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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.

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General comments

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.

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 watershed-scale 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 gap-filling 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



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coefficients.

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.

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.

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.

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.



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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?

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

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 windspeed 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.

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?

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-



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2486.2006.01280.x). Thus the author's estimate of indirect N2O emissions from groundwater sources should be presented as an upper bound.

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.

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.

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.

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.

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)



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? 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?

Please also note the supplement to this comment:
http://www.biogeosciences-discuss.net/8/C5219/2012/bgd-8-C5219-2012-supplement.pdf

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