Review comments #2:

General:

Many thanks for the paper “The ATMONSYS water vapor DIAL: Advanced measurements of short-term variability in the planetary boundary layer”.

This paper consists of two main ideas:

a) the description of the system ATMONSYS water vapor DIAL and

b) “claiming” of resolving turbulent latent energy fluxes (in different sections of the paper).

I would withdraw the paper and would rewrite it containing part a) as

part a) needs more information and

part b) is not sufficient. The unit of the turbulent latent energy flux is W/m2. There are no (height-dependent) latent energy flux values presented in the paper.

I would need some more explanation regarding the topic “the paper present novel concepts, ideas, tools, or data”.

There are already published water vapor DIAL systems and (even in more detail) latent energy flux measurements with DIAL systems.

Please, check

Christoph Senff, Jens Bösenberg, and Gerhard Peters, Measurement of Water Vapor Flux Profiles in the Convective Boundary Layer with Lidar and Radar-RASS,

https://doi.org/10.1175/1520-0426(1994)011<0085:MOWVFP>2.0.CO;2

Behrendt, A., Wulfmeyer, V., Senff, C., Muppa, S. K., Späth, F., Lange, D., Kalthoff, N., and Wieser, A.: Observation of sensible and latent heat flux profiles with lidar, Atmos. Meas. Tech., 13, 3221–3233, https://doi.org/10.5194/amt-13-3221-2020, 2020.

Many thanks for this review. The comments helped to significantly improve this manuscript. We carefully revised the manuscript based on the comments by both reviews. When going through the individual points of criticism, we came across a small but relevant error in the previous calculation of water vapor absorption coefficients. Therefore, all plots including DIAL data were recalculated and are now in their updated version. As a result, some of the comments do not apply anymore.
A detailed explanation on the changes made, due to the recommendations and requests, is following below.

In more detail:

- Lines 40-42: It seems that a permanently measuring system for the WMO is the goal of the ATMONSYS developers? Otherwise it makes no sense to refer to the requirements of the WMO.
    - The reference to the WMO requirements seems to have been misleading. As the ATMONSYS lidar is an experimental development, we don't claim it to be an operational instrument that immediately helps in the improvement of numerical weather prediction. However, from a developers perspective, the WMO requirements can be seen as goal system performance parameters. Recent studies incorporating single high-resolution water vapor lidars show persisting effort in the improvement of model parameters based on enhanced knowledge gained by lidar systems. Therefore, we would argue that the WMO observational requirements are a fair motivation.
    A respective sentence, clarifying that the ATMONSY lidar isn't thought to be operational, has been added (l. 42-44/736-737).

- Only absolute values of the required uncertainties are given at the web page of the WMO. But, the authors indicate 5% which is a relative uncertainty. The truck in Fig. 1 looks like a mobile system which could also be used at different areas. In more dry areas one would need a system with an uncertainty of less than 2-10 g kg^{-1} (marked/breakthrough, threshold values of the WMO for altitudes at near surface). Aside, I didn't find requirements defined for air specific humidity at larger altitudes at this web page. I know that in arid regions the water vapor content can easily be below 1 g kg^{-1} (at ground and at lofted altitudes). Given the presented lidar ATMONSYS, I don't understand the argument of line 40-42.
    - We apologize for a mix-up of numbers due to different sources. Indeed, the given uncertainty numbers are given as absolute values. The numbers for both uncertainty and temporal resolution have been adapted to the actual entries for "high resolution numerical weather prediction inside the PBL"as they are noted on the respective website.
    As we show in Sec.4.2, the statistical uncertainty of our measurements is below 5%. Therefore, regarding the statistical uncertainty, the ATMONSYS uncertainties should even fulfill the WMO "goal", regardless of arid or moist regimes, which is due to the measurement principle itself. A respective remark has been added to l. 238-239. References to the WMO values are changed within the entire manuscript.

Sec. 2.2:

- Why is no beam expander installed? Is the emitted light polarized? If yes, how is the transmission of the different polarization directions and the retardation (of all used channels) in the receiver?
    - Good point. This idea was discussed during the setup but ultimately dropped. An explanation is added in l. 132-135.
    - The missing information on the polarization of all wavelengths was added in l. 131).
    - The retardation of the s-polarized Raman signal is now included (l. 145-148).

