Author's response to reviews

Title: Bartonella seropositivity in children with Henoch-Schonlein purpura

Authors:

Joan L Robinson (jr3@ualberta.ca)
Donald W Spady (dspady@ualberta.ca)
Errol Prasad (ERrol.Prasad@dkml.com)
Dorothy McColl (Dorothy_Mccoll@hc-sc.gc.ca)
Harvey Artsob (Harvey_Artsob@hc-sc.gc.ca)

Version: 2 Date: 1 February 2005

Author's response to reviews: see over
February 1, 2005

Editor
BioMed Infectious Diseases

Dear Editor,

We are submitting a revised version of manuscript 1715912815490531. We have addressed the reviewers’ comments as follows:

**Reviewer #1 (Michael Giladi)**

**Major compulsory revisions:**

1. The reviewer has legitimate concerns about interpretation of the IFA test that was used for *Bartonella henselae* serology. Unfortunately, the sensitivity and specificity of the assay that was used have not been definitively determined. However, positive and negative controls were used. We agree that the specificity of the assay may be low, but this would not explain the statistically significant higher seroprevalence in the cases than in the controls. To deal with this issue, we now state in the discussion: “The *B. henselae* seroprevalence rate in controls of 21% and 14% in the current study and the Florida study respectively are higher than in a summary of North American studies where the rate ranged from 2% [11] to 6% [6]. However, the seroprevalence was 37% in adult blood donors and 18% in children with respiratory illnesses in a study done in British Columbia, Canada [12]. It is well recognized that interpretation of the IFA is subjective, and it is probably not valid to compare titers obtained in different laboratories. Because there have been no previous seroprevalence studies in Alberta, it is not clear if our seroprevalence rate is higher than predicted or if the specificity of the IFA is low. However, neither would explain the higher seroprevalence rate in cases than in controls.”

We agree it would have been very helpful to have more clinical data on the controls (especially data on their cat exposure and underlying illnesses) but did not have access to this data.

2. We clarify in “Methods” that the technicians were blinded as to the source of the specimens and that all specimens were run in the same batch.

**Minor compulsory revisions:**

2. We agree that it was incorrect to describe bacillary angiomatosis as a “vasculitic rash”, so have omitted this sentence.

2. We now omit the comment that Ayoub et al used serology rather than other methods to look for *Bartonella henselae*. We agree that our own study suffers from the same shortcoming as only an indirect test was employed to look for evidence of infection, but explain in the discussion that our data does not prove that there is an etiologic relationship between *B. henselae* infection and HSP.
Study population
As suggested, we replaced the term “acute and convalescent sera” with “paired sera”.

Serology
We corrected “formal saline” to read “normal saline”.

Statistics
We explain that a chi-square test was used for the comparisons.

Results
As suggested, data was analyzed by season of the year when the onset of HSP occurred, but there appeared to be no relationship between the month of onset and the seropositivity rate (new Table 2).

Discussion
The discussion was shortened considerably, including changes to the paragraph about PCR.

Reviewer #2 (Abdullah Sakarcan)
Major Compulsory Revisions
2. The reviewer asks why we included so many children with remote HSP rather than just children with current HSP. The first reason is that we were concerned that serology might not detect recent cases, as seroconversion takes an unknown amount of time. The second reason is that from a practical point of view, we thought that it would be difficult to find a large number of children with a current diagnosis of HSP. Very few children with HSP are admitted to hospital, and it is difficult to get pediatricians to think about a study when they see outpatients. It took us about 12 months to recruit patients for the study, and we only managed to enroll six cases of current HSP during that time period.

2. We had failed to define remote HSP. We now clarify: “Patients were considered to have current HSP if onset of initial or recurrent symptoms was less than 42 days prior to enrollment, recent HSP if symptoms started 42 or more days prior to enrollment but had not yet resolved, and remote HSP if symptoms started 42 or more days prior to enrollment and had resolved.”

3. As suggested, we omitted the paragraphs about the incidence of HSP in different climates and the potential role of antibiotics for therapy of HSP.

Thank-you for agreeing to review our manuscript again. We would be happy to consider further revisions.

Yours sincerely

Joan L. Robinson MD