**Atmospheric
Chemistry
and Physics
Discussions**

Interactive
Comment

Interactive comment on "The influence of the spatial resolution of topographic input data on the accuracy of 3-D UV actinic flux and irradiance calculations" *by* P. Weihs et al.

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ANSWERS TO REFEREE 2:

We thank the reviewer for her/his valuable comments which helped to considerably improve the quality of the manuscript. We adressed all the comments of the referee. The detailed answers follow

Referee 2 (Ref 2): General comments This paper presents both irradiance and actinic flux measurements and calculations in a mountainous region in order to study the influence of the spatial resolution of a digital elevation map on 3D radiative transfer calculations. Results are shown for 305 nm and for three case studies. I found the

Full Screen / Esc

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Interactive Discussion

Discussion Paper

paper very interesting. It is well written and the subject is original and within the scope of ACP. The challenging geographical conditions to perform the measurements make it even more valuable. However, some important issues must be corrected or clarified. In brief (see below the detailed comments): * I think there is confusion about what a higher resolution (or pixel size) means.

Authors response (AR)

We will perform the changes throughout the paper. Lower resolution will correspond to a larger pixel size. (see below)

Ref 2:

I found difficult to correlate the main conclusions with figures 11-16.

AR

An analysis concerning the agreement between measurements and modelling will be added. This analysis will be followed by the analysis concerning the intercomparison of the calculations performed with the different resolutions . . .. See below

Ref 2:

Some formulae and units must be corrected.

AR

Changes will be made

Ref 2:

Specific comments Abstract 1. Introduction Page 27174, lines 12-13: The reference Rieder et al. (2008) is duplicated both here and in the reference list.

AR:

One Rieder et al.(2008) will be removed from the reference list.

Ref 2: * Line 15: . . .to changes in cloudiness. . .

AR: will be changed to changes in cloud coverage according also to suggestion of referee 1.

Ref 2: * Lines 17-18: I think that in the way the sentence is written it is incorrect. The text "solar zenith angle (SZA)" written between parentheses may be understood as a synonymous of the elevation angle when it is not (zenith angle is the complementary angle of the elevation angle). Then, the definition given in lines 18-19 corresponds to SZA, and not to the elevation angle. In any case, I think this basic definition is not necessary.

AR:

Sentence will be changed to: "On a daily basis short term variability (i.e., hours or minutes) in UV radiation is related mostly to changes in cloud coverage (e.g. Calbo, et al., 2005; Simic, et al., 2008), whilst over the whole day variability in UV radiation is determined by changes in solar elevation (e.g. Schwander et al., 1997)), aerosol optical depth (AOD) and total ozone (e.g. Feister and Grewe, 1995)."

Ref 2:

* Page 27175, line 28: in the reference Madronich (1987), the author does no deal with experimental measurements of actinic flux.

AR:

Madronich will be removed from this sentence

Ref 2:

Line 20: Do the authors mean: "these factors are not fully taken into account. . ."?

AR:

Sentence will be changed to:

In the present study actinic flux is not defined as the radiation received from all solid angles but only from the upper sky hemisphere (2p sr).

Ref 2:

* Page 27176, line 27: McKenzie et al.

AR : Correction will be made.

Ref 2: * Page 27177, lines 23-24: Please delete "previous" or "previously".

AR: Correction will be performed

Ref 2: * Line 28: ". . .three case studies. . ."

AR: Correction will be performed.

Ref 2:

2. Methods and data 2.1 Locations and measurement data Âů Page 27178, line 27: ". . .were used:. . ."

AR: Correction will be performed

Ref 2:

* Page 27178-9, lines 27-1: How or why these specific days and times were selected? Have the authors run more calculations at other times? Do these days and times represent typical cases? Did the authors any statistics?

AR

Following explanation will be added:

These specific days were selected because they are clear sky days. The time of the day of the measurements selected correspond at noon to the overpass time of the AURA satellite (with OMI instrument onboard which may provide information on the atmosphere (mainly column ozone and aerosol load)). The validation of the OMI re-

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trieval of ground UV using ground UV measurements was performed in a previous study (Wagner et al., 2010). One case study with low solar elevation (24th February, 08:20) was however added In order to investigate the effect of DEM resolution on UV also for low solar elevation.

Ref. 2:

2.2 Model setup 2.2.1 1-D radiative transfer model 2.2.2 3-D radiative transfer model Geometrical problems arising in 3-D model calculations: pixel discontinuity * Page 27180, lines 20-22: at large SZA (i.e. close to $90°$) the expression 1/cos(SZA) tends toward infinity (not to zero). At the end of the sentence please delete "sza".

AR:

Correction will be made.

Ref. 2:

Uncertainty estimations of 3-D model calculations * Page 27181, lines 11-12: ". . .uncertainties in the model input parameters. . ."

