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Reply on RC2

Sandra L. LeGrand et al.

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-AC2, 2022

**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.

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.

Thank you for your review. We have made several of the suggested changes in the revised manuscript and believe it has strengthened and clarified the paper.

Major Comments

1) 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.

Response: Thank you for this comment. While we acknowledge that broad conclusions cannot be extrapolated from a single case study review (which we also state in our conclusion section), we have chosen not to include additional case studies for the following reasons:

- This paper is primarily meant to serve as an introduction to and discussion on the usage of the albedo-based drag partition in the WRF-Chem AFWA module, rather than a robust evaluation of the scheme. We fully agree with the reviewer that additional studies are necessary to achieve that goal and hope this paper will serve as the basis from which the broader modeling community can build on.
- We wanted to maintain a focus on a well-studied and well-simulated convective event that occurred in an area with a robust PM_{10} monitoring network.

We recognize that the Hyde et al. (2018) study concluded that the AFWA module tended to both over- and underestimate dust concentrations. However, we have some concerns about the Hyde et al. evaluation methodologies that lead us to suspect the underestimated cases may be an artifact of misleading data interpretations. Specifically, Hyde et al. compared 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, especially since the PM_{10} monitoring stations are not evenly distributed in this domain (e.g., the PM_{10} monitoring stations are primarily clustered around the Phoenix, Arizona metropolitan area). For example, the model may incorporate several grid cells with low PM_{10} values (correctly) in areas with no PM_{10} station coverage, causing the simulations to look artificially lower than the observed patterns. The reverse could also occur if the storm is offset from the sensor locations, making the simulated dust concentrations appear higher than they should be.

We also note that the Hyde et al. study primarily assessed the representativeness of their forcing conditions by comparing simulated rainfall accumulation patterns to radar data. While they supplemented this analysis with comparisons of maximum simulated and observed wind speed assessments, their general approach is not a sufficient means for properly evaluating the storm structure and morphology. This is especially important for convectively driven case studies, where storm morphology is equally important to the overall intensity of surface winds for effective dust event simulation. It's possible that some of the Hyde et al. case studies captured the rain patterns but not the outflow boundary conditions that actually drove the dust (e.g., intense convective storms with strong gust fronts versus poorly organized "squishy rain blobs"). Since the Hyde et al. paper only reviews model performance for one of their nine dust event case studies, it is difficult to tell if all of their dust simulations are reliable assessments of the AFWA module.

That said, since we knew the dust event associated with our chosen case study passed directly over the Phoenix area monitoring stations, we were more likely to accept the Hyde et al. conclusion that the AFWA module grossly overestimated dust conditions. Furthermore, the AFWA module has been available to the WRF-Chem user community for over a decade. Anecdotally, community members have shared with us that they tend not to use the AFWA model for this domain without making substantial tuning or input dataset adjustments because of its propensity to overestimate dust production in the area north of the Gulf of California (which we clearly see in our CTRL simulation).

2) 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.

Response: We appreciate the reviewer's comment and acknowledge the legacy of terminology confusion in dust literature. We have attempted to clarify terminology throughout the paper accordingly. Please note, we are a bit hesitant to explicitly state what the albedo-based drag partition scheme represents physically due to some of the issues we raise in our discussion section. Rather, we prefer to think of it as a parameterization for representing a component of the dust emission process that the default AFWA dust emission module wouldn't be able to characterize otherwise. The albedo scheme is not sophisticated enough to differentiate obstacles blocking mobilized sediments from reductions in wind shear stress. From our perspective, it offers an empirical relationship between shadowing and aerodynamic parameters, that, in theory, can be used to estimate drag partitioning conditions and their general effects on dust entrainment.

We added additional commentary to the Eq. (10) description to help clarify how the normalized albedo calculation links to area-integrated roughness conditions.

