

Interactive comment on:

**“Ultraviolet actinic flux in clear and cloudy atmospheres: Model calculations and aircraft-based measurements”**

by G. G. Palancar et al.

## **Anonymous Referee #3**

**We thank the referee for the insightful and helpful suggestions which resulted in an improved manuscript.**

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### **General Comments**

This paper discusses the comparisons of modeled actinic fluxes to measurements performed with an aircraft in a large range of conditions. Thematically the paper is appropriate for ACP but there are issues to be delineated before it is accepted for publication. The paper contains interesting results, but some questions arise concerning the use of the model. In the model, aerosol effects have not been taken into consideration and I believe this is a handicap of the paper. There is also some concern about how the authors interpret the statistical results of the comparisons. The abstract is rather general and I would suggest to specify more quantitatively the findings instead of giving statements like the one in lines 9-11. The simple conceptual model is not included in the objectives of the paper as they are given in the introduction. Consequently sections 4.2 and 4.3 come unexpectedly in the scene. I found useful the approach presented in Figures 6 and 7.

### **Specific Comments**

**3322, 9-10:** The numbers give here should be adjusted to reflect my comments at (3329, 8-18). The same stands for the statement “good agreement”.

**Answer:** We have added the word “average” to avoid possible misinterpretation that each individual measurement agrees with this remarkable accuracy.

**3327, 4:** What parameters contain the aircraft data files that were used as inputs to the model?

**Answer:** Time, latitude, longitude, altitude, and temperature.

**3327, 7-8:** Is this statement correct given that no aerosols were taken into account in the model?

**Answer:** Correct.

**3327, 14:** Excluding of aerosols is not a good choice. At least typical conditions should be included.

**Answer:** We disagree. Most of the flights were over the non-urban regions of the U.S., where aerosol effects on actinic fluxes are relatively minor, especially above the boundary layer. This is quite clear from Figure 1, where the clear sky values show excellent agreement with our model in the absence of aerosols. The focus of our paper is on clouds vs. cloud-free skies, which is complex enough without adding large aerosol loadings. That of course could be an interesting future study.

**3327, 17:** I suggest to give a brief description of the land over the area covered by the measurements, to justify the selection of the albedo values.

**Answer:** see p. 3326, lines 22-23: *...flights over different surfaces (open sea, coast, large cities, meadows, mountains, etc.*

**3328, 5-6:** Why the authors have chosen to degrade the spectral measurements to such broad integrals? The contribution of the UVB is now almost negligible; hence the analysis reflects effects mainly on UVA flux.

**Answer:** We wanted to avoid the uncertainties related to UVB absorption by ozone, both stratospheric and tropospheric. Again, we are focusing on the effect of clouds and would like to reduce the influences of other factors.

**3328, 18-19:** Good agreement is a very vague statement particularly as differences of larger than 20% are seen in the comparisons. Generally the 2 examples shown do not represent the entire campaign, as Table 1 suggests.

**Answer:** We stand by our statement that the agreement in the upwelling radiation is excellent, and we would like to emphasize that it is not trivial to obtain considering the need to model both the ground albedo, and the scattering from the atmosphere below the aircraft. By the way, this parameter would be expected to be sensitive to aerosols, and the good agreement is further justification for our neglect of aerosols in this study (see earlier concern from this reviewer)

We disagree that the days are not representative. The statistics shown in Table 1 clearly show similar values on the two example days (7 and 13 Aug) as for the other 15 days.

**3328, 21:** Table 1 suggests that the upwelling flux is underestimated on average by 10% and the spread is +/- 50%, which is not good agreement

**Answer:** The average cloud-free upwelling flux measured/model ratio is  $1.1 \pm 0.2$  (not  $\pm 50\%$ ). For reasons explained above, the prediction of the upwelling flux is quite difficult. It is indeed a remarkable good agreement when a model predicts the upwelling UV radiation to within 10%, considering the flights over a large variety of surfaces (urban, agricultural, forest, grasslands, desert, oceans), different solar zenith angles, aircraft altitudes, surface altitudes. and other environmental conditions (e.g. pollutants).

