Geoscientific Model Development Discussions

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Comment on gmd-2022-157

Anonymous Referee #2

Referee comment on "Application of a satellite-retrieved sheltering parameterization (v1.0) for dust event simulation with WRF-Chem v4.1" by Sandra L. LeGrand et al., Geosci. Model Dev. Discuss., https://doi.org/10.5194/gmd-2022-157-RC2, 2022

This paper implements a vegetation sheltering parameterization into the WRF-Chem AFWA dust model and tests it for a case study in the American southwest. Previously, vegetation coverage was inferred from “greenness” factors, which may under-represent brown and non-photosynthetic vegetation in arid regions. The parametrization here uses vegetation shadows derived from MODIS to determine a vegetation height to represent roughness lengths.

Overall, the paper is well-written and straightforward and I would like to see it published in GMD. The manuscript has the potential to advance the representation of dust emissions in numerical models and constrain the scalable factors inherent to dust parameterizations. The motivation and application of the sheltering factor seems solid, but only a single case study (with mostly qualitative results) is presented to test the new parameter.

Major Comments

- The main finding (at least for this case study – taken from Hyde et al. 2018) is that switching to the sheltering parameter decreases the area of dust source regions and therefore dust emissions. The case study was selected because the default AFWA scheme initially overpredicted dust, so implementing the sheltering parameter would naturally lead to a better fit between modeled and observed dust. However, there were an equal number of cases from the Hyde et al. (2018) ensemble that showed AFWA underestimating dust, which means the sheltering factor would lead to a worse fit. We can’t infer the impact of this parameter from a single case. I suppose once the parameter is released to the community it will be tested more and time will tell if it ends up being used. But, it would go a long way to test this parameter for a case study where dust was underpredicted too.
- There is a lot of confusion and debate in the dust parameterization literature over “roughness” factors and the scaling of dust emissions based on vegetation. Partly it’s

because there are multiple roughness effects and the terminology gets muddled. Thus, some schemes are probably double counting the effects of surface roughness on dust emissions (Webb et al., 2020). I recommend *overexplaining* what this new shielding term is representing physically and to be more explicit in what all these roughness effects and dust source terms do (sections 1-2). I have added the prefix "Terms" to specific comments where terminology could be confused and more explanation would be helpful to readers. For instance, does it or other terms represent the production or dissipation of momentum by roughness elements (or is that a PBL scheme effect?)? Or just shielding (i.e. dust gets caught in an obstacle or canopy and can't loft freely)? What about plant (stem/trunk) area reducing bare soil area? Etc.

- There is little discussion about the role of meteorology in the dust forecast. Since dust emissions scale as windspeed^3, small wind speed errors can lead to large dust errors. It's always hard in dust modeling to tell where the errors come from – the meteorology or the dust scheme. I would like to see more justification for why the errors in this case study were determined to be from the dust scheme and not the meteorology.
- How much of the PM10 is from other aerosol species than dust in the model?

Specific Comments [Line Numbers or Section]

[44-46] – A lack of representation of roughness elements is one reason for poor dust forecasts, the way it's written here makes it seem like it's the only or the major reason. Other important reasons would be model resolution, representation of cold pool and precipitation processes, source grid map, etc. Bukowski & van den Heever have done some work on the role of dust-lofting cold pools and model resolution (2020), but they also have a new paper (2022) showing that surface type and roughness effects are the most sensitive / important factor for predicting dust concentrations in cold pool dust events (haboobs) – similar to the July 2014 case study modeled here. This reference may help with motivations for this paper.

[48] – How is U* calculated in the model? Is it diagnosed like U10? In Eq. 1 Us* is a function of U10 and not U* - just checking that the model level / physical processes going into these equations are the same for comparing CTRL and the ALT simulations.

[51] – The approach here is to modify the surface Us* to include roughness elements (surface and above). But with the drag partitioning method of splitting up U*, there is also an Ur* term to represent roughness effects. Why did the authors seek to modify the Us* to include roughness elements instead of incorporating the shielding term into Ur*?

[65] – What about roughness elements like biocrusts, which are typically flat and sprawling?

[94-95] – Terms: describe more what the drag partition here refers to (Ur*?)

[95-96] – What about a dust underprediction event? See major comment #1

[98-100] – What about the meteorology? What if this convective case study is just difficult to get right?

[132] – Terms: is S the so-called "erodibility" map in some models?

[134] – Terms: aerodynamic roughness length – also is this part of the double-counting problem?

[143-151] – The description of S is confusing. Probably don't need the original formulation or Eq. 8, just how it is used here.

[154-155] – Walker et al. (2009) and Saleeby et al. (2019) are good references for showing the effect of high-resolution dust source maps for mesoscale modeling applications.

[169] – Terms: "normalized" appears in the name of Uns* (normalized surface friction speed) - is the "normalization" from the albedo normalization by Fiso or some other part? When I see "normalized...speed" my assumption that speed is the normalizing variable in the factor but I don't think that is the case here.

[180-181] – Dust schemes are mostly based on empirical fits to data. Was Eq. 13 fitted to some data that might be affected by simply substituting Us* into it? I.e. if Eq. 13 was tuned to dust observations, changing the denominator might de-tune that relationship.

[185 - 187] – Terms: excess wind friction speed. What is this physically? It seems like a model diagnostic more than something physical.

[193-195] – So whenever MODIS fails (missing data), Us* = 0 so there are no dust emissions in those pixels (or the whole domain if the retrieval fails broadly)? Why not default to the CTRL parameters if there is missing data?

[Section 2.3] – Is wet deposition of dust included? What about the convective transport of dust? These should also be added to Table 3.

[220] – 40 vertical levels is pretty coarse, especially for convective events. Since this is a cold pool case, how many levels are there in the boundary layer?

[258] – How would one go about tuning Cs? Also, there are already tuning constants (C) in dust models. Why go through the extra steps and use Cs rather than the classic C tuning in the bulk flux equation? Either way the model is being tuned to some sort of observation.

[315] – The simulated reflectivity also produces less widespread precipitation than the observations. What if the high dust levels in the control case is from less rain leading to insufficient wet scavenging of dust and not from over-emission? Could you compare precipitation measurements to precipitation in the model?

[329-330] – Not sure what this comment about shrubs and grasses has to do with the point preceding it.

[347] – The statement about soil moisture being important here contradicts the statement in [350-351] about it being relatively unimportant. It's probably just a wording issue. Note that Bukowski & van den Heever (2022) also found soil moisture to be relatively unimportant in haboobs.

[470-471] – Terms.

[Table A1] – It would be great to have more in-depth descriptions of variables. E.g. rather than just calling something a "constant," describe what that constant represents. Also a column for units would help.

[Fig. 3] – Maybe add variables to plot in case readers forget the long description. E.g. Source Function (S).

[Fig. 3] – The colorbar scale for sandblasting makes it look constant. Range of values may need to be adjusted to see heterogeneities.

[Fig. 4] – Panel a is tough to figure out with the overlap.

[Fig. 8] – Maybe its's the scaling of the colorbars again, but it is very difficult to see temporal changes in any of the variables.

[Fig. 11] – The colors in this colorbar are tough to discern since brown-orange represents low values and high values.