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## Reply on RC1

Sandra L. LeGrand et al.

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Author 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-AC1>, 2022

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\*\*Please note - all figure/table numbering referenced in this document refer to the numbering from the initial submission version of the manuscript unless otherwise specified.

**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 in the comments below.**

Response: Thank you for your review. We have made several of the reviewer's suggested changes in the revised manuscript and believe it has strengthened the paper. Regarding the domain and model configuration comments, we did an extensive series of sensitivity tests to establish our model configuration and limit the potential for simulation errors created by issues with the environmental forcing conditions from the parent WRF model on the dust simulation. These efforts are thoroughly documented in the report by Gallagher et al. (2022).

The Gallagher et al. study investigated the sensitivity of the simulated forcing conditions from the parent WRF model driving the dust simulation to model initialization (spin-up) time, initial atmospheric conditions, model resolution (both horizontal and vertical), planetary boundary layer scheme settings, land surface model settings, cloud microphysics settings, and cumulus scheme settings. While there are certainly more model configurations that could be tested, findings from the Gallagher et al. study helped us establish WRF model settings that effectively simulated the convective structure, evolution, surface winds, and general placement of the storm cell that generated the case study dust event discussed here in our dust modeling paper. We stress that the Gallagher et al. (2022) report is meant to complement this work and focuses on many of the atmospheric components relevant to the reviewer's concerns. The scope of our study here is to address the dynamic land surface and dust entrainment aspects of dust storm simulation, not the underlying atmospheric forcing studied in our related work.

We've updated the text from the first paragraph of Sect. 2.2 (model configuration) to make this more clear.

"We used WRF-Chem v4.1 for our test case simulation with WRF parent model configuration settings suggested by Gallagher et al. (2022) and chemistry settings from LeGrand et al. (2019) and Letcher and LeGrand (2018). The study by Gallagher et al. investigated the sensitivity of WRF-simulated atmospheric forcing conditions for the dust event studied here. In particular, they focused on the effects of model initialization (spin-up) time, initial atmospheric conditions, horizontal and vertical model resolutions, and several WRF physics package settings to determine the optimal model configuration that minimized environmental forcing condition errors on the dust simulation."

We recognize that some GMD readers outside the U.S. may have difficulty accessing the Gallagher et al. (2022) report from its official host site. A copy of the report is also available on ResearchGate: [https://www.researchgate.net/profile/Sandra-Legrand-2/publication/362509421\\_Simulating\\_Environmental\\_Conditions\\_for\\_Southwest\\_United\\_States\\_Convective\\_Dust\\_Storms\\_Using\\_the\\_Weather\\_Research\\_and\\_Forecasting\\_Model\\_v41/links/62ed81660b37cc3447718b53/Simulating-Environmental-Conditions-for-Southwest-United-States-Convective-Dust-Storms-Using-the-Weather-Research-and-Forecasting-Model-v41.pdf](https://www.researchgate.net/profile/Sandra-Legrand-2/publication/362509421_Simulating_Environmental_Conditions_for_Southwest_United_States_Convective_Dust_Storms_Using_the_Weather_Research_and_Forecasting_Model_v41/links/62ed81660b37cc3447718b53/Simulating-Environmental-Conditions-for-Southwest-United-States-Convective-Dust-Storms-Using-the-Weather-Research-and-Forecasting-Model-v41.pdf)

Regarding the quantitative metrics, we added additional commentary on quantitative surface wind speed assessments (please see our response to a later comment).

**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.**

Response: Thank you for the suggestions. We have added these references accordingly.

**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.**

Response: We appreciate the reviewer's comment and agree that shortening the previously published model component descriptions would make the paper more streamlined. However, while there are several other works documenting AFWA module equations and the processes they represent, several published sources also contain misinformation about how the AFWA module works. This lack of consistency in the literature is likely due to the eight-year gap between when the AFWA code became publicly available through the WRF-Chem framework in 2011 and when the original developers published the first in-depth overview of the AFWA module in LeGrand et al. (2019). Due to the poor documentation heritage associated with the AFWA code, we strongly feel that GMD readers will benefit from a comprehensive overview of the AFWA module components discussed in this paper, especially with respect to parameters like the source strength field ( $S$ ) that we removed or modified as part of our experiment.

