Author’s response to reviews

Title: The MEDEA FAR-EAST study  Conceptual framework, methods and first findings of a multicenter cross-sectional observational study

Authors:

Sophia Hoschar (sophia.hoschar@googlemail.com)
Jiang Qi Pan (panjiangqi@qq.com)
Zhen Wang (775320302@qq.com)
Xiao Yan Fang (xiaoyan.fang@helmholtz-muenchen.de)
Xian'e Tang (251496417@qq.com)
Wei Qi Shi (swq007@tongji.edu.cn)
Rong Xiang Tu (turongxiang@163.com)
Peng Xi (shtjmt2010@163.com)
Wen Liang Che (chewenliang@tongji.edu.cn)
Wang Hong Bao (wanghongbao@tongji.edu.cn)
Ya Wei Li (shahuwang@163.com)
Kurt Fritzsche (kurt.fritzsche@uniklinik-freiburg.de)
Xue Bo Liu (1xb70@hotmail.com)
Karl-Heinz Ladwig (ladwig@helmholtz-muenchen.de)
Wenlin Ma (mawenlin@tongji.edu.cn)

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Reviewer 1: Yves Chaput

We appreciate that the reviewer provided us with such detailed and structured remarks.
As to the suggested minor changes, we like to thank the reviewer for his helpful remarks and have made all suggested corrections regarding style and grammar accordingly.

Regarding the major remarks, we carefully would like to address the reviewers’ comments on a point-by-point basis.

Background

1. Although generally well written The BACKGROUND section could benefit from some clarifications in order to help the reader follow the author's reasoning (...)

I would rewrite the paragraph beginning between lines 67 and 81. There are just too many vague and subjective phrases such as "...conservative outlet..." on line 67 or a "...complex network of inner barriers..." on line 71 or "symptom vagueness" and "symptom incongruence" on line 76. Furthermore, the referred to Figure 1 does not really clarify this in any significant way. I grasp that the authors are trying to illustrate the complex decisional algorithms involved as an individual seeks help for an AMI. However, the paragraph just adds to this confusion. These complex interactions should be more precisely detailed.

We thank the reviewer for his suggestion on restructuring the background section. We have restructured the second, third and fourth paragraph and hope that it improves the readability. Furthermore, we have rewritten the paragraph beginning between lines 67 and 81 (see lines 61-85) to outline our understanding of the “inner barriers” in more detail. As Figure 1 does not add any additional information to the rewritten paragraph, we have deleted it.

2. The primary objective of this study is not clear. Just saying that it was to "apply a holistic approach" is fine but you have to define what you consider to be a holistic approach. Especially in relation to the paragraph above, which in itself is not particularly clear. I would also clearly state the preliminary findings that the authors wish to present in relation to the total data collection.

Again, we want to thank the reviewer for this suggestion which we have followed. We hope that with the rewritten paragraph (see point 1) we have given context to what we mean by “applying a holistic approach”. Furthermore, we have outlined which preliminary findings we would like to present in this paper in more detail (line 96-98)

Methods

3. The first phrase, line 91, should be in the last paragraph (i.e., the aims section) of the BACKGROUND section.
We like to thank the reviewer for this statement. We have integrated line 91 in the last paragraph of the background section.

4. In the paragraph beginning with line 107 it is not clear whether the 38 emergency stations are fully private in comparison to the outpatient departments of public hospitals. Indeed it would be pertinent to slightly elaborate the differential private / public medical offerings in the city given that costs may be a factor influencing MED Er visits.

We agree with the reviewer, that the complexity of the topic warrants further detail.

With major reforms in 2009, China aims to provide universal health insurance by 2020. Currently, insurance covers costs for visits to the Emergency departments in public hospitals. Transport via ambulance is only partly covered for depending on the insurance. Ambulance centers do not provide treatment, as they are only responsible for the ambulance services. We have therefore deleted the information related to the number of emergency stations and added details regarding the cost of emergency treatment in public hospitals.

