Reviewer's report

Title: Assessing social cost of cancers due to nitrate in drinking water in the EU for the case of colon cancer

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Reviewer: Mikael Skou Skou Andersen

Reviewer's report:

In general this is an innovative article addressing an important issue, namely how to arrive at figures for the external costs related to nitrate in drinking water. The article is well written and informative.

The article takes epidemiological results for the potential relationship between nitrate and colon cancer as the starting point for an analysis of the social costs related to nitrate in drinking water. While there are few problems with the part of the article that presents and applies monetary valuation for lost life years, the underlying derivation of an exposure-response function for colon cancer as well as the modelling of nitrogen losses is found to be lacking in some respects.

- Major Compulsory Revisions

1. The colon cancer risk derived from the article by De Roos et. al. implies that for the sensitive groups, the number of colon cancer is simply doubled in areas where for drinking water the nitrate threshold is assumed to be exceeded. This somewhat simplistic risk estimate is a problematic starting point in relation to the aim of assigning a damage estimate per kg N used in agriculture (the figure of 0,7 euro per kg N mentioned in the abstract) and hardly allows for a conclusion on the disaggregated damage cost per kg N.

2. While the ms. refers to the article by Gulis et. al. and notes reasonable consistency to results for colon cancer, it fails to explore to which extent Gulis could be taken as a more appropriate starting point for deriving an actual exposure-response function for colon cancer in relation to nitrate content in drinking water. More effort should be devoted to justify the choice of De Roos. Are there design flaws in the Gulis study? Are further epidemiological studies available that would allow the authors to derive an exposure-response function on basis of a broader meta-analysis of available evidence, rather than picking simply one – incidental? - study? If the De Roos study should be regarded as a particular strong or reliable study, the article needs to justify better. Brief reference to the relative merits of case control studies versus cohorte studies in the context of nitrate would be helpful for the readers.

3. p. 3: the need for epidemiological studies: are there shortcomings or flaws in the Iowa womens cohorte study? (Weyer et. al)

4. p. 5, line 6: “nitrate via drinking water is about 4 times smaller than amounts
from food" – reference (6) is a magazine – while De Roos states that drinking water can make up as much as 50% in cases of non-compliance with MCL guidelines. Please clarify.

5. p. 6, line 3-4: I found this statement overtly conservative, given that relations to bladder and ovarian cancers were identified in the Iowa cohort study. Again, why is the EFSA magazine referred to, when nitrate is an environmental issue – please check other EU authorities for statements on nitrate and health. The World Bank used the Weyer et al. results for its study “Cost of pollution in China”.

6. Section D: I found this important section in the article to be extremely brief and uninformative in relation to other sections. A balanced article needs to devote more effort to explore and explain how the results for nitrogen leaching are arrived at, as this is an important area of research in its own. Reference to existing models and approaches and a more reflected choice of approach would need to be included.

7. Section D refers to the concept of ‘mean nitrogen leaching intensity’ with reference to figure 3. Again a dichotomous use of standard exceedence is used, this time it leads to a logarithmic relationship to leaching intensity. No explanation is offered as to how the mean nitrogen leaching intensity was calculated more specifically, which would be required.

8. p.9 Why is reference done to private well depth? Is this because the procedure in section D is used only for the part of exposure related to smaller drinking water supplies? If so, this needs to be stated explicitly and not left to the guessing of the reader.

9. p. 9 “We did not consider temporal trends of nitrate in groundwater”. The MITERRA model by Velthof et. al. is referred to. This model appears to assume that there is a one-to-one relationship between nutrient loss and nutrient leaching – a quite strong assumption underlying the presented calculations and results, which requires some accompanying reflections. What is the possible margin of error introduced here? Would there be other leaching-models available without such strong assumption and why were their use not preferred? A crucial point to which the authors should add quite some text to explore and justify their approach. Does the MITERRA model for instance allow for appropriate differences in leaching between sandy and loamy soils – if not please clarify the limitations. Is MITERRA stronger on the air pollution aspects of nitrogen than on the leaching side?

10. Section E: the statement that data on exceedance of 25 mgNO3/l in large public water supplies are not available for member states other than Netherlands is impossible to accept. The Dutch figure stems from national data, and the authors need to check what data is available at member state level for other member states too. Such data can often be found summarised in English language in SOE-reporting or OECD environmental performance reviews.

11. Section F. “Implicitly we assume that the association with meat consumption
in the EU is the same as in Iowa”. In fact it is also assumed with the risk factor that meat consumption is at the same level throughout the EU as in the US corn belt – so the median threshold needs some correction in the case of Europe. I refer back to my remarks on De Roos versus Gulis and the need for a more informed meta estimate for colon cancer and nitrate, possibly with a more elaborate exposure-response function. Or perhaps the difference between results in Gulis and De Roos can be used to derive a correction factor for Europe if the meat relationship should be regarded as crucial.

12. p. 15: line 3-6: in consequence of the above remarks regarding as well the basic risk estimate as well as the leaching calculations the unit damage cost figure is not well consolidated, and the text should signal the difficulties with arriving at a per kg estimate on basis of the data and models employed here. The risk estimate procedure as well as the leaching modelling framework suggests that the figure would be an upper bound value. Inclusion of the figure in the article abstract might invoke misunderstandings without proper underlining of the uncertainties. The same goes for table 2.

13. p. 17 line 12-14: “The range of unit health costs for the 11 EU countries found in this paper is 1-7 euro/kgN nitrate leaching would then correspond to a range of 0,1-2 euro/kgN fertilizer input” According to tb. 2 the 1-7 euro is per capita! However, due to big differences in leaching between mineral fertilizer and manure-N table 2 would need to distinguish clearly what sort of N-application reference is made to.

14. p. 18 line 1-10: why only reference to fertilizer-N when in line 13 concluding also with respect to manure-N. A small table would be helpful to show the relevant figures now scattered in text.

- Minor Essential Revisions

15. p. 11, line 6 refers to figure 1, equation 2 – no such equation is there – (and list of figures + captions missing).

**Level of interest:** An article of importance in its field

**Quality of written English:** Acceptable

**Statistical review:** No, the manuscript does not need to be seen by a statistician.