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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Impacts of cirrus clouds heterogeneities on TOA thermal infrared radiation" *by* T. Fauchez et al.

Anonymous Referee #2

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General comments

The paper examines TOA brightness temperature differences between full3D RT and the plane-parallel approximation under varying assumptions of cloud height and cirrus heterogeneities at different resolutions. The paper finds that differences can be very significant at a resolution of 1 km, which is the typical resolution of a number of space-based instruments, such as IIR, MODIS and SEVERI (operational NWP satellite). Indeed, the differences can be as large as 10 K for typical semi-transparent cirrus. The differences are shown to be chiefly governed by the sub-pixel optical depth heterogeneity and the magnitude of the difference between cloud-top and surface temperatures. This study is to be welcomed, as there have been no (that this reviewer has seen) 3D simulations of cirrus in radiance space at infrared wavelengths. The only

other studies of 3D cirrus radiative transfer studies in the infrared have been conducted in flux space only. The results presented by the authors are new and have not been published elsewhere.

The study is timely, as NWP centers are now assimilating IR cloudy radiances at lower resolutions (12 km) than considered in this paper, but the results will still have important implications for those centers, as cloud heterogeneities will still need to be taken into account event at the lower resolutions. Moreover, NWP centers with improved supercomputing can now routinely run local area models at horizontal resolutions of about 1 km, and can locally be run at several hundred metre resolutions. The radiative transfer models used for assimilation still assume the PPA, albeit in flux space. However, the paper shows that the BT differences can be significant as a function of sub-pixel variance of optical depth, and so therefore, this must also be significant in flux space as errors would not be expected to cancel to such a significant degree!

This paper should certainly be published as it is a comprehensive study using new approaches to the representation of ice optics in 3D, and the 3D model used is shown to be in excellent agreement with an independent 3D RT model. Moreover, the simulated cirrus case in shown to agree well with observations of the same cloud in terms of its vertical extent and height. However, the paper can be improved by noting the following points.

1. The typographical errors noted in the pre-published version still persist to some degree, the authors are again asked to give the manuscript to a native English speaker and writer. The most persistent error occurs when describing some variable as a function of another variable. Often ". . .in function of" please re-write this as ". . .as a function of. . .." This error occurs throughout the manuscript, please correct throughout. Other errors are noted in the minor corrections below, but not all.

2. Fig. 4. The authors do not show how well the 3D model represents the observed distribution of IWC in both the vertical and horizontal directions. The IWC values can

be quite large. From the model fields, the authors should simulate the CloudSat radar reflectivity at 94 GHz, and compare the radar reflectivities directly. This will then give an indication of how well the 3D model represents the distribution of IWC.

3. Small ice crystals are as the authors show, very important to the transfer of radiation at thermal infrared wavelengths. However, to study the impact of 3D cloud optical properties, they use the PSD parameterized by Field et al. (2007), which is based on the bulk measurements of cirrus and are not directly related to the cloud edges, where small particles will be important. How confident can the authors be that the Field 2007 parameterization is representative of the PSD at the edges of cloud? The authors need to state how important the omission of cloud-edges is to their work, if not, then why not?

4. The equations are written in radiance space, but the results are given in terms of brightness temperatures. Moreover, satellite instruments measure radiances and NWP centers assimilate radiances and not brightness temperatures. Figures 8 and 9 give results in brightness temperature, which are significant, but this may not necessarily be true in radiance. Can the authors state that they have shown that differences in radiance are significant, and the absolute radiance differences are greater than the radiometric noise of the instrument? The authors should also include in Figures 8 and 9 the radiometric noise of IIR.

5. Concerning the general conclusion that assumption of ice optical properties is not as important as the PPA. This may be true, although, there are cases, which are above the radiometric noise of IIR in Figures 13 and 14. However, SEVERI, for instance, has a radiometric noise of only ± 0.2 K and as such the results presented in both figures are significant for that instrument. With improved instrumentation the results presented can still be significant and should not be written as if correct 3D representation of the ice optical properties is not important. Please re-write accordingly.

6. As alluded to previously, some NWP centers now assimilate cloudy radiances at

lower horizontal resolutions than considered in this paper. Can the authors say how important the 3D effect is at lower resolutions, and the implications that this may have for NWP centers? Can the authors suggest a correction that NWP centers might apply to correct the solution obtained assuming the PPA?

