

We thank both referees for their careful reading of what was indeed too long a paper, and for their many constructive suggestions. We have adopted most of the referees' suggestions and believe the revised papers are substantially improved. Note that we already addressed the comments of referee #2 on the submitted version of the paper prior to publication of this paper on ACPD. For completion, we have included the response to these comments, which we previously provided before the paper's publication on ACPD.

As suggested by both reviewers and the editor, we have split the paper into two. That is, the first paper describes the derivation of the dust emission parameterization and its comparison against the dust flux compilation and the previous GP88 and MB95 parameterizations. The second paper describes the implementation of the parameterization into CESM, the comparison of the resulting simulations against measurements, and assesses whether the new scheme reduces the need to use a source function in dust cycle simulations. In consultation with the editor, we have included both papers under the present submission. We believe this is both more logical and more efficient than submitting the second article separately, because many of the additions in the second paper (summarized below) were included in direct response to referee comments.

Because we made substantial revisions, we include below a list of the main changes, after which we provide a detailed response to the reviewer comments (in blue font).

- We have corrected the inconsistency in the treatment of MB95 identified by referee #1. We have also used the cross correlation technique to make the data comparison statistically independent from the data sets used to calibrate the model's dimensionless coefficient. This has simplified the data analysis and provided better support for our conclusions. We have also added a "notation" section to improve the paper's readability.
- We have expanded the modeling component in the second paper in five main ways. First, we have expanded the comparison against AERONET AOD by also including an analysis of variability on seasonal and daily timescales. Second, we have added comparisons against limited satellite measurements, and against surface concentration and dust deposition measurements. Third, we have included a statistical test (the *bootstrap*) to assess whether model improvements are statistically significant. Fourth, we present new results on the global map of the dust emission coefficient and its seasonal variation (Figs. 2 and S3). And finally, we now discuss the effect of the aerodynamic roughness length in several places in the Results and the Discussion sections. These last two additions are in direct response to comments by referee #1.

In order for the referees to more readily identify our changes, we've included versions of the paper in which the sections that have been substantially revised are marked in red.

Anonymous Referee #1 comments

The paper first presents in detail a new physically-based dust emission scheme that is fitted and compared with a compilation of vertical dust flux measurements from different campaigns. The new scheme is then implemented in the CESM climate model and evaluated with AERONET Sun-photometers. The authors conclude that 1) the new scheme better reproduces the vertical flux measurements than existing parameterizations, 2) its use in a climate model produces better agreement against AOD than simulations using a source erodibility function, and 3) the need for

a source function is partially eliminated by the additional physics accounted by the new scheme. It is also suggested that models using source functions may have underestimated the climate sensitivity of the global dust cycle.

General comments:

I acknowledge the authors' profound understanding of the physics of dust emission and their effort to provide an alternative physically-based dust emission scheme potentially usable in global models. The paper is excellently written and represents an original contribution that is suitable for ACP. I do not see the paper as just a model development and evaluation. The physical insights provided by the authors justify, in my opinion, its publication in ACP (instead of GMD for example). I would say that more than replacing the concept of source erodibility function, they are providing a method by which the source function is dynamic and dependent on the threshold friction velocity of the soil. This represents an advance and opens the door for future improvements of the dust cycle in climate models particularly when simulating other climates.

[We thank the reviewer for his/her positive comments here.](#)

Nevertheless, I have several concerns about aspects of the paper in terms of format, content, and conclusions. I recommend publication if these concerns are properly addressed. I believe that the paper needs major revisions.

[We thank the reviewer for his/her careful reading of our manuscript, and for the constructive and helpful comments that helped us to further improve the paper. We've implemented most of his/her suggestions and have included detailed responses below.](#)

Format:

I agree with the first referee that, although the paper is excellently written, it is very long and cumbersome (main text and the supplementary material) (This is why I rated the presentation quality as good although the paper is very well written). The supplementary material is 30 pages including a lot of text. The paper may be separated into 2 parts (I leave this decision to the editor). The first part could be the model description and the comparison with the dust flux measurements. Some of the tables, figures and text in the supplementary material could then fit into the main text of the first part. The second part could include the model simulations and their evaluation.

