Reply to reviewer 1's comments:

We are very appreciative of reviewer 1's helpful suggestions. To address reviewer 1's comments, we have modified the manuscript listed below.

Anonymous Referee #1

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General comments:

This paper compares the properties of atmospheric dust from the Saharan deserts and the Asian deserts based on exhaustive analyses using a coupled climate-microphysical section model, CALIPSO global dust measurements, and ground-based AERONET measurements. The comparison provides useful insight into the dust lifting, transport, and deposition, in these two major dust sources and downwind regions, which can improve our understanding of the contributions of the two dust sources to the global dust distributions. The paper is well organized and written and is acceptable for publication after revisions are made. I am not an expert of dust transport modeling and cannot comment too much on the model and simulations. However, the paper appears to be lack of a more quantitative assessment of the model simulation results. It would be helpful if the authors can provide a brief discussion about how well the model simulation can reproduce the reality and give some general idea about how much the difference (such as in the simulated dust fluxes) could be due to the model simulation itself, though it may be hard to do this. Figures 1, 10 and 11 appear to contain some useful information that the readers can use to assess the model simulation results. Just by eyeballing at figures 10 and 11, it looks like the model tends to overestimate the dust extinction when compared with the CALIPSO aerosol extinction. Have the authors done a systematic assessment of the model simulation? Any observation datasets of dust were used to constrain the model simulation?

Yes, we have done a systematic assessment of the model simulations in a separate JGR paper (Su and Toon, 2009) titled "Numerical simulations of Asian dust storms using a coupled climate-aerosol microphysical model (J. Geophys. Res., 114, D14202, doi:10.1029/2008JD010956, 2009).". We validated the simulations against observations from the ACE-Asia field campaign. We also used observational datasets including AERONET data at six study sites as well as the NIES lidar data to constrain the model simulations in Su and Toon (2009). A brief discussion about the model validation has been added to the paper (see page 6, lines 23-26).

The dust source regions ("Saharan Desert" and "Asian Desert") studied in this paper were not clearly defined. It is not clear to me if the "Saharan Desert" is referred to all the sources located in the North Africa (Sahara/Sahel region) or to a particular desert/geographic area. One sentence in the paper (p29520, lines 25-) says "The dust flux (orange dashed line in Fig. 2) crossing the 10_E plane (10_S–40_N) at the western edge of the Saharan Desert . . ." Looks like here the "Saharan Desert" is referred to a particular desert/area east of 10_E. There are sources on the African continent west of 10_E that can emit certain amount of dust (see Prospero et al. 2002) which can be transported to the Atlantic Ocean. To answer the question why the dust optical is generally larger over the Atlantic than over the Pacific, which appears to be one goal of this paper, the dust from the sources west of 10_E should also be taken into account. The geographic regions of the dust sources studied in this paper should be defined. To do this it would be useful to show a map of sources where emit dust (this can be a map of dust amount emitted in 2007 used in the model simulation).

The Saharan Desert refers to all the sources located in the North Africa including the part west of 10E plane. The 10E plane is used to indicate a reference location for comparisons in this paper. To address reviewer 1's suggestions and concerns, the definitions of Saharan Desert and Asian Desert have been added into the manuscript (see page3, lines 1-3). Also, in response to reviewer 1's comments, we have added another plane near the west coast of Africa (10W plane) based on Prospero et al. (2002) and Su and Toon (2009). Other planes in Africa have been extended westwards accordingly (10W, 55W, 100W, and 145W) (see pages 7-10, figures 2, 3, 4, and 5). Only 3% of all the dust lifted over Africa comes from the western side of the 10W plane.

Specific comments:

1.P29516, line 6: Liu, Z. et al., 2008 a or b?

It should be Liu, Z. et al., 2008b (see page 3, line 14).

2. P29520, line 25: "The dust flux. . .", please clarify the flux here is westward or eastward or both. It is not clear to me if the dust sources on both side of the 10E plane or only the sources east of 10E are included in the model simulation. The sources west of 10E they should be included (see my general comments), and the 10E plane, which appears to be a reference for comparisons in this paper, should be moved to a longitude near the west coast of the North Africa.

