Dr. Hugues Goosse, editor
Climate of the Past

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Dear Dr. Hugues Goosse,

We would like to thank you again for this opportunity to resubmit our manuscript and also many thanks to the reviewer for the detailed feedbacks. We greatly appreciate the time that the editor and the reviewer dedicated to our manuscript. We have addressed all the reviewer's comments carefully. Please find below our response to the reviewer.

Sincerely,

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Response to the referee #2

General Comments:

1) I commend the authors for improving their paper and conducting extra analyses that support their statements/conclusions. I think this paper will be an important contribution to the literature after some minor wording clarifications.

Thanks very much for your comment.

I have several relatively minor wording and figure clarification suggestions, listed by line number or Figure number below:

2) Line 3-5: thank you for adding some wording about potentially relevant model weaknesses

3) Line 21: 'induce a constant dryness' – suggest adding relative to what time period

We added 'with respect to the 1000 – 1849AD' in line 22:

Lines 21 - 22: 'These feedbacks are intensified during the period 1850 – 2099 AD due to the anthropogenic influence, thus reducing the role of modes of variability on droughts in this period. Eventually, the land - atmosphere feedbacks induce a constant dryness over the Mediterranean region for the late 21st century relative to the period 1000 – 1849AD.'

4) Introduction up to Line 60: nice introduction to Mediterranean drought and previous literature

Thanks very much.

5)Line 84: suggest 'paleoclimate' proxies instead of natural (as opposed to synthetic?)

We modified the word to 'paleoclimate' as suggested.

6) Line 89-95: 'The results show…in the middle years'- I initially asked the authors to acknowledge Parsons/Coats and Namias because they also examine drought trajectories. This description is a bit long given the paper is about Med. drought, not North American drought, so I would remove all of this summary of Parsons/Coats except to state that others have examined drought trajectories before (but in other locations or time periods).

We deleted the summary about Parson and Coats (2019) and Namias (1960). Instead, we briefly mentioned the work from Parson and Coats (2019) together with other literature on the U.S droughts in lines 86 - 88:

Lines 86 - 88: 'Modelling studies on the long-term variability of droughts are focused on the U.S continent, mainly to investigate the variability and mechanisms of South Western United States (SW) droughts (Coats et al., 2013, 2016; Parsons et al., 2018; Parsons and Coats, 2019) and North American pan-continental droughts (Coats et al., 2015; Cook et al., 2016b).

7) Line 134: 'guarantees' seems like a very strong word for the benefits of slightly higher model resolution I the CESM1- I suggest ''provides" or something along those lines instead

We changed the word to 'can improve'. Now the sentence is:

Line 130 – 131: 'Hence, using a model that provides a seamless simulation for the period 850 – 2099 AD with a relatively finer spatial resolution can improve a representation of precipitation related processes.'

8) Line 143: 'we present a conclusions'- fix wording

We corrected the sentence to:

Lines 138 - 139: Finally, the conclusions are presented in Section 4.

9) Line 148:'where the 2005…RCP8.5'- what about the 150 yrs before this? Why specifically state it's RCP, then not say anything here about historical/last millennium forcing.

Lines 156-159: this paragraph seems to fit better with the phrase/description of rcp forcing in the first paragraph of section 2.1. suggest combining so forcing is described all together instead of in pieces

We modified the text as suggested. We removed 'where the 2005 …' and added the new text in lines 154 - 157.

Lines 142 - 143: 'Two simulations are used: a continuous transient simulation of 1250 years (850 - 2099 AD) and a control simulation of 400 years at perpetual 850 AD conditions'

Lines 153 - 156: '[…] Then, the period 2005 – 2099 AD is run with the RCP 8.5 scenario. The transient forcing follows the third Paleoclimate Modelling Intercomparison Project (PMIP4; Schmidt et al.,2012)– fifth Coupled Model Intercomparison Project (CMIP5; Taylor et al., 2012) protocols.'

10) Line 161: suggest removing 'confined to'

We removed 'confined to' and put in parenthesis the latitudes and longitudes of the region.

Line 159: 'The focus area of the study is the western and central Mediterranean region (15 W –28 E and 33 -45N; Fig. 1).'

