Atmos. Chem. Phys. Discuss.,
https://doi.org/10.5194/acp-2019-896-RC1, 2019


Atmospheric Chemistry and Physics Discussions

# *Interactive comment on* "UV radiation measurements in Marambio, Antarctica during years 2017–2019 in a wider temporal and spatial context" *by* Margit Aun et al.

**Anonymous Referee #2**

Received and published: 29 November 2019

General comments

The manuscript discusses the Marambio dataset of ultraviolet (UV) irradiance collected by a GUV multichannel instrument since 2017. Daily erythemal doses and daily maximum UV indices recorded in two seasons (2017-2018 and 2018-2019) are presented and compared. Additional measurements, such as column ozone concentrations, cloud coverage, aerosol optical depth and surface albedo are also shown. The two-year UV dataset is compared to previous (2000-2008) measurements in the same location, using a NILU-UV radiometer, and in other Antarctic stations (mostly for the 2017-2018 season).



Any dataset of UV irradiance collected in the Arctic, if properly presented and discussed, is undoubtedly interesting and potentially worth publishing. However, the manuscript in its present form is not suitable for publication, and must be significantly improved in both its form and content. Indeed, results are often presented in a semi-qualitative way, they are quickly mentioned without a proper interpretation and without an in-depth, quantitative discussion (e.g. "On some days the values were below the long-term minimum", "the monthly averages were below the long-term values", "daily doses were much larger", "continuously above the long-term average", etc.). Basically, the "so-what" question remains unanswered: what is the relevance of the results? What are the novelty and the final message of the article? What were exactly the objectives of the study? Some of the conclusions are trivial, such as that "in Antarctica the main factor determining the UV levels is total ozone" (l. 384) or that "lower cloudiness in similar SZAs means more UV radiation reaches the ground" (l. 266-267) or that "higher average albedo will lead to higher recorded UV doses" (l. 297). Do they represent the outcome of the study? And, most of all, are the authors sure they formally proved these statements?

More specifically, the many proxies (cloud cover, total ozone, aerosol optical depth, surface albedo) presented in Sect. 2.2.4 should be better exploited to quantify the influence of the various atmospheric factors on UV irradiances at the surface and explain the observed changes. Indeed, all factors are discussed separately, without rigorously proving their connection and their impact on UV irradiances. Instead, narrow-band or spectral information, in place of spectrally-integrated (broadband) quantities, should be employed to better disentangle the effect of different variables (cf. Specific comments). The statistical analysis should also be improved, trying to quantify the significance of the observed differences/variations and discussing the limitations of the chosen indicators (e.g., how much can daily maximum UV indices change due to cloudiness, and how representative can they be for the study?). Moreover, additional information about instrumental calibration, processing, QA/QC, and traceability of all used datasets should be provided, in order to convince the reader that datasets from different stations and

times can be properly compared. Finally, the discussion on the comparison of measurements in Marambio and other Antarctic stations (which is even part of the title) should be considerably expanded (e.g. by addressing the geographycal distribution of the UV irradiance in Antarctica and not only, as it is now, comparing the 2017-2018 and 2018-2019 seasons - using quite incomplete datasets, by the way).

Specific questions

As anticipated above, the manuscript should be improved from the following angles:

1. additional details about the instruments (stability, different spectral/angular sensitivities, QA/QC) and their corresponding datasets (calibration/traceability, corrections, etc.) should be provided. Since different kinds of instruments, furthermore from different stations/institutes (Sect. 3.3) and at different times (Sect. 3.1-3.2) are studied, it is of paramount importance that comparability of the data is ensured. For this purpose, it would be useful that some of the details discussed in manuscript acp-2019-930 by Lakkala et al. (now withdrawn) were included here. Uncertanties for the specific datasets (not only for the general kind of instrument) should be also clearly reported, in order to assess the significance of the observed differences. For instance, does any comparison between 2000-2008 and recent datasets make sense with 19% expanded uncertainties? Finally, from a more technical point of view, some readers could be interested in knowing how difficult it is to provide reliable measurements and cope with the harsh conditions of the Antarctic continent. For example, is an "inner temperature of 40°C" (l. 153) inside an instrument easy to keep?

