Response to comments of Anonymous Referee #1

We note that page and line numbers indicated by Referee #1 refer to the manuscript submitted to ACPD, not the document posted on the ACPD website.

Comment 1
The authors show that, because of the homogeneous and continuous snow cover at the Greenland site, those measurements are potentially useful for detecting systematic drifts in satellite data. More details would be useful in that section (P23). In that regard, perhaps it would be prudent to mention that possible changes in albedo due to changing organic aerosol depositions could affect that in future.

Response:
The comment regarding "that section (P23)" refers to the last sentence of the Conclusions. Instead of expanding the Conclusions, we will add a new subsection to the Discussion, move the last paragraph of the Conclusion to this new subsection, and add more details. The new subsection will read as follows:

"Results presented in this study showed that measurements at a high elevation site located at the center of a major ice sheet, such as Summit, are very helpful for satellite validation. Because of the high homogenous surface albedo at this site, cloud effects are suppressed, resulting in very small day-to-day variations when comparing data from space and the ground. The low variability afforded the detection of systematic problems in the satellite dataset and is also helpful for detecting potential long-term drifts in satellite UV observations. Compared to lower-elevation sites, Summit is less affected by increases in air temperature and their effect on albedo. For example, He et al. (2013) found that changes in short-wave surface albedo observed in Greenland between 2000 and 2012 were most pronounced at elevations between 500 and 2,500 m, ranging between -0.025 and -0.055 per decade. In contrast, the decadal change at elevations above 3,000 m was only -0.013. Future reductions in albedo due increased deposition of organic aerosols cannot be excluded. For example, the expected increase in boreal forests fire activity (Kelly et al., 2013) could have a significant impact on black carbon (BC) deposition. The BC content in the Summit snowpack is currently very low with the highest value given in the literature being 1.5–2 ng g^{-1} (Hagler et al., 2007; Doherty et al., 2010). During May and June 2011, the mean BC content measured over the first 1–3 cm of the snowpack was 0.3±0.3 ng g^{-1} and simulations suggest that its impact on albedo is negligible (Carmagnola et al., 2013). By taking into account the relationship between BC and snow albedo (Hadley and Kirchstetter, 2012), we conclude that even a 10-fold increase in BC at Summit would not significantly affect our ability to detect drifts in satellite UV data using ground based measurements at this site."

References:
Carmagnola, C. M., Domine, F., Dumont, M., Wright, P., Strellis, B., Bergin, M., Dibb, J., Picard, G., Libois Q., Arnaud L., and Morin, S.: Snow spectral albedo at Summit, Greenland: measurements and numerical simulations based on physical and chemical properties of the snowpack, The Cryosphere, 7(4), 1139-1160, 2013.

Hadley, O. L., and Kirchstetter, T. W.: Black-carbon reduction of snow albedo, Nature Climate Change, 2(6), 437-440, 2012.

Hagler, G., Bergin, M., Smith, E., and Dibb, J.: A summer time series of particulate carbon in the air and snow at Summit, Greenland, J. Geophys. Res., 112, D21309, doi:10.1029/2007JD008993, 2007.

Doherty, S. J., Warren, S. G., Grenfell, T. C., Clarke, A. D., and Brandt, R. E.: Light-absorbing impurities in Arctic snow, Atmos. Chem. Phys., 10, 11,647–11,680, doi:10.5194/acp-10- 11647-2010, 2010.

He T., Liang S., Yu Y., Wang D., Gao F., and and Liu Q., Greenland surface albedo changes in July 1981–2012 from satellite observations, Environ. Res. Lett. 8, 044043, 2013.

Kelly, R., Chipman, M. L., Higuera, P. E., Stefanova, I., Brubaker, L. B., and Hu, F. S.: Recent burning of boreal forests exceeds fire regime limits of the past 10,000 years, P. Natl. Acad. Sci., 110(32), 13,055-13,060, 2013.

Comment 2
Minor Points. P4, line 2. Should that be “older” rather than ”newer” norm. Please clarify the sentence.

Response:
To avoid confusion, we will delete the sentence “These differences should be taken into account when data of the present paper are compared with measurements that refer to the newer norm.”

