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Atmospheric Measurement Techniques Discussions

Interactive comment on "SEALDH-II – a calibration-free transfer standard for airborne water vapor measurements: Pressure dependent absolute validation from 5–1200 ppmv at a metrological humidity generator" *by* Bernhard Buchholz and Volker Ebert

Anonymous Referee #1

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This paper presents a comparison of the SEALDH-II hygrometer with the German PTB water vapor standards. The essential aspect of SEALDH-II is that it is calibration free and this validation effort closes the traceability chain with the German water vapor standard.

While I have only minor comments regarding the comparison with the German standard itself, this paper raises significant questions regarding its position within the water vapor observation community. The questions, which I outline below, need to be addressed

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before this paper can be published. Therefore I would evaluate this paper as accept after major revisions.

General comments:

I have only minor comments regarding the technical work itself and go into detail these below. However, the larger concern is the novelty and importance as expressed in this paper.

The authors claim that SEALDH-II is a novel hygrometer, which is a calibration-free, tunable diode laser spectrometer that bridges the gap between metrological water vapor standard and field deployed hygrometers. However, last year the authors published work (Buchholz et al., 2016) on the novel Hygrometer for Airborne Investigations (HAI), which they developed in cooperation with the Research Center Jülich. The claims made in that paper read very similar than the claims made about SEALDH-II. Both are claimed to be calibration free, with some level of metrological traceability. However, the HAI paper by the same lead author is not even referenced here, which is quite odd. It is not clear what the connection is between these two instruments and which is more novel than the other. The authors should clarify the connection between these two instruments, before claiming that SEALDH-II is a novel hygrometer.

The authors also claim that this effort is the first metrologically validated humidity transfer standard. This may or may not be completely true. During the AquaVIT -II campaign this community made a dedicated effort for a metrological validation of a number of instruments. The authors collected all observations, but never released the metrological reference observations. However, they presented this work at several conferences. That work may actually be the first metrological validation of several transfer standards.

One of the drivers for the AquaVIT campaigns was the disagreement between some aircraft and balloon borne observations. Water vapor observations of less than 5 ppmv were the most important range of this disagreement. Given the uncertainty of SEALDH-II, this instrument would not contribute to this concentration range. Despite the un-

questioned quality of the observations presented here, this raises the question of the importance of their results, especially given the frequent reference to AquaVIT. The authors should clearly point out, whether they have achieved a metrological validation of a field deployed instrument that can measure stratospheric water vapor (100 hPa, 5 ppmv) with an uncertainty of less than 5-10%.

Specific comments:

Line 18: I believe that the term 'bridges this gap by implementing an entirely new concept' is overselling their result. While their work is important, TDLAS technology is not new and has been around for quite a while. The authors own work on HAI show that this is not an 'entirely new concept'. Furthermore, given the relatively large metrological uncertainties at true stratospheric water vapor concentrations, I don't see, where a gap is being bridged.

Line 25, 'first metrologically validated': Aren't the AquaVIT-II and to some extent even the AquaVIT-I measurements metrologically validated?

Lines 38 and 57: The tropical tropopause is highly relevant for atmospheric water vapor and may show values of less than 1 ppmv. This lower limit is a common value for some regions and seasons.

Line 46: The target accuracy for field weather stations is certainly a lot lower than 15%. Field weather stations report relative humidity and 2%-5% accuracy (in RH) are more common requirements.

Line 50: WVSS-II instruments are another variant of TDL instruments. They are commercially available instruments but probably not standardized.

Lin 55: Currently, in situ observations of water vapor are done at least up to 10 hPa and at gas temperatures of less than -90 deg C.

Line 79: ... 'does not facilitate a clear accuracy assessment'. This study is still highly valuable and able to characterize the status of in situ observations during that cam-

paign. The fact that large differences, seen in earlier campaigns, were not repeated there is of great value, even though there is no direct metrological connection.

Line 87-88: Differences of a factor of two stimulated AquaVIT. Disagreements of 10% were largely considered within the individual instrument uncertainties.

Line 90f: The goal of AquaVIT was to evaluate instruments under controlled conditions, not to rigorously evaluate each instrument's uncertainties. No gold standard was included since no recognized standard was available for this setup.

Line 128: Systematic differences of 20% and more were seen during AquaVIT at the lowest mixing ratios, i.e. below 3 ppmv. The authors should point out that SEALDH-II would not help addressing this concentration range.

