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Climate of the Past Discussions

CPD

Interactive comment

Interactive comment on "Dynamics of the Mediterranean droughts from 850 to 2099 AD in the Community Earth System Model" *by* Woon Mi Kim and Christoph C. Raible

Anonymous Referee #2

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Review of manuscript

Dynamics of the Mediterranean droughts from 850 to 2099 AD in the Community Earth System Model

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submitted for publication to Climate of the Past

General comments:

The authors present an interesting analysis of Mediterranean drought in the NCAR CESM- the work has relevance for both paleo drought dynamics as well as cur-

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rent/future drought in the region. Specifically, they use several drought metrics (which is a much appreciated comparison) to compare background drought frequency in a CESM Last Millennium simulation to variability in the Old World Drought Atlas then go on to use the CESM last millennium and historical/RCP8.5 extensions to examine ocean-atmosphere conditions associated with drought in past and present/future climate conditions. This is interesting work with valuable implications. However, in my opinion the authors have based their analysis and conclusions on the ability of the CESM to simulate pre-instrumental drought occurrence/frequency (and do not sufficiently prove that the CESM can do this) and draw several conclusions that appear to be based on visual comparisons of data in figures that I found hard to believe (and in some cases appeared simply incorrect) without further quantitative support.

Below I have listed my main concerns:

(1) CESM simulation data are not easily accessible without contacting the researcher who ran the simulations. (As noted below, I wanted to try replicating the authors' analysis by comparing the CESM data to the OWDA data, but the CESM data are not publicly available)

(2) Choice of geographic region for the Southern Mediterranean regional mean time series. I created a regional mean time series using the same region the authors used, but for the GPCCv2018 instrumental precipitation data and plotted the correlation coefficient (r-squared) for each grid point in the region- most of the grid points in the box only share ~20-30% of variance with the regional mean time series.

I wonder if perhaps some parts of the Mediterranean region experience drought at different times/magnitudes in the instrumental data (also in the CESM data)? Why did the authors choose this region?

Please provide more evidence that drought/precipitation in the region varies coherently (e.g., suggest showing more information in Figure 1 other than a map and a box).

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(3) The authors gloss over a critical comparison of the paleo to the model data (lines 179-182) - they conclude the background drought statistics (occurrence/frequency) in the CESM are similar to the OWDA. Yet, an examination of figure 1c suggests to me that the drought occurrence in the model and paleo data are quite dissimilar- the bulk of droughts in the CESM are centered around 6-10 years in length, and in the OWDA the distribution is centered around ∼1-4 year drought lengths. This discrepancy is quite striking to me, and I was surprised when the authors claim these distributions are comparable.

If the authors want to make this claim, I suggest using some sort of metric (e.g., something like a Mann-Whitney or Wilcoxon rank-sum test or some sort of distribution comparison metric) to show these two drought occurrence distributions are statistically similar. Even a report of the median, mean, and range would be more helpful than the visual comparison. I also suggest the authors use other metrics such as showing average drought occurrence per century (e.g., see Figure 3 in Parsons et al., 2018, J. Clim.).

Other suggestions include comparing the power spectra (PSD) of the OWDA and CESM PDSI. For example, I made Southern Mediterranean regional mean time series of PDSI from the OWDA and from the CESM1 LME run2 (this is an admittedly lower resolution version of CESM1; Otto-Bliesner et al., 2015; but the background drought statistics in the CESM LME and higher resolution versions of CESM are quite similar, at least in SW North America -e.g., Parsons and Coats, 2019, JGRA) over the 850-1849 CE time period. I found the power spectra show quite dissimilar behavior for the CESM and OWDA PDSI variables, with varying discrepancies as varying frequencies depending on how I standardize them.

(4) Comparison of CESM with instrumental/reanalysis data: the authors missed an opportunity to validate the performance of the CESM in the historical/instrumental era against instrumental/reanalysis data. The authors show (e.g., Figures 3,4,6,7) background geopotential height, SST, etc. anomaly patterns associated with drought, but

they have not used instrumental-based data to show the model can accurately simulate the observed climate, and I remain unconvinced the background drought statistics are similar to the OWDA (see Main Concern (3) above).

