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## ***Interactive comment on “Drought-associated changes in climate and their relevance for ecosystem experiments and models” by H. J. De Boeck and H. Verbeeck***

**Anonymous Referee #3**

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The manuscript “Drought-associated changes in climate and their relevance for ecosystem and models” by De Boeck and Verbeeck describes a modeling exercise aimed at understanding how relevant is it to consider light and temperature other than precipitation in drought research. The authors outline that during drought the precipitation, global radiation and temperature covary while in manipulation experiments, drought is imposed just excluding rainfall through the use of rain roofs.

The manuscript is well-written, concise and clear. The hypothesis and the research question are well stated and defined. In my opinion the presented work can be considered as a benchmark and provides an important step towards the understanding and

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modeling of drought mechanisms.

The structure of the paper is in general good, graphic quality is appropriate.

For the aforementioned reasons I consider the manuscript relevant and mature for publication after minor revisions.

I have only one main concern, which is related to the fact that the authors do not describe the accuracy of ORCHIDEE during droughts. I have to say at the beginning that I do not know really well ORCHIDEE. However, I remember some papers that reported poor performances of this model during droughts and/or when applied for modeling carbon fluxes in Mediterranean ecosystems (e.g. Jung 2007; Keenan 2010). For this reason I would invite the authors to describe critically the performance of ORCHIDEE during droughts (maybe testing the performance of the model at the selected sites during a drought event) and also to report the time series of GPP and NEE obtained in “natural drought” and “precipitation only drought” conditions. The model is crucial in this analysis and I would like to be sure that the conclusions (and also the advice provided to the experimental community) are not driven by model structure.

Specific Comments:

### **Introduction**

The introduction is concise and well written. It gives a competent overview of the literature and on the need of this analysis. It also covers newer and relevant papers. However, I would expand the description of the role of the different environmental variables considered in the study during drought as well as how temperature, precipitation and radiation covary during drought. For instance I would move part of the Discussion section (e.g. paragraph 4.1 lines 10-12) to the Introduction.

### **Material and Methods**

Pag 467 line 12. Please specify which data have been transformed.

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Pag 467 line 23. The authors define the value 17mm of rain in 6 hours as threshold to identify the end of the drought. Why? The value of 17 mm can have different effects whether occur over a grassland or a forest, and also can have different effects depending on the soil depth defined in the model. Could you please clarify the reasons behind the use of this value?

## Results

Fig 3. In my opinion would be useful to show relative values, normalizing the values for the annual NEE/GPP/TER, instead of absolute differences (thus % of variation to the mean instead of absolute values). Would be also interesting provide the same plot for the period with the imposed drought (the 26 days of drought).

## Discussion

Results are in general well discussed. I have only 3 minor critics:

Paragraph 4.1. Part of this paragraph should be move in the Introduction section, this can help the reader to understand why and how temperature, precipitation and radiation covary during drought (e.g. lines 10-16 p 471)

P 472. Line 20: “Our modeling results support that the conditions under natural drought are more favourable. . .”. P472 line 23 “higher temperature and sunshine hours would raise leaf temperatures closer to metabolic optima in spring and autumn”..I agree but here the synthetic drought was imposed in summer. Could you really state that your results support the Larcher’s hypohotesis? I would remove or modify this part.

P 473 Line 17-20. “Forest may therefore. . .” I agree with that but this is very speculative because the authors tested only the effect of one long drought and not the effect of several repeated short droughts.

In the manuscript the authors reported very often, but not always, “natural drought” and “precipitation only drought” in quotation marks. I would prefer always the quotation marks.

Jung, M., Le Maire, G., Zaehle, S., Luysaert, S., Vetter, M., Churkina, G., Ciais, P., Viovy, N., and Reichstein, M. (2007), Assessing the ability of three land ecosystem models to simulate gross carbon uptake of forests from boreal to Mediterranean climate in Europe, *Biogeosciences*, 4, 647–656

Keenan, T., R. Garcia, A.D. Friend, S. Zaehle, C. Gracia and S. Sabate (2009), Improved understanding of drought controls on seasonal variation in Mediterranean forest canopy CO<sub>2</sub> and water fluxes through combined in situ measurements and ecosystem modelling, *Biogeosciences*, 6, 1423-1444

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