

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

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Review of “Overview of the Global Monsoons Model Inter-comparison Project (GMMIP)” by T. Zhou et al.

Summary:

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.

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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.

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

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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.

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 — 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-equilibrium, reviewed by Emanuel et al. 1994 QJRM, 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.

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

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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.

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 ap-

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parent 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 — 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.

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.

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?

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

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(a).

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.

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?

Signed, William Boos

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