Author's response to reviews

Title: Personality and time delay to treatment of acute myocardial infarction.

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Version: 3 Date: 22 February 2011

Author's response to reviews: see over
Dear member of Editorial board
Thank you for your valuable comments.

The initial consideration of CHD generally are not relevant and should be omitted, the quality of the English in places is poor and the final conclusion is too dogmatic. Using the measurement tools they did suggests that personal factors etc were unrelated, but the tools used are imperfect and not comprehensive, so some modesty would be in order here.

Comments to the member of The Editorial Board
We have completely revised the introduction and deleted the first parts
We have tried to correct the linguist errors.
We have revised the conclusion in the discussion, we acknowledge that the instrument may be suboptimal for the purpose of studying time delay.

Comments to reviewer no 1.

Dear Cheuk-Kit Wong
Thank you for your valuable comments

The authors did not really separately analyze the STEMI gp from non-STEMI gp but in their table 2, the proprotion of STEMI was similar in all quartiles of time to presentation. Theoretically a proper sub-gp analysis on STEMI should be done but this is likely to show similar result to the overall cohort.

We agree that this method is likely to produce the same results, since the proportions are similar in all groups of time delay. We prefer not to do subgroup analyses since the groups will be small.

Comments to reviewer no 2.

Dear Kim Smolderen
Thank you for your valuable comments.

1. Including the 2 complete separate foci of the study so that you end up with a more scattered approach and it is really hard to convey a true clinically relevant message in your study.

Comment no 1.
We have revised the introduction. The manuscript is now much more focused on time delay.

2. Although some of the authors’ statistical work was clarified, I still find it hard to understand what the authors actually did. It almost looks like they took each dichotomous psychosocial variable as dependent variable in separate logistic regression models and entered quartiles of delay times as well as age and sex as independent variables. Additionally, they used a similar approach for the psychosocial continuous variables using GLM. This is really not the way the data should be analyzed. First, of all, creating multiple models like that (i.e. multiple comparisons), increases risk of Type I error, and that is why we avoid this
approach of multiple comparisons in statistics all together. Delay time studies typically apply advanced modeling to their data, in order to allow for an adequate interpretation where delay times are always the dependent variable and psychosocial or other patient factors the independent variables and these are entered all at once in the model. What prior studies did was either dichotomizing their delay times (e.g. >6 hours vs. < 6 hours) and do logistic regression modeling or use advanced models such as cumulative logit modeling.

Comment no 2.
Table 2 consists of repeated analyses with dichotomous psychosocial variable as dependent variable in separate logistic regression models and delay times as well as age and sex as independent variables. We agree that this increased the risk of type 1 error, however, there was no significant finding. In the revised manuscript, we followed the recommendation to perform a multiple logistic regression with >6 hours as dependent variable. Since many of the independent factors are strongly interrelated, we performed a backwards stepwise regression. All variables in table 2 were entered – only depression (p=0.06) was retained in the final model.

3. Again, using the quartiles of delay times is problematic and clinically interpretable intervals should be used to analyze the data. The categories =<2 hours, between 2-6 hours, >6hours are generally adopted in this tradition of research, in current guidelines, in perfusion studies, and in that way, findings can be directly translated to each other and can be compared.

Comment no 3.
We have followed your suggestion and changed the categories from quartiles to three groups <2 hours, 2-6 hours and > 6 hours. This is shown in table 2 page 15-16.

4. Still, exclusion of those aged 71 years or older is problematic. The argument that was used by the authors that their cohort would be a more ‘pure’ group and that older patients have more comorbidities and silent infarcts anyways, is not valid. In your research, you want to approach reality as much as you can in order to be able to generalize your findings. People that are 71 years or older also experience MI’s and are an important part of that disease population. Also, it is known that older age is associated with an increased risk of delaying their response to go to the hospital for an acute MI. You can imagine that differences in presentation, symptoms and psychosocial factors may explain part of this association with delay. I would strongly suggest addressing this in your discussion/limitations.

Comment no 4.
We have now addressed this in the limitations on page 10.

5. The Stroop test does not seemed to be used in the delay work. Yet, its use it is brought as one of the selling points of this study?

Comment no 5.
In the revised manuscript, we present the main findings of the relationships between CWT and time delay in the text (Results, last para). There were no significant relationships between CWT and time.

6. It is confusing to have different numbers of sample sizes throughout your study, depending on the assessment that was being done. Try to come up with one sample size, including patients that had data on all variables of interest.

Comment no 6.
In the revised manuscript, we define our study sample as those with complete information on personality, depression and coping (n=323). In addition, a subgroup analysis of those with CWT (n=180) is presented in the text. This is mentioned in the abstract and in the methods p. 5.

7. There are a lot of typos in the revision: e.g., alder, tex, god, strop, references, bee…

Comment no 7.
We have gone through the manuscript and tried to correct the typos and errors.

8. Given the multiple concerns I have, I would suggest the authors to re-think the focus of the paper, as well as their analytic work. I think the paper could really benefit from that. I hope my comments may be helpful to the authors.

Comment no 8.
We are very grateful for all your help. We changed focus of the paper, the categories of time are new, we have performed a multiple logistic regression as suggested and the study sample of 323 patients is clarified.