We have taken all of the review comments into consideration. We would like to thank the reviewer for these comments which we believe have substantially improved the m/s. The methodological parts are now better described and the formulas are now given in a more correct fashion. Discussion about uncertainties have been expanded. Below we reply in detail on how and if we taken the individual comments into consideration.

Reviewer #2

Our response given below each comment

1 General comments

The paper 'Nitrogen surface water retention in the Baltic Sea drainage basin' addresses relevant scientific questions within the scope of HESS. The topic is relevant both scientifically and supporting river basin management and control of N loading to the Baltic Sea. The methods are not very novel themselves, but estimation of N surface water retention in the whole of Baltic sea basin is. My proposal is major revision of the manuscript, taking into account all comments.

My major concern is almost total lack of uncertainty discussion. The authors present one number, 380 000 t of N as annual retention, but not any uncertainty estimates/ranges with different parameterizations by the MESAW model.
Discussion is somewhat short – the authors only mention that high retention in lakes is in accordance with earlier studies – but do not give proper credit to many published N retention studies in parts of the Baltic Sea catchment area, and compare their results to only those of Mörth et al. (2007). It is also misleading that in Intro, the authors refer mostly to **in-stream** retention studies, but in Discussion they point out that **in-lake** retention is of high importance.

ANSWER: We appreciate the comment that the estimation of nitrogen surface water retention in the Baltic Sea is novel. In fact we are not aware of any studies that have assessed the nitrogen retention in all the Baltic Sea drainage area besides the study by Mörth et al (2007).
Regarding uncertainty, we agree that this is a complex issue, and a quantitative assessment of the uncertainty associated with such complex mechanisms is, we believe, beyond the scope of this paper but offers potential for future work. Nonetheless, it is important to recognise and in a qualitative way discuss the uncertainty. We have thus included these uncertainty aspects at a variety of places throughout the revised m/s and have now included a particular uncertainty sub-section in the Results and Discussion.
We don't agree that the references given in Introduction are biased towards those of references on instream retention and that lake retention is less emphasised. In fact most references in the Introduction is of general character including both instream and lake retention. In all cases, this obvious confusion is now better explained in the Introduction.

2 Specific comments

Title is good and abstract well written.
Section 2 Material and methods:
-the authors mention that retention is assumed to be the same for source categories P (point sources), dominated by inorganic load, and sources category S (total losses) which include varying shares of N, more in organic-N form. In these models, the assumptions are needed, but this assumption could be discussed in uncertainty discussion

ANSWER: We have modified the general formula 1 and removed R_1, R_2 and R_3 and instead replaced it with R which was specific for this study. The formula 1 now reads as:

$$L_i = \sum_{i=1}^{n} (1-R)S_i + (1-R)P_i + (1-R)D_i + \varepsilon_i$$

We agree that such differentiation of retention to the various source categories would have been ideal but is almost impossible to parametrise and would have required more data upstream the river mouths and also data on inorganic and organic nitrogen. Nonetheless this relevant issue has been better included in the discussion part

Section 3 Results and discussion:
-To make it more clear, the authors should mention also the estimated total gross N load, 950 000 t N annually. Also here, comparison to earlier estimates would be reasonable to have.

ANSWER: Thanks for the remark on the missing information about the estimated gross load from the model. It is now included in the very beginning of the Results section.
We are not aware of any other studies of gross emissions estimates for these 117 Baltic Sea basins besides a very old study conducted by the first-author 15 years ago (which was felt outdated to include).

-it is true that there is not apparent relationship between specific N load and share of wetland area, but from Fig 5b we can notice that load is always low in basins where wetland-% is >15%

ANSWER: The statistical analysis do not give any statistical significant parameters for wetlands (Table 1). It should be noted that the classification of wetlands is rather rough from the data source and given as joint expression of all wetlands ranging from marches to peatland bogs. We don't have any possibility to include this is the analysis. In all cases for the reasons given in the paper it will have less importance for the overall objective to estimate the total N-retention in surface waters for the 117 basins. The reasons for the relatively low unit-area loads for the basins with >15% wetland area is due to the fact that they are all located in Finland or northern Sweden with low population densities and little agricultural area. This information has been included in the revised m/s.