AR: Change will be performed

Ref 2:

3-D Model validation * Page 27182, lines 8-9: did the author mean: ". . .showed UN-CERTAINTIES . . . less than 13% for irradiance and less than 25% for actinic flux in the UVB?" or maybe they meant an agreement WITHIN 13% and 25%, respectively. If this is the case, does this uncertainty represent an average or a maximum value? In no case 25% is a small difference, but if this is an average value it would imply much larger differences between the 3-D model used in this work and the experimental measurements. How do these differences affect the conclusions of this work?

AR: This section will be changed to:

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Full Screen / Esc

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Interactive Discussion

Discussion Paper

These showed a maximum discrepancy between the 3-D-model simulations and measurements of up to 13% for irradiance and up to 25% for actinic flux in the UVB. It should however be borne in mind that the accuracy of spectral UV irradiance and spectral actinic flux measurements are estimated to be in the region of $\pm$ 5% (Bais et al., 2001) and $\pm$10% (Bais et al., 2003) respectively.

Ref 2: 3. Methods and data * Sections 2 and 3 have the same name

AR:

Section 3 will be titled 3. Results

Ref 2:

3.1 Effect on Parameters of digital elevation map resolution

3.1.1 Altitude * Page 27182, lines 20-25: First, I consider that 50 m is a better resolution than 800 m. This is to say that when we go from 800 m to 50 m we are increasing the resolution and decreasing the pixel size. From this point of view, these sentences are confusing. For example, I understand that if the altitude goes from 1594 m at 50 m resolution to 2146 m at 800 m resolution, it is overestimated when the resolution decreases. Please check this point.

AR:

This section will be changed to:

"In general DEMs with larger pixel size (lower resolution) lead to an underestimation of the model altitudes of the observation sites at higher altitudes and to an overestimation of the altitude of observation sites at lower altitudes, especially in cases of very uneven topography such as deep valleys with steep sides. So for the case of Kolm Saigurn, located in a narrow valley increasing the DEM pixel size resulted in an overestimation in altitude which increases from 1594 m at 50 m resolution to 2146 m at 800 m resolution. At the higher sites of Sonnblick and Hafelekar, a decrease in altitude from 3054 m to

2849 m at Sonnblick and from 2278 to 2015m at Hafelekar with increasing DEM pixel size was seen. At the other stations the dependence of pixel altitude on digital elevation map resolution is less than 50m."

Ref 2: * Page 27183, lines 1-2: I guess the altitude input parameter is the surface elevation. I think the last term maybe a clearer one.

AR: The altitude effect on UV was simulated with the SDISORT RT code by changing the surface elevation input parameter whilst keeping all other parameters constant

Ref 2: Âů Page 27183, line 5: "The decrease in altitude at higher DEM resolution,...". Please, see the comment about Page 27182, lines 20-25.

AR:

Sentence will be changed to "The decrease in altitude at lower DEM resolution"..

Ref 2:

3.1.2 Changes to the horizon * Page 27183, lines 11-12 and 15-16: In these lines, the concept of resolution and pixel size is properly used (according to my previous comments). Consequently, it is not consistent with what the authors stated before. * Lines 16-18: When the authors state "the decrease" they are giving the sign or direction of the change, so that the minus sign is redundant (or even confusing). I would say a decrease of 3% or a change of –3%.

AR:

Will be changed to a decrease of 3%

Ref 2: Âů Lines 21-22: Here the authors mention that the equation A11 is used to correct the Irradiance values while in the figure caption equation A12 is mentioned. Please check and correct this point.

AR:

C14075

Equation A11 is correct. Text will be changed in figure caption

Ref 2: Åů Lines 23-28: Is the direct beam present at this solar zenith angle (57°)? Or are these effects only for the diffuse component? If the change in the horizon led to block the direct beam I would expect a much larger difference.

AR:

The direct beam is present for these simulations. The section will be changed to: The results were obtained by multiplying the calculated diffuse component using a 1-D RT model with the corrections factors from Eq. [11] (and then adding it to the direct irradiance in order to obtain the global irradiance) and by using the same calculation method for the actinic flux by multiplying the diffuse actinic flux with the percentage of the sky upper hemisphere which is visible. Results for UV irradiance for a solar zenith angle of 57 degrees are shown in Fig. 3. The values are normalized to the simulations using the 50 m resolution DEM. A strong increase in irradiance and actinic flux (not shown here) of up to 12% is found for Kolm Saigurn, whilst for the other stations the changes to the horizon only lead to uncertainties in irradiance and actinic flux determination of < 5%.

