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Biogeosciences
Discussions

Interactive comment on "Budget of N_2O emissions at the watershed scale: role of land cover and topography (the Orgeval basin, France)" *by* G. Vilain et al.

G. Vilain et al.

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General comments

Dear Referee#1 We sincerely acknowledge you for your valuable comments on this manuscript. We answer to your comments in the following. Major part of your comments and questions are taken into account in the revised paper in order to improve the quality and understanding of the study presented here.

In details, we have provided here the answers to each item specified in your comment.

*This paper presents the results of a measurement-based upscaling exercise for N2O

emissions at the watershed scale, using, for direct sources, land-use and topography maps coupled with measured emission fluxes for differents land uses and at different topographical positions within the landscape (using results from the same watershed published earlier by the same team). The two main objectives of this paper, i.e. i) watershed-scale N2O budget, and ii) analysis of the sensitivity to the input data used for the upscaling, are straightforward and well treated in the manuscript. The paper is well written, scientifically sound and the results are certainly original enough to warrant publication in BG.

Author's response: Thank you

*The most significant result of the paper is presumably the strong sensitivity (25%) of the watershed-scale N2O budget to the topography-induced spatial variations in N2O emissions by crops. By contrast the sensitivity to land use representation and databases can be considered to be negligible (5%) in comparison with the overall uncertainty in N2O emission budgets at this kind of scale. The differences in N2O fluxes between shoulder, slope and footslope were demonstrated in earlier papers of the author's, so the novelty here resides in the consideration of topographical position for the upscaling of N2O emissions to the landscape scale, rather than the actual observed differences in emissions between positions in the landscape. Another significant result is the small fraction contributed by indirect sources from the hydrological network (streams + groundwater), compared with direct emissions by soils.

*However, I am concerned overall by the absence of uncertainty analysis for watershedscale estimates. The upscaling exercise yields a "best "estimate" (using Topo + MOS + Ecomos) of 14210 kg N2O-N yr-1, or of the order of 1.3 kg N2O-N ha-1 yr-1, which, the authors argue, compares favourably with other observation-based, as well as modelling-based, estimates obtained for other watersheds or landscapes. Yet no uncertainty range is provided for this number, nor for any of the sub-totals for the different land uses or for the indirect emissions calculated from measured dissolved N2O and water-atmosphere emission models. It is clear that the uncertainty in emissions at this scale is very large. At least the authors could go back to the uncertainties in individual measured fluxes, the uncertainties in annual-scale estimates due to gapfilling procedures, or the uncertainties (e.g. standard error / confidence intervals) in the mean measured fluxes for the different classes of land use and topographical position (as published in their previous paper), and then calculate how these uncertainties propagates into the watershed-scale estimates. The same could be done for the indirect emissions, with errors in both measured dissolved N2O and in the water-air exchange coefficients.

Author's response: You are right. The uncertainty analysis of the watershed analysis was then performed and added to the text.

*Further, I find it a little frustrating that the paper concludes that it is important to account for the topographical index in the upscaling of fluxes, since footslopes are potentially larger (over-proportional) emitters, without actually discussing anywhere why this is so. The mechanisms driving enhanced N2O emissions by footslopes are not alluded to: are they due to a higher soil moisture/WFPS (and thus higher denitrification rate) than upslope? to enhanced mineral N (esp. NO3-) availability from runoff? It really seems as though the extensive datasets collected across this watershed should be able to provide interesting clues.

Author's response: These factors were discussed in detail in Vilain et al. (2010), but for more clarity, we resumed it briefly in the text: "Two main factors drive these highest emissions by footslope soils: (i) a much greater soil moisture which enhances denitrification and then higher N2O fluxes, and (ii) a higher mineral N availability (NO3−) resulting from runoff (see Vilain et al., 2010 for more details)."

*Apart from a few technical and language errors (indicated in the annotated PDF of the online manuscript, attached to this review), only minor revisions need be addressed, which are detailed below.

Author's response: we thank you for your detailed review of the manuscript, we took

into account all of your revisions on the attached annotated pdf.

Specific comments

*Section 3.1 (and figure 4): a brief description of the sampling strategy should be provided (even if the details are given in previous papers); in particular, which positions in the landscape were sampled (shoulder, slope, footslope)? How many samples were taken? Were there differences along the transect? Please provide error bars (confidence intervals) in fig. 4.

Author's response: We added in the text more precision about the sampling strategy and provided error bars in the Figure 4.

