

Dear Adrian and co-authors,

I made below a few comments related to your submitted manuscript as I found several cases of mis-representation or inaccurate description of the model and data I have been developing with my collaborators. Hopefully, you will find them useful to improve the manuscript.

Regards,
Paul Ginoux

Lines 29-31: *“Many of the traditional dust emission models (TEM) assume that the Earth’s land surface is devoid of vegetation, then adjust the dust emission using a vegetation cover complement, and finally calibrate the magnitude of simulated emissions to dust in the atmosphere”*

The calibration is mostly related to numerical discretization of the momentum and continuity equations. Emission of dust in numerical models depends on the discretization of surface winds. The surface winds are inferred from the pressure level wind vectors derived by solving numerically the momentum equations. The numerical discretization of these equations will be affected by the numerical resolution. Obviously higher resolution will resolve sharp topographic variations with stronger downslope winds. On the other hand, flat terrain without roughness elements using low resolution will generate stronger gustiness. So, changing model resolution has a non-trivial effect on surface winds. Concerning dust emission, the flux depends on the cubic power of surface winds (see Equation 2), which will amplify wind bias related to model resolution. This implies that “tuning” dust emission is a required method to simulate scale-aware tracer with numerical model. This is also true for other tracers, such as sea salt emission from the oceans.

Lines 82-83: *“The common approach to modeling dust emission in ESMs uses globally constant values of aerodynamic roughness length (z_0), which are static over time and fixes $R(z_0) \approx 0.91$.”*

This is incorrect. In ESMs the momentum roughness length is calculated at every time steps and in every grid cells as a function of terrain variations, vegetation cover, snow cover, etc.

Line 81-85: *“The common approach to modelling dust emission in ESMs... This emission is then reduced by a function of vegetation cover and ultimately ‘tuned’ down to match observed in the atmosphere.”*

I am unaware of any ESMs who have implemented dust emission as described. I can certainly speak for NASA and GFDL models (Ginoux et al., 2001; Evans et al., 2016).

Lines 103-104: *“The u_{s*} is obtained directly from ω_{ns} , the normalised and rescaled shadow (1-albedo), enabling an albedo-based dust emission model (AEM; see Appendix for full description of the implementation)”*

Do I read correctly that you are scaling the friction velocity using 3 parameters with an exponential function of ω_{ns} ? Am I right that you will have to rescale ω_{ns} for any other satellite instruments with different viewing angles or radiometric characteristics? Is this not a global tuning?

Line 128-132: *“Evans et al., 2016”*

The characterization of GFDL model (Evans et al. 2016) is not correct. You may want to read the paper. We are not using $E=1-A_v$. The bare surface is calculated using an exponential function of the canopy (LAI) and stems, twigs, litters (SAI). The dust emission is calculated in each land tiles (primary, secondary vegetation, pasture and cropland) independently. Then the flux of dust is passing through a flux-exchanger into the atmosphere while a flux down from turbulence and settling is going in the land model. The latest ESM4 includes also tiles from fires and rangeland, in addition for taking into account slopes (Dunne et al., 2020; Horowitz et al., 2020). I will disagree with you when calling such detailed and consistent modeling of dust cycle a “crude model representation”

Lines 133-135: *“When the TEMs are applied in dust-climate ESMs it is assumed that this parameterization is adequate for climate projections. In contrast, the albedo-based scheme for sediment flux and dust emission (AEM; Eqs. 3, 4 & 5) represents the drag partition physics without pre-tuning to a fixed land surface condition, without the need for E, and thereby removes these additional sources of uncertainty.”*

The main point of using climate model has been missed here. Despite their approximations, ESMs simulate the different Earth’s climate systems consistently over time using different projection scenarios. While the proposed used of ω_{ns} (the normalized and rescaled shadow) is considered fixed (beyond MODIS period), ignoring vegetation and land use changes. The AEM technique is inadequate for future or past climates.

