

## ***Interactive comment on “Nitrous oxide emission hotspots from organic soils in Europe” by T. Leppelt et al.***

### **Anonymous Referee #2**

Received and published: 18 August 2014

This manuscript reports a fuzzy-logic, spatially explicit, land-use based, multi-site calibrated model approach for European wide upscaling of N<sub>2</sub>O emissions from organic soils. Given the potential of organic soils to act as strong N<sub>2</sub>O sources upon cultivation, and the massive pressure on pristine peatland to serve as fertile cropland, this is a timely issue. The authors are the first synthesizing existing literature by this approach, adding some element of novelty and developing the first spatially explicit N<sub>2</sub>O budget for European organic soils. The approach omits important drivers for N<sub>2</sub>O emission from drained organic soils such as mineral N content and C to N ratio of organic matter. The upscaling based on parameters that can be regionalized however, more than compensates for this. The use of soil pH as a partial proxy for C to N ratio is elegant! Hence, the strength of this study lies in the comparison of model-based regionalized N<sub>2</sub>O emissions (a Tier 2 approach?), with land-use average, and best

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practice IPCC Tier 1 approaches. At the same time, the study suggests that Europe wide N<sub>2</sub>O emissions from organic soils are underestimated by IPCC default methodology. The manuscript is well written and of great relevance for the wider audience of BGS and policy makers.

The modelling approach and regionalization is rather transparent, with the exception of mean annual water table and bulk density. I understand that “land-use specific frequency distribution functions of observed water table in database” (P 9143 L. 19-20) are the only way to go, but I doubt that it is the annual water table alone but rather its seasonal fluctuation which determines the magnitude of N<sub>2</sub>O emissions. Did you include seasonal WT fluctuation in your statistical analysis of the overall data set? If so, say something about it in the paragraph page 9146, L. 24 ff.

Minor comments

Title: The focus on “N<sub>2</sub>O emission hotspots” in the title is fancy but takes it to the edge. Even though there is a good deal of text in chapter 3.4 trying to advocate identified “hot spots” (fig. 8) as targets for “N<sub>2</sub>O mitigation” (P. 9156, L. 15), I find that this chapter adds comparatively little new information and should probably be condensed. Moreover, figure 8 inherently relies on the scaling exercise carried out. Therefore, I would prefer a title containing the terms “scaling” or “upscaling” together with “land-use”. Alternatively, I would find it justified to talk about a Tier 2 approach (as you propose yourself on P. 9158, L. 15), without knowing the requirements put forward by IPCC for this in detail.

Table 2: give significance levels for the correlation coefficients.

P. 9145 L. 20. No outliers shown for N<sub>2</sub>O flux in box plots of figure 2. Why?

P 9146, L. 24 ff. “. . .a relationship between annual or seasonal climatic variables, e. g. soil/air temperature or precipitation and N<sub>2</sub>O emissions could not be observed”. This statement holds for the entire data set? Since you did not continue with a “global

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model” (independent of land-use type) and later use autumn precipitation as a driving variable for annual N<sub>2</sub>O emission in grasslands, I wonder whether you need to say it at all.

P. 9149 L. 1: what is “anthropogenic N fertilizer”? Skip “anthropogenic”. Elsewise, I fully agree with the explanation given for the lack of a significant fertilization effect in the cropland model.

P. 9151 L. 21-23: Soil pH as a driver for forest emissions: here, I feel you are cutting corners by suddenly switching from soil pH as a proxy for C/N to pH as a proxy for “nutrient availability”. Add one sentence highlighting that pH < 5.5 still allows sufficiently low C/N ratios to legitimize your C/N threshold stated earlier.

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Interactive comment on Biogeosciences Discuss., 11, 9135, 2014.

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