Response to Reviewer 1 for the second review

We are grateful to the reviewer for the very careful examination of our manuscript. We believe the revised manuscript is improved as a result.

1. 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.

Response: Done (Removed the text from Line 47).

2. 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.

Response: We have revised the manuscript to correspond to this comment. However, we did not use *w* as the reviewer suggested but retain "radial wind speed" based on comments from the 2nd reviewer.(Lines 53-57): "The radial wind speed is considered in Eq. (1) because the neglect of the wind velocity along the beam may be a largest source of error in RASS measurements (e.g., May et al., 1989; Angevine et al., 1994). However, we could not consider the radial wind speed in our experiments, because strong clutter sometimes contaminated the Doppler spectrum and masked the atmospheric echo in the vertical beam observation.

3. Line 83 : are applied => were applied

Response: Done (Line 82).

4. 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.

Response: We have removed the sentence in the revised manuscript (Line 135).

5. 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 ...

Response: This was a typo as the reviewer pointed out. We have revised the manuscript to respond to this comment (Line 171): "In order to measure the audible sound pressure level (SPL) pattern, we installed the PAA on a standing frame (Fig. 1b) for temporal use to radiate sound horizontally." We also revised Fig. 1a and its caption (Line 835) so that the standing frame appears more clearly. See Fig. 2R for detail.

6. 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 …(Prof. Kamakura, 2018, personal communication)

Response: We would retain this sentence because this was what Prof. Kamakura said to us. He was an expert of PAA speakers and has published a review paper on PAA as a co-author (Gan et al., 2012).

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

Response: We think that the reviewer is considering something like the far field region (distance) of antenna determined by the length of the radio wave and the size of the antenna. However, the range to produce audible sound from ultrasound is determined not only by those geometric factors; rather, it is also determined by the nonlinear response of the air to the interaction between two collimated high-frequency sound beams, which might be a function of temperature, pressure, and/or humidity.

8. 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.

Response: We have removed the text in the revised manuscript (Line 203) and added "with light winds" (Line 358) to read "since the operational radiosondes were launched in the morning of fine days with light winds, an inversion layer was frequently observed (Fig. 6)."

9. Lines 209-211:

9-1. 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...

9-2. **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.

Response:

9-1 and -2. We have removed the text in the revised manuscript (Line 206).

10. 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.

Response: We have revised the manuscript and added temperature and relative humidity to respond to this comment (Lines 678-679): the attenuation coefficient could increase with altitude for the former, while it decreases for the latter (e.g., Fig. 3, >1 km AGL, where T = 20.2°C and h_r = 76% near the surface).

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

Response: Done (Line 241).

12. 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.

Response: We have revised the caption for Table 3 to respond to this comment (Lines 830-831): "and mean wind speed aloft (20 — 1200 m AGL) with standard deviation. Means and standard deviations are not vector but scalar statistics.

13. 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.
Response: Done (Line 296).

14. Line 310 : 'as shown in Fig. 5' can be removed since Pr is now in Fig. 6.
Response: Done (Line 305).

15. 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)~~.

Response: We have revised our manuscript to respond to this comment (Line 306-310):" the first gate is too close to the antenna. In fact, Lataitis (1992) suggested that factors…speaker systems can lead to a significant gradient…"

16. 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
Response: Done (Line 313).

17. 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.

Response: Done (Lines 334-339).

18. Line 350 : **low peak power** => lower received power (?)

Response: We intended to mention about the lower peak power transmitted from the PAA. We have modified our manuscript to make it clear (Line 346):" the low peak power mentioned previously (Fig. 2)."

19. Line 439 : **may also be reflected** => is also reflected

Response: Done (Line 435).

20. 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...

Response: We have modified our manuscript to correspond to this comment (Line 439):" the former can observe up to the highest range gate as the latter as long as the received power is more than about -10 dB.

21. 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.

Response: Done (Lines 532-533)

22. Line 592 : **frequency** => its frequency

Anyhow, I do not find that this last sentence is very useful (lines 591-592). I would remove it.

Response: We have removed the text in the revised manuscript (Line 587).

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

Response: We have revised our manuscript to respond to this comment (Line 576):" The sound wave was then steered windward with the default beam width (~5°)"