

REVIEW FOR RAATIKAINEN ET AL.

This modeling study examines whether the Hallett Mossop process can explain the high ICNCs observed within an Arctic stratocumulus deck during ACLOUD campaign, using LES simulations. Their results show that the current widely-used parameterization for the Hallett-Mossop mechanism cannot reproduce observations, unless its efficiency is enhanced by factor of ten. Alternatively, other microphysical formulations (e.g. terminal velocity calculation or HM temperature efficiency) should be modified in certain ways that can positively impact ice production from this process, to achieve a realistic ICNC representation.

The large discrepancies between INP-ICNC observations in Arctic clouds have been documented for a long time, with SIP being hypothesized as the potential cause. However, atmospheric models still struggle to reproduce the microphysical structure of Arctic clouds, even when SIP parameterizations are accounted for. This is largely due to the fact that SIP descriptions remain highly uncertain, which is why sensitivity studies like this one are very useful for the cloud modeling community.

Overall, I find the design of the study and the sensitivity tests satisfying, however I believe that results are not properly discussed. For example, while observations of micro- and macro-physical properties are available, they are not used for evaluation of e.g. different schemes or different HM formulations. Moreover, there is hardly any discussion on the differences in using different microphysical schemes (these sensitivity tests are neither discussed in the abstract nor in the conclusion section). I also think there some inconsistencies between the background information provided in this study and the relevant papers that are cited as reference.

A more detailed list of comments is given below. I don't think it would take a long time to the authors to address these - for this reason I consider the suggested revisions as minor.

Main comments:

Line 10: 'SIP depends strongly on model parametrizations', this is a very generalized statement (and not anything new). Should become more specific on what kind of parameterizations you refer to

Line 14: '*cooler marine regions*', cooler than what

Line 20: '*simplified cloud microphysics*.' Could you provide reference? I do not necessarily agree. I think two-moment fully prognostic schemes are rather complex

Line 32: '*The process is effective at cooler temperatures around -15 °C*'. Takahashi et al. (1995) with a rather unrealistic set-up showed maximum efficiency round -15°C. However, numerous more recent studies have shown that this process can be active over a much wider temperature range

Line 34: Not true. Figure 7 in Keinert et al (2020) shows that the process can be effective above -10°C. It is **more** effective below this temperature

Line 187-189: What was the criteria for adjusting surface fluxes and subsidence? These choices can have a large impact on the simulated LWP/TWP.

Line 87-89: I don't understand why the existence of large rimed ice crystals are only indicative for the Hallett-Mossop and drop-shattering process. The efficiency of collisional break-up highly depends on ice particle size and riming (Phillips et al. 2017)

Line 243-245: RSx10 overestimates ICNCs and underestimates LWP. This is not addressed in the discussion. Overall it would be helpful if all ICNC and INP units are the same throughout the whole text to facilitate comparison. E.g. observations are given in L^{-1} , while all simulated values in kg^{-1} .

Figure 2: It should also illustrate a timeseries of IWP to assess which simulation is more realistic compared to the measured values ($4.1\text{--}9.5\text{ g m}^{-2}$). A shaded area in the individual panels that indicates the observed ICNC, LWP and IWP ranges would also be very useful for evaluation

Line 245-246: I don't understand why the RSx5 simulation is not considered more realistic since it better reproduces the observed ICNCs. It also gives more realistic LWP values within the observed range ($48\text{--}82\text{ g m}^{-2}$). Could the time-delay of ICNC enhancement in this simulation be due to the short spin-up time? (not enough to allow cloud dynamics to develop in time, which are critical for many microphysical processes?)

Line 250-251: This statement is not clear; what is considered high and low INP value? 'Moderate' values also produce the desired ICNCs when combined with SIP

Line 253: Not clear. What do you mean by the term 'removal mechanism'? Do you refer to cloud condensate removal, ice mass or liquid mass removal? The different LWP values for example suggest different impact on liquid mass removal mechanisms.

Line 255-257: I am not sure to which sensitivity simulation in this study do you refer to. In their RS experiment they showed that the simulated ICNCs were 10-20 times lower the observed - not necessarily that the ice production rate should be multiplied by 10 (which is the case with SI rate in Young et al). In their DM10_SIP experiment indeed they multiplied primary ice production (DeMott et al 2010) by a factor of ten, but this simulation accounted for many SIP mechanisms (not just RS). The relevant statement should be more accurate to avoid misinterpretation. Moreover, they showed that the combination of RS with another SIP mechanism was essential to reproduce the observed ICNCs. Maybe along these lines or during the last section it should be discussed that the RS multiplier could actually account for the missing contribution of another SIP mechanism

Figure 3: It would be very useful to see how timeseries of LWP also differs between the two microphysical schemes. Again the observed range of values should be marked in the Figure to facilitate evaluation

Section 3.2: In my opinion the similarities between the two simulation set-ups is overemphasized, while there is hardly any discussion about the obviously statistically significant differences. ICNC is 35% larger (which is mentioned), however IWP is also twice as larger in SALSA. LWP might also reveal notable differences.

Line 283: I guess SIP rates are added to the ice crystal number tendency equation in the code. Why is it stated here that ICNCs are not directly related to SIP rate?

Line 316-317: I would also argue that SB despite being simplified, it agrees more with observations (ICNC, IWP, etc)

Figure 7: Timeseries of IWP would likely be useful too

381-382: I still do not understand why collisional break up is not considered a potential mechanism. Have you looked for fragmented ice particles in the probe images?

Lines 409-410: I don't understand why a better fit to the temperature measurements was not used for the LES initialization. Clearly the authors preferred to adapt a warmer and more unstable profile.

Conclusions: results related to the different microphysics schemes are not discussed

Typos:

Line 320: water content (**LWC**)

Line 359 : Hallett