Author's response to reviews

Title: Mortality among adults: gender and socioeconomic differences in a Brazilian city

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Version: 3 Date: 23 November 2011

Author's response to reviews: see over
Reviewer's report
Title: Social inequality in mortality: gender and socioeconomic differences in a Brazilian city
Version: 1 Date: 12 October 2011
Reviewer: Gwenn Menvielle

Reviewer's report:
This paper deals with socioeconomic inequalities in mortality in a big city in Brazil. The authors use a large dataset and study mortality during a recent period. They use a deprivation index to measure SES. This manuscript provides new results on a topic less studied.
I have several comments.

Major revisions
1. In the abstract, add the CI with the RR. Please also add the RR for breast as well as its CI. Then we'll see if this RR is statistically significant.
   We included CI and RR for all diseases mentioned in the abstract, as you recommend. By the way, since the abstract should not exceed 350 words, we had to rewrite it, eliminating some sentences.

Background
2. Second paragraph. Please be a bit more specific. The authors mention that “disagreements can be found in the literature”. Is this found for total mortality? For some specific causes of death? For all causes of death?
   We added to this paragraph that this refers to some specific diseases.

3. Fourth paragraph. The authors refer mostly to Brazilian literature. I can understand it, as some results are likely to have been disseminated only locally. However, most readers will not be able to find and read these studies. Therefore, the authors should also give the main message/conclusions of the cited manuscripts.
   As the editors asked us to reduce the article size, we deleted this part. To explain all results obtained by authors of four articles, the introduction would become too long. In this sentence, we would like to explain that the majority of Brazilian articles only analyzed one aspect of mortality (such as overall mortality rates, homicides, so forth). In other words, in Brazil there are few studies that investigate several causes of death, as we did.

Methods
4. Third paragraph. The authors use a deprivation index. Has this index been used and validated before? Did the authors test the robustness of their results using other definition of the deprivation index?
   Several social indicators have been used in Brazilian studies. Because of socioeconomic diversity within our country, studies focused on different Brazilian
regions have selected specific socioeconomic variables that would have a strong discriminatory capacity in the definition of socioeconomic strata in each scenario. In other words, the choice of socioeconomic indicators for the composition of strata is based on the context of the Brazilian region studied. Consequently, there are great differences among Brazilian studies in relation to the selection of socioeconomic variables used to compose the strata.

To analyze the social inequalities in mortality in Campinas, our research team conducted several analyses previously to define the variables and method. The study of the same four socioeconomic parameters and several others social indicators provided by Brazilian Census was also analyzed through the distribution of scores divided into quartiles. The results obtained from quartile distribution were very similar to those described using the technique applied in this study. Other variables related to demographic characteristics, education, income, and housing quality were also tested, but the output groups had less internal homogeneity and less heterogeneity among groups. Therefore, we strongly believed that this simple model was adequate to our proposals.

Methodological details underlying the construction of the index are also provided in the article of our authorship entitled *Reduction of social inequalities in life expectancy in a city of Southeastern Brazil*, which is published in *International Journal for Equity in Health* (2011; 10: 36).

5. Fifth paragraph (page 5-6). The text is not clear. What makes the understanding difficult is the use of two different age bands for total mortality (all ages) and for cause specific mortality (20+). Why did the authors use two different age bands? What is the rationale underlying this choice? This is never explained, and it is not clear what the results for the age group 0-20 for total mortality add to the paper.

Thanks to your appropriate comments, we reevaluated our analyses and decided to estimate mortality rates only for people aged 20 or more. For the majority of specific causes of death that we selected (with exception of prostate cancer), there were deaths only from the age of 20. As we standardized the age band, we think our analyses are clear now.

**Discussion**

6. Page 10, second paragraph: When you refer to Brazilian literature, please explain which factors you are mentioning, as the literature will not be found and understood by most readers.

Thank you so much for indicating this problem. We included the main factors we were mentioning, which are: delayed childbearing, hormone replacement therapy use, and frequency of alcohol intake.

