Reviewer’s report

Title: Enteral nutrition in the critically ill child with shock: a prospective observational study

Version: 1 Date: 13 November 2007

Reviewer: Sirak Petros

Reviewer’s report:

General
In this study, López-Herce et al. present their data on the efficacy and tolerance of enteral nutrition in the critically ill child with shock. Although similar studies have been reported in adults, data on critically ill children in shock are few.

Major Compulsory Revisions (that the author must respond to before a decision on publication can be reached)

1. In general, there are a number of mistakes in wordings and sentence formulations, which should be edited appropriately.

2. There are a number of similarities regarding the data in the present paper and a paper published by the same authors recently (Lopez-Herce J et al. Risk factors for gastrointestinal complications in critically ill children with transpyloric enteral nutrition. Eur J Clin Nutr. 2007, Feb 28 ; [Epub ahead of print]). The authors should explain the need to present this paper, since they have already reported the relationship between shock and tolerance to enteral nutrition in their publication mentioned above, which, taking the figures reported, must have involved the same patient population.

3. Abstract: This should be better formulated so that the reader can catch the whole picture. For instance, the total number of study patients (those with and without shock) is not mentioned. There are also no data on the rate of complication of TEN in the control group.

4. Background: Page 4, lines 7-10: There is no conclusive evidence that transpyloric nutrition would lead to a reduction in the risk of pulmonary aspiration. This is reported in recent studies (Esparza J et al. Intensive Care Med 2001; Montejo JC et al. Crit Care Med 2002; Neumann DA et al. Crit Care Med 2002). However, transpyloric nutrition is generally associated with higher success rates in volume of feed than gastric nutrition in critically ill patients. The authors should consider rewriting their statement in order to avoid misunderstanding. Furthermore, the statement that enteral nutrition is generally not administered in patients with shock is not correct. The point is whether one aims to administer the total caloric supply via the enteral route or whether one would limit the enteral feed volume to an amount just enough to keep the bowel active.
5. Patients and Methods, Page 5, first lines:

- What guideline was the basis to use such a definition of shock? Generally, every patient requiring catecholamine infusion to stabilise blood pressure despite adequate volume load is considered as being in shock.

- Why did the authors restrict volume load to 20 ml/kg? There may have been patients who lost more than just 20 ml/kg. This figure seems low considering the large body surface area of a child. Taking into consideration that the majority of patients were suffering from cardiac conditions requiring surgery, volume administration might not have been easy. However, it is not clear from the description whether every patient under dopamine infusion received that drug for shock or just to augment cardiac function, which should not necessarily be shock. Or did the authors mean therapy-refractory shock? Therefore, the authors must elaborate their definition of shock and the background for such a definition. Since their definition of shock is the crucial background of the study, a clear scientifically based methodological definition is a requirement.

- Which blood pressure figure do the authors mean? Systolic BP or mean arterial BP?

- How many hours after the admission to the ICU was enteral nutrition started? This may have an impact on the complication rate, particularly if enteral nutrition is started late.

- Trying to achieve a normocaloric feeding via the enteral route is often difficult in the critically ill. Furthermore, there is no evidence that doing so may have any benefit in terms of outcome. Literature data show that rushing enteral nutrition in the first days may increase the rate of regurgitation, abdominal distension and diarrhea. Therefore, it is not clear why the authors increased the volume of feed relatively faster.

- Page 6: The authors state that they documented altered liver function based on elevation of ASAT, ALAT and bilirubin. However, there is no clear evidence to consider these lab data as signs of altered liver function.

6. Results:

- Generally, the authors should refrain from repeating data in the text that are already provided in the tables.

- The authors have stated in their methodology that shock definition was based on blood pressure results. However, they did not provide blood pressure figures of their patients.

- Page 7/8: Since this is supposed to have been a prospective study, it is not clear why parenteral nutrition was started in more than a fifth of the shock patients. Was PN started as a bridging until EN volume reached target? The authors should elaborate on this aspect.

- Page 8: Regarding complication rate in relation to early and late administration of EN, the authors did not mention their definition of early versus late. Furthermore, the number of patients to analyse is low to claim any statistical
significance or insignificance.

7. Discussion:
- Page 9, last paragraph: The authors claim that they did not find any difference regarding EN tolerance between post cardiac surgery patients and others. However, they did not provide any data in the section of results.
- Page 10, paragraph 2: increasing cardiac output with catecholamines does not necessarily correlate with improvement in splanchnic perfusion. There are studies that have demonstrated that splanchnic perfusion deteriorates under epinephrine, although cardiac output is increased (Sakka SG at al., 2007; Jrejci V et al. 2006). There are also differences between epinephrine and norepinephrine regarding their effect on the splanchnic system (Nygren A et al. 2006; Guerin JP et al. 2005; Meier-Hellmann A et al. 2000).
- Page 12: The first paragraph contains data already presented in the previous section; thus the authors should avoid repetitions.

8. Conclusions: 5th line: What do the authors mean with “excessive rest” regarding GI complications?

--------------------------------------------------------------------------

Minor Essential Revisions (such as missing labels on figures, or the wrong use of a term, which the author can be trusted to correct)
There are a number of spelling errors that should be addressed by the authors.
--------------------------------------------------------------------------

Discretionary Revisions (which the author can choose to ignore)

What next?: Reject as not sufficiently sound

Level of interest: Reject as not of sufficient priority to merit publishing in this journal

Quality of written English: Needs some language corrections before being published

Statistical review: No, the manuscript does not need to be seen by a statistician.