Reply to Dr Ray Leuning

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We thank Dr Ray Leuning for providing additional comments that will help us to improve the manuscript. In this reply we quickly respond to his comments, as the discussion phase is closing. The reviewer comments are in smaller italic font and our responses are in normal font.

Considerable emphasis has been given by others to my comments concerning the definition of a model parameter as being a constant. I have done enough ecophysiological modeling of vegetation to know there is no such thing as a constant parameter in biology, there are few enough in physics. My criticism concerns the use of parameters that are adjusted every two days and then claiming that the model explains 83% of the variance in the annual half-hourly measures NEE (L313).

We do not claim that this explained variance is very high in cases such as compared to process based models, but stating explained variance seems to us a necessary information.

Of much greater interest is to understand or describe the variation in the parameters shown in Fig 4 in terms of more fundamental processes. These 'sub-modules' may then have more stable parameter values but I concede that this leads to increasing model complexity and hence the possibility of over parameterization.

Indeed understand and interpret the parameter dynamics with specific sub-modules are interesting. However our goal was not to develop a generally applicable ecosystem model. Nevertheless, given the limitations we discussed in the text, the model can still be used to interpret fluxes from other sites, the parameter values are, however, site specific.

A major concern that still remains is what seems to me an artificial separation of 'direct response from biotic changes' (L207). How can one separate cause from effect that is independent of the model chosen? This is acknowledged by the authors on P21 when they contrast their results with those of Richardson et al. (2007) who used a different model and thus attributed different degrees of control of NEE etc by climatic and biological factors. Figures 5 and 6 are thus not very useful because they are specific to this paper.

We agree that the way a model can represent data depends on the model structure and that, consequently, different model structures could give different results. Recognizing the larger seasonality of CO_2 fluxes in a deciduous forest as compared to evergreen or mixed forests and recalling the discussion of the limitations in Richardson et al.'s approach, we assert that we developed a more generally applicable approach, better suited to analyze the type of forest we work with. This is because the approach of Richardson et al. will not resolve the functional changes within the year with a similar high accuracy as our approach.

The scientific benefits of this work are not limited to methodical improvement, but are also interesting for the interpretation of the data from our site itself. The specific feature of the Sorø site was the long-term trend in NEE; we deem it of general scientific interest to show that this is strongly influenced by ecosystem internal dynamics. Figures 5 and 6 are thus still specific to the site and provide information on the interannual flux variability at this site, although we acknowledge that there is slightly repetition (different ways of plotting for NEE) in Figure 5 and 6.

> *Note that the oscillations in the parameter k seen in Fig. 4a are likely an artefact of overfitting. There also seems to be a strong negative correlation between rb and E0 during the growing season (Fig 4b).*

Over-fitting usually occurs when a model is too complex; our model does not seem so complex, as to be over-parameterized. However as pointed out by the reviewer, the high oscillations in the parameter *k* could be still an artifact. Therefore, in the revision, we will test to hold the parameter k constant as the median of the current estimates and repeat the entire analysis.

The correlation coefficient for *rb* and *E0* in Figure 4 at DOY 120-270 is -0.21, $p<0.05$. In the pre-review phase, the reviewer also suggested to hold E0 constant. We did not take the advice because we interpreted the seasonal variation in the temperature sensitivity as e.g. changes in the substrate quality or contribution of above and below ground respirations. Therefore we found it still reasonable to allow it vary throughout the year.

> *My earlier criticism of the paper and one reason for recommending that it not be published concerns the question 'what advances our understanding and knowledge?' In my opinion this is best achieved by a combination of modeling and data and the paper by Wu et al follows this path. Unfortunately, I think the model they use is not adequate for the task they propose, and because model performance is*

now totally dependent on the seasonal and interannual variation in parameters that has been determined by fitting the model to the data every two days.

The seasonality of ecosystems is strongly dependant on the structural development of the vegetation. Our model and procedure (with a moving window approach) can clearly catch these dynamics. Therefore, we do not accept the critique that our model wouldn't be adequate for the task we propose, simply because we only "artificially" reproduce this seasonality. It is also especially discouraging for us as the reviewer acknowledges that this critique led to the decision of rejection.

t is highly unlikely that this model and the parameter values shown in Figure 4 will be applicable to any other site and this site-dependence greatly diminishes the value of the model and the findings of this paper.