[Following the comments by both reviewers we have now indeed split the paper into two along the lines the reviewer suggests here. We have also extended and improved the second paper, guided by the reviewer's further comments on this part, which we address below.](#)

This second part is, in my opinion, a bit weak and should be extended and more detailed given the strong conclusions reached by the authors. I believe that further insight on the spatial and temporal distribution of their dimensionless dust emission coefficient should be provided for example (this is what I referred to as dynamic source function).

We are grateful for the referee's insight that the dust emission coefficient can be interpreted as a dynamic source function, and now compare and discuss the spatial distribution of the dust emission coefficient against the Zender and Ginoux source functions in Fig. 2 and L451-68. We also show the seasonal cycle of the dust emission coefficient in Fig. S3.

Content and Conclusions: I have some questions and concerns on several aspects of the paper.

1) In my opinion some of the conclusions of the paper are overstated based on the results shown. It is true that the results show that CESM represents better the dust cycle compared to AOD measurements than previous schemes as implemented in CESM. However this cannot be generalized.

This is another good point. We have balanced the conclusions to reflect this, and are now careful to point out that our parameterization improves the CESM dust module specifically.

The implementation of the other schemes used in the comparison is specific to CESM and the simplifications assumed are not necessarily fair or convenient for the other schemes. For example the scheme of Marticorena and Bergametti 1995 (MB95) is oversimplified as it is used here. It does not include the drag partition component, which is critical within the scheme's paradigm as shown by a number of studies. The lack of global databases cannot be a justification in this case as there are several global databases on the aerodynamic roughness length available. Also the smooth roughness length can be estimated from the FAO soil texture database that is used by the new scheme.

The reviewer is correct here that we did not use the MB95 parameterization in CESM in its entirety, and so cannot state that we compare the simulations to the case with this parameterization implemented. We consider this a miscommunication on our part: the default dust emission parameterization in CESM is the DEAD module of Zender et al. (2003), with some modifications by Mahowald et al. (2006). The DEAD module is largely based on, but is not identical to, MB95. Notably, it indeed does not account for spatial variations in the aerodynamic roughness length. We now clarify this in the (second) paper, and refer to the original parameterization as Z03 (after Zender et al., 2003), rather than MB95. It is beyond the scope of the present papers to implement an aerodynamic roughness length parameterization into CESM, although we agree that this has great potential to improve CESM's dust module.

The use in many models of a source function (interpreted as an erodibility function) partially accounted for the lack of realistic roughness length estimates in arid regions in the past. This is one of the reasons why I think that some of the discussions and part of the conclusion when comparing to other previous schemes are not well balanced. These aspects should be properly discussed and the conclusions balanced accordingly. I rated the scientific quality as fair but I believe that with a more balanced discussion the scientific quality would be excellent.

The reviewer makes a good point that, historically, the inability to use realistic roughness lengths probably created another reason to use preferential sources functions. However, from our search of the literature, none of the key papers introducing and using source functions mention this

(Ginoux et al., 2001; Tegen et al., 2002; Zender et al., 2003; Grini et al., 2005; Koven and Fung, 2008). In fact, the only paper we found that links the need for a source function to a lack of roughness length maps is the recent Menut et al. (2013) (p. 6506), which we now cite (we would be grateful for references if we overlooked any relevant papers). Nonetheless, we agree with the reviewer that, even though it was seemingly not specifically recognized at the time, early modeling studies probably benefited from a source function in part because they were unable to account for spatial variations of the aerodynamic roughness length. We now discuss this point on lines L716-23.

2) In relation to the previous point, I am confused about the use of friction velocity (eq. 2) and soil friction velocity (eq. 3). I understand the use of soil friction velocity in the new scheme but I do not understand how both the friction velocity and the soil friction velocity are calculated and/or used when comparing the different schemes (both when comparing the dust fluxes and within the climate model). If I understand correctly the new scheme uses the soil friction velocity but MB95 and GP88 use friction velocity. If I am correct this has several implications. First, the notation in equations 26 and 27 may be wrong as they refer to soil friction velocity when in reality it should be friction velocity. The same happens with the threshold friction velocity and the soil threshold friction velocity. In MB95 and GP88 it should be threshold friction velocity and not soil threshold friction velocity.

