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Comment on acp-2021-969

Jonathon Wright (Referee)

Referee comment on "Tropospheric warming over the northern Indian Ocean caused by South Asian anthropogenic aerosols: possible impact on the upper troposphere and lower stratosphere" by Suvarna Fadnavis et al., Atmos. Chem. Phys. Discuss., https://doi.org/10.5194/acp-2021-969-RC2, 2022

Overview:

Fadnavis et al. discuss the broader climatic impacts of eliminating South Asian emissions, examing the atmospheric response from pole-to-pole and upward into the stratosphere. The results stimulate a number of interesting questions and I enjoyed reading the paper, but still there are some logical and contextual gaps that need to be addressed, especially in section 3. These are perhaps more important because the results are based on integration of a single climate model under different emissions scenarios. The simulations are very much worth analyzing and reporting, but the presentation leaves me skeptical of some of the conclusions for this model specifically along with the generalizability of the results to the natural atmosphere. Addressing questions of generalizability is beyond the scope of this paper (though I appreciate the discussion along these lines in section 3.1), but the reliability of the conclusions for this model system can be addressed more comprehensively.

In addition to my suggestions for this paper below, I have tried to outline some ideas and questions that inspired me while reading the paper. I set out to be brief but did not always succeed. The authors should take these suggestions not as "I want you to include all of these in this paper" but rather "you should make this paper more coherent, and may want to consider these ideas for future work". If the authors want to discuss any of these ideas further, they are very welcome to contact me! Please see also optional editorial suggestions in the annotated draft.

General comment 1: The concept of the aerosol-induced secondary circulation introduced in the paper is an intriguing one, but the description of this is unclear and potentially misleading. If I were to only read the abstract or conclusions, I would imagine a large anomalous overturning that links the convective regions over the Bay of Bengal and the Arabian Sea all the way to the Southern Hemisphere. However, this picture does not match either the circulation response as illustrated in Figure 5 or the background circulation during March-May. In particular, Figure 5 does not support the statement "In the UTLS, [the aerosol] are further transported to the southern hemisphere and downward into the troposphere." I can see a little bit of what you are talking about in the supplementary figure, but I am skeptical of this description and it needs to be better justified. I'll outline an alternative hypothesis below, both to illustrate why more justification is needed and as an idea that might be interesting for you to explore further.

The ITCZ in the Indian Ocean region is still located in the Southern Hemisphere in March and early April, migrates north to near the equator from late April to early May, and then slowly proceeds north into the Asian summer monsoon region over late May and most of June (see, e.g., Figure 2 of Schneider et al 2014). With this context, the tropical circulation responses look more like a weakening of the tropical overturning Hadley-type circulation in this region. How could the aerosols produce this response? My guess: the aerosol enhancements in the tropics are mostly located near the surface. The cross-equatorial flow that feeds the ITCZ is anticyclonic in the hemisphere containing the ITCZ, and therefore must be cyclonic in the upstream hemisphere. The effect of the relatively shallow aerosol layer on radiative heating will represent an anticyclonic vorticity source upstream, which should tend to weaken moisture supply from the Northern Hemisphere, and might also delay the northward propagation of the ITCZ in May by making the environment just north of the equator less favorable for the ITCZ to move into. Hoskins et al. (2020) and Hoskins and Yang (2021) provide very clear explanations of these processes for the solstice-season ITCZs that might be useful.

Aerosol changes in the subtropical Southern Hemisphere might also have an impact, maybe by disrupting the effects of extratropical waves on intraseasonal active and break phases along the March-April ITCZ (much of the tropical MJO appears to be driven by moisture advection modulated by SH wave activity; see e.g. Li 2014). This hypothesis would be consistent with the opposing vertical velocity responses in the 60-75E and 75E-90E bands around 40S. We cannot tell the characteristics of that anomaly in the extratropical wave, but you could check it in maps of upper level geopotential height and winds to see if it matches the hypothesis articulated by Li (2014). From what I've read I think this explanation for the Indian Ocean ITCZ change is more likely than the first, but both might play some role.

