

Answers to Reviewer #1:

Major comments:

1.) My main point is that I am missing a clear description of the wind retrieval algorithm in a separate section or at least a specific paragraph within section 3. The information for the wind retrieval is spread among several places within section 3 (minor comment no. 11 and 14) and section 4 (background subtraction and a 2-km running mean filter).

The description of the retrieval algorithm was clarified, especially in section 3. Section 4 holds still the information about background subtraction and smoothing as these are not specific for the DoRIS wind retrieval but standard lidar processing techniques.

Details about the changes to improve the description of the wind retrieval algorithm can be found in the answers to Minor comments 11 and 13:

“We found for the north-west telescope $D0 = DNWT = 0.9917 \cdot D_{seeder}$ and for the south-east telescope $D0 = DSET = 0.8776 \cdot D_{seeder}$.

For the actual wind retrieval the lookup table $D_{model}(T,w)$ is restricted to the measured temperature $T(z)$ at the altitude z and then the line of sight wind $(w(z))$ is retrieved by finding $D_{model}(T=T(z),w(z)) = D(z)/D0$. Finally the wind offset due to the wavelength offset $(\Delta\lambda_0)$ is subtracted.”

2.) My second major comment is related to the wind retrieval accuracy. The statistical uncertainty is stated in the abstract (line 5) and the conclusion (line L8), although the manuscript does not provide any details, how these numbers were obtained and derived. Chapter 4 contain numbers of 9 m/s and 19 m/s, and it is also not clear, how these numbers were derived – from the signal intensity fluctuations? So either a paragraph about the wind retrieval accuracy should be added, or the sentence about the statistical uncertainty should be deleted in the abstract and conclusion. *The description of the method for error calculation and propagation is now included in the manuscript. The corresponding parts of the manuscript have been updated. The modifications of the manuscript are described in detail by the answers to the Minor comments 16 and 18.*

In summary:

“To estimate the measurement uncertainty we calculate the shot noise of the number of photons received and use Gaussian error propagation throughout the retrieval process.”

The lookup table $D_{model}(T,w)$ was numerically differentiated and the local slopes $dD_{model}(T,w)/dT$ and $dD_{model}(T,w)/dw$ are used for the error propagation.

Minor comments:

3.)

Title: It is not clear what “twin” refers to – wind and temperature? It is also not described in the text. Thus I suggest removing “twin” from the title.

Removed “twin” from title

4.)

P2780, L25: The given references are not suitable for winds above 15 km , e.g. Liu et al. 1997, does not report about wind measurements above 15 km; others are missing e.g. Garnier and Chanin 1992, or Gentry et al. 2000. As the paper reports about direct-detection Doppler lidar using iodine filter technique,

the related work for tropospheric winds with this technique should be mentioned within 1-2 sentences and proper references should be given, e.g. Liu's papers 1997, 2002, and 2007.

Added citations and appropriately used the citations of Liu:

"... A similar technique was successfully applied to a tropospheric lidar system (Liu et al., 1997, 2002, 2007). "

5.)

P2781, L15: The sentence "Up to now .. " is misleading, because Huang et al. reported about temperature and wind lidar measurements up to an altitude of 50 km already (but not above). So it should be reformulated to make this clear.

The sentence was changed accordingly:

"Up to now no simultaneous temperature and wind measurements by Rayleigh lidar above 50 km have been published (Huang et al., 2009). Here we present the first simultaneous temperature and wind measurements by Rayleigh lidar up to 80 km."

6.) Figure 2: It is not motivated in the text, why the absorption line S57 is shown in Fig. 2. I understood from the text that L38 is used. Also the text (P2783, L14) is not clear. What is the temperature of the cell with some remaining I2 in the bulk phase? Does it refer to the S57?

We include the proper reference now in the text:

"... for cells with some I2 remaining in the bulk phase (S57 in Fig. 2), due to varying I2 vapor pressure."

7.) Fig. 2 and text: It should be stated in the text, where the relative position of the transmitted laser pulse frequency wrt the I2 absorption line is placed for wind measurements (steepest slope?)

The figure was updated and the seed wavelength is marked. The caption and the text were updated accordingly.

