Reviewer’s report

Title: Assessing fetal growth impairments based on family data - A tool to identify babies at risk?

Version: 1 Date: 1 August 2007

Reviewer: Russell Kirby

Reviewer’s report:

General Comments on ‘Assessing fetal growth impairments based on family data – a tool to identify babies at risk’

The authors should clearly specify what is meant by ‘risk’ in the title and ‘mortality’ in the abstract and introduction. The paper appears to focus on neonatal mortality risk, so why not say this explicitly?

On the title page, Dr. Olsen’s affiliation is incorrectly indicated as being in Louisiana, the correct state abbreviation is ‘CA’. Dr. Basso’s state abbreviation could be more correctly indicated as ‘NC’.

Abstract:

In the first sentence, clarify what is meant by ‘mortality’ and ‘other health conditions’? Do the authors mean to imply across the lifespan, or explicitly associate low birth weight with infant health? Do the authors have an explicit research hypothesis? If so perhaps this might be stated in the abstract . . . ‘We hypothesized that . . . etc’.

The methods section is fairly sparse considering that the methodology for estimating expected birth weight is relatively novel. The results could include some more specific quantitative findings. At present this information is presented in cursory fashion.

Background:

Reference 1 deals with perinatal and infant mortality, but the first sentence implies more. If the authors wish to imply broader associations of low birth weight with child or adult mortality, these assertions can and should be documented with other references.

Consider referencing other material on approaches to estimating optimal birth weight and deviation from optimal birth weight. This reviewer is traveling presently and doesn’t have full access, but recalls an interesting paper from Australia two or three years ago on this topic, also in a BMC journal (either Pregnancy and Childbirth or Pediatrics).

Do the authors have any explicit hypotheses – if so, the last paragraph of the background section is the best place to state these. The last phrase of the last sentence of the background, “which was calculated . . . ’ is not necessary, as this...
is discussed in considerably more detail in the methods.

Methods:
The dataset is well described. The authors might justify the choice of 100g as opposed to 50g rounding for birth weight. Presumably the data from 1997 on were converted to completed weeks of gestational age to be consistent with the earlier data. For references to SAS procedures, unless there is something specific in the documentation it is not necessary to cite the specific procedure. A general reference to the version of SAS used, in the body of the text, should be sufficient. Somewhere in the methods some details on how expected birth weight was calculated should be discussed. Instead some of this material is included in the first paragraph of the results section.

Results:
The equation at top of p 10 should be expressed in conventional mathematical form, using beta-naught for intercept, beta-one for slope, etc.

In the discussion of neonatal mortality results, do the models take year of birth into account? Given that mortality risks improved considerably over the lengthy study period but improvement varied across the gestational age distribution this might be important. Also, the analysis includes only sibships of first and second born infants, neither of which was a stillbirth? This appears to be the case from reading the manuscript, but could be stated more clearly in the methods.

Do not interpret your results in the results section – merely present them here (ie second para on p 10 comparing fit of Danish and Norwegian models).

Figures 1 and 2 – spell out 1.st and 2.nd – it’s only a few more keystrokes and there is plenty of room!

Figure 3 – include a note indicating what the bars at each data point represent – presumably these are 95%CIs? But this should be stated clearly.

Discussion:
The discussion would be strengthened by reference to a specific hypothesis or (es) in the first paragraph, followed by interpretation of this study results in relation to the hypothesis.

The decision re not adjusting birth weight for year might make sense, but mortality is an entirely different issue. In the limitations, some discussion of generalizability re stillbirths, multiple births, etc should be included. What about the role of maternal morbidity associated with a specific pregnancy (either first or second)?

How much additional information is provided based on the methods presented in this paper? Is it clinically significant and useful for obstetrical practice?

Are the authors aware of a recent paper by Boulet et al (Am J Obstet Gynecol 2006) which also examines mortality risk in relation to birth weight and
gestational age (but not among sibships)?

-------------------------------------------------------------------------------

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

The authors should make an explicit statement of hypothesis(es), then test these and discuss. The authors should also make a frank and candid assessment of the contribution of their work to the field.

-------------------------------------------------------------------------------

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

The authors should also address and revise the manuscript as suggested in the general comments above.

-------------------------------------------------------------------------------

Discretionary Revisions (which the author can choose to ignore)

What next?: Accept after minor essential revisions

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 declare that I have no competing interests.