Reply to Editor on HESS-112 followed by answers to comments of Referee 1 and Referee 2

Editor Decision: Publish subject to revisions (further review by editor and referees) (16 Nov 2020) by Shraddhanand Shukla
Comments to the Author:
Dear Authors,

I have now received the reviews from both original reviewers. Although both reviewers are recommending Accept after minor revisions, in my opinion at least some of their comments (which are valid) are more in moderate category than minor. In particular I want to highlight the following comment from reviewer #2, which is an important one about clarifying the scope of this study.

"In the concluding remarks the authors seem to imply that this experiment was to estimate the limits of the impact of internal forcing and these are more idealized results. This makes more sense to me, but you must note that these are more idealized results. I think it might be best to state that very prominently in the introduction in order to guide the reader through what you're achieving and the limitations of this study."

The manuscript can only be formally accepted for publication after reviewers comments are fully and satisfactorily addressed. Please carefully go through reviewers comments and provide detailed response and revise manuscript accordingly. As per reviewers availability I'd definitely try to seek their review again.

Thanks again and I look forward to seeing the revised version of this manuscript.

Shrad

Author response to the editor comments

Dear Editor

Thank you for your comments and to the reviewers who contributed with their comments to improve the manuscript.

As suggested, we have sent the manuscript for an English Language Editing (please see the certificate at the end of this document to ease the reading and to avoid confusion due to language issue.

Regarding comment of reviewer # 2, We did this following modification in the manuscript at the introduction line 78 to 83: This study aims to estimate the limits of the impact of internal forcing of initial soil moisture over West Africa region using a Regional Climate Model. Experiments carried out are sensitivity studies that give idealized results of the effect of the initial soil moisture. In this study (part I), the sensitivity of mean climate simulation to initial "wet" and "dry" soil moisture conditions is investigated.

Thank you again and best wishes

Arona

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Title

Influence of initial soil moisture conditions in a regional climate model study over West Africa: Part 1: Impact on the climate mean

Authors

Brahima Koné, Arona Diedhiou, Adama Diawara, Sandrine Anquetin, N'datchoh Evelyne Touré, Adama Bamba, and Arsene Toka Kobea

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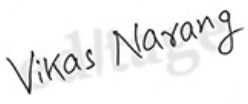

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Reply to the comments of referee 1 on HESS-112

The authors have responded to all initial comments and have, for the most part, revised the manuscript accordingly and satisfactorily. I have a few comments below that either still need further input or are new comments. Overall the manuscript still needs minor revision before publication. One of the weaknesses in the paper still remains in its use of the English language, **there are a number of sentences that do not make sense** or are run-ons. Additionally, I think the authors could still **add some description fo their introduction and summary/conclusion** to make the paper flow better.

Major/Specific Comments:

1. ***Comments:***

One of the main concerns of both reviewers and the editor was your use of only 2 years in the study. **I don't understand your reasoning noted in your response to the editor that since the sensitivity to initial soil moisture anomalies is only one season you only need two experiments.** I still find this concerning, though the authors have at least provided references that show this experiment design in use in other papers.
In the concluding remarks **the authors seem to imply that this experiment was to estimate the limits of the impact of internal forcing and these are more idealized results. This makes more sense to me, but you must note that these are more idealized results. I think it might be best to state that very prominently in the introduction in order to guide the reader through what you're achieving and the limitations of this study.**

It may also be prudent to include the Figure with "wet" and "dry" years that was included in the author responses to give the reader some indication of how "extreme" these years were. This Figure wasn't included in my review report for some reason, so I'm not sure if its the same as something already included.

> ***Author's response****: Thank you for your comment. We indicated in the introduction your suggestion "that these experiments were to estimate the limits of the impact of internal forcing and these are more idealized results". The figure with "wet" and "dry" years were also included in the manuscript as recommended.*
>
> **Author's changes in manuscript**: *We did this following modification in the manuscript at the introduction line 78 to 83:* This study aims to estimate the limits of the impact of internal forcing of initial soil moisture over West Africa region using a Regional Climate Model. Experiments carried out are sensitivity studies that give idealized results of the effect of the initial soil moisture. In this study (part I), the sensitivity of mean climate simulation to initial "wet" and "dry" soil moisture conditions is investigated. In part II of the article, the influence of initial soil moisture conditions on climate extremes will be explored.

