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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.

Anonymous Referee #3

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Dear authors,

The authors have written a manuscript on the important topic of the elevated heat pump hypothesis and its (observed) impact on the South Asian monsoon. The topic is hugely relevant given the large rapidly increasing population of the region and large aerosol emissions due to industrialization and use of cooking fires. The topic is also a controversial one, with many arguments both for and against using both observational and modeling work. It is therefore important that new articles in this field advance the debate.

The manuscript uses newly available data of cloud cover and cloud type derived from Meteosat-5 observations together with reanalyzed temperature and observed precipitation data, compared in a pair each of high and low aerosol years according to AOD.

Unfortunately I have some major concerns with the paper that need addressing. Firstly, only two samples are offered for each of the high and low aerosol years. This may be unavoidable given the availability of these data, but it does prevent an accurate test of the EHP mechanism being performed. This is especially pertinent given the number of external drivers acting on the South Asian monsoon, as well as the complex spatio-temporal evolution of the monsoon, and its variability. Secondly, and perhaps more importantly, no attempt seems to have been made to separate the effects of absorbing and scattering aerosol: AOD alone is used. Given that the EHP hypothesis is specifically related to absorbing aerosols (such as black carbon) then it needs to be high or low loads of this type of aerosol that are used for the compositing. Merely using AOD could mix up more traditional direct-effects relating to sulphate scattering, or indeed aerosol indirect effects that are more prevalent in regions of high sulphate load.

Please see the detailed comments below.

Specific comments: 1. Page 10126, line 4: probably the Himalayan foothills should also be mentioned along with the Tibetan Plateau.

2. Page 10126, line 8-9: one could perhaps replace southwest Asia with the Thar Desert (Rajasthan) and the Arabian Peninsula.

3. Page 10127, lines 1-3: I think the mention of initial conditions here is a misnomer. Time-mean simulations of the monsoon in climate models do contain biases but these are not at all related to initial conditions: at these scales the monsoon is a boundary value problem. Even at the seasonal forecasting scale, evolving boundary conditions play a more important role than initialization. I suggest the focus of this sentence is changed.

4. Page 10127, lines 18-22: another interpretation of the weakened north-south gradient in SST is that it leads to the monsoon circulation (Somali jet) shifting southward, or that the cooler Arabian Sea limits the supply of moisture to the monsoon.

5. Page 10128, line 6: as in my earlier comment, I suggest being more specific about southwest Asia.

6. Page 10128, line 12: much of the black carbon comes from inefficient burning in cooking fires, rather than industrial sources. When multiplied over the large population this is significant.

7. Page 10128, lines 16-19: the sinking region of the Asian monsoon Hadley circulation lies south of the equator, in the Mascarene High. The sinking motion you describe here induced by the EHP mechanism should be referred to as an anomalous sinking motion.

8. Page 10128, line 20 and throughout the manuscript: the word "drawdown" is not commonly used to describe the monsoon. "Withdrawal" is the common term.

9. Page 10128, line 21: is the heat low you are referring to here some anomaly relating to the EHP or the time-mean heat low of the South Asian monsoon? (Your text is not clear.) If the latter, the monsoon heat low resides south of the Himalayan foothills (the monsoon trough), the strongest heat low part being over southern Pakistan.

10. Page 10129, line 1: you should say something on how important or unimportant aerosol indirect effects are for absorbing aerosol such as black carbon (compared to their much more obvious impact with sulphate aerosol, for example).

11. Page 10129, line 8: strictly speaking, it is the heating of the tropopause over the Tibetan Plateau by sensible heating. Similarly on line 14, change "of" to "over".

12. Page 10130, lines 6-16: It is not clear whether or not the work described in this paragraph is from the original EHP work of Lau.

13. Page 10132, line 1: why are extreme-aerosol years relevant for testing the EHP

mechanism? It is only absorbing aerosol that we are interested in for the EHP. The danger is that several other mechanisms could be mixed up here, preventing a fair test of the EHP. Sulphate, for example, would have obvious direct and indirect effects that may both act to restrict the monsoon, and make it appear as if the EHP doesn't work.

14. Page 10134, lines 7-9: we would only expect higher UT temperatures over the Tibetan Plateau using the EHP mechanism in high absorbing aerosol years.

15. Page 10134, lines 9-11: the second testable aspect of the EHP deems a more in-depth look at monsoon behaviour. I don't see why even if the EHP works it would enhance northern Indian rainfall in May. The typical onset date for the monsoon in India is around June 1 (with a standard deviation of 7 or 8 days). However that applies only to Kerala, on the south west coast. As can be seen from a diagram such as http://www.imd.gov.in/section/nhac/dynamic/newnormalonset.jpg provided by the India Meteorological Department, onset dates become progressively later as one moves north and west. The only part of the north that may end up with a May onset is in the far east, in Assam state etc. More typical onset dates in the IGP region range from 10 June to 1 July, so I don't believe we would expect to see any impact on May precipitation in the north.

16. Page 10134, line 19: insert "air over the" prior to "Tibetan Plateau".

17. Page 10135, discussion of Fig. 5: would May not be a better choice to examine the vertical temperature structure, since it is closer to the monsoon onset? Are the results of this figure the same if it is produced as a composite difference of high minus low years, rather than just the high years as in this case?

18. Page 10135, discussion of Fig. 6a/b: I refer to my previous point about northern India rainfall in May - this may suggest a problem for how the finer detail of the EHP hypothesis, but not its general mechanism. In addition, one could argue that Fig. 6b does indeed show enhancement of monsoon convection. Since one is expecting the monsoon to begin over south India at the start of June (and in late May in the Bay

of Bengal), then this diagram does suggest an enhancement of the monsoon. This wouldn't be inconsistent with an EHP-type mechanism. Generally however I think the sample size is small.

19. Page 10136, line 24: it is only contrary to the hypothesis if the aerosol loading is absorbing. If there are scattering components such as sulphate then more immediate direct and indirect effects may act to weaken the monsoon. It would be worth citing the recent paper of Bollasina et al. (2011, Science) "Anthropogenic Aerosols and the Weakening of the South Asian Summer Monsoon".

20. Page 10136, lines 26-29: similar to the onset, I think August is the wrong month to look at for changes in the withdrawal. I suggest mid-to-late September.

21. Page 10136, discussion of Figs. 6-8: since we are interested in the EHP then it may be more useful to examine rainfall of which many products are available. The frequency of convection diagnostics used here would be more suited to looking at indirect aerosol effects (perhaps for sulphate).

22. Page 10138, lines 4-6: due to upwelling feedbacks in the coupled ocean-atmosphere system, temperature gradients are not directly related to precipitation change. See for example Levine and Turner (2012) Climate Dynamics 38 and references therein.

23. Page 10138, discussion of microphysics: it would be worth mentioning that we expect these effects certainly from sulphate aerosols. What do the cited references of observational measurements of the region say about relating absorbing aerosol such as black carbon to cloud microphysical effects?

24. Page 10140, conclusions: I refer to my earlier comments on timing for points 1, 2, 4 of your conclusions.

25. Page 10141, line 15: the EHP effect would only be most observable in these years if the aerosol loading is coming mainly from absorbers.

26. Table 1: June convection may be better.

27. Figs. 4, 6, 7, 8: what are the units of frequency: occurrence per month?

28. Fig. 6: the state boundaries should be more clearly defined, especially in panel a, given that no latitude/longitude axes are given.

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