Atmos. Chem. Phys. Discuss., 14, C6520–C6526, 2014
www.atmos-chem-phys-discuss.net/14/C6520/2014/

Atmospheric Chemistry and Physics Discussions

Interactive comment on "A two-habit model for the microphysical and optical properties of ice clouds" *by* C. Liu et al.

Anonymous Referee #1

Received and published: 4 September 2014

General comments

The paper presents a two-habit mixture model to represent the general microphysical, optical and radiative properties of cirrus. The authors base their two-habit mixture model on recent observations that show ice crystals evolving from simple compact shapes to more spatial aggregated shapes with increasing ice crystal maximum dimension (observations that are generally shown throughout the literature as well as aggregation simulation studies). The habit mixture can be changed continuously across the PSD without discontinuities. The authors use the latest light scattering methods to compute the single scattering properties of their model and include both surface roughness and hollow hexagonal cavities. They make extensive use of in situ, laboratory, and satellite remote sensing measurements, inclusive of polarization, to show

that some form of habit mixture is required to consistently simulate the observations from across the spectrum and as a function of scattering angle. A single hexagonal column model is shown not to simulate the same observations to the same degree as the habit mixture model. The results presented are worthy of publication and the paper is thorough and understandable. Although their model consists of only two habits, the second habit is composed of 20 hexagonal monomers, this leads to the following questions and points that the authors need to address before the paper can progress to ACP. The points and questions below are not considered major but need to be addressed to improve the paper. Points to consider.

1. The aggregate model consists of 20 monomers, why 20? Why not 10, 15, 18? Please justify why 20 has to be used. Is it the case that to satisfy the measurements of Dmm and IWC this many monomers is required? From light scattering calculations it is shown in Figures 5 and 6 of Baran (2009) that adding hexagonal monomers beyond 3 components does not significantly change the phase function (asymmetry parameter) due to the aggregates being spatial, i.e., multiple scattering between monomers is not significant. Indeed, the g-values are shown to asymptote. The results contained in Baran (2009) are based on an ice aggregation model developed by Westbrook et al. (2004). In the case of the aggregate model proposed by the authors how many monomers are required for the phase function (asymmetry parameter) to asymptote?

2. In the construction of the aggregate model are intersecting planes avoided? This should be stated.

3. Please could the authors state the orientation-averaged area ratio and fractal dimension of their aggregate model in the paper and how do these compare to observation?

4. In the appendix please also include the full co-ordinate (x,y,z) geometry of the hexagonal aggregate model and are the aspect ratios of each monomer kept constant at a value of 1?

5. The definitions of maximum dimension between the SCM and ice aggregate are

not the same. If definitions were the same what effect would this have on your calculations when comparing properties of the same maximum dimension? This will not fundamentally alter their conclusions but will impact their calculations to some degree, the question is how important is it? Is the definition of maximum dimension applied to the aggregate robust under different viewing geometries?

6. I dispute the use of the term “spectral consistency”. What is shown in the paper is that the two component model is more consistent between 5 wavelengths, and the wavelengths are composite band-averages rather than monochromatic differences. To be truly spectrally consistent the authors need to show that the model is monochromatically consistent across high-resolution radiance spectra spanning the visible, near-ir and long-wave regions as demonstrated by Baran and Francis (2004). At the moment, the authors may only state that their model fits composite band-averaged measurements comprising of five wavelengths.

7. Figure 4. Could the authors be more quantitative? especially when measurements of IWC are over many orders of magnitude. I suggest plotting PDFs of measurements and model results over intervals of Dmm and IWC and using a statistical method to quantify the goodness of fit?

8. Figure 6. These are bulk comparisons. The authors employ a number of different light scattering methods to compute the scalar optical properties as a function of D. I would also like to see a figure showing a plot of the scalar optical properties as a function of maximum dimension to show that there are no discontinuities occurring between the different light scattering methods.

9. The paper does not at all discuss how cloud vertically inhomogeneity and 3D cloud effects may impact their results. Some discussion of these effects is warranted, especially with regard to the more recent study by Fauchez et al. (2014), found here http://www.atmos-chem-phys.net/14/5599/2014/acp-14-5599-2014.pdf

Minor points and typos

1. Page 19547 line 10, “an ensemble habits”-> “an ensemble of habits..”

2. Page 19547 line 15 consider adding this citation Baran et al. (2014), as the paper demonstrates the importance of constraining habit mixture models and PSDs, assumptions regarding the former and latter are shown to significantly affect climate model calculations of SW and LW fluxes at TOA (Baran, A., P. Hill, K. Furtado, P. Field, and J. Manners, 2014: A Coupled Cloud Physics-Radiation Parameterization of the Bulk Optical Properties of Cirrus and its Impact on the Met Office Unified Model Global Atmosphere 5.0 Configuration. J. Climate. doi:10.1175/JCLI-D-13-00700.1, in press.)

