Reviewer’s report

Title: Modulation of innate immune responses at birth by prenatal malaria exposure and association with malaria risk during the first year of life

Version: 0 Date: 04 May 2018

Reviewer: Göte Swedberg

Reviewer's report:

This work is quite impressive, and stands out by including large enough numbers of subjects to make the results significant. However, some of the groups including acute PM and non-exposed are small in comparison with others. What strikes me as a bit odd is that the group "Past PM vs non exposed" stand stands out by showing significant differences for all cytokines, while the results for other groups are more varied. Could this be a result of the differences in numbers of voters in each group? You may want to comment more on the effect on the various numbers of individuals included in the analyses and the consequences for the significance on the results. Another factor to consider that is not mentioned clearly is the effect on IPT and any treatment during pregnancy for the outcome. It may or may not influence the results, but I think it may be worth mentioning how many were in the CSST/IPT arm and how many in the IPT only arm. Also, how many were actually treated for malaria during pregnancy? One more point is any practical implications on the findings here in relation to the recent paper in Journal of Infectious Diseases describing the impact on screening and treatment.

Are the methods appropriate and well described?
If not, please specify what is required in your comments to the authors.

Yes

Does the work include the necessary controls?
If not, please specify which controls are required in your comments to the authors.

Yes

Are the conclusions drawn adequately supported by the data shown?
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Yes
Are you able to assess any statistics in the manuscript or would you recommend an additional statistical review?
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I recommend additional statistical review

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Please indicate the quality of language in the manuscript:

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