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Atmospheric Chemistry and Physics Discussions

Interactive comment on "ACE-FTS observations of acetonitrile in the lower stratosphere" *by* J. J. Harrison and P. F. Bernath

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This is a very good paper. The authors have made laboratory measurements of CH3CN absorption cross sections over the appropriate T/P conditions, and have used these to retrieve CH3CN profiles from ACE spectra. The zonal averaged profiles look very plausible and appear consistent with previous observations.

There are however a few points that should be explained better and/or clarified:

Regarding the MkIV/ACE discrepancy above 25 km. The fact that Kleinboehl et al [2005] fitted pseudo-lines to the PNNL cross-sections is besides the point. Any other extrapolation technique would have had equal if not larger extrapolation uncertainties. So the salient is not the pseudo-lines, it is the limited T/P range of the lab PNNL mea-

surements, which were the only cross-section data available in 2004.

The chosen window for the ACE retrievals is narrower than that used in Kleinboehl et al. The authors claim that this reduces uncertainties from interfering CH4 and H2O lines, which is true. But there is also a downside to making windows narrower which is that the "continuum level" or "Baseline term" becomes less certain. But this downside is not discussed or quantified. Could the difference in the chosen window widths partly explain the differences between MkIV and ACE at the higher altitudes?

You have a whole paragraph discussing minimum and maximum altitudes for the CH3CN retrieval, but you don't explain why it is necessary to prescribe the altitude range. Why can't you do the retrievals for all altitudes and then look at the errors bars later to decide what the useful altitudes ranges. This way, you would not be pre-judging what the H2O or CH3CN profiles should be. At mid/high latitudes you can get days when the tropopause altitude drops to 6km, which would allow CH3CN retrievals to extend much lower, if you let them.

I was surprised to read that a quadratic baseline term was added to the forward model to minimize residuals arising from non-voigt H2O lineshapes. Firstly, I strongly suspect that the main problem in fitting strong H2O lines in the ACE spectra comes from the neglect of the finite FOV. Secondly, if the non-Voigt lineshape is really the issue, why not use the appropriate non-Voigt lineshape, as explained in Boone et al.[2007], rather than fit a non-physical baseline quadratic term?

In the fig 4 caption, replace "Six MkIV profiles" by "The average of six MkIV profiles"

Interactive comment on Atmos. Chem. Phys. Discuss., 13, 3323, 2013.