Geoscientific Model Development Discussions

Geosci. Model Dev. Discuss., referee comment RC1
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Comment on gmd-2022-157

Anonymous Referee #1

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-RC1, 2022

The manuscript entitled "Application of a Satellite-Retrieved Sheltering Parameterization (v1.0) for Dust Event Simulation with WRF-Chem v4.1" presents an albedo-based sheltering parameterization development to be used in dust transport modeling, namely WRF-Chem. The work presents a novel concept, and can potentially advance desert dust modeling. The structure of the paper is good, with extended information, clear methodology and solid scientific work. There is one major issue in my opinion that more testing should have been done for the domain configuration and there is a substantial lack of evaluation metrics. More information is given on the comments below.

- Very well structured introduction with adequate information of available parameterizations. In Line 63 please add the reference: Spyrou, C.; Solomos, S.; Bartsotas, N.S.; Douvis, K.C.; Nickovic, S. Development of a Dust Source Map for WRF-Chem Model Based on MODIS NDVI. Atmosphere, 2022, 13, 868. https://doi.org/10.3390/atmos13060868, which is an up-to-date use of NDVI in defining dust sources. In Line 77 please add the reference. Skamarock et al. (2019). This reference is written later on, but it is best to put it here, where is the first mention of WRF.
- Section 2.1.1. This section is unnecessary large and mostly a repetition of the AFWA processes already described in other works. I would suggest limiting this section to half a page by only keeping the equations that are mostly relevant to this work. For instance the S parameter equation and analysis is not needed. The sentence "Essentially, S is a spatially varying tuning parameter ranging from 0 to 1 that assumes erodible material accumulates in low points in the terrain." is enough.
- Section 2.1.2. Line 177. The process by which the daily MODIS-derived fields are incorporated is not clear. Are they a part of the WPS process or a module is created that reads and re-grids the MODIS files while the model is running? As is written I assume this happens during runtime. Can you expand a bit?
- Section 2.1.2. Line 179. You use the 10m wind speed that is derived while WRF is running. Why not use the first model level wind speed? In general we try to avoid the 10m speed as the 10m wind components are diagnostic quantities. If we need wind speeds this close to the ground it is best to lower the first model level as close as we can and increase the vertical levels used. This is critical as the dust emissions are very

sensitive to small changes in wind (as the authors state). If possible I would like to see changes between using 10m wind speed and first level wind speed (where the first level is close to 10m). It is entirely possible that the differences are negligible and 10m wind speed is adequate.

- Section 2.2. Just a small note for those unfamiliar with the area, the dust source area should be noted clearly.
- Section 2.2. The meteorological conditions and weather patterns that led to this event should be described in detail. For example Mean sea level pressure and wind patterns at the surface and at 850hPa should be added (even from the model simulations, if weather maps are not available). Is the event related to a density current? I see later on that you use NEXRAD. Is this the reason?
- Figure 2. Mark the X spot more clearly. Add a circle maybe?
- Section 2.3. Line 233. The 12 hour initialization is not adequate to generate a proper dust concentration background. In general 5-15 days are needed for this, but seeing as the dust event is very quick and localized one can assume that 12 hours is enough. Still this needs to be expanded upon.
- Section 3. A more thorough statistical analysis is needed. There are no statistical indexes calculated. Also the text stracture is a bit confusing. In my first read I thought that no timeseries was created until I saw figures 12 and 13. This needs to be written again in a more concise and analytical way. A statistical evaluation should also be performed, even a rudimentary one with all the available data for wind speed and PM10. Unfortunately qualitative analysis in not enough.
- The authors also state that "small shifts in the simulated dust position could greatly affect the apparent skill of the simulated output". This is correct but an effort should be made to setup the model in such a way to try to see if the wind and dust forecasts can be improved. Even using different initial conditions, or SST. Right now the selection of the domain was done based on another work which provided good results, but maybe this setup is not adequate for this study. More testing is needed in order to have a proper domain basis to evaluate the methodology.
- The results section is clear and the shortcomings of the methodology are presented. I would like to see a more extensive analysis on the benefits of using the proposed methodology in dust modeling

Should the above be addressed I would like to see this work published in GMD.