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Interactive Comment

Interactive comment on "Thin and subvisible cirrus and contrails in a subsaturated environment" *by* M. Kübbeler et al.

Anonymous Referee #2

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Kubbeler et al. present very interesting in-situ data and modeling of subvisible cirrus and contrails from the 2008 CONCERT campaign. In my opinion, the paper presents a new perspective on an important scientific issue: that the full global picture of cirrus and their climatological import maybe undervaluing the potential role of subvisible cirrus in sub-saturated regions. Overall the manuscript is well-written, well-referenced, and supported by compelling figures. I recommend that the paper be published in ACP, and I offer these minor comments:

(1) On page 31156, 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.

(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.

(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.

(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.

(5) On page 31159, beginning line 12, a precise definition of several previous subvisibility 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.

(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 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.

(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)?

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(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). 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).

(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.

Interactive comment on Atmos. Chem. Phys. Discuss., 10, 31153, 2010.

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Interactive Discussion

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