"… where $\omega_{dir}(0°)$ is the daily nadir "black-sky" albedo for MODIS band 1 (620-670 nm wavelength) and f_{iso} is the band 1 BRDF isotropic weighting parameter. In essence, Eq. (10) integrates the albedo across viewing angles for a single illumination angle (solar noon), producing an areal shadow estimate that, in theory, represents non-erodible roughness element conditions for an integrated (500 m pixel) area (e.g., Fig. 1 from Zeigler et al., 2020). We then determined daily u_{ns*} by:..."

3) 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.

Response: We fully agree with the reviewer that thoroughly assessing the simulated environmental forcing conditions driving the dust event is an essential step in the dust scheme evaluation process. The Gallagher et al. (2022) paper referenced in both the case study description (Sect. 2.2) and the model configuration overview (Sect. 2.3) was meant as a complement to this study. These authors conducted an in-depth review of our case study event evolution and performed extensive model configuration sensitivity studies to determine "best recommendations" for our parent WRF model initialization source and model configuration settings. Gallagher et al. also evaluated and documented errors in simulated forcing conditions for our case study event so this paper could focus on addressing the dynamic land surface and dust entrainment aspects of dust storm simulation rather than the atmospheric components.

We agree that our initial manuscript submission did not make this clear and have updated the text accordingly. For example:

The end of the first paragraph of Sect 2.2 (case study description): "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." (Note - this refers to a new Fig. 1 that wasn't in our initial submission.)

The first paragraph of Sect. 2.3 (model configuration): "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."

The first paragraph of Sect. 3.2 (environmental forcing simulation results): "…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 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 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)."

Please note that we do highlight issues with the simulated meteorology in our discussion on the rotation/position and timing of the gust front (e.g., Sect. 3.2; Paragraph 2 and Fig. 7). Due to these meteorological discrepancies, we adjusted our evaluation approach and limited our simulated dust assessment to a qualitative evaluation of the overall storm system rather than performing a more traditional point-based comparison against hourly PM_{10} observation records.

4) How much of the PM10 is from other aerosol species than dust in the model?

Response: We included the standard suite of aerosols covered by the GOCART "simple" module configuration option in WRF-Chem for completeness and to activate the model PM_{10} calculation functions. This setting incorporates dust, sea salt, black carbon, organic carbon, and dimethyl sulfide (DMS) into the background PM_{10} estimates. In this case, most PM_{10} comes from either dust or sea salt transport. Sea salt and DMS distributions are relatively isolated to areas over the ocean and the immediate coastlines. Dust is the primary source of PM_{10} over land. Contributions from black carbon, organic carbon, and DMS were negligible, except in dense urban areas along the southern California coastline. All inland PM_{10} estimates on the order of 300 μg m^{-3} or higher were primarily due to dust transport.

We added the following text to the beginning paragraph of Sect. 3.: "We compared our simulation results against observed PM_{10} concentrations (Fig. 11-13). For this case, most of the simulated PM_{10} came from either dust or sea salt transport. Sea salt and dimethyl sulfide distributions are relatively isolated to areas over the ocean and the immediate coastlines. Dust is the primary source of PM_{10} over land. Contributions from black carbon, organic carbon, and dimethyl sulfide to simulated PM_{10} totals were negligible, except in dense urban areas along the southern California coastline."

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.

Response: Thank you very much for pointing us toward this paper. We added the following sentence to the end of the final paragraph in our introduction section accordingly: "This theory is further strengthened by recent findings from Bukowski and van den Heever (2022), who found accurate roughness effect characterizations are critical for predicting dust patterns associated with cold pool events similar to our chosen case study."

Regarding challenges with model configurations mentioned by the reviewer, we agree that these are important elements to consider. However, we opted to focus our introduction on challenges specific to dust emission modeling that extend beyond issues common to atmospheric modeling in general.

[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.

Response: The WRF model estimates spatiotemporally varying values of u_* in the surface layer scheme that handles critical parameters for simulating the vertical behavior of mean airflow and turbulence properties within the lower bounds (approximately the lowest 10%) of the atmospheric boundary layer. For our experimental configuration, this occurs in the MYNN surface layer module (*module_sf_mynn.F*). The dynamic 2-dimensional u_* scalar value is not associated with a specific model level and gets derived through the scheme's similarity theory functions used to parameterize turbulent closure schemes for atmospheric conditions near the land surface. Our WRF configuration also calculates the U_{10m} diagnostic in the same MYNN surface layer module (lines 1221-1224) using the u_* parameter and a semi-empirical log-wind profile relationship for diagnosing wind speeds at different heights above ground level.