11) Line 165: The authors used the EOF of instrumental data to define a region, then use this region to study dynamics in the model- this is fine, but note that when I have looked at the EOF of precipitation in other regions, the instrumental and cesm leading EOF modes/patterns don't always essentially show the same spatial distributions.

Thanks for your comment. We assume that the difference in precipitation between models (not only in CESM but also in many other GCMs) and observation are expected due to the model difficulties at representing the precipitation (mostly in regional scale) well. In our analysis, we also noted that there are some differences in spatial and temporal distributions of precipitation between the model and observation over our region of studies (Fig 2). We mentioned these differences and also statistical similarities between the model and observation in the manuscript to justify the usage of CESM (Sect. 3.1., lines 245 – 250.)

12) Line 174-175: I think it would be helpful to clarify what the MW U test is testing (e.g. null hypothesis)- authors later in paper present p-values, etc, but the reader is never told how to interpret these (e.g., does a low p value mean the distributions are or are not statistically distinguishable? In other words, is the null hypothesis that the two distributions are distinct or similar?)

We added in lines 172 - 173 a sentence mentioning about the null hypothesis of MW U test:

Lines 172 - 173: 'The null hypothesis of a Mann-Whitney U-test states that the distributions of both populations (in this case, the transient and the control simulations) are equal.'

Hence, a low p-value means rejecting the null hypothesis, indicating that the two distributions are different to each other.

13) Methods up to Line 187: thank you, nice overview of the Methods

Thanks very much for the comment.

14) Line 208: 'area weighted averaged' seems awkward– are the authors trying to state 'the area-weighted average is calculated'?

We changed the sentence to:

Line 206: 'Then, the area- weighted average of each index is calculated over the Mediterranean region.'

15) Line 212: '10 percentiles' – suggest '10th percentile'

We corrected the word as suggested.

16) Line 213-215: this sentence is long and seems to run several phrases together that need to be separated. Suggest: 'This method, which imposes…extreme percentiles,…drought. Thus, we only'

We corrected the sentence as suggested.

17) Line 221-222: 'This separation…Parsons and Coats (2019)'- see my comment above- I'd suggest just using this sentence/citation here, and eliminating most of the sentences giving details about North American drought in the Introduction.

We left this citation in the Method section as it is and modified the introduction according to the comment #6.

18) Line 233: what is 21th century? Suggest '20th'

Thanks for the point. It was 20th and we corrected it.

19) Line 235: change 'associated to droughts' to 'associated with droughts'

We corrected 'to' to 'with'.

20) Lines 245-250: thank you for assessing model performance

We would like to thank you very much for your detailed comments about the model performance during the previous revision phases.

21) Line 252-253: 'This is also the case…(p values of 0.01)'- please clarify 'this' and also see above note on p-value interpretation and null hypothesis clarification for reader. Also, the previous sentence discusses a student t-test, so is this sentence discussing the M-W U test, student t-test, both?

We noted that we did not mentioned that we used the t-test to compare the time series of scPDSI for the entire present period (1901 – 2000) in the Method section. Thus, we added in the Section 2.4 in lines 236 - 237, a sentence stating about t-tests we used.

Lines 236 - 237: 'The time series of scPDSI during this period are statistically compared to each other using the t-tests with the null hypothesis of equal means between two time series.'

Also note that we slightly rearranged the sentences in this Section (Sect. 2.4).

For the lines 252 – 253 (now in line 251), we changed 'this' to 'the same'. We also slightly modified the paragraph to clarify the content.

Lines 250 - 252: 'Comparing the time series of scPDSI (Fig. 3. (a) and (c)), the summer means between the model and OWDA are statistically similar to each other (p-value from the t-test of 0.28). The same happens between the OWDA and observation but with much lower confidence level of 1\% (p value of 0.01).'

23) Lines 254-257: thank you for adding this description/discussion

Thanks very much again to your comments during the previous revision phases.

24) Line 273: 'annually averaged OWDA'- seasonal? but at annual resolution?

