REVIEW OF BACER ET AL., 2020, ACPD

Summary:

The study tries to better understand cold clouds simulated by a GCM by showing the ice number microphysical rates both at global and regional scales. Surprisingly, freezing of cloud droplets was found to be the largest source of ice crystals, while snow formation by aggregation and accretion are the dominant sinks. The relative importance of sources and sinks does not change significantly with a different freezing scheme. Changes due to global warming are also briefly studied with the help of a separate model simulation.

The study is an interesting read and is novel given that it shows microphysical number rates, which are rarely shown or discussed in published literature and therefore represents a welcome expansion of the standard analysis of ice cloud properties. The study lacks some in-depth analysis on the reasons for the importance of some of the process rates and has some methodological issues. Given the unique perspective on cold clouds, I would encourage the authors to add a discussion on the potential weaknesses of the presented results and ideas for overcoming them in future model developing work on cold cloud processes.

Despite (or because of) its novelty, the presented analysis still has a large potential for improvement. I added a list of (1) general and (2) specific sources of concern and study weaknesses that the authors still need to address before the manuscript can be published.

General comments:

I. **Mass microphysical rates**
Why did you include only the number and not the mass rates in your analysis? The title suggests you are studying both mass and number rates. By including mass rates would be easier to get a more complete picture of how your model works. I assume the mass rate hierarchy could look quite different from the number rate hierarchy.

II. **Sublimation**
Why is sublimation not considered as a sink of ice number in the analysis, particularly after being mentioned in Eq. 1? I think it would be good to find a way to include sublimation in the analysis or at least estimate its impact.

III. **Closing the number budget and "numerical tendencies"**
How close are you to closing the number budget?
Are (sources+sinks)*model timestep = ICNC?

Your manuscript offers an often neglected insight into sources of ice, which is rarely seen in publications. However, you do not include "numerical tendencies" in the analysis. I think it would be valuable to show all numerical/unphysical tendencies (correction terms) that significantly perturb the ICNC budget besides the mentioned physical tendencies.
An example of such unphysical sink of ice (that you did not mention in the

manuscript) is the maximum ICNC correction term.

The ice cloud community should become more aware of all such terms and think about ways to avoid imposing such unphysical limits in the models of microphysics. By doing so, the ICNC picture would be complete, and you could close the sources and sink budget. This is in my opinion more important than limiting your analysis to the tendencies with physical meaning only. A strong additional message coming out of your work could therefore be that the very "volatile" ICNC budget is significantly modified by "numerical tendencies".

IV. **FREE term**

I don't think you can physically justify the existence of FREE by simply referring to it as "liquid origin cirrus". The work of Krämer et al., 2016 associates liquid origin cirrus to deep convection (which is DETR in your case) or frontal ascent (e.g. warm conveyor belts). Wernli et al., 2016 shows a peak in liquid origin over the storm track region due to slow frontal ascent.

However, in your simulations, FREE is strikingly high over continents and orography. We know wave clouds could be formed by homogeneous freezing of cloud droplets (Heymsfield and Miloshevich, 1993), but that should not matter much in a climatic sense.

Homogeneous freezing of cloud droplets is to my understanding of ice cloud formation mechanisms climatically irrelevant outside of deep convective updrafts (and those are taken care of by deep convective scheme and DETR tendency).

I would therefore argue that one of the partly unphysical tendencies mentioned in the upper comment is your FREE term. I believe FREE is to a large extent just a temperature correction term that freezes the cloud droplets at temperatures <-35°C. Ideally, other processes in the model should take care of that and freeze most of the cloud droplets at warmer temperatures. Such terms appear also in other models. Do you believe we should be worried if they represent such a dominant source of ice? Why?

How would ICNC look like if you neglected the FREE tendency? Would a short experiment without the FREE source term help understanding its real climatic importance?
FREE, as you mention, does not happen very often, but results in huge ICNC. Therefore I would also expect the FREE term to often exceed the maximum ICNC threshold of 10^7 m^{-3} and therefore be immediately limited by the "maximum ICNC correction" IC sink. The net climatic effect of such a tendency may therefore be limited.

In summary of my lengthy comment, I believe the manuscript would benefit substantially if you better explored the causes of FREE.

V. **Relative importance of specific sources and sinks of ice**

It is hard to understand the relative importance of specific source and sink processes only by looking at the zonally averaged Fig. 2 and 3. Could you add plots showing the relative importance of each process, i.e. a division of a specific source or sink process with the total source or sink tendency. Would a similar type of plot help in exploring the regional importance of several sources and sinks of ice in the discussion of Fig. 1 and Fig 4?

It may be easier to understand the importance of the separate microphysical rates if you would include also figures/information about:

a. Probability density function distributions for each microphysical rate, plotted only when the rate has a non-zero value.
b. Occurrence frequency of each of the microphysical rates.

VI. **SEDI tendency**

Why is the vertically integral of SEDI so negative? Shouldn't we think of sedimentation only as a redistribution of ice crystals? Shouldn't the column integrated net SEDI be equal to zero?