*Related to the above, Section 4.1: I find this paragraph a little confusing. The authors argue that "over a year nitrification is the main process occurring in soils, with the denitrification process occurring only during specific conditions", and yet actual flux measurements show that N2O emissions are higher in footslopes, where soil moisture is generally higher and denitrification is expected to prevail. I do not overall find the argument (of nitrification being the main N2O source at the landscape scale) convincing, as this is based on laboratory incubations and potential rates. Even Fig.4 seems to suggest otherwise, with potential denitrification being of the same order as potential nitrification, while the N2O/NO3 ratio being around 160 times greater for denitrification. Or do the authors actually mean that nitrification is the phenomenon that occurs most often over a year and in most places across the landscape, while quantitatively it is denitrification which produces the bulk of the N2O, even if this occurs in hotspots over time and space?

Author's response: Your understanding was right and we rephrased the paragraph to make it clearer:

"From these measurements and laboratory experiments we can assume that over a year, nitrification would be the process which occurs most often in soils across the landscape. On the contrary, the denitrification process would occur in less occasions and rather in some wet hotspots (such as the footslope positions) during specific conditions such as fertilizer application associated with a higher soil moisture and hypoxia, conditions necessary for the denitrification process to take place (Bateman and Baggs, 2005; Davidson and Schimel, 1995; Linn and Doran, 1984). But quantitatively the denitrification contribution can produce the bulk of N2O as the amounts of N2O produced by denitrification are much greater than by nitrification (see the N2O/NO3- ratios)."

*Section 3.2.1: please provide brief definition of "first-order, second-order" streams for the layman.

Author's response: we added a sentence to briefly define the concept of Strahler order: "Strahler stream order are used to define stream size based on a hierarchy of tributaries, first order being the smallest permanent stream"

*Section 3.5.1, l19: it appears there are different formulations for the water-air exchange coefficient (KN2O) in the literature (e.g. Clough et al, GCB 13, 1016-1027, 2007). The stream turbulence parameter of KN2O, largely dominant over the wind-speed parameter in small streams, is very dependent on the formulation used, and the difference in flux will be directly proportional to this term. Were several parameterisations tested? Was KN2O verified/validated independently in this sudy? This surely represent a large source of uncertainty for in-stream emissions. Indeed Table 1 shows uncertainties of the order of 50-100% for individual fluxes.

Author's response: The variability in Table 1 is here due to the variability of the concentrations within summer and winter periods (see figure 3).

KN2O which is calculated with the equation given by Wanninkhof (1992) and by Borges et al. (2004), have been validated by field experiment as shown in Garnier et al. (2009), (see attached fig1 and fig2).

Note that values at 8th order (not relevant here) are those determined by Garnier et

al. (2001) in the fluvial sector of the Seine downstream from Paris for oxygen (see below) and applied for N2O. Garnier J., Servais P., Billen G., Akopian M. & Brion N. (2001b). The oxygen budget in the Seine estuary: balance between photosynthesis and degradation of organic matter. Estuaries 24(6) : 964-977. See atached fig3

*Section 3.5.2: is there potentially a double-counting of N2O emissions as calculated from groundwater discharge (EF5g) and from the dissolved N2O in streams (EF5r) (section 3.5.1) ? The text suggests that all N2O contained (dissolved) in the groundwater is either released to the atmosphere from agricultural drains, or through the soil via the unsaturated layer. However, groundwater discharge through drains eventually reaches the hydrological network via ditches, and the dissolved N2O adds to that present in the stream - unless there is an instantaneous release of dissolved N2O at the drain exit points. How can it be ascertained that some of the dissolved N2O is not emitted twice in the calculations?

Author's response: We assume that all dissolved N2O contained in the groundwater or drain water is instantaneously released to the atmosphere once reaching the hydrological network based on field experiments (see Garnier et al., 2009), giving the EF5g coefficient. The EF5r coefficient rather corresponds to the in-stream production and release. The possibility of double-counting the N2O emissions is then limited.

*Further, I would also object that in the case of groundwater dissolved N2O reaching up to the soil, not all N2O molecules will reach the atmosphere as they may be consumed by microbes along the way (see e.g. Chapuis-Lardy et al., Soils, a sink for N2O? A review. Global Change Biology (2006) 12, 1–17, doi: 10.1111/j.1365-2486.2006.01280.x). Thus the author's estimate of indirect N2O emissions from groundwater sources should be presented as an upper bound.

