Author's response to reviews

Title: The Casualty Chain Inventory: A new scale for Measuring Peritraumatic Responses: a cross-sectional study

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Author's response to reviews: see over
To
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The Casualty Chain Inventory: A new scale for Measuring Peritraumatic
Responses: a cross-sectional study

Date: 08.03.2011

Dear Editor,

Thank you very much for your letter with extensive comments from two expert reviewers. According to your
suggestion, we have revised the manuscript, and hereby submit the paper with the changes. We have
commented on all points raised by the reviewers, and think that this has improved the quality of the paper. The
changes are highlighted in the comments and in the manuscript.

Yours Sincerely;
Laila Skogstad

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Reviewer's report
Title: The Casualty Chain Inventory: A new scale for Measuring Peritraumatic Responses: a cross-sectional study
Version: 1 Date: 7 January 2011
Reviewer: Benjamin Vicente

Reviewer's report:
It is a reasonably well written manuscript on a subject mostly relevant to those with closely related research interest. Despite the above some of its limitations make it not suitable for publication in its present form.

Review no 1.
Point 1.
The inventory proposed (CCI) was developed based on some of the items of the IES and the PTSS-10 therefore the validation using as gold standard the same instruments from where it was derived seems not strong enough.

Answer point 1.
The items representing dissociation in the CCI are based on two questions from IES. None of the items in PTSS-10 are used in the CCI. We chose to analyse the correlations between the CCI and PTSS-10 to compare the relationship with both IES and PTSS-10 and to check if there was a stronger correlation between the CCI and IES compared to the CCI and PTSS-10. There was lower correlation between the CCI and PTSS-10 compared with IES, but the correlations between the CCI and IES were never higher than .54 (perception in hospital). Based on this finding it is appropriate to use the CCI and IES in the same analysis. During the validation, the CCI was compared with IES, both with and without the two common items. This point has been outlined in the revised manuscript, (page 10): “The CCI was compared with the IES both with and without the dissociation items, showing no significantly different correlations”.

Point 2.
Furthemore having applied all the measures together at the same time and sharing items surely influences patients response. This could be solved or clarify using the 3 and 12 month data as a double check which is encouraged and its inclusion in the paper desirable.

Answer point 2.
We have compared the CCI mean scores at scene of injury and in hospital with IES at three and 12 months, using the patients who had answered at all measurement points (n= 181). For the scene of injury, the correlations at baseline were .54, at 3 months .50 and 12 months .41. The corresponding figures in hospital were .56, .56 and .48 respectively. This strengthens the reliability of the CCI. However, we prefer not to present these data in the revised manuscript, as we then would include follow-up data, which are outside the main focus of the present study.

Point 3.
Last but not least, the timminig in which the instrument where used (weeks after the accident) pose some doubts regarding the CCI ability to identify patients at risk, which suggest or involve the ability to predict whose patient will develop postrautematic stress simtoms.
Answer point 3.
The time at which stress reactions should be measured is an important question. One may argue that it should preferably be in the acute state, e.g. at the time and place of injury or shortly after. Such measures, however, raise both practical and ethical questions. Assessments at a later stage may be associated with recall bias. The average level of symptoms goes down after trauma. A recall bias is most likely influenced by the state at the time of the measurement. Accordingly, having the measurements few weeks after the accident most likely underscores the symptoms in the acute phase. In line with this, we consider that the usefulness of the CCI most likely had been even greater if it had been completed during the acute phase. We have added two sentences in the revised manuscript (page 13): “The time of assessment (weeks after the accident) raise questions regarding the CCI’s ability to identify patients at risk. Even though a recall bias may be present, those with symptoms after some weeks are most likely at greater risk for developing symptoms also at a later stage.”

The CCI was only answered at baseline, but there were no statistically significant differences in posttraumatic stress scores (IES) between those who answered within 4 weeks compared to those answering within 5 – 8 weeks or those answering between 9 – 12 weeks or even after 13 weeks. The CCI was a significant predictor of posttraumatic stress (measured by IES) also at 12 months.

