We would like to thank both reviewers for the careful reading of the manuscript and the very constructive comments and suggestions, and we hope to have addressed them satisfactorily.

We have provided our answers to the comments and questions below in line in blue color.

Review of "Accurate humidity probe for persistent aviation-contrail conditions" by Dyroff et al. (2025)

Overview

Persistent contrail cirrus has become a main concern in the aviation industry and scientific community for its radiatively warming impact on the climate. Although the formation mechanism and conditions of persistent contrails are well understood, sensitive and accurate humidity measurements at aircraft cruising altitudes remain the key restriction to improve numerical weather prediction models for robust forecast of persistent contrail conditions.

This paper presents a newly developed humidity probe based on tunable infrared laser direct absorption spectroscopy (TILDAS) that can measure water vapour (H_2O) mixing ratios in the range of persistent contrail formation conditions, even down to a few parts per million by volume, with high accuracy, low noise and offset. Without pre-calibration, both prototypes 1 and 2 implementing slightly different electronic systems, data acquisition and analysis software show excellent agreement between each other and with the reference dew-point generator. Besides accuracy, the stability of the two humidity probes was tested in series over 5-day operation, proving the instrument robust. Attenuating optical power improves significantly the linear response of the humidity probes in high H_2O volume mixing ratios.

Overall, the manuscript is well-written and describes in detail the set-up, characterisation, and test of the new humidity probes. The paper is high relevant, and it fits very well with the scientific scope of AMT. I still have some comments that I like to see addressed before publishing this paper.

General comments:

1. The author describes the TILDAS humidity probes are tailored to measure humidity promoting contrail formation and are suitable for commercial and research aircraft deployment. The prototypes seem handy to measure low H_2O mixing ratios in laboratory settings. All the testing described in the paper were done in well controlled lab environment. However, as far as I am concerned, the testing is not complete to demonstrate the reliability of the prototypes for in-situ measurements aboard aircraft, despite the multiple-day operation.

1) The temperature change in the lab was up to 8°C/day (L221). What impact it have on the selected line strength, the accuracy and stability of the probes? Pogány et al. (2015)

stated that even the temperature variation during the day in the lab could have a significant effect on the absorption line strength. I would like to see a figure in Sect 3 to show whether the spectra were affected by the temperature change.

The reviewer is certainly correct that the linestrength of the chosen absorption line depends upon the gas temperature via the temperature-dependent population of the lines' ground-state energy level. We have calculated the temperature dependence of the linestrength of the absosption line probed to be 0.004 K^{-1} using the approximation given in Eq. 1 of Gianfrani et al. 2003. This temperature dependence is part of the spectroscopic fitting algorithm and hence temperature variations of the sample gas do not produce changes in the derived mixing ratio.

We have added a Table with spectroscopic parameters of the probed line and their uncertainties stated in the HITRAN database.

2) What are the author's views on real atmospheric measurements on aircraft with strong vibration, large pressure variation, weigh larger temperature difference ~70°C between the surface and upper troposphere (than the tested 8°C/day). Did the author perform sensitivity tests on the effect of low pressure and temperature on the spectra, the accuracy and stability of the probes, like in Buchholz et al. (2014), evaluating the instrument in simulated low pressure and temperature environment?

No tests of the two porotypes beyond those presented in this paper were performed.

We have added Section 5. Outlook and described how the instrument will be operated.

3) How is the attenuation of optical power implemented so that the humidity probe can keep its linearity when transitioning from conditions with low H2O values to the ones with higher values? Or sacrifice the measurements in the lower atmosphere?

Yes, it would be a choice to make before deployment. We do not plan to implement an option to switch mid-operation.

We added this to the outlook section.

4) Have the authors learned any of these during the EcoDemonstrate flight campaign? It might worth being shared in the discussion.

We have added a paragraph in the discussion.

