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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Particle backscatter and relative humidity measured across cirrus clouds and comparison with state-of-the-art cirrus modelling" *by* M. Brabec et al.

M. Brabec et al.

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We thank Rob MacKenzie for his insightful comments which helped improving the manuscript significantly.

Major points 1. I don't see any reference to previous balloon-borne studies. The authors should put their work into the context of previous studies such as those of DiDonFrancesco et al. (2006) and the HIBISCUS campaign in general.

We agree, this was an oversight. A selection of publications is now included in the manuscript.

2. In section 2 there should be some discussion of instrument response times and of how the data from the two instruments were merged. This is needed to bat away any misgivings a reader may have on looking at Figure 1 that the "offset" between humidity and cloud backscatter is an instrumental - or data analysis – artefact.

A discussion of response times and instrument synchronization is now included in the manuscript.

3. The discussion in section 2.5 (p9560) needs a little refinement. Whilst it is true to say that pure lagrangian depositional growth in radius-space has no numerical diffusion, the cirrus scheme as a whole does have numerical diffusion, because of the treatment of sedimentation. When describing the re-allocation of ice to pre-existing bins, it should be made clear whether mass or number is preserved. On p9561 it was not clear to me why mixed-phase cloud processes were discussed, since they do not seem to be relevant for the paper. If the ZOMM model simulates transport of water substance through mixed-phase clouds I would expect it to handle coagulation (and splitting), which it appears not to (p9559, line 19).

Indeed, the treatment of sedimentation in the Eulerian grid space reintroduces some numerical diffusion. We discuss this now in Section 2.6, including some arguments why the corresponding error is expected to be small.

4. The ms discusses sensitivity to small-scale-wave frequencies, but not to the chosen amplitude (1K) nor to the choice of ice aspect ratio or nucleation mechanism. There should be some discussion of why the modelling sensitivity study did not vary these parameters.

These are good points and it took most time to treat them properly. We treat the aspect ratio new at the end of Section 2. The type of nucleation – homo- vs. heterogeneous – it now already mentioned in the abstract and discussed in some detail towards the end of Section 6. In the same context we discuss the chosen amplitude.

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Minor points Please remove all references to state-of-the-art: it begs many more questions than it answers and is not needed.

OK, all removed, except in the description of CFH.

P9555, line 16. Please say why it is important to use a different platform (balloon vs aircraft), and pick this point up again in your Conclusions.

This was not well stated in the original manuscript and has been improved in the context of citing the HIBISCUS observations. In the Conclusions we take this point up again and suggest that there is no evidence of platform-specific biases.

P9556, line 6. Does the version of the ECMWF analysis data used include ice supersaturation (Tompkins et al., 2007)?

Yes, ice supersaturation is included in the version used – and this is now stated in the manuscript.

P9557, line 12: please replace "red" with "infrared" for consistency with previous discussion.

Done.

P9566, line 14: add "half as geometrically thick" presumably?

We agree, is added.

P9567, line 6. You might consider citing some of the literature on mass accommodation coefficients here (without opening the whole can of worms). I might be so bold as to suggest that the study by MacKenzie and Haynes (1992) provides a theoretical basis for the variation of "accommodation coefficient" when this parameter encapsulates surface kinetic effects as well as true mass accommodation.

Done.

Regards, M. Brabec et al.

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Please also note the supplement to this comment:
http://www.atmos-chem-phys-discuss.net/12/C6643/2012/acpd-12-C6643-2012-supplement.pdf

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