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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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Many thanks for the constructive comments! We agree with most of the points and give our detailed responses below.

1. I was confused by the title. The way it is currently worded suggests that surface temperature was derived from UV/Vis.

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

2. The authors devote much discussion to the cloud parameters. More description

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of the surface temperatures should be given as well as the method of comparison. For example, were monthly means used for both data sets? There should be enough description so that others may be able to reproduce the results. The GISS data consist of near-surface measurements and satellite data (skin temperature) over ocean. The satellite skin temperature measurement cannot be made in the presence of clouds, so an analysis is made (Reynolds).

Author comment: We added a new section (2.6) to describe the ST data set in more detail. We also added the information that we used monthly averages of ST. It is true that most information on ST over oceans in the GISS data set is retrieved from satellite observations. However, potential biases in the satellite data (e.g. due to clouds) are corrected using in-situ observations from ships and buoys. This information is also provided in the new section 2.6.

3. The authors state that O2 absorption is not much affected by instrument degradation. While this may be true, the interpretation of the O2 absorption is certainly affected by degradation. As the authors point out, it is sensitive to cloud and surface albedo as well as cloud fraction (that is derived by measurements that are sensitive to degradation). I find the interpretation of the O2 absorption to be difficult as it involves a combination of factors. The analyses with CTH and CF are more easily interpreted.

Author comment: It is true that the conversion of the O2 absorptions into CTH depends on the uncertainties of the CF product. Fortunately, the effect on our correlation study is negligible, because of two reasons: a) the uncertainty of the HICRU effective cloud fractions are rather small, especially for small CF. Also, to minimise effects due to instrument degradation, an empirical degradation correction over the period 1996-2003 was applied. From inspecting the spatial patterns of the correlation of CF with ST and the O2 absorption with ST, it becomes clear that the O2 signal dominates the ST to CTH relationship. b) Even if there are some residual degradation effects from a possibly imperfect degradation correction, this should have only little effect on the correlation analysis. First, we checked that the amplitude of the monthly anomalies is larger than

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the trends of the effective cloud fraction over the period 1996-2003. We think that this is a specific advantage of the correlation of monthly anomalies. Second, the positive and negative anomalies occur with without systematic temporal patterns. Even including or excluding the strong ENSO does not substantially change the observed correlations. As also suggested by one of the reviewers, in the revised version we excluded some of the Figures showing results of the O2 correlation analysis to avoid confusion.

4. The authors mention that the CTH as well as CF are in good agreement with ISCCP. For CTH, much more important than sampling times, etc. is the fact that two different quantities are being measured; the O2 A-band is sensitive to the cloud vertical structure including geometrical thickness, whereas the IR observations used in ISCCP are not. Therefore, to avoid confusion, the CTH comparison should not be mentioned.

Author comment: We agree that it can be misleading to compare GOME CTH to ISCCP cloud top pressure. However, since we find the comparison between both data sets very important (a more detailed comparison was also asked for by one of the reviewers), we decided to keep the CTH comparison in the revised version of our manuscript, and to add even a more detailed comparison of cloud cover and cloud top height between our results and ISCCP in a new section 2.5. This comparison includes not only annual means but also seasonally averaged data. To allow a meaningful comparison, we also developed correction schemes for the conversion of ISCCP cloud properties into effective cloud fraction and effective cloud top height. From our comparison we derived the following conclusions: A) First, in general good agreement between both data sets is found. Especially the application of our corrections schemes proved to be very important. We were especially surprised by the rather good agreement between both cloud top height data sets. B) The comparison also indicates that GOME CTH data provide independent and complementary information to ISCCP data. Especially complementary information on the vertical cloud structure is very interesting.

It might also be fair to mention that not only the GOME CTH data, but also the cloud top height derived from IR observations is affected by the vertical cloud profile, especially

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in the case of thin Cirrus

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Interactive Comment