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Atmospheric Chemistry and Physics Discussions

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Interactive Comment

Interactive comment on "Dependence of cloud fraction and cloud top height on surface temperature derived from spectrally resolved UV/vis satellite observations" *by* T. Wagner et al.

T. Wagner et al.

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Anonymous Referee #1

The topic of the paper is relevant to the climate-cloud feedback discussion, and fits well in ACP. The correlation of cloud data from oxygen absorption measurements by satellite with surface temperature data is new and contributes independent information to the study of climate-cloud feedback. The conclusions from the paper are however quite weak. If the following major and detailed comments are taken into account, the manuscript could be accepted.

Author comment: We thank the reviewer for the positive assessment of our study. We agree with the reviewer on most of the suggestions and explain in detail below our responses to these suggestions.

Major comments: 1. The paper describes an effort to correlate monthly cloud observational fields from GOME (CF and CTH) with surface temperature (ST) fields, and contains many correlation figures. The spatial correlation patterns between CF and ST are at some places strong. However, there are several figures which do not show a clear result. For example, the spatial correlation plots of Fig. 11-13 are not convincing. They do not show a clear seasonal cycle and contain much noise. Please remove Figs. 11-13, or condense them. The paper should be more focussed and only show clear results.

Author comment: We agree and removed Fig. 11-13.

2. The CTH is derived from the O2 absorption measurements and CF, as described in Sect. 3. The logical expectation of the reader is that in the remainder of the paper only CF and CTH are discussed. But this is not the case. In Sect. 4 the authors jump back and forth between discussing CTH and O2 absorption: Fig. 7 shows O2 absorption, Fig. 8 shows CTH, Fig. 9 shows O2 absorption, and Fig. 10 shows CTH. This is confusing. Since the O2 absorption is an intermediate quantity for retrieving CTH, it should not be shown so many times; Fig. 3 suffices. Only CF and CTH should be discussed. Both are independent cloud quantities. Also the title of the paper only mentions CT and CTH. Therefore, in Fig. 7 (bottom panel) O2 absorption should be replaced by CTH, and Fig. 9 can be removed.

Author comment: We agree that our way of presenting was confusing. The reason why we showed the correlation plots for the O2 absorption was that it is the directly retrieved quantity from the GOME spectra. If only the results for the CTH are shown, it might not be clear how much of the observed relationship between CTH and ST is caused by changes in CF or O2 absorption. For that reason, in the revised version we kept the lower panel of Fig. 7 (new Fig. 9) unchanged. This figure shows in particular that the spatial patterns of the CTH-ST relationship are dominated by the correlation of

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the O2 absorption and ST. We removed Fig. 9 to make the presentation more clear.

3. The paper states that models should reproduce these observational results. This seems a too strong statement for results which are not always clear and convincing themselves. It is a pity that the authors do not show any model results. This would have strengthened the paper. The analysis does not go beyond the correlation, which is merely sufficient.

Author comment: We agree that having shown model results would have strengthened the paper. However, as we are experts for satellite data analysis, our major aim was to present these novel experimental results to the scientific public. We hope that our results stimulate modelling scientists in trying to reproduce our results. As suggested by another reviewer, we added recommendations for the development of a GOME simulator from model results (new section 3.3). A GOME simulator would allow a much more direct comparison between model results and measurements. In particular, the detailed vertical cloud structure could be considered.

4. From the description of the CTH algorithm, it seems that the algorithm is missing two important processes: Rayleigh scattering and surface elevation. Is this true? If so, a quantitative estimate of this neglect should be given in Sect. 4.2.

Author comment: Rayleigh scattering is included in our RTM. To make this more clear we added this information explicitly in section 2.4. Concerning surface elevation it is true that in the current version of our algorithm it is not included (as stated in section 3.3 of the original manuscript). Thus for several regions of the world (with altitude $>$1.2km and CF $<$7%) no meaningful determination of the CTH is possible. These regions are indicated by the gaps in the Figures 6, 10,and 11. We made this more clear in the respective Figure captions of the revised versions. We also pointed out that for continental areas $<$1.2km, still an error remains, especially for small CF. Nevertheless, in this study, we are primarily interested in the correlation of the monthly anomalies, which are hardly affected by these systematic errors. We added this information to the

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text in section 2.4.

Detailed comments Title and related questions

1. The title is not covering the contents of the paper: the dependence of cloud properties on surface temperature is not derived from satellite observations, but from correlation analysis between satellite cloud observations with surface temperature data.

