

## ***Interactive comment on “Evidence for long-lived polar vortex air in the mid-latitude summer stratosphere from in situ laser diode CH<sub>4</sub> and H<sub>2</sub>O measurements” by G. Durry and A. Hauchecorne***

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### General Comments -

In this paper, a set of distinct structures are observed in the CH<sub>4</sub> and H<sub>2</sub>O vertical profiles from the stratospheric balloon flight of a high-time resolution spectrometer over the South of France in June 2000. The profiles are then compared with previous balloon flight measurements and with the simulation of a high-spatial resolution PV advection model to demonstrate the origin of the sampled structures. The authors use isentropic maps and a meridional cross section of PV distribution to suggest that the observed

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structures originate from the presence of Arctic polar vortex air masses at midlatitudes. This is a very interesting paper that provides further evidence on the existence of long-lived coherent polar vortex structures in the stratosphere at midlatitude even in the summer. The paper is a very valuable contribution to ACP and the reviewer suggests the paper is published once the following comments have been addressed:

## Specific Comments -

- 1) The study is based on the vertical profiles retrieved from the SDLA measurements. How do these vertical profiles compare with those from other instruments from the 20 June 2000 balloon flight? Are they in good agreement in terms of fine and large scale structures?
- 2) Line 20, p. 1242: 'Orsolini (2001) further studied these vortex remnants' leads to the idea that Orsolini studied the observations from the SPADE campaign in April 1993. The study from Orsolini (2001) focuses on the spring and summer 1998. The authors should reformulate this sentence.
- 3) Line 26, p. 1242: the authors highlight the 'impact of potentially chemically-active and ozone-depleted polar air' in structures that have been 'adverted to mid-latitude'. Later (i.e. line 21, p. 1246), the authors state that this issue will be investigated in a future study based on the numerical simulations from a chemical transport model. Again, if ozone measurements were performed on the day of the 20 June 2000 balloon flight, how does the ozone vertical profile compare with those from the SDLA measurements? If evidence of ozone depleted air is found in the ozone profile, I would recommend the authors to comment on this in the paper as an introduction for the future study.
- 4) Line 3, p. 1244: The authors should also specify the absolute accuracy of the SDLA instrument on that flight.
- 5) It is surprising to see a tropospheric methane mixing ratio of about 1.3 ppmv in Figure 1 at around 2 km of altitude for the 20 June 2000 flight profile. Also, it seems

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that this methane vertical profile contains more fine scale variations below an altitude level of 20 km compared to those from the 16 October 2001, 10 May 1999 and 23 February 1999. Could the authors explain this? Has the SDLA instrument experienced any particular technical problem on this flight?

- 6) Line 5, p. 1244: The time of the 20 June 2000 balloon flight is missing.
- 7) Line 20, p. 1244: The following sentence 'The matching between both methane values' can lead to confusion. The authors should reformulate this sentence.
- 8) Line 14, p. 1245: The MIMOSA description does not contain the vertical resolution of the model. It needs to be included.
- 9) Line 26, p. 1245: The authors state: 'However, the expected uncertainty on the location of advected structures is increasing with time'. The reader would appreciate further details on the uncertainty of the model. This statement requires clarification.
- 10) In reference to the following statement 'to further investigate the possible presence of polar air remnants from the 2000-Arctic vortex at the time and place of the SDLA flight in Gap' (i.e. line 21, p. 1244), this paper could be substantially enhanced if it would show a comparison between the SDLA methane vertical profile and a MIMOSA modified potential vorticity (or equivalent latitude) vertical profile interpolated at the time and location of the measurements. Such a comparison would help to address the uncertainty of the MIMOSA model (i.e. see previous comment).
- 11) Figure 3: The sub-section figure labels (i.e. a-b-c-d) can not be found on the isentropic PV contour maps.
- 12) Figure 4: The authors highlight the presence of a white circle for the trajectory of the balloon. Should the readers be referred to figure 3?
- 13) Figure 5: A line should be included in the figure to represent the 20 June 2000 balloon trajectory.

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