

Interactive comment on “An inverse modelling approach for frequency response correction of capacitive humidity sensors in ABL research with small unmanned aircraft” by N. Wildmann et al.

Anonymous Referee #2

Received and published: 2 June 2014

Review of ‘An inverse modelling approach for frequency response correction of capacitive humidity sensors in ABL research with small unmanned aircraft’ by N. Wildmann et al.

The paper provides a method to correct for the slow sensor response of a polymer based humidity sensor. The first section describes the relation between the water vapor concentration inside the polymer and the capacitance of the polymer. The second section described a physical diffusion model of water through the polymer. The third section describes the method for the correction of the sensor response delay due to the diffusion of water in the polymer under non-static conditions. The last section shows an

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application to atmospheric measurements and the derived frequency response spectra.

Despite the highly welcome details, which this paper provides, it suffers from some weaknesses, which need to be addressed. I would recommend publication only after some major changes, which are described below.

General comments:

The manuscript presents three weakly related theoretical descriptions of the polymer sensor: Section 2.3 describes the relation between water vapor absorbed in the polymer and the capacitance of the sensor. Section 2.4 describes the calibration and section 3 the dynamical model. These sections appear not to be connected and don't build on each other.

The inverse model used for signal restoration is only described as block diagram, without giving any specific details on how it was implemented. It is not clear, how this was done in detail and the reader will not be able to apply their method. Much more detail should be provided here.

The calibration of the sensor (Figure 3) seems to relate RH to the molar mixing ratio. However, this is not the molar mixing ratio in air, which wouldn't make sense, and it may not be the molar mixing ratio in the polymer, since it is not at all clear, how this would have been measured. A simple calibration procedure would have related the ambient relative humidity to the measured capacitance. Since the sensor is used in atmospheric measurements one would have expected a calibration also at temperatures below freezing. The authors probably performed such a calibration and should show it. Much more clarification is required here. In particular, the authors should provide an uncertainty estimate on the calibration results. How does the calibration routine make use of the theoretical model developed in section 2.3?

It appears as if their time lag correction method uses a fixed diffusion coefficient for

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any single flight. The authors pointed out that the diffusion coefficient is temperature dependent and unless a single RPA flight is isothermic, this temperature dependence should be considered. It is in fact considered by Miloshevich and Leiterer. The temperature dependence of the diffusion coefficient should be given and this issue needs to be discussed.

The discussion of state of the art requires a short review of surface and radiosonde observations, which are of more relevance to the manuscript than the stratospheric and upper tropospheric reference discussed in the manuscript. Sections 2.1 and 2.2 should be rewritten with this in mind.

Specific comments:

Page 4408, Line 12: When mentioning a turbulence scale of 10 m, the speed of the platform is missing to be able to understand how fast the sensor should be.

Page 4408, Line 13: 'such a sensor' and line 11 'capacitive humidity sensor' is a little short to introduce the polymer based humidity sensor. The authors should include a sentence or two in the abstract to introduce the sensor better.

Page 4411, Line 10: Add reference to Miloshevich, L.M., et al., 2004: Development and validation of a time-lag correction for Vaisala radiosonde humidity measurements. J. Atmos. Oceanic Technol., 21, 1305-1327.

Page 4411, Line 16: Psychrometers are used in some manual observations, but most stations use polymer based humidity sensors. However, fast response is not an issue in these observations.

Page 4411, Lines 20-24: Most of these techniques are significantly older than the references provided. More appropriate references should be used. Vaisala is the market leader in operational humidity sensors and may be referenced.

Page 4411, Lines 26: Although AquaVIT was a comprehensive campaign, its focus was stratospheric and upper tropospheric measurements, not the regime of interest in

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the current manuscript. The uncertainty estimate resulting from this campaign does not apply to measurements in the boundary layer. This reference could be deleted.

Page 4412, line 25: There are a number of commercial available polymer sensors for operational radiosondes, which should be mentioned as well. Some of these may even have faster time constants, although I am not sure about that. In any case the list of sensors the authors have looked at could be presented.

Page 4413, line 4: What do the authors mean by 'most reliable'? How do they define reliability and under what conditions was it tested?

Page 4413, line 6: What do the authors mean by 'rely on calibration and tests'? Any sensor should be calibrated and tested before use in the field. What has been done differently here?

Page 4413, line 16-20: This paragraph should be deleted.

In the equations it is hard to distinguish the upper case C (Capacitance) and the lower case c (water concentration). The authors might want to use a different letter for the concentration. Page 4414: The authors should point out the assumption that C_{poly} is constant and independent of the absorbed water.

Page 4419, Line 24: In the experiment description it is not clear whether the measurements were done at constant air flow or at static air. I assume the measurements were done switching two different air flows at constant temperature, but this is not clear.

Section 4.2: The authors should compare their results to the Miloshevich et al. (2004) or Leiterer et al. (2005). Is their result superior to the methods of Miloshevich or Leiterer?

Technical comments:

Page 4408, Line 10: Delete 'the promising platforms of '

Page 4408, Line 12: Change 'in the order' to 'on the order'

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Page 4408, Line 19: Change 'direct contact to the earth' to 'direct contact with the earth'

Page 4408, Line 20: Delete 'therefore'

Page 4408, Line 21: Delete '(upper troposphere)'

Page 4408, Line 24: Change 'The water vapour concentration' to 'Water vapour'

Page 4409, Line 1: Delete 'further'

Page 4409, Line 3: Explain what the 'structure parameter for humidity' is.

Page 4409, Line 6: Delete 'and different experiments'

Page 4409, Line 13-14: Replace 'high sampling rate with short time responses' with 'high sampling rate and a short time response'

Page 4409, Line 15: Replace 'and accuracy, of course' with 'and high accuracy.'

Page 4409, Line 20: The DLR HALO aircraft is a dedicated high altitude observatory, not really a boundary layer platform.

Page 4411, Line 3: Change 'dynamics of the sensor are' to 'dynamics of the sensor is'

Page 4412, line 17: Rephrase the expression 'which can be realized easiest for small RPA'

Page 4415, line 16: Change 'on the sensor polymer surface' to 'in the sensor polymer'

Page 4416, line 2: Change 'In each case, temperature' to 'In each case, the temperature'

Page 4416, line 5: Change 'It arises that while' to 'While'

Page 4416, line 6: Change 'depend' to 'depends'

Page 4416, line 13: Change 'the dynamics are' to 'the dynamics is'

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Page 4417, line 3: Change 'like' to 'such as'

Page 4421, line 26: Change 'ascends and descends' to 'ascents and descents' (also other occurrences)

Page 4422, line 2: Delete 'either simply'

Page 4422, line 3: Change 'or apply' to 'or to apply'

Page 4422, line 4: Miloshevich et al. (2004) is the better reference

Interactive comment on Atmos. Meas. Tech. Discuss., 7, 4407, 2014.

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