Reply to Anonymous Referee #1

1 General Comments

In this paper, the authors performed a set of 3D and 1D radiative transfer simulations with varying solar and measurement viewing angles. The goal of the study is to characterize and disentangle the various effects, problems and shortcoming that stem from 1D compared to 3D radiative transfer. The simulations seem appropriate for this endeavor, the results seem reasonable and I liked the review of possible 3D effects. However, I think the manuscript falls a bit short on the discussion and future implications of the results. The manuscript could include the following two key points before I would want to recommend to publish it.

We would like to thanks the anonymous referee #1 for his careful look at our paper. In the text below we answer point by point to anonymous referee #1 comments and questions. We have also included the necessary changes in the manuscript. Also, we changed the way to plot the figures, now the various effects showed in the manuscript are relative (in %) to the 3D reflectances (the truth) at the given spatial resolution. We have also over-imposed the MODIS reflectance accuracy ~3% to each of these plots in order to show when the impact of 3D and/or heterogeneity on the reflectances are significant or not.

In addition, to be clearer for explaining the cloud heterogeneity, we change the structure of the paper by first presenting the total differences between 3D and 1D reflectances and then the PPH bias, the THEAB and the 3D effects.

We also add Fig. 4 and Fig. 5 to illustrate the THEAB and 3D effects, respectively

We added a new section 4.4 on the 3D effects with a new figure (Fig. 14) and a table (Table 2).

The conclusions has been deeply re-written.

Also our manuscript has been proofreaded by a native English speaker.

1. Could you please discuss the magnitude of errors that you find in reflectances compared to current satellite observations? I guess that todays reflectance measurements are on the order of 5 to 10%? How does that relate to or impact your findings?

This is a very pertinent remark, indeed we agreed that it would be valuable for the reader to compare the impact of 3D and heterogeneity effects to the reflectance measurements accuracy.

Currently for MODIS, such accuracy is estimated at 3% (Xiong et al., 2005; 2017). All the figures discussion about the amplitude of 3D and heterogeneity effects are now in percentage in order to be compared to the retrieval accuracy and some discussions are added comparing the measurements accuracy and the magnitude of cloud heterogeneity impacts.

2. I think the manuscript would greatly benefit from a short discussion or sensitivity test regarding the impact of your findings on actual retrievals. Could you for example provide estimates for errors in retrieved effective radius. I know you promise a future study grounded

on optial estimates with spectral information from all ranges but could you not provide or discuss first estimates for a retrieval like Nakjima-King?

We agreed with referee #2 that this is an important point to show the impact on the optical property retrieval, however these results have already published in (Fauchez, T., Platnick, S., Sourdeval, O., Meyer, K., Cornet, C., Zhang, Z., and Szczap, F. Cirrus heterogeneity effects on cloud optical properties retrieved with an optimal estimation method from MODIS VIS to TIR channels. AIP Conference Proceedings, 1810(1): 10 040002, 2017b.) at a 1km spatial resolution and submitted in (Fauchez, T., Platnick, S., Sourdeval, O., Wang, C., Meyer, K., Cornet, C., and Szczap. F. *Cirrus horizontal heterogeneity and 3D radiative effects on cloud optical property retrievals from MODIS near to thermal infrared channels as a function of spatial resolution)* in the upcoming JGR special issue: "3D Cloud Modeling as a Tool for 3D Radiative Transfer".

In this study we have shown that when only considering cloud horizontal heterogeneity effects, the largest retrieval errors are associated with TIR retrievals due to the PPH bias. However, when both cloud 3D and heterogeneity effects are considered, the solar reflectance-based retrievals have the largest error for spatial resolutions less than 500–1000 m, while the TIR-based retrievals have the largest error above this resolution due to PPH bias.

COT and CER retrieval errors using SWIR/VNIR reflectance measurements are of the order of 10 % for a Sun at zenith but they can be up to 20 % [at 10 km spatial resolution] and 100% [at 50 m spatial resolution] for COT and CER retrievals, respectively, for an oblique Sun.

1.0.1 Specific Comments

• p.4 l.20-26 Please state if the domain has cyclic boundary conditions. I guess this is important because the interaction radius may be quite far?

Yes the domain has cyclic boundary conditions.

