**Review of manuscript «Simultaneous and co-located wind measurements in the middle atmosphere by lidar and rocket-borne techniques» by Lübken, Baumgarten, Hildebrand, and Schmidlin.**

General comments: This excellent and well-written manuscript compares measurements results of horizontal winds in the height region 20 to 75 km. For the first time, lidar measurements enhanced with a technique called Doppler Rayleigh Iodine Spectrometer (DoRIS) are compared with rocket-borne "starute" measurements, as well as with datasondes and radiosondes. The techniques are described, including their advantages and disadvantages. The most important conclusion is that DoRIS is a reliable technique to measure winds in the middle atmosphere, with the caveat that care must be taken to exclude data from heights where aerosol scattering is present. On the day of the comparison that was below about 30 km.

Figures 1 to 4 make it easy for the reader to understand what is being compared. All figures are of high quality, including symbol sizes and the choice of line colors.

Specific comments:

(1) I recommend a paragraph discussing the **Eulerian** nature of the lidar measurements (wind measurements at a fixed geometric position in the atmosphere), the **Lagrangean** nature of the radiosondes (the weather balloon drifts with the atmospheric flow and measures the wind at that time and place), and the intermediate nature of the starutes and datasondes. Given these differences of reference frame, the very good agreement is almost surprising. In a wind field that is constant in space and time, Eulerian and Lagrangean measurements ought to give exactly equal results. If the wind field changes in space and/or in time, even ideally precise measurements in Eulerian and in Lagrangean reference frames come out differently. Is it possible to estimate how much of the (small) remaining differences shown in Figures 8 to 11 might be due to the different reference frames? If it is, we can learn even more about the atmospheric flow characteristics.

(2) As this manuscript and other publications show, measurements and analysis techniques have vastly improved in recent decades. I submit that it is time we also moved on from the **assumption of zero vertical wind.** Although there likely were no measurements of the vertical wind w during the observations for this manuscript, we do know in a general sense what average w to expect on time scales of many hours and spatial scales of 100 km and more: mm/s to cm/s (e.g., Körner and Sonnemann, 2001; Berger and von Zahn, 2002). We also know, in a general sense, what w fluctuations to expect locally and on time scales of tens of minutes: typically $\pm 3$ to $\pm 5$ m/s but not more than $\pm 10$ m/s (e.g., Fritts et al., 1990; Hansen and Hoppe, 1996; Collis, 1997). Most of the references I have found here pertain to the summer mesosphere and many to altitudes higher than those required for this manuscript. Nevertheless it must be possible to estimate the influence of such typical vertical winds on the observations in this manuscript. – At zenith angle α, an actual vertical wind w would appear as a line-of-sight wind $w_{apparent} = w \cdot \cos \alpha$ . When this line-of-sight wind is misinterpreted as a horizontal wind, it would appear as

$u_{apparent} = w_{apparent} \cdot \sin \alpha = w \cdot \cos \alpha \cdot \sin \alpha = \dfrac{w}{2} \cdot \sin 2\alpha$ . Assuming $w = 3 \dfrac{m}{s}$ and $\alpha = 30°$ ,

we obtain $u_{apparent} = 1.5 \frac{m}{s} \cdot \sin 60° \approx 1.3 \frac{m}{s}$. With $w = 3 \frac{m}{s}$ and $\alpha = 20°$, we obtain

$u_{apparent} = 1.5 \frac{m}{s} \cdot \sin 40° \approx 1 \frac{m}{s}$. Therefore, features in the meridional wind in Figures 7 to 11 up to 1.3 m/s may be attributed to a non-zero vertical wind, and up to 1 m/s in the zonal wind in those figures. This argumentation is for the vertical wind variance observed in other publications on time scales of tens of minutes. It is easy to see that the much smaller average vertical wind (mm/s to cm/s) gives effects smaller than the graphic linewidth in those figures. – A similar argumentation ought to be carried out concerning page 5 line 5, the zero-vertical-wind assumption in the starute analysis.

(3) It might seem worthwhile to plot Figure 7 as a hodograph in addition. Such a hodograph would presumably show whether the variations are due to one or several gravity waves, and also their vertical propagation direction. It is possible that the authors have tried this, and that it turned out not to be worth showing.

(4) On page 8, lines 6-12 in section 5, the authors discuss the backscatter ratio. I did not find the source of the signal from molecular scattering only. This is presumably from the $N_2$ Raman channel of the lidar. It should be mentioned.

Technical corrections:

(1) The authors may wish to consider excluding the datasonde launches of 4/5 March and 10 March from Table 1, as they are not used in this manuscript. There may be a good reason to keep them in the table, even if this reviewer does not know it.

(2) Typos:
   a. P. 2 l. 8: satellite-borne
   b. P. 2 l. 31: atmosphere
   c. P. 3 l. 24: lowercase "figures"
   d. P. 5 l. 29: so-called
   e. P. 6, l.9: descent
   f. P. 6., l. 15: consider "decrease" instead of "decline"
   g. P. 8 l. 20: rocket-borne
   h. P. 8 l. 27: proves

(3) Word explanation:
   a. P. 3, l. 20: burble fence: perhaps a figure or a bit more detailed explanation of this unusual word?
   b. P. 6, l. 6: "on top of" can be understood in two ways; consider "on"

(4) Commas before subordinate clause (new grammatical subject) starting with "which":
   a. P. 1 l. 16
   b. P. 2 l. 9
   c. P. 2 l. 19
   d. P. 2 l. 23
   e. P. 3 l. 33
   f. P. 4 l. 17
   g. P. 5 l. 2

(5) Commas that may be removed:

a. P. 5 l. 22, after "We note"
       b. P. 7 l. 24, after "This means"
   (6) Other correction:
       a. P. 8 l. 21: consider "This technique has been well established for many years…"

**References:**

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