Atmos. Chem. Phys. Discuss.,
https://doi.org/10.5194/acp-2019-793-RC3, 2019

Atmospheric Chemistry and Physics
Discussions

Interactive comment on "Global Distribution and 14-Year Changes in Erythemal Irradiance, UV Atmospheric Transmission, and Total Column Ozone 2005–2018 Estimated from OMI and EPIC Observations" *by* Jay Herman et al.

Anonymous Referee #3

Received and published: 13 November 2019

The manuscript by Herman et al., "Global Distribution and 14-Year Changes in Erythemal Irradiance, UV Atmospheric Transmission, and Total Column Ozone 2005–2018 Estimated from OMI and EPIC Observations" presents a study of estimated surface UV and 14-year trends from Ozone OMI time series, as well as UV estimates utilizing measurements from the Earth Polychromatic Imaging Camera (EPIC). The topic of this manuscript is relevant and interesting and suitable for the scope of the journal. However, I see many areas where the weaknesses and uncertainties of the applied methodology were not properly discussed and addressed. I consider it takes a major

revision, before the paper is modified and revised to the form, which can be accepted.

I strongly agree with the other reviewers and do not repeat those points all here. However, I do want to further stress few points particularly in the evaluation of the Anonymous Referee #2.

Absorbing aerosols. In this methodology no effort is done to account for that effect. However, it is a strong source for potential bias in satellite-based surface UV. And it can be a strong and wrong source also for the trend estimate, since any real trend in absorbing aerosols shows up as an erroneous trend in surface UV. And absorbing aerosols make a two-fold effect. Increasing absorption as such means a reduced level in surface UV, which this method does not take into account at all. But this absorption effect results additionally high-biased cloud modification factor, CT. In case of increasing fraction of absorption, for a given AOD, the TOA reflectance decreases, which in the current method means higher CT value and thus higher surface UV. Unfortunately, this impact is then just opposite to the true impact of increased aerosol absorption in the surface level UV.

So the above reasoning makes the reader wonder how much there is this effect involved for instance in the Figure 18. By the way, I assumed there was a typo, so it should be Russia-Indonesia (not India) and not 120W, but 120E. Is this right? There are typically very strong fires in Indonesia (and peat fires are particularly strongly absorbing at UV, while there is not much absorption at visible) and also discussion about the long-term trends in the fires activity. So, there should be some discussion about these effects (if those regions were included at all in the analysis).

In addition to the absorbing aerosols, it was surprising that nothing was said about areas of potential "snow contamination" in the estimated UV. If -60 to 60 latitudes are included, there are still large areas of seasonal snow cover. Moreover, these are also regions of likely trends in this snow cover. About both aspects, Bormann et al. 2018 is illustrative, there are significant regions within -60 to 60 with seasonal spring time snow cover variability and trends have been also detected of snow melt occurring earlier. Based on what you wrote, one would assume that you used surface reflectivity of 0.05 and same constant everywhere (although it was not stated explicitly). Then, over snow covered regions, this means the satellite measured "excess" reflectivity due to the high snow reflectivity in reality, is put erroneously to the cloud attenuation (meaning too low CT value). Similar to the problem of absorbing aerosols, this has now double effect. Higher surface reflectivity should result in higher surface UV due to the surface reflectivity alone. But in your method, the surface reflectivity (enhanced by snow) is not considered and moreover too strong cloud attenuation is assumed, both aspects contributing to the too low surface UV. This means that there are regionally large biases in the estimates surface UV, but perhaps even more importantly that there can be large artificial biases and errors in the trend estimates too. These things should be considered (or at least discussed thoroughly).

Bormann, K.J., Brown, R.D., Derksen, C. et al. Estimating snow-cover trends from space. Nature Clim Change 8, 924–928 (2018) doi:10.1038/s41558-018-0318-3

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