7. Page 11, paragraph on smoking: I do not totally understand the reasoning of the authors. They mention higher smoking prevalence among people with lower education. Then we would expect higher lung cancer mortality rates among people with low SES. This is not reported. Maybe the higher prevalence is
reported only among young people whereas among older people the reverse situation is still observed? In any case, more explanation is needed.
In order to provide a better explanation, we rewrote this paragraph, adding new information. In Brazil, the smoking prevalence among high income groups has declining quickly, while among low income groups the pace has been less accentuated. In Campinas and in other Brazilian cities, the smoking prevalence is greater in most- than in least-deprived groups. Because of this situation, we strongly believed that, as it occurred in developed countries, in Brazil in the next couple of years we will observe a social gradient in lung cancer mortality. At this moment, we cannot see this gradient, due to long latency period of the cancer.

References
8. Please translate the titles in English.
We inserted the titles in English of articles that were written in Portuguese. Both titles and abstracts in English are available at the own journals. But, in respect to the authorship, we preserved the titles of books chapters in Portuguese.

Minor comments
Methods
9. Sixth paragraph. The analysis for the 5 ICD chapters is conducted among people aged 20+ (table 3). This is not mentioned. As it is, it seems that these analyses are conducted among all ages, and only the analyses for more specific causes of death are computed among people aged 20+. Please combine this paragraph and the following, and clarify the text.
As we explained above, we decided that it would be better to focus on people aged 20 or over. We rewrote these sentences in order to make our choices clear.

Results
10. First sentence. Not clear. Which strata is mentioned?
We rewrote this sentence in order to make our ideas clear.

11. Last paragraph. The authors say that the rate ratios are generally very large in women when compared to men. I would tone it slightly done and simply say “larger”.
Thank you for correcting us. We corrected the word.

12. Page 11, first row: the word “familiar” is not appropriate here.
We really appreciated this correction. But, as the editors asked to shorten discussion section, we deleted this part. Anyway, the correct expression would be “family income level”.

13. Page 12, second paragraph: It could be shortened.
We shortened this paragraph, as you recommend.
Level of interest: An article of importance in its field
Quality of written English: Acceptable
Statistical review: Yes, and I have assessed the statistics in my report.
Declaration of competing interests:
I declare that I have no competing interests.
Reviewer’s report
Title: Social inequality in mortality: gender and socioeconomic differences in a Brazilian city
Version: 1 Date: 12 October 2011
Reviewer: Rasmus Hoffmann
Reviewer’s report:
Major Compulsory Revisions
No major compulsory revisions

Minor Essential Revisions
1. Results, 1st paragraph, 1st sentence: deeper than what?
Thank you for indicating our mistake. We corrected this expression.

2. Please briefly explain what the aging index is.
We explained what means aging index in the text. Aging index is also called “elder-child ratio” and it refers to the ratio of population aged 60 (or 65) and over per 100 individuals aged less than 15 years old. Since Brazil is a developing country, we considered the cut-off point “60 or over”.

3. Results, 2nd paragraph (middle): more pronounced than what?
With new results obtained from a standardization using Campinas total population as reference, we rewrote this phrase, correcting the comparisons.

4. Results, last but one paragraph: RR is not a rate difference.
We replaced this incorrect expression by “rate ratios”.

5. Discussion, 11th paragraph: Please use terminology more carefully: “higher socioeconomic differences in mortality rate ratios” is probably not what you meant.
Thank you for correcting us. We rewrote this sentence.

6. Can you try to explain why the social mortality gradient is larger for women than for men?
We added some sentences with our hypothesis about this phenomenon. We believed that, although mortality rates are smaller among women than among men thanks to female behaviors are more conducive to health, the impact of socioeconomic differences in mortality are stronger among females. As it is well-known, poor people in Brazil have greater difficulties to access health services, facing long waiting lists to visit a specialist, poor quality healthcare, difficulties to buy medicines, so forth. Therefore, rich women can take advantage using private medical services. On the other hand, male behaviors, which are more injurious to health, are disseminated in all social classes. Consequently, mortality rates are higher among men than among women, but the socioeconomic differences in male mortality are smaller.
Discretionary Revisions

7. My main recommendation is that you check whether it is possible to draw maps that reflect the SES and the mortality level for each of the 49 city areas. By grouping them into three SES groups you lose a lot of information and finally come up with a SES mortality gradient that everyone would expect. A map would offer additional visual information on the spatial pattern of SES and mortality that may lead to interesting interpretations.

