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

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.

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?

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.

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

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.
- “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.
- 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.

- 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"
- 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?
- 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?