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Biogeosciences Discussions

Interactive comment on "Modeling the vertical soil organic matter profile using $^{210}Pb_{ex}$ measurements and Bayesian inversion" *by* M. C. Braakhekke et al.

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GENERAL COMMENTS

It's a very interesting paper which demonstrates a Bayesian approach in far more detail than most of the papers in this field that were published before. The graphics are often creative (e.g. the violin plots are somewhat uncommon but very informative), but need more work. I would suggest merging or removing a number of figures, as indicated below. Moreover, I have to admit that after reading this paper a bit more thoroughly, I found some serious problems that I had missed before. For example, the definition of the pools should be clarified (e.g. what measurements are associated with which model variables). Moreover, the model does not implement a dynamic vegetation module, but assumes constant litter input from the soil, yet the authors draw

the conclusion that rhizodeposition has no strong effect on soil C based on results of this model.

Furthermore, I get the impression that the authors, in enthusiasm, are mixing up means and end . In particular, you should either in conclusions or in discussion, return to your research questions and systematically list your answers: i)Question: What is the relative importance of bioturbation, liquid phase transport, root litter input and decomposition? Answer: In Loobos, liquid phase transport is the most important mechanism, in Hainich, liquid phase transport and bioturbation are of similar importance ii) Question: How much organic matter is present as material potentially transportable with the liquid phase. Answer: Not entirely sure(?). iii) Question: What use are 210Pbex methods? Answer: 210Pbex measurements explained some observations at Loobos, but none at Hainich. You should also dramatically reduce the number of graphs, by merging Fig. 4 and Fig. 9. Fig 5. And Fig. 10. Fig 6 and Fig 11, Fig 7 and Fig 12 so the reader is not lost in the number of graphs and can better compare the two sites.

Also consider skipping two of the calibration setups (strong priors/weak priors/including 210Pbex data). You don't elaborately discuss the differences in outcomes and in particular since you come up with 3 cases for each run, this distinction is very confusing but does not add to the reader's understanding of the importance of your work. Just mention that you tried different setups, but present your most important run (strong priors/including 210Pbex).

Since my recommendations involve additional calculations, I had to use the classification "major revisions. Still, I don't think that my objections are insurmountable and I am looking forward to receive a revised version of the manuscript.

SPECIFIC COMMENTS

1- Abstract, Line 10-15 "13 parameters, related to. . ." how were these parameters selected? More particularly, why are litter inputs constant, whereas I would think that these are more uncertain than decomposition constants. In 2.2.1. I learn that "An-

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nual root litter input was taken from Smit et al., (2001)". I (assume, but I) would like to know whether these measurements were taken from the same site, or used as proxies? This should give me an idea of the kind of uncertainty that is introduced by using prescribed input of litter. I would like to see reruns using a model for a-priori parameter distributions that deals with uncertainties in litter inputs as well.

2- Please merge table 1 and 2

3- Introduction, "To reduce. . .numerical models". Use at least 2 sentences.

4- Introduction, "While these. . .one profile". The same point (depth profile is very important/more important than lateral gradients) is repeated three times. Consider something such as: "Most drivers (e.g. . ..) are exerted at the surface, and due to. . .the cm scale is very important . Individual mechanisms moreover, are often restricted to particular layers".

5- Introduction, "as is currently dome in most SOM models": Parton et al., (1987) is about the century model, which uses a layered soil physical structure. Hence it is not an appropriate example (even though I would accept the point that many models do not implement a vertically explicit soil). Please check Schimel et al., (1994) as well.

6- Introduction, "Therefore, in order for a SOM profile model to be robust over different ecosystems and soil types". You continue to describe model applications where you allow parameter vectors to differ between the two sites. This approach has its own merits in terms of process understanding, but you have not made any effort to show that your model works for different ecosystems. In fact, I think that you will need to discuss what is needed to have your model performing across ecosystems and how your work has contributed with respect to this aim.

7- Introduction, "Hence, it is generally. . .as equifinality". Please consider something like "A particular observation can be explained from different and conflicting mechanisms, a problem referred to. . ."

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Interactive Comment

8- Introduction, “In past studies. . .at depth”. You did not use 13C/14C, so please skip this section and start with fallout radio-isotopes instead. Then contrast briefly with the 13C/14C method (as you already do) when explaining why radio-isotopes are more interesting.

9- Introduction, “ii) How much. . .particulate material” you haven’t (really) discussed before that conversions from and to mobile material is a problem. Please consider to rephrase as something like “Which fraction of organic material can be lost from a forest ecosystem through leaching”.

10- Introduction, “iii) what is. . . optimized parameters”, unclear and needlessly complicated sentence, consider something like “How does the . . .method improve our understanding of. . .”.

11- (Sect. 2.1.) “We focus. . .optimized parameters”. Unclear, did you mean “on model parts that are sensitive to optimization parameters?”

12- (Sect. 2.1.). “Immobile decomposition products of litter flow from. . .to and from . . . to. . .” Something that is immobile does not move or flow by definition, please rephrase.

13- (Sect. 2.1.). “Were started without any carbon in the profile” Why?

