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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.

Anonymous Referee #2

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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,

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which is a problem in regions with ice.

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.

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. 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. 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!

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

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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.

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

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

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Interactive Comment