2. It is good that the authors kept the introduction short and focused on the instrumental set-up, characterisation and tests, given it is a technical paper. However, the motivation for developing a new humidity probe might seem a bit weak. The authors listed a range of available precise in situ hygrometers and pointed out they were designed for research campaigns. WVSS-II and IAGOS ICH, as successful aircraft-based H_2O instrumentation, which are also simple, robust, and autonomous, were mentioned. But are there any limitations in the WVSS-II and IAGOS ICH making the developing of a new humidity probe so pressing and beneficial to the persistent aviation topic?

We have added a wider discussion of existing technology and their limitations to the introduction.

3. Some more information in the instrument design section should be added, e.g., dimension, weight, sample volume, optical path length, and power consumption of the TILDAS humidity probes.

Figure 2 has been updated and now shows the cell dimension.

The sample-cell volume of 72 cm^3 was given on Page 3, L74.

We have added specifics of the anticipated packaged humidity probe in the Outlook section, which has been added to the manuscript.

4. To support the contrail-avoidance decision tools, the humidity probe should deliver robust and accurate measurements with minimal maintenance and long-term stability. For example, IAGOS ICH sensors are taken back to the lab for calibration after ~500 flight hours to ensure data quality (Neis et al., 2015). We can already see a slight decrease in the slope of prototype 1 in the 50-h period. And in Figure 12, the prototype 2 had to adjust its laser scan to achieve better agreement with the prototype 1. Sometimes, the slope, offset and r_2 still fall out of the uncertainty range despite mostly good agreement. Can the authors explain how to achieve long-term stable and accurate measurements without re-calibration while deploying the probe for long-time routine operations? Any online monitoring and self-adjustment available?

We have added a sentence: *This type of adjustment is a routine task in the build process of any Aerodyne laser spectrometer.*

We are grateful to the reviewer to point this detail out. Indeed, we found a weak correlation of the calibration slope and ambient and sample cell pressures. We are convinced this correlation is not a limitation in the spectroscopic retrieval, but rather a limitation of our humidity delivery system.

In our humidity delivery system, we have set the zero-air flow via a manual needle valve. The flow through this valve is critical and thus depends on the upstream pressure. We determined that the calibration slope decreased with higher pressure, which is exactly what one might expect when the zero-air flow increases with higher pressure.

In our analysis we are not correcting for this rather clear correlation and remain with our statement of uncertainty. However, we include a new figure that shows this behavior and explain our finding in the text of Section 3.4 Accuracy.

5. The design of the TILDAS humidity probe make it preferably installed in the pressurized cabin of an aircraft, like the WVSS-II. Can the authors comment on the ambient temperature measurement that should be used to convert H_2O mixing ratio to local RH_{ice}? Relying on the temperature data from the aircraft itself? As RH_{ice} governs the fate of persistent contrail cirrus, how large is the uncertainty then in RH_{ice}?

We have added a paragraph in the Discussion that puts the uncertainty of our humidity measurements and those of the measured ambient pressure and temperature provided by the aircraft into context.

6. The author did not discuss the response time of the TILDAS humidity probes. Based on Fig. 5 and 7, they seem to respond fast to the change of H_2O mixing ratios, even below 20ppmv despite almost negligible crawling effect when switching between zero-air and H_2O measurements. The IAGOS sensor increases its response time from a few seconds at 273 K to a few minutes below 233 K (Neis et al., 2015). Therefore, it is also of high interest and relevance to see if the response time of the TILDAS increases under cruising conditions.

We have added a new Section 3.2 Response to humidity changes. The response time during the experiments was derived and is indicated in Figure 6.

Unfortunately, we cannot predict response time in an airborne deployment as it depends on the air intake and sample tubing configuration as well as flow rate. If all parameters were the same as in the lab, we predict slightly better response as the sample cell will be heated.