**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?**

Response: For this study, we incorporated the MODIS-derived fields through an auxiliary channel while the WRF-Chem simulation was running. The report by Michaels et al. (2022) fully documents how WRF-Chem pipes these data from the auxiliary feed through the chem driver to the AFWA module. The Michaels et al. report also provides detailed step-by-step instructions and scripts for acquiring, processing, re-gridding, and ingesting the MODIS fields, which we note in this section and again in the model code availability section near the end of the manuscript. While it makes sense to eventually add these processes to the WRF Preprocessing System (WPS), these additional steps were beyond the scope of our study.

We've updated the sentence to clarify.: "To incorporate  $u_{s^*}$  into the AFWA dust emission, we configured WRF-Chem to ingest daily MODIS-derived  $u_{ns^*}$  fields (Eq. (11)) that had been interpolated to the model grid domain into the WRF-Chem framework through an auxiliary channel at model runtime and modified the dust emission equations to use  $u_{s^*}$  in place of  $u_*$ ."

**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.**

Response: Thank you for this insightful comment. After looking into this, we found that our lowest model level is already approximately 10 m above ground level, and differences between the lowest model level wind and 10 m wind fields are relatively negligible. Finally, we ran a brief test simulation that used the first model level winds instead of the 10 m winds and found no major changes in the dust simulation; therefore, we think that in this case, and in cases with a sufficiently high vertical resolution near the surface, the 10 m wind speeds are appropriate.

However, there are additional considerations we would like to address. Specifically, we caution against using the lowest model level winds in place of the 10 m diagnostic wind speed here for a few reasons:

- First and foremost, we're applying an established methodology from Ziegler et al. (2020) to explore if the albedo-based drag partition parameterization in its current form can improve AFWA module-simulated dust emission patterns. Critically, the Ziegler et al. (2020) approach, derives the albedo-based partition using the 10 m wind speed as an input, so it is possible that wind speeds associated with heights closer to the land surface may worsen outcomes.
- The empirical components of the  $u_{ns^*}$  equation (Eq. (11)) were initially derived relative to what Chappell and Webb (2016) referred to as the freestream wind speed flowing above engineered roughness elements in a wind tunnel environment. This freestream wind speed value may not have a direct physical equivalent to a real-world setting, but replacing it with wind speed values closer to the immediate ground surface would not make sense in this context.
- The core AFWA module equations are based on wind friction speed, not wind speed.

From the AFWA module perspective, changing the vertical model level heights will not directly affect the AFWA module calculations. Wind speed is only used in the conversion of  $u_{ns*}$  to  $u_{s*}$ .

- Our lowest model level being situated approximately 10 m above ground level is a coincidence. We used the WRF v4.1 default vertical atmospheric level distribution set by the WRF model's real executable (*real.exe*). Older or newer versions of WRF may not adhere to this standard. Furthermore, these particular vertical-level settings may not always be appropriate for all domain or case study forcing conditions. For example, the current WRF v4.1 hybrid vertical coordinate is not a consistent height above ground level. Instead, it is dependent on the vertical distribution of temperature and pressure, especially close to the ground. For events with dramatic changes in temperature and pressure, the effective height of your lowest model level can vary both in time and space, whereas the 10 m diagnostic wind ensures a common reference height. Setting a dependency on the vertical level configuration may make it challenging for others to apply the drag partition treatment in their respective WRF-Chem model configurations.

**Section 2.2. Just a small note for those unfamiliar with the area, the dust source area should be noted clearly.**

Response: Unfortunately, we cannot directly attribute dust emissions for this event to a specific source location because the satellite imagery was cloud-obscured. For this analysis, we can only speak to "dust sources" in terms of how the aerodynamic roughness length ( $z_0$ ) and vegetation masks are applied to their respective model configurations (e.g., Fig. 4). It's important to remember that a general lack of widespread dust entrainment in our simulated test cases configured with a drag partition treatment does not necessarily imply a lack of dust sources in the associated area. Rather, roughness elements may have suppressed dust generation by blocking or reducing momentum transfer from the atmosphere to the soil surface. We've added an additional "storm summary" figure to Sect. 2.2 to help readers conceptualize the general placement and forcing conditions associated with the main dust wall to help alleviate confusion regarding where dust entrainment likely occurred.

