Reviewer's report

Title: Historical Trends in Survival of Hospitalized Heart Failure Patients: 2000 versus 1995

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Reviewer: Robbert R Sanderman

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General
The paper is clear, well written, but has some methodological limitations that could be at least mentioned in
the Study Limitation section:

1. The authors adjusted for the most important clinical and demographical variables, as well prescription of
ACEI and beta-blockers. Yet, several other changes in drug therapy during 1995-2000 or the presence of
other comorbidities (e.g renal insufficiency, COPD) might have influenced the secular trends in survival:

For instance, the following variables might have been selected for adjustment:

a. Prescription of digitalis. This is very important, especially for women, as posthoc analysis in the DIG trial
has shown that digitalis (in certain blood concentrations) might increase mortality in women, but not in men.
Previous studies have shown that prescription of digitalis has decreased after the publication of the DIG trial
(1997), yet this might explain (among others) why mortality rates did not change significantly in women.
b. Prescription of other classes of medication that were proved to increase mortality, such as class I
antiarrhythmic agents or first generation Calcium-channels blockers. The prescription of these agents also
decreased between 1990-2000.
c. A comorbidity score would have captured better the disease severity, especially that half of the patients
missed the LVEF measurement.

2. It is not clear in the methods section how the authors selected the variables for adjustment, e.g based on
significance, literature evidence, both? We see in the various models from Table 2 that age, history of CHF
or prescription of ACEI or beta-blockers were selected, yet not diabetes, which was also mentioned in the
literature as an important predictor of survival. It is also not clear what is in fact discharge medication,
probably previous discharge medication? I assume so, as the prescription rates of ACEI and beta-blockers
are higher as in-hospital medication than as discharge medication. I think that authors should mention it
clearly in the paper.

3. In the analysis, the authors mention that characteristics of patients were compared by computing means
or medians (for skewed distribution). However, in the paper are reported only means. I would expect that
age has a skewed distribution, as most of the patients with HF are elderly. Median may say a bit more about
the age distribution, as I expect a higher percentage of elderly women.

4. Women had a higher percentage of preserved LVEF, the so-called HF syndrome with preserved LVEF. I
would expect that in 2000 even a higher percentage of women had a preserved LVEF, as this syndrome
was more and more recognized as an individual entity. If so, some studies report better survival rates in
patients with HF and preserved LVEF when compared to those with depressed LVEF, and this might
explain the similar survival rates. On the other hand, we can also notice that a lower percentage of women
than man receive an ACEI in 2000, and this may also have affected the mortality rates. The lack of clear
evidence regarding the effectiveness of ACEI in HF with preserved LVEF may explain the lower prescription
rates.

5. It is indeed difficult to draw a final conclusion regarding secular trends in survival, as on the one hand the
complexity of hospitalized cases has increased in time, due to advances in outpatient care, and on the other
hand given the observational study design there is the risk of selection bias (e.g healthier patients receive a
beta blocker) and survival treatment selection bias (patients who live longer have more opportunities to
access to the treatment).
Major Compulsory Revisions (that the author must respond to before a decision on publication can be reached)

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

Discretionary Revisions (which the author can choose to ignore)