Comment 3
P4, line 19. Puzzling that the correction was not applied. I presume that’s because the calibration difference is small compared with the errors related to albedo. If so, this should be stated.

Response:
The following sentence will be added: “This difference is within the uncertainty of UV measurements from other ground stations and the QASUME instrument (Gröbner et al., 2005), and a correction was therefore not applied.”

The reference

Gröbner, J., Schreder, J., Kazadzis, S., Bais, A. F., Blumthaler, M., Görts, P., Tax, R., Koskela, T., Seckmeyer, G., Webb, A. R., and Rembges D.: Traveling reference spectroradiometer for routine quality assurance of spectral solar ultraviolet irradiance measurements. Appl. Opt., 44(25), 5321-5331, 2005.

will also be added. According to this reference, the uncertainty of the QUASUME instrument is “8.8% to 4.6%, depending on the wavelength and the solar zenith angle.”

Comment 4
P7, line 2. Please specify the typical and maximum time differences between groundbased and satellite overpass measurements.

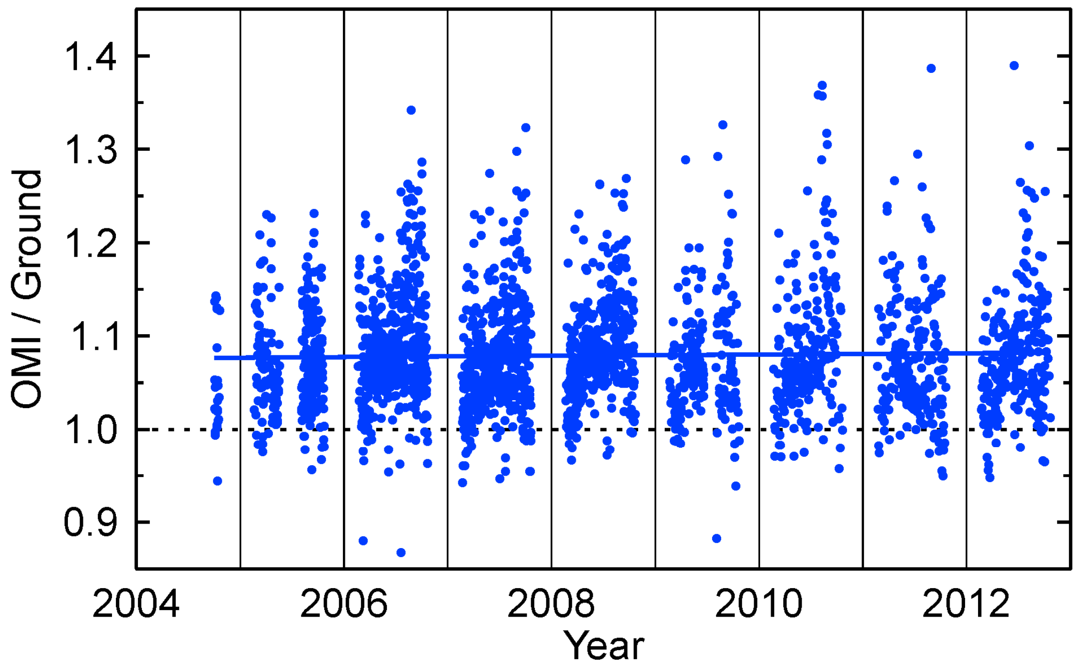
Response:
As described in detail in the first paragraph of Section 3, the maximum time differences between ground based and satellite overpass measurements is quantified with the variable t_m. Because the sampling frequency of the ground based instruments is different, different values for t_m were chosen, ranging from 5 minutes to 60 minutes. The typical time difference has not been specified and the following sentence will therefore be added:

"Sites that use multi-channel filter radiometers provide a sample every minute; the maximum time difference is therefore 30 seconds. Typical time differences for the other sites range between 7.5 (Barrow and Summit) and 30 minutes (Sodankylä and Jokioinen)."

Comment 5
P7, line 18-19. Can this assumption be explored a little, and perhaps justifying the assumption? For example, you could add a new figure showing a multi-year time series of monthly or annual differences (and means). This would be best done for the Greenland site, to back up the statement made in the last sentence of the abstract.

