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Atmospheric Chemistry and Physics
Discussions

Interactive comment on "Determining the infrared radiative effects of Saharan dust: a radiative transfer modelling study based on vertically resolved measurements at Lampedusa" *by* Daniela Meloni et al.

Anonymous Referee #2

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Review of paper: acp-2017-591 "Determining the infrared radiative effects of Saharan dust: a radiative transfer modelling study based on vertically resolved measurements at Lampedusa" by D. Meloni et al.

General comments

In this paper radiation closure experiments are made in order to determine the infrared radiative effects of dust and to assess the role of dust size distribution (SD) and refractive index (RI). To this aim, in situ data from aircraft (ATR-42 and Falcon), surface

(AERONET, radiometer, pyranometers, pyrgeometers and pyrometer) radiosonde and satellite (IASI) measurements are utilized for the closure. The measurements come from the ADRIMED/ChArMEx campaign in 2013. The vertically resolved simulations are performed with the MODTRAN radiative transfer model (RTM) initialized by in-situ vertical and remotely sensed columnar SD and RI along with data for a series of surface and atmospheric parameters relevant to LW radiation transfer, coming from radiosoundings, spectrophotometer measurements, ECMWF reanalysis and MODIS satellite products. The assessment lies in comparing simulated and measured LW irradiances and brightness temperatures (BTs), while the dust LW radiative forcing (ARF) and atmospheric heating/cooling rates (AHR) is estimated with the RTM. Three cases (summer days) during a period of dust intrusions (late June and early July) are examined, and the study is performed for Lampedusa in central Meditteranean, in proximity to northern Africa and Sahara.

The study is detailed and makes synergistic use of a variety of data. Some interesting findings are reported, from which some are not always new, e.g. that the dust LW radiative effects are non negligible or that the heating rate profile of dust depends on its vertical distribution as well as on SD and RI. Yet, some others provide new information and give insight regarding the role of dust SD and RI for their LW radiative and thermal effects and for BT, e.g. that using dust RI from local dust sources (Algeria and Morrocco, DB2017) produces best agreement with observations or that the use of inaccurate, although optically equivalent SD and RI has a large impact on the dust ARF. The paper is well organized and nicely written although it sometimes lacks clarity in the discussion of its results.

The main issue is that the paper seems to fail to convince about the best performance and appropriateness, and to provide a clear message on what is the optimal combination of dust properties for achieving the radiation closure. The relevant messages drawn from the simulations-measurements comparisons of LW and WINDOW fluxes, and of BTs, are not consistent and appear to be somewhat contradictory, as it is for example the case in Table 3. Even the authors state (page 15, lines 20-21) that “the MODTRAN spectral resolution impacts the standard deviation of the model-measurements differences, making the results obtained with different AOPs equivalent”. More specifically:

Main Comments

1) In general, quite small differences between the 7 examined configurations, consisting in different model setups (Table 2), are found between results obtained without aerosols and with aerosols, as well as between the 6 configurations with aerosols (3 columnar and 3 in-situ). This does not help to draw a clear conclusion on which one configuration and aerosol properties combination is the best, although this isn expected to be the main finding of such a radiation closure study.

2) The ascertained/computed differences of each one of 7 configurations with respect to measurements (LW, WINDOW, BTs) mostly fall within the range of uncertainty of measurements, making difficult to decide on which one is really the best configuration.

3) A main conclusion drawn from the analysis is that there is a systematic model overestimation of upward LW fluxes within the peak of dust layers, in all 3 days. In other words, there seems to be an inherent problem with the modelling tool, which needs to be assessed.

4) The estimated small differences between the no-aerosol and aerosol configurations, indicate that the RTM LW computations are relatively insensitive to dust.

5) The reported conclusions are sometimes contradictory. For example in page 17, lines 14-15 it is stated that dust RI from DB2017 produces the best agreement with observations, but this is not supported by and it is not in line with the results of Table 3 where NOAER and COL1 also provide good results, even better than INSU3, if all three parameters, i.e. LW, WINDOW, BT, and three days are considered.

6) It is not clear why BTs were computed and are reported only at 3 levels, which

sometimes are not collocated with the peaks of dust layers; why similar BT computations were not made at more levels.

7) The conclusions drawn from the BT analysis are different from those obtained from the analysis of LW fluxes. This is for example the case of the results of profile 42, in Figures 10 and 11. May this point to a possible modelling problem/inconsistency?

8) The role of clouds is not reported. Were all the tree days/cases cloud-free? If so, how is this confirmed/ensured? A relevant discussion should be made since the effect of clouds on LW is significant and interplay or even dominate the effect of dust (e.g. possible implications for Fig. 2).

Specific Comments

1. Page 1, line 28: define IASI acronym.

2. Page 4, Figure 1: the AERONET AOD may also be overplotted.

3. Page 4, line 18: the reported angstrom exponent is high, it is about the maximum one; give a more realistic value (range).

4. Page 5, lines 32-35: why? Please explain.

5. Page 5, lines 35-37, “The pyrometer . . . for the IRP BT”: this sentence is oversimplified. A quick look at the 3 figures reveals significant differences between BT and irradiances. For example, what happens in June 24 and 25 (when LW-WINDOW curves do not have peaks, opposite to IRP BT)? What about the role of temperature and clouds?

6. Page 7, line 7: define FWHM acronym.

7. Page 7, line 30: up to which altitudes? How much the use of standard profiles can affect the radiative fluxes? Was any sensitivity study performed to assess this? Especially the LW fluxes should be sensitive.

