Reviewer’s report

Title: Community acquired bacterial meningitis in Cuba: a follow up of a decade.

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Reviewer: Annunziata Faustini

Reviewer’s report:

I note that the authors 1) have provided more information about bacterial meningitis (BM) surveillance, characterizing it as an enhanced surveillance which gets continuous complementary information from different sources for any confirmed BM case; 2) have specified the aims of the paper more clearly.

However, I also note that 1) these revisions (see above) have not been taken into account in the discussion; 2) important limitations still affect the methods used in the analysis; 3) some major observations have not been taken into account at all in the revision.

Let us consider these points one at a time.

1) Although surveillance assessment is not one of the authors’ stated aims – indeed they explicitly exclude it in their answer – they do deal with this topic at the beginning of the discussion. Moreover, there is no discussion at all about what sort of influence the introduction of enhanced surveillance might have on the number of reported cases.

2) Analyzing temporal and spatial distribution of BM incidence and mortality should rely on age-standardized rates in order to make comparisons that are adjusted for the differences in age composition of population groups. Apart from the question of the indicator, temporal trends should first be analyzed with an appropriate test for trend on the assumption of linearity, but this was done in neither the original manuscript nor the revision. The polynomial function the authors present here is justified only if the trend is not monotonic. Here, unfortunately, we never get to see the graph of the trend so as to determine whether a polynomial function is necessary, nor have we any information about the choice of the joint points or the underlying assumptions.

The association analysis is apparently clearer now than it was., but the factors related to management of BM cases were analyzed separately from those related to patients’ demographic and clinical characteristics. This approach is not advisable, since it makes it impossible to adjust all the risk factors reciprocally. This choice may have been dictated by the very low number of patients for whom information about BM management is available (just under 24%); however, this would suggest the advisability of excluding the variables related to BM management, rather than analyzing them separately, since the results are very likely to be biased.

Introducing in multivariate analysis as an independent binary variable each category of the
variables related to profession and housing for children and students, precludes analysis of the
relative risk between the different categories and also lowers the power of the analysis.

Methods and criteria of selection of the final model are not reported. Did the authors carry out a stepwise analysis? Did they select variables using significant test criteria, such as F-to enter or F-to remove? Apparently they used a threshold value of the association estimate instead of the probability of a statistically significant association. What test did they use to compare different models?

3) Not all the methods were clearly described, e.g. for the monthly estimates of incidence.

The number of BM cases with complete information about demographic and clinical risk factors was not reported, nor was the number of meningitis cases with “unknown aetiology”

The numbers of patients for each category of risk factors as well as the crude estimates of association between death and co-factors were not reported either in Table 4 or in the text about the variables excluded from the multivariate analysis. This makes it impossible to evaluate the possible collinearity between the co-factors, as I pointed out in my first review. Moreover we cannot be sure that age and gender were included in the multivariate analysis, since the mortality ratio between the age groups equals about seven (figure 1), but age was apparently excluded from the analysis with a RR < 1.3, and the results were given in terms of age- specific rates.

The interaction was not considered at all, though the authors refer to it in their answer.

Some imprecision to be corrected:
- p.7, the definition of delayed hospitalisation should be ‘greater than one day’, instead of “equal or greater than one day”
- p.8, the term bivariate analysis usually refers to the inclusion of two factors at a time in association analysis with the outcome. Here univariate analysis should be used instead of “bivariated analysis”.

**Level of interest:** An article of limited interest

**Quality of written English:** Acceptable

**Statistical review:** No, the manuscript does not need to be seen by a statistician.

**Declaration of competing interests:**

I declare that I have no competing interests