We thank both reviewers for the constructive comments, which have improved our paper substantially. Below please find our point-to-point response to each reviewer's comment:

Responses to Reviewer #1's comments:

This is an interesting modeling paper that focuses on the vertical distribution of dust and how that varies depending on the three main source regions listed in the title. The paper is overall well-written and interesting, but has a number of issues in comparisons against data and it is not clear what this paper contributes that is not already in the literature. The paper needs major revisions.

Major comments:
1. Although this seems to be solid work (but see comments on methodology below), the rationale for this work is in my opinion not really articulated. Reading the abstract, I did not really learn anything that was not already in the (excellent) previous related paper by Froyd et al. (2022) and by other papers on the contributions of different dust source regions by Tanaka and Chiba (2006), Chin et al. (2007), and Kok et al. (2021). In other words, it's not really clear to me what the reader can learn from this paper that is not already in the literature. For this paper to be published, that should be convincingly articulated in the abstract and other key parts of the paper.

Thanks for the comments. We have highlighted our major findings which are not the research focus in the previous studies:

1. We have validated our model by comparing against the new global size distribution measured by Kok et al. (2021). Shown in Kok et al. (2021), aerocom models commonly underestimates larger particles and overestimates small dust particles by order of magnitude.
2. In our paper, we focus on the upper tropospheric and lower stratospheric (UTLS) dust abundance and their origin (Figure 8, 9, 10). Since dust are emitted from surface, surface measurements of dust are needed and helpful to validate our model. In the revised manuscript, we highlight our findings of UTLS dust. We consider it not a focus in previous dust modeling studies.
3. As mentioned by the reviewer, one novelty is the usage of the NASA Atom dust global and vertical profiles (Figure 4, 5). The global dust distributions especially in UTLS provides an excellent and unique opportunity to model the dust-climate interactions.

We rewrite the abstract and introduction and highlighted those points.

2. The processing of data and comparisons against measurements needs more rigor and detail:
1) My understanding is that the cruise data from (Mahowald et al., 2009) mentioned

in 2.5 represent daily values, not monthly values as listed on line 194. Comparing this against an annually averaged climatology as you do opens you up to tremendous biases. For instance, the cruise data might be weighted towards a particular season (e.g., summer, which would bias the data high relative to an annual mean). Also, the daily concentration can vary by orders of magnitude so comparing against an annual mean will generate a very large representation error. At the least, these issues need to be discussed and realistic error bars need to be included in your comparisons, as done for instance in Huneeus et al. (2011). You also should match the season or month in which the cruise data are taken for a more meaningful comparison.

We added error bars in Figure 3 following the method suggested by Huneeus et al. (2011) and Mahowald et al. (2008).

In line 235, we added discussions:
"In order to explain the bias, we show the error bars by the median 66 % of the modeled daily averaged concentrations (denoted by the vertical dashed line) for each cruise data following the method suggested by Mahowald et al. (2008) and Huneeus et al. (2011)."

2) The “long-term” measuring station data is a misnomer as it is really only >3 days of data (see Mahowald et al., 2009, p. 251). So this data is subject to largely the same issues as the cruise data. Please explain that as well.

Clarified in Line 176:
“Data compiled by Mahowald et al. (2009) contain the dataset of short-term measurements from cruises and monitoring stations with daily averaged surface dust concentrations.”

3) The comparison against the Atom dust data is one of the more novel parts of the paper as I think this hasn’t been done before. How exactly were the ATom data processed? Does each data point represent an average over some time period (e.g., an hour) or over some area? Were the different measurements simply averaged or are you showing median values? And here the same problems with representation errors apply, which should be discussed. And you should at least match the month or season of the measurement, rather than compare against an annual climatology (or maybe you did? If so, it’s not clear from the text and figure 4).

In the revised manuscript, we have compared our simulations with ATom data along the flight track to make apple-to-apple comparison.

In Figure 4, each data point represents measured dust concentration below 990 mb along the flight track. In the meantime, we use the simulated dust concentrations along the Atom flight track.

Clarified in Figure 4 caption:
“Figure 4. Comparison of the dust concentration below 900 mb with particle diameter less than 4.5 µm simulated by CESM1/CARMA with the NASA ATom 1 airborne campaign. Both model and observations are sampled along the NASA ATom-1 flight track.”

