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Atmospheric Chemistry and Physics
Discussions

Interactive comment on "Analysis of actinic flux profiles measured from an ozone sonde balloon" *by* P. Wang et al.

Anonymous Referee #2

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Actinic flux (AF) is fundamental to atmospheric chemistry, but is highly variable and therefore difficult to quantify, particularly in the presence of clouds. Instruments for accurate AF measurements have been developed but can be expensive and difficult to operate, especially chemical actinometers. The basic concept of the paper, to build and deploy a simple AF instrument with ozone sondes and thus obtain frequent vertical profiles, is excellent. True, this is at green wavelengths that are not the most relevant for photolysis (UV wavelengths would be more relevant), but even so it is a very good test for radiative transfer models in the presence of clouds, and confidence at green wavelengths increases confidence at UV wavelengths. Another strength of the paper is the analysis of cloud data from satellites, to help understand what the detector actually saw during the flights. Temporal and horizontal changes in cloud optical thickness have

to be considered, and the authors did a nice job with that.

There are however a few issues that should be addressed to improve the manuscript.

ISSUE 1: The general discussion of cloud effects on actinic radiation is weak theoretically. There is quite a bit of literature on these effects, and while the authors cite some of the key papers, they don't use content from those papers to help them interpret their own results. This leads to assertions that are not true, or not relevant, or misleading. Specific points needing improvement:

31175/19-21: The peak AF is not at cloud top, but usually below that. For very thick clouds, the peak is barely below cloud top, but for thinner clouds the peak could be at the midpoint between cloud top and bottom, and even closer to cloud bottom if the surface albedo is high (e.g. over snow). So it is incorrect to automatically place model cloud tops at the same height as the peak measured AF. Since the comparison to the model is only rough, it really does not make much difference, except that it propagates a wrong conceptual understanding.

Figure 1b and text p.31177/5-15: The correlation between AF and irradiance is in general non-linear, as it depends on the diffuse/direct ratio and therefore on the presence/absence of the direct solar beam. So seeing a quasi-linear correlation only says that the scatter is too large to detect the non-linearity, and is not a confirmation of accuracy.

31178/25: While multiple scattering helps to increase the AF, it is not required. Single scattering alone can do that.

31178/28-39 to 31179/1-3: This is not limited to isotropic scattering, and was earlier shown to occur by Madronich (1987, cited but not used). The exact value of mu_0 depends on the directional distribution of the diffuse radiance, limiting to 0.5 (cos 60deg) for isotropic light but mu_0 = 1/sqrt(3) (cos 52 deg) in the commonly used delta-Eddington approximation. The authors are mixing some of these concepts in lines 31179/1-10.

31180/15-17: Enhancements of surface actinic flux in the presence of broken clouds have been measured and explained in earlier work, e.g. Lantz et al. (JGR,101,14693,1996) for JNO2, Crawford et al. (JGR, doi:10.1029/2002JD002731) for spectrally resolved AF, and these should be cited here.

31180/18: Again multiple scattering helps, but is not required to cause enhancement. Single scattering would suffice, depending on the COT. For thin clouds, single back-scattering probably dominates over multiple scattering. Also, this backscattering can occur not only at cloud top but throughout the cloud.

31185/8-10: How do you know that you are really testing the pseudospherical part of the code? What is the air mass correction factor (as a function of altitude) at 78 degrees, compared to simple secant(sza)? How much worse would the agreement be, if you used only plane-parallel?

31185/22-23: It is not surprising that increasing COT makes little difference at low sun, high altitude. In these conditions, the direct solar beam dominates. Reflected radiance is proportional to cos(sza), so even if clouds reflected 100% of the incident radiation, the contribution to the AF is much smaller than that from the direct beam. Furthermore, increasing cloud optical depth from a large value (say 50) to even larger values (100, or 1000) makes only small changes to cloud reflectivity because it is already close to 100%. That is why it does not help much to increase the COT. It seems premature to invoke the need for fully spherical calculation based on this result.

31186/16: The enhancements near cloud top are larger than above the cloud only for high sun (small sza). The opposite occurs for low sun, as also seen in Fig. 11, and as already discussed above (changing sign at 52 or 60 deg, depending on approximation).

31187/6: Vertical position is also extremely important, as shown in Fig. 7: A factor of two error is seen in the model because it does not have the low clouds. Note that these

errors are largest in the lower atmosphere, which is where AF is most relevant to air quality (NO2/O3 ratios, etc.). So while the altitude range of error seems small, it is a most important part.

ISSUE 2: Normalizations, and showing the effect of clouds. The several normalizations performed (model/observation, clear/cloudy) cause loss of useful information. I understand that the green AF detector is uncalibrated, and sometimes changes even between flights. That is ok. But the model IS calibrated (quite well, based on Fig. 6), and could produce results in units of AF, for example photons cm-2 s-1 nm-1. At the very least, the model can produce AF relative to clear sky, i.e., CMF(z). Figures 7-11(d) show the vertical profiles of AF from the model and the observations, both normalized to 1.00 at 30 km, but this should not be confused with the CFM(z). It would be interesting to plot also CMF(z) (obviously from the model, as this is not available from the measurement). This could be an additional curve on the same plot (the numbers should be similar although a bit larger). For example, in Fig. 8d, the peak AF (at about 1.5km) is 2.0 x the value at 30 km. But the value at 30 km is also larger than clear sky (because of cloud reflection from below), so the actual CMF peak at 1.5km is larger than 2.0, perhaps as high as 3.0. In other words, normalizing both model and observations to 1.00 at 30 km causes loss of information about the CMF(z). Because of this excessive normalization, the manuscript doesn't show CMF(z), one of the original objectives of the work. (Figure 4 does show CMF at cloud base and cloud top, but with much scatter - see below).

OTHER MINOR ISSUES

31172/21-23: Palancar et al. 2011 also presented most of their results in terms of the CMF statistics, based on thousands of UV observations from aircraft. How do CMFs in the present work compare with those of the earlier study?

Figure 2: Difficult to see. Most of the discussion (p. 31178) is for 0-5 km so I suggest showing only 0-5 km, or at most 0-6 km in the plot, since you in any case dismiss values

above 5 km as being due to unknown changes in AOT and surface albedo. (But later you normalize to values at 30 km, so this argument is not being applied consistently).

Figure 4a: Strange to plot ratio of top/base vs. ground irradiance. Why not plot the reciprocal, base/top vs. ground irradiance? It should be close to a simple straight line through zero, rather than this unnecessary hyperbolic-looking function. This was done in Fig. 6 for global irradiance, and it looks nice there.

Figure 4b: why such a large scatter when there are no clouds? Agreement for clear sky should be better, CMF = 1.00.

31180/20: The figure contradicts the text, that cloud top CMF is more sensitive to sza. For example, at COT = 40, CMF at cloud base decreases by a factor of 2 (as sza goes from 30 to 60) while CMF at cloud top only decreases by 30%.

31180/25: This makes no sense. How can you get such large CMF ($\sim$ 2 or more) for very low COT? How large a bias in COT do you need to gain a factor of 2 in the CMF? Judging from your model curves, you would need COT $\sim$ 30 to get CMF $\sim$ 2, instead of COT $\sim$ 0. This is a very large bias. Please clarify.

31184/10 and Fig. 8: it would be interesting to see the model results BEFORE the ad hoc COT reduction from 30 to 20. Otherwise the logic becomes circular: if measured COT has to be changed to get agreement with modeled actinic flux, then there is no closure.

31186/14: an -> a

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