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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Dust aerosol impact on North Africa climate: a GCM investigation of aerosol-cloud-radiation interactions using A-Train satellite data" *by* Y. Gu et al.

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This is an interesting, timely and relevant contribution to research on global climate change. More information, however, about the performance of the model (items 1 and 2) must be provided before publication. There are also questions and complaints about the clarity of the text. When a paper hits a point around which of several aspects of our field have been spinning, good writing is critical.

This GCM study implements attention-getting relationships of aerosols and ice clouds reported by Jiang et al. (2008, 2011). Jiang et al. made a leap by linking the column-integrated estimate of AOD from MODIS to the microphysics of ice clouds that occupy a small fraction of columns nearby. I hope that Jiang et al. are mostly correct, as an

advance in this area would be welcome. Can we be confident of the claim (Jiang et al., 2008) that "dynamical conditionsŠ cannot explain the precipitation differences for the polluted and clean clouds, suggesting that aerosol cloud-precipitation interactions may play a dominant role in contributing to the suppressed rainfall when aerosol is abundant"? With such questions in mind, the reader eagerly jumps into the present Gu et al. manuscript that makes a test with a dynamical model. Unfortunately, the manuscript is not incisive enough, in its approach to a rather subtle problem. Jiang et al. (2011) are aware of the limitations of the current state of the art, for example, when noting "with the better height-resolved aerosol and cloud data from CALIPSO and CloudSat, we will continue this work to provide a height resolved Re parameterization for simulating the aerosol effect on cloud particle size." This manuscript will confuse some readers. The authors are right in describing their topic as "a challenging problem" (see lines 8-11 on page 31405). But they overstate by claiming that "Inadequate understanding of the relationship between microphysics and dynamical processesŠ" is "due primarily to the lack of accurate global-scale observations." We actually lack the proper atmospheric observations at ANY scale.

Results must be more carefully qualified and the reader duly cautioned. Investigations from decades earlier (i.e., Liou and Ou, 1989, referenced in the manuscript) used simple one-dimensional models which were adequate for calling attention to the problem. A more recent study of related effects for liquid water clouds referenced in this manuscript (Johnson et al., 2004) employed a large-eddy model. Here we have only a coarse resolution, 4 degree by 5 degree GCM. Can the essential aerosol-cloud-radiation physics that is the focus of the manuscript be convincingly modeled at this coarse scale? Perhaps it can. But the authors must show us the degree to which the GCM physics at this scale are credible.

1. How does the OLR and TOA Net Solar flux from the GCM compare with well-observed satellite data over North Africa? This should be shown in two new figures (OLR bias of GCM and TOA Net Solar bias of GCM) over 10S-30N and 20W-50E.

2. Are the OLR differences (DIR_IND-IND) in Fig. 7c well above the noise level of the GCM? Two more figures (a plot of the interannual variability of OLR in the GCM and another with the same for satellite data) are needed to show that the space-time scale of the study is valid.

3. More information is needed in Section 3 (Offline studies starting on page 31413). The IR forcing in Figure 4a is a strong function of cloud altitude, which is not stated. It would also be helpful if the text called out the visible optical depths of the IWP clouds, perhaps at two points in Figure 4a-c and two in Figure 4d-f, for comparison with AOD. I like Figure 4.

4. Page 31414 refers to Figure 4a-c with the confusing statement "while semi-direct effect can be inferred from the results for cloudy conditions." The previous page informs these calculations are "off-line", meaning that clouds were assumed a priori, not generated by a GCM under some aerosol condition. To diagnose a semi-direct effect due to aerosols, one would have to run a GCM and then note the cloud response. Figure 4a-c tells nothing about the semi-direct effect. Figure 4a-c would have information about the direct forcing of aerosols to a cloudy column, if we knew the height of the clouds.

5. More information about the CTRL, IND and DIR_IND experiments is needed (pages 31415-31416). What is the surface albedo over North Africa? CTRL has "clean" clouds and "direct radiative forcing is not included." In that case, is the "background AOD of 0.1" in CTRL used to parameterize the "clean" clouds?

The discussion on the bottom of page 31417 and top of 31418 on overall aerosol effect(direct+semi-direct+indirect) and the indirect effect only is interesting. But in the Conclusion that follows, there are statements for which I could not easily find support in the manuscript. Examples follow.

6. The Conclusion states on page 31418 "When ice clouds are present, the aerosol semi-direct effect plays an important role." Where is this demonstrated?

7. The Conclusion states on page 31418 "In a GCM setting, aerosol absorption of sunlight heats the lower troposphere and reduces cloud cover and cloud ice water amount." The difference between experiments DIR_IND and IND should illustrate the impact of the aerosol absorption of sunlight. Then why is cloud cover enhanced (not reduced) in Figure 7b?

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