We add in the revised manuscript, line 24 page 4 the following sentence: "… *assuming cyclic boundary conditions are imposed at the edges of the domain*"

• p.4 l.28-33 Please give a more detailed description of the simulation so that one could reproduce your setup. What is the surface albedo? Was aerosol used?Water vapor background profile?

The detailed description of the simulations are already presented in the Part I of the paper. However, we agreed that it is valuable to be presented in this second part too. Ocean surface albedo values for NIR/SWIR channels are set at 0.05, there is no aerosol and the mid-latitude summer atmospheric profile is presented in Fig 2 of Fauchez et al., 2017a.

We add line 9, page 5: "*No aerosol is added and the surface is Lambertian with a constant albedo of 0.05. No aerosol and atmospheric absorption are considered."*

• p.5 l.24 IPA does not necessarily mean that the scene is vertically homogeneous. 1D RT is very well capable of simulating vertically inhomogeneous atmospheres numerically. This phrasing of yours kept me wondering till the end of the manuscript if you actually averaged the ice water content vertically or not. Please make that more clear.

You are right, the IPA implies the horizontal inhomogeneity and pixel independence not the vertical inhomogeneity. However, for the retrieval of optical properties from space radiometers

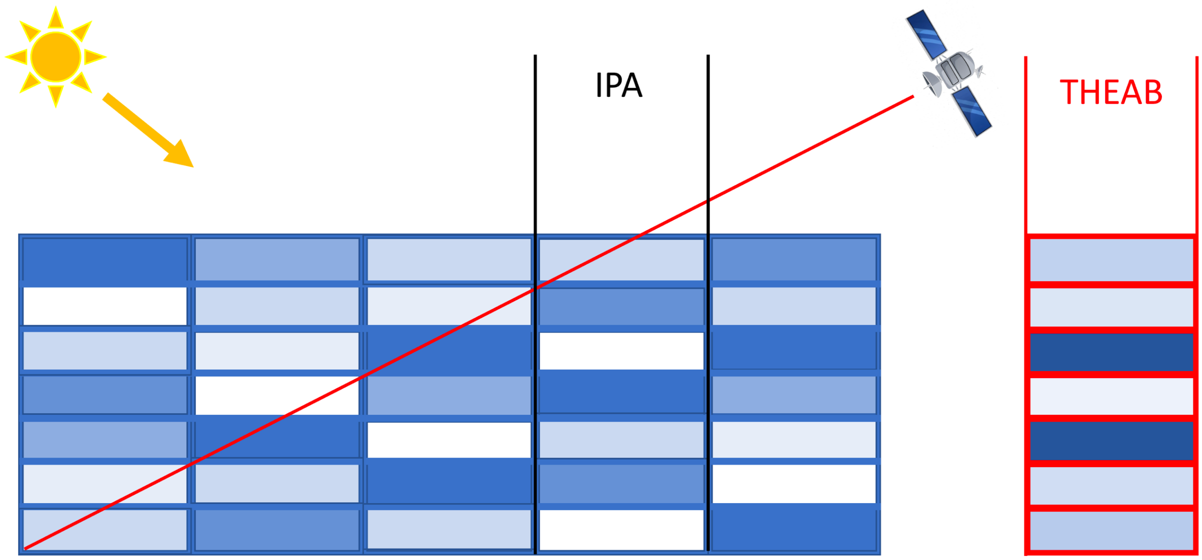
only one crystal size/shape is retrieved regardless the vertical extension and inhomogeneity of the cloud column. In our study the cloud extinction is vertically inhomogeneous, however only one crystal size and shape is assumed in the whole cloud domain for simplification of the problem and assess the extinction coefficient variability effects only. The impact of the vertical variability of ice crystal size/shape is very interesting to study but out of the scope of this paper.
We have remove the "*vertically inhomogeneous*" of L24, P5.
We have added this sentence in the section presenting the microphysical model: "Note that while the microphysical properties are homogeneous, the extinction coefficient varies horizontally and vertically."

• p.9 l.4 I think a schematic would be very helpful for the tilted part. I am wondering which slanted path you used, i.e. did you take the optical properties along the sun angle or from viewing geometry. Out of the two, which one would you think is better? Also, did you use an interlaced grid like for example Wissmeier2013 https://doi.org/10.1175/JAMC-D-12-0227.1?

