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Interactive comment on "Understanding land surface response to changing South Asian monsoon in a warming climate" *by* M. V. S. Ramarao et al.

M. V. S. Ramarao et al.

sanjay@tropmet.res.in

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Review Comments: This manuscript seeks to understand the land surface response to global warming through a series of experiments using the LMDZ atmospheric model coupled to the ORCHIDEE land surface model. The authors report results from experiments where the atmospheric model is forced with SSTs from coupled model simulations (IPSL model; bias corrected) with historical (HIST; anthropogenic & natural) forcings, natural only forcings (NAT) as well as a future (RCP4.5) scenario. They analyze the surface air temperature, precipitation, evapotranspiration, and soil moisture from these simulations in order to understand how the soil moisture behaves in the

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future scenario and when changes in this quantity may be detectable.

The text in the manuscript needs to be a little tighter -inconsistencies in figure captions and clarity of wording. Furthermore, some of the conclusions need to be revised.

Response: We are thankful to the reviewer for providing thoughtful comments and offering important suggestions for improving the manuscript. We have addressed all the suggested comments and suggestions. The revised manuscript is more concise, clarity of wording is improved and inconsistencies in figure captions are corrected. We have also revised some of the conclusions, as suggested by the reviewer.

The claims of attribution of precipitation changes over India to anthropogenic forcings are overblown given that these are atmospheric model experiments. At best it is indicative of an influence and calls for higher resolution coupled models with better land surface representation. But to my eye the claims of a difference in trend between the HIST and NAT experiments is not borne out and most likely is within the noise (variability of the NAT run as per their own definition) - which they have curiously not bothered to test.

Response: We understand the reviewer's point. In the revised manuscript, we have made suitable revisions and addressed this point. The specific revisions are given in the response to Detailed Comments below.

I also feel that the analysis does not delve into whether the reduced soil moisture plays any role in the reduced precipitation given the literature on how monsoon precipitation is substantially from local sources (in addition to transport from ocean areas).

Response: We understand the reviewers' point. This study is mostly focused on the land surface hydrological response to the changing monsoon precipitation. As pointed out by the reviewer, monsoon precipitation is influenced by large-scale dynamics, organized convection, local moisture sources, etc., isolating the impact of soil moisture on precipitation requires separate experiments and is beyond the scope of this study.

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Detailed comments: 1. Section 1 Introduction: The reference to ground water depletion is misleading, as it seems to imply that the drying is penetrating into the aquifers. This depletion is purely due to over-pumping and if anything has probably acted to increase soil moisture where it has been exploited.

Response: We agree with the reviewer. Accordingly, the sentence is removed from the revised manuscript.

2. Section 2.1 Model and experiments: The explanation of the experiments is misleading. These are not “long-term simulation experiments follow CMIP5. . .” In fact these are AGCM experiments that use CMIP5 simulations to provide SST boundary conditions. There is a difference! In the same paragraph it is mentioned that HIST and NAT runs “include natural forcings (e.g. volcanoes, ENSO)”. The ENSO is not a climate forcing in the same sense as a volcano or GHGs. This is a mode of internal variability of the climate system and as such should not be in the list of forcings.

Response: Thanks for the comment. We agree with the reviewer and the sentence in the text is removed in the revised manuscript. We also noted that ENSO is a mode of internal variability of the climate system and we modify the list of natural forcings as "volcanoes and solar variability" in the revised manuscript.

3. Section 3.2 Simulation of climate trends over the monsoon region: The sentence “A climate model’s credibility is increased if the model is able to simulate past variations in climate” should include “when given realistic forcings”.

Response: The authors thank the reviewer for the comment. The sentence is now suitably modified in the revised manuscript.

4. Table 1: Just showing the correlations will not be sufficient to assess model fidelity. This table will be better off if replaced by a Taylor Diagram.

Response: Thanks for the suggestion. The Taylor skill for the water balance components is assessed and the we will replace Table 1 by Taylor diagrams in the revised

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manuscript (Fig 4 in the revised manuscript).

5. Figures 1 & 2: The time period of the comparison is not mentioned. Response: As suggested by the reviewer, the legend of the figures will be modified accordingly by including the period of comparison.

6. Figures 4, 5, and 6: The figure quality is less than adequate.

Response: Thanks for the comment. The modified figures are included in the revised manuscript (Figs, 5,6, & 7 in revised manuscript).

7. Figure 7: Caption unclear. Must be revised. Response: Authors thank the reviewer for the comment. Figure caption is revised in the manuscript (Fig. 8 in revised manuscript).

8. Figure 9: Text says the region over which averaging is done is Central India (74.5-86.5E, 16.5-26.5N) but figure caption says otherwise. Which one is it?

Response: We thank the reviewer for pointing out the mistake in the figure caption. The region used is Central India (74.5-86.5E, 16.5-26.5N) as mentioned in the text. The figure caption is corrected accordingly.

9. There is something odd about Figure 9 a, and 9 c. These two show a sharp drop around 2010. I wonder if there is some discontinuity in the data for these two fields before being smoothed by the 20-year running mean. For 20-year smoothed fields, they do appear very noisy!

Response: We verified the data time series for these two fields without applying a 20-year running mean. Although large interannual variations are noted in the data time-series, there is no discontinuity as such.

10. Although 9 a shows that the “detectable” change first appears in 2010, there are subsequent times when it goes back under the detectable level. Any comments on that?

