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Discussions

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

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We deeply appreciate the comments from Dr. T.R. McVicar. They are very beneficial to this work and our future work. Please find our point-by-point response below. Corresponding modifications have been made for the manuscript. We hope our reply will satisfy the expectation from the reviewer.

The manuscript by Gao et al investigates how various meteorological indicators of drought capture patterns of drought in boreal forests in Finland. Results are interesting and this is likely to be a valuable contribution to the field, however, I do a number of suggestions.

1) P8093, L5: there is a very interesting dichotomy here, in that in a warming world

over the last 60 years Sheffield et al (2012) find no increase in drought, yet we seem to be more sure about the simulated future then we are about the observed past: why is that? I suggest that the Sheffield et al (2012) paper should be cited here and the degree of certainty we have about the future of drought be toned down.

AR: We agree with this comment that we should have a broad view and consider the trend of drought in the future from both sides. Since referee #1 asked us to improve the homogeneity of the introduction, we have rephrased the introduction and this sentence has been deleted in the revised manuscript.

2) Section 2.2, I encourage to explore the use of reflective remotely sensed indices to track the drought conditions in Finland. Caccamo et al (2011) compare agreement of 4 vegetation greenness indices and 4 vegetation water indices with standard climatological drought indices. As snow is going to confound these signals in Finland use of the monthly vegetation condition index (which compares the per-pixel response of one month (e.g., Aug 2015) to the range in conditions observed for all Augusts in the population, McVicar and Jupp, 1998, pp 419) when vegetation growth is not limited by temperature is encouraged. The same form can be used for microwave soil moisture data.

AR: We think it is very interesting to study drought by using remote sensing products. However, there are few limitations to use them over Finland. Firstly, because of relatively low fractional coverage of the forest canopies in Finland (Pulliainen et al., 2014), the optical properties of the forests are highly impacted by the under-storey vegetation. Secondly, Finnish territory has a high fractional coverage of lakes and rivers that images of the MODIS resolution of approximately 500 m are almost without exception impacted by water bodies that pollute the signal. The images of higher resolution (Landsat 30 m, Aster 15 m, Spot 20 m) are in turn of so low temporal frequency that taking the relatively high probability of cloudiness. Thus, it is difficult to gain a data coverage sufficient for a representative time series of fractional coverage of forests impacted by drought by the end of season. All in all, the indices applied in Caccamo et al. (2011) are not as such applicable without intensive research and post processing, if at all, for assessing the impacts of droughts on Finnish forests. Such laborious further research is out of the scope of this study. However, we agree that it is worthwhile to continue exploring and developing satellite data products in order to find one suitable for this purpose. The use of microwave soil moisture data for this aim has to be explored as well. The SMOS products are of low resolution and targeting them for heterogeneous boreal landscapes is still under development. Also, the microwave remote sensing can only provide surface soil moisture in the upper centimeters of the soil but not the root-zone soil moisture.

3) P8102, L20: Allen et al. (1998) defines a crop reference evapotranspiration (Eto) this is NOT a formulation of potential evapotranspiration (ETp). They are different concepts, and cannot be equated. ETo is used for water scheduling of irrigation areas and uses key prescribed (or fixed or reference) land surface parameters for crops. ETo, like ETp, does provide an estimate of atmospheric evaporative demand (AED); in comparison pan evaporation (Epan) is a measurement of AED. AED is umbrella term under which ETo, ETp and Epan all sit, however this does not mean they are equivalent terms. The most obvious way to check that ETo is not a ETp formulation is to consider the surface resistance (rs). In Allen et al.'s (1998) ETo the rs has a prescribed value of 70 s/m, this is much larger that what is implied in the meaning of a ETp, where rs = 0 s/m. Hence, there is some confusion regarding the concepts of crop reference evaporation and potential evaporation; this need improvement. As a scientific discipline we must be very clear about definitions, and must ensure that clarity comes to this currently muddied subject. We all have a role to play to achieve this goal, and it only comes through careful thinking about the fundamental (or underpinning) conceptual definitions. Nowhere in (Allen et al., 1998) does it sug- gest that ETo replaces estimates of ETp. After downloading the FAO56 report from http://www.fao.org/docrep/X0490E/x0490e00.htm please searched for the term 'potential evaporation' and it is only found twice in the body text and in these two instances the authors are not equating crop reference evaporation with potential evaporation. Additionally in Chapter 1 of (Allen et al., 1998) they

state (on page 30 of the PDF file) "The use of other denominations such as potential ET is strongly discouraged due to ambiguities in their definitions." This can be found by searching for the word 'potential' in the FAO56 report.

If you have the date to calculate ETo then you have the data to calculate Penman's (1948) formulation, as provided by Shuttleworth (1993), of ETp which Donohue et al (2010) showed to be the most appropriate form of ETp when considering a changing climate. The Penman formulation of ETp is also a physically-based form of ETp, meaning that all the key variables that govern the evaporative process are explicit in the formula (McVicar et al 2012), which is important when considering the widely reported reductions in: (1) AED estimates (via ETp and ETo); and (2) AED observations (via Epan). It is important that the hydrological community better understand fundamental concepts of ET; you need to ensure that a form of ETp is used here (not Eto).

AR: Thank you very much for pointing out this critical mistake and helping us to get a deeper understanding of the differences between ET0 and ETp!

In Vicente-Serrano et al. (2010), the SPEI was firstly proposed by using precipitation and PET. In the paper, it is stated that the purpose of including PET in the drought index calculation is to obtain a relative temporal estimation, thus, the method used to calculate the PET is not critical. However, in Vicente-Serrano et al. (2010), the concepts of PET and ET0 were muddied. As can be seen in the Methodology part (page 1700), Penman–Monteith method was proposed for calculating PET. Fortunately, this mistake was corrected and clarified in Vicente-Serrano et al. (2014). ET0 is used for calculating SPEI. In the section 1.2 of the supplementary material of Vicente-Serrano et al. (2014), it is clearly stated that: "Estimating the evaporative demand of the atmosphere is really only possible locally, because in addition to atmospheric (meteorological) conditions it is also influenced by surface conditions (e.g. type of surface, vegetation type, soil conditions). For this reason the parameter Reference Evapotranspiration (ET0) has been established. This is defined as the evapotranspiration rate from a reference surface with no water constraints. Allen et al. (1998) strongly discouraged the use of other concepts including potential ET, because of ambiguities in their definitions. Moreover, ET0 enables assessment of the evaporative demand of the atmosphere independently of vegetation type and growth conditions, and it is spatially comparable among different climate regions. This is because ET0 measurements refer to the ET from the same reference surface, and the only factors affecting ET0 are climatic parameters. Consequently, it can be considered to equate to the evaporative demand by the atmosphere, and according to Allen et al. (1998) "ET0 expresses the evaporating power of the atmosphere at a specific location and time of the year and does not consider the crop characteristics and soil factors." Although transpiration accounts for the majority of water loss to the atmosphere (Jasechko et al., 2013), evaporation and transpiration occur simultaneously and there is no easy way of distinguishing these two processes; they are considered together when atmospheric water demand is estimated.".

Therefore, we consider our method were correct by using Penman–Monteith method to calculate ET0. However, we mixed the concepts of ET0 and ETp as the reviewer pointed out. Therefore, we have corrected "PET" to be "ET0" in our manuscript. Moreover, we introduced the software we used for calculating SPEI, which is the SPEI function in R package SPEI version 1.6 (Beguería and Vicente-Serrano, 2013), in the manuscript.

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