Response to author

Reviewer 1 (2nd review)

Thanks for the new manuscript and for the efforts you made to answer to both reviewers. I particularly appreciated the detailed description of the results in the literature, the addition of the received power in Fig. 6 and 8, the appendix addition, the paragraph on security issues, the use of the horizontal displacement and the answers to my own remarks, including Fig.1R.

However I noted some points that still require improvements. **Your** text is written **in bold characters**, my proposal is in normal characters.

Lines 45-48

When using RASS techniques, one or more acoustic sources are co-located with an antenna, and the profiler provides the vertical profile of the speed at which the acoustic disturbance propagates vertically (Angevine et al., 1994) from the measurement of Doppler spectrum

- ➔ When using RASS techniques, one or more acoustic sources are co-located with an antenna, and the profiler (from the measurement of Doppler spectrum) provides the vertical profile of the speed at which the acoustic disturbance propagates vertically (Angevine et al., 1994).

I am still not satisfied with this: you can remove the words in brackets if you prefer.

Lines 53-59

Thus, a vertical profile of the speed of sound can be converted to a profile of virtual temperature. The radial wind speed is considered in Eq. (1) because the neglect of the wind velocity along the beam is the largest source of error in RASS measurements (e.g., May et al., 1989; Angevine et al., 1994). However, we do not consider the radial wind speed in our experiments, because strong clutter sometimes contaminates the Doppler spectrum and masks the atmospheric echo in the vertical beam observation. This issue is addressed in later sections.

- ➔ Usually experimenters use the vertical beam to minimize the wind velocity correction. The neglect of w in Eq. (1) may be a large source of error in RASS measurements (e.g. May et al., 1989; Angevine et al., 1994). In spite of this, we were not able to consider w in this work because strong clutter sometimes contaminated the Doppler spectrum and masked the atmospheric echo in the vertical beam observation. This issue is addressed in later sections.

6-1. **We appreciate the reviewer's suggestion. However, we did not find any discussion on the benefit of the range correction at the inversion levels in Görsdorf and Lehmann (2000)** : you are right. I extrapolated their results and it is a mistake.

Line 83 : **are applied** → were applied

Lines 136-138 :

Indeed, the vertical velocity correction can decrease the accuracy of RASS in situations with calm wind and a lower reliability of vertical wind measurements (Görsdorf and Lehmann 2000).

I do not approve this sentence, that cannot be used out of context to justify that you neglected w: there is not enough indication of the wind vertical velocities under your experimental conditions, even if it is early in the morning, under anticyclonic conditions and weak horizontal wind. No geographic indication is given to discard the possibility of sea breeze or mountain breeze for instance. I would then remove the sentence. The reader will easily understand that it is better not to make the velocity correction, when the error is likely to be larger than the correction.

Line 174 : 'flame' ????

Do you mean 'a vertical frame'? This could be a reason why you cannot provide measurements at low elevation (high elevation in this position) as required by Reviewer 2 (see her/his remark 2.3). As far as this remark is concerned, I cannot see how Fig. 2 has been revised. I'm wondering whether there would not be a confusion between the 'zenith angles' you indicate (0 to 40 deg) and the 'elevation angles' that Reviewer 2 mentions (0 to 40 deg ie 50 to 90 deg zenith angles). Anyhow I understood that your system is designed to work at high elevation ...

Line 176 : **because a range of 10 m would be necessary to complete producing audible sound**

- ➔ because a range of 10 m at least is necessary to produce audible sound

Line 178 : instead of the **personal communication,** could not this be simply explained by considerations of geometry?

Line 205: **which may be preferable for the formation of the inversion layer** : I would not use 'preferable' since the quick variation of the inversion layer is more a drawback than a positive point. OK not to mention it now (and remove the highlighted text above). It is enough to mention this point and the resultant variability in the 1-hour results you show in the following.

Lines 209-211:

Using the hourly mean may also reduce the daytime downward bias (e.g., Görsdorf and Lehmann 2000; Adachi et al., 2005)
It would not reduce the downward bias of the vertical velocity but the effect of this bias on the data dispersion. Neither Görsdorf and Lehmann 2000 nor Adachi et al., 2005 said that 1 hour averaging would reduce the bias, although they mentioned the downward bias of w and its effect on their results (10 min- averages and 30 min, respectively). Görsdorf and Lehmann 2000 suggested not to use the vertical velocity for long-term measurements and climatological investigations, to avoid the downward bias, but said that w should be taken into account for comparisons of individual profiles. However, shorter integration times (shorter than 10 minutes) would increase the systematic error in the w estimation. You perfectly know what Adachi and al. 2005 said…

which could be attributable to insects or hydrometeors that are undetectable (Angevine, 1997)
→ other factors could contribute : see for instance

Muschinski and Sullivan, 2013. Using large-eddy simulation to investigate intermittency fluxes of clear-air radar reflectivity in the atmospheric boundary layer, 2013 IEEE Antennas and Propagation Society International Symposium