"The seed wavelength was chosen to optimize the wind retrieval when using cell L38, as described in section 3."

8.) Laser pulse length and FWHM at 532 nm should be added in the text.

The information is now included in the text:

"The pulse length is about 8 ns, leading to a natural line broadening of about 20 MHz. For a similar laser type the pulse width was measured to be less than 70 MHz \citep{Fiedler-ITGRS-1999}."

9.) P2783, L17: It should be added in the text, that the 4 pm filter is realised by a double etalon at 532 nm. Some descriptions of the etalon should be given, e.g. plate distance, FSR, Finesse. What is meant by "double"? Is it 2 etalons in serial implementation? Also it is not mentioned, how the 130 pm bandwidth is realised during nighttime.

The missing information is now included in the manuscript, with a reference to a detailed description of the double etalon system.

"The bandwidth of the 532 nm detection branch is 130 pm during nighttime using a single interference filter and about 4 pm during day using a double etalon system to reduce the solar background. Details about the double etalon

system can be found in von Zahn et al. (2000).“

10.)

Fig 3 contains a “high finesse wavelength meter”. This is not described in the text. Some details (commercial?), accuracy, and purpose in the setup should be given in the text.

We

“The I2 absorption was measured directly by scanning the cw-seed laser. During the scan we measure the wavelength with a commercially available wavelength meter (HighFinesse WSU) with a precision of better than 10 MHz over one hour.”

11.)

P2784, L11: How is the temperature influence handled in the retrieval algorithms? Is it a correction on the retrieved winds or is the Dmodel calculated for the actual temperature profile and then the wind derived from this Dmodel(Tactual)? The retrieval method should be described in 2-3 sentences.

The manuscript was updated accordingly:

“For the actual wind retrieval the lookup table Dmodel (T,w) is restricted to the measured temperature T (z) at the altitude z and then the line of sight wind w(z) is retrieved by finding Dmodel (T=T(z),w(z)) =D(z)/D0 . Finally the wind offset due to the wavelength offset ($\Delta\lambda_0$) is subtracted.”

12.)

P2784, L27: Is the normalisation performed at 1 specific altitude? Is this indicated by the grey arrows in Fig. 6 left?

The manuscript was updated and now includes the missing information:

“.. combined by normalizing the lower altitude channels to VH (or VDH) in an overlapping height range of about 7 km. The lower boundary of this overlapping height range is indicated with grey arrows in (Fig. 6).”

13.)

P2785, L13ff: Do the parameters wavelength offset and D0 used in the wind retrieval. How is the parameter DNWT and DSET used in the wind retrieval?

It is not clear, because this parameter should affect both the channels before and after the I2 cell?

The manuscript was modified to make more clear how the parameters DNWT and DSET are used.

“We found for the north-west telescope $D0 = DNWT = 0.9917 \cdot Dseeder$ and for the south-east telescope $D0 = DSET = 0.8776 \cdot Dseeder$. “

Is this parameter range-dependent (due to its sensitivity to alignment), and are there different parameters for the L/M/H channels?

The parameter is not range dependent, as the parameter DNWT and DSET is introduced through the measurement of the channel efficiency D0. We have not observed a range dependent D0, and we do not expect to see range dependent effects on the optical bench as the range information is scrambled when the light is transferred from the telescopes through the multi-mode fiber.

DNWT and DSET and D0 are only needed for the channels VH and VDH. As the other channels are normalized to the VH and VDH channel these channels do not need separate DNWT and DSET.

14.)

Fig. 7 and text: What is causing the high backscatter ratio below 34 km on January 17: It is written in the text, that the station was outside the polar vortex on January 17 – so the signal is not coming from PSC?

Yes, the elevated backscatter below 34 was observed when the station was outside the vortex (Fig. 10). This elevated background aerosol level could be caused by the two big eruptions of Aleutian volcanoes. Okmok blew up on 12 July and Kasatochi erupted on 7 Aug, 2008.

Also the temperatures (ECMWF) around 30 km are about 20K warmer than required for STS, even more for the other PSC types.

15.)

