Interactive comment on "Assessing various drought indicators in representing drought in boreal forests in Finland" by Y. Gao et al.

Response to Referee #2: G. Ayzel

Review of the Gao et al. article "Assessing various drought indicators in representing drought in boreal forests in Finland".

We deeply appreciate referee G. Ayzel for his encouraging and helpful comments on our work. Our point-by-point response to all the comments are listed below. We also did the modifications accordingly in the revised manuscript. We hope our reply will satisfy the expectation from the referee.

General comments:

Nowadays we have large amount of drought-related articles in hydrology and these number will increase because of great importance of this topic in respect with changing conditions of climate and human-water interactions at all. I spent about two days for reading this article and now can say that authors did a great attempt to deep in research and analyze their results in different directions. I hope that this article take a noticeable place among another distinguish regional drought-related papers.

In this article authors try to answer some interesting question: "Is it possible to estimate drought event in a robust way with quite different metrics using LSM model on the regional scale and under shortage and weakness of available data?". And the answer (of course): "Yes". And this is a great attempt not only for estimation climate-watershed responses, but also for understanding how nature works and what kind of disasters it stores.

Methodical structure of the article is clear and sharp. Using the LSS-based model JSBACH provide wider range of possibilities for hydrological cycle processes simulation, criteria estimation and followed analysis. Combining regional scale unbiased meteorological reanalysis with extra measurements from field sites also provide the way we can think about various time and space scales interactions within hydrological system.

Of course in every paper we can find a lot of reasons to prevent publication (in the section of specific comments we will tell about that) but I highly rank this paper and for my opinion it aggregate all that modern geographical paper needed: data from various sources, physically-based model implementation, clear visualization, deep and critical description and analysis of results.

Specific comments and suggestions:

Section 2: not so clear why authors use only ECWMF-ERA reanalysis (of course after bias correction) without any attempt to use real meteorological observations with any interpolation technique;

Authors response (AR): The ECWMF-ERA interim reanalysis data are referred to as perfect boundary conditions for regional climate models (Jacob, 2001) and thus used to force REMO. This is because they are based on the observed state of the atmosphere and represent the best estimate of large-scale conditions covering several decades (Kotlarski, 2007). Moreover, the climate variables required by the boundary forcing are more than what the gridded observation data, for instance, E-OBS, CRU TS v. 3.23 and FMI gridded observational data, could provide.

REMO run serves as dynamic downscaling of ERA-INTERIM data to produce the forcing data for the land surface model. The variables required by JSBACH are as follows: temperature, precipitation, net shortwave radiation, potential net shortwave radiation at surface, net long wave radiation at surface (clear sky) and wind speed. Air temperature and precipitation are corrected for biases using FMI gridded data as reference.

Furthermore, the three JSBACH site simulations were forced with observation data because they are micrometeorological site. However, this kind of observational sites are relatively new and sparse compared to meteorological sites.

Additional information about updated soil hydrology scheme required (8098, 21);

AR: The updated soil hydrology scheme here referrers to the five layer soil hydrology scheme, which has been described briefly in this paragraph. The five layer soil hydrology scheme has been newly introduced in JSBACH. For a more detailed description of the updated five layer soil hydrology scheme in JSBACH and how it affects soil moisture memory, please see Hagemann and Stacke (2015). For the aim to improve the clarity of the text, we have changed "the updated soil hydrology scheme" to "the five layer soil hydrology scheme" throughout the text.

Any LSM model have a large amount of parameters. How to set up these parameters? Apriori, global databases or calibration were implemented or one another standard technique?

AR: That's quite correct, the parameter set of JSBACH is large. Photosynthesis and soil hydrology related processes are controlled by parameters attributed to PFTs and soil types respectively. For our regional runs, the global values of the attributed parameters are preserved and the reader is referred to original model publications and the references therein for parameter values (Raddatz et al., 2007; Reick et al., 2013; Hagemann and Stacke,2015).