OWDA website is JJA, not annual (but at annual resolution)- please clarify for reader unfamiliar with OWDA

Thanks for the point. We added 'summer' in the sentence. Now, the sentence is:

Line 272: 'The annually resolved summer (JJA) OWDA is based on tree ring reconstructions, which are known to be biased towards the growing season.'

25) Section 3.1: in general this model validation section is much improved and provides nice context for the reader about CESM performance specific to Med drought

Thanks very much again to your comments during the previous revision phases.

26) Line 310: '100 years running means'- suggest '100-year running means'

We corrected the word as suggested.

27) Line 348-349: I disagree with the statement 'we expect to see a similar response…if droughts are influenced by the same externally forced variability' – I think there's a logical mis-step here. If the same drivers were influencing each drought metric this would be true, but as the authors describe, the metrics use different inputs/variables- one is just basically precip, the others include evaporative demand. Soil water/PDSI is influenced by temperature and evaporation- after an eruption, cooling could decrease evaporative demand, and impact evaporation sensitive metrics (and not precip unless some large-scale circulation changes occur, I would imagine). The analysis in the next paragraph seems to support this very point (volcanic eruptions are followed by wet periods in SOIL and PDSI). So would we actually expect all metrics to co-vary with forcing?

Thank you for your comment. We agree with the problem that the referee points out and we were not aware of this during the previous revision phases. As we also mention in the manuscript, as all drought metrics use different inputs and calculation schemes, some are more sensitive to the changes in some input variables (for example, SPEI), while some are not (for example, PDSI) (Vicente-Serrano et al., 2015) and this sensitiveness also depends on the region of study (Raible et al., 2017). Thus, as the referee says,

it is expected that drought indices respond differently to the changes in precipitation and temperature caused by the eruptions.

Here we removed the sentence 'we expect to see … ' (Previously in lines 348 - 349) and changed lines 347 - 349:

Lines 346 - 349: 'In terms of the timing of the occurrence of droughts over the period of 850 - 1849 AD (Fig. 5. (d)), coherent changes among all indices are not identified, which is expected due to the different input variables and calculation schemes among drought indices. This fact also indicates that each index responds differently to the changes in precipitation and temperature, hence potential evapotranspiration, caused by the externally forced variability, e.g., the volcanic forcing.'

Raible, C. C., Bärenbold, O., & Gomez-Navarro, J. J. (2017). Drought indices revisited–improving and testing of drought indices in a simulation of the last two millennia for Europe. Tellus A: Dynamic Meteorology and Oceanography, 69(1), 1287492.

Vicente-Serrano, S. M., Van der Schrier, G., Beguería, S., Azorin-Molina, C., & Lopez-Moreno, J. I. (2015). Contribution of precipitation and reference evapotranspiration to drought indices under different climates. Journal of Hydrology, 526, 42-54.

28) Lines 383-384: 'Hence…other indices'. I would avoid saying this as you show in Figure 5D and make a strong case earlier that these drought indices are generally dissimilar. You also state 'in terms of timing of the occurrence of droughts...coherent changes are not identified'- so are the drought metrics co-varying or not? These sections/findings/conclusions still seem to conflict with one another. The previous sentence also seems to show the only ¼ of droughts overlap using these different metrics.

We also mentioned in line 379 that the SOIL overlaps not the full period but 'the 36%, 25% and 29% of droughts in the scPDSI, SPEI and SPI, respectively' (which is roughly ¼ as the referee mentions), also showing quite acceptable values of Pearson correlations with other indices over the entire period (lines 381 – 382). Thus, we assume that not fully but a part of the mechanisms given by SOIL can be transferred to other indices.

With this reason, we slightly modified the sentence by adding the word 'partially':

Lines 382 - 383: ' Hence, the results in the following sections can be partially transferred to the other indices.'

29) Section 3.3: thanks for comparing cesm control and forced drought frequency etc per century- much clearer

Thanks very much again to your comments during the previous revision phases.

30) Lines 389-390: these are the composites of all drought years, right? Might be helpful to clearly state this section/paragraph present results for all years, and that after you will break up analysis into different parts of drought

We reformulated the sentence by adding that we analyzed the entire drought events in line 390. Now the sentence becomes:

Lines 389 - 390: 'To get a first glance of the atmospheric circulation during drought conditions, we analyze the mean circulation conditions during all short (1 and 2 years of duration) and long (more than 3 years) Mediterranean droughts together.'