4) Please include basic statistics in your comparisons against data (Figs. 3b, c, 4c, 6b), including correlation coefficients and root-mean-square error.

Done, the statistics are added in the figures.

5) Figure 5 has some issues. The legend in (a) is too small to read; please enlarge. The line styles are confusingly labeled in that there are two solid lines (one with symbols) and no dashed lines. The difference between the two simulated lines is also not clear from the figure caption. Most importantly, how is the comparison between measurements and simulations done and is this an apples-to-apples comparison? I assume a simple average was taken for all measurements within the specified latitude band? But then how was the simulation average constructed? Did you select the closest match in space for each measurement and then average those? Or did you take the average for the entire region in the monthly-averaged simulation? The latter would be problematic for some of the reasons above and the former would be more of an apples-to-apples comparison.

We fixed Figure 5.

For the comparison with the ATom dust vertical distributions, both model and observations are sampled along the flight track as well in the revised manuscript. Along the flight track, the simulated dust concentration in the closest model grid is compared against the observations. We averaged all the measurements as well as the model output along the flight track in the latitude band
In Line 170 - 172, we clarified as:
“To directly compare with ATom dust vertical profiles measured by PALMS, we sample the simulated dust concentration with geometric diameter between 0.1µm and 4.5 µm along the ATom flight track.”

6) The comparison against AERONET data (Fig. 6) also needs more detail. First, some objective criteria should be provided that justifies picking the specific sites that were used. Second, since it’s more common to use AOD in the visible wavelength, could you provide a reason why you used AOD at 1020 nm? I assume because a larger fraction of the signal is dust at that wavelength? Third, since your comparison depends on the accuracy of the simulated AOD of other aerosols, please provide maps of the simulated dust and non-dust AOD to help interpret your results, for instance in the supplement.

We picked the AERONET inside the major dust emission regions shown in Figure 2. Clarified in Figure 6 caption: “The measured AOD from 18 AERONET ground sites located inside the major dust emission regions (Figure 2) are denoted by the color-coded circles.”

In the revised manuscript, we switched the wavelength to the mid-visible AOD at 532 nm in Figure 6. We also added one panel showing the simulated AOD without dust in the model. The comparison shows that without dust in the model, simulated AOD is only 24% of observations.

Following the suggestion by the reviewer, we add panels (Fig.6b and Fig.6d) on the simulated AOD without dust. In Line 309 - 317, we added discussions:

“The simulated aerosol optical depth (AOD) at 532 nm wavelength from CESM1/CARMA is compared to the measurements near dust source regions from 2014 to 2018 for most of the Aerosol Robotic Network (AERONET) sites (Figure 6a). We use 18 AERONET sites inside of the major dust emission region shown in Figure 2. On average, the model underestimated the annual mean AOD of the selected 18 AERONET sites by 19%. The model underestimates the averaged AOD by ~14% in North America and ~25% in Middle East. Figure 6b shows the simulated AOD without dust emitted in the model underestimated the AERONET AOD by 74% on average. The simulations with and without dust emission suggest that dust contribute to over 50% of simulated AOD in the selected AERONET sites.”

3. Section 3.2: It seems clear that your model underestimates dust because it underestimates the various measurements of surface dust by ~10-60% (Figs. 3 and 4) and underestimates AOD in dusty regions by ~10-20% (Fig. 6). Why don’t you simply scale up the factor C on line 149, which as you note is an “arbitrary constant” anyways? Since your dust does not feedback onto the meteorology (at least, I think so; please specify this in section 2.1), you could simply do the rescaling offline.

Thanks for the comments.
In the original manuscript, our simulated dust underestimated the measured dust concentration from University of Miami network by 70%, while overestimated the dust concentration from the complied dataset (Mahowald et al., 2009) by a factor of 3.75. We have done sensitivity simulation by scaling the factor C from 0.6 to 1.5. As shown in Table1, the simulated dust concentration with scaled C underestimated the University of Miami network by 56%, while overestimated the dataset from Mahowald et al. (2009) by a factor of 5.7. In another word, simulation with the scaled C improved the comparison against the University of Miami data, however made the comparison against the data from Mahowald et al. (2009) worse. We decide to stick with the original C factor.

Table1. Comparison of the base model and the revised model.