3. Page 19548. In the discussion of surface roughness a citation to Ulanowski et al. (2014) should also be added, which can be found here http://www.atmos-chem-phys.net/14/1649/2014/acp-14-1649-2014.pdf

4. Page 19548, the word “numerous” is in my opinion not justified as Figure 7 shows one example of a laboratory measured phase function and one example of an in situ measured phase function. Please re-write accordingly.

5. Page 19548, when discussing the PN I believe you are missing a number of Gayet et al. citations. Please include some of those citations in your manuscript.

6. Page 19549, line 1. All the citations are somewhat biased towards particular groups, what about work that has used the dual-viewing ATSR-2 instrument and multiple viewing MISR, for instance McFarlane et al citation should also be added here. Here are some suggestions. http://onlinelibrary.wiley.com/doi/10.1029/2007JD009191/abstract
http://onlinelibrary.wiley.com/doi/10.1029/1999JD900842/abstract
http://www.opticsinfobase.org/ao/abstract.cfm?uri=AO-44-19-4060

7. Page 19552. Lines 13-19. This argument is only true if the monomers making up the aggregate are sufficiently separated from each other so that multiple scattering between monomers is negligible. There might be instances where the constructions are such that the phase functions could be different between different realizations due

to multiple scattering between monomers.

8. Page 19551, line 4. The authors state "..seldom does an cloud model..." This statement has been addressed by Baran et al. (2014), whom show that an ensemble model can indeed be consistently applied across the spectrum to simulate different measurements from the UV to radar frequencies, see http://onlinelibrary.wiley.com/doi/10.1002/qj.2193/abstract

9. Page 19553, line 16. Please add in the discussion of surface roughness the Ulanowski et al. (2014) citation and also Ulanowski et al. (2006), which is found here http://homepages.see.leeds.ac.uk/∼lecsjed/huiyi/habit/habit_Aug_2011/papers/sdarticle%5E

10. Page 19553, line 20, the application of surface roughness to the two component model is this applied to all sizes? If so, please state this or the size range over which it is applied.

11. Page 19554, is the determination of in situ IWC based on the PSD integration assuming some mass-D relationship or bulk measurements of IWC? In the former case, is the exponent assumed in the mas-D relationship the same as the fractal dimension of your aggregate model?

12. Following equation (1) Dmm= should follow?

13. Page 19555, line 3, "..is the density of ice"-> "..is the density of solid ice.." and please state the density of solid ice assumed.

14. Page 19555, line 8, the 11 field campaigns, the PSD measurements, what was the range of particle size measured? Was the maximum particle size < 1 mm? This is important as it has implications for the effects of shattering on their dataset, see Korolev et al. (2013). Indeed, in this section the authors should state whether their datasets are affected by shattering or how shattering was minimized in their case.

15. Page 19555, line 12 suggest replacing "under" by "colder than"

16. Page 19556, line 23, they use the term "solved", their "solution" is not unique as a combination of microphysical models could be used to give similar results, I believe the word "solved" is not warranted. Please re-write this sentence accordingly. Indeed, the above paragraph lines 12-21 directly contradict the statement contained in section 4.

17. Page 19557 line 5, do they mean "increases" rather than "decreases" as particle size increases? (Auer and Veal, 1970).

18. Page 19557, section 4.1. I assume all single-scattering calculations are for random orientation? If so please state this.

19. Page 19558, line 5, suggest "repeated" rather than "recaptured".

20. Page 19558, line 29, how well does the value of the asymmetry parameter of the habit mixture model compare against observations?

21. Title section 4.2, no need for the word "the" in the section title.

22. Page 19559, line 15, there are exceptions to featureless phase functions at backscattering angles such as the cases discussed by Gayet et al. (2012) and Baran et al. (2012). http://www.atmos-chem-phys.net/12/9355/2012/acp-12-9355-2012.pdf and references therein and other studies.

23. Page 19559, in the discussion of number concentration measured by the PN on line 21, the likely effect of shattering on this instrument should be discussed.

24. From Figure 8, what is the value of the phase function at 180o given by the two component model? And how does this value compare against the CALIOP observations given in Baum et al. (2011)?

25. Page 19561, line 9, do you mean the far-infrared? In which case you should cite Cox et al. (2010) located here http://onlinelibrary.wiley.com/doi/10.1002/qj.596/abstract;jsessionid=AA7EBE2992CB5F4DD

Interactive comment on Atmos. Chem. Phys. Discuss., 14, 19545, 2014.