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Biogeosciences
Discussions

# *Interactive comment on* "Soil respiration compartments on an aging managed heathland: can model selection procedures contribute to our understanding of ecosystem processes?" *by* G. R. Kopittke et al.

**Anonymous Referee #2**

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I reviewed the paper by Kopittke et al entitled "Soil respiration compartments on an aging managed heathland: can model selection procedures contribute to our understanding of ecosystem processes?". The authors present a study that combines measurements, experimental trenching and empirical model of soil respiration for over 1 year. The authors used a data analysis workflow to test which variables and which empirical model fitted best the observed data (minus extreme values). Using this approach the authors calculated the annual contribution of total soil respiration plus its autotrophic and heterotrophic components in three stands of different ages of Calluna.



Empirical models are used to represent the variability of observed data and therefore they are not a mathematical description of an ecosystem process; which is the main objective of process-based models. That said, the authors need to make a better case in this manuscript for the use of empirical model selection procedures as a tool for understanding ecosystem processes. How these empirical fits describe the ecosystem processes? A clear example about how these empirical models cannot represent ecosystem processes is that they fail to represent the extreme values reported on 21 March 2012. See the large differences between measurements in Figure 4 and model simulations in Figure 9. The extreme values presented in Figure 4 (which likely represent an important flux at the annual scale, and are driven by key ecological processes as discussed in page 16251 lines 26-28) were purposely excluded from the analysis because these empirical models cannot simulate this variability in the observed data (i.e., they cannot simulate ecosystem processes). Therefore, I encourage the authors to think and discuss on the applicability and limitations of empirical models for understanding ecosystem processes as stated in the title. At the minimum the word "empirical" should be included in the title.

A model is a partial representation of reality and ideally it should reflect the underlying biophysical mechanisms to truly represent and contribute to our understanding of ecosystem processes. Empirical models, such as the ones used in this study, do not translate into mathematical equations the ecosystem processes. At their best these models represent the patterns and magnitudes of the measured variables and it is interpreted that they could provide insights about processes. Furthermore, empirical models cannot be used to extrapolate observations outside the range of measured values and therefore lack of predictive power. Thus, the main question is: If empirical models do not represent ecosystem processes per se (as they are developed to fit observed data); how can empirical model selection procedures could contribute to our understanding of ecosystem processes? This is the main goal of the manuscript but I respectfully believe it needs to be developed with more clarity along the manuscript.

The authors used the RMSE as a statistic for model selection (Figure 8), but generally for model evaluation there are several parameters that are important to consider. These are the RMSE, the correlation coefficient and the standard deviation that could be represented in a table or in a diagram (Taylor, 2001). Furthermore, models that have different number of parameters are generally evaluated via the maximum likelihood approach known as the Akaike's Information Criterion (AIC) (Burnham and Anderson, 2002). I strongly suggest including this statistic when evaluating models that have different numbers of parameters.

Importantly, I do not think that including the AIC and other statistics will change the results as the "best" empirical model includes only soil temperature (arguably the simplest empirical model and the one with the highest parsimony). Thus, if the simplest model is used to describe this system, then it is difficult to justify such a complex model selection to interpret such a simple system. This argument relates back to my main comment about the applicability of empirical models to describe ecosystem processes. Why is needed to go through the workflow provided if temperature alone explains the best most of the variability observed? What does this means in terms of ecosystem processes? This comment is debatable and I encourage the authors to discuss it in a revised version of this manuscript.

The manuscript is quite long in its present form. The main objective is to propose a model selection protocol to find the best empirical model that fit the observed variables (not the ecosystem processes) and to interpolate in time the autotrophic and heterotrophic components of soil respiration. However, the reader can be easily distracted with discussions about extensive justification on the experimental design (arguably pseudoreplication), discussion of data not presented clearly (some will be presented in further studies (e.g., page 16250 line 6; page 16276 line 19), and discussion of global change implications (page 16276 and abstract), which some may be moved to supplementary information. Maybe a way to focus and shorten the paper is to better discuss model selection procedures and its implications/limitations to our understand-



ing of ecosystem processes; which is the title of this paper.

The results of the modeling selection does not support the hypothesis presented in page 16244 lines 10-14). I agree on this hypothesis, but it is not clear how the authors interpret that a multi-level modeling approach provides different results than those expected. Is this site specific? How these results from empirical models are interpreted in terms of ecosystem processes? A more clear discussion about this hypothesis is needed to link it with the main objective and title of this paper. Furthermore, where is the clear answer to the question posed in the title? The discussion is somewhere in the manuscript but it is not clear and easy to understand in the current form of the manuscript.

Comments in detail

Introduction. Which is the difference between "components" of soil respiration and "compartments" of soil respiration? These definitions are used interchangeably across the manuscript and needs to be clarified or unified.

Line 3 page 16240 – it is unclear the statement "modeling tools which generally include model variables. . .." Please define the difference between modeling tools, model variables, and empirical models in the main text.

Page 16241 line 24. Define which are the "underlying drivers". This is ambiguous.

Page 16242 line 19. Define what "modeling tools" mean. This is ambiguous.