With regard to the application, we did not aim to develop a universal model, but a tool to explore the data. As with all empirical approaches simulating complex systems with simple models, the degree of generality of the parameter values is small. We never intended to extrapolate the model parameter to any other sites, or even to the future of this site, as our study clearly showed that the variations in the parameters are responsible for the strong trend of the CO_2 fluxes. On the other hand, the model and procedure, together as a tool, can still be applied at other sites to obtain other site-specific parameter time series and to evaluate the impact of functional change.

Again, I acknowledge similar problems with other models. Many of these points are recognized in the Discussion, but in my opinion the paper does not add significantly to what is already known and does not describe new methods or insights and hence I recommend that it not be published.

As also pointed out by other reviewers, we need to make a stronger case why this study is interesting and add significantly to what is already known. This has been mainly addressed in our response to reviewer #1. Nevertheless, as a direct result of this analysis, we now better understand the strong trend of increasing carbon uptake at this site. It is true that the information would be of greater interest if more sites were included in the analysis. However we intend to discuss the strength and limitations of this approach in more detail in the revision.

My final criticism concerns the overuse of correlation analysis and a lack of mechanistic description of basic process governing the carbon balance of their forest. Clearly, GPP is driven by photosynthetically active radiation absorbed by leaves and hence there must be a high correlation between the two on a diurnal basis. The correlation will necessarily decrease with increasing averaging time because GPP depends on many other factors, such as how much leaf area is present (phenology) and the rate of diffusion of CO2 through the stomata which in turn depends on the water availability to the roots and so on.

L475-477 Respiration fluxes at the soil surface depends on the relative rates of carbon input by the roots to heterotrophs and the rate of autotrophic respiration. Carbon in the soil accumulates from photosynthesis from past and present seasons. The degree of correlation between GPP and soil respiration will never be simple, it depends on the size of the soil carbon stock relative to the input rate via the roots as well as on microbial activity which varies with soil water and temperature distributed over the depth of the root zone. Processes such as these processes cannot be captured by the model used in this paper, nor will they be elucidated by simple correlation analysis.

We agree that we tend to over-interpret the correlation analysis as causal relationships. We will be more careful with the writing in the revision. We used correlation analysis to detect patterns and obtain information, e.g. that soil water content was strongly negative correlated with GPP during summer. Although it does not develop new understanding of the processes, the results are still relevant to understanding the site. We will shorten this part and make it more concise in the revision.

Detailed comments

L183 Eq 3. Is this valid given the serial correlation in the data?

This equation is not precise enough as it still needs the support of the text to explain the moving window approach, we will change this.

L269 and elsewhere. Do not confuse correlation with causation. Soil moisture was measured at very shallow depth.

We fully acknowledge this critique and will be careful in the revision. We will further address the issue of the shallow soil water content measurements in future revisions of the manuscript.

L330 – 333 Of course one would expect a better fit by continually adjusting parameter values.

To achieve a better fit is one of the motivations for using the moving window approach.

> *L353 the effect of climate variability on TER than on GPP for example. TER is generally dominated by soil respiration and this depends on the amount of carbon and nutrients available for root and microbial respiration modulated by soil temperature and water content through the whole root zone. This buffering will decrease the correlation of TER with climate variables compared to above-ground processes such as photosynthesis and leaf growth and senescence.*

This comment is appreciated and it is indeed a limitation of our study. The air temperature was dampened in the whole soil profiles, thus the estimated direct effect on TER would be underestimated. We will carefully discuss this in the revision.

> *L403-407. The ranking of ecosystems in terms of 'functional change' given here is not valid because different models were used by different authors. The same model should be used for such a ranking to be valid. In any case, how does this help us understand ecosystem function?*

We have clearly indicated that the method was different in these different studies in our discussion and also addressed this as an uncertainty in the estimated impact of functional change. We agree one needs to use same model for different sites, or use different models for one site, to fully evaluate whether the gradient was the real differences between sites or artificial differences across models. It is not practical to include different sites in this study because our focus is the present site. However, we will try to evaluate the effect of using different model on the results.

We do not agree with the assertion that "in any case, how does this help us understand the ecosystem function". While understanding mechanism is the key in science, describing the phenomenon is also important to reveal patterns and generate hypotheses and further research.

> *L488 A steady average LAI is not sufficient, you also need to consider timing of leaf out and leaf fall.*

We will emphasize the importance of phenology more in the discussion. Our approach has the ability to describe this structural change as the seasonal variation in the parameters; it is an inherent strength of our approach.

Table 2 Only show data for half the matrix. Fig 2 is very hard to read, especially in monochrome. I suggest plotting the mean or median with shading of ± 2 s.d to indicate the spread across years.

We will change this in the revision.