Author's response to reviews

Title: Caveolin-2 Associates with Intracellular Chlamydial Inclusions Independently of Caveolin-1

Authors:

Wilmore C Webley (wilmore@microbio.umass.edu)
Leonard C Norkin (lnorkin@microbio.umass.edu)
Elizabeth S Stuart (esstuart@microbio.umass.edu)

Version: 3 Date: 14 July 2004

Author's response to reviews: see over
Dear Dr. Borthwick,

We received our manuscript MS: 1411745315335340, entitled “Caveolin-2 Associates with Intracellular Chlamydial Inclusions Independently of Caveolin-1” along with General comments after the first review from one reviewer. We appreciated the reviewer’s general comment stating that “I believe that the authors have adequately addressed the major issue and feel that the manuscript is appropriate for publication in BMC”. The reviewer, however, also listed 3 issues that she suggested the “authors should take into consideration”.

We feel that the first issue concerning the amendment of figure 4 to include a control blot to show that there was enough *Chlamydia* of the correct serovar on the caveolin-2 stained blot is relevant and we have addressed this by adding this blot. The *Chlamydia* stained blot was run at the same time and using the same amount and serovar as the caveolin blot. As can be seen, the *Chlamydia* anti-sera-stained blot has adequate signal, demonstrating that there is definitely no cross-reactivity of the caveolin antibody with the chlamydial EBs (see figure 4).

On the second issue, which dealt with the need for enlarged figures of the colocalization, we feel that this is not a necessity and that the message of the work would not be compromised if this was not done. As the reviewer stated, we do not believe that the enlargements “would in anyway change the interpretation of the data”. In addition, since all the photos were taken at the same resolution (X600), to make this change would require taking a different set of confocal pictures of everything we have presented at a higher resolution. This is a tremendous undertaking and we therefore feel that the editorial staff should make a decision on the acceptance of this manuscript without this suggested change.

Finally, we feel that the intent of our Z-section in figure 2 was to show that caveolin-2 is not associated with the EBs, but is associated with the inclusion membrane. By demonstrating that the caveolin-2 stain disappears as we go deeper into the Z-sections,
while the *Chlamydia* stain remains relatively unchanged, we have demonstrated just that. We do not know the mechanism by which the caveolin proteins are associating with these inclusions and we have revealed that in our previous publications and here in the discussion of this manuscript. Our knowledge of the caveolin proteins as well as our concept of what is happening has led us to believe that the type of staining pattern seen here is consistent with what should occur. The reviewer does not agree and seem to think that we should provide more detailed justification for this staining pattern. Since this type of discussion has the potential to be very lengthy as we attempt to support our working theories, we believe that as in the case of the above, the editorial staff should make a decision about the acceptability of the manuscript for publication without further exploration of this issue, especially in light of the fact that the reviewer does not think this should prevent publication of our manuscript.

Dr Borthwick, we hope that you will find that we have addressed the major issues here and as an extension of my phone conversation with you on July 14th, 2004, have provided the clarification and additional information necessary to meet the criteria for publication in *BMC Infectious Disease*, and look forward to your editorial decision.

Yours Sincerely,

Wilmore Webley, Ph.D.