We are grateful to the reviewer for noticing this inconsistency! Indeed, MB95 is formulated in terms of the friction velocity, and we have corrected this in the text (Eq. 25) and also in our comparison against dust flux data. GP88 is based on a personal communication with P. R. Owen (of Owen (1964) fame), so GP88 does not provide a derivation of their parameterization, and also do not carefully define their “friction velocity”. However, GP88 converts wind speed measurements taken over an airport with approximate roughness length of 1 cm to the u^* over an “eroding field” with roughness length of 20 μm (p. 14,234 in GP88). Since a bare soil with a characteristic particle size of 300 μm has a roughness length of $\sim 20 \mu\text{m}$ (using $z_0 = D/15$; e.g., Sherman et al., 1992), GP88 is formulated in terms of the soil friction velocity u^* as defined in our paper.

Second, this may have important effects on your results. As I outlined previously, MB95 should include the drag partition scheme that accounts for the partition of the stress exerted on non-erodible roughness elements and on the bare soil. In your scheme this is implicit through the use of the soil friction velocity. However, this is treated differently in MB95. It is not surprising to observe the results of the model with MB95 in Figure 6a. The scheme features low emission fluxes in sandy regions where the clay fraction is low as the dust fluxes mostly scale with the clay fraction if the drag partition scheme is not used.

For consistency, we now also use the drag partition component of MB95 for this scheme’s comparison against the dust flux compilation. Furthermore, in the second paper, we now clarify that CESM uses the DEAD module (Z03), which does not include a drag partition parameterization, and no longer refer to this as MB95 (see our comment on this above).

This raises some questions: - how did you calculate the soil friction velocity used by your scheme within the CESM model (how do you calculate eq 3 in the model)? – Did you use the

soil friction velocity when applying MB95 and GP88? That is what I can assume if I follow your equations 26 and 27, unless it is just a typo. - What are the implications for the model simulations and for the comparison with the dust flux measurements?

In particular, concerning the comparison with the dust flux measurements how did you treat MB95 (Figure S2 shows the vertical dust fluxes as a function of the soil friction velocity)?

Since the DEAD module does not include a drag partition scheme, the friction velocity u_* and the soil friction velocity u_*' are identical in CESM. Thus, our simulations are unaffected by correcting the inconsistency in Eq. 27 of the previous paper. Of course, the comparison with the dust flux measurements is affected, as described above, and has been corrected (see Section 3.5). We also discuss now in the caption of Fig. S2 that we convert u_*' to u_* to plot the predictions of all three schemes on one plot.

In any case, whether there a typo and or problem or not, I ask the authors to clarify these aspects. I would like to see a discussion on the use of a drag partition scheme compared to the new formulation. In particular it would be very helpful that the authors explain the calculation of the soil friction velocity in the model. This would not only be helpful to strengthen the paper but also would help potential users of the scheme in the future.

We have included in Section 2.2 of the first paper a discussion of the use of the friction velocity u_*' in MB95 versus the soil friction velocity u_* in our parameterization. Furthermore, we now note below Eq. (6) in the second paper that CESM does not use a drag partition, such that $u_*' = u_*$ in this model.

Detailed comments:

- Abstract: Please reformulate the abstract balancing the conclusions. I believe it is not completely fair to state that the new scheme better reproduces the measurements than existing parameterizations. 1) Not all existing parameterizations have been tested, 2) the new scheme was fitted with the same data that was used for the evaluation, and 3) the other parameterizations tested contained potentially harmful simplifications.

This is a good point, as we were not able to do a completely fair comparison in the original paper. To address this comment, we have improved our data analysis by using the “cross-correlation” technique, such that the model prediction for each data set no longer uses those particular data to ‘train’ the model (i.e., that particular data set is not used to determine the dimensionless coefficients in Eq. (18) when comparing the parameterization against that particular data set in Fig. 5). Furthermore, we balanced the abstract by noting that the new parameterization reproduces the measurements compilation with less error than the existing parameterization *that we were able to test against*. Finally, we specify in section 3.5 that MB95 had to be simplified in a common manner to be able to be compared against the measurements compilation. We also now note that this simplification is actually more consistent with measurements than the original MB95 formulation.

