Biogeosciences Discuss., 9, C8391–C8411, 2013
www.biogeosciences-discuss.net/9/C8391/2013/


**Biogeosciences Discussions**

# *Interactive comment on* "The relative importance of decomposition and transport mechanisms in accounting for C profiles" *by* B. Guenet et al.

**B. Guenet et al.**

bertrand.guenet@ua.ac.be

Received and published: 20 February 2013

*Answer to comments from the reviewer.*

*Comments from the reviewer were left intentionally in this document and written in roman font. Our answers are written in italics.*

Comments from M. Braakhekke:

This paper describes a modelling study of soil carbon cycling at a long term bare fallow experiment performed on a Chernozem soil in Russia. Soil carbon profile measurements from several time points up to 58 years into the experiment as well as an unmanaged site were used to calibrate soil carbon models. Two formulations of organic matter decomposition and three formulations of soil organic matter transport, were



combined in a factorial model calibration experiment. Calibrations were performed in a Bayesian framework.

This study probably represents the most comprehensive comparison of model formulations for soil organic matter transport done so far. Also the comparison of a representation of limitation of SOM decomposition by labile substrates with a simple first order kinetics model is relatively new. Especially the combination of vertical transport combined with this new decomposition model is very innovative. This topic is also relevant since the combination of the two may have important implications for long term soil carbon cycling, although this still quite uncertain. Long term bare fallow experiments represent a very useful "test bed" for studying these processes because stopping of fresh litter input may lead to increased limitation of SOM decomposition.

Unfortunately, the execution of the experiment is done rather poorly to my mind. The authors have performed the calibration in a Bayesian framework–which I commend–but several unnecessary simplifying assumptions were made which reduce the merit of the results. Furthermore, there are several issues with the results which suggests that mistakes were made. The exact details of the study setup cannot be fully checked because the methods section is lacking much important information. Finally, I also believe that too strong conclusions are drawn from regarding the importance of the nature of SOM decomposition.

It seems unavoidable that the calibrations have to be redone. Furthermore, although I classified the paper as in need of major revision, it may be necessary to submit a new manuscript, in view of my many criticisms. I'll leave this decision to the editor.

For clarity I divided my comments into several levels of importance labelled "suggestions", "strong suggestions", and "required". Suggestions are points that I believe would improve the paper but do not strictly need to be followed. Required means that the authors should follow the advice, or provide adequate reasons for not doing so. Strong suggestions are in between these two.

Finally, a paper describing a similar calibration study I performed is currently under review for BG and available in BGD (Braakhekke et al., BGD, 9, 11239–11292, 2012). It has many similarities with the current study and might be helpful to elucidate some of my comments.

*First of all, we would like to sincerely thank Dr. Braakhekke for the deep review he provided. He probably worked on our manuscript for days and we greatly appreciated the effort. We (and probably most of our colleagues) are not used to receive such detailed reviews and all the comments were very helpful.*

General comments

It is not clear how the posterior parameter estimates were derived. The usual approach for models that cannot be analytically inverted is to use a Markov Chain Monte Carlo method to sample the posterior distribution. However, no posterior distributions are shown and, aside from the dashed lines in figures 4-5 (which are incorrectly derived to my mind; see below), no information is provided on posterior parameter uncertainty. This suggests to me that the authors simply used a classical gradient method (e.g Levenberg-Marquardt). If this is correct, I would strongly suggest redoing the calibration with an MCMC approach. Contrary to what the authors assume (page 14154, lines 8-10) the posterior distributions may be quite non-Gaussian and may even be multi-modal, since the models are complex and likely over-parameterized w.r.t. the available data. This can only be evaluated by approximating the full posterior distributions. (strong suggestion).

