Overall, this manuscript presents a valuable and comprehensive dataset on aerosol hygroscopicity from the HYGRO19 campaign, combining Raman lidar, COBALD-RS41 balloon measurements, tandem nephelometry, and complementary aerosol observations. The large number of hygroscopic aerosol layers retrieved from RALMO is particularly valuable and demonstrates the potential of remote-sensing observations to provide vertically resolved information on aerosol hygroscopicity under ambient atmospheric conditions. The comparison with independent in situ techniques is also an important strength of the study. The observed variability in hygroscopicity between different periods and its association with aerosol chemical composition provide useful insights into the factors controlling aerosol water uptake. However, I believe that the interpretation of the intercomparison and the term "robust characterisation" requires some further clarification. The different techniques probe distinct optical properties (backscatter versus total scattering), wavelengths, particle populations, and sampling conditions; therefore, the retrieved hygroscopicity parameters are not directly equivalent. The authors appropriately acknowledge that the comparisons are primarily qualitative, but the implications of these methodological differences and the associated uncertainties should be discussed more explicitly before drawing general conclusions regarding the capability of remote sensing to quantitatively characterise aerosol hygroscopicity. Although the paper is too lengthy for a research article, some material could be included in a supplementary session. Here are my comments.

Major comment:

1. The Comparability of fβ and fσ. A core strength of this paper is its multi-instrument study, but the physical quantities compared are fundamentally different optical properties. The RALMO Raman lidar and COBALD backscatter sondes measure the aerosol backscatter coefficient βaer at a 180 ° scattering angle, whereas the tandem nephelometers measure the hemispheric scattering coefficient σsp**.** The authors acknowledge that these absolute values are not directly comparable due to differences in polar geometry and wavelength. They note that a "more quantitative comparison would require dedicated Mie simulations". However, despite this major physical caveat, the manuscript proceeds to make qualitative "good agreement" assertions throughout the text and presents them side by side in Tables 6 and 7. To elevate the scientific quality of this paper, given the importance of this issue to the intercomparison, I encourage the authors to conduct at least a preliminary Mie scattering analysis. Since they already have size-resolved particle distributions from the collocated SMPS system, they should perform a preliminary Mie calculation for the case studies. Specifically, they can estimate the theoretical ratio of the backscatter Hänel parameter γβ(355 nm) to the scattering γσ(550 nm), providing a physical baseline for the systematic discrepancies observed in Tables 6 and 7 (e.g., why γβ is frequently higher than γσ on the same day).

2. The chemical composition data (NABEL station filters) are collected strictly at the surface, whereas hygroscopicity is evaluated at different heights, including the Free Troposphere (FT). The assumption of using surface chemistry as a proxy for decoupled FT layers. May request a more thorough discussion of the potential errors introduced by this extrapolation, especially in cases where boundary-layer air is not well mixed into the free troposphere. They can use their own RALMO profiles of potential temperature and water vapour mixing ratio (r).
3. In section 4.3.3, the authors state: *"Overall, no consistent spectral dependence of the retrieved hygroscopic parameters was identified across the different instruments"*. However, in Section 5 (Conclusions), they state: *"A weak wavelength-dependence was also observed, with larger hygroscopicity parameters at shorter wavelengths"*. The authors need to clarify this apparent contradiction. They should explicitly define where and under what conditions the "weak wavelength-dependence" was statistically supported.

Minor comments

Line 7: The manuscript refers to “three independent approaches.” Since the Raman lidar, COBALD-RS41, and tandem nephelometer measurements are based on different optical quantities and are not fully independent, the authors may consider using “three complementary approaches” instead.

Lines 464-465: “The tandem nephelometers' measurements are only representative of near-surface conditions.” Please revise to “the tandem nephelometer measurements are representative only of near-surface conditions.”

Line 639: The terminology “RALMO measurements alone enable the retrieval of aerosol hygroscopicity parameters” may be slightly too broad. Since the retrieval requires specific atmospheric conditions, particularly a well-mixed layer and suitable RH structure, I suggest changing this to “RALMO measurements can enable the retrieval…” or explicitly retaining the conditions under which the retrieval is applicable.

Lines 424-425: the statement “during summer more wind profiles could be monitored due to lower cloud cover and bird migration”. Can you explain bird migration? Bird migration is not obviously connected to the availability of wind lidar measurements, or is that a typo?

General comment

The authors could consider moving some of the detailed case studies from Section 4.3 to the Supplementary Material. This could help shorten the manuscript and allow the main text to focus more clearly on the methodology and interpretation of aerosol hygroscopicity retrieval.