Reviewer's report

Title: Ward staff education on severe sepsis/septic shock and mortality of ward patients: an original hypothesis.

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Reviewer: Flavia Machado

Reviewer's report:

This is an interesting paper describing an education process in six Italian hospitals aiming to improve care in sepsis patients. Training leading to mortality reduction is not a new concept in sepsis or in other fields. Although there are already many papers in the literature devoted to describe the results of sepsis protocol implementation, this study has an interesting and original approach. The originality is the concept that training in sepsis would lead to a reduction in all-cause and all-patients mortality, regardless of the presence of sepsis. However, although the hypothesis is quite interesting, I have serious concerns about the methods used to address it in this study.

Major compulsory revisions

1. The major issue in my opinion is derived from the hypothesis of the study. Is the assessment of all-causes hospital mortality in a general population without sepsis a reasonable outcome for a before-after study which intervention was sepsis education? It is tempting to believe in this hypothesis as sepsis protocols are very wide in their nature, meaning that its implementation would generate a quality improvement in many aspects not only related to sepsis care. However, I am not sure we can attribute only to the intervention the reduction in hospital mortality found here using this design. We would need a better definition of before and after periods (see comments bellow) and a better description of the population in these periods to assure severity of disease and diagnosis categories did not vary during study period. The authors had access to hospitals register to generate mortality rates. Was this the only variable assessed? There are many known mortality risks factors that were not measured or at least not mentioned. Without knowing that population were similar, how can the mortality reduction be attributed to education? Even the four variables used to adjust mortality analysis are not shown. So, I would suggest the authors to get as much as information as possible to better characterize their population across the years.

2. Although for the purpose of the study sepsis incidence is not an absolute required data, it would be nice to have it along the years, as well. Moreover, we need some type of performance measurement to indicate training really results in change in sepsis bundles or in any other quality indicators.

3. Another major issue is the absence of data in hospital mortality, as well as
sepsis mortality, across the study period. Hospital mortality and sepsis mortality are decreasing over the years. The mortality rates in each year needs to be shown to assure the reduction was present only after intervention and not progressively during the precedent years.

4. The intervention is not well characterized. Why authors state that reaching only 30% of training in two years is adequate? It seems to me a low rate of training in a quite long period of time. If training was initiated in October 2007 and only 25% of people was trained up to December 2008, what is the possible explanation for the reduction in mortality already found in this first year? Do you think that training only one quarter of your goal would really result in such a impact? And training a 10% more would increase it further? These results would be really impressive, thus it urges to better describe what you really have done.

Minor essential revisions

Title

1. It seems inadequate as authors were not only talking about ward patients. Emergency rooms and ICU were among the nine settings included in the study.

Abstract

The abstract is quite confusing.

2. The method section should state clearly what was the study period, how pre intervention data was collected. First and second education period (as described in methods) means the same as first and second year after education (as described in results)? The expression "end of the study period" is used in the abstracts results. What does it mean?

3. Some sentences looks out of the scope like "For most of the cumulative hospital mortality, irrespective of the presence of severe sepsis/septic shock" because we only get the aim of the study (the impact in mortality regardless the presence of sepsis) latter on in the discussion section.

4. I would not agree that with the conclusion where two years of training is considered as a "short period". I think for training it is quite long.

5. The authors can't conclude that effect is obtained in a short period if education is focused in the wards, as they do have trained ER and ICU teams.

Methods

6. The participating hospitals could have a better description, with the exact number of beds, staff and ICU beds. Who is in charge during the night in the wards? Only the daily staff is described.

7. As already mentioned, the timetable for the study is rather confusing. How the time for collection was defined? If intervention started on the last quarter of 2007, why should you collect data since 2003? For the mortality assessment, why the first period is a 11 months and the second on only 8? And why they were compared with a four year period pre-intervention and not just one or two years
before? It is well known that mortality is decreasing over time in this last decade in many places around the world. If you take into account such a long period of time, this would compromise your ability to associate the reduction in mortality with your intervention. In the data source of general hospital mortality section times are October 2003 to October 2009, in the statistics section it is written December 2003 to August 2009. Please, be consistent.

8. The statistics needs more clarity.

Results

9. As already state, data is not available. Authors should state clearly how many patients were analyzed in each period, type of admission, diagnosis, age, gender, LOS, Charlson score, mortality rates and any other information they have to characterize the two (or three) population: pre intervention, pos intervention first period, pos intervention second period.

10. How did the authors measure the percentage of hospital training? It is not clear in the methods section.

11. I did not get the meaning of "training was homogeneous in all the study hospitals". What did you mean? The percentage of trained healthcare workers was similar? The training was completed in the same time?

Discussion

12. I don't think authors can state that the mortality significantly reduced when the percentage of educated staff increased from 25 to 30%. This sounds quite arbitrary, as 30% is not a recognized goal to be reached. In the second paragraph they also state that this percentage is "quite good". I would suggest authors to be more technical in their statement. What is considered in the literature a goal to be reached? I miss references here.

13. 2nd paragraph - The statement that the percentage of treatment is underestimated for the wards included in this analysis needs better supporting data. To calculate the total percentage certainly the authors have the exactly percentage of treatment in each hospital, setting and also the total numbers of healthcare providers. Thus, it would be easy to measure the real percentage of treatment in the selected settings.

14. 3rd paragraph - The influenza epidemic is not the only potential cause of case mix in this study. As already mentioned, the authors should state clearly which were the main causes of hospital admission during the study period.

15. Limitation should be better described, containing many of the aspects mentioned above.

Conclusion

16. The second paragraph is inadequate and should be removed.

Discretionary Revisions

1. CAPS are used inadequately. Examples: abstract Regional, Relative Risk

2. This text would benefit from a native English-spoken reviewer.
Level of interest: An article of importance in its field

Quality of written English: Acceptable

Statistical review: Yes, but I do not feel adequately qualified to assess the statistics.

Declaration of competing interests:

I declare that I have no competing interests.