Author’s response to reviews

Title: Bacterial adherence to mucosal epithelium in the upper airways has less significance than believed

Authors:

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Sir!

I am grateful for the comments and suggestions concerning my article “Bacterial adherence to mucosal epithelium in the upper airways has less significance than believed” and I would like to express my sincere thanks to the referees for the constructive criticism. I want to comment on the referee's points as follows:

Reviewer one:

1) I agree that a more detailed description of the techniques is necessary. I have now in the discussion described in more detail what scrape and brush techniques are and how they could influence the results. With the scrape and brush techniques, one might expect that epithelial cells from both the epithelium surface and epithelial cells from the secretion are collected. It is probable that most of the collected cells are obtained from the secretion but there is no proof of that. In any case, what we definitely know is that scrape and brush techniques do collect a huge number of secretion epithelial cells besides an unknown number of epithelial surface cells. That means that adherence to cells collected by brush and scrape techniques does not prove adherence to the surface. I have explained that better in my changes.

2) I think the reviewer is right here. It would have been nice to have some kind of positive control but I guess this is difficult to achieve as one has to get mucosal biopsies where one really can expect to find bacterial adherence. I am not sure that I can find such a mucosa. The reviewer suggests "some sort of tissue" but the only tissue I know of where a biopsy might be positive in this regard is gut (where the bacterial number is almost infinite) and even in the gut it could be hard. Also, the gut mucosa should then be cut out in the same way as I have cut out the tonsils and palate mucosa. Instead of performing new experiments in this study, I have changed the text and have chosen to mention this weakness of the study.

3) I have added arrows and labels to the figures.

4) I have changed the text in the abstract and made the same changes in the text, following the referee's recommendations.

Reviewer 2:

1) The reviewer does not believe that the results go "far beyond" what I have published before. Well, I think there is an important distinction between this study and the mentioned study: In the present study, besides adding new material (specimens), I calculate the probability that my results are really significant, i.e. the frequency of adherence is compared with the risk that the method is inadequate. This calculation, together with the new specimens and also the fact that the focus when examining the specimens in this study was placed on bacterial adherence, makes this a far more interesting...
study concerning the phenomenon of bacterial adherence than the earlier study.

2) I agree that it is questionable that the study shows that bacterial adherence is of low significance for infection. I should have expressed what I meant more clearly: It is the bacterial adherence to the epithelial surface which is of low significance, whilst bacterial adherence to the shed epithelial cells or to other structures in the secretion may still be important. I have changed the text in the discussion, making this clearer and the article better.

With the changes made, I hope the paper will be suitable for publication. Again, I would like to express my thanks to the reviewers, who have really made a very good and objective review.

Yours, sincerely,

Anders Ebenfelt