Response:
For clarity, "P7, line 18-19" refers to the sentence "It was further assumed that neither OMI nor ground-based data drift over time."

The supplement contains plots of the ratio of OMI/Ground as a function of time for every site and data product. Because of the large influence of clouds, these ratios show large scatter for all sites but Summit. We therefore agree with the referee that the dataset for the Greenland site (i.e., Summit) is best suited for assessing possible temporal drifts in the OMI dataset (see also Comment 1). As discussed in Section 5.1.5, the ratio of OMI/Ground for the Daily Dose dataset (DP (4)) exhibits a pronounced annual cycle with lower values in the spring than fall. We therefore consider the overpass dataset (DP (1)) for assessing temporal drifts. The associated plot is shown below. A regression line fitted to the data points indicates a statistically insignificant drift of 0.07±0.11% (±2σ) per year. (Note that the plot without the regression line was already part of the supplement).



We note that ground based data are not available at Summit for the periods 18 May 2005 – 1 August 2005 and 21 June 2009 – 1 August 2009. These data gaps can potentially affect drift estimates. Drifts were therefore also calculated for monthly average ratios using the method by Bernhard (2011). This method corrects for errors in calculating a monthly average caused by non-uniform distribution of missing days. The analysis was performed twice, first by allowing for up to 5 missing days when calculating a monthly average, and second for allowing for up to 10 missing days. Results are summarized in the table below. The quantity *n* specifies the number of years for which a monthly average could be calculated for the two cases. Uncertainties refer to the 2-sigma level. This analysis confirms that there is no evidence for a drift of either OMI or the instrument at Summit over the 8-year period considered in the paper.

Month	***n***	**Annual Trend**	***n***	**Annual Trend**
	Up to 5 missing days		*Up to 10 missing days*	
March	4	-1.2% ± 1.1%	8	-0.4% ± 0.5%
April	5	-0.3% ± 0.4%	7	-0.2% ± 0.5%
May	5	-0.2% ± 0.9%	6	-0.2% ± 0.5%
June	5	0.1% ± 0.7%	6	-0.1% ± 1.0%
July	5	0.3% ± 1.4%	6	0.4% ± 1.1%
August	6	0.0% ± 0.8%	8	0.1% ± 0.8%
September	6	-0.1% ± 1.1%	7	-0.4% ± 1.0%

We feel that the information provided above is too detailed to be included in the paper. Instead, we will replace the sentence “It was further assumed that neither OMI nor ground-based data drift over time.“ with “Potential temporal drifts of the OMI dataset were assessed with data from Summit, the site with the least cloud influence. A linear regression fitted to a time series of the ratio of OMI and ground overpass data (DP (1)) revealed a statistically insignificant drift of 0.07±0.11% (±2σ) per year. The absence of drifts was further confirmed by analyzing monthly average data.”

Reference:

Bernhard, G.: Trends of solar ultraviolet irradiance at Barrow, Alaska, and the effect of measurement uncertainties on trend detection, Atmos. Chem. Phys., 11, 13029–13045, doi:10.5194/acp-11-13029-2011, 2011.

Comment 6

Tables 2 and 3 contain rather a lot of detailed information. The authors should consider moving them to the appendix, or to the supplement . Similarly for Figure 13? Figure 5 could be omitted, and simply replaced by a simple summarising sentence the compares the agreement for overpass data, and daily dose data (similar systematic differences, but smaller error bars in the daily doses).

Response:

We suggest the following compromise:

- Keep Table 2 in the paper but move Table 3 to the supplement.
- Figure 13 is already part of the appendix and we think it should stay there as it forms the link between the paper and the many plots of similar layout that are provided as supplements.
- Figure 5 will be moved to the supplement, and the difference between Figure 4 and 5 will be discussed in more detail in the paper as suggested by the referee.

Comment 7

P42, line 4. Specify the wavelength region that applies for this CMF.

Response:

“at 360 nm” will be added.

(As described in Section 2.2., the CMF used by OMI is derived from the measured reflectance at 360 nm.)
