Answers to the reviews of “the impact of secondary ice production on microphysics and dynamics in tropical convection” by Qu et al.

Reviewer 1:

The topic of this paper is very timely, with a recognised need to study and quantify the impacts of SIP. Tropical convection in particular is important to quantify as the tropics are thought to be a region where cloud-climate feedbacks may be substantial, and there are many uncertainties. The approach is also state-of-the-art with high-resolution modelling and observations. I think the paper is a worthy topic for ACP, and has some important findings so should be accepted, but I felt a few points needed to be addressed. An important part is bringing the important evidence to the forefront of the paper so that it is very clear to the reader. I have made some suggestions below that may help.

Thank you very much for your positive review! We answered your questions in paragraphs written in blue color.

General

The paper presents compelling evidence that SIP mechanisms are important in a tropical MCS. If I have understood correctly I think the strongest evidence is presented in Figures 8 (histograms of Nice); perhaps Figure 9a; and Figure 18 and 19a.

If this is the case, and you agree with me, could the paper be arranged to highlight these points so that it is clear what the main evidence is? E.g. the large ice crystal number mode seen in histograms of the observational data could only be reproduced with SIP switched on in the model. And the broad distribution of IWCs, extending to large values could only be reproduced with SIP switched on. Maybe this could be highlighted in the abstract?

We made the following change in the abstract to highlight the points (in bold):

“Secondary ice production (SIP) is an important physical phenomenon that results in an increase of ice particle concentration and can therefore have a significant impact on the evolution of clouds. In this study, idealized simulations of a mesoscale convective systems (MCS) was conducted using a high-resolution (250-m horizontal grid spacing) mesoscale model and a detailed bulk microphysics scheme in order to examine the impacts of SIP on the microphysics and dynamics of a simulated tropical MCS. The simulations were compared to airborne in situ and remote sensing observations collected during the High Altitude Ice Crystals - High Ice Water Content (HAIC-HIWC) field campaign in 2015. ***It was found that the observed high ice number concentration can only be simulated by the models which include SIP processes. Inclusion of SIP processes in the microphysics scheme is crucial for the production and maintenance of high ice water content observed in tropical convection.*** *It was shown that SIP can enhance the strength*

of the existing convective updrafts and result in the initiation of new updrafts above the melting layer. Agreement between the simulations and observations highlights the impacts of SIP on the maintenance of tropical MCSs in nature and the importance of including SIP parameterizations in models."

There is a question though because the model fails to produce larger concentrations and water contents at the higher altitudes (11-12 km). Hence, the question I would ask is, is the modelled storm strong enough compared to the observation. If the model were more intense you may find that more ice would be present in both the 6-7 km bin and the 11-12 km, so you may not require quite as much SIP to explain the observations. I think it is worth considering.

Very Good question. Apart from this study with idealized simulations, we also conducted some real case simulations with both BASE-3ICE and SIP-3ICE configuration over Cape Verde (CADDIWA field campaign, https://www.safire.fr/en/content_page/campagnes-en-cours/caddiwa.html). From these real case simulations, we had very similar conclusions: baseline simulations significantly underestimated the IWC and N_i, whereas SIP simulations gave much better agreements to the observation data. This might be an indirect confirmation that the idealized simulations used in this study dose give, to a certain degree, reasonable results for the tropical MSCs.

In addition to this, we also did some further tests with different w_{max} (please see the answer for the next question). A higher w_{max} dose increase the IWC for the BASE-4ICE simulation, although the impacts are relatively small and will not change the conclusion that BASE-4ICE simulation significantly underestimated the IWC in both altitude ranges. Increasing from w_{max}=10 m s^{-1} to 15 m s^{-1} for CTR simulation, the IWC will increase 20 % and 50 % for 6-7 km and 11-12 km. For SIP-4ICE simulation, the increases will be 1 % and 63 % respectively. The increased values (w_{max}=15 m s^{-1}) of IWC of CTR simulation are still much lower than the non-increased value (w_{max}=10 m s^{-1}) of SIP-4ICE simulation. For higher altitude between 10 and 11 km, the increase of IWC for SIP-4ICE simulation will only improve the agreement between SIP-4ICE and observation. However, since the scope of the paper is to study the impact of adding SIP in the simulation, we think it's better to keep the w_{max} the same for all simulations. A further clarification is added to the text to briefly describe the possible impact factor of w_{max} (please see the answered of the next question).

Did you do any simulations with a stronger updraft forcing to investigate the sensitivity to the updraft forcing?

We made additional tests with different maximum updraft forcing speeds. A stronger initial updraft forcing speed produces slightly stronger convection therefore larger IWC (0-20% increase below 9 km, up to 50% between 10-11 km). The impact on N_i is range from -44% to 63% below 11 km. However, both SIP simulations (w_{max}=10 or 15 m s^{-1}) still produce much higher IWC and N_i in most of altitudes between 5 and 11 km comparing to those from both CTR simulations. One particular note is that between the altitudes of 10 and 11 km, SIP simulations with w_{max}=15 m s^{-}

[1]. does produce higher IWC (63% higher mean IWC). Therefore, the underestimation of IWC shown in Fig 9b might be partly caused by the uncertainty of the strength of the convection. We added a short description at the end of section 5.3:

"Another possible explanation of the underestimation is the uncertainty of the strength of simulated convections. In this study, the maximal updraft nudging speed w_{max}=10 m s^{-1} is used as the default value. Simulations with different w_{max} are also tested. Using w_{max}=15 m s^{-1} in SIP-4ICE simulation will produce 63% higher averaged IWC between 10 and 11 km than that produced by default SIP-4ICE simulation with w_{max}=10 m s^{-1}. For SIP-4ICE simulation, the impact of w_{max} for lower altitudes between 6 and 7 km is negligible. The CTR simulation with w_{max}=15 m s^{-1} produces higher IWC compared to the default CTR simulation with w_{max}=10 m s^{-1} (20% and 50 % higher for altitudes range of 6-7 km and 10-11 km respectively). However, these values are still significantly smaller than those of the default SIP-4ICE simulation."

