Reviewer's report

Title: Incidence, Mortality Patterns and 5-year Relative Survival Ratio of Prostate Cancer among Residents in Singapore from 1968 to 2002

Version: 6 Date: 18 September 2008

Reviewer: Francesco La Rosa

Reviewer's report:

Methods and results
small sample sizes precluded a detailed analysis of the other ethnic groups.
Since the study is population based the word sample should not be used
Linear regression on log-rates
Despite having at least a subgroup showing non-linear trends in rates and a easy
to use, freely available software (i.e. the joinpoint regression) , the Authors simply
ignored my suggestion to improve statistical analysis. Otherwise Poisson
regression could have been performed in Stata (a likely Stata output table was
added to the Authors’ reply).
Survival analysis by stage
Since the information on local tumor spread was available, it should have been
used in the analysis. Also incidence rates by stage and period should have been
considered instead of considering the composition (i.e. percentages variation
over time). Moreover no use was made of the reviewer’s suggestions: the paper
is now almost centered on the analysis by stage. The observed trend in 5 years
relative survival is complex and the analysis based on incomplete staging data
may be misleading instead of contributing “to the understanding of prostate
cancer epidemiology and the differences among Asians.”
Overall incidence and mortality rates for the whole study period are quite
meaningless.

Discussion
“…If screening is effective, it should detect cancer at an earlier stage in the
natural history among asymptomatic persons…”
This phenomenon may happen also for ineffective screenings. The relative
survival may be influenced even if a screening is ineffective (i.e. if it is effective in
anticipating diagnosis but not in improving health outcomes). Both the diagnosis
of non progressive cases and the unobservable time span between diagnosis at
screening and diagnosis due to symptoms of the disease (in the absence of
screening) contribute to an apparent survival improvement.
The incidence and mortality rates of the unknown metastatic group also reflect an
increased number of non-metastatic cases in this group. For this reason we
excluded the unknown metastatic group from our analysis, as it could skew the study’s findings.

In the group with unknown metastatic status the number of non-metastatic cases is likely to be unknown and the exclusion of this large group from the analysis will lead to biased results.

The following statements seem contradictory and really difficult to understand:

“We observed a divergence in incidence and mortality trends for all residents and Chinese from the 1990s (Figures 1a and 1b), thus the rising incidence rates were not accompanied by corresponding improvements in the RSRs over time, suggesting no improvement in prostate cancer treatment. However, the prognostic outlook for the Chinese nonmetastatic cases was more favorable for those diagnosed post-1990 compared to those diagnosed in the 1980s (Figure 2a), suggesting possible changes in treatment modalities.”

This new version of the paper does not improve over the second version of the paper. On the contrary it seems to me that the paper was worsened and particularly the discussion is now exceedingly long and confused. The Authors’ discussion seems not justified by results and is hampered because it relies heavily on the analysis by metastatic status with as much as 38% of cases with missing information and variation over time. Many suggestions were disregarded and addition was preferred to real change leading to a final paper that, in my view, cannot be published.

**Level of interest:** An article of limited interest

**Quality of written English:** Needs some language corrections before being published

**Statistical review:** Yes, and I have assessed the statistics in my report.

**Declaration of competing interests:**

'I declare that I have no competing interests'