Overall, the authors provided satisfactory responses in the revised manuscript. However, a few remaining issues need to be addressed. Note line numbers are from the updated (not the redlined) version of the manuscript

1.) Lines 39 - 44: The authors specifically call out the World Meteorological Organization (WMO) requirement #379 for improving High-Resolution Numerical Weather Prediction and use a subset of the 'breakthrough' levels as motivation to construct their instrument. If this science motivation is valid, why leave out the horizontal resolution requirement, in this case 5km, since observation systems have to meet all these requirements. This point is clear from the description of the scientific need (cited on the WMO website[1])

> "High-resolution (HR) Numerical Weather Prediction (NWP) focuses on observing systems required by high-resolution NWP models… The added detail is made possible by a finer computational grid on a specific area, more detailed specification of terrain, more sophisticated prescription of physical processes **mainly based on explicit rather than parameterised formulations, and, importantly, denser and more frequent observations** (with respect to global NWP) **to specify appropriately detailed initial conditions**." [emphasis added]

In the first round of reviews, I mentioned this instrument has no realistic path to meeting such a dense horizontal spacing. And the author's response was:

> "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."

While this may be true, it ignores the point – development of single instruments used for parameterizations is not being requested here. This instrument does not provide the observations required to improve High-Resolution NWP and would not in the future simply due to economic considerations. So I am confused by the assertion in [line 40] that "the need for high resolution measurement data is reflected by the breakthrough requirements" of High-Resolution NWP goals. Ignoring the dense spatial observations needed to provide initial conditions is a misunderstanding of the science requirements of WMO #379.

I suggest simply dropping the references to this particular WMO goal within the following sections, as it is not relevant
Lines 39-44. "~~The need for high-resolution measurement data is reflected by the breakthrough requirements on measurement resolutions formulated by the World Meteorological Organization (WMO, https://space.oscar.wmo.int/observingrequirements) which, for "High-Resolution Numerical Weather Prediction" in the PBL are asked to be at an uncertainty level better than 5 g/kg, with vertical resolutions $\Delta z \leq 200$ m and temporal resolutions $\Delta t \leq 60$ min. Those requirements can be seen as a desired threshold for instrument development, even though the herein presented ATMONSYS DIAL is an experimental system which is not intended for routine operation~~."

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[1] https://space.oscar.wmo.int/applicationareas/view/2_2_high_resolution_numerical_weather_prediction

And in the conclusion lines 738-741 “The instrumental uncertainties have been demonstrated to stay below a level of 5 g/kg, ~~as demanded by WMO "breakthrough requirement" for operational PBL water vapor profilers used for monitoring and data assimilation into meteorological models. Despite fulfilling these requirements, the ATMONSYS DIAL is of experimental nature and not yet intended to be operational~~.”

2.) Lines 203 ”... the ATMONSYS has been designed without an additional HSRL channel, following the thoughts of (Späth et al., 2020), effects of the Rayleigh-Doppler-broadened signal can be assumed to be quite low if the online frequency is chosen to be near the inflection point of the absorption line.”
RD errors are **minimized** when at the inflection point. But it is unclear if the instrument was operating at that point. In Figure 13 b, the DIAL data has couple sharp features which are not seen in the collocated Raman observations. Could not these be RD errors, especially since it is said that at least one lines up with strong aerosol gradients? This philosophy of minimizing RD errors raises a further concern. Operating at an inflection point seems impractical since water vapor number density changes significantly from season to season. It does not appear that ATMONSYS utilizes multiple absorption lines of varying line strength to always operate at the inflection point. If the instrument is wavelength tuned (to optimize the optical depth for minimum relative error) based on the atmospheric conditions and seasons, Rayleigh-Doppler-broadening errors should not be ignored under all conditions. For example, if this instrument was operated in dry & cold conditions, would it be wavelength tuned away from the inflection point and reintroduce RD errors at strong gradients? Perhaps it is better to say something like the following… For this measurement set, taken on a humid summer day when the system could be operated near the inflection point of the absorption feature, we assumed RD errors were small. But then the data shown in Figure 13b may indicate otherwise.

3.) Line 240 “During some measurement periods, the calculated humidity profiles show an odd artifact of too high concentrations at low levels’. And line 247, “As the reason for the behavior remains to be unclear, the presented data hasn’t been modified by any correction function.” This is a significant improvement over the first manuscript, and likely more helpful to community developing DIAL instrumentation.
Line 680 “The reason for the steep humidity increase in the DIAL data towards the ground might be detector issues…” It seems plausible that this could also be caused by stray light from the outgoing pulse. Its decay could create a sloping baseline (e.g, during this time duration) which results in bias.
Line 706 “In contrast to Fig. 13a, the issue of unrealistically increasing humidity concentrations at the lowest altitudes is not apparent at this point of time, indicating problems with nonlinear signal behavior during daylight conditions and thin clouds.” The data shown does not fully support this statement. While it is not as pronounced as the daytime case, it appears that the bias remains in the night case. There is increasing humidity (~2 g/m^3) from 700m to 400 m in the DIAL data that is less pronounced (~0.75 g/m^3) in ARTHUS and radiosonde. So the conclusion that the problem lies with the nonlinear signal behavior during daylight conditions

seems less likely. I fully agree with what is stated in line 720 – more data is clearly needed before definitive conclusions are reached.

4.) Section 5.2. Since the Raman lidars has been calibrated to radiosonde ascents, please state clearly in the text if the Raman lidars used the same radiosondes for calibration that is being compared in figure 13 a and b

4.) Based on the above points, I suggest a slight revising of the conclusion
Lines 758 “However, a potential problem with non-linearities caused by overload of the transient digitzer pre-amplifiers has been recognized under certain conditions as e.g. fragmented wisps of clouds passing over the lidar. This, together with steep gradients of aerosol concentration, has shown to be potentially problematic for DIAL humidity measurements “
I suggest that “caused by” could be changed to “perhaps caused in part by”. And it would be helpful to note that RD errors were not corrected in this case.

Minor grammatical errors

1. Line 4: Titanium or Ti:Sapphire not ‘Titan-Sapphire’
2. Line 172: “almost overflow from single shots” suggest changed to “partial overflow”