31) Lines 400-405: what about the significant differences in temperature in the maps shown in the temperature maps in Figure 8

Thanks for your point. We did not include a discussion of the differences in temperature as we focused more on the regions with similarities knowing that the internal variability is the potential drivers of Mediterranean droughts (from the result of the previous Section 3.2). Moreover, the region with significant differences in temperature anomalies show rather weak values of anomalies (for example, over North African and Asian continents). The difference in the temperature anomalies over Siberia is quite striking. However, this difference is not reflected in the difference in the circulation patterns (geopotential heights) between the control and transient simulations. Thus, we do not expect that this temperature pattern could remotely affect the Mediterranean climate.

In the revised manuscript, we briefly mention about the statistical dissimilarities in the temperature field in lines 407 - 412.

Lines 407 - 412: 'Some statistically significant dissimilarities between the control and transient simulations are noticeable mostly in the temperature anomalies. Over the regions where the temperature anomalies are statistically different the anomalies are rather weak, except the warming in Siberia. However, this positive temperature anomaly in Siberia is not associated with a geopotential height pattern, showing statistically indistinguishable geopotential height anomalies in the region between the control and transient simulations (Fig. 8). This indicates that there is no change in the circulation pattern over this region that can possibly be connected to the Mediterranean drought condition.'

32) Line 425-426 'Figure 7'- Figure 8?

Yes, Figure 8 is correct. We corrected the number in the revised manuscript.

33) Line 439-440: can you clarify these are composites of all drought years, to contrast w transition etc years later?

We modified the sentence to:

Lines 446 - 447: 'Considering all short and long droughts, the simulation shows that droughts occur more frequently during the positive phase of annual and winter NAO than during the non-drought period'

34) Line 447: 'more frequent' – change to 'more frequently'

We corrected the word as suggested.

35) Line 454: '49% in' suggest change to '49% of'

We corrected the word as suggested.

36) Line 457: suggest 'In the case of ENSO'

We corrected the word as suggested.

37) Line 458: relative to what background frequency? You did a nice job contextualizing the background frequency of NAO relative to non-drought periods, what about La Nina here?

We added 'relative to the non-drought period' at the end of the sentence in line 463.

Line 464: 'the frequency of La Niña-like is 40% in the initiation years relative to the non-drought period.'

38) Line 475: 'some mechanisms associated with this circulation is'- change to 'are'

We corrected the word as suggested.

39) Line 479: 'This is supported' – suggest 'This mechanism is supported' or other clarifying wording

We modified the sentence to:

Lines 485 - 487: 'The presence of the atmosphere - soil interaction during the transition years is supported by the increases in frequencies of positive surface temperature (TS) and sensible heat flux (SH), and negative soil moisture (SOIL), evapotranspiration (EV), and latent heat flux (LH) anomalies during this period'

40) Line 502: suggest change 'noticed' to 'apparent'

We changed the word as suggested.

41) Line 518: suggest 'reflected by' (not 'to')

We changed the word as suggested.

42) Line 523: please clarify if you mean a 'reduction in positive NAO and a reduction in La Nina like conditions', or 'a reduction in NAO, and general La Nina like conditions during drought'- right now, the wording seems ambiguous

The first one is correct. Thus, we modified the sentence as:

Lines 528 - 529: 'the non-detrended 1850 – 2099 AD period shows reduced frequencies of both positive NAO and La Niña-like conditions during droughts'

43) Line 526-527: 'Hence, the role of ..La Nina..': This explanation makes sense, but note that many CMIP5-class models also generally become more 'El Nino like' (weaker tropical Pacific E-W gradient) with warming, so this could be also just caused by the fact that global warming could separately impact both La Nina and local drought, right? See for example Seager et al who show that most cmip5 models become more el nino like:

https://doi.org/10.1038/s41558-019-0505-x

Thanks for the suggestion. We extended the paragraph discussing about this bias in CMIP5 model, and this issue may have some implication in our results in lines 533 - 538.

