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Interactive comment

Interactive comment on “Restoring the top-of-atmosphere reflectance during solar eclipses: a proof of concept with the UV Absorbing Aerosol Index measured by TROPOMI” *by* Victor Trees et al.

Anonymous Referee #1

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General comments:

The manuscript by Trees et al. describes a technique to correct for the change in the top-of-atmosphere (TOA) solar spectral irradiance during a partial or annual solar eclipse. The technique is based on earlier works by Koepke et al. (2001), Bernhard and Petkov (2019) and Ockenfuß et al. (2020). However, the authors generalized these calculations by also including the case of an annular eclipse (The formulas presented by Koepke et al. (2001) only consider partial and total eclipses). The method makes satellite measurements collected during a solar eclipse available for the retrieval of

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Discussion paper

properties of the Earth's atmosphere. The technique is sound and the validation results show convincingly that it is working. The topic is suitable for Atmospheric Chemistry and Physics.

My main issue is that a solar eclipse is a very rare event and applicability of the method is therefore limited. If data from one or two satellite orbits have to be discarded because of contamination by the Moon's shadow, the data loss is minor and interpolations using data from adjacent orbits should satisfy most needs. The more interesting question is whether the technique is accurate enough to detect changes in atmospheric properties during an eclipse and could help to evaluate atmospheric processes initiated by an eclipse. For example, there could potentially be a change in aerosol properties during the eclipse because aerosol hygroscopic growth is likely affected by the reduced air temperature (and the resulting effect on relative humidity) during an eclipse. For the correction method described by the authors to be useful, the uncertainty of the correction must be smaller than the expected change in aerosol properties induced by an eclipse. What is the evidence that the uncertainty is indeed sufficiently small? The authors should try to estimate the uncertainty of their correction as it applies to the ultraviolet (UV) Absorbing Aerosol Index (AAI). This would further demonstrate the strength of their method and increase the scientific relevance of the paper.

The authors chose to validate their method by comparing retrievals of the UV AAI with and without correction for the Moon's shadow. The UV AAI is a hard-to-interpret indicator of aerosol absorption properties. An alternative metric would be the aerosol absorption optical depth (AAOD), which is similarly defined as the widely-used aerosol optical depth (AOD), except that optical depth refers to the absorbing part of aerosols only and not to the extinction (from absorption and scattering), as it is the case for the AOD. Hence the AAOD is a more useful quantity to describe aerosol absorption properties than the AAI. It would be helpful if the authors could briefly explain how the AAI relates to the AAOD and/or provide a reference.

Specific comments

The abstract should be improved for clarity. Technical terms that are not commonly used should be avoided or defined. For example:

– L7: “eclipse obscuration fraction” is not a commonly used term. While I don’t object to its use after it is properly defined, it might be better to either avoid this term in the abstract, or provide a short verbal definition.

– L9: The sentence “We verify the calculated obscuration with the observed obscuration using an uneclipsed orbit.” is misleading. The paper compares *data products* obtained with and without obscurations, not obscurations per se.

– L12: The sentence “. . .would result in [. . .] in a maximum Moon shadow signature in the AAI of 6.7 points increase.” is difficult to understand without reading the paper first and should either be reworded or deleted.

L54: Regarding: “Such wavelength-independent approximations of the eclipse obscuration fraction based on the the overlapping disks indeed could work well to estimate the shortwave fluxes.” “works well” should be quantified. Weather an approximation “works well” depends on the desired accuracy. Also, the word “the” before “overlapping” is repeated.

L90: Regarding “I depends on mu = cos(theta) where theta is the viewing zenith angle,” This is misleading as it could be interpreted that I(theta) = I (0) * cos(theta). If the Earth were a Lambertian Reflector, the radiance would be independent of the viewing angle theta (i.e.: I(theta) = I(0)). Since the Earth is not a Lambertian Reflector, the radiance *will* depend on the viewing angle. While this dependency could be expressed as a *function* of cos(theta), this is not clear from the sentence. I suggest to just mention that I depends on the viewing angle without mentioning cos(theta).

L94: “ The fraction of sunlight that is blocked by the Moon is the eclipse obscuration fraction, f_0.” This is not a good definition as “sunlight” is not a physical quantity. Furthermore, because of solar limb darkening, f_0 is not defined by the geometric area of the solar disk that is blocked by the moon, but depends on wavelength – see for example Figure 7. This should already be mentioned here. To emphasize the wavelength dependence, the symbol f_0(lambda) should be used instead of f_0. So I would say:

"The fraction of the TOA spectral irradiance E_0(lambda) that is blocked by the Moon is the wavelength-dependent eclipse obscuration fraction, f_0(lambda). The remaining solar spectral irradiance at TOA is (1-f_0(lambda))E_0(lambda)." In general, it would be helpful to add "(lambda)" after all spectral quantities that depend on wavelength.

Eqs. (6) and (7). Please replace E_0 and f_0 with E_0(lambda) and f_0(lambda) to emphasize that these are spectral quantities like I(lambda). See also Figure 7.

L290: Before explaining how to interpret the AAI, its definition (i.e., Eq. (16)) should be presented and explained.

L292: While it may be possible to calculate the AAI in the present of clouds, is the result of any value? Absorbing aerosols are typically close to the surface (at least in the vicinity of urban centers) and cannot be "seen" by a satellite below a moderately thick cloud.

Adding to my general comment, it is beyond the scope of the paper to discuss the value of the AAI to characterize aerosol absorption. Still, the authors should better explain why they chose this parameter to validate their correction method. For example, Eq. (16) could lead to a AAI different from 0 for the case of non-absorbing small aerosol particles. Hence the AAI could potentially indicate absorbing aerosols when in fact non-absorbing aerosol was present.

Figure 15: The scatter is rather large. So the figure's value to validate the correction method is rather limited for $X > 0.7$. This could be mentioned.

Technical comments

L41: "have been taken," > "have been observed," L51: Why “instead”? The verb “approximated” already implies that this is a simplification.

L168: low > small

L241: For clarity, please explain “scanline”. (E.g., the line at Earth’s surface defined by the satellite swath that is roughly oriented East-West)

L275: “Sect..” > “section.”

L317L Moon > Moon’s

L331: 110405 > 11405

L358: delete “still”

L415: What is “chord”?

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