The authors estimated the direct radiative forcing efficiency of the dust aerosol ($DRFE_{dust}$) at some points in the Taklimakan Desert and the Sahara Desert by employing the satellite-based equi-albedo method and the radiative transfer model (RTM) simulation. In the manuscript, the authors asserted that the proposed equi-albedo method has unique advantage in estimating the $DRFE_{dust}$ because there is no need to consider the microphysical properties of dust. The authors also analyzed the differences in the dust microphysical properties between two in-situ sites of the Sahara Desert and the Taklimakan Desert (i.e., Tamanrasset and Kashi. respectively) and their impacts on $DRFE_{dust}$. The quantification of the dust radiative forcing efficiency is a meaningful and still a challenging task. However, the data and materials in the manuscript do not seem to be enough to support some conclusions. In my opinion, there are several places where the manuscript should conduct additional work.

**General comments:**

1. Dust aerosol is the object of this study. The authors should elaborate how to distinguish dust particles from other kinds of aerosols. The criteria are important to make sure that the estimated DRFE in this study is for dust aerosols. The authors declared that they chose the cases of dust storms in the Sahara and the Taklimakan deserts. However, the aerosol loadings were very low for most dust storm cases in Table 1 of the manuscript (with aerosol optical depth at 0.55 μm as low as 0.11). It is not completely convincing. The authors need to provide evidence that the estimated direct radiation effect of dust aerosol ($DRE_{dust}$) and $DRFE_{dust}$ in this study were "real" for dust.

2. In this study, the authors proposed the satellite-based equi-albedo method to estimate the radiative forcing effects and efficiencies of dust aerosols. The authors also asserted that this method has unique advantage as there is no need to consider the microphysical properties of dust. This method is on the basis of an assumption—the shortwave (SW) radiative fluxes at the top of atmosphere (TOA) of the clear sky are equal over the regions with similar land surface albedo (LSA) and solar zenith angle (SZA). On one hand, the influences of atmospheric profiles (including the vertical distributions of water vapor density and ozone density) on the upward solar radiative flux at TOA are obvious. They cannot be ignored relative to the magnitude of dust radiative forcing. The atmospheric profiles can be very different for different "clear sky" conditions over the same region. So, the assumption doesn't sound reasonable. The authors should prove it. On the other hand, "similar" is an ambiguous criterion. The authors should clarify the conditions of LSA and SZA quantitatively, which make the above assumption tenable.

3. Some important parameters and conditions were not defined clearly in this paper. For example, (1) the effects of dust aerosol particles could exhibit opposite warming or cooling effects at the TOA, in the atmosphere, or at the surface. The authors did not specify the $DRFE_{dust}$ that was estimated at at which level (i.e., at the TOA, in the atmosphere, or at the surface) in this manuscript. Although the observations of satellites are conducted at the top of the atmosphere, it does not mean $DRFE_{dust}$ only can be calculated at the TOA. (2) The direction of radiative flux (upwelling or downwelling) was also not mentioned throughout the context, which is vital information for calculating of the $DRE_{dust}$ and $DRFE_{dust}$. (3) The radiative forcing efficiency is defined as the rate at which the atmosphere is forced per unit of aerosol optical depth (AOD) taken at a reference wavelength. The reference wavelength is an important information considering that AOD varies with wavelength. The 550 nm and 500 nm are generally adopted in literature. The value of $DRFE_{dust}$ will be changed by taken different wavelength as reference. However, it was also not specified in this study. (4) The "clear sky" condition is a basic condition for estimating the $DRFE_{dust}$. But it was not clearly stated in the manuscript. Normally, the clear sky is identified as the cloud-free and low aerosol loading sky condition, other than the sky without cloud and aerosol particles. It is necessary for the authors to specify the condition of clear sky in this study. (5) The wavelength range of SW should also be given. The authors used different broadband satellite produces (e.g., land surface albedo, radiative flux) in the SW range. SW $DRFE_{dust}$ from this study and some different preivous studies were also adopted for comparison. The ranges of spectral integration in SW were not exactly same for different previous studies (e.g., 0.2-4μm, 0.28-3μm, or 0.3-5μm). Therefore, the authors should also specify the ranges of shortwave for various parameters in this study, which are surely contributed to the differences of the $DRFE_{dust}$ results. (6) It is well-known that the instantaneous aerosol radiative forcing effects and efficiency change obviously over time. As I understand it, the instantaneous DRE and DRFE were estimated at the transit moment of the Aqua in this study. The time and sky conditions corresponding to the $DRE_{dust}$ and $DRFE_{dust}$ results of the Taklimakan Desert and the Sahara Desert should be illustrated. It is important for the comparison among different results of this study and with other previous studies. (7) The LSA is a key factor which influences the Earth-atmosphere radiation budget. LSA is calculated from the white-sky albedo (WSA) and black-sky albedo (BSA) weighted by the fraction of diffuse skylight radiation. However, only WSA was shown in the paper. The BSA which is as a function of incident solar direction, has never been mentioned. The authors should give more details on how to obtain the LSA and its influence on the assumption of the equi-albedo method in this study.