Sec. 2.3:

- Line 158: The digitization range of the transient digitizer 12 bit, yielding to the dynamic range of the DAQ of 1/4096. The detector coupling to the digitizer needs to be explained (compare line 640). The AOD-dependent maximum range of water vapor measurements (lambda_on) should be given with this number and a standard atmosphere at mid-latitudes.
    - This part of the description was probably misleading. Although the digitizer works with 12 bit, the memory depth of the digitizer is 24 bit, from which one bit is taken as flag clip in case of signal overflow. As we operate the system only for 10s (1000 shots), we effectively only use a maximum memory depth of 21 bit. Therefore, the effective resolution of 1 bit would be 500 mV/2^{21} ~ 0.2m µV, which is below the transient digitizer noise of ~0.5mV.
 We hope this explanation, together with the changes in l. 164-168 meets your concerns.
- Lines 189-190: How stable are the emitted wavelengths? How is the seeding working? Which control is installed in the Ti-Sa that it follows DL1 and DL2 from shot to shot? How is the wavelength of the emitted light monitored?
    - At this point a reference to the previously cited paper Vogelmann et al. 2022, including detailed information on those features, is added.
- Lines 208-210: The “logistic function” may be necessary, but needs more explanation. There must be a “physical motivation”! Otherwise, the authors should avoid discussing data below 1.1 km altitude. As I read, the median value is fitted to the “closest available radiosonde”. Hence, it seems to be a height-dependent calibration? What is done in case of a variable water vapor profile (known and also later shown). What is done if radiosonde data are not available?
    - With the newly calculated data (not being affected by erroneous absorption coefficients anymore), the DIAL data behavior changed slightly.
 Nevertheless, during some (but not all) measurement periods, data still shows odd behavior at the very lowest altitudes. We still assume that this is an issue of some detector overflow but, considering your justified remarks, we decided to not do any corrections on the data anymore.
- At detailed error analysis of the ATMONSYS data is missed! This is an absolute prerequisite for a newly constructed system.
    - We agree. So far, there has been a error analysis included within Sec. 4.2. However, it seems that this Section wasn’t explained in a sufficient manner. Therefore, we now included a more detailed explanation of our error calculation within Section 2.3.

- Table 2: More information is needed to the errors, the measuring range (under certain conditions). The authors didn’t show data until 4.5 km altitude. That’s why, I’m a bit anxious about this value in the table. Beam divergence of the laser: please, indicate that the given value is the full angle beam divergence (some scientists think about a half angle when using the words “laser beam divergence”).
    - Information on the statistical uncertainty and measurement range are now given. The values are a conservative estimation based on realistic simulations. Changes in SNR, (distribution of) humidity, (distribution of)

aerosol, and choice of the actually tunable wavelength for λ_{on} , however, do alter the specifications.
    - For consistency reasons with the shown data, both Table 2 and Fig. 3 now only refer to a max. altitude of 3.5km
    - The divergence is describing the full angle, which is now clarified within the table

Sec. 3:

- Lines 230-244, RAMSES lidar: I didn't find the measurement errors and the vertical resolution in the description.
    - A respective sentence has been added at the end of the "RAMSES"-subsection.
- Lines 245-252, ARTHUS lidar: I didn't find the measurement errors in the description.
    - Details on the error calculation, including respective references, have been added to the "ARTHUS" subsection.
- Lines 253-265, Doppler wind lidar: I didn't find the measurement errors in the description.
    - A respective sentence has been added at the end of the "Doppler wind lidar"-subsection.
- Lines 259-260: I didn't get the content of the sentence "We removed the data with a high noise level by filtering with a relatively low Signal-to-Noise Ratio (SNR) + 1 threshold of 1.000 to keep the data availability high."
    - This admittedly strange value, still leading to filtering of data, comes due to the wind lidar's system-internal calculation of SNR values. A short sentence of explanation has been added to this line.
- Lines 266-270, Radiosonde: I didn't find the measurement errors in the description.
    - Information is now added.
- Lines 271 and follows: Measurement day: 18 July 2021.
    - All occurrences are changed accordingly.
- It seems to me that a new air mass has moved over the old one during this day as the depolarization of the particles (lines 284-286) and the wave structure in Fig. 7b indicate. Hence, it might have not been the best day for presenting the data.
    - We agree that there is a change in aerosol type at higher altitudes during the day, which could also indicate a change of air masses at those levels. However, the air mass inside the PBL seems to stay the same during the day, which is the time range where we investigated general system capacities, short-term variability and turbulence characteristics.
      For the intercomparison between the different lidars and sondes (Fig. 13), to our opinion, it should not be disturbing whether or not there is a change in air masses between the two shown timesteps.