We take the optical properties along the viewing geometry, this is different from the Tilted Independent Pixel Approximation (TIPA, Varnai et al. 1999), which have computed the optical properties along the sun angle.
Thank you for providing us this reference that we did not have. The TICA approach (i.e. paNTICA, parameterized nonlocal tilted independent pixel approximation) described in this paper (Wissmeier et al., 2013) is similar to the TIPA of Varnai et al., (1999). In our study we only account for the tilted view of the line of sight crossing various extinction through different cloudy columns while the ice crystal size and shape are constant. We explain our method in page 9, line 13 to 16:" For each pixel we have re-created a 1D cloud for which the vertical extinction is the averaged oblique extinction of the 3D heterogeneous field. In other words, we have run 1D RT using the oblique columns crossed as adjacent vertical cloud layers (i.e., tilted the oblique columns crossed to a vertical column)."
In order to highlight the relationship between THEAB and TIPA, we included the following sentences into the manuscript:

In essence, THEAB can be considered a variant of the Tilted Independent Pixel Approximation (TIPA) used in earlier studies (e.g., Várnai and Davies, 1999; Wapler and Mayer 2008; Frame et al., 2009; Wissmeier et al., 2013*), but with the tilting based on the view direction instead of the solar direction. A somewhat similar concept to THEAB was used in Evans et al. (2008), where reflectances were related to cloud properties calculated along the slanted line of sight.*

Also, as you mentioned, a figure will be very helpful, we add it in the manuscript and refer it as Fig. 4 in the new version.



The computation of the THEAB was helpful to understand why the sign of ΔR(3D-1D) depends on the spatial resolution (for Sun at zenith and off-zenith). We explain the change of sign due to the THEAB in section 4.3 Tilted and homogeneous extinction approximation bias (THEAB) :

"A somewhat similar concept to THEAB was used in Evans et al. (2008), where reflectances were related to cloud properties calculated along the slanted line of sight. Each tilted line of sight crosses large, medium and small extinctions through many different columns leading to an average optical paths similar between each tilted columns and therefore the field of view appears more homogeneous than the one with independent cloudy columns (1D assumption) with small optical thickness juxtaposed to large optical thickness. This effect is shown in Fig. 11 where we can see that the optical thickness field at 50 m spatial resolution view from 60° zenith angle is much smoother than the one see from nadir. We can also see that the extinction plumes are stretched out, spreading and smoothing the cloud extinction over the columns. Indeed, for a voxel horizontal and vertical sizes of 50 m and 72 m, respectively, and a $\Theta_V = 60°$, the line of sight reaching the top of a given voxel from its center (see Fig. 4) then cross horizontally 72 × tan(60) ~ 125 m, i.e. two adjacent voxels before reaching the underneath cloud layer. "

Because this effects occurs also for the Sun at zenith it is not due to the TIPA, therefore we had no need to compute new time expensive computation for TIPA which is implicitly included in the 3D radiative effects.

• p.10 l.21-26 Would it not make sense to have a look at 45°and 45°+90°? Is there a particular reason you did not examine that?

There is no particular reason to not look at these angles except that for computational time reasons we had to select a small number of viewing geometries. Indeed, at 45° the line of sight is parallel to the fallstreaks, at 45°+90° the line of sight will be perpendicular to the fallstreaks that would not differ too much to the other viewing azimuth of 90° and 180°.

• p.12 l.19-21 Isn’t that particularly interesting for retrievals that use both channels? Wouldn’t Nakajima King for example suffer from this even if there would be a linear relationship between the errors in those two channels?

This is an interesting question, both are solar channels (0.86 and 2.13 μm) and they produce similar 3D effects (shadowing, side illumination and horizontal transport). However, because the absorption is different between these two channels the amplitude of the effects is also different. Obviously, because the amplitude of the 3D heterogeneity effects is different between 0.86 and 2.13 μm this will impact the cloud optical property retrieval through a Nakajima and King method (or other similar method using a combination of NIR/SWIR channels, see for instance Zhang et al., (2012, 2013), Marshak et al., (2006)). Note that those differences are much smaller than between TIR and NIR/SWIR channels (see Fauchez et al. 2017b). Also. Fauchez et al., (2017a) have shown that for TIR MODIS channels (centered at 8.52, 11.01, 12.03 and 13.36μm), the difference of cloud absorption (and scattering) between those channels leads to different 3D and heterogeneity effects, which later impact the cloud optical property retrieval using thermal infrared channels such as the split-window technique (see also Fauchez et al, 2017b and 2018).