- Introduction and other parts of the paper: It is important to distinguish between the parameterization proposed by MB95 in their paper and the simplification of MB95 that is used in your model. The drag partition aspect should be clearly outlined.

We have clarified below the introduction of the MB95 dust emission equation (L547-9) that this paper also includes a drag partitioning component to calculate the threshold in the presence of roughness elements. For consistency, we now use this drag partitioning method (described in the supplement) for the comparison of MB95 against our compilation of dust flux measurements.

Section 2.3: - Equation 19a should include the expression $u^* > u^*_t$ to be formally correct. Please specify again that u^* refers to soil friction velocity.

Done.

- Also note that your parameterization is very dependent on the clay fraction of the soil through f_{clay} and (mostly) through u^*_t (which depends on f_{clay} in your model). Since the dimensionless dust emission coefficient C_d decreases with increasing u^*_t , the areas with less clay fraction will show strong dust fluxes since the threshold depends on the humidity of the soil (which depends on the clay fraction through the Fecan formulation). So the smaller the clay fraction, the larger the dust flux (This is clearly seen in Figure 6d where the hot spots in North Africa appear to be in regions with low clay fraction).

This is an interesting comment, and we have now included the FAO clay fraction map used in the model as Fig. S1 to provide additional insight into how the clay fraction affects dust fluxes in our scheme. The reviewer rightly observes that the clay fraction in our model has two opposing effects: on the one hand, it scales the dust emission flux through f_{clay} (see Eq. 18 in the first paper) and on the other hand the clay fraction scales the threshold moisture content above which the threshold friction velocity increases (Eq. 4). Consequently, the effect of the clay fraction on dust emission fluxes is not straightforward, and the map of dust emission fluxes for the K14 simulation (Fig. S2d) does not show an obvious correlation with the map of the clay fraction. However, because the strong exponential dependence of the dust flux on the clay fraction in Z03 is replaced by a linear dependence in K14, areas with low clay content, such as sand dunes, produce relatively more dust in Simulation IV than in Simulation I (compare Fig. 3d and Fig. S1), as the reviewer rightly observed. Note, however, that Fig. 3d (formerly Fig. 6d) plots the relative change with Simulation I, such that the bright red areas correspond to regions where dust fluxes have been substantially increased (for instance regions with low clay fraction), but these areas are thus not necessarily dust hot spots. We discuss the above points on L486-506.

I would like to see an average map of your “dynamic” soil erodibility and I believe that in North Africa it will correspond to Sand and Dunes (Sand and dunes in the FAO contain low amounts of clay and large amounts of sand). Please give some insight on this issue. In my opinion your model is giving good results. Satellite data shows that hot spots in Africa coincide with sandy regions. However I expected that to be the case because of enhanced saltation and low roughness length. But that it is just a belief. Could you please develop on this? It would be helpful that you include this discussion.

Per the reviewer suggestion, we have now included a map of the average dust emission coefficient C_d as Figure 2c. C_d anti-correlates strongly with soil moisture (Fig. S4), and does not show an obvious correlation with the presence of sand dunes. However, since CESM not use an aerodynamic roughness map, it might very well be that a model that does account for the spatial variability of the aerodynamic roughness shows higher fluxes in sand dune regions. We discuss this point on L486-506.

Section 3.5 -Equations 26 and 27: Is there a typo? I believe MB95 refers to friction velocity, not to soil friction velocity. Also please add $U'^* > U'^*_t$. What are the implications? How this affects the comparison with dust flux measurements?

See response to similar comments above. We've added $u_*' > u_*'_t$.

-Equation 27: I note that the formulation is wrong. Please read <http://dust.ess.uci.edu/facts/aer/aer.pdf> from Charlie Zender (page 33-34 and equations 3.67 and 3.93). It doesn't seem to have very important implications but I still believe that you should use the correct formulation. We should stop propagating errors in our papers. What are the implications for your results?