**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?**

Response: The atmospheric evolution of this event is fully explored in the report by Gallagher et al (2022) referenced at the beginning of Sect. 2.2. Note, the Gallagher et al. report goes into great detail on the synoptic, mesoscale, and local conditions associated with the entire lifecycle of our focus dust case study event using a broad collection of analysis fields, radar composites, and observations for support. The new conceptual storm summary figure mentioned above should help readers visualize the general forcing conditions associated with the storm event.

We've added the following sentences to the end of the first paragraph of Sect 2.2.: "Figure 1 provides a conceptual overview of the general environmental forcing conditions associated with the dust event. For a more in-depth review of the storm evolution, including synoptic, mesoscale, and local condition assessments using a broad collection of analysis fields, radar composites, and observations for support, we encourage readers to review the Gallagher et al. (2022) report." (Please note - the new Fig. 1 referenced here was not part of the original manuscript submission.)

**Figure 2. Mark the X spot more clearly. Add a circle maybe?**

Response: We've enhanced the Phoenix marker in Fig. 2 to make it stand out better.

**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.**

Response: We agree with the reviewer that extended model spin-up times are often necessary for spinning up background dust (and other aerosols). Indeed, the majority of the dust associated with our case study event was localized and produced by dust lofting along a convective outflow boundary. The Gallagher et al. (2022) study reviewed the model sensitivity to initialization time and found that extending the spin-up time to 24 hours (i.e., starting the simulation on 2 July 2014, 1200 UTC instead of 3 July 2014, 0000 UTC) caused the simulation to diverge from the observed pre-convection environment, degrading the overall simulation accuracy. The aforementioned update to Sect. 2.3 (model configuration) notes this model spin-up time assessment.

**Section 3. A more thorough statistical analysis is needed. There are no statistical indexes calculated. Also the text structure 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 is not enough.**

Response: Thank you for your comments. The results section begins with an overview of the  $u_{ns}$  field, reviews the environmental forcing conditions and dynamic components of the dust emission scheme, and ends with an assessment of the resultant dust-related parameters produced by each test configuration. It's not entirely clear which parts of the text's structure were confusing to the reviewer. However, we have attempted to improve the text. In particular, we have updated sentences introducing Figures 8-14. While Fig. 8-14 are all time series plots, Fig. 8-11 and Fig. 14 are spatial time series plots. We have added "time series of <parameter>" text lead-ins throughout Sect. 3-4 (including figure captions) to help clarify.

The Gallagher et al. (2022) companion assessment included a statistical analysis of surface wind speeds in the innermost model domain where the main dust event occurred. They found that the average wind speed bias for the entire forecast period was  $+0.59 \text{ m s}^{-1}$ . However, most of this overestimation occurred at night, outside the main convective period. We have updated the text from the first paragraph of Sect. 3.2 accordingly: "...The model was able to reproduce the storm's general structure and timing, including the formation of the initial quasi-linear convective system and the collapse of the convective line into individual cells. Furthermore, the simulated near-surface wind speeds were in good agreement with wind speeds observed at ASOS stations. However, simulated wind speeds peaked 1 to 2 hours early in some locations with slightly higher (about  $+1 \text{ m s}^{-1}$ ) intensity. According to Gallagher et al. (2022), these minor wind speed errors may be partly due to erroneous land use characterization, particularly in the higher terrain elevation areas where the storm initiated. Gallagher et al. also performed a full statistical analysis of simulated surface wind speeds against all available ASOS wind speed data from the innermost domain (D03). The average wind speed bias for the entire forecast period was  $+0.59 \text{ m s}^{-1}$ . However, a large portion of this overestimation occurred during non-convective nocturnal periods (3 July, 0500–1500 UTC and 4 July, 0800–1600 UTC)."

