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Atmospheric
Chemistry
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Discussions

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 #1

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General Comments

This paper considers the accuracy of dry temperature retrievals derived from GPS radio occultation (GPSRO) measurements, in the context of climate monitoring with this technique. The paper outlines the implementation of the CHAMPCLIM retrieval (CCR) at the University of Graz and compares with retrievals from GFZ, retrievals derived from other measurements and numerical weather prediction (NWP) analyses. Differences with the GFZ retrievals are explained in terms of different and improved use of a priori information in the CCR retrieval.

Throughout the paper the authors suggest that the CCR does not overweight the a priori information, but it is difficult to quantify the importance of the a priori information in the CCR retrieval in the stratosphere from the results presented. A key reference on the use of satellite measurements in climate studies is “On systematic errors in satellite sounding products and their climatological mean values”, JR Eyre, QJRMS, vol. 113, 279–292. This points out the importance of the averaging kernel when assessing the role of a priori in a satellite retrieval. It would be useful to present vertical profiles of the diagonals of the averaging kernel matrix for both the optimised bending angles and the temperature retrievals (see specific comments for more details). The paper would also be improved by more details on the CCR. Eg, how are the ECMWF and MSIS data combined at 65 km? In addition, although the authors claim that the temperature is initialised at 120 km, I suspect that the hydrostatic is effectively initialised at a much lower height than this. The authors also need to consider the implications of ECMWF operationally assimilating GPSRO measurements, on the use of ECMWF information in the CCR. I also suggest that they contact GFZ to discuss an other possible reasons for the CCR - GFZ differences in the stratosphere.

These points, and the specific comments below, should be addressed before publication.

Specific Comments

section 2.2

Page 3237, “no "2nd initialisation" is needed to initialise the hydrostatic integral”. The hydrostatic integral is initialised at 120 km, so you do perform a 2nd initialisation.

Please clarify

“This ingests minimal a priori and allows for clear tracing of the amount of non-observed information entering the retrieval.”. This “clear tracing” is not currently obvious to the reader. You need to present averaging kernels for the temperature retrieval to enable clear tracing of the non-observed information. These should be included in the paper.

section 2.3

Gobiet and Kirchengast (2004) employ a search strategy to find the MSIS bending angles that give the best fit to observations in the 45 – 65 km and also introduce an ad-hoc scaling factor. Are these still used? How do you deal with the transition and inconsistencies between ECMWF and MSIS at 65 km?

Is the Gobiet (2005) reference generally available? I feel that more details of the CCR are required in the paper.

The use of ECMWF between 30 – 65 km. ECMWF now assimilates GPSRO measurements operationally, so the GPSRO observations used in CCR and the ECMWF a priori will not be independent from Dec 2006. How will this be accounted for in your retrieval for climate monitoring beyond 2006?

Page 3238, 2nd paragraph. Estimating the errors from the variance. It is noted that a widely adopted approach, estimating the errors from the RMS relative to the a priori, can overestimate the observation errors if the a priori is biased. Can you quantify the magnitude of the overestimation? It seems surprising that this is significant. Assume that the error is estimated from the RMS is taken from 65 to 85 km. The a priori bending angle value with a tangent height at 65 km is $\sim$ 3 micro-radians and we generally assumed the errors, $\sigma_b \sim$ 15 % of the background value. The CHAMP observation errors are typically $\sigma_o \sim$ 3 micro-radians. If the bias in the a priori is of order σ_b, then it is unlikely to have a big impact on the σ_o estimate (increased by a few percent) and subsequently the optimised bending angles.

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Discussion Paper

Page 3239, end of first paragraph, last sentence, the transition between background and observation dominance. Please quantify what is meant by “background dominance”. The optimised bending angles are given by

$$\alpha_{\mathrm{s}} = \alpha_{\mathrm{b}} + \mathrm{B}(\mathrm{B}+\mathrm{O})^{-1}(\alpha_{\mathrm{o}} - \alpha_{\mathrm{b}}) \tag{1}$$

so the averaging kernel is given by for the optimised bending angles is

$$\mathrm{R} = \mathrm{B}(\mathrm{B}+\mathrm{O})^{-1} \tag{2}$$

It would be useful to plot the diagonal values of the averaging kernel, R_{ii}, as a function of height for 2 cases where the background dominance starts at 45 and 65 km, respectively. This might help quantify “background dominance” for the reader.

At and above some height, say h_b, the optimised bending angles will be effectively equal to the a priori bending angles. I.e., if $h \geq h_b$ than $\alpha_s \simeq \alpha_b$. Therefore, h_b is effectively the height of the temperature initialisation. This is because the a priori temperatures have simply been mapped to refractivity and then bending angle. These a priori bending angles have then been mapped back to refractivity with an Abel transform and then the hydrostatic is integrated to yield the original, a priori temperature information. It is a closed loop and failure to reproduce the original a priori temperatures will be a result of limitations in the forward modelling and inversion. Can you estimate h_b for the cases where background dominance starts 45 km and 65 km? This will be a more realistic temperature initialisation height than the 120 km that is quoted in the paper.

Section 3.3.1, the use of “three different sources of information (one observational and two a priori) leading to an overemphasis of the a priori ...”. But the CCR also uses 3 sources of information, observation, ECMWF and MSIS, so 3 sources does not necessarily lead to an overemphasis of the a priori. Please explain what you mean here.

Section. 3.3.2, “MIPAS data is not biased against ECMWF, since the latter is used as a smoothing constraint rather than for Bayesian combination”

This is misleading. The MIPAS retrievals are based on a minimizing a cost function which penalises $(\mathrm{x} - \mathrm{x_b})$ departures. The fact that these departures are weighted by a matrix called smoothness constraint matrix, rather than an error covariance matrix is not relevant to the basic mathematics of the inversion (See Rodgers book, problem 10.2; Titterington gives a Bayesian interpretation of the smoothness constraints in Astron. Astrophys, **144**, 381–387, 1985); if the background profile is biased, the solution vector will be biased. However, the degree of bias will (once again) depend on the averaging kernel of the MIPAS retrieval. It may be that the observations are given so much weight in the height intervals of interest that biases in the final solution are not very sensitive to biases in the a priori. This should be investigated further, before MIPAS and CCR biases can be regarded as “entirely independent”. The current explanation is not sufficient.

Section 4.1.

If the biases in the GFZ - CCR retrievals are caused by the GFZ retrievals being attached to the ECMWF a priori more strongly, why are these biases bigger than the CCR - ECMWF biases (Fig 7)? This suggests that other factors are biasing the GFZ data. Please consider.

The “1 to 2 K” standard deviation below 26 km seem large - any explanation?

Technical Corrections

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