

Interactive comment on “Scale dependence of cirrus heterogeneity effects. Part I: MODIS thermal infrared channels” by Thomas Fauchez et al.

Thomas Fauchez et al.

thomas.j.fauchez@nasa.gov

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Reply to Anonymous Referee #2

We would like to thank reviewer #2 very helpful comments who has widely contributed to improve the substance and the form of the paper.

The authors present the impact of the horizontal heterogeneity of cirrus clouds on TOA brightness temperatures for 4 TIR MODIS channels. The study is based on a “realistic” cirrus case simulated using the 3DCLOUD model, MODIS Collection 6 ice crystal properties, and the 3DMCPOL radiative transfer code. This study discusses the impact of the plane parallel homogeneous bias (PPHB) and of the horizontal radiative transport (HRT) in various conditions of optical depth, optical depth inhomogeneity, and viewing angles. The paper also discusses the optimum horizontal resolution that

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minimizes the horizontal heterogeneity effects on TOA brightness temperature.

General comments: The simulations and the results are solid. The simulated cirrus case is well adapted to illustrate the PPHB and the HRT. However, the impact of this choice on the conclusions of the paper should be discussed. It would be important to know to what extent these results could be generalized. The main characteristics of the simulated cloud should be given in the abstract (lines 7-9).

After “A realistic 3-D cirrus field is generated by the 3DCLOUD model” we added : “(average optical thickness of 1.4, cloud top and base altitudes at 10 and 12 km, respectively, consisting of aggregate column crystals of $Deff=20$ m)”

The reasoning and the story are sometimes difficult to follow. Introductory and linking sentences would be sometimes helpful for the clarity of the manuscript.

With comments of reviewer #1 and #2 we have improved the clarity of the manuscript, especially in the conclusion.

My recommendation is to publish this manuscript after clarification on the several points listed above and hereafter.

1)Title: The title could specify that this paper discusses cirrus heterogeneity effects on TOA brightness temperatures. “cirrus heterogeneity effects” is too vague, in my opinion.

We agree that the title is not sufficiently explicit. However, because we would like the first sentence of the title to be the same in part II of this study, we prefer not to mention brightness temperatures at this point. We rephrased the title as follow: “Scale dependence of cirrus horizontal heterogeneity on TOA measurements. Part I: MODIS brightness temperatures in the thermal infrared channels.”

2)Goal of the paper: Page 3, lines 17 to 21: Please explain the choice of these 4 TIR channels. In which MODIS algorithm(s) are they used and what are the retrieved geophysical parameters?

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These channels are not currently used to retrieve optical properties with MODO6. They are only used by the operational algorithm to infer cloud and surface temperatures. However, as they correspond to atmospheric windows, future versions of the MODIS standard product may include them. This is already the case for instance with the Imaging Infrared Radiometer (IIR; Garnier et al., 2012, 2013) in retrieving optical thickness and particle effective size. At this point of the introduction the utility of these channels has already been discussed (second paragraph). Therefore, we added the following sentence explaining the interest of these channels to the paragraph concerning thermal infrared retrieval technique:

“For example the Split Window Technique (Inoue, 1985) applied to the Advanced Very High Resolution Radiometer (AVHRR Parol et al. (1991)) and the Imaging Infrared Radiometer (IIR) onboard CALIPSO (Garnier et al., 2012, 2013) is used to retrieve CED and COT from the brightness temperature difference of two different window channels in the infrared atmospheric windows where gaseous absorption is small.”

“... the impact of horizontal heterogeneity...” Please specify impact on which quantity (TOA BT, optical depth, CED, other?).

The impact on both, TOA radiation and retrieved product. We mention that after: “. . . the impact of horizontal heterogeneity on both, TOA radiation and retrieved products,”

3)Realistic cirrus case: The rationale for the choice of the “realistic” cirrus case should be clearly presented. Table 1 should be presented and discussed in more detail. I agree that assuming a “constant” CED of 20 m (page 6, lines 9-12) is “realistic”, but it is not typical nor statistically representative. The fact that TIR techniques are often limited to CED between 5 and 50 m (page 6, line 10) clearly does not mean that all CED are so small (as shown in Table 1). Please clarify the rationale.

We agree that the use of “realistic” needs more details. We have changed the sentence in page 5 lines 32-33: “The simulated cirrus field is thus suitable to study the impact of cloud heterogeneity on radiative transfer at various scales.” to: “To be as realistic as

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possible, we have chosen the properties of our simulated cirrus to be close to average values observed in different studies (reference in Table 1) and set the CED to $20\mu\text{m}$ as the sensitivity of retrievals in the thermal infrared is often limited to CED below 40 m. The chosen cirrus geometry, which corresponds to an uncinus structure is also the most common form. among the variety of cirrus.”