We thank the reviewer for his/her vigilance on this point, as the error in White (1979) has indeed been propagating through the literature for decades now. We have taken care not to propagate this ourselves and Eq. (25) is indeed the correct formulation noted in Sherman and Namikas (1997), Charlie Zender's monograph, and Kok et al. (2012) (note that

$$u_*^3 \left(1 - \frac{u_{*t}^2}{u_*^2} \right) \left(1 + \frac{u_{*t}}{u_*} \right) = u_*^3 \left(1 - \frac{u_{*t}}{u_*} \right) \left(1 + \frac{u_{*t}}{u_*} \right)^2.$$

- The same datasets used to fit key parameters of the new scheme are used to evaluate the model in comparison with other dust emission schemes. Why in figure 5 you only show the comparison with the measurements used for the fitting and not the other measurements? That would probably be the fairest comparison since any of the schemes would have been fitted with the data.

We are not quite sure what the reviewer means here as the comparison shown in Figure 5 includes all the data sets in our vertical dust flux compilation that meet our quality control criteria. Note that that the data sets of Fratini et al. (2007) and Park et al. (2011) did not meet the horizontal homogeneity criteria (see Section 3.1) and are thus not plotted in Fig. 5, but we did use these datasets to test and calibrate our relation for the dust emission exponent (Fig. 4a).

Note that we have improved the data analysis section through the use of the "cross-correlation" technique so that the model's prediction for a particular data set is no longer calibrated in part by that same data set (see more detail on this point above).

Section 4.1.1 This section needs more details: - What is the soil threshold friction velocity used (I refer to the dry)? - How the model calculates the soil friction velocity and the friction velocity? - Does MB95 use the soil friction velocity too? How is this justified? - Why not using a drag partition scheme?

The formerly Section 4.1.1 is now Section 2.1.1 in the second paper. We now mention how the dry threshold is parameterized, that $u_*' = u_*$ in CESM, that MB95 uses the friction velocity, and that CESM does not use a drag partition scheme.

Page 6393, line 17: How does Ginoux et al 2001 account for the effects of vegetation?
It is a fixed mask based on topography without seasonal variation. Why not using a seasonally dependent fbare?

They used AVHRR data to identify bare soil and included only those regions in their source function; see p. 20,257 of Ginoux et al. (2001). It is indeed a fixed mask without seasonal variation.

Figure 5 caption: Include the names in the caption.

To avoid making the caption unnecessarily long, we now mention that “Data set names are as defined in Section 3.1.” We’ve added similar statements to the captions of Tables 1 and 2, and to Fig. 4.

Supplementary material: Please include the acronyms when presenting the database in the beginning to better link with the main text. There are some inconsistencies between the acronyms used in the supplementary material and the main text (for example ZP vs ZV)

Thank you for catching these inconsistencies – we’ve corrected them. We also now note the data set abbreviations in the title of each relevant section in the supplementary material.

Figure S2: What is the colour for each scheme? Please add it to the caption. Also, please include a supplementary figure without tuning. Why not all the comparisons appear in Figure 5? Please include them.

We’ve added the definition of each line, and included plots without tuning as Fig. S3. Our dust flux compilation consists of 11 data sets, seven of which are included in Figure 5. The reason for the omission of four data sets (FC07 and PP11) is that these data sets do not meet the “homogeneous terrain” quality control criteria that is needed to predict the dust emission coefficient Cd. We discuss this at the end of the 1st paragraph of Section 3.1.

As a final comment: Please check all your equations again. I was not able to capture any other typo in your equations but it usually happens and unfortunately errors are propagated in the dust modeling community and models give good results for the wrong reasons!

We much appreciate the reviewer’s vigilance as small errors, sometimes with big implications, creep in easily. We have carefully checked our equations several times and are as confident as we can be that they do not contain any errors.

Anonymous Referee #2 comments

The above referenced manuscript is well written, generally well prepared, and I can find no corrections needed with it regarding language or grammar or typographical errors. However, it is overly long, and almost all parts of it can be compressed and shortened by eliminating extraneous information and writing more concisely. I still give Reviewer Question 3, “Presentation Quality”, an “Excellent” rating.

Thank you for the positive rating. We realize that our manuscript was relatively long, and have split the paper into two per the referee’s suggestion. We would certainly be grateful for any suggestions to improve the writing further and to shorten the manuscripts.

