Author's response to reviews

Title: SRH and HrQOL: Does Social Position Impact Differently on Their Link with Health Status?

Authors:

Cyrille Delpierre (cyrildelpierre@yahoo.fr)
Michelle Kelly-Irving (kelly@cict.fr)
Mette Munch-Petersen (mette@munch-petersen.dk)
Valérie Lauwers-Cances (lauwers@cict.fr)
Geetanjali D Datta (geetanjali.datta@umontreal.ca)
Benoit Lepage (lepage@cict.fr)
Thierry Lang (lang@cict.fr)

Version: 2 Date: 15 December 2011

Author's response to reviews: see over
December, 17th 2011

Dear Editor,

Thank you for your response and your review. It is our pleasure to submit a new version of our paper entitled "SRH and HrQOL: Does Social Position Impact Differently on Their Link with Health Status?", integrating most of the reviewers critics. The number of words is now of 4210.

We carefully considered every point raised by the reviewers. All the modifications are presented in the response to reviews and appear in red in the manuscript. Below are comments made by reviewers and the modifications we made to the manuscript or reasons why we did not make the requested modifications.

I hope that we answered to all of your requests. This new version of the manuscript has been reviewed, and approved by all contributing authors.

Sincerely yours,

C. Delpierre,
37 allées Jules Guesde 31073 Toulouse, France
Tel: 05-61-14-56-33
Email: delpier@cict.fr; cdelpier@hsph.harvard.edu
Reviewer's report

Title: SRH and HrQOL: Does Social Position Impact Differently on Their Link with Health Status?

Version: 1 Date: 6 November 2011
Reviewer: Min-Woo Jo

Reviewer's report:
This study is meaningful because it examined the influences of socio-economic positions regarding physical health conditions on health outcomes such as self-rated health and quality of life in the national representative sample. However, in order to validly derive a conclusion as what authors formulate, I think authors should consider the following several points.

- Major Compulsory Revisions

Authors excluded participants younger than 18 years old but I think authors should exclude people younger than 24 or 29 years old. A considerable people among young adults aged 19~24 or 29 will be well-educated people (i.e. >12) so including young adults might aggravate a biased conclusion.

We wanted to study the relative effect of education on the association between physical health conditions and SRH/HRQoL, independently of the age effect. But we agree with the reviewer that because younger people are likely to have a higher level of education it could aggravate a biased conclusion. That is why all our analyses are adjusted for age and we think that this strategy is better than excluding a part of the population.

Also, when household income was considered, researchers should think the number of household member. Who are more rich between one person who earns €25,000 annually and a family of four that makes a money €50,000 during same period?

We agree with the reviewer that taking into account the number of household members may be more relevant than household income. In consequence, all the analyses were performed again using income per consumption unit. Results regarding income were modified in consequence in the results part and in the tables. Whatever the definition used for income, the main conclusions were mostly unchanged.

We added these sentences in the paragraph “socioeconomic position”, page 10: “Annual household income per consumption unit corresponds to the total income reported within the household divided by the number of consumption units of the household. The OCDE scale gives a weight of 1 to the first member of the household, a weight of 0.5 for any other adult and a weight of 0.3 for any child of less than 14 years. It was categorized in 4 classes according to quartiles (< Euro 9,900; 9,900-14,300, 14,300-20,400, > Euro 20,400)"

In addition, authors should consider other confounding factors for SRH and QOL. Various factors including comorbidities that can be obtained from the NHS can affect SRH or QOL so those factors should be included in the multivariate model.

First of all, regarding comorbidities, it has been shown that people with low SES are more likely to have more comorbidities. In consequence, the probability to report poor health because of comorbidities is higher for them. However we observe that the relative impact of having a physical health condition is higher for people with high SES. Thus we think that comorbidities are not likely to bias our results.

Moreover our paper is focused on the effect of socioeconomic position on the association between physical health conditions and SRH/HRQoL Of course many factors can affect subjective health but we did not want to analyse these factors. Only those affecting both subjective health and socioeconomic factors were therefore interesting to us. This is why we adjusted for age. Some other variables could influence this relation, like ethnicity, but such data were not available in our study.
Authors provided ‘% adjusted for age’ in all tables, what are the values? Are they standardized value according to the number of age group in the specific year or expected values from the age-adjusted model? Authors must provide more explicit descriptions in the manuscript and tables.
This is the expected values from the age-adjusted model.

Minor Essential Revisions
In Table 1, units of PCS and MCS are not %.
% was replaced by mean in the table 1

- Discretionary Revisions
Authors described that “Social gradient was steeper for FL than for low back pain, with both socioeconomic indicators (education and income)” in page 10 but it might be not correct. The amount of steepness can be dependent on the classification of variables therefore I think that those expressions can be somewhat conclusive.
In table 1, the difference in the prevalence of FL between the highest educated people versus the lowest group is stronger than for chronic low back pain (14.4% vs 7.6% for FL and 15.3% vs 12.3% for chronic low back pain, in men). This is also true for women and when using income instead of education. That is why we consider our sentence to be correct.
Reviewer's report

Title: SRH and HRQoL: Does Social Position Impact Differently on Their Link with Health Status?

Version: 1 Date: 9 November 2011
Reviewer: Jennifer Dowd

Reviewer's report:
This paper uses data from a representative French national health survey to test whether the association of self-rated health (SRH) and Health-Related Quality of Life (HRQoL) with two more functional limitations and chronic back pain differs by socioeconomic position (SEP). The paper is motivated with reference to the hypothesis that different social groups may have different health expectations that would lead them to translate specific health conditions into subjective health status differentially. “As expectations seem to be higher among people with high SEP, the same disease may have a more negative impact on SRH among them than among people with low SEP.”

