence of the Conclusions, for clarity.</li> <li>- References with organization authors (e.g., WHO) will need to be reformatted.</li> <li>- Figure 1 would benefit from additional clarity, especially by adding percentages to each type of exclusion and to the total visits used for analysis.</li> </ul> |
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**VERSION 1 – AUTHOR RESPONSE**

Reviewer 1

# Although this study collected a relative large cohort, the assumption of normal vital signs fill in for the missing data had made this study unreliable. An analysis excluding pediatric patients may be conducted since the majority of missing data were pediatric patients.

We deeply appreciate this feedback. While Single (Deterministic) Imputation has utility, we took this feedback to heart and reworked the entire analytic methods of our models. We have used multiple imputation models to handle missing data, and included extensive details about methods and citations to support those.

# Analysis of severity was examined by the number of abnormal vital signs.

However, this method may inherited massive heterogeneity since different disease may composed of completely different presentation. Ex: Trauma patient with hypotension and tachycardia may represent a severe hypovolemic shock with only two vital signs abnormality. A high mortality rate in trauma patient with hypovolemic shock in rural area is inevitable since prehospital time were long. While patient suffered from septic shock could present with 4 vital signs such as high body temperature, low blood pressure, tachycardia and tachypnea. The administration of antibiotics may successfully reduce mortality of sepsis patients. Comparison of disease severity could be switch to ICD-9 / ICD-10 for a better insight of severity comparison.

Thank you for your comments and we recognize that heterogeneity is a huge issue in this analysis. We have chosen this approach precisely because it DOES incorporate this heterogeneity to model the undifferentiated nature of emergency care, where any given patient may be suffering from one or multiple surgical or medical conditions. By deliberately avoiding disease codings such as ICD we are attempting to both avoid the bias introduced by “clinical diagnoses” in regions which lack definitive testing for making those diagnoses and to investigate the impact of supervision on the “undifferentiated patient” as seen early in the treatment course (e.g. at triage) versus late (e.g. discharge diagnoses). We hope the reviewers understand these deliberate choices.

# Please explained why data missing occurred in 10 months of the 2009, and 3 months of the 2010. The preliminary phase of the program (Direct Supervision) ran for five consecutive months from (Nov 2009 until Mar 2010). The second phase of the program (Indirect Supervision) did not begin for three months (July 2010) while staff was recruited, accounting for the timing gaps in question.

# A paired matching comparison of the high risk mortality patient in two groups: supervision (direct and consultation) group and independent group may be more suitable to the conclusion point 4. With the improvement to our methods based in the suggestions about single imputation above, our final model showed a benefit for both direct and indirect supervision. We have clarified language throughout including in the conclusion point 4.

Reviewer: 2

### Major Concerns

1) Throughout the manuscript, the assumption is that the results show a mortality benefit for high-acuity patients from direct physician supervision. However, the physician described in the study site description section was a US-trained emergency medicine physician. It seems like testing direct supervision from Uganda-trained physicians should be recommended to see whether the effects observed were attributable to supervision from a clinician with an MD, with US training, or with emergency medicine credentials.

We are in complete agreement with the reviewer. A multi-tiered study with Ugandan and US-trained physicians with and without emergency medicine training would be the ideal comparison. However, this paper discusses a natural experiment within a newly established emergency department that never had Ugandan physician staffing. Likewise, with the first class of Ugandan emergency medicine specialty-trained residents starting in 2017 not all of those comparison groups are yet available. The author group has a manuscript underway from a second site which had "Independent Care" Non-Physicians practicing alongside Ugandan physicians without emergency medicine credentials that we hope to publish shortly.

2) Because all eligible records were used and no power analysis was done, I'm a bit concerned about an over-reliance on arbitrary p-value thresholds to determine whether an effect exists. I would at least address the trend in magnitude and direction of the odds ratio estimates for the 0-1 and 2 abnormal vital signs groups (especially for Independent vs. Direct).

We agree and appreciate these comments. We have avoided the use of p-values in any of our discussion and with our refined model showing a much clearer effect, we hope these direct comparisons are much clearer.

### Minor comments:

- I don't love the term "narrowly" trained to refer to nurses in the Abstract and Introduction. This word has been eliminated in both cases.

- Kindly remember to spell out abbreviations before using them (NPC) and to place the abbreviation in parenthesis next to the full term at first mention (LMICs, SSA).

NPC has been removed in all cases, and parenthetical mentions corrected

- First paragraph -- why are there two estimates (45 and 90) for years of training needed? This has been clarified to be "between 45 and 90 years" based on the current estimate of 5-10 graduates produced per year.

- The order in the 3rd paragraph of the Intro. is a bit confusing. It seems like the documentation of the "few short courses" for teaching NPCs in SSA should be mentioned earlier.

