Reviewer’s report

Title: Phagocytic ability of neutrophils and monocytes in neonates.

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Reviewer: Pascal M Lavoie

Reviewer’s report:

The authors present interesting data about a comparison of phagocytic ability of neutrophils and monocytes between neonates and adult on whole blood using a vitro assay. The assay (Phagotest) has been described before although its clinical relevance with the increased risk of infection in infants is unclear in absence of independent validation of their data with other bacteria. Also, they are some other major issues with how the data is presented that makes me unsure of the validity of the results (see below).

Major revisions:

1. The authors raise several ambiguities and contradictions from previous studies looking at phagocytic activities in innate immune cells due to difference in techniques used, but do not necessarily clarify those ambiguities in their study. For example, they write that one of the main novel features of their findings is the demonstration that the defect in phagocytosis is due to a reduction of receptors involved in bacterial (E.coli) intake (page 10, top paragraph). However, this is not supported adequately by any data more specifically measuring receptor expression or function. It would help put some flesh to this manuscript if they had attempted to identify which receptors are responsible for the differences observed using inhibition experiments (using receptor antibodies, etc.).

2. In order to be able to interpret the data in figure 1 and table 2, the authors should include negative controls (i.e. phagocytes left on ice) or at least clarify whether they subtracted the % obtain in negative controls from the experimental samples, as well as give the reader an idea of the sample viability and mean fluorescence index obtained in the FITC channel for both experimental and negative control conditions.

3. Were duplicates/triplicates used in the experiments? CV between replicates should be presented in the methods in order to help clarify whether the difference they observed between individuals is beyond experimental variability (hence detection level) of their assay.

4. In 2010, the use of FSC/SSC parameters in flow cytometry to define monocyte and neutrophil population is largely suboptimal and should probably include co-staining with more specific surface markers. This is especially true for identification of neutrophils which fall into other granulocytes’ gating parameter.

5. The manuscript would benefit from English editing.
Minor revisions:
1. The authors mention in their introduction that the risk of infection in preterm infants is as high as 0.4%. It would be relevant to clarify that this figure only considers early-onset infections as the risk of late-onset and nosocomial infection in the neonatal period can reach much higher proportions.
2. Several sentences in the manuscript need better referencing. Ex: Page 4, first two sentences,
3. How preterm were the preterm samples?
4. Lack of differences in gender and mode of delivery could be more simply presented in text (i.e. to say that no differences were observed at p>?).

Level of interest: An article of limited interest

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.