Statistical analyses of PM10 are less straightforward. As discussed in Sect. 3.3, the EPA PM<sub>10</sub> stations are not equally distributed across the domain. Instead, these stations are tightly clustered around population centers (e.g., the dense station network surrounding the Phoenix metropolitan area). As a result, any misalignment in storm position can

substantially affect the reliability of point-based  $PM_{10}$  station comparisons. This is especially important to consider for our case study given the slight position and timing offset of the storm (e.g., Fig. 7c). Hence, we chose to limit our  $PM_{10}$  assessment to a qualitative analysis of the maximum  $PM_{10}$  value simulated along the gust front.

Some studies (e.g., Hyde et al., 2018) compare hourly average  $PM_{10}$  observations against hourly average simulated  $PM_{10}$  values on the county level. While this may make sense for widespread dusty conditions, this approach may not work well for highly localized haboob conditions like our focus case study event. We tested this hypothesis for our case study using the combined Maricopa/Pinal county area (e.g., Fig. 12), keeping in mind that the main dust wall crossed directly over most of the  $PM_{10}$  stations surrounding Phoenix. This assessment approach made the CTRL configuration appear to perform better than the ALT3 and ALT4 configurations artificially because the two alternate configurations incorporated several grid cells with low  $PM_{10}$  values (correctly) in areas with no  $PM_{10}$  station coverage. If we attempt the same exercise with hourly maximum county values (which largely mirror our outflow boundary max  $PM_{10}$  assessment shown in Fig. 12) instead of hourly average county values, we still end up with deceptive results due to the minor position and timing offsets that affect when the simulated storm entered/exited the combined Maricopa/Pinal county area boundaries.

Accordingly, we maintain our position that point-based  $PM_{10}$  quantitative analyses for this case study event would be misleading.

**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.**

Response: Please see our previous comments and text adjustments about the parent WRF model configuration. We appreciate the reviewer’s comment (and recognize the importance of correctly simulating the environmental forcing conditions for simulated dust entrainment assessments). However, we also acknowledge that the mesoscale details of mesoscale convective system (MCS) evolution are a source of irreducible uncertainty within WRF. For example, operational mesoscale models like the High-Resolution Rapid Refresh (HRRR; e.g., Benjamin et al., 2016), often experience difficulties with the timing, location, and morphology of convective storms. So, while the large-scale forcing conditions and convective initiation were well captured by our simulation, the exact timing, shape, and location of the resulting MCS were subject to error. Of the two challenges, we consider it more important for the simulation to capture the structure and underlying dynamics of the storm(s) rather than the(ir) precise location, as the latter is easier to adjust and account for than a misrepresentation of the convection as a cluster of thunderstorms, supercell, or mesoscale convective system instead of the observed quasi-linear convective system. Additionally, there was an extensive amount of work put into determining our model configuration to limit and document errors in the predicted wind field in Gallagher et al. (2022).

**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**

Response: We appreciate the reviewer’s comment. For this preliminary analysis, the benefits of the methodology are manifest in the vast improvements we see with ALT3 and

ALT4 over the initial CTRL configuration. This paper aims to show the weakness of the existing approach, and in the context of a single storm, introduce the adapted module with a drag partition included. We agree that continued research is needed (which we highlight in both the discussion and conclusions). If future studies warrant, continued use of satellite-derived roughness information and its effects on dust emission in the AFWA module could markedly improve investigations of the role of short- and long-term changes in vegetation on dust emission patterns. This, in turn, could be of benefit to model users interested in drought hazard, climate change, land management, and post-wildfire condition modeling applications.

We added the following commentary to the end of the conclusion section.: "The benefits of using a drag partition methodology in the AFWA module are manifest in the vast improvements we see with ALT3 and ALT4 over the initial CTRL configuration. Follow on studies investigating the benefits of the approach over longer simulation periods are needed. However, we anticipate that satellite-derived roughness information and its effects on dust emission in the AFWA module could markedly improve investigations of the role of short- and long-term changes in vegetation on dust emission patterns. This, in turn, could be of benefit to model users interested in drought hazard, climate change, land management, land use/land cover change, and post-wildfire condition modeling applications."

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

Response: Thank you. We appreciate the support.

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