Reviewer's report

Title: Phagocytic ability of neutrophils and monocytes in neonates.

Version: 1  Date: 19 November 2010

Reviewer: Morgan R Peltier

Reviewer's report:

This is a well-put together study that made great use of the clinical samples available. I have some suggestions with the manner in which the data were analyzed and presented that I hope the authors will find helpful. Although I have written much, none of these criticisms are likely to affect the validity of the author's conclusions. Overall, this paper makes a solid contribution to the field. Please consider the following suggestions...

Major compulsory revisions: None

Minor Essential Revisions:

The manuscript is readable but there are many grammar errors throughout that must be corrected before the paper is published.

BACKGROUND:

I think the first 2 paragraphs of this section plus the sentence just before the methods are sufficient for this section. The remaining material should be moved or reserved for the discussion.

RESULTS

For Table 1: Show whether or not there are any statistical differences between term and preterm neonates. Proportions need to be compared with Fisher's exact test.

Please present the data shown in Table 2 as a 4-panel figure and indicate all three of the comparisons made (Neonatal vs. Day3, Day3 vs. Adult, Neonatal vs. adults).

Figure 1: Needs to have error bars onto the plots. Authors also need to annotate the figures to indicate which differences are statistically significant. I would also put monocytes and neutrophils into separate panels. Some things that are clearly not significant such as delivery mode or gender may be better off only mentioned in the text of the results section (with P-values).

P-values need to be given after every result brought up in this section.

Please present the results of the Spearman rank correlation as a scatter plot figure with a line fit to the data. You also need to ensure that no outliers are present.
DISCUSSION:
The authors should account for some of the other factors that changed between birth and day 3 that may account for the observed differences in neutrophil and monocyte numbers and function. For example breastfeeding, weight loss, progesterone declining and the exposure to bacteria.

The authors state that the clinical relevance of their findings are unclear, but I think they suggest a biological mechanism by which neonates may be at greater risk for systemic infections than adults and for which preterm infants may be at sill greater risks. They may wish to elaborate on this more.

The paragraph on how there are likely to be differences between pathogens and the review of the different toll-like receptors seems out of place in the manuscript since only E. coli was studied and TLRs were not measured on the cells.

Discretionary Revisions:
METHODS: The authors really should consider consulting with a biostatistician who may have better mathematical tools for analyzing this dataset. The methods used seem a bit over simplistic and may not be appropriate for some of the factorial aspects of their study design. Also, since some of the data is working with percentages (which are not normally distributed), the authors should arcsine transform their data prior to analysis.

The authors should consider using a statistical model that takes into account potential effects of gender and its interactions with group (effects of gender may be different between neonates and adults). It’s alright to lump males and females together if you have shown that there is no difference with at least 80% power. By including these factors in the model, however, you may increase the power of your analysis to detect smaller differences between age groups and time points. Since the design includes repeated measures, the responses are best analyzed using a generalized estimating equation. After fitting the model, estimate fitted means (which will be adjusted for effects of gender) and compare day 0 with day 3 and with each other and with adult cells after using contrasts. For analyses confined to the neonates you need a similar model that includes term and preterm in the model or gestational age at delivery as a variable in the model. I realize, however, that given the small sample size that it may not be possible to do all of these things. In that case the approach that the authors have taken of lumping different delivery methods, and genders together for paired and unpaired t-tests maybe justifiable.

Level of interest: An article whose findings are important to those with closely related research interests

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

Statistical review: Yes, and I have assessed the statistics in my report.
Declaration of competing interests:

I have no competing interests