8. Page 7, line 33, regarding the absorbing gases: similarly, it would be worth to discuss/assess the sensitivity of fluxes to these parameters, especially given the scaling applied to their vertically distributed values.

9. Page 8, about ECMWF: The use of reanalysis data is inevitable in this case. However, an assessment of the induced uncertainties associated with their coarser resolution could be made by comparing similar ECMWF data with available measurements for the other two days. This could provide an estimation of induced uncertainties in June 22.

10. Page 8, line 10: a few words about the measured aerosol properties and the identified aerosol layers can be added. For example, apart from the layers and their extension neither information is given nor reference is made to the type of aerosols in each layer, with reference to corresponding measurements that cloud provide this kind of information.

11. Page 8, line 14: so, what values of emissivity were assumed in the study? Do they differ and how much from day to day.

12. Page 8, Figure 4: the quality should be improved, e.g. by thicknenning the curves, so that the coloured curves can be more easily discerned.

13. Page 8, line 29: As mentioned, different factors influence and differentiate the AERONET and in-situ SDs, one important being their different value, .e. columnar versus vertically resolved. The value of detailed measurements is that they provide vertically resolved SDs. Therefore, emphasis should be given to them. Discuss a bit more how the measured SDs differ to AERONET ones, referring to their agreement and disagreement. For example, larger differences appear in June 22 than in July 03. Refer to this difference referring to the nature of vertical profiles of Fig. 3 and the type of aerosols that are present in the different layers of every daily profile.

14. Page 9, line 19: explain why this choice of water soluble RI was made and not any

other.

15. Page 9, lines 20-26: Table 2 is not discussed enough. It should be said more clearly what exactly has been done and how the Mie-based computations of AOD compare to AERONET ones, whenever applicable, i.e. in visible wavelengths.

16. Page 10, lines 16-17: does this refer to July 03? In Table 3 no results for INSU2 are displayed.

17. Page 10, lines 19-20: why the stronger infrared emission? Is it a matter of larger mass? Please explain.

18. Page 10, line 24: clarify that “all cases” refer to LW, WINDOW and IR BT.

19. Page 10, line 34: here it should be clarified what is exactly the spectral interval/coverage of the measurements (IRP). This not clear based on what is said in page 7, line 6, about the IRP centered at 3 wavelengths etc. It is essential to clarify what is exactly the spectral coverage of measurements since they are used as the reference to which the simulations are compared, and given the significant sensitivity of theoretical computations to the spectral interval. Also explain why the reduction in WINDOW irradiances has different magnitude despite the same spectral reduction (0.4 microns) in different spectral parts.

20. Section 4.1: what is missing is a critical approach providing insight into possible physical reasons for better agreement between the 7 examined cases. A quite exhaustive and very detailed description of results is made, referring to various numbers (Table 3). This is not enough while it turns to be confusing to the reader. What is more important is to determine which set of AOPs is more efficient and compared better to the measurements for the 3 cases. The discussion should conclude on this, stating at least if there is a “best” choice or if there is not and why. Moreover, in both cases, the discussion should provide a physical basis for the outcome of the analysis and the closure of Table 3. For example, a summary of the results of Table 3 should point to

NOAER being the most efficient simulation, providing better results than the other 6 sets of AOPs in 4 cases (out of totally 9, i.e. 3 days by 3 parameters). NOAER is followed by COL1 (3 cases with best performance) and INSU3 (2 cases). So, questions may arise, like why simulations without aerosols should be more appropriate/realistic, or why INSU3, which may be expected to be the most realistic, is finally not.

21. Page 12, line 3: as to upward LW, authors may want to comment on why the smallest differences are for COL1 in Table 5, while the smallest RMSDs in Table 4 are for INSU1.

22. Figure 7: why only points for NOAER, COL1 and INSU3 are given and not for the other cases? All these appear in Table 6.

23. Page 12, lines 15-16: add "in-situ" before SD. This sentence needs to be re-written, since it is introduced all suddenly without being given evidence and discussed based on the results of Fig. 7.

24. Page 12, lines 17-19: while discussion is made no results are shown/given.

25. Section 4.2.1: A quite exhaustive discussion is made making frequent reference to numbers that differ a while between the 6 examined cases. Also the question arises why NOAER sometimes performs equally or better than dust-including cases. It could point to potential artifacts due to counteracting effects of other parameters than aerosol, which affect the LW radiation transfer and BT.

26. Page 12, lines 35-36, "These differences . . . airborne instrumentations": so is there an inherent problem with the model?

27. Page 14, line 20" add "was" before "evaluated".

28. Page 14, line 21, "resampled": how it was done?

29. Page 14, lines 3237: why there is difference on what provides the best match with reference to best match with the measured spectra and BTs?

30. Page 15, lines 2-3: this is not applicable to 780-980/cm for June 22 and 28.

31. Page 15, lines 20-22, "In our case, . . . AOPs equivalent.": what exactly is it meant by this? By which means. Please explain. Is it implied that this (having very high resolution) is preferable? If so, why? If valid, it would mean that AOPs are not important for accurately computing LW radiation and dust LW radiative effects. Is this the meaning?

32. Page 15, line 28, "The combination . . . downward": this is not clearly evidenced in the discussion of sections 4.1 and 4.2.

33. Page 26, Table 2: the Table needs further/better explanation, it is not very easy for the reader to understand what exactly is the information given in this Table.

34. Page 37, Figure 5: what have been the criteria for the design of flight paths.? Nothing is said about this and deserves to be mentioned in the text.

35. Page 44, Figure 12: wavelengths could be added, e.g. on the top x-axis.

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