Fig. 5:

- There is a need to specify the local time relation to UTC since the local time indicates when the sun is at its highest and one may expect the largest height of the PBL. It seems that the largest height of the PBL is at 16 UTC which equals to 18 local time. Please, correct me and explain the details

    - The information on local solar noon (~11 UTC) has been added to the caption.
    - Regarding your second point, in accordance to comparable criticism by Review#1:
      The PBL height determination purely based on the gradients of aerosol/humidity may lead to deviations in comparison to the thermodynamic/kinetic energy PBL height.
      To our understanding, a proper PBL height determination based on wind data requires horizontal wind information to identify the low level jet nose. However, the Doppler wind lidar next to the ATMONSYS system has been operated only at vertical stare. Calculating the temporal standard deviation of the vertical wind on each height leads to vertical profiles of σ_w. However, it seems to be the case that an automated PBL height determination based on this measure very much relies on personal choices for thresholds (e.g. : https://doi. org/10.16993/tellusb.1876).
      As a work around, we performed PBL height calculations based on the bulk Richardson number with the available radiosonde data. The respective values are included into the manuscript (Tab. 3).
      Above that, there was a VAD scanning Doppler lidar approx. 7 km away producing horizontal wind speed data. The sparse data that is available from those measurements, however, confirms a maximum in wind speed at around 1500m agl during that time.
      Therefore, we trust the radiosonde data as a rough estimation of the PBL height development.
      A table with the calculated values as well as explaining sentences have been added to the subsection “Measurement day: 18 Jul 2021”.

- It would also be possible and helpful to indicate the PBL height in one of the charts.
    - We decided to include the non-continuous bulk Richardson number PBL heights in Table 3. Explanations to this decision are given in l.332-341.
- The rainbow scale of the colors in charts b and c is not suited for the given values (especially not in the PBL).
    - We’re not quite sure how to interpret this comment. In both cases b and c, the scale covers the entire value range.
      Especially with respect to Fig 5c: In our opinion, it should not be of utmost importance to distinguish between one-digit values, but rather to recognize the apparent change in aerosol types, which is enabled by the chosen scale of colors.
- Why do some charts show clouds and others not as all charts are from one system?
    - The clouds are prominent in the particle backscatter coefficient, but only vaguely discernable in the particle depolarization ratio because in water clouds, depolarization is close to the molecular background. Water vapor mixing ratio and fluorescence backscatter coefficient are not directly related to clouds and thus show no cloud signature.
- I missed a chart of the ATMONSYS data in this Fig.!
    - We’d also like to have one. But regardless of our own wishes, ATMONSYS did not measure uninterrupted during that day.
      Therefore, for the sake of good visualization, we only use the uninterrupted

RAMSES data to deliver a general overview of the atmospheric conditions of the measurement day and use the ATMONSYS data only for specific time intervals.

- There might be differences between the values measured by ATMONSYS and RAMSES (comparing Figs. 5a and 6a; time between 12 and 12.6 UTC; at least in the altitude range between 2.5 and 3.5 km)?
    - There are two aspects that have been changed:
        - First, the former units for humidity in Fig. 5 have been g/kg – in contrast to g/m^3, which is used for all the other plots. This has been changed to guarantee consistency
        - Second, as explained earlier, the former DIAL data has been afflicted by falsely calculated sigma values. This has been changed. To meet the criticism regarding a correction of values in the lowest altitudes, the DIAL values – in this raw representation – are too high below 0.5 km at this point of time.
          At higher altitudes, values are of similar magnitude.