Page 1624 line 28. Which is the difference between a "process-based" model and "more empirical" model? This is definition should be clear in this manuscript as the models used are empirical but used to interpret ecosystem processes. Could you give an example of a process-based model and a "more empirical" one for soil respiration?

Page 16243- Describe what "multi-level modeling" approach is, how it is being used and how are you applying it in the study. This is somehow in the text but I suggest to be clearly explained in the introduction. How a multi-level modeling is being used

for longitudinal and clustered in space data of soil respiration? How this relates to the current study? Why is better than other approaches?

Page 16244- How stands be of "similar age", but at the same time have a younger and older stand? I think it is better to be clear about the ages and on the interpretation of young and older. Then in line 25 of the same page the authors say that the stands are of "different ages". This is confusing. . .maybe just use the real ages of the stand to avoid ambiguity in the interpretation of the results.

Page 16245. True replication within natural ecosystems is extremely difficult, thus the interpretation of the results and how we treat statistical analyses has to be carefully done. This is of critical importance when extrapolating results in time and space thus I suggest to be very careful when interpreting the results outside the main objective. For example I suggest removing section 4.4. This is not part of your main objective and has assumptions that are not clearly discussed.

Page 16245 lines 4-10 this is repetitive from introduction.

Page 16245 lines 16 what do you mean by nutrient-poor. Explain nutrient-poor with respect to which threshold. Also give soil texture data for all your sites. This is arguably an important parameter for estimating soil respiration in space but it is not included in any of your empirical models.

Page 16247 line 16. 10 cm of buffer zone from a trench seems to be somehow short. Do you expect any "boundary effects", how realistic were these measurements considering potential effects (if any).

Page 16247 lines 24-27. There is no guarantee that the plots were similar before the trenching as no measurements were done before trenching (i.e., initial conditions of the experiment). According to the authors measurements started 3 days after trenching. . ..please discuss the implication that no pre-treatment spatial heterogeneity was evaluated. How this could affect the interpretation of the results?



Page 16248 lines 1-7. The study seems a little bit rushed. The authors let the trenched plots to "stabilize" only for 4 months and only observations after 21 September were included in the analysis. Is four months "enough" for decomposition of all that organic material to properly interpret Rh? Probably not and it is probably the reason why the authors did not find differences in heterotopic respiration across the sites. This potentially rapid organic matter decomposition must be clearly discussed in light of interpretation of Rh in time and space.

Page 16249. Photosynthesis measurements are not clearly discussed and the authors state that they are evaluated in more detail in another publication. Why is this not discussed and which publication is this?

Section 2.7.2. Please include the validation of the soil moisture model. This is important for the interpretation of the results and for validation of the empirical models that used this input variable.

Section 2.7.4. As discussed earlier: The authors used the RMSE as a statistic for model selection (Figure 8) but generally for model evaluation there are several parameters that are important to consider. These are the RMSE, the correlation coefficient and the standard deviation that could be represented in a table or in a diagram (Taylor, 2001). Furthermore, models that have different number of parameters are generally evaluated via the maximum likelihood approach known as the Akaike's Information Criterion (AIC) (Burnham and Anderson, 2002). I strongly suggest including this statistic when evaluating models that have different numbers of parameters.

Page 16256 line 15. Please define "parameters reasonableness". Which is the basis of the thresholds that were selected?

Results section. The authors used a series of statistical analyses to test for the observational data (section2.7.1). I encourage including the analysis used, the statistic (e.g., F, t) and the p-value when referring to statistical differences. So far it is only reported the p value but it is hard to track which test was used to evaluate the differences. This

will help the readers better understand the text.

Page 16258 lines 23-27 (and associated discussion). I feel that the diurnal variation does not add much to the manuscript and is quite weak as only few manual measurements were performed. To simplify the manuscript I suggest removing these results and the associated discussion.

Page 16261 lines 13-14 and 27-8. Please explain how the authors arrive to the conclusion that the parameters of the models are considered reasonable. Which were the assumptions or thresholds?

Section 3.7. There are multiple ambiguities in this paragraph. What do "was so similar", "were so small" and "the most attractive Rs model choice" mean? Please be clear and avoid using terms that are meaningless such as those described in section 3.7.

Page 16266 lines 10-14 and page 16269 lines 20-23. These lines seem contradictory. First the authors say that there were no differences in patterns of soil moisture and then they sate that there were differences. Please clarify.

Remove section 4.4

Table 1. Include soil texture and soil bulk density.

Figure 5. Consider removing it.

Figure 6. Include validation of soil moisture data.

Figure 7. Please indicate what the error bars mean and the small letters. One can imagine what they mean but it is always advisable to be clear.

Figure 8. Consider including AIC.

References

Burnham, K.P., Anderson, D.R., 2002. Model selection and inference. A practical information-theoretic approach. Springer, Berlin Heidelberg New York. Taylor, K.E.,



2001. Summarizing multiple aspects of model performance in a single diagram. J Geophys Res-Atmos 106, 7183-7192.

Interactive comment on Biogeosciences Discuss., 9, 16239, 2012.