Comments by Reviewer #2

> 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.

We would like to thank the reviewer for the meaningful comment. However, if from one side we agree that two cases are not enough (for this reason we added two more cases with an opaque cirrus clouds and a biomass burning event) on the other, we we were not trying to oversell our research but think that our manuscript lacks of clarity because the main goal is to evaluate the differences in term of net radiative effects among that the more sophisticated and simpler different lidar techniques. In theory, for the purpose of this manuscript, it can be used synthetic signals instead of real measurements, where the optical and geometrical aerosol and cloud properties are well known and quantify how the lidar technique/data processing affects the raidative transfer calculation, using FLG as metric. Our cases aims to show the existence of these not negligible differences arising from the diversity of lidar techniques/data processing, for the first time quantitatively. The statement that the reviewer is happy with the use of an RTM as the metric to assess the systematic effects in the retrieval of aerosol forcing using lidar is a strong encouragement for us to continue this work and assess the impact on much larger dataset.

> 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 intercomparison 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 detail.

We agree that the microphysics parameterization of the cirrus cloud plays a fundamental role in calculating the net radiative effects of cirrus clouds and aerosol layers. For our calculations we used the empiric parameterization as found in Heymsfield et al., 2014. As stated in the paper mentioned in the comment, each parameterization shows pros and cons. However, as stated in the previous answer, our analysis can be carried out in principle on synthetic signal where the microphysics is fully known and still quantitatively describe the differences for the different retrievals. In fact, we are interested in the relative values between different lidar techniques/data processing. To reach this goal we use a RTM on the different retrieval and calculate the relative discrepancies (we applied the same parameterization for all the retrieved profiles). In future analysis we are going to take into consideration different parameterizations. Nevertheless we added some additional paragraphs where we clarify our choice and state how different parameterizations can affect the results citing properly the suggested BAMS paper.

> 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.

Agreed that the word "proxy" is misused and generates confusion. For this reason we changed completely the manuscript title into a simpler form:"Impact of the different lidar measurement techniques and data processing on evaluating cirrus cloud and aerosol direct radiative effects.". This new title version we think is simple and clear and really reflects what has been done in the paper.

> 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.

Agreed, we added a paragraph to describe in detail how the Fu-Liou-Gu radiative transfer model works and why it works good for reach the objective stated in the manuscript.

> 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.

We agree that the word "forcing" is often misused. Of course we calculate the net radiative effect of cirrus clouds and aerosol layers. We added a paragraph to describe the computation we performed and we substitute in the entire manuscript the word "forcing" with "effect".

> 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.

We agree and we changed accordingly the manuscript adding and acknowledging ARM, Cloudnet and ACTRIS work.