

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

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Referee 1:

General comments The main message of the manuscript is that “experimental and modeling efforts should taken into account other environmental factors than merely precipitation”. If they decide to do so, however, the manuscript in its present form does not provide much of a quantitative framework for this. Results are presented in figures or in tables as changes in K or %. For the results to be used directly in either experimental or modeling studies, information on the physical magnitude of the changes as well as the background value and not just the relative changes should be provided. This can

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be solved by extending Table 2 to include the following information on station level, or to insert an additional table listing the mean across stations (including the fields in Table 2):

– Average minimum temperature [K] – Average mean temperature [K] – Average maximum temperature [K] – Average sunshine hours [h] – Increase in sunshine hours during drought [h] – Average RH – Increase in RH during drought [% RH] – Average VPD [hPa] – Increase in VPD during drought [VPD]

This would facilitate direct application of the main findings presented in the manuscript. The drought characteristics (count, av length, max length) could be skipped if necessary since these numbers are a direct consequence of the arbitrary choice of the 0.01 and 1 mm thresholds.

–> We changed to table so the “normal” values now accompany the deviations (anomalies). As suggested, we omitted count, average and maximum length to keep the table at a workable size (Table 2).

Droughts are the key focus of the paper and the authors clearly thought about how droughts should be defined (i.e. as an extreme relative to a local climatology). However, it is not clear from the text (in particular Page 466, Lines 23–26) how this is exactly done. The authors mention a ratio (“the ratio of running averages without significant precipitation equaled 0.01 across the dataset”), but without clearly specifying what this ratio is (i.e. in a complete sentence like “the ratio of ... to ...”). Please be clear to the readers what exactly is done here. Does it mean the ratio of the total number of days of periods or the ratio of the number of events exceeding x? If possible or necessary, illustrate it graphically.

–> We changed the text to clarify how the ratio was calculated (li 92-95)

In addition to this, it is not clear from the text what is done with droughts that start before March 15 or end after October 15. Is data from outside the March 15–October

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15 period taken into account when a drought starts or ends outside this period?

→ This is now addressed (li 102)

Experiments with the ORCHIDEE model form a significant part of the manuscript, but some key information is missing that is needed to interpret the results. For instance how is the direct impact of radiation, temperature and VPD on stomatal opening modeled, and how is this different for grassland and forest? The authors go into quite some depth over what the model does (input/output), but it would be more relevant to the focus of the manuscript to learn about its key equations and assumptions that deal with vegetation/atmosphere exchange during drought (specifically radiation and VPD control).

→ In the revised methods section, the different ORCHIDEE equations and submodels linked to the model's response to atmospheric conditions (radiation, temperature, VPD) and soil drought are now stated (li 158-170). For the different equations we refer to the appropriate literature. In our opinion, listing and explaining all the relevant ORCHIDEE equations in detail would inflate the length of the manuscript unnecessarily.

Apparently the current manuscript is a follow-up study of De Boeck et al. (2010) in which the focus was on heat waves rather than droughts. It would be of interest to present a short but quantitative comparison between drought and heat wave conditions, so that the added value of this manuscript with respect to previous work becomes clear to the reader.

→ Part of the discussion already addressed the connection between heat waves and droughts. We have added a part here to more explicitly couple this to the heat wave paper (li 255-258).

Specific comments Page 464, Line 7/Page 466, Line 10/other: Each station record is not a dataset by itself if they all come from the same source (i.e., KNMI). I would prefer the use of: "observational records of weather stations across Europe".

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→ changed throughout the ms

Page 465, Line 4: Change "recent examples" into "a recent example" if only one example is given.

→ done

Page 466, Line 16: Why not provide the formula here that is used to calculate VPD from daily mean values?

→ done (li 84)

Page 471, Line 13: It is not true that more sensible heat always leads to less clouds, see e.g. the paper by Ek and Holtslag (2004).

→ Wording changed to reflect that there are exceptions (li 251).

Figure 1: In my experience hPa is more commonly used for VPD in vegetation modeling than Pa.

→ done

Referee 2:

I have enjoyed reading this well prepared manuscript. The topic is timely and novel, the results are very relevant both for modelling and for experimental approaches for climate and climate impact research. I see one area for further improvement of the paper: The simulation study is currently restricted to one special case, i.e. the mean drought duration of 26 days. For this special case, very interesting differences between "natural drought" and "precipitation only" occur, which furthermore differ between vegetation types. I feel that the scope should be broadened here. I suggest testing at which drought duration this difference between vegetation types starts, and when the forests also begin to show reduced productivity. This could easily be done with the model as it is parameterized currently and a gradient of drought durations.