2. a more refined analysis should be performed on the dataset. This involves:

2a. a better use of the spectral information included in the measurements and of the ancillary data. As far as I understand, every antarctic station includes at least one instrument with narrow-band or even spectral capabilities, with channels centred at wavelengths where the ozone absorption is strong or weak. By analysing separately the different spectral components, the effects of ozone absorption might be disentan-



gled from that of other (more spectrally-flat) factors, such as clouds. Indeed, from the abundance of proxies mentioned in Sect. 2.2.4, the reader would expect a more advanced, multivariate analysis including all the data, and a quantification of the relative importance of each factor, which is however missing in the present text. At the moment, only the maximum daily UV index and UV daily doses (which, except for a time integral, convey the same information) are used (they are also derived quantities, not directly measured by the radiometer). Average doses in the UV-A band are reported very quickly in Sect. 3.1 (l. 298-303) without any plot, table or a proper discussion ("other factors also contribute", l. 303). Yet, measurements at this wavelengths could be important to assess the effect of clouds. Without this in-depth analysis, statements such as "in Antarctica the main factor determining the UV levels is total ozone" (l. 384), "lower cloudiness in similar SZAs means more UV radiation reaches the ground" (l. 266-267) or "higher average albedo will lead to higher recorded UV doses" (l. 297) remain unproven and too general, and make the reader wonder what the point of Sect. 3.1 is. The problem here - I guess - is how much each factor contributes, and which factors are the most important based on a convincing analysis. For example, in November and February 2018-2019, cloud cover is higher compared to the same months of the previous season, but UV irradiance is also greater. Intuitively, this is likely due to the ozone increase in 2018-2019, but this is not proven here (a wary reader could suspect that the instrument drifted!).

Besides spectral analysis, other methods such as use of RAFs, data splitting between clear and cloudy days, analysis at fixed SZAs (to say nothing of radiative transfer calculations!) could be employed to assess the relative importance of each environmental factor on UV levels.

Finally, I would like to draw attention to the fact that tables (1-2) are not the best way to report information about the effects of the different variables, to understand the correlation with UV levels and to facilitate quantitative interpretation.

2b. improved statistics. The statistics provided in the text are very basic, and they

could lead to wrong interpretations. For example, in Fig. 1 the maximum UV index ranges from 2.5 at the South Pole to 7.9 in Palmer. Also, Marambio and Palmer, which are relatively close to each other, show a rather large difference of 1.7. Are these differences a result of "chance" (e.g., short-term effects due to clouds) or are they really representative of different kind of environments? Another example, taken from the abstract (l. 25-26): "The maximum UV index (UVI) in Marambio was only 6.2, while, during the time period 2000-2008, the maximum was 12". Are the authors sure that the maximum yearly/daily UV index is the best indicator to be used in the text? Maxima are suceptible to very specific conditions of clouds, ozone, and period when the instrument is in operation. Can they use more robust statistics, other than maximum values, or indicators? Could they show some statistical distributions, at least?

Overall, a more quantitative approach should be considered throughout the text. Just quoting the abstract: l. 24, "Measurements in Marambio showed lower UV radiation levels in 2017/2018". Can you quantify the decrease? Can you assess the significance of this difference? L. 20, "the radiation levels were below average": how much below?

To help the authors strengthen this part, and to improve readability, I would suggest creating a new section on the analysis "Methods" employed in the study.

3. State the objectives of the study in the Introduction in a more specific (and less ambitious) way than "discover the temporal variation in UV irradiance levels ... and see the results in spatial context". Indeed, if discovering temporal variations of UV radiations in Marambio is the purpose of the paper, the authors themselves must admit that this was not achieved in the study ("definitive conclusions ... cannot be made based on only two seasons", l. 381-382, this also contradicting what previously stated in the Introduction: "Now, 9 years later, it is possible to search for signals of changes in UV radiation that could reflect the observed changes in the levels of stratospheric ozone"). Also, analysis of the "spatial context" (the second point of the stated purpose of the study) is very poor. Rather, resize the aim stated in the Introduction and split it in more specific, verifiable goals/scientific questions to be answered throughout the text



and in the Conclusions.

Technical corrections

- title, "... in a wider temporal and spatial context": too general, it does not mean anything. Could you rephrase it to be more specific?

- l. 20, "radiation": please, be more accurate. Measurements are of "downward global irradiance of UV radiation";

- l. 34, "10 to 400 nm": did you mean 100 nm? Usually, extreme UV is not considered in the solar spectrum;

- l. 35, "absorption in the atmosphere": too vague, can be removed and explained later (l. 38);

- l. 36, "geometrical" or "astronomical"?

- l. 37, "geophysical" or "atmospheric and geophysical"?

