Author's response to reviews

Title: Chromobacterium haemolyticum-induced Bacteremia in a Healthy Young Man

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Version: 2 Date: 6 August 2013

Author’s response to reviews:

August 3rd, 2013

Philippa Harris
Executive Editor
BMC Infectious Diseases

Dear Dr Harris:

I, along with my coauthors, would like to ask you to re-consider the attached manuscript entitled “Chromobacterium haemolyticum-induced bacteremia in a healthy young man” for publication in BMC Infectious Diseases as a case report. The manuscript ID is 1092497116959462.

We have carefully reviewed the referees’ comments and made the suggested changes to our manuscript. In addition, we have provided point-by-point responses to their comments on the following pages.

We thank you and the reviewers for your thoughtful suggestions and insights, which have enriched the manuscript and produced a more balanced and better account of the research. We hope that the revised manuscript is now suitable for publication in your journal.

Thank you for your consideration. I look forward to hearing from you.
Responses to Reviewer Comments:

Title: Chromobacterium haemolyticum-induced Bacteremia in a Healthy Young Man
Version: 1 Date: 26 June 2013
Reviewer: Charis Marwick
Reviewer's report:
Reviewer's report
Reviewer comment #1. This is an interesting case which highlights the importance of obtaining a microbiological diagnosis in septic patients, as well as identifying an unusual pathogen.

Our response to comment #1. We thank the reviewer for carefully reviewing our manuscript and for the suggestions, which improved the quality of our manuscript. We have revised our manuscript per those suggestions.

Major Compulsory Revisions

Reviewer comment #1. Is this really the first report of human infection with Chromobacterium haemolyticum? In the discussion, the authors report that it has previously been isolated from a clinical sputum sample – is it certain that this was not infection? If so, the authors should clarify the nature of the "clinical" sample. At the end of the discussion, the authors imply that previous cases thought to be C. violaceum might actually have been C. haemolyticum, due to difficulties distinguishing between the two. This should be raised earlier.

Our response to comment #1. We deeply appreciate these meaningful comments. We have added "C. haemolyticum, strain MDA0585T, is a gram-negative bacillus, and has been isolated from a clinical sample" in paragraph 2 of the Discussion. In addition, we have moved the text towards the end of the Discussion to the beginning; it is now the first paragraph of the Discussion.

Reviewer comment #2. Is it really so essential to distinguish between C. violaceum and C. haemolyticum in terms of clinical management? The authors
report that C. haemolyticum has high beta-lactam resistance but a recent report in BMC ID (ref 6 in the current report) suggests that C. violaceum is also hard to treat and requires combination therapy (which would likely include cover for both?). It is still interesting to definitively identify a new pathogen but the point about differences in clinical management is perhaps over-emphasised. If there really is an important difference in management then that needs to be made clearer by spelling out the differences in antibiotic sensitivities. The authors should report any sensitivity data they have for the isolate from this case to clarify this issue.

Our response to comment #2. Thank you for carefully reviewing our manuscript, and thank you for these suggestions. We have added the section, “We have investigated…Table 1” to paragraph 3 of the Discussion. We have also revised Table 1 to clearly differentiate between C. violaceum and C. haemolyticum. In addition, as suggested, the clinical management was perhaps over-emphasized. Therefore, we have changed the word “crucial” to “important” in paragraph 3 of the Discussion section, and have changed the word “essential” to “important” in the Conclusion.

Reviewer comment #3. It needs to be clear earlier whether or not the patient had necrotising fasciitis. I thought he did until the second last paragraph of the whole report. The fact that the pathology was not diagnostic needs to be mentioned in the case presentation and the authors should avoid saying that “the patient was found to have necrotising fasciitis on day 6 based on the LRINEC score” in the discussion. The LRINEC score risk stratifies rather than diagnoses (as correctly applied in the case presentation).

Our response to comment #3. We deeply appreciate these meaningful comments, and we completely agree with your suggestion. We have corrected the second paragraph in the Case Presentation section and the third paragraph in the Discussion section.

