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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 #2

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General comments

Efforts to simplify complex systems into the governing system fundamentals can greatly advance scientific understanding, and this paper may represent such an effort. The paper is well organized and clearly written, and worthy of publication in ACP. However, there are some serious drawbacks that need to be addressed before it can be published in ACP, as described below.

Major comments:

1) While this paper does address ice cloud microphysics, it is only addressed to the extent necessary for obtaining optical properties from a given ice particle size distribution (PSD). This point needs to be made more clearly in the paper. Thus the content of

the paper may be more appropriately expressed using a title like "A two-habit model for the optical properties of ice clouds". After all, the model conserves two microphysical properties but does not predict them.

2) Page 19555, lines 6-7: Since D = ice particle maximum dimension, it is not possible for both Vc(D) and Va(D) to be proportional to D3. Numerous papers show Va(D) to be roughly proportional to D2, and the exponent on D for Vc(D) lies typically between 2.5 and 3. So what expressions were used to represent Vc(D) and Va(D)?

3) Page 19555, lines 13-15: Here it states that Eqns. 1 and 2, using the THM habit fractions, can be used to calculate IWC and Dmm from the observed PSD. A research paper should provide the necessary information that allows other investigators to test the study's findings. This is not possible for this study since the relationships for Vc(D) and Va(D) are not reported, but evidently these volumes are calculated as described in Appendix A. It could be very useful to the cloud physics community if these volumes could be related to their maximum dimension D in log-log space, with V-D power laws given. For example, this may allow other investigators to generalize the results reported in Fig. 4 to other cloud physics applications.

4) Page 19555, lines 13-15: Do these calculations use the gamma PSD fits noted above in lines 10-11? One might assume that they do, or else why are the gamma fits mentioned? Nonetheless, this point should be clarified.

5) Page 19556, lines 3-11: The agreement between measured and computed IWC in Fig. 4 is remarkable; so remarkable that it is hard to believe if taken at face value since. That is, direct in situ measurements of IWC (e.g. CVI or CSI probes) compared against IWC calculated from collocated PSD measurements, using either ice particle mass-dimension (m-D) or mass-area (m-A) power law relationships, show a great deal of scatter; see for example the comparison in Fig. 6 of Lawson et al. (2010, JGR). This is the best agreement I have seen between direct measurements of IWC and IWC calculated from measured PSD & m-A expressions, and still there are differences of

a factor of 2 or even 3. Questions that naturally arise when inspecting Fig. 4 are: a. What are these measured IWCs? Are they direct measurements from probes like the CVI, or are they calculated from PSD measurements assuming some m-D or m-A expression(s)? b. If they are calculated from PSD measurements, what m-D or m-A expressions were used? c. What are the expressions for Vc(D) and Va(D), used to calculate IWC in the THM? If the measured IWC was calculated from observed PSD, then what is actually being compared in Fig. 4 are two calculations; one based on observed PSD and some undisclosed m-D or m-A expression(s) and the other based on Eq. 1 in the THM. If this is the case, then the agreement observed in Fig. 4 is plausible since much of the natural variability will be removed by invoking the m-D or m-A expression(s). The same concerns noted above for IWC also apply to the Dmm comparisons.

6) Page 19557, line 5: The convention in cloud physics for aspect ratio is to define it as more than or equal to 1.0 for columnar ice crystals and less than or equal to 1.0 for planar ice crystals (e.g. Lamb & Verlinde, 2011: Physics and Chemistry of Clouds, Cambridge; see Ch. 8).

7) Page 19559, end of Sec. 4.1: Please comment on the importance of the random distribution of aspect ratio and size regarding the aggregate components. For example, to what extent do the optical properties change when a realistic fixed aspect ratio/monomer size assumption is imposed?

8) Page 19560, line 9: Wang et al. (2014) is not referenced. Wang et al. (2013a) and (2013b) are referenced, but are not cited in this paper apparently. The Wang et al. 2013 papers do not retrieve the scattering phase function of ice clouds from satellite observations (which the Wang et al. 2014 paper allegedly does).

9) Page 19560, line 20: How is Deff defined in this study? Some investigators use extinction to define it, others use PSD projected area, and so on.

10) Page 19560, lines 17-20: Mishra et al. (2014, JGR) show that Deff = 100 μm is

also common for cirrus clouds. Please consider adding a THM bulk phase function for Deff = 100 μm to Fig. 8, showing how insensitive the phase function is to Deff.

11) Page 19561, lines 1-3: Could another explanation be that Deff is smaller over land (relative to the oceans)?

Technical comments:

1) Page 19547, line 10: ensemble habits => ensemble of habits?

2) Page 19547, line 29: growth, => diffusion growth, ?

3) Page 19548, line 9-10: ice crystal particle => ice particle?

4) Page 19567, line 14: ranage => range?

5) Page 19568, line 21: dada => data?

6) Figure 7: The text within Fig. 7b refers to a wavelength of 0.804 μm, but the caption refers to a wavelength of 0.86 μm.

Please also note the supplement to this comment:
http://www.atmos-chem-phys-discuss.net/14/C6956/2014/acpd-14-C6956-2014-supplement.pdf

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