Response to Referee #1

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.

We thank the reviewer for the constructive comments that helped to improve the manuscript.

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.

The idea that reduced condensation removal by precipitation may solve this issue was purely speculative and we did not conduct sensitivity studies to this end and therefore we removed this statement from the manuscript. In the revised version of the manuscript, we try to more clearly point out why a temperature-weighted scheme for saturation vapor pressure in combination with an increased efficiency of the WBF process introduces a negative bias in low clouds. As the amount of low-level ice clouds remains more or less constant for different values of γ_{thr}, the amount of liquid clouds strongly decreases and therefore also the amount of clouds in general. The decrease in liquid clouds is mainly caused by the more efficient WBF processes which more efficiently turns liquid into ice clouds over continents compared to oceanic regions. In the standard setup of ECHAM, liquid clouds are already biased low in those regions which is even further enhanced by a more effective WBF process. As liquid clouds seem to react rather sensitively to a more effective WBF process, even minor changes of γ_{thr} can have strong effects on the amount of liquid clouds and we think that setting γ_{thr} to $2.5 \cdot 10^{-6}$ $\mathrm{kg\,m^{-3}}$ might already be a reasonable value to improve the WBF process. This value might be a good compromise between improving cloud cover over snow and ice covered surfaces by simultaneously not further worsen clouds in other regions. These new explanations are now in the manuscript in order to respond to the reviewer's remark.

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.

We agree with the reviewer that increased mixing seems indeed be a good way of tuning Arctic clouds. As requested by the reviewer also in one of the minor comments below, we added a new, more detailed discussion of the effect of this adjustment on liquid and ice clouds. Increased mixing was also able to improve cloud phase as the liquid bias in winter is now also reduced, which further shows that this might be a good option to improve clouds. In the revised version of the manuscript, we try to emphasize the positive effect of increased mixing on cloud cover, even though we still think that it might be questionable whether one can physically justify such a measure as the model already mixes too strongly in the Arctic with its stable boundary layers in comparison to surface observations.

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.

We revised the description of the CALIPSO-COCCP dataset. Section 2 now contains a more detailed description of the observational dataset (i.e. cloud detection thresholds, information on vertical resolution, phase discrimination). In the revised version of the manuscript, a more detailed review of uncertainties and issues for retrieving clouds in the Arctic using CALIPSO-GOCCP is included (i.e. lidar attenuation by liquid clouds, cloud detection thresholds that might not be representative for Arctic region and also possible effects of spatio-temporal sampling of satellite data). Nevertheless, we think that our claim of an overestimated low-level cloud fraction in ECHAM6 is valid. We compared modeled (ECHAM+COSP minus ECHAM) to observed (GOCCP minus ground based observations) cloud cover profile differences and see a similar underestimation for modeled clouds when using a satellite simulator compared to the cloud fraction from ECHAM6's cloud cover scheme. Even though comparing modeled and observed difference in cloud cover profiles is not an "apples-to-apples" comparison (because of different definitions of what is a cloud), we see that COSP derived cloud properties mimic real world issues of the actual lidar. Therefore, the reported overestimation of low-level clouds in the model is a "real" signal and not just due the observational issues in the GOCCP dataset.

Specific comments

1. Line 6 page 1, this overestimation is also due to overestimation of high-level cloud.

We added the explanation that the overestimation of total cloud cover is due to an overestimation of low- and high-level clouds to the abstract.

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.

Done.

3. Line 33-35 on page 3, I am wondering what SST and ice concentration data you used in your model run?

We use monthly observations of sea surface temperature and sea ice concentration from the AMIP II dataset. We added this to the manuscript.

4. Line 11-12 page 4, I am wondering how you are able to divide each model grid box into 40 subcolumns?

In the revised version of the manuscript we elaborate more on how those subcolums are created.

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.

The reviewer is right that this introduces a complication. But since the observations are for the same period, and since the biases are widespread, we think that the conclusions are valid. In any case, since CALIPSO and CloudSat are available only since 2006, there is no possibility to go for another period for the evaluation.

6. Last line on page 4, consider changing "higher" to "greater".

Done

7. In the 1st paragraph of section 3, you might want to point out there is underestimation of cloud cover over open water.

Done.

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.

We additionally show from ERA-Interim to also have information on temperature and humidity profiles on a wider spatial scale to show that there is a difference between snow/ice covered regions and not snow/ice covered regions. Looking at relative humidity, ECHAM6 seems to generally overestimate it over the continents, but this overestimation is most strongly pronounced in those regions we observed the strongest positive biases in low-level clouds, which make us confident that this overestimation actually exists.

9. Line 18-23 on page 7, it would be interesting to see the impacts of the adjustment on liquid and ice cloud cover.

In the revised version of the manuscript, we added the impacts of the adjustment on liquid and ice cloud cover. As we already stated above, the approach of increased mixing seems promising as this measure not only reduces the cloud cover bias of low-level clouds but also addresses helps to reduce the overestimated bias of liquid clouds.

10. Line 9-10 on page 8, the differences also include bias in high-cloud.

The revised manuscript now explicitly points the reader to this fact.

11. Line 19 on page 8, "below" should be "above"

Using "below" in this sentence is correct, as we refer to condensation. Nevertheless, we see that this sentence can be misunderstood and modified it to be better understandable.

12. Line 28-29 on page 8, how about the changes in low-level clouds?

With total cloud cover, we mean total, low-level cloud cover. To avoid confusion, we now just call it "low cloud cover".

13. Line 28-31 on page 10, please reword this sentence.

Done.