2. ***Comments:***

With respect to my initial comment 3 on the normal distribution. I am aware that the use of the normal assumption is not new. My question is whether you think it is appropriate to use that assumption here. For example, precipitation is better approximated with a gamma distribution on seasonal timescales, etc. The approximation of normal can be problematic for variables that are not normal, and this may be the case here.

***Author's response**: Thank for your comments. There is a little literature on the probability distributions of annual, seasonal and monthly precipitation in the study region. Markovic (1965) investigated the probability distribution of annual precipitation in the western USA and southwestern Canada using the chi-squared statistic to measure the goodness of fit of sample data to selected probability distribution. He concluded that annual precipitation can be best approximated by the 2-parameter lognormal (LN2) and gamma (GAM) distributions. Sheng Yue and Michio Hashino in their work over Japon suggested that the Pearson type III (P3) and the log-Pearson type III (LP3) distributions are acceptable distribution types to represent statistics of precipitation in Japan with the LN3 distribution as a potential alternative. These studies make an attempt to determine the probability distribution types of annual, seasonal and monthly precipitation across these regions. However, such studies have never been done over West Africa. You are right, the GAM distribution was frequently used to represent monthly and seasonal precipitation (Ropelewski et al.,1985; Wilks & Eggleston, 1992), but it is worth to note that the Gamma distribution **is useful for variable which is always positive** (Than et al. (2017), Jaeger and* Jaeger and Seneviratne, 2011). *However, for the biases or changes (including positive and negative values), a normal mode type distribution is more suitable (Gao et al., 2016).*

3. ***Comments:***

You have an entire section on how your experiments influence surface fluxes but only devote a line to these results in the concluding remarks, which makes me think that they are less important. Please add some context to the concluding remarks on why these results are significant to this experiment design and the field.

***Author's response:** Thank you for this comment which helps to improve the reading of the manuscript and the presentation of the results.* We added a paragraph in the concluding remarks on these significant results in the context of our experiments.

Author's changes in manuscript: *We did this following modification in the manuscript at the conclusion line 473 to 478:* Our study showed significant impact of initial soil moisture conditions anomalies on the surface energy fluxes. We observed in wet (dry) experiments that the cooling (warming) of surface temperature was associated with an increase (decrease) of sensible heat flux, a decrease (increase) of latent heat and an increase (decrease) of the depth of the boundary layer over the region, with different magnitudes varying from one sub-region to another.

4. ***Comments:***

Similarly I think your end of section summaries could be improved and put into the context of your experiments. For example, line 447 - 450 you discuss the cooling and warming of surface temperature, but what does that have to do with your wet, dry, etc. experiments?

***Author's response:** Thank for your comment. You are right. We rewrote the section summaries to put them into the context of our experiments.*

Author's changes in manuscript: *We did this following modification in the manuscript at the section 3.2 line 435 to 440:* Summarizing the results of this section, in the wet experiments, the cooling of mean surface temperature is associated with a decrease of latent heat flux, an increase in sensible heat flux and the PBL depth over most of the studied domain. Conversely, in the dry experiments, the warming of surface temperature is associated with an increase of the latent heat flux, a decrease of the sensible heat flux and PBL height.

Minor Comments:

1. ***Comment:*** *Line 41-45: References are needed.*
   Author's response: *Thank for your comment. We added reference.*
   Author's changes in manuscript: *We did this following modification in the manuscript at the section 1 line 45 to 49:* Schär et al. (1999) sustained that the role of soils may be comparable to that of the oceans. The solar energy received by the oceans is stored in summer and used to heat the atmosphere in winter. The precipitation received by the soils is stored in winter and contributed to moisten and cool the atmosphere in summer.

