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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.

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An updated version of the manuscript is provided as supplementary material, where new text is marked with red font colour.

Anonymous referee #1

We thank the referee for the nice opening comments, pointing out the value of our efforts. We are especially happy to see that the relevance for ICI is noticed.

=== Referee comment ==
The method to handle the beam filling effect should be better described. The role of

the CloudSat observation is very ambiguous. How does it provide the 3D structure?

The CloudSat observations are not coincident with the limb sounder measurements. Can you explain how the CloudSat observations are used to describe the statistics of the 3D cloud structure? Statements such as l10-11 p 20954 should be explained. It is specified that CloudSat retrievals are not involved, but l 21 to 27 p 20954 look like the description of a retrieval, although rather simple. This needs clarification.
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In order to keep the manuscript relatively short, we avoided to repeat too much information found in RY09:

Rydberg, B., Eriksson, P., Buehler, S. A., and Murtagh, D. P.: Non-Gaussian Bayesian retrieval of tropical upper tropospheric cloud ice and water vapour from Odin-SMR measurements, Atmos. Meas. Tech., 2, 621–637, doi: 10.5194/amt-2-621-2009 , 2009

and we refer to that paper also in several places to remind about this. For a detailed discussion we feel forced to still refer to RY09, but new text has been added (in Sec 3.2) to hopefully clarify the issues raised in a broad manner.

=== Referee comment ==
Section 3.6.2 presents an error analysis of the partial IWP. A reference is given but a few sentences on the way these errors are estimated would help. The errors are given in percentage. Can you specify what you mean?
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A clarification has been added.

=== Referee comment ==
The diurnal variations of the two variables are presented in section 4.3. Only one season is observed with SMILES. Do you expect changes in the diurnal cycle between seasons? I assume the model outputs are also only

considered for the same season. Can you confirm? Could you briefly check the differences in diurnal cycle between the seasons from the models?
==

The model data are matched seasonally with SMILES. This is now made clear in the manuscript. Unfortunately, we have just stored the model data that are needed for the figures, and we can not easily check differences between seasons.

=== Referee comment ==
The differences between the satellite estimates are very large (Figure 7). It is difficult to agree that there is a general good agreement in the spatial distribution. The spatial distribution as well as the magnitude of the ice contents are very different. Can you elaborate on these differences?
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For most quantities the differences would be remarkable, but for cloud ice mass retrievals this must still be considered as a satisfactorily agreement. For example, compare to the differences in IWP found in Fig. 3 and 4 of Waliser et al., Cloud ice: A climate model challenge with signs and expectations of progress, JGR, 2009. Or Fig. 5 of Eliasson et al., Assessing observed and modelled spatial distributions of ice water path using satellite data, ACP, 2011. Roughly, observations can differ with at least a factor 5 between mean tropical IWP, and models even more. As well as that the spatial distributions often disagree in remarkable ways.

We can also refer to Sec. 4.2.3, showing that other retrievals for the same SMILES data are roughly a factor 3 lower compared to our retrievals. Having this in mind, we are happy to just be a factor 2-3 lower than CloudSat, and that for reasons we understand quite well.

Regarding our Fig. 7, our opinion is that the spatial distribution of SMILES and CloudSat match closely. Odin-SMR deviates for reasons discussed (ENSO fluctuations). The fact that SMR and SMILES are a factor two lower than CloudSat is clearly expressed.

In fact these difference are stressed before we comment on the agreement (p20965, line14-16): "Beside these aspects, there is a good agreement in the geographical distribution ..."

=== Referee comment ==
The satellite retrievals presented in this study have little sensitivity to particles smaller than 100microns (l 24 p 20970). Ice clouds in models are often assumed to have radius of the order of 30 microns in climate models, and as mentioned l10-11 p 20972, the ice from the model only considers the small ice particles. So how can those two values (model and satellite) be compared? Can you clarify?
==

The simple answer would be that we are just doing what everybody else is doing! For example, this issue is not resolved in Waliser et al (2009) or Eliasson et al (2011). In fact, we have gone further than most in some of our papers. Model "precipitating ice" (PI) was included in ER2010

Eriksson, P., Rydberg, B., Johnston, M., Murtagh, D. P., Struthers, H., Ferrachat, S., and Lohmann, U.: Diurnal variations of humidity and ice water content in the tropical upper troposphere, Atmos. Chem. Phys., 10, 11 519–11 533, doi:10.5194/acp-10-11519-2010, 2010.

as well as in these two papers:

Johnston, M., Eriksson, P., Eliasson, S., Jones, C., Forbes, R., and Murtagh, D.: The representation of tropical upper tropospheric water in EC Earth V2, Climate Dynamics, 39, 2713–2731, doi:10.1007/ s00382-012-1511-0, 2012.

