Author's response to reviews

Title: The implementation of the Medical Regulation Office and Mobile Emergency Attendance System and its impact on the gravity profile of non-traumatic afflictions treated in a University Hospital: a research study

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Author's response to reviews: see over
REVISIONS PROVIDED

FIRST REVIEWER: LARS LIND

Major Compulsory Revisions

1. SAMU is not known to everybody. It cannot therefore be used in isolation in the title, abstract etc. This has to be explained.
   ➔ This was corrected in the title and in the text.

2. You do not have to validate the APACHE II score. This has been done several times in the past and could be removed.
   ➔ The authors believe that the comparisons done between the two periods using the Roc Curve analysis is important to the paper, so the method of comparison was sustained but using more appropriated terms.

3. Please define comobidity and in-hopital complications
   ➔ This was defined in the text.

4. It is not given somewhere if the total number of subjects differed between the two periods.
   ➔ this was corrected in the “Method” section.

5. I think the analysis presented in table 4 does not add anything to the main message of the study. Please delete.
   ➔ The table was deleted.

6. In general, the manuscript is very talkative. Please try to condence especially the background and discussion. The message is clear that the introduction of SAMU improved the flow to the ED. This massage will come through more clearly if you cut down the background and discussion by 50% and concentrate on the main message.
   ➔ This was done: the text was condensed and the reading was made more agreeable.

Minor Essential Revisions (such as missing labels on figures, or the wrong use of a term, which the author can be trusted to correct)
➔ This was reviewed and corrected in the whole text.
SECOND REVIEWER: BEKELE AFESSA

Major Compulsory Revisions:

1. The study compares two months, five year apart. Several demographic and health care delivery changes as well as advances in medical diagnostics and treatment may have occurred during this five year interval. These changes and advances may play confounding roles. I request that the authors address the issue that these confounding factors may have influenced their findings.

   This issue is now answered in the “Discussion” section.

2. SAMU was reorganized in January 2000. Considering the lack of inexperience during the first year, the post-SAMU period should include experience of 5 years. Limiting the study to just one month weakens the strength of the study.

   The authors agree with the reviewer but the post-SAMU period of one month, one year after the medical regulation office being fully functional reflect the real experience of the whole group of professionals and is not different from the results of the last two year.

3. The authors excluded patients whose length of hospital stay was less than 12 hours. Was there a difference in the proportion of admitted patients who were discharged in less than 12 hours between the two periods? This information may have some importance for health care delivery.

   This was not addressed in the study because the patients that stayed less than 12 hours was not considered, using the hospital principles, as being hospitalized, mainly because the majority of these patients had low gravity afflictions that could be solved in out-of-hospital clinics. Curiously, the Apache II scores for these patients were essential the same for the two periods of study and the results of the whole populations were not essentially different when they were included.

4. Define co-morbidities in the methods section.

   This was done.

5. Define complications in the methods section.

   This was done.

6. Describe more information about the hospital setting. How many beds does each department have? How many and what type of intensive care units does the hospital have? How many total ICU beds does the hospital have?

   This was done in the “Background” section.

7. On page 5, the last sentence of the fourth paragraph, the authors cited references 18 to 25 to justify their use of APACHE II in their study. Several of these references do not use APACHE II. More over, APACHE II is usually used in patients admitted to the ICU.

   This was corrected and new references was added.
8. The authors stated the area under the ROC of the APACHE II in their study population. I suggest they provide information about the calibration of APACHE II.

Respecting the suggestion of another reviewer, we suppressed calibration of APACHE II because it was exhaustively tested and applied. But we made comparisons using the ROC curve, pairing the calculated and observed mortality for the two populations.

9. Table 4 does not deliver the information clearly. I believe the authors are telling the reader that there were statistically significant differences in the APACHE II predicted mortality rate between survivors and non-survivors in all groups during both study periods. I suggest stating this fact in the text and deleting Table 4. If the authors insist in keeping the table, it needs to be redone.

The table was deleted.

10. The authors need to tell us whether there were statistically significant differences in severity adjusted mortality rates between the two periods. This can be done by using multiple logistic regression analyses or calculating the standardized mortality ratio with its 95% confidence intervals.

This could not be done because the collected data do not allow us to do multiple logistic regressions because this was not the original idea of the study. We sorry for this inconvenience.

11. The discussion section provides an excellent description of SAMU. However, most of it has no relationship to the current study.

This was corrected.

12. The discussion should include a section to address the potential limitations (weaknesses) of the study.

Some new discussions were introduced in the respective section addressing the limitations of the study.

Minor Essential Revisions

1. Spell SAMU or replace it with descriptive words in the title. As written, most readers (including this reviewer) will not understand the title.

This was corrected in the whole paper.

2. The study included patients older than 13 years. Why was 13 selected to define adulthood?

This issue is now addressed in “Methods” section of the paper.

3. On page 5, second paragraph, change “discharge or death” to “alive or dead”.

This was done.

4. The authors utilized 3 softwares for statistical analyses. SPSS alone would have been adequate to perform all analyses.
The SPSS was introduced later in the analysis of Roc Curve because this was suggested by a local advisor. The other statistics had already been done using other softwares.

5. Add the versions to the statistical softwares.
   ➔ They were added.

6. Use one decimal place for the percentages including the tables (E.g. 83.9 instead of 83.89).
   ➔ This was done.

7. In some of the tables, the authors used 2, 3 and 4 decimal places for the p values. I suggest being consistent.
   ➔ It’s difficult to attend this suggestion because some p values are too small and significant to be represented by two decimal places. Other values can be represented by only 2 decimal places.

8. Change mortality “coefficient” to mortality “rate”.
   ➔ This was done.

9. Change the title of Table 3 to “Differences in APACHE II mortality risk between the two study periods”.
   ➔ This was done.

10. Replace “(x ± s)” with “, mean ± SD, %” in Table 3.
    ➔ This was done.

Thank you.
The authors.