The reviewer is correct that the u_{s*} parameter is a function of the model 10 m wind speed diagnostic (and MODIS-derived fields) and not u_*; however, we note that the parent WRF model uses u_* to diagnose the 10 m wind speed diagnostic. As a result, U_{10m} and u_* should exhibit similarities in their general spatial patterns under stable atmospheric conditions.

Please see our response to the next comment for more information on how u_* relates to u_{s*}.

[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*?

Response: We focused on u_{s*} instead of u_{r*} since dust emits from the soil. The wind shear stress acting on the roughness elements (τ_r) and the immediate soil surface (τ_s) are related and must sum to the total wind shear stress (τ) acting on the land surface. Note that $\tau_r \square u_{r*}^2$, $\tau_s \square u_{s*}^2$, and $\tau \square u_*^2$. This conservation, therefore, means that the sheltering (shielding) term is already accounted for by u_{s*}.

We added a more thorough overview of the drag partitioning concept to the introduction section to help clarify this point and address similar topics brought up in later comments.

"Sediment mobilization schemes are often represented in terms of wind friction speed, u_*, a scalar parameter commonly used to describe processes related to wind shear stress (τ; note that $\tau = \rho(u_*)^2$, and ρ is air density). Near the land surface, u_* represents the total wind shear stress (τ) acting on both the horizontal soil surface (τ_s) and roughness elements (τ_r) (i.e., $\tau = \tau_s + \tau_r$; see Raupach, 1992, Raupach et al., 1993). This process is typically termed "drag partitioning" and is often expressed in terms of u_* rather than τ. Since τ is proportional to u_*, we can similarly divide u_* into soil surface (u_{s*}) and roughness (u_{r*}) components (i.e., $u_* = u_{s*} + u_{r*}$). The wind shear stress that reaches the immediate soil surface (i.e., τ_s) governs particle mobilization, so dust emission models driven by u_{s*} (or wind erosivity) instead of u_* may produce better outcomes (e.g., Darmenova et al., 2009; Okin, 2008; Webb et al., 2020)."

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

Response: While technically any landscape element (biocrusts included) contributing to atmospheric drag will, in turn, affect dust emission, we are unaware of any studies quantifying the biocrust contribution to the drag partition. In theory, the albedo-based drag partitioning scheme should pick up on any raised element casting a shadow. At present, we tend to focus more on larger roughness elements like trees, shrubs, grasses, topographic features, and man-made structures. However, we suspect that dust emission modeling uncertainties caused by poor representation of biocrust soil aggregate binding effects on sediment supply probably far outweigh soil crust-related drag partitioning simulation errors (e.g., Rodriguez-Caballero et al., 2022).

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

Response: See previous response. We have updated the part of the introduction (Sect. 1) where we introduce drag partitioning to help clarify this term.

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

Response: We fully acknowledge the need for and encourage the future study of the proposed parameter for a broader variety of dust events. Specifically, an underprediction case study would be a particularly interesting research case. With respect to the findings of Hyde et al. (2018), we refer back to our response with regard to the gaps and flaws in their methodology that lead to those conclusions. Specifically, the underlying atmospheric component is critical to resolve and evaluate correctly with respect to underestimating dust concentration. A simulated convective storm may reflect the maximum intensity and direction of observed wind speeds at the surface but have a smaller spatial footprint, completely different storm structure, or notable shift in geographic location that can suppress the resulting dust emission and transport. As such, our study refers frequently to the conclusions of Gallagher et al. (2022) to ensure the accuracy of the atmospheric forcing conditions and give us confidence that our results primarily reflect the nuances of the dust emission module. We highly encourage future and follow on studies to apply a similarly critical eye to the underlying atmospheric state.

