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Atmospheric Measurement Techniques Discussions

Interactive comment on "Assessing the Impact of Clouds on UV-visible Total Column Ozone Measurements in the High Arctic" *by* Xiaoyi Zhao et al.

Anonymous Referee #1

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The paper by Zhao et al. presents a cloud detection method for ground based zenith sky DOAS measurements. One particular advantage of this method is that it can be applied to measurements at high latitudes, for which the range of SZA is typically limited. The authors then use their cloud classification results to test the impact of clouds on measurements of the total ozone column. The cloud effect is quantified by comparison to a) direct sun Brewer/Dobson measurements and b) to model results. The authors find that excluding cloudy data reduces the uncertainties of the ozone measurements. This paper can be a potentially important paper for many groups performing and analysing zenith sky DOAS measurements. However, in its current form, many aspects are not (well) addressed, and also several findings need further clarification.

The paper needs major revisions, which are detailed below.

Major points:

A) I think the logic of the paper needs to be changed. In its current form the authors develop their cloud detection method and then directly apply it for the selection of the ozone measurements. In addition they use meteorological observations of cloud properties for the further assessment of the cloud effects on the ozone measurements. In my opinion, the first logical step after the development of the cloud classification algorithm would, however, be to compare the results of the cloud classification algorithm to independent cloud observations (e.g. meteorological observations) in order to validate the new algorithm. After successful validation, the algorithm could be applied to the ozone measurements.

B) The choice of the wavelength pair for the calculation of the CI is not be well justified. The authors write 'The 450/550 pair was found to be the most reliable one for the ZS-DOAS instruments used in this work'. It should be made clear in which respect the new choice is better over the other suggested wavelength pairs. (and what is meant with 'reliable'?) In my opinion the new pair is probably even problematic, because the measured radiance at 550 nm strongly depends on the ozone content (at high SZA). Assuming e.g. an ozone VCD of 300 DU and an AMF of 10, this results in an ozone optical depth at 550 nm of about 0.28. This can have a significant effect on the CI and makes the CI dependent on the ozone amount.

C) The effect of the surface albedo is not discussed. I would expect that it has a systematic influence on the CI. At high latitude sites the surface albedo changes strongly over the year. The authors should discuss this effect and should explain how they deal with the variability of the surface albedo.

D) The SZA dependence (e.g. in Fig. 2) of the model simulations and the measurements is very different (especially for the minimum values). The authors should discuss possible reasons for these differences (maybe related to change of albedo during the year)? Also information on the input for the model simulations should be given, especially the ozone VDD and the surface albedo used for the simulations. In Fig. 1 several jumps are seen for the simulation results. What is the reason for these jumps? They seem to be not realistic.

E) In Fig. 1 it is seen that high clouds can have very similar CI as clear sky observations. The authors should check if this result is reasonable. If this simulation results are correct, I have some doubts about the ability of the algorithm to detect high clouds. These clouds might have a considerable effect on the ozone measurements.

F) The authors skip individual measurements, which are indicated as cloudy. I am not sure if this is a good procedure, because it leads to a variable selection of measurements (different numbers, different SZAs), which can have a systematic effect on the derived average ozone results. Also, if only a small number of measurements remains, the total uncertainty might increase. The authors should investigate how the selection of measurements affects the derived average O3 VCD. What is the minimum number of required measurements in a sequence? There is another, related point: it is written, that in some cases the SZA range of the selected measurements is shifted from the standard range (86-91°). How large is the maximum shift of the SZA range? For which situations is a shift applied? How does the shift affect the ozone results?

G) The effect of instrument degradation should be addressed. The authors write that in particular the differences in the calibration for the GBS instrument might be related to instrumental changes. The occurrence and strength of changes in the instrumental properties should be stated. Also gradual long term degradation should be investigated.

Minor points:

1) Can the authors explain, for which atmospheric conditions measurements fall into the category 'intermediate'?

2) In several parts of the paper, the cloud effects are referred to as 'random', e.g. in the abstract. In other parts, e.g. on page 3, line 17 it is stated that 'This leads to a random uncertainty of 3.3% for TCO calculated using the NDACC ozone AMF LUT between 86-91° SZA.' Then in the next sentence it is written 'In fact, clouds are the largest source of random uncertainty in ZS TCO.' In my opinion, cloud effects are systematic. Of course, depending on the cloud type, they might have different effects on the derived O3 VCD. Thus they can indeed introduce a random component. The authors should discuss these aspects in more detail. They also should be clear whether cloud effects are random or/and systematic.

3) In Fig. 3 the fitted curve seems to be not a pure Gaussian. Please provide details of the applied fit function.

4) Fig. 3: which SZA are included in these results?

5) Title: maybe add 'ground based' between 'on' and 'UV'?

6) Introduction: on page 3, lines 9-10, also the following reference might be included:

Erle F., Pfeilsticker K., Platt U, On the influence of tropospheric clouds on zenith scattered light measurements of stratospheric species, Geophys. Res. Lett., 22 , 2725-2728, 1995.

7) On page 5 it is written: 'Due to the decreased resolution at the edge of CCD, the ozone differential slant column densities (dSCDs) were retrieved in the 450-545 nm window, instead of the NDACC recommended 450-550 nm window.'

The Chappuis ozone absorption has no fine spectral structures. Is a high spectral resolution really needed for the ozone analysis in the visible? Maybe the NDACC window can still be used?

8) Fig. 2: Why have measurements for SZA > 85° been removed?

9) Section 3.2: How do the results based on the temporal variation agree with the

results derived from the CI threshold method?

10) Section 3.3: It is written that ‘The inclusion of ozonesonde data in the AMF calculations improves the results, especially under vortex conditions (Bassford et al., 2001).’

This statement is unclear to me. Is the use of ozone sonde data an addition to the existing NDACC LUT? Is the original NDACC LUT used in this study or and updated LUT?

11) Page 14, line 18: It is written: ‘Theoretically, the cloud-screened TCO datasets (GBSCS and SAOZCS) should have lower random uncertainties than the conventional TCO datasets (GBS and SAOZ).’ I am not sure about this statement. One general effect of the cloud filter is that it removes measurements of a sequence. Thus the information content should be smaller than for a complete sequence. Also the selection of measurements becomes variable: e.g. on some days measurements for small SZA, and on other days large SZA might be filtered. This will lead to different biases and probably to an increased ‘random’ uncertainty.

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