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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Arctic cloud cover bias in ECHAM6 and its sensitivity to cloud microphysics and surface fluxes" *by* Jan Kretzschmar et al.

Anonymous Referee #1

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The manuscript by Kretzschmar et al. shows a positive bias in cloud cover over the Arctic from global atmospheric model ECHAM6 with comparison with CALIPSO, and studies the possible causes of this difference, and presents their efforts to remove this bias with different parameterization in the model. The efforts include adjustment of moisture/heat exchange between surface and atmosphere, and tune of the effectiveness of Wegener-Bergeron-Findeisen process, and to allow supersaturation with respect to ice with a new parameterization of saturation water vapor pressure. The paper is generally well written and concise, which I particularly appreciate. The primary concern I see is the effectiveness of the new parameterization of the saturation water vapor pressure over both sea ice/snow and open water, which the authors may need a better presentation of their results. My recommendation is that the manuscript

needs be revised prior to publication to better present the effectiveness of their new approaches. More details follow. Major comments: 1. By allowing super saturation regarding to ice, the differences of low-level cloud between model and CALIPSO are somewhat reduced over sea ice/snow especially with smaller ice mixing ratio thresholds, as shown in Figure 6. The benefit is accompanied by the drawbacks that the differences over other areas become negative, with even more negative differences over open water, e.g. over the GIN seas and Barents Sea. As shown in Figure 1 and 2, such negative differences exist with original parameterization. So, the new parameterization may lead to another issue, underestimation of cloud cover, over the open water area, including the newly open water in the Arctic in summer and autumn. The authors mentioned the reduced condensation removal by precipitation may solve this, which may trigger other issues. The authors may want to clarify this in the manuscript. 2. The adjustment of surface/atmosphere heat/moisture strength seems working fine to me. In the manuscript, the authors said "For sea ice covered surfaces, ...while only minor changes in the cloud cover bias are found for summer". (Line 18- Line 23, page 7). I see the cloud cover after the adjustment agrees with CALIPSO really well over sea ice with scaling factor 5 as shown in Figure 4. The differences in winter are small, and the apparent difference in the later summer and autumn might be due to the CALIPSO shows more cloud cover over newly open water in the Arctic Ocean, while the model cloud cover are over sea ice only. This suggests this adjustment somehow works, even though the mixing is already too strong over the sea ice, as the authors discussed. 3. Model has positive bias in cloud cover, especially in low-level cloud, when compared to CALIPSO. CALIPSO has low cloud amount bias when compared to surface based observations, as studied by Blanchard et al. (2014) and Liu et al. (2017). The overestimation over sea ice may be appearing as significant considering CALIPSO's underestimation of low-level cloud. Specific comments: 1. Line 6 page 1, this overestimation is also due to overestimation of high-level cloud. 2. Line 21 page 2, please spell Acronym out at its first appearance, like CALIPSO; also after the first appearance, there is no need to spell it out again, like COSP. 3. Line 33-35 on

page 3, I am wondering what SST and ice concentration data you used in your model run? 4. Line 11-12 page 4, I am wondering how you are able to divide each model grid box into 40 subcolumns? 5. Line 19-20 page 4, you have model runs from 2007-2010, when sea ice extent in the late summer and autumn were significantly reduced. The cloud cover is greatly affected by this. It would be good to have the model runs from other years without such sea ice extent changes, which was not available in this study due to the computational cost as the authors pointed out. 6. Last line on page 4, consider changing “higher” to “greater”. 7. In the 1st paragraph of section 3, you might want to point out there is underestimation of cloud cover over open water. 8. Line 11-12 on page 6, unless you show there is no humidity bias over other surface types, this claim may not be valid. 9. Line 18-23 on page 7, it would be interesting to see the impacts of the adjustment on liquid and ice cloud cover. 10. Line 9-10 on page 8, the differences also include bias in high-cloud. 11. Line 19 on page 8, “below” should be “above” 12. Line 28-29 on page 8, how about the changes in low-level cloud? 13. Line 28-31 on page 10, please reword this sentence. References included in the comment: Blanchard, Y., Pelon, J., Eloranta, E. W., Moran, K. P., Delanoë, J., and Sèze, G.: A synergistic analysis of cloud cover and vertical distribution from A-Train and ground-based sensors over the high Arctic station EUREKA from 2006 to 2010, J. Appl. Meteorol. Climatol., 53, 2553–2570, 2014. Liu, Y., Shupe, M. D., Wang, Z., and Mace, G.: Cloud vertical distribution from combined surface and space radar–lidar observations at two Arctic atmospheric observatories, Atmos. Chem. Phys., 17, 5973-5989, https://doi.org/10.5194/acp-17-5973-2017, 2017.

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