7. In the discussion section, the authors reclaimed the novelty design of the TILDAS humidity probe and its good accuracy and stability. However, I am not convinced so far by the deployment of such a probe on an aircraft for autonomous measurements for supporting contrail avoidance strategies. The measuring technique employing the absorption of H_2O in the near-infrared range has been well established in airborne hygrometers, such as JPL laser hygrometer (May et al., 1998), SHARC (Kaufmann et al., 2018), HAI (Buchholz et al., 2014, 2017), SEALDH-II (Buchholz et al., 2018), which can measure down to a few parts per million with low offsets and uncertainties. Interests and potential exist to have some of the prototypes adapted for routine H_2O observations for improving contrail forecasts. Furthermore, WVSS-II using the same technique has been long in operation in the (T)AMDAR network. I suggest the authors extend the discussion when discussing the performance of the prototypes by make cross comparisons with similar instruments in the aspects of technique details, measurement capabilities, airborne deployment feasibility so that the readers can easily follow the novelty, simplicity and reliability of the presented humidity probe. Thus, the suitability of the instrument for measuring in aviation contrail environment presents itself to the readers.

We have changed the wording from novel to new.

We appreciate the information that some of these instruments have potential to be adapted for routine H2O measurements. As far as we know they have not been made available as commercial products. We have provided references to the instruments mentioned and invite the reader to study their techniques in detail as discussing all the differences and similarities is beyond the scope of this paper.

We have added a paragraph in the introduction that puts published uncertainties of various instruments from the AquaVIT-4 intercomparison into context.

In the Discussion we have added a paragraph that brings the uncertainty of our humidity measurements into context with uncertainties of atmospheric temperature and pressure measurements. This is relevant as contrail formation depends on relative humidity which in turn depends on p and T. We also added a paragraph that compares uncertainties of other instrument to reference standards to provide context for our results.

Specific comments:

L32 "Supersaturation with respect to ice is a prerequisite for persistent contrails": In addition to ice supersaturation, contrail cirrus may also be persistent in slightly ice subsaturated regions depending on the sizes of ice particles according to Li et al. (2023)

We thank the reviewer for pointing out this publication.

L33 "Small variation in humidity at cruising altitudes dramatically affect their lifecycle" -> "… their life cycle by controlling ice crystal formation, growth and dissipation (Unterstrasser and Gierens, 2010)."

Reference included.

L33 "spatially and temporally resolved humidity measurements" is vague. It should explicitly be high spatial and temporal resolution measurements.

This is corrected.

L38: References after contrail formation: Petzold et al. 2020, Li et al. 2023, Gierens et al., 2020

These references are now included.

L79: The thermistor type? What is its uncertainty? Only one thermistor is inserted in the cell body. Is it located in the milled of the cell body?

The thermistor location in the middle of the sample cell body is now indicated in Figure 2. The thermistor type is now specified in Section 2.1 Optics.

Figure 2: I think the dimensions of the measurement cell worth being noted in the figure.

Figure 2 has been updated and now shows the cell dimension.

L113: The data recording interval of sample detector and baseline detector is quite clear. The authors also performed dark signal check. How is this reflected in the data acquisition and analysis? Or is this just recorded for inspection?

The detector dark signal at the end of each scan is used to perform a zero-offset correction before spectrum normalization. This is now further explained inSection 2.3 Spectroscopy.

Figure 5: The abbreviation ADEV for Allan deviations needs to be explained while it is not note elsewhere.

The definition is now provided in Section 3.3 Noise.

Figure 6: Element 11 missing in the caption.

A temperature sensor was added to the Figure as element 11. The numbering in the caption was changed accordingly.

Figure 7 (bottom left and right), 8 (top), 9, 10 and 13: I find the units of H_2O mixing ratio in “ppbv” are unnecessary because the lowest H_2O volume mixing ratio to be detected was a few ppmv.

All plots were changed to units of ppmv.

L201: “is” -> in

Corrected.

L203: “where” -> were

Corrected.