Minor comments.

1. Abstract. Line 1. ".. study on. . .." -> ". . .study of. . .."

2. Line 8. "..resulting of. . ." -> ". . .resulting from. . .."

3. Throughout the paper "plan-parallel" -> "plane-parallel"

4. Line 9. ". . ..view zenith.." -> ". . .zenith view. . ." should this be nadir?

5. Introduction. Line 21. Infrared radiation is emiited by the earth's surface and atmosphere. Please correct.

6. Line 21 ". . .to the.."-> ". . .to. . ."

7. Line 23 cloud radiative effect is now generally preferred to radiative forcing. Please change.

8. Page 27461. Line 3, satellite instrumentation also presently measure microwave radiation. Please correct.

9. Line 10. ". . .bias on. . ."-> ". . .biases in . . ."

10. Line 11. Again "in function. . ."-> "..as a function. . ."

11. Section 2.1 line 25. ". . .for that an important sedimentation. . ." this sentence does not make sense please re-write.

12. Page 27464. Line 12 Please define MODIS and supply a reference.

13. Line 17. "On Fig 4a. . ." -> "In Fig 4a. . ."

14. Line 19 "on 25 May" -> "on the 25 May"

15. Line 26 “. . .same with the IWC increase twofold.”-> “. . .same but with the IWC increased by two-fold.”

16. Line 28 replace “resumed” with “summarised”

17. Section 2.2. In this section some clarifications on the cited references is required. The 3D ice optical property parameterization is fundamentally based on the single-scattering properties described by Baran et al. (2013) [Q. J. R. Meteorol. Soc. (2013) DOI:10.1002/qj.2193] and the parameterization used is the same as that developed by Baran (2012) [Atmospheric Research 112 (2012) 45–69]. The Baran et al. (2009) citation should remain as that was the first to show a direct couple between (IWC, cloud temperature) and the optical properties of atmospheric ice. You might also like to sate in this section that previous parameterizations based on De alone to not fully describe the 3D effects as De varies vertically only, with littler variation in other dimensions. The authors do state something along these lines but the statement could be stronger.

18. Page 27465 lines 13-15. I would re-write this sentence as follows. “Furthermore, ...are smooth enough to be approximated by the Henyey-Greenstein phase function (reference), and these are assumed in the. . .model.”

19. Section 2.3. page 27467 line 1 “neglectful”-> negligible

20. line 24 “source flux”->”the source flux”

21. The comparisons against SHDOM was achieved assuming a specific geometry, were a number of other geometries assumed? If so please state this.

22. Line 24 units of radiance is incoorect please correct.

23. Section 3. Sub-section 3.1. Line 15 The layers are also assumed to be infinite in horizontal extent? If so please state.

24. Please re-write sentence beginning on page 27472 as it is difficult to read.

25. Line 16 the linear relationship between sigma(tau) and the differences are linear if

tau <2? Rather than for all tau. Please be more precise.

26. Line 15 page 27473 “in a cirrus” -> “in cirrus”

27. line 28 “it is maximum”-> “it is a maximum”

28. Section 3.4 page 27475 line 21 “in the order”-> “on the order”

29. Page 27476 line 1. You mean infinite horizontal layers?

30. Section in the summary you missed out SEVERI, this is an geostationary satellite that should be mentioned.

31. Page 27477 line 4 “simulated to the nadir”-> “simulated at nadir”

32. Page 27477. In this section you might also like to state that the SEVERI radiometric noise is about $\pm$0.2 K, in this case 3D effects become important for tau (1 km) < < 0.4!

FIGURES

1. Fig. 1 (b) Please place units over the color bar.

2. Fig. 4 (b) There is no scale for the CALIOP observations. Is it the measured backscatter coefficient or volume extinction coefficient? Please state and insert a color bar of whatever the measurement is.

3. Fig. 5. A plot of the IWC and cloud-temperature would be useful here as this would help the reader interpret the results more easily. Caption CII-12 -> CII-2?

4. Figs. 8 and 9 please also insert the absolute accuracy of the IIR radiometer and not just the model noise.

5. Fig. 17. Please insert the noise of SEVERI and state at which optical depth 3D effects become important.

Interactive comment on Atmos. Chem. Phys. Discuss., 13, 27459, 2013.