Review of : **'Application of parametric speakers to radio acoustic sounding system' from A. Adachi and H. Hashiguchi, in AMTD.**

Overall recommendation: Minor revisions required

General comments:

The paper presents a new method to use the RASS system for wind profilers. This novel system gets around one major defect of the RASS which cannot be used continuously in inhabited areas due to the noise pollution. The parametric speakers allow to measure the secondary difference frequency of ultrasonic waves emitted at 37 and 40 kHz. Ultrasonic waves are rapidly absorbed in the air, whereas the secondary wave of around 3 kHz are transported and backscattered like the wave of classical RASS. The characteristics of the parametric speakers are their low side lobes (an advantage) and their higher directivity (perhaps a disadvantage, I'm not sure to be able to conclude). The paper describes an accurate comparison between both systems, in order to assess the results of the parametric system.

The manuscript fits the standard of AMTD and is well organized. The abstract reflects the content. The problem is clearly introduced although some points have to be improved. The paper is well documented although I find that the works from other authors are sometimes too quickly cited and would deserve to be developed further. The instrumentation is well described and the comparison is accurately and seriously conducted. Figures are useful, clear and accurately described.

I've got a unique reservation. I did not experience RASS systems myself but I heard about the difficulty to retrieve the level of inversion layers when individual profiles of RASS are compared to radiosonde profiles. These difficulties are hidden by time averages and uncertainties calculation on the whole profiles. The authors mention these difficulties, cite authors who explain how to cope with them, but I think it is not enough to make the reader trust the method. I make some proposals in the following to clarify this point. Note that I do not question the results that are presented in this manuscript.

Specific comments, minor remarks, suggestions:
Page 2, line 24: I do not understand the use of 'if' in this sentence.
Line 29: I would write 'versus' instead of '/' in 'RASS/radiosonde'.

Page 3, line 47: 'measures' → I suggest 'provides'
Line 48: I would add 'from the measurement of the Doppler shift between the emitted and backscattered radar wave', after 'propagates vertically'
Line 49: 'among' → 'between'

Page 4, lines (54-55): This issue about the vertical velocity correction is very important and not well presented. It is obvious that w has to be included. The fact that you could not use w is an issue, and you show in the following how you cope with it (1-hour averages). The way the issue is introduced, at the beginning of the paper, is not satisfying especially the sentence : 'the vertical wind speed is considered in Eq (1) because the neglect of the wind velocity ….'. I explain why.
At the beginning (p 4), it is obvious that the correction has to be done. You mention p 7 (lines 112-115), that w could not be used due to the automobiles echo. So I deduced you had used the oblique beams instead of the vertical one. I understood in the following that you did not.
Please, could you clarify this issue in your presentation, perhaps by saying at the beginning that you had to neglect w and that this issue would have to be discussed?

If you cite May's and Angevine's papers it could be interesting to add (further on in the manuscript) some details of their discussion, including the discussion on the downward bias in the profiler velocity measurements mentioned in Angevine et al., 1998: the 1-hour average is a way to mitigate this effect.

Minor comment: in lines 57, 59 and 61 respectively, you use brackets for two different purposes: with 'bias (systematic error)', 'standard deviation (precision)', the brackets provide the meanings whereas in 'bias (standard deviation)', you present an alternative. The latter use of the brackets is continued through the manuscript. The first use could be presented in a different way.

Line 63 : I would add 'accurate' before 'corrections'
Line 64: if relevant, I would add 'thermodynamic' before 'constants'

Lines 61 to 64: I was not convinced by the results you cite (Görsdorf's and Lehman's work) as long as I had not read the paper. My reservation was about the one year time average that reduced the bias as expected. When reading this paper, I was convinced and I found additional interesting information that would deserve to be discussed in your own paper (but probably not in the introduction):

- the reason for the range correction would be helpful to comment (see my point, Page 16, line 245). Görsdorf and Lehman are not the only ones who mention this range correction: Angevine et al., 1998 also do, but in the references you cite, Görsdorf and Lehman discuss the benefit of the correction at the inversion levels, which is a major advance and could feed your discussion.
- the distinction between climatological investigation and individual profile comparison is also an important point to comment: you can hardly compare errors amounts when they are computed with varying time-scales (unless you discuss the conditions).

To conclude, I would say that the use of this reference comes too early and that you could take more advantage in discussing it further on.

In general, I suggest that you give more details about the results and the uncertainties that are reported in the literature, concerning the RASS assessment. For instance, Angevine et al., 1998 made a comparison of Tv during 1 month, at one level (396m agl) between a RASS system (UHF) and tower instruments. The uncertainties that they reported are expected to be different from those obtained for instance by Moran and Strauch (1994) who used a RASS with a VHF profiler, during 5 weeks (400 profiles, compared to RS). Moreover, in each case, some information should be given about the corrections (vertical velocity correction, range correction …).

