Response to the Reviewer #2:

First of all, we would like to thank the reviewers for the efforts. The comments and suggestions have been very helpful to us. Please find below the reviewer comments in black, followed by author's response in blue.

1) The manuscript by Sakai et al. mainly describes the designing and the performances of an automatic Raman lidar system designed for the measurement of water vapor mixing ratio profile during daytime and nighttime conditions. According to the authors, the final goal of the work is to show the positive impact that water vapour Raman lidar measurements may potentially have if assimilated in a heavy-rain forecasting system.
The manuscript is sufficiently well written and outlines in detail the experimental setup of the Raman lidar. The stability of the Raman lidar calibration is assessed over the test period of the instruments, while the correction for the system incomplete overlap is calculated using radiosounding data from a nearby station. Intercomparison statistics versus radiosoundings and GNSS measurements for both the profile and the integrated water vapor content are used to validate the Raman lidar measurements.
Regardless of my specific concerns about the conclusions presented by the authors to support the validation of the Raman lidar measurements, more in general, I think that this manuscript does not demonstrate what the title would like to claim, i.e. the positive impact of the Raman lidar measurements on an heavy rain forecasting model.

Thank you for the critical comments on our manuscript. We agree with your claim that the manuscript does not show the final goal that is to show the positive impact of the lidar measurements on a heavy rain forecasting model. However, we would like to say that this is the first step of our study aiming to the goal, that is to say, to describe the experimental setup of the low-cost mobile Raman lidar and the validation of measurement by comparisons with other humidity sensors and model. To avoid potential misleading, we would like to change the title to "Mobile water vapor Raman lidar for heavy rain forecasting: instrument description and comparison with radiosonde, GNSS, and high-resolution objective analysis".

The study on the impact of using lidar data on the heavy rain forecast with a nonhydrostatic mesoscale model has been published in English (Yoshida et al. 2018) that showed a positive impact on the analyzed humidity field. We believe that the current manuscript meets the main subject area of AMT that comprise the development, intercomparison, and validation of the measurement instruments.

2) Focusing on the section where the comparison with high resolution local analysis data is reported, the authors' expectation is to demonstrate, from the Observation minus-Background (O-B) comparison on a limited time period (less than 5 months), that the Raman lidar can improve the rain forecasting system because it is able to reveal an evident bias in the analysis model output.
－This is indeed a demonstration of the well known value of Raman lidar measurement to assess the performance of the model analysis output.

We agree with you that the Raman lidar can reveal the bias of the model analysis output. We think that this finding (i.e. positive bias in the local analysis model output from the lidar data) is one important outcome of the study.

3) To demonstrate the impact of lidar observations on any forecasting system a data assimilation experiment or alternatively an Observing System Simulation Experiments (OSSE) must be carried out. Various examples are available in literature of lidar data assimilation experiments (e.g. Wulfmeyer et al., 2006, https://journals.ametsoc.org/doi/10.1175/MWR3070.1). The authors state that they are currently studying the impact of using lidar data with a nonhydrostatic mesoscale model for simulating heavy rainfall in the Kanto area in Summer 2016, citing a paper in Japanese: to my opinion the outcome of these experiment must be embedded in the manuscript by Sakai et al. because it could be the only possibility to add more substance to the manuscript and create a real scientific interest in the readers.

As stated before, we would like to publish separately the result of data assimilation experiment from this manuscript. However, if this manuscript does not meet quality standard of AMT, we should be able to revise the manuscript by embedding the result of data assimilation experiment by Yoshida et al. (2018). In that case, we would like to change the first author to Dr. Yoshida because the main topic of the manuscript would change.

4) In addition, the lidar described in the manuscript does not add new knowledge about innovative, more advanced technological solutions than the other home-made and commercial Raman lidars operating around the world. Besides, also about the intercomparison of Raman lidar measurements with radiosoundings, GNSS, MWR and FTIR, many other papers are available in literature using more robust approaches (Bhawar et al., 2011, https://rmets.onlinelibrary.wiley.com/doi/pdf/10.1002/qj.697; Beherendt et al., 2007, https://journals.ametsoc.org/doi/full/10.1175/JTECH1924.1).

Thank you for introducing the papers on the validation of Raman lidars. By reading those paper, we found that there are more robust approaches of the intercomparison than ours. However, we think that our approach is still useful for the validation because the distances of the measurement instruments were much smaller (less than 100 m) than them.

In response to your claim that the manuscript does not add new knowledge about innovative or more advanced technological solutions than the existing Raman lidar, we would like to point out that one advanced technological solution of the mobile Raman lidar (MRL) is that it can be easily deployed to remote site and start the measurement in a few hours after the deployment. That is very beneficial for investigating what measurement locations are effective for the heavy rain forecasting. To our knowledge, such a small mobile Raman lidar has only been reported by Chazette et al. (2015) and few intercomparison paper has been available.

5) The authors themselves, when trying to assess of the Raman lidar system performance which should be able to provide continuous profile of the water vapor mixing ratio, they do clearly show that during daytime the lidar has very limited performance, providing measurements with an uncertainty lower than 30% up to about 1.0-1.5 km above the ground level, which is also the region where the overlap correction is applied. These performances are even lower than a few of commercial Raman lidars and for sure does not allow to achieve the desired impact on a data assimilation system.
However, as I said before the impact must be concretely demonstrated and the considerations provided in the manuscript are not sufficient to this purpose.
I must also note that the authors honestly acknowledge that the maximum measurement altitude achievable with the Raman lidar system is limited during the daytime and that, though in theory this does not prevent the data assimilation (though I am concerned about the

total uncertainty budget in this region), there are the limited information provided by the lidar in the boundary layer and obviously above.

Even though the maximum measurement altitude is limited to 1.0-1.5 km in daytime, it is still useful for the heavy rain forecast because the height of the inflow of moist air that can cause heavy rainfall downwind is mostly around 0.5 km in Japan (Kato, 2018). Moreover, Yoshida et al. (2018) has shown a positive impact of the MRL data on the analyzed humidity field as mentioned before. We also note that we intendedly limited the performance of the lidar (but still meets our requirement) to reduce the total cost (< 200 K US dollars) because it makes easier to distribute the MRL around the forecasting areas to increase the chance of catching the inflow.

As for the overlap correction, we estimate the total uncertainty is at most 20% where the overlap correction is applied. We have added this estimation in the revised manuscript.

6) This pushes the authors to state that the development of a diode laser-based differential absorption lidar (ongoing) will allow to improve the range and the quality of the measurement for their rain forecasting system. This statement sounds like a "certification" of the insufficient performance of the Raman lidar for the proposed objective. Therefore, I'd propose the manuscript rejection, but I hope to see the authors submitting soon a new manuscript showing concrete results related to the impact of DIAL measurements or, at least, of the current night time Raman lidar measurements on a rain forecasting system.

Thank you for encouraging us to further study to improve heavy rain forecasting system. Improving the forecast accuracy and lead time of heavy rain is an urgent issue in Japan. In fact, heavy rain caused floods and landslides that killed over a hundred of people in the southwest Japan in June 2016, July 2017, and July 2018. So we decided to develop the MRL at first before completion of the development of the diode-laser-based DIAL even though the measurement performance was limited. We think that this manuscript is an important step of our study.

We wish to thank the reviewer again for his or her valuable comments.

Sincerely Yours,
Tetsu Sakai

References:

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