2. ***Comment:*** *Avoid language like "tends to cause", etc. as it lessens the impact of the results.*
   Author's response: *Thank for your comment. As suggested, we have sent the manuscript for an English Language Editing (please see the certificate at the end of this document) and these expressions have been removed and changed in this revised version.*

   Author's changes in manuscript: *Please see the revised manuscript.*

3. ***Comment:*** *You've introduced some abbreviations and didn't use them continuously, please make sure you did this. E.g. page 7 "mean bias" vs. "MB".*

   Author's response: *Thank very much for this remark. We used the abbreviation introduced in the whole revised manuscript.*

   Author's changes in manuscript: *Please see the revised manuscript.*

4. ***Comment:*** *Line 230: What do you mean by "peak mode of change"? You had been previously saying "peak of change" - Which I'm also not sure what that means. Do you mean "maximum change"?*
   Author's response: *Thank for your comment. Yes, we mean by "peak mode of change" the "maximum magnitude of change". We have sent the manuscript for revision in English and these are the expressions that have been reported.*
   Author's changes in manuscript: *We replace "peak of change" by maximum change to make it more comprehensive. Please see through the revised manuscript.*

5. ***Comment:*** *Grammatical Comments: I'm not going to list all of these, but I suggest you still revise your manuscript for English language. There are a number of places that the sentences don't make sense, are "clunky", or just are incomplete. A few examples are below:*

Author's response: *Thank for your comment. We revised the manuscript for English language and take in account all your remarks. Please see the Certificate confirming that the issue with English Language has been addressed.*

Line 36: "The strength of soil moisture impact on land-atmposphere coupling is variable according to the place and with the season." You can simply say: "The strength of soil moisture impacts on land-atmosphere coupling varies according to location and season" and this will greatly improve the clarity of the sentence.

Author's response: *Thank you very much for your suggestion. Please check lines 42-43*

Line 91: "The scheme of the large-scale precipitation used is from Pal et al. (2000), the moisture scheme is the SUBEX (SUBgrid EXplicit moisture scheme) takes in account the cloud variability scale sub-grid, and the accretion processes and evaporation for stable precipitation following the work of Sundqvist et al., 1989." Can be restated: "The large-scale precipitation scheme is from Pal et al. (2000) and the moisture scheme is the SUBEX (SUBgrid EXplicit moisture scheme). The SUBEX take into account the sub-grid scale cloud variability, and the accretion processes and evaporation for stable precipitation following the work of Sundqvist et al., 1989."

Author's response: *Thank you for your proposition. Done. Please check lines 99-102*

Line 114: The uncertainties reduction related to the absence of reliable observation system over the region (Sylla et al., 2013a; Nikulin et al.,2012), we validated the simulated precipitation based on two products..." I'm not sure what this means.

Author's response: *Thank you. The sentence was reworded as follows:* Due to the coarse resolution of the climate observing network over the region, we validated the simulated precipitation based on two satellite derived products (Sylla et al., 2013a; Nikulin et al.,2012): *Please check lines 122-124.*

Line 172: "In the aim to identify the extreme years (driest and wettest) impacted by the dry and wet experiments among the five years simulations (2001 to 2005), we display Changes in daily soil moisture for 5 years (2001 to 2005) and their climatological mean during JJAS over West African domain, from dry and wet experiments with respect to their corresponding control experiment in Figure 2." This is a run-on and clarity would be increased if it were simplified.

Author's response: *Thank you. We reworded as follows. Please check lines 178-183:* To identify the extreme years (driest and wettest) impacted by the dry and wet experiments among the simulation period (2001–2005), we determined changes in daily soil moisture and their climatological mean during JJAS over the West African domain from dry and wet experiments with respect to their

corresponding control experiment. These changes are presented in Fig. 2, which shows that the weakest and strongest impacts of the dry experiments were observed in 2003 and 2004, respectively.

Line 188: "...for JJAS 2003 and JJAS 2004 and their corresponding simulated from control experiments..." And their corresponding what?

