

# ***Interactive comment on “Optical depths of semi-transparent cirrus clouds over oceans from CALIPSO infrared radiometer and lidar measurements, and an evaluation of the lidar multiple scattering factor” by A. Garnier et al.***

## **Anonymous Referee #1**

Received and published: 13 March 2015

In the paper “Optical depths of semi-transparent cirrus clouds over oceans from CALIPSO infrared radiometer and lidar measurements, and an evaluation of the lidar multiple scattering factor” Garnier et al. present an interesting analysis of cirrus properties from multiple spaceborn sensors. The use of independent retrievals and perfectly collocated measurements gives insight in the assumptions in the related retrieval algorithms, making it a relevant publication for many researchers. The paper could benefit, however, from a more clear definition of the research question and a better structure in the presentation to better highlight these interesting results.

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For example:

1) The authors should think if a different title would reflect better the content of their paper. The current title indicates that the paper is about the optical depths of semi-transparent cirrus, while the main results are related to improvements in the retrieval algorithms of the two used instruments. Additionally, the final part of the title “and an evaluation . . .” makes this part of the paper sound as an independent addition, while it is a core part of the main argument.

2) In the introductory section, it is stated that “the relationship between infra-red absorption and visible extinction optical depth is investigated in detail”. Together with the title, this indicated that a main result of the paper will be the study of this relationship though instrument synergy. In contrast, in the main body of the paper, this ratio is derived by simulations, and used as a reference to correct the CALIOP retrieval.

3) Several methods and descriptions are presented in various sections of the the paper. For example, section 4.2 (p. 2158, l. 13-18) introduces a new dataset, based on the reported two-way transmittance in CALIPSO products, when the constrained technique is not selected. I feel it would be easier for the reader if this dataset was introduced in the beginning together with the other CALIPSO products. Similarly, the discussion section 4.5 introduces some equations for the apparent lidar ratio  $S^*$ , these also should be introduced earlier.

In total, I think that the paper title, introduction, and structure could be modified to better highlight the results and arguments of the authors. I give more examples of such in the specific comments below. I invite the authors, however, not to address them in a one-to-one basis, but rather rethink how to better highlight the main argument of their work.

Specific comments:

p. 2146: l. 19-: A more detailed review of literature is missing in the introduction.

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References for more recent research dealing with similar questions as the one studied here would help put the present paper in context. For example the work seems to have similar approach and questions with Josset et al. 2012. You should mention how your work is similar / different with such previous studies.

p. 2149 l. 4: Mineral dust aerosols can be found in specific regions / latitudes. Please mention if this could introduce any biases in your analysis.

p. 2150 l. 26: Please provide some justification (citation, previous experience, ...) why the change from 0.3K to 0.5K is enough to account for the expected possible differences. For example, the bias of the two  $R_{bg}$  estimates could be checked by calculating 1) true, 2) 100km, and 3) modelled values in cloud-free scenes.

p. 2152 l. 24 - 25: Extinction profiles in cirrus clouds using the constrained technique have not been introduced before (e.g. in section 2.1)

p. 2156 l. 7: Why did you select only the specific latitude band? Please explain and comment if/how this choice will affect the representativeness of your results.

p. 2156 l. 11 – 14 (also p.2157 l. 7 – 11): The two different sets of cirrus clouds (type 1, type 2) should be introduced in more detail, given their importance in the following sections. Currently the relevant information is spread and harder to follow. For example, do the two datasets actually sample different types of cirrus? It would be useful to provide some physical intuition why you expect the two types to have the different properties described in p.2157 l. 7 – 11. Do the two subsets have similar geographical distribution?

p. 2158 l. 12 – 18: You introduce here a new dataset for cirrus optical depth. I feel it would be more clear to introduce all used datasets in the beginning (e.g. section 2).

p. 2160 l. 8, 11: More references are needed to better support your claim.

p.2160 l. 11-22: An interesting technique is introduced in these lines, a key part of the paper's argument. As before, I would expect to mention it, together with other

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used algorithms and datasets, in the start of the paper, or at least mention it in the introduction.

p. 2161 l. 11 – 22: This is a very nice description of multiple scattering factor. However the factor has already been discussed in many parts of the paper. It's fine here, but I suggest it would be more useful at the beginning of the paper.

p. 2166 l. 11: “microphysical parameters” typically refers to particle size, shape etc. “Extensive properties” should be used instead to describe lidar ratio and depolarization factor.

p. 2167 l. 19-20: Please discuss how these values compare with other published in the literature.

Technical corrections:

p. 2153 eq. (7): The typesetting of the exponents should be improved.

p. 2158 l.10: please rephrase “as optical decreases”

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Interactive comment on Atmos. Meas. Tech. Discuss., 8, 2143, 2015.

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