

Interactive comment on “Projected change in atmospheric nitrogen deposition to the Baltic Sea towards 2020” by C. Geels et al.

Anonymous Referee #1

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General comment:

This manuscript describes the results of a modeling exercise where the effect of the new stricter European legislation on national emission ceilings on nitrogen deposition to the Baltic Sea is studied. The main research instrument is the well documented Danish DEHM atmospheric dispersion model which has been widely used in air pollution studies. The manuscript is quite straightforward and the work as well as the results are presented in an efficient manner. The chosen strategy involves the identification of a “representative” meteorological year during the period 1995–2004 whose meteorological data is then used to drive the model, first with the emissions of 2007 and then with the projected emissions in 2020. The contributions to the deposition load from the countries bordering the Baltic Sea and from shipping is assessed by using tagging

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techniques. Although some questions arise about their chosen approach, the authors argue their case rather well, and I also appreciate their discussion (in Chapter 2.4) on the uncertainties brought about by this approach (however, see also below). While this study considers the future nitrogen deposition caused by changes in anthropogenic emissions alone, it is my belief that also this type of simplistic model applications are needed, and may turn out to be valuable contributions to the evaluation of the effects of proposed environmental policies.

Specific comments:

Introduction in general: References need to be included in several places where the authors discuss the atmospheric fate (chemistry, transformation, lifetimes, incorporation into aerosol, deposition etc.) of the nitrogen compounds. As it is now, the authors make specific claims and present several details which cannot be considered common knowledge.

Please, be systematic in the introduction of the chemical compounds by their common names before using the chemical formulas to represent them (e.g. NO_x and NH₃ are used on p. 21535, line 19 while NO_x is introduced on p. 21535, line 24 and NH₃ seems not to be introduced at all).

Please, avoid using terms such as “N compounds” or “N components” instead of nitrogen compounds or nitrogen components (you might also keep in mind that you define N as “reactive nitrogen” on p. 21536, line 6–7). This comment also concerns the text of the manuscript in other places.

P. 21534, line 23: “(Krishnamurthy et al. (2010) and references herein)” – “herein” should probably be replaced with “therein”?

P. 21536, line 17: The last sentence of this paragraph is confusing. Please, spell out the “etc.” at the end and maybe also consider rephrasing.

P. 21536, line 21: What does “Such” refer to?

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P. 21536, line 28: The first sentence of the chapter starting here seems to state that the aim of this work was to study the deposition for 2010 “as well as” the deposition from the present day emissions (2007). I would rather suggest emphasizing that your aim was to study the changes in the deposition if the new emission ceilings are adopted. . .

P. 21536, line 29: Here the abbreviation (NEC) is used while in other contexts the new legislation is referred to as NEC-II. Maybe this is intentional for some reason? However, it seems a bit confusing.

P. 21537, line 1: I also don't like the statement that the DEHM model was used to calculate the change in the deposition – I would suppose that the model calculates the deposition(s) and the changes are inferred afterwards by comparing the model results for the different years.

P. 21538: Several chemical formulas and/or abbreviations are used without proper introduction.

P. 21538: line 25: According to this last sentence of this paragraph several particle size classes were used in the model calculations. Are particle densities a factor in the deposition calculations and what densities were used?

P. 21539: The resistance analogy method for the calculation of the deposition velocities of both gases and particles is described in one longish paragraph. The description is awkward to read, and the inclusion of the symbols for the various terms of the mathematical formulation only confuses the reader, as not a single equation is shown. Since deposition velocities are probably the most important detail in the model calculations of the deposition, it might be a good idea to present this in a more structured way: clearly separating the gases and the particles and including at least the most basic equations if their terms are discussed using the symbols.

P. 21541, line 3: In the last sentence a claim is made about the better accuracy of the tagging method compared to another. Are there any references for backing up this

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argument?

