Answers to the referees of the ACPD - manuscript

'Thin and subvisible cirrus and contrails in a subsaturated environment'

by M. Kübbeler et al.

Review Referee 1:

Minor comments:

1. On page 3155 I believe the repetition of hours of flight encounter are redundant to the abstract and should be removed (line 10).

   Is removed.

2. Same page, line 14. Please add a paragraph which defines 'subvisible'. I know there are various definitions but given the central theme of this work please define why 'visible' cirrus are not considered? What is the delineation?

   New sentence (new ms page 1, right column): *'The observed cirrus clouds were mostly thin or subvisible to the eye from aircraft (for definition of visibility see section 3.1). They were detected in subsaturated environments, ... '*

3. Section 2.2 on page 31157: There has been extensive work recently on the shatter artifact of ice crystals concerning the probes used in this paper. Of particular interest is the work of Korolev et al. Given the topic I believe a paragraph or two of expanded explanation needs to be added as the few lines here essentially dismissing this artifact are not sufficient. Specifically I would like to see an attempt at a possible size of the artifact, perhaps using Korolev's data, instead of acknowledging but assuming it to be small.

   New paragraph (new ms end of page 2 - beginning of page 3) : *'Shattering of ice crystals at the inlets of the instruments can not be completely excluded, though the impact of shattered ice crystals on the total number or size distribtuion is minor at temperatures lower than ∼240 K, because the ice crystals are typically not as large as at higher temperatures, as Krämer et al. (2009) and de Reus et al. (2008) discussed. The size and amount of the fragments of shattered ice crystals can not be estimated for the probes used here by interrival time correction (see Korolev et al., 2011, and references herein) because those times are not recorded.'*

4. Page 31167 line 15: Add 'the' before 'lowest'

   Done.

5. I believe Table 1 is redundant to the text explanation. As it does not add additional information I think it should be removed.

   Is removed.

Review Referee 2:

1. On page 31155, line 18, the encountered conditions are described as 'unusual'. I think the authors end up making a good case that perhaps the encountered conditions actually are not all that unusual and may occur rather more broadly than previously recognized.

   'Unusual' is removed.

2. On page 31157, line 3 and 9, I believe the mention of calibration to ensure 'precision' of the FISH instrument would more appropriately refer to accuracy.

   'Precision' is replaced by 'accuracy'.

3. On page 31157, explicit uncertainties are stated for most measured parameters, but not for particle size and number measurements made by the PN, CPI, and FSSP.

   The following sentences are added to the manuscript (new page 2, right column):

   PN: *'The accuracy of the extinction coefficient is estimated to be within 25% (Gayet et al., 2002).'*

   FSSP: *'The assumption on ice crystal shape generates uncertainties in the sizing. In the sub-µm range, differences between spherical and spheroidal particles are of the order of 50 nm in diameter, while this difference may increase to 1 µm in the sub-10µm range, and to $>$ 2 µm in the super-10µm range (estimates are based on Borrmann et al., 2000). The uncertainty of the ice crystal number concentrations, caused by the inaccurateness in determining the particle sampling volume and sampling statistics errors, is in the order of 100% for contrail and cirrus encounters with number concentrations ¿ 0.1 cm^{-3}.'*

   CPI: *'The uncertainties on CPI derived microphysical parameters are 25% and 50-75% for particle size and concentration, respectively (Gayet et al., 2002)'.*

4. The clause following the colon on page 31159, line 22 is difficult for me to follow and I think it distracts from the main point of the sentence. Perhaps 'portray' on line 23 is not the most appropriate verb choice.

   New sentence (page 3,right column): *It can be seen that the most frequent RH_{ice} is always 90 %, i.e. the slightly sub-saturated conditions in the cirrus and contrails mirror the clear sky conditions, since air from the environment is continuously entrained into them. Both cirrus and contrails are in the evaporation stage.'*

5. On page 31159, beginning line 12, a precise definition of several previous sub-visibility definitions is given. The paper then details how these definitions appear not to fit the visibility thresholds observed from in the aircraft. The authors then infer that in-situ visibility must be reduced with respect to ground or satellite

observation (line 28). I find this to be an interesting and logical inference, but it does not seem certain; should it not be possible to resolve this definitely for these cases using archived satellite imagery? I believe a satellite image at the time of 12.3 UTC on Nov. 17, 2008 would also add useful context generally. Since the in-situ visibility criteria is a rather specific (and different than elsewhere) interpretation of subvisible, I think it would help to make this definition explicit in the footnote on page 31155.

New footnote (new page 2, left column): *'the term thin/subvisible is used in the following to describe cirrus which are thin or subvisible by eye from aircraft (see also section 3.1).'*

Thanks for the suggestion to show satellite images! We added two (new Figure 4, page 4) and could state now (new page 4 to 5)
'Thus, all cirrus should have been visible based on this measure and indeed were visible from satellite, as shown in Figure 4 for two flights.'