We also have added two nuances on the realism of our simulations (after the previous sentence): “Two nuances should be mentioned here: i) as seen in Table 1, most of the cirrus parameters cover a wide range of values which means that our simulated case, while realistic in the average sense, does not represent more extreme situations. ii) this paper is focused only on. ...”

The impact of this choice on the conclusions of the paper should be discussed. In particular, how does it impact the highlighted difference between the 8.52 m channel and the three other channels?

This is a very interesting remark, indeed when the crystal effective size increase, the single scattering albedo in the different thermal infrared channels tends to converge between 0.5-0.6 (represents the well-known geometric optics lower limit). For instance here are the values for $D_{\text{eff}} = 20\text{ m}$: 0.75, 0.42, 0.47 and 0.51 and $D_{\text{eff}} = 80\text{ m}$: 0.57, 0.51, 0.53 and 0.53 for channels centered at 8.52, 11.01, 12.03 and 13.36 m, respectively, used in MOD06. As you can see, for large crystal size there are less differences between channels which have single scattering coefficients close to the value at 13.36 m for $D_{\text{eff}} = 20\text{ m}$, where the absorption is strong and the scattering weak.

We have added in the conclusion: “Note that these simulations were performed for a unique CED of 20 m, common in cirrus clouds but relatively small. However, for example, increasing CED to 80 m leads to a convergence of the single scattering albedo across all TIR channels towards values between 0.5-0.6 (0.5 being the geometric optics limit). This implies less scattering and thereby less horizontal transport in the 8.52 m channel (in this study). The differences between channels should thus be weaker

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and consequently the impacts on cloud optical property retrievals, which depend on the radiance relative difference between channels. Also, because single scattering albedo values for all the channels at $\text{Deff} = 80$ m are close to that at 13.36 m for $\text{Deff} = 20$ m used in this study, all the channels for $\text{Deff} = 80$ m will have a similar heterogeneity effect on TOA BT across spatial resolutions than for the 13.36 m channel presented in this study.”

Page1, line 7: “A unique but realistic cirrus case is simulated...”: Why is the cirrus case “unique”? Do you mean that only one case is simulated?

We made several simulations from a single cirrus fields. We have rephrased the sentence as “A single but ...”

4)Averaging and aggregation: Please define “averaging” and “aggregation”, and use consistent terms throughout the paper. Below are some examples (there are more in the text):

We should use averaging instead of aggregation, because this is a linear averaging that we performed on BT or optical thickness. We have modified aggregation into averaging in the whole manuscript.

Page 7: line 17: “...averaged to the scale being considered...”. Please detail the averaging process. Which parameter? We now mention that this is an arithmetic averaging.

We have changed “RT” into “radiances” which is the quantity arithmetically averaged and then converted to BT. We added: ... “averaged to the scale being considered and converted to BT (for simplification reason, we will refer this process as BT averaging.”

Page 7, line 26: “..aggregation..” Please explain what “aggregation” means.

Aggregation has been replaced by averaging in all the manuscript.

Page 7, line 30 : “..the averaged BT..” Are you averaging BT? I am surprised because the observations are radiances (same comment page 10, line 8).

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As mentioned two questions earlier we now specify that these are the radiances which are averaged and then converted into BT.

Page 10, line 7: “, while 1-D BTs are directly computed at the xkm scale after aggregating the 50 m optical thickness” My understanding is that 1-D BT are computed using an averaged optical depth. Is is what you mean?

Yes this is what we mean.

We rephrase it as: while 1-D BTs are directly computed at the xkm scale from the averaged optical thickness.

5)Other comments (mostly for clarification): Page 3, lines 24-25: “we describe the heterogeneity and 3-D effects” For more clarity, it is suggested to specify PPHB and IPAE (or horizontal radiative transport).

Done

Page 5, line 9: Figure 1, caption: what is ‘Cirrus 1’?

We have deleted all the reference “cirrus 1” as only one cirrus has been used in this study.

Page 5, line 29: “For the cirrus used in this study...” Is it cirrus 1 listed in Table 1? Please clarify. Introduce Table 1 earlier. The references listed in Table 1 should be presented and discussed in the text.

Yes, as only one cirrus has been simulated we removed “cirrus 1” from the text.

Thanks to one of your previous questions, we now give more details in the text concerning this table. We also now reference the authors listed in the caption of the table directly in the text after: “. . . listed in the literature (. . .)”

Page 5, line 34: ‘...vertical variability of the geometrical and optical thickness..’ Please clarify. I don’t understand the notion of vertical variability of such quantities.

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We have changed “vertical variability of the geometrical and optical thickness” to “vertical variability in optical properties”

Page 6, line 3: for more clarity, title of Sect. 2.2 could be “ice crystal optical properties”.

We agree. Done.

Page 6, line 4: “cirrus optical property parametrization”: not entirely clear to me...what about “bulk scattering properties? Is there really a parametrization?”

