Author's response to reviews

Title: Fluid Lavage in Patients with Open Fracture Wounds (FLOW) An International Survey of 984 Surgeons

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Author's response to reviews: see over
RESPONSE TO REVIEWS

REVIEWER COMMENT 1:

Page 9, lines 300-309: The authors need to more explicitly point out that their study design does not although for identification of potential non-responder bias. This issue is potentially of considerable importance in many studies of physicians’ behavior/opinions. Although the authors clearly do not deem it to be an important consideration in their study, the importance of this issue can not be summarily ruled-out unless it is specifically analyzed. And, rest assured, some readers will consider it to be important, and I would suspect that some reviewers might even force the authors to obtain additional data to address this issue. In my own research I have been in the very same spot – reviewers have forced our research team to institute a strategy to evaluate the non-responders. But, again, I do not think that this additional analysis is absolutely necessary in the broad perspective of your data and its future importance in guiding/organizing a multicenter trial.

It is plausible, even in your study, that non-responders might comprise a group with significantly different opinions/practices than responders. Therefore, by simply increasing the sample size of only one (responders) of these two groups (non-responders and responders), your study does not achieve, as you say, “a sufficient sample for precise estimates”. What you really have accomplished by increasing the sample of responders is to further restrict the strength of the analysis to only one of these two potentially different groups – this occurs even though you have enhanced the statistical power for analyzing the responders. Although non-responder analyses can be difficult to achieve, attempts should always be made in survey studies. This is accomplished by re-sending surveys at specified times after the cut-off for “responders” has been defined. In most cases one attempt to re-send surveys to non-responders is sufficient. In my research, we now always make two additional attempts to evaluate the individuals that are defined as non-responders.

I request that the authors, short of re-sending surveys to non-responders, make efforts to more explicitly demonstrate that they recognize the limitations of their methodology in evaluating non-responders. This can be accomplished with this suggested revision of your paragraph on lines 300-309:

“The strengths of our study include: 1) the use of a rigorous process for the development of the questionnaire items with active surgeon participation; 2) a comprehensive sampling of surgeons of North America and Europe as well as academic and non-academic centres, and 3) an acceptable survey response rate of approximately 56% that helps to limit, but not eliminate, non-responder bias (6,29). Our response rate of over 900 surgeons provided a robust data set for the general purposes of our study, and it exceeded the level for our anticipated study
precision. Nevertheless, future studies that are aimed at more rigorously evaluating potential non-responder bias will need to institute a strategy to: 1) define non-responders by a temporal cut-off, and 2) re-administer the survey one more or more times in an attempt to achieve a sufficient level of non-responder compliance for statistical evaluation. Similar strategies have been employed in survey studies of orthopaedic surgeons’ behaviors/practices on topics where responder bias would seem highly likely (Skedros et al., 2006) compared to those that would seem, similar to the present study, unlikely (Skedros and Pitts, 2007).”

The last sentence is optional – the authors do not need to reference the work of this reviewer; I have inserted these two studies here since they provide examples that I could think of off the top of my head. Certainly there are other studies that have made efforts to make the same point that responder/non-responder analysis cannot be summarily discounted without some attempt at specifically analyzing it.

RESPONSE: We have edited as suggested by this reviewer and included the last sentence. We believe the suggestions are extremely helpful and thank this reviewer for assisting us in outlining the issue of non-responder bias.

REVIEWER COMMENT 2:

Page 11, lines 358: Please add more specifics here. For example, including the p value would be helpful so that the point is clear without having to obtain reference no. 4.

RESPONSE: We have added specifics as suggested by reviewer.

REVIEWER COMMENT 3.

Line 118, I would add the term "surgical" before the word "management" to differentiate it from the most important treatment factor, which is probably early administration of antibiotics - or at least it is equally important.

RESPONSE: We agree. We have revised as suggested.

Lines 122-128 the information about road traffic accidents seems to be of only peripheral relevance. Statistics about open fractures specifically and the DALYs or impact of infected open fracture may be more directly relevant, if they exist.

RESPONSE: We thank this reviewer for the comment. We would prefer to keep this information if space allows. We are unaware of DALY data.

REVIEWER COMMENT 4

discussion: I was struck by the number of respondents who believed that antiseptic irrigation (iodine, etc) was more effective than saline, when it may be
actually worse and there is certainly no evidence that it is any better. this might be worth a comment.

RESPONSE: We have commented as suggested.