Atmos. Chem. Phys. Discuss., 3, S437–S441, 2003
www.atmos-chem-phys.org/acpd/3/S437/

Atmospheric Chemistry and Physics Discussions

Interactive comment on "The roles of dynamical variability and aerosols in cirrus cloud formation" *by* B. Kärcher and J. Ström

Anonymous Referee #2

Received and published: 25 April 2003

General comments

This is an extremely worthwhile and very well written contribution to ACP that includes an interesting and compelling analysis of vertical motion and predicted ice crystal concentrations in cirrus. The emphasis on the likely effects of mesoscale gravity waves is an important statement to make. The agreement obtained between the simplified model of nucleation and the observed measurements attributed to the concentrations of cirrus crystals is remarkable. The implications are clearly elucidated with regard to modeling cirrus formation and sensitivities to aerosol and atmospheric changes. This might be considered a seminal paper in these regards. Yet I feel that this excellent paper will not close the door on some important issues. For this reason, I choose to take what I must acknowledge as extreme "devil's advocate" positions in expressing some of the specific concerns that I have personally or have heard expressed by other scientists. These are listed as specific comments along with a few other items.

Specific Comments

(1) One could scarcely find a more contentious issue in cirrus studies than the assertion that existing techniques give good quantitative information on cirrus ice crystal sizes and concentrations. The CVI method may be highly precise and accurate. Nevertheless, there remain questions about what precisely it measures. If one can somehow always sample in "young" cirrus, then the CVI may measure cirrus ice crystal concentrations, as stated in the abstract. Nevertheless, what it does measure are the residual particles, from ice crystals in a certain size range. The assumption, stated in the paper, is that one cloud particle equals one residual particle. Essential in accepting this assumption is accepting the referenced Gayet et al. (2002) paper as definitive proof that the CVI, laser diffraction and extinction probes provide quantitative and consistent data on cirrus ice crystal concentrations. That short contribution using a single case from INCA does not make it clear to me that high concentrations of small ice crystals are a required fact in all cases in order to describe extinction measurements. This deserves more comprehensive evaluation and perhaps that is forthcoming. Additionally, the odd relationship between the CVI and the FSSP-300 as a function of concentration shown in Gayet et al. does not spur confidence that either probe is measuring accurately at any concentration. Consequently, some are still left to ponder if current measurements of small crystals in cirrus are not affected by unresolved artifacts of all of the various sensors that cannot yet be ruled out (e.g., Field et al. 2003).

(2) What is absent for me in the discussion of the effect of the vertical velocity probability distribution function is a statement that it is tacitly assumed that air parcels follow such vertical motions for the scale of displacement necessary to saturate parcels sufficiently to initiate homogeneous freezing. Should this be and is this always the case? It is not obvious to me. I would be more convinced when 2- or 3-dimensional numerical simulations manage to produce both the wave structures that are stated to be responsible for the vertical motion distribution and the high predicted ice concentrations that

ACPD
3, S437–S441, 2003

seem to always be present in the cloud measurements.

(3) The authors acknowledge that the probability of vertical motions outside the range of resolution at 5 cm/s could be greatly underestimated. It seems that this could skew the probability distribution in such a way that agreement with CVI concentrations could not be achieved. Sensitivity to poor resolution of vertical motion at smaller velocities was examined, but consideration was not explicitly given to simply not resolving the percent of observations in this range. Am I incorrect in thinking there could be reason to do so? The scenario of "less waves" in Figure 7 gives a hint at what the result would be and it is interesting that this simulation seems to lead to better agreement between predicted ice concentrations and CVI data in the "NH". Figure 8 gives one a sense for how even greater underestimate of the fraction of smaller scale vertical motions would lead to a conclusion that either the vertical motion measurements are wrong or the CVI data do not accurately represent ice concentrations in cirrus.

(4) I do not fully understand why heterogeneous ice nucleation was allowed to provide the same, relatively unlimited concentrations of ice crystals, as homogeneous freezing. The contention later in the paper that the heterogeneous freezing process cannot compete with homogeneous freezing in the rather strong updrafts is supported by data (e.g., DeMott et al. 1998) and previous modeling studies (e.g., DeMott et al. 1997; Gierens, 2003; Karcher and Lohmann, 2003). Particularly difficult to understand and not well explained is how a lower steady state supersaturation (peak ice relative humidity of 130%) in the ambient air is sustained in the "NH" versus the "SH". Is this value set by heterogeneous nuclei and the lower range of vertical motions? Does that mean that cirrus should be more extensive in the "NH"?

(5) The use of the "NH" versus "SH" terminology that has been propagated by recent publications from this particular field study is too general and premature. The results in this paper are based on short-term measurements at point locales in the two hemispheres, so the general applicability of the measurements to all regions and all times remains to be confirmed in future studies.

ACPD

3, S437–S441, 2003

Interactive Comment

Technical corrections

(1) There was some confusion regarding descriptions of the color coded plots in different figures. The text is not always consistent with the captions and vice versa. Please check these carefully.

(2) Seifert et al. is referenced here and in a few previous ACP papers, but I have not found it on the web site. Please remove if it is not under discussion or published.

References Noted

DeMott, P.J., S.M. Kreidenweis, and D.C. Rogers, The susceptibility or ice formation in upper tropospheric clouds to insoluble aerosol components, J. Geophys. Res., 102, 19575-19584, 1997.

DeMott P.J., D.C. Rogers, S.M. Kreidenweis, Y. Chen. C.H. Twohy, D. Baumgardner, A.J. Heymsfield, and K.R. Chan, The role of heterogeneous freezing nucleation in upper tropospheric clouds: Inferences from SUCCESS, Geophys. Res. Lett., 25, 1387-1390, 1998.

Field, P.R., R. Wood, P.R.A. Brown, P.H. Kaye, E. Hirst, R. Greenaway, and J.A. Smith, Ice particle interarrival times measured with a fast FSSP, J. Atmos. Oceanic Technol., 20, 249-261, 2003.

Gayet, J.-F., F. Auriol, A. Minikin, J. Ström, M. Seifert, R. Krejci, A. Petzold, G. Febvre, and U. Schumann, Quantitative measurement of the microphysical and optical properties of cirrus clouds with four different in-situ probes: Evidence of small ice crystals, Geophys. Res. Lett., 29 (24), 2230, doi:10.1029/2001GL014342, 2002.

Gierens, K., On the transition between heterogeneous and homogeneous freezing, Atmos. Chem. and Physics, 3, 437-446, 2003.

Kärcher, B. and U. Lohmann, A parameterization of cirrus cloud formation: Heterogeneous freezing, J. Geophys. Res., 108, doi:10.1029/2002JD0033220, in review, 2003.

ACPD

3, S437–S441, 2003

Interactive Comment

Full Screen / Esc

Print Version

Interactive Discussion

Discussion Paper

Interactive comment on Atmos. Chem. Phys. Discuss., 3, 1415, 2003.

ACPD

3, S437–S441, 2003