Author's response to reviews

Title: Trauma management incorporating focused assessment with computed tomography in trauma (FACTT) - potential effect on survival

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Author's response to reviews: see over
Cover letter for Revision 1

Manuscript 1199718740321152_R1: Trauma management incorporating focused assessment with computed tomography in trauma (FACTT) – potential effect on survival

Revision 1 of manuscript 24th March 2010

Comments of the authors in red (point by point responses)

To the editors of Journal of Trauma Management & Outcomes

Dear Sirs,

Here is our revised manuscript with the point by point responses. We marked our responses red. We hope that everything is to your satisfaction.

Kind regards,

Karl-Georg Kanz, Stefan Huber-Wagner and co-authors

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Dear Dr Kanz, e-mail 4th March 2010

Your manuscript has now been peer reviewed and the comments are accessible in PDF format from the link below. Do let us know if you have any problems opening the file.

We would be grateful if you could address the comments in a revised manuscript and provide a cover letter giving a point-by-point response to the concerns.

Please also highlight (with 'tracked changes'/coloured/underlines/highlighted text) all changes made when revising the manuscript to make it easier for the Editors to give you a prompt decision on your manuscript.

We look forward to receiving your revised manuscript by 1 June 2010. If you imagine that it will take longer to prepare please give us some estimate of when we can expect it.

Please don't hesitate to contact me if you have any problems or questions regarding your manuscript.

With best wishes,

The Journal of Trauma Management & Outcomes Editorial Team
ANSWERS TO THE REVIEWERS’ COMMENTS

Answers to Reviewer’s report #1:

Version: 1 Date: 14 January 2010
Reviewer 1: Michael Rieger
Level of interest: An article of importance in its field

Reviewer’s report:
General: This investigation of trauma management incorporating focussed assessment with computer tomography in trauma with potential effect on survival is a valuable contribution. The study demonstrates very nicely the paradigm shift in the primary emergency room management of polytraumatised patients: the unstructured mess with the realisation of time-consuming investigations without clear task assignment will leave in favour of a clearly defined emergency algorithm to which the whole body-CT contains as a leading diagnostic modality. The seriously injured patient is accompanied by a shock room team with defined duties and responsibilities.

Discussion
The authors indicate that the shock room algorithm used by them which includes the whole body-CT as an integral component decreases the mortality. Please highlight that the use of whole body-CT combined with your trauma algorithm lowers the mortality of polytraumatised patients. Other reasons for the lower mortality are e.g., the shorter praehospital time as well as the shorter time intervals up to the diagnostic measures (FAST: 4.3 versus 8.7; Chest x-ray: 8.1 versus 16.0; WBCT: 20.7 versus 36.3). Also differences in the clinical parameters (amount of infusion, shock etc.) as well as therapeutic measures (number of the operations) could possibly have an influence. This would be well to be mentioned in the discussion.

We have included your suggestions. See changes page 10 second paragraph, line 5-24.

Page 8: acronym acronym FACTT

Corrected
Answers to Reviewer's report #2:

Version: 1 Date: 20 February 2010
Reviewer 2: Teun Saltzherr
Level of interest: An article of importance in its field

Reviewer's report:
Major Compulsory Revisions
The authors describe the impact of a new evaluation and treatment algorithm (FACTT) combining whole-body CT (WBCT) with ATLS/ECT standards in severe trauma patients. In this interesting article the new algorithm is described into detail which can be helpful for other centers who want to implement WBCT/FACTT. They compare the outcomes within their hospital with national data from the DGU. Based on their findings the authors conclude that FACTT is fast, safe and may increase survival.

In general, the article could be more concise in order to bring the message to the reader more understandable. The article in its current form is too long and especially the methods and discussion section have to be reduced in length.

We have shortened several paragraphs as suggested. Details see below.

Second, what is ‘focused’ in Whole-body CT imaging? ‘Screening’ or ‘Non-selective’ would be more appropriate.

We thought very long about the correct term and decided us consciously to use the term “focused assessment with computed tomography in trauma (FACTT)”. We understand “focused” not in an anatomical or topographical sense. However, “focused” is intended to emphasize the focus on 1) life-threatening injuries, 2) a focused decision on how to treat the patient (damage control vs. definitive care) and 3) detecting other serious injuries quickly. So “focused” (clear aim) is more than just screening a patient (no specific aim). This is the reason why we would like to retain this term. See changes on page 9, discussion, first para.

The manuscript suffers from some weaknesses. My main concern is that the authors make the suggestion that the largest effects of FACTT come from the WBCT. However, in my opinion comparison of the Standardized Mortality Ratio (SMR) of the LMU with the DGU does not show the potential effects of the WBCT on probability of survival but the (potential) additional effect of the ATLS/ECT based workflow. Reason for this assumption is that patients with WBCT in the DGU group show no better survival rate (no significant SMR). Therefore, the better Ps in the LMU group has to be the result of other differences in protocol.

