Reply to comments from Referee #1

Dear Referee #1,

many thanks for your helpful comments on our manuscript. We were able to incorporate the majority of your suggestions during the review. Your questions and remarks really helped to improve the paper. Please, find below our detailed answer to your comments. We hope that you find your comments sufficiently considered and would once again thank you for your time and support in improving this paper.

Yours sincerely

A.Y. Bossa and B. Diekkrüger

General Comments and recommendation

The authors present results from a long-term study of soil respiration at several sites within a catchment. These data cover temporal and spatial variability of the soil CO2 flux, and , thus allow to investigate the effects of the well know temporal driver (soil temperature, soil moisture) and the more complex factors affecting spatial variability. In general, I like the approach of structuring such a dataset using a multivariate statistical approach on a simplified dataset (only mean values of spring and summer), in order to cluster the data and to continue the analysis with theses clusters. But 10 sites have been clustered in 6 clusters! This means cluster size is 1-3 sites. For each cluster (size 1-3!) – if I understand it correctlymodel parameter were fit to the measured time series of respiration rates using the time-dependent factors soil moisture and soil temperature and the independent factors litter depth , C org and so on. If this is so, 4 “time independent “parameter (Eq.4 . p. 699: a, b, c, d,) were fit e.g. for a “cluster” consisting of 1 site? If so, this model is substantially over parametrized, and differences between the clusters can hardly be interpreted (what is the meaning of negative a, b or c factors?) This is surprising, since the issue of calibrating large numbers of parameters and “complexity of models” hampering the understanding of processes (Pumpanen et al. 2003) is mentioned in the first paragraph of p.695. (Why not fitting a parameter set to all sites, and doing then the cluster analysis?) All the following analysis is questioned by this issue.

Answer:
Our objective is to analyze how the heterogeneity of the catchment is affecting the CO_2 pattern so we preferably based the cluster analysis directly on the environmental properties rather than model parameters. We analyzed 10 sites which we found to differ most significantly. The aim of the cluster analysis was to confirm the selection process. 10 sites have been grouped into 6 clusters, showing strong consistencies with the criteria behind the site selection (i.e. significant differences of various factors such as the topography, soil type and proximity to the river). We were expecting a high number of clusters and were not surprised to have 6 clusters.

We strongly think that clusters built up of only one site are not over parameterized (considering the 4 time-independent parameters a, b, c and d used). It could be a problem if we are using a very limited number of observations per site, which is not the case here. Our analysis is based on weekly measurements over several years (from 2006 to 2012). As you can see from the table just displayed below, the Pearson correlation matrix is not critical.

Pearson correlation matrix of fitted parameter set (estimation based on the nonlinear approach developed).

Variables	Ko	ΔE	α	a	b	c	d
Ko	**1**						
ΔE	**0.982**	**1**					
α	-0.241	-0.330	**1**				
a	**0.976**	**0.975**	-0.211	**1**			
b	-0.023	-0.078	-0.203	-0.238	**1**		
c	**0.980**	**0.977**	-0.219	**0.999**	-0.220	**1**	
d	-0.042	-0.102	-0.212	-0.256	**0.998**	-0.238	**1**

The statement "*complexity of models hampering the understanding of processes (Pumpanen et al. 2003)*" was mentioned regarding process-based models such as HYDRUS, as used in our work, and where up to 103 model parameters (including 35 hydrological parameters, 63 heat transport parameters, 3 CO_2 transport parameters and 2 CO_2 production parameters) have to be determined for a single site. In our study, the hydrological parameters were fitted using observed soil moisture, the soil heat parameters using soil temperature and the soil respiration parameters using the measured efflux data.

Concerning the negative values of parameters a, b, c and d, we found your comment very relevant and to avoid contradictions and confusions we decided to strictly respect the assumptions of Fang et al. (1998) by constraining time-independent parameters a, b, c and d to positive values. We therefore recalculated the model and a new parameter table is provided in the table section (as follows) and dependent figures such as Fig. 5 are also updated accordingly.

Tab. 3. Factor parameters obtained for the different clusters (cf. Eqn. 4). k_0 = reaction rate at reference temperature To [T^{-1}], ΔE = activation energy [J/mol], α = parameter describing soil moisture dependency [S^{-1}], a = cluster constant, b = root biomass factor, c = litter layer factor, d = organic matter factor.

	ko (10^4)	ΔE	α (10^{-4})	a	b	c	d
Cluster 1	122174.1	71230.9	-2.8	24435.5	29.8	300889.0	4.7
Cluster 2	3161665.5	90049.7	-3.1	398409.6	103.3	15510557.5	0.001
Cluster 3	111183.2	67752.1	-2.7	4329.2	976.2	0.001	6273.1
Cluster 4	134141.2	67797.4	56.3	8181.9	0.001	0.001	0.0
Cluster 5	81559.0	70860.6	-2.6	6409.6	135.6	268244.2	74.8
Cluster 6	686832.0	72262.6	-1.8	0.009	15836.6	0.4	52895.5

Also the last jump to the Hydrus 1 D model seems to be quite far to me and a lot of assumption have to be made. It is possible, but it is hard to say what comes from the data measured or model assumptions included. Hence, I doubt whether such data should be interpreted at all. Cited literature is not allays used to the point, and some reference which would fit better are lacking.
In a way, I hope that I misunderstood or overlooked something and that the authors can explain me what I missed, because the analysis and the manuscript looks liek a lot of work. But from my current point of view I recommend to reject this manuscript.