Level of interest: An article whose findings are important to those with closely related research interests
Quality of written English: Needs some language corrections before being published
Statistical review: No, the manuscript does not need to be seen by a statistician.
Declaration of competing interests: I declare that I have no competing interests

This manuscript has been proof read by OnLine English - Superior English editing Division of PressCorps Pty Ltd (ABN 90097684636) www.oleng.com.au. Moreover, a native English-speaking colleague has revised the manuscript, and also the changes that have been made.
Reviewer's report
Title: The Casualty Chain Inventory: A new scale for Measuring Peritraumatic Responses: a cross-sectional study
Version: 1 Date: 1 February 2011
Reviewer: Eric Bui

Reviewer's report:
This is a very interesting paper on the development of a new scale measuring peritraumatic responses.

Review no 2

Major Compulsory Revisions

Point 1
My main concern is about the lack of discussion of the rationale/findings with regards to the peritraumatic distress inventory which also includes one factor with “threat” and “physical reactions” factor.

Answer point 1
We wanted to study the relationship between perceived threat, physical reactions and sensory perception and to see whether they loaded on the same or on different factors.

See also comments to point 2.

Point 2. Background
The authors have well described the relationship between the peritraumatic dissociation and subsequent PTSD but the relationship between PTSD and peritraumatic distress is not clearly described. More generally, the authors should more clearly explain the contribution of this new scale with regards with existing measures of peritraumatic responses. Is the main contribution of this is new instrument the assessment of peritraumatic sensory perception (as the peritraumatic distress inventory already assesses perceived threat and the PDEQ, peritraumatic dissociation)?

Answer point 2,
The relative importance of cognitive, dissociative and sensory perception as predictors or posttraumatic stress symptoms and PTSD is not fully understood. Peritraumatic distress (Brunet A. et al.,) connected to the A2 criterion (fear, helplessness or horror), and dissociation are peritraumatic reactions predicting PTS(D), but sensory perception may also explain PTS(D) The main contribution of the new instrument is the assessment of peritraumatic sensory perception. The factor analysis showed that dissociation was a separate factor, although rather highly correlated with the perception factor (at scene of injury .42 and in hospital .46). In our study the level of fear however, loaded on the same factor as the perception items. It remains to be seen whether perception will be a stronger predictor for later PTS.

Accordingly, the main contributions of the new instrument are
a) That it has the ability to measure peritraumatic sensory perception and
b) Has the ability to compare the relative contribution of the sensory perception and items representing fear, dissociation and lack of autonomy (feeling stuck) for subsequent posttraumatic symptoms. (page 4)

In future studies, we would be interested in whether the sensory perception may be a stronger predictor for PTS than more cognitive reactions like fear. Treatment may be more difficult for bodily and sensory perception, which largely involves the autonomic nervous system, than for threats that are more cognitive (e.g. fear of driving a car after an accident). These points (1 and 2) are added in the revised manuscript (page4).

Point 3. Background
Furthermore, I’m not sure to understand if the authors imply that pain may be a peritraumatic reaction in the absence of physical trauma, or if an increase of peritraumatic pain (in presence of physical trauma) may be a predictor of subsequent PTSD symptoms (if the latter, the authors may wish to cite prior research on the subject (e.g. Boudou et al, 2007).

Answer point 3
As pointed out by the reviewer, pain is of great significance as a predictor of PTS, and we have added the suggested reference in the revised manuscript. Even though physical damage or childbirth can cause pain (Boudou et al, 2007), the perception of pain is psychological (can be blocked by general anaesthesia). The level of pain is also influenced by psychological factors. E.g. during childbirth, the pain may be reinforced by fantasies of complications for the child or the mother. In addition, psychological distress like intense fear or depressive despair may also be painful. We have added the following sentences in the revised manuscript (page 3): “Even though pain is a significant risk factor in physical trauma, the level of pain is also influenced by psychological factors like the level of fear or autonomy. In addition, intense psychological despair (anxiety or depression) may also be painful even in the absence of physical injury”.

Minor Essential Revisions
Point 4. General Comments:
The phrasing is at times clumsy and the authors may wish to have the manuscript proof read by a native English speaking person. (It seems for example that the authors used the terms psychological distress and PTSD on the same level).

