Atmos. Chem. Phys. Discuss., 14, C6807–C6812, 2014
www.atmos-chem-phys-discuss.net/14/C6807/2014/

Atmospheric Chemistry and Physics Discussions

Interactive comment on "Overview and sample applications of SMILES and Odin-SMR retrievals of upper tropospheric humidity and cloud ice mass" *by* P. Eriksson et al.

Anonymous Referee #2

Received and published: 9 September 2014

General:

The paper describes a new dataset of ODIN-SMR and SMILES retrievals of relative humidity and cloud ice mass. An equivalent retrieval scheme has been applied for both instruments and comparisons between the results of both seem consistent. However, there are still issues to be solved, especially for cases of high relative humidity and in case of higher values of cloud ice mass where the datasets deviate strongly from CloudSat observations.

The paper is well written but, as detailed below, lacks clarity in some cases. Further,

differences are often explained by a shift towards the a-priori where I would expect a more physical explanation.

Specific comments:

Abstract, P20946L14, 'However, this "all-weather" capability allows a direct statistical comparison to model data, in contrast to many other satellite datasets.' Conclusions, P20973L8, 'Accordingly, the retrievals can be classified as "all-weather" and averaged values can be directly compared to means derived for e.g. an atmospheric model, which is in contrast to many other satellite retrievals.':

I do not agree with the authors in this respect: what do you really gain by comparing model results to measurements which are influenced by a-priori assumptions? In the extreme case of no measurements at all, this would mean that you could still have new insights by comparison with only the a-priori, which is certainly not true. I have the impression that the problem with the applied method is that one cannot quantify how strongly a certain result is influenced by the a-priori (which is e.g. possible with methods based on optimal-estimation).

Abstract, P20946L26, 'This deviation is caused by the fact that different particle size distributions are assumed, and an influence of a priori data in SMILES and Odin-SMR retrievals.':

As discussed below, especially the second part of this explanation is not really convincing. I think that a physical explanation for this is missing and, thus, the sentence should not be formulated as strong as it is now.

P20946L25, 'the agreement to Aura MLS humidity data is good.'

This is a pure qualitative statement which provides no real information. Could this be formulated more quantitatively?

P20948L15, 'These retrievals change character around a tangent altitude of about 10 km':

Could this change be explained more clearly? Is it because below this altitude the limb-views become opaque and one observes more like an extinction spectrum than limb-emission?

P20949L2, 'Hence, seasonally averaged diurnal cycles can be obtained by SMILES.'

As I understand the SMILES observations, this is not really true: there is no temporally homogeneous sampling of the all local times but a slow variation of the local times during a season.

P20954, P20957L18, P20958:

Could you give an estimation of the influence of interfering spectral signatures from other trace gases (e.g. ozone) on the retrieval results? How large is the error of any assumptions on those gases? Further, how large is the effect of the modelling of the water-vapour continuum?

P20955:

While it is explicitly stated what are the elements of the measurement vector y, the exact elements of x are not easy to get from the paper. Could this be summarized clearly at some point?

P20958, chapter 3.5:

Water vapour vmr-values are introduced here but no more discussed in the rest of the paper in any comparison. Thus, this is merely a technical information and has no use in the scientific framework of an ACP-paper. Thus, in my opinion it should be skipped.

P20958L10, 'Relative humidity has lower (relative) errors thanks to a lower influence of temperature uncertainties.':

Why is this the case? Does the temperature (from ECMWF) not play a significant role in the error assessment when relative humidity is calculated?

P20958L10, 'The errors are somewhat larger than for SMILES.':

Could you state this more quantitatively?

P20958, 'Error estimation':

Could you give a value on the altitude resolution of the relative humidity product?

P20959L26: 'A third error comes from the a priori usage which biases the retrieval towards the a priori mean. . .'

How has this been estimated and what is the reason for the strong effect above 1000 g/m2? Saturation?

P20960L14: 'while the dynamic range corresponding to cloud scattering is $\sim$100 K':

Is the observed extinction really only due to scattering? How strong is the contribution of absorption (from an estimate based on Mie-calculations, I would assume about 10% for 100 um particles)? If my estimation would be right, you could also 'see' small ice-particles by volume-absorption with a dynamic range of $\sim$10 K (if all ice-mass would be present in the small particles). However, the presence of such particles might not be distinguished from enhanced relative humidity by the retrieval.

P20962L3, 'The secondary peak in the SMILES and SMR PDFs around 85 %RHi corresponds to observations affected by cloud scattering.':

The retrieval process is still not clear to me: does the database used for the retrieval of RHi only contain cloud-free cases or also cloudy ones? The secondary peak seems to indicate that the retrieval cannot distinguish between cloud-affected measurements and cloud-free ones i.e. that the variables relative humidity and ice-mass are highly correlated. If this is the case, it should clearly be stated in the paper. However, in that case the statement 'rely on a-priori information' (P20962,L5) seems not adequate since the retrieval 'thinks' that there is information in the measurements and increases the water vapour to compensate for the large extinction which is in reality due to ice

particles.

P20964L15, ‘An analysis of top-of-the-atmosphere radiative impacts would likely find the more frequent low pIWP range most important.’

This speculation should be explained a bit further.

P20965L3, ‘The remaining factor, also $\sim$1.5, is mainly related to the lower PDF of SMILES and SMR for pIWP260 hPa above $\sim$500 gm$-$2.’:

I don’t understand this explanation: is the ‘different PSD-explanation’ not also partly responsible for the lower PDF above 500 g/m2? In that case the two factors 1.5 cannot just be multiplied and explain the factor of 2.3 higher pIWP by CloudSat. I also doubt the explanation ‘by a similar bias towards the a priori mean a noted for %RHi’ (P20965L5). As I understood, the RHi bias is due to the difficulty to separate signals of RHi from those of clouds. However, in case of clouds with high pIWP, the extinction signal is so strong, that it should be difficult to be biased by the a priori.

P20967, chapter 4.3:

Why are only 6h running means shown for SMILES? Would e.g. 3h means not be interesting for indication of further features? Further, in any case, not only the means but also the variability around the means should be given.

Figure 4:

What is the reason for the horizontally layered structures in Odin-SMR? Is this some sampling issue?

Technical comments:

P20948L29, ‘a instantaneous’:

-> ‘an instantaneous’

P20954L17, ‘a constrain’:

-> ‘a constraint’

P20960L6, ‘2103’:

-> ‘2013’

Figure 1:

Are these measurements or simulations?

Figure 13:

In the legend for CAM the symbol should be open instead of solid.

Interactive comment on Atmos. Chem. Phys. Discuss., 14, 20945, 2014.