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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Cirrus cloud occurrence as function of ambient relative humidity: A comparison of observations from the Southern and Northern Hemisphere midlatitudes obtained during the INCA experiment" *by* J. Ström et al.

J. Ström et al.

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Comments on revision

The manuscript has been modified in the following way. The simple model approach based on the probability of water vapor advection is removed. As a result of this, figures 7, 8, and 9 are removed, and the derived CWC from this model in figure 10 is removed as well. Added to the manuscript is the simulated CPF using the microphysical trajectory model by Haag et al. 2003.
These changes resulted in a slightly modified structure and new headings for the manuscript. Below we summarize the new structure and the content in the different sections. Responses to reviewers specific comments follows the summary.

2. Methodology

2.1 Description of the cloud probes

Description of the CVI, FSSP-300, PMS-2D, and Polar Nephelometer as before.

2.2 Relative humidity and cloud presence fraction

Introduction of the relative humidity measurements and the conceptual figure 1. The content is essentially as before, but with some rephrasing to make the points more clear.

3. Observations

3.1 Transition between clouds and cloud-free air

This is the section that includes the simulated CPF. The simulated results are compared with the observed CPF in figures 2 and 3. Similarities and differences are discussed for sub-saturated conditions.

3.2. Variation of cloud detection thresholds

Here we discuss how the CPF change for different thresholds based on the CVI and FSSP-300 observations presented in figures 4 and 5. Essentially the same content as before, but with revised text in response to the reviewers comments on the aerosol vs. cloud issue. Emphasis is made to the fact that we make no distinction between "clouds" or "aerosols" and that the instrument detection limits is the sole objective measure. See also response to specific comments below.

3.3 Onset of freezing in cirrus clouds This section presents the CPF observed by the Polar Nephelometer (figure 6) and highlights the different freezing thresholds during the two campaigns.

3.4 Cloud water content The distributions of CWC (figure 7) remain in the manuscript to show the small difference between the two campaigns despite the different onset of freezing.

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4 Conclusions The modifications mentioned above are reflected in the conclusions

REV1

Rev 1 General comments
This paper describes measurements relating to cirrus cloud formation made during the INCA experiment, and the authors develop a model by which these measurements are interpreted. The paper contains worthwhile and interesting material, but the data analysis leaves a number of questions.

Specific comments
Rev1 comment 1. Section 2, Methodology, p 3305. This definition of CPF(RHI) appears a natural one for in situ measurements; it involves simultaneous measurements of RHI and cloudiness. We expect to find nonzero CPF over a range of values of RHI above and below.

Response: Non-action comment
Action: Non

Rev1 comment 2. The discussion of the impacts of different measurement techniques on resulting measured cloud presence fraction, CPFmeas(S) (Section 3) is clear. The use of the terms "NH" and "SH" to identify the two aircraft campaigns is unfortunate, however, as (despite the authors' caveat) the discussion of the differences appears to refer to hemispherically averaged differences.

Response: We believe that the reviewer must agree that the risk that an average reader of ACP will mistakenly interpret our results as hemispherically averaged differences has to be considered minimal. That this is an in-situ observation paper should be clear to anyone. As for all campaign papers such as this manuscript the measurements provide merely a snapshot in time and space and strictly speaking only represents the time and space were the observations where made. Although, the INCA data it self

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represents only a month of campaigning at each location, we see no problem in discussing the results in terms of possible hemispheric differences.

Action: Besides the caveat we frequently add the word "campaign" after the abbreviations NH and SH to further emphasize that we refer to specific observations and to hemispheric grand averages.