The manuscript as submitted really comprises two parts: (A) which is comprised of parts 1 through 3 of the manuscript, regarding the derivation, justification, and sensitivity testing of a new dust source/emission parametrization scheme for modelling, with assessment of the new scheme’s performance using a quality-controlled compilation of dust flux measurements: and (B) comprising section 4-xx of the manuscript, the usage of this new dust emission scheme in a climate model (CESM) with comparison and assessment against AERONET observations. It is really as if it is two separate ideas/concepts/topics merged together into one. Part (A)- including sections 1 through 3 of the submitted manuscript- is not appropriate for ACP, appears to me to be outside of the scope of the journal, and should be reformatted and separately submitted to the sister journal “Geoscientific Model Development” (GMD) where it belongs- there is really no need for the journal GMD if material like this is submitted to ACP instead!

The referee suggests that much of our original paper, specifically Sections 1-3, is outside the scope of ACP and instead belongs in GMD. In order to respond effectively to this comment, we include below each journal’s “Aims and Scope” from their respective websites (emphasis in italics is ours):

- “Atmospheric Chemistry and Physics (ACP) is an international scientific journal dedicated to the publication and public discussion of high quality studies *investigating the Earth’s atmosphere and the underlying chemical and physical processes*. It covers the altitude range from the land and ocean surface up to the turbopause, including the troposphere, stratosphere and mesosphere.”
- “Geoscientific Model Development (GMD) is an international scientific journal dedicated to the publication and public discussion of the *description, development and evaluation of numerical models of the Earth System and its components*.”

Section 2 of our paper describes new physics of dust emission, and uses this to derive a dust emission parameterization, which is then tested against measurements in Section 3. Sections 1-3 thus investigate a physical process, and develop a theory and parameterization to describe it. These sections do not describe, develop, or evaluate a numerical model of the Earth system. Therefore, we consider Sections 1 - 3 of the original paper to be within the scope of ACP, and not within the scope of GMD.

The second section, centred around part 4 of the submitted manuscript (the usage of the new dust emission scheme in the CESM model and assessment against AERONET observations) is appropriate for ACP and should be submitted, perhaps simultaneously, to ACP as a separate manuscript. By doing so, it would also eliminate the problem that the full submitted single manuscript is overly long. Thus, for ACP Reviewer’s Question 1, Scientific Significance, “Does the manuscript represent a substantial contribution to scientific progress within the scope of this journal (substantial new concepts, ideas, methods, or data)?,” I am forced to give it a Poor rating because much of is not “within the scope of the journal.”

In response to this comment and similar comments from referee #1, we have split the paper into two along the lines suggested by the referee.

With regards to Question 2, “Are the scientific approach and applied methods valid? Are the results discussed in an appropriate and balanced way (consideration of related work, including appropriate references)?” in general, Section 4 of the manuscript, comprised of the usage of the new dust emission scheme in the CESM model and comparison to AERONET observations, is of Excellent quality. However, I noted some lack of balanced consideration in the first part, the derivation and testing of the dust emission scheme, so I must give it a “Good” rating overall. I will discuss these concerns below.

Firstly, there are regions, in fact some of the Earth’s dust hotspots, where a significant amount of mineral aerosol is produced from sources that are nevertheless classified by geomorphologists as “supply limited” (see lines 232-233), thus the statement (line 233) that they are “probably less important in the global dust budget” represents a too-quick dismissal of these circumstances that must be more explicitly justified if it is included, and additional discussion added of how the parametrization might be changed for/applied to supply-limited environments.

This is an excellent comment. There might be some confusion here, arising from the slightly different meanings of the term “supply-limited” in the geomorphology versus the aeolian transport contexts. In the aeolian transport context, a supply-limited soil means a soil for which the horizontal saltation transport rate *at a given point in time* is restricted by the availability of sand-sized sediment in the soil. In the geomorphological context, especially as applicable to dust hot spots, a supply-limited region can refer to an area whose dust emission is controlled by the limited supply of sediments over, say, a year. For instance, a “supply-limited region” can refer to an area that emits strongly only after a flood has delivered a fresh supply of sediment. Although such a region can thus be described as “supply-limited” in the geomorphological context, whenever substantial dust emissions do occur, for instance after such a flood, the soil is not “supply-limited” at that point in time in the aeolian transport context. That is, the horizontal saltation flux is not usually limited by the availability of appropriately-sized sediment. Our theory thus would apply to such a soil at the time that these emissions occur (subject to the other constraints discussed in the article). We thus do not intend to imply that emissions from such regions is not important in a global context, and we thank the reviewer for pointing out this point of confusion. We have made some revisions to Section 2.3 and added a paragraph to Section 3.6 (formerly 2.4) to make this point more clear, and to remove the suggestion that “supply-limited regions” are not important in the global context.

