We appreciate your detailed and insightful comments, which have helped improve the clarity and precision of our work. Each point has been thoughtfully addressed, and we believe these revisions have enhanced the manuscript. Thank you for your contribution to this work.

1) There is no general description of the RALMO lidar. The paper should al least have a simple but adequate schematic of which channels are used and how these channels come about. An accompanying table listing the instrument specifications should also be added.

We include references to detailed descriptions of RALMO, further, our method is not specific to the lidar. Hence, we do not agree that a schematic is needed here.

2) The system constants in Eq. 1 require a bit more introduction. The lidar equation for PRR temperature should be introduced first.

We agree. We added the lidar equation and explained the relevant variables. Please refer to equation 3 and lines137-141 in the revised manuscript with track changes.

3) 'GRUAN', not 'GRAUN'

Fixed

4) In contrast to the ack of an overall system overview, the Licel (digitisation system) is mentioned a couple of times, without explaining what it is. Is it the Analog/photon counting combination that is referred to or merely the digitised signals?

We added a paragraph explaining the Licel data acquisition system. Please refer to line 63 in the revised manuscript with track changes.

5) A number of references to in-depth descriptions of the Raman lidar technique for water vapour and temperature lidar, including error analysis are missing. E.g.

- David N. Whiteman, "Examination of the traditional Raman lidar technique. I. Evaluating the temperature-dependent lidar equations," Appl. Opt. 42, 2571-2592 (2003).
- Leblanc, T., Sica, R. J., van Gijsel, J. A. E., Haefele, A., Payen, G., and Liberti, G.: Proposed standardized definitions for vertical resolution and uncertainty in the NDACC lidar ozone and temperature algorithms – Part 3: Temperature uncertainty budget, Atmos. Meas. Tech., 9, 4079–4101, https://doi.org/10.5194/amt-9-4079-2016, 2016.

We agree and have included Whiteman's reference under the RALMO section (line 74). However, we consider the Leblanc paper to be unrelated to our study, as it focuses on Rayleigh temperature measurements, which do not align with the scope of our work.

6) I recommend restructiring the manuscript to first clearly describe the methods used to retrieve the temperature profiles.

- In the results section an optimal estimation method appears out of the blue. The term OEM is not explained.

  We agree. We added a subsection explaining the basics of OEM along with the relevant equations. Please refer to subsection 2.3 in the revised manuscript with track changes.

- Also a description of the GRUAN sonde products is needed. Was the uncertainty information in the GRUAN profiles used?

  We agree and have now added a new subsection briefly explaining about the radiosondes used. We also referenced the papers for the RS92 and RS41 GDP (GRUAN data product) for more detailed information. We do not use the uncertainty of the GDPs. Please refer to subsection 2.2 in the revised manuscript with track changes.

7) Please add some clarifying labels to Figure 1 to guide the reader. E.g. where is t0? What happens sometime in 2012 (make reference to the text) and also just before 2013?

As per Whiteman's suggestion, we have adopted the use of an ensemble of radiosondes rather than relying on a single radiosonde calibration at t0. Please refer to major comment #2 from Whiteman for further details. Hence, this is obsolete after switching to the ensemble approach. We have also explained the drop observed in 2012. Please refer to line 223 in the revised manuscript with track changes.

8) I believe that the continuous background method can be used to monitor sudden changes in instrument behaviour by following the value of the calibration constant. However, an external source is always needed for an absolute calibration. Therefore, the accuracy is always limited by the accuray and uncertainty of the external reference and, in this case the uncertainties of the lidar. This needs to be further elaborated.

This is correct. Now with ensemble the accuracy is improved and can be characterized with the RMS. While the absolute value depends entirely on the external reference, the trend is to the greatest extent independent of the external reference. Furthermore, we are very transparent that the background calibration still requires an absolute reference and that it's the trend that becomes essentially independent from the external reference. Please refer to lines 36 and 53 in the revised manuscript with track changes.