Answer point 4
This manuscript has been proof read by OnLine English - Superior English editing Division of PressCorps Pty Ltd (ABN 90097684636) www.oleng.com.au. This is a recommended Australian company with both scientist and language-educated personnel. Moreover, a native English speaking colleague has revised the manuscript, including the changes that have been made.

We are using the term posttraumatic stress (PTS) symptoms, not the diagnosis PTSD. PTS or PTSD is related to a traumatic event, but anxiety and depression may also develop after a traumatic event. We consider these reactions as psychological distress after a traumatic event. We have definitely not used PTS and PTSD on the same level. We have considered (in another paper) that a PTS level of IES>35 may be associated with PTSD, but other criteria have to be present as well to make a formal
diagnosis of PTSD. We have added one sentence about this in the revised manuscript (page 3): “In order to get a diagnosis of Post traumatic stress disorder (PTSD) a high level of post traumatic stress symptoms (PTS) must be present together with other diagnostic criteria.”

**Point 5. Background**
The authors may want to clearly spell out why it is clinically interesting to investigate the differences in peritraumatic responses between two time points.

**Answer point 5**
It is of clinical interest to explore if there are differences in mean scores at the two measurements points; e.g. if there are more symptoms at scene of injury compared to in hospital or vice versa, or if there are more patients with symptoms at one of the measurement points. The duration of perceived threat is of importance regarding the risk for subsequent symptoms. Some threats are of too short duration to cause later distress (e.g. having narrowly avoided a serious car accident), whereas longer duration is a risk factor.” By measuring perceived threat at two time points, it is possible to see whether e.g. those with a high level of perceived threat both at the scene of injury and in hospital are at greater risk than those who are equally afraid in one of these situation”. Sentences are added into the revised manuscript (page 4).

**Point 6.**
I believe examining the correlations between the CCI score and the IES-R and PTSS-10 is part of the validation of the scale (convergent validity). If not, (as the authors wrote in the discussion section), then, in order to strenghten the paper, the authors might wish to clarify why it is important to examine this association. Specifically, they may want to say that they aim to examine the predictive power of this measure on subsequent PTSD symptoms.

**Answer point 6.**
It is important to examine the association between the CCI score and IES/PTSS-10 to make sure that the instruments are assessing different phenomena. If the correlation had been too high (e.g. >0.70), the new instrument (CCI) will not have the ability to add anything as a possible predictor of PTS. These sentences are added into the revised manuscript (page 11).

**Point 7. Methods**
The authors may wish to spell out “GCS” when used for the first time.

**Answer point 7**
We apologize. It is now corrected in the manuscript: Glasgow Coma Scale (page 5).

**Point 8. Methods**
It seems appropriate to also report the mean time elapsed between the the trauma and the assessment, as well as the mean duration of the hospital stays.
**Answer point 8.**
As suggested by the reviewer, we have added these two mean values in the revised manuscript (page 7).

**Point 9. Methods**
Furthermore, it is also likely that perceptions may have changed over time during the hospital stay. I wonder if/how the authors have addressed this issue.

**Answer point 9.**
The reviewer raises an important issue concerning the possible changes in perceptions during the hospital stay. In addition, the level of stress and life events after discharge may be confounding factors. We have no data to study this. We have added two sentences in the revised manuscript (page 12): “Perceptions may have changed over time during the hospital stay and even after discharge. We have no data to address this issue, which may be a focus of future studies”.

**Point 10. Methods**
Development of the CCI:
It would be interesting to specify if the “Medical doctor” works on physical trauma?
It would also seem interesting to describe in more details how the items have been selected. Did the authors rely on any qualitative data? Did they brainstorm and select 8 items from a larger set of items?...

**Answer point 10**
We have added that the Medical doctor works on physical trauma (page 5).

The items were selected from 1) DSM-IV criterion A2 (fear) 2) two dissociation items (feeling as If the situation was unreal and emotionally numbing) were derived from the IES, 3) loss of autonomy (feeling emotionally stuck) is an additional item from DSM-IV criterion A2 (helplessness). 4) The four sensory items (visual, auditory, olfactory and pain/tactile) were supposed to be the most relevant, and to our knowledge, have not been studied in this context.
If the Editor wants, we may give further information about the process of developing the CCI, but we think this may be of limited interest for most readers.