Rev1 comment 3. Section 4. Interpretation. While our comments (below) may reflect our misunderstanding of the work here, we suggest that at least part of the difficulty lies in the presentation, which we find extremely unclear. Since S > 1 no evaporating clouds are included. Equation (1) is not consistent with the definition of $CPF_{meas}(S)$ given in Section 2, and with the measurements that are presented there. $CP_{Fmeas}(S)$, S < Sc, is the fraction of the measurements at given S (supersaturation with respect to ice) for which there is cloud. Equation (1), instead, gives something like the fraction of measurements for which S0_ S in which S0 _ Sc for all S0. We can call this model cloud presence fraction $CPF_{mod}(S)$. The discussion surrounding the derivation of $CPF_{mod}(S)$ would yield $CPF_{mod}(S)$ = 0, S < Sc, $CPF_{mod}(S)$ = 1, S > Sc, because all phenomena that would create cloud at values of S between S = 1 and S = Sc are neglected. This approximation includes the "important simplifications" discussed on p 3312. A one parameter model is chosen that captures the decrease of cloud presence fraction with increasing supersaturation. Therefore all the physics is contained in the one parameter (S0) and some discussion should be given as to the sensitivity of the results to variations in this parameter, and possible physical interpretations of the magnitude of the best fit value. Moreover, Equation (2) is fit to data outside cloud but assumed to hold inside cloud as well. While it is plausible to assume that the probability p(S) is a decreasing function of S both outside and inside cloud, the conditions in cloud, including stronger updrafts, stronger radiative cooling, etc., may well significantly perturb p(S). For these reasons, comparing $CPF_{mod}(S)$ with $CPF_{meas}(S)$ has questionable value. The logic in this exercise should be at least clarified before the paper is accepted.

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Response: Unfortunately, we failed to make this part of the study any more clear and since this model approach appeared to be the major objections by the reviewers we have removed it completely from the manuscript. Perhaps this can be returned to in a subsequent study, because it is fascinating how much of the observed cirrus properties could be related to simply the advection of water vapor.

Action: The simple model approach is removed from the manuscript.

Rev1 comment 4. The calculation of an equivalent adiabatic water content is suggestive but, as the authors state, in view of all the important simplifications made, the values calculated are unphysical. Perhaps then the value of the calculations is in assessment of the importance of the neglected processes. A sentence or two addressing this point might be of use.

Response: See response above.

Action: The derived CWC is removed as a result of deleting the simple model approach.

Rev1 Technical corrections. I suggest that this paper be edited by a native English speaker before its final submission. Note typo in line 14, p 3312: inequality should read 1 _ S _ Sc.

Action: Manuscript improved.

REV2

Rev2 Major comments 1: The criteria, which determine whether the air parcels are inside or outside of the cirrus should be discussed more in detail. The FSSP measurements show an considerable fraction with particles diameter between 0.6 and 0.9 micrometer (black line subtracted by the green line of Figure 4) along at very dry condition (RHI of 5-60%). If the particles with a size of 0.6-0.9 um in diameter were ice particles, they would evaporate in $\sim$0.02-0.04 s at T = 227 K and RHI = 0.5. The chance that such small ice particles exist along at such dry condition (RHI = 5-60%) is in fact

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zero. Therefore, it is most likely that the air parcel contains only particles d <= 0.9 um are not ice particles at all RHIs (Fig. 4). It is even more probable to detect non-ice particles at higher RH as aqueous particles can grow in size due to water and enhanced trace gas uptake at higher RH. The threshold number density 0.001-0.003 cm^{-3} for CVI measurement is probably not a good measure for the cirrus-cloud-indicator either. A considerable fraction of air parcels (up to a CPF of 50%) at very dry condition show particles (RHI as low as Dl 5%, Figure 3, which indicates that the temperature is $\sim$30 K above the ice frost point !!!). They are unlikely ice particles, if they are not extremely large (the corresponding information of ice water content measured by CVI would be useful to clear this point).

Response: The manuscript is revised to meet the reviewers confusion on this point. We emphasize in the text that we distinguish the presence or non-presence of clouds by the different instrument detection thresholds, which is the only objective means of measure available to us. The composition and phase of the small particles are unknown to us and the continuum aspect of the aerosol/cirrus cloud system makes it impossible to say what is what, as pointed out in the beginning of the manuscript. The fact that the CPF respond to the changes in RHI suggests that the small particles, at least in part, are composed of water. The Polar Nephelometer is capable of distinguish phase of an ensemble of particles, but the "cloud" must be sufficiently dense for the instrument to respond (clearly mentioned in the text). Why the reviewer objects to the CVI threshold 0.001-0.003 cm^{-3} and not the other CVI thresholds is not clear to us. The whole point of these figures is that we don't subjectively impose a criterion for what is a "cloud" or not. That is why we plot so many different thresholds over the domain of available measurements. Note that the cut-off for the CVI is the same (ca 5 micron aerodynamic diameter) irrespective of the number density. We purposely used Cloud Presence Fraction rather than Cirrus Presence Fraction, to avoid having to get caught in a polemic discussion of -what is a cirrus?-. The word "cirrus" is used in the title as this work by all means relate to the study of this cloud type.

