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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Spectroscopic evidence for β-NAT, STS, and ice in MIPAS infrared limb emission measurements of polar stratospheric clouds" *by* M. Höpfner et al.

Anonymous Referee #3

Received and published: 1 December 2005

This is a well-written and interesting paper which combines lidar and infrared data to identify the composition of PSCs which are traditionally classified by their backscatter and depolarization properties, rather than their composition. The authors have used a very complete and time-consuming method to assign the composition of the PSCs observed in three specific instances. Then, a simpler, more tractable method is presented for evaluating the entire winter. What I do not see, however, is any connection between the two. Was the simpler method tested against the more rigorous method? How did it fare? How sensitive is it? Would the inclusion of other bands allow for a more constrained but still efficient process?

Returning for a moment to the spectroscopic analyses of Section 3: The assignment of the “best” composition for the three examples shown in Figs 4-6 looks reasonable in a qualitative way, but can it be quantified? can you tell me what to look for specifically? Is it just the least variation over the whole window that defines “best,” or are there more focused spectral regions you used when tuning the radii, and judging goodness of fit? Did you look at residuals between the fits and the observations? how much “better” were the chosen identities than the ones not chosen?

When looking at Fig 4, I do see that the overall qualitative fit is best with the Biermann beta-NAT optical constants. But the significant difference between Biermann and Toon makes me wonder if the Richwine data isn’t at least as good as those two, suggesting that alpha-NAT might also be a possibility.

(Thank you for including the dashed, no-PSC spectrum. It’s crucial for separating the important parts of the spectrum from the irrelevant parts - but it’s hidden under the legend! Please change the scale, or move the legend, or something.)

On the same note, on p. 10698, line 11-12, you suggest that the Biermann[mol] indices might be more appropriate, yet in Fig 4, right hand side, the [coa] plot looks like a better fit. How do you weight the two in deciding what is “better,” especially since the results plotted for the Richwine alpha-NAT indices look very similar to the [mol] results.

Section 4 presents a simpler, quicker method for assaying the whole winter, but I found it difficult to really understand what you are doing. Is the whole process based solely on the 820 cm-1 band? Why were 792 and 832 cm-1 also chosen? Did they provide the best discrimination among the possible compositions? In looking at the laboratory data in Fig 1, it seems that including points near 1400 and 1450 cm-1 would be very useful for discriminating between NAT and NAD.

Figure 9 is confusing and frustrating in several ways. Please describe it more thoroughly. Is everything plotted in Fig 9 theoretical, i.e., calculated from indices and radii and number densities, but not from MIPAS observations? What is the solid black line?

ACPD

5, S4257–S4260, 2005

Interactive Comment

How are the boundaries of the four regions determined? How did you test the placement of the boundaries; that is, what is the uncertainty in the assignments near the boundaries? Where would your three cases from Figs 4-6 fall on this plot? How many lidar/MIPAS coincidences do you have? 7641? Could they be plotted like Fig 9 to show how clustered or not the observations are?

I would recommend this manuscript for publication after the authors provide some quantification of their spectroscopic method, and a comparison of the accuracy of the simple method to that of the more rigorous approach.

Other comments: pp. 10694-95: I don't understand why you used ranges of water and nitric acid mixing ratios to calculate the STS composition when you state in the next paragraph that you determined the abundances of the trace gases directly from the MIPAS data. Please clarify.

I, too, was quite confused by your switching back and forth between volume and number density when discussing Fig 7. Please clarify and remove any unnecessary switching. Could the entire argument (and figure) be presented in terms of volume?

Thank you for taking such care in discussing detection limits and radii and volume restrictions.

Figure 8: I would recommend switching the dashed and solid lidar lines. It is easier to compare three solid lines than to ignore one solid line while comparing two solids and a dashed line.

Minor comments: page 10690, line 23: missed one translationĚabsorption of the v3 band of NO3-

page 10692, line 21: the word "respectively" is not appropriate here, as you don't have a former and a latter, or an A and B that are assigned to Biermann and Norman.

"Respectively" is used to indicate which members of two pairs should be associated. 10693, 11: would read smoother as "environment which allows direct derivation of

ACPD

5, S4257–S4260, 2005

microphysical”

10694, 17: is “kernal” spelled correctly?

10696, 14: why is there an “e.g.” in the middle of the line? What is the “example” of “exempli gratia”?

10696, 24: I agree with Reviewer #2 that there is no need to introduce incorrect vapor profiles and that you should rework that portion of Fig 7. Does that also imply that Equil II should be calculated for the other panels as well? Also, “the reason” needs a singular verbĚ “is”, or “may be”, or “could be”.

Fig 1 caption: the word “and” is unnecessary

Fig 10 caption: should read “ “MIPAS 1” indicates”

Interactive comment on Atmos. Chem. Phys. Discuss., 5, 10685, 2005.