Unfortunately, we cannot include maps to illustrate the spatial pattern of socioeconomic indicators and mortality levels of 49 health care unit areas, since the manuscript would be too long. The editors asked us to reduce the manuscript size.

Anyway, we would like to thank you for this idea. Possibly, we will try to investigate this spatial approach in a next manuscript.

8. Sometimes you speak about the relationship between social inequalities and health. It would be clearer to say “the relationship between social status and health”. Your expression may be misunderstood because there is also a separate discussion about whether or not the degree of social inequality influences health, which is not the topic of your study.

Thank you so much for this comment. This expression was replaced by “the relationship between socioeconomic status and mortality/health”.

9. This use of terminology also negatively affects your title: if you spell it out, it says “socioeconomic differences in social inequality in mortality” while your study is simply about socioeconomic differences in mortality.

Our new title was changed to Mortality among adults: gender and socioeconomic differences in a Brazilian city.

10. Discussion, 4th paragraph: “decreasing gradient” is misleading, I guess you mean decreasing death rates.

Thank you so much for correcting us. We included this phrase: “decreasing breast cancer mortality rates from the upper to the lower socioeconomic levels”.

11. Discussion, last but two paragraph: “population as the unit of analysis”. It is more precise to say that the city area is the unit of analysis.

Thank you for showing our mistake. We corrected to “primary health care area as the unit of analysis”.

12. Discussion, last but one paragraph: The first and second sentence contains a repetition.

We deleted the repetition.

Minor issues not for publication:

Generally, some language correction is needed. Here I cite just some examples:

- Too much use of the article “the”
We deleted the excesses, which are indicated by an English teacher.

-Mortality data would sound better than death data.
We replaced “death data” by “mortality data”.

- First paragraph of the discussion: “the highest mortality excess are…”
We rewrote the first paragraph and corrected this sentence.

- Discussion 10th paragraph: reference numbers not correct.
We corrected it. The reference number “20” is in brackets.

- Conclusion, 2nd paragraph: it is crucial to cooperate.
Thanks for your observation. Now, in the conclusion, it is written: “(…) it is crucial that there be cooperation between public safety organizations (…)”

- All over the text, I think the capital letter for Low, Middle and High SES are not necessary.
These words were rewritten using lowercase letters.

- The style of referencing is not consistent superscript numbers versus numbers in brackets.
We fixed this problem. All reference numbers are in brackets. We really appreciated your careful reading.

- Please rearrange the line spacing in Table 1 as it is hard to see where a new row starts.
We apologized for that. We rearranged the line spacing in Table 1.

**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, the manuscript does not need to be seen by a statistician.  
**Declaration of competing interests:**  
I declare that I have no competing interests
Reviewer’s report
Title: Social inequality in mortality: gender and socioeconomic differences in a Brazilian city
Version: 1 Date: 21 October 2011
Reviewer: Mercè Gotsens

Reviewer’s report:
General comments:
Thank you for the opportunity to review this paper which aims to analyse social inequalities in mortality, among residents of a Brazilian city in the period 2004-2008. The subject of this paper is relevant. As the authors say, there is little similar research from Brazil. I find many areas throughout the manuscript that need revision.

The authors use the direct method to standardize mortality rates using the Campinas population by sex as reference population. Then, they compare the rate ratios by sex. But, Adjusted rates can only be compared with each other, when the same reference population is used. Thus, it is necessary to recalculate mortality rates using the Campinas population as reference and rewrite the discussion section according to new results. On the other hand, one of the strengths of this study is to analyse different specific causes of death.