4. The authors declared that there are only sparse in-situ measurements in the main dust source regions. One of the obvious advantages of satellite

measurements is to obtain the continuous spatial information in a large region. However, only the results of $DRE_{dust}$ and $DRFE_{dust}$ at a few pixels in the Taklimakan Desert and the Sahara Desert were estimated and shown in this manuscript. Readers may prefer to see the results of dust radiative forcing over the whole regions obtained by the satellite-based method in this study. I suggest the authors giving more results in Figures 7,9 and10.

5. The authors asserted that "Previous studies proved that the results in this paper are reasonable and reliable". "Table 2 illustrates the SW $DRFE_{dust}$ of the Sahara Desert and the Taklimakan Desert in previous studies. García et al. (2012) evaluated the $DRFE_{dust}$ based on the GAME model and the AERONET retrievals, which indicated that the mean $DRFE_{dust}$ is around −35 W $m^{-2}$ $\tau^{-1}$ in the Sahara Desert and −45 W $m^{-2}$ $\tau^{-1}$ in the Taklimakan Desert in similar observational conditions…" (Lines 443-446). However, there was no $DRFE_{dust}$ result in the Taklimakan Desert in the referred literature. According to García et al. (2012), only three AERONET stations in Asia (i.e., Sacol, Dalanzadgad and Yulin) were adopted as mineral dust stations in this previous study. None of them located in the Taklimakan Desert (see Figure 1 in García et al.,2012). The authors need to give a reasonable explanation on the authenticity of the above data. Moreover, the authors also need to pay special attention to that the definitions of the aerosol DRE and DRFE published in the open literature might be very different (e.g., instantaneous value, daily average value, monthly average value, multi-year average value, multi-year monthly average value). Even for the same concept, the difference in statistical method can also lead to the difference in quantity of these values. For example, daily average result is estimated by taking the average of the 24 instantaneous hourly values in some studies, but by taking the average of the instantaneous values throughout the daytime in some other studies. The authors should notice the detail of each $DRFE_{dust}$ value in the literature. They were not the same in Table 1. So, it is not appropriate to direct compare the values of different results in this table.

**More specific comments:**

1. Section "1 Introduction": The authors need to introduce the research status of dust radiative forcing, especially in the Sahara Desert and the Taklimakan Desert. Numerous existing studies have done. The authors need to survey literature and summarized them.

2. Line 112: The equation of the DRE in this study is not commonly adopted. I can't find such a definition in any of the given references.

3. Line 119 "Based on the assumption, the $F_{clr}$ were estimated…": The authors need to give more details on how to calculate $F_{clr}$ based on the

satellite observations in the equi-albedo method.

4. Line 154 “The particle aspect ratio is set to 0.8.”: The authors need to provide references to support this setting.

5. Line 165 “…(AERONET) inversion products”: The authors need to give more details on the inversion products.

6. Line 229: The authors need to give more specific information on the CERES single scanner footprint (SSF) level 2 dataset.

7. Lines 254-255 “The TOA SW radiative flux distribution shows the highest value over cloud conditions.”: From Figure 6, this description is not always true. For example, the bottom right corner in Figure 6f.

8. Line 256: Please explain “the SW albedo of the aerosols in the cloud”.

9. Line 281 “According to the definition, the $DRFE_{dust}$ represents the $DRE_{dust}$ of a certain AOD at per unit area”: It is not consistent with the previous definition in section 2.

10. Line 326 “However, the contents of the SW radiative flux change little…”: Please explain “the contents of the SW radiative flux”.

11. Subsection “4.1 Dust microphysical properties”: The authors need to give more details on the retrievals of these dust microphysical properties in this subsection.

12. Line 360: Please provide the definition of moderate aerosol particle.

13. Line 365: I am surprised at the result of the peak radius (1.71 μm) of dust particles at Tamanrasset station. The authors need to double-check it and compare with the results from other literature.

