This paper by Sepulveda Araya et al compares ice-cloud radiative heating rates derived from four different radiative transfer schemes. The paper is very well motivated, well organized, and well written. The analysis is sufficiently thorough to explain why the different schemes produce different heating rates, and the presentation and discussion are very clear. The results of the intercomparison are quite nuanced, but there are two primary points: (1) accounting for ice crystal complexity generally weakens CRH, and (2) temperature-dependent schemes can lead to large CRH differences at cold temperatures characteristic of the upper troposphere.

The paper is well suited for ACP and will appeal not only to readers in the ice microphysics, cloud-radiation, and cloud-climate communities. I learned a lot from the paper, and think it has several valuable lessons for folks such as myself, who care about the role of ice clouds in climate but have little technical knowledge of the radiative transfer schemes on which our work relies.

The paper could be published as is, with some technical corrections. Nevertheless, I had a couple of general suggestions and number of line comments (mostly just clarification), so I will suggest minor revisions in case the authors wish to incorporate any of it. I look forward to seeing the paper published.

-Adam Sokol

General comments/suggestions (all purely optional)

1. The figures and analysis all focus on vertically resolved CRH which, for reasons discussed in the Intro, is important for many reasons. Layer-averaged CRH may provide an additional, simpler perspective on interscheme differences. In Fig 2b,e, for example, showing layer-averaged CRH would indicate whether the positive and negative lobes of the dipole cancel out, or whether the net effect is nonzero. This could help qualify which downstream impacts may be affected by interscheme differences; for example, a layer-averaged difference near 0 wouldn't be so important for planetary-scale circulation, but ~10 K/day certainly would be.

   For Fig S8 showing the layer-averaged CRH would convey whether the total heating is dependent on cloud thickness, or if the total is invariant but distributed differently throughout the cloud as the thickness varies. If the schemes produced different results in this regard, I would think that would be quite interesting.

   Another option (closely related to the layer-averaged heating) would be to show the TOA CRE on top of each heating matrix. The CRH differences suggest that the TOA CRE differences would be very large in some of these cases.

2. The assumption of vertically uniform q_i and effective radius seems significant, since observations show that this is typically not the case. Uniformity is certainly the most simple approach, and prescribing some vertical structure would also involve big assumptions, so I agree with the authors' approach here. And while adding some vertical structure would affect the absolute values of CRH, my

intuition is that it would not have a big qualitative impact on the interscheme differences. However, I think a brief discussion of this is probably warranted, either in the Methods or Discussion section.

3. The explanations of interscheme differences focus on three parameters: the absorption coefficient, asymmetry parameter *g*, and SSA. It would be helpful to clarify the impact of ice crystal habit assumptions on these three parameters early in the paper, perhaps when they are first introduced around line 50 or when the schemes are compared in section 2.1. The sentence on line 169 implies that habit assumptions manifest through the *g* – I am not sure if it is also supposed to imply that habit has little impact on *K_abs*. Either way, it might be useful for many readers to have a couple of sentences early on that explicity connect the dots between habit assumptions and these three important parameters.

Line comments

Line 58 - I thought this sentence was saying that the global ice CRE was somehow positive when surface roughness was taken into account. I checked the citation and realized the 2 W/m2 was *relative* to the case without surface roughness. This could be clarified with wording such as "…roughness, which can increase the global SW CRE by 2 W/m2…"

Line 108 – while it's nice to see a citation, Sokol et al (2024) did not feature any RTM simulations

Table 2 – the meaning of the Temperature column is specified for tests 2a and 2b, but not for the other tests. I think these are cloud top temperatures, but it would be good to specify.

Lines 135-136 – I am curious to what extent the lower air density can explain the increase in maximum heating rate as the cloud is moved upward. There may also be a contribution from the increasing difference in temperature between the lower-tropospheric emission temperature and the ice cloud temperature. Quantifying the impact of the density change would be relative straightforward using Eq 1, converted to height units so that density appears explicity.

Line 137 – I am confused here about q_i being the mixing ratio *per volume air*. I understand the point being made, but I find this wording confusing.

Line 146 – here co-albedo is defined as the "amount of absorbed to scattered radiation". Shouldn't this be the ratio of absorbed to total attenuated (absorbed+scattered) radiation?

Line 178/182: is it more accurate to say that the Baran schemes show stronger SW heating at the smallest cloud thicknesses (as opposed to the highest altitudes)? For most of Fig 3, the opposite is true, with weaker heating in the Baran schemes regardless of altitude. Is the dependence of this result on cloud thickness just a reflection of the underlying dependence of SSA and *g* on q_i?

Line 183: again, useful to specify that the “stronger absorption than in Fu” is only at the very low end of cloud thicknesses.

Line 212: **a**cross

Line 213: “10% less cloud-top cooling than the Fu scheme”. Fig 4h shows negative values at cloud top for the deep clouds. Doesn’t this mean *more* cloud-top cooling than Fu?

Line 224: “show **o** in-cloud LW…”

Line 234: excepting -> except

Line 238: Does the trend really disappear below an IWP of 10 g/m2? Or is it just that cloud-top CRH in the Fu scheme changes sign? It seems the general trend of weaker CRH values (be they positive or negative) still applies below 10 g/m2.

Line 290: based on the earlier discussions, doesn’t forward scattering act to increase CRH? As written here, it seems to suggest the opposite. Maybe the wording could be adjusted to make clear that it is the absorption capacity that wins out in spite of the increase in forward scattering?