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Interactive comment on "Overview of the Global Monsoons Model Inter-comparison Project (GMMIP)" *by* Tianjun Zhou et al.

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Dear William,

Thank you for your constructive comments. For reading our responses easily, we copied your comments in italic.

This paper presents a high-level overview of outstanding issues in monsoon variability, then proposes a series of climate model integrations that might be used to better understand the causes of monsoon variability. The introduction is well-written and concise, and does a particularly nice job of quickly summarizing what is known and not known about the coupling between regional and global-scale variations in monsoon circulations. The idea of a model intercomparison focusing on monsoons is well-motivated and compelling, and I am sure that new understanding will be generated by this work.

But some aspects of the experimental design should be clarified and perhaps modified. The "orographic perturbation" experiments do not seem designed to address scientific questions for which there remains considerable uncertainty, and there is some lack of clarity in the associated methodology. The possibility that model bias may interfere with the ability to draw conclusions should be given more consideration. I list more details on these major issues below, along with some minor technical details.
Major scientific issues:
1. Most of the proposed "orographic perturbation" experiments are not appropriately designed to test any hypotheses for which there exists considerable uncertainty. There are several key issues here: a. It is widely agreed that eliminating all elevated topography from climate models results in a dramatic weakening and southward shift of South Asian monsoon rainfall; this was shown in Hahn and Manabe (1975), Prell and Kutzbach (1992), Boos and Kuang (2010), Wu et al. (2012), and others, with no disagreement amongst those papers. So it seems strange to devote simulations by such a large number of modeling groups to verifying this well-accepted result.

Response:
Thanks for the comments. While the large-scale response of eliminating all elevated topography from climate models is almost the same among the published papers, regional scale features are different. The aim of the "orographic perturbation" experiment is to quantitatively understand the regional response to the orographic perturbation from both the thermal and dynamical perspectives. Because the model dynamics and physics are different among the CMIP6 models, the response in each model may be different across temporal and spatial scales. The results will be very helpful to quantitatively understand the topography effect on the atmosphere and associated physical processes, such as the distribution, intensity, and frequency changes in the precipitation over wide monsoon regions. In addition, the"orographic perturbation" experiments are listed as Tier-3 experiments, viz. the lowest priority, although we wish a large number of modeling groups would do the experiments, the experiments probably will be done only in several modeling centers majored in monsoon research.

b. The manuscript overstates the controversy concerning ways in which Asian orography affects the monsoon. I would agree that there is a widespread belief that controversy exists, but if one actually reads the recent literature one will find little actual disagreement. Wu et al. (2012) clearly state that elevated orographic heating is primarily important for a "northern branch" of the South Asian monsoon that exists north of 20N and lies "along the southern margin of the Iranian Plateau-Tibetan Plateau in the subtropics." That view is very consistent with Boos and Kuang (2010), who showed that Tibetan Plateau surface enthalpy fluxes indeed produced a large fraction of summer rainfall along the plateau's southern margin, but made negligible contribution to the interhemispheric monsoon circulation and the main rainfall maxima, both of which lie south of 20N. Boos (2013, CLIVAR Exchanges) reviewed the agreement between Wu et al. 2012 and Boos and Kuang 2010, and discussed the lack of disagreement in recent literature concerning the influence of topography on the South Asian monsoon. So while it would be interesting to see results from the proposed orographic perturbation experiments, I think the authors should seriously consider whether it is desirable to use such a large amount of modeling and computational resources to examine something that is not fundamentally controversial when one reads the literature closely.

