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Atmospheric Measurement Techniques Discussions

Interactive comment on "Differential absorption radar techniques – Part 1: Surface pressure" *by* L. Millán et al.

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[12pt]article color

Response to reviewer 1: Differential absorption radar techniques: Surface pressure

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We sincerely thank reviewer 1 for his/her thoughtful comments on the previous draft, we hope this new version is more suitable for publication.
In the course of making the corrections, we decided to change the title to: 'Differential absorption radar techniques: surface pressure' deleting the 'part 1' since the next part will be about either pressure profiles retrievals or water vapor retrievals. Below are our responses to the reviewers comments in red.

The paper proposes a DiAR system near the 60 GHz O2 absorption band for retrieving surface pressure. The paper clearly describes the underpinning theory and presents a detailed performance analysis in terms of accuracy and precision for different atmospheric scenarios. The paper is generally well-written and clear. I would like to pinpoint at three areas that may need some improvement.
1. I am not a spectroscopy expert but I would expect possible biases related to the modeling of the absorption band. Is it possible to add an additional perturbation to the ones already used in Table 1 and account for such an uncertainty (e.g. by using a different absorption coefficient model)?

The following sentence was added right after the sentence 'Table 1 summarizes the perturbations used.' which concludes the paragraph describing the systematic errors: The spectroscopic uncertainties such as the errors due to the atmospheric absorption continuum (O_2, N_2,H_2O), the line strength, and line width, as well as the line mixing due to close spacing of the O_2 lines around the 60 GHz spectral region will be investigated in a future study but presumably their impact will be small (i.e. given the successful DAR measurements from Flower and Peckham (1978); Lawrence et al. (2011)), and calibrated out as part of a vicarious calibration campaign and can be better bounded through dedicated laboratory measurements.

2. It would be informative to mention what are the user requirements for surface pressure. I guess the target here is the NWP community.
The following sentence was added at the end of Section 5 (Error characterization and tone selection): Furthermore, these measurements should provide useful constraints for numerical weather forecasting (Flower and Peckham, 1978; Lin and Hu, 2005) closing the large gaps in the measured surface pressure coverage particularly over the oceans. The quantitative impact of these measurements needs to be investigated using an Observing System Simulation Experiments (OSSE) or similar technique to study the impact on data assimilation and forecast system performance.

3. It is difficult to understand the impact of hydrometeor differential absorption. What are the differential PIA signals we are confronting with for instance for the frequency pairs used in Tab.2? And what is the uncertainty on top of such signals produced by PSD variability? Maybe an additional plot could help.
We carefully considered this suggestion, however showing the differential PIA signals for several scenes as well as for the frequency combination on table 2 resulted in a overly confusing figure. Furthermore, the point of this paper was to show the feasibility of the surface pressure retrievals as well as quantify its uncertainties, while reducing

as much as possible the discussion of technicalities. Those are left to further studies if they are plans to fly such an instrument.

Minor comments:

Page 5798 after Eq (1). It is a little bit misleading what is said with respect to the normalized surface backscattering (of course for surface the Equation looks a little bit different and it is not just replacing eta with sigma (which of course have completely different units!)
Deleted the surface backscattering reference

Calibration uncertainties are expressed in dB not dBZ
Yes, because they represent the difference between two radar pulses dBZ1 - dBZ2 = dB(Z1/Z2) = dB

Not clear why the horizontal resolution should be 400mx400m (what is the rationale for that?)
The reference to the horizontal resolution was deleted, instead this sentence was added at the end of section 2 when discussing the radar system: assuming a sub-second integration time resulting in a sub-km scale horizontal resolution

Page 5804 line 12 and page 5809 line 10 'retrieved' should read 'retrieve'
Done

Not clear what is the meaning of +-1 dBZ at page 5804 line20
Changed to: with return values of around +- 1dBZ, etc

Fig6-8 : I am not so sure that the logarithmic scale is the right choice for the histogram (I do not care if the IWC is causing an error of 0.00001 mb)
We decided to leave the figure as it is, because the log-scale was the only way to capture the range of the uncertainties, and also, although nobody cares about an error of 0.00001 mb, it gives a reference of the magnitude, as well as, showing that we did take its really small contribution into account.

Fig4 and fig5: wrong units for hydrometeor column . It should be kg/m2
corrected

Fig3: Maybe change the color scheme (2 curves are basically red)
They were both red, I changed one to dotted rather than solid

Fig2: why for the bottom panel there is no reflectivity in the FM simulation above 7km?
This sentence was added in the caption: Forward model simulations are only shown for those altitudes with hydrometeors, the surface return is represented by a dot.

Fig.10 You may cut the x-axis at 6 or 7 hPa
Done

It is not clear what the numbers reported in the abstract correspond to. Maybe better talk about worst case scenarios or your cumulative distribution; the term 'generally' is too vague.
They refer to the estimates using the optimum frequencies (cumulative distributions). The numbers are exactly the same as in section 6: Summary and conclusions, that

explain where do they come from.