Reviewer # 1

Review on "*Aerosol direct radiative effect over clouds from a synergy of OMI and MODIS reflectances*" by de Graaf et al.

I had three major concerns/questions for the original manuscript. The first is about the anisotropy factor, the second is about why OMI and MODIS observed cloud reflectances differ significantly when their overpassing time is only 15 minutes away, and the last question is about the sampling rate of method described in this paper for deriving DRE of above cloud smoke. The authors have addressed these major concerns/questions carefully and thoroughly.

However, I still have a few minor questions and comments left. They have to be addressed before the manuscript can be accepted for publication.

- Even in the revised manuscript, the definition of the DRE derived from the combined OMI-MODIS observation is still not clear and precise enough. As pointed out in *[Zhang et al.*, 2016], the all-sky DRE of aerosol is defined as $DRE_{all-sky} = f_c\overline{DRE_{cloudy}} + (1 - f_c)\overline{DRE_{clear}}$, where f_c is the cloud fraction, $\overline{DRE_{cloudy}}$ and $\overline{DRE_{clear}}$ is the averaged cloudy-sky and clear-sky DRE, respectively. Take a hypothetical example. Assuming that we have an OMI-MODIS pixel with a cloud fraction $f_c = 0.5$. The $\overline{DRE_{cloudy}}$ due to above-cloud smoke is 40 Wm^{-2} and $\overline{DRE_{clear}}$ is 1 Wm^{-2}. Which of the following values does the method described in this paper reports? 1) $\overline{DRE_{cloudy}}$=40 Wm^{-2}, 2) $f_c\overline{DRE_{cloudy}}$=0.5*40 Wm^{-2}=20 Wm^{-2}, or 3) $DRE_{all-sky}$=20 Wm^{-2} +0.5*1 Wm^{-2}=20.5 Wm^{-2}. This question should be clarified early in the paper, for example, in Section 2. It is an important question because the answers will help the readers understand precisely the meaning of the DRE from this study, as well as how to compare the DRE from this study with previous ones such as *[Zhang et al.*, 2016].
- Another question, which is related to the question above, is about how to scale the OMI spectrum to match MODIS observation. If I understand correctly, the reflectance of a cloudy pixel observed by OMI can be decomposed into $R_{OMI} = f_{c,OMI}R_{cld+aer} + (1 - f_{c,OMI})R_{clr}$. Similarly, the reflectance observed by MODIS is $R_{MODIS} =$

$f_{c,MODIS}R_{cld+aer} + (1 - f_{c,MODIS})R_{clr}$. It is not clear to me what the "scaling" in section 3.5 means. Is the "scaling" intended to match R_{OMI} and R_{MODIS}? What is the "scaling" factor and what is its physical meaning? These questions are important, and they need to be clarified in the context of the above equations.

- Page 2 line 20, there are a few noteworthy previous studies on the DRE of above cloud aerosols that might deserve being cited here, e.g., *[Peters et al.*, 2011; *Feng and Christopher*, 2015; *Zhang et al.*, 2016] *and a very recent study [Kacenelenbogen et al.*, 2019]. Some discussion should be made about the originality and significance of the current study w.r.t. these previous studies as well as those from the leading author.
- Page 4, equation (3), again what is the exact definition of DRE_{aer} here? See my first and second questions above.
- Page 7, similarly, what is the DRE derived from *SCIAMACHY?* Is it $\overline{DRE_{cloudy}}$, $f_c\overline{DRE_{cloudy}}$ or $DRE_{all-sky}$?
- Page 10, line3, "*and 0.35 in the red pixel*". Should it be "*and 0.35 in the blue pixel*"
- Also, what does FRESCO stand for?

Feng, N., and S. A. Christopher (2015), Measurement-based estimates of direct radiative effects of absorbing aerosols above clouds, Journal of Geophysical Research-Atmospheres, *120*(14), 2015JD023252–n/a, doi:10.1002/2015JD023252.