Measurements	Correlation coefficients		MNB		RMSE	
	[a]old	[b]new	old	new	old	new
Mahowald et al. (2009)	3.75	0.13	3.75	5.7	31.64	43.83
The University of Miami network (Prospero, 1989; Arimoto et al., 1996)	0.84	0.83	-0.7	-0.56	0.88	0.87

[a]Base model with the factor C of 0.6; [b]Revised model with the factor C of 1.6.

4. Section 4: relating to my first comment, previous work by for instance Tanaka and Chiba (2006), Chin et al. (2007), and Kok et al. (2021) has studied how different source regions contribute to the global distribution of dust. The authors should discuss how their results differ from these previous works and what new information we learn from their study that we didn't already know from those previous studies. Also, are the relative contributions of the different source regions supported by experimental constraints on source provenance of deposited dust?

Thanks for the comments.

a. Please refer to our response to comment1, we highlighted the difference with previous studies in the revised manuscript.
b. We highlighted the difference between our study and previous work on dust global distributions near line 331:

"In this section, we show the global distributions and source attributions of dust from the surface to the lower stratosphere. Consistent with previous studies (Tanaka et al., 2006, Chin et al., 2007 and Kok et al., 2021), modeled North African dust accounts for about 50-60% total global dust loading (mostly in the lower troposphere). Validated by the recent global NASA ATom measurements, our study calculated the dust source attributions in each altitude and the dust source attribution in the anticyclone of the Asian summer monsoon region. We show that the Asian dust with less annual emission than the North African dust is transported higher and become dominant in the upper troposphere and lower stratosphere (UTLS)."

In the revised manuscript, we added discussions related to dust depositions found in Amazon and Greenland:
Line 356:
"The trans-Atlantic transport of the African dust to Amazon Basin in the northeasterly trade winds are observed (Yu et al., 2015; Swap et al., 1992; Prospero et al., 2014). Based on satellite and in-situ deposition data, Yu et al. (2015) quantified the deposition of African dust in the Amazon basin. Consistently, our simulated dust over Amazon Basin is primarily transported from North Africa."

Line 378:

"Our model suggests that the contribution of North African and Asian dust to the surface dust in the Arctic is similar. Significant contributions of Asian dust are confirmed through ice core isotopic analysis of the dust deposited at the ice camp in Greenland (Bory et al., 2002; 2003)."

Other comments:

1. Doesn't your source region in North Africa include the Sahel? It's difficult to see in Figure 2. If so, then "Saharan" in the title and at many places in the paper should probably be changed to "North African".

Thanks for the comment and we are sorry for this confusion. Our source region in North Africa includes the Sahel. We have changed "Saharan" to "North African" throughout the revised manuscript.

2. Relatedly, please specify the exact coordinates of the three source regions shown in Figure 2 for clarity.

We agree with the reviewer's comment, and we have added the exact coordinates of the three source regions in Figure 2. We clarified in Figure 2 caption:
"The regions of interest are denoted by the black boxes. The coordinates of the three regions are (1) North African source (20°W-35°E; 10.4°-36.9°N), (2) Middle Eastern source (35°-60°E; 6.6°-38.8°N), and (3) Asian source (55°-60°E for 40.7°-48.3°N, and 60°-125°E for 25.5°-48.3°N)."

3. Lines 18-19: The statement about uncertainty due to shape and density seems a bit odd here. Although this statement is probably correct, the effects of dust shapes and density are not explored (or even discussed) in this paper. So I'd suggest either discussing this in the paper or removing it from the abstract.

We deleted the sentence in the revised manuscript.

4. Lines 59-61: I think the statement here is not quite right. My understanding is that Kok et al. (2021) found that models simulate a ~65% contribution of North African dust and that this is an overestimate, not that models overestimate North African dust by 65%. Please correct.

Thanks for point out our error, and it is corrected.

5. Line 104-5: Zender et al. (2004) is two decades old so the range of factor of four in dust burden in that study is not meaningful for the current state of knowledge. Please cite more recent work here and adjust the sentence accordingly.

We deleted the sentence in the revised manuscript.

6. The statement is made in a few places in the paper (e.g., lines 118-9) that the model is "constrained" by the Atom measurements. But my reading of the paper is that the authors merely compare their simulations against the Atom measurements and don't use those measurements to constrain the model. Please correct accordingly.

