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Geoscientific
Model Development
Discussions

Interactive comment on "The application of the Modified Band Approach for the calculation of on-line photodissociation rate constants in TM5: implications for oxidative capacity" *by* J. E. Williams et al.

Anonymous Referee #1

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This paper describes some modifications to an established photolysis code, its implementation in a global atmospheric model, and a quantification of the effects on the tropospheric budgets of a number of important atmospheric trace gases. Improvements in the simulations of these trace gases are demonstrated by comparison with aircraft and satellite measurements. The paper provides a useful reference for users of the model described, and will be of some value for other readers interested in the sensitivity of atmospheric composition to photolytic processes. The paper is appropriate for publication in GMD, but there are a number of deficiencies that need to be

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addressed before publication, and some of these are outlined below.

One weakness of the paper is that the J-values are evaluated indirectly, through their impact on observed ozone distributions, rather than directly, by comparison with observed J-values under particular conditions. The paper would be more valuable if the authors were able to demonstrate clearly that the J-values and their variation were more realistic with the new scheme (which is of immediate concern to readers interested in adopting the scheme) before they start comparison with other observations (which is of greater concern for those running the TM5 model). There is some qualitative comparison with observed J-values in Section 3, but a more quantitative comparison with observations (or even with best-guess J-values calculated by a more complex scheme) is needed here.

The paper would be of greater value to readers if the authors could extract more general conclusions about the strengths and weaknesses of the approach. The study demonstrates and quantifies differences in trace gas budgets for one particular atmospheric model versus a simple off-line approach, but it is not clear how widely applicable the results are. What general benefits might other users of the scheme expect to see?

Given the importance and computational expense of photolytic calculations, it would be useful to have some statement of the computational costs or benefits of this approach compared with both simpler and more detailed calculations. Without this it is not possible to judge the computational merits of the approach adopted.

Specific Comments

p.2283, l.24: "truncated (optimized)"; it would be better to choose one word or the other. Is truncation the only optimization applied here?

p.2284, l.1-3: The logic of the wavelength bin grouping is not presented here; it would be useful to include a brief statement to explain why these groupings were chosen.

p.2284, l.8-10: Similarly, it would be helpful to indicate briefly why the shifted wave-

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length bins are used. How does higher SZA lead to he need for this (greater attenuation at short wavelength end of bin) and how have the size of the shifts been decided?

p.2284, l.15-23: This paragraph needs to be clearer about the methods used. It claims a "full solution of the radiative transfer equation" here, but notes in a later section that it uses a two-stream approach ahnd that "the Mie-scattering component is not included in this study" (p.2286, l.17). If this is a two-stream solution for clear and cloudy conditions that accounts for isotropic scattering (only) from cloud droplets and aerosol, then this should be stated very clearly here.

p.2285, l.22: It isn't helpful to show zonal mean r_eff in Fig.S1 averaged over locations where there are no clouds, as the reader can't tell whether higher values indicate larger droplets or just fewer cloud-free locations. Can you plot r_eff averaged over locations with clouds only?

p.2286, l.11: Are the aerosol fields described here climatological (i.e., prescribed off-line and not explicitly transported in the model)? If so, please state this.

p.2286, l.17: If Mie scattering is neglected, state clearly that scattering from aerosols is assumed to be isotropic (if this is indeed the case). If not integrated, how many orders of scattering are considered?

p.2286, l.27: some clarification is needed for "interpolated". What measures are taken to ensure that the total flux is maintained?

p.2287, l.12-13: It may be more relevant to plot the fractional change in sec(SZA) in Fig.S2 rather than the absolute change in SZA, given that this is proportional to the difference in slant column.

p.2287, l.17: if scattering is isotropic, insert "isotropic".

p.2287, Eqn.5: The terms are reversed here: snow should be 0.7, ocean and bare soil low (0.01) and vegetation perhaps 0.05? It is strange to have land surface albedo lower than that for the ocean as suggested by Figure S3, please check these numbers. What

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wavelength interval were these chosen for?

p.2292, l.7-16: It would be helpful to provide some indication of the origin of the differences in the TOA spectra used. Does this reflect measurement issues or some natural variability? Is one of them more appropriate than the other for the general user?

p.2293: As noted in the general comments above, it would be valuable to have a clearer comparison with J-value measurements illustrated here. The qualitative comparison presented here is weak; a better quantitative comparison is needed, even if it just contributes to an additional figure to show how the J-values are better under particular conditions.

p.2296, l.14: How well does TM5 capture the diurnal cycle of ozone? This can be important when comparing monthly mean ozone, as nighttime biases may mask the effect of any changes due to photolysis during daytime.

Table 4: The budget for CH2O presented here doesn't balance; there's a large sink term missing. Or perhaps deposition is expressed in Tg(C) instead of Tg(CH2O)?

Table 6: The caption is not clear. What are the masses shown here? They aren't consistent with the previous tables.

Figure 1: The information shown here is fine, but the presentation needs to be clearer. Given the difficulty of spanning 3 orders of magnitude on the J-value color scale, I suggest a monochromatic (or perhaps dichromatic, 0-5, 10-750) scale where the intensity is represented by the saturation (larger values shown by stronger colors). On the difference scale, the blue colors should be paler. The figure would also be more legible if the individual panels were larger, as in Figure 2; the axis labels are not needed except on the left hand and bottom plots.

Figure 6: The colors of the BA and MBA lines should be swapped so that they are consistent with figs 5, 7 and 8. The captions should state what the error bars represent.

Fig S2: panels are arranged left/right, not top/bottom. It would be helpful to mark the

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terminator on the figure (SZA differences at night are of no concern) and to center on the daylight half of the globe (either show 12 UTC or center map at 180E for 24 UTC).

Technical corrections

p.2282, l.26: Liu reference should be 2009

p.2285, l.20: place -> placed

p.2285, l.24: remove brackets and describe in full: smaller cloud droplets exist over the land and larger droplets over the ocean.

p.2290, l.19: opaque -> transparent (!)

p.2291, l.25: remove "that"

p.2996, l.23: Ordnez -> Ordonez

p.2298, l.8: inverse to -> inverse of

p.2302, l.12: period at end of sentence.

p.2303, l.24: remove "even"

p.2304, l.28: Zimmerman

Interactive comment on Geosci. Model Dev. Discuss., 4, 2279, 2011.

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