Line 131: I doubt that this is the 'first comparison' with a metrological standard. Water vapor has been measured for a long time and a lot of validation efforts have happened, not all published. The AquaVIT-II activities, in which the authors have played an important role, is just one example.

Line, 158, 165, 345-349: The lower limit of 3 ppmv is a significant limitation, since the <10 ppmv range is essential for stratospheric observations. At 5 ppmv an uncertainty of 3 ppmv makes the measurement effectively useless for stratospheric research. This should be discussed in greater detail.

Lines 207ff: There are other calibration free instruments. HAI, published by the authors is one of them. Some of the frostpoint hygrometers, which are being used on aircraft and balloons may be considered calibration free in the same sense. Other TDL instruments are equally considered calibration free under the definition of the authors.

Line 216, 'accuracy': JCGM (2008) recommends not using this term in a quantitative sense. The authors should explain what they refer to here.

Lines 220ff: The authors point out later in the manuscript, that calibration in the strict sense improves the measurements only, if the ambient conditions can be replicated

during the calibration. They should elaborate on this topic and consolidate the various paragraphs throughout the manuscript.

Lines 264-267: Delete. These sentences contribute nothing and could be deleted.

Lines 301f, 'One has to compare . . .' No, this comparison does not have to be done. The purpose of AquaVIT was very different and a metrological standard was not available at that time. This statement should be deleted.

Lines 306fff (section 4.1): Isn't the point of controlled static setups to minimize the impact of dynamic effects on the uncertainty estimation? Fundamentally the uncertainty of the SEALDH-II cannot be better than that of the THG. Therefore, the authors should quantify the impact of the THG dynamic effects on their static uncertainty estimation of SEALDH-II, if that is possible.

Line 314: What is an 'indirect, inertia, thermal adjustment process'? The authors should find a better term for what is meant here.

Line 342: PHG should be Primary Humidity Generator.

Lines 365ff, 'It is important . . .': What does this sentence mean? Any uncertainty estimate always implies that the true uncertainty could be smaller. It could also be at the estimate. Lines 376 through 378 are somewhat contradictory. The authors place great value that the measurements presented here are the first metrological validation of SEALDH-II. How would non-metrological validations done previously provide contribute? Do the authors imply that non-metrological validations are equally useful or even better suited to address the uncertainty issue at low pressures and low mixing ratios? As shown in this manuscript, the uncertainty of SEALDH-II at true stratospheric values (low pressure and low mixing ratios) is too large to be scientifically relevant.

Lines 428f: Why the authors would want to suppress this systematic pressure dependence? In instrument comparisons and atmospheric measurements the systematic biases are often the determining factors.

Line 667f: What do the authors want to say here? The sentence as is doesn't make sense.

Figures 6-8: The abscissa should be shown as the Log of P. This makes it easier to relate the altitude and emphasizes the lower pressures, where water vapor is more challenging.

The authors use the term 'calibration-free' excessively and should reduce it to the necessary amount. The term is defined in a dedicated section and does not need to be repeated subsequently.

The authors do not seem to be completely familiar with the water vapor observation community. Stratospheric water vapor is also observed on large and small balloons reaching all the way into the middle stratosphere. These measurements use a variety of techniques, none of which are referenced, but should be referenced. Water vapor is also measured using remote sensing (Raman and DIAL lidar), which are technologies comparable to their own. In particular DIAL measurements are considered calibration free and traceable measurements.

Lines 61-64: These statements are much too broad and even incorrect. The vast majority of water vapor observations has been quite sufficient for validation studies of models. The limiting factor in model validation is usually the availability and coverage of these observations, not their quality. The authors should change this statement.

Technical comments:

Line 52: standard (singular)

Line 60: delete 'a quite'

Line 60: measurements (plural)

Line 73: Better: The latter is particularly important for investigations in heterogeneous regions in the lower troposphere as well as for investigations in clouds.

Line 101: . . . inside the aircraft. . .

Line 281: Replace ‘)(‘ with ‘, ‘

Line 359: Delete ‘(primary standard = calibration-free)’, which is a meaningless repetition here. Also delete ‘calibration-free’ in the same line, which is again a repetition.

Line 155, 389, 413: What is the meaning of ‘holistic’ in this paper? Better to delete this term.

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