Author's response to referee #2

July 18, 2014

Many thanks for the detailed review. In the following I will comment on each point. The referee comments will be repeated in blue italic before the answer. In general, we believe we need to more clearly describe in the revised manuscript for which purpose we developed this sensor model and where the limitations and benefits are. Since the referee mentions the comparison to [Miloshevich et al.(2004)] several times, we need to address better how measurements from RPA serve a different purpose than radiosonde and ground observations. This concerns the requirements of a sensor, and also the environment, in which the sensor is used.

General remarks

- *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 sections build on each other, since the dynamical model describes the evolution of water vapour in the polymer, but our only measurement variable is the capacitance of the sensor. Therefore, section 2.3 is needed to be able to translate the measured capacitance to water vapour concentration and vice versa. Section 2.4 is needed in order to find relative humidity from the water vapour concentration at the surface of the sensor c_s, and determine C_{poly}, which is a parameter of the physical model.

- *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.*

  Application of the method can be described in five steps:

  1. Calculation of average water concentration in the polymer c_m for a time series of capacitance of the sensor according to Section 2.3.
  2. Set up of the state-space model according to Section 3.1.
  3. Conversion of the state-space model into a transfer function according to Section 3.2, Eq. 16.
  4. Deconvolution of the measured average water concentration signal c_m in the polymer with the transfer function in order to find the water concentration at the surface of the polymer c_s (Eq. 17).
  5. Recover the relative humidity from the surface water concentration c_s through the calibration polynomial (Section 2.4).

  The block diagram in Figure 7 of the manuscript is a block diagram that was created with the commercial software MATLAB Simulink, and in a first approach, MATLAB Simulink was also used to perform the signal restoration. To integrate the method into a C++-based post-processing software for the meteorological raw data, the Octave C-library was used. Octave is an open-source project for numerical operations, that also includes equivalents to most of the Matlab functions.

The function *lsim*, with the inverse transfer function and the measured signal as input variables, applies the deconvolution for real time series data. There is other software available to perform this numerical task, and it should be left open to the reader, how the mathematics of the model are applied.

- *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?*

In Figure 3, the relative humidity is related to the water concentration on the polymer surface, which in a static state is equivalent to the water concentration throughout the whole polymer. Section 2.3 describes how the water concentration in the polymer is found from the measured capacitance of the sensor element. The label on the ordinate of the figure is misleading and will be changed to 'water concentration c / mol m^{-3}'. The step of calibration is necessary, because adsorption kinetics can not be modelled, but the calculated water concentration in the polymer c needs to be connected to the relative humidity in the air (see answer to Specific Comment #9). Of course, for the user of such a sensor, the physics of the sensor is usually not as important and can be treated as a black box. Then, a calibration that directly relates the measured capacitance to relative humidity is the best way and will give good results.

Unfortunately, the facilities that are available to the authors do not allow calibration below freezing. The focus of research is on convective boundary layers in central European summer. Temperatures below freezing are usually not experienced in this regime. In future, research in Arctic boundary layers or in higher altitudes could be of interest and further studies should follow to optimize the sensor model for the low temperatures in these environments.

The calibration chamber which is used was calibrated against a secondary standard with an accuracy of 0.4% relative humidity. A root mean square error of less than 1 %RH between the calibration curve and the measured values is typically found for the capacitive humidity sensor calibration in the chamber.

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

In the case of Miloshevich and Leiterer, vertical profiles up to several kilometres with a radiosonde are regarded, where temperature differences of tens of Kelvin are observed in one ascent. Therefore, temperature dependence of the diffusion coefficient is crucial in this case. In our case, experiments in the boundary layer are of highest interest, wherein only small temperature differences occur.

In Figure 6 of the manuscript, two step responses are shown at two different temperatures. It shows that the diffusion coefficient at 5 °C is smaller than the diffusion coefficient at 20 °C. However, experiments have also been done at 37 °C, and no significant difference to the diffusion coefficient at 20 °C could be observed (see Fig. 1). This means, the temperature dependence of the diffusion coefficient is more critical for low temperatures, which were not the focus of the study, as described above. Also, in straight and level legs that are performed mostly for turbulence measurement, the temperature variance is usually not much higher than a few Kelvin. These small temperature differences are not critical for the sensor model.