While it is true (Lines 193-195) that “the threshold for fragmentation of soil dust aggregates might be the most relevant threshold for dust emission under many conditions...” and clearly the research of Kok and collaborators in recent years applying brittle fragmentation theory to understanding dust production is inspired, elegant and probably the greatest recent advance in wind erosion science, I do believe it is a bit overstated herein as the implicit sole significant mechanism for dust production.

We appreciate the reviewer’s positive comments here, and agree with his/her comment that we should be careful not to minimize the importance of processes other than fragmentation to dust emission. As we mention on lines 99-103, the choice to focus on the fragmentation process is partially borne out of the suggestion from Kok (2011) that fragmentation is the most important dust emission process, and partially borne out of the practical considerations that we needed to choose a threshold for a single process to derive our parameterization. The main assumption going into the derivation of Eq. (18) is that of a threshold process described by a normal distribution, and Eq. (18) thus theoretically applies to any dust emission process described by such a threshold, not just fragmentation. We have rewritten the first paragraph of Section 2.4 to make this point more clearly, and have also moved the whole Section 2.4 to the end of Section 3 (so 3.6), to improve the organization.

Again, at least regionally on Earth there are places where significant dust production comes from other mechanisms, those described here in Section 2.4 including “dust emission from crusted soils... and from

sand particles with clay coatings.” If, as the authors state, “the parameterization’s functional form is also valid for dust emission controlled by other thresholds,” it should have been demonstrated against data set(s) for such circumstance(s), and the apparent (to me) lack of such testing raises concern.

We have rewritten parts of Section 3.6 (formerly 2.4) to address this concern. Specifically, we now explain in more detail that our derivation of the parameterization does not require any specific assumptions about fragmentation, such that Eq. (18) theoretically applies to any dust emission process controlled by a normally-distributed threshold. We now also note in the first paragraph of Section 3.6 that “although our parameterization theoretically applies to dust emission from soils dominated by processes other than fragmentation, the dimensionless coefficients in Eq. (18) could be quite different for such soils. We are not aware of any experimental data sets that meet our quality-control criteria that could be used to estimate the dimensionless coefficients for these different soils.”

In lines 404-412 the authors state “...it does not account for dust emission due to saltator impacts that do not produce fragmentation but that nonetheless produce dust by ‘damaging’ the dust aggregate.... It also does not account for the lowering of an aggregate’s fragmentation threshold through the rupturing of cohesive bonds by impacting saltators. These effects might dominate for very erosion-resistant soils, such as crusted soils.... increases in ψ might not produce corresponding increases in u^*_{st} for some soils. An example of such a soil is a sandy soil or which dust emissions occurs primarily from the removal of dust coatings on sand grains, and such soils might thus be poorly captured by the present theory...”: the parameterization must be tested against such datasets, but if I understand section 3 of the manuscript correctly, it isn’t- and section 3 thus looks a little bit like cherry-picking. Data sets should have been included in section 3 explicitly representing dust production from crusted soils and dust production demonstrated to be from damaging of aggregates and/or removal of coatings on sand grains! If such data sets are simply not available, additional explanation and justification is needed, and the discussion of the limitations of the parameterization should be increased.

Part of the problem here is that we do not know what processes control dust emission for any given data set. However, as far as we’re aware, none of the published dust flux data sets that meet our quality-control criteria are for crusted soils or for soils for which dust emission occurs primarily through the removal of clay coatings from sand grains. We have rewritten Section 3.6 (formerly 2.4) to make these points more clearly, and would be very interested in any suggestions the referee might have to further clarify these limitations. See also our responses to the referee’s other comments above.