Reviewer’s report

Title: Birth-weight, insulin levels, and HOMA-IR in newborns at term.

Version: 1 Date: 25 November 2011

Reviewer: Deborah Ehrenthal

Reviewer’s report:

Major Compulsory Revisions:

This reports a small study of the association of insulin and HOMA IR levels at birth with birthweight groupings. This is an interesting and important area for research. The author’s use of a homogenous population adds to its value. It seems to confirm earlier observations.

1. The authors state that insulin secretion is reduced in type 2 DM. Impaired might be a more accurate characterization.


3. Revisions recommended to the methods:
   • Specify what defined a “healthy mother”.
   • Describe if glucose tolerance testing is a part of routine care in the community—were the women tested?
   • Please define for the reader the “HOMA-IR index” and why you used it.
   • Do you have mother’s height so you can calculate BMI?
   • Your discussion mentions “matching” the mothers but I did not see this described in your methods.
   • You selected only babies at term and classified them as LBW, HWB and NBW. How was gestational age determined? Did you consider classifying them as SGA/AGA/LGA?

4. Revisions recommended to the results:
   • The authors should consider including the data they used to develop the cut-offs for the HOMA IR unless it has been previously published.
   • Please show tests of significance from the ANOVA that make it clear where there are differences in the insulin levels with the NBW infants for each of the LBW and HBW groups (there is a p-value that the post-hoc Bonferroni will give you when comparing the different groups)
   • You include an OR but do not explain if it is the results from a bivariate or multivariable logistic regression. What else did you include in your model? Was it only maternal prepregnancy weight? Any infant characteristics such as sex?
• The point estimate for the OR for the association of HOMA IR was 1.9, which was not significant likely due to the small sample size. I would be careful stating that there was no association, rather that you were unable to adequately test that due to the small sample size.

5. Revisions recommended to the discussion:
• The authors should discuss their findings in the context of other studies of newborns, rather than to studies of children and adolescents. It is fine to draw that connection later, but they are not comparable to their study.
• More caution in their statement about the absence of association with HOMA IR and LBW is warranted given their lack of power.
• Be careful with the speculations about the placental mechanism and provide more detailed rationale if you plan to include them.

Level of interest: An article of importance in its field

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 to declare.