**3329, 5:** Isn't the presence of clouds confirmed by the cameras?

**Answer:** Yes, qualitatively. The figure shows the quantitative effects of clouds on actinic flux.

**3329, 8-18:** I do not agree with the interpretation of the statistics of Table 1. For each flight there is a large spread in the data ranging between +/- 7% to +/- 40%. This implies that the agreement between model and measurements is generally not very good. The statistics for the entire data set is based on the average ratios from all days hence the calculated range is small. This shows some consistency in the results between different days, but tells nothing about the agreement between model and measurements.

**Answer:** The table presents values averaged over each flight. We certainly did not mean to imply that each individual measurement will agree to within a few percent. To remove the possibility of misinterpretation, we have added the words "averaged over each flight":

**3329, 11:** *"The total actinic flux, averaged over each flight, agrees..."*

**3329, 19:** It would be better to show in Figure 3 the fraction of measurements (in %) instead the number of data. This would clearly show that the comparisons under cloud free conditions are much better.

**Answer:** The much larger number of clear-sky points would overwhelm the scale. Logarithmic scaling would distort the frequency distributions.

**3329 8-29:** Why downwelling fluxes agree better than upwelling even under cloud free conditions? Could it be the selected constant albedo or boundary layer aerosols which are not taken into account?

**Answer:** Yes. Unfortunately these are highly variable as the aircraft passes over different areas.

**3330, 5:** " : : values that approach the experimental and modeling uncertainties." This is probably true for down welling flux but for upwelling the expected experimental or modeling uncertainties are certainly lower than 20 or 40%.

**Answer:** As mentioned above, the relative (%) uncertainty in the upwelling flux is expected to be much larger than for the downwelling flux. Of course, the upwelling is usually a much smaller contribution to the total, so it is less critical.

**3330, 7-10:** In the absence of clouds there should be no doubt that aerosols, which were not considered in the model, reduce the observed flux. I think this clearly suggests that choosing to use zero aerosols in the model was a wrong decision.

**Answer:** Aerosols can increase or decrease the actinic flux. Low altitude aerosols can backscatter solar UV back into the middle and upper troposphere, and thus increase the total actinic flux. At the surface, aerosols usually reduce the irradiance, but reduce actinic flux less,

and can even increase it, by converting collimated sunlight into diffuse radiation. The magnitude and even the sign of the aerosol effect depend sensitively on aerosol optical properties (especially on the single scattering albedo) and vertical profile relative to the aircraft. For these reasons, we don't think that using a single typical aerosol profile would improve our comparisons, while considering many different local aerosol profiles would increase the complexity of this study beyond its intended scope.

**3330, 11-16:** The small SZA dependence might be also a consequence of the imperfect angular response of the detectors. For upwelling flux the picture is indeed too complex to make accurate attributions. I would add again here the effect from excluding aerosols in the model. It would be interesting to perform some sample runs with typical aerosols in the model and see if the comparison improves (not only with respect to SZA, but generally for upwelling flux).

**Answer:** The imperfect angular response of the detectors could indeed contribute to the small SZA dependence. We also agree that the upwelling flux is more complicated, and the larger variations are more likely real variations in the radiation environment, than instrumental uncertainties.

**3332, 19:** I would remove the word "collimated". It does not help and, in any case, the direct solar radiation is not parallel.

**Answer:** the collimated-to-diffuse conversion, with its associated factor  $2 \cos(\text{SZA})$ , is fundamental to the development of the analytic model. Indeed it is the reciprocal of this factor that is the lower limit slope of Fig. 6.

**3333, 1:** I do not think that Figure 8 helps, particularly as the right panel is a bit confusing with the upward reflection of clouds to the aircraft. I suggest to remove it.

**Answer:** This figure, with the associated text, has been moved to appendix A.

**3332, 17:** This section presents an interesting simplification for actinic flux aloft. In the first place it comes unexpectedly in the paper which to this point is focused on the interpretation of the model to measurement comparisons. I am puzzled as to whether this discussion is appropriate for this paper. At least I would suggest to the authors to try to tight it better with the rest of the paper.

**Answer:** This section has been moved to appendix A.

**3339, 2:** See comment at (3329, 8-18).

**Answer:** Please, see answer at (3329, 8-18).