Major Compulsory Revisions:
While this question is of large general interest and has been addressed in several different contexts to which this French dataset could be a new contribution, I have reservations about the paper for the following reasons. First, the authors are not transparent enough about how their logistic and multivariable regression models translate into the numbers in their tables. Partly due to this problem, the empirical findings are not consistent with the conclusions of the authors and how this reflects on the broader literature regarding the use of subjective health measures to study social inequalities.

1.) Specifically, I have a problem with the interpretation of results such as:
Page 10: “In age-adjusted models, the odds of reporting poor SRH in case of lower back pain were higher in more educated men than in those with lower educational attainment.” From my reading of the tables and implied models, this statement is either wrong or misleading. Based on Table 2, given that the proportion reporting poor SRH in the presence of back pain is 28% for the highest education group while it is 47.8% for the lowest education group, how can the odds for reporting SRH be higher for those with the most education based on these models? The model used and the method for calculating odds based on the model coefficients needs to be explained more specifically. Are these odds calculated adding the main effects of education plus the interaction effect? It seems rather that the ORs might reflect just the interaction effect, which does not accurately describe the results statement above. If only the main effect is described, you are only describing a relative increase in odds associated with back pain compared to those in the same education group, not compared to a different education group.

This is a very important point and we agree that the statistics are not clear in the text. As said by the reviewer, the proportion of people reporting poor SRH is higher among people in low SEP in absolute term, regardless of the presence of a health condition. However, the relative increase of this proportion in the presence of disease is higher among people with a high SEP, as measured by the interaction term. Thus the interaction term was tested in a multivariate model in which was included a term for health condition (FL or low chronic low back pain) and a term for socioeconomic position (for ex: FL*education). As some interactions were significant, we presented results by stratifying by socioeconomic position and then calculated odds to report poor SRH in the presence of a health condition (FL or low back pain) in each socioeconomic group. The same was done with HRQoL instead of SRH.
We rewrote the section on statistics to render it clearer and also modified the presentation of tables 2, 3, 4, as their subscripts.
2.) Similarly in Table 4, it is not clear what “regression coefficient” represents—is it the coefficient on functional limitations alone or the interaction between the highest education category and functional limitations compared to the lowest with functional limitations? Please be explicit about your model and what coefficients are being reported. The subscripts in the table state the comparison groups, but it is still not clear whether by result for education <12 compared to education=12, for example, includes main effect of education + interaction (main effect of FL drops out)?

The same approach as that used with SRH was used with HRQoL. We tested the interaction between health condition and socioeconomic position and as some of them were significant we presented the decrease (“regression coefficient”) of PCS and MCS score in the presence of disease stratified by socio economic group. As said before, we rewrote the section on statistics to render it clearer and also modified the presentation of tables 2, 3, 4, as their subscripts.

3.) Because of these problems in conveying the results of the empirical analysis, it is difficult to assess the contribution of the paper to the literature in this area. My impression is that the conflicting results in the paper are a result of these problems with interpretation and may not reflect the underlying results from these data. Looking at the tables in which the scores are stratified by education level, it is quite obvious that even with the same SRH or HRQoL, the lower one’s education the more likely they are report FL or back pain. This would seem the most important result to answer the authors basic question of whether subjective health measures accurately compare health status of different social groups. Looking at the data in this way, the use of the subjective measures underestimates health inequalities in both instances, unlike the conclusions of the authors (Page 6) that inequalities are underestimated for SRH and overestimated for HRQoL.

Regarding the relevance of our strategy, as said in our introduction, our objective is to assess whether the socioeconomic position had an influence on the relationship between health conditions and subjective health status and to assess if this influence was different by using SRH and SF-36 as a measure of subjective health. To do that, it seems to us that analysing the interaction term between SEP and health conditions on subjective health is a good approach. Our results show that the presence of health conditions leads to a bigger relative increase in the proportion of people reporting poor SRH among people with a high SEP. Thus in the case of disease, the relative inequalities regarding SRH between people in high and low socioeconomic position is decreased. The results were in the opposite direction by using HRQoL instead of SRH. As we adopt the same analysis for SRH and HRQoL, it is unlikely that the different influence of socioeconomic position on the relation between health conditions and SRH and between health conditions and HRQoL is an artefact due to statistical methods.

Our conclusions seem to us correct and our methodological approach relevant when aiming to show that analysing social inequalities by using SRH or HRQoL as a measure of health can lead to under or overestimations.

We agree with the reviewer that for a same level of SRH or HRQoL, people in low SEP are objectively sicker than people in high SEP. Thus by using SRH to measure health inequalities, the risk is that inequalities are higher by using objective measure of health than subjective measure of health. Our approach, which leads to the same conclusion, uses a different method (relative approach not absolute). We show that in the presence of disease, the impact on SRH is worse among people with a high SEP. Thus although people with high SEP are less sick than their counterparts, they are more likely to report the impact of the disease on SRH than people with a low SEP. Thus by using SRH for measuring social inequalities in health, we underestimate the values of inequalities compared with measures of morbidity.
Minor Essential Revisions:

4.) Results section—it seems that the authors refer to p=.12 as significant in the paragraph about men but not significant in the paragraph for women? The sentence “the interaction test being non significant” was added in the third paragraph page 10 to precise that the test was not significant.

5.) “length of education” is awkward wording—level or years of education? Length of education was replaced by years of education

6.) First paragraph page 10—“length of educational and income” –grammar Should be “The social gradient.” No punctuation at the end of the paragraph. Punctuation was added. Lower length of educational and income was replaced by “the social gradient” as requested.