Atmos. Meas. Tech. Discuss., 8, C2871–C2875, 2015
www.atmos-meas-tech-discuss.net/8/C2871/2015/

Atmospheric Measurement Techniques Discussions

Interactive comment on "Global cloud top height retrieval using SCIAMACHY limb spectra: model studies and first results" *by* K.-U. Eichmann et al.

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Received and published: 7 September 2015

Global cloud top height retrieval using SCIAMACHY limb spectra: model studies and first results — Eichmann et al.

Introduction

The authors have presented work associated with the SCIAMACHY limb spectra collected between 2002 and 2012. Within this work they have presented: a simple technique to determine cloud top heights from the SCIAMACHY measurements; results from a brief simulation study that highlight issues associated with the technique as applied to the SCIAMACHY measurements; and some preliminary cloud top height results derived from the SCIAMACHY time series.

The paper has demonstrated that there remains significant work to be done before the

SCIAMACHY cloud top height data set can be used for detailed scientific study but it has also demonstrated the technique has potential. The problem with the technique is its simplicity. I can be easily convinced that it works nicely to flag cloud top heights for the purpose of retrieving trace gases with limb spectra. It is also easy to see how the simplistic choice of an arbitrary threshold like 1.4 can be used for the detection of polar stratospheric clouds with SCIAMACHY spectra as these clouds have a limited range of physical and optical properties and are measured by SCIAMACHY over a limited range of solar and scattering angle conditions. However, it is definitely not a straightforward thing to see how the simplistic choice of the threshold of 1.4 can be reliably used over all SCIAMACHY geometries and over all the different types of clouds and aerosols that are seen by the instrument. The paper falls short in that it leaves the reader with more questions about the first results than answers provided by these results.

I believe this technique and the simulation studies have merit and should be published within AMT in a revised paper that focuses much more on the systematic biases associated with the simplistic technique and much less on the first results. I actually think the authors have done most of the work but failed to present it in a clear fashion within Section 4. The authors should lengthen Section 4 such that the results that are simply stated are fleshed out with extra text and figures. This change in focus of the paper would give the reader much more confidence in any results that are presented in future works.

If I am correct and the authors have done an extensive range of simulation studies then it should not be too much effort to include them within the paper and what appears to be major revisions are actually only minor. I would be happy to look at this paper again after these changes have been made.

Major comments

The authors need to better explain all of the features seen in both the spectra (Figure 3) and the parameters derived (Figure 5) from this limb scan. My first look at the

spectra shown in Figure 3 led me to believe that there were two clouds present in this scan. The closeness in magnitude of the spectra measured at 18.68 km and 15.43 km naively indicate a scattering layer not consistent with a Rayleigh background atmosphere. Then of course the behaviour of the spectra measured at the lowest tangent altitudes definitely indicates a cloud. The CIR and CI clearly indicate a cloud at 12.13 km, using a threshold of 1.4, but to a naïve reader the behaviour of the CIR and CI might also indicate a cloud one tangent altitude higher. This simple analysis that was supposed to convince the reader that the technique works raised questions about how many cloud tops it was missing. More explanation is required.

I had difficulty understanding why the authors included the discussion on limb optical thickness (page 8308) and horizontal sampling (page 8314). I believe this is to setup the comparison with nadir measurements that is done later in the paper. However, it was not clear that this was the case and I feel these sections need some work to better clarify their usefulness.

As mentioned above the presentation of the simulation studies was not convincing. The following three points need to be addressed to increase reader confidence in the final results.

1) I believe that SCIATRAN is an excellent radiative transfer model and can be used for a variety of simulations. However, within section 4 the authors did not convince me that SCIATRAN has the required accuracy to effectively analyse the systematic biases associated with choosing a threshold of 1.4 for the entire SCIAMACHY data set. This is not an indictment of SCIATRAN but a statement related to how the authors presented it as a tool. The only statement of SCIATRAN's accuracy comes on line 11 of page 8310 and this is a general unconvincing statement that referred to a study presented in 2003 and is valid only for clouds of optical depth greater than 5. I believe SCIATRAN is up to the task, the authors just need to better demonstrate its utility for this particular problem. This can be done through more references and specific simulations of relevant SCIAMACHY limb spectra. 2) The discussion around Figure 6 was somewhat

confusing. I believe the authors were attempting to demonstrate that the two specific clouds they used for their simulations are detected with SCIATRAN simulations for some subset of SCIAMACHY viewing geometries. This needs to be tightened up and more comprehensive to help interpret the results. 3) I believe the paper will be much better if many more simulation results are included that better illustrate the points the authors are trying to make on pages 8312 and 8313. On these pages a subset of extra results are mentioned, but not in a fashion that gives the reader enough information to accurately assess the systematic biases associated with the results that are presented later in the paper.

Minor comments Line 12 page 8296: "scattering of . . ." needs to be changed

Throughout the paper the notation tau_N (read as tau subscript N) and many other tau_X's are used. I think N here refers to nadir but I'm not certain. Also, sometimes no subscript is used. This needs to be tightened up.

Line 4 page 8297: Nabro is later mentioned to have erupted in June, 2011

Line 5 page 8297: Is "particle top height" a good term to use?

The expression "in the order of" appears in a few places. This should be "on the order of".

On line 23-page 8298 a cloud with an optical depth greater than 0.3 is called opaque while on the next page, line 13, a cloud with an optical depth less than 1 is called thin. I think I understand that these are different types of clouds but how can one be thin and the other be opaque if they have the same optical depth?

Line 8-page 8300: I believe the authors meant the SAGE instruments.

Line 5-page 8303: The blue numbered circles do not give the tangent heights of SCIAMACHY. This needs to be reworded.

I believe that the tangent height knowledge mentioned on page 8304 is for the occul-

tation mode and the 50 m accuracy is not relevant for the limb scatter work presented here.

The word “colour” is used sometimes and “color” shows up at other times in the figures.

Line 6 page 8308: cloud index should be colour index.

In Figure 5 the order of plots a) and b) should be changed as a) is derived from b).

In Figure 6 it needs to be explained that T_C and T_A in the plot are the taus referred to in the caption and that these subscripts are different within the text. Also, a colour scale would be nice.

Summary

I think that this paper will be suitable for publication in AMT once the simulation studies are presented in a more comprehensive fashion. I will be more than happy to look at this work again once this has been completed.

Interactive comment on Atmos. Meas. Tech. Discuss., 8, 8295, 2015.