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Comment

***Interactive comment on* “Estimation of nitrogen budgets for contrasting catchments at the landscape scale” by E. Vogt et al.**

E. Vogt et al.

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The authors thank the Anonymous Reviewer for their comments. We have responded to the specific comments below. (Note: "Authors' reply:..." inserted after the original text from the Review ("Referee #2:..."))

Referee #2: A weaker part is the description of results, which is sometimes confusing and repetitive. This holds specifically for various tables and figures that appear to be redundant. Furthermore, the uncertainty assessment can be written more clear. Finally, the referencing is generally fine, but lack some important articles. All aspects are detailed below.

Authors' reply: Please see the authors' replies below which address the referee's com-

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ments specifically.

Referee #2: Description of results: In my view the paper needs some reorganizing, in terms of order and included tables and figures. Now the results section switches from input terms (3.1 and 3.3) to output terms (3.2 and 3.4) and then suddenly to all inputs (3.5) and outputs (3.6), being an order inconsistent with the results in Tables and Figures. Furthermore, it sometimes refers to figures on spatial variation while it in reality discusses the total catchment numbers. This makes it all very confusing.

Authors' reply: Upon reflection, the authors think that the referee may have a good point here. The authors agree that the paper would benefit from a revised order in which the results are presented. How this would be done is detailed in the responses below.

Referee #2: Regarding the N budget, all the information is actually included in Table 4 and the Tables 2 and 3 and the Figure 6-8 are completely redundant and can be left out. Table 2 and 3 give the gives the mean numbers in Table 4 for each catchment but only multiplied by the catchment area of 621 ha and 895 ha, respectively. This area (mentioned in the text) can be added to Table 4 as a footnote and then each reader can make the calculation if he likes. Figure 6 and 7 simply repeats the average data in Table 4 on inputs and outputs, respectively, and Figure 8 is the balance with its uncertainties, as given in Table 4.

Authors' reply: Table 2 and 3 were originally given as numbers are a little more exact than those in Table 4. However, the authors recognise that this does not make a big difference, so that a footnote to Table 4 about the catchment size may be sufficient, i.e. Tables 2 and 3 will be left out of the paper. However, the authors disagree about the referee's suggestion to leave Figures 6-8 out of the paper as these are important data visualisations. The authors consider Figures 6 and 7 beneficial to the paper as they illustrate a) the proportion the budget components have to the total catchment input/output, and b) the differences between the Moorland and the Grassland catchment.

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Figure 8 is considered essential to the paper as it visualises the overall catchment budgets, including errors associated with the individual budget terms. Thus, the authors would like to keep Figures 6-8 in the paper. However, the y-axis of Figures 6 and 7 will be changed to the same scale (as requested by referee #1).

Referee #2: The logic order is in my view
3.1 Overall catchment N budgets are given with a discussion of uncertainty.
3.2 Spatially variation of the catchment N inputs and output terms Each section could then be subdivided in e.g. (i) input terms (land surface N input, atmospheric N deposition and biological N₂ fixation) , (ii) output terms (N harvest, N emissions and fluvial N export) and (iii) balances, following the same order as used in the methodological description in Section 2.

Authors' reply: The authors agree that a re-ordering of the results sections would be beneficial to the paper. However, we still think that it would be better to first present the different budget terms, followed by the overall budget. Thus, the order of the results and discussion section will be revised and presented as follows: 3.1 Catchment N inputs 3.1.1 Atmospheric N deposition 3.1.2 Agricultural land surface N input 3.2 Catchment N outputs 3.2.1 Atmospheric N emissions 3.2.2 Fluvial N export 3.3 Total N budgets 3.4 Uncertainties 3.5 Comparison with a regional approach

Referee #2: Section 3.1 can then completely do with the information in Table 4 with a footnote on the area in hectare. Thus skip Table 2 and 3 and Figure 6-8.

Authors' reply: The authors agree that Table 2 and 3 could be left out of the paper. However, they consider it important that Figure 6, 7 and 8 stay in the paper (see response above).