In addition to the lat-lon distribution of upper-level response in the southern hemisphere, there are two relatively straightforward things that you could do to check these possibilities. First, compare the circulation response month by month to the seasonal mean response. Maybe even step through the full seasonal cycle with reference to the changing location of the ITCZ, since the solstitial dynamical responses might be easier to interpret and can then be linked to the springtime transition. Second, you might look at the evolution of the circulation response over time. If I have understood correctly you start all simulations from the same inital dynamical conditions, with one year of spinup to introduce and equilibrate to the emissions perturbations. My guess is that the aerosol changes in the southern hemisphere should be accumulating over time, and would be mainly linked to pulses of supply via that boreal wintertime cross-equatorial flow into the ITCZ. If this is indeed the case, then you should see a strong adjustment in the circulation over the first couple of years, including the spinup year. If either of these helps to explain the changes, then it might be worth including them in either the main text or supplement.

The question is then: how to explain the protrusion of increased aerosol (especially BC and OC) in the tropical upper troposphere, which then seems to get sucked toward lower levels in the southern hemisphere tropics. Here I think it is again helpful to remember that we are looking at circulation anomalies and that the ITCZ may be weaker but there is still a lot of convection there. Another thought is that, if you look at the mass streamfunction for the overturning circulation in the tropical southern hemisphere, there are really two overturnings, one linked to cumulus congestus that diverges around 400-500 hPa and one linked to deep convection that diverges around 200-300 hPa. These patterns in the aerosol are rather reminiscent of that, and so I wonder if it just indicates entrainment into that spectrum of convection a little bit above the glaciation level, which then invigorates the convection in the middle-to-upper troposphere and enhances aerosol wet deposition (the negative anomalies above and below). Here some details of the model are important: is aging of BC and OC represented, increasing the hydrophilic fraction? Are mixed phase clouds permitted, and how is the partitioning of liquid and ice represented in these? What are the roles or efficiencies of BC and OC as CDNC and IDNC?

This is all just speculation, and it's kind of strange to think about an ITCZ that is somehow both weaker/stabilized in the lower troposphere and more intense/destabilized in the upper troposphere. However, the main point is just that you need more justification and explanation to support your description of the 'secondary circulation' response.

General comment 2: It is difficult to keep track of all the comparisons in section 3.2. Part of this is the presentation, which is very full of numbers, and part of this is the lack of detail about differences in measurements or methologies across the studies being referenced. For example, the three studies mentioned in L259-263 report in-atmosphere aerosol forcing that is an order of magnitude larger than yours. Is this just the difference in winter versus spring, e.g. in aerosol loading or solar zentith angle or both? Was there particularly strong burning during the years they measured that enhanced the relative loading of black carbon? Were they just overestimating the fraction of BC in the column? It would benefit the paper a lot to include more context and comparison here beyond just the quantitative results.

General comment 3: Section 3.4 is missing some important context. For example, I was well into the section before I realized that the heating rates are total heating rates, rather than radiative heating rates. First, I think that some background context would be helpful for the heating rates. How large are these differences in heating rates relative to values in Aerooff or CTL? Where are the changes opposing the mean heating as opposed to acting in the same direction? Are they tendencies in temperature or potential temperature? If temperature, it might be worth considering a switch, since much of the focus is on changes in the UTLS. It would also be great to have more decomposition of the heating rates (i.e., SW + LW + non-rad or SWclr + LWclr + clouds and turbulence). Second, I think background context would also be helpful for the water vapor changes. Since the spatial gradients in water vapor volume mixing ratio are large, changes could be reported in % relative to Aerooff or CTL. This might also be more physically meaningful given the logarithmic dependence of water vapor's greenhouse impact as a function of concentration. Third, many of the interpretations are difficult to judge as a function of simply these Eulerian cross-sections in longitude-pressure. The similarities between the Arabian Sea cross-section and the zonal mean could be taken to mean that 'this slice dominates the response' or they could be taken to mean 'zonal advection is efficient'. In the latter case, how much can we trust some of the meridional features you are highlight, such as the pathway of enhanced aerosol extending southward and toward the surface and its possible effects on water vapor? Finally, transit times to several of the regions you highlight in the stratosphere, both at low and high latitudes are several months at least. You do mention this at one point (L468-469), but I'd recommend to mention it earlier and more often because this is essential context that some readers may not be familiar with. In any case, these long transit times cannot be related to cross-tropopause exchange during spring on the evidence presented in this paper, and indeed the seasonal cycles shown in figure 8 suggest that cross-tropopause transport in this region is pretty weak during March and April. Given these limitations, why focus so much on the stratospheric response in a paper that is rooted in changes over the North Indian Ocean during MAM?

