Review of Fadnavis et al.

I commend the authors for their effort in addressing some issues with the original manuscript:

(1) regarding a possible explanation for the spatial maximum in transmitted solar radiation at the surface in west India at L389:

(2) for including AERONET AOD in this version of the manuscript.

(3) reducing the number of terms in the OMI SO_2 trend model

I have a few remaining major issues with this paper:

1) The AOD simulated by the ECHAM6-HAMMOZ climate-chemistry model does not agree well with that observed by MISR or AERONET (Fig. 1).

Could the model low bias in terms of AOD be due to a clear-sky bias of the AERONET and MISR observations? The authors could study this by including only times when there is not optically thick cloud in the time-average of the model AOD. Cloud information from the model could be used to filter the model AOD.

Because of this disagreement and the vertical transport of aerosols in the Asian monsoon to the UTLS, I think the authors need to have much larger uncertainties on the warming of the lower stratosphere by these (sulfate) aerosols: the abstract states that the warming reaches 0.6±0.25 K. I would expect that the uncertainty on the warming should probably be comparable to the magnitude of the warming. From the uncertainty on the aerosol abundance available to reach the lower stratosphere (see Fig. 1e), I would expect a relative uncertainty of at least 75% on the warming (assuming a linear relationship between local aerosol concentration and local temperature).

The authors should remember that their warming estimate is obtained by the difference between two simulations (one being the control) and that differencing amplifies uncertainty if there is any kind of (numerical) noise in the model. Also, the method to determine the uncertainty of the warming estimates is not provided. The relative uncertainty on the SO_2 trend of 20% (=0.97%/4.8%) could be added (in quadrature) to any other independent source of uncertainty.

2) The uncertainty on the SO_2 trend from OMI got smaller compared to the earlier version of the manuscript, i.e. 4.8±0.97 %/yr versus 4.8±1.3 %/yr. How did that happen? The uncertainty should have gotten larger with fewer terms.

3) The paper focussed on the monsoon season because convective transport in this season lifts boundary layer aerosols to the UTLS. But this is also a season with strong rainout and this might be leading to model bias since the rainout will be difficult to simulate accurately.

Furthermore, by limiting this paper to only the monsoon season, the conclusions are less interesting than a paper that providing the same analysis over a full year.

4) Overshooting convection (L313) is suggested as a pathway for aerosols to reach the stratosphere, but wouldn't the aerosols grow into cloud droplets with so much humidity and such strong convection? I suppose this is addressed by L75 of the introduction.

Why examine the SO_2 emissions from India only? The emissions from China have changed by the same magnitude but opposite sign (L83). The net effect might be very small if emissions from India and China were both perturbed according to their respective trends (based on OMI). Chinese emissions are very relevant to the Asian monsoon region.

Based on these issues (particularly #1), I feel the quality of this paper is somewhat low and the work is of limited scientific interest (see previous paragraph and issue #3). I do not find the model simulations convince me in terms of being realistic and yet the stated uncertainties are rather small. I suppose I could accept this paper if realistic and adequately described uncertainty calculations were included.

Other general comments

The model does not include nitrate, although it is acknowledged that it is an important aerosol species (L94,97,100,102, 258).

The reference list needs to improved. A consistent format is needed. Capital letters should not be used in common nouns in the article title, except for the first word of the title. See specific comments below.

Specific comments

L50: .The -> . The

L51: "Network of aerosol observatories … Forcing (ARFINET)" -> "aerosol observatories … Forcing Network (ARFINET)"

L73: Monsoon -> monsoon

L75: is -> are

L78: "pole ward" -> "poleward"

L86: "region" -> "monsoon (15-45°N, 30-120°E)"

L87: "tropical" -> "annually-averaged tropical (15°N-15°S)"

L95: "as a major … component" -> "as major … components"

L107: "forcing, for" -> "forcing. For"

L125: bases -> based

L192: OX -> Ox

L201: State the model's vertical resolution near the tropopause for a relevant latitude (i.e. 30°N).

L232: "satellite oservations" -> "observations via remote sensing"
(to include AERONET)

L233 (and elsewhere): Use a comma after all leading prepositional phrases ("In Fig. 1a-b,"). See L208 for correct usage.

