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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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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

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

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Interactive Discussion

Discussion Paper

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.

Authors-Response

We have addressed the two major concerns of this reviewer. We have extended the analysis period to 13 years by augmenting the Meteosat-5 convection data with rainfall data from GPCP from 2000 – 2012. We have expanded the aerosol analysis to include AOD from both MISR and MODIS from 2000 – 2012, as well as SSA data from the MERRA aerosol reanalysis to confirm that the aerosols are, indeed, absorbing. We have provided additional information from Dey and DiGirolamo (2010), who prepared a 9-year climatology of aerosol properties over the Indian subcontinent and reported a seasonal increase in absorbing aerosols from dust and biomass burning over the IGB during the pre-monsoon season.

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Reviewer - Comment

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.

Authors-Response

Done

Reviewer - Comment

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

Authors-Response

Done

Reviewer - Comment

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.

Authors-Response

In our revisions, this discussion of model deficiencies was deleted.

Reviewer - Comment

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

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that the cooler Arabian Sea limits the supply of moisture to the monsoon.

Authors-Response

We thank the Reviewer for this additional information. We have added it to the discussion in the second paragraph of the introduction.

Reviewer - Comment

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

Authors-Response

Done

Reviewer - Comment

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.

Authors-Response

Changed to “black carbon from fossil fuel combustion”

Reviewer - Comment

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.

Authors-Response

Done

Reviewer - Comment

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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.

Authors-Response

Done

Reviewer - Comment

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.

Authors-Response

This is an anomaly relating to the EHP as described in section 3.2 of Lau et al. (2006). We have clarified this in the text.

Reviewer - Comment

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).

Authors-Response

As stressed several times by this Reviewer, “It is only absorbing aerosol that we are interested in for the EHP” (comment #13). We have done additional work to show that the aerosols during the period of interest are indeed absorbing. We feel it is out of the scope of this study to contrast the indirect effects of sulphate and absorbing aerosols.

Reviewer - Comment

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".

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Authors-Response

The first reference to this process has been changed to “over”. The second has been deleted in our revisions.

Reviewer - Comment

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.

Authors-Response

We have moved this discussion to section 5.2 of the revised manuscript. We believe it is now clearer that this work came from the observational study performed by Lau and Kim (2006), not the original modeling study (Lau et al. 2006).

Reviewer - Comment

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.

Authors-Response

We agree with the reviewer that it is important to make the distinction that we are testing years with extreme amounts of absorbing aerosols. The particular sentence referenced here is no longer in the revised manuscript, but throughout the manuscript we have now made the distinction that we are looking for signals during high absorbing aerosol years.

Reviewer - Comment

14. Page 10134, lines 7-9: we would only expect higher UT temperatures over the

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Tibetan Plateau using the EHP mechanism in high absorbing aerosol years.

Authors-Response

We have changed the text to clarify that we are analyzing years with high amounts of absorbing aerosols, as also explained in Comment #13.

Reviewer - Comment

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.

Authors-Response

We have addressed this issue in our study because it was raised in the original hypothesis. The following is an excerpt from section 3.1 of Lau et al. (2006):

"Notice that in May (Fig. 4c), over central and northern India (15–25° N), due to solar dimming, the air near the surface cools more than the air immediately above it. As a result a stable air mass exists in the lower troposphere, which is likely to inhibit convection. However, the EHP effect appears to be able to by-pass the stable air mass, by drawing in warmer and moister air from the south above the stable air mass (above 700 hPa) and induce convection over the foothills of the Himalayas to the north of the stable air mass."

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Later in section 3.2, Lau et al. (2006) state:

"As a result of the aerosol induced upper troposphere warming over the TP, and the lower-level heating and forced ascent over northern India, significant increase in rainfall over northern India ($\sim$20° N) is found in May, suggesting an advance of the monsoon rainy season (Fig. 5d)."

We agree with the reviewer that it seems unusual that the EHP effect could advance the onset date of the monsoon in northern India so drastically. In a later publication (Lau and Kim, 2011*), the authors downplay the component of the hypothesis that predicts the early onset of the monsoon precipitation in northern India. They state:

"...the possible enhancement of rainfall over the foothills of the Himalayas in May is only a possible early signal which is important for the local population but not critical to the entire outcome of the EHP. We submit that such an increase is still not proven by either NB or LK06, because of the use of coarse resolution GPCP rainfall data set in both analyses. To detect the early response of rainfall in May, there is a need to use high resolution rainfall data such as TRMM (see Figure 1) as well as in situ observations with high temporal resolution to resolve the orographically generated rainfall along the narrow strip over the Himalayan foothills, downstream of the increased lowâĂŘlevel meridional flow toward the foothills."

Since we had the high resolution convection data from Meteosat-5, we felt it was worthwhile to report what could be seen in the data.

*Lau, K. M., and Kim, K. M.: Comment on "'Elevated heat pump' hypothesis for the aerosolâĂŘmonsoon hydroclimate link: 'Grounded' in observations?" by S. Nigam and M. Bollasina, J. Geophys. Res., 116, D07203, doi:10.1029/2010JD014800, 2011.

Reviewer - Comment

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

Authors-Response

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Done

Reviewer - Comment

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?

Authors-Response

We have done as this Reviewer suggested. We present now the vertical temperature structure for the months of May for all of the high aerosol years in Figure 7 and for the low aerosol years in the Supplement, as shown in Figure S1.

Reviewer - Comment

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.

Authors-Response

As stated in our response to Comment #15, we addressed this issue because it was part of the original hypothesis and because we have high resolution convection data to augment the lower resolution data used in previous studies. In our revised conclusion section, we do note that enhanced convection is visible in some parts of the Himalayan foothills in the Meteosat-5 data, but not in the GPCP data.

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Reviewer - Comment

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".

Authors-Response

This particular sentence is not included in the revised version of the manuscript, but in the general discussion of precipitation during July, we emphasize that we are looking at the behavior for years with high loads of absorbing aerosols.

Reviewer - Comment

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.

Authors-Response

In the revised manuscript we do not address the withdrawal issue.

Reviewer - Comment

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).

Authors-Response

We have done as suggested by this Reviewer and added the rainfall information for the extended period of study of 13 years.

Reviewer - Comment

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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.

Authors-Response

We thank the Reviewer for this comment. After closer examination of Ramanathan et al. (2005) and Meehl et al. (2008), we see that they attribute the reduced monsoon precipitation to the cooler ocean temperatures themselves rather than the reduced temperature gradient. We have changed the text accordingly.

Reviewer - Comment

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?

Authors-Response

We have substantially revised this discussion and this comment now does not apply to the new text.

Reviewer - Comment

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

Authors-Response

This was addressed previously where these comments appeared first.

Reviewer - Comment

25. Page 10141, line 15: the EHP effect would only be most observable in these years

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if the aerosol loading is coming mainly from absorbers.

Authors-Response

This sentence does not appear in the revised version.

Reviewer - Comment

26. Table 1: June convection may be better.

Authors-Response

In June, more aerosol washout occurs and we believe it is more appropriate to analyze May, while the aerosol build-up is still strong.

Reviewer - Comment

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

Authors-Response

Yes, it is per month, and it has been added to the legends.

Reviewer - Comment

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

Authors-Response

We have added the latitude/longitude labels.

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