Reviewer’s report

Title: Maternal and perinatal factors associated with hospitalised infectious mononucleosis in children, adolescents and young adults: record linkage study

Version: 2 Date: 21 October 2010

Reviewer: Sally Glaser

Reviewer’s report:

The authors have been quite responsive to the critiques, and their paper is improved and more up-to-date in its consideration of the relevant literature. However, I remain concerned that the authors should make it clear throughout the paper that they are addressing a small subset of all IM cases, namely those severe enough to involve hospitalization. The paper still does not describe what the underlying pathogenesis and risk factors may be for clinically severe IM, and whether there is any indication that this reflects a different process than “regular” IM. Therefore I think it is potentially misleading to make comments about “IM” without the modifier “hospitalized”.

Specific comments follow.

Abstract

For the Background, I suggest some rewording:

Background line 2: “Infectious mononucleosis can follow…
Background line 3: “…and has been shown to increase risk for multiple sclerosis…

Background

1. Page 4, line 3: Primary EBV infection occurs in infancy and childhood throughout the world, not just in industrialized countries.
2. Page 4, end of first paragraph: Perhaps it would be relevant to mention the risk factors identified by reference 4.
3. Page 4, paragraph 2, line 2: EBV was found in the Burkitt’s lymphoma cells themselves.
4. Page 4, paragraph 2, line 6: In discussing risk, it isn’t necessary to say “subsequent”.
5. Page 5, paragraph 1, lines 4-6: Is the quote from Purtilo and Sakamoto directed to the pregnant women themselves rather than their offspring?

Results

1. Page 9, paragraph 1, line 12: I don’t see information about twins and singletons in Table 3, nor yet information relevant to IM by age at diagnosis.
2. Page 9, paragraph 2: I would think that the multivariate analyses would justify a table, since they are really the findings that matter most.

Discussion

1. Page 11, paragraph 1, lines 4-5: The “face validity” argument is not convincing. The association of IM with MS and HD could itself reflect misclassification of IM.

2. Page 11, paragraph 2, lines 4-: I think we need something more here about clinically severe IM beyond that it represents the “strongest reactions” to infection. What is known about such patients What about their immune function? Are their significant and potentially relevant comorbidities? The work of Hjalgrim et al. (NEJM 2003) does provide support for the idea that persons with clinically severe IM are at risk for HL, since their data are based in part on hospitalized cases.

**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.