Authors could compare patterns associated with drought (using a metric such as 2D pattern correlation) in the model to observed/reanalysis geopotential height (ERA5 or 20th Century Reanalysis) and SST (NOAA ERSSTv5, HadSST, etc.), as well as drought occurrence in the model to instrumental data (GPCCv2018 precipitation, Dai PDSI, CRU precipitation).

Example of how other authors have made these comparisons among model and instrumental/reanalysis data: Figure 2 in Parsons et al., 2018, J Clim., Figure 2 in Coats et al. 2013, GRL, Figure 2 in Stevenson et al., 2015, J Clim.

Importantly, as the submitted paper is written without this comparison, I am left unsure/unconvinced the analysis presented in the paper is not just based on a model that can't simulate the relevant parts of the climate system for the study.

(5) The authors do not address several of the known shortcomings in the CESM model (e.g., frequency/strength of ENSO events; Parsons et al., 2017, J. Clim, Figure 6; Bellenger et al., Clim. Dyn, 2015 for a comparison of ENSO characteristics among models and instrumental data) and what the implications of these shortcomings could be for their study, especially because the authors make claims about likelihood of ENSO events before/during/after droughts.

I suggest the authors consider the findings of Ault et al. (2014, J Clim), who show that the background power spectra/statistical characteristics of drought/precipitation (e.g., white noise, power law, etc.) are critical for drought magnitude and duration, and many CMIP5-class models do not show the same background variability as instrumental/paleo data in many regions.

(6) Especially when future warming is considered, it is important to focus on metrics

of drought that don't just focus on 'atmospheric centric' supply and demand, especially if ecosystem/water resource drought impacts are important. See Swann et al., 2016, PNAS, and Swann (2018) who note that drought severity/impacts in a warming climate can be grossly overestimated by use of variables/metrics such as PDSI. I appreciate that the authors included 10cm soil moisture, but given that surface soil water content can basically just follow precipitation variability in many regions, and thus not really reflect full depth soil moisture trends (e.g., Berg et al., 2016), I think it would be helpful for the authors to show that they are analyzing variables actually relevant for plants/ecosystems/water resources in a warming climate, and not just supply/demand from the atmosphere. At least a discussion of some of these points could really strengthen the paper.

âĂČ Specific comments:

Lines 13-14: the authors just list one or two types of drought (meteorological), but what about hydrological, agricultural/ecosystem, socioeconomic types of drought?

Line 22: 'climate hot-spot'- please cite a paper that shows this

Line 23: 'increase in drought episodes' – again, please cite a paper supporting this

Lines 45-46: 'attributed to the increase in the atmospheric greenhouse gases (GHG) concentration, which causes . . . decrease in precipitation over the region' - citation?

Line 52: 'unprecedent intense drought projections' – citation?

Lines 63-64: The separation of ocean-atmosphere conditions during various drought stages has been done before- nice to acknowledge previous work (e.g., Parsons and Coats, 2019; Namias, 1960).

Lines 76-77: 'warm-dry temperature-hydroclimate co-variability at multidecadal timescales' confusing wording

Line 92: 'high horizontal resolution' is subjective (and now closer to 'average resolution' in many CMIP6 models)

Line 102: Why not use the CESM LME (Otto-Bliesner et al., 2015)? There are more iterations, with several RCP8.5 extensions (and a much longer 1000 yr piControl run that is easier to compare w the last millennium runs given the similar length of simulations), allowing for a more complete analysis of internal variability. Is the background climate state that much better in the ∼1 degree vs the ∼2 degree version of the model?

I ask because the authors explicitly state on lines 119-120 that they are interested in studying internal vs externally forced variability, and multi-model ensembles provide an ideal experimental framework for doing this.

Line 107: the years 2001-2020 AD/CE are not the future

Lines 103-112: Suggest just citing Lehner et al. for the model description

Line 127: As in Main Concern (2), please show the region varies coherently in instrumental/paleo and the version of CESM used here

Lines 131-132: removing a linear trend over the 1850-2099 time period looks quite problematic to me (e.g., Figure 9)- removing a linear trend over this time period will add in non-climatic variability artifacts from the trend removal. It looks to my eye like there is a trend ∼1900-2000, then a separate trend ∼2000-2099.