You might be right to some extent. However, we cannot understand your argumentation completely. You argue that the “reason for your assumption is that patients with WBCT in the DGU group show no better survival rate (no significant SMR)”. The SMR for the DGU and the LMU group as well are not specified in WBCT and non-WBCT patients.
The SMR of the DGU/LMU thus includes WBCT and non-WBCT patients. We emphasize that 26.3% of the DGU patients received WBCT compared to 86.3% of the LMU patients (table 2). So the use of WBCT seems to be the most noticeable difference between the two groups and thus mainly responsible for the difference in the SMRs (0.69 vs. 0.99 [RISC-based]).

You are right, we cannot prove that this observation is based on the use of WBCT alone. There might be additional influence of the ATLS/ETC based workflow itself. In our recently published report (Lancet 2009) we made extensive calculations for the DGU-group that proved that WBCT is an independent predictor for survival ($p \leq 0.002$) even if adjusted for a series of potential confounders. This is to our opinion a strong evidence that WBCT plays a key role between the two groups (LMU vs. DGU).

To express that the difference in the SMRs might not only be based on whether WBCT was performed or not we made clear that other factors like the trauma algorithm itself or shortened times to acquire sonography and chest x-ray as well as clinical parameters as the shock rate or the infusion amount as well as therapeutic measures (number of the operations etc.) could possibly have had an influence on our results.

See changes page 10 second para (similar to the suggestions of reviewer #1).

Second, we advise the authors to formulate their findings and conclusions carefully and based on their own data. For example, in the conclusion they state that FACTT reveals unexpected or hidden diagnoses with a major therapeutic impact. Although this might be true, this conclusion cannot be drawn based on the data presented.

You are right. This is not a conclusion that can be drawn based on our findings. We weakened this sentence.
See changes page 13, paragraph “conclusion”

Another example is the statement in the discussion section that FACTT is certainly justified with regard to the increased radiation exposure as their population had a mean ISS of 32.5. However, this is not entirely true because the inclusion for registration in the DGU was an ISS $\geq 16$. Therefore, the population of patients that did receive a WBCT but had an ISS of 15 or less was not assessed. Before such a conclusion can be drawn these numbers need to be analyzed.

You are right. The exact criteria when a WBCT in trauma patients should performed are not yet clear. Most experts state that the higher the suspected ISS is the more WBCT seems justified. However, patients with an ISS $\leq 15$ have not been analysed in this study. So we weakened this sentence.
See change on page 12, line 25.
Specific remarks:

Abstract
Background:
- describe the aim of the study more clearly and specify the word impact.
- change ‘life threatening’ in ‘life-threatening’.

We have done so. See changes in the abstract/background, page 2

Methods:
- change ‘from the 2002 to 2004 DGU database’ in ‘from the DGU database from 2002 till 2004’.
- capitalize the first letters of the mentioned scores.

We have done so. See changes in the abstract/methods/results, page 2

Manuscript

Background
- delete the second paragraph because it adds little to the manuscript. Instead describe into more detail which evaluation methods most hospitals use which participate in the DGU.

The information which evaluation methods most hospitals use which participate in the DGU is yet given in the first paragraph. To our opinion, the second paragraph comprises a relevant overview over the literature in this field. So it is a “classic” information that must be given in the background section of a serious article. The editor does not give any word limitation, so there is no need to take this short paragraph out.

- the first time abbreviations are used they should be written out in full.
- change ‘instable’ into ‘unstable’.
- suggest changing ‘multislice’ in ‘4-slice’.

We have done so. See changes in the page 4, line 19+27.

Methods
- suggest changing ‘stethoscope’ in ‘physical examination’.

Stethoscope stands for “physical examination”. As it is one of the major basic tools to detect immediately A- and B-problems we would like to keep at this clear and concrete term. In some critical situations this simple tool might be more lifesaving than a sophisticated CT-scanner. See change on page 5, line 4.
- indications and exclusion criteria for WBCT in the LMU should be described in
the methods section.

We have done so. See changes page 5 line 7-10.

- to reduce the text in the methods section I suggest to delete the description on
workflow in the LMU. This is already adequately presented in Figure 1.

This paper was submitted to an open-access journal to be accessible everywhere in the
world. For stuff of level I centers this section may be dispensable, however, physicians
in level II-III centers or second/third world countries will need and appreciate this
detailed description.

- provide more information on the WBCT with respect to contrast-enhancement
and arm repositioning, etc.

We have done so. See changes page 6, line 9.

- was the inclusion period for both populations from 2002-2004? Of course, yes!
- add the word ‘insertion’ after ‘chest tube’. Done so. See changes page 6, line 13 and in
table 1.

- capitalize the first letters of the mentioned scores. Done so, page 7, 1st para.

- delete the paragraphs in which an explanation is given for the trauma scores.
Instead I advise to provide references to articles or websites where an
explanation can be found for the scoring systems. Furthermore, consider
removing the paragraph on weaknesses of the TRISS to the discussion section.