Author's response: We agree with you, as we better explained this pathway in our previous publications (Vilain et al., 2011). For being more synthetic we first decided to remove such a paragraph here, but as it seems to reduce the comprehensibility of the article we added, following your suggestion, these sentences:

“Moreover, besides losses to the atmosphere, further reduction of N2O might be taken into account as soil microbes can consume N2O molecules when reaching up the atmosphere (Chapuis-Lardy et al., 2006). Then, this expressed flux should be considered as an upper bound flux (see Vilain et al., 2011).”

*Sections 4 and 5: please provide uncertainty ranges for all results: batch slurries, mean measured fluxes from different land use types, and indirect emissions. The uncertainties in watershed-scale estimates of N2O emissions should be obtainable using error propagation methods from individual incertainties of Section 4. *Tables 2, 3 and 4 should also indicate uncertainty ranges.

Author’s response: We added in the text all the uncertainties for the results and calculated uncertainties in watershed-scale estimations, which we also added in the text. We also added the uncertainties associated to each value on Tables 2, 3 and 4.

*Section 6.1, l12-15: the authors mention two fates for the N2O produced in soils: either direct emission, or solution into soil pores and groudwater, leading to indirect emissions. A third fate is consumption by soil bacteria; not all produced N2O is subsequently emitted, much is recycled.

Author’s response: you are right, but as the study concerned only nitrous oxide emissions and not the entire nitrogen cycle in the soil we did not mention this process. We have added few lines in the text following your remark: “). A third fate also exists, not leading to emissions (and then not discussed here) is the consumption by soil bacteria; therefore not all produced N2O in soils is then subsequently emitted, but can be recycled.”

*Section 6.1, l17: it should be made clear that for indirect emissions, no flux measurements were made as such, but that the estimates presented are concentration-based model estimates. As indicated above, the term KN2O is modelled and much depends

on the kind of parameterisation used.

Author’s response: we better explained this in the text

*p10838, l27 – p10839,l6: the main criteria deciding whether the upscaling from crop fields to landscape scale was biased in this study is whether “average” or “representative” fields in terms of fertilisation practices were sampled. If such was the case (was it?), then not including the fertilisation rate as a spatial variable was not crucial, assuming that N2O emission is proportional to applied fertiliser when considered at the field scale (which is the foundation of the fertiliser emission factor concept). It has been argued, however, that the relationship is not linear, with over-proportional emissions at very high fertilisation rates.

Author’s response: The sampled field was assumed to be representative of the whole watershed in terms of agricultural practices and especially fertilizer application. We specified it in the Material and methods section and discussed it further in the manuscript.

*Section 6.3, p10840: it is argued that agroforestry of footslopes (the planting and harvesting of trees in riparian zones) would have both ecological and economical benefits. Why then is the hypothetical scenario of an abandonment of cropping in low topographical positions based on a replacement of crops by grasslands (l17 p 10840) ? Why not use woodlands and forest emission rates, which are even lower (see Table 2) ? Is it because in economic terms grasslands would have the edge over forests, and therefore the farmers’ preference in the short term? Would grasslands need to be grazed in order to be profitable, and if so, what are the implications for their N2O emissions?

Author’s response: We agree that this part was not clear. We rearranged this paragraph to better introduce our grassland/bioenergy crop scenario. We preferred this scenario to that of agroforestry for its simplicity of implementation of the field and the economic advantages in the short term compared to the agroforestry. Of course this opens up broad prospects for research; the work on the production of biofuels (and more particularly as an alternative in the vegetative buffer strips) still being in its infancy.

Interactive comment on Biogeosciences Discuss., 8, 10823, 2011.