Figure 9: In the first cycle, the prototype 1 obviously measured slightly lower values than the prototype 2 at each step above about 50 ppmv, which was not repeated in the second and third cycles. Do the authors have any ideas on the cause of this?

We were unfortunately not able to link this to a specific parameter or event. We rely on the statistical approach to intercompare the instruments for extended periods, as we did, to derive uncertainties.

References: 1 Not all DOI are inserted as links. 2 Not all journal names were abbreviated. 3 The style of the references with a long author list should be unified.

The reference style was adjusted to AMT standard.

References mentioned in the comments:

Buchholz, B., Afchine, A., and Ebert, V.: Rapid, optical measurement of the atmospheric pressure on a fast research aircraft using open-path TDLAS, Atmos. Meas. Tech., 7, 3653–3666, https://doi.org/10.5194/amt-7-3653-2014 , 2014.

Pogány A, Wagner S, Werhahn O, Ebert V. Development and Metrological Characterization of a Tunable Diode Laser Absorption Spectroscopy (TDLAS) Spectrometer for Simultaneous Absolute Measurement of Carbon Dioxide and Water Vapor. Applied Spectroscopy. 2015;69(2):257-268. doi:10.1366/14-07575

Buchholz, B. and Ebert, V.: Absolute, pressure-dependent validation of a calibration-free, airborne laser hygrometer transfer standard (SEALDH-II) from 5 to 1200 ppmv using a metrological humidity generator, Atmos. Meas. Tech., 11, 459–471, https://doi.org/10.5194/amt-11-459-2018, 2018.

May, R.D. (1998), Open-path, near-infrared tunable diode laser spectrometer for atmospheric measurements of H2O, J. Geophys. Res., 103, 19161-19172, doi:10.1029/98jd01678.

Kaufmann, S., Voigt, C., Heller, R., Jurkat-Witschas, T., Krämer, M., Rolf, C., Zöger, M., Giez, A., Buchholz, B., Ebert, V., Thornberry, T., and Schumann, U.: Intercomparison of midlatitude tropospheric and lower-stratospheric water vapor measurements and comparison to ECMWF humidity data, Atmos. Chem. Phys., 18, 16729–16745, https://doi.org/10.5194/acp-18-16729-2018, 2018.

Neis, P., Smit, H. G. J., Krämer, M., Spelten, N., and Petzold, A.: Evaluation of the MOZAIC Capacitive Hygrometer during the airborne field study CIRRUS-III, Atmos. Meas. Tech., 8, 1233–1243, https://doi.org/10.5194/amt-8-1233-2015, 2015.

Li, Y., Mahnke, C., Rohs, S., Bundke, U., Spelten, N., Dekoutsidis, G., Groß, S., Voigt, C., Schumann, U., Petzold, A., and Krämer, M.: Upper-tropospheric slightly ice-subsaturated regions: frequency of occurrence and statistical evidence for the appearance of contrail cirrus, Atmos. Chem. Phys., 23, 2251–2271, https://doi.org/10.5194/acp-23-2251-2023, 2023.

Unterstrasser, S. and Gierens, K.: Numerical simulations of contrail-to-cirrus transition – Part 1: An extensive parametric study, Atmos. Chem. Phys., 10, 2017–2036, https://doi.org/10.5194/acp-10-2017-2010 , 2010.

Petzold, A., Neis, P., Rütimann, M., Rohs, S., Berkes, F., Smit, H. G. J., Krämer, M., Spelten, N., Spichtinger, P., Nédélec, P., and Wahner, A.: Ice-supersaturated air masses in the northern mid-latitudes from regular in situ observations by passenger aircraft: vertical distribution, seasonality and tropospheric fingerprint, Atmos. Chem. Phys., 20, 8157–8179, https://doi.org/10.5194/acp-20-8157-2020, 2020.

Gierens, K.; Matthes, S.; Rohs, S. How Well Can Persistent Contrails Be Predicted? Aerospace 2020, 7, 169. https://doi.org/10.3390/aerospace7120169