L257: Dumka et al., 2014 is cited but the reference list only has Dumka et al., 2010. I believe the statement regarding 50% of the aerosols being located above 4 km is not present in the 2010 paper. Also, this statement cannot be generally true; the fraction will be much less than 20% in polluted places and perhaps this value is appropriate for a remote observing location in the Himalayas where the surface is at an altitude of 2 km.

L264: If there are models that perform better than ECHAM6-HAMMOZ in terms of accurately simulating AOD, the authors should justify their decision to prefer ECHAM6-HAMMOZ.

L266: "High" -> "The large"

L276: most -> mostly

L277: thunderstorm -> thunderstorms

L290: I found this statement interesting. Good!

L298: I question whether deep convection (i.e. to the low-latitude upper troposphere) occurs over water (Bay of Bengal). Convective transport of relevance to the monsoon region is occurring in southeast Asia.

L306: aerosols -> aerosol

L318: "Sulfate" -> "The sulfate"

L329: "occurs on a daily scale" -> "it is of short duration (i.e., days) and is episodic".

L339-341: The authors speculate in many places in the paper (search for "may" or "likely"). I find it excessive and in this case, I wonder if the anomaly is significant (i.e., real).

L344: More speculation with little support… . An equally "likely" explanation in my opinion is the much greater concentration of sulfate in the lower troposphere between 50-70°N.

L349: move "~0.1 W m^{-2}" before "over" in L348.

L367: The thermal anomaly is really not that large. (-1 x 10^{-3} K/day leads to a 0.1 K change after 100 days).

L374: "CO_2" -> "the CO_2"

L379 (Sect. 5.2): I believe the first paragraph here could be quite confusing for readers. The trend in radiative forcing from Ramanathan et al. is provided (but with the wrong units, should be W/m^2/yr) and then the next sentence presents the magnitude of the anomaly due to sulfur emissions found in this work. But the numbers are really not comparable since Ramanathan et al. were looking at a much longer and earlier period. I realize the authors may be trying to simply cite this related work here but I fear that readers will believe they should somehow compare the radiative forcing anomaly simulated in this work to the trend from Ramanathan et al. and/or Padma Kumari et al. This can be remedied by simply starting the sentence at L382 with "While not directly comparable to these previous studies, … ".

L384: The authors are cherry-picking the evidence. Figure 6b does not show a very coherent pattern. While the tendency is for negative solar radiation at the surface in northern India, west India is not the only exception (e.g. east India).

L387: "connecting the boundary layer of the ASM region to the UTLS" sounds poetic, but it is not demonstrated in this paper. I believe the reduction in surface radiation is mainly due to aerosols in the boundary layer and the aerosols in the UTLS have a very minor contribution to the received shortwave radiation at the ground. This can be tested by removing the aerosols from the UTLS and looking at the change in shortwave radiation at the surface.

L389: "values of clouds" -> "cloud fractions"

L390: 5.1 -> 5.1.

L393, 395, 401, 402: I don't believe any of these uncertainties (i.e., too small).

L405: subsidence is not discussed in section 5.3. I suggest that "and subsidence" is removed here.

L412: Remove "the strong subsidence" or demonstrate it. This comment applies to L468 too.

L426: "liquid-origin history" -> "liquid origin"

L434: "anomalies are negative" does not belong in this sentence. Please reword so that this is a proper sentence.

L460: Re: "~-1.38", I question whether not only the "8" is a significant digit, but even the "3".

L462: There is not "good" agreement between the offline calculations and the model. Also, "minor" is absolutely not acceptable in the next sentence.

L547: "Beig,G." -> "Beig, G." (there are spaces missing throughout the references, particularly in the author lists.

L565: indian -> Indian

L648: "PadmaKumari" -> "Padma Kumari"

L659 (and elsewhere): 2018 -> 2018.

L667: (AIRS). -> (AIRS),

L672 (and elsewhere): "et al." is not acceptable for ACP last time I checked.

L686: "et al,." -> "et al.,"

L745: Do not use italics.

L758: "S, A." -> "S. A."

L746: Brenninkmeijer -> Brenninkmeijer,

L821 (Fig. 3): The black vertical bars are not described in the caption and should be removed because they block the colour contour plot.

L868: Net -> net

L869: radiations -> radiation