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Response: We understand the reviewers' point that a detectable change in soil moisture first appears around 2010, then the change is not prominent until 2050s and thereafter remains detectable till the end of 21st century. One can note coherent evolution of the soil moisture and precipitation variations (Fig.10, revised manuscript). In addition, we also see more persistence in detectability of soil moisture as compared to that of precipitation. This is consistent with the result that the soil moisture spectra is dominated by lower frequency variations as opposed to the precipitation spectra (Delworth and Manabe, 1988). This point is mentioned in the revised manuscript.

11. Section 6 Conclusions: The conclusion "The results from our study suggest that the declining trend of monsoon precipitation over South Asia and weakening of large-scale summer monsoon circulation during the post-1950s are largely attributable to anthropogenic forcing." is not supported by the analysis. As indicated earlier, the difference in trend between the HIST and NAT experiments is not borne out and most likely is within the noise (variability of the NAT run as per their own definition) - which they have curiously not bothered to test.

Response: We agree with the reviewers' comment on 'attribution'. The statement in conclusions is suitably modified in the revised manuscript accordingly (Page 13, line 3). The linear trend in the monsoon precipitation time-series in HIST for the period (1951-2005) is -0.8 mm d-1 (55 yr)-1 and exceeds the 95% confidence level. On the other hand the linear trend in the NAT time-series for the same period is -0.01 mm d-1(55 yr)-1 and is not statistically significant.

12. Figure S2: If the full time-series 1866-2005 for both HIST and NAT were plotted, the differences if any will be clearer perhaps.

Response: As suggested we have plotted the HIST and NAT time-series for the period 1886-2005 and the differences in the two time-series are clearer (Fig. 1 is shown below).

13. The claim "The simulated decrease of mean monsoon precipitation over the Indian

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region during the post-1950s is accompanied by a weakening of large-scale monsoon circulation and is consistent with observations" must be supported by the analysis or a suitable reference to a study showing circulation changes in "observations".

Response: This point is well noted. We have referred in introduction, a previous study by Krishnan et al. (2013) which showed the circulation changes in observations. This reference is included in the revised manuscript to support the observed circulation changes.

14. The sentence "The present high-resolution simulations are scientifically interesting, particularly given that the CMIP5 models driven with same scenario generally show a slight increase in mean precipitation over the Indian region, associated with large uncertainties (Chaturvedi et al., 2012)" should be corrected. Their figures 3 and 8 clearly show that models can and do simulate reduced precipitation in the different scenarios among the different models.

Response: We agree that some of the CMIP5 models analysed by Chaturvedi et al., 2012 show a decrease in mean precipitation over Indian region. The sentence is corrected in the revised manuscript.

15. Figure S3 caption needs to say what the difference is between. Response: The difference is HIST-NAT simulations of LMDZ model for the period 1951-2005. The figure caption is modified in the revised manuscript.

Interactive comment on Earth Syst. Dynam. Discuss., 6, 943, 2015.

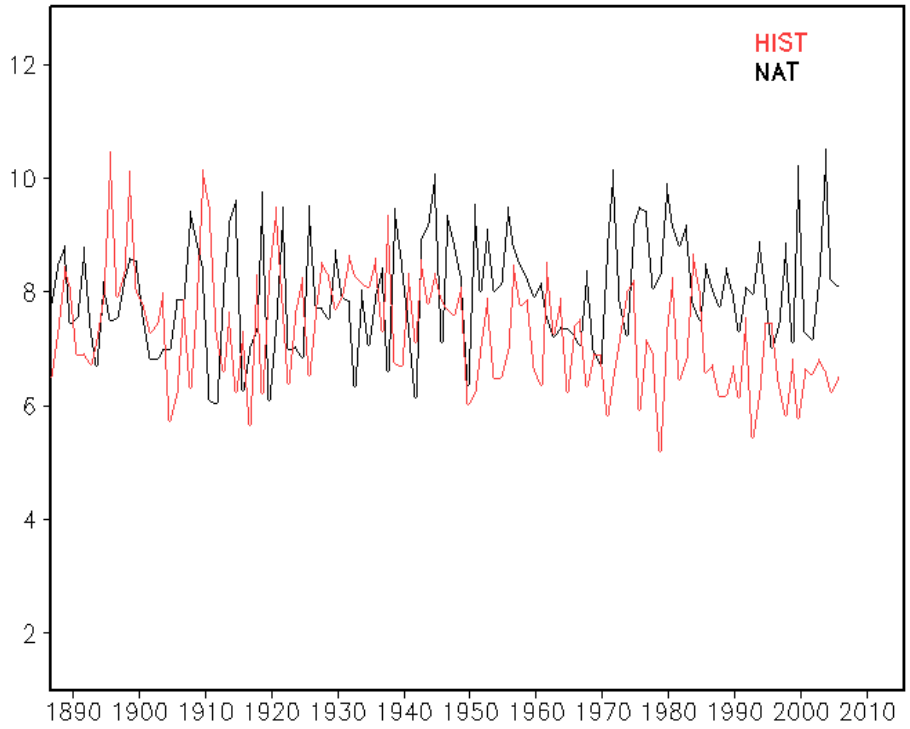

Fig. 1. Area averaged time series of JJAS mean precipitation (mm d-1) from LMDZ (red) HIST and (black) NAT simulations during 1886-2005.

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