Reviewer's report

Title: Case report: Patient with occult dental abscess presenting with infective endocarditis caused by Brevundimonas vesicularis

Version: 1 Date: 14 October 2006

Reviewer: Jacob Gilad

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General

B. vesicularis (BV thereafter) is a rare human pathogen and therefore new reports of infection with this organism may assist in further elucidating its epidemiology, pathogenesis, diagnosis and treatment. This case report is thus an important contribution. However, there are major concerns that merit clarification / discussion in order to make it publishable.

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Major Compulsory Revisions (that the author must respond to before a decision on publication can be reached)

1. Title + throughout paper – this paper adds two important points to our understanding of BV – (1) being the first case of SBE with BV; (2) a case that demonstrates the therapeutic challenges spanning antibiotic choice, determination of in vitro susceptibility and therapeutic failure in the context of a serious condition caused by a rare organism about which there are few or no data. The fact that the patient had an occult dental abscess is less important because this has been associated with SBE with other organisms and because the contribution of abscess to SBE is speculative. Therefore, the title, abstract and discussion should be revised to focus more on the situation described above rather than the dental abscess. The source of infection is important but is discussed adequately at the beginning of the discussion section.

2. Abstract – therapeutic failure is mentioned in the conclusion but should be communicated to the reader also earlier in the abstract. Moreover, the conclusion regarding optimal regimens is not fully supported by the new data in this paper (see below). This should be revised into a less conclusive statement.

3. p. 3 + table 1 - the authors rightfully review BV infection cases reported this far in the literature. There are two recent case reports that should also be added to the table and other parts of the paper (where relevant):


4. p. 3 + table 1 – reference 11 describes a case cluster in a dialysis unit and cannot be considered community-acquired infection.

5. p.4, lines 1+8 – respiratory symptoms and pulmonary infiltrates are mentioned but we are not told whether they are a part of SBE? Is this a septic complication? Congestive heart failure? How do the authors explain these findings?

6. p. 4, line 12 – should state according to Duke's criteria whether SBE in this case is definite, probable, possible… Findings in this paragraph should better ordered (e.g. major and then minor criteria).

7. p. 4, line 14 – how many blood culture vials were collected and how many of them did grew BV? This should be discussed since BV may be a contaminant and is an even more peculiar finding in a patient who lacks any risk factors.

8. Especially if only one culture vial is positive - were other causes of culture-negative SBE ruled out (e.g. Q fever, Mycoplasma, Bartonella, etc.).
9. p. 4, last para – the author should report the %probability of organism ID in the API system used. Moreover, it is more important to report that the key features of BV were present and consistent with the biochemical gallery ID. These key features may include: orange pigment, weak oxidase activity, motility, no growth on MAC, sugar alkalinity, positive esculin hydrolysis, negative PYR, positive alkaline phosphatase, susceptibility results to vancomycin, polymyxin B and desferrioxamine. Indole production and nitrate reduction are typically negative in BV but listed AS positive in the paper. This should be explained or corrected.

10. p. 5, lines 6-10 – BV usually requires more than 24 hours to grow while CLSI guidelines for Pseudomonas require <24h of incubation. How did the authors overcome this? Did the organism grow well on standard susceptibility media (MHA) or did they test the organism on blood agar?

11. CLSI breakpoints of 2006 rather than 1997 should be used.

12. p. 5, line 10 – the choice of initial therapy warrants clarification. First, cefazolin is hardly an antipseudomonal agent and the author should explain why ceftazidime or cefepime were not chosen as first-line agents. Second, the recommended gentamicin dose, especially in serious infections, in an adult with normal renal function is 5.1 mg/kg that is 350mg/d for a 70kg man rather than 240mg/d. The author should elaborate on the dosing of gentamicin.

13. p. 5, line 11 – it is reasonable indeed to treat a presumed oral source of SBE during iv therapy for SBE, but the switch to A/S is unclear and should be addressed? What additional microbial coverage was sought?

14. p. 5, line 16-18 – after A/S had supposedly failed, it was switched to ceftriaxone, and later to ciprofloxacin because of drug hypersensitivity. Again, the authors should comment on agent choice. This (hypersensitivity) might be a classic indication for aztreonam therapy.

15. p. 6, line 2 – Pseudomonal endocarditis principally requires 6 weeks of combination therapy (e.g. see Mandell's chapter on SBE) . The authors should comment why did they choose a shorter protocol?

16. p. 8, line 3-7 – on the basis of a single case of therapeutic failure, one cannot conclude that disk-diffusion is unsuitable. The author should discuss the technical limitations of performing agar diffusion testing with fastidious organisms such as BV (see comment above) and lack of breakpoints (use of breakpoints for Pseudomonas is merely an assumption). The author should report the zone diameter measured with A/S in their case and clearly need to test this isolate for MIC, at least for A/S and other drugs administered to the patient, in order to elucidate whether the problem lies with disc diffusion method, or with the ability of susceptibility testing of BV to predict clinical outcome in general.

17. p. 8, last para – statement regarding regimen effectiveness is premature. The current case, as opposing to this statement, does not prove that cephalosporin therapy is successful since this patient did not complete a course of cephalosporin therapy and because efficacy of SBE therapy is measured not only by initial improvement but also prevention of relapse. As for quinolones, this is the only case in which they were utilized for BV infection and one cannot issue a therapeutic recommendation based on a single case. At most it might be stated that current evidence and the presented case, favor these agents, but that additional data are needed and close follow-up in order to detect treatment failure is warranted.

Minor Essential Revisions (such as missing labels on figures, or the wrong use of a term, which the author can be trusted to correct)

1. p. 4, line 5 – report patient hemodynamic parameters on admission.

2. p.4, line 6 – WBC count <10,000 is hardly an "elevated count".

3. p. 4, line 10 – please report the TST (PPD) results of this patients.

4. p. 4, line 13 – echocardiographic findings should be more detailed (may be incorporated into legend of Fig 1), to include vegetation size, exact location, cardiac function (LVEF). Is this TTE or TEE? In figure 1, the vegetation should be pointed out with an arrow and normal cardiac structures surrounding it (LA, LV etc.) should be also marked.
5. p. 5, line 4 – present all initial clinical data first, then microbiology data, and then treatment and outcome. The case report section should be organized better.

6. p. 5, line 5 – what did this "serial survey" include?

7. p. 8, line 11 – "standardized" is preferred over "simultaneous".

8. reference 1 is outdated. Refer to 8th edition of this textbook.

9. p. 5, line 14 – a flare-up of fever is noted but no previous sentence describes defervescence.

10. p. 5, line 15 – how do the authors explain sudden femoral-inguinal involvement? Was this further investigated? Were possible complication such as septic arthritis / osteomyelitis / septic embolism ruled out?

11. p. 6, line 1 – make it clearer that gentamicin was discontinued and that cipro was given alone, as in table 1. Were gentamicin serum levels measured?

12. p. 6, line 2 – was a follow-up echocardiogram performed?

13. p. 8, line 1 – authors discuss previous susceptibility data. This sentence should refer the reader to table 2.

14. Table 2 – S, R, N need to be explained in table footnote.

Discretionary Revisions (which the author can choose to ignore)
none

What next?: Unable to decide on acceptance or rejection until the authors have responded to the major compulsory revisions

Level of interest: An article of importance in its field

Quality of written English: Acceptable

Statistical review: No

Declaration of competing interests:
'I declare that I have no competing interests'