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Atmospheric Chemistry and Physics Discussions

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 #2

The technique employed by Wagner et al. to estimate cloud properties is a very useful and under-appreciated one. Its chief advantage is that it does not require knowledge of the cloud or environmental temperature, which can be problematic even for optically thick clouds; instead it requires that pressure be known as a function of height, but this is by comparison very well known. The height determination also requires an independent determination of the cloud fraction. A disadvantage of their technique is that the heights are harder to interpret, since they are not strictly the top of the cloud but rather a point somewhere between the top of the highest cloud and the bottom

of the lowest (which means that multiple cloud layers, as pointed out, cause a bit of ambiguity in the result). Also, in lieu of temperature one must know the surface albedo, which is a problem in regions with ice.

Author comment: First, we want to thank this reviewer for these positive comments. We mostly agree with the reviewers general assessment of the strengths and weaknesses of our technique. However, it might be fair to add that also for the IR techniques, the retrieved CTH depends to some degree on the vertical profile of cloud extinction.

My overall feeling on this manuscript is that instead of showing correlations of dubious relevance to climate, it should have simply shown seasonal mean maps of global cloud height. This would be a much more sensible thing to compare with models, and would be easier to look at and interpret. A more careful comparison of results with previous studies (e.g. ISCCP) would also be called for, as the authors could home in on differences and try to explain them in terms of the different measurement techniques and what this tells us about the clouds responsible. What they have actually presented is probably about the same thing one would get from ISCCP or other previous cloud climatologies.

Author comment: We think that it is a good idea to add seasonal mean maps of CTH and CF. We followed this suggestion in the revised version of our manuscript. We also added a detailed comparison to ISCCP data (new section 2.5). In order to make the comparison more meaningful, we developed a correction procedure to convert the ISCCP data 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 height and cloud optical depth (the surface albedo is also taken into account). This correction procedure strongly improved the agreement between both data sets. We think the agreement is really remarkable and much better than could have been expected. Nevertheless, still systematic differences between both data sets occur, which can be mostly related to the different sensitivities of both techniques to the vertical cloud structure. We conclude

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that the GOME CTH indeed add independent and complementary information to the ISCCP data. We thank the reviewer very much for this suggestion which caused us a lot of work, but also brought new and important insights.

The correlations are hand-wavingly argued to be relevant to climate feedback, but this is a highly doubtful claim that in any case has certainly not been demonstrated anywhere. Most correlations of this type turn out to be robustly reproduced by essentially all climate models, despite the fact that those same models run with higher CO2 then make very different feedback predictions (see e.g. recent paper by V. John and B. Soden). The problem was pointed out by some of the same papers cited here (by Bony et al for example), namely, that these correlations are dominated by local dynamics that have nothing to do with the subtle constrains deciding feedback.

Author comment: We changed our conclusions to point out that the interpretation of our results with respect to cloud climate feedback is not straight forward. In particular we pointed out the difficulties in generalising the findings of local correlations to climate change predictions. Nevertheless we still think that our new method provides new and independent information on the relation of cloud properties to surface temperature which will be useful for the study of cloud climate feedback.

The idea of using these as a test of models is interesting in principle, but given the difficulty of interpreting quantitatively their estimates and the inability of models to simulate oxygen absorption, it does not seem like this would be of any added value to what is already happening with the development of the ISCCP simulator and its use in most climate models nowadays.

Author comment: We think that the fact that to date a GOME simulator does not yet exist, is not a strong argument. Such a GOME simulator could in principle be easily constructed and applied to model results. In the revised version of our manuscript we inserted a sub-section with recommendations for the development of such a simulator (see also below). The new and detailed comparison to ISCCP data showed us that

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GOME CTH data indeed provide complementary information to the ISCCP data sets. It would be worth using this information in future comparison exercises.

Only if the authors could show that their estimates carry some useful, independent information would it then be possibly worthwhile to consider perhaps a GOME simulator!

Author comment: This is a very good suggestion! A GOME simulator could provide a link between modelling data and GOME observations. We added a new sub section (3.3) with recommendations for the construction of such a GOME simulator. We thank the reviewer very much for this valuable suggestion!

I would think that a strong point of their procedure would be the ability to discriminate clouds from surface ice, since the latter would surely involve more oxygen absorption even if the albedo were similar. This is a region where clouds are very hard to identify unambiguously, let alone place in terms of altitude. Of course, Cloudsat and Calipso will soon revolutionize that. Nonetheless I would have expected more investigation of this advantage, whereas it seems that the authors have instead thrown up their hands and declared this to be the place they trust their data the least. That may be true in the absolute, but in comparison to thermal or other techniques, it may be precisely where they have the most to offer.

Author comment: Many thanks for this comment! In general we agree that valuable information on clouds over surfaces with high albedo can be derived from satellite observations of the O2 absorption. Nevertheless, the retrievals are rather complex, especially for clouds with low optical depth and low CTH. In such cases, the discrimination of cloud effects and varying surface albedo (e.g. caused by the aging of snow) are still difficult to discriminate. Such retrievals should be investigated and applied to measurements in the near future; the necessary tools are in principle available. However, the most critical point for our correlation studies is that in some cases it is not clear if a surface is actually snow-covered or snow-free (e.g. during late autumn in high latitudes). In such cases, cloud effects (which mainly decrease the O2 absorption) could

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be mis-interpreted as snow effects (which mainly increase the O2 absorption). Thus we still think in this study, high latitude regions should be treated with some care. We hope that in future algorithms, the full potential of O2 absorption measurements can be exploited. A critical prerequisite will be the availability of unambiguous information on the presence of snow or ice on the ground. We added this information to section 3.2.

Pg. 17127 Comment on qualitative information. This statement assumes a link between the correlations reported here and cloud feedback, which I very much doubt.

Author comment: We agree and we skipped the sentence

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