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# *Interactive comment on* "LISA: a lightweight stratospheric air sampler" *by* Joram J. D. Hooghiem et al.

**Anonymous Referee #1**

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The paper by Hooghiem presents a new technique for sampling of stratospheric air which may be well suited to add to the available techniques of cryogenic whole air sampling and AirCore. The paper is well written, although the introduction is a bit like a collection of information on sampling techniques and the use of stratospheric trace gas measurements, but lacks a clear argumentation line. The subject is very much in line with the scope of AMT.

I have a range of minor suggestions/questions and one major observation. The major observation explained below should be clarified before publication.

Major issues My main issue is the discussion of the differences between AirCore and LISA. First, it would be extremely valuable to see the paper by Chen et al., describing the data evaluation of the AirCore system used here. More importantly, I have doubts

about the way that the fractional contribution of a contamination source (fc) is derived in section 6.4. In my view, calculation of fc from observed CO2 [CO2] and deviations between this observation and the expected stratospheric value ([CO2]s) should be calculated as follows (values in parenthesis are mixing ratios):

[CO2] = fs * [CO2]s + fc * [CO2]c = (1-fc) * [CO2]s + fc * [CO2]c

With the subscripts as defined in the paper. Solving this for fc yields:

fc = ([CO2] – [CO2]s) / ([CO2]c-[CO2]s)

If I assume that ([CO2] – [CO2]s) is the difference of 0.84 ppm CO2 between observed CO2 in LISA and in AirCore, I need to make an assumption on ([CO2]c-[CO2]s) to calculate fc. If we assume that -[CO2]s is stratospheric CO2 at about 390 ppm (actually 395 is more realistic), and that the contamination is from tropospheric CO2 with a mixing ratio of around 405 ppm, then I calculate fc to be 0.84/(405-395) = 0.084, and not 0.0021 as derived in Table 6.

This might actually also explain the differences observed in April flights with respect to the September flights. During Fall NH CO2 is expected to be much smaller, thus the difference between contaminant and actual stratospheric mixing ratio is much lower than during spring (when tropospheric CO2 may be up to 10 ppm higher). This is also in line with the much higher deviations found in the laboratory experiments when having larger concentration differences.

Minor/specific observations.

p.3. l. 1: the Engel et al. trend is only representative for the mid-latitudes of the Northern Hemisphere, above 24 km altitude.

p.3. l. 3.: A reference from 1983 may not be very good to point to current deficits in GCMs.

p.3. l. 26.: could you be more specific on the allowed weight?

p. 3. L. 29.: as LISA samples during ascent, has possible outgassing of $CO_2$ from the Styrofoam be considered?

p. 5 . section 3.1. Could you comment on how dry the test samples were? This will make a very large difference, especially for $CO_2$.

p.6. l. 13.: How were the bags preconditioned. Why was N2 used and not ambient air?

p. 7. Section 4.: At which temperature was the flow characterized? Strat. Temperatures are much lower, which may influence pump performance quite strongly.

p. 7. L 8 sample, not samples

p. 7. L. 11 (and elsewhere in the manuscript): please be consistent in the use of L vs. L STP.

p.8. l. 25: please restrict this to 30 hPa, as the flow was not measured at lower pressures.

p. 9. L. 6.: Is burst pressure of the bag temperature dependent?

p. 9. L. 10: 240 K is actually very warm for the stratosphere.

p. 12. L. 17 l. 27: please be consistent in using only one value for the deviations. If the value of 0.84 ppm is used, this is much larger than stated in Engel et al., 2017. Have the authors made an uncertainty estimate for the flight on April 26, considering that no AirCore coils temperature is available?

p. 12 l. 26: It is not only molecular diffusion.

p. 12/13. Section 6.1.: This whole section lacks a conclusion. First, which is the best estimate of the deviations, and second what could cause the strong deviations during the April flight (see also my major comment above). Have pollution/outgassing been considered to explain the differences?

p. 13. L. 14.: this sentence is odd. How should a prolonged sampling time result in

increased vertical resolution?

p. 14.l. 7.: this sentence is wrong, uncertainty is mentioned twice.

p. 14. Section 6.4. see major comment above. I believe that the contaminating fraction may be much higher.

p. 15. L. 24: this is not up to 0.84 ppm. 0.84 ppm is actually an average deviation if all samples are considered.

Figure 2: the colours for the different bags types are virtually undistinguishable.

Figure 3: I wonder if all panels are needed here. I suggest removing panel b. Table 6: please explain the different columns on the table heading, not only in the text.