Reviewer's report

Title: Ischemic stroke incidence in Santa Coloma de Gramenet (ISISCOG), Spain. A community-based study.

Version: 2 Date: 7 January 2008

Reviewer: Mauricio Avendano

Reviewer's report:

Authors have made a great effort to improve their paper, and I acknowledge this as an improved version of the manuscript. However, I still believe the paper has a number of important problems, which will need to be addressed:

Major Compulsory Revisions

1. The paper still fails to make an important distinction between the study of the impact of socioeconomic status on the occurrence of disease (i.e., incidence), and the study of its impact on healthcare utilization (i.e., hospitalization). The distinction is essential because it has implications on the interpretation of the results and the framing of the study. Again: Is the purpose of the study to understand whether socioeconomic status (SES) is related to health or whether SES is related to care? It is not fully clear.

2. Authors tried to address the above problem by stating that their purpose is to present their methodology, i.e., to construct an indicator of SES based on income data. I am afraid this does not solve the problem; although this may be the first time this is done in the Italian context, several previous studies have used area-based income data to approach SES and have shown that it is a reasonably valid method to measure individual SES. In my view, focusing the paper on this method does not add sufficiently to the body of research on SES (and proving that this method is applicable requires more than what is possible to do with their available data, e.g., to compare individual with area-based income data). Instead, the paper needs to improve in focus by placing itself in one of the two fields as described in point 1 above. I believe this more substantial focus would also be of greater interest to an international audience.

3. In relation to point 2 above, I suggest authors rephrase their abstract conclusions in a way that the substantial results of the paper are reflected, i.e., not only focusing on the methodology.

4. Although authors focused now the introduction on their methodology, the results and discussion section are now mainly focused on the content and their findings. This discrepancy may be solved by appropriately focusing a bit more their introduction on the main findings rather than placing a focus on their method.

5. Table 1: It is unclear what the row ‘relative difference’ is. It is certainly not the relative difference between the two extreme years. Please explain this in the
Table and specify reference categories if needed. Furthermore, it would be more consistent to present both relative and absolute difference estimates for all regions—it is unclear why this is done only for Milan and not for other regions.

6. Results: It may be useful to use subtitles in the results section to distinguish two main parts of the results: (a) Declines in rates over time, in absolute and relative terms; and (b) inequalities in health by income by period, in relative and absolute terms.

7. Authors state that patterns are similar for all cities. Is this true for both absolute and relative measures? Please discuss this in more detail.

8. It is stated in paper 7 that interaction tests were performed, but results of these interactions are not presented in Tables. Were there any interactions by gender, income level, year and city?

9. Discussion: Authors present an extensive discussion of their findings on trends in the discussion (pages 9 and 10). This would definitely be better placed in the result section, embedded within the main findings.

10. In the results section it is sometimes stated that patterns differ across cities, but it is not clearly explained in which way this pattern is different. Please be more specific on describing these regional differences.

11. Discussion: I feel the discussion section (and the abstract conclusions) still lacks integration; what is the main conclusion of the paper? Apart from the finding that this area-based income variable is related to health, which other conclusions can be drawn from this study? I suggest authors attempt to clarify what is their central message and state it in the discussion and abstract conclusions.

12. Tables: Please specify in legends the covariates included in which model, i.e., which were the covariates in the models for the ratios? For what are rates in Table 1 standardised? The age distribution of which year was chosen for standardisation?

Minor Compulsory Revisions

13. I believe references 25 and 26 could be replaced by peer-reviewed papers, which in this area are abundant.

14. Page 9, paragraph 2 first 3 lines: authors state that Bologna has the lowest rates in each year; if I understand correctly, it is actually the opposite: Bologna seems to have the highest rates in each year.

15. Page 10, paragraph 2: The underlying effect of different regional policies is highlighted in this paragraph. This is inconsistent with a general tendency for associations with income to be very similar across the regions.

16. Page 10, paragraph 2, line 1: Replace ´different limits´ by ´different limitations´.

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 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