**Author's response to reviews**

**Title:** Trends in prevalence of four modifiable ischaemic heart disease risk factors by educational level and gender in The Nord-Trondelag Health Study 1984-2008

**Authors:**

Linda Ernstsen (linda.ernstsen@hist.no)
Bjørn Heine Strand (BjornHeine.Strand@fhi.no)
Sara Marie Nilsen (sara.m.nilsen@ntnu.no)
Geir Arild Espnes (geirae@ntnu.no)
Steinar Krokstad (steinar.krokstad@ntnu.no)

**Version:** 3  **Date:** 18 March 2012

**Author's response to reviews:**


Response to the Editor and the reviewers (in total 5 pages including comments with accompanying response)

**Editor's Comments:**

1. The three long paragraphs under 'Comment' are difficult to follow through because of their length (one page per paragraph) and the lack of focus. These paragraphs should be split into smaller paragraph with each a clear key point and conclusion.

Authors’ comments: We have restructured the “Discussion” with sub-headings related to each of IHD risk factors. Further, and as suggested, we have made concluding remarks at the end of each paragraph. However, due to suggestions from the reviewers, we have not succeeded in making each paragraph any shorter.

2. The comment on the link diabetes trends and smoking cessation is highly speculative. This link is indirect at best, through a possible effect of smoking cessation on overweight. The only cited evidence comes from a controlled trial, but not a population-based study on trends or inequalities in DM. I recommend removing this part of the discussion.

Authors’ comments: We agree (this was also commented by the second reviewer) and this part is now removed from the discussion.

3. The authors are requested to check for grammar errors that may occur at a few places
Authors` comments: The manuscript has been edited for language before resubmission, thus we hope that it is totally free from any grammar (or language) errors.

Reviewer 1 Michael Davidsen:

Reviewer`s report

This paper shows results on trends in prevalence and educational inequalities in four modifiable risk factors for IHD from a major Norwegian Health Survey. The paper is very thorough, read-worthy and coherent and a sound addition to scientific knowledge.

Minor essential revisions

1. Most of the intro is written in present - however the objective of the paper is stated in the past ("... was..."). Please check for consistency throughout the paper.

Authors` comments: We have stated the aim of the study (in the abstract and at the end of the background) in the past (was) in relation to the traditional way of presenting a study. However, we believe it is common practice that some arguments in the Background-section may be written in present.

2. Education as a proxy for socioeconomic position

In one sentence It is stated that highest education is used and in another that ISCED is used. I suggest you combine this in one sentence - highest education is measured using ISCED.

Authors` comments: The formulation has been revised.

3. IHD risk factors

First paragraph: it is stated that you have hypertension if DBP is <90 - it should be >.

Authors` comments: The punctuation error has been corrected.

4. In the statistical methods the words 'the individual with' is repeated.

In the comments on diabetes, there are some sentences which are a bit hard to read. Please check.

Authors` comments: We have rephrased some of the formulations in the statistical part and the comments on diabetes have been revised.

Discretionary revisions
1. The main title of the paper pertains to trends in prevalence. However the chosen model focuses on trends in educational inequalities. You should consider changing the title of the paper to reflect this fact.

Authors’ comments: We agree. We hope that the new title is found to be more in accordance with the content.

2. Introduction last paragraph: ‘For example, diabetes is increasing worldwide.... ’ - the following seems to be not an example but more some kind of rationale for the paper.

Authors’ comments: We agree. We have removed the phrasing ‘For example’.

3. Material and methods, study population
   The sentence on number excluded and participating in HUNT1-HUNT3 is very hard to read - please clarify. You may consider using a table. And please drop the years of investigation, it is repeated too many times.

Authors’ comments: We have restructured this paragraph and revised it as suggested.

4. Statistical methods
   Second paragraph: Please consider making the description of computing ridit scores a bit shorter and instead make a reference.
   Third paragraph:
   It is stated that RII and SII are education-specific - I do not understand this..
   The interpretation of RII and SII are nicely presented. However doesn't this conflict with the ridit-scoring where the person with the highest education has the loest ridit and persons with loest education the highest ridit?

Authors’ comments: We have discussed this suggestion, however, as most readers may not be familiar with RII and SII we believe that the description should be as it is. We also believe that our description of the ridit score is in accordance with the description on how to interpret the RII and SII.

5. Comment
   A general comment: the authors use two measures of inequality, one relative and one absolute. This makes sense because the two measures due to different assumptions (1) may show different results. This fact is not mentioned in the paper.

Authors’ comments: We agree. We have argued for the measurement of both absolute and relative inequalities in the last part of the Background.
6. Strengths and limitations
It is stated that SII is low when the prevalence is low even with high relative inequality - please provide a reference.

Authors` comments: We have rephrased this sentence and added a reference.

10. Throughout the paper I found the term 'educational attainment', 'educational level', educational difference, social disparity and social position. Please check that you use the terms as intended.

Authors` comments: We have revised the manuscript and do hope that our use of terms is clear.

Reviewer 2 France Lert:
Reviewer`s report
This paper describes trend in social inequalities in 4 major risk factors of ischemic heart disease in Norway in 3 rounds of the HUNT survey over a 22 year period among the 40-59 population. Social status is measured by educational attainment. The analysis is stratified according to gender. The social gradient is assessed using RII and SII.

The analysis is properly performed and such description is useful to assess situation in a range of countries with different social structures, health care and insurance systems and health policy.

Essential revisions
1) However some information is lacking regarding the HUNT study: information should be given about the Nord Trøndelag county is representative of the Norway social situation (population size, social and economic features etc.) as authors pointed out that the findings are not necessarily generalizable to the entire country (p14)

Authors` comments: We agree. We have added some more information about the Nord- Trøndelag county.

2) The authors chose the 40-59 population to study changes in health behavior. This choice is questionable since behavior patterning occurs early in life. As
regards future trends younger age group might have been more relevant. The authors should explain why they did not use younger population data to assess social trends in health behavior.

Authors` comments: We agree. The reason behind our choice of using middle aged participants was decreasing response rate among the youngest and the elderly. This is now clarified in the paragraph “Study population”.

3) As regards smoking behavior in women and men. It is not only the social diffusion of health behavior which is at stake but rather a trend towards a narrowing of difference in behavior between men and women related to gender issues.

Authors` comments: We have added e sentence with a reference on narrowing differences in behavior between women and men (last paragraph under the sub-heading “Smoking” in the discussion).

4) The fact that diabetes is increasing in both men and women is attributed to weight gain after smoking cessation and not to other factors such as nutrition based on Davey-Smith’s paper (reporting an intervention trial), which is questionable and should be further discussed.

Authors` comments: We agree. As suggested by the Editor, this part of the discussion is removed.

5) The impact of diagnosis and treatment of hypertension and cholesterol and their social gradients should be further discussed since medical intervention could narrow or widen social inequalities.

Authors` comments: We have made new comments on medical treatment (with references) in each of the paragraphs, hypertension and high total cholesterol, in the discussion part.

6) In their conclusion, author should give some insight on the mechanisms underlying the observed trends.

Authors` comments: We have tried to meet this request by rephrasing some of the conclusion in the manuscript and in the abstract.