Line 149-150: linear temperature trend is removed, but then authors study the impacts of warming using this drought metric, which includes temperature. . .so have the authors removed temperature changes, then try to study the impacts of warming on drought? This reasoning doesn't make sense to me. Perhaps a more clear explanation of trend removal would help (?).

Lines 140-155: As in Main Concern (6): I think all of these drought metrics/variables, with the exception of upper 10cm soil moisture, do NOT reflect actual impact on plants/ecosystems in a warming climate. Also, upper soil water content can diverge from deeper soil water – authors should show that this is a useful metric here that is distinct from precipitation alone if they want to argue that their study has relevance for ecosystem impacts.

Lines 161-164: This drought counting method appears similar to Herweijer et al. 2007; Coats et al. 2013b- did the authors come up with this metric, or can they use a similar metric to previously published work (if so, please cite) to maintain consistency across the literature?

Lines 168-170: see above note about similar methods in Parsons and Coats as well as Namias.

Lines 179-183: As in Main Concern (3): Please be more quantitative. To my eye, these distributions do not appear similar- the OWDA shows droughts that are mostly 1-4 yrs, and the CESM shows droughts centered around 8 yrs. Please use a more quantitative method to compare drought time series power spectrum and/or drought frequency in paleo and model data.

Lines 187-188: difficult to visually compare these different drought metrics in lower panels in Figure 2 because the x axis limits are different.

Lines 204-205: 'no noticeable changes in occurrence of droughts' - is this to the eye? Can you use a more quantitative method to show this (e.g., running counts of droughts in 50 yr windows or something like that)?

Lines 205-206: 'not driven by external forcing': again, this conclusion appears to be drawn based on a visual comparison, which seems insufficient to me. Lehner et al. (2015, ESD, Figure 5) use running correlation to compare model output, which I imagine could be applied here, as could some sort of wavelet/coherence analysis between volcanic forcing time series and the OWDA and CESM data. Also, Superposed Epoch Analysis or Composite Analysis could be used with volcanic forcing time series/large eruptions. At minimum, it would be great to see a time series showing the external forcing to be able to compare to the drought time series in Figure 2.

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Line 209-210: sentence wording is confusing/complicated

Lines 211-215: So if the r value is 0.78, doesn't this imply that only ∼60% of variance is shared by the two time series?

Lines 218-220: 'control simulation presents 29 droughts'- this comparison with the transient simulation is non-sensical/misleading given the two simulation lengths are different. Can the authors instead present the average numbers of droughts of various lengths per century (e.g., Parsons et al., 2018; Coats et al., 2015, Figure 5). This gets around the issue of having different length time series and gives more meaningful information about drought risk standardized to a given time window (e.g., number of droughts per 100 or 500 years).

222-224: Is this the first time these patterns have been presented? Seems that a paper like Markonis et al. 2018 (Nature Communications) or other similar papers have previously presented similar patterns associated with hydroclimatic variability.

Lines 229-236: Similar to the point I raise in Main Concern (5)- It is well documented that this model simulates ENSO events that are too strong and too frequent (e.g., Bellenger et al., Clim. Dyn., among others)- how does that impact these results? For example, if the model simulates too strong, too regular ENSO events that unrealistically influence global climate, then is it surprising that a signal from ENSO is apparent in European drought/climate? And is this finding meaningful if it's based on model bias?

Figure 3 caption: the caption states 'means are not statistically significant'- unclear. Please be more specific. Also please clarify if data are annual, JJA, etc. in figure caption. Additionally, the significance dots are nearly impossible to see on the dark red/blue background

Lines 246-250: Are these % changes in drought/rainfall meaningful (e.g., for agriculture, ecosystems), or do these changes fall well within normal climate variations that don't have a large impact? Also, is the background variability (e.g., standard deviation, mean) of rainfall in the CESM realistic, or can we chalk this up to model bias?

