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Atmospheric Measurement Techniques Discussions

Interactive comment on "Can turbulence within the field of view cause significant biases in radiative transfer modelling at the 183 GHz band?" *by* Xavier Calbet et al.

Anonymous Referee #2

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This paper looks at the effect of the atmospheric turbulence in the radiative transfer simulations in the 183GHz band in order to analyze the origin of the differences that have been noticed recently between the calculated and observed radiances in this band. This is an important contribution because the sources of this differences are numerous, from colocation effects to spectroscopic uncertainties, and may compensate or enhance each other due to non-linearities.

The paper is very shaped and its focus is clearly stated: to what extend sub-FOV turbulence can explain these discrepancies?

However, in addition to the points raised by Referee #1 and Stefan Buehler, I have

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Discussion paper

some comments that should be quickly handled.

#1: Radiosonde data have been used to compute the turbulence: how many doublets compose the dataset? I didn't find the relevant information in the paper while this should be specified in order to test the robustness of the computation, if that makes sense for the computation of turbulence.

#2: Following Stefan Buehler's comment: the Taylor expansion seems to lack some cross terms. I would like to see the full Taylor expansion where B=f(R,T), and then decompose into a part only in R, a part only in T and a cross term.

#3: The evaluation of turbulence is computed using 1 single tropical profile, extracted from ECMWF analysis. Why not an observed profile? With the CINDY/DYNAMO campaign in 2011, a lot of high-resolution profiles are available. Could the author explain why they chose a modelled profile? Is it that straightforward to apply midlatitudes/almost polar coefficients for turbulence to a tropical situation? I think that this should be discussed a little bit in the analysis (p6, lines 1-6).

#4: Also, the final computations are performed for a zenith angle of 60°. Surely a nadir test would result in smaller impacts, but that would give the expected range of amplitude.

#5: In summary, the message that I keep from this work is that including the turbulence term in an error budget when performing cal/val should be almost mandatory, at least for 183GHz band sounders. I thus believe that a message like this one should be stated at the end of the paper: this would constitute a strong recommendation for launching doublets of RAOBS while planning a cal/val framework for microwave (183GHz at least) instruments.

Minors comments: #1: Maybe at a little "e" p4, line 22, after "water vapour partial pressure" so that the terms in Fig 1 can be related to the text.

#2: Fig 3 should be modified a little bit. The computations are done for some channels

of SAPHIR: the reference to the channels would be clearer if the notation f0 +/- XX (C1; C3; C6) was used in the inset, with f0 defined in the legend. Another cosmetic point: The figure would be easier to read if the same color with different patterns (plain for dB/dR; dotted for d2B/dR2) was used for the same frequency. Now it is confusing.

#3: There seems to be a confusion between the colors of Fig 4 and the Discussion section (p6, lines 11-29). The text mentions a brown line that I don't see and I think that it should be the green line. Please check. I think that this might be due to the colors used: the lines (eps=1500; dR=0.32) / (eps=5;dR=3 10ˆ-3) are too similar to distinguish. The colors need to be modified to avoid colors that the eye cannot clearly distinguish.

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