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

Response: Please see our previous comments. We agree with the reviewer that accurately simulating the environmental forcing conditions is a critical step in the dust modeling process. We also agree that convectively driven case studies add an extra layer of complexity to the problem. However, we took several steps in our modeling setup phase to limit the potential for errors (e.g., Gallagher et al., 2022) and account for them in our dust modeling assessment when they occurred.

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

Response: We note that some communities and published sources have labeled the source strength parameter (*S*) used in the AFWA module as an "erodibility" map. However, we prefer to avoid that description. Others have called it a "dust source" map, which is also inaccurate. Ginoux et al. (2001) initially established the *S* parameter as a means for integrating large-scale areas of loose erodible sediment supply primarily associated with Holocene-era mega lakebeds into a relatively coarse-resolution global dust transport model. It was a creative way of incorporating sediment supply into a dust transport modeling framework using readily available parameters with global coverage in the absence of real data.

Adaptations of the Ginuoux et al. (2001) topographic dust source strength parameterization approach are used in several modeling frameworks (e.g., Barnum et al., 2004; Collins et al., 2011; LeGrand et al., 2019, Vukovic et al., 2014), which is not surprising given that it was one of the first globally portable modeling techniques established for characterizing dust sources, is relatively easy to implement, and is readily accessible as a pre-calculated field available for download from multiple sources. However, the term "erodibility map" has been applied to several terrain-based datasets over the years (e.g., Cremades et al., 2017; Ginoux et al., 2001; Grini et al., 2005; Jugder et al., 2018; Kimura, 2016; Parajuli et al., 2014; Zender et al., 2003). The phrase has become a sort of "catch-all" term for fields used to characterize some aspect of sediment supply or dust source strength. It's difficult to know which model or field the reviewer is referring to here.

We updated the *S* field description in Sect. 2.2.1 to the following: "The *S* parameter (originally described by Ginoux et al., 2001) represents the availability of loose erodible soil material at a given location based on the degree of topographic relief of the surrounding area. This approach assumes that soil composition remains consistent over time, and the simulated land surface will neither run out of dust material nor acquire new dust material through fluvial or atmospheric deposition as the simulation evolves. Some papers refer to this spatially varying *S* field as an erodibility or dust source map. However, both labels provide an inaccurate description of how *S* functions in the AFWA module. Accordingly, we will not use that terminology here. Essentially, *S* is a spatially varying tuning parameter ranging from 0 to 1 that assumes erodible material accumulates in low points in the terrain,..."

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

Response: Here, the aerodynamic roughness length (z_0) parameter refers to the length scale or height above ground level where the mean atmospheric flow, but not turbulent flow, is integrated to zero. The AFWA module uses a simple z_0 = 20 cm threshold to prevent dust emission from forested and urban areas (e.g., LeGrand et al., 2019). In this capacity, the z0 mask has the potential to create a "double counting" problem for vegetation once the new drag partition is added, which is why we remove it as part of our experimental design.

The ALT1 and ALT2 model configurations are nearly identical, with the only difference being that we removed the mask for z_0 > 20 cm from the ALT2 bulk vertical dust emission flux equation (Eq. (6); see Table 4). In this specific case study, however, removing the z_0 mask had little effect on the simulated dust patterns due to the overlap in the areas masked by z_0 and the vegetation mask built into the *S* parameter field (e.g., Fig. 4).

We've added the following text to the Eq. (6) description: "Here, z_0 represents the theoretical height at which the mean wind speed near the surface falls to zero due to surface-induced drag (e.g. Stull, 1988; Zobeck et al., 2003). Importantly, this z_0 parameter is not part of the new drag partition treatment, but rather a value from the parent WRF model used for a variety of land surface and air flow processes and diagnostics."

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

Response: Thank you for your comment. While the formulation of the *S* parameter has been covered by several other published sources, there are also several published sources containing misinformation on how the *S* term in the AFWA module is calculated and what it represents. Due to the poor documentation heritage associated with the AFWA module and the *S* term in general, we strongly feel that GMD readers will benefit from a comprehensive overview of how the S parameter constructs, especially because we remove this particular field as part of our experiment.