I know this is not possible due to the inclusion of sublimation of falling ice crystal into the sedimentation tendency. Could you therefore (1) analyse that tendency separately and (2) verify if the net SEDI is now close to be balanced.

I don't understand the reasoning you give explaining the disagreement between SEDI+ and SEDI- in lines 245-247. Isn't SEDI- in level X same as SEDI+ in level X-1? (if we take care for the sublimation of falling ice)

Moreover the median vertical profiles in Fig. 4 suggest that the vertical integral of sedimentation should be a small values, and not a significantly negative tendency as shown in Fig. 1. The zonally averaged perspective shows SEDI- being more dominant than SEDI+ at all levels of the atmosphere. Why is there such a disagreement between Fig 4 and Figs. 2+3?

VII. **Summary chart**

You could add a summary chart (maybe a pie chart for sinks and sources of ice or a bar chart) that summarizes the importance of several sources and sink processes. Table 3 is to some extent doing that, but tables are hard to read (and also table 3 is not really giving us a budget perspective). I think that such a visualization (maybe in relative, not absolute terms) would be a nice key figure of the paper.

VIII. **Effects due to global warming**

Section 4.4.2 is currently very weak and doesn't really provide much of robust novel findings. The only robust feature is the upward shift of ice rates/ICNC. The changes to ICNC, IWC, IWP, source and sink processes cannot be considered robust when comparing only 1 year of data (!). This is confirmed by no significance in zonally averaged plots (I don't consider a 70% significance level adequate).

The upward shift in clouds (and therefore sources/sinks of ice) is not novel, so

I suggest removing the section and rather focus on digging more into the model to better understand the above mentioned points.

If you really want to keep it, you should substantially expand your analysis. A climate change or cloud feedback perspective on the shifts of ice phase with global warming would certainly need some new plots, e.g. changes in ICNC, IWC, IWP, specific and relative humidity, a cloud feedback decomposition (or at least changes in cloud radiative effects assuming an adjustment term to take into account changes in clear sky quantities/changes between a CRE and a cloud feedback perspective). Maybe also changes in static stability, radiative heating, etc.

Moreover, you did not take advantage of the high frequency output data. How does the ICNC distribution shifts (a) in total (b) in specific temperature ranges? What about IC sources and sinks?

Specific comments:

- Line 4:
  How could you compare microphysical process rates with observations? Sadly, I think it's hard to measure the relevant number process rates with the available in-situ or remote sensing data. Observations currently lack the evolution perspective, and rather give snapshots of cloud properties.

- Line 13:
  You could verify whether cloud diabatic heating rates increase in the upper troposphere with the additional model diagnostics.

- Introduction: A reference mentioning the work by Dietlicher et al., 2019 who showed the cloud volume based on source may be appropriate, although the distinction is not necessarily a process-rate based one. A reference to Gyrspeerd et al., 2018 may also be appropriate given their cirrus classification scheme.

- I would find it useful if you started the paper by showing the ICNC zonal average and ICNC burden plots (S1 and S2a) and compare that with observations (Sourdeval et al., 2018 and Gryspeerdt et al., 2018). Why does your model overestimate ICNC in the extratropics while simulating too little ICNC in the tropics?

- Section 2.1/2.2:
  Is snow diagnostic? Is it removed from the atmosphere in one timestep? Does it affect radiation or not?

- Line 123:
  The convective scheme should detrain some ice also at temperatures warmer than -35°C. A recent publication by Coopman et al., 2020, for example, shows that the average glaciation temperature of isolated convective clouds over Europe is about -21°C. That may be worth mentioning in the text as a potential problem of the

scheme and reason for low ICNC bias in mixed phase compared to observational data by Sourdeval et al., 2018.

- Section 2.3:
  Please describe how each of the IC sinks works (not only refer to older publications, given the central role of such processes in your paper).
  Is there a temperature dependence (particularly for aggregation, accretion, and self collection)?
  Is there any size dependence?

- Section 3:
  Do you run your global warming simulation in present-day CO2 concentrations? If so - do you expect any influence from not changing CO2 levels to those expected in year 2080 in the RCP6.0 scenario?

- Lines 230-233:
  In fact, upper-level gravity wave activity, particularly strong in the tropics, can generate temperature fluctuations responsible for strong nucleation tendencies.

  Is this right? Your model resolution is about 3°x 3°, which is orders of magnitude larger than the relevant length scales for gravity waves. So the model cannot resolve those directly.
  Moreover, the model used doesn't seem to have a parameterization that would add a gravity wave updraft spectrum in to the vertical velocity and in such way represent the influence of gravity waves on ice nucleation. I guess the used TKE-based updraft only gives one vertical velocity value per gridbox, not a distribution.
  I believe the reason for high ice nucleation rates in the tropical upper troposphere therefore lies in a combination of cold temperature and high relative humidity.

- Line 240:
  On the contrary, SEDI+ is low at upper levels because the crystals are too small to fall out and at lower levels because the number of ICs is a small.

  That doesn't sound right or I simply don't understand it. Wouldn't that be true for SEDI- and not SEDI+.
  ICs are small at upper levels, but I don't know why this would limit the SEDI+ tendency. I would assume SEDI+ tendency to be larger in locations where ICNC is large and where IC radius is small. This points rather at the upper troposphere.

