

Interactive comment on “Inventories of N₂O and NO emissions from European forest soils” by M. Kesik et al.

M. Kesik et al.

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1. We do fully agree that the soil pH can significantly vary on microsite scales. But it is difficult to predict the consequences of this variability for chemo-denitrification in a given soil, since several factors such as production (mainly nitrification), transport and consumption processes (e.g. nitrification/ denitrification) are interacting. We have discussed your suggestion to include an algorithm to describe microsite variability of pH in the model, which may follow in some way the functionality of the “anaerobic balloon”. We will try such an approach in the coming versions, and will test if this will improve the predicting capability and accuracy of the model. We do agree that not addressing the microsite variability of soil pH and the missing discussion of its consequences for NO emissions (not so much for N₂O, since chemo-denitrification of N₂O is of minor importance) was a shortfall of the previous version. In the revised version we added some sentences to address this point and discussed the consequences. We also introduced a sentence mentioning that this a field which needs to be developed in coming

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model versions: “Furthermore, in our model approach we do not consider the microsite variability of soil pH. It has been shown in several studies that e.g. in acid forest soils the soil pH can vary on a microsite scale for up to 3 pH units (e.g. Häussling et al., 1985; Bruelheide and Udelhoven, 2005). If nitrite production by e.g. nitrification would mainly be associated with the higher pH microsites and if one would disregard transport and other consumption except chemo-denitrification, the model would certainly overestimate NO production by chemo-denitrification. For the given reasons, there is a need to develop algorithm which are addressing microsite variability of soil pH in future model versions.”

In our response to reviewer 2 we give explanations why the MSF approach can not fully cover the uncertainty range as estimated with Monte Carlo type approaches. This does also apply for the settings we have chosen to predict the lower limit of N trace gas emissions. E.g. the minimum scenario also involved the maximum pH value. This must not necessarily produce an absolute minimum. If the maximum pH for a grid cell is in the neutral range and not above e.g. 8, one may get slightly lower estimates if one chose pH 5, i.e. there is a need to define ranges for some parameters (most parameters such as SOC are not critical) for which lowest N trace gas emission predictions are expected. But this range will partly depend on the range observed (estimated) for each grid cell, so that the procedure would get rather complicated (see also our comments to reviewer 2) and that one would finally end with Monte Carlo type approaches. We are sure that this will be the end of the line, if we succeed to further optimise the code with regard to computation time. We introduced several sentences to the manuscript to properly address this problem: “Due to the underestimation of maximum N trace gas emissions the uncertainty estimates with the MSF method are not fully satisfactory, but represent at present the best uncertainty estimate we can achieve. The full application of the Monte Carlo method (or of comparable methods) to all grid cells would be the favourable method to estimate prediction uncertainties. But for this a further optimisation of the model code with regard to the reduction of computation time is required. Due to the lack of an uncertainty range for regional N deposition, the effect of this on

N trace gas fluxes was not included in the uncertainty analysis. However, Fig. 5 shows on a site scale that variations in N deposition will significantly feedback on soil NO and N₂O fluxes even in one year simulation runs. I.e. increases in N deposition by e.g. 50% would increase simulated N₂O and NO fluxes at our 19 test sites by approx. 38% or 21% (Fig. 5).”

2. We do agree that our data suggest that some site differences are at least partly due to differences in the forest floor humus type (mull//moder dichotomy). The humus type can be regarded as an indicator for nutrient availability, C:N ratio and decomposability of SOC at a site scale. You therefore asked how this dichotomy is addressed in the model. We introduced a new paragraph in the model description section to provide insights. In the model we tried to address the development of different humus types in the field with an optimisation of the SOC fractionation, i.e. that a forest floor with a mull humus type has a higher fraction of easily decomposable SOC as compared e.g. to a forest floor with a rawhumus humus type. Furthermore, also the physical soil characteristics such as density and porosity are affected by the humus type in the model. These assumptions are based on the evaluation of soil science textbooks and have been described in detail by Li et al. (2000) and Stange et al. (2000). We supplemented the text as followed: “Since several authors have discussed the importance of the forest floor humus type for N trace gas emissions (e.g. Brumme et al., 1999; Butterbach-Bahl et al., 2002a), we shortly want to discuss how the PnET-N-DNC model deals with the effect of humus type on processes involved in N trace gas emissions. In the model the effect of the humus on N trace gas production is indirect: the humus type influences the partitioning of SOC into different fractions (humus, humads, litter) with their specific decay constants (Li et al., 2000). For a forest floor with mull as humus type the SOC fractions with short turnover times are highest, medium in moder and smallest if the humus type is rawhumus. This does affect the C as well as N availability and, thus, the C and N turnover rates and finally also the processes involved in N trace gas emissions. Furthermore, the humus type is also influencing the density and the porosity of the organic layer, which do also result in differences in soil climatic conditions such as

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water and temperature distribution.”