Abstract from Muschinski, 2014 (MST14 in Sao Jose de Campos) : There have been numerous observational studies that show systematic differences between the mean vertical wind velocity and the mean Doppler velocity measured with a vertically pointing radar or sodar beam. Some of these biases are caused by deficiencies in the hardware or software of the radar or sodar system, but some biases are real, that is, they are of geophysical origin. One group of geophysical vertical-velocity biases results from non-zero covariances of reflectivity fluctuations and vertical-wind fluctuations within the resolution and during the dwell time. These covariances can be interpreted as reflectivity fluxes, which must not be confused with refractivity fluxes. Here, I present observations and computer simulations of reflectivity fluxes in the atmospheric boundary layer, and I discuss some of their theoretical and practical implications.   (I was not able to find a better reference)

To conclude, I find that this issue on the daytime downward bias of w is not well presented here. You should remove it, if you do not find a better way to include it in the discussion.

Line 227 and item 19 about (ISO, 1993): the website indicates:

‘Only informative sections of standards are publicly available. To view the full content, you will need to purchase the standard by clicking on the "Buy" button.’

So thanks for the Appendix you added. Your remarks lines 678-685 are relevant. To illustrate Fig. A1, you could also add some comments about Fig 3 : near the surface, T is 20.2C (Table 3) and the relative humidity is close to 100% (if I’m right). According to Fig. A1, the attenuation is close to its lowest value at 3 kHz whereas it is closed to a peaking value at 40 kHz.

And your remark on the altitude effect could come just after.

Line 246 : the suggestion from reviewer 2 is better than mine (→ were obtained from an altitude).

Response to 30.3 : thanks a lot for your Fig. 1R.

This graph is interesting and may explain the σ value in Fig. 9a. You can see ‘low’ frequency eddies at 200m, peaking every 10 min (8h03, 8h13, 8h23,   , 8h43). ‘ low’, when compared to the turbulence energy spectrum. This may be due to some physical pulses of turbulence (affecting w, or T or both). I agree with you that the time-height cross section would have been different if w had been measured! But such a periodicity is probably not an accident.

It also seems to me that the minima at 200m, correspond to the cold events between 400 and 700 m.

Note: no clear periodicity appears in the data you provide for October the 15th (response 28) even after removing the diurnal tendency.

Anyhow, you are right. All of this is beyond the scope of your paper.

Response to 33.4 : usually, we compute vector averages of the wind and calculate the standard deviation from the standard deviation of both components of the wind …. You can let it as it is, but tell it please.

Lines 300-301:

along with the corresponding statistics for the data.

- ➔ along with the corresponding statistics for the data and the received power for both PAA and acoustic speakers.

Line 310 : 'as shown in Fig. 5' can be removed since Pr is now in Fig. 6.

Lines 311-314 :

gate is too close to the antenna, and factors including the recovery of the receiver and incomplete overlapping of the electromagnetic and acoustic beams due to the special separation between the antenna and speaker systems could lead to a significant gradient in the receiving power at this gate (Lataitis, 1992).

- ➔ gate is too close to the antenna. Lataitis (1992) explained that factors including the recovery of the receiver and incomplete overlapping of the electromagnetic and acoustic beams due to the special separation between the antenna and speaker systems can lead to a significant gradient in the receiving power at this gate ~~(Lataitis, 1992)~~.

Line 317 :
It is noteworthy that the most of the highest range gates → It is also noteworthy that ~~the~~ most of the highest range gates

Line 319 : my opinion is that the received power is the true limiting factor, when comparing the PAA to the acoustic speaker. I agree that the wind can also be a limiting factor, but it seems to me that the lower received power of the PAA is the major factor. However, your study of the wind effect is interesting and I would not change anything.

Lines 338-343 :
RASS with respect to radiosonde were in good agreement with results reported in previous studies (e.g., Görsdorf and Lehmann 2000), despite no correction for vertical velocity, which could have been partly because of the experiments being conducted on fine days with light wind and the application of a relatively long averaging time. In addition, removing the first gate data from the statistics may also contribute to the results.

➔ RASS with respect to radiosonde are in good agreement with results reported in previous studies (e.g., Görsdorf and Lehmann 2000), despite no correction for vertical velocity was done. This could be partly because the experiments were conducted on fine days with light wind and because of the application of a relatively long averaging time. In addition, removing the first gate data from the statistics may also have contributed to the good results.

Line 350 : **low peak power** → lower received power (?)

Line 439 : **may also be reflected** → is also reflected

Line 443 : see my remark for line 319. In Fig. 5c-d, 6b-c-d, the limiting factor is clearly the receiver threshold at -10dB…

Lines 536-537 :
sound pressure levels, the human body becomes warmer until death from hyperthermia has been estimated to occur at levels greater than 180 dB.

- ➔ sound pressure levels, the human body becomes warmer until death from hyperthermia. This has been estimated to occur at levels greater than 180 dB.

Line 592 : **frequency** → its frequency
Anyhow, I do not find that this last sentence is very useful (lines 591-592). I would remove it.

Between lines 573 and 587, you should indicate in this paragraph, the beam width you finally selected.