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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.

Anonymous Referee #2

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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 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

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and clarity of wording. Furthermore, some of the conclusions need to be revised. 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. 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). 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. 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. 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”. 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. 5. Figures 1 & 2: The time period of the comparison is not mentioned. 6. Figures 4, 5, and 6: The figure quality is less than adequate. 7. Figure 7: Caption unclear. Must be revised. 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.

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Which one is it? 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! 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? 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. 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. 13. The claim “The simulated decrease of mean monsoon precipitation over the Indian 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”. 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. 15. Figure S3 caption needs to say what the difference is between.

Please also note the supplement to this comment:

<http://www.earth-syst-dynam-discuss.net/6/C431/2015/esdd-6-C431-2015-supplement.pdf>

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