*Indeed, we used a classical gradient method for reasons that were not clearly explained in the manuscript. We have used a variational scheme to minimize a cost function based on the calculation of the gradient of the cost function with respect to the parameter assuming that all uncertainties are Gaussian (Tarantola, 1987). This is a strong assumption that greatly simplifies the calculation and characterization of the solution.*



*- First, we should mention that this study is a part of a bigger project aiming to represent the soil C profile in the global land surface model ORCHIDEE. One objective is to be able to optimize the vertically-discretized soil carbon model embedded in ORCHIDEE using various data streams. In a first step, we decided to start with a theoretical model, isolated from ORCHIDEE, to select and characterize the best soil carbon model to be included in the ecosystem model. The second step will be to use the whole model and to optimize with a variational approach all parameters of ORCHIDEE. The choice of a variational approach versus a monte carlo one follows from running time constrain that may be prohibitive with a monte carlo approach and a complex process-based model.*

*- Following that point, we need to recall that we have already performed several parameter optimization studies with the ORCHIDEE model using the same variational approach (santaren et al 2007, Verbeeck et al. 2011, Kuppel et al. 2012) and that through these studies we gained confidence that the level of non linearities considered in the proposed soil carbon models do not prevent the variational algorithm to obtain a satisfactory solution, provided few cautions. The only requirements or cautions are the need to perform several optimization starting from different prior parameter values, randomly distributed in their allowed range of variation. We then select the case that provides the lowest cost function. With this approach we are much less sensitive to potential local minima. We have included the description of this sensitivity tests in the manuscript and their justification.*

*'The optimization assumes that the errors associated to the model parameters and the observations can be described with Gaussian Probability Distribution Function (PDF). Assuming Gaussian PDF, this approach is sensitive to potential local minima. We therefore perform 20 optimization starting from different prior parameter values, randomly distributed in their allowed range of variation. We then select the case that provides the lowest cost function. With this approach we are much less sensitive to potential local minima. It makes use of prior information on the parameters, minimizing an objective function that measures the distance between modelled and observed car-*

*bon vertical profiles and between prior and optimized parameter values (using a least squares approach).'*

The authors assume that the errors on the parameters and the observations are uncorrelated (page 14155, line 1-2). While the ignoring correlations in the observations may be acceptable, I believe that this assumption is unjustified for the parameters. Since the amount of data used in the calibration is quite limited compared to the number of optimized parameters, all models are presumably over-parameterized, which means strong correlations between the parameters can be expected. This must be considered, also for determining the predictive uncertainty (cf. my point related to Figs 4 and 5, below). If the posterior distribution is sampled using MCMC, the posterior sample will automatically reflect these correlations. In case a gradient approach is used to derive the mode, the linear correlations can be derived from the Jacobian at the posterior mode (see e.g. Omlin  Reichert, Eco. Mod., 1999). (required)

*We agree with the reviewer that the error correlation between the parameters might be significant. Given our assumption of Gaussian error, we thus have calculated the linear correlations, i.e. the full posterior error covariance matrix. We now discuss the values of the parameter error correlation.*

*For instance 'The optimized parameters and their associated variances are presented in the Table 1 and 2. For some models, important correlation factors are observed (supplement material Fig. 1). Considering the method used to optimize the parameters, these important correlation factors make complicated the presentation of the model output within an envelope. Therefore, we present the model results using the optimized parameter without envelope (Fig. 4,5 and 6). The most important correlations are generally observed between $\mu$ and kSOM which control the input and the output of the SOM pools but also between c and kSOM for MIN2-TD which both control the SOM mineralization.'*

*or 'Finally, the correlation matrix (supplement material Fig. 1) show some cases with*



*important correlation between parameter making difficult the calculation of an envelope but generally, correlation factors are low indicating that our model are not over-parameterized in spite of the limited amount of data compared to the number of optimized parameters.'*

The conclusions drawn from the model results regarding the importance of labile substrate limitation of SOM decomposition for long term soil carbon dynamics are too strong, considering the uncertainty in the models, a the limited amount of data used in the calibration, and our limited understanding of soil carbon cycling. The superior fit of model formulation FS2 is to be expected since it has one additional parameter compared to FS1 (cf comment first on Results section below). But even if model FS2 would score better om a statistic that considers the number of parameters, this would still not be hard evidence for the importance FOM-SOM feedbacks. I would recommend constructing an additional first order kinetics model with three pools instead of two. If this model would also perform worse than model FS2, this would lend a bit more support for the priming formulation. However, I believe that the amount of available data in this study in general precludes the possibility of such hard conclusions. The wording of several sentences in the discussion needs to be adjusted accordingly (page 14158 lines 5-8 and 24-27). (required)

*We wrote another models with three pools and therefore more parameters and we added the results in the new version of the manuscript. We also revised the discussion to avoid over interpretation of our results.*

*For instance: 'For the data we use, we first show that the substrates interactions representation proposed by Wutlzer and Reichstein (2008) is interesting formulation to represent cases where FOM is not at the equilibrium such as in the bare fallow soil.'*

