Reviewer’s report

Title: Genetic evidence of multiple loci in dystocia - difficult labour

Version: 3 Date: 30 December 2009

Reviewer: Héctor Lamadrid-Figueroa

Reviewer’s report:

The authors have only superficially and partially addressed the major issues raised by this reviewer.

1. No new information relative to the selection process of the 107 healthy patient control group is provided in the new version of the manuscript other than an isolated phrase “Cases and controls were not matched”, and the inclusion of a descriptive statistics table.

   It seems to be rather odd since the description of the case sample is very detailed. There are several possibilities for the selection of the controls: Was it a random sample of subjects from the Swedish Multigeneration Registry? Was it a sample comprised of unaffected relatives of the cases? Were there unaffected women in the same hospital as the cases? All this possibilities relate to different potential biases that need to be discussed in the manuscript.

   Regardless of the control selection method, the descriptive statistics shown in table 3 c are worrying. According to this table, mean age of the controls was 28.4 years with a standard deviation of 4.7 years. Compare this with 30.3 years (S.D. 4.8 years) of the 104 case sample. Performing a t-test to test the hypothesis of difference of the means with these data yields a p-value of 0.0076 which is very statistically significant. Even more worrisome is the difference in the Body Mass Index (22.7 vs. 27.4 kg/m2), where the t-test yields a p<0.0001! Birth weight is no joke either, yielding a p-value of p<0.0001 as well. I was unable to perform other comparisons since the authors inexplicably omitted time of labor and the standard deviation of gestational length. However, I think the point here is that this control group has serious comparability issues. I am not suggesting that these particular variables are confounders, that is a discussion that needs to be made by the authors, but since every variable presented in the descriptive statistics is significantly different in cases and controls, the validity of any hypothesis test performed using this control group is seriously challenged.

   If the authors cannot provide a reasonable explanation for this lack of comparability and how it does not imply the presence of bias (if in fact it does not) I would suggest excluding all results involving the control group from the manuscript.

2. Major compulsory revision number three was not addressed at all by the authors.
Level of interest: An article of importance in its field

Quality of written English: Needs some language corrections before being published

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.