We deleted the word "constrained" in the revised manuscript.

7. Line 138: please specify the wavelength for which these are the assumed optical properties and how those optical properties vary with wavelength / radiation band. Also, unless this is for ~400 nm or something, your dust is much too absorbing according to current understanding (e.g., Sinyuk et al., 2003; Balkanski et al., 2007; Di Biagio et al., 2019). I don't think that matters for this study since you're not looking at radiative effects, but it should be noted.

We clarified near line 111 in the text:
"We assume that dust has a density of 2.65 g/cm3 and use wavelength dependent refractive indices (RI) (Yu et al., 2015b). The RI at 532 nm is 1.53-0.006i in the model, which is independent of the dust source region, even though these properties vary with dust source in reality. Note that the reported imaginary part of the refractive index of dust aerosol ranges from 0.0006 to 0.0048 according to previous studies (Sinyuk et al., 2003; Balkanski et al., 2007; Di Biagio et al., 2019), which suggests that our model may overestimates the absorption aerosol optical depth from dust aerosol."

8. Line 143: Please include a more appropriate and standard reference for this statement on the basic physics of dust emission.

Done, we added more appropriate references:
"The process is driven by surface stress, which is usually expressed as friction wind velocity (Ginoux et al., 2001)."

9. Line 163: please elaborate on what you mean by "secondary activation" here.

We clarified near Line 141:
"Particles are primarily activated at the cloud base. CESM1/CARMA considers the activation of particles, including dust, from the entrained air above the cloud bases (secondary activation, Froyd et al., 2022; Yu et al., 2019)."

10. Line 169: what do you mean by "solubility" here, exactly? Is this the assumed

fraction of the dust particle mass that is soluble? If so, isn't 0.2 much too high? Please include some justification for this particular model choice.

The solubility are tunning parameters wildly used in climate models (Liu et al., 2012, MAM3, Yu et al., 2015b). The larger the number is, more soluble the particles are. We clarified near line 147 in the text:
"For below-cloud scavenging, the tunning parameter for aerosol's solubility in CESM1/CARMA is 0.2 for dust and 1.0 for sea salt (Yu et al., 2015b)."

11. Line 184-6: what diameter type did the PALMS instrument measure, exactly? If it's based on time-of-flight, then I assume it's the aerodynamic diameter? And I assume the model simulated geometric diameter? Did the authors make corrections to convert between the two diameter types? If not, why not and what sort of errors from neglecting this do you expect?

PALMS aerodynamic diameters were converted to geometric diameters using a constant density and shape factor as described Froyd et al. (2022). In the manuscript, we compared the simulated dust mass concentrations (with geometric diameter less than 4.5 um) with the ATom PALMS dust (with geometric diameter less than 4.8 um). In the revised manuscript, we explicitly clarify that near Line 168.

12. Line 209-210: This statement doesn't make sense to me. Silt is >2 um diameter.

Sorry for the confusion, we clarified the typo:
"In this study, we simply adjust the mass fraction of the emitted dust in the silt bins with geometric diameter greater than 2 µm from 90% to 94%."

13. Figure 9: For clarity, please note in the caption that this is a zonal average.

Done

14. Line 427-428: Do you have support for the statement that dust contributes only 0.04% and 3% to aerosol mass at 100 and 200 hPa, respectively? I didn't see any results in the paper that directly address that. Perhaps you could include results of non-dust mass concentrations in the supplement.

Thanks for pointing it out. We add a supplement Figure S6 and show that the simulated dust mass fraction in the Asian summer monsoon region.

Responses to Reviewer #2's comments:

The authors present a global dust transport modelling study to provide a quantification of the contribution of key dust source regions in North Africa, Asia, and the Middle East to the global dust distribution. The CESM1/CARMA model results are evaluated with ground measurements of dust size distribution, vertical profiles from ATom aircraft observations, and AERONET sun photometer measurements.

The research question is compelling as well as relevant. However, the present work does not make use of its potential and shows considerable weak points. Primarily, it does not adequately motivate what distinguishes this work from previous model studies.

Thanks for the comment. Please see the reply to Reviewer1 comment 1. We have highlighted our findings in the revised manuscript.