*or 'Finally, the MIN3 formulation never obtained the lowest BIC, suggesting that the best fit is not ever obtained with the most parameterized model. It indicates that for the case when priming model improves the fit with the data, it is not just an effect of the* *number of parameter increase.'*

It is incorrect to refer to the limitation of SOM decomposition by labile substrates as the "priming effect". The priming effect, as I'm sure the authors are aware, is simply the observed increased (heterotrophic) respiration under certain conditions. This can be caused by many factors, including stimulation of microbial activity due to increased availability of labile substrates (see Kuzyakov et al.,SBB, 2000). Calling the dependence of SOM decomposition on FOM priming is mixing up cause and effect. A more appropriate description could be "substrate interactions". This should be corrected in various locations in the MS. Calling model formulation FS2 a "priming model" may be acceptable, because one could argue the model is designed to capture the priming effect. (required)

*We modified it the entire manuscript*

The term "priming", "priming model", and "priming effect" are used with double quotes throughout the paper. Double quotes are used to introduce a new term or to signal that a term is term is used in an unusual way. This only needs to be done only the first time this term is used. Please remove the quotes for all the other times the term is used. Possibly the quotes could remain in both the abstract and the introduction. (strong suggestion)

*Done*

Throughout the paper "over estimation" and "under estimation" are spelled incorrectly. Please write "overestimation" and "underestimation". (strong suggestion)

*Done*

Abstract Line 1: Please write either "Soil is the major terrestrial reservoir" or "soil is one of the major terrestrial reservoirs". (required)

*Done*



Line 10: "advection or diffusion", replace "or" with "and" (suggestion)

*Done*

Introduction

Page 1414, line 7: please write: "...first soil layers which is/was considered..." (suggestion)

*Done*

Page 1414, line 11: "an increasing attention"; please remove "an". Attention is an uncountable noun. (required) *Done*

Page 14147, lines 18-23: Bruun et al. (SBB, 2007) compared a model with dispersion (diffusion) and advection to a model with advection only. Therefore I don't think it's really correct to write that no clear comparison between transport representations has been *Done* so far. Just indicate that in the current study the comparison is done more formally and comprehensively and adds the diffusion-only representation, and the two representations of decomposition. (strong suggestion)

*Done*

Page 14148, line 18: please remove "has" and "for years". (suggestion)

*Done*

Methods

The paper is missing much important information on the modelling and optimization techniques. Although some things may be guessed or deduced, it is better to provide enough explicit information so that the study could in principle be reproduced by others. Certain information could included in an appendix or online supplementary material (I'll leave that to the authors' discretion) but the methods section definitely needs to be extended. (required)

Specific aspects related to the models that should be included are: – the top and bottom boundary conditions of used; depth of the bottom boundary

– the model solution; i.e. what numerical scheme is used? What are the thicknesses of the layers of the spatial discretization; the same as the measurement depths?

– the precise setup of the simulations; e.g. the initial conditions and the simulation length. Is the model run until steady state to acquire the estimate for the steppe soil?

*We added the following paragraphs to precise some technical aspects: "All the models were developed using R 2.11.1 and run at a yearly time step. The models run from the ground (0 cm) until 200 cm and the vertical resolution is 5 mm for each layer. To compare with the data, C stocks are then summed each 20 layers to obtain 10 cm layers. Moreover only the layers until 150cm are used since the data could be converted in kg C m-2 only until 150cm. The equations were solved using the deSolve library (Soetaert et al., 2010). This library solves a system of ordinary differential equations resulting from one dimensional partial differential equations that have been converted to ordinary differential equations by numerical differencing. We run the models with the steppe condition (i.e. with input of FOM) during 2000 years to reach the equilibrium. The steppe was assumed to be at the equilibrium and we used the C stocks obtained after the 2000 years run for the steppe. Then we run the model for 58 years without FOM input to reproduce the 58YBF plots condition. From this run, we extracted the data after 20 and 26 years to reproduce the 20YBF and 26YBF plots condition, respectively."*

Also the algorithm for doing the calibrations and its precise setup should be described.