<u>Sec. 4:</u>

- Lines 306-308: Never trust colored plots when looking for data out of the colors. But its okay, PBL top is ca. 1.2 km.
    - You're right. Those lines have been changed in accordance to the radiosonde based PBL top calculations (see remarks for Fig. 5).
- Lines 317-319: The same explanation is necessary for the wind.
    - Thank you for pointing this out. The wind representation is in absolute values – a corresponding explanation has been added.
- Line 322: delete "drastically" as it is "only" the factor of 2.
    - Deleted.
- Lines 325-327: The wind direction changed at the PBL top. Could this indicate a different air mass?
    - There might be a misunderstanding. The presented data only refer to vertical wind speed, not to horizontal wind directions. The Doppler Lidar next to the ATMONSYS lidar was in vertical-stare only and, therefore, cannot provide horizontal wind information.
      The radiosonde wind data does not show a particular change in horizontal wind directions apart from a regular Ekman spiral behavior.

- Lines 350-353: What a pity.
    - Indeed...

<u>Fig. 6:</u>

- Could you please confirm that the data are larger than the system noise level?
    - Yes.
      This can also be seen from Fig. 8 (right side, blue line), where the median value of all statistical single-profile relative standard deviations from the 1h

period is plotted. An explanation on how this statistical error is calculated, meeting your above request on a more detailed description of uncertainties, has been added to Sec. 2.3.

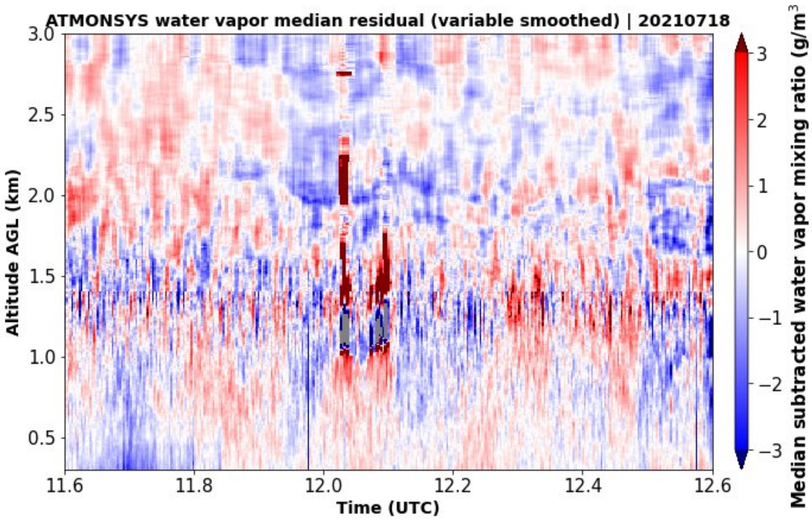
- Could the “wave-like” structure of the particle backscatter coefficient between ca. 1.8 and 2.5 km altitude be caused by the slightly existing wind sheering?
    - Fair question, which we cannot answer for sure, as is now included in the manuscript (l. 365/366)
      Possible explanations could be wind sheer, orographic obstacles leading to waves or maybe even convection-driven vertical momentum that is propagating into higher altitudes.

Fig. 7:

- a) looks much noisier than the other charts especially > 1.2 km. Why should the water vapor show different structures? Would it be possible to smooth the water vapor data in altitudes > 1.2 km?
    - Good point. First, there has been a small mistake in the caption of Fig. 7. Originally, it stated that Figs. a/b show relative deviations (in contrast to the scale description of a, which is actually right). As a matter of fact, Fig. 7a really shows absolute deviations. The caption text has been corrected accordingly. The reason for absolute deviations in Fig. 7a is to enable a visual pattern recognition within the PBL. The colors there would be much more transparent if the visualization would be changed to relative deviations as well.

      To our understanding it is plausible that the fluctuations of water vapor and aerosol are independent from each other as they have different sources and, more important, different vertical distributions. As can be seen from Fig. 6b, Aerosol concentrations reach comparable values above 2.5 km as below 1 km. Humidity, in contrast, shows a general decrease over height. Therefore, water vapor looks much noisier above the PBL.

      In principal, it would be possible to apply a data smoothening above 1.2 km. We attached the corresponding plot, where, above 1.2km a temporal, vertically linear increasing, smoothening is applied. The increase of the smoothing interval starts with 10s at 1.2km up to 90s at 3km altitude. However, we fear that such a representation could lead to misunderstandings in its interpretation as it would be inconsistent to the other subplots.