While there are some improvements in the representation of large dust particles, the comparisons with aircraft profiles point to known model uncertainties in vertical transport and wet removal, which are not adequately addressed and raise questions about the robustness of the estimates of vertical transport contributions.

The modelled size distribution nevertheless shows weaknesses for dust particles beyond 10 µm diameter, and the opportunity is missed to investigate the role of giant dust particles, which according to measurements make a significant contribution up to the mid-troposphere but are not considered or underestimated here and in many other models.

I also miss a detailed description of the relevant transport processes in the respective regions and their seasonal cycle.

Unfortunately, the implications for example on heterogeneous freezing are only speculated.

Language wise, the manuscript is already in a good state, although there is a tendency that abbreviations are not explained. However, the quality of the figures needs to be improved in terms of label font size and a meaningful axis and colour bar labelling.

Thanks for comments. Those comments are addressed in the detailed comments section below.

Therefore, the manuscript can only be recommended for publication after major revisions.

Detailed Comments:

1. Page 7/8: Why are the comparisons with ATom2-4 not shown in the main text? In addition, the model results should be evaluated with CALIOP observations and ground-based lidar measurements.

Thanks for the comment. We compared ATom1 data with the simulation on the vertical distribution of dust in global remote area. We did include comparison with other phases of Atom in the supplement. Since we need discuss the distributions in the two oceanic basins in various latitude bands, we worry the figure can be too busy if we include all 4 phases of Atom data in the main text.

In the study we compared modeled dust to the surface and airborne measurements. The airborne aerosol instruments measures dust directly. Since ATom's data is pole-to-pole, and we think the climatology derived by the ATom data is sufficient for current study. However, we do agree with the reviewer that CALIOP provides good constrain on the simulated dust abundance in the troposphere as shown in Yang et al. (2022) and Yu et al., 2015.

2. Figure 1: Please provide meaningful label for the y-axis or explain in the figure caption. Despite the improvements in the representation of coarse dust particles compared to the AeroCom models, particles larger than 10 µm are still underestimated. This fact should be discussed in more detail, especially with respect to the generally underestimated role of giant dust particles in current dust models.

We clarified in the figure caption for Figure 1 near Line 201:
"The dust mass size distributions are divided by the total dust mass integrated over the size range (i.e. the area under each dM/dlnD curve)."

The reviewer is correct that CESM-CARMA still underestimated the coarse-mode dust (>10 µm) by 48%. In the study, we focus on the dust vertical distribution especially in the upper troposphere and lower stratosphere. The coarse-mode dust (>10 µm) falls out and make extremely limited contribution to the upper troposphere.

We added discussion near Line 195:
"The simulation show that the model underestimates the coarse-mode dust with diameter larger than 10 µm by ~48%. The modeled total dust concentration at surface can be biased low, while modeled dust in the upper troposphere is not significantly affected as giant dust particles sediments quickly."

3. Pages 14–16, Sect. 3.3 and 3.4: Possible reasons for the discrepancies between model results and observations have to be more sufficiently discussed. What is the reason for choosing the wavelength 1020 nm, would not 500 nm or 550 nm be more common?

We use 18 AERONET sites inside of the major dust emission region shown in Figure 2.

In the revised manuscript, we use the mid-visible AOD at 532 nm in Figure 6. We also added one panel showing the simulated AOD without dust in the model following suggestion by Reviewer #1. The comparison shows that without dust in the model, simulated AOD is only 24% of observations.

In Line 309 - 317, we added discussions:
"The simulated aerosol optical depth (AOD) at 532 nm wavelength from CESM1/CARMA is compared to the measurements near dust source regions from 2014 to 2018 for most of the Aerosol Robotic Network (AERONET) sites (Figure 6a). We use 18 AERONET sites inside of the major dust emission region shown in Figure 2. On average, the model underestimated the annual mean AOD of the selected 18 AERONET sites by 19%. The model underestimates the averaged AOD by ~14% in North America and ~25% in Middle East. Figure 6b shows the simulated AOD without dust emitted in the model underestimated the AERONET AOD by 74% on average. The simulations with and without dust emission suggest that dust contribute to over 50% of simulated AOD in the selected AERONET sites."

