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Interactive comment on "Potential impact of carbonaceous aerosols on the Upper Troposphere and Lower Stratosphere (UTLS) during Asian summer monsoon in a global model simulation" *by* Suvarna Fadnavis et al.

Anonymous Referee #2

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This paper considers the impact of doubled BC and OC aerosol emissions in the South Asia monsoon region using the ECHAM6-HAM model. This model has coupling between aerosol and convective processes but lacks important indirect effects and an interactive ocean surface. The authors find a systematic positive impact of doubled BC and OC emissions on the rainfall over India and eastern China. Since it does not account for the sea surface temperature (SST) feedback (e.g. Ganguly et al., 2012) and aerosol indirect impacts, this study should provide additional analysis that helps improve understanding of the impact of aerosols on the atmospheric state.

I recommend this paper for publication after a major revision. In particular, the dynamical response to the aerosol forcing should receive a more thorough analysis and focused discussion. Given the lack of leading order feedback effects in these simulations, aspects that are likely to persist when these feedbacks are included should be given more attention since they have more value. In contrast, the impact on the rainfall is not such an interesting result since it is not likely to be robust given the disagreement with Ganguly et al. (2012).

Major comments:

1) Using fixed SSTs is a major limitation of this model study. The Ganguly et al. (2012) study indicates that surface heating feedbacks have an impact on the rainfall over India. Thus they cannot be ignored. The authors acknowldege this limitation but the value of their study diminishes if atmospheric processes that a exert a leading order impact are neglected. This study would have been of more interest if the process differences between the Ganguly et al. (2012) simulations and the prescribed SST simulations with ECAHM6-HAM had been evaluated.

2) On page 19, lines 405-408, the authors state that "Positive anomalies in cloud ice and ICNC (in the upper troposphere) may be due to enhancement in ASM deep convection...". The model makes it possible to discern such process details in contrast to observational studies. Similarly on page 15, lines 321-323, "may" is used to describe a process that can be diagnosed from the model. The use of "may" is routine in other instances where transport impacts on aerosols and other tracers are considered in this paper. It makes it seem as if the authors are not sure of the transport effects of the doubled BC and OC emissions. I suggest the "may" qualifier be removed and if there is some uncertainty in the interpretation of the model processes, then this uncertainty should be explicitly noted and discussed. Expanding on the aerosol impact on the circulation state would add value to this study given its limitations. For example, is there an organized circulation structure (both diabatic and isentropic) that is characteristic of the South Asia monsoon region? This subject is covered to various degrees in other

studies but lacks the emphasis it deserves in this study. A more focused discussion of the dynamical response to the aerosol forcing is needed and a comparison with the dynamical regime in the Ganguly et al. (2012) study may help to improve understanding of the aerosol sensitivity in this region.

Evaluation of vertical transport in the model is worthwhile in the case of the "subgrid" convective transport since the convection parameterization contributes heating tendencies that impact the circulation. Figures 3, 4 and 5 have the circulation anomaly vectors presented. In the case of UTLS transport, it is next to impossible to see the transport pattern in many cases since the lower altitude vectors dominate the scaling. Figure 5 compensates for the poor readability of the other figures and justifies the link between enhanced penetration of aerosol into the lower stratosphere and the increased vertical circulation through the tropopause which has substantial statistically significant regions.

3) Figure 5 indicates that it is not just the Hadley circulation which changes but the low altitude jet structure between 10 and 25 N. This has an impact on the rainfall pattern as well.

4) Indirect effects of BC and OC are not included in these simulations. It would have been much more worthwhile to consider the impact of increased IN and the increased cloud evaporation due to aged BC fractions in cloud liquid and ice phases. It is not at all clear that these indirect effects would not substantially change the results. Thus the lack of indirect aerosol forcing is a serious limitation of this study. The authors should include more discussion of this limitation than the cursory mention on page 14. The conclusions of this paper have substantially reduced relevance considering the lack of important feedback processes including SSTs and cloud indirect effects.

Minor comments:

p3. l33: replace "being" by "is", "population and economies" by "regions in terms of population and economy" p3. l35: replace "major" by "main" or "primary" p3. l38:

replace "contribute largely" by "substantially contribute" p3. l41: replace "significantly large" by "large" or "significant" p21. l445: replace "evidences" by "evidence" p21. l446: replace "show" by "shows"

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