Referee #2: Section 3.2, however, should be accompanied by maps on the spatial variation of all the catchment N inputs and N output terms. What misses then is land surface N input, biological N₂ fixation (but this may be so constant that it can be left out), and N harvest. So, here it would be relevant to add 2 maps.

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Authors' reply: Maps of the land surface input and of the harvest cannot be given due to confidentiality agreements with the farmers. Biological N₂ fixation was roughly estimated for the whole catchment surface (see Section 2.3.3), therefore no spatial maps exist for this budget term.

Referee #2: Uncertainties: The uncertainty section is interesting as such, but I have 2 suggestions here as far as its quantification is concerned 1 The general terminology used in the paper is that a given N budget term is considered to be accurate (estimated uncertainty $\pm 10\%$) or have a relatively low uncertainty (of $\pm 20\%$) or higher and then estimates of $\pm 30\%$ and $\pm 50\%$ are used with one exception, ie N₂ emissions which go from -50 to $+200\%$. The word "considered" already makes clear that the uncertainty is based on expert judgement and in this context, it is much better to introduce/use an explicit classification of uncertainties, as done by e.g. Kros et al. (2012) in Biogeosciences, instead of an implicit approach. These authors mention that since there is little quantitative information on the uncertainty of the model inputs, they decided to use certain levels of uncertainties depending on the model input term. That is exactly done in this study, and can best be made explicit 2 What I suggest is to have separate section 2.5 on uncertainties, where the classification system of 10%, 20%, 30% and 50% is introduced and allocated to the different terms with the argumentation: could come in one table. This section should then also contain Eq.2, plus a rationale for using the equation (a reference?).

Authors' reply: The authors are grateful for this helpful suggestion. Thus, in the method Section 2.2 "Catchment N budgets", a table will be included, classifying the estimated uncertainties as such, referring to Kros et al. (2012) with a similar classification scheme: - Accurate: $\pm 10\%$ - Low uncertainty: $\pm 20\%$ - Moderate uncertainty: $\pm 30\%$ - High uncertainty: $\pm 50\%$ - Exceptionally high uncertainty: individually set errors This table will also specify which uncertainty class was associated with which budget term, so that the reader has a better overview of the different uncertainties for the budget terms. However, the authors still consider it easier for the reader to leave

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the explanation why specific uncertainties were associated to the budget terms within the text of the following sections. This way, it is first described how data were derived, followed by the conclusion which uncertainty these data carry. The new names of the different uncertainty classes of the table (e.g. low or moderate uncertainty) will, however, be used in the text. The square root of the sum of squares (RSS) is used as a standard statistical method to calculate the overall error of contributing variables, i.e. aggregating uncertainties. The authors thus consider it unnecessary to underpin Equation 2 with a reference.

Referee #2: Repetition: Apart from reducing redundancy by restructuring the results section please have a close look at the paper again on unnecessary repetition in certain para's. To give one example, on p7, line 8-10, deposition estimates are said to have a relatively low uncertainty in the range of $\pm 20\%$ and this information is repeated in different wordings on p7, line 19-23.

Authors' reply: This example is not a repetition, as the two sentences give uncertainty estimates for different budget terms which were derived with different methods. It is therefore necessary to differentiate between these two different deposition estimates. Firstly, in lines 8-10, the uncertainty of the locally very variable dry NH_3 deposition, derived by an intensive measurement programme and local modelling at 25 m resolution, is estimated to be $\pm 20\%$. In lines 19-23, the wet NH_x and dry and wet NO_y deposition, derived by national modelling at 1 km resolution, is estimated to be $\pm 20\%$ for this purpose as these deposition components are largely driven by non-local sources.

Referee #2: References: If referring to Kros et al. (2012) for classifying uncertainties, please quote: Kros, J., G.B.M. Heuvelink, G.J. Reinds, J.P. Lesschen, V. Ioannidi and W. de Vries, 2012. Uncertainties in model predictions of nitrogen fluxes from agroecosystems in Europe. Biogeosciences Discuss., 9, 6051–6094.

