Atmos. Chem. Phys. Discuss.,
https://doi.org/10.5194/acp-2019-793-AC3, 2020

Atmospheric Chemistry and Physics Discussions
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Interactive comment

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.

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Received and published: 19 February 2020

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 modiïňĄed 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 modiïňĄcation factor, CT. In case of increas- ing fraction of absorption, for a given AOD, the TOA reïňĆectance 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.

Reply: The effect of absorbing aerosols is now included based on the work of Torres et al. (see revised paper page 5). The paper has been extensively revised and reorganized so that marking all the small individual changes in the text is not feasible. Most of the changes are listed in detail in response to reviewer #2 and the extensive major changes are marked in yellow in the revised manuscript.

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 was a typo as the referee stated. However, the old Figure 18 is no longer in the paper There are typically very strong ïňĄres in Indonesia (and peat ïňĄres are particularly strongly absorbing at UV, while there is not much absorption at visible) and also discussion about the long-term trends in the ïňĄres 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 signiïňĄcant 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 reïňĆectivity of 0.05 and same constant everywhere (although it was not stated explicitly).

Reply: The reflectivity varies as a function of latitude and longitude based on TOMS data (Herman and Celarier), but the average value is 0.05.

Then, over snow covered regions, this means the satellite measured "excess" reïňĆectivity due to the high snow reïňĆectivity 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 reïňĆectivity should result in higher surface UV due to the surface reïňĆectivity alone. But in your method, the surface reïňĆectivity (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 artiïňĄcial biases and errors in the trend estimates too. These things should be considered (or at least discussed thoroughly).

Reply: The spatially resolved reflectivity climatology data set Rg used was derived from TOMS data (Herman and Celarier, 1997) for snow/ice free conditions (this is stated on page 20 of the revised manuscript and was in the original). However, I found that use of

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Discussion paper

a nominal single average reflectivity RG = 0.05 for snow/ice free conditions makes little difference in the time series (i.e., well within the error bars for trend estimates) in the results and trends. Surface reflectivity in the UV is small almost everywhere (most land, oceans, and vegetation). An exception are some desert regions in Libya, where small regions can reach 0.1. White Sands National Park in the US (gypsum sand) is another small region where RG is larger than usual. This paper is a study of cities, where these exceptional conditions do not apply. Most of the cities considered in this study have little or no snow. Some, like Moscow, Russia have considerable snow in the winter, but none in the summer months when erythemal irradiance is a maximum. The current study does not take into account the effect of snow cover for the considered latitude range. The effect is that erythemal irradiance is underestimated in winter conditions usually when the SZA is large and erythemal irradiance is small due to atmospheric absorption and scattering.

Reply: The following sentence has been added on page 4: The effect of snow and ice on the surface reflectivity during winter months has been ignored in this study. This means that the already low amounts of erythemal irradiance during winter in high latitude cities because of high solar zenith angles is further underestimated in the presence of snow and ice.

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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