Editor Comments

Thank you for your manuscript submission entitled "Modeling wind erosion on rangelands in the United States: Application and model validation" [Paper #2012JF002375] to the Journal of Geophysical Research - Earth Surface. I have now received a recommendation from the Associate Editor (Douglas Sherman) and 3 reviews of your manuscript, which are attached for your reference. Based on the comments, I find that your manuscript may be suitable for publication after substantial revisions.

Like the AE and reviewer 2, I found this to be a straightforward and generally well-written manuscript, but also one that is somewhat limited in its scope - a validation exercise of a single model. Like those readers, I feel that you are potentially missing a trick here by not demonstrating precisely how the model is an improvement on other approaches - not just through comparison of model skill as published by previous workers, but by reanalysis of your same (comprehensive) data set using the different approaches. We have tended not to publish straight validation exercises of individual models in JGR-Earth Surface, for the simple reason that such exercises (while important) do not always demonstrate a clear and considerable advance in our geophysical understanding of the underlying processes. Given the global relevance and timeliness of this topic, and the positive comments of the reviewers, I am willing to consider this work, but I would like to see you make more of an effort to justify its publication in a general Earth surface processes journal rather than one that is more subject-specific. Use this platform as a means of demonstrating conclusively, using an impressive data set, why your approach is so advantageous, and why it yields an improved physical understanding of the problem.

New analysis has been conducted using our dataset to test additional models. As a result substantial revisions of the manuscript have been made throughout. The main point of the exercise - besides providing the community with a model they can use – is to show that one gets very different results when one considers that vegetation changes the spatial distribution of shear stress on the soil surface (as the *Okin* [2008] model does) than when vegetation is used to modify the soil threshold shear velocity to obtain a landscape-scale surface threshold shear velocity (as other models do, e.g., [*Marticorena et al.*, 1997b] and [*Shao*, 2008]). In the former case, transport occurs in the model in some places in the landscape and not others and the fraction of the landscape that is subject to detachment/transport increases as the excess shear velocity (i.e. $u_* - u_{*t}$) increases. In the latter case, transport must begin across the entire landscape as soon as the shear velocity exceeds the threshold. The former is in line with field observations and physical intuition, and also yields better results for landscapes where there is significant vegetation, such as many rangelands. This important point has been made more strongly in the Abstract, Introduction, Discussion and Conclusion.

In light of the new scope of the manuscript, we have also changed the title to:
“Evaluation of a new model of aeolian transport in the presence of vegetation.”

The changes are throughout the text and are too numerous to be listed in detail, but in our responses below, we have made effort to be as explicit as possible about the nature and location of the change.

In addition to the changes requested by you, the AE, and the reviewers, we have added a table of Notation, which became necessary given the additional symbols that must be invoked for the new model comparisons. To the best of our knowledge, it is exhaustive.

As you consider how to revise this manuscript, I would also ask you to pay particular attention to the abstract, which as written is rather uninspiring. You state that there is currently no wind erosion model that works on 'these or similar systems' (by which you mean rangelands? It's difficult to be sure), but then state later that model parameters are in the range suggested by previous studies and that model predictions are in the range of field estimates. That leaves the reader wondering what is actually new here, and what the fundamental contribution of your manuscript is.

The abstract has been revised and the importance of the new model in terms of how we understand the process of aeolian transport in the presence of vegetation has been clarified in the Introduction, Discussion and the Conclusions.

Finally, I disagree with reviewer 1 about cutting the description of the *Okin [2008]* model - a reader should not need to dig up that paper just to understand what you are using. The length of the model description is adequate and should not be changed. The description of the Okin [2008] model has been kept.

line 231: 'is related to'

The suggested change has been incorporated.

407-410: if you are going to cite a figure in the text, then it must be in the text, not relegated to an appendix. I think this figure should be moved to the main text. The brevity of the appendix means that this material could be incorporated into the main text as well with little difficulty.

The material in the Appendix has been moved to the end of "Methods and Data" as "2.6 Sensitivity of errors to uncertainty in site parameters". The figure of the Appendix was moved to the "Results", it was renamed as Figure 6.

451-460: this material has not previously been introduced in the manuscript, and so does not belong in the conclusions. This is very suitable for the discussion, however, and could be expanded.

This paragraph has been moved to the end of the Discussion.

Please format all citations and references following JGR style - see the Author's Guide for full details.

All citations have been formatted according to the latest JGR style according to AGU's Author Guide.

Tables: In Table 4, please check that all symbols are correct; some of the symbols have not reproduced in the converted PDF. Table 8 can be deleted.

This table has been checked and we converted it to PDF and no errors were found. Table 8 has been deleted.

Figures: please use consistent fonts and font sizes for axis labels on all figures.

This suggestion has been followed wherever possible.