4. Pages 19/20, Sect. 4.2: How can these contributions to the vertical layering be interpreted with respect to the model uncertainties in vertical mixing shown in Fig. 5? What are the relevant meteorological processes behind these transport patterns? Describe in detail and refer to literature.

Thanks for the comment.

a. From Figure 5, in the revised manuscript, we compared the modeled dust vertical distribution with the ATom observation along the flight track instead of the climatology. The new comparison is indeed improved especially in Northern Hemisphere.
b. Shown in Figure 5b, modeled dust agree with observed dust in the Atlantic basin in the tropics. Modeled tropical dust over the Pacific basin overestimated the atom1 observation by one-order of magnitude. However, model's performance varies with seasons. For example, model underestimated the ATom3 observation by one-order of magnitude, while agree with ATom2 and ATom4 observations (in supplement figures). In general, modeled annual mean distribution of tropical dust is of large uncertainties (in Figure 8). Especially the convective transport parameterization for a climate model with coarse resolution is still highly uncertain.

We added discussions near Line 406:
"Note that the tropical dust in the middle and upper troposphere over the Pacific basin is overestimated by one order of magnitude compared with the Atom1 observation (Figure 5). However, model's performance on the tropical dust varies with seasons. For example, model underestimated the Atom3 observation by one-order of magnitude, while better agreements are made compared with Atom2 and Atom4 observations (Supplement figures S2-S4). In general, modeled annual mean distribution of tropical dust is subject to large uncertainties (in Figure 8). Especially

the convective transport parameterization for a climate model with coarse resolution is still highly uncertain."

5. Page 21, Sect. 5: The effects on cloud freezing are only speculation. Could you make any conclusions on the basis of your model simulations?

Thanks for the comment. Cirrus cloud and its connections to dust is indeed very interesting and important to know. In current study, we aim to better understand the spatial-temporal distribution of dust especially on the vertical distribution from surface to the cirrus altitudes. The implication to dust heterogeneous nucleation and climate forcing is an interesting study we plan to work on for the next step.

Line 439 - 441, we clarified in the manuscript:
"In the meantime, a high occurrence of cirrus clouds is found by satellites (Sassen et al., 2008; Nazaryan et al., 2008). The relative contributions of dust particles to the cirrus cloud in the ASM region remain unquantified and worth future evaluation."

6. Page 24, lines 484/485: Is this actually a result of this study? I could not find a corresponding description in the m

Both model and observations suggest that nitrate and organics are elevated in the ASM anticyclone (Yu et al., 2022).
We clarified the sentence to:
"The model suggests that the dust forms a local maximum in the ASM anticyclone as well as organics and nitrate (Yu et al., 2022)."

References

Arimoto, R., Duce, R. A., Savoie, D. L., Prospero, J. M., Talbot, R., Cullen, J. D., Tomza, U., Lewis, N. F., and Ray, B. J.: Relationships among aerosol constituents from Asia and the North Pacific during PEM-West A, J. Geophys. Res. Atmospheres, 101, 2011–2023, https://doi.org/10.1029/95JD01071, 1996.

Balkanski, Y., Schulz, M., Claquin, T., and Guibert, S.: Reevaluation of Mineral aerosol radiative forcings suggests a better agreement with satellite and AERONET data, Atmos. Chem. Phys., 7, 81-95, 10.5194/acp-7-81-2007, 2007.

Bory A J M, Biscaye P E, Svensson A, et al.: Seasonal variability in the origin of recent atmospheric mineral dust at NorthGRIP, Greenland, Earth and Planetary Science Letters, 196(3-4): 123-134, https://doi.org/10.1016/S0012-821X(01)00609-4, 2002.

Bory A J M, Biscaye P E, Grousset F E.: Two distinct seasonal Asian source regions for mineral dust deposited in Greenland (NorthGRIP), Geophys. Res. Lett., 30(4), https://doi.org/10.1029/2002GL016446, 2003.

Chin, M., Diehl, T., Ginoux, P., and Malm, W.: Intercontinental transport of pollution and dust aerosols:

implications for regional air quality: Atmos. Chem. Phys., 7, 5501-5517, 10.5194/acp-7-5501-2007, 2007.