Theoretically, a temperature dependence of the diffusion coefficient could be added to the model in Eq. 13. This would lead to non-constant coefficients of the state matrix in Eq. 14. In this case, the

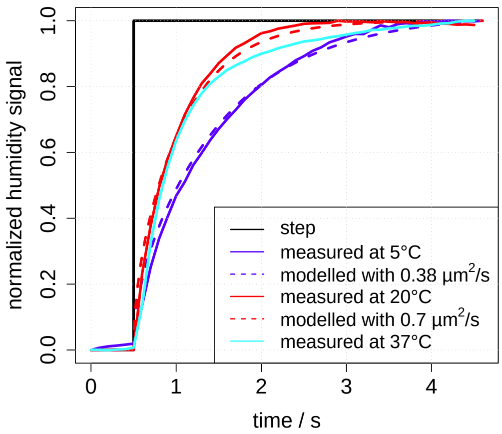

Figure 1: *Step responses of the same sensor at different temperatures. The y-axis is normalized to zero for humidity before the step and one for humidity after the step. This is legitimate since the dynamics is not depending on the step amplitude.*

domain of linear time-invariant systems is left towards a time-variant system. It is of course still possible to model such systems, but the complexity is significantly higher and out of the scope of this study.

- *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.*

We only partly agree that surface and radiosonde observations are of more relevance to the manuscript. Of course, the type of sensor that is described in the manuscript is more often used in ground stations and radiosondes, but the goal of the work is to enable area-average flux measurement of latent heat with this type of sensor. A radiosonde is not suitable for this purpose, because it constantly rises vertically, and a surface station can only provide spot measurements. In-situ measurements of area-averaged latent heat fluxes can only be done with airborne measurements like the ones that are mentioned in Section 2.1.

We will change the beginning of Section 2.1 to include more information about state of the art radiosonde and surface observations:

'A variety of sensors is used to measure water-vapour concentration in the atmosphere. Polymer-based absorption hygrometers are used for observations of relative humidity in most modern weather stations, where very fast response time is not an issue [Kuisma et al.(1985)]. Some of the most common and most modern radiosondes are the Vaisala RS92, GRAW DFM-09 and Modem M10. All of them carry capacitive polymer based humidity sensors and claim fast response times. However, radiosondes are designed to provide accurate vertical profiles, but no information about turbulent fluxes. The most widely used instrument for ground based flux measurements is the LI-7500A gas analyzer by the company Li-Cor [Eckles(2001)]. In airborne measurements, [...]'

The continuation of the section can be found in the answer to specific comment #6.

Specific comments

1. *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.*

Typical airspeed of our airborne system is around 25 m s^{-1}, which means we are targeting a sensor which is able to measure up to frequencies of 2-3 Hz without dominant time-lag errors.

2. *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.*

   Added text: 'The sensor under investigation is a polymer-base sensor of type P14-Rapid, by the Swiss company Innovative Sensor Technologies (IST) AG, with a surface area of less than 10 mm^2 and a negligible weight.'

3. *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.*

   The reference [Miloshevich et al.(2004)] will be included in a revised manuscript.

4. *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.*

   We agree that polymer based humidity sensors are mostly used in modern weather stations and will rephrase the sentence accordingly in the revised manuscript.

5. *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.*

   References were chosen that are related and close to the field of research in which the authors are working (small research aircraft / RPA applications in the boundary layer). Following references will be added in the revised manuscript:

   - [Buck(1976)] for Lyman-α instruments
   - [Campbell et al.(1985)Campbell, Tanner, and Gauthier] for krypton hygrometers
   - [Eckles(2001)] for TDLAS
   - [Kuisma et al.(1985)] for Vaisala's capacitive humidity sensors

6. *Page 4411, Lines 26: Although AquaVIT was a comprehensive campaign, its focus was stratospheric and upper tropospheric measurements, not the regime of interest in the current manuscript. The uncertainty estimate resulting from this campaign does not apply to measurements in the boundary layer. This reference could be deleted.*

   We agree that the experiment in the AquaVIT experiment is focussed on stratospheric and upper tropospheric measurements. It was mentioned, because not many comparitive studies of water vapour sensors exist, and many of the systems that were tested in AquaVIT are also used in boundary layer experiments. In fact, the instruments that were compared in this studies are the kind of sensors that are used for airborne latent heat flux measurement, which is usually not done with capacitive humidity sensors. The text will be changed in the following way:

   'The most recent and comprehensive study of hygrometers for airborne meteorology is probably the Aquavit experiment described in [Fahey et al.(2009)], that was carried out at the facilities of the University of Karlsruhe. A total number of 25 instruments were compared in a climate chamber under laboratory conditions, including several TDLASs and Ly-α instruments, as well as a DPM. The experiment targeted measurements in the upper troposphere and lower stratosphere, which is why the experiment was done at very low temperatures. A similar experiment for conditions as they are found in the ABL is not known to the authors. '

Model	**Company**
P14 Rapid	IST AG
G-US.171R2	U.P.S.I.
HIH4030	Honeywell
HYT-241	Hygrosens
SHT75	Sensirion
HMP50	Vaisala
DigiPicco	IST AG
HTM-B71	Tronsens

Table 1: List of tested polymer based humidity sensors

7. *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.*

   Table 1 gives a list of sensors that were looked at. All of the sensors were calibrated in a climate chamber for a relative humidity range of 10% to 90%, and temperature dependence of the calibration was looked at. All of them have also been tested in flight. These sensors are not specifically designed for meteorological purposes, but are a general selection of commercially available polymer based humidity sensors.

