Biogeosciences Discuss., 11, C4559–C4563, 2014
www.biogeosciences-discuss.net/11/C4559/2014/

Biogeosciences
Discussions
Open Access

Interactive comment on "Effects of experimental nitrogen deposition on peatland carbon pools and fluxes: a modeling analysis" *by* Y. Wu et al.

Anonymous Referee #2

Received and published: 22 August 2014

Wu et al. describe the application of a peat-vegetation model to a fertilisation experiment in Canada, aimed at mimicking the future likely increases in N deposition. They provide a model-data comparison for the control and fertilised treatments, as well as a simple uncertainty study to some model assumptions. Based on this they run scenarios for 80 years to simulate the long-term effect of N deposition on the productivity and vegetation composition of peat lands. While I find the study to be of great interest to Biogeosciences, I have several concerns with this study.

Major comments

The methodology is fairly hard to follow. The manuscript would greatly profit from a concise description of the PEATBOG model (possibly including a conceptual figure) in terms of what it does and what it does not simulate. In the current version of the

manuscript, some of this material is not present (e.g. how do the vegetation dynamics work, how is production and nutrient uptake modelled?), or it is spread in the introduction (p102754 l 26) and the results section (p10284 l1ff), which makes it hard to appreciate what the model does. Of course, the model has been published elsewhere, but a reader should be able to understand what a model does without having to refer to the model description paper frequently.

The authors state that their model "consistently emphasises mass-balance principles" (which I think is trivial), but then they modify the modelled GEP and RE estimates by some empirical scalar to compare this to chamber measurements. It is unclear to me whether this correction occurs after the modelling, or whether the model has been rerun with the correction applied. They then state that (p 10282 l 2ff) these corrections accounted for the model's LAI biases, but it is unclear, how this estimate was derived, and whether this means that the LAI biases of the model were corrected. In particular p 10279 l 20ff left me puzzled as to what as actually done.

A particular concern is that the authors have applied a specific GEP factor for each fertilisation experiment, effectively compensating for a tendency of the model to overestimate the growth response to fertilisers. Applying a fertilisation specific calibration for the dominant process somewhat reduces the utility of using a process-based model to simulate the effect of fertilisation on foliar N and production. If the response had to be calibrated specifically for each treatment, this inspires little confidence into the predictive capacity of the model.

Step four in the model development only really become clear when one hits Figure 8. I strongly recommend to move this section to the model description and also outline the reasoning and main effects there. Importantly, it also remains unclear whether the results in Figures 1-7 are based on version 1 or version 3.

The authors state that they inferred GEP from NEP and RE, but how is it then possible that there are more points of GEP than of NEP and RE in Figure 1 and 9?

The authors state that the 2001 GEP is overestimated by the model. To me it seems that rather there is an jump in GEP between 2001 and the remainder of the time series. Why blame the model?

Minor comments

Abstract line 11ff. At this stage, the reader does not yet know that you mean leaf N concentrations and its effect on GEP - please make this clear.

Introduction:

P 10274 L 3-5: these estimates are based on assuming no air-pollution control, whereas the newer scenarios assuming air-pollution control suggest declining N deposition over Canada. Locally this may be different of course, but here and in the discussion there should be a reflection as to how realistic the simulations and experiment in assuming a quadrupling of N deposition are. See Lamarque et al. 2011

P 10274 L 10 - 15. This text confuses time-scales. Peatlands have arguably been a large C sink during the holocene, but their contribution to the current net terrestrial C uptake is likely rather small, given the small NEE and limited geographic extend.

P 10274 L 24ff. Yes, but see Janssens et al. 2010

P 10275 L 26ff: This is unclear, partly because one does not know what the PEATBOG model is actually doing. I would recommend to create a new Section 2.1, in which the model is briefly described (see comment above), and this explanation is integrated

P10279 L 7ff. To remove all doubts, this paragraph should end with, “the results presented in Fig 1-9 are based on version X, unless stated otherwise.”

P10280 L 1: This paragraph would follow more logically the first paragraph of this section. Maybe move the model strategy paragraph to a separate section, in which the PEATBOG model is also briefly explained?

P 10280: L 17: I’m left puzzled as to why the ER data were corrected linearly and by

year. Why would one not assume that the bias between day-time and daily average RE was constant with time?

P 10283 L 7: As this refer to the fertiliser response, it should read “with increasing N inputs/fertilisation”.

P 10284 L 1: This mentioning of model details would be more suitable within the Methods section - comes a bit out of the blue here

P 10285 L 11 I cannot see this in Figure 5, possibly due to the large variability there?

P 10293 L 24: This is one of the places where an introduction to the PEATBOG model would really help to understand what is going on.

P1ß296 L 3-5: Would one not rather assume that the model converges to a new equilibrium without a phase of C losses? Please explain why you think that in the long term N fertilisation would cause C losses.

P 10296 L 19: How dod you extrapolate to 700 years. This is not clear.

Table 3: I think that it’s worth highlighting in the text that despite tuning the GEP, PEATBOG overestimates PEAT storage by a factor of three, even though the vegetation cover seems to be appropriately simulated. Why?

Figure 5 & 6: The long-term trends are difficult to spot because of the large interannual variability. I recommend smoothing these results using an 8 year running mean.

References:

Janssens, I A, W Dieleman, Sebastiaan Luyssaert, J A Subke, Markus Reichstein, R Ceulemans, Philippe Clais, et al. 2010. “Reduction of Forest Soil Respiration in Response to Nitrogen Deposition.” Nature Geoscience 3 (5). Nature Publishing Group: 315–22. doi:10.1038/ngeo844.

Lamarque, Jean-François, G Page Kyle, Malte Meinshausen, Keywan Riahi, Steven J

Smith, Detlef P Vuuren, Andrew J Conley, and Francis Vitt. 2011. “Global and Regional Evolution of Short-Lived Radiatively-Active Gases and Aerosols in the Representative Concentration Pathways.” Climatic Change 109 (1-2): 191–212. doi:10.1007/s10584-011-0155-0.

Interactive comment on Biogeosciences Discuss., 11, 10271, 2014.