Reviewer's report

Title: Elevated risk of stillbirth in males: systematic review and meta-analysis of more than 30 million births

Version: 1 Date: 25 July 2014

Reviewer: Dagfinn Aune

Reviewer's report:

This is an interesting meta-analysis of >30 million births comparing stillbirth rates among males and females. The study found a 10% higher relative risk among males compared to females. The review/meta-analysis appears to be well-conducted and I mostly have minor comments/suggestions that should be clarified or corrected.

Why was 1990 used as the start date of the literature search?

Page 7, statistical analysis: inverse variance weighting refers to a fixed effects model, while a random effects model also has an added variance component which makes the weighting of different-sized studies more equal when there is heterogeneity.

Random effects models are not always conservative, for example when smaller studies find stronger associations than larger studies, an exaggerated summary effect estimate may arise. I suggest to delete this sentence.

Did the authors make any attempts to test statistically for heterogeneity between subgroups with meta-regression analyses? Could the authors add a supplementary table with all the subgroup analyses, heterogeneity statistics, and number of studies in each subgroup? Did the authors conduct any subgroup analyses by the methodological quality of the studies? It was mentioned in the methods section that the methodological quality was assessed, but I didn’t see any mention of this later in the manuscript (unless I have missed it).

Page 9, line 6: text size appears to be slightly smaller than in the preceding sections.

I’m wondering how meaningful it is to calculate attributable fractions (proportion of cases that can be prevented by removing or reducing the risk factor) by sex. I guess most would see gender as a non-modifiable risk factor and therefore it’s not possible to change, although it may be possible to intervene on modifiable risk factors that may put males at higher stillbirth risk. Please clarify. For this reason I’m also wondering if it’s appropriate to compare the attributable fractions of gender with those of smoking.

Page 12, last section: it is stated that when the gestational cut-off for the definition of stillbirths is placed early (20+ weeks) there is stronger evidence of
increased stillbirths in males (summary RR=1.11, 1.08-1.13), but on page 11 it was stated that the summary RR was 1.16 (95% CI: 1.05-1.29) when the gestational cut-off point was 28+ weeks, which is slightly stronger than for 20+ weeks (although the CIs overlap and probably there is no between subgroup heterogeneity).

Would it be possible to use the name of the first author and publication year as identifiers in the figures instead of the geographic location?

Page 13: For the sentence “Indeed our results may be conservative as the meta-analysis is based on crude rates and not based on adjusted rates for potential confounders” – perhaps add that the adjusted estimates were slightly stronger than the crude estimates. Often adjustment for confounders attenuates rather than strengthens the risk estimates and without this info I first thought it was an error.

Quality of written English: Acceptable

Statistical review: Yes, and I have assessed the statistics in my report.

Declaration of competing interests: I declare that I have no competing interests