We have changed “parametrization” to “coefficients”. We have also removed “bulk” which is confusing.

Page 6, lines 5-6: “Note that TIR....between 5 and 50 m”. Why this sentence here?

We deleted this sentence.

Page 6, lines 7- 9: “...Holz et al. (2015) better consistency between ...the IRsplit-window technique....and (VNIR/SWIR/MWIR) techniques, as well as with lidar retrievals.....”. This sentence is very confusing and I do not think that it is entirely correct. You are talking about the consistency between techniques and retrievals. Are you talking about retrieval of optical depth, or CED, or both? “Split-window technique” suggests CED. “Lidar retrievals” suggests “optical depth”. Holz et al. (2015) discuss only optical depths, but not CED. Please clarify.

To avoid confusions, we have remove “lidar retrievals” from sentence.

Page 6, line 32: “... as will be explained...” Specify in which section.

We now mention section 4.

Page 7, line 21: Figure 5 According to the caption, this is now optical depth at 0.86 m not introduced earlier. Please explain.

This was a labeling error, all optical thicknesses in this study are at 12.03 m.

Page 7, line 33: “decreasing” resolution can be misunderstood. The notion of coarse

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or fine resolution would avoid any confusion.

Indeed, we replaced it with “coarsening resolution”

Page 8, lines 8-13: The authors are discussing Fig. 5, and I am surprised to find these 6 lines with results from another paper. Why not discuss BT 3-D – BT 1D from Fig. 5?

At this point of the manuscript we do not yet discuss the new results. Thus, we reference previous studies to introduce the new results.

HRT section: please re-organize the text for more clarity. - Lines 1-2 page 9 (HRT effect only when BT from 3-D and 1-D at the same resolution of 50 m) should be at the beginning of this sub-section, because important for a good understanding of the discussion.

We believe that this sentence is better here because the assertion “3-D and 1-D BT are computed at the same spatial resolution (50m)” is valid only for Fig. 6 and 7 in this section.

- Figure 6: it is suggested to add arrows to point to the areas of specific interest discussed in the text. A second panel showing BT differences between 3-D and 1-D could be helpful.

We think that adding an arrow would not be useful here because we refer in the text to the region as a function of the optical thickness which is clearly seen regarding the right Y-axis. Also, another panel could overload the information in the figure.

- page 8, line 29: can you give an example of cloud optical property retrievals that use a combination of the 8.52 m and 13.36 m channels?

The cloud top property retrievals require the use of MODIS channels centered at 8.52 m and 13.36 m.

We changed the sentence to “... will impact cloud-top property retrievals (emissivity, cloud top height, etc.)...”

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- Figure 6, caption: I don't see the BTs computed at 11.01 and 12.03 m.

This was an error, they are not in the figure. We have removed such a reference from the caption.

Lines 5- 6, page 9 (“as seen in Fig. 6...”) could be useful earlier in text the when Fig. 6 is described.

We modified the sentences “This effect is stronger at 8.52 m where the cloud scattering is significantly larger and cloud absorption smaller. As a result the BT differences between 3-D and 1D are larger at 8.52 m than at 13.36 m “ to the following: “The 3-D BT fields looks more homogeneous than the 1-D BT field where no smoothing occurs. Because this difference is amplified with the number of scatterings, the channel at 8.52 m shows a stronger smoothing than at 13.36 m, ...”

- page 9, line 8: “..negative BT values dominate because fewer FLIPs come from thick and cold areas, decreasing the BT of these pixels..”. Why “fewer”?

The “fewer” is confusing and useless, we have removed it.

- Page 9, lines 12-25 and Figure 7: for more clarity, it is suggested to superimpose averaged BT (FLIP) vs optical depth. These simulations are using CED=20 m. Would the difference between the 8.52 m channel and the 3 other channels be as important for a larger CED, for instance 100 m? I think that it should be discussed.

We do not quite understand what is meant by “superimpose averaged BT (FLIP) vs optical thickness”.

We added this sentence in page 9 before line 25: “Note that, according to MOD06 ice radiative models, the single scattering albedo of large ice crystals in the other channels will converge to values close to that of the 13.36 m channel at CED=20 m. Therefore, the HRT in the three other channels will be similar to that of the channel centered at 13.36 m. “

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-Page 9, line 25: In my opinion, this sentence is a little weird.

We have clarified this sentence as follows: “Obviously, the effect of both PPHB and HRT on TOA BT strongly depends on the spatial resolution as discussed in the next section.”

Page 12, line 1; “ We can also see in Fig. 8 (b)” Are you actually discussing both Fig. 8a and 8b? Please clarify.

Yes, we refer at both Fig. 8(a) and (b). We thus removed the “(b)”

Page 12, lines 7-8: “... When the effects on BTs are roughly the same for all channels, the MAD... impact on retrieved products may be mitigated (not show here) “ Please develop. Are your referring for instance to larger CED? If yes, I think that it should be shown.

