Author's response to reviews

Title: Socioeconomic environment and cancer incidence: A French population-based study in Normandy

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Author's response to reviews: see over
Author's covering letter for initial submission

Title: Socioeconomic environment and cancer incidence: A Franch population-based study in Normandy

Authors:

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Comments: see over
Dear Editor,

We are pleased to submit you a revised version of the article entitled “Socioeconomic environment and cancer incidence: a French population-based study in Normandy”, submitted at first on July 10, 2013 and referenced 9961757310314104. As you suggested, we have addressed all of your concerns taking into account all of the recommendations of the reviewers. Please find below the revisions made in response of the reports of the reviewers.

Referee 1: Jose Leopoldo Ferreira Antunes

This study assessed socioeconomic inequalities in cancer incidence in two regions in Normandy, France, from 1997 to 2004. There is no news in acknowledging differences in the incidence of tumors in specific anatomic locations, and that some cancers have a higher incidence and mortality have been extensively reported worldwide during the last decades. Authors justified their assessment by saying that incidence has been less studied than mortality. I argue that the relation between cancer incidence and socioeconomic status is dynamic, and needs to be continuously monitored, thus justifying the assessment of contemporary population settings.

We have taken into account this remark and have corrected the introduction by deleting the statement that the study is innovative; and adding a statement on the importance for the relation between cancer incidence and socioeconomic status to be continuously monitored.

Acknowledging socioeconomic inequalities in health is important to improve theoretical models. For many health outcomes, socioeconomic status must be a distal determinant in explicative models assessing specific proximal aetiological hypotheses. We cannot wait socioeconomic differences to disappear; we must comprehend why deprivation associates with disease. What poor people do that puts them into a higher risk of disease? The study of socioeconomic inequalities in health may contribute to spot opportunities for effective cancer prevention. I argue that concluding that health actions should target the deprived,
rather than the global population, is a poor justification for the study of health inequalities. Rather than being in competition, targeting and universal approach are strategies to be combined and jointly used in health promotion. There are several literature reports on health interventions that have contributed to reduce health disparities for having been implemented in such a way that the universal approach was effectively and immediately ensured.

We have supplemented the discussion section by focusing on the fact that socioeconomic status is one element that has to be targeted for effective cancer prevention. We have taken the second comment into account and changed the discussion section by emphasizing the need to have policies that combine targeting and universal approaches.

Please provide background information on Calvados and Manche.
At the beginning of the “Study population” subsection, we have included demographic and economic information on the populations of Calvados and Manche.

I wonder why the study period ended in 2004.
When the study began, the available geocoded data were for the period 1997-2004. This period is now extended to 1997-2009 so we have performed new analysis on the study period 1997-2009 and the new results were included in the article. The results are almost equivalent, except for the following differences: In males, (1) cancer of the bladder, liver, central nervous system, gall-bladder and unknown primary sites have higher incidence among the disadvantaged; (2) melanoma have higher incidence among the advantaged; (3) kidney cancer is no longer significant. In females, (1) unknown primary sites have higher incidence among the disadvantaged and (2) liver cancer is no longer significant.

I do not agree that a study can be French at all. Studies are international by principle. Researchers do have nationalities; research does not.
We are well aware that this study is international and even if the data come from a specific region of France, the results enrich the international knowledge on social disparities in cancer incidence.

I doubt that the area-level assessment is reliable. IRIS areas seem to be too small to allow for a stable estimate of site-specific cancer incidence. I suppose that many areas would have too few cases (or none at all) for several types of cancer, which prevents stable estimates of incidence. I also suppose that authors knew that, and judiciously avoided reporting descriptive information on incidence for site-specific tumors at the small-area-level.

We used IRIS for the area-level assessment because we wanted to be able to detect the heterogeneity in deprivation between areas. By extending the study period to 1997-2009, we have reduced the number of areas with few cases; it does remain to be an issue. With respect to the comment that we avoided reporting descriptive information on incidence by site-specific tumors, we believe this information was sufficiently conveyed in table 1 of our original submission. Table 1 provides the frequencies of each tumor site for the entire study area. Knowing that there are 1496 IRIS, the reader can have an idea of the number of areas with few cases.
Referee 2: Alex D McMahon

The IRIS areas are large with up to 4811 inhabitants. Is this best that can be done in France? Other countries can do record linkage on an individual basis using deprivation scores for much smaller areas.

We confirm that the IRIS area is the smallest area available in France. Indeed, even if the maximum reached 4811 inhabitants, the average is still 755 inhabitants.

The authors should concede that methods from the area spatial statistics had to be used because of the nature of the data. I wouldn’t like peer reviews of other cancer registry analyses bringing this up as a suitable method, just because it can be ran through WinBUGS.

The use of this method has been motivated by the spatial nature of the data. There were extra-Poisson variability in the data and the Bayesian approach permitted the integration of it, avoiding the risk of underestimating the standard error and wrongly concluding a significant effect of deprivation on cancer incidence.

The downside of this method is the presentation of metrics that are not very useful, like the EDI coefficient. The main results (see abstract) are actually the PAF results, which don’t have 95% CIs.

To complete the EDI coefficient, we have put in the tables the exponential of the EDI coefficient which reflects the excess risk related to EDI. We couldn’t provide PAF CIs because they are not easily calculable, just an approximate 95% confidence interval can be obtained using delta method or bootstrap resampling techniques (Laaksonen M. Population Attributable Fraction (PAF) in Epidemiologic Follow-up Studies. National Institute for Health and Welfare 2010)

Point 3 leads on to a request for a presentation of the actual RRs that are used in calculation of the PAF. These would provide information on absolute and relative inequalities, and can have 95% CIs.

We have included in the article all the RRs used for the calculation of the PAF with their 95% CIs which provide information on the variability of the PAFs.

The method for calculation of the RRs should be explained better. For example, some detail and further description should be provided on the alpha parameter in 1.3. Explanations concerning the calculation of the RRs were added to the article. In the previous paper, alpha was missing in the equation in 1.3 subsection. This mistake has been corrected.

I think that clustering and auto correlation are the selling points of the method. Over dispersion isn’t really a problem with aggregated date. The underlying cohort of individuals could be under dispersed and the researcher would never know.

We have noted this remark but we haven’t included it in the article because it doesn’t suggest any changes.
There is a sound argument that a Bayesian analysis is only truly Bayesian if informative priors are used. The analysis in the paper is just using an available technology that has a Bayesian capability.

We have noted this remark but we haven’t included it in the article because it doesn’t suggest any changes.

We hope you will find our manuscript of interest.

Yours faithfully,

Joséphine Bryère