Response to Referee #1

We are very thankful to the reviewer for excellent suggestions that will contribute to greatly improve the clarity of the paper. In the following, the reviewer's comments are in bold and are followed by our response.

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.

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 semitransparent 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.

REPLY: The title could be changed to:

"Lidar multiple scattering factors inferred from CALIPSO lidar and IIR retrievals of semi-transparent cirrus clouds optical depths over oceans"

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.

REPLY: we will add the word "*retrievals*" in the title, and we will also change this statement to:

"the relationship between infrared absorption and visible extinction optical depth ***retrieved from CALIPSO*** *is investigated, based on…".*

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.

REPLY: this data set will be introduced in Sect. 2.1.

Similarly, the discussion section 4.5 introduces some equations for the apparent lidar ratio S*, these also should be introduced earlier.

REPLY: these equations will be moved to Sect. 2.1, which will also be reorganized to also introduce more clearly the apparent optical depth and the visible optical depth, the extinction profiles, as well as the extended data set.

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.

REPLY: we will follow this suggestion.

Specific comments:

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

REPLY: the reference "Lamquin et al. (2008)", which was initially later in the paper, and the reference "Josset et al. (2012)" will be in the introduction, page 2146, after line 23.
The following will be added in the revised version of the manuscript:

"Previously, Lamquin et al. (2008) conducted a closely related study by combining infrared retrievals from Atmospheric Infrared Sounder (AIRS) with apparent optical depths retrieved by the authors from co-located CALIOP measurements. More recently, Josset et al. (2012) conducted a similar analysis using IIR data and apparent optical depth retrievals constrained by ocean surface measurements from both CALIOP and CloudSat. Here, infrared absorption optical…"

In addition, the following sentences will be inserted in Sect. 4.4, page 2163, after line 12:
"Lamquin et al. (2008) also find larger values of η_T *at temperatures colder than 210 K than at 230-240 K. Josset et al. (2012) report a mean value* $\eta_T = 0.61 \pm 0.15$ *for mid-layer temperatures colder than 233 K, but by taking* $(\tau_{vis}/\tau_a)_{expected}$ *from Eq. (10) constant and equal to 2.25."*

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.
REPLY: The sentence should actually read:
"scenes containing ***dense*** *depolarizing aerosol layers such as mineral dust are discarded."*

We will add the following sentence:
"They represent less than 1% of the total number of scenes".

p. 2150 l. 26: Please provide some justification (citation, previous experience, ...) why the change form 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.
REPLY: we write that the random error is "*arbitrarily*" augmented from 0.3K to 0.5K" (p2150, line 27), and as such recognize that we are not providing or even attempting any justification. The intent is to give an order of magnitude rather than an accurate assessment, because this random error estimate has no impact on our analyses.

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)
REPLY: they will be introduced in Sect. 2.1.

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

REPLY: The following sentence is now inserted at the end of the introduction of Sect. 4 (p2156, after line 23):

"Tropics are chosen for this discussion for simplicity, because biases due to computed R_{BG} are known to vary with latitude (Garnier et al., 2012a), but this does not affect the representativeness of the 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 are 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?

REPLY: type 1 and type 2 clouds will be defined in Sect. 2.2 where we introduce the notion of measured (type 1) and computed (type 2) background radiance (page 2150, lines 18-26). This part of the text will be (changes in bold):

"The background radiance, R_{BG}, is preferably retrieved from cloud-free observations in neighboring pixels along track as identified by CALIOP at a distance chosen to be smaller than 100 km from the analyzed pixel. ***The cloud layers for which these conditions are fulfilled are identified as "type 1 clouds"****. If these conditions are not found, R_{BG} is computed using the FASt RADiative (FASRAD) transfer model (Dubuisson et al., 2005) and ancillary atmospheric and surface data from the GEOS 5 model of the Global Modeling and Assimilation Office (Rienecker et al., 2008).* ***This second ensemble of cloud layers is called "type 2 clouds". Type 1 and type 2 clouds*** *will be evaluated separately as their sources of uncertainty are different. Indeed,* ***for type 1 clouds****, R_{BG} is derived purely from observations and is expected to be unbiased with respect to measured radiances".*

More details will be added in Sect. 4.1. Page 2157, lines 7-11 will be replaced with:

"Type 1 and type 2 clouds are mutually exclusive and appear to have different properties. As seen in Fig. 5c, the fraction of type 2 clouds is larger at colder temperatures and the number of type 1 clouds is not significant at 193-203 K. Type 2 clouds are found to represent more

than 85% of the analyzed clouds in the Western Pacific, in the Indian Ocean, and in the Atlantic Ocean, and to represent 76% overall in the tropics. Type 1 clouds are expected to be isolated cloud systems of small horizontal dimension or at the edge of large systems, whereas type 2 clouds are expected to be embedded in large cloudy areas. This is consistent with the fact that most of the type 1 clouds have a geometric thickness Δz between 1.5 and 3 km whereas type 2 clouds are deeper, with Δz mostly between 3 and 6 km and up to 8 km (not shown)".

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).
REPLY: this dataset will be introduced in Sect. 2.1

p. 2160 l. 8, 11: More references are needed to better support your claim.
REPLY: Heymsfield et al. (2014) is a recent paper and includes measurements from numerous campaigns in various atmospheric conditions, which is why we chose this reference. We will write:
"(Heymsfield et al.,2014, and references herein)".

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 used algorithms and datasets, in the start of the paper, or at least mention it in the introduction.
REPLY: section 2.2 will start as follows (new sentence in bold):
"The IIR is a passive instrument providing calibrated radiances in 3 channels in the atmospheric window (8.65, 10.6, and 12.05 μm), with a medium spectral resolution of about 1 μm, and a spatial resolution of 1 km per pixel over a 69-km swath. ***IIR channels are optimized for retrievals of cirrus optical and microphysical properties, such as ice crystals effective diameter (Garnier et al., 2012a, 2013).*** *The IIR 12.05 μm channel, which exhibits the largest absorption by cirrus clouds, is chosen for this analysis."*

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.

REPLY: the text previously located p2162, lines 12 to 22 will be moved to the introduction, page 2145, after the sentence ending line 26.

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.

REPLY: the sentence will read:

"*Several extensive parameters related to ice crystal microphysics are retrieved*…"

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

REPLY: the following will be added:

"These findings are qualitatively consistent with airborne observations over the Pacific Ocean as reported by Yorks et al. (2011)."

Technical corrections:

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

REPLY: OK.

p. 2158 l.10: please rephrase "as optical decreases"

REPLY: now reads:

"*as optical depth decreases*"