Di Biagio, C., Formenti, P., Balkanski, Y., Caponi, L., Cazaunau, M., Pangui, E., Journet, E., Nowak, S., Andreae, M. O., Kandler, K., Saeed, T., Piketh, S., Seibert, D., Williams, E., and Doussin, J. F.: Complex refractive indices and single-scattering albedo of global dust aerosols in the shortwave spectrum and relationship to size and iron content: Atmos. Chem. Phys., 19, 15503-15531, 10.5194/acp-19-15503-2019, 2019.

Ginoux, P., Chin, M., Tegen, I., Prospero, J. M., Holben, B., Dubovik, O., and Lin, S. J.: Sources and distributions of dust aerosols simulated with the GOCART model, J. Geophys. Res. Atmos., 106, 20255-20273, doi: 10.1029/2000jd000053, 2001.

Huneeus, N., Schulz, M., Balkanski, Y., Griesfeller, J., Prospero, J., Kinne, S., Bauer, S., Boucher, O., Chin, M., Dentener, F., Diehl, T., Easter, R., Fillmore, D., Ghan, S., Ginoux, P., Grini, A., Horowitz, L., Koch, D., Krol, M. C., Landing, W., Liu, X., Mahowald, N., Miller, R., Morcrette, J. J., Myhre, G., Penner, J., Perlwitz, J., Stier, P., Takemura, T., and Zender, C. S.: Global dust model intercomparison in AeroCom phase I, Atmos. Chem. Phys., 11, 7781-7816, doi: 10.5194/acp-11-7781-2011, 2011.

Kok J. F., Adebiyi, A. A., Albani S., et al. : Contribution of the world's main dust source regions to the global cycle of desert dust, Atmos. Chem. Phys., 21, 8169-8193, doi: 10.5194/acp-21-8169-2021, 2021.

Mahowald, N., Jickells, T. D., Baker, A. R., Artaxo, P., BenitezNelson, C. R., Bergametti, G., Bond, T. C., Chen, Y., Cohen,D. D., Herut, B., Kubilay, N., Losno, R., Luo, C., Maenhaut,W., McGee, K. A., Okin, G. S., Siefert, R. L., and Tsukuda, S.:Global distribution of atmospheric phosphorus sources, concentrations and deposition rates, and anthropogenic impacts, Global Biogeochem. Cy., 22(4), GB4026, doi:10.1029/2008GB003240, 2008.

Mahowald, N. M., Engelstaedter, S., Luo, C., Sealy, A., Artaxo, P., Benitez-Nelson, C., Bonnet, S., Chen, Y., Chuang, P. Y., Cohen, D. D., Dulac, F., Herut, B., Johansen, A. M., Kubilay, N., Losno, R., Maenhaut, W., Paytan, A., Prospero, J. A., Shank, L. M., and Siefert, R. L.: Atmospheric Iron Deposition: Global Distribution, Variability, and Human Perturbations, Annual Review of Marine Science, 1, 245-278, doi: 10.1146/annurev.marine.010908.163727, 2009.

Nazaryan, H., McCormick, M. P., and Menzel, W. P.: Global characterization of cirrus clouds using CALIPSO data, Journal of Geophysical Research-Atmospheres, 113, doi: 10.1029/2007jd009481, 2008.

Prospero, J. M.: Mineral aerosol transport to the Pacific Ocean, Chem. Oceanogr., 10, 188–218, 1989.

Prospero, J. M., F.-X. Collard, J. Molinie, and A. Jeannot: Characterizing the annual cycle of African dust transport to the Caribbean Basin and South America and its impact on air quality and the environment, Global Biogeochem. Cycles, 29, 757–773, doi:10.1002/2013GB004802, 2014.

Sassen, K., Wang, Z., and Liu, D.: Global distribution of cirrus clouds from CloudSat/Cloud-Aerosol Lidar and Infrared Pathfinder Satellite Observations (CALIPSO) measurements, J. Geophys. Res. Atmos., 113, doi: 10.1029/2008jd009972, 2008.

Sinyuk, A., Torres, O., and Dubovik, O.: Combined use of satellite and surface observations to infer the imaginary part of refractive index of Saharan dust: Geophys. Res. Lett., 30, 10.1029/2002gl016189, 2003.

Swap R, Garstang M, Greco S, et al.: Saharan dust in the Amazon Basin, Tellus B, 44(2): 133-149, https://doi.org/10.1034/j.1600-0889.1992.t01-1-00005.x, 1992,.

