Atmospheric Measurement Techniques Discussions

Atmos. Meas. Tech. Discuss., referee comment RC2
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Comment on amt-2021-361

Anonymous Referee #2

Referee comment on "Towards the use of conservative thermodynamic variables in data assimilation: a case study using ground-based microwave radiometer measurements" by Pascal Marquet et al., Atmos. Meas. Tech. Discuss., https://doi.org/10.5194/amt-2021-361-RC2, 2021

Review of AMT-2021-361 Marquet et al: Towards the use of conservative thermodynamic variables in data assimilation: preliminary results using ground-based microwave radiometer measurements

The study analyzes the potential of introducing two moist-air conservables as control variables to a 1D var data assimilation system in order to improve short-term forecasts for fog conditions. A model setup with the new variables is compared to a conventional model setup based on a case study from the SOFOG3D campaign. New control variables require updated background error covariance matrices which are evaluated for stability under several meteorological conditions. The new setup is evaluated in model and observation space.

General comments

The study is clearly written and the methodology is well described. The results are a good addition to the field and are well presented. The manuscript is convincing and I recommend publication after the following comments have been addressed.

Specific Comments

- The aim of the study is generally clear. Yet, abstract, introduction and conclusion present different main scopes of this paper. I encourage the authors to align the main

scope of the paper throughout the manuscript's sections.

- Ice-free conditions are assumed (ll 97). Yet, Fig 2 reveals the presence of most probably ice-containing cirrus clouds in the analyzed case study. How is the resulting TB signal considered in the analysis and which errors are induced by the ice-free hypothesis not holding true?

- I find 'preliminary results' in the title very misleading. Which part of the shown analysis makes the study preliminary? I suggest to modify the title or to add an explanation to the conclusion section.

- Sec. 4.2: I am missing a comment on the performance of channel 8 in the analysis in Fig 5. Why does this channel show higher deviations in comparison to the other temperature sensitive channels? How are contributions by uncertainties in the forward model versus observational errors in the measured TB considered in the comparison?

- Sec. 4.3: I suggest adding another Figure to illustrate the characteristics of the Jacobians as given in the text.

- Conclusion: I find the conclusions from the results sections 4.2 and 4.3 too short (ll 317-319). The added value of the new setup in particular for forecasts in foggy conditions should be highlighted as well as of the assimilation of TBs. The summary of the main results of both the comparison in model and observation space could be expanded by one or two additional sentences.

Technical Details

l4: should read: SOFOG3D
l300: should read: motivation
l 106: no period needed in section title
l 113, 116: replaced weighted with 'weighed'
l 147: should read "such as a HATPRO" (Humidity and Temperature PROfiler, Rose et al, 2005) instead of giving long title in l 187
l 255: do the authors refer to the first seven channels instead of eight?
l 289: sentence is unclear.

ll 300-306: descriptions would better fit to introduction or Sec. 2
l 312: should read ‘first’ instead of ‘firstly’

Figs 3-6: add label and unit to color-bars
Fig. 5: channel numbering should be as in text (1-13 (or 1-2; 4-14) instead of 0-12); explanation of the dashed blue lines should be added to figure caption. It would be desirable to have the analysis text closer to the figure to improve readability of the manuscript.