Response:
Thanks. In the revised version, we replaced the statement of "the relative roles of the two effects remain controversial" with "the relative roles of the two effects deserve further investigation" **(P4, L2)**. The statement in section 5.4 of "remains debatable" is replaced with "needs further study" **(P10, L25)**. In addition, the model resolutions in most old GCMs (100km or so) are not high enough to resolve the complex topography over south slope of TP and we hope the higher resolution models of CMIP6 could better resolve these effects. Recent progress indicates that the surface entropy over northern India is quite sensitive to the large-scale thermal forcing of TP and cannot be solely attributed to the barrier effect of TP (Wu et al., 2015, Climate Dynamics; He et al., 2015, Scientific Reports). Since these results may be model-dependent, we hope other modeling centers can also do the experiments. In addition, this experiment

is listed as Tier-3 experiment (viz. low priority) and honestly we expect only a few modeling centers that have specific interest in the monsoon to do the experiment.

c. Turning off sensible heat fluxes from all Asian topography higher than 500 m in the proposed "TIP" domain amounts to imposing a huge negative heat sink over roughly half of the Asian continent. The authors propose to suppress sensible heat fluxes from most of the red and orange regions in the "Asia" box in Fig. 5, which includes parts of continental India as well as much of China and Mongolia âĂŤ regions not thought to be involved in "elevated heating" when it is discussed in the monsoon literature.
In other words, it would be surprising if the monsoon did not weaken when surface sensible heat fluxes were suppressed over one-third to one-half of Asia, whether or not that terrain was elevated! These experiments thus don't clearly test the idea that elevated heating from Tibet or from the slopes of the Himalaya are a key forcing for the South Asian monsoon (and as stated above, both Wu et al. 2012 and Boos and Kuang 2010 already agree that elevated heating from those regions forces precipitation along the Himalayas but not the interhemispheric South Asian monsoon circulation). Finally, modern theory for tropical atmospheric dynamics places surface latent heat fluxes on the same footing as surface sensible heat fluxes in their influence on large-scale flow (e.g. see theories for convective quasi-equilbirium, reviewed by Emanuel et al. 1994 QJRMS, or theories for the energy flux equator discussed by Kang et al. 2008, J. Climate p. 3521), so it is unclear why there should be a special emphasis on surface sensible heat fluxes. I thus suggest the authors reconsider the design of the TIP-NSH experiment.

Response:
We agree that the latent heat fluxes are very important over the tropical oceans to produce low level instability for moist convection. However over the land in Asia, the link of local evaporation and precipitation is relatively weak (He et al., 2015, Scientific Reports), and the sensible heat flux is the major term which causes the PV anomaly at the surface to draw water vapor from the Indian Ocean. Meanwhile the latent heat

flux (evaporation) is also affected by the SH. Therefore the SH is regarded as the main driver of the behavior of the low level atmosphere and possibly also the upper troposphere and lower stratosphere (Wu et al., 2016, Science China Earth Sciences). The importance of elevated heating has been emphasized by He (2016, Theor. Appl. Climatol.), and the responses can be obtained from the differences between amip-TIP-nosh and AMIP (which is the control run). Again, to examine whether the responses are model dependent, we hope other modeling centers will do the experiment. This experiment is listed as Tier-3.

d. The methodology for eliminating the surface sensible heat flux in the orographic perturbation experiments is unclear and may lead to different approaches being taken by different modeling groups. The manuscript states that, as in Wu et al. (2012), surface sensible heating will be suppressed by setting "the vertical diffusive heating term in the atmospheric thermodynamic equation" to zero. But does this mean that heat will accumulate just above the surface and will not diffuse upward through the boundary layer, so that the column will eventually become unstable to dry convection or to grid-scale overturning? And how exactly does suppressing this vertical diffusion alter the land surface energy budget . . . e.g. will land surface temperatures and longwave emission become very high because heat cannot diffuse away from the land surface? Participating models may have dramatically different methods of parameterizing the subgrid scale vertical redistribution of surface sensible heat fluxes. If one wanted to suppress surface heat fluxes (which is debatable, see previous point) it would seem better to prescribe a heat sink in the bottom layer of the atmosphere that is exactly equal to the surface sensible heat flux at that time step. Then the net land surface energy budget will not be directly altered, the surface sensible heat flux will not heat the atmosphere, and one does not need to worry about the various ways in which different models represent vertical diffusion.

