Reviewer's report

Title: Effect of socioeconomic status on three-year mortality after first-ever stroke: Nanjing Stroke Registry

Version: 1 Date: 10 May 2006

Reviewer: Maria Rosvall

Reviewer's report:

General
The study investigates the association between SES and mortality after ischemic stroke. The study covers an important topic and is undertaken in a country with a different developmental history than modern western populations. It is rather clearly written and of potential interest to an international audience. However, there are several points for discussion and weaknesses with the study which should be made more clearly.

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

1. The first comments have to do with selection bias.

The population of the study is those hospitalized at Jinling Hospital, Nanjing. However, the authors do not make clear if there are also more hospitals in the area where the study population came from that handles stroke events. If so, are there different taxes in different hospitals, i.e., where the richer people seek some hospitals and the poor people other hospitals?

Do people from lower SES more often seek local traditional doctors than seeking hospitals? If so, how would this affect the results? This would also be a problem with regard to deciding if this is the first-ever stroke or if a patient has been to other hospitals or doctors before not registered at the Jinling Hospital?

What about those in the area who die due to a stroke at home (probably most often lower SES groups) are they taken to Jinling Hospital, Nanjing for an autopsy?

The authors state that a considerable amount of stroke patients in rural areas were misdiagnosed and couldn’t be treated by local traditional doctors. Couldn’t have been enrolled in our registry. These patients mightn’t have been enrolled in our registry What is a considerable amount? How might this affect the results?

2. The authors look at mortality after a first stroke event. What was the proportion lower SES and higher SES at baseline?

3. The authors have not looked at different time periods with regard to mortality which is often done in these kind of studies, i.e., look at 28-days mortality and more long-term mortality for example after 28 days but within 3 years. The mechanisms behind the deaths are probably different where factors such as co-morbidity and the size of the stroke is of more importance for short-term mortality, while risk factor levels such as for example blood pressure levels are of more importance with regard to long-term mortality. It would be interesting to also see the results based on this categorization.

4. Why have the authors chosen to only investigate ischemic strokes and not hemorrhagic strokes, this should be explained in the background section together with other issues mentioned below. I think the Background section is too short as it stands and the references are not in order.

5. Blood pressure should be added as a continuous variable and not dichotomized. If possible treatment for hypertension and also BMI should be added as confounders.

6. The section of potential mechanisms on p.11, last paragraph should be rewritten and more extensively discuss this issue.

7. The study population represents a hospital-based stroke population and not the general population as claimed on p.11 last paragraph. This should be stated in the Discussion section.
8. How come that the association between educational level and income on the one hand and stroke mortality on the other not only diminishes after adjustment for confounders, but changes direction? Do the authors have any comments on that? This should be commented on in the abstract, discussion and conclusions.

9. The manuscript should be sent for linguistic revision as there are missing words and words that are misspelt throughout the manuscript.

10. In the background section the authors should justify what their study would add to the studies already done on the association between SES and mortality after a stroke. Are there reasons to believe that the associations between SES and stroke mortality would be different in a Chinese population than in a western population? Would the mechanisms be different?

11. Why was the cut-off level for fasting blood glucose as high as 7.0 mmol/L?

12. Please clarify the sentence on p.10, 1st paragraph starting with “And correlation”

13. Please clarify the sentence starting with Population-based Sino-MONICA on p.11 last paragraph. Also in the next sentence, which demographic characteristics are the authors referring to?

14. In the conclusion the authors state that after economic reforms there has been a great development in China with regard to GNP, income and so on. I think this should be mentioned also earlier in the Discussion section and shortly in the Background to set some of the background for the study.

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

15. In the abstract the last sentence in the result section “there must be words missing” I don’t understand this sentence.

16. The HRR seems very high with regard to the lowest vs the highest income level could this be due to the fact that there were only 2 deaths in the highest income category?

17. In table 1 there were no cases of mortality after a stroke event among farmers, however, in table 2, the hazard ratio compared to non-manuals was 5.23?

18. In table 1 in the foot notes not should be added after if

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 whose findings are important to those with closely related research interests

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'