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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Investigation of the "Elevated Heat Pump" hypothesis of the Asian monsoon using satellite observations" *by* M. M. Wonsick et al.

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The authors attempt to evaluate the "Elevated Heat Pump (EHP)" hypothesis using new satellite data. Using the same technique (composite analysis) as in Lau and Kim(2006), the authors found the supporting evidences of EHP as well as many other evidences do not support EHP. I found the authors' interpretation of results are not convincing. Followings are major concerns that I suggest the authors to address in the final version.

1) EHP is originally established based on modeling study. Lau and Kim (2006) was the first observational evidences supporting the EHP, based on composite analysis of four

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high AOD years and four low AOD years over northern India. On the other hand, Wonsick et al. (2013) used only two high and low AOD years to make composite. The authors' justifications are (a) the data used in their study has higher temporal and spatial resolution, and (b) the smaller higher contrast is better because the predicted patterns should be evident during extreme aerosol years. First of all, the benefit of higher spatial, temporal resolutions is minimal when monthly mean is used for analysis. Higher temporal, spatial resolution could be beneficial if the data is used to look at EHP related patterns over high terrain area of the southern slope of the Tibetan Plateau. Second, more importantly, I cannot agree with the authors' argument that smaller, strong case provide better, clear signal. Indian monsoon is very complicated, multi-scale phenomena, and there are a lot of known strong climate factors other than aerosols. To filter these signals and show aerosol-induced signal, we need more cases, not less. Lau and Kim (2006) used four cases, and Wonsick et al. (2013) used two cases. I do not see how composite with smaller cases can do better than that with more cases.

2) For better comparison, I would like to suggest the authors to choose the same season/month to compare with Lau and Kim (2006). For example, Lau and Kim used April-May to define AOD level, but Wonsick et al used MAM. Also, Wonsick et al. did not choose same months as Lau and Kim for rainfall map and temperature anomalies.

3) The authors used the frequency of occurrence of convection to compare with rainfall shown in Lau and Kim. There are correlated, but not the same since rain rate has strong regionality.

4) In section 5.1, the authors raised questions on the accuracies of model. This is a real issue. However, the differences among three modeling studies compared in this section are not necessarily representing the inaccuracies of modeling. All three models are different each other. However, I believe there are even bigger differences in aerosol species and optics used in each models. Some model does not have dust. Some has more absorbing aerosols than others. The authors also emphasize the importance of SST gradient (10138, lines 4-5). I don't think that the SST gradient between India

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and Indian Ocean is important to the monsoon. That could be just results of monsoon circulation (oceanic upwelling/downwelling, and mixing) and rainfall (evaporation). It is known that mid-tropospheric temperature gradient between the Tibetan Plateau and Equatorial Indian Ocean is driving force of Indian monsoon, not local SST gradient.

There are many minor mistakes/misunderstanding through out the manuscript. I believe the authors will fix before publication.

Interactive comment on Atmos. Chem. Phys. Discuss., 13, 10125, 2013.