Thank you so much for this correction. We recalculate all mortality rates using Campinas total population age structure as standard. With new results, we rewrote the sections of results and discussion.

1) Abstract, first paragraph: The objective of this study is not to analyse the impact of social inequalities. It is to identify the magnitude of these inequalities. Thus, it is necessary that the authors rewrite the objective of the study in this section. In addition, in the methods section it should be included the design of the study.

Thank you for correcting the objective of our study. We modified it in the abstract. In the methods section, we described that we carried out an ecological descriptive study.

2) Methods, third paragraph: It would be appropriate to justify the selection of these indicators to determine the socioeconomic strata of the 49 areas.

In the methods section, we included sentences to explain how this selection was conducted. Several indicators from Brazilian census were tested, but the four indicators alone demonstrated a strong discriminatory capacity.

3) Methods, third paragraph: I don’t understand very well how the authors have divided the 49 areas into three groups (low, middle and high strata). The four indicators don’t have the same direction, ie a high value in the proportion of heads of household with less than one year of schooling indicates lower socioeconomic status while a high value in the proportion of heads of household with more than 10 years of schooling indicates otherwise. Is would like confirm this is taken into account when calculating the final score?
Thank you for this observation. We rewrote the methods sections to clarify this question. The following sentence was added: “The ranking order always ranged from the worst to the best socioeconomic condition, assigning the first position to the most socio-economically vulnerable area.” In other words, the ranking of each indicator were conducted in the correct direction. Also, we would like to explain that the division of the 49 heath care areas units is based only on administrative reasons of municipal health sector. Therefore, these areas are not homogenous in terms of living conditions. There are some areas that encompass both expensive condominiums and slums. Because of this, the two education indicators and two income indicators have opposite directions.

4) Methods, third paragraph: It is necessary to explain how the authors have ensured one third of the total population in each stratum. Have the authors calculated tertiles of the variable? We add a phrase to better explain how we divided the areas of health care units into three groups. After the ranking all areas based on average score, we defined that each of three strata should represent around one third of total population. From that areas ranking, we added up the population of each area till obtaining a total that represented approximately 33.3% of Campinas population. We recognized the limitations of this method, by dividing the total population into three equal portions. But, in spite of this limitation, this method detected profound inequalities in mortality. And, on the other hand, this procedure prevent that some stratum had small proportion of the city population.

5) Methods, fifth paragraph: Are all deaths assigned a health care unit area? If not it would be appropriate to specify the percentage of missing. In the period 2004-2008, from the total of 27,945 deaths, only 404 (1.4%) could not be assigned to a health care unit area. We included one sentence in the strength paragraph of the Discussion to illustrate that this low proportion does not affect the mortality rates that we estimated.

6) Methods, fifth paragraph: It would be necessary to explain why the authors have chosen individuals over 20 years for the analysis of specific mortality. For cause-specific mortality rates, we decided to adopt “20 years or more” as cut-off point, because for most of the specific diseases selected there were no deaths registered under 20 years of age. The exception was prostate cancer. The age of 40 was defined as cut-off point, because deaths from prostate cancer started at this age. We included a sentence in the method section to explain these cut-off points.

7) Methods, fifth paragraph: I understand that the authors have chosen a standard population for men and another for women. Adjusted rates can only be compared with each other, when the same reference population is used. Thus, the authors should calculate again age-adjusted rates using only one reference population if they want to compare rates and rate ratios by sex.
Thank you for this recommendation. We recalculated all mortality rates using total population age structure as standard.

8) Seventh paragraph: Authors should justify why they only have analysed specific mortality with a minimum of 60 cases in each sex. We selected these specific causes of death with this cut-off point (n=60) in order to avoid random fluctuations due to small number of cases and then to provide a minimum number of deaths for comparison among strata. For some diseases, the numbers of deaths are very small in some socioeconomic strata. A sentence was included in the methods section to explain why we defined this minimum limit.

9) Result, second, third, fourth and fifth paragraph: The authors should include also the confidence interval of RR in the text. In addition, number of decimals of the text should be the same as that of the tables. We added the confidence interval of rate ratios in the manuscript. Also, all values from RR were corrected to two decimal places.

