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Comment on acp-2021-599

Anonymous Referee #2

Referee comment on "Inferring iron-oxide species content in atmospheric mineral dust from DSCOVR EPIC observations" by Sujung Go et al., Atmos. Chem. Phys. Discuss., https://doi.org/10.5194/acp-2021-599-RC2, 2021

This is a very valuable study that describes a methodology of deriving hematite and goethite concentrations from the UV-VIS single-viewing space imager EPIC aboard the DSCOVER. Obviously, deriving such detailed aerosol property as hematite and goethite concentrations from this type of space sensor requires numerous hypothesis and a priori information, however, the authors do quite a careful work on their educated definition. The methodology is then applied for a series of carefully selected case studies that fulfill the conditions for an optimal application of the methodology. The study also presents a revision of a large diversity of the refractive indexes of hematite and goethite reported in literature, and an analysis justifying the choice made in the study. Elements of validation of the obtained hematite and goethite concentrations using in situ data are presented as well, which is not an evident task for satellite derived aerosol property. I would like also to acknowledge the work done on revision of the literature. The study is certainly in the scope of ACP, it is a solid work and indeed worth the publishing. I have just a few main questions which I believe addressing could strengthen the study, but I leave to the authors to decide if to include the elements of reply in the manuscript or not.

The evaluation of the uncertainty in the derived percentage of iron oxide due to different refractive indexes assumed is very interesting and informative (Fig. 11). I would think about at least two other assumptions that require evaluation of the uncertainty in the derived concentrations. First, the real part of the refractive index, which is fixed. However, generally, the values of real and imaginary parts are related and the imaginary part is varying here. Specially, the real part of iron oxides is quite high (Fig. 1, a), so the real part of mixture is expected to vary quite a bit depending on the iron oxide fraction. I would suggest calculating the effective refractive index and simulate satellite signal for internal mixture of nonadsorbing dust host and iron oxides for the corresponding real and imaginary refractive indexes and then deriving (under the fixed real part assumption) the hematite and the goethite concentrations for this synthetic signal. How will it compare to the initially used hematite and goethite fractions? Indeed, it is mentioned in the paper that Di Biagio et al. 2019 conclude that the real part is generally source- and wavelength-independent with a range of 1.48-1.55, but this range seems to be big enough to cause the derived iron oxides fractions variability. Second, 1 km aerosol height is assumed. The assumption is justified by generally good agreement of EPIC derived absorption with AERONET, but this is on average and fluctuations in specific cases are expected. The dust over hot desert surfaces, specially over Sahara, is lifted to rather higher than 1 km altitudes and sensitivity of UV to the dust altitude is known. What if to conduct a similar test as in the case of real refractive index? That is, to simulate the satellite signal for dust at different altitudes and invert for the iron oxides fractions using the fixed (1 km) dust altitude. These exercises can evaluate uncertainty in the derived fractions due to these two assumptions and provide a valuable error bar. The effect of presence of carbonaceous aerosols (mixture with smoke) can be evaluated in the same manner.