*More details are now added in the section 2.3 Parameters optimization.*

*For instance: 'Our approach is based on Santaren et al., (2007). The optimal parameter set corresponds to the minimum of the cost function (18) and contains both the mismatch between modelled and observed fluxes and the mismatch between prior*



*and optimized parameters. x is the vector of unknown parameters, xb the prior parameters, H(x) the model outpus and y the vector of observations. Pb describes the prior parameter error variances/covariances, while R contains the prior data error variances/covariances. An efficient gradient-based iterative algorithm, called L-BFGS-B (Zhu et al., 1995) was used to minimize the cost function. This algorithm prescribes a range of values for each parameter. At each iteration, the gradient of the cost function J(x) is computed, with respect to all the parameters. The L-BFGS-B algorithm does not provide uncertainties or error correlations between optimized parameters but, when the J(x) is minimized, it calculates the posterior error covariance matrix on the parameters Pa from the prior error covariance matrices and the Jacobian of the model at the minimum of the cost function, using the linearity assumption (Tarantola, 1987). Absolute values of the error correlations close to 1 imply that the observations do not provide independent information to differentiate a couple of parameters.'*

Please consider writing equations more professionally, e.g. with MS Word Equation Editor, LATEX, or similar software. Single line equations are difficult to read, particularly Eq. (14)-(15). (strong suggestion)

*Done*

The units of the variables in the methods section are used inconsistently: C stocks are expressed in t ha-1, the diffusion coefficient is expressed in cm2yr-1, and the advection rate is expressed in mm yr-1. I would strongly suggest using consistent units, preferably SI. For graphs the quantities can be converted if necessary. (strong suggestion)

*Done*

Page 14150 lines 3-8: how was the soil in the LTBF site kept bare; just by ploughing? Please explain. (suggestion)

*The LTBF is weeded manually, we now added this information.*

The explanation of the method used to account for compaction of the LTBF soil with respect to the steppe site is not clear. What exactly is meant by the “floor of the steppe”? Was this correction applied on the model results or on the measurements? (required)

*Since we calculated the C stocks in kg C m-2 the compaction effect is integrated in the data using the bulk density. The same data in kg C m-2are used to optimize the parameters and therefore the compaction effect on transport is implicitly taken into account.*

*We used a ‘decompaction’ function only to better represented on the graphics what is observed on the site (the bare soil floor is under the steppe floor). Because this part was not that clear, we modified as following: “To take into account graphically the compaction effect on soil depth, we define the point at 0m depth as the floor of the steppe and then the soil layers were assumed to be linearly compacted through time since 1947 to reproduce the observed final difference of 10 cm between the two bottom horizons.”*

*We also added in the section 2.3: “The compaction observed on site its effects on transport are taken into account through the use of the bulk density in the stocks calculation in the dataset. The compaction effects are implicitly represented in the model as the optimization was performed with the stocks expressed in kg C m-2.”*

Here it is written that the soil was sampled in 10cm depth increments down to 150cm, i.e. 15 data points per profile. However, section 2.3 and Fig 4 5 indicate 12 points per profile. Why the difference? (required)

*The bulk densities were only available until 120cm therefore even if the soil was sampled until 150cm (and measured in kg C kg-1) we can have data in kg C m-2 only until 120cm. We now added this information: “Since bulk densities are available only until 120cm, the soil profiles in kg C m-2 are calculated only until 120cm depth.”*

Equation (1): in the text it is written that the C stocks are expressed in t C haôĂĂĂ1. However the units of the variables in this equation lead to units of g cmôĂĂĂ2 for the



C stock. Please correct. (required)

*Done*

Fig 1: there are arrows leading from the SOM to the FOM pool, but this does not correspond to any process or feedback explained in the model description. (required)

*We assume that a part of the SOM decomposition products is as labile as FOM and therefore returns to the FOM pool. We added this information.*

Page 14151, line 5: this sentence, up to the comma is awkward. Consider revising. (suggestion)

*We rephrase this sentence. “The models used in this study split the total OM in two pools, the FOM and the SOM for each soil layer (Fig 1).”*

Equation (3): this equation is incorrect because the change of the FOM stock is also affected by input and transport, which are not included. Consider defining a decomposition flux (e.g. Fdec), and later defining the rate of change of the stock as the sum of all fluxes, including input, transport and decomposition. Alternatively, one could write the rate of change of a stock by one specific process PR as follows: This applies also to equations (4)-(7). (strong suggestion)

