

Interactive comment on “Combined wind measurements by two different lidar instruments in the Arctic middle atmosphere” by J. Hildebrand et al.

Anonymous Referee #2

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Summary:

The paper describes the setup, analysis and combined wind measurements of two wind lidars at ALOMAR covering the range from 30 to 110 km. It is novel, because two wind lidars of different type – a Rayleigh wind lidar and a sodium lidar – were used with the same telescopes and thus are sampling almost the same atmospheric volume in a common altitude range of 80–85 km. One focus of the paper is an extended description of the Rayleigh wind lidar data analysis and results, following the paper by Baumgarten (2010). The paper presents a novel approach for stratospheric-mesospheric composite

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wind lidar observations and discusses results of applying this approach to derive wave-like structures in the wind field. The paper is clearly worth publishing after some minor modifications, which are mainly for clarification and traceability of the results, and a re-structuring of the text to improve the clarity of the presented methods.

Major comments:

1) My major comment is related to the structure of the paper. The title of the paper is related to the combined measurements and analysis of two wind lidars for the same volume. Thus I would expect to see only results of this analysis in chapter 4 (results). But a large part of chapter 4 is still related to the analysis of the raw data of the RMR lidar; actually a large part of the manuscript is related to this RMR analysis and it should be mentioned in the abstract. Also the data analysis for the RMR lidar is found on different places in the manuscript, e.g. ch. 2.1., ch. 3, ch. 4.1, 4.2, and 4.3, always mixed with some remarks to the sodium lidar analysis. I would prefer to have the analysis of the RMR and sodium lidar from the raw data to the retrieved winds (which are used for the comparison) in two separate chapters for each lidar, and then have a results section, where only the combined measurements are discussed.

2) My second major comment is somehow related to the first one. It is described in ch. 2.1. how the corrected Doppler ratio D' is obtained, but not how the Doppler frequency shift (and related LOS wind speed) is obtained from this Doppler ratio. One might find this again in ch. 4.1. Fig. 5, where the modelled Doppler ratio is discussed as a function of wind speed and temperature. So I would strongly recommend to re-structure the paper to make it more clear, how the measured LOS wind speed is obtained from the RMR raw data, and not to spread this information on several chapters of the manuscript.

Minor comments:

Abstract: It should be added, that both lidars were using the same telescope; this was not so clear from the term “common volume wind measurements” – this could

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have been also used, if the telescope were separated by only a few meters. Also the common altitude range of 80–85 km should be given in the abstract.

P4124, L21: Altitude range of radar instruments should be given here for clarification.

P4125, L10: references from mid-altitude wind measurements of Chinese groups should be added, e.g. Shen et al. (2009), Chin. Opt. Lett., or Xia et al. (2012), Opt. Exp.

P4125; L20 and L25: The date of the joint radar/lidar campaign and from the presented measurements should be given.

P4126, L2 and P4129, L3: It should be explained, why the FOV of both lidars is tilted by $600 \mu\text{rad}$. Is it to avoid interference?

P4126, L11: I would avoid using the term “point” measurements for lidars, because they are actually more like a cone (small diameter, large range); point measurements are related to in-situ sensors; a suggestion is to use “largely different sizes of the sounding volumes”.

P4126, L20–25: The relationship for MHz to m/s conversion for this wavelength should be given here; the wavelength of the seed laser and the stabilisation to the absorption line should be given for clarification.

P4127, L1: It is mentioned, that the pulsed laser offset is monitored on single pulse basis. It should be mentioned, how this is achieved: heterodyne measurements or wavelength meter?

P4127, L4 and L9: The receiver FOV is significantly larger than the emitter divergence? What is the reason? Beam broadening? Is the emitter divergence given as $\pm 2\sigma$ or $\pm 3\sigma$ value?

P4128, L9: It should be shortly explained, why there is such a large difference in the E-parameter for both telescopes.

P4129, L20: What is meant by “error” of the backscatter signal. Is it the variation from the time series? Should be more clearly described.

P4129, L23: Type of “thin clouds” should be specified – noctilucent, tropospheric?

P4130, L5: It should be shortly explained, how the wind speed error is obtained; also how the measurement uncertainties in the Fig. are obtained; In addition it should be added in Fig. 3 caption, why the Doppler ratio (right) is shown only up to 80 km; while the signal (left) is shown up to 100 km.

P4130; last abstract: please check numbers given in the text and table 1 for the number of different pointing options, e.g. Table 6 has 6 vertical pointing periods, while text says only 5; opposite direction were used 2 times (and not only 1 time); RMR vs. Na is stated 4-times in Table and 3-times in text

P4131, L19: What is meant by “hypothetical”? Is this the derived “zero-wind” speed, by using an actual temperature profile and response from Fig. 5? I would avoid the term “hypothetical” and instead describe what is plotted. If the black line in Fig. 4 is the derived profile by calculation, than it would be useful to show the actual “mirror” axis, obtained from the “blue” and “red” profile.

P4132, L13: ECMWF is introduced without explaining the abbreviation and model setup (model analysis used? Horizontal/vertical resolution, model version?)

P4133, L26 and ch. 4.4.: Is it common for the stratosphere/mesosphere to apply a fourth-order polynomial for deriving the undisturbed wind profile? References should be given.

P4134, ch. 4.3: Is this variation of the E-factor only affecting the SE-Telescope? This should be explicitly mentioned. Fig. 7: It is hard to see the correspondence of the RMR and sodium wind profile between 80-85 km, because same colours are used. One option would be to provide this altitude in a second figure, or provide two figures for each NWT and SET, or use different colours.

P4139, L15: For the first time it is mentioned, that the data is analysed with a time resolution of 1 h only (compared to previous studies). This should be mentioned also in the main part of the text, e.g. ch. 3 and the averaging time of previous studies should be given.

P4139, Conclusion: It should be clearer that the described methods (validation, calibration) were used for the RMR lidar.

Editorial:

Abstract and other places: “two independent branches”; the term “branches” should be replaced by a more appropriate one: telescopes or optical paths.

P4126, L9: “over the sky” can be deleted.

P4128, L6: I was confused by the equation for Etelescope, because I read it first as (Dseeder) divided by Etelescope. Thus I would propose to move the equation for Etelescope directly in line 5.

P4129, L13: Add “is tilted by $600 \mu\text{rad}$ ” for clarification.

P4130, L16: “33 s” instead of “33 ms”

P4130, L15: “runs” should be avoided throughout the manuscript, e.g. replace by “periods”

P4131, L11: As several equations for the Doppler ratio were introduced, the symbol “D” should be added to avoid confusion.

P4136, L25, and other places, e.g. Fig. 6: “snapshot” for ECMWF profile should be avoided; only refer to ECMWF profile at a specific time.

P4141, L16: Please check, if the reference is a PhD thesis. I found only a Master thesis with this topic.

Fig. 1, caption: It should be mentioned that the coloured numbers indicate beam

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diameters.

Fig. 2, caption: add “additional entrance (solid line) after the RFS”.

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