Marshak, A., S. Platnick, T. Várnai, G. Wen, and B. Cahalan (2006), Impact of three-dimensional radiative effects on satellite retrievals of cloud droplet sizes, *J. Geophys. Res., 111*, D09207, doi:10.1029/2005JD006686.

Zhang, Z., Ackerman, A. S., Feingold, G., Platnick, S., Pincus, R., and Xue, H. Effects of cloud horizontal inhomogeneity and drizzle on remote sensing of cloud droplet effective radius: Case studies based on large-eddy simulations. *Journal of Geophysical Research: Atmospheres*, 117(D19), 2012. D19208.

Zhang, Z. (2013), On the sensitivity of cloud effective radius retrieval based on spectral method to bi-modal droplet size distribution: A semi-analytical model, J. Quant. Spectros. Radiat. Transfer, 129, 79–88, doi:10.1016/j.jqsrt.2013.05.033.

1.0.2 Minor remarks

• p.1 l.15 “by” should be with?

Yes, done

• p.2 l.12 “thicknesses” should be singular

Yes, done

• p.5 l.01 shown “in” table

Yes, done

• p.5 l.16 I assume you meant 100e9 for all simulations? This is impressive if that is a single core performance which would be 2e6 photons per sec. Or was that parallelized on multiple cores/nodes? If so please state the number of core hours.
Thank you for pointing this out, it was a mistake, the computational time is about 3.5 days for 100 billions of photons for each simulations (spread on 2048 batches). Below is a detail of one of the simulations.:
Total time = 3 days, 13 hrs, 14 mins, 2 secs
Mean time per trial = 0.80 millisecs
Mean time per scatter = 145.60 microsecs
Mean time per batch = 149.83secs
102400.00 Mtrials completed with 2048batches

We have corrected the computational time in the manuscript and explain that those simulations have been parallelized on 2048 core each.

• p.5 l.27 Conversely?
Yes, done

• p.7 l.12 "more" should be "higher"?
Yes, done

• p.7 l.16 + fig.2 Please change the color of the 1D markers and put them on top, I could not see your claims.
We assume that you mean Fig. 4 and we made the change in the figure.

• p.7 l.20 effects should be singular?
Yes, done
• p.8 l.24 green? . . . and I thought I am not colorblind. . .
No, you are not! That was a mistake, we have corrected it according to the color change you asked for this figure.
- ***p.9 l.02 temperatures should be singular***

Yes, done

• p.10 l.7 remove "issue"?
Yes, done

• p.11 l.6 remove "view"?
Yes, done

• p.11 l.34 remove the "a" in "for a various view"
Yes, done

• p.12 l.08 process should be plural
Yes, done

• p.12 l.09 Simulation should be plural?
Yes, done

• p.12 l.12 take should be taken
Yes, done

• p.12 l.13 maybe change "here are ranged to" "here range from"
Yes, done

• p.12 l.21 "shown" should be "show"?
Yes, done

• p.13 l.06 "this" should be "these"?
Yes, done

• p.13 l.20 insert "from" after "ranging"
Yes, done

• f ig.1 Error in caption, reference to (f) is (e)
Yes, done

• f ig.2 if you mention 50m here I am wondering was it something else in fig1?
No, both are at 50 m spatial resolution so we now mention it too in Fig.1.
• f ig.3 I would like it very much if you could provide short conclusions here already in the caption
We add: "Because of the non-linearity between R1D and τ, the τ retrieved from averaged $\overline{R1D}$ is smaller than the averaged $\bar{\tau}$ retrieved from the two R1D."
• f ig.4 As mentioned earlier, please update the colors so that a reader can distinguish the markers
Done, thank you.

• f ig.5 please add the idea of the panels and colors to the caption. I.e. left to right are zenith angles, colors are view zenith angles
Done, thank you
• fig.6 put brackets around equation references? Change null to _s = 0
We changed equations 2 to Eq.2 and changed null to 0.
• table 1 MOD06 optical properties please write out the names and symbols
Done, thank you
• table 1 why use diameter here when you use radius everywhere else?
We changed it to effective radius of 10 micron
• table 1 _ for channel 2 differs. . . is .83 micron correct?
No it should be 0.86 micron, thank you.
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