*We modified the equations following the comment.*

Equations (4)-(5): There seems to be a mistake related to the parameters r and e. It is written that these parameters partition the decomposition flux of FOM into a fraction flowing to SOM and a fraction lost as CO2. First, this means that these two quantities are fully dependent according to r = 1 - e. Thus, really only one parameter needs to be defined here. Second, in Table 1. it is indicated that r is not included in the calibration and fixed at 0.4, while e is included (but for some reason the posterior estimate is 0.5 for all models). This seems incorrect: either both of them must vary, or both of them must be fixed. In any case the sum of the two quantities must always equal 1, otherwise mass balance errors will occur. If this is a mistake, the calibration must be redone. (required)

*We defined e+r $\neq$1 to take into account the FOM that is protected very quickly once it enters into the soil without any decomposition. But finally, we revised our assumption and we now removed the r parameters and we simply defined r=1-e as suggested by the comment.*

Page 14152, line 6: Where does notation "FS" for the decomposition models come from? A more intuitive acronym might be better. (suggestion)

*We now use MIN for mineralization instead of FS.*

Eq. (7): This equation assumes that the microbial biomass always in equilibrium with FOM. Please mention this explicitly in the text. (required)

*We added this information*

Why were the 20 and 26 year BF data not included in the calibrations? To leave some data for validation? I would suggest using all profiles, since the amount of data is quite small as it is, and it seems that the value of the 20YBF and 26YBF profiles for validation is limited. (suggestion)

*We now use all the data to optimize*

Why was the decomposition rate of FOM (kFOM) not included in the calibration? Was there enough a prior information to fix this parameter? If this cannot be adequately defended reruns should be done in which this parameter is included. (required).

*We reruns the model with an optimized kFOM.*

Page 14154, line 7: please "contrasted" with "contrasting" (suggestion)

*Done*

Page 14154, line 12: please write "least squares" approach" (required)

*Done*



Page 14154, line 15: "the Fick's coefficient"; remove "the" (required)

*Done*

Page 14154, line 19: replace "than" with "as" (required)

*Done*

Page 14154, line 18-20: were the prior distributions also Gaussian? (required)

*Yes but with a very large prior error. This information is now added to the manuscript: "Prior estimates for each parameter are given on table 1. We use such values as prior because they are in the same range as parameters already published (Baisden et al., 2002; Bruun et al., 2007; Braakhekke et al., 2011) but because the c parameter have never been estimated before we consider the prior as noninformative and we set a very large prior error."*

Page 14154, line 20-21: if the parameters should be "as free as possible to fit to the data", uniform priors (possibly for all positive reals) would more appropriate. I would remove or rephrase this line. (strong suggestion)

*We removed this sentence.*

Page 14154, line 21: replace "possibly" with "possible" (required)

*This sentence has been removed.*

Page 14154, line 19-20: please provide also a reference for the justification prior estimate of the c parameter of the priming model. (required)'

*Unfortunately, we cannot give any reference because this is the first time that such relation is used and in Wutzler and Reichstein (2008), from which the equation was based, no parameter are given.*

Page 14154, line 22: "observational error" refers only to the variance of the measurements. I would suggest using the phrase "variance of the model-data residuals". (required)

*Done*

Page 14154, line 25-27: "to fulfil some statistical hypothesis...". Please explain this more clearly, possibly with a reference. Also, the "cost function" has not been explained previously. Finally, it is not discussed in the results whether this hypothesis is actually fulfilled. (required)

*We rephrase this part and we added more details: "Given that the error on the measurement part could be estimated from the existence of several replicates for each profile, we choose the measured standard deviation as error on the observations. At its minimum, J(x) should be close to the half of the number of observations (reduced chi-square of one)."*

Page 14155, line 6: please write "mean squared deviation". (required)

*Done*

Page 14155, line 5: the reference should "Gauch et al., 2003", not "Hugh" (required)

*Done*

Page 14155, line 11: replace "then" with "the". (required)

*Done*

I believe the SB, NU and LC are not widely used quantities, so they could use a bit more explanation as to their precise interpretation. (strong suggestion)

*We now give more details: "SB provides information about the mean bias of the simulation from the measurement. NU indicates the capacities of the model to reproduce the magnitude of fluctuation among the measurements. It can be considered as an indicator of the model's capacities to reproduce the standard deviation of the data. LC is an indication of the dispersion of the point over a scatterplot, i.e. the capacities of*



