Answer to RC1

Author replies are in red

The paper deals with the description of UV indices evaluation using the Earth observation system in Europe. This, so called UV-Index Operating System, or UVIOS exploits both radiative transfer models and the data available from Meteosat Second Generation and Meteorological Operational Satellite-B as well as the information available from Tropospheric Emission Monitoring Internet Service, Copernicus Atmosphere Monitoring Service and the Global Land Service. The simulations include the account of main factors affecting UV radiation: ozone, clouds and aerosols as well as ground elevation and surface albedo with resolution of 5 km and 15 minutes. This work is highly important "for the provision of operational early warning systems that will help raise awareness among European Union citizens of the health implications of high UVI doses" as the authors wrote. I like the idea of this approach and its technical solution and can recommend paper for publishing. However, I have several comments, which are presented below. Natalia Chubarova

We want to thank the reviewer for all the valuable comments and suggestions. We believe that following the proposed revisions our study was substantially upgraded.

1. row 57. I would recommend to add the references to the numerous EEAP reports (for example, ENVIRONMENTAL EFFECTS OF OZONE DEPLETION AND ITS INTERACTIONS WITH CLIMATE CHANGE: 2014 ASSESSMENT).

Reference to EAAP report has been added to the revised version of the manuscript.

2. row 73. The only mentioning UV-A as a spectral region, where the NO2 play important role seems to be misleading. See, for example, Table 7-1 from the Ozone Assessment 2006 (Chapter 7) concerning the role of NO2 and SO2 effects on erythemal irradiance. Should be clarified in the text.

We tried to clarify this sentence by expressing as follows: "Atmospheric gases play a crucial role in attenuating UV irradiance, specifically NO2 is a major absorber in this spectral region (e.g. Cede et al. (2006)), while O3 is the main absorber at lower (UVB) wavelengths".

3. Organic gases like formaldehyde can be also important in both UV-B and UV-A regions. I would recommend to re-write this part taking this into account.

We have added the following sentence to the updated manuscript: "Other gases that have significant absorption in the UV include SO2 (Fioletov et al., 1998) and HCHO (Gratien et al., 2007), but their –usually-smaller atmospheric abundances, result in minor effects to incoming UV (with major exceptions such as volcanic incidents)."

4. row 103. The areas with extremely high positive UV trends over Northern Eurasia over the 1979-2015 period were shown recently in (Chubarova et al., 2020). (https://www.mdpi.com/2073-4433/11/1/59).

We have added the following sentence: "Chubarova et al. (2020) found a long term increase of 3% per decade in UV at Northern Eurasia, for the 1979-2015 period". Thank you for the excellent suggestion.

5. row 108. The reference should be given concerning the turnout point in UV trend in 2007.

The reference to Zerefos et al. (2012) has been added.

6. row 145. This is not exactly so, since the method proposed by Jean Verdebout used geostationary Meteosat instruments data. This should beaccounted for in the text. (Verdebout, J., A method to generate surface UV radiation maps over Europe using GOME, METEOSAT, and ancillary geophysical data, J. Geophys. Res., [Atmos.] 105, 5049–5058, 2000.)

The correct reference of Verdebout is provided in the new version of the manuscript. Thank you for the careful reading.

7. row 160. The authors should begin this part mentioning that using their approach they could combine information on input parameters from different satellite sources to provide the better quality UV estimates. I would recommend to re-write the text.

In the revised manuscript we provide a completely updated version of this paragraph. Now we believe that the presentation of this part, as an introduction to the UVIOS system, has been substantially improved. Thank you for the valuable comments and suggestions.

8. row 188. I do not see the information on factor of asymmetry of aerosol phase function in the list, which is one of the important aerosol parameters, necessary for model simulations. Also I do not see the cloud amount parameter in the list. I understand the difficulties with its application but this should be discussed here in the text.

The asymmetry factor was also retrieved from the MACCv2 climatology (Kinne, 2019). The specific information has been added to the manuscript. Analytical discussion for clouds is already provided in lines 192-196, as well as a reference wherein more information can be found (Taylor et al., 2016). We do not believe that further discussion for clouds is necessary.

9. row 198-199. The references should be provided to the internet link at least.

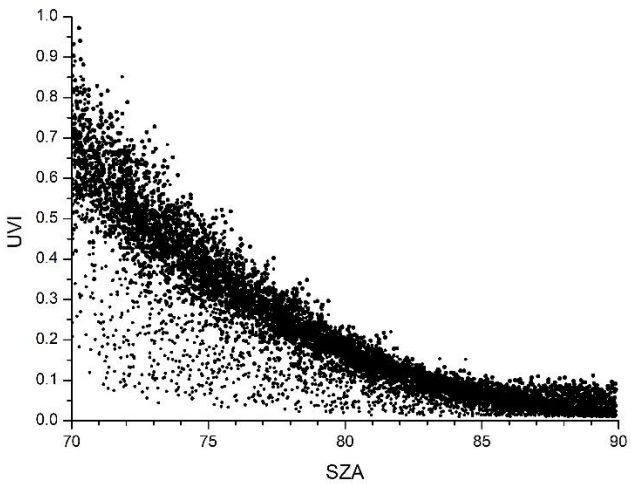
References have been added.

10. row 231. The title should be changed. Like ”The description of the geophysical parameters”, for example.

We agree with the proposed new title which describes better this subsection. It was changed in the updated version of the manuscript.