   The sensors HYT-241, HTM-B71, SHT75 and DigiPicco have a digital output that only allows sampling rates smaller than 10 Hz. HIH4030 and HMP50 are analog sensors. All of these sensors showed a slower time response than the P14 Rapid. The G-US.171R2 did show very fast response to humidity changes, but was extremely sensitive to temperature changes as well and the long-term stability of the calibration was not given.

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

   By reliability we had a long-term stability in mind, but since this is rather a subjective impression that is not backed with a long-term study, we agree that the sentence should be changed and will be in a revised manuscript.

9. *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?*

   A physical model describes the relationship between two quantities, in this case capacity and water concentration, only using natural constants, shape and material properties of the investigated sensor. This is the case for the proposed model, but the polymer property C_{poly} is not known *a priori* and therefore has to be evaluated through an experiment. Also, the adsorption can not be modelled accurately, because important properties like adsorption enthalpy and porosity of the material are not known. Since the kinetics of adsorption are presumably dominated by the diffusion kinetics (see also [Tetelin and Pellet(2006)]), we decided to use a calibration between water concentration on the sensor surface and relative humidity instead. This was not made very clear in the manuscript and Section 2.4 will be changed accordingly in a revised manuscript.

10. *Page 4413, line 16-20: This paragraph should be deleted.*
    The paragraph is somewhat important to describe the connection between the physical model and the dynamical model. Following the advice of the editor we introduced the connection of the different sections in Section 2.2. Also, more references between the different parts are added in the revised manuscript. Therefore the paragraph will be deleted here.

11. *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 Cpoly is constant and independent of the absorbed water.

We are afraid that deviating from the standard symbols for concentration c and capacitance C could even confuse the reader more. In Page 4414, line 4 it is pointed out that C_{poly} is a constant.

12. *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.*

    The experiment was done with a constant air flow. This information will be added in the text.

13. *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?*

    It is quite difficult to compare the results of the study quantitatively to the work of [Miloshevich et al.(2004)] and [Leiterer et al.(2005)]. The mentioned references do not show how well their approach works to resolve turbulent structures. The main focus is on vertical profiles. However, since the model that is used in the references, just like the model proposed in this study, describes a dynamical system of first order, we assume that the result should be quite similar, if the parameters of both models are estimated equally well. The main difference in the methods is that we derived a physical model, instead of postulating first order behaviour with a given time constant from the start. The advantage of the physical model is, that we get a better understanding of how parameters of the sensor affect the dynamics. Despite that, a physical model can be enhanced with other physical effects that might be important for the dynamics of the sensor, even if this will lead to much higher complexity, as seen in the discussion about temperature dependent diffusion coefficients.

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

    Done.

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

    Done.

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

    Done.

17. *Page 4408, Line 20: Delete 'therefore'*

    Done.

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

    Done.

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

    Done.

20. *Page 4409, Line 1: Delete 'further'*

    Done.

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

    The structure parameter, or structure function parameter [Stull(1988)], is a characteristic parameter of a turbulent signal in the locally isotropic subrange, and can also be used to estimate fluxes of the corresponding quantities [Wyngaard and Clifford(1978)]. This information will be added to the revised manuscript, including the references for a more detailed description.

22. *Page 4409, Line 6: Delete 'and different experiments'*

    Done.

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

    Done.

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

    Done.

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

    The reference to the DLR HALO will be removed in the revised manuscript.

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

    Done.

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

    The sentence will be changed to:

    'Considering all the sensors types mentioned in Sect. 2.1, only the capacitive humidity sensor can easily be integrated into a small RPA, at the present state of the art.'

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

    Done.

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

    Done.

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

    Done.

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

    Done.

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

    Done.

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

    Done.

34. *Page 4422, line 2: Delete 'either simply'*

    Done.

35. *Page 4422, line 3: Change 'or apply' to 'or to apply'*

    Done.

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

    The reference [Miloshevich et al.(2004)] is added in the revised manuscript.

References

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