We've updated the section in an attempt to clarify the content from our perspective. In response, we've edited for clarity where we perceived areas for improvement.: "Of note, the AFWA module uses interpolated values of *S* initially derived from a 1/4° elevation dataset. In addition, the *S* field incorporates a vegetation mask that blocks dust emission (i.e., *S* = 0) from vegetated areas derived from a 1° ☐ 1° resolution land cover dataset. While these settings may be appropriate for some modeling applications, the coarse nature of these input datasets likely limits the spatial viability of *S* at mesoscale and convective-permitting model resolutions (e.g., Saleeby et al., 2019; Vukovic et al., 2014; Walker et al., 2009)."

[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.

Response: Thank you for the suggestion. We've added these references to the text.

[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.

Response: From this comment, we understand that the reviewer is asking about different parameters by which something is normalized. Indeed, the normalization in u_{ns*} is different from the normalization in ω_n. We use the terminology and symbology for u_{ns*} adopted from previously published resources. Further, we think that the equations are sufficiently clear regarding the use of u_{ns*} in the model and what factors go into the normalization process.

[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.

Response: Per LeGrand et al. (2019), *C* is set to 1.0 in the AFWA module based on recommendations from Darmenova et al. (2009), Laurent et al. (2006), and Marticorena et al. (1997). We did not attempt to refit or tune the empirical constants associated with Eq. (13) or any other equation in the model for this study. Modifying tuning parameters in a meaningful way with a single case study event would be difficult at best. While tuning can be useful, it is generally best done after reviewing simulation outcomes for several events and extended periods. Even with tunings applied, the "best parameter configuration" may only be relative to a specific model configuration, domain setting, application, or region. Additional research would be necessary to discern if additional tuning adjustments are needed to fully optimize the dust model performance with a drag partition included.

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

Response: Excess wind friction speed is the wind shear stress that governs mass flux. We have rephrased the text to help clarify.: "This replacement of u_* with u_{s*} in Eq. (13) makes the saltation equation consistent with the physics of aeolian transport in the presence of roughness, where excess wind friction speed at the soil surface (i.e., friction speed above the threshold required for particle mobilization) governs the saltation mass flux (Webb et al., 2020)."

[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?

Response: Thank you for raising this issue. We did not encounter any issues with missing data in our case study, but we can see the potential for missing data problems to occur. The reviewer is correct in that the equations would interpret missing data as $u_{s*} = 0$. Rather than defaulting to the control parameters, we suggest users fill the data gaps by interpolating nearby points (assuming the missing data gaps are minimal), using climatological u_{ns*} values (something we've already suggested exploring in our discussion section to save on processing time), or using an input dataset from a previous time period.

We've added the following text to the end of Sect. 2.1.2: "We note the potential for missing data problems if the MODIS retrievals have poor coverage. If this scenario occurs, we recommend users consider filling the gaps through interpolation techniques (assuming the missing data gaps are minimal), using seasonal or monthly climatological u_{ns*} values, or using a u_{ns*} input dataset from a previous period."

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

Response: All relevant configuration settings are listed in Table 3. A copy of our entire configuration file (i.e., *namelist.input* file) is also available (see Michaels et al., 2022; Appendix B). Our study uses the Georgia Institute of Technology–Goddard Global Ozone Chemistry Aerosol Radiation and Transport (GOCART; Chin et al., 2000; Ginoux et al., 2001) "simple" modules as they are implemented in WRF-Chem v4.1. Once lofted, dust particles become a relatively passive "tracer" unless WRF-Chem aerosol feedback settings are activated (which we did not incorporate in our study to maintain consistency in forcing conditions across the simulations for all five test configurations). Vertical transport and time spent in suspension are primarily governed by the balance of simulated updrafts (including convective processes), atmospheric mixing, and dust deposition rates. Dust deposition in GOCART is mainly driven by gravitational settling and dry deposition, though GOCART will often remove dust from the atmosphere under rainy conditions even without the use of indirect feedback code adaptations (e.g., aerosol effects on modeled cloud microphysics and precipitation). While others have explored the use of more sophisticated wet deposition treatments with WRF-Chem/GOCART/AFWA (e.g., Tsarpolis et al., 2018), these modifications are not part of the standard WRF-Chem v4.1 baseline code distributed by NCAR.