11. row. 248. “However, since such measurements are associated with very low UV Index (<1). Depending on different parameters (ozone, cloud amount).” Should be proved by simulations.

This part was revised accordingly in order to be clearer and indicate the cloud information availability and reliability issues under high solar zenith angles (>70 deg). Concerning the fact that under such high solar zenith angles the accompanied UVI levels are <1, below we plotted indicatively the UVI data of Thessaloniki for solar zenith angles >70 degrees. The new version of this paragraph changed as follows: “UVIOS calculations at high solar zenith angles (>70 deg) are retrieved assuming cloudless skies since the MSG COT product is not available in these conditions, facing reliability issues (Kato and Marshak, 2009). This has an effect on the quality of the UVIOS overall performance at high solar zenith angles, where there is no cloud information as input to the model in order to quantify the consequent impact on UVI. However, such measurements under high solar zenith angles are accompanied with very low UVI levels (<1) both in the performed RTM simulations and in the ground-based measurements. This inconsistency, even if does not affect UVIOS UVI results associated with dangerous effects on human health, nevertheless it is still affected by the rest of input parameters (i.e. ozone, aerosol etc) mitigating the UVIOS uncertainty in the absence of cloud information under such high solar zenith angles. There is more discussion in the next section on how we use these data for the UVIOS validation.”



The retrieved UVI data of Thessaloniki for solar zenith angles >70 degrees.

12. row 261. The reference should be given to the Albedo product. The parenthesis is missed.

Done.

13. row 268. I would recommend also to add the reference to Table1 here.

The reference to Table 1 was added.

14. row. 271. What is the range of overestimation?

The range overestimation during 2017 is from 0 to 0.4 in terms of modified normalized mean bias (see at Eskes at al., 2015), with the same range of values over the study region (Europe). We revised this part accordingly in order to include the above information.

15. row 279. Misprint (I Note)

Thank you for the careful reading. It was corrected in the updated version.

16. row 316. Previously you mentioned the threshold of 75 degrees for MSG COT retrievals (row 246)? Should be clarified.

The 75 degrees in row 246 was a misprint. It was corrected in the updated version of the manuscript.

17. row after 333. I do not find the information on altitude correction. Since all other factors are analyzed here it should be also discussed here even you have the detailed analysis after.

The suggested information was added in the manuscript as follows: "Although UVIOS simulations were corrected for changing UVI with respect to altitude (see Section 3.2.3), the correction cannot be perfect for higher altitude stations. The reason is that it is not possible to take into account all different factors (aerosol load and properties, atmospheric pressure, surface albedo) (e.g., Blumthaler et al., 1997; Chubarova et al., 2016) which affect the change of UVI with altitude."

18. row 338. I would propose to replace "while" on "and"

This paragraph has been completely re-written.

19. row 340-348. I would propose to re-write this part in a more compact way. This is obvious.

The specific section has been re-written in a clearer and more concise way.

20. row 357. Remove the extra dot, please.

Removed.

21. row 370-371. The values should be given.

The values were added in the document.

22. row. 452. The reference should be given or it should be clarified that this has been obtained using model simulations provided by the authors.

The provided numbers are based on the UVIOS simulations. A clarification sentence has been added.

23. row 458. “conditions”

Corrected.

24. row 468. – “conditions”.

Corrected.

25. row 475 – change to: and “in case”

Corrected, thank you for the careful reading.

26. row 500. Concerning the changes with altitude: there are other factors in addition to TOC, which may influence the altitude dependence like aerosol and surface albedo. Please, look for details in our paper (Chubarova et al., 2016, ACP, https://acp.copernicus.org/articles/16/11867/2016/)

Related discussion has been added in the revised Section 2.4.

27. row 507. The estimates in term of UVI should be made here to be consistent with other sections.

This part was revised accordingly in order to include the surface elevation correction and effect in terms of UVI. The updated text is the following: “Indicatively, the average maximum surface elevation correction in terms of UVI for the DAV station (due to UVIOS input deviation from to actual elevation) was of the order of 1.6 (15%), while for INN and AOS it was 0.5 and 0.6 respectively (6%) and for the VAL station close to 0.8 (8%).”

28. row 543. SSA? Seems to be misprint. SSA is usually used as single scattering albedo abbreviation. Here you describe the albedo effects. If you are speaking about real SSA, this should be made closer to aerosol effect discussion.

This part of the manuscript was not clearly written. The discussion was indeed for the single scattering albedo (SSA), and now it has been moved directly after the discussion for the AOD uncertainties.

29. row 926. “result in”

Corrected.

30. row 1036. It would be nice to see in this Table also the RMSE and R statistics, like in Table 5.

In the updated manuscript, the RMSE and r were added in Table 4, similar to Table 5.

31. Figure 6. The name of Y-axis should be changed.

The name of Y-axis in Fig. 6 was changed into “UVI difference (TEMIS – Brewer)”.

32. row. 1275. (a) – is not clear. To my understanding the angular dependence due to 3D geometry should be taken into account. Please, clarify.

In the revised manuscript, this part has been explained more clearly in the text and in the Figure 15 (a) caption. With this plot we simulated the shadow volume at the surface level of a cloud relative to the SEVIRI angle view, as a function of various cloud heights and solar zenith angles. To this direction we tried to highlight in a simple way the observed ray tracing by SEVIRI in the presence of clouds, and hence, the angular dependence due to the 3D geometry. We want to thank the reviewer for this apposite remark and the overall analytical review that upgraded our manuscript. Thank you.