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***Interactive comment on* “Technical Note: Reanalysis of upper troposphere humidity data from the MOZAIC programme for the period 1994 to 2009” by H. Smit et al.**

Anonymous Referee #1

Received and published: 29 July 2014

1 General recommendation

Users of the MOZAIC relative humidity data have long waited for a fix of the problem that appeared after 2000. In this paper Smit et al. now describe the cause of the problem and how it has been solved. Most importantly they state that the corrected data are available to interested researchers, and thus the paper is a very welcome contribution. It is appropriate for publication in ACP. I have only a couple of minor comments which might help to make the paper clearer at some points.

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2 Minor comments

Page 18906

Line 11: "is not possible from thermodynamical principles" is not correct. It does not occur in the atmosphere because there are plenty of condensation nuclei that trigger condensation as soon as liquid saturation is slightly exceeded. Please rephrase.

L. 22: "large variability of observations" is a bit unclear. Is it the humidity field that has large variability (I think so) or are the observations so imprecise?

P. 18910, LI. 1-8, Figure 5: Please define what you mean with "fractional coverage of MOZAIC upper tropospheric humidity data". Explain what it is good for.

P. 18912, LI. 20-25, Figure 7: I do not see what you want to say here. Is it good or bad? My impression is that this paragraph should be shifted into the next section, after the first paragraph there. The figures 7 and 8 should be interchanged accordingly.

P. 18915

L. 6: "differences of slopes are close to zero", fortunately not the slopes themselves.

L. 8: The word "reduces" is misleading here. The impression is that the problem gets smaller, but since the offset a is often negative, the problem gets worse. You could simply state $a_{post} - a_{pre} \approx -0.2 \dots - 0.4$.

P. 18916

L. 23/24: what is the recovery factor?

L. 28: How small?

P. 18917, LI. 12-17: I suggest to state typical values in this paragraph.

P. 18918, L. 27/28: 1) "ppmv". 2) how can the FISH instrument become optically thick?

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P. 18919

L. 1: "neglect" sounds a bit strange here. What about refuse, reject, dismiss, avoid...

Ll. 16-22: To my view the comparison with OJSTER does not look very well, at least not at the higher RH values. Don't overstate.

L. 23: "proof of validity". Please change this. There is no proof of whatever. All that we see is that the pdfs look quite similar and this underpins a good quality of the MHC data in a statistical sense, but it does not prove anything.

Figure caption of Fig. 7, L. 3: delete "for details" once.

Figure 8: Although the linearity is very good with respect to RH_{UC} , the relation is not at all linear with respect to T . How do you define calibration coefficients at untested T values? A simple linear interpolation might be insufficient.

Figure caption of Fig. 11: explain meaning of the bars and the central lines.

Figure 14: too small, noisy, and hardly readable.

3 Typological errors and other issues

P. 18912, Ll. 23-25: I suggest to avoid the use of "=" in these sentences.

Eq. 2 and text thereafter: write p_{air} consistently.

P. 18915, L. 29: I suggest to replace "and co-workers" with "et al.".

P. 18916, Ll. 4-16: Is there a difference between RH and RH_D ? Both are described "ambient relative humidity".

Reference Neis et al. This is unpublished material "in preparation". It should not appear in the references list. The text should reflect that it is a planned publication, e.g. "will

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be provided elsewhere (Neis et al.). The use of two figures from a planned publication is unusual as well. I suggest not to cite Neis et al. in the figure captions. Instead Smit et al. should be quoted in the planned paper once these figures are re-used

Interactive comment on Atmos. Chem. Phys. Discuss., 14, 18905, 2014.

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