- Section 4.3 and Fig 4.

  Why is detrainment so important over Sahara? Why at such higher altitude?
  I assume the number of points taken for the Sahara figure is small due to the low amount of ice clouds there. That may be added in the discussion.

  Another general conclusion of this section could be that a clean (southern) Indian Ocean is very similar to a more polluted N. Atlantic?
  Moreover, I think many atmospheric scientists would rather call that region as

"Southern Ocean", as in this large project https://www.eol.ucar.edu/field_projects/socrates , for example. When talking about Indian Ocean we normally think of tropics.

I still cannot understand whether FREE is an important source of ice or not. A sensitivity experiment in which FREE source would be turned off could help determining that by looking at changes to ICNC.

- Line 310: Is DETR really maximal at -35°C? I cannot see that from the Fig. 2. Detrainment tendency should be probably maximized at temperatures closer to -50°C (220 K) in the tropics, if we believe the FAT theory (Hartmann and Larson, 2002).

- Section 4.4.2:
  It is hard to understand whether we see only a shift or some change in ice rates. A temperature vertical axis would therefore be more appropriate for Fig 5.

- Line 313-320:
  You mention the ICNC increase in the upper troposphere. Isn't this only a shift due to the expansion of troposphere?
  If IC radius decreases and if this change is important, you may want to show it in a separate plot.

- Line 318:
  Isn't an increase in cloud persistence in contrast with your comment on decreased upper tropospheric anvil clouds due to increased static stability? I thought the high cloud fraction decreases with warming?

- Line 315:
  Why do you think the LW atmospheric heating is associated with the cloud base temperature? Are you talking about heating within the atmosphere? Or at the top-of-the atmosphere (TOA) radiative effects?
  I don't think the cloud base temperature matters for the TOA LW effects. Deep convective clouds have a large LW CRE, despite having a very low cloud base (with high temperatures). Maybe some of Mark Zelinka's numerous publications on the topic may help.

- Line 315:
  Also, you talk about the additional upper tropospheric warming due to climate change but never explain why should we care if the upper troposphere is slightly warmer? (compared to the arguably more important or at least more studied influence of changes in high clouds on the TOA radiative budget and climate sensitivity)

- Line 315-316:
  I think the sentence "thicker cirrus…" is incorrect. Why only thick cirrus? Also, most cirrus aren't optically very thick.

- Lines 319-320:

I am not sure if the interpretation of the result of Sanderson et al., 2008 is correct, so it may need to be rewritten. Sanderson et al., 2008 found the IC fall speed to be important in modulating the mainly LW cloud feedback (and hence climate sensitivity) not because the IC fall speed would change between the present day and global warming simulation (IC fall speed is not calculated interactively in their simulations, given the use of a tuning parameter). However, a smaller ice fall speed leads to more high clouds. That in turn leads to a larger LW altitude (positive) cloud feedback, which is the dominant high cloud feedback. On the other hand, a smaller present-day cloud fraction due to large ice fall speed, leads to less high clouds and a smaller high cloud feedback and smaller climate sensitivity.

- As you talk about ice clouds and not only cirrus, you may want to also explore/mention the cloud phase negative optical feedback due to global warming (Tan et al., 2016, maybe also Bodas-Salcedo 2018 and 2019, Lohmann and Neubauer, 2018).

- Conclusion (in general):
  It may be appropriate to think a bit more about some of the questions I listed below and include some of that in the discussion:

  What did you learn about the model by exposing the number tendencies that you couldn't by simply taking the ICNC fields?
  Is there something that we should be worried about? Why? What is causing it?
  What are the potential weaknesses of the study? How does this compare to other work (if any exists – maybe for mass rates)?

References

- Bodas-Salcedo, 2018: Cloud Condensate and Radiative Feedbacks at Midlatitudes in an Aquaplanet
- Bodas-Salcedo, 2019: Strong Dependence of Atmospheric Feedbacks on Mixed-Phase Microphysics and Aerosol-Cloud Interactions in HadGEM3
- Dietlicher et al., 2019: Elucidating ice formation pathways in the aerosol-climate model ECHAM6-HAM2
- Gryspeerdt et al., 2018: Ice crystal number concentration estimates from lidar-radar satellite remote sensing. Part 2: Controls on the ice crystal number concentration
- Gryspeerdt et al., 2018: Technical note : An automated cirrus classification
- Hartmann and Larson, 2002: An important constrain on tropical cloud-climate feedback
- Lohmann and Neubauer 2018: The importance of mixed-phase and ice clouds for climate sensitivity in the global aerosol–climate model ECHAM6-HAM2
- Sanderson et al., 2008: Toward Constraining Climate Sensitivity by Linear Analysis of Feedback Patterns in Thousands of Perturbed-Physics GCM Simulations
- Sourdeval et al., 2018: Ice crystal number concentration estimates from lidar-radar satellite remote sensing - Part 1: Method and evaluation

- Tan et al., 2016: Observational constraints on mixed-phase clouds imply higher climate sensitivity