I find out that soil was parameterized over 10 m depth, but according to the Fig. 1 in Finland no soils with the same thickness;

AR: As introduced in 8098 18-21: "The five layer structure is defined with increasing thickness ... and reaches almost 10 m depth below the surface. However, the soil depth to the bed rock, determines the active soil layers." This means the soil depth and the number of active soil layers in JSBACH simulations are limited by the soil depth until bed rock, if it is less than 10 m. For the area that soil depth is more than 10 meters, the simulated soil depth and soil layers follow the defined 10 m depth and 5 active soil layers.

Not so clear description of JSBACH run for equilibrium of balances (8099, 24-26);

AR: In the soil-vegetation system, there are heat, moisture and carbon storages that dynamically respond to the climatic drivers. As soil carbon storages have long time-constants, a thorough spin-up procedure was conducted. The precise procedure to obtain equilibrium for the soil water, soil heat and soil carbon storages was as follows: two consequent forward runs with current day climate data and CO2 concentration of 1900; a thousand year run of soil carbon model with the photosynthetic net primary production input from the latter forward run; a 79 year long spin-up run with randomized yearly climate forcing and observed CO2 concentration from 1900 onwards up to year 1979. As only the last step is relevant for the soil variables of relatively short response times – soil moisture and soil temperature – we only give a description of that in the text. Similar spin-up was performed for site simulations by recycling the meteorological forcing for 30 years prior to the actural runs. These technique details are not explictly *mensioned in the text. However, due to the importance of spin-up of soil variables in model simulations, this message is simplified in the text as :"Prior to the actual regional and site level JSBACH simulations, long-term spin-ups were conducted to obtain equilibrium for the soil water and soil heat ." .*

No description for PTF abbreviation (8101, 2);

AR: Thanks for pointing out this mistake. We have added some description of PFT in section 3.1 JSBACH land surface modelling in the revised manuscript, as follows "Diversity of vegetation is represented by plant functional types (PFTs). A set of properties are attributed to PFTs with respect to the various processes JSBACH is accounting for.".

Not so clear description about calculation periods of used indicators (moving window, aggregation, averaging etc.) - is the constancy of calculation time scale kept?

AR: To increase the clarity about this, we have emphasized that the SPI, SPEI and SMA are standardized indicators that show the degree of anomalies to long-term means over the aggregation period, while SMI describes the instantaneous soil moisture status normalized with total soil moisture available to plants. In this study, daily SMI was used. The SPI, SPEI and SMA were calculated with 4 week (28 days) aggregation time frame, but they were updated every day with running inputs over the 30 year period. Both 4 week aggregation time frame and 30 year study period are considered to be of sufficient duration climatologically under WMO guidelines (World Meteorological Organization, 2012).

The reasons for choosing 4 weeks as the aggregation period is shown in 8102 3-9 as follows. The 1 month SPI/SPEI reflects short-term climate conditions, thus it can be related to short-term soil moisture stress, especially during the growing season (World Meteorological Organization, 2012). A time frame of less than 1 month is not recommended as the strong variability in weekly precipitation may lead to erratic behavior in the SPI (Wu et al., 2007). However, the "moving window" of a minimum of 4 weeks with daily updating is acceptable (WMO, 2012). Also, by using the monthly running SPI/SPEI, we can follow the development of meteorological drought more closely and investigate their relationship with daily variations of soil moisture indicators.

It will be hard to understand common point of SPI and SPEI indexes without any prior knowledge in this field of study (more clear description needed);

AR: We think the referee is asking about 8102 16, which is also asked by referee 1 as " for what aspect SPEI is similar to SPI?". We have made our descriptions more specific. Please find our revisions in section 3.2.4 in the revised manuscript.