Good remark. We have done so. See changes on page 7

Results
- why include 22 patients in the analysis when they did not receive WBCT?
Suggest excluding these patients for analysis.

You are right. 22 patients did not receive WBCT due to immediately necessary
emergency surgery. Explanation already given on page 8 line 15-17. Inclusion criteria
were patients from 2002-2004 presenting with an ISS ≥ 16, who had been admitted
directly from the incident scene to the hospital and in which information about WBCT
had been documented (WBCT yes/no). There are patients in the LMU and the DGU
group that did not receive WBCT. After applying the inclusion criteria the numbers are
exactly as presented. So there is no need for taking the 22 patients out.
- delete the data on ‘whole collective’ in Table 1 for this is almost equal to the DGU data (comprises 97% of the same patients).

This is correct. We have done so. See changes in table 1 and on page 8, line 21+27.

- reduce the amount of information given in Table 1. My suggestion is to delete all prehospital information, delete heart rate and SBP and instead give % of patients with shock, delete Hb, PRBC (%), operation rate (%), MOF and NISS. Adjust the order of data in demographics, vital parameters and trauma scores.

You are right. Table 1 is a little bit overcharged. We have adopted most of your suggestions. However, we would like to present the prehospital data as well as the transfusion, operation and MOF data to be able to assess the patients comprehensively. In contrast to the ISS the NISS expresses that the LMU patients have been significantly more severely injured compared to the DGU and thus should remain in the table. See changes in table 1.

Furthermore, consider making an extra Table in which the 24-hour mortality and the GOS outcomes are presented. (Delete ‘Deceased (%)’ because this will confuse the readers with the GOS score of 1 having an other percentage).

You are right. This might be confusing. We cleared this by deleting “deceased”. See changes in table 1.

- statistical analyses methods should not be described under the Table but in plain text in the methods section.

As many readers will not read the plain text we would like to give the most important facts for the understanding of the table in the legend.

- in Table 2 there is no need to give 95% CI of normal percentages. In the SMR ratio this can be of value.

95% CIs can be given also in such a context. But it is not essential. Hence, we changed this. See Table 2.

- the reduction in time to first chest X-ray and FAST could be an interesting finding. Why was there a reduction? If the authors have an explanation for this, I advise to mention this in the discussion section. If not, I suggest not mentioning these intervals because it distracts attention from the time to WBCT, which is the most interesting finding.

As suggested we wrote possible explanations for this in the paper. See changes page 10, line 15-19.
- in Figure 1: add an ‘a’ in pneumothorax. Change ‘repirator’ in ‘respiratory’. Add an interspace in central venous line. Add descriptions for all abbreviations used in Figure 1. Suggest presenting Figure 1 into ABCDE order of the ATLS guidelines.

Good remarks. We have adopted the suggestions. Fig.1 is already sorted by ATLS guidelines.
See changes in fig 1.

Discussion

- the authors should discuss their findings such as reduction in time, increased Ps within their own center with respect to the current literature and current trauma care in the other centers of the DGU. By doing this, they will increase the readability of the manuscript and make it stronger.

Reduction in time has yet been discussed. See changes on page 10, second para. To the best of our knowledge there is only few literature discussing increased Ps in polytrauma patients. We discussed and incorporated two of those references. See changes on page 10, line 2-3 + page 10, line 22-24.

- Discuss the potential biasses which could also contribute to the differences in LMU and DGU outcomes.

We refer to the section “limitations” on page 12/13, last paragraph, were we already discussed several potential biases.

- the paragraph described in the methods about the weaknesses of TRISS and why they used both RISC and TRISS have to be described here. We also advise the authors to address their choice for using the TRISS while only 50% of the patients had available data for using TRISS.

As suggested above we deleted these paragraphs. However, we gave links to the relevant literature for the reader that wants to have detailed information on this. As TRISS is still the most widely used method for measurement of expected outcome in patients with trauma we chose to use it also in our study. See changes on page 7, first paragraph.

- Delete or at least reduce all extra information on potential advantages of WBCT from other studies on which the current manuscript does not provide any data (e.g. missed injuries or therapeutic changes, etc.). This seems relevant but this article is not a review and therefore in my opinion literature related to the outcomes should be described.
As there are at least 80 really relevant articles on WBCT in the literature we think that bringing 32 of them is reasonable (the rest of the literature cited is not related to WBCT). Of course this is not a review, but to our opinion it should be allowed to mention at least the most important articles. This is usual in scientific papers. As explained above we are addressing a broad readership all over the world thus bringing the basic literature is fully intended as a service for them.

- the REACT trial's aim was to assess the effects of the location of the scanner, respectively in the trauma room versus in the radiology department, on several clinical outcomes and not the effects of WBCT as such.

We made this more clearly. See changes on page 12, second paragraph.