Sec. 4.2:

- This section is not straightforward. There are no formulas given at all. For me it would be helpful to have first insights into the errors of the measurements (together with an error propagation analysis) and afterwards a second subsection with observations of the atmospheric variability.
  - Thank you for this feedback. We agree that the introduction of error propagation was too sparse and that it would make more sense to introduce them at an earlier point within the manuscript. Therefore, we included a detailed explanation on the calculation of the statistical uncertainties in Sec. 2.3. – now also represented by its title. By this, Figs. 7/8 should be much better understandable.
- Fig. 8 and all the explanations regarding this Fig. make no sense with the sentence at lines 425-427.
  - Allow us to convince you otherwise. Only shortly after the radiosonde ascent, for which the data is shown within Fig. 12, the DIAL data is afflicted by clouds. Therefore, another consistent time period had to be chosen for the data analysis covering a longer time period. The time period 11.6-12.6 has been chosen consistently for most of the other plots as this was a time period without major cloud occurrences nor any major weather change. Including the radiosonde ascent data from 1 h earlier is done for two reasons: (1) to get a general idea whether or not the humidity structure changed entirely - which is not the case.
    (2), Fig. 8 shows a profile-to-profile variability within the DIAL measurements. The idea of this representation is to get an impression to what extent this variability can be explained by instrumental noise or "atmospheric noise" - which is then discussed for this Figure (e.g. based on the "narrowing" of variability at higher altitudes).

- Line 423: Usually, the ascent speed of a radiosonde is 5 m/s. This means, that the radiosonde reaches an altitude of 5x60x5 m = 1500 m in 5 min (compare also line 587).
  - Thank you for spotting the mix-up of numbers. Up to the altitude of 3km – where noise starts to become problematic – the radiosonde needs 10 min.

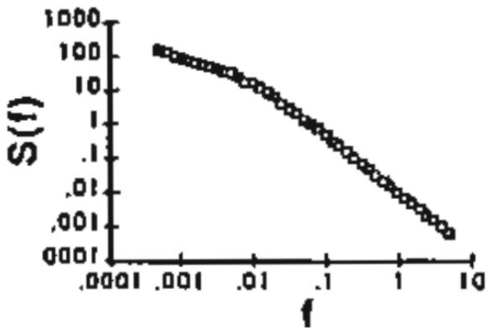
Sec. 4.3, Turbulent spectra:

- Lines 430-431: There is not only the mechanically caused turbulence but also the thermally.
  - Indeed. This information has been added.
- Line 432, “strong winds in the free troposphere”: the wind speed is almost the same in the upper PBL and the lower free troposphere (< 2.5 km)?
  - “Strong winds” has been changed to “Stronger winds”. The horizontal wind speed from that particular day isn’t visualized within this paper. However, horizontal wind speed generally increases towards higher altitudes due to lower friction, typically reaching a local maximum at the “low level jet”. This behavior is validated by the respective radiosonde data which is not presented in this manuscript.
- Lines 432-433: Please, numbers of eddies diameters and PBL heights => for crosschecking.
  - Added.
- Lines 438-439: Please, refer to the two given references in more detail.
  - Thank you for reminding us of the Senff et al. 1994 publication, which has been gotten off our memory. We included it in here as well and made the sentence more specific
- Line 448: How long did you average? With other words, how long did you assume a “frozen atmosphere” (Taylor hypothesis). The same equation needs to be defined for the quantity “wind vertical velocity”.
  - The information of 2h has been added in the subsequent sentence, the equation for wind is now defined as well.
- Line 463: “good agreement” => how good?
  - The statement “good agreement” is based on visual perception, which is now clarified for the respective sentence.
- Lines 474-475: The selected measurement day seems to be not optimal? Why not taking another day?
  - Following this logic, such analyses would exclusively make sense for static atmospheric states over absolute homogeneous terrain, in the end targeting for the smoothest turbulence spectra in “lab-like” environments.
    From personal conversation with other humidity lidar operators, it is quite common that perfect Kolmogorov behavior isn’t achieved. We reason, and discuss this within the manuscript (Sec. 4.3), that it is worth looking at non-ideal cases and investigate whether such analyses give information on turbulence chracateristics over heterogeneous terrain – potentially delivering a good method to better understand transport processes if future measurements would show equal behavior. To our knowledege, this is a novel perception to the topic of Kolmogorov anlaysis for humidity lidars, calling for further, future, investigations.
- Line 480: “Thus, the co-spectra can be seen as frequency spectra of the latent heat flux.” => but the latent heat flux is more than spectra. It is a height-dependent value in W/m2. The error analysis (including error propagation) is missed.
  - The formulation has been misleading. The respective sentences have been changed and extended (l. 551-555).

