Reviewer's report

Title: Survival of patients with operable breast cancer (Stages I-III) at a Brazilian public hospital - a closer look into cause-specific mortality

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Reviewer: Eleonora Dorsi

Reviewer's report:

This is a manuscript about the 5 year survival rate of women with operable breast cancer at a Brazilian public hospital. In general, the question posed by the authors is well defined and the methods are appropriate. I have the following comments and recommendations:

The abstract should be improved according to the following comments.

The terminology about the standard of development of countries has recently changed and the terms “developed country” is no longer used, it has been substituted for “high-income country”, “middle-income” and “low-income”, reference number one (Harford, 2011). I recommend the authors use this terminology in throughout the manuscript.

In the background section, in the end of the first paragraph there are some statements about the survival rate in Goiania, the authors should provide a reference for them.

In the end of the background section (page 4, last paragraph), the main goal of this study seems to be “to evaluate cause-specific survival of female patients with operable breast carcinoma (stages I-III) treated at HC-UFMG from 2001 to 2008”. The rest of this phrase “and to compare our findings...” is not part of the main objective, it is more a part of the discussion of the results, as well as the last statement “We will also discuss how different approaches to survival studies can change the results”. I recommend summarizing the main objective according to the above.

The background is well supported by the literature review, but there are lots of old references (before 2000) and an excess of references (50). I recommend selecting the most important and newer references, and using only those.

In the methods section, the authors should provide the number and date of approval of the study protocol in the UFMG Ethics Committee.

The authors should also provide the sample size calculation to address the main objectives of the study. The methods section should be better organized starting with the study’s design, population, sample size, methods, data collection, variables, and statistical analysis. In the 6th and 7th page, the second paragraph starting with “Data from the Surveillance Epidemiology and End Results (SEER from the United States...” is not part of the methods of this study, the authors did
not analyze and present results of the SEER, so I recommend suppressing this part of the methods section.

In the statistical analysis the authors should provide the p-value for entry and the p-value for getting out the variables of the multivariable Cox model. The authors say that they used the chi-square test for linear trend to compare the frequencies of T and N over the study. I recommend not using such abbreviations (T and N). They did not show the results of these comparisons using the chi-square, only the log-rank test of the comparisons of rates of survival (Table 1), so I recommend excluding the chi-square and including the p-value of the log-rank also in the Kaplan-Meier curves.

In the end of the methods section (page 9), there is a phrase about the databases where the literature review was done, I suggest changing this to the background section as well as to give more details on the literature search (key-words, languages, date of the search, criteria of inclusion and exclusion of studies etc).

The Results section should be better organized. I recommend starting with the main results of the study, the 5-year global survival and the 5-year survival according to stages. They are at page 11, second paragraph.

In page 10 there are some abbreviations (IDC and ILC) that should be written out.

In the end of the Discussion, page 13 third paragraph, the statement about SEER should be in the Discussion section.

In the Discussion, I recommend starting with the discussion of the main results that are presented at page 16, 1st and 2nd paragraphs).

Page 14, 1st paragraph, they state that “In our study, the tumor stage was the strongest predictor of survival” but they present in the multivariate model three separate variables: tumor size, lymph node status and histologic grade”, I recommend changing the statement according to the results.

One important limitation of this study is the lack of measures of socioeconomic position, like schooling or income. The authors state that they are important predictors of survival but do not present any of them, only the SUS/Private insurance as a surrogate. Why didn’t they collect and present other measures, like schooling? If they did not have access to them, they should include this in the limitations of the study. If is possible to access them, I recommend including in the analysis.

In Table one, I recommend including first the age of participants that is presented only in the end of the table.

**Level of interest:** An article of importance in its field

**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