Lines 254-255: similar to Main Concern (4), what about in 20th century reanalysis, ERA5, or some similar reanalysis product vs GPCCv2018 or CRU precip? Or Dai PDSI?

Lines 257-260: 'The starting point is. . .to one or both of them'- confusing wording

Lines 262-269: So in other words, there is about equal odds of being in a drought during various NAO or ENSO phases? This seems important because the authors claim on lines 294-295 that a certain combination of NAO and ENSO conditions are important for initiating drought. . .but it appears to me as though there are nearly equal odds of this happening ($\sim$60%) based on the phase of NAO/ENSO. Is this interpretation incorrect?

Lines 298-310: I don't see how Fig 8 proves the point. Basically, it looks to me as though drought starts off dry and then transitions to less dry conditions at end of drought, and this is distinct from wet years.

Lines 325-327: Similar to Main Concern (4); I have not been shown how the model performs compared to instrumental/reanalysis for the relevant variables over Europe/Mediterranean, so these conclusions don't mean a lot to me.

Lines 337-340: 1) I see no major changes in distribution of drought in Figure 10- are these distributions distinct? Please see previous comments related to statistically distinguishing distributions (and not visually distinguishing), especially when they appear to overlap. 2) Any future changes in ENSO in this model should be interpreted with caution as most CMIP5 models, including this one as far as I can remember, struggle to reproduce the observed trends in the tropical Pacific (see Coats and Karnauskas, 2017, GRL as well as Seager et al., 2019, Nature Climate Change).

Lines 344-345: As figure 9 shows, trends in the region are not linear 1850-2100, so

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trend removal is problematic over this time period. Perhaps it makes sense to remove the trend 2000-2099, but otherwise the authors could be adding an artifact of trend removal into the analysis.

Lines 358-359: ‘our analysis shows that the overall similarities’: as stated above, the authors never actually showed this statistically, just a visual comparison.

Line 383: ‘climate over the region to a drier climate have started earlier than reported in the modern observational era’: to back up a statement like this, I’d again like to see that the model is simulating instrumentally observed climate during the relevant temporal overlap in the historical run (e.g., show Mediterranean precip./PDSI time series in model and instrumental data) before claiming that any drying has happened earlier than reported.

Technical/typo errors and corrections:

Line 4: ‘the internal variability’ – perhaps a stylistic choice, but suggest removing ‘the’ Line 37: ‘on ENSO’ – do the authors mean ‘to ENSO’? Line 40: change to ‘autumn and spring’ Line 74: during ‘the’ last millennium

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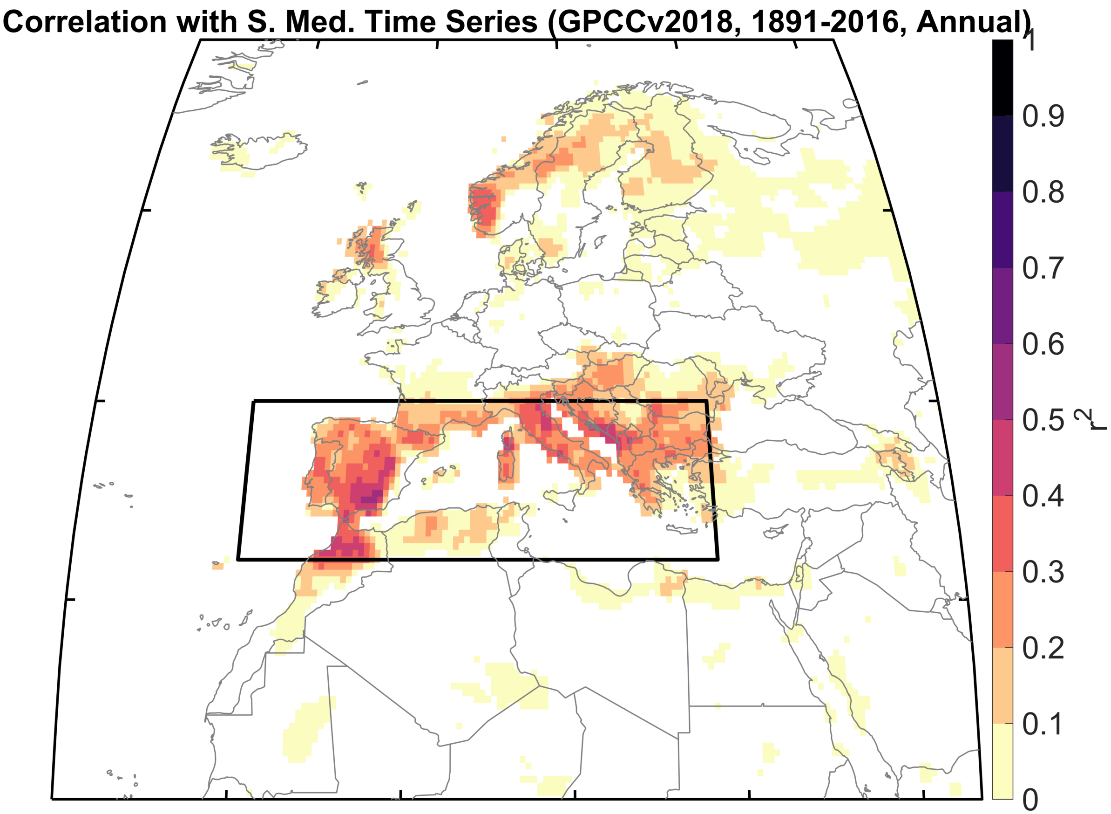