5. Line 117, Some of the rating scales do not appear to be validated for the language and this should be mentioned in a limitations section.

We thank the reviewer for this close observation, and have included a fitting statement in the limitations section.

6. I would think about shifting section 2.3 (Sample size and drop-out analysis) into the RESULTS section and substituting the present Table 1 with a new Table 1 consisting of data on the study population. Again, I would not present any data analysis (drop out analysis) in the METHODS section but rather, in the RESULTS section.

Following the suggestion of the reviewer, we have shifted the section on sample size and drop-out analysis in the results section. We have carefully read the submission guidelines of BMC Emergency Medicine, and have not found any restrictions to figures or tables in the background section. It states, that “Tables should be numbered and cited in the text in sequence using Arabic numerals” so that we suggest to keep Table 1 on Chinese prehospital delay studies in the background section as we have referred to it several times in the text. We have added an appropriate figure to illustrate the results related to the paragraph sample-size and drop out analysis.

7. Same comment as to Table 2, which should be placed in the RESULTS section, with only the methodology used to obtain this data in the METHODS section.
We want to thank the reviewer for this comment. As we use Table 2 to outline methodological details and not to portray any findings from this study, we propose to keep Table 2 in the methods section.

8. I am a bit uncertain as to the pertinence of Table 3. It does not give the reader a good understanding of why the scales were selected and what the type of data the authors hoped to obtain from them. One of the objectives of this manuscript is to detail the methodology used (in relation to the hypotheses elaborated in the BACKGROUND section). I would have liked to read something like "We used these 3 scales in order to obtain these types of variables (and so on). This is just not obvious from either the text (lines 172 to 174) or Table 3 as it is presently structured.

We have carefully read this suggestion and agree with the reviewer that this section is to short. We have outlined the methodology in further detail, so that Table 3 becomes obsolete.

9. With the time factor being so crucial in this study I was somewhat surprised that it comprises but one single phrase (line 152) of the RESULTS section. One would certainly like to have very precise and complete knowledge at to exactly how time of onset was determined. Also, what is the % variance in this triangulation technique?

We want to thank the reviewer for his careful attention to our main variable “prehospital delay”. We have added details as to how we obtained the time of symptom-onset. Regarding the % variance of the triangulation technique mentioned in the paper Moser et al., we have to admit that neither Moser and colleagues nor our investigation has determined the variance. Nevertheless, we have included all details on how to report the data collection of the variable “prehospital delay time” to meet current standards in prehospital delay literature.

RESULTS

We want to thank the reviewer for his understanding regarding our idea to present “preliminary data”. Nevertheless, we have restructured table 3 (which presents our descriptive data) for more clarity. We have included three more findings on psychological patient characteristics in order to include preliminary findings of the self-administered questionnaire.

DISCUSSION

10. As with the BACKGROUND section the RESULTS section could benefit from some rearranging. The first paragraph of the RESULTS section should state what the authors think is their primary finding (i.e., their findings on pre hospital delays). They start off by reviewing the literature worldwide. I would restructure this.
We like to thank the reviewer for this useful suggestion on content and readability. Following the suggestion of the reviewer, we have rewritten the first paragraph to discuss our results, starting with our main variable. In the subsequent paragraphs, we have discussed all other reported results on patient characteristics and factors surrounding symptom onset.

11. A corollary is that they do not highlight their stated secondary aim, that being elucidating those complex factors precipitating a decision to seek help for an AMI. Figure 1 is not a result per se but rather, a conceptual framework to be tested using real data.

We want to thank the reviewer for his skeptical remark about not clearly discussing our secondary aim. We have therefore added three exemplary findings regarding psychological patient characteristics as we feel that data obtained from our questionnaire is among the innovations that we can bring to prehospital delay research in China. As this paper was designed mainly to introduce methods and show the validity of the data collected within the Chinese population, we plan to test our “concepts” using the data we have collected in future publications.

