Reviewer's report

Title: Increasing incidence and mortality of infective endocarditis: a population-based study through a record-linkage system

Version: 1 Date: 19 October 2010

Reviewer: N Fernández-Hidalgo

Reviewer's report:

BACKGROUND
1. The objective of this study is too general. A/few more concrete objective/s is/are needed.

METHODS
1. Due to the retrospective design based on hospital discharge records (HDR), I have many doubts about the quality of some variables. First of all, as it is addressed in the Discussion section, the main limit of the study is the lack of validation of IE tracked by ICD9-CM discharge codes. Secondly, complex variables based in different items (Charlson index, for instance) can be easily underestimated.

2. Time to mortality should be defined. It is crucial to know what is considered day 0. If day 0 is date of admission, health care-associated cases would have altered means/medians. The same consideration can be done in order to evaluate the length of stay. Probably a good option would be day of diagnostic or first day of specific treatment. However, this data can be difficult to know from HDR.

- Minor Essential Revisions

ABSTRACT
1. The objective of the study should be stated in Background.

2. Please write level of significance (p) at the end of the first sentence in Results.

BACKGROUND
1. First paragraph: ‘patients with health care-associated infections’ and ‘those with nosocomial exposure’ are two redundant concepts.

2. First paragraph: ‘intravenous drug users’ is an emerging problem in some developed countries while in others is past history.

METHODS
1. Infective endocarditis (IE) should be defined. On the other hand, it should be clarified whether IE cases are left-sided or right-sided, and the number of prosthetic episodes.

2. Codes (421.x, 098.4, 112.81… and the remaining all over the manuscript) should be explained.
3. Please clarify if ‘previous surgery’ means ‘previous CARDIAC surgery’.

4. Better than previous heart failure, it would be very interesting to evaluate acute heart failure during admission.

5. As Cox proportional hazards regression results are not shown later, I would remove its reference from the Methods section.

RESULTS

1. At the end of the second sentence (‘The number of incident…’) the level of significance should be added, in order to better assess the importance of an increasing incidence of IE.

2. First paragraph: While talking of ages, please add ‘years’. Moreover, is the age increase statistically significant?

3. ‘Table 3 shows SURGICAL ‘ treatment and outcomes of IE’.

4. Next sentence ‘The median length of stay increased from 23 to 32 days when including hospital transfers’. It should be clarified whether the length of stay was evaluated in the whole series or only in patients alive at discharge. Both points of view should be distinguished.

5. It is said that percentage of cardiac valve procedure in the index admission or in the following year heavily depends on age. Was this trend statistically significant?

6. Previous studies on IE patients transferred from community hospitals…
   …demonstrated that transferred patients had not a greater mortality than those diagnosed and treated in a tertiary care setting. This discrepancy should be discussed later in the Discussion section. Hospital transfers were performed between same level hospitals or to reference hospitals?

7. Again, 30 and 90 days mortality data are not very useful if a clear definition of the Day 0 is not provided.

8. ‘Figure 1 shows that the increment of incident cases observed among residents in the Veneto Region and the surge in case-fatality produced a growing number of deaths associated with IE, from 23 in 2000 to 63 in 2006 (90 days follow-up).’ Better than crude results, it would be of interest to show rates.

9. Eliminate reference to Cox regression if data are not shown.

10. It is said that 23.6% of patients underwent surgical treatment. It would be interesting to show how many of these patients underwent cardiac surgery after being discharged.

DISCUSSION

1. Discussion about 30 days and 90 days mortality should be re-written if definition of Day 0 is modified.

2. It is said that ‘…previous nosocomial exposure have been demonstrated to be
meaningful features in our cohort’. In this study, previous admission did not increase from 2000 to 2006. Furthermore, previous nosocomial exposure is a variable not included in the mortality analysis.

3. ‘…, according to literature, we confirm a high mortality associated to IE, particularly in older ages and in subjects with congestive heart failure’. In the literature, congestive heart failure usually refers to acute congestive heart failure occurring during admission. However, in this study (if I have not misunderstood), congestive heart failure refers to a previous condition.

4. As cardiac surgery is not analyzed (in deep) as a factor for mortality, discussion about early surgical treatment should be shortened.

5. ‘The high proportion of patients with previous hospitalization may explain the consistent – although limited – microbiological data, being S. aureus the leading microorganism isolated.’ Was this affirmation confirmed with data from this study?

TABLE 1

1. Data from 2000 and 2006 years are provided. However, p for trend across years is provided. It would be more visual to add an intermediate category.

2. Is there any mistake in ‘Previous congestive heart failure’? Total cohort percentage (12.2%) is greater than 2006 cohort percentage (10.3%).

3. Has Charlson index been evaluated as a quantitative variable instead of categorical?

TABLE 2

1. Please define codes.

2. It is possible to know the number of methicillin-resistant S. aureus? If so, is there any trend across years?

TABLE 3

1. As in Table 1, it would be more visual to add an intermediate category.

2. Please, provide separately cardiac surgery during admission and after discharge.

TABLE 4

1. Charlson index has been evaluated as quantitative variable instead of categorical?

2. Charlson index has been evaluated adjusted by age?

3. I understand that heart failure is previous a previous diagnostic. Was acute heart failure during admission evaluated as a risk factor for mortality?

4. Calendar year (itself) is not a risk factor for mortality. For that reason, should not be included in the analysis.

FIGURE 1

1. In would be better to add percentages to crude data.

- Discretionary Revisions
WHOLE STUDY

As this article is going to be published in 2010 or 2011, it would be very interesting to provide data until, at least, 2008.

BACKGROUND

1. First paragraph: Authors can reference some current articles on health care-associated infective endocarditis.

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.