Reviewer's report

Title: Disease-specific survival for limited-stage small-cell lung cancer affected by statistical method of assessment

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Reviewer: Andrew Roddam

Reviewer's report:

General

This manuscript describes the application of four different statistical models for the estimation of disease specific survival (DSS) probabilities using a dataset on limited-stage small-cell lung cancer. The main conclusion of the manuscript is that you can get somewhat differing estimates of DSS depending on which method you use. In itself, this conclusion is not that surprising since the methods considered have very different assumptions:

a. Boag log-normal cure model: mixture model estimating proportion cured and survival according to a log-normal distribution
b. Kaplan-Meier: non-parametric estimate of the survivor curve
c. Cox: semi-parametric estimate of the survival distribution
d. Log-normal: parametric estimate of the survival distribution

The paper compares estimates of overall DSS and then DSS by 4 subgroups of patients.

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Major Compulsory Revisions (that the author must respond to before a decision on publication can be reached)

One of my main concerns with the manuscript is that the methods section is rather poorly written. I would have hoped for some clear statements about what each of the models assumes (not relegated to an appendix) and how the model is estimated. It is something of a surprise that so many different software packages were used to estimate these effects since most packages provide similar functionality.

Following on from this comment the discussion section should be revisited – as has been noted by previous reviews of this manuscript the authors pay little attention to the differences between models with and without specific cure parameters. In fact some of the statements are somewhat misleading – for example comments about estimating the proportion cured from the survival function. From a modelling viewpoint these are two very different classes of models and at the very least this should be more carefully discussed. In a similar vain why have the authors chosen to assume log-normal survival distributions? There are many other continuous distributions which are sometimes appropriate for survival analysis – for example the Gamma distribution. Would the paper not benefit from at the very least a discussion of these alternatives?

For neither the overall analysis nor for analysis when the authors have divided the patients into 4 groups there is no statement of how many events occur by year 1, 3 and 5 or within groups A, B, C or D by years. It may not be that surprising that the largest differences between methods is found in the subgroup analyses when the event numbers are very small and power limited. More attention is clearly needed to this important issue.

Did the authors assess whether the assumptions of the various models were satisfied for the subgroup analyses? Further there is no mention in the results section that the assumptions were checked and satisfied for the main analyses.

Why were Boag models not considered for subgroup analyses?

My final comment is that it is not clear what this paper adds to what is already known in the literature and the authors should be more explicit here.

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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)

What next?: Unable to decide on acceptance or rejection until the authors have responded to the major compulsory revisions

Level of interest: An article of limited interest

Quality of written English: Acceptable

Statistical review: No

Declaration of competing interests:
I declare that I have no competing interests