Response to comments by Stelios Kazadzis (Editor)

Comment 1
Page 8943 Line 27 to 8944 line 18 Since DP4 is calculated directly from DP1 it has to be more clear that better results shown for DP4 are actually a result of averaging the daily irradiance variability due to clouds.

Response:
We will add the following at the start of the section indicated in the comment:
"We will show in the following that the different results for DP1 and DP4 are a consequence of the different sampling and averaging schemes of ground and satellite data."

Comment 2
Page 8946 Case 2 comments. There is a question about the enhancement of measured UV due to the presence of snow and clouds. Which can be a third factor for the negative bias. case 1 comments. Here there is a question on how the OMI model works using high surface albedo and in the presence of clouds. Does it take into account the above mentioned effect?

Response:
We will add the following at the end of the Case 2 discussion:
"During periods of scattered clouds, the UV irradiance at the surface can exceed the clear-sky irradiance (e.g., Mims and Frederick, 1994). Such enhancements occur when the solar disk is not obstructed while clouds in the vicinity of the Sun increase the diffuse component over the value for clear skies. High surface albedo may increase this effect further (Bernhard et al., 2010). The OMI UV algorithm does not account for this effect and this omission may contribute to negative biases for overpass data (DP (1)) when scattered clouds are present. The magnitude of the effect is modest, however, because cloud enhancements of the UVI by more than 10% are very rare in the Arctic (e.g. Bernhard et al. 2007; 2008), and also the frequency of enhancements between 0 and 10% is typically small (e.g., less than 12% of all measurements at Summit (Bernhard et al., 2008) and even less at sites where overcast skies are the norm, such as Barrow in the fall (Bernhard et al. 2007))."

Regarding the comment pertaining to Case 1:
The OMI UV algorithm does not consider the possibility of clouds enhancing the surface irradiance, both for Case 1 and Case 2 conditions (e.g., the CMF is always <=1). We believe that this point is already sufficiently discussed in the paper and we do not intend to change the manuscript further.

The following reference will be added:
Mims, F. M. III., and Frederick, J. E.: Cumulus clouds and UVB, Nature 371, 291, doi:10.1038/371291a0, 1994.

References Bernhard et al. (2007, 2008, 2010) have already been cited in the paper:

Bernhard, G., Booth, C. R., Ehramjian, J. C., Stone, R., and Dutton, E. G.: Ultraviolet and visible radiation at Barrow, Alaska: climatology and influencing factors on the basis

of version 2 National Science Foundation network data, J. Geophys. Res., 112, D09101, 30 doi:10.1029/2006JD007865, 2007.

Bernhard, G., Booth, C. R., and Ehramjian, J. C.: Comparison of UV irradiance measurements at Summit, Greenland; Barrow, Alaska; and South Pole, Antarctica, Atmos. Chem. Phys., 8, 4799–4810, doi:10.5194/acp-8-4799-2008, 2008.

Bernhard, G., Booth, C. R., and Ehramjian, J. C.: Climatology of ultraviolet radiation at high latitudes derived from measurements of the National Science Foundation's Ultraviolet Spectral Irradiance Monitoring Network, in: UV Radiation in Global Climate Change: Measurements, Modeling and Effects on Ecosystems, edited by: Gao, W., Schmoldt, D. L., and Slusser, J. R., Tsinghua University Press, Beijing and Springer, New York, 544 pp., 2010.

Comment 3

Sodankyla and Jokioinen case Sodankyla: Since CLOpt=0 ratios of DP1,2 and 4 are in the order of 1.04 to 1.09 this theoretically means that also ratios of DP3 and 5 are similar for these cases. Here there is a systematic overestimation and since Jokioinen and Sodankyla instruments are regularly intercalibrated i can not see any obvious reason to see this only in one of the two instruments. So since you are talking about a cloudless sky, summertime solar elevation, very low aerosol site this deviation can be either a result an ozone difference between OMI and the Sodankyla Brewer or an overestimation linked with the clear sky radiation OMI code. It would be interesting to discuss this too.

Response:

To address this comment, it is helpful to compare the biases for Sodankylä and Jokioinen side by side. Accordingly, Table 1 lists biases (expressed in %) for data products DP1 and DP4, for Sodankylä and Jokioinen. Biases are shown both for all sky ("all") and clear sky ("CS") data. The latter were obtained by filtering the OMI datasets for CldOpt = 0.

Table 1: Comparison of biases for DP1 and DP4 for Sodankylä and Jokioinen, in percent.

	Bias @ Sodankylä				Bias @ Jokioinen				Sodankylä minus Jokioinen			
Month	DP 1, all	DP 1, CS	DP 4 all	DP 4, CS	DP 1, all	DP 1, CS	DP 4 all	DP 4, CS	DP 1, all	DP 1, CS	DP 4 all	DP 4, CS
June	9	6	5	8	6	5	2	4	3	1	3	4
July	6	4	6	5	3	0	-1	0	3	4	7	5
August	5	6	6	10	4	3	2	5	1	3	4	5

"all" data were filtered for SZA<84° and Dis< 12.
"CS" means "clear sky." Data and were filtered for SZA<84°, Dis< 12, and CldOpt = 0.