Johnston, M. S., Eliasson, S., Eriksson, P., Forbes, R. M., Wyser, K., and Zelinka, M. D.: Diagnosing the average spatio-temporal impact of convective systems. Part 1: A methodology for evaluating climate models, Atmos. Chem. Phys., 13, 12043–12058, doi:10.5194/acp-13-12043-2013, 2013.

For this paper we have taken the model data from (now accepted)

Johnston, M. S., Eliasson, S., Eriksson, P., Forbes, R. M., Gettelman, A., Raisanen, P., and Zelinka, M. D.: Diagnosing the average spatio-temporal impact of convective systems - Part 2: A model intercomparison using satellite data, Atmos. Chem. Phys., 14, 8701–8721, doi:10.5194/acp-14-8701-2014, 2014.

When discussing with the co-authors to that paper, it became clear that it was not possible to include PI in a consistent manner for all participating models, and we together found it best to just include "cloud ice". And this decision then happened to influence this paper as well.

That is, it surprisingly difficult to include model PI in the comparison. However, for this paper we don't think this is a critical point, as we mainly discuss changes compared to ER2010, where a more careful analysis was made and PI was included. As was already mentioned in the manuscript, ER2010 found that adding PI did not change the picture in any important way.

The existing text discusses the problems, see start and end paragraphs of Sec. 4.3.3, but does not enter e.g. the complicated issue of the true size of "cloud ice particles" (e.g. models could assume too small particles, as old in situ measurements were affected by shattering). We found it simply out of scope with a review of the background problems. Anyhow, the main point is that in terms of ice mass, "cloud ice" still seems to dominate inside the models (for the altitude range of concern, the reversed at lower altitudes), adding the precipitating part does not strongly change the picture. This was at least found in our others papers, that we also cite in the end paragraph.

=== Referee comment ==
The main drawback of the SMILES retrieval is said to be the limited spatial coverage (l25 p 20973). However, in section 2.1, it is specified that SMILES covers up to 65 N. Why is the retrieval limited to 30 N?
==

Our methodology works the best when there is some distance between the tropopause and the sounding altitude (to have a clear and high lapse rate). For this reason, we only process data out to +-30deg in latitude. This is explained in

Ekström, M., Eriksson, P., Rydberg, B., and Murtagh, D. P.: First Odin sub-mm retrievals in the tropical upper troposphere: humidity and cloud ice signals, Atmos. Chem. Phys., 7, 459–469, www.atmos-chem-phys.net/ 7/459/2007/, 2007.

Finally, "minor comments" are fixed, see revised manuscript. Thanks for pointing out these errors.

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Anonymous referee #2

We thank the referee for very useful comments, that for sure are worth consideration. We have done our best to answer and react on the comments, but on the same time we would like to mention that these comments mainly refer to the retrieval methodology and set-up. These questions were not our focus in this manuscript. We see this as an application of an existing retrieval algorithm on a new dataset (SMILES) and an initial analysis of the obtained data.

If no information is given, full article references are found above.

Several comments refer to "a priori influences". For RHi we think our analysis holds where this explanation is given (but see also first answer below). For example, the precision for RH in fact increases when going towards very low values (see Ekström et al., 2007), and the high bias of SMR and SMILES in dry regions is clearly an a priori issue, caused by the fact that the retrieval database has too few cases below 20 %RHi.

On the other hand, for pIWP we now realise that there can exist "saturation effects" in ways we have not understood previously and the manuscript is changed accordingly.

More comments on this issue below.

=== Referee comment ===
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).
==

First of all, this point refers only to the RH retrievals, and then a bit less than 10% of the data points. Further, at least for this application it should be possible to derive something that would indicate the "measurement response". In any case, we have no problem of identifying the data points of concern, and could easily remove them if we had thought that it would be beneficial.

To be clear, our comment in the abstract refers to that we set a RH value also for the measurements where cloud influences remove all measurement information on water vapour. Let us here comment another question of the referee (P20962L3). A single database is used to retrieve RH and pIWP, that is, "cloud free cases and cloudy ones" use the same database (commented added below Eq. 2. to clarify this). Hence, we don’t assign a hard-coded value to RH in cloudy cases, the retrieved RH is a result of the database. The result is the mean RH, in and around clouds, among the cases in the

database matching the measured radiances. A standard OEM inversion would give a similar result, if the a priori value was adjusted following the fact that the measurement shows signs of cloud influence.