6. On page 31162, line 27, the description of the orange highlighting in figure 7 is a bit different than what I see on figure. The flight-path highlighting appears red to me

   changed (new page 6, right column and caption of new Figure 8.)

   and the vertical bar highlighting the contrail is clearly orange. Since the contrail doesn't extend through all layers, perhaps a different graphical approach could be taken there.

   We left the Figure unchanged, but changed the Figure caption to *'The time of the contrail event is indicated by the orange bar.'*

7. On page 31163, I'm not positive what aspect of the MAID model is being referred to as 'kinetical'. I think this references the explicit treatment of molecular kinetics in the vapor flux calculations at the ice particle surfaces (as in Bunz et al. 2008). If this is the case, then the deposition coefficients used in the simulations should be made explicit (i.e. alpha somewhere between .005 to 1)?

   We added to the MAID description (new page 6, right column): *'... accomodation of water on ice* $\alpha = 1$ *...'*

8. Though the major focus of section 4.2 on page 31166 is on the several-hour evaporation (sublimation) times for the larger crystals (as shown by Fig. 11), it is not clear in Figure 11 that the lifetimes for growth and sublimation are essentially identical (easy to miss on statement on p. 31167, line 7).

   In the bottom panels of the Figure it is noted that the times correspond to growth/-evaporation. The sentence (new page 9, right column) is rewritten to *'In Figure 12 (new number) the ice crystal numbers (top panel), their maximum sizes in dynamical equilibrium (middle panel) and their growth/evaporation times (bottom*

panel) are shown in dependence on temperature for different vertical velocities (cooling rates). Growth and evaporation times are nearly identical in the simulations due to the identical cooling and heating rates prescribed in the simulations and are thus shown only once here.'

Given the mirror dynamical and kinetic treatment in MAID, it isn't surprising that the growth/sublimation processes should be mirror each other in lifespan. However, I think that it is worth noting that experimental evidence of sublimation as the exact reverse of growth is not certain (see for instance J. Nelson, Sublimation of Ice. J. Atmos. Sci, 1998).

Nelson (1998) stated *'Pruppacher and Klett (1978) assume that sublimation is the exact reverse of growth, while Young (1993) states that sublimation shapes have more extreme crystal aspect ratios than growth shapes, but can also be more isometric (equidimensional).* I think that Nelson (1998) refers more to the shapes but to the times, thus we didn't introduce it here.

But, we referenced Nelson (1998) at the beginning of section 4.2 (new page 8, left to right column): *'From the case study presented in the last section the question arised, if the time cirrus clouds can live in a subsaturated environment may represent a substantial part of their total lifetime. Nelson (1998) stated that 'the time for an initially solid polyhedral crystal to completely lose all facets ... is calculated to be approximately 50% of the crystal's total lifetime independent of ... the undersaturation.'*

To evaluate in more detail the times that ice crystals can live in a subsaturated environment, we performed a series of idealized simulations of cirrus cycles.'

9. I find it interesting and somewhat surprising that for weak updrafts, the modeled particle lifespans are essentially independent of temperature. I would have expected particles to persist longer at lower T. From Fig. 11, I can now see how this comes about in the model, but perhaps it is worth commenting on in the paper.

   We added the following sentence (new page 10, right column): *'It seems to be surprising that the times for growth/evaporation do not change with temperature, but keeping in mind that the total amount of ice decreases with temperature, it becomes obvious that the times per ice unit are getting longer accordingly.'*

Review Franz Immler:

1. My major concern is that the authors fail to explain why this should be interesting. Obviously, most of the flight paths happened to be at the lower part of the cirrus which quite typically is a subsaturated region where particles that have been falling down from the cloud above are evaporating. In fact this phenomenon can be observed by plain eye, giving ice clouds (cirrus) its distinctive fibrous look.

   The importance of our findings are explained in the abstract:

   '... such thin/subvisible cirrus can exist for time periods of a couple of hours and longer in a subsaturated environment and thus may represent a considerable part of the cirrus coverage.'

   and in the Introduction:

   'The measurements and model simulations presented here highlight that not only investigation of the global distribution of upper tropospheric supersatured regions is important in terms of cirrus cloud occurrence, but that subsaturated areas containing thin/subvisible cirrus should likewise be considered.

   The point is that it is that the long lifetime of cirrus in subsaturated regions was not studied in greater detail before.