Labels on Fig 1 are very difficult to read, and the x-axis label 'Lambda (λ)' is not particularly informative to the reader.

We have redrawn this figure.

Associate Editor (Remarks to Author):

The matter of aeolian erosion from drylands is an important issue that will likely be of increased focus with anticipated increased stresses from climate and anthropogenic changes. In that context, the research reported in this paper has the potential to be a valuable tool for understanding, predicting, and managing aeolian erosion. The authors have pieced together an extensive data set from several shrub-dominated (or shrubby/grassland) environments to test the efficacy of the *Okin [2008]* model. There are several matters that deserve some consideration by the authors. First, the reviewers have done a good job of working through the manuscript and the authors should evaluate each of the comments carefully.

The seemingly random use of units at different points within the manuscript needs to be addressed.

The length units have been changed to meters throughout. Mass measurements throughout are in grams. The units for the total horizontal mass flux was g m^{-1} d^{-1} have been maintained, though we realize some might prefer g m^{-1} s^{-1}. Because collectors were deployed for a matter of days or weeks, we believe that these units are preferable because they better represent the timescale of the measurement. These units have been used in other studies published in JGR including Bergametti, G., and D. A. Gillette (2010) 115, F03044 and Gillette, D.A. and A.M. Pitchford (2004) *109*, F04003.

And I agree with the general comments that the methods require elaboration and that the value of this contribution would be increased had the data been used to evaluate other models and the Okin model with these data.

Substantial revisions throughout have been made to elaborate on the contribution this paper makes vis-a-vis other models and other ways of envisioning transport in vegetated systems. The methods have been elaborated upon and selected site data have been included in Supplementary Material.

Some specific comments:

1. Line 127: When you use the term 'validate' here as the purpose of the paper it makes me uneasy because it speaks to a motivation that cannot be interpreted as unbiased. Indeed, the concept colored my interpretation of most of what you report after this point.

The focus of this paper was to use extensive field data to evaluate the *Okin* [2008] model by using a variety of statistical and analytical approaches, and it is our aim to interpret the results without any biases. To mitigate AE's concern and the potential concern of readers, we've decided to change the term "validate" to "evaluate". In light of the new scope of the manuscript, we have also changed the title to: "Evaluation of a new model of aeolian transport in the presence of vegetation."

2. Line 140ff: Why use the field sites that did not have the appropriate characteristics? How inappropriate would a site need to be before it was not considered usable?

We did not use field sites that did not have the appropriate physical characteristics. As this line indicates, sites were chosen that weren't established for the purpose of this study. The sites we utilized, indeed, include every BSNE stem that we could find in the United States with at least three BSNE traps (so horizontal flux could be calculated) that was still installed at the time of the data collection and that we could gain access to. We required access to sites because new data had to be collected for every stem. Emails to colleagues and the aeolian listserv were sent out and this is every site we could find that met these minimum criteria. We provide this information here for the Editor, AE, and Reviewers. However, we do not believe that this belongs in a published manuscript, so no changes have been made.

3. Line 148ff: There is absolutely no reason to believe that a monotonic decrease in mass flux with elevation "...ensures that sediment in traps is of local origin..."

This is true, but it is also true that sites without a monotonic decrease of mass with height almost certainly are dominated by non-local sediment, which is why we used this as a criterion for excluding certain stems. This statement has been modified as "The latter criterion suggests that the sediment in traps is not dominated by non-local sources".

4. Line 163: each...site

The suggested change has been incorporated.

5. Line 167: upwind or downwind of the BSNE stem?

The transects started at the BSNE stems and moved outward at three different directions separated by 120° to obtain information about the area surrounding the stem. The text has been modified to read “At non-JER sites, all measurements were conducted along three 50-m transects oriented at 100, 220, and 340 degrees from due north and set up beginning 5 m from the BSNE stem.”

6. Line 189ff: There is not enough information presented here, or in the Li et al. (2007) paper to adequately evaluate the quality of the transport rate measurements. The authors assume throughout the paper that their values for transport rates are measurements, but they are indeed only statistical estimates based on a few point measurements (this is recognized in the line 265 text), and thus subject to some degree of uncertainty.

We agree with AE that $Q_{t,act}$ is subject to certain degree of error as we used a limited number of BSNEs to measure the wind erosion flux across the study sites. For most of the sites, measurements were not replicated (i.e., multiple stems at the same location intended to measure the same area). We have provided additional information about the fitting (r^2) in the Section 3.1 as well as in Table S1. The following text was added: “The fit of $q(z)$ to Equation (1) generally gave very good fits (Table S1 in Supplementary Material). Coefficients of determination for these fits, r^2, are not particularly useful because many of the sites had only three BSNE traps on a stem and Equation (1) has three parameters, thus resulting in $r^2 = 1$. However, for sites with more than three BSNEs (i.e., all sites excluding the Utah sites) the fits are generally very good, with only two being fit with $r^2 > 0.9$.”