The referee is correct that there is a limit when the retrievals should be disregarded, long before there is no measurement information at all. However, we still think our approach simplifies the comparison to model data, when a limited part of the data are affected. For example, it is well known that AIRS underestimates the mean RH in cloudy regions (see e.g. Johnston et al, 2012, cited above), as data are missing for the more humid cloudy regions. A direct comparison between AIRS and model data can then only set a lower limit for the model’s mean RH, but AIRS low bias is of such magnitude that this makes the exercise quite meaningless considering the quality of the model today (which explains the development of "satellite simulators"). With our approach we try to give an unbiased estimate of regional mean RH, but the partial lack of measurement response should of course be considered.

Ending up here we realised that we had missed to give any value on the associated error, and such a discussion has now been added to the manuscript. This discussion is based on the fact that ECMWF shows a similar PDF peak (that also gives some confidence in that we provide acceptable data also for cloudy observations). Text has been added to Secs. 4.1.1 and 4.1.2.

=== Referee comment ===
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.
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Changed, see initial comment.

=== Referee comment ==
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? ===

Yes, not very informative! A quantitative value has been added.

=== Referee comment ==
P20948L15, ‘These retrievals change character around a tangent altitude of about 10 km’ ===

The text has been expanded already in response to referee =1.

=== Referee comment ==
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. ===

Yes, correct. This is why we wrote "seasonally averaged ..." and not only "diurnal cycles". And some lines above we wrote "While this does not give a instantaneous full diurnal coverage ...". Hence, we don’t think we give the wrong impression here, the caveat of concern is explained.

=== Referee comment ==
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? ===

This information is found in Ekström et al [2007] and Eriksson et al [2007] for RH and ice mass, respectively:

Eriksson, P., Ekström, M., Rydberg, B., and Murtagh, D. P.: First Odin sub-mm retrievals in the tropical upper troposphere: ice cloud properties, Atmos. Chem. Phys., 7, 471–483, www.atmos-chem-phys.net/7/ 471/2007/, 2007.

The impact of interfering gases such as ozone is negligible compared to other errors. A 30% error in water vapour continuum gives an error of 3.9 to 0.6 %RHi (decreasing with altitude).

We understand the problem for the reader of having essential information about the retrievals spread out over several articles, but we don’t think that this article is the place where to solve this problem. Instead, we are planning to repeat the complete error characterisation as part of the next version of the retrieved data.

=== Referee comment ==
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? ===

Text has been added to Sec 3.2 to clarify the content of x (partly based on existing text taken from removed section on VMR, see below).

=== Referee comment ==
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.

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This section is now removed. First paragraph of conclusions modified to inform data users that there also exists a VMR version of the datasets.

=== Referee comment ==
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?
==

This text was found in a section now removed (see point above) and no change has been made. However, our latex file included in fact an explanation, but eventually we did not include it for reasons of brevity. To give a reply to the question we include that text here:

A way to understand the difference in temperature interference is the fact that, for the low tangent altitudes considered here, the SMILES and SMR retrievals resemble the ones discussed in \citet{buehler2005simple}. They showed that brightness temperature can be mapped to relative humidity, quite accurately, for instruments of AMSU-B type without involving any temperature information at all. However, the obtained \rhi\ is not for a fixed altitude, but follows the “weighting function”. For SMR and SMILES we aim for data at fixed pressure levels, which increases the sensitivity to temperature, but it is still smaller for \rhi\ than for retrievals targeting VMR.

=== Referee comment ==
P20958L10, ‘The errors are somewhat larger than for SMILES.’: Could you state this more quantitatively?
==

As above, no longer valid as the text is removed from the manuscript.

=== Referee comment ==
P20958, ‘Error estimation’: Could you give a value on the altitude resolution of the relative humidity product?
==

See text added to Sec 3.6.1.

=== Referee comment ==
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?
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The error was estimated in

Kasai, Y., Rydberg, B., and Möller, M.: Retrieval theoretical basis of NICT/SMILES level-2 products: upper tropospheric cloud ice mass and water vapor, Tech. rep., National Institute of Information and Communications Technology, Tokyo, Japan, 2014.

