Reviewer's report

Title: High inter-observer agreement of observer-perceived pain assessment in the emergency department

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Reviewer: Yves Chaput

Reviewer's report:

Thank you for giving me the opportunity to review this manuscript. The topic itself appears to be well within the scope that BMC Emergency Medicine typically covers.

I have read the manuscript and have a few major queries that I would like to submit to the authors. Major points are those I would like the authors to respond to (in changes to the manuscript if in agreement with the comment and if not, then by a counter argument in their response letter). Minor points are non essential queries that I leave up to the authors to address (or not) without the need to make any further comment in a return letter.

My 1st comment is that the writing style should be reviewed. I would strongly suggest that the authors have their manuscript read by a native English (UK or American) colleague for some basic corrections. The problem here is that the writing itself makes it difficult to assess the message the authors are trying to convey. One often has to read a phrase several times over in order to fully understand it.

My 2nd major comment pertains to being more careful about the use of the cited material. A case in point is one of the premises of this study. In the Background section, second paragraph first line the authors state that "Pain is the most common complaint in an ED, citing reference [2¬]. The reference cited does not support this contention. In fact, the reference cited is a study that characterizes the distribution of self-reported pain scores among common ED diagnoses. It did not assess prevalence. Other authors have studies prevalence though, such as Cordell et al., Am J Emerg Med 20, 2002, Johnston et al., J. Emerg Med 16, 1998 and Tanabe et al., J Emerg Nurs 25, 1999. As such, if the authors are citing "prevalence" then they should use references where prevalence was assessed.

My 3rd major comment pertains to the second stated objective of this study, which does not appear to be either clear or well set up in the Background section. I am referring to the passage in the Background stating that the authors will assess whether "certain patient groups had higher agreement rates than others". This is a rather vague hypothesis.
One assumes that the authors are referring to the brief discussion of the "various factors such as age and gender and previous pain experience" mentioned in the second paragraph of the Background. I would however more fully develop the hypothesis as to what variables or parameters the authors are specifically looking for.

As a corollary to the above point, in the Methods section some of the variables could be fleshed out a bit, such as the location variable (not quite certain what the authors consider "wardroom"), why specialty was limited to surgery and not internal medicine…

My 4th major concern is about the methodology (which is not quite clear) used and this may have some bearing on the statistics in this study. If only 2 individual nurses were involved and paired throughout the study (Dick and Jane for instance) then the statistical procedures appear to be appropriate. However, if multiple nurse pairs were used (Dick and Jane, Dick and Harry, Jane and Nancy, Bob and Charles … ) then the authors would have to detail that in the results section and honestly, how this would affect the statistics I would be hard pressed to say (as I am not a statistician). Perhaps the Fleiss kappa would be more appropriate under such circumstances?. The authors should review this with their statistician.

My 5th major point concerns the Discussion. First, this is a statistical study so what do the authors think of a 0.55 kappa. What does that imply and how should it be interpreted. All the more so as the authors state that all of the elements for a strong Kappa were in place in this ED (highly trained nurses with a lot of experience in rating scale use). % agreement is fine but the "raison d'être" of Kappa is to eliminate chance within this % agreement.

A 1st minor point is in the Discussion section. In the first paragraph they state that the pain assessment was "independent of where the assessment took place"… While technically correct the phrase is rather all encompassing and in actual fact, the authors only assessed 2 particular "places". I would probably rephrase it in a more specific manner i.e., "was independent of whether the assessment took place in the emergency room or in the … "

A 2nd minor point is I'm not quite certain what the purpose of the second paragraph of the discussion section is. It seems to just hang there. The authors present no patient rated data here so as to compare with the nursing data. They neither present any data concerning nurse's education or personal factors or patient's stage of illness. The discrepancy in nurse ratings found in this manuscript is not really the object of any discussion either.
Are the methods appropriate and well described?
If not, please specify what is required in your comments to the authors.

Yes

Does the work include the necessary controls?
If not, please specify which controls are required in your comments to the authors.

Yes

Are the conclusions drawn adequately supported by the data shown?
If not, please explain in your comments to the authors.

Yes

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