- l. 38, "all absorb or scatter": confusing, some of them only absorb or only scatter;

- l. 43, "spring" –> "Antarctic spring" (or define months);

- l. 46, "the loss of stratospheric ozone has stopped": unclear, the "ozone hole" still recurs every year;

- l. 46-52: text should be reorganised, since concepts and bibliographic references repeat;

- Fig. 1: including information about total ozone in the figure would be useful to understand the effects of clouds on max UV indices;

- Sects. 2.1.1 to 2.1.6: the description of the stations should be homogenised (i.e., the same characteristics for different stations should be mentioned), and it should be limited to the relevant topics for the paper. If, as stated in the Introduction, cloudiness, surface albedo, total ozone and aerosol load are important parameters affecting the

UV irradiance, then basic information about these parameters should be provided for each station. Also, is the horizon the same at each site?

- l. 135: it is not described how the spectrum is derived from single narrow-band measurements. This information is needed before discussing erythemal doses and UV indices;

- l. 136: specify here how the "maximum" UV index is defined (i.e., on what average time interval - one-minute averages are mentioned only later, at line 159);

- l. 136: summarise how ozone is derived, how perturbation by multiple scattering in clouds and from the surface is overcome/taken into account in the ozone retrieval;

- l. 137: provide more information about calculations, and refer to Sect. 2.2.3 (or anticipate the contents of this section here);

- l. 138, "well-calibrated": provide details about calibration;

- l. 138, "can be within 5%": so, what is the uncertainty for the specific dataset in Marambio? Is it indeed 5%?

- Sect. 2.2.2: why was the instrument replaced with a new one?

- l. 154: although the instrument is described in detail in another work, it is important to summarise here the main outcomes, since they are useful for the interpretation of the present results

- l. 156: similarly to the previous point, it is important to summarise what corrections were applied

- l. 163, "show some wavelength dependency": what does it mean?

- l. 168, "in the Northern Hemisphere": how does that relate to the southern hemisphere?

- l. 169, "range of 1 +/- 0.05": it is not clear that this is a ratio at this point;



- Fig. 2: is there are drift of the ratio from 2017-2018 to 2018-2019?

- l. 174: it is not clear from here why this climatology is useful (the reader understands only later in the text that climatological reference values are calculated from these data);

- l. 181: does it also apply for Marambio?

- Sect. 2.2.4: the proxy data are here discussed without providing an explanation of their use to the reader. This makes reading confusing, please anticipate how the proxies are employed in the analysis;

- l. 186, "provide a chemical isolation": unclear;

- l. 186-187: rephrase this sentence. Why are you talking about "a" station?

- l. 192, "octants": do you mean "oktas"?

- l. 199-203: this paragraph can be removed, just cite the reference publication;

- Sect. 2.2.5: shouldn't the numbering be 2.3?

- Sect. 2.2.5: which of these instruments are actually used in the analysis?

- l. 217, "PE": don't use acronyms that were not introduced before and that are not recurrent;

- l. 217, "double Brewer": did you mean "double-monochromator Brewer"?

- l. 226, "factory calibration": this factor has never been updated?

- l. 226, "a procedure": any bibliographic reference?

- l. 240, "... not synchronized with the changes in SZA": so, what is your explanation? Can you anticipate here an answer?

- l. 262, "for the months during which the solar irradiance is the highest": why September and March are not included?

- l. 263, "average cloudiness was lower": can you be more quantitative?

- Table 2, caption: explain what are the numbers in parentheses; "AOD", include wavelength;

- Table 2, "values that contribute to the higher UV levels in season... are in bold": is there any assessment of the statistical significance of the differences to establish if these values contribute relevantly to higher UV levels? Are differences between seasons greater than their uncertainty?

- l. 276, "the disparity is especially large": can you be more quantitative (e.g., significance)?

- l. 295-296, "slightly higher": please, be more quantitative;

- l. 301, "was larger": how much?

- l. 306: "there were periods": which ones?

- l. 310 and 315, "On some days" ... "several days": how many? Are the difference significant?

- Fig. 7: the white line (climatological average) is not clearly visible;

- Fig. 7 and 8: x-axis should report dates (not day of year) for comparability with previous figures and with the dates mentioned in the text;

- l. 338, "The results from the analysis of O3 data are in good correspondence to the recent recorded UV levels": how can the authors state that the correspondence is good?

- l. 344-350: how important are the observed could cover changes on UV irradiances at ground?

- l. 356, "pattern in" –> "is". However, how can a pattern be seen, from this incomplete dataset?



- l. 358, "the peaks in spring are mainly caused by ozone": have you proven it?

- Fig. 10: what is the grey line in the first panel? Why are data missing?

- l. 379, "a number" ... "several": exactly what numbers?

- l. 395, "analyzis" –> "analysis"

---

Interactive comment on Atmos. Chem. Phys. Discuss., https://doi.org/10.5194/acp-2019-896, 2019.