We note that elevated atmospheric dust concentrations associated with the main dust event generally align with the location of the outflow boundary in all simulations. PM_{10} concentrations immediately under the rain-producing convective cells are an order of magnitude or more lower (e.g., Fig. 11). While additional studies are necessary to fully resolve the issue, we suspect that the model improvements made through drag partition incorporation far outweigh simulation errors introduced by the general lack of wet deposition treatments, at least for this case.

[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?

Response: Thank you for addressing this. Vertical resolution in the boundary layer, especially for convective events, is an essential component. While 40 vertical levels may initially sound coarse, our distribution of model levels ensured that each level was spaced no further than 1 km apart throughout the entirety of the atmosphere and placed 10 levels within 1 km above ground level. This was the default configuration of the WRF model v4.1 when we first performed our study and adhered to the suggested community guidelines. This compressed distribution of model levels closer to the surface allows our simulations to benefit from the improved vertical resolution where it is most essential for adequately resolving the temporally and spatially smaller intense vertical motions that contribute to the development of convective storms, while also reducing computational burden in the upper atmosphere where vertical motions are weaker and larger in temporal and spatial scales. However, we acknowledge convective forecasts are sensitive to changes in vertical resolution. As such, Gallagher et al. (2022) investigated the effects of increasing the number of model levels to 65 for our case study event and found the improvement in forecast skill to be negligible, while the computational cost was notable.

[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.

Response: How to best tune C_s or any of the other tuning constants in the AFWA module is an interesting question but is outside the scope of this study. While not discussed in this paper, the AFWA module does incorporate an optional "classic C" global tuning constant for the bulk vertical dust equation that users can set at run time (referred to as c_α in LeGrand et al., 2019). We note that tuning one versus the other will likely result in different patterns since the C_s scaling parameter changes a thresholded cubic relationship, while c_α is a simple linear scaling approach. The extent to which those tuning approaches are meaningful is beyond the scope of this study.

[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?

Response: Thank you for bringing this to our attention. This is a valid criticism given our evaluation of the atmospheric forcing conditions did not directly involve comparison with precipitation measurements. Our atmospheric validation component, Gallagher et al. (2022), opted not to focus on precipitation verification due to the relatively low number and tight clustering of in-situ precipitation observations. We focused more on overall storm morphology than resulting precipitation, ensuring that convective phenomena and lofting winds were well represented. However, we wish to point out that the predominant phenomena driving dust emission and transport, in this case, was the gust front ahead of the quasi-linear convective system. This surface boundary remained just ahead of the radar signatures for the event duration until it started to collapse and dissipate towards the end, approximately 4 July 2014, 0800 UTC. Also, with regards to the reflectivity spatial coverage, we recognize that direct comparisons of spatial extent come with the caveat that the observed radar is a national composite of multiple radar returns, stitched together from various heights above the surface, while our simulated reflectivity is consistently at 1 km above ground level and may occasionally represent different "slices" through the storm. Lastly, the overabundance of spurious dust emission in the CTRL simulation emanated from the area north of the Gulf of California. This particular area was well west of the main storm event and largely cloud/precipitation free in both the simulation and observation data.

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

Response: We reworded this sentence to clarify: "We note that the simulated u_* values are generally an order of magnitude stronger than their associated u_{s*} partition. This outcome is likely due to the albedo scheme interpreting the relatively "dark" terrain surface of our domain caused by the prevalence of shrubs, grasslands, forests, and built-up areas."

[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.

Response: Thank you for the comment. We restructured this block of text to help clarify. Also, please note, this part of our assessment focuses on how air density and soil moisture moderate the wind friction speed threshold required for sediment mobilization ($u_{*\mathrm{ts}}$), not the overall dust emission flux. This aspect of the model behavior is important because there are simulated $u_{*\mathrm{ts}}$ values in the forest-covered mountain areas, which should not be generating dust, that are well below the simulated u_* values. Once we remove the z_0 and vegetation masking elements of the original model configuration (i.e., once we shift to ALT3 and ALT4 configuration settings), the only component of the dust module preventing dust emissions from these areas is the drag partition treatment.