Response:
The sensible heating (vertical heat diffusion in atmosphere) is set to zero for each step

in the atmospheric model **(P7, L6-8)**, and will not be accumulated at the model surface. The sensible heat flux in the atmospheric model is zero (Fig. R1a), while the sensible heat flux in the land model continues to be updated (Fig. R1b), and the procedure will not affect the land surface energy conservation (Fig. R1c), and the net surface radiation balance on the surface of the atmosphere model is reasonable (Fig. R1d).

2. This manuscript seems to assume that model bias will not compromise the ability of the proposed experiments to provide insight on the cause of monsoon variability. For example, the authors state at top of p. 6 that comparing prescribed SST integrations with fully coupled integrations will allow the authors "to determine the importance of SST variability to long and short-term trends in the monsoons." But later they state that "simulations with specified SST generally have low skill in simulating the interannual variation of the summer precipitation over global monsoon domains". So it is very possible that the specified SST integrations will have such large bias that it will not be possible to use them to understand long- and short-term trends. This problem is difficult, at best, to fix, but I would have at least liked to see more acknowledgment of this problem and more attempts to gauge model skill through comparison with observations. For example, the authors state that comparison of pre-industrial control simulations with the Tier-2 experiments will "allow us to determine which parts of apparent decadal variations in the monsoons are caused by underlying SST, and which are forced solely from externally driven sources, such as volcanic emissions." But what if all of the models have a strongly biased response to volcanic emissions? Some users of the GMMIP archive might compare with observations and stratify models by their skill in simulating, e.g., the response to Pinatubo, but this cannot be assumed âĂŤ there are numerous examples of model intercomparisons in which every model in an ensemble is treated equally. The bottom line is that I suggest more discussion of the possibility that model bias will make it difficult to draw conclusions about causation, and more concrete proposals for how to deal with this bias if it is found to exist. Otherwise one runs the risk of gaining little new understanding from the proposed large amounts of simulation.

Response:
Thanks for your comments. Yes, climate models have been showing and will continue to show bias in many aspects. We have to balance the needs of scientific research and the performances of the current state of the art climate models. A multi-model intercomparison approach is a useful way to provide insights for reducing the uncertainty due to model bias. This is the reason why MIPs for CMIP6 are needed. As suggested, the impact of model bias on the conclusion should be discussed. We have added a paragraph in the revised manuscript:

"We acknowledge that attention should be paid to the model bias in the analysis of model outputs, although multi-model ensemble/intercomparison approach is a useful way to better quantify the uncertainty related to model bias" **(P10, L2-3)**.

In addition, the analysis of GMMIP will focus on both monsoon circulation and monsoon precipitation. Although SST-driven AGCM simulations generally have low skill in the simulation of monsoon precipitation over the Asian-Australian monsoon domain due to the neglect of air-sea coupling and model bias, the large scale monsoon circulation changes have significant skill at both interannual and inter-decadal time scales. This has been demonstrated in many published papers. Thus at the top of p. 6 of the original manuscript, we revised the statement: "to determine the importance of SST variability to long and short-term trends in the monsoon circulations and the associated precipitation" **(P6, L5)**.

Minor technical issues:
3. After the introduction, the manuscript quickly becomes somewhat difficult to read for those who are not deeply familiar with the CMIP terminology. This could be easily remedied by clearly explaining the meaning of various terms when they are first introduced. E.g. what are the "DECK" experiments? What is a "pacemaker" experiment? It is possible for the reader to figure out what is meant by a pacemaker experiment,

but a clearer statement and references to literature discussing the history and caveats of pacemaker experiments would be very helpful. On p. 4, line 31 the terms "Tier-1" and "Tier-3" are used without being previously defined, and I was confused about what these terms meant until they were defined a full page later.

