

Interactive comment on “Fu-Liou Gu radiative transfer model used as proxy to evaluate the impact of data processing and different lidar measurement techniques in view of next and current lidar space missions” by Simone Lolli et al.

Anonymous Referee #2

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First of all, I apologize for my late review.

This manuscript aims to understand and quantify the relative importance between the impact of retrieval method and data processing on radiation at the top of the atmosphere (TOA) and at the surface, used as a measurement/processing guideline for ground-based lidar networks and current/future satellite missions. Analyses and conclusions are based on two case studies, one dust and the other cirrus cloud. While this

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manuscript fits the scope of AMT, I feel that more substance is needed for a publication.

1) I am afraid that my main concern is the substance of the manuscript. I strongly support the idea of using radiation as an ultimate evaluation metric, but I feel that the manuscript was submitted too early and that the content is very much on the thin side. To make this manuscript useful, it would be good to address the following issues:

a. The representativeness of cases: I agree it is not necessary to present overwhelming cases, but a synthesis from many cases is needed. This issue becomes even more crucial when the manuscript claims to be “in view of next and current lidar space mission”, which is about a global scale and a longer time scale. I like grand statements like that to tell readers what the paper is about, but we also need to be careful not to oversell it. To be scientifically rigorous, I would think that the authors need to get the climatology of dust layer and cirrus clouds (either doing analyses on their own or taking information from the literature) to provide context of whether these two cases represent the majority of the observations, or they are actually outliers. Without that context, we really cannot say much from two cases. Once the climatology is available, then the authors can carefully select cases and think about a strategy how to best cover a wide range of dust/cirrus characteristics.

b. The methodology: The authors recognize the need of actual radiation measurements for their work, but unfortunately, they didn't go further to do it. For ice clouds, there is a BAMS paper <http://journals.ametsoc.org/doi/pdf/10.1175/BAMS-88-2-191> talking about radiation closure. Although that paper focused on inter-comparison of various retrieval methods and had a different purpose from the manuscript, it shows how sensitive shortwave/longwave fluxes and radiances are to ice cloud properties. Without comparing with radiation measurements, it is hard to know if the retrieval shown in the manuscript is good enough to be used to provide any recommendation. Additionally, the current form very much just reports numbers of “net radiative forcing” without any discussions. Note that there can be compensating errors from input variables in radiation calculations, so the resulting radiative effects should be discussed in more

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detail.

2) The manuscript title is unnecessarily complicated and does not capture the key points. Essentially, this manuscript uses various input aerosol/cirrus properties (from retrieval) to compute radiative fluxes at TOA and at the surface, and then uses these fluxes to evaluate whether retrieval itself or the vertical resolution of profiles plays a more important role in the resulting fluxes. With this objective, radiative effect is the important component, not the choice of the radiative transfer code. Any decent radiative transfer code can do the work. I also don't think proxy is the right word to use. A title is supposed to be precise and to grab attention. For the sake of the authors, I will strongly recommend changing the current title to a simple yet effective one that truly reflects what has been discussed.

3). Following the comment above, it will be better to highlight why the Fu-Liou-Gu code works well for this study. My guess is that it has a rather sophisticated way to characterize optical properties for both aerosol and ice clouds, which is worth mentioning.

4). The misuse of radiative forcing. While some people loosely use radiative forcing and radiative effects and treat them like they are the same, they are, by definition, not the same. I believe what the authors did in the manuscript is calculating radiative effects, not forcing, although no description is ever given in the manuscript. Please clarify and describe it clearly.

5). Referencing could be better. For example, the first paragraph in Introduction should use some proper, more specific citations. And, Page 2, Line 4: Surely, Holben et al. (1998) is the standard citation for AERONET. But to demonstrate "Cloud and aerosol optical properties have been studied... ", papers using AERONET for studying cloud and aerosol should be added here. Also, it would be better to recognize and include studies using ARM or Cloudnet or ACTRiS observations. Same comments for satellite observations.

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