Biogeosciences Discuss., 8, C5548–C5550, 2012
www.biogeosciences-discuss.net/8/C5548/2012/

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.

Anonymous Referee #2

Received and published: 31 January 2012

Title: Budget of N2O emissions at the watershed scale: role of land cover and topography (the Orgeval basin, France) Authors: G. Villain et al.

This paper describes an ambitous task to estimate river-basin scale emissions of nitrous oxide. Although this greenhouse gas contributes only a small fraction to global warming, it receives much research attentrion. A lerge number of papers has been published in the past 2-3 decades, representing an enormous amount of fi8nancial resources from research funds. Despite this enormous effort, the uncertainty in the estimates of N2O emissions at the global scale has not been reduced. Now this paper describes an attempt to generate more reliable estimates for N2O emissions at the

scale of a landscape, including both the direct emissions to air, and the indirect emissions from nitrogen leached from soils or lost through surface runoff, and subsequent denitrification in groundwater and riparian zones, and surface water. The paper is well written, well structered and reads easily.

However, there are some major questions related to the approach, and to the uncertainties in the estimates. In section 3.5.2 the authors mention that they assumed that all N2O in groundwater is relaesed to the atmosphere from drains or by diffusion from the groundwater table to the unsaturated zone. This assumption at least merits some more discussion. This assumption implies that there is no denitrification in the groundwater in this river basin. The reasoning behind this assumption is missing. Denitrification in the unsatruated zone will occur if there electron donors available, so apparently this is not the case. So some more explanation is needed. Similarly, the authors need to argur why N2O moving through the unsaturated zone would not be prone to denitrification. It needs to pass through the soils, so could be reduced by denitrifiers.

Similarly, readers may wonder if there are no riparioan zones in this landscape, or is there no denitrification in such zones? Many literature studies indicate that riparian zones may be important sources of N2O at the landscape scale. Or is the N2O flux estimated for springs actually the site where riparian zones are expected. If so, is there no double counting of emissions, since all N2O in groundwater is assumed to be released to the atmosphere?

This may, however, be totally unimportant, since the estimated contribution of direct emissions is 96%, but yet I think the assumptiuons need to be argued in more detail.

The second comment relates to the uncertainty. The emission coefficients and estimates of total emissions are different for the various approaches, which stem from the same measurements. So an estimate of the uncertainty in the emission coefficient would be very helpful for readers to understand the differences between the methods for upscaling. These uncertainties may be far larger than the differences between the

upscaling methods. Perhaps this has all been published in the previous papers by this group, but should at least be mentioned here.

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