10) Result, fifth paragraph: “breast cancer rate was 50% greater in…” Table 5 doesn’t show this value. We rewrote this sentence, including the exact value that is showed in Table 5. We explained that, between the extreme strata, the RR is equal to 0.70, which represents that the mortality rate from breast cancer is about 50% higher in the high stratum.

11) Result, sixth paragraph: This paragraph belongs to the discussion section. In addition, as I said above these adjusted rates can’t be compared by sex. We moved this sentence to the discussion section, as you recommended. Now, after adjustments about standardization, our estimates can be compared by sex.

12) Discussion, first paragraph: The authors say that the analysis of mortality in Campinas reveals the impact of socioeconomic status on population health. This is not true because the analysis carried out by the authors only reveals the magnitude of the association between mortality and socioeconomic status. This paragraph was rewritten, eliminating the part about the impact of socioeconomic status on population health.

13) Discussion, fourth paragraph, last sentence: “In Brazil, some of this factors...” It would be appropriate to specify what factors the authors refer. We added a sentence in order to illustrate which factors would be more prevalent among groups with high socioeconomic status. The articles that we cited show high prevalence of delayed childbearing, current use of hormone replacement therapy, frequency of alcohol intake in the high income groups.

14) Discussion, seventh paragraph, penultimate sentence: “...with rates differences of 79% among men and 94% among women”. The authors should
review the article by Singh et al. because it is strange that rate differences take these values. Could it be that this article refers to relative risks rather than rate differences?

Thank you for showing our mistake. We corrected the phrase. That study refers to the relative risks.

15) Discussion, eleventh paragraph: It would be necessary to explain how the results indicate a male premature mortality. Moreover, the authors say that the results show association between social and cultural factors and the adoption of unhealthy behaviours. This statement is incorrect. The results can be explained in part by unhealthy behaviours and these behaviours are influenced by social and cultural factors. On the other hand, in this paragraph the authors also compare the results between men and women. It is important to note that men present higher mortality than women, not only because of being more exposed to certain behaviours but also through the concept of the social construct of masculinity, ie, how men behave in our society, implying that they tend to engage in more behaviours involving risks (for example driving at high speed, consumption of alcohol, etc.) than women, thus increasing their risk of disease, injury or death. However, this paragraph will only make sense, if the authors calculate age-adjusted rates using a single reference population.

We added an explanation about male premature mortality, which can be observed mainly in the younger age groups.

Thank you so much for your valuable comments. We added one paragraph to explain this relationship between social and cultural factors and health. We reinforced that the male behaviours related to health are shaping by gender roles in our society.

16) The discussion of the results of mortality due to respiratory diseases is not given.

We added one paragraph to discuss the mortality from respiratory diseases.

Minor Essential Revisions:

1) Methods, sixth paragraph: Authors should specify what mean ICD-10 (International Classification of Diseases).

We added, after the abbreviation, the specification.

2) Methods, eighth paragraph: “mortality rate ratio…” should be “mortality rate ratio (RR)…”

The abbreviation RR was added, as you recommended.

3) Discussion, third paragraph: It is necessary to modify the format of the references 14 and 2.

We would like to thank you for this observation. We have corrected it.

4) Discussion, tenth paragraph: “showed higher mortality rate in affluent areas 20” should be “higher mortality rate in affluent areas”
We have corrected our mistake.

5) Table 2: “1066” should be “1,066”. In addition, it would be appropriate to use the same number of decimal. In the column "Men-low" number of deaths stratified by age is equal to 956 while total of deaths is 955. In the column "Women-low" number of deaths stratified by age is equal to 591 while total of deaths is 592. Finally, the value 0.83 (CI: 0.76 to 0.89) should be in bold type.

Thank you for these corrections. We added the separator (,) for thousands in the Table 2. We adjusted all rates and ratios, using the two decimal places. As we eliminated the age groups “0-9” and “10-19”, the sum of deaths changed. Anyway, we assured that number of deaths stratified by age is equal to the total number of deaths. Finally, the new rate ratios that showed statistical significance are in bold type.

