

Interactive comment on “Retrieval of temperature profiles from CHAMP for climate monitoring: intercomparison with Envisat MIPAS and GOMOS and different atmospheric analyses” by A. Gobiet et al.

Anonymous Referee #2

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General Comments:

The paper presents an assessment of temperature retrieval from radio occultation measurements using a processing software developed at the University of Graz. Temperature profiles are validated using co-located profiles from other satellite observations and model analysis.

Processing of radio occultation data does require some a priori information; such a priori information can contaminate obtained profiles in particular when using such data

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for climate applications. The authors argue that their processing system is not very sensitive to these a priori assumptions.

I feel that further information is missing that shows how much a priori information has entered the retrieved profile at stratospheric altitudes. Suggestions for further information is provided below. Once this information is provided, a publication is recommended.

Specific Comments:

page 3231:

"data sparse regions" given that the main information in NWP models is these days provided by satellites, such regions do not really exist anymore. Radio occultation is rather complementary to other satellite observations.

page 3238-39:

"As background information..." How is MSIS combined with ECMWF? Are 91 ECMWF levels used for more recent periods and is there any effect on the obtained temperature profiles? What will be the effect of improved ECMWF temperatures, e.g. the removal of the stratospheric cold bias in future analysis? Will this generate a fake trend at higher altitudes over time? Could you further quantify the error of the observation and the background to see their individual contributions?

page 3239-40:

"Finally a rough..." Could you provide further information on how many profiles are removed?

page 3243:

"This yields no ..." von Engel compared the full processing from orbit to temperature. Here, only level 1a (?) and upward is compared.

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page 3234:

"Since CCR uses... " Taking into account the remarks made by J. Wickert, is the selection of profiles affected by this processing problem at GFZ? What kind of profiles are removed in the quality control and will this change once the processing problem has been removed?

page 3244:

"since the latter data..." Just because MIPAS uses a smoothing constraint with ECMWF data does not mean it is independent of ECMWF. It just uses a different cost function.

page 3245:

"In addition... " has there been any attempt made to compare bending angles directly? They should be less affected by structural uncertainty.

page 3250:

Discussion of Figure 2: All plots show a bias of up to 0.5K at altitudes below about 13km. Water vapor does not seem to be the cause, low latitudes show a smaller bias than high latitudes. What is the reason for this bias?

page 3251:

"Such a large bias..." This part (and others) needs to be reworked with respect to the comment made by J. Wickert. Otherwise one would expect this bias also to affect GFZ retrievals. It is also surprising to find this bias between CCR to GFZ AND CCR to MIPAS, leaving the impression that this is an atmospheric feature not picked up by CCR - and GFZ is doing a better job albeit their processing problem mentioned earlier. In addition, remarks made earlier on the low-sensitivity of the chosen initialisation to a priori biases seems to be proven wrong here. Could you provide some profiles with different a priori biases and how they affect the CCR retrieval? The impact of a priori data should decrease exponentially with decreasing altitude, but in Fig. 2 and 3 it

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seems to be more linearly decreasing. Is such a high bias of ECMWF likely, e.g. when compared to other measurements at these altitudes?

Technical Corrections:

page 3245:

"As co-location ..." This appears to be a central criterion for all comparisons and should thus be moved to maybe Section 3.2

page 3255:

"excellent cooperation...": suggest rewording

Interactive comment on Atmos. Chem. Phys. Discuss., 7, 3229, 2007.

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