Authors' reply: Please see response above.

Referee #2: The paper by de Vries et al.(2011) is quoted very often, but not always

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adequately. This paper focuses on the continental (European) scale only, but it is part of a special issue on N flux assessment going from local to continental scale and several papers in this issue would be appropriate to refer to. To give some examples: on p3, line 5-7, the authors state that N balances have recently been developed and applied at various scales, but then they should not refer to the continental scale paper, but to the introduction in this special issue, which is also de Vries et al (2011). De Vries, W., P. Cellier, J. W. Erisman and M.A. Sutton, 2011. Assessment of nitrogen fluxes to air and water from site scale to continental scale: an overview. *Environmental Pollution* 159: 3143-3148.

Authors' reply: On page 3 (or 8991), lines 5-7, the reference has been changed to the overview paper of de Vries et al.: "Nitrogen budgets as indicators of environmental pressure have recently been developed and applied at various scales (de Vries et al., 2011), ranging from the farm and field level..." On the same page, lines 23-24, the reference has also been changed to the overview paper: "However, accurate estimation of N fluxes at high spatial resolution poses a significant challenge (de Vries et al., 2011)..."

Referee #2: When referring to N fluxes in European rural landscapes on p3 line 17, it would be good to refer to at least two other recent landscape papers in this special issue, i.e.: Kros, J., K.F.A Frumeau, A. Hensen and W. de Vries, 2011. Integrated analysis of the effects of agricultural management on environmental quality at landscape scale. *Environmental Pollution* 159: 3170–3181. Dalgaard, T., N. Hutchings, U. Dragosits, J.E. Olesen, C. Kjeldsen, J.L. Drouet and P. Cellier, 2011. Effects of farm heterogeneity and methods for upscaling on modelled nitrogen losses in agricultural landscapes. *Environmental Pollution* 159: 3182-3192. When referring to different budget approaches on page, it is crucial to refer to Leip et al (2011) who gave the most complete review on this topic Leip, A., W. Britz, F. Weiss and W. de Vries, 2011. Farm, Land, and Soil nitrogen budgets for Agriculture in Europe calculated with CAPRI. *Environmental Pollution* 159: 3242–3252.

Authors' reply: The authors are grateful for the suggested references and will include them in the revised manuscript.

Referee #2: Finally, at the end of the intro, the authors state that to their knowledge, "this is the first study which includes high resolution atmospheric modelling combined with a detailed spatial landscape inventory of field specific agricultural activities" to estimate N budgets at landscape scale. Well I do not think this is true. The above mentioned reference by Kros et al (2011) in Environmental Pollution 159: 3170–3181, does the same and should be mentioned here. A unique aspect may be the quantification of N export, but then this should be said.

Authors' reply: The authors agree that Kros et al. (2011) is an important paper to cite and thus it will be included as a reference (see response above). However, the authors disagree that the only difference between this paper and Kros et al. (2011) is the quantification of stream N export. The level of detail is distinctively different between these two papers. The landscape in Kros et al. (2011) covers an area of 600 km², compared to 36 km² of this study. Estimates of NH₃ emission and deposition, for example, are done at a resolution of 250 x 250 m², compared to 25 x 25 m² in this study. This allows for a very different model validation via measurements. The authors of Kros et al. (2011) themselves say that the reliability of NH₃ deposition modelling results increases with decrease in spatial resolution. Furthermore, agricultural activities in Kros et al. (2011) are not based on an inventory of field specific agricultural activities. Rather, national farm data were used to derive activities with a fertiliser distribution model. This also does not include any animal activity on individual fields. The authors thus argue that the sentence at the end of the introduction is valid as is. However, the authors would like to extend it to highlight also the fluvial export data: "To the authors' knowledge, this is the first such study which includes high resolution atmospheric modelling combined with a detailed spatial landscape inventory of field specific agricultural activities and intensive measurements of fluvial export."

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