Table 1 indicates that biases for "all sky" and "clear sky" data are consistent to within ±3%, with one exception (DP 4 for August at Sodankylä, where the difference is 4%). In six cases, biases for "all sky" are larger than biases for "clear sky", while in six cases the opposite is true. It can therefore be concluded that there is no clear systematic difference depending on whether difference between the two sites are assessed based on all data or data that were additionally filtered for CldOpt = 0. This confirms the paper's statement pertaining to Sodankylä (Page 8954, Line 22): "Between June and August, a bias of 4–9% is apparent in DP (1), (2) and (4), both for all data and data filtered for CldOpt = 0."

Table 1 also shows that biases are positive (OMI > Ground) for both sites, with one exception (DP 4 for all sky data measured in July at Jokioinen, where the bias is -1%). A positive bias of several percent has been reported by many researchers for locations with low albedo (see also response to comments of Referee #2).

The comment by the editor "I can not see any obvious reason to see this only in one of the two instruments" may have been prompted by our statement pertaining to Jokioinen (Page 8956, Line 13): "Between April and November, the bias is less than ±6% when SufAlbedo is 0.02." This statement may incorrectly imply that the bias has no preferred direction. For clarity, we will change the sentence to "Between April and November, the bias ranges between -1 and +6% when SufAlbedo is 0.02."

The last four columns of Table 1 show the difference of the biases for Sodankylä and Jokioinen. Differences range between 1 and 7%; the median is 3.5%. A difference of this magnitude is within the combined uncertainty of the measurements from the two sites. It would therefore be very difficult to attribute these difference to "ozone differences between OMI and the Sodankylä Brewer" or an effect linked to "the clear sky radiation OMI code," as suggested in the comment. Additional compounding factors include differences in latitude and surroundings of the two sites. Because of the many factors that could contribute to the small differences in the OMI/Ground bias between Sodankylä and Jokioinen, we feel that it is not warranted to analyze this issue in more detail.

Comment 4
OMI grid vs measurement In general and in the case of few or scatter or broken cloud situation within an OMI grid there are two cases: a. the sun is visible during the spectroradiometer measurement b. it is not Assuming these two cases within an OMI grid where for both the cloud cover (or measured OMI CLOpt) is equal; in the first case there will be an OMI underestimation and in the second an overestimation. In my opinion the magnitude of the two differences is not equal as without the direct component the overestimation will be larger. So there is a case that statistically and when averaging for a number of observations especially in the presence of clouds, to have a systematic positive bias on the results. Would you agree on the above ? Should it be mentioned on the results discussion?

Response:
We agree with the first part of this assessment but not with the conclusion that our results may "have a systematic positive bias" in the presence of clouds.

Enhancement of surface UV irradiance by scattered clouds is much smaller than attenuation of UV resulting from clouds with moderate to large optical thickness. For example, ratios of all sky to clear sky UV irradiance do not form a normal distribution. Instead, distributions are greatly skewed towards values smaller than 1 (see for example Fig. 7 of Bernhard et al. (2007) for Barrow or Fig. 7 of Bernhard et al. (2008) for Summit). This was stated in the paper (page 8941, line 3): "The quantity ρ_i defines a distribution, which in most cases cannot be well represented by a normal distribution." We therefore illustrated the difference between OMI and ground based measurements with box whisker plots, which are a good way to emphasize asymmetrical distributions.

In addition, histogram of the frequency distribution of the OMI/Ground ratio are available as supplements.

In a statistical sense, the average is much more sensitive to a skewed distribution than the median. This is the reason why we assessed biases between OMI and ground data using the median in the Discussion section (page 8947, line 3): "If not otherwise noted, systematic differences or "biases" discussed below refer to $\widetilde{\rho}_4$ and are expressed in percent (e.g., $\widetilde{\rho}_4$ = 1.05 corresponds to a bias of 5%)."

To further assess possible misrepresentations of the OMI biases, we also quantified the difference between OMI and ground data based on monthly averages (Fig 6. of paper). We found that the average ratio $\overline{R}_4$ calculated from the monthly average daily doses agrees very well with the median $\widetilde{\rho}_4$ calculated from the distribution ρ_4 (page 8944, line 25) and concluded that "biases between OMI and ground data assessed with match-up data [i.e., $\widetilde{\rho}_4$] are robust." This confirms that our results are not affected by a systematic positive bias in the presence of clouds, as suggested by the comment. For clarity, we will change the following sentence (page 8944, line 25) from:

"The median $\widetilde{\rho}_4$ agrees well with $\overline{R}_4$ for all sites and months, suggesting that biases between OMI and ground data assessed with match-up data (Fig. 4) are robust and also applicable to monthly doses."

to

"The median $\widetilde{\rho}_4$ agrees well with $\overline{R}_4$ for all sites and months, suggesting that $\widetilde{\rho}_4$ is an appropriate statistical quantity to assess systematic biases between OMI and ground data. The average $\overline{\rho}_4$ is less appropriate for this assessment because it is more affected by the skewness of ρ_4 distributions."