Minor Essential Revisions
Reviewer comment #1. The strain is called MDA0585T at the end of the case presentation but MD0585T at the start of the discussion. Please correct this.

Our response to comment #1. Thank you for carefully reviewing our manuscript. We have corrected our manuscript as per the comments.

Reviewer comment #2. A normal reference range for CRP should be given as this varies widely between different clinical settings.

Our response to comment #2. We deeply appreciate these meaningful comments. We have added the reference range.

Reviewer comment #3. The authors should clarify why consent was from the parent rather than the 26 year old patient himself.
Our response to comment #3. We thank you for these suggestions. We apologize for our oversight. We have obtained consent from the patient.

Discretionary Revisions
Reviewer comment #1. Some of the sentence structure is a little unclear. For example, the sentence fourth from last in the case presentation would be clearer if written: “Vitek…and API 20 NE tests identified the organism as C. violaceum.” Otherwise, it sounds like it could have been a mixed infection.

Our response to comment #1. We deeply appreciate these meaningful comments. We have revised the sentence, per your suggestion. Thank you again.

Reviewer’s report:
Title: Chromobacterium haemolyticum-induced Bacteremia in a Healthy Young Man
Version: 1 Date: 28 June 2013
Reviewer: Gavin Barlow

Reviewer’s report:
Reviewer comment #1. This report is interesting but would benefit from being edited by someone with English as their 1st language or with a high level of fluency to iron out some of the quirky grammar, etc.

Our response to comment #1. We thank the reviewer for carefully reviewing our manuscript and for the suggestions, which improved the quality of our manuscript. We have revised our manuscript, per those suggestions.

Major:
Reviewer comment #1. Please provide more detail about the surgery undertaken - What was observed (i.e. did it look like necrotising fasciitis) and was it a simple fasciotomy or was a fasciotomy and debridement undertaken and if so to what extent was soft tissue debrided, etc.

Our response to comment #1. We deeply appreciate these meaningful comments. We added the sentence “The patient was diagnosed with necrotizing fasciitis…and further debridement was avoided” in paragraph 3 of the Case Presentation section.

Reviewer comment #2. Please specifically state how many days of IV antibiotic therapy were given. Also, specifically state how long the gentamicin was continued for (throughout or for shorter period than IV ciprofloxacin) and it would be useful to state the patients weight and whether adequate peak levels of gentamicin were achieved, etc.

Our response to comment #2. We deeply appreciate these meaningful comments. We have added the sentence, “The antibiotics were changed to
intravenous ciprofloxacin (900 mg/day) and gentamicin (400 mg/day [6 mg•kg⁻¹•day⁻¹]). This antimicrobial regimen was continued for 28 days. After that, gentamicin was discontinued. The trough value of gentamicin was 0.3 µg/mL (6.1 µmol/L) on days 8, 10, and 18" in the last paragraph of the Case Presentation section.

Reviewer comment #3. In discussion, please reference the "current acceptable regimens" for necrotising fasciitis.

Our response to comment #3. Thank you for carefully reviewing our manuscript. We have added the reference.

Reviewer comment #4. Please clarify the pathology findings in the main report section; what was the pathology report and how did this correlate with the operative appearances; necrotising fasciitis is predominantly a clinical diagnosis based on presentation and operative appearances, rather than on histopathology.

Our response to comment #4. We thank you for these suggestions; we completely agree with your suggestion. We have added “The pathology of the dorsum of his left foot revealed inflamed skin and soft tissue, with necrosis consistent with necrotizing fasciitis” to paragraph 3 of the Case Presentation section. We have also corrected the second paragraph in the Case Presentation section and the third paragraph in the Discussion section.

Reviewer comment #5. There are rather a lot of authors - did all authors really have an active roll in contributing to the manuscript!? Only those who did should be authors; others who for example looked after the patient, but did not contribute to the report can be acknowledged.

Our response to comment #5. We apologize for the inconvenience. We have deleted the name of the author who only looked after the patient, and have described the author contributions more clearly.