**Author's response to reviews**

**Title:** Changes in health care utilisation following a reform involving choice and privatisation in Swedish primary care: A five-year follow-up of GP-visits.

**Authors:**

Anders Beckman (anders.beckman@med.lu.se)
Anders Anell (anders.anell@fek.lu.se)

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**Author's response to reviews:** see over
Cover letter – review
Dear editor

We are grateful for the time and effort of the reviewers and we have tried to enhance our manuscript according to their suggestions. Please see below for our point-to-point response (Q=question, A=answer):

Referee 1, Igor Svab
Q: The authors should explain a bit more about the reform of primary care in Sweden and how it was implemented. The theory seems OK, but the exact mechanisms of the reform should be more clearly described, because the average reader is not knowledgeable about the Swedish specificities
A: We have added an explanation of the Swedish health care system in the introduction. We also added information about mechanism of reform, particularly related to Region Skåne.

Q: More international references on utilisation of primary care would be welcome.
A: We have added a few references on utilisation of primary care, but Swedish primary care differs from other countries in terms of a relatively low use of primary care and comparison is not straightforward.

Q: Please add translations of titles of articles in Swedish.
A: Translations are added

Referee 2, Kaja Polluste
Q: Authors describe the strengths of their study, but are there also any limitations?
A: Limitations are considered in the Discussion section

Q: Table 1 on page 11 should be replaced with Table 2.
A: Table numbers are corrected

Q: Please explain the choice of the range of age (25-84). Why were not included the age groups 18-24 and people aged 85 and more?
A: An explanation is given below and in the manuscript

Referee 3, Bo Burström
Q: The analyses were stratified by age groups (25-44, 45-64 and 65-84 years). It is not clear exactly why these age groups were chosen – was there empirical grounds for this division?
A: An explanation is given in the description of variables. This stratification has been used before by one of the authors.
Q: Income is considered as a proxy for health needs, which may be reasonable. However, no mention is made of other potential candidate measures (e.g., education).

A: An explanation is given in the discussion.

Q: Furthermore, income has been used as a dichotomous variable (below/above median income) – why not use income quintiles to allow more variation, and to enable studying potential linear relations between income and utilisation? On what grounds was it decided to dichotomise income?

A: Comments are added in the discussion about the use of dichotomised income. Dichotomous variables have been used before by one of the authors.

Q: The income measure is presented as family income, but no mention is made of whether it is equivalised for family size, the number of family members who live off this income. This should be specified.

A: Family income was not equalised regarding family size. This is specified in the variables section.

Q: The underlying outcome is number of visits, but the analyses report odds ratios for the likelihood of one or more visits. Were any analyses made of changes in the average number of visits per individual in different groups?

A: Average number of visits is calculated and a new table is inserted. This is commented in the results and discussion section.

Q: As it stands now, the regression analyses present more of changes in the likelihood of making one or more visits, which would indicate that a greater proportion of persons in a particular category have made one or more visits – not counting those who already made one or more visits in 2007/2008 and also in 2010/2011.

A: We are not clear on what the referee means, but the data on visits per individual in different groups is calculated. This is now included in the manuscript. However, for this manuscript our main purpose was to show the relative effect of choice, depending on income and not changes in average numbers.

Q: Analyses used multilevel logistic regression – in order to account for repeated measures in the same individual. Was this the only analytical method considered? Did the authors consider looking at the average number of visits in different groups?

A: See above

Q: The increase in number of visits was twice as high among men as among women, and highest among men aged 45-64 years. This is not further commented – was this expected?

A: A sentence was missing. The increase was for visitors and this has been corrected and commented.
Q: There are two table 1, the second one seems to be referred to as table 2. As the tables partly contain the same information, they could be combined.

A: The tables are now combined and the numbering corrected

Q: There seems to be an error in the calculation of visits to GP-all for men 65-84 years – the percentages do not seem correct in relation to the numbers, this should be checked.

A: There was an error in the absolute number of the change in visitors and this is corrected.

Q: In table 3 one line describing variance (women 25-44) is empty, variance is missing.

A: Values of ICCare inserted

Q: Figure 1 and figure 2 provide little information, as there is no interaction in table 3 for this age group. The interaction with age and income is little described, how do the authors interpret this finding?

A: Figures 1 and 2 shows no interaction, as there is no interaction. However, it well illustrates the difference between income groups. The interaction between choice and income is only detectable in the oldest age group, where individuals with family income above mean benefit more from choice than individuals with income below mean. This is commented in the discussion.

Q: The authors state that individuals with high income are more likely to have made a visit to a GP after the choice reform, and the establishment of more private primary care providers in the region – were any analyses done of the impact of new private providers?

A: We have added information about average and median CNI (social deprivation index) for the new private providers compared to average and median for all providers. The lower index for new private providers suggest that they have generally not established in socially deprived areas.

Q: Were any geographical analyses done relating the establishment of new providers to increases in the number/likelihood of visits?

A: No, but we have added information about CNI for new providers that gives a more precise measure (and indirectly a measure about geographical location).

Q: The authors also mention differences in health literacy between groups as one possible explanation for the results. Were there any analyses of differences in abstaining from seeking care between groups?

A: In our interpretation the question of abstaining is the antithesisof our study and not explicitly investigated.

Q: The authors reiterate that income is a good indicator of health – again it is not evident why the analyses have not used more variation in the income measure.
A: This is commented in limitations in the discussion section.

Q: In summary, the topic and the study are interesting but the manuscript would need some revision in response to the comments given. It would be interesting to see sensitivity analyses using income quintiles rather than below/above median; and analyses of the average number of visits before and after the reform rather than only odds ratios. Tables and figures should be revised according to the comments.

A: See individual answers above.

Q: The authors could expand on the explanation of geographical differences in establishment of new practices – are there analyses to support that?

A: We have added data and analysis of this, see above.