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Interactive comment on “Testing and evaluation of a new airborne system for continuous N_2O, CO_2, CO, and H_2O measurements: the Frequent Calibration High-performance Airborne Observation System (FCHAOS)” *by* Alexander Gvakharia et al.

J.R. Pitt (Referee)

joseph.pitt@manchester.ac.uk

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It has now become well-known in the trace gas measurement community that analysers employing Tunable Infrared Laser Direct Absorption Spectroscopy (TILDAS) techniques on aircraft can exhibit a strong sensitivity to changes in cabin pressure. This is currently a major limitation to the utility of these instruments for airborne sampling, as operators either have to accept large altitude-dependent biases in their final dataset or

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very low duty cycles (as the instrument must be recalibrated at each altitude). Many important trace gas species (e.g. N2O and C2H6) have much stronger absorption lines in the mid-infrared than the near-IR; whilst cavity-based measurement techniques for these species have developed significantly in recent years, TILDAS is still the most commonly used technique for measuring many trace gases in the mid-IR. Therefore improving the accuracy and duty cycle of TILDAS instruments during aircraft sampling is important if we are to improve our understanding of key greenhouse and pollutant gases.

This paper presents a novel calibration strategy to tackle this issue, resulting in greatly reduced altitude-dependent biases with a 90% duty cycle. The switch to controlling mass flow instead of pressure is a clever idea, removing issues associated with pressure instability during the sample-calibration transition. The method employed here will certainly be of great interest to anyone currently operating TILDAS analysers on aircraft, but it also provides the potential for reducing biases and/or increasing the duty cycle for other airborne instrumentation. I recommend its publication in AMT, but I have the following suggestions for minor revisions.

The water broadening correction is mentioned at the end of section 2.1. Did you determine the water broadening to air broadening ratio yourself experimentally? If so perhaps state this explicitly because at the moment it makes it seem like TDLWintel does this automatically without user intervention (unless Aerodyne tested your instrument before sending it to you – in which case mention this because as far as I'm aware it isn't what they don't usually do). Assuming you determined the coefficients yourself, could you add a brief outline of the general approach taken (e.g. H2O injection/dew point generator/etc. . .) and the uncertainties associated with it please? The uncertainty associated with this water vapour correction is often on the same order as the other uncertainties so it's important to consider it.

Section 2.3 is presented in a rather confusing way. I think this is largely because the use of two in-flight calibration gases is introduced here, but the fact that one of them is used as a check gas is not mentioned until section 4.1. It wasn't until I got to this point much later in the text that I fully understood what was going on (e.g. why the long-term drift in instrument slope needed quantifying in the lab) – it would be much better if it was explicitly stated in section 2.3 that a single-point calibration strategy with an additional check gas was used in-flight. Additionally, the term "linearity" is used in a way here that isn't intuitive to me. I would stick to using the word "slope" throughout (or "gain" if an alternative is needed), as what is being tested here is the extent to which the instrument slope drifts with time. In my mind instrument linearity is the extent to which the linear fit used here is applicable, not whether the coefficients are drifting with time. To assess linearity three cylinders with different mole fractions are therefore needed. This hasn't been done here, but these instruments are known to have a good linear response so it's probably safe to assume non-linearity is a small component of the uncertainty budget.

Section 3.2: having had many discussions with Aerodyne about this following on from our 2014/15 campaigns, I am fairly certain that the main source of the large gradients in mole fraction you see during profiles is indeed an optical fringe (or possibly multiple fringes), activated by the change in cabin pressure. However we do also see artefacts in the measurements associated with aircraft acceleration as well – for us these manifest themselves as much smaller-timescale features which would not be captured by the calibration strategy here.

Section 4: our experience is that there is significant flight-to-flight variability in the cabin pressure artefact, making it impossible to apply a single correction throughout a campaign. This is because, while the FSR of the fringe at ground level stays constant, its initial phase is variable and unpredictable. We did experiment with doing two deep profiles at the beginning and end of a flight on tank air to try and calibrate out this effect, but the drift during the flight resulted in a large uncertainty and we abandoned this approach. Essentially I'm not convinced that this effect is repeatable enough to take data from a single null flight and use it to correct other flights, so I'd probably remove that paragraph (unless you have evidence to the contrary). The method you've developed here seems far superior to any that could be developed from the null test results.

I'd be interested to know more about the suspected contamination which has resulted in the 0.6 ppm CO2 offset between the Picarro and the FCHAOS systems. Have you been able to identify which of the systems the contamination is associated with? Reading the manuscript I initially assumed that it was the FCHAOS regulator/tubing that was under suspicion, but it would be good to make this explicit, or if it is still unknown which instrument was contaminated to state that. Was there any change to the setup in future campaigns where this offset was not observed? The regulator and tubing used for the FCHAOS here are pretty standard, so if there are contamination effects associated with either it would be good to know about them! Or is the theory that the cylinder itself became contaminated (e.g. due to a mistake during regulator flushing)? Surely in that case you would also expect to see the offset in the check cylinder data in Figure 8? In the first half of the campaign (before the cylinder switch) there may be a sign of a negative bias in CO2, but there doesn't appear to be a corresponding positive bias in the second half, so this doesn't really tally with a single cylinder contamination. Also in that case you'd expect the Picarro bias only to be present in one half of the campaign but I can't see any evidence of this in Figure 7. If you haven't made any further progress in diagnosing this then no worries, but it would be good to include any extra details you do have.

The fact you don't see the same effect on the H2O measurements is very interesting, but I can't quite see this from the plots included here. Could you add another column in Fig. 4 showing H2O please? I know the tanks were dry but you are assuming (in my view reasonably) that the artefact is a simple offset shift so it shouldn't matter what the absolute value of the H2O mole fraction is – even if it is completely dry I'd expect the fringing to affect this zero-offset reading. I don't doubt your word here, but the explanations offered as to why the fringe would affect H2O less don't really make sense

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to me either so I could do with a bit more detail on these. Surely the fringe amplitude will increase with laser intensity, if anything making the problem worse? The relevant signal-to-noise here is the strength of the absorption peak (not the laser intensity) relative to the fringe amplitude. If the H2O line in question is the one at ∼2227.5 cm-1 then this tends to be a weak feature relative to the N2O and CO2 lines, so again I would have thought that H2O would be more affected by the fringe. I'm also not sure why having a line frequency of ∼2227.5 cm-1 compared to the CO2 line at ∼2227.6 cm-1 (for instance) would reduce the fringe interference. It is definitely true that a very wide absorption feature would suffer less from a fringe with a small FSR, but I don't think the H2O line is wider than the other peaks here? Sorry to labour the point on this, but the fact that you don't observe the fringe effect on the H2O measurement could be a really useful piece of information in trying to further mitigate this issue, so I'm keen to better establish the cause of it.

Section 5: Could you put details of the altitude and variability in altitude during the runs in here please? I assume it was essentially performed at a single level but if so it would be good to be clear about this just so the reader knows there are no vertical gradients convolved in here.

Specific points:

P3 L2 – Minor point but the LGR FGGA in O'Shea et al. is a near-IR instrument

P3 L26 – Typo: missing space

P5 L18 – ". . .within our 1 Hz precision. . ."

P7 L9 – ". . .at the same flow-rate. . ."

P7 L12 – What interpolation technique was used?

P8 L8 – ". . .within our 1 Hz precision."

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