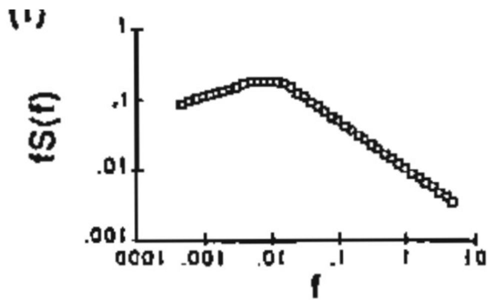
- Lines 506-507: The measurement equipment is only useful if its noise is less the "noise" of the observations. I feel, that the white noise from the instruments in the frequency spectra must be observable at higher frequencies and/that the -5/3 dependency is resolvable at lower frequencies than the white noise.
    - We agree. This sentence is formulated in a misleading way. From a pure theoretical perspective, assuming that the temporal resolution of the instrument is much higher than 10s, one would leave the inertial subrange with the -5/3 dependency towards high frequencies. This would ultimately lead to white noise behavior.
      However, as can be seen by turbulence analysis for wind measurements (e.g. in https://doi.org/10.5194/amt-13-969-2020), the inertial subrange reaches much deeper into high frequencies than can be shown with data sampling every 10s.
      However, there could already exist white noise at the shown frequencies if the instrumental noise would be too high. To our understanding, this isn't the case as the -5/3 behavior can be seen at the highest frequencies that are resolved by the 10s measurements, validating the ATMONSYS DIAL data quality for turbulence investigations at the given resolutions.
      The sentence has been slightly reformulated to make our point clearer.

Fig. 10:

- The results are not understandable. Why is the co-spectral power largest at the lowest frequencies (largest eddies) at 358 m AGL?
    - Please note that these are co-spectra that are not pre-multiplied with the frequency; therefore, a drop-off at low frequencies is not necessarily to be expected, cf. Fig. 8.9(e) of Stull (1988).



    - This would be different for pre-multiplied co-spectra, cf. Fig. 8.9(f)



- Why differ the results so much from the observations of Senff et al. (from 10 July 1991, PBL top at 1100 m)? Both observations (Senff et al. and Speidel et al.) are made during summer time and the boundary layer heights are almost the same.
    - Regarding the co-spectra, the explanation is given within the previous point. Nevertheless, looking at the general structures, to us, the results aren't necessarily that different, but a comparison might be complicated by differences in the resolved frequencies. Senff et al. measured with a temporal resolution of 60s and a vertical resolution of 75m. It might well be, that those resolutions weren't sufficient to resolve a Kolmogorov-behavior within the co-spectra. Looking at Fig. 10 from Speidel et al., obvious Kolmogorov-like behavior only starts at frequencies >10^{-2} Hz. This part of the spectrum wasn't resolved by the 60s measurements from Senff et al.
    - In addition, taking into account the reasoning that surface heterogeneity can lead to different spectral characteristics, measurements at different locations might as well lead to general deviations.
      In the specific case of the local measuring site in Lindenberg, there are surrounding landscape structures that could even more explain your previous point. Considering that the lowest shown frequencies in Speidel et al. belong to times of a bit more than 0.5 h (1000-2000s), and taking into the account the horizontal wind speed of 3m/s, this would correspond to structures of ~3-6km. Both the "Scharmützelsee" (Lake Scharmützel) as well as the town "Fürstenwalde/Spree" are in the direction from where winds on that day were coming from. It doesn't appear to be unrealistic, that such structures cause low-frequency contributions to the spectra. (Sentence added to l. 619-621.

- The section is titled "Turbulent spectra". This represents the presented results.
    - Ok.
- The unit of the turbulent latent energy flux is W/m2. There are no (height-dependent) latent energy flux values presented in the paper.
    - True. The scope of this paper is the introduction of the ATMONSYS lidar for the observation of humidity short-term variability. In order to categorize its general suitability for the calculation of latent energy fluxes we performed a general turbulence spectra analysis. The atmospheric interpretation of calculated latent energy fluxes, to our impression, would be interesting in another publication with pure focus on atmospheric processes. This clarification has been added to (l. 551-555).