General comment 4: Many of the figure elements are too small for me to discern. It should be possible to improve most of these by modifying axis ranges (e.g. zooming in on the regions that are highlighted in the text), axis styles (e.g. linear to logarithmic or vice versa), and internal elements of the plot (e.g. vectors versus streamlines, density of vectors, etc.).

References:

Hoskins, B. J. & Yang, G.-Y. The Detailed Dynamics of the Hadley Cell. Part II: December–February. J Climate 34, 805–823 (2021).

Hoskins, B. J., Yang, G. â□□Y. & Fonseca, R. M. The detailed dynamics of the June–August Hadley Cell. Q J Roy Meteor Soc 146, 557–575 (2020).

Li, T. Recent advance in understanding the dynamics of the Madden-Julian oscillation. J Meteorol Res 28, 1–33 (2014).

Schneider, T., Bischoff, T. & Haug, G. H. Migrations and dynamics of the intertropical convergence zone. Nature 513, 45–53 (2014).

Specific comments:

- I suggest to specify March-May after "spring" in the abstract.

- There are a lot of regions to keep track of in the paper; it might be good to eliminate all use of "the North Indian Ocean" and always use "the Arabian Sea", "the Bay of Bengal", or "the Arabian Sea and Bay of Bengal"

- General note for region descriptions: South India or North Indian Ocean should use capitalized South and North, but southern India and northern Indian Ocean would generally not.

- L55-57: I am not sure I understand what these numbers mean -- 97% of what? It means that only 3% were in the coarse mode? It would be helpful to clarify.

- L72: Check number formatting here, I think this should be x10^n

- L121: Might be useful to write out these abbreviations (e.g. "HAM") for those interested in the model

- L122-123: Here is another place where simplification might help -- is there a reason to use POM here and OC elsewhere in the manuscript?

- L127: According to prescribed microphysical properties or all aerosol are treated equally?

- L167: There should be a citation for MODIS Terra AOD; probably it is this one: MODIS Atmosphere Science Team: MODIS/Terra Aerosol Cloud Water Vapor Ozone Monthly L3 Global 1Deg CMG, https://doi.org/10.5067/MODIS/MOD08_M3.061, 2017\. This doi citation ahould be used in place of the link because it is fixed to the dataset and version.

- L170: Here too, probably: Diner, David: MISR Level 3 Global Joint Aerosol monthly product V002, https://doi.org/10.5067/TERRA/MISR/MIL3MJTA.002, 2020.

- L178: Are simulated aerosol processes processed to support like-for-like comparison with the model (e.g. cloud clearing)? Could this matter for the validation?

- L190: Can all of the potential sources of bias between CTL and the observed AOD be expected to scale in simple and consistent ways across the sensitivity simulations?

- L194: "Fair" is a rather equivocal word to use here; I read it as you think the model performance is not particularly good, but there are a number of other ways to read it and this may not be what you mean at all. It would be helpful to be more specific here.

- Fig2: These spatial distributions are more different than what I had expected from the text, but I think that is because you are only evaluating the AOD over South Asia and surrounding seas. Maybe zoom in to 45E-100E and equator to 40N?

- L204: Should be section 3

- L206: This sentence should be rephrased for clarity, the distributions are not from Aerooff, they are from the difference between CTL and Aerooff, right?

- L260: Please check the dates, it looks like both studies here included at least a couple of years in addition to 1999

- L279: Here the regions meant are again a little vague, especially in comparison to some other parts of the paper

- L320-324: It feels like Fig S2 is doing a lot of work here; why put it in the supplement instead of the main text?