Fig. 1.

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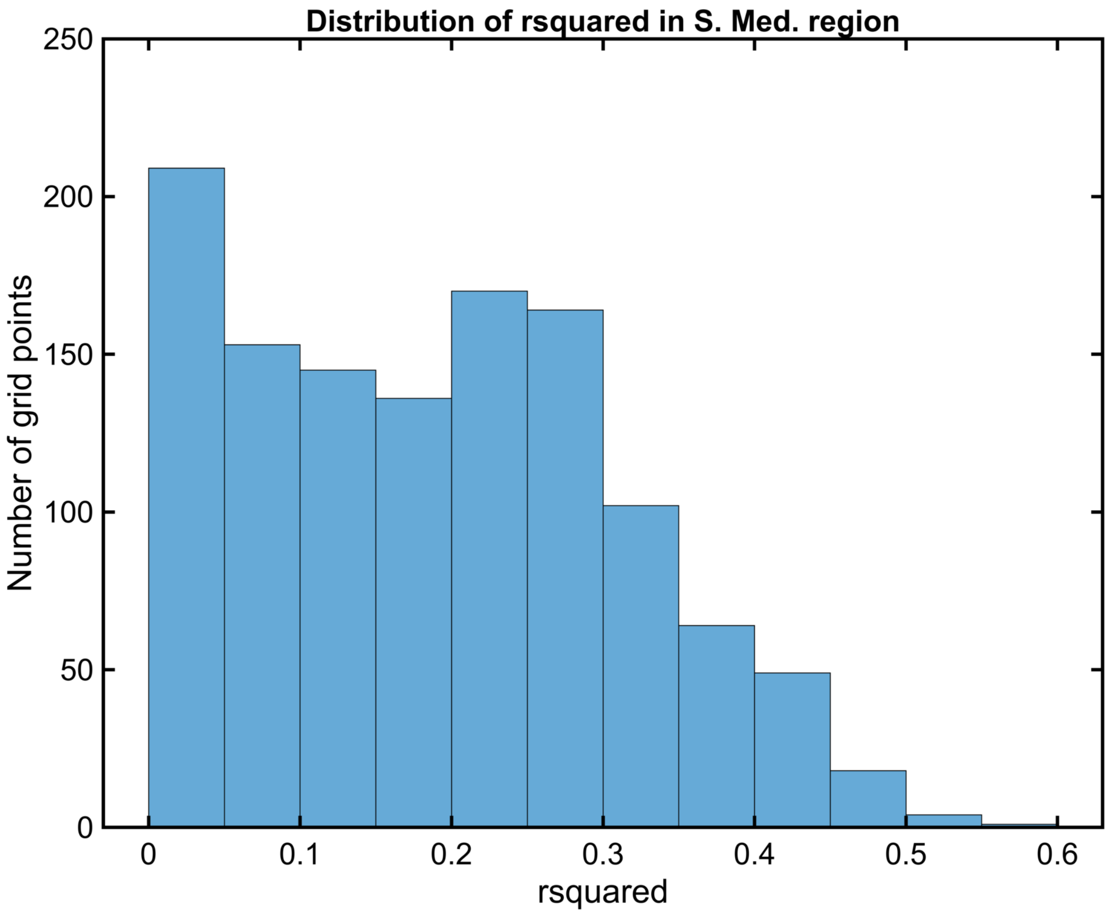

