Review of Sun et al. "The performance of Aeolus in heterogeneous conditions using high-resolution radiosonde data"

Summary:

The manuscript describes a method to assess the performance of the wind lidar mission ADM-Aeolus for vertically heterogeneous atmospheric profiles. The errors introduced by vertical gradients in wind speed combined with vertical gradients in backscatter coefficients lead to errors in the derived wind speed due to the sampling of the atmosphere in range bins of 250-2000 m. The main body of the manuscript describes the construction of profiles of aerosol and cloud backscatter coefficient derived from radiosonde measurements. These backscatter profiles from the radiosonde together with the wind profile are used to assess the wind error due to atmospheric variability within one range-gate.

The manuscript addresses an important topic for ADM-Aeolus and future wind lidar missions and is thus of relevant for publication. Nevertheless I recommend substantial revision and major modifications, before it could be considered for publication, due to major weaknesses in structure (major comment 1), content (major comment 3,5), and methodology (comment 2, 4) of the manuscript.

Major Comments

1) Although the manuscript claims to assess the Aeolus measurement performance in heterogeneous conditions, a major portion of the manuscript (ch. 3 and 4) is dedicated to a rather lengthy description of the construction of backscatter profiles from relative humidity profiles from radiosonde data, which was published before by the author in Zhang et al. (2010). The manuscript should focus much more on the implications for Aeolus as stated in the title of the paper. Thus I would recommend to drastically shortening ch. 3 and 4, e.g.

- Ch. 3.1 describes different methods of cloud layer detection in radiosonde relative humidity data, which is mainly presented in Zhang et al. 2010, and does not need to be repeated here. The assumptions for applying it to high-resolution data could be discussed in 1- 2 paragraphs. I would recommend removing Fig.2, because a comparison of relative humidity RH from radiosonde with RH from ECMWF does not provide any insights for the topic of the paper.

- Ch. 3.2 reviews the well-known relationship between relative humidity and lidar backscatter coefficient with numerous references. For the purpose of this paper (effect on Aeolus performance) this should be shortened to the applied method and basic assumptions for the used parameters and limited to the relevant references.

- Ch. 4.1.1, 4.1.2, 4.1.3: I do not understand the purpose to compare both the WR95 and Zhang2010 method for a limited dataset. To my opinion it would be sufficient to show results from Zhang2010 method for the purpose of this paper. Obviously no significant difference between the two methods exists and the method is only applied to a very limited dataset (1 site, 1 profile per day).

- Ch. 4.2.1: It is not valid to apply the growth factor to backscatter coefficient and lidar ratio in case of clouds, as presented in Fig. 9. This scaling is only applicable for the aerosol layers. Thus only Figure 9 (lower right) does make sense, but shows also the weakness of the method by using climatological values for clouds with sharp gradients at the borders.

2) The authors claim that high variability is introduced in the atmospheric database by high-resolution radiosonde data, but in fact they are using mainly climatological input for cloud and aerosol backscatter, which is slightly modified by radiosonde RH profiles. I see two major weaknesses in their methodology:

- The high-resolution radiosonde data is only used for cloud-layer detection, and then climatological values for cloud-backscatter and extinction are used from Vaughan (2002). This introduces artificially sharp gradients of backscatter and extinction at the cloud boundaries, and no variability of backscatter/extinction within the cloud. The variability of backscatter/extinction within clouds is underestimated with this method. This drawback and its consequences are not discussed in the manuscript. What is the advantage of using this dataset wrt vertical cloud variability instead of the database constructed by CALIPSO observations as presented by Marseille et al. (2011)?

- Similarly the method of constructing aerosol backscatter profiles is based on a climatological profile, which is modified by the relative humidity profile of the radiosonde (eq. 25). I doubt that eq. 25 can be used for that purpose, because the hygroscopic growth factor is based on a concept of modifying a backscatter profile with a reference RH (e.g. eq 22), but eq. 25 uses a climatological profile, which was obtained not for a reference RH but includes a whole range of RH values. Also the resulting backscatter profiles in Fig. 10 show much less variability (low to high quartile) than the ones measured by the UV lidar. So I do not understand, how the authors could conclude that the aerosol backscatter profiles are representative (p. 21).

Because of these 2 limitations, I do not consider the proposed method as being better appropriate for simulating atmospheric variability rather than the method already discussed in Marseille et al. (2011), especially for the variability in the aerosol/cloud backscatter and extinction.

3) Although some validation of the atmospheric database is presented in chapter 4, statistics of the atmospheric database (as in Marseille 2011) for median, quartiles the profiles and their gradients are not presented in the manuscript. So it is difficult to assess the content of chapter 5 (influence on Aeolus performance), if the content of the database is not well described (e.g. histograms, percentile profiles). In addition the authors should investigate and discuss in more detail the thickness of the cloud layers in comparison to the range-gate dimensions, which was shown of relevance in chapter 2. Does the database contain clouds, which are thinner than the range gate dimensions? What is the thickness of the radiosonde detected clouds in comparison to the Cloudsat/CALIPSO detected clouds (e.g. as presented in Fig. 5 for cloud base/top). Is the cloud thickness distribution such, that a cloud is contained in several range gates, and only the uppermost range gate is influenced by a cloud, which is not fully covering the range gate?

