Atmospheric Measurement Techniques Discussions

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Comment on amt-2020-506

Nataly Chubarova (Referee)

Referee comment on "Real-time UV index retrieval in Europe using Earth observation-based techniques: system description and quality assessment" by Panagiotis G. Kosmopoulos et al., Atmos. Meas. Tech. Discuss., https://doi.org/10.5194/amt-2020-506-RC1, 2021

Review of the paper

Real-time UV-Index retrieval in Europe using Earth Observation based techniques and validation against ground-based measurements by Panagiotis G. Kosmopoulos et al.

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

Comments:

- 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).
- 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.
- 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.
- 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).
- row 108. The reference should be given concerning the turnout point in UV trend in 2007.
- row 145. This is not exactly so, since the method proposed by Jean Verdebout used geostationary Meteosat instruments data. This should be accounted 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.)
- 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.
- 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.
- row 198-199. The references should be provided to the internet link at least.
- row 231. The title should be changed. Like ”The description of the geophysical parameters”, for example.
- 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.
- row 261. The reference should be given to the Albedo product. The parenthesis is missed.
- row 268. I would recommend also to add the reference to Table1 here.
- 271. What is the range of overestimation?
- row 279. Misprint (I Note)
- row 316. Previously you mentioned the threshold of 75 degrees for MSG COT retrievals (row 246)? Should be clarified.
- 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.
- row 338. I would propose to replace “while” on “and”
- row 340-348. I would propose to re-write this part in a more compact way. This is obvious.
- row 357. Remove the extra dot, please.
- row 370-371. The values should be given.

- 452. The reference should be given or it should be clarified that this has been obtained using model simulations provided by the authors.
- row 458. "conditions"
- row 468. – "conditions".
- row 475 – change to: and "in case"
- 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/)
- row 507. The estimates in term of UVI should be made here to be consistent with other sections.
- 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.
- row 926. "result in"
- row 1036. It would be nice to see in this Table also the RMSE and R statistics, like in Table 5.
- Figure 6. The name of Y-axis should be changed.
- 1275. (a) – is not clear. To my understanding the angular dependence due to 3D geometry should be taken into account. Please, clarify.

Please also note the supplement to this comment:
https://amt.copernicus.org/preprints/amt-2020-506/amt-2020-506-RC1-supplement.pdf