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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 #1

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This is a very good paper proposing a possible explanation for discrepancies between calculated and observed radiances at 183 GHz, which is an important issue for climate science and weather forecasting.

Firstly, I agree with the comments made by Stefan Buehler.

I think the occurrence of biases arising from non-linearity is a more general issue than the effects of small scale turbulence, though it is a legitimate hypothesis that this is the main mechanism for the observed biases.

The paper does not consider other sources of non-linearity error that could also generate biases, for example clouds (and here I stress bias due to non-linearity of cloudy

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Discussion paper

RT, not just because clouds are not well known or detected). Also large scale discontinuities such as weather fronts. The paper does not attempt to prove that, globally, the size and prevalence of sub-fov scale variability due to turbulence is consistent with the measured bias. The paper notes that when sampling is biased to scenes with low turbulence no bias would be found, using this to explain why the bias was found in the Bobryshev study. But as this is plausible it equally follows that sampling has a major impact on the bias, and therefore comparisons with the size of turbulence at selected locations is simply one realisation of a pdf of biases due to this effect that can range from zero (as the paper notes) to considerable. Therefore would we not expect significant variability in the bias, depending on meteorological conditions, and also local effects? I feel as the paper is proposing turbulence as a major mechanism for explaining the observed bias (in global fields, and similar everywhere) it needs to be able to demonstrate that the effect has the same geographical variation (or lack of it). I found this discussion to be missing in the paper.

However the paper is short and only presents this as a hypothesis. I think they provide sufficient evidence to show that this hypothesis is not easily dis-proven, but agree more work is needed. I therefore feel that the paper is an important contribution and should be published without major changes. However I would like to see these two minor points addressed, in addition to the points raised by Stefan Buelher: 1. Sub FOV scale variations due to turbulence is one example a more general issue with errors arising from non-linearity in the radiative transfer - there are others.h 2. We need to prove that on a global scale the size and variability of biases arising from this mechanism is consistent with observed biases - at present it is only shown it is capable of producing biases of a magnitude not inconsistent with observations, a necessary but not sufficient condition to consider this the most probable explanation for the global biases seen.

By and large the text was highly readable with few typos / grammar errors, but I expect the journal will pick these up e.g. Moore rather than more on p4, "These kind of turbulences" is an awkward expression.

In summary this is a very interesting paper and a very important contribution to this discussion and I congratulate and thank the authors for their work.

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