

Interactive comment on “Aerosol-cloud interactions in the NASA GMI: model development and indirect forcing assessments” by N. Meskhidze et al.

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1. This study investigates the influence of varying meteorological fields and cloud droplet parameterizations on the aerosol indirect effect (AIE). While I know of previous studies comparing various droplet parameterizations, investigating the effect of varying the meteorology on the AIE is potentially more interesting. I also find the NASA GMI concept, where various model components can be interchanged and tested, intriguing. However, this paper is based on methods and assumptions that I find fundamentally flawed.

To address these concerns, we have implemented the CLIRAD-SW solar radiative transfer model (Chou, 1992; Chou and Suarez, 1999) in GMI to calculate online the

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shortwave (SW) fluxes at the top of the atmosphere (TOA). In the revised manuscript the relevant discussion on the calculations of cloud optical depth (COD) and indirect forcing (IF) is updated accordingly. The relative differences in IF have not changed, although the absolute value of IF has changed somewhat. For example, IF using the GISS fields are -1.15 W m^{-2} as opposed to -1.0 W m^{-2} before. No qualitative changes whatsoever in discussion and conclusions arise by switching the radiative transfer code.

2. IF is defined as the difference in incoming shortwave flux at the surface between present day (PD) conditions and preindustrial (PI) conditions (i.e. PD value - PI value). This difference is given as a positive value both in the text and in Table 2. However, the AIE represents an increase in cloud albedo, i.e. less incoming solar radiation in the PD case. Hence, IF should be negative, and I find that not realizing this reveals fundamental lack of understanding. Later, the authors discuss the PD vs. PI change in solar fluxes at the top of the atmosphere (AIE), and these values are negative.

We all know that indirect forcing cools climate. Not including the negative sign at some parts of the manuscript was just an oversight. We thank the reviewer for pointing this out.

3. Additionally, I strongly disagree with the use of a scaling factor for the IF, and the assumptions that this scaling factor is based on. The reasoning for the scaling factor of 0.5 is the similarity between the NASA GMI simulation (with NASA GISS meteorological fields) and the NASA GISS simulation itself. The similarity is neither striking nor remarkable, as described by the authors. There are non-negligible differences between the two simulations. Finding that the global averages of two cloud parameters (and their PD-PI values) are somewhat similar for the two modeling frameworks, one assumes that the indirect forcing for the two frameworks should be the same. For this assumption to be acceptable, one would have to compare many more model fields, and not only as global aver-

ages.

Although not discussed in detail, we did an intercomparison between the fields and they looked quite similar (with some non-negligible differences obviously). Nevertheless, this is now a non-issue, as we have implemented a full radiative transfer algorithm for the indirect forcing calculations (which by the way also gives a global average scaling factor of 0.5).

4. If the NASA GMI currently lacks processes that would lead to realistic IF values, I suggest that these processes are implemented before a resubmission of the paper. This will make the NASA GMI a suitable host model for testing parameterizations and meteorological fields in the future. In its current state, I do not find this paper scientifically sound and I do not recommend it for publication.

Please see response to comment #1.

References

Chou, M.-D.: A solar-radiation model for use in climate studies., J. Atmos. Sci., 50, 6738211;690, 1992.

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Interactive comment on Atmos. Chem. Phys. Discuss., 7, 14295, 2007.