It is really necessary for approval of JSBACH robustness to provide comparison of simulation scenarios using real and reanalysis meteorology on selected sites. It also allows to estimate significance of meteorological inputs errors in overall error of modeling;

AR: We agree with the reviewer that estimating errors in simulated soil moisture from meteorological inputs is interesting. This could be done in the future work. In this study, we are aiming to assess different drought indicators for their ability to represent drought in boreal forests in Finland. We used simulated regional soil moisture as the input for soil moisture indicators. Bias corrections against gridded FMI observational data were conducted to guarantee realistic ranges of variability of the regional driving data and thus the probability density functions of bias corrected air temperature and precipitation are correct. However, there is no means to generate a perfect consistent dataset containing all the variables for regional runs that would not contain some of the regional uncertainty intrinsic in the regional climate model even thought the general weather situation follows the ERA-Interim boundaries. The real site meteorology, in its turn, better accounts for local variations in precipitation. Thus we show soil moistures derived with both regional and site drivers with the observed soil moisture data series.

It is not clear why Figure 2 subplots are not have the same timescale (and why this timescale does not match with modeling period)? On the Figure 2c 2006 year should be place because of its great significance for that paper;

AR: The characteristics of the three sites are described in Table 1. The periods of measured soil moisture at the three sites are different. Figure 2 shows the comparisons between the simulated and measured soil moisture in the common periods.

On the Figure 2a,b no one remarkable bias in SMI in 2006 placed in compare to the other years;

AR: The observed soil moisture in 2006 in Sodankylä is with missing data, but not *because of no bias. The agreement of SMIs in the drought period in 2006 in Hyytiälä shows the capability that the model captures this severe dry event.*

The most controversial place in this article is that we have no one really significant metric proves JSBACH efficiency – only figures (also quite controversial). And this place in the article really need more clear description (maybe authors can add some supplementary materials like a graphs or datasets);

AR: In this study, we are aiming to assess different drought indicators for their ability to represent summer drought in boreal forests in Finland. We used simulated soil moisture as the input for soil moisture indicators. In order to be able to set the thresholds values for drought causing visible effects on forests, it is essential that the simulated soil moisture can capture the basic soil moisture dynamics consistently over a large region. To derive some metrics to evaluating JSBACH efficiency, we would need to have more soil moisture observational data over multiple sites with a longer time period. Typically, those metrics are defined with 30 year period data. This is the main constrain for our model evaluation at present. Moreover, deeper understanding of the deficiencies of stimulated soil moisture by the model requires a sophisticated work regarding to the model physics. That is beyond the scope of this study but these results can be used also to guide such work.

It is not a good idea to use 0.5 threshold of correlation coefficient for making strong conclusions about something efficiency. I recommend adding p-value on the Figure 3 (or add some phrases in text);

AR: We agree this comment and added the phrase in the caption of Figure 3, as " Those correlation coefficients are statistically significant (P <0.01)". We also want to clarify that we did not use 0.5 as a threshold to evaluating the correlation coefficients. We were describing the figure. Confidence in a relationship is formally determined not just by the correlation coefficient but also by the number of pairs in the data. Those time correlations were done with 2760 pairs (30 years multiply 92 days) of data at every grid box in this study. Those derived correlation coefficients are highly significant.

It will be great to add to supplementary material maps of a spatial distribution of R correlation coefficient (because of lack information provided by the median estimations);

AR: We agree. We have provided those spatial distributions in the supplement.

Authors often use term "soil memory" but in the text we have no one word about assessment of this period duration;

AR: It is possible to quantify the soil memory from the simulation results, however, to assess the length of soil memory is beyond the targets of this study and is related to the further model calibration and development referred to in an answer above. Therefore, we only used "the buffering effect of soil moisture and the associated soil moisture memory" to explain the delayed and extended effect of weather variables on soil moisture.

Authors provide information about the proportion of the damaged forests sites, but really significant information for that work is not about reached number, but about the spatial distribution of damaged sites. If authors have this information about spatial distribution it is necessary to provide it for readers and to do some additional analysis that could correct overall conclusions.

AR: We agree with the referee's suggestion. Unfortunately, we donot have the forest health observation data in our hand. We obtained this data from Muukkonen et al. (2015), and we have added some brief descriptions of this data in the revised paper. The shortages of this data and its influences on our result have been discussed also. For readers with more interesting about the spatial distribution of the forest damages, please find it in Muukkonen et al. (2015).

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