"Air density is the only spatiotemporally varying parameter in the calculation of $u_{*\mathrm{ts}}(D_{\mathrm{s,p}})$. Though we can discern a slight reduction in $u_{*\mathrm{ts}}(D_{\mathrm{s,p}} = 69\ \mu\mathrm{m})$ over time immediately under the convective line (Fig. 8m-p), the overall effect of air density on $u_{*\mathrm{ts}}(D_{\mathrm{s,p}})$ for this case is relatively negligible. Under air-dry soil conditions, $u_{*\mathrm{ts}}(D_{\mathrm{s,p}} = 69\ \mu\mathrm{m})$ ranges between 0.17 to 0.19 m s^{-1} across most of the domain. These results also align with findings by Darmenova et al. (2009) in their assessment of the sensitivity of the Marticorena and Bergametti (1995) dust emission scheme to uncertainties in its required input parameters.

We also see relatively little change in the $f(\theta)$ field during the dust event, except for the area associated with a line of precipitation that occurred within the convective cell behind the main wall of dust (Fig. 8q-t). The $u_{*ts}(D_{s,p}$ = 69 µm, θ) maxima over the Mogollon Rim aligns with isolated areas of convective precipitation that occurred earlier in the simulation (Fig. 8u-x). Note, however, that the $u_{*ts}(D_{s,p}$ = 69 µm, θ) values along the Mogollon Rim adjacent to this maxima are around 0.2 to 0.3 m s^{-1}, comparable to the southwest Arizona region where the dust event occurred, and, for the most part, are well below the simulated values of u_*. This particular aspect of the model behavior is important because the only component of the dust module preventing dust emissions from these areas in the ALT3 and ALT4 simulations is the drag partition treatment."

[470-471] – Terms.

Response: We rephrased these sentences; however, it was not entirely clear to us which terms the reviewer had concerns over. "However, modeled 10 m wind speeds will, in most cases, be reduced over forested areas due to the aerodynamic drag from the trees on simulated wind speeds (note, these particular aerodynamic roughness effects are governed by the z_0 settings in the parent WRF model, not the drag partition treatment). Even if the u_{ns*} values were high for a model grid cell with forest cover, the simulated winds would likely be reduced by the internal model physics, potentially mitigating (or masking) a shortfall in the drag partition correction."

[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.

Response: Thank you for the suggestion. We added units where appropriate in terms of [L]=Length, [T]=Time, and [M]=Mass. Due to the size of the table, we believe the second column provides enough information to aid the reader in keeping track of the large number of parameters in this paper. The table is meant to serve as a "quick reference" guide for readers. Unfortunately, providing a glossary of in-depth descriptions would defeat this purpose and make the table prohibitively long.

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

Response: Thank you for this suggestion. We have updated the figures accordingly.

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

Response: Please note, the sandblasting field is constant across most of the domain. We include this figure to bring awareness to the general lack of influence this parameter has on the bulk dust emission flux beyond serving as a scaling factor (which we highlight in the text).

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

Response: Thank you for the suggestion. We've updated the legend on Fig. 4a to help clarify.

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

Response: Thank you for the comment. We debated quite a bit about how to set the color bar gradients. The primary concept we want this figure to communicate is that the drag partition has more influence on the model results than any temporal change. If we were to change the color bars, that visual cue would be much less apparent.

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

Response: Thank you for the comment. We understand the concern; however, we chose this color bar based on accepted guidance for colorblind-friendly palettes. The high and low gradient is easy to discern in the continuous spatial plots but can be a bit confusing in extreme edges of the domain for the station-based plot. Given that the macro patterns are discernable in areas relative to the discussion the figure was intended to support, we opted to keep the figure shading since the EPA station data are also publicly available for direct review.

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