Biogeosciences Discuss., 6, C4005–C4009, 2010
www.biogeosciences-discuss.net/6/C4005/2010/

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.

B. Scharnagl et al.

b.scharnagl@fz-juelich.de

Received and published: 17 January 2010

Reply to comments of Anonymous Referee #2

Minor comments

(1a) The synthetic data approach makes for an interesting case study, but the known parameters had the effect of "shooting fish in a barrel".

See reply to comment (1.3) of Anonymous Referee #1.

(1b) 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.

We agree in that the bounds of the feasible search space (which also define the uniform priors) have been chosen in the light of the known (true) pool sizes. This was mainly for graphical reasons. It simplifies the display of the results on a common probability scale. However, we do not agree in that the logical bounds stretch from zero to the amount of TOC. For example, it is known that the amount of microbial biomass carbon (BIO) usually makes up only a very small portion of TOC. The same holds true for the most labile pool (DPM). Many studies have shown that the largest part of TOC is made up of slow and recalcitrant pools. One may therefore argue that the amount of humified organic matter (HUM) is not expected to be close to zero. In the revised manuscript we will adjust the bounds of the feasible search space to be more exemplary. However, we like to point out that the particular choice of the priors remains subjective. But this is not a conceptual problem from our point of view. It does not affect the outcome of this study.

(2) Please check a more recent number for global soil C. [. . .]

We are grateful for pointing our attention to this very interesting paper. We will include this recent estimate on the global SOC stock in the introductory section of the revised manuscript.

(3) The reduction equation for k (equation 2) is a bit unfortunate as it does not follow Liebig's Law, in which case one of the factors would constrain the decomposition rate. [. . .]

BGD

6, C4005–C4009, 2010

We totally agree in that the multiplicative formulation of the rate modifying functions is simplistic and likely an inappropriate representation of real-world processes. The expression suggested by Anonymous Referee #2 appears to be more reasonable indeed. But as the referee pointed out, it was not the intention of our study to explore this (or any other) assumption inherent to ROTHC.

(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.

We will make this more clear in the revised manuscript.

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

See reply to comment (2.1) of Anonymous Referee #1.

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

We are not sure if we understand the comment correctly. On p. 9338, bottom we explain the relation between classical inversion algorithms that only seek the best estimate (in some predefined sense, for example Eq. 5) of the unknown model arguments and the Bayesian approach that aims to estimate the underlying pdf of the unknowns (which includes the optimum values). The approach utilized herein therefore extends calibration algorithms beyond finding a single best value. Classical inversion methods can be combined with some first-order approximation to yield uncertainty bounds on the best estimate. However, in practice it is very difficult (if not impossible) to check the validity of the underlying assumptions of this approximation.

BGD

6, C4005–C4009, 2010

(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.

DREAM is a self-adaptive MCMC scheme that uses the current states of the other Markov chains to generate proposal points in the respective chain under consideration. This is done by Differential Evolution which uses a multiple of the difference of the location of different chains. The expression "tunes the scale and orientation" refers to the self-adaptation of the proposal distribution. We will replace the word "tune" by "adapt" in the revised manuscript. If the proposal distribution is too wide too many candidate points are rejected with slow convergence of the sampler. On the contrary, when the proposal distribution is too small, too many candidate points are accepted with high acceptance rate, but slow movement of the chain and it therefore takes a lot of draws to explore the target distribution. The efficiency of MCMC samplers thus heavily relies on a good selection of the proposal distribution used too generate candidate points in the individual chains. For more information on these aspects and a detailed description of the DREAM algorithm please refer to Vrugt et al. (2008, 2009, cited in the discussion paper).

(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?)

We will add numbers (base on KL divergence) that quantify the benefit of using an informative BIO prior in the Abstract and Discussion section of the revised manuscript. Microbial biomass is not routinely measured in incubation studies, but see Nicolardot et al. (1994) and Follett et al. (2007) (both cites in the discussion paper) for some exceptions. One message of this study is that prior knowledge on microbial biomass adds valuable information on carbon dynamics and – as briefly discussed in the discussion paper – would help to assess the utility of SOC models in general. We

BGD

6, C4005–C4009, 2010

therefore advocate to conduct these kind of measurements more regularly, especially when the data are to be interpreted by inverse modeling.

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

We will move these passages to the Discussion section.

(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? [. . .]

The formulation mentioned is indeed misleading. From the results shown in the discussion paper it becomes intuitively clear that extending incubation beyond 900 days would add more information especially on the slow pool (HUM). However, because of correlations between pool sizes other pool estimates would also benefit from longer incubations. We are especially grateful for pointing our attention to the Kullback-Leibler (KL) divergence as a quantitative measure for the information contained in the incubation data. In the revised manuscript, we will report on results for longer incubations. We will also add a graph of KL divergence over incubation time.

(11) Minor comment: tense punctuation, capitalization, and the spelling of "let's" in a few places.

We appreciate this comment. We will try our best to resolve these shortcomings in the revised manuscript.

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