Response:
Thanks. We have revised as suggested. In addition, a paper of CMIP6 design which clearly described the CMIP6 core experiments such as DECK has been cited **(P4, L29):**

Eyring, V., Bony, S., Meehl, G. A., Senior, C. A., Stevens, B., Stouffer, R. J., and Taylor, K. E.: Overview of the Coupled Model Intercomparison Project Phase 6 (CMIP6) experimental design and organization, Geosci. Model Dev., 9, 1937-1958, doi:10.5194/gmd-9-1937-2016, 2016.

4. Unclear what is meant by "model climatology" on p. 6, line 8. Is this a cyclic seasonal cycle of daily resolution, or the full, interannually varying daily time series of SST from the coupled CMIP6 integration?

Response:
It is the former. We have clarified this in Appendix I **(P12, L12)**.

5. Equation (1) is introduced in method (b), but it also defines the "constructed SST" introduced in (a), with the linear decay of the relaxation time in the buffer zone already "built in". My point is that it would seem more clear to introduce equation (1) in method (a).

Response:
We added an equation for method (a) to make the difference between (a) and (b) clearer **(P12, L15-16)**.

6. Page 12, line 9: isn't 50 m a very deep mixed layer depth for the East Pacific, which *is the main region of interest for the "IPO" pacemaker experiment? This could result in a factor of 2 or more difference in the effective restoring times for SST in the IPO and AMO pacemaker experiments. Would at least be nice to see some mention of why it's acceptable to use a 50 m mixed layer depth in the East Pacific.*

Response:
The choice of 50m for hist-resIPO is based on Kosaka and Xie (2013, Nature). For the hist-resAMO experiment, we use a restoring time of 2 months following Boer et al. (2016) **(P12, L26)**.

7. The box marked around the "Asia" domain in Fig. 5 does not agree with the coordinates given in Table 2. Should there be agreement? If not, what do the boxes in Fig. 5 represent?

Response:
The domain description has been revised. Now the text is consistent with the figure **(P25)**.

On behalf of the co-authors,
Xiaolong Chen

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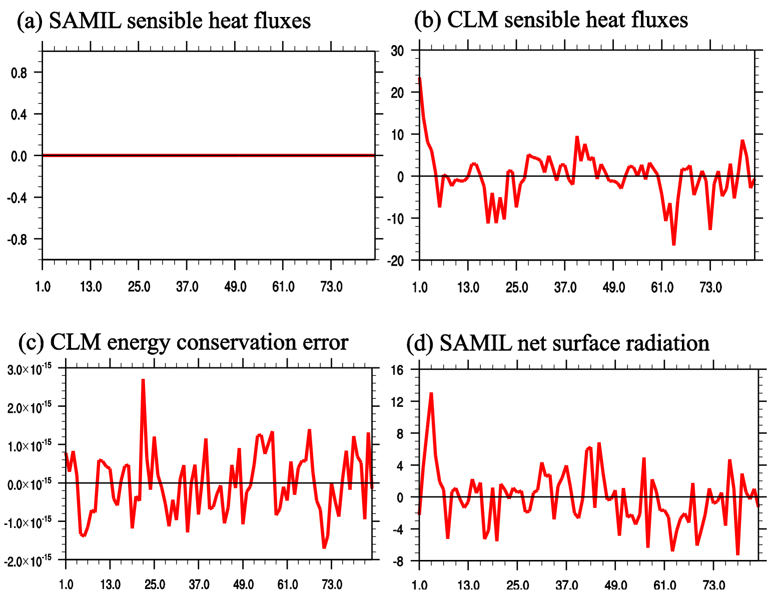

Fig. R1 The evolution (the abscissa is in units of months from the startup run of the model) of various variables anomaly in the IAP/LASG atmospheric model (SAMIL) and land model (CLM) in the amip-TIP-nosh experiment respectively: (a) Sensible heat flux in atmosphere model, (b) sensible heat flux in land model, (c), the error in energy conservation in land model, and (d) the net surface radiation in atmosphere model.