Fig. 2.

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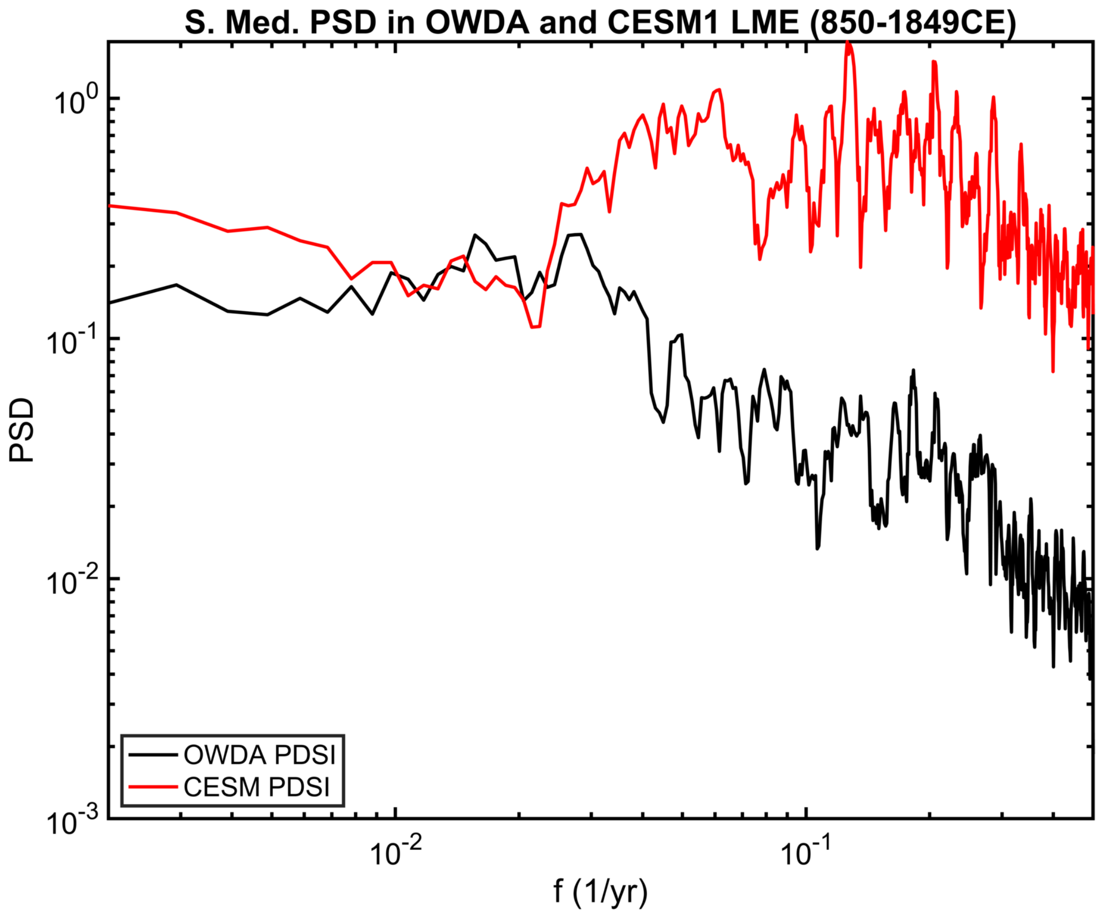

Fig. 3.

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