

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

Anonymous Referee #2

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This is a short paper reporting quasi-global distributions of acetonitrile (CH₃CN) in the lower stratosphere, which were obtained by inverting ACE-FTS occultation spectra recorded between 2004 and 2011 from the tropopause to about 30 km. The retrievals use a new set of absorption cross-sections, measured in the laboratory at appropriate spectral resolution and temperature/pressure conditions. A stratospheric lifetime is calculated from the profiles. The paper is generally well written, with some sections being, however, dense and sometimes unclear. The paper is also mainly technical, discussing the retrieval aspects, while overlooking to some extent the comparisons with correlative datasets -especially MLS- and geophysical analyses; see below for detailed comments. However, all in all, the results seem robust and reasonably well characterized. They provide the first extensive distributions of CH₃CN, and the paper should therefore be of general interest to ACP readers. I have no objections for publications if

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the comments below are addressed.

GENERAL COMMENTS

Page 3328: Presumably the retrievals don't use an a priori. It would be good to stress this.

Page 3330, lines 16-18: It would be useful to say a word how the classification to biomass burning plume is made (more than referring to Tereszchuk) and how reliable it is for the purpose of this paper. It would also be useful to show the biomass burning profiles (e.g. as the median or average of these) and see how they differ from the background profiles (what are typical vmr for the biomass burning plumes?). More generally it is unclear to me why the biomass burning observations are not presented in the paper. They would give the paper more weight.

Page 3331 and Figure 2. The profiles are built from a set of profiles in latitudes bins and for several years and this is justified by the fact that the inter-annual variability is small. But how much is it? And how does it compare to the latitudinal (or hemispheric) variability? In fact all profiles shown in Figure 2 seem to be similar within the retrieval error. The question of what is significant and what is not should be addressed more critically. Relating to the above, one would also like to know if the biomass burning activity does not cause larger variability (in space and time). Why is it not discussed here?

Page 3331, Figure 3: The text around Figure 3 is very descriptive. Is there an explanation for the seasonal variability? In the Northern hemisphere the enhancement are above 50° in winter. Is this explained by chemistry? And if there is a chemistry effect, would it affect the lifetime estimated in section 7 (winter vs summer). The tropopause altitude should be indicated in Figure 3 (or are the measurements all for sure in the stratosphere?)

Page 3333, last paragraph before section 7. The MLS v3.3 CH₃CN data seem to be

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publically available. I understand that they should be used with caution but considering that they are the only correlative dataset (matching the time period covered here with ACE-FTS) the authors should provide better comparisons (a least one Figure) and supporting discussions.

Page 3335, before section 8. The lifetime is about 5 times larger than previous estimate. The statement “this work supports the view that acetonitrile is long-lived” falls short. A tentative explanation and/or a short discussion would be needed.

MINOR AND TECHNICAL CORRECTIONS

Page 3324, line 18: Distinction should be made between the tropospheric lifetime and the stratospheric lifetime discussed later in the paper. The 6 months is presumably a tropospheric lifetime.

Page 3325. Equation 1 is not discussed elsewhere; R1 could be removed

Page 3326, line 24. Remove “between 1993 and 2004”, which is already mentioned above.

Page 3331, line 4: “The 18 latitudes bins from -90° to $+90^{\circ}$ ”

Page 3331, line 12: It is unclear what this Gaussian distribution means. Is it useful at all?

Page 3325, lines 12 to 21. This paragraph is a bit confusing. It relates to the tropospheric sources and sinks and gives details that are not relevant for the paper. I would suggest shortening.

Reference: Tereszchuk 2013 should be 2012 (as also quoted in the text Page 3329, line 27)

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