

Cleveland, November 1, 2015

Editor  
World Journal of Clinical Pediatrics  
Baideshing Publishing Group

Re: Manuscript No: 22151

Dear Editor,

Thank you for the opportunity to revise our manuscript and address the editorial and reviewer's comments. We will address them sequentially below. All modifications to the revised manuscript have been done with "Track Changes" so as to facilitate their identification by the Editor.

#### 1) Editor's Comments

We appreciate your notation regarding the Journal's language policy. Five of the 6 authors are native English speakers. The 6<sup>th</sup> author has lived in this country for 25 years and has native fluency. I respectfully request that the requirement for additional language certification be waived.

We have shortened the "Aim" to 20 words or less, as requested.

We are providing an Audio core tip, as requested.

The references have been reformatted to include DOI and PMID numbers for every article. A few of our citations are websites and these, of course, do not have DOI or PMID.

We have completed the "Comments" section of the revised manuscript based on the format provided; we hope you find it acceptable.

We have included Figure 1 as a ppt file embedded in the word document so as to facilitate your editing, as requested. Figures 2 and 3 are produced from our scientific plotting software and come only tif formats. Please let us know if that is a problem. Should you need to edit figure 2 and 3, we would need to manually create them from scratch using the raw data through a different software package. Please let us know if that is the case.

#### 2) Reviewer 1

*"The manuscript is well written and covers a gap in our knowledge on this topic. In this manuscript, DPT is clinically relevant in predicting time from withdrawal of life support to death. Precisely, DPT is more useful in predicting death within 60 minutes of withdrawal of life support than within 30 minutes. Additional calibration and modifications of this important tool could help guide the intensive care team and families considering DCD. I*

*think that some graphics will help the reader to understand better. Do all patients consenting to donations also consent for an autopsy? This data are relevant and this aspect can be highlighted in the manuscript.”*

We appreciate the reviewer’s observations. We are unclear as to what additional graphics the reviewer would like to see. We originally included a figure depicting the flow of patients in the study sample to better guide the reader on the various modes of death encountered in our PICU. We then included 2 figures showing the ROC curve data, one for the DPT 30 and another for DPT 60. We also have 3 tables, one with the original factors involved in the DPT calculation and two others with the raw data cutpoints for the DPT 30 and 60. These tables and figures summarize the entire data presented, so although we would gladly include additional elements for clarity, we are unsure as to what the reviewer is referring to.

Organ donation and autopsy are independent and not mutually exclusive processes. A family might elect to have an autopsy and not donate organs, donate organs but not have an autopsy, have both or have none. In cases when an autopsy is mandated by law (suspected child abuse or foul play), the autopsy takes priority over organ donation and the donation process only moves forward with permission for the County Medical Examiner. Conflicts between organ donation and autopsy are relatively rare situations in our practice. We did not have permission to collect data on which patients underwent an autopsy in this sample, so we regret we cannot further elaborate on this issue in the present study.

### 3) Reviewer 2

*“This is an interesting paper based on a very large number of PICU admissions. The numbers of patients entered into the study are however small because of the nature of the study. What is disappointing and ultimately makes the study impossible to interpret is that of the 70 patients in whom treatment withdrawal occurred only 2 donated organs. This makes the applicability of the data presented rather worthless. I would encourage the authors to work with other centres to try and increase the cohort of evaluable patients.”*

We are thankful for the reviewer’s input. We respectfully disagree with the reviewer on the assertion that the study is impossible to interpret because only 2 patients donated organs. Perhaps the reviewer did not completely understand our objective so we are happy for the opportunity to clarify. This is not a study of organ donation rates and the comment that 2 patients had successful donation after circulatory death was only made to highlight the fact that many additional opportunities exist to expand this pool of donors. Our study had a simple objective: to externally validate a newly developed pediatric bedside tool that has shown good accuracy in predicting death within 30 and 60 minutes of withdrawal of life support. This tool was originally developed by others using a single institution sample and requires additional external validation. To that effect, we applied the tool to our well-documented remote (external) sample for external validation. The only variables that truly matter for interpretation of these data are the mode of death (i.e. withdrawal of support), the pre-withdrawal Dallas Predictor Tool (DPT) scores to predict death within 30 and 60 minutes, and the actual time interval between withdrawal and death. We believe our data are interpretable and accurately evaluate the prediction

made by the DPT regarding time interval to death in our external sample. To further clarify this issue, we have included a paragraph in the discussion addressing the reviewer's observation and highlighting the opportunity for greater number of donors from this non-traditional pool of donors.

Once again, we are grateful for the opportunity to revise our manuscript and hope the Editor will find our clarifications acceptable.

Sincerely,

A handwritten signature in black ink, appearing to read 'Alex Rotta', with a stylized, cursive script.

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