12. I have difficulty with the purpose of their second paragraph, which is usually reserved for reviewing data concordant / discordant with the primary finding or, alternatively, detailing the study's secondary findings. The present second paragraph could easily be condensed and put lower in the Discussion priority list.

We thank the reviewer for another detailed suggestion. We have deleted the paragraph as it was and have discussed our results through the use of the Chinese publications (which we outlined in Table 1) one after another. Given that we want to show the validity of our results in a Chinese population, we felt that it is nevertheless important to compare our findings with preexisting literature.

Reviewer 2: Paul Jennings

We appreciate the general judgement of the reviewer stating that it to be “an overall interesting qualitative study”.

TITLE

1. I don't think the reference to a 'conceptual framework' needs to be included in the title, as this is not really presented in any detail in the study proper. Also, there is no novel methods used in this study so would remove reference to methods also.

To the best of our knowledge, this is the first study on delay issues during acute myocardial infarction which attempts to clarify the patients' decision process by going substantially beyond
the usual assessment of the disease related symptom pattern. For the very first time and based on theoretical considerations, we aim to understand perceptual and biographic barriers to an adequate response patterns of AMI victims in the probably most dramatic and life threatening phase of their lives. We have added more details in the methods and discussion section in order to clarify this.

BACKGROUND

1. Page 2, Line 10: A timeframe needs to be added to the incidence reported.

We thank the reviewer for this suggestion. We have changed the incidence to one-year incidence and have included the year.

METHODS

1. Page 5, Line 132. There are two sections presenting information on sample size; here and Page 8, Line 1 (“2.6 Sample Size Calculation”). This content should be merged into one section. (Perhaps separate sample size and drop out analysis?).

As already suggested by reviewer no. 1, we have shifted the sample size and dropout-analysis into the results part. We would suggest the sample size calculation to remain in the methods section, as it was conducted prior to the start of the study, whereas the recruited patient sample represents first results.

2. Page 5, Line 54. Include p value associated with test of statistical significance here and throughout, whenever reporting statistical significance

We thank the reviewer for this valuable reminder and have reported all p-values where appropriate.

3. Page 8, Line 6. Sample size calculation refers to multivariate analysis, however there are no MV analyses throughout manuscript. Please check sample size calcs.

The purpose of this manuscript is to outline the methodological and scientific basis of the MEDEA FAR EAST Study in order to reference this manuscript in all further analyses still to come. Please note that the MFE study is more a research platform eligible to serve multiple purposes and singular research questions in future which yet have to be captured in the sample size calculation. Therefore, it is not surprising that the data presented in this methodological oriented paper would not require the sample size of this study.
RESULTS

1. Page 8, Line 45. Figure 1 should be labelled as figure 2.

We thank the reviewer for this valuable remark and have renamed the figure accordingly.

2. Page 9, Line 43. Actual p value should be reported throughout (rather than p>0.5).

Again, we want to thank the reviewer for this other important reminder regarding the reporting of nonsignificant results. We have included all actual p-values where appropriate.

3. “The numbers / differences between sites should be included within results.”

We would like the reviewer for this comment and have added the following results in the section “patient characteristics”: “The mean age, the proportion of male patients, and prehospital delay did not vary significantly between the four recruitment hospitals (p=0.17; p=0.42, p=0.88).“

REFERENCES

1. “There are lots of references. Could be culled somewhat. “

Following the suggestion of the reviewer, we deleted obsolete references the manuscript. However, it should be acknowledged that, in this study, we carefully screened the research which has been performed in China which we considered important enough to summarize in this context. Furthermore, the bulk of references was caused by referencing the background of all questionnaires and instruments employed in this investigations, contributing to 24 references from the methods section alone.

TABLES

1. Figures 2 and 3; x axis intervals are not mutually exclusive (ie 35-40, 40-45, etc). These should be rectified.

We would like to thank the reviewer for his particular attention regarding the details. The intervals have been rectified accordingly.