Specific

In the introduction it may be worth mentioning the recent study by James et al. 2021, ACP. On the importance of SIP during drop – ice interactions.

The study of James et al. (2021) was referenced following the reviewer's comment.

"systematic studies of the effect of SIP on cloud microphysics with the help of cloud simulations have begun only in the last few years" I think this is not very accurate, there have been other previous studies.

We change the phrase as:

"For the last few years, there are many new efforts on systematic studies of the effect of SIP on cloud microphysics with the help of cloud simulations"

Page 3, lines 92-95. It is quite vague to say this paper uses "simplified parameterizations" here, with no extra detail. I would prefer to have this in the paper where it could also be justified. As written, I don't have much confidence in the modelling. It gave me a negative feeling, without knowing exactly what was done at this stage.

We changed the phrase into:

"In the absence of a consensus on SIP parameterizations, these two processes were described by two specific parameterizations proposed in the literature, which provide a sufficient enhancement of Ni above the melting layer consistent with in situ observations in the MCSs"

Equation 2: I am guessing that this is added to the vertical wind as an extra term? i.e. so wt = Equation 2 + w_model … otherwise there would be no downdraft. Is this the case? Also, Equation 2 only gives positive vertical winds.

Line 168 – typo on this line “there have been”

Done.

Equation 3 does not give any dependence on temperature, whereas we know there is a thermal peak around -15 deg C. I think this could be mentioned / clarified in the text. Could this affect your results? Maybe the thermal peak could lead to more ice in the 11-12 km bracket, if the ice particles were mainly formed higher up in the cloud, and did not have chance to grow to large sizes and precipitate out?

Very good question! In current version, the approach for FFD is not temperature dependent except for the temperature range. We are currently looking into the impact of the thermal peak around -15°C as well as the gradual activation of the FFD near the temperature of -3°C (particularly for winter temperature inversion cases, e.g. freezing rain). Both could have strong impacts on the simulation results under specific conditions. We are planning to deal with this topic separately with a dedicated study. On the other side, the current paper aims to demonstrate the impacts of just adding significant number of secondary ice in the high-res simulations while being aware that there are still large knowledge gaps on the SIP processes (very different FFD rates from literature and 5 other mechanisms are not modeled at all). We added a clarification in section 3.3.2:

“Within the temperature range, the current parameterization is not temperature dependent. Further studies are undergoing for exploring the impacts of variation due to temperature.”

Page 16, point 2 line 486. There is an argument by Wojciech Grabowski (https://acp.copernicus.org/articles/21/13997/2021/acp-21-13997-2021.pdf) that the increase in temperature obtained is offset by the weight of condensate in the air (see argument on page 2 of the above paper). It seems to be in conflict with your point 2. Maybe it is the vapour growth rather than the freezing step that leads to the increase in buoyancy?

The series of works by Grabowski (including the one mentioned above) are related to the problem of convection invigoration in liquid clouds. The time of phase relaxation (τ_p) in liquid clouds is typically quite low, and it ranges from a few seconds for maritime clouds to tenths of a second for continental convective clouds. The time of phase relaxation characterizes how quickly the system consisting of a population of cloud droplets and water vapor will get to equilibrium. In the consideration of invigoration of convection, it is important how much water vapor excess is available and how quickly the excess water vapor above the saturation point can be depleted by droplets to generate buoyancy sufficient for the convection invigoration. The main point of Grabowski’s works is that in liquid convective clouds supersaturation and τ_p is typically small and enhancement of droplet concentration will result in a relatively minor increase of buoyancy. In our study, we are considering primarily ice clouds, which typically have a time of phase relaxation of the order of minutes and tens of minutes (e.g., Korolev and Mazin, 2003, JAS, https://doi.org/10.1175/1520-0469(2003)060<2957:SOWVIC>2.0.CO;2). Such cloud may remain in high supersaturation over ice for a long time (e.g., Korolev and Isaac, 2006, JAS, Fig.10, https://doi.org/10.1175/AMSMONOGRAPHS-D-17-0001.1). In this case, if a large number of ice particles is introduced in such environment, they will rapidly deplete water vapor due to small phase relaxation time and release latent heat, which will induce and/or invigorate the convection.

In the point 2 of the conclusion, we stated that the latent heat from vapor deposition is the main source for the increase in buoyancy. However, one phrase in the section 6.2 appears confusing:

"With large RWC and rain mean-mass diameter, the rising parcels had a high SIP potential, which eventually led to increased freezing and increased latent heating and buoyancy, thereby enhancing secondary convection."

We change it to:

"With large RWC and rain mean-mass diameter, the rising parcels had a high SIP potential, which eventually led to greater N_i, vapor growth on ice, increased latent heating and buoyancy, thereby enhancing secondary convection."