Author's response to reviews

Title: Hypothermia in a Surgical Intensive Care Unit

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Author's response to reviews: see over
Response to comments made by Dr Rainer Lenhardt:

Thank you for your valuable comments regarding the manuscript. The manuscript has been revised according to the comments made and we have responded point-by-point to the specific comments in the section below:

About major Compulsory Revisions:

1. We have formulated a clear hypothesis to the study and stated a clear reason to done it: 
"With the progress in surgery and anesthesia more advanced technology was introduced, procedures are using more complex techniques, lasting longer and sometimes are made with larger incisions. These procedures are done in older and severely ill patients who present significant underlying medical problems and have sometimes undergone previous extensive surgery. The choice of anesthesia technique is also changing and combined epidural and general anesthesia are being used more often. These are reasons that may increase risk of intraoperative and immediate postoperative hypothermia. Whereas effective warming measures are available and more information about the adverse effects of hypothermia has been reported, the incidence of hypothermia at the time of admission to the surgical ICU is still frequent. Re-identifying significant predictive factors would help in decreasing this incidence. Such information could be helpful to prevent unnecessary risks and adverse outcomes and could decrease the current frequent incidence of hypothermia”

The purpose of this study was to estimate the prevalence of hypothermia on admission to a surgical ICU and retrospectively identify their clinical predictors. A second objective was to prospectively evaluate the outcome measured in terms of ICU length of stay (LOS) and hospital mortality.
We abandoned the sentence about the incidence of hypothermia greater in UCI patients than in PACU patients because we really don’t have data about it.

2. In fact we think hypothermia should be considered as patient temperature below 36°C and all efforts should be made to maintain perioperative temperature above that value. However other studies have considered lower limits of temperature in the definition of hypothermia and in the methodology of their works. That is what we try to explain in discussion:

“A controversial point of our study is the considered definition of hypothermia as Tc < 35°C. Others have chosen the same value of core temperature or even a lower one as definition of hypothermia. Meanwhile many approaches have used 36°C as the cut off point between hypothermia and normothermia in ICU and studies have considered it in their methodology”

3. Our rates of warming and temperature monitoring in operating room were indeed very low. Now we have results to comment with anesthesiologists and try that they have more concerns regarding temperature monitoring and the use of rewarming techniques. At this propose we put on the discussion: “If patients undergoing major procedures with long lasting anesthesia became hypothermic more often, they were probably not warmed actively as we could suspect from the active warming rate and rate of temperature monitoring of this set of patients.

And

“In our hospital, and as we could demonstrate with this study, warming rates were very low and the rate of temperature monitoring was even lower, predisposing patients to hypothermia. Like Sessler stated ”the minor and major complications of hypothermia are
thus well documented. In some patients mild hypothermia is likely to be dangerous. In others it will be uncomfortable and slow recovery”. Like him we propose that intraoperative core hypothermia should be avoided and we think the proposed management guidelines should always be observed.”

4, 5. As we stated on point 1: “The purpose of this study was to estimate the prevalence of hypothermia on admission to a surgical ICU and retrospectively identify their clinical predictors. A second objective was to prospectively evaluate the outcome measured in terms of ICU length of stay (LOS) and hospital mortality.”

That is why we have calculated odds ratio.

I think now it is clear that in our consecutively admitted patients, in our surgical ICU, we studied retrospectively predictors of hypothermia and in combination, in the same patients we studied prospectively their outcome measured as ICU length of stay and hospital mortality. In the first version of the manuscript we wrote that we have evaluated the incidence of core hypothermia. We think the correct term is prevalence because we measured the number of individuals with a given temperature at a given point divided by the population at risk not the number of new cases occurred during a given interval of time divided by the population at risk at the beginning of the time interval and so we changed it.

About Minor Essential Revisions:

1. We have removed the reference about that old paper and now we wrote: “The consequences of shivering include an increase in cardiac and systemic energy demand, raising oxygen consumption and carbon dioxide production and an increased cardiac

2. We think that is yet formulated in the point 1 About major Compulsory Revisions.

2. In discussion: “A limitation of our protocol is that core temperatures were estimated from the aural canal using an infrared thermometer. Infrared measurements may introduce a degree of variability that could be avoided with carefully positioned thermocouples. However, previous observations state that infrared thermometers are very accurate for determining a patient’s temperature when used by those who routinely perform thermometry in hospitalized patients and they can provide accurate estimative of core body temperature.”

3. and 4. We have made those corrections

5. we tried to improve our written English and we tried to put a more clear order in our discussion.

6. We have made that correction.
7. In discussion: “…we concluded that anesthesia lasting longer than 3 hours was a predictor of core hypothermia with statistical significance. If patients undergoing major procedures with long lasting anesthesia became hypothermic more often, they were probably not warmed actively as we could suspect from the active warming rate and rate of temperature monitoring of this set of patients.”

7. In discussion “We concluded that the amount of intravenous intraoperative crystalloids was a significant risk factor for developing hypothermia on arrival on ICU and that was already identified in previous studies. In fact the infusion of crystalloid solutions at room temperature may significantly contribute to intraoperative hypothermia because warming fluids to core temperature requires body heat. Several studies had demonstrated that infusion of warmed fluids helps in the prevention of hypothermia and reduces the incidence of postoperative shivering. A limitation of our protocol was the absence of data about how often had been used warmed intraoperative fluids and what methods were used to warm them.”

8. We provide the citation about it.

We hope that these revisions adequately address the concerns of the reviewer and of the editorial team.

With best regards
Response to comments made by Dr Michael Diringer:

Thank you for your valuable comments regarding the manuscript. The manuscript has been revised according to the comments made and we have responded point-by-point to the specific comments in the section below:

About major Compulsory Revisions

1. All variables that were entered in the model are now listed in the results and we explain our methodology in methods. We have reconstructed the model in the case of the logistic regression model for ICU length of stay and hospital mortality. In results section we list the variables when we refer the model:

In methods: “A multiple regression binary logistic with forward conditional elimination was used to examine covariate effects of each factor on core hypothermia, ICU LOS and hospital mortality and calculate OR and their 95% CI. Covariates with a univariate p<0.1 in the respective univariate analysis were entered in these models. In the model for ICU LOS and hospital mortality the categorical variable temperature at admission was also entered.”

Table 3,4 and 6 were modified and we include in legend the variables entered in the models.

2. We extensively edited and modified the discussion and made more interpretation of results. We made a suggestion for interventions and for a future study in this particular field: “Improved awareness about perioperative hypothermia and its complications with a strictly adherence to the recommendations about temperature monitoring and thermal management guidelines should play a role in decreasing adverse outcome. It may be
advantageous to take steps in the way to decrease prevalence of hypothermia and conduct a study to validate that hypothesis.”

3. We made an effort and tried to improve our written English.

About Minor Essential Revisions:

1. In the discussion: “Although the objective of this study was exploring preadmission clinical factors to hypothermia, we included SAPS in the list of predictive values because this way we could have a measure of severity of illness in the patients admitted to ICU. Measured after admission to ICU, this score reflects not only the pre admission physiologic alterations but also variables like age and co morbidities that were not altered with admission. Only with this premises should the predictive value of SAPS II be evaluated.”

2. In the discussion: “Only noncardiac surgical patients were admitted to the study. Since none of them was submitted to neurosurgical procedures, hypothermia as a therapeutic tool had not been applied.”

We hope that these revisions adequately address the concerns of the reviewer and of the editorial team.

With best regards