Fig. 8: Scale of vertical wind should be rather +/-20 m/s rather than +/-100 m/s; the different colors and styles (solid, dotted) of the lines used in Fig. 8 should be described in the caption.

The scale was chosen to be comparable with the other panels in figures 8 and 9. We prefer to keep the scale so the reader can easily see the effect of the horizontal wind.

16.)

P2787, L25: How were the numbers for the statistical uncertainty of 9 m/s and 19 m/s derived?

The section about the data analysis was extended accordingly:

“To estimate the measurement uncertainty we calculate the shot noise of the number of photons received and use Gaussian error propagation throughout the retrieval process.”

17.)

Fig. 10: To my opinion this Fig. 10 with the ECMWF model fields is “nice-to-have”, but not relevant for the main portion of the paper, which describes a new instrument and first analysis. Also the content of Fig is not discussed in detail in the manuscript and the main information from the ECMWF analysis is already included as profiles within Fig. 8 and 9. Thus the author could think about removing Fig. 10.

We think that Fig. 10 is helpful for understanding the dynamical situation on a hemispheric scale (for example comment 14). So we prefer to keep the figure.

18.)

P2789, L9: an uncertainty of 20 m/s is reported at 80 km, while a number of 10 m/s is provided in the abstract.

The numbers given in the abstract correspond to line of sight wind.

While the values in this section were for line of sight wind and horizontal wind.

We have clarified this in the text now.

“The minimum measurement uncertainty of the line of sight wind for 2 km range resolution and 2 h time resolution is 0.6 m/s at about 49 km

altitude. At an altitude of 80 km the uncertainty of the line of sight wind increases to about 10 m/s, resulting in an uncertainty of the horizontal wind of 20 m/s. “

Editorial comments:

1.) P2780, L2: delete “shift” or write “Doppler frequency shift”; also “system” could be replaced by “lidar”

Modified manuscript accordingly:

“A direct detection Doppler lidar ...“

2.) P2780, L3: more common to use “random error” or “precision” (or “accuracy”) rather than statistical uncertainty

Modified manuscript accordingly:

“The random error of the line of sight wind ...“

3.) P2780, L9: “multi-wavelength” instead of “multicolor”; also wavelengths should be given explicitly.

Modified manuscript accordingly:

“.. extend the existing multi-wavelength observations of aerosol and temperature performed at wavelengths of 355 nm, 532 nm and 1064 nm.”

4.) Fig. 1: R/M/R acronym not explained in caption or text.

The figure was updated so we do not use the acronym.

5.) P2781, L8: The sentence “Radar observations ...” contains 2 times the MST radar. For the second time no height interval is given. Is it the altitude range of 80-90 km? Sentence should be rewritten.

The sentence was rewritten:

“Radar observations covering the altitude ranges of 1-15 km and about 60-110 km using the MST radar, MF radar or SKiYMET meteor radar are continuously available..”

6.) P2782, L3: “is about 4 MHz for a wavelength of 532 nm ..”; wavelength should be stated here, because it was not introduced before.

Modified manuscript accordingly.

7.) P2780, L5: Is “only” referring to “only a signal ratio of” or “only a precision of”. This is unclear. The word “only” can be deleted.

Modified manuscript accordingly.

8.) P2780, L7: delete “only”. I suggest inserting “In order to measure the horizontal wind vector, we use two ..”

Modified manuscript accordingly.

9.) Fig 3: BWT acronym not explained.

The acronym is now defined in the caption of the figure.

10.) Fig. 5 a) it should be mentioned in the caption that the 0.4/0.6 black lines are isolines

Modified manuscript accordingly.

11.) Fig. 6: grey arrows in left figure should be explained; right: only a red and a black line is shown, but no red and blue horizontal bars.

Corrected and extended the caption accordingly.

12.) P2785, L9: Is there a reference, where the retrieval and the accuracy of the backscatter ratio for this system is described? If yes, this reference should be added.

Added a reference to the most recent application of the retrieval method, however the signal level of the signal level of the lidar was increased since then.

13.) P2788, L6 and L26: “.” dots should be replaced

Modified manuscript accordingly.

14.) References, Friedmann: “Doppler lidar”

Modified manuscript accordingly.