Reviewer's report

Title: The impact of maternal HIV infection on cord blood lymphocyte subsets and cytokine profile in exposed non-infected newborns

Version: 1 Date: 26 August 2010

Reviewer: Florence Buseyne

Reviewer's report:

1. Is the question posed by the authors well defined? Yes
2. Are the methods appropriate and well described? Methods are appropriate, but their description is incomplete
3. Are the data sound? Yes
4. Does the manuscript adhere to the relevant standards for reporting and data deposition? Yes
5. Are the discussion and conclusions well balanced and adequately supported by the data? Discussion needs major changes
6. Are limitations of the work clearly stated? No, infected and uninfected mothers differed according to the mode of delivery, smoking and drug abuse. This point should be restated in the discussion section.
7. Do the authors clearly acknowledge any work upon which they are building, both published and unpublished? Yes
8. Do the title and abstract accurately convey what has been found? Yes
9. Is the writing acceptable? Yes

Reviewer's report

General comment

Borges-Almeida and co-workers address the important issue of immune alterations in uninfected newborns of HIV-infected mothers. Their results are new, interesting and quiet clearly presented. Most of my comments relate to the discussion section that is too general and does not address the points directly related to results. Improving this last part of the manuscript will increase the relevance of the whole work.

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

1/Methods section, T cell proliferation paragraph: I do not understand the last phrase, neither the meaning of taking PHA as a reference as it was the single stimulus “Proliferation was measured by CD3+ percent blasts, in which baseline proliferation was PHA”.
2/Tables 6, 7 and 8: Definitions of mature, intermediate and immature cells are lacking. The text states that CD45/CD34/CD19/CD22 combination is most useful to define B cells alterations. This combination is not the most frequently used to define immature B cells. The use of several markers to define B cell differentiation deserves a paragraph in the discussion section.

3/Discussion section, paragraph 1: The relationship between high maternal serum TNF-alpha level and low birth weight is interesting but is not discussed. Has it been previously described in the context of HIV infection, other diseases, drug use or prematurity?

The relationship between duration of maternal HAART and birth weight deserves more than a single phrase that is a replication of the results.

4/Discussion section, paragraph 2: The authors wrote that alterations can be explained by maternal HAART treatment and placental drug transfer. However, their data do not support the effect of HAART, as children haematological parameters were not related to the duration of ART. As mother and children had similar haematological alterations, it would be of interest to investigate whether maternal and infants' alterations are correlated.

5/Discussion section, paragraphs 3 to 7 regarding CD5+ B cells: Reading of these paragraphs is frustrating as there are many general statements and few comments directly related to data. As an example, there is no comment on the relationships between the % of CD19+CD5+ B cells and IL-7 and IL4- production by PBMCs, which is an important result of the study.

6/Discussion section, paragraph 8: The discussion regarding CD8 activation is irrelevant in this paper, as CD8 activation was not assessed in the study.

Minor Essential Revisions (such as missing labels on figures, or the wrong use of a term, which the author can be trusted to correct)

7/Abbreviations used should be defined, as an example haematological abbreviation used in table 2 and 3.

8/Tables and figures can be simplified:
Table 3 and 4 can be fused in a single one.
Tables 6-7-8 can be fused in a single one.
Table 9 and 10 are mostly a repetition of figures 1 and 2, a choice has to be made.

Discretionary Revisions (which are recommendations for improvement but which the author can choose to ignore)
None

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

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

Declaration of competing interests:

I declare that I have no competing interests.