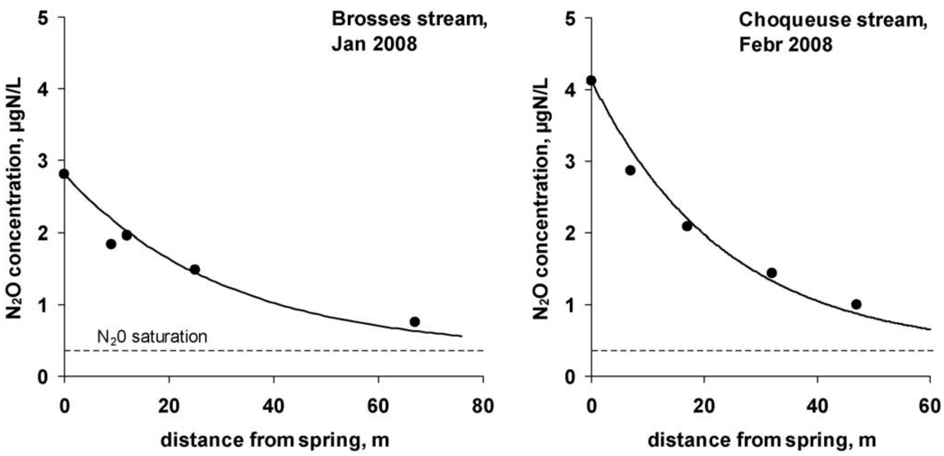

Fig. 1. Observed decrease of N2O concentration downstream from the spring or the tile-drain outlet in two small streams in the Brie region. The curve represents the simulation respectively with a KN2O of 0.36

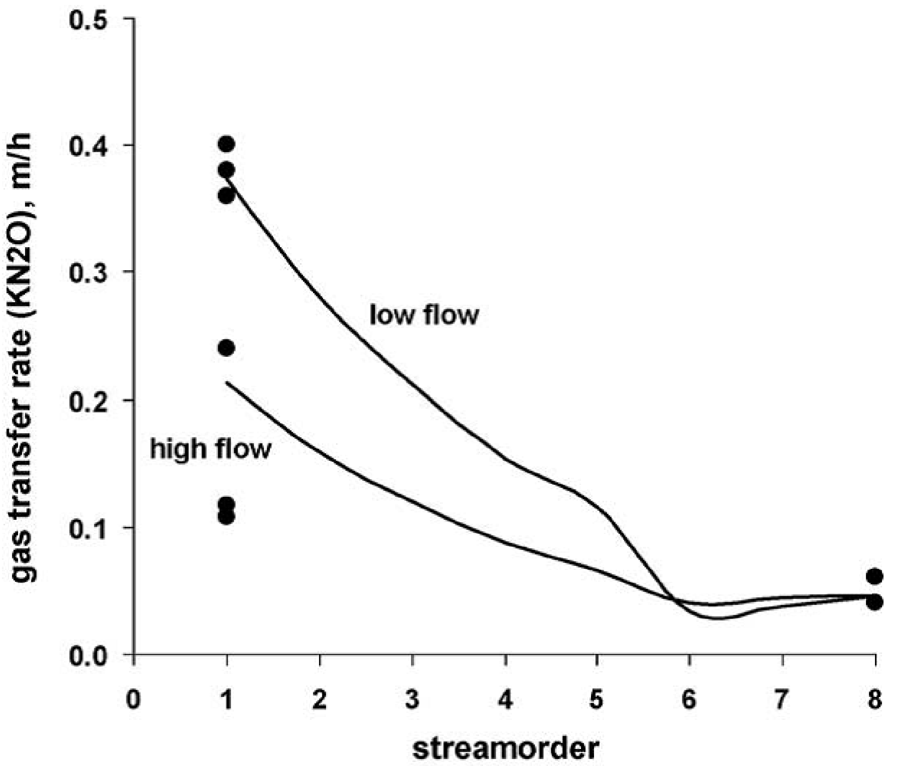

Fig. 2. Experimental values for 1st order streams fit well with the calculated ones according to the Wanninkhof (1992)'s and by Borges et al. (2004)'s equation .

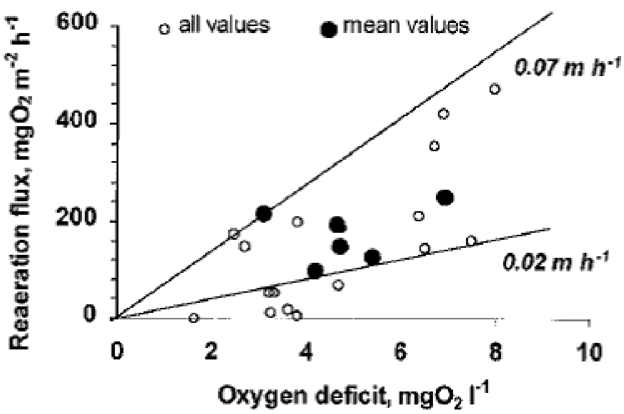

Fig. 9. Estimates of the reaeration coefficient in different reaches of the downstream Seine River (from Paris to the mouth of the estuary).

Fig. 3. Estimates of the reaeration coefficient in different reaches of the downstream Seine River (from Paris to the mouth of the esturay)