Lines 145-149: *“To understand the extent to which AOD estimates the spatial variation in dust emission magnitude and frequency we calculated the probability of dust occurrence modeled by the dust optical depth ($DOD>0.2$) using the criteria established previously (Ginoux et al., 2012). We note the stated limitations of DOD to be largely restricted to bright land surfaces in the visible wavebands which implies reduced performance over areas where vegetation is present.”*

This sentence contains several misunderstandings of our latest method developed with my co-authors to derive DOD.

In our 2012, we used the collection 5.1 of MODIS Deep Blue (DB), which provided aerosol products over bright surfaces. Since 2013 Collection 6 MODIS DB aerosol products have been extended to cover most (without snow or cloud cover) land surfaces (Sayer et al., 2014). All subsequent papers deriving DOD is using Collection 6.1 MODIS DB (e.g. Pu and Ginoux, 2017, 2018a, 2018b, 2020; Yu and Ginoux, 2021). A second update is the method to calculate DOD. Since Pu and Ginoux (2017), DOD is calculated using a quadratic function of aerosol optical depth (AOD) and the single scattering albedo (SSA). In our 2012 paper, DOD is calculated using an on/off switch depending on the value of the Angstrom Exponent (AE). Then a threshold is applied to detect the highest frequency to correspond to actual dust sources. The method has been compared to independent geomorphological data over the Chihuahuan desert (Baddock et al., 2016) to prove that MODIS DB DOD is able to successfully detect high-resolution dust sources. It will be necessary to add a note the text stating that you are referring to an old dataset long replaced by thoroughly validated values using the latest MODIS aerosol products. Preferably, you replace the sentence by referring to more recent thoroughly validated values using the latest MODIS aerosol products.

Line 149-150: *“To calculate DOD, we used wavebands available from monthly Moderate Resolution Imaging Spectroradiometer (MODIS; MOD08 M3 V6.1) at a 1-degree pixel resolution (Platnick, 2015)”*

These are monthly gridded products at 1 x1 degree resolution from 1 km daily pixels. Using coarse resolution monthly aerosol products is totally inadequate to compare with point source dust plumes. The difference with MODIS DB DOD from my team is that it is 0.1 x 0.1 degree twice-daily products, which was shown to be appropriate to detect tiny dust sources (Baddock et al., 2016).

It is unclear how you get these DOD. Citing Platnick, 2015 is not helpful. What MODIS Level 3 products are you using? Are you using Dark Target or Deep Blue algorithm? Maybe you are using a blend of the two algorithms? DOD is not part of these products, so a description on how you obtain DOD from MODIS would be helpful, especially that you assimilate it to Ginoux et al. (2012) which is outdated.

Line 154: *“We also provided a theoretical basis for TEMs formulation to be incorrect.”*

I commented earlier that your description of TEMs formulation is mostly incorrect.

Line 166: *“using MODIS data at 250 m spatial resolution with visible to thermal infrared wavebands”*

This is incorrect. Only bands 1 and 2 are provided at 250 m. Bands 3 to 7 are at 500 m resolution. Bands 8 to 36 are at 1 km resolution. Red is band 2, blue is band 3 and green band 4. Deep blue is band 8 or 1 km pixel. Then 10 x 10 pixels are aggregated to provide 10x10 km Level 2 daily aerosol products.

Line 171: “*DOD modelled frequency describes DOD > 0.2*”

In Baddock et al. (2016), we used $DOD > 0.75$ over the Chihuahuan desert, but Pu et al. (2020) used 2 threshold values (0.2 and 0.02) depending on the continent. Choosing a DOD threshold should be adapted to the objectives of the study but using gridded 1-degree monthly products is too coarse spatially and temporally to study dust sources.

Lines 248-249: “*the TEM is driven by wind speed attenuated by aerodynamic roughness which is fixed over space and static over time,...*”

If the surface conditions don't change (no snow, no vegetation or land use changes) this will be true, but most ESMs (or TEMs) include such changes when resolving the boundary layer properties.

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