Ref. 2:

3.2 Influence of digital elevation map resolution on accuracy of UV irradiance and actinic flux calculations 3.2.1 Influence on geographical distribution of UV * Page 27185, line 4: ". . . as the increase in. . .". Did the authors mean: "as the differences in. . ."? In any case I was surprised by increases (or differences) larger than 100% in irradiance and actinic flux only because the shading effect. Thus, could the authors please mention or explain the conditions which may lead to increases (or differences) larger than 100% in irradiance and actinic flux?

AR:

Yes we thank both reviewers for their comments about this figure. We had to recheck

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Interactive Discussion

Discussion Paper

the calculations that were used for this figure. There were done using an older version of the programm which does not include the pixel discontinuity correction. The results shown in fig. 5 (50m) are at some pixels slightly too high and at other pixels slightly too low because of the missing pixel discontinuity correction. In fig 7 (irradiance at 800m resolution) there was an error in the calculations (that is the reason for the ratio of 800m to 50m calculation that is much too high). The reason lies in an error in the number of the photons used for the calculation of the absolute unit which does not correspond to the number of photons really used for the calculation. We apologize for this mistake. The ratio of 800 m irradiance to 50 m irradiance is now not larger than 2, which may easily be explained by changes to the horizon (also because of the inclination of the pixel) and by shading. The other results (fig.6,8 and 10-16) were obtained using the methods shown in appendix A or in Wagner et al. 2011 and are correct.

Ref 2:

Âů Page 27185, line 7: "... to be between 0.3 and 2." I guess these are the results for actinic flux. Please add also the range for irradiance.

AR: Will be changed according to the new results also....

Ref 2:

3.2.2 Influence on UV irradiance incident on horizontal planes at the selected stations * Page 27185, line 22-23: "...increasing DEM resolution..." or increasing DEM pixel size?

AR:

With increasing pixel size is correct.

Ref 2:

3.2.3 Influence of digital elevation map resolution on accuracy of UV actinic flux calculations * Page 27185, line 25-27: "The Sonnblick... valley station." I do not understand

the sentence. Is it about Sonnblick or Kolm?

AR: Section will be changed to:

"For the simulation for Sonnblick and surroundings there is a strong underestimation of the calculated actinic flux for Kolm. The explanation lies probably in the fact that local factors (reflections from surrounding objects and from the topography) have a strong influence on the measured actinic flux."

Ref 2:

4. Conclusions Among the main conclusions of this work authors state that: * "the uncertainties increased with increasing DEM pixel size leading to calculated actinic flux values that were too high by up to 20%" (Page 27187, lines 25-26). and also that "The DEM resolution required to obtain small uncertainties in 3-D radiative transfer modeling needs to be high enough to represent the local topography adequately" (Page 27188, lines 11-<12). These conclusions are saying that a better resolution does lead to a better agreement between model and measurements (using the latter as a reference). However, it is hard to me to see this fact from figures 11 to 16, where the results of the model calculations using different DEM resolutions are compared against measurements. Looking at these figures I can see neither a trend nor a better result (considering the differences between the experimental and the modeled values using different DEM resolutions). Even more, I cannot see any biases (systematic overestimations or underestimations) and, in many cases (e.g. Figs 13, 14, 15, 16), calculations using the 50 m resolution show the largest differences respect to the experimental values. Thus, I think this point should be clarified.

AR: Second section of conclusions and the analysis of the figures will be changed including among others following statements:

Concerning the simulations of the UV irradiance "Fig. 11 to 13 show the 3-D model irradiance simulations represented by the shapes with the corresponding error bar cal-

culation uncertainty (see section 2.2.2) and the measurements with the corresponding measurement uncertainties indicated by the coloured areas. If there is an intersection between 3-D model error bar and the measurement uncertainty area we can state that there is an agreement between measurement and model. This is achieved at the three stations for all three case studies except for the 50 m resolution calculation for Kolm. We also can only affirm that there is a statistical significant difference between two calculations performed with two different DEM pixel size if there is no overlap of their error bars. Here we will assume that the 50 m pixel size calculation is the most accurate and it will be taken as a reference. For the investigation of DEM pixel size on irradiance for Innsbruck and surroundings we did not find any effect on irradiance at noon, but an increase was found for the station at Innsbruck at 400 m and 800m DEM resolution at 08:20 UTC. We also found an increase in simulated irradiance at Hafelekar for DEM pixel size larger than 100m also at 08:20 UTC. For the "Sonnblick and surroundings" case, where the topography is more pronounced, but where the simulations were performed for a solar zenith angle of 32.7 degrees, we found an effect of pixel size on irradiance calculation uncertainty for the valley station at Kolm Saigurn with irradiance values too high by 11% at larger DEM pixel sizes.