3. We do agree that the ratio of NO:N₂O fluxes can be a good indicator for the underlying microbial processes. We also followed your recommendation to overlay NO and N₂O fluxes (see new Fig. 10). However, we do not fully evaluate the reasons why this ratio does vary on spatial and also temporal scales (seasonality). Such an detailed evaluation of our huge dataset does take some time. We feel that this is in the moment beyond the focus of this manuscript, but we will certainly do so and will work on a new manuscript which is taking up your idea and will compare the outcome with already published work from your site, e.g. on the conceptual “hole in the pipes” framework. We added the following paragraph to the end of section 3.2 to address the above discussion: “Fig. 10 shows the calculated NO:N₂O emission ratio for EU forests. The figure shows that for Central Europe and most of Sweden simulated NO emissions are dominating over N₂O emissions, whereas in other parts of Scandinavia, UK and South/ South East Europe N₂O emissions dominate over NO emissions. In our model simulation the NO:N₂O ratio was significantly correlated with the soil parameters SOC ($r = 0.129$), mineral soil pH ($r = - 0.360$) and atmospheric N deposition ($r = 0.356$). However, all these correlations are rather weak.”

4. Yes, we fully agree with this statement. There some good indications that soils high in SOC tend to have higher N₂O emissions. This is supposed to be due to tight coupling of C and N turnover processes in soils. In the model study by Li et al. (2005) and Sixt et al. (2004) the arguments and results of the still limited targeted field observations are summarised. We have discussed this issue several times and there is certainly a need for further field studies, comparing N₂O (NO) fluxes for soils with different SOC contents and C:N ratios of the organic matter. Especially, the latter factor, the C:N ratio may be of crucial importance in soils rich in organic SOC, since this ratio may summarise/indicate the decomposability of organic matter. We added some additional sentence to the discussion section to address this point: “We only can assume that C-rich soils from former peatlands, which have widely been drained in Fennoscandia for

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improving forest growth (Paavilainen and Päivänen, 1995) are indeed a stronger source for atmospheric N₂O than other soils poorer in C content in this area. In agreement with field studies, also other modelling studies dealing with effects of management practices such as no-till on N₂O emissions from agricultural soils do show that the magnitude of N₂O emissions is most likely positively correlated with SOC (Six et al., 2004; Li et al., 2005). However, further field studies on soils differing in SOC but also in the ratio of C:N ratio are needed to further evaluate this coincidence and to proof the model algorithms and predictions.”

5. The Simpson et al. (1999) paper was not intended to provide new estimates for Europe, but rather to illustrate the range of values obtained by different assumptions. In the cited paragraph the authors mainly tried to calculate a minimum and maximum scenario assuming on the one hand that none of the European forests are affected by atmospheric N deposition or, on the other hand, that all forests are affected by atmospheric N deposition. The latter possibility was noted as “speculative” (sec. 4.4.1), but without any clear basis for defining the forest type the procedure used seemed reasonable for the purpose intended - to get the upper and lower bounds. To address this previous discussion, which was not all meant as a criticism of your previous upscaling approach (Davidson & Kingerlee, 1997), we reworded this paragraph: “Simpson et al. (1999) presented calculations of soil-NO emissions with a range of methods (a modified version of Skiba et al., 1997; BEIS-2 - from Novak and Pierce, 1993; Yienger and Levy, 1995; Davidson and Kingerly, 1997) accounting in some of these for N inputs from atmospheric N deposition or fertilizer. The range of estimates was very large. Using the Davidson and Kingerly methodology to derive an upper and lower boundary, with the extreme assumptions of no N-affected forest, or 100% N-affected forest, yielded a range of 13 to 350 kt N yr⁻¹ for forests. However, it must be noted that the range of uncertainty for NO emissions from forest soils is significantly lower and that the previously mentioned upper value is not realistic, since most forests in North Europe receive atmospheric N input of less than 10 kg N ha⁻¹ yr⁻¹. Compared to the approaches documented in Simpson et al. (1999), the PnET-N-DNDC”

6. We followed your recommendation and now also provide within Fig. 6+7 a comparison of mean annual site fluxes with the mean of multi-year model simulations (inserted graphs). The enumeration is adopted to the original graph and the Tables 2 and 3. Additionally, the r^2 for the N₂O and NO emissions of all sites and the standard errors for each site are given in the smaller graphs.

Technical comment: We also hope that the figures and tables will be larger in the printed version. We will contact the production office to ensure this.

All of us appreciated your comments and the comments of the anonymous reviewer.

Sincerely yours, Klaus Butterbach-Bahl

Interactive comment on Biogeosciences Discussions, 2, 779, 2005.

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2, S551–S556, 2005

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