-the term 'Other' is misleading, if these areas are practically all forests (are they?), the authors should include Fig 5d) of forests also into discussion

ANSWER: Indeed most of the land use category 'Other' is forest which was stated only once in the initially submitted m/s. We have included clarification of this in the revised m/s and in the Table headings.
We have included the following sentence on the missing comment on Figure 5d: There is a clear negative relationship between the unit-area loads of nitrogen and the share of 'Other' land (i.e. primarily forest')

-the authors could also acknowledge PLC database by HELCOM which they use a lot, and to include reference /web-page. Which institutes provide data to that database?

ANSWER: The PLC-reports and the data source is already properly acknowledged in the Material and Methods section. We have added the web-site to make it even more clear (www.helcom.fi). Below a list of the organizations providing data to PLC data base from the different countries and we believe it will become too exhaustive according to us to include it in the paper since it will include a lengthy addition of this form:

DCE -Danish Centre for Environment and Energy Aarhus University
Vejlsoevej 25
DK-8600 Silkeborg

Estonian Environment Agency
Mustamäe tee 33
EE-10616 Tallinn

Federal Environment Agency
Section II 2.2
Woerlitzer Platz 1
Dessau-Rosslau

Environmental Protection Agency
Environmental Status Assessment Department
River Basin Management Division
Juozapavičiaus st. 9,
LT-09311 Vilnius

Institute of Meteorology and Water Management,

National Research Institute
Jordana Str. 10/11
PL-40 056 Katowice

Saint-Petersburg Public Organization
"Ecology and Business"
Sabrikovskaya Str. 37, Office No. 307
Post Office Box 66
RU-197374 St. Petersburg

Department of Aquatic Sciences and Assessment
Swedish University of Agricultural Sciences, SLU
P.O. Box 7050
SE-750 07 Uppsala

Finnish Environment Institute (SYKE))

Mechelingatan 34a,

FI-00260 Helsingfors,

PB 140 Helsingfors

-the authors present very detailed results of lake and in-stream retention in Table A2, but do not discuss of the average percentages of these. How is the share between these estimates and how reliable/uncertain they are? For example for Neva river basin, retention in total surface water is estimated as 0.74, but lake+In-stream retention (0.91) seems not to be in accordance with the total?

ANSWER: Averaged over all basins, mean lake retention is 25% whereas the estimated instream retention is 5%. This information is included now.
Table A2 refer to the independent estimates of lake and in-stream retention respectively plus the total. For obvious reasons the independent percentages for lake and instream retention cannot be simply added (see methods). For example in Neva the instream and lake retention is 0.262 and 0.652, respectively. Certainly the combined retention is 0.74 according to the following simple calculation of total retention: 1- ((1-0.262)*(1-0.652))=0.74.
We assume that the reviewer have anticipated an additive response which for obvious reasons is not true.
Given the confusion we have modified formulas 3 and 4 and replaced it with:

$$R_i = 1 - \frac{1}{1+\lambda_1\sqrt{drainagearea_i}} * \frac{1}{1+\lambda_2\frac{lakearea_i}{drainagearea_i}} \quad i = 1,2,\ldots,n \quad (3)$$

3 Technical corrections
-the last paragraph of Intro should be more concise and short, with no details on population and land use. Instead, there could be introducing parapgrah in 2 Materials and methods, describing the area

ANSWER: This text part is appropriately moved to Intro to Section 2.

-the estimates of annual N loads in Table A2 give an over-optimistic impression of the accuracy, e.g. Odra 70 289 195 kg N/yr !, I would propose to use tonnes N/yr

ANSWER: Unlike many other papers in similar fields, we in fact give all input data for the model as well as outputs for each single river basin so the analysis can be replicated if needed. We feel that this is in line with the recent trend to publish ‘rawdata’ in peer-review journals. We leave this question to the Editor-in-Chief to decide. We have no problem to make the indata and result files much more aggregated and without the decimal precision.