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Biogeosciences Discussions

Interactive comment on "Information content of incubation experiments for inverse estimation of pools sizes in the Rothamsted carbon model: a Bayesian approach" *by* B. Scharnagl et al.

Anonymous Referee #2

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Scharnagl, Vrugt, Vereecken and Herbst estimate the prior distribution of ROTHC model parameters using a Bayesian inverse approach with the DREAM (adaptive Metropolis-type) algorithm. The authors demonstrate the importance of including prior information on microbial biomass to constrain other parameters in the model and the role of bias in pool size estimates in biasing other pool sizes (9344:20). In all the paper is an excellent contribution and is publishable after the authors address these minor comments.

1. The paper was largely well-written and interesting. The synthetic data approach makes for an interesting case study, but the known parameters had the effect of 'shoot-

ing fish in a barrel', the uniform priors in Figs 3 and 5 are pretty tightly bounded around some logical values given the known value. A more exemplary uniform prior would stretch from 0 to the mass of the soil (or C equivalent). These are the logical bounds for the pools.

2. Please check a more recent number for global soil C. The most recent estimate that I know of is 1672 Pg organic C in the northern circumpolar permafrost alone, [Tarnocai et al 2009 GLOBAL BIOGEOCHEMICAL CYCLES, VOL. 23, GB2023, doi:10.1029/2008GB003327] which they argue is about half of the global total, which would then be on the order of 3350 Pg C.

3. The reduction equation for k (equation 2) is a bit unfortunate as it does not follow Leibig's Law, in which case one of the factors would constrain the decomposition rate. These multiplicative equations are used frequently, and it's not the author's intent to explore this assumption, but I'd argue that a better expression would be k*=min{f(T),f(theta),f(gamma)}k.

4. Please be more explicit about what components of the study were synthesized and which come from incubation results to avoid confusion on the part of the reader.

5. I agree with Referee #1 on the Xu and Fox references. The approach is interesting but not wholly novel in environmental science.

6. p. 9338, bottom. numerical optimization can give an estimate of an underlying pdf in the form of uncertainty.

7. Please describe what is meant by 'tunes the scale and orientation' of the distribution in the DREAM description. Does this contribute to the efficiency? 'Tune' often sets off red flags when describing parameters or optimization routines.

8. Some numbers in the abstract will help quantify the ideas the authors are trying to advertise. By how much did adding microbial biomass prior information help? (Also, is this a likely 'known' in real SOC studies?)

BGD

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9. Some passages in the Results section read more like Discussion points, but I tend not to be a stickler about this.

10. p 9342:21 and elsewhere. Is it true that the best results were found after 900 days? is there any value in extending experiments longer for the slower pools then? It would be interesting to see a graph of the variance decreasing over time. Or a formal measure of the information content divergence, like the Kullback-Liebler difference. I note that this paper uses the term 'information' frequently while addressing it implicitly rather than explicitly. A graph of K-L information divergence (or just the variance, or normalized Shannon entropy from uniform [max] entropy to the derived pdf) over experiment time from uninformed prior to posterior distribution would be illustrative.

Minor comment: tense punctuation, capitalization, and the spelling of let's [http://www.thefreedictionary.com/let's] in a few places in a few places (e.g. p 9333 L 10, p 9336 L. 19, P. 9336 L. 7, 9337:23, 9342:11)

Interactive comment on Biogeosciences Discuss., 6, 9331, 2009.