In our simulations we see, on average, a linear relationship between log(pIWP) and change in Tb, dTb. This means that e.g. each extra 100g/m2 gives a smaller change in dTb, but we see no obvious saturation. However, this could be an artefact of the simulation set-up, and there could exist a saturation in the observational data. The PSD assumption can be involved, as correctly pointed out by the referee in a later comment. We have assigned relatively small errors to our assumption of solid ice spherical particles, but we now suspect that the error due to this assumption is probably much larger at high pIWPs.

In short, it is likely that we underestimate the asymmetry parameter for high pIWP, and this results in an overestimation of dTb (tested by simulations). This became clear while working on an article manuscript where we compare solid sphere and the "soft particle" approximations with DDA calculations of more realistic particle shapes. One thing stands out clearly, that solid ice particles tend to underestimate the asymmetry parameter for size parameters above 2. That is, our simulations underestimate the increase of the asymmetry parameter with pIWP, while in the real world this relationship between pIWP and asymmetry parameter could cause a saturation in dTb.

In summary, it is likely that there exists saturation effects in the measurements that we so far have missed to capture in our simulations. We now understand this better, but it was already envisaged that these were main points for the next version of the retrievals. As we wrote at the end of Conclusions: "The main points for improvements are then: revise the PSD assumptions, to use single scattering properties for more realistic ice particle shape(s), ..."

=== Referee comment ==
P20960L14: 'while the dynamic range corresponding to cloud scattering is 100 K':

Is the observed extinction really only due to scattering? How strong is the contribution of absorption ... ==

Based on discussion in

Eriksson, P., Rydberg, B., and Buehler, S. A.: On cloud ice induced absorption and polarisation effects in microwave limb sounding, Atmos. Meas. Tech., 4, 1305–1318, doi:10.5194/amt-4-1305-2011, 2011b.

we guess that the impact of absorption is probably very low, but not zero. So the statement was wrong. Accordingly, we have changed "scattering" to "extinction".

=== Referee comment ==
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: ...

==

See answers above.

=== Referee comment ==
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. ==

This comment is based on unpublished work, done in another research group, and we can not back it up properly now. Anyhow, the comment was a side-track and is now removed.

=== Referee comment ==
P20965L3, 'The remaining factor, also 1.5, is mainly related to the lower PDF of SMILES and SMR for pIWP260 hPa above 500 gm2.': I don't understand this explanation ==

The first factor 1.5 refers to the tropical mean pIWP. We have not derived this factor for different ranges of pIWP, and the referee is right in that this difference to CloudSat probably mainly originate from deviations at high pIWP. But this does not explain the complete difference of concern, and the remaining factor seems also to be linked at effects at high pIWP. But as commented above, we now also consider saturation beside a priori issues. And text adopted accordingly.

=== Referee comment ==
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. ==

We decided to go for 6h running means due to two facts, that the size of the dataset is relatively small and that only seasonally averaged diurnal cycles can be obtained by SMILES (as discussed above). The impact of both these facts increases when decreasing the time averaging. We don't say that it is impossible to obtain information from SMILES for e.g. 3h running means, but this requires a more careful analysis of e.g. the impact of single very high pIWP. In short, how many measurements are required to obtain stable statistics for the different regions? This is a question we would like to answer, but is out of scope of this paper. And as written in the first sentence of the abstract, we are here only providing "example applications". We hope that others, or we if time permits, will apply these retrievals for more detailed analysis.

We don't see which variability that we could add. We don't think that the variability of individual measurements is of interest here. Anyhow, that variability is far from Gaussian and is not easily captured in a simple way. For example, the median pIWP is 0. This variability is best reported as the complete PDF, as done in Fig 5. We can't derive any daily regional mean values, the sampling of SMILES is just too coarse for that. Regional mean values are only stable when averaging at least months of data, and we can just estimate the mean seasonal cycle, not any higher order statistics.

=== Referee comment ==
Figure 4: What is the reason for the horizontally layered structures in Odin-SMR? Is this some sampling issue?
==

A very good observation, we had not noticed that. Yes, this must be a sampling issue. The following text has been added: "Some zonally aligned structures can also be discerned for SMR, such as a stripe of low values across Africa. This should be a consequence of the fact that Odin's scanning does not a have fixed latitude pattern and the sampling frequency of a region can vary between seasons and years."

Finally, "technical comments" are fixed, see revised manuscript. Thanks for pointing

out these errors.

Please also note the supplement to this comment:
http://www.atmos-chem-phys-discuss.net/14/C8114/2014/acpd-14-C8114-2014-supplement.pdf

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