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Action: The text is revised to try to make the aerosol/cloud issue more clear.

Rev2 Major comments 2: The model describe in this MS is highly questionable based on the following points: a) The exponential distribution of the RHI (Eq. 2) is valid only under the assumption that ice particles do not form at all. As authors mentioned, once the ice particles nucleated, the growing ice particles will reduce to gas phase H_2O and bring the RHI to 100% (equilibrium). The deviation from ice saturation is kinetically controlled. I do agree that the RHI distribution for RHI >= 100% is almost exponential (Figure 2 and the black line of Figure 3 of Haag et al., 2003) without ice formation. However, the RHI distribution outside the cirrus cloud differs considerably from an exponential distribution (coloured lines of Figure 3 of Haag et al.). b) The agreement between model and measurement (Figs. 7,8,9) is not justified. At first, the good agreement is only achieved for FSSP data with a size threshold of 0.5 micrometer in diameter (Fig.8a) and CVI for n = 0.001 cm^{-3}. However, as mentioned in comment 1, many data points at dry condition can not be interpreted as ice particles, their lifetime for evaporation is too short. The comparisons shown in Fig. 7a and 8a are therefore not adequate. Secondly, for higher number density (CVI) and higher cut-off size (FSSP), an higher critical saturation is required (Sc=2.7-2.9 for CVI and 1.9-2.1 for FSSP, respectively). The authors argued that the higher Sc may be explained by that fact that the excess water vapour is required for the ice particles grow to the detectable size (page 3315, lines 15-17). I made a rough estimate: for FSSP, the detectable size of 1 um in diameter (Fig.8b), an excess water vapour of 2.5 ppb is required to allow 1 cm^{-3} of ice particles to grow to a size of 1 um in diameter. The vapour pressure over ice at 226 K is about 280 ppmv (at 200 hPa). The ice particles (n= 1 cm^{-3}, d = 1 um) contain a water amount of only $\sim$1E-5 in saturation ratio S. For CVI instrument, the ice particles (n= 1 cm^{-3}, d = 5 um) contain a water amount of also only $\sim$1E-3 in S. These small values are far away from the discrepancy between the modelled Sc of 1.9 to 2.9 and Sc of 1.3 to 1.6 for ice formation. On the other side, the measured CPF increases to unit at Sc of 1.3 to 1.6 for both CVI instrument (n= 1 cm^{-3}, Fig.7b) and for FSSP (Fig.8b), unlike themodel results. c) The authors mentioned also that

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the difference between modelled Sc and S is the adiabatic cloud water content. I think that the nature is more complicated. The CWC is determined mainly by the difference between the actual temperature and the frost point (if one takes the total water), if the ice particles are in equilibrium with the gas phase. The time scale for equilibrium could vary, depending on ice number density. However, in the present model, it seems that the CWC is only a function of number density (Fig.7), which is not physically. Minor comment: I can not follow the steps to derive the CWC distribution (shown by Fig. 10, blue line) from the model. More detailed explanation is required. This is also related to my question mentioned in comment 2c). If I understand your approach probably, 40% of total air parcels should have a CWC of more than 1.7–1.9 in saturation ratio S = a super saturation of 1.7–1.9 (see Fig. 7b and page 3515, lines 22-24), which is about 91 to 101 mg m^{-3} at T = 226 K. But, the blue line shows only a tiny fraction with CWC > 90 mg m^{-3}.

Response: The model part referred to by the reviewer is removed see comment to reviewer 1.

Action: See comment to reviewer 1.

Interactive comment on Atmos. Chem. Phys. Discuss., 3, 3301, 2003.