Reviewer's report

Title: Minimum follow-up time required for the estimation of statistical cure of cancer patients: verification using data from 42 cancer sites in the SEER database

Version: 1 Date: 16 November 2004

Reviewer: Claudia Spix

Reviewer's report:

General

The question posed by the authors is not new, but it is of considerable and continuous interest. The question is well defined. The methods are partly appropriate and partly questionable. With respect to the main method, not enough information is given to judge the appropriateness. Some detail is missing, which would be required to replicate the work exactly, but generally this would be possible. The data are basically of high quality, however, some information regarding this is missing. The paper is well organized. The discussion is in part relatively vague. The conclusions follow directly from the results, but the results lack information on their reliability. Title and abstract are ok. Writing is generally acceptable with the exception of some strange sentences.

The question is relevant, the idea of the authors is rather neat, but the implementation is partly rather poor.

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

1. The method for estimating the parameters of the log-normal distribution and the goodness-of-fit test is questionable. You estimate the parameters by minimizing the test statistic you then use to test the fit. Usually such parameters are estimated by maximum-likelihood and the comparison to an assumed distribution is performed by the Kolmogoroff-Smirnoff- Test. The Chi-Sq test requires to classify the data. You did not indicate how the class-widths were chosen. The results for small numbers of patients can depend heavily on the class width chosen. The Chi-Sq test is a test for difference, the question here however is one of equivalence. If you want to use a test for difference, do at least chose a p-value well above 0.05.
2. Any method for judging goodness-of-fit has the disadvantage that it tends to easily find differences for large numbers of patients and to overlook them for small numbers. Please discuss this.
3. The quality of the results in this work is highly dependent on a. The quality and completeness of follow-up in the SEER database and b. The reliability of the cause-of-death information for the cases. Please give some indication or some reference with respect to these aspects. If appropriate, discuss how problems in these areas could have biased your results.
4. No information whatsoever is given, which allows to judge the stability, variability or reliability of the results. This is essential! Confidence intervals for example would be very helpful. Some sites have so many cases that it might be considered to split the data in half (e.g, by the two areas the data
came from or randomly) and then to compare the independently obtained results.
5. Other methods have been suggested for estimating cure, e.g. comparing mortality rates to the
general population or relative survival. Both these have the advantage of not requiring reliable
cause-of-death information. Please compare your results to other methods more formally and
discuss the advantages/disadvantages of your approach compared to them.
6. Appendix: You use only survival times of those who were actually followed to death. It is unclear
how the estimates might be influenced by loss-to-follow-up and right-censoring in your dataset.
Please discuss this.

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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)

1. Were distributions other than the log-Normal considered? If not, why not? If yes, on which basis
was the log-Normal considered superior?
2. The term survival rate, though commonly used, is inaccurate. Kaplan-Meier or life-table (actuarial)
methods estimate the proportion of survivors, not a rate. The term cure rate is equally inappropriate.
Note that both your references [11,15] speak of “proportion of patients cured” not “cure rate”.
3. The sentence on page 6 “We applied the property of the lognormal distribution” is totally unclear.
Do you mean the quantiles as presented by Limpert [25]?
4. A general problem for such analyses including survival data over a long time span is that the
survival experience of patients diagnosed many years ago does differ considerably from that of
current patients (e.g. due to improvements in screening and therapy). Both your estimates and the
Kaplan-Meier estimates are based on more “old” cases than on concurrent cases, especially the
cancers with the long time spans until cure. Please discuss this.
5. In the discussion a number of papers are referenced as being in agreement with this paper. This
is however presented in very vague terms. Be more specific.

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Discretionary Revisions (which the author can choose to ignore)

1. Language: Not “the minimum year” but rather “the minimum number of years”
2. It would be helpful if the diagnostic groups were identified by ICD or ICD-O.
3. The patient numbers for the sites definitely not following the log-Normal distribution are not given.
4. Strange expression on page 7: lognormal cancer sites.
5. Strange expression on page 9 : …the cure can never be estimated during the life-time of human.

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: Needs some language corrections before being published

Statistical review: No

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