Review of Bacer et al., round 2

The authors have nicely responded to most of the questions and the manuscript is much clearer now. I think the paper is almost ready to be published. However, I still see one major issue, which the authors should urgently address (if not here then at least in future studies). I also add a few more suggestions, which may improve bits of the manuscript.

<u>Detraining ice water content, but not ice crystal number</u>

Please point out the issue of detraining ice mass but not number from deep convective clouds. Maybe also add a note on it in the conclusions, as most readers do not carefully read all details of the model description.
I understand that detrainment and coupling with convection hasn't been the main focus of the study, however – given its importance for global climate, it should be mentioned in a clearer way somewhere in the text.
(where I take this point to acknowledge that the authors significantly improved the description of their model and its coupling to the detrainment)

The IC radius of more than 100 μm at the formation timestep (as shown by Figure 2 of the response document) is clearly not realistic at the 200 hPa level in areas dominated by detrainment. You also mentioned that at such levels you expect homogeneous nucleation to dominate, which is, again, not compatible with such large ice crystal radii in locations dominated by convective detrainment (e.g. Jensen et al., 2009) or very thin TTL cirrus clouds (e.g. Jensen et al., 2015). See also Heymsfield et al., 2014, Heymsfield et al., 2017 for a more general perspective. Naively, I could imagine that your nucleation scheme would take that into account and homogeneously nucleate enough ice crystals, but that clearly doesn't seem to be the case.

Many GCMs solve the problem by assuming the size of detrained ice crystals in order to get their number. This is also an oversimplification, which however prevents the formation of huge ice crystals and the decoupling between (large) ice water content and (relatively small) ice crystal number observed in your results with the BN09 scheme over the tropics. There might indeed be other, more elegant fixes of the detrainment number and large ice crystal problem.

Minor comments

<u>General comment: ICNC changes</u>
I am puzzled to see how little the ICNC burden for cirrus clouds changed. Why is that?
What confuses me is that you take the 200 hPa level as representative for cirrus. However, the ICNC changes there are significantly different compared with the integrated ICNC burden for cirrus.

I think you actually nicely prepared the answer to my question with Figure 4 in the response document to reviewer 1. I find the binning by temperature very informative. Could you add one in the paper and comment it briefly in the text? It would be great to see the anomalies of BN+LD, KL+BN, and BN+BN with respect to DEF. Also, I like its right panel too as it clearly summarizes the main ice crystal number changes (if you extended it to the lowest temperature range). I think extending the panel beyond 30°S, where the heterogeneous freezing doesn't occur too frequently, should also not affect its general interpretation Maybe this right panel with the extended temperature range alone would be enough to strengthen some of your conclusions, if you include it in the manuscript.

To sum up, I miss a discussion in section 3.1 (and conclusions) which is more clearly pointing at the (1) ICNC increase in mixed phase due to heterogeneous freezing, (2) ICNC increase around 230 K due to heterogeneous freezing in cirrus, (3) ICNC decrease at the coldest temperatures due to the PREICE effect. Is there a way you could prove the last point? It sounds plausible from the text, however, a more direct proof would strengthen your findings.

Page 2, lines 31-33:
Homogeneous nucleation is dominant? Yes, but based on some modelling studies. Not all of them. At least that's what one of the authors nicely showed in a previous study (see plot below from Barahona et al., 2017). Of course, I would agree that homogeneous nucleation is the dominant source of ice crystal number or similar statements, but not when looking at ice crystal nucleation events.

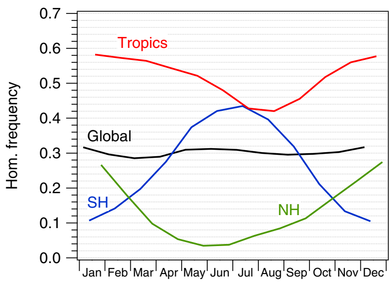

Figure 8. Monthly mean homogeneous ice nucleation frequency for the Tropical (latitude −30° to 30°) and the Northern (NH, latitude 30° to 60°), and Southern (SH, latitude −30° to −60°) extratropical regions.

Page 4, line 33:
-You referenced the wrong Tost et al., 2006 paper (looking at the references)
-Do you use the scheme of Tiedtke 1989 or Tiedke 1989 with modifications from Nordeng 1994? There are some significant differences between the two, so it is better to be precise here.

Page 7, line 20:

"The only expedient adopted by the CLOUD submodel is…."
That word (expedient) does not make sense in the context used. Please use simple and understandable words instead.

Page 11, line 18:

Cziczo et al., 2009 is not a good reference for northern hemisphere, being dominated by heterogeneous nucleation, as it talks/speculates about lead-containing natural dust. Li et al., 2012, Storelvmo et al., 2014, Gasparini et al., 2016 and also Barahona et al., 2017 show some of that, although being highly model and parameterization dependent.

Page 12, line 29-30:
"Such a reduction…at this altitude." please rephrase this sentence as it is not grammatically correct.

Page 14, line 13-14:
Could you add a sentence explaining why the mixed-phase is less sensitive to the change in ice nucleation scheme?

Page 14, line 19-20
These mentioned ice crystals are simply too large compared to any kind of observations at such levels of the atmosphere.

Page 16, lines 7-9:
I would either rephrase this or remove the reference to Lohmann et al. 2008 as the sentence gets very hard to understand, when explaining that Lohmann et al. 2008 did not include the competition effects.

Page 16, line 10:
also Gasparini et al., 2018 for a recent ECHAM-HAM paper showing IWP anomalies

Page 16, line 11:
add a reference for your liquid water path estimates

Page 16, line 13:
Duncan and Eriksson 2018 might be a fresh good reference for IWP variations between observation and reanalysis datasets.

Page 16, line 19-20:
SCRE becomes less strong. I would not call that an "increase", despite its value becoming less negative.
For me an increase in SCRE would imply a stronger reflection of clouds and consequently a more negative SCRE.

Page 16, line 21:
NCRE diminishes
please rephrase also here to a more clear expression – maybe "NCRE becomes more negative"

Page 17, line 5-8:
If your scheme produces fewer and larger ice crystals that indeed might lead to increased precipitation. However, this will contribute to the large-scale precipitation budget, and not to the convective one.

Your interpretation of the result is therefore not explaining the increase in convective precipitation.

Page 17, line 10-13:
I would argue that the changes in LCRE and SCRE are quite considerable and not negligible (as you also mentioned earlier in the text).

Figure S4:
I would suggest to rather plot the radiative anomalies in absolute terms, using W/m^2 as a unit. Relative changes are tricky to interpret, in particular when looking at SCRE (and NCRE).

Table 2:
Why do you compute also a spatial standard deviation when showing globally averaged results? Please use just global annual means, and get the (temporal) standard deviation from those 5 points (I agree 5 points are rather at the lower limit, but that is certainly more meaningful than looking at variability in both space and time).

References

Jensen et al., 2009: On the importance of small ice crystals in tropical anvil cirrus
Jensen et al., 2015: The NASA airborne Tropical Tropopause Experiment
Heymsfield et al., 2014: Relationships between ice water content and volume extinction coefficient from in situ observations for temperatures from -0 to -86°C: Implications for spaceborne lidar retrievals.
Heymsfield et al., 2017: Cirrus clouds
Storelvmo et al., 2014: Cirrus cloud susceptibility to the injection of ice nuclei in the upper troposphere
Duncan and Eriksson, 2018: An update on global atmospheric ice estimates from satellite observations and reanalyses
Gasparini et al., 2018: Cirrus cloud properties as seen by the CALIPSO satellite and ECHAM-HAM global climate model