14. Line 392-393 “The higher value of SSA shows that dust aerosol particles scatter more predominantly and strongly in the Taklimakan desert (Kashi)…”: That cannot be clearly obtained from Figure 14.

15. Lines 399-403 “The ASY value increases with the particle size”, “It can be found that the dust aerosols from the Sahara Desert (Tamanrasset) have higher values of the ASY than those from the Taklimakan desert (Kashi) …”: They seem to contradict with the previous results those the peak radius of dust particles at Tamanrasset (1.71 μm) is smaller than that at Kashi (2.24 μm).

16. Lines 403-404 “The high values (over 0.80 at 440 nm) reflect the dominance of the absorbing of dust aerosols”: The authors need to give an

explanation.

17. Lines 404-406 “The stronger backward scattered energy may cause higher negative radiative forcing in the Taklimakan Desert (Kashi)”: It is not always true at TOA, in the atmosphere, or at BOA.

18. Lines 416, 475: I cannot find the results of backward scattering coefficients.

19. Lines 417-419: The mean $DRFE_{dust}$ results obviously disagree with those in Figure 8. How did the authors draw a conclusion that “The results are in good agreement with those estimated by the satellite observations”?

20. Lines 424, 427-428: The authors need to give more details on the calculation of 9.0%, 7.6% and 6.8%.

21. Lines 425-426 “Even for the same dust microphysical property, the $DRFE_{dust}$ varies significantly according to whether the dust particles are considered spherical or non-spherical in different methods”: Particle shape or morphology is microphysical property.

22. Table 1: Please double-check the data in this table. The ranges “-32.2~-44.3” and “-41.5~-47.4” were obtained due to the SBDART radiative transfer calculation methodology employing different Mie or T-matrix models. The differences cannot be considered as the ranges of $DRFE_{dust}$ variation.

23. Lines 462-463 “The compared results show that the $DRFE_{dust}$ derived from the satellite-based equi-albedo method is closer to that in previous studies with lower uncertainty”: I cannot find the evidences that the proposed satellite-based equi-albedo method with lower uncertainty.

24. Line 466 “Therefore, the uncertainties can be evaluated more reasonably.”: Please explain and certify “more reasonably”.

**Some technical comments:**

The authors need to read through the manuscript carefully and correct the grammatical errors. I just picked a few of them:

1. Lines 58-60: The large spatial variability of aerosols and the lack of an adequate database on their properties makes $DRE_{dust}$ and $DRFE_{dust}$ much difficult to estimated (Satheesh and Srinivasan, 2006).

2. Lines 78-80: Thus, the assessment of the SW $DRFE_{dust}$ and microphysical properties of the dust over these regions is important for evaluating regional and global climate changes.

3. Lines 156-157: Santa Barbara Disort Atmospheric Radiative Transfer (SBDART) is an RTM that calculates the plane-parallel radiative transfer of the earth-atmosphere system (Ricchiazzi et al., 1998).

4. Lines 272-273: To avoid the influence of the LSA and SZA in estimating the $DRFE_{dust}$, pixels with LSA of 0.16–0.20 and SZA of 32–38 degrees are chosen to derive the $DRFE_{dust}$.

5. Line 324: The sensitivity test of SW radiative flux at the TOA to changes height of dust layer.

6. Lines 325-326: As Fig.11 shown, the SW radiative flux at the TOA was decreased with the height of dust layer was increased from 0km to 18km.

7. Lines 326-327: However, the contents of the SW radiative flux change little with the height of dust layer increased (within $1.5Wm^{-2}$, 0.47%), which is little than CERES observation errors.

Some figures in the manuscript were hard to read:

8. Figure 2: The longitude and latitude are not shown in the figure. The regions of the two red boxes do not seem to correspond exactly to the images of MODIS Aqua.

9. Figures 3,5-10,12-15: Axis labels and legends are very small in these figures.

10. Figure 8: The units of $DRFE_{dust}$ should be added.

11. Figures 14-16: It is recommended to use “Mie” (starting with capital letter) instead of “mie” in these figures.

12. Figures 14,16: The lines in these figures are hard to distinguish.

The full names of acronyms should be given as they appear for the first time and keep case consistent in the full text:

13. Line 114: the full name of sensor CERES should be given as they appear for the first time in the full text.

14. Figure 3: the full name of “WSA” should be given.

15. Lines 194-195 and some other places in the text: “Aqua” and “AQUA” are suggested keeping consistent.