No, we just mention here that differences between the curves for small pixel sizes are smaller than for large pixel sizes. This means that the horizontal heterogeneity and 3-D effects are less wavelength dependent for high spatial resolutions than for coarse ones. We added these sentences: “Note that these differences are dependent on the CED for which the single scattering albedo varies with wavelength. For very large CED (>80 m) the single scattering varies less between wavelengths (about the value of CED =20 m for 13.36 m), reducing differences between channels and therefore the overall impact in the retrieval.”

Page 12, line 14 to page 13, line 24: - The total number of pixels found in the 4 optical thickness categories is 52131. I was expecting $40000+10000+1600+400+100+40+16+1=52157$, which is close. Please explain the difference between these 2 numbers. - The total number of pixels found in the 4 optical thickness heterogeneity parameters categories is 12129. I was expecting $10000+1600+400+100+40+16+1=12157$, which is close. Please explain.

We made a mistake when calculating the number of pixels for the very large optical

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thicknesses and very large optical thickness heterogeneity. Because of rounding, we missed some pixels. We have corrected the value now to be 1,089 and 117 pixels, respectively.

How is the heterogeneity parameter computed? Is the definition given page 13 line 4 the same as page 5, line 16? I am not sure because the reference is different. Please clarify.

This is Szczap et al., (2000) and not (2014), thank you for having notified this.

Page 14, lines 11-13: I don't fully understand. Looking at Fig.12, I would say that the saturation in BT appears at about 8 at 30 degrees and at about 9 at 0 degrees. Please clarify and perhaps illustrate the "saturation" in Fig.12.

We agree and have modified the values accordingly.

Page 14, line 22: "...We can also see this in Fig. 13 (f) where.." Please describe Fig.13 first. Fig. 13 and Fig. 12 could actually be shown and discussed before Fig. 11.

We agree with the reviewer. Fig.12 becomes fig. 11, fig. 13 becomes fig. 12, and fig. 11 becomes fig 13. The text associated to the figures has also changed.

6)Technical comments:

Page 1, line 18: in Earth's climate and radiative budget

Done

Page 2, line 1: "cirrus clouds reflect part of the incident solar radiation into space due, but this albedo effect is generally negligible..." It looks like something is missing

The "due" was too much. We have remove it.

Page 2, lines 5 and 6: "by taking accurate observations of their optical properties" Please rephrase.

".. by improving the retrieval of cirrus cloud optical properties"

Page 2, line 8: “from microwave to visible ranges” Please specify, for instance spectral ranges.

Done (few millimeters) and (up to 0.4 m)

Page 2, line 35: Top Of Atmosphere (TOA): not consistent with page 1, line 2.

We remove the capital letter in page 2 line 35 and add “ the”

Page 3, line 6: (under 20 m). Please specify. Do you mean CED under 20 m?

Yes, we now specify CED.

Page 3, lines 17-18: this sentence should be rephrased.

This sentence was unclear, we rephrased it to: “However, because such studies focus only on stratocumulus clouds, which are very different from cirrus and because they were only conducted for the common imager solar reflectance channels, their conclusions cannot be simply extrapolated.”

Page 3, lines 22-24: the long sentence is confusing. As it is, I read that the ice crystal model used in MOD06 is simulated by the 3DCLOUD model.

We added a “then we discuss on” between the two parts of the sentence.

Page 7, line 23: “we see that 3-D and 1-D BTs, decrease “ delete comma

Done

Page 8, line 2: “...Fauchez et al. (2012, 2014) have shown...”

Done.

Page 9, line 4: “highly asymmetric regarding” I don’t understand.

We have replaced it by “very dependent on”

Page 9, line 7: “ for very largest values..” : for the largest values? Please quantify.

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We changed it to “very large values”

Page 9, line 19: “ the emission temperature between large optical thicknesses”. I don’t understand.

We replace it by “ the brightness temperature..”

Page 11, line 23: ‘....rapidelly “ rapidly

Done

Page 11, line 24: “..through this is more clearly visible at 500 “. even though?

Yes, we replaced “through” by “even though”

Page 11, line 32: “ the single scattering albedo is about 0.3 larger than the value “. Please rephrase.

We rephrased it: “. . . 0.3 above the value. . .”

Page 12, line 32: ‘...we decided pixels..” Please rephrase

We replaced it by “we sampled”

Page 13, line 13: ‘ in on the figures “ Please correct

We removed the “on”

Page 14, line 2:” and may be generalize to cirrus with similar patterns..” Please correct generalized

Done.

Please also note the supplement to this comment:

<http://www.atmos-chem-phys-discuss.net/acp-2017-51/acp-2017-51-AC2-supplement.pdf>

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