- Fig5: The vectors and other key elements here are very small. I understand why it is included in later figures, but is it necessary to include the 10-50 hPa part of this figure? This is not really discussed and the differences are small. It would be helpful to eliminate this and make the aspect ratio larger in the horizontal direction as well.

- L393: This makes sense if the additional heating is from additional latent heating above the glaciation level, but how do you attribute it to that process specifically?

- L395: Is this unbroken channel of ascent entirely convective, or mainly convective with radiative heating balancing adiabatic cooling in slow ascent above?

- L398-399: It seems plausible that this water vapor is transported from the Arabian Sea, but it's very difficult to judge this conclusion from Eulerian slices/zonal means alone. Also, I'm not sure if I'm missing something, but I cannot readily make this connection through comparison of fig 7 to fig 5. Please clarify the logic behind interpreting the results this way.

- L403: Since this sentence refers to the impact of temperature change on water vapor, how do you link this effect specifically to the heating caused by carbonaceous aerosols? How do you rule out adiabatic warming of air containing aerosol, or mixing near the tops of slightly invigorated cumulus congestus below?

- L406: Please specify how 'lower stratosphere' is defined in this sentence, in terms of the vertical coordinate.

- L407: It seems possible that the water vapor increases in the high-latitude stratosphere could be more about aerosol effects in mid-latitudes and how they modify wave activity propagating upward in the springtime polar vortex; given the season, maybe even in the timing of final warming (in the NH) or vortex strengthening (in the SH).

- L417: typo

- L417: Is the sulfate impact on longwave radiation mentioned here explicitly diagnosed? What determines this impact, and how can a 'negligible impact' lead to water vapor enhancement through such a deep layer?

- Fig 7: Please make the distinction between (b), (d), and (f) clearer. I spent way too much time puzzling over how the zonal mean response (f) in the high-latitude stratosphere could be so different from the 55-70E section (e)!

- L465-466: Is diabatic heating in the polar lower stratosphere positive or negative in the equinoctial seasons?

- L490: This should be true for LW heating in the troposphere below the level where optical depth to TOA ~ 1, but the LW effect of increasing concentrations above that point should be negative (enhanced emission across the water vapor bands with less coming back than is emitted). More water vapor through the whole troposphere should shift the optical depth ~ 1 level upward, so does deepen the layer where water vapor increases positively impact heating, but the upper troposphere should remain above it. Enhanced heating through the upper troposphere may thus be linked more to increased SW absorption or increased latent heating. If you have the individual terms from your model, it may be worth looking at them.

- L494: specify whether this forcing is at surface or at TOA

- L501: should specify South Asia here, right?

- L505: "0" should be "o"

- L515-516: maybe put the latter part of this sentence (+4.33 ...) in parentheses and then add "alone" to avoid confusion with the Aerooff results discussed at the beginning of the paragraph

- L516-519: Which troposphere is referenced here, Arabian Sea or Indo-Gangetic plain? Both? I'm still not sure why the tropospheric heating acts to intensify convection rather than stabilize the atmosphere; shouldn't this be considered more as a result of convective invigoration rather than a cause? From Fig S2 it does look like there is some intensification of deep convection above the glaciation level over the western Arabian Sea and Arabian Peninsula, but the opposite seems true over the Bay of Bengal. Then again, I am having some trouble reconciling figures S2 and S3, so maybe I have misunderstood?

- L527-528: Is it increased evaporation or increased temperature that leads to this? How does the balance of P-E change?

Supplement:
- L58: There is an extra space between m and g

- Fig S2: what are the vectors? I have assumed that they are the vertical and zonal motion, which seems consistent with OLR, but not with figure S3.

- Fig S3: How to reconcile stronger ascent over 10-20N in the Bay of Bengal section with reduced cloud cover, enhanced OLR, and CDNC+ICNC as shown in figure S2?

- L101: "anomalies aerosols" -> "aerosol anomalies"?

Please also note the supplement to this comment:
https://acp.copernicus.org/preprints/acp-2021-969/acp-2021-969-RC2-supplement.pdf