   In addition, it is stated the conclusions:

   '... a consequence of our findings is that the coverage of thin/subvisible cirrus in subsaturated air might be not negligible. Krämer et al. (2009) presented frequencies of occurrence of RHice inside of cirrus derived from a large airborne in-situ data set. Though subsaturated cirrus exist in their observations, the thin/subvisible cirrus in general and specifically those under subsaturated conditions may be underrepresented because they are hard to find by aircraft or even not detectable by in-situ instrumentation. .... Here, we suggest to quantify in future studies the contribution of thin/subvisible cirrus in subsaturation to the total cirrus coverage to assess their importance for the impact of cirrus on climate.'

   We feel that this satisfactorily explains why it is interesting to study ice at relative humidities below ice saturation.

2. I think the paper should go through some substantial revisions with putting more emphasis on the time scales of growth and evaporation that have been investigated by the model.

   This topic has been discussed in Section 4.2: Cirrus ice growth and evaporation times, see also Figure 12 (new number).

3. The introduction should provide some information on the current knowledge on the kinetics of ice formation and dissipation, including laboratory investigations. In the abstract it is mentioned that the clouds are expected to dissipate, the introduction should explain why and how fast. The part about modeling should be more detailed on the mechanisms and parameters controlling these time scales.

   We understand the idea of providing the complete current knowledge of the kinetics of ice formation and dissipation. However, we feel that Section 4.2: Cirrus ice growth and evaporation times is sufficient to meet the concern of the paper. Indeed, Section 4.2 addresses the question why and how fast ice clouds dissipate.

4. In the conclusion it should be noted that the observed distribution of relative humidity in cirrus (fig.2) is not typical for cirrus and was caused by the specific flight paths of this campaign where most of the time the lower parts of cirrus were probed. Also, it should be noted that a measurement uncertainty of 10% in the relative humidity does allow the possibility that the air was not that much subsaturated after all. (E.g. if the measured RH= 95% and the temperature uncertainty is 0.5 K the air might well be saturated.)

   Since we didn't mention the relative humidity frequencies of occurrence in the conclusions, we introduced a paragraph in section 3.1 (new page 4, left column):

   'The lower peak of the RH_{ice} frequencies is found during CONCERT because during all flights comparable situations -namely evaporating frontal cirrus- were probed. However, note that the uncertainty of the RH_{ice} measurement is 10%, i.e. the subsaturation could have been lesser. '

5. However, provided that the results are well established, it might be worth thinking about implication of the finding (i.e. the long livetime of ice particles in subsaturation) on some effects of cirrus, e.g. on the redistribution of water in the upper troposphere. This might be of great interest in particular when it comes to ice clouds in the tropical transition layer and their dehydrating potential.

   At the end of the introduction (new page 2,left column) we included:

   'Such long lifetimes of ice crystals in subsaturation are of relevance for the atmosphere because e.g. the seeding of lower-level clouds by precipitationg ice crystals depends on their fall distances and further, sublimation of ice crystals from cumulonimbus anvils supplies the upper troposphere with water vapor (Nelson, 1998).'

Detailed comments:

• **Introduction**

6. page 31155 line 14 onwards: These paragraphs reports on the results and should not be part of the introduction, instead some information on current knowledge of the behaviour of ice in subsaturated condition would be helpful.

   See comment on Point 3.

• **section 3**

7. Its not clear to me whether the observation of (young) contrails really contributes to the conclusions of this paper. In fact it seems to me that including the contrails is more confusing than clarifying the major findings of this manuscript and should be left away.

   We want to reference here the comment of Referee #1 with regard to the inclusion of the contrails and we agree with this statement:

   'Contrails are somewhat less interesting given the large amount of literature on their properties but their inclusion here is appropriate and there are nice results considering their interaction with existing cirrus ice crystals.'

8. Figure 10 and 11 are very difficult to read and the explanation in this section are therefor not so easy to follow. Please revise the figure, use more clear colors to distinct different axis and data. The explanation refers to a paper by Krämer and Hildebrandt 2010. Since this is a conference contribution only and both are co-authors of this manuscript it might be worth thinking about including some part of the conference paper here.

   We feel that the way the figures are plotted are state of the art in model studies and we don't see how we could further improve them. In addition, we again would like to reference the other Referees, evaluating the figures as 'compelling' and 'appropriate'.

   The reference Krämer and Hildebrandt 2010 is removed from the paper.

Technical correction:

9. page 31160 line 4: The notation 210/9 means 210 divided by 9 and should therefore not be used here, maybe 220 (9) could be used. The better idea would be to reduce the information to such figures that are really required to understand what you are trying to say.

   The numbers are changed as suggested.

10. p31161 section 3.2 title: It should be A340, I guess.

    Has been corrected.

11. p31166 line 2 I am not sure if cirrus really ”lives” ... probably terms like ”exist” or ”are present” is more appropriate.

    Has been changed.