14- (Sect. 2.1.1.) Please rephrase “root litter are externally input”

15- (Sect. 2.1.1.) (This is my most fundamental objection, because numerous conclusions depend on it. . .) You use a distinction NLS (non leachable slow organic matter) and LS (leachable slow), yet in continuation you argue that “The LS-OM (is LS-OM the same as LS?)” represents potentially leachable material, the bulk of this material is in fact immobile due to the adsorption to the mineral phase”. How do you distinguish this “bulk of LS-OM” which is “in fact immobile” from non-leachable slow organic matter, or how is non-leachable different from immobile?

16- (Sect. 2.1.3.) “210Pbex is input. . .is negligible”. Using “immediately bound” already entails that “time needed for adsorption in negligible”. Remove either immediately or

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"time. . .negligible".

17- (Sect. 2.1.3.) The atmospheric. . .set to 1 (see also Sect. 2.3.2.). I don't understand this section, 1 what? Section 2.3.2. does not really clarify this.

18- (Sect. 2.2.1.) Consider reformulating "The area. . .was planted", something like "Pine was planted on sand dunes in the early 20th century" makes better reading.

19- (Sect. 2.2.1.) "Due to" starting two consecutive sentences.

20- (Sect. 2.2.1.) "Due to. . .soil fauna". Use two sentences here.

21- (Sect. 2.2.1.) "The grass. . .ingrowth cores" mentioned before, were these measurements taken at the same site? What kind of uncertainty is associated with these measurements? Why is this uncertainty not expressed in the a-priori parameter ranges?

22- (Sect. 2.3.1.) My objection here has to do also with the distinction between NLS and LS (LS-OM). Either in this section or in an additional section you should explain how particular measurements are associated with model variables, e.g. what kind of measurements did you use to evaluate model outputs, how do NLS/LS compare with lab or field measurements.

23- (Sect. 2.3.2.) Remove repeated "the".

24- (Sect. 2.3.2.) Consider removing "the this. . .shown in", just refer to Fig 2 in brackets after "the predictions".

25- (Sect. 2.4.2.) "The priors. . .minimum at 0" just because the parameters should be positive, does not force us to conclude that a log-normal prior is appropriate.

26- (Sect. 2.4.2.) "maximum likelihood at 0.014 yr-1" Why?

27- (Sect. 2.4.2.) "showed that . . .the data". If the parameters are unconstrained by the data, than the model is either complete nonsense (uniform posterior distribution over

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an extended range), or over-parameterized (uniform posterior distribution, very strong correlations). Please discuss which is the case here.

28- (Sect. 3.) Multi-modality is a complex phenomenon to show. If it is the case, we would have to see the MCMC chain moving to and from different optima with similar frequency. However, we motivate the assertion that there is convergence from the observation that the MCMC chain no longer jumps to radically different parameter sets. Hence the same observation can support either argument.

Moreover, there is a problem with the notion of similarity of the different optima, because how similar is "similar"? An explanation on the selection criteria is needed in particular when in continuation you argue that particular cases are much stronger than others (e.g. Further confidence in case B is gained. . .Kindler et al., (2011)). If one case is much stronger than the other, according to definition, the other is not really a case any longer.

The easiest way out I think is to simply skip the terminology. You can just argue that you used three optima for a closer inspection without using the confusing term "multi-modality".

29- (Sect. 3.1.3.) "Virtually all. . .phase transport", consider rephrasing, use something like "most organic matter infiltrates the soil with moisture".

30- (Sect. 3.2.3.) "The posterior. . .Figs. 7-10" the reader is lost here: which graph shows what? And why? Also you should reduce the number of graphs drastically as indicated in continuation.

31- (Sect. 4.1.) You have come to a peculiar conclusion here: 210Pbex which was not measured at Loobos but at a similar site. Yet it is well described by the model. In Hainich, it was measured, yet "was much less informative (Sect. 4.6.)".Combined with several other statements in the paper, I get the impression that you simply didn't see much use for 210Pbex. If this is the case, you should draw this conclusion instead of

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arguing that “further study. . .is needed (4.6.)”

32- (Sect. 4.3.) Reconsider the number of sentences starting with or including “the fact that”.

33- (Sect. 4.3.) “Provide strong constraint” consider reformulating, something like “constrain KLS within a small range.”

34- (Sect. 4.3.) “Here, a decrease. . .this pool”. Consider reformulating, I understand what is meant, but the section reads as if the MCMC has a mind of its own. Something like “The profile that was observed can be explained in different (conflicting) ways”.

35- (Sect. 4.5.) “The general. . .Sects. 4.1 and 4.2.”. You just seemed to do exactly what you state here is impossible (considered the general validity of the model based on BC results) in the previous section “For all. . .very similar.”

36- (Sect. 4.5.) “this conclusion. . .LS-OM pool.” I pointed out similar issues before: for the reader it was not clear exactly what you were comparing with the LS-OM pool.

37- (Sect. 4.5.) “Our results. . .Rasse et al., 2005”. Similar to a previous comment : as I understand, you are prescribing the production of root litter. No wonder that you cannot explain many soil C measurements from rhizodeposition.

38- (Sect. 4.6.) Discussed already in some points about 4.1.

Interactive comment on Biogeosciences Discuss., 8, 7257, 2011.