Reviewer’s report

Title: Phagocytic ability of neutrophils and monocytes in neonates.

Version: 1 Date: 17 November 2010

Reviewer: Thorsten Orlikowsky

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Dear editors,

the manuscript „Phagocytic ability of neutrophils and monocytes in neonates“ of A. Filias and coworkers addresses the question if an impairment of phagocytic capacity of neutrophils and monocytes is attributed to the outcome of sepsis in newborns. The results of this study stress emphasis to the fact that the phagocytic capacity of neonatal monocytes is not affected when compared to adults and describe a reduced phagocytic capacity of neonatal neutrophils. Even though the phagocytic capacity of neutrophils is significantly reduced at the time of delivery it increases within 3 days post delivery and becomes indistinguishable to the phagocytic capacity of the adult counterparts.

As I do not feel adequately qualified to assess the statistics the manuscript should be seen by a statistician.

In general, the present study presents clear evidence that the phagocytic capacity of monocytes and neutrophils of neonates may not contribute to the course of bacterial infection. The readout assay selected for this experimental approach is well described and suitable to assess the phagocytic capacity. On the other hand, the authors clearly point out the limitations of the assay with regard to the dependence of opsonisation and the expression level of several receptors (FcRII, CR3 and TLRs) on the surface of neonatal phagocytes. The results are thoroughly discussed and compared to the findings of other groups working in this field. The experimental conception of the present study is advantageous for the field, because the authors monitor the phagocytic ability over an interval of three days (day of delivery up to three days after birth). Additionally, the study encompasses data from preterm and in-term neonates and discriminates between vaginal and caeserian delivery and sex. Therefore, the findings presented in this manuscript are important to those with closely related research interests.

However, some phrasings in the discussion are distressing (“The clinical relevance of our findings is unclear” p.10). Moreover, the point that bacteria other than E.coli may induce alternative pathways of binding, uptake and killing reserves to much space in the discussion section since the experimental setup utilizes E.coli only. In my opinion the authors should focus more on post-phagocytic events (i.e. ROS production, phagocyte-induced-cell-death, inflammatory responses) which are likely to contribute to the outcome of E.coli infections.
The charts should show the phagocytic capacity of adult neutrophils and monocytes categorized in a comparable way as shown for the neonatal counterparts.

In conclusion, the manuscript should be accepted after minor essential revisions.

Yours sincerely

**Level of interest:** An article of importance in its field

**Quality of written English:** Acceptable

**Statistical review:** Yes, but I do not feel adequately qualified to assess the statistics.