Tanaka, T. Y. and Chiba, M.: A numerical study of the contributions of dust source regions to the global dust budget: Global Planet. Change, 52, 88-104, 10.1016/j.gloplacha.2006.02.002, 2006.

Tegen, I. and Lacis, A. A.: Modeling of particle size distribution and its influence on the radiative properties of mineral dust aerosol, J. Geophys. Res. Atmos., 101, 19237-19244, 10.1029/95jd03610, 1996.

Tegen, I.: Modeling the mineral dust aerosol cycle in the climate system: Quat. Sci. Rev., 22, 1821-1834, 10.1016/s0277-3791(03)00163-x, 2003.

Vernier, J. P., Thomason, L. W., and Kar, J.: CALIPSO detection of an Asian tropopause aerosol layer, Geophys. Res. Lett., 38, doi: 10.1029/2010gl046614, 2011.

Wu, C., Lin, Z., Liu, X. : The global dust cycle and uncertainty in CMIP5 (Coupled Model Intercomparison Project phase 5) models, Atmos. Chem. Phys., 20, 10401-10425, doi: 10.5194/acp-20-10401-2020, 2020.

Yang, K., Wang, Z. E., Luo, T., Liu, X. H., and Wu, M. X.: Upper troposphere dust belt formation processes vary seasonally and spatially in the Northern Hemisphere, Communications Earth & Environment, 3, doi: 10.1038/s43247-022-00353-5, 2022.

Yu, H. B., Chin, M., Winker, D. M., Omar, A. H., Liu, Z. Y., Kittaka, C., and Diehl, T.: Global view of aerosol vertical distributions from CALIPSO lidar measurements and GOCART simulations: Regional and seasonal variations, J. Geophys. Res. Atmos., 115,doi: 10.1029/2009jd013364, 2010.

Yu, H. B., Chin, M., Bian, H. S., Yuan, T. L., Prospero, J. M., Omar, A. H., Remer, L. A., Winker, D. M., Yang, Y. K., Zhang, Y., and Zhang, Z. B.: Quantification of trans-Atlantic dust transport from seven-year (2007-2013) record of CALIPSO lidar measurements, Remote Sens. Environ., 159, 232-249, doi: 10.1016/j.rse.2014.12.010, 2015a.

Yu, P. F., Toon, O. B., Bardeen, C. G., Mills, M. J., Fan, T. Y., English, J. M., and Neely, R. R.: Evaluations of tropospheric aerosol properties simulated by the community earth system model with a sectional aerosol microphysics scheme, J. Adv. Model. Earth Syst., 7, 865-914, doi: 10.1002/2014ms000421, 2015b.

Yu, H. B., Chin, M., Yuan, T. L., Bian, H. S., Remer, L. A., Prospero, J. M., Omar, A., Winker, D., Yang, Y. K., Zhang, Y., Zhang, Z. B., and Zhao, C.: The fertilizing role of African dust in the Amazon rainforest: A first multiyear assessment based on data from Cloud-Aerosol Lidar and Infrared Pathfinder Satellite Observations: Geophys. Res. Lett., 42, 1984-1991, 10.1002/2015gl063040, 2015c.

Yu, P. F., Froyd, K. D., Portmann, R. W., Toon, O. B., Freitas, S. R., Bardeen, C. G., Brock, C., Fan, T. Y., Gao, R. S., Katich, J. M., Kupc, A., Liu, S., Maloney, C., Murphy, D. M., Rosenlof, K. H., Schill, G., Schwarz, J. P., and Williamson, C.: Efficient In-Cloud Removal of Aerosols by Deep Convection, Geophys. Res. Lett., 46, 1061-1069, doi: 10.1029/2018gl080544, 2019.

Yu P, Lian S, Zhu Y, et al.: Abundant Nitrate and Nitric Acid Aerosol in the Upper troposphere and Lower Stratosphere: Geophys. Res. Lett., https://doi.org/10.1029/2022GL1002582022, 2022.

Zender, C. S., Miller, R. L. R. L., and Tegen, I.: Quantifying mineral dust mass budgets:Terminology, constraints, and current estimates, Eos Trans. Am. Geophys. Union, 85, 509–512, doi: 10.1029/2004EO480002, 2004.