*the model to reproduce the shape of the data."*

Results

The number of calibrated parameters ranges between 3 and 5 for the 6 models. However, the statistical indicators used here do not reflect this, nor is it mentioned in the discussion. It is not surprising that a model which has more parameters has a better fit. When comparing the models please use a goodness of fit statistic that considers the no. of parameters such as the Akaike information criterion, or (better) Bayesian information criterion. (required)

*We now used also the Bayesian information criterion to compare the model.*

Fig 3: There seems to be a problem with the calibration results: the fit for model III is worse than that for models I or II, which should not be possible. If the number of parameters is not considered, the fit of model III should always be at least as good as for models II and III because it includes both the processes included in the other two models (diffusion and advection). This may be caused by lack of convergence of the fitting algorithm. (required)

*We double-checked our convergence and the results are ok. Advection transports C only from the top soil to the deep soil, whereas diffusion may act in the both directions. Therefore, the distribution of C in the profile in the model with advection and diffusion is controlled by two mechanisms acting in opposite directions. It may induce a worst fit with a more parameterized model.*

No distinction is made between profiles that were used in the calibration and those that weren't, neither in the graphs nor in the discussion. I as mentioned above, I think it would be better to include all data in the calibration. If the authors prefer to use only a subset of the profiles, the distinction should be clear in the discussion of the model results. (required)

*We now use all the data to optimize*

Table 1: I assume the columns with the "values for formulation" indicate the posterior modes. Please report additional information out the posterior distributions, i.e. the uncertainty and correlation structure. If possible show graphs of the marginal distribution of each parameter, or otherwise box plots. Also compare this with prior uncertainty. (required)

*We added the uncertainty and we also now give the correlation matrix for each model.*

Fig 3: It is impractical to have to refer to Fig. 2 to for the interpretations of the roman numerals. I would suggest writing the meanings of the letters a-e as titles above the bar graphs and explaining the roman numerals in the caption (possibly with abbreviations for the model formulations). Also the font size should be increased. (suggestion)

*We removed the roman numbers and we changed them for codes representing better the mechanisms included in the models.*

Figs 4-5: Similar to previous point: rather than labelling each graph with "C stock" it would be better to label each graph with the corresponding model formulation and the site. Also here the font size needs to be increased. (suggestion)

*Done*

Figs 4-5: there seems to be a misconception related the predictive uncertainty as indicated by the dashed lines. It is not fully described how these are derived but I guess that the authors made three simulations per plot: one with all parameters at the mode and two with all parameters at the mode $\pm 1$ standard deviation. This not the correct way to assess the predictive uncertainty because the correlations between the parameters are ignored. Likely strong correlations exist (cf my 2nd general point, above) which means that the simulations where all parameters are increased or reduced simultaneously are not meaningful. In some cases the dashed lines cross, which the authors interpret as the model not being realistic. I strongly suspect that this in fact caused by ignoring the parameter correlations. Instead, the predictive envelopes should be



determined from results of an ensemble of simulations with parameters sampled from the posterior distribution, in which the correlations are considered. A sample derived with MCMC could be directly used for this. Alternately, one could sample the parameters from a multivariate normal distribution around the posterior mode, but again the parameter correlations must be taken into account. (required)

*We reperformed the optimization as previously explained and we present the variance for each parameters in the table 2 and 3 but we do not represent the envelope and we do not discussed the model using the envelope because of the correlation between some parameters. This is clearly mentioned in the text 'The optimized parameters and their associated variances are presented in the Table 1 and 2. For some models, important correlation factors are observed (supplement material Fig. 1). Considering the method used to optimize the parameters, these important correlation factors make complicated the presentation of the model output within an envelope. Therefore, we present the model results using the optimized parameter without envelope (Fig. 4,5 and 6). The most important correlations are generally observed between $\mu$ and kSOM which control the input and the output of the SOM pools but also between c and kSOM for MIN2-TD which both control the SOM mineralization.'*

*or 'Finally, the correlation matrix (supplement material Fig. 1) show some cases with important correlation between parameter making difficult the calculation of an envelope but generally, correlation factors are low indicating that our model are not over-parameterized in spite of the limited amount of data compared to the number of optimized parameters'*