Kacenelenbogen, M. S. et al. (2019), Estimations of global shortwave direct aerosol radiative effects above opaque water clouds using a combination of A-Train satellite sensors, *Atmospheric Chemistry and Physics*, *19*(7), 4933–4962, doi:10.5194/acp-19-4933-2019.

Peters, K., J. Quaas, and N. Bellouin (2011), Effects of absorbing aerosols in cloudy skies: a satellite study over the Atlantic Ocean, *Atmos. Chem. Phys*, *11*, 1393–1404.

Zhang, Z., K. Meyer, H. Yu, S. Platnick, P. Colarco, Z. Liu, and L. Oreopoulos (2016), Shortwave direct radiative effects of above-cloud aerosols over global oceans derived from 8 years of CALIOP and MODIS observations, *Atmospheric Chemistry and Physics*, *16*(5), 2877–2900, doi:10.5194/acp-16-2877-2016.

Reviewer # 2

I reviewed a previous version of this manuscript. My main issues with the previous version were (1) discussion of the anisotropy factor B and related uncertainty; (2) choice of a threshold UVAI=0 as a baseline for the uncertainty calculations; and (3) the fairly simplistic nature of the 2016/2017 data analysis (mentioning ORACLES but not using the data). In this revision the authors have expanded the discussion of (1) and (2), which I appreciate, and added MODIS and OMI above-cloud satellite time series for (3) while noting that the comparison with ORACLES data is better suited for a separate paper (which I hope they do). This makes it more convincing, in my view, than the previous submission.

As a result I do not have technical objections to the publication of this manuscript, although the other reviewer (Z. Zhang) is more of an expert in the forcing aspect than I am, so I would defer to their judgment.

I have a few minor comments, but otherwise find the manuscript acceptable for publication after technical corrections. I would be happy to review these corrections if the Editor feels it would be helpful, although I do not think it is necessary, provided the other reviewer is satisfied.

Previous comment on POLDER: the authors had cited a paper in preparation which compared the OMI/MODIS results against POLDER. I'd suggested the authors provide the results here or remove the reference, since we can't see the results otherwise (given it's a paper which has not been submitted yet). They replied that they have added the information and also removed the citation. It's not clear to me where the information about this comparison has been added, as it doesn't seem to be in the original section; there is a brief mention in section 4.2 of POLDER but that seems to be it? Mentioning just in case something was inadvertently omitted, but I think it is ok as-is.

Page 7 line 27: a reference for the MODIS sensor should be added (one is already provided for the other satellite instruments used). I don't have a particular strong feeling about which, but Salmonson et al (1989) is often used: https://ieeexplore.ieee.org/document/20292

Page 10 line 2: Acronym FRESCO needs to be defined at first use.

Page 21 line 4: Acronym AERONET needs to be defined at first use. Also, state which version you are using and provide a reference. It should be the current version 3, with citation Giles et al (2019): https://www.atmos-meas-tech.net/12/169/2019/amt-12-169-2019-discussion.html Also define the data level being used. I see this is level 1.5 rather than the standard level 2; after checking the AERONET website I see level 2 is not available yet for Ascension Island in 2017. It is worth mentioning the difference between levels and stating why level 1.5 is used here.

Page 22 lines 30-32: VIIRS and OMPS acronyms should be defined at first use. Also, there is more than just SNPP now, NOAA20 (formerly JPSS1) launched in late 2017.

Section 4.3.3 and more generally: in this section (and elsewhere) the authors say “error” often. I think a lot of these times, they really mean “uncertainty”. For example, page 19 line 9 I think the authors mean the “uncertainty” in the DRE retrievals, not the error, since we don’t have a truth to compare to. If possible it would also be good for the authors to clarify whether the estimates they provide in the paper refer to typical levels of uncertainty (e.g. 1-sigma), maximum likely uncertainty, or similar.