The flux in the paper includes both sides. In response to reviewer 1's comments, we have added another plane near the west coast of Africa (10W plane) based on Prospero et al. (2002) and Su and Toon (2009). Other planes in Africa have been extended westwards accordingly (10W, 55W, 100W, and 145W) (see pages 7-10, figures 2, 3, 4, and 5). Only 3% of all the dust lifted over Africa comes from the western side of the 10W plane.

3. P29521, lines 1-3: "This is about 94…"

This sentence has been modified due to a new plane near the west coast of Africa (10W). The following has been added to the paper: The flux across the 10W plane is about 70% of the total amount of Saharan dust (1547 Tg) lifted in the model for the year 2007(see page 8, lines 10-12).

4. P29522, lines 13-14: "during other parts of the year there is less convection over Central America and our model suggests that dust is more likely to reach the Pacific." This is a quite interesting conclusion. However, this does not seem to be always true (Fig. 2, green line). This is true only when compared with the dust transported to the 80W plane (Fig. 1, red line). Regarding Fig. 2, the authors should explain why the dust flux at 125W is enhanced for some months (i.e., 2, 4, 10, 12) compared with the flux at 80W. Is it because that some dust transported out of the latitudional boundaries (10S and 40N) at 80W are transported back within the boundaries at 125W?

This apparent re-transported dust was likely related to a small error in computing the dust flux as a function of altitude. However, after we chose the new reference planes as reviewer 1 suggested (10W, 55W, 100W, and 145W) and recomputed the fluxes more carefully, this issue disappears (see Fig.2).

5. P29525, lines 13-16: the 10E and 105E planes were selected in the paper as a reference for the Saharan dust analysis and Asian dust analysis, respectively. The authors should provide a reason for this selection (here or earlier). There are sources downwind these reference planes that can certainly contribute to the airborne dust, especially for the sources downwind the 10E plane in the North Africa. The authors should briefly describe how to deal with the sources downwind these reference planes. Again, this calls for a map of dust emission.

In response to reviewer 1's comments, we have added a new plane near the west coast of Africa (10W plane) based on Prospero et al. (2002) and Su and Toon (2009). The 10W and 105E planes were selected in the paper as references for the Saharan dust analysis and Asian dust analysis because they both located near the last of the sources. The dust sources (Saharan Desert and Asian Desert) have been also defined (see page 3, lines 1-3).

6. P29528, Eq. (1): "δ" is used here to denote the lidar ratio and later in Eq. (2) to represent for the volume depolarization ratio. Other character should be used. In the lidar community, "S" is normally used to represent for the lidar ratio, and "α" for the extinction coefficient. And, the equation is confusing. The denominator should be the two-way transmittance of the atmosphere between the layer top (or the lidar depending on where the attenuated backscatter is normalized to) and the point in the aerosol layer under consideration. This equation is valid only when the aerosol layer considered is homogeneous along the laser beam. For inhomogeneous layers, the exponential term should be an integral.

In response to reviewer 1's suggestions, "S" has been used to represent for the lidar ratio, and α has been used to denote for the extinction coefficient in Eq. (1). The assumption of the homogeneous aerosol layer along the laser beam has been added in the manuscript as suggested by reviewer 1 (see page 14, lines 19-25).

7. P29528, Fig. 10: please clarify if the CALIPSO extinction profiles were derived from the CALIPSO data products or by the authors using Eq. (1).

It's by Eq. (1) (see page 15, lines 16-17).

8. P29531, Fig. 12: it would be more helpful to mark the dust plumes and cloud layers directly in the figure (also for Fig. 14).

To address reviewer 1's concerns, "dust plumes" and "clouds layers" have been marked in Fig.12 and Fig. 14 (now Fig.14 and Fig.16).

9. P29533, section 3.4: how about the comparison of size distributions retrieved by AERONET and modeled in this paper for these two sites? Does the nonsphericity of dust particles play a role on the SSA computation?
On page 20 lines 9-10 we added “Unfortunately size distributions retrieved from the AERONET data were not available for these sites on these days”. We did not include shape factors in computing the single scattering albedo.