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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Snow-induced buffering in aerosol–cloud interactions" *by* Takuro Michibata et al.

Anonymous Referee #1

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This manuscript demonstrates the impact of falling hydrometeors on the effective radiative forcing of aerosol-cloud interactions in MIROC6 by modifying their microphysics from diagnostic precipitation to prognostic precipitation. The authors find that the large deviation in ERF(aci) in the old scheme is significantly improved as the magnitude of ERF(aci) is reduced. A series of sensitivity tests reveal that prognostic snow has substantial impact on both shortwave and longwave forcing induced by aerosols. This is an excellent modeling work, particularly that the impacts of many physical processes relating to aerosol-cloud interactions are quantified, as illustrated in Figure 4. However, there are some issues need to be clarified before accepted for publication.

Major concerns:

1. ERF(aci) is determined not only by changes in optical properties of cloud and precipitating particles, but also by changes in the cloud cover, particularly in GCM. In the text, the authors only show that the simulated cloud cover is "in good agreement" with the observation (although I think underestimation of high clouds is quite severe). However, the aerosol-induced change in cloud cover is not discussed at all.

2. From this manuscript, it is not clear to me how the aerosol scavenging process works in MIROC6 DIAG or PROG version. I suppose that PROG can simulate aerosol washout more realistic with falling rain and snow, and both ERF(aci) and ERF(ari) could be influenced.

3. This manuscript primarily focuses on the SW, while there is very little discussion about LW. The change in ERF(aci)-LW only attributes to "adjustment induced by snow together with its radiative effect" (Line 139). It is way too brief. The authors should elaborate the physical processes related to the "adjustment".

4. The authors claim that significant reduction in ERF(aci) well corresponds to snow water path (SWP) geographically (Fig. S3). It seems not true for me, particularly over the Southern Ocean. In contrast, the correlation between CLWP (Fig. S4) and ERF(aci) (Fig. 1) looks very high. It may imply that the change in ERF(aci) is primarily determined by CLWP change, and its relation with SWP is not so important.

5. The sequence of inference in Section 4 is complicated and not straightforward to me. The authors fist state that SWP susceptibility is correlated to LWP susceptibility (which I think a little weak) and propose a hypothesis that riming is important to reduce the optical depth of clouds. However, the sensitivity test clearly reveal the importance of riming (Fig. 4). That is, the sensitivity experiment should be considered as the basis of speculation, not the supporting evidence to the hypothesis.

Minor points:

1. It seems to me that Figure 4 only consider the changes in ERF(aci)-SW because snow radiation is to increase ERF here. If it is the case, the figure caption and the

description in the X-axis need to be more clear.

2. Line 87: What is the standard time step in MIROC6 as the sub-time step is 60 s?

3. Line 95: Does it mean PD and PI simulations use the same SST and sea ice?

4. Line 108: WBF should be spelled out.

5. Line 110: "... were returned... " Does it mean the simulations without TOA balance are not used in analysis?

6. Line 123: This argument regarding ERF(ari) and ERF(aci) is quite indirect. Why not just show the values of these two terms?

7. Line 126: "i.e., close to zero" Its meaning is ambiguous.

8. Line 144: "only the latter process..." Should be "former"?

9. Figure 3: the description in X-axis should be CLWP instead of LWP.

10. Line 174: "water mass suspended in the atmosphere" Does it mean cloud only?

11. Line 175: If the total mass is the same, this argument holds. However, in this simulation, the total mass in PROG is larger than DIAG, and then the optical thickness of cloud+precipitation in PROG can be larger than that of cloud only in DIAG.

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