Lines 533 - 538: 'Note however, that model biases in representing large scale modes of variability, in particular ENSO, might be relevant. Many CMIP5 models have problems in realistically reproducing the cold SST in the east Tropical Pacific. Therefore, these models would show less La Niña events in the future warmer conditions (Seager et al. 2019). An overall increase in El Niño-like condition here (Fig. 14. (b)) can also be partially related to this bias. Nevertheless, considering this bias does not affect the result that the interaction among the regional variables is changed by the increase in temperature, causing more intensified regional land-atmosphere feedbacks during this period.'

44) Line 536: 'Hence, when the linear trends…are not taken into account' by this do you mean not removed, or if they are removed? please clarify wording

To clarify this part, we modified the sentence as:

Lines 547 - 549: 'Hence, when the anthropogenic effect is removed (i.e., the variables are detrended), the mechanisms involved in droughts remain unchanged during 1850 - 2099 AD. The comparison of non-detrended with the detrended variables, thus, indicates that no other factor than the anthropogenic influence in temperature is the cause of the severe dryness in this period.'

45) Line 579-581: 'Moreover…warming scenario' – good point, may be good to cite Seager et al (see above) or another similar paper here that discusses this point

We cited and included a brief discussion on Seager et al. (2019) in lines 592 - 594. Also, refer to our response #43.

Lines 592 - 594 : 'Many CMIP5 models show an overall warming of the Tropical Equatorial Pacific reducing the west-east gradient of SST which is different from what is observed in the present period (Seager et al., 2019). This model bias to the observation implicates reduced La Nina condition in CMIP5 models in a warmer world.'

46) Line 603: 'surface-based indices indicate droughts': suggest clarifying wording as this is confusing unless you have read Berg et al 2017

- for example, what is 'surface' here for the reader who isn't familiar with this paper/topic?

Overall, nice discussion of this topic.

We wanted to refer the upper-level surface soil moisture. We changed the word in line 617, and modified slightly the paragraph in lines 614 - 618:

Lines 614 - 618: 'The reason is that most of the commonly used offline drought indices, such as scPDSI, are based on a water balance that considers only the atmospheric moisture supply and demand, and these indices tend to overestimate drought risks in the future warming scenario (Berg et al., 2017; Mukherjee et al., 2018; Swann et al., 2016). Moreover, Berg et al. (2017) found that the upper-level soil moisture indicates droughts, whereas the mean 3-meter soil moisture shows wet or relatively weak dry conditions compared to the surface level.'

47) Line 610: 'followed'- suggest 'conducted' or similar wording change

We changed the word to 'conducted' as suggested.

48) Line 614: 'under the current climate change scenario': does this mean a specific SSP/RCP from CMIP, or just if current warming trends continue? 'scenario' is a potentially loaded word given the modeling focus of this paper. Suggest clarification.

We meant under the RCP warming scenarios (2.8, 4.5 and 8.5) based on Giorgi et al. (2008) and Lehner et al. (2017). Thus, we changed the word to 'under the future warming scenarios' in line 629.

49) Figure 2: suggest labeling 'summer' and 'winter' on maps, also labeling the dashed and solid lines/graphs with legends for easier reference

We added the labels and legend in the figure.

50) Figure 4: 'composites' of what?

We modified this part as 'Pearson correlation coefficients between the scPDSI and anomalies'.

51) Also, what is CR21? Do the authors mean 20th century reanalysis? Isn't this then CR20?

Yes, it is CR20. We corrected it.

52) Figure 5: generally, much improved and comparison is helpful for control vs transient- thank you

Figure 6: thank you for including the time series- much easier to interpret wavelet/coherence

Thanks again for your comments during the previous revision phases.

53) Figure 8: Interesting how distinctive the temperature patterns are during droughts in control vs forced runs (assuming I'm interpreting the stippling correctly)- is this mentioned in the main text?

We addressed this issue in our response #31.

54) Also, 'distributions' – do the authors mean to say 'differences in distributions' (otherwise, what does it mean for a distribution to be significant?)

We modified the sentence to 'where the distributions between the control and transient simulations are statistically different to each other.'