Author’s response to reviews

Title: Physician workload associated with do-not-resuscitate decision-making in intensive care units: an observational study using Cox proportional hazards analysis

Authors:
Kuan-Han Lin (okonkwolin@gmail.com)
Shu-Chien Huang (cvshuang@gmail.com)
Chih-Hsien Wang (wchemail@ntu.edu.tw)
Chau-Chung Wu (chauchungwu@ntu.edu.tw)
Tzong-Shinn Chu (tschu@ntu.edu.tw)
Yen-Yuan Chen (chen.yenyuan@gmail.com)

Version: 2 Date: 19 Nov 2018

Author’s response to reviews:

Point-to-point Responses to “REVIEWER”

Dear Dr. Tomohiro Shinozaki,

Thank you for reviewing our revised manuscript and also thank you for this opportunity for us to revise this manuscript again. The point-to-point responses are as follows:

1) In my understanding, DNR order had been submitted at most once per patient because survival analyses (K-M estimation and Cox regression) were employed at patient’s level. However, in Fig 3, each physician’s rate of DNR per patient-day seems too high. For example, physician 8 who treated 47 patients during 237 patient-day (Figure 2) had the rate of 2.95 per patient-day; thus his/her DNR decision should have been made 237*2.95 = 699 times! Do I misunderstand the data analyzed in this study? As I believe all data in the study should be consistent with each other, please summarize main DNR data (analyzed by survival methods), or provide clear explanation for this data.

To Reviewer: Thank you for the opportunity for us to clarity this confusion. In Figure 3 and Figure 4, “Rate of DNR per patient-day, %” for each physician means the incidence rate of writing a DNR order for his/her patient for a hundred patient-day. Therefore, for example, Physician 8 who treated 47
patients during a total of 237 patient-days (Figure 2) had the rate of 2.95% or 0.0295 per patient-day. Thus, the number of cases he/she wrote DNR orders for was 237*0.0295 = 7 (cases). For avoiding this confusion Reviewer raised, we changed “Rate of DNR per patient-day, %” in Figure 3 and Figure 4 to “Incidence Rate of Writing a DNR Order per Hundred Patient-day”. We also changed all “rate of DNR decision” in the text to “the incidence rate of writing a DNR order”.

2) It is surprising that almost no correlation between DNR rate and number of patient-in-charge was observed at the physician’s level (Fig 4). Was this caused by the problem mentioned in the above comment 1, or is it real relation at the physician’s level?

To Reviewer: Thank you for raising this issue. The incidence rate of writing DNR per hundred patient-day for each physician and the average number of patients they cared for per day were not significantly correlated (Figure 4). This may due to the small sample size (15 physicians) for this analysis. Correlations calculated on data collected from a small sample (30 or fewer) can be affected substantially by any changes in values, including the addition of an outlier or transformations of the variables. [Goodwin, Laura D, and Nancy LL. Understanding correlation: Factors that affect the size of r. The Journal of Experimental Education 2006. 74:3; 249-266]. Based on the individual level of physicians on Figure 4, we expect that the correlation may be statistically significant if Physician 15 is excluded. In addition, ecological bias may play a role in accounting for this result as mentioned by Reviewer. We, therefore, added “our results still suffer from ecological bias” in Strengths and Limitations following Reviewer’s suggestion.

3) The authors’ reply “Accordingly, the statistical methods used in our study were not unique, and our study results may be as valid as prior studies focusing on end-of-life medical decision-making”: this can also be described as “our study results may be as invalid as prior studies focusing on end-of-life medical decision-making”. Although I understand the additional complicated analysis will impose the unpleasant burden on the authors, unfortunately in current status, the results at physician’s level (which would suffer from ecological bias) and those at patient’s level (which may be confounded by unmeasured physician’s variables) are inconsistent and we cannot judge which is a more reliable result. If they decide not to use frailty model analysis or robust standard error for Cox regression, please provide clear caution in limitation section in Discussion.

To Reviewer: Thank you for your kind understanding. We followed your suggestion to add “some insufficiencies in methods might hurt the outcomes of this study. For example, some potential confounding variables, e.g. unmeasured physicians’ variables, were not included in the multivariate Cox proportional hazards regression model. In addition, although the multivariate Cox proportional hazards regression model is most popular for survival data analysis, there may be concerns that our results still suffer from ecological bias” in the third paragraph of “Strengths and Limitations.”

We would like to thank you again for the suggestions. We are grateful for how your suggestions have benefited and advanced our approach to the study. Please do not hesitate to let us know, and to do more revisions if you still have any suggestions. In summary, we hope that the revised manuscript is more suitable for being published in BMC Medical Ethics.