Discussion and conclusions

The SB, NU and LC indicators are useful quantities for evaluating model performance. However, although these indicators are reported for the different models, their interpretations are not discussed (except for the last line of the conclusions). Simply reporting these quantities without discussing what they mean in terms of the model-data comparison does not really add any insight. Either add additional discussion on this, or simply report and discuss only an overall goodness of fit statistic. (strong suggestion)

*The indicators are now used more for the discussion. For instance 'We found in the results section that for the MIN1 formulation, the worst fit to the data is always observed when only diffusion is represented, except for the steppe. In particular, the LC values are higher indicating that the shape of the data is not well represented when using only diffusion. When the MIN2 (priming model) is used, the worst fit was always with advection except for 58YBF. In this case, the LC values are high indicating that using advection only is not sufficient to reproduce the shape of the profile.'*

No attempt is made to interpret the transport equations in terms of the underlying processes bioturbation and transport with the liquid phase. I think the discussion would benefit from this. This also relates to my next point. (strong suggestion)

*We added a paragraph to discuss these phenomenon: 'Diffusion is often used to account for transport of plant debris and particulate organic matter by soil fauna whereas advection is used to represent C transport with the liquid phase (O'Brien and Stout, 1978, Wynn et al., 2005, Braakhekke et al., 2011). Soil fauna activity is closely related to the SOM availability (Decaëns, 2010). Therefore, the importance of soil fauna in the transport C in our sites decreased when FOM input are stopped and therefore when SOM availability is reduced. This suggests that different pools of SOM could be transported through different mechanisms. The more labile may be transported mainly by bioturbation whereas the more stabilized may be transported with the liquid phase. To our knowledge, this is the first time that different transport mechanisms are suggested for different pools of C.'*

The cease of fresh organic matter input in the LTBF experiment will negatively affect the soil fauna, which means bioturbation will be reduced. This in turn is expected to reduce mostly the diffusion coefficient. This means that D likely is different before and after the start of the bare fallow experiment, which has not been considered in this study.



This may have been a necessary simplification but it should be discussed, particularly since this concerns a Chernozem soil which are thought to be strongly influenced by bioturbation. (required)

*See above*

Page 14158 line 6: "shows off"; incorrect usage. (required)

*We modified this sentence*

Page 14158 line 19: replace "when" with "where" (required)

*Done*

Page 14158 line 24: remove the last "s" in "decomposers" (required)

*Done*

Fig. 6: what is meant by "carbon input"? Since the LTBF sites show also input here, I assume it includes also the input due to transport. Is this then net input (difference between flux at the top and at the bottom of the layer)? Also, please indicate which model formulation was used to derive the results in this figure. (required)

*The caption was not very clear, by Input we mean FOM stocks. As explained above, there is still FOM in the LTBF as we consider that a fraction of the SOM decomposed may be as labile as the FOM and is therefore incorporated to the FOM pool. All the models share the same input scheme. We modified the caption: "Fresh organic matter for the four profiles calculated by the model. The steppe, the 20YBF, the 26YBF and the 58YBF are represented by the green lines, the dark blue lines, the light blue lines and the red lines respectively. All the models share the same input scheme. We assume in the models that a fraction of the SOM decomposed may be as labile as the FOM and is therefore incorporated to the FOM pool"*

Page 14159 line 15: "in face of"; incorrect language. (strong suggestion)

*We modified the section header*

Page 14160, “the oldest is the SOM...” This conflicts with figure 3e. This is the oldest bare fallow site but here the differences between the TA and TAD are largest. But even if this was correct, I don’t think it can really be said that it has been shown that “different transport mechanisms are identified for different pools of C” (lines 17-19). (required)

*Since we used roman number to identify the model the fig. 3 was not very clear but our sentence was true, we hope it is now clearest on the model. We also modified the sentence corresponding to the page 14160 l17-19: “To our knowledge, this is the first time that different transport mechanisms are suggested for different pools of C”.*

Page 14161 line 18: replace “this” with “the” (strong suggestion)

*Done*

Page 14162 line 7: add “a” before “crossing” (required)

*Done*

Page 14162 line 9: remove “the” before “some”. (required) Consider revising the whole sentence.

*Done*

Conclusions section: this section does not really have any conclusions. The points made are more appropriate for the discussion or even results section.

*We moved this section in the discussion*

---

Interactive comment on Biogeosciences Discuss., 9, 14145, 2012.