**Point 11. Methods**
The authors may want to explain why they relied on both parametric (Student’s test) and non parametric (Spearman’s correlations) analyses?

**Answer point 11**
Students’ t-test was used to compare means that are normally distributed. Spearman’s correlations were used to study the relationship between two variables. We think that this has been explained in the statistics section.
Point 12. Methods
The authors indicated in the introduction that they measured the CCI at 2 timepoints “in order to get a measure of the duration of the responses” however, they do not seem to describe nor discuss the duration of the responses. Did they mean that prolonged peritraumatic responses (during the hospital stay) might be better predictors of PTSD than brief responses (only during the event)? If so, the authors might wish to address this question with multivariate analyses.

Answer point 12
In the multivariate analyses, we first entered the mean score at the hospital as a predictor, then the mean score for place of injury. Finally we entered both at scene and in hospital together as a sum score. It turned out that the scores in hospital were a stronger predictor than both the score at scene of injury and the total sum score.

We have added the following sentence in the revised manuscript (page 7): “We also entered the mean scores for the CCI at place of injury and in hospital (data not shown), but the CCI value in hospital was a stronger predictor for PTS symptoms.”

At a later point we can study whether one of the values, either at scene of injury and in hospital, are a stronger predictor for PTS than the mean value of the two measurement points.

Point 13. Results
It would be interesting to also report the SD for the different variables (in addition to 95%CI).
Table 2 and the manuscript text report different Cronbach’s alpha values for the perception factor in the hospital.

Answer point 13
We think that the mean values are of greatest interest. The 95% CI is a measure of the range of the means. We agree that SD gives additional information, but in order not to present too many figures, we prefer not to add SD unless the Editor insists.

The different Cronbach’s alpha values have been corrected in the revised manuscript (Table 2, page 9)

Point 14. Discussion
Again, the authors might want to discuss the findings with regards to papers on the impact of peritraumatic pain and subsequent PTSD as well as findings from the peritraumatic distress inventory (e.g.: Bui et al, Brunet et al....).

Answer point 14
We have commented on the important issue of pain in point 3.
We agree with the reviewer that peritraumatic distress would also be interesting to discuss. However, both the design and the manuscript have to be limited. We would also prefer to have some data for comparisons in order to discuss this point more extensively. See also our point 15.

Point 15. Discussion
As the CCI measures peritraumatic reactions, it would have been very interesting
to assess the convergent validity with other measures of peritraumatic responses (PDI and PDEQ). This point should be acknowledged in the limitations.

**Answer point 15**

We agree with the reviewer that such assessments would also be interesting. However, both the design and the manuscript have to be limited. We have, however, added on sentence on this point (page 12): “It might have been interesting to assess the convergent validity with other measures of peritraumatic responses like the Peritraumatic Distress Inventory (PDI). However, the main focus of this study was sensory perception”.

**Point 16. Discussion**

The authors alternate between psychological distress and PTSD, so that it is difficult to tell if the authors think that CCI is associated (and predictive) of psychological distress or PTSD symptoms. If the former, then, the fact that PTSD does not account for all types of psychological distress should be mentioned. If the latter, then the authors might want to mention that the study was limited by assessment of PTSD symptoms by self report questionnaires (as opposed to interviewer rated) and by the time frame of the assessments of “PTSD symptoms” which might not always correspond to that of PTSD per se (>one month) (i.e.: “PTSD symptoms” within one month of the trauma exposure are not per se PTSD symptoms, but acute stress disorder symptoms).

**Answer point 16**

In this paper posttraumatic stress symptoms are addressed. These symptoms may be associated with subsequent acute stress disorder (ASD), PTSD, other anxiety disorders or depression. We have revised the manuscript and removed the wording of psychological distress. The relationship between PTS and later psychiatric diagnoses will be addressed in other papers.

**Level of interest:** An article whose findings are important to those with closely related research interests  
**Quality of written English:** Needs some language corrections before being published  
**Statistical review:** No, the manuscript does not need to be seen by a statistician.  
**Declaration of competing interests:** I declare that I have no competing interests

Concerning the English language, please see comments to reviewer 1.