Chapters 2.2 and 2.3: The “reference meteorological year” was not identified via the analysis of the meteorological parameters over the chosen period (1995-2004) and picking the most average or representative year with respect to them, as one might expect. Instead, the most representative “deposition year” was identified, using the 2007 emissions and the actual meteorologies during the 10-year period as the basis of the selection. Would the end result have been the same (i.e. year 1998) if the reference year had been chosen simply as the average “meteorological year” based on meteorology alone and if not, how would it affect the modeling results? In addition, understanding the reasoning behind this basic decision is complicated by the fact that it is not clear from the text which average of the full 10-year period is meant on p. 21541, line 17, the actual average deposition during 1995-2004 or the average from the runs with the 2007 emissions? The same applies to the statement on p. 21541, line 18, of the +/-17% variability in deposition – it is not clear from the text if what is meant is the variability of actual deposition or the variability of the 10-year simulations with 2007 emissions. What is the logic behind the selection of 1995-2004 for meteorology and then the “reference emission year” 2007 outside of this chosen meteorological averaging period? Furthermore, if the purpose of selecting a reference meteorological year was simply to reduce the number of required model runs, was it ever considered to pick a set of years to make the present day calculations with actual meteorologies and realized emissions? An obvious choice would have been the years 1997, 1999, 2000, and 2003 and averages based on them, as they correspond to the years of the Baseline Scenario of the Clean Air for Europe (CAFÉ) Programme.

P. 21553, line 5: The point of the sentence (“This supports that. . .”) beginning on this line escapes me.

P. 21553, line 10: I find the sentence (“It is therefore necessary to include. . .”) somewhat contradictory with the approach chosen in this study since, unless I have misunderstood the previous chapters, actual multi-year deposition time series were not used

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as the 10-year simulation was carried out using emissions of 2007 only.

Referencing: The status of some central references is “in preparation” or “submitted” which makes it somewhat cumbersome to evaluate the manuscript properly. Hopefully this situation is improved at the time of the publication of the paper.

Table 3: It is stated that the changes in N emissions (rightmost column) are taken from Table 1. However, this cannot be the case as Table 1 does not have any decimals, but obviously the origin of both sets of numbers is the same. One wonders if the percentual changes in Table 3 could be given as integers also?

Table 4: The “N input reduction” in the caption should be N emission reduction.

Figures in general: I would suggest including, as the first figure, a map of the Baltic Sea with the bordering countries (preferably entire countries, not just some fragments) as well as other discussed areas and basins clearly identified. Latitude and longitude information would also be advisable. While there is a map of the area included in the Supplement, it would improve the reading experience of this article if one was spared from bouncing back and forth between two separate documents for central information like this. Furthermore, the map in the Supplement is smudgy, with some important countries (Germany, Denmark) not even identified. The scale of the map also seems a bit weird (0-407 kilometers ?!) and the (obviously) web graphics quality is much inferior to what should be expected from graphics in print versions of scientific articles.

Figure 1: What are the deposition velocities “appropriate” for land and water surfaces? Could they be explained in the text in 2.4? Note the typo in the second “appropriate”, on the last line of the caption.

Figures 2 and 3: Please, replace “Simulated total deposition” with “Simulated total annual deposition” in the captions. Should “commen” be spelled common? The figures are too small - both at the web site and especially in the “printer-friendly” version. The legends are illegible, but appear to be the same in both panels of each figure and thus

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only one legend should be enough per figure.

Figure 4: Why must the pies be 3D as this causes a visual effect that distorts the contributions from the countries on the front and far sides of the cake? I would suggest using 2D-pies, with the same order of countries on both years. Then also the colors of the countries would remain the same from pie to pie and be easier to compare visually.

Supplement:

Figure S1: I was not able to locate this figure in the address www.helcom.fi. See also the comment on figures in general above.

Tables S2-S5: “Aaland Sea” should be replaced by the proper name, Sea of Åland – or at least by Åland Sea which is used in the map.

Interactive comment on Atmos. Chem. Phys. Discuss., 11, 21533, 2011.

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