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→ We would refrain from putting too much emphasis on the model and the very specific results such as the exact day when forest productivity starts to decrease. Such detailed output data depend much on the model itself and the exact input (meteorological) data. The focus of the manuscript was not intended to be on the model, but the intention was rather to see the model results as an illustration of the importance of environmental conditions not considered so far in many drought studies. We purposely only discuss the important trends shown by the model, as these are much less dependent on model specifics and the time series chosen. Note that the last figure shows more details about tipping points. We discuss its repercussions in broad lines (li 279-281, li 318-325), namely that forests start to get negatively affected towards the end of the drought (as opposed to fast negative responses of grassland), without really quantifying this turning point as we consider a precise assessment to have limited potential for extrapolation. These trends are also confirmed by theory and a recent study by Teuling et al. (li 281-289)

Some minor remarks: 465 lines 27 ff: Clear and testable hypotheses are missing

→ The testable hypothesis is 'We then use these data as input in a process model (ORCHIDEE) to test with a simulation exercise whether significant differences in ecosystem carbon exchange exist between droughts when taking into account or not conditions other than lack of precipitation' (li 64-67). As in this paper we first quantify what the environmental conditions during droughts are naturally, we cannot make precise a priori hypotheses on their potential impacts. Note that we did add a part in the introduction on the possible connection between droughts and changed meteorological conditions (li 57-62).

470 lines 24 ff: I would welcome more discussion on the definition of drought and its implications for this topic. The authors consider consecutive days without rainfall as a drought. But total amount during the vegetation period could also be a good definition, as the often cited summer of 2003 was not extreme concerning the first definition, but concerning the second.

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→ We altered the discussion to address this comment (li 239-245).

470 line 15: this is my major point! When will they start to be limited? You can test this.

→ We believe that assessing exactly how long it takes before drought stress starts taking place in forests as well may be feasible, but would not be very relevant as this depends on a range of circumstances (model used, environmental conditions, soil type, site history, etc) which cannot be extrapolated very straightforwardly. Figure 4 shows that the positive effect of natural droughts in forests is disappearing towards the end of the drought. See also reply to earlier comment.

Figure 3: Differences and similarities between sites should be discussed. E.g.: GPP in the Braaschaat grassland differs considerably from the other two grassland runs, why?

→ The sites in themselves only differ in the meteorological data – all other input into the model was equal (see li 113-137), so any differences in output are merely a reflection of differing meteorological conditions prior, during and after the drought.

Referee 3:

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.

→ ORCHIDEE is a widely used and tested state-of-the-art global vegetation model.

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The long list of published studies where ORCHIDEE is tested should be convincing enough for the performance of ORCHIDEE during droughts (li 171-176). We agree with the Referee 3 that based on the two mentioned studies where ORCHIDEE was tested for Mediterranean ecosystems you could conclude that ORCHIDEE performs poorly under drought in these particular ecosystems. But these studies should be interpreted with care. The conclusions from the paper of Jung 2007 state that all three tested global models (ORCHIDEE, LPJ, BIOME BGC) could be improved for the very specific behavior of Mediterranean ecosystems. The Keenan 2009 paper compares the ORCHIDEE model with the GOTILWA model which is specifically developed for water limited ecosystems in the Mediterranean. It is quite obvious that the latter performed better in this specific case. Based on the fact that there are plenty of studies published where ORCHIDEE is tested under different drought conditions, we chose not to add another model performance test of ORCHIDEE during drought in our manuscript. We rather refer to a selection of the studies where ORCHIDEE has been tested under different conditions (including drought) all over the globe, including the Hesse and Brasschaat sites used in this study (li 171-176).

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.

→ We feel that altering this part of the discussion would break the flow and logic therein, especially as due to the request of referee 1, we have added a part into this section (li 255-258). However, we have altered the introduction to already introduce the knowledge of the connection between droughts and heat waves, suggesting that this could imply that drought conditions will differ from normal (li 57-62).

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

→ done (li 108-109)

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?

→ done (li 121-122)

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).

→ If we understand the reviewer correctly, this would imply a figure with twice as many bars, as the changes compared to the mean would have to be depicted for both natural and precipitation only droughts. This would be feasible, but we would prefer not to do so as we feel that this would complicate and overcrowd the figure. As for adding another plot just for the 26 days, we originally had included these plots, but omitted them as they didn't reflect the aftereffects well. However, figure 4 depicts the course of deltaNEE during and after the drought, which should provide some insight into the details of the drought-response dynamics.

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)

→ see earlier comment

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P 472. Line 20: "Our modeling results support that the conditions under natural drought are more favourable. . .".

→ The condition "as long as plants have adequate water supply" is essential here, as we have shown that once this condition is no longer fulfilled (earlier in grasslands, later in forests), then the natural drought conditions become increasingly unfavourable (li 305-318, li 322-325). This is also relevant for the next comment: conditions associated with natural drought are likely beneficial to growth/net carbon uptake in any season (not only in spring/autumn) as long as the water supply is adequate.

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 hypothesis? I would remove or modify this part.

→ This part has been rewritten for clarity (li 292-304), as the conclusion was not merely applicable to spring or autumn, but also to summer on the condition that enough water was present. We specifically refer to leaf temperatures as the relevant parameter for heat stress, with a reference to a previous study in which this was proven experimentally.

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.

→ This is true, but note that we specifically refer to 2003 as an example of gradual soil water depletion, which did cause substantial negative effects on forest growth. A reference has been added (li 323-324).

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.

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→ We would prefer only to use the quotation marks when introducing the names, but if the editor decides that they need to be used throughout the ms, we will do so.

Interactive comment on Biogeosciences Discuss., 8, 463, 2011.

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