6) Table 5: Last footnote is incorrect

Thank you so much for your careful reading. The last footnote was deleted.

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

Declaration of competing interests:
I declare that I have no competing interests
The Associate Editor wishes to pass on the following comments:

Dear authors,

We thank you for this interesting and well written manuscript that is of potential interest for BMC Public Health. In addition to the reviewers' comments, we would like you to address the following issues:

- Could you please delete all references to “statistical significance”? Please do not indicate that differences are “statistically significant”, just indicate that “there were differences”. Do that in the abstract and main text. Please see the following for information:
  In both abstract and main text, we deleted all references with this expression “statistical significance”, as you recommended. We only wrote if there are or there are no differences. Thank you very much for sending us this editorial and the articles related to.

- It would be useful to know the distribution of the population size in the 49 health center service areas: eg mean population size and minimum, maximum, and 25th and 75th percentiles of area population size.
  We included information about the median, minimum and maximum of the population size in the methods section.

- In the limitation section, indicate why you were not able to consider more local areas.
  We considered all local areas. The Municipal health sector divided the total area of the city into 49 health care unit areas in order to provide better assistance to the population.
  We included one sentence in methods sections to explain this item.

- Perhaps the Discussion is too long. Is there a way to shorten it without losing important information?
  We tried to shorten the discussion, eliminating excesses. But, unfortunately, as the reviewers asked for more information (e.g. to include the explanation about aging index, one paragraph about respiratory diseases, other about the reasons of women have higher mortality rates than men, so forth), our discussion became longer.

- Here and there, there are residual grammatical mistakes in the article. Could we ask you to ask to some English colleague to carefully check the manuscript? I would like to remind you that there is no text edition at BMC Public Health before publication.
  An English teacher from US helped us to correct our manuscript.

Sincerely yours,

Basile Chaix, Associate Editor
We also need you to make the following changes:

1. Please document within your manuscript if the data you have used is openly available. If it is not openly available, please document the name of the ethics committee which approved its use. We would be grateful if you could address the comments in a revised manuscript and provide a cover letter giving a point-by-point response to the concerns. At the end of methods section, we added a sentence in order to explain that all data used in this manuscript are freely and universally accessible online. Then, ethical clearance was not necessary.

Please also ensure that your revised manuscript conforms to the journal style (http://www.biomedcentral.com/info/ifora/medicine_journals). It is important that your files are correctly formatted.

We look forward to receiving your revised manuscript by 23 November 2011. If you imagine that it will take longer to prepare please give us some estimate of when we can expect it.

You should upload your cover letter and revised manuscript through http://www.biomedcentral.com/manuscript/login/man.asp?txt_nav=man&txt_man_id=1313538213595689. You will find more detailed instructions at the base of this email.

Please don’t hesitate to contact me if you have any problems or questions regarding your manuscript.

Thank you.

With best wishes,

Jim

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To submit your revised manuscript
When you have revised your manuscript in light of the reviewer's comments and made any required changes to the format of your paper, please upload the revised version by following these instructions:

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4. With the 'Upload files' tab, please upload the revised version of the manuscript and press 'Submit new version'. Please wait for the confirmation page to appear - this may take a few moments.
Dear editors and reviewers,

Beyond all changes requested by you, we made other alterations, which are cited below:

- We added a paragraph after the limitations section to explain about the high quality of mortality data in Campinas. Although the reviewers hadn’t required this information, it is very important to add it, because ensures that our estimates are not underestimated or overestimated.
- We replaced the reference number 41 (old reference number 39) by an article that has just been accepted in a Brazilian journal. Thereby, readers will be able to find the reference that we are mentioning.
- We added two references to better answer the questions of reviewers. By doing this, we had to reorder the references numbers.
- As a reviewer asked to translate the articles titles (written in Portuguese) into English, we added “[In Portuguese]” at the end of each reference corrected.
- In the Acknowledgements section, we included two names to which we would like to thank.

The authors