Author comment: We changed the title to 'Dependence of cloud properties derived from spectrally resolved visible satellite observations on surface temperature' to avoid misunderstandings.

2. The surface temperature (ST) data are not described at all. Please devote a subsection to the surface temperature data. Mentioning a URL is not sufficient.

Author comment: We added a new section 2.6 with more detailed information on the ST data set.

3. Why are UV/vis satellite observations mentioned in the title? The UV channels of GOME are not used in this paper.

Author comment: We replaced UV/vis with visible.

4. This brings me to the following question: The O2 absorption band is at 630 nm. The PMD measurements used for cloud fraction determination are probably visible radiation data but this is not specified in the paper. Please describe in Sect. 3 at which wavelength the effective cloud fraction is determined. It should be at a close wavelength to the O2 absorption band, otherwise the two properties should be scaled.

Author comment: The effective cloud fraction is determined from PMD data in the red and green spectral range. From sensitivity tests we found that the cloud fractions determined independently in both PMD channels are almost identical. Therefore we conclude that no scaling has to be applied. We added this information to section 2.4.

Abstract Line 5 mentions the cloud radiative feedback. Line 19 mentions a cloud cli-

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mate feedback. Is this the same feedback? If so, please use the same term.

Author comment: We use the term 'cloud radiative feedback';

Line 23: Please reformulate (see major comment 3)

Author comment: We replaced the original text by 'Climate models should thus aim to reproduce our findings. For that purpose recommendations for a development of a processor to convert model results into the cloud sensitive quantities observed by the satellite are given.';

Section 1 Line 10: Solomon

Author comment: corrected

Line 22: The cloud fraction is an effective cloud fraction. This should be made clear from the start.

Author comment: We added 'effective' to this part of the text and at several other places (including the headings).

Section 2 is too short and can easily be fitted into Sect. 3.

Author comment: We combined sections 2 and 3 in the revised version of our manuscript.

Section 3 Sect. 3.1: Which range does the effective cloud fraction have? Is the cloud albedo used (which cannot be measured by GOME) or the cloud reflectance?

Author comment: We replaced cloud albedo by cloud reflectance. We also added information about the range of effective cloud fractions (0-1) in section 2.2.

Sect. 3.2: which O_2 band is used? Is there no overlap with other gases?

Author comment: The O_2 band at 630nm is analysed. We added the information that 'besides the O_2 absorption also spectra for the absorptions of H_2O and the oxygen dimmer O_4, as well as a Ring spectrum are included in the analysis'. Additional details

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can be found in Wagner et al. [2006a].

Sect. 3.3: What is the unit of O2 absorption? In the figures of O2 absorption there is no unit used, but only a normalized quantity.

Author comment: We added the following information to the end of section 3.2: 'The retrieved quantity of the DOAS analysis is the differential optical depth (DOD) (for the spectral resolution of the GOME instrument) of the O2 absorption at 630nm. With the knowledge of the O2 absorption cross section, the measured O2 DOD can be converted into the O2 slant column density (the O2 concentration integrated along the atmospheric absorption path). In order to avoid any dependence on the actual value of the O2 absorption cross section, in this study we decided to apply a normalisation approach to the retrieved O2 DOD (for details see section 2.4).'

p. 17122: l. 11: Which phase function is used for the cloud particles?

Author comment: We used a Henyey-Greenstein with an asymmetry parameter of 0.85. We added this information to section 2.4.

l. 12-15: Is the normalization also depending on viewing zenith angle and azimuth? Are the observations of O2 absorption normalized to the maximum observed during some period, e.g. per month? Please note that due to the normalization to the maximum, outliers may influence the result.

Author comment: In the original version of the manuscript, we used only one set of maximum O2 absorption as function of the solar zenith angle, that means that we ignored potential effects of the viewing angles. Initiated by the reviewers comments, we investigated the dependence on the viewing geometry (the line of sight angle, and the relative azimuth angle). We found a slight dependence on these parameters (few percent), which is also confirmed by radiative transfer simulations. We decided to completely reprocess our data set with new values of the maximum O2 absorption determined individually for each line of sight angle of GOME. Since the relative azimuth angle for a GOME observation depends on season and location, we determined 6 individual sets of maximum O2 absorptions (as function of the solar zenith angle) for each month, three for the three line of sight angles and 2 for each hemisphere. These sets of maximum O2 absorption were generated by an automated routine using all GOME measurements. By visual inspection of the derived values it was ensured that no outliers affected the retrieved maximum O2 absorptions. We added this information to section 2.4. Using the new maximum values, the retrieved GOME CTH changed slightly. However, the results of our correlation analysis are almost identical.

p. 17123: l. 9: What do you mean with global surface elevation? The surface elevation of the pixel? If the CTH algorithm is not including surface elevation in the retrieval, this should be mentioned earlier. How will this neglect of surface elevation influence the CTH results of Sect. 4?