Sec. 5

- I would like to see more detailed discussed inter-comparisons with other measurements to proof the performance of the ATMONSYS.

  So do we, however, there wasn't a larger temporal overlap between the three different instruments.

Sec. 5.1, Radiosonde:

- Please avoid long explanations (lines 546-574), as the well-known fact “A spatial mismatch due to radiosonde drifting would be a more plausible cause.” replaces the discussion before.

  With the updated figure, this subsection has now been partly rewritten and shortened.

- There are many inter-comparisons published, but references to them are very rare (for instance to the campaign COPS; https://projekte.uni-hohenheim.de/cops/).

  True. We extended our citations by corresponding references – including COPS (l. 651-653).

Sec. 5.2, Other water vapor lidars:

- The error bars of all systems are missed.

  They are now included.

- Lines 633-635: It is only possible to present inter-comparisons which are in detail discussed including the correction term G. There is no sense for presentations of “half-inter-compared” results.

  Thank you very much for this substantial and understandable criticism.
  Purely looking at this from a theoretical standpoint, you are right that the G term should be considered. Nevertheless, there are practical reasons against the implementation of the G term.

  As is discussed at l. 192-202, the G term consists out of two terms (let’s call them G1+G2). The 2nd term G2 is specially relevant in regimes where molecular backscatter dominates over particle backscatter. For lower tropospheric profiling, Bösenberg 1998 says “In most cases G2 will not have a major influence on the measurement accuracy” – due to aerosol load at those altitudes.
  For PBL measurements, the 1st term G1 is more relevant than G2 as it includes the derivation of the backscatter-coefficient-ratio, meaning that this term is big at altitudes where aerosol concentrations are changing rapidly – as it is the case for our measurements (Fig. 6b). The problem now is how to calculate this derivation which is numerically unstable for noisy signals. If one would consider a high-resolution bin-to-bin derivation, the humidity concentrations would receive an odd oscillation. If, on the other hand, signal smoothing is applied, G1 stays big in altitudes where there is actually no change in aerosol concentrations – adding an unnecessary error on healthy signals. This leads to broad “stripes” of erroneous humidity values. We made test calculations for the shown time period where we saw that the relative errors made by omitting G is around 2% in areas with no rapid aerosol change. This is below our overall statistical uncertainty and, therefore, to us, it appears to be better to neglect G rather than adding artifacts on healthy signals.
  As a second estimation, we considered Bösenbergs argumentation around Table 2 in Bösenberg 1998. There, he calculated relative errors for an assumed atmospheric aerosol load. In our specific case, the atmosphere has about a factor 5 less extinction as compared to Bösenbergs assumptions. In his Table2, this would lead

us to his “entrainment zone (EZ)” uncertainties, which for 1σ would lead to maximum relative errors of 2.7%, matching well to our test calculations.

The proper handling of G, in our opinion, calls for a special case sensitivity study on its own – which is out of scope for this manuscript.
We aren’t aware of any existing work on this specific issue.

- Line 640, full saturation of the channel: I didn’t get this idea. Usually, this should be avoided by the proper design of the detector coupling to the digitizer.

True. However, the ATMONSYS lidar is a non-operational, experimental system. The issue of non-linearity can already come up at 50% of the voltage range, knowledge that we gained only afterward by communication with the transient digitizer manufacturer. A respective explanation has been added (l 166-169/702-710).

- General remarks:
  I would need some more explanation regarding the topic "the paper present novel concepts, ideas, tools, or data". ·

  We have slightly rewritten the abstract and conclusion to better highlight the novel aspects of our work. Specifically, our paper presents several innovative contributions:

  - First presentation of data from new measurement tool: This is the first presentation of data from the ATMONSYS DIAL, which operates using a newly developed Titan-Sapphire (Ti:Sa) laser concept.
  - Atmospheric humidity variability: Our study addresses questions on short-term atmospheric humidity variability from the planetary boundary layer (PBL) deep into the lower free troposphere and shows:
  - High-resolution inter-comparisons: We conduct rare inter-comparisons between high-power humidity lidars with high tempo-spatial resolution.
  - New perception of Kolmogorov spectra for humidity: We introduce a new perception of "energy steps" in Kolmogorov spectra from humidity lidar, particularly in relation to heterogeneous surroundings.

  In our opinion, these topics are not yet exhausted and represent advancements in the field that are worth being published to a broad scientific community.