Page 5, line 66: you could also add 'the distortion of the acoustic wave due to turbulence and vertical temperature gradients (Lataitis, 1992)'

Line 68 : 'A wind profiler/RASS has been used' → Joined measurements of wind profiler and RASS have been frequently used'.

Line 71: 'Important limitations of this method include the emission' → 'Among the limitations of this method, an important one is the emission'

Line 80 : 'is expected' → 'would be promising'?

Page 7, line 100: please provide the latitude of the site
Line 106: 'The MRI wind profiler' → 'The MRI 1357-MHz wind profiler'
Line 113: see my previous remarks about w (p. 4)

Page 8, line 119: 'In the experiments' → 'For the experiment purpose'
Line 122-123: about pseudorandom frequencies. This is either too much or not enough explained for a non-expert reader. Is it necessary to mention? I suggest to move this specificity to Table 1.

Page 9, line 134-137: am I right to say that the beamwidth of the speaker should be larger than the MRI beamwidth? Does that mean that you start with degraded conditions with the default sound beam? Is it the point you raised when you spoke of the parametric speaker directivity (line 22, page 2)? I raise this point since I wondered at the beginning whether the directivity would be an advantage or a drawback.
Line 137: turbulence will broaden the sound beam width, especially inside the boundary layer, but it will also reduce the measurement range, as you said before.
Line 137: 'aloft' is not accurate enough and I wonder whether it is appropriate here.

Line 138: 'measured in the field' I would add: 'with a sound level meter'. Could you describe the measurement protocol?

Line 143: 'and is **therefore** significantly more annoying' (therefore can be added if you find it relevant).

Page 11, line 161: at 10-m height?
Lines 163-167 : my first reaction is that the launching occurred in a period of the day when the time temperature gradient is important and the boundary layer top level is rapidly increasing. You discuss this point later on, but you could already add a few comments. I also suggest to add a column in Table 3, with the time difference between the launching and the sunrise time. I calculated 2h30 to 4h. Am I right?

Line 170: I do not understand the meaning of 'availability'. Do you mean 'practical relevance'?

Page 12, line 182: tell me if I am wrong, but it seems to me that the reference (ISO 1993) is not easily available. Could you provide (in an appendix) the equations to calculate the sound attenuation coefficient and the attenuation, derived from the RS measurements?

Page 13, line 200: 'the PAA also reached a minimum altitude of 1.1 km AGL' → 'the PAA reached an altitude of 1.1 km AGL'.
Lines 203-206: this unique example is not enough to draw a conclusion. So, I would use 'may reach' instead of 'can reach' and indicate that the propagation level will be systematically compared between the 2 systems.
or, you can simply say that the example in Fig. 4 is promising….

Fig. 5: usually, the backscattered signal of wind profiler radars is artificially corrected at the first gate to avoid receiver saturation. Are you sure it is not the case for RASS systems? Could this be a reason for the increase of the echo power between the first and the second gate?

Page 15, line 228: 'available' → 'convenient'
Line 231: 'immediately' → 'sharply'

Fig. 6: I suppose your sample of profiles is the best you could provide. Would it be possible to have a look on the corresponding Received Power profiles?
Why is there such a large difference in the number of data (320 and 237 in Fig. 6a) while the measurement range is the same for both systems? On the opposite, in Fig. 6d, 96-90=6 is small while the PAA vertical range is 200 m lower than the acoustic speaker. I probably miss something.

I suppose you tried comparisons with RS data averaged during 15 min or 30 min. Were the results so bad?

Page 16, line 245: '(e.g. Figs 6n and 6c)' → 6a also.
Line 249 : I suggest that you give here the reason for the range corrections presented by Görsdorf and Lehmann and that you briefly explain the processing.
The range corrections should be applied to the whole profile (and not only at the first gate), to mitigate the discrepancies at the inversion levels.

Lines 251 to 253 (page 16, 'The RASS data were averaged for about an hour. The RS data were smoothed by 100 m running means to match the RASS observations)' should be moved p15, just before the comments on Fig. 6.

Page 18, line 278: as said before for Fig. 6, I'm not convinced by the argument of the link between the smaller data number for the PAA and the height coverage. In addition, the wind is relatively stronger at 1300m in Fig. 5a, while the height coverage is similar for both systems. However, I also find it necessary to discuss the effects of time evolution on the temperature profiles and of wind.

Page 19, line 296: what is the standard deviation of the temperature increase?
Line 297: 'In this case' is awkward.

Lines 298-300: The comparison with the RS is good for an average of one-hour and you well explain that such an average is necessary to mitigate the errors due to the lack of the vertical velocity measurement. I agree that there are several reasons that could explain the RASS standard deviation during one hour, but that should not affect the average (unless surface covers or advection processes would be drastically different between the RS launching site and the RASS site). So what do you mean by 'degrading the statistics'? Increasing the standard deviation?