Author comment: We mention this limitation earlier (in first part of section 2.4) in the revised version of our manuscript. We also added more information on the consequences of ignoring the surface topography at the end of section 2.4. For the results of the correlation analyses, the influence of this limitation can be neglected.

l. 11-12: Which specific agreement is meant? Which differences are found? Please note that the effective cloud fraction is not an ISCCP cloud quantity.

Author comment: We added a much more detailed comparison between GOME and ISCCP cloud properties (new section 2.5). We introduced a comparison of annual and seasonal mean maps of both data sets. In order to make the comparison more meaningful, we developed a correction procedure to convert the ISCCP data products into quantities more similar to the quantities retrieved from GOME. This correction procedure includes radiative transfer modelling of the radiance and O2 absorption using the ISCCP results on cloud amount, cloud presure and cloud optical depth (the surface albedo is also taken into account). Using these conversion schemes much improved agreement is found. We also state at several parts of the text that for the comparison

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of CTH data from GOME and ISCCP no perfect agreement could be expected.

Section 4 p. 17124: l. 14: Which radiative transfer modelling is meant here to determine the relation between CTH and ST? Do you mean thermal IR modelling + convection?

Author comment: We used our radiative transfer modelling approach outlined in section 2.4. To avoid misunderstandings we changed the sentence into : '.using our radiative transfer model results (section 2.4).'

p. 17125: l. 6: please specify if these errors are precisions (random errors) or accuracies (biases).

Author comment: For CF accuracies and for the O2 absorption precisions are given. For the O2 absorption systematic errors cancel out during the normalisation procedure. This information is now added to the text.

l. 20: Do you mean that Rayleigh scattering is not included in the retrieval of CTH? If so, please mention this in Sect. 3, and give here a value of its impact on CTH. This is also related to the impact of the possible neglect of surface elevation on CTH.

Author comment: Rayleigh scattering is fully included in our retrieval. However, as stated in section 2.4 (section 3.3 of our original manuscript), one inversion scheme was used for a wide range of solar zenith angles. This is done to simplify the retrieval scheme and because the effect of clouds on the normalised O2 absorption was found to be very similar for different solar zenith angles. This simplification, however, leads to increasing errors for large SZA. For SZA $<70^\circ$ the errors are typically $<$ 5%, for SZA of 80° they can reach values up to 20%. We added this information to the text.

p. 17126: l. 7: effect

Author comment: corrected

l. 11: what is the magnitude found? Please clarify.

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Author comment: We added the following information to the text: 'an upward shift of a thin cloud located above a thick cloud can cause an increase of the O2 absorption which is at maximum still three times smaller than the corresponding decrease of the O2 absorption caused by the same vertical shift of a thick cloud.'

Section 5 p. 17128: l. 19: What is meant with "cloud heating"? Do you mean excess radiative absorption in clouds which warms the cloud? Or do you mean positive radiative forcing at the surface or at the tropopause due to clouds? Please clarify.

Author comment: We mean the greenhouse effect of clouds. We modified the text accordingly.

p. 17129: l. 3-4: remove: "changes associated with strong"

Author comment: Corrected

Figures and captions

Fig. 1: which geometry is used?

Author comment: The line of sight angle was set to 90° and the solar zenith angle to 20°. We added this information to the Figure caption.

Fig. 2: The colour scale is unclear, because all cloud fractions between 30 and 100 % are blue.

Author comment: A new version of the figure is provided with an updated colour scale (new Fig. 4). Also seasonal averages are now shown.

Fig. 3: To which quantity is the O2 absorption normalized? Is the meaning of the quantity "normalized O2 absorption" here the same as in Fig. 1?

Author comment: The O2 absorption is normalised to the measured maximum O2 absorption for the same values of the solar zenith angle, the line of sight angle and the relative azimuth angle. The extraction of these maximum values of the O2 absorption

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from the measurements is now described in detail in section 2.4 (old section 3.3). We added this information to the caption. In Fig. 1 a similar normalisation was applied. But there the maximum O2 absorption was derived from the radiative transfer modelling.

Interactive comment on Atmos. Chem. Phys. Discuss., 7, 17117, 2007.

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