

# ***Interactive comment on “Spatio-temporal variability of soil respiration in a spruce-dominated headwater catchment in western Germany” by A. Y. Bossa and B. Diekkrüger***

## **Anonymous Referee #1**

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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 CO<sub>2</sub> 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

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cluster size is 1-3 sites. For each cluster ( size 1-3!) – if I understand it correctly-model 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. 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.

#### 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 assumptions of Fang are not met- what this means to the model should be discussed. Or better avoided.

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

Terminology: site-specific vs specific-site?

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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 it’s 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

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

Table 5. molecular diffusion coefficient ogf CO<sub>2</sub> 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

Fig. 4 Why did the direction of F<sub>2</sub> change from winter to summer? All the patterns seem to be head- down. It seems arbitrary...

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Interactive comment on Biogeosciences Discuss., 11, 691, 2014.

**BGD**

11, C70–C72, 2014

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