Concerning the simulation of actinic flux some of the statements will be: "A good agreement between model and measurements is only obtained for Innsbruck 1230 UTC (Fig. 14). For Innsbruck 0820 UTC model calculations underestimate actinic flux for Hafelekar and partly for Innsbruck and Lans. For the simulation for Sonnblick and surroundings there is a strong underestimation of the calculated actinic flux for Kolm. The explanation lies probably in the fact that local factors (reflections from surrounding objects and from the topography) have a strong influence on the measured actinic flux. If we intercompare the model simulations performed with the different DEM resolution we can state for the "Innsbruck area" actinic flux calculation the uncertainty increased with increasing DEM pixel size leading to calculated actinic flux values that were too high by up to 20% (if we assume that the 50m pixel size calculation represents the correct "accurate" reference). For Sonnblick and surroundings, model calculations for

Kolm Saigurn are also affected by changes in DEM pixel size. An increase in actinic flux of approximately 15% was obtained for DEM pixel size larger than 100m.

Ref. 2:

Appendix A Calculation of irradiance in the 3-D model * In half of the equations, the slash (which is used as the division sign) appears as a subscript. Also, in some equations the product is designated with a dot, while in others is not. In equation A11 the dot before the sin function has no sense. Please rewrite the equations in a consistent way.

AR: This will be checked throughout the appendix.

Ref. 2: * Page 27189, line 2 and 10: it should be ". . .the following equation" * Page 27189, line 5 and 17: Units should be W m–2 nm–1. * Page 27189, line 9: I guess authors meant: direct irradiance * Page 27189, line 22: I would use a period after ". . .box is reduced,. . ." * Page 27190, line 4: calculation. * Page 27190, line 6: Units should be W m–2 nm–1. * Page 27190, line 15: Nrea, diff. * Page 27190, line 16: Please delete de colon. * Page 27191, line 21-22: I would say: We simulated diffuse. . . (and not: We may. . .) * Page 27192, line 11-12: I would say: If the angle subtended by the obstruction is larger. . .

AR:

All these changes will be performed.

Ref. 2:

* Page 27192, line 12: I would use a period after ". . .equal 0". Also, f is equal to 1 or f equals to 1. Also, I do not understand the result when f equals 0. In this case, H is also zero (Eq. A11). Thus, ïĄědif is zero (Eq. A12) and, consequently, Drea is equal to zero (Eq. A2), which means that the diffuse irradiance incident at a pixel (the final result of the model simulation) is zero. Is that correct? I think it could be useful to clarify this point.

Interactive Comment

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Interactive Discussion

Discussion Paper

AR:

The section will read: "H is the integral over the solid angles of the sky hemisphere

(11)

Where q is the elevation angle of the solid angle and w the azimuth angle. f is a simple numerical factor to account for the sky covered by the horizon. If the obstruction is larger than q, f equals 0., if the angle subtended by the obstruction is smaller than q f is equal 1…" With this "correct" definition the diffuse irradiance can not be equal to zero. …

Ref. 2:

* Page 27192, line 20: Units should be W m–2 nm–1. * Page 27192, equation A13: According to Eq. A4 Ntot should be replaced by Ptot. * Page 27193, line 3: Units should be W m–2 nm–1. * Page 27193, equation A15: Ntot should be replaced by Ptot.

AR:

Changes will be made according to the referees suggestion

Ref. 2:

References Åů Page 27193, Line 15-19: I did not find the reference to Arola et al. (2002) in the text. Please include it in the text or delete it from the reference list.

AR:

We will delete it

Ref. 2:

* Page 27194, line 33: It should be Degünther instead of Deguenther. AR: Change will be made

Ref. 2:

* Page 27195, lines 21-26: I did not find the reference to Koepke et al. (2006) in the text. Please include it in the text or delete it from the reference list.

AR: Will be deleted

Ref. 2:

* Page 27196, lines 3-4: I did not find the reference to Kylling and Mayer (2001) in the text. Please include it in the text or delete it from the reference list.

AR: Will be deleted

Ref. 2:

Page 27197, lines 9-12: the reference is duplicated.

AR: One Rieder et al. 2008 will be removed from the reference list.

Ref. 2:

Figures * Units in all the figures should be written as mW m−2 nm−1.

AR: Changes will performed

Ref. 2:

Why in Figures 11-13 the measurement uncertainty is $\pm 5\%$ but in Figures 14-16 it is $\pm 10\%$ even though the same place, day and instrument were used?

AR: One uncertainty is for the irradiance and one for the actinic flux measurements which assumed to be higher according to the experts of our team.

Please also note the supplement to this comment:
http://www.atmos-chem-phys-discuss.net/11/C14069/2012/acpd-11-C14069-2012-supplement.pdf

Interactive comment on Atmos. Chem. Phys. Discuss., 11, 27171, 2011.