Thank you for this edit and it has been changed.

- There's redundant language at the end of the 1st paragraph of the Methods section.

This has also been changed.

- Under Data Collection or Limitations, can you quantify the scope of the issue of patients not having a phone?

This has been clarified in text. "Most of this loss to follow was due to lack of phones for the discharged patients (Had no phone: 82.3%, n=6,592; Invalid number: 6.9%, n=553) with only 10.7% (n=856) being loss to follow up for other reasons."

- An explanation of how these 11 variables were selected for the final model (2nd paragraph of Data Analysis) would be valuable.

This has been added

"All variables with a univariate p-value less than 0.15 were included in the final model."

- A quick, parenthetical explanation of why 3 months of data were missing for 2010 is warranted. The preliminary phase of the program (Direct Supervision) ran for five consecutive months from (Nov 2009 until Mar 2010). The second phase of the program (Indirect Supervision) did not begin for three months (July 2010) while staff was recruited, accounting for the timing gaps in question.

- I would add the % to the number of patients meeting inclusion criteria (1st sentence of Results).

This has been included.

- Sentence with reference to Figure 3: cut "was"

Done

- Thank you for performing diagnostics on your logistic regression model.

We appreciate your shared appreciation of model diagnostics.

- At the end of the 2nd paragraph of the Discussion, isn't it also possible that vital signs were preferentially taken on sicker patients when the rate of complete vitals was lower early in the study period? That could also explain why vitals appeared to improve.

Table 1 now shows complete vitals by age group and better represents that vitals were taken at a higher rate for each age group but the preponderance of children in the early phase led to lower overall rates of complete vitals.

- 3rd paragraph of Discussion -- I think you mean to refer to acuity groups rather than mortality groups.

This has been changed.

- Under Limitations -- need to specify that the mortality rate of 0.08% in those with complete follow-up was among discharged patients (if I understand correctly).

This has been clarified.

- I recommend adding something like "...for the vast majority of patient encounters" to the end of the 1st sentence of the Conclusions, for clarity.

This has been added.

- References with organization authors (e.g., WHO) will need to be reformatted.

This is corrected

- Figure 1 would benefit from additional clarity, especially by adding percentages to each type of exclusion and to the total visits used for analysis.

Figure 1 has been extensively reworked for clarity.

## VERSION 2 – REVIEW

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| <b>REVIEWER</b> | McGee, Blake<br>Georgia State University |
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| <b>REVIEW RETURNED</b>  | 02-Jun-2022   |
| <b>GENERAL COMMENTS</b> | Great work on the revisions. I suggest adding a sentence to the second-to-last paragraph of the Introduction section that explains why these trends matter to the study at hand -- something simple like the need to account for the changing baseline health of the Ugandan population given the timeframe of the study. Otherwise, well done. |

### VERSION 2 – AUTHOR RESPONSE

| Reviewer #2   | Response to Reviewer   |
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| Great work on the revisions.  | Thank you so much for all your editorial input and we feel the manuscript is much stronger thanks to this feedback.  |
| I suggest adding a sentence to the second-to-last paragraph of the Introduction section that explains why these trends matter to the study at hand -- something simple like the need to account for the changing baseline health of the Ugandan population given the timeframe of the study. Otherwise, well done.  | This has been added, “Any longitudinal evaluation of mortality occurring during this time period therefore needs to take into account this changing baseline.”   |
| You seem to have used an incorrect version of your manuscript when creating the revised version, as the 'Strengths and limitations of this study' section was missing and a separate panel (SUMMARY BOX) was present which is not part of the BMJ Open article format. Please delete the 'SUMMARY BOX' section and replace it with the 'Strengths and limitations of this study' section from the original submitted version. Please also check for any other inconsistencies that may have arisen due to this version error. | We appreciate you catching this error. We have replaced it with the “Strengths and limitations of this study” section. The draft has been checked over for any other version control problems and none found.                    |
| Please revise the title of your manuscript to include the research question, study design and setting. This is the preferred format of the journal. Eg, “Emergency physician supervision of non-physician clinicians and mortality outcomes in the context of task-sharing for emergency care in rural Uganda: a retrospective analysis of single-centre training programme” (or similar).  | This has been changed to “Mortality impact of emergency medicine physician supervision on non-physician clinician task-sharing for emergency care in rural Uganda: retrospective analysis of a single-centre training programme” |
| Please revise the abstract to ensure that it is formatted according to our Instructions for Authors ( <a href="http://bmjopen.bmj.com/pages/authors/#research">http://bmjopen.bmj.com/pages/authors/#research</a> ), including all relevant subheadings and required details.   | The abstract has been reformatted to fit formattin   |