Author's response: *Thank you.* "…we determined changes in daily soil moisture and their climatological mean during JJAS over the West African domain from dry and wet experiments with respect to their corresponding control experiment." Please check line 181.

References:

Gao, X.-J., Shi, Y., and Giorgi, F.: Comparison of convective parameterizations in RegCM4 experiments over China with CLM as the land surface model, Atmos. Ocean. Sci. Lett., 9, 246–254, https://doi.org/10.1080/16742834.2016.1172938, 2016.

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Markovic, R. D. (1965) Probability of best fit to distributions of annual precipitation and runoff. Hydro. Paper no. 8, Colorado State Univ., Fort Collins, Colorado, USA.

Ropelewski, C. F., Janowiak, J. E. & Halpert, M. S. (1985) The analysis and display of real time surface climate data. Monthly Weather Review 113, 1101–1106.

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Thanh N.-D., Fredolin T. T., Jerasorn S., Faye C., Long T.-T., Thanh N.-X., Tan P.-V., Liew J., Gemma N., Patama S., Dodo G. and Edvin A.: Performance evaluation of RegCM4 in simulating extreme rainfall and temperature indices over the CORDEX-Southeast Asia region. Int. J. Climatol. 37: 1634–1647. Published online 28 June 2016 in Wiley Online Library (wileyonlinelibrary.com) DOI: 10.1002/joc.4803, 2017.

Wilks, D. S. & Eggleston, K. L. (1992) Estimating monthly and seasonal precipitation distributions using the 30- and 90-day outlooks. J. Climate 5, 252–259.

Schär, C., Lüthi, D., Beyerle, U. & Heise, E. The soil-precipitation feedback: A process study with a regional climate model. J. Clim. 12, 722–-741 (1999).

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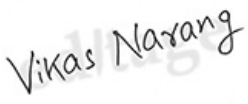

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Reply to the comments of referee 2 on HESS-112

I appreciate the edits the authors have made to the manuscript. The authors, however, did not address one of my main concerns, which was that even as a sensitivity analysis their choice of permanent wilting point and field capacity may be rather extreme. I believe either the authors misunderstood my comment, or I am misunderstanding their methods (either is possible).

In my comment 4 I expressed concern about the authors using a global value for wilting point and field capacity, because in reality both of these values change dramatically within their domain. The authors cite the methods of Hong and Pan (2000), but in Section 2.2. of that study the authors say that "the version of soil model used in this study uses a uniform vegetation fraction and soil texture value over land", which would produce global values of field capacity and wilting point in their model, making the use of single global values (0.1 and 0.47) analogous to the use of local values.

If the model that the authors use in this current study does not use a uniform soil texture and vegetation fraction, then the soil model would not have uniform values for wilting point and field capacity. I am not familiar with the model so I cannot say if this is the case. I would ask that the authors clarify for the reader either that (1) their soil model uses uniform values of wilting point and field capacity (as does Hong and Pan, 2000) or (2) be clear that even though their soil model has spatially varying values of field capacity and wilting point, that they use a single global value. I ask this only so that readers will be able to follow the methods the authors are using.

Lines 18-20 in the abstract stating that "we initialized the soil moisture at the wilting points and field capacity with dry and wet soil moisture initial conditions", for example, is untrue if the authors are using a model that has spatially varying wilting points and field capacities, but initializing all locations using a single value. The authors could instead say that they initialize the soils at volumetric fractions of 0 and 0.49 everywhere, which would be more clear for the reader.

> ***Author's response:*** *Thank you very much for your comment. You are right. As confusion may come from the issue with English language, the manuscript has been deeply edited to ease the reading. The line has been reworded in the abstract.*

> ***Author's response:*** *Thank for your suggestion. We did this following modification in the manuscript at line 161-163:* We initialized the dry and wet soil moisture initial conditions (in volumetric fraction $m^3.m^{-3}$) respectively at the minimum value ($=0.117*10^{-4}$) and the maximum value ($=0.489$) derived from ERA20C dataset over the West Africa studied domain.

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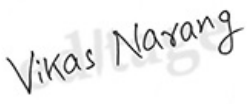

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