Review of “Secondary ice production processes in wintertime alpine mixed-phase clouds”

The paper uses data from CLACE-2014 and the WRF model to investigate possible explanations for the observations of high ice particle concentrations in mixed-phase orographic clouds. The findings suggest that surface sources of ice particles are not the main source, but the collision of ice particles with other ice particles, leading to fragmentation can explain the observed high ice particle concentrations. An enhancement of 3 orders of magnitude is seen in the simulations, which is broadly consistent with the observations.

I felt the introduction was too long. The material is good, but it felt more like a “literature synthesis” section, rather than an introduction. The introduction could be shorter, contextualising background information, stating the problem, and stating the response. The literature review is good, but I felt there was a bit too much detail for an introduction and it could be in a separate section after the introduction.

Page 10, equation 1: my understanding of the results of Takahashi et al. (1995) is that the ice fragments are produced during collisions with rime particles. Further, Vardiman (1978) also suggests it is more the rimed particles that produce fragments. Therefore, is it appropriate to say that collisions between cloud ice-snow and snow-snow produces fragments? In Takahashi et al.’s paper one of the ice spheres grew by collection of supercooled water drops. They say “our laboratory results support Takahashi (1993) hypothesis that ice crystals are generated by collision between large and small graupel”. So I am unconvinced that collisions with un-rimed particles should produce fragments. I see the scaling in equation 2, but this is more to do with size, rather than degree of riming.

The second scheme (equation 3) appears to be more realistic, however it also requires more inputs, and it wasn’t clear how these were calculated. For example, the rimed fraction of the most fragile ice particle – is this calculated in the model? How? Is it based on the observations (e.g. line 503)? This could be stated here for clarity. If it is based on the observations, how was it calculated? Or was it an estimate from images? If so, are the images statistically representative?

Mode 2: “this mode has been studied only once in the laboratory study of Latham… “ there is now another study, see James et al. (2021).

I think it would be worth mentioning where the Lawson DS parameterisation comes from. You mention the laboratory studies for other secondary ice mechanisms, but don’t mention that Lawson et al. is based on fitting a curve in a model so that the model matches aircraft measurements. The shape of the curve is specified priori to have a D^4 dependence.

Reading through the rest of the results and discussion I am in agreement with the findings of the paper. Figure 3 is quite convincing that ice-ice collisional breakup should be important in these cases. Maybe the Takahashi parameerisation as implemented overestimates the number of ice particles compared to the Phillips implementation (because it is applied to all ice categories). I think this would be worth mentioning anyway, because it seems like applying it to all ice categories should lead to an overestimation.

I agree that the DS mechanism may not be strongly active in these cases because observations showed there were not many large drops present.