4) Within Ch. 2 the analyses of the influence of the molecular backscatter and extinction profile in combination with a vertical gradient in wind speed is not studied as a separate case, which is a serious limitation of the manuscript. This will lead already without any clouds to a systematic (because of the non-random profile) error in the Rayleigh wind. Equations should be presented in ch. 2 and also a possible correction of this error could be discussed, e.g. by a modification of the height assignment of the Rayleigh winds. This error in Rayleigh wind or height assignment is also obvious in Fig. 14 for altitudes above 10 km were no clouds are present in the database (as discussed ch. 5 on p. 1427). I would strongly suggest including a theoretical analysis in ch. 2, deriving a correction for that, and discussing the residual error after this correction in ch. 4.

5) The author claim at several places of the manuscript, that the variability of the backscatter profiles leads to systematic biases, which are larger than the systematic error requirement for the Aeolus mission of 0.4 m/s. I do not agree with the authors that these errors can be considered as systematic errors, and thus can be compared to the systematic error requirement for the Aeolus mission. I consider an error as systematic, if it shows up over a significant portion of a dataset (e.g. time series over a portion of the orbit) as an under- or overestimation of the true wind speed. A prominent example would be the systematic error of AMV´s due to a systematically wrong height assignment. But this is different for a lidar, because the variability of the atmosphere within one range gate can be randomly distributed (as the authors assume correctly in ch. 2.1, 2.2.) ; the thickness of the backscatter layer, the location of the backscatter layer and the wind speed gradient could be randomly distributed within one range gate. Thus this could lead to a random over- or underestimation of the wind speed for several observations. Thus in my view the investigated error leads to an increase in the random error due to atmospheric variability. In addition I recommend mentioning the view, that the errors could be considered also as height assignment error rather than a wind error, as introduced by the weighting function in eq. 1

Minor Comments:

Ch. 1:, p. 1397: The statement about the ECMWF effective model resolution and the provided numbers of 15-20 is quite strong and controversy, compared to the factor of 7 given by Skamarock (2004). It should be discussed, how the effective model resolution is obtained/defined leading to a factor of 15-20 or the sentence should be re-formulated.

Ch. 2: It would be nice to provide a figure with a schematic sketch of both situations analysed in ch. 2.1 and 2.2 with all the notations, e.g. z_l, z_0, z_c, l, and assumed profiles for backscatter and wind profile.

Ch. 2: It should be noted that equation 2 and the subsequent analyses includes the simplifying assumption that the backscatter signal can be separated by the instrument perfectly in a particle component and a molecular component. The instrument senses the total backscatter, which consists of β_p+ β_m, but eq. (2) and the analysis in ch. 2 assumes that the Mie channel is only containing signal from β_p (with no signal from β_m) and the Rayleigh channel contains signal only from β_m (with no signal from β_p). This is a simplified assumption wrt the behaviour of the real Aeolus instrument (see e.g. discussion in Dabas et al. 2008, Tellus).

Ch. 2.1, p. 1400: The definition of $U_p^M=u_0+\alpha z_c$, requires that z_c is provided in coordinates such that $z_c=z_0$ at the bottom of the range gate with wind speed u_0. Z_c is not the absolute height (AGL).

Ch. 2.1., eq. (9): The subscript "m" is missing in the second term of ε.

Ch. 2.1., p. 1402, discussion of Eq. (10): It is stated that the Rayleigh channel error grows quadratically with the cloud layer transmission τ_c, although eq. (10) contains τ_c^2 in the enumerator and denominator. This statement is only valid for small values of τ_c^2.

Ch. 2.1, p.1402: A literature reference should be given for the used value for the vertical wind shear of 0.004 s^{-1}.

Ch. 3, p. 1406: It is only stated that the used radiosondes are of high-quality, whatever this means. Instead numbers for accuracy and precision should be given for the used parameters.

Ch.3, p. 1406: It is stated that only 12 UTC radiosondes were used. What was the reason for not using the 0 UTC radiosondes, which could have extended the dataset also to night-time observations?

Ch. 3: The use of the symbol RH (for Relative Humidity) in this chapter is at some places misleading (e.g. eq. 23), because actually the value of the scattering cross section σ(RH) or the backscatter coefficient β(RH) is meant.

Ch. 3.2: It is not mentioned, which climatological profile (median or other profile) was used from Vaughan et al. 1998.

Ch. 3.2: It is not mentioned in the text, which values are used for backscatter and extinction for a water cloud from Table 3, which contains several types. Is there another distinction using the altitude of the detected cloud?

Ch. 4.2: The type of the UV lidar should be specified in the text - commercial?

Ch. 4.2.2, Fig. 10, 11: As stated in the text, the UV lidar observations are influenced by the telescope overlap below 300 m. From Fig. 10 it seems that the backscatter profiles are even influenced above 500 m from the overlap. As no valid backscatter profiles can be obtained below 300/500 m, I would remove those altitudes from Fig. 10/11 (right) for the UV lidar.

Ch. 6, Conclusion and Ch. 4.1.3: I consider the statement in the conclusion about comparison of vertical profile of cloud coverage from radiosonde database compared to ECMWF rather strong, because the radiosonde database is limited to 1 site (de Bilt) and 1 time (12 UTC. Such a general statement about ECMWF model cloud coverage as presented in the conclusion is not supported by this study.

Ch. 6, p. 24:It is argued that the atmospheric database shows factor 4 higher backscatter than the UV lidar below 700 m. Due to the overlap issue and the presented profiles in Fig. 10 (right), I would not consider the UV lidar measurements reliable below 500 m (or even 700 m).