Answer:

We think it's important to clarify our approach, which should be seen as an extension/improvement of the works of Fang et al. (1998) in a more complex ecosystem

environment with permanent river discharge and with a considerable diversity in soil properties, slope and moisture gradient. The approach rolled out aims to: (1) link time-dependent and time-independent properties; and (2) take into account the possibility that a single formalism controlling all sites at the same time (with a single parameter set) could not be sufficient for describing the CO_2 pattern. We think it is necessary to compare the results of our approach, where a total of 7 model parameters were used for a single site, to the results of a wildly used process-based model HYDRUS_1D for which we calibrated up to 103 model parameters for a single site and where more assumptions are usually made.

Specific comments

The Fang (1998) Model uses soil porosity as parameter, which was replaced by the bulk density. If bulk density goes up, porosity goes down. This means for Rms (Equation 1 , p. 698) the assumpitions of Fang are not met- what this means to the model should be discussed. Or better avoided.

Answer:
We found your comment very relevant and as already mentioned previously, we now strictly respect the assumptions of Fang et al. (1998) by replacing bulk density by porosity and by constraining time-independent parameters a, b, c and d to positive values. An updated parameter table 3 is now provided as given before.

Equation 2 (p. 699) "Phi"P is the same as "Phi"?

Answer:
We agree that "Phi"P is the same as "Phi" and we now only use "Phi" to avoid confusions.

Terminology: site-specific vs specific-site?

Answer:
We completely agree that using “site-specific” and “specific-site” in the same text may also lead to confusion, so we keep all as “specific-site” across the text.

Several times , the “velocity constant for water sorption and desorption “ is mentioned (e.g. p. 699, L21; p 708, L8;). I'm not happy with the use in this context. Maybe it0s called like this in the original reference, but in this context the alpha is used to describe the effect of soil moisture on soil respiration

Answer:
We used the same denomination for “alpha” as provided in the original reference Richter et al. (1996). We agree that this expression may not be very catchy and therefore we changed it to “parameter describing soil moisture dependency”

Tab.1 Please include the details of all 10 sites!

Answer:
It is now added below Fig. 1 as follows:

Site	Soil type	Slope (%)	Elevation (m)	Bulk density [g cm^{-3}]	Root biomass [g m^{-2}]	Organic matter [g m^{-2}]	Litter thickness [m]
WA1	S-B: Gleyic Cambisol	8.84	600	0.82	131.25	10029.67	0.02
WA6	S-G: Stagnic Eutric Gleysol	5.59	598	0.91	73.20	12272.50	0.02
WA7	S-G: Stagnic Eutric Gleysol	5	598	0.90	73.20	12272.50	0.02
WA10	B-S: Cambisol	5.3	597	0.67	177.73	10029.67	0.04
WA11	B-S: Cambisol	7.29	598	0.70	177.73	10029.67	0.02
WA15	S-B: Gleyic Cambisol	12.87	601	0.70	131.25	10029.67	0.02
WB3	S-B: Gleyic Cambisol	12.75	604	0.82	131.25	12511.83	0.03
WB4	S-B: Gleyic Cambisol	14.25	605	0.76	131.25	12511.83	0.02
M1	S-B: Gleyic Cambisol	7.29	616	0.76	28.58	11019.17	0.04
M8	S-B: Gleyic Cambisol	7.29	617	0.84	52.40	7330.17	0.08

Table 5. molecular diffusion coefficient ogf CO2 in air? It is always the same, or at least only depending on T and P. You mean the diffusion coefficient in the soil? Should be 1/1 000 000 of that in Table 5

Answer:
We found your comment very relevant and realized, after verification, that there was a problem of data management leading to errors. In fact, the molecular diffusion coefficient of CO_2 in air as well as the molecular diffusion coefficient of CO_2 in water are constant for all sites as now provided in the Tab. 5 just below and corrected in the manuscript. The units are rather mm^{-2} d^{-1} instead of mm^{-2} s^{-1} as provided before.

Tab. 5. Estimated parameters of the HYDRUS-1D model: Air Diff.= Molecular diffusion coefficient of carbon dioxide in air [mm^{-2} d^{-1}]; Water Diff.= Molecular diffusion coefficient of carbon dioxide in water [mm^{-2} d^{-1}]; OCDP microorganisms= Optimal CO_2 production by soil microorganisms for the entire soil profile [µmol m^{-2} s^{-1}]; OCDP roots= Optimal CO_2 production by plant roots for the entire soil profile [µmol m^{-2} s^{-1}].

	M1 (C1)	M8 (C2)	WA1 (C3)	WA7 (C4)	WA10 (C5)	WA11 (C6)
Air Diff.	1373760	1373760	1373760	1373760	1373760	1373760
Water Diff.	152.9	152.9	152.9	152.9	152.9	152.9
OCDP microorganisms	10.40	9.48	11.31	6.32	10.65	7.27
OCDP roots	13.42	7.80	9.74	10.49	11.90	7.33

Fig. 4 Why did the direction of F2 change from winter to summer? All the patterns seem to be head- down. It seems arbitrary...

Answer:
We would like to mention that it's not an arbitrary pattern and would like also to clarify that the arrows in Fig. 4b,d are not dependent on the graduation signs of the axes F1 and F2, but are indicating the gradients of CO_2 emission rate. As it can be seen the F2 arrow is indicating an increase emission rate from site M8 to the sites WA11 / WA15, while the arrow of F1 is indicating an increasing emission rate from sites WA6 / WA7 to M1. As it can be also seen, this figure (Fig. 4b,d) is consistent with Fig. 2. It's clear to us that the change in the F2 direction is due to the combined effects of the three time-dependent variables T, θ and CO_2, since the time-independent variables are assumed to not significantly change in the short term of the investigation. We clearly figured out from our results that from the spring to the summer, T increased, while θ decreased and CO_2 increased. This already made a significant difference between spring and summer and we agree that further studies should investigate in which rate each time-dependent variable may affect the pattern direction.