Pages 19 and 20 and Fig. 8:
Are the 1-hour profiles of the RASS centered around 30 min after the RS launch (8h30 in the caption of Fig. 8)? Why did not you center them on the launch time since you are interested in the low layers?

I do not find that the 1min-RASS profiles in Fig. 8 are closer to the RS profile than the RASS 1-hour average. It is true for some points, but not for all and it is not surprising: I think it is not significant to compare the 1-min RASS profiles to the RS profile since 1-min RASS estimations include fluctuations (temperature fluctuations and fluctuations linked to the measurement process). 1-min is too short to include enough time (or space) scales of turbulence. So the 1-min profiles reveal some snapshots, which could be very different, 2 min later and a fortiori, different from a RS profile, measured 400-m apart (in addition to the fact that the radiosonde takes around 3.25 min to reach 1300 m). It could be interesting to check the time evolution of the RASS profiles during the hour. Would this evolution seem erratic like in a turbulence process? Or could we see a slow increase of the inversion layer? (I do not require that you include this in your next manuscript, but it could be interesting to discuss this point). As far as the inversion levels are concerned, I tried to join the points of the 1-min profiles in Fig. 8. The first inversion level that is detected at around 650 m with the RS is found at around 520 m and 500 m by the acoustic and parametric speakers respectively. This discrepancy could perhaps be decreased by a range correction, but there again, you could not conclude due to the lack of vertical velocity information (that can hardly be neglected when two successive 1-min profiles are compared).

Nonetheless I agree with your conclusion p. 20: 'a comparison with measurements that have both small spatial difference and high time resolution is needed to evaluate the PAA-RASS measurements.'

Page 22, lines 341 to 345: I do not agree with these arguments since they describe atmospheric phenomena whose time scales are larger than 1 min. I think, as said before, that the variability comes

from the local turbulence and also perhaps from the physics of measurement that is probably different between the 2 systems. Could the difference in the beam widths play a role?

The fact that the standard deviation (0.43) in the 1-min statistics of PAA vs acoustic is around the same as the standard deviation (0.4) of the 1-hour statistics of both RASS vs RS reinforces the idea that small scale processes are responsible for the variability (in addition to processes at larger scale like the increase of the inversion levels with z and time).

Page 23, line 364: see my questions about Fig. 6 (p. 15)
Line 368-369: I do not understand:' On the other hand, the results also suggest that the reason may include the effect of wind aloft (e.g. Fig. 5a)' since the height coverage is the same for this specific case.

Line 369: 'wind aloft' should be defined here and not p. 24, lines 364-365. Are you sure wind is measured at 1 m? (1 to 1200 m AGL). How did you compute the standard deviation of the wind?

Page 24, line 384: the linear regression for the acoustic speaker is not relevant. I would remove it from Fig. 10.

Lines 388-389: 'This suggests that the acoustic speaker RASS **keeps on observing** at a high altitude **even** in relatively high wind conditions.'

Page 27, line 423: 'may be distorted' → 'may have been distorted'.
Line 424 : 'may be needed to be steered' → might have been steered'.

Page 29, line 455: 'does' should be removed.

Page 30, line 473: I do not understand 'but <u>frequency</u> is likely to be less'.

Additional question: does an emission around 40 kHz require an authorization?

As a conclusion, I would be pleased to see your future works with this system, with more favorable conditions such as: the possibility to measure w, some range corrections, measurements during the whole day. I am still wondering whether the parametric system would be more efficient than the acoustic system at inversion layers (due to its narrower bandwidth).

Additional suggestions for tables and figures:

Table 3: I would use U instead of W (even if W is not w) and would add '(1 to 1200 m AGL)' after 'wind aloft'.

Figure 5: 'except for the first range' → '**(**except for the first range**)**'.

Figure 6: 'from 08:30 JST' is not convenient for the 4 profiles. See also my previous remark about Fig. 8, concerning the center of the 1-hour time average.
'The error bars represent 2 σ in the RASS **hourly** observations' ('**hourly**' added).

Figure 7: I would say RASS vs. radiosonde instead of radiosonde vs. RASS, but the editor will confirm or refute.

Figure 8:
'RASS with acoustic speakers (red) **between 8h01 and 8h02**'
'RASS with parametric speaker (blue) **between 8h02 and 8h03**'

Figure 9:
Same remark as in Fig. 7 for the use of vs.
‘**(**except for the first gate**)**’
‘a normalized frequency diagram **(color scale)**’
‘the mean **hourly** data were plotted in (b)’

Figure 10:
‘wind speed aloft **(1-1200m)**’
Please remove the linear regression for the acoustic speaker or present two legs: one horizontal from 1.5 to 5.5 m/s and another oblique one for stronger winds.