Reviewer's report

Title: Reappraisal of the outcome of healthcare-associated and community-acquired bacteramia: a prospective cohort study.

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Reviewer: M D Parkins

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Gentil et al describe a prospective cohort followed in 2006/7 for two months of patients attending one of 15 hospitals in Spain with community acquired, healthcare associated bacteremia (CA-HCA). These authors present data on 192 specific episodes that they detail in their manuscript relative to a cohort of 149 community acquired without specific risks of CA-HCA.

Major Issues

1. The authors present data (which is quite delayed) – specifically when looking at outcomes following a BSI event and are (somehow?) related to a prior work involving BSI published in 2010. This prior manuscript focused more on an epidemiological background, and only briefly addressed factors associated with mortality. It needs to be clarified how the two data sets are separate and where overlaps exist – as the numbers look to be different.

2. The relevance of the data presented now that was collected in 2006 in relation to community epidemics of MRSA and ESBL Enterobactericeae is questionable. The authors describe their data in a “current state” which is not strictly appropriate.

3. The authors conclude that outcomes are no different for CAHCA infections and that criteria need to be revised. This certainly is at odds with other published data and their own findings. With the evident different etiologies, and different treatments provided – it would seem that the HCA-CA condition was recognized in their cohort and compensated for appropriately with individualized management strategies.

4. The inclusion of pneumococcus as a cause of BSI and the definition of inappropriate treatment is somewhat misleading as penicillin remains the drug of choice even in resistant isolates outside of CNS infections. Given the high number of pneumococcal isolates in their data set (particularly in the CA-HCA population) – what is the impact if these are removed?

Minor Issues

1. The authors refer to ESBLs in the manuscript as being a potential factor in observing more resistance in the community and increased risk of inadequate
initial treatment. There data in Table 4 merely reports cefotaxime resistance for the Enterobactericae – are we to presume this to equate to ESBLs?