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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Spatial distributions and seasonal cycles of aerosol climate effects in India seen in global climate-aerosol model" *by* S. V. Henriksson et al.

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The impact of anthropogenic aerosols on the South Asian summer (southeastern) monsoon has recently attracted attention from the climate community and become an active research topic. This paper reports yet another effort to use climate models including aerosol representation to study Systems impact. In the study, Data authors used the ECHAM5-HAM model. A series of simulations were designed and conducted in an attempt to derive different monsoonal responses to various aerosol forcings among other factors such as SST gradient reduction. Unfortunately, the methodology adopted in this study, i.e., to use a climate-aerosol model with fixed SST is inadequate. Discussions

to address several emphasized issues including circulation and precipitation change forced by aerosol effects. The authors must have already realized this weakness and, as a matter of fact, they clearly indicated it in Section 2. The reason for adopting such a method offered in the paper, i.e., lack of computational resource needed for conducting coupled model runs, however, is not appropriate. Therefore, as it stands now, the paper contains conclusions drawn with inadequate methodology and thus is not proper for publication in ACP. If conducting long integrations with coupled ocean model is indeed not feasible, the authors could use the current configuration to address adequate issues such as the aerosol distribution and radiative forcing without considering responses. Thus, I would encourage the authors to make a major revision to the paper, at least to shift the emphasis of analyses (e.g., on Figure 13 and related results).

We thank Anonymous Reviewer 2 for feedback. The major concerns of the Reviewer are taken into account both by making long mixed-layer ocean model integrations in addition to those with fixed SSTs and by expanding on the correlation interpretations (such as in Figure 13 in the original manuscript), devoting a full section to them, now Section 6.

To comment further on the methodology, when a fixed SST configuration is adopted, the ocean surface becomes stiff to atmospheric perturbation, so does the surface latent heat flux (evaporation). Aerosol forcing is often local. However, the monsoon circulation is powered by large-scale forcing where ocean-land temperature and energy contrast play a major role (i.e., large-scale stability) and the response and feedback to aerosol forcing from ocean in various scales are critical to understand the impacts of aerosols on monsoonal circulation and precipitation. This has been widely discussed in literature. Using a fixed SST configuration and also integrate the model for only 10 years leave a lot of rooms for uncertainty regarding the results. There are a few other issues in the analyses, such as using a single rainfall aggregate rather than precipitation pattern in discussion. Comparing to the basic methodology problem, these are rather secondary.

These issues have now been addressed by making 50-year simulations with a mixed-layer ocean, which responds to the aerosol forcing. The remaining limitations are discussed, most importantly that the mixed-layer ocean model can not take into account adjusted horizontal and vertical heat fluxes in the ocean as well as the remaining uncertainty due to the complexity of the problem. The simulations have been extended to get more statistically reliable results. Precipitation anomaly patterns are also now shown for the mixed-layer ocean simulations.

In addition, reanalysis data and prescribed SST should already reflect system responses to anthropogenic forcing including that of aerosols. The aerosol forcing applied in the model, therefore, is an additional forcing to the system and the responses would only be useful to evaluate the sensitivity of the system to the forcing, not adequate for addressing the real response of the system in particular comparing to observations. Actually, the simulations with assumed cooling over northern Indian ocean would be more close to the actual situation under aerosol forcing (note that the total aerosol forcing would lead to surface cooling regardless whether absorbing aerosols exist or not). The comparison between SSTMODIF and ZERO would be more close to actual response to aerosols, although the model would still need to be integrated long enough to allow the system to reach equilibrium.

The prescribed SSTs in their default form in the model are taken from a coupled atmosphere-ocean simulation including greenhouse gas forcing, but not aerosol forcing, as described in Section 2. Therefore the suggested "double-counting" of aerosol forcing is not an issue. As for longer integrations, such have now been done with a mixed-layer ocean model reaching equilibrium with that setup. The mixed-layer ocean results are now reported as the results with the largest amount of aerosol effects taken into account.

The paper is also suffering from lack of focus in discussion. The authors barely discussed the purpose along with physical/dynamical background and processes surrounding these simulations, and most importantly, in the discussions of results. The

discussion lacks depth and thus leaves many questions unanswered. For example, why the total radiative flux anomaly appears positive in the summer due to anthropogenic aerosols? Why the modeled seasonal cycle of BC surface concentration appear to be different than observations? Why the indirect effect causes positive TOA anomaly in wintertime shortwave radiation?

Discussion and motivation has been significantly widened in response to all the comments by the four reviewers. The total radiative flux anomaly is positive for some months in the summer mostly due to longwave feedbacks involving clouds, now discussed in Section 4. The modeled seasonal cycle of BC surface concentrations shares many similarities with the observations including a drop in monsoon months and higher concentrations in winter as discussed in the text and the multi-year mean concentrations are mostly within each others variability. However, some discrepancy also remains, which is probably to a large part due to resolution and boundary layer issues at the Himalayan slopes. The shortwave forcing is probably a smaller negative number in the wintertime with the indirect effect included because of higher precipitation and consequently stronger wet removal in that model setup.

Certain modeling issues are also worth explanation. The authors mentioned that the no-absorbing-aerosol run was simply configured by setting the single scattering albedo to 1. Did this apply to the bulk of aerosols or just BC? Due to the difference in extinction coefficient, this simple configuration could cause different total forcing. Also, what was the assumption regarding cloud radiative properties in the runs excluding indirect effect? When the indirect effect was included in the model, was the scavenging of aerosols during activation included? These are needed details for the reader to understand the results regardless the emphasis of the analysis is about climate response or aerosol forcing.

The answers to all these detailed questions can now be found in Section 2.

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