If transport at the field sites is dominated by the movement of sand-sized particles, then most of the transport will be occurring at a level beneath the lowest trap. It is hard to evaluate this because sediment size data are not presented.

Sediment size data are not available. As we discussed earlier, this was a *post hoc* effort, meaning that much information was not available, and in many cases, samples may no longer be available. While the AE’s comments may be true, it nonetheless disregards the practical aspects of measuring aeolian transport in vegetated environments, particularly during long-term and remote deployments, such as those used here. Interference with (growing) vegetation in the rotation of BSNEs means that they cannot be situated extremely close to the ground. Long-term remote deployments require most investigators to account for this in establishing their stems. In short, the absence of sediment size data is beside the point *in this case* because flux measurements in vegetation regions are so constrained by realities on the ground. The data that the AE asked for (transport closer to the bed) would be either suspect or impossible to obtain without disturbance of the sites.

It is also unclear at what elevations the traps were installed, in the sense that if the authors state an elevation (and they do not in this paper), does the elevation represent the top, bottom, arithmetic trap center, or geometric trap center?

The height of BSNE traps was measured at the time that the sites were visited because traps were installed by different personnel at various times in the past. The data were not presented here because we had a large number of individual traps BSNEs and it is unclear that this information (for 65 sites, at least three entries) would add substantially to the manuscript or the analysis. This level of detail seems gratuitous, but to provide information about the general trap heights, the following text was added to Section 2.1: "The lowest traps were located ~0.1-0.15 m above ground surface and the top traps were mounted at about 1 m high."

Heights were calculated as the arithmetic center of the openings. Text in section 2.1 was revised to state: "The mass of sediments collected in the BSNE traps was divided by the inlet area of the trap (1×10^{-3} m^2) and the time of the collection to obtain the time-averaged horizontal mass flux density *q(z)* in g m^{-2} d^{-1}, where *z* is the height of the arithmetic center of the inlet above the ground (m)."

The latter issue is of particular importance for the curve fitting. If we assume that the 0.1 m trap spans the elevations .075 to 0.125 m, then the arithmetic center is 0.10 m but the geometric center is at 0.097 m. If the bottom of the trap is at 0.10 m, then the arithmetic center is at 0.125 m while the geometric center is at 0.122 m. These differences seem small, but because they are at the bottom of the array where the transport rates will be greatest, the influence on the total transport rate estimate will be amplified - perhaps substantially (see Ellis et al., 2009 for greater detail).

We believe that the AE overestimates the importance of the *Ellis et al*. [2009] results vis-à-vis whether arithmetic or geometric centers (AC v GC) are use for curve fitting, again, *in this case*. First, the flux v height curve used here [*Shao and Raupach*, 1992] was not evaluated by Ellis et al (2009), so it is unclear how much of a difference it would make in this case. Second, the results of *Ellis et al*. [2009] do not show clearly that GC is better than AC. Indeed, AC has generally the same or higher r^2 and generally lower sum of squared error (see Tables 3 and 4 therein) than GC. The reanalysis of the *Namikas* [2003] data provides but a single point. Third, all data has error, whether direct measurements or, as in this case, estimates based on fits. What matters with respect to the error of a dataset is how much the error influences the results in a particular application. In this application, the error we're concerned with is the error of the horizontal flux measurements with respect to the ability of the models to estimate the horizontal flux. One would certainly get slightly different values for measured horizontal flux if one used GC instead of AC, but how does the difference between these values compare to model error? If the model error is much greater than the difference in horizontal flux estimates using GC vs AC, then the use of GC vs AC is a difference without a distinction. In this case, differences in flux calculations using GC vs AC are likely not anywhere near model error, and so we consider this point to be moot.

7. Values for RMSEL should all have units. So should your intercept values.

Intercept values have units of g m^{-1} d^{-1} and these have been added. However, RMSEL does not have units: $\log(Q_{t,pred})-\log(Q_{t,act})=\log(Q_{t,pred} / Q_{t,act})$. The units cancel for $Q_{t,pred} / Q_{t,act}$, so RMSEL doesn't have units.

8. Lines 322: Text suggests that Figure 2 depicts data for Kawamura and Lettau and Lettau models. Not indicated in Fig caption or on Fig.

This figure has been removed.

Some other general comments:

I am uncomfortable with the use of ρ to represent both density and Pearson's correlation coefficient (typically denoted as r).

ρ has been changed to r throughout.

I am also curious about why no values for the coefficient of determination are reported. It is a key estimation of the degree of statistical explanation that your model has. This would be particularly useful in a comparison of the Okin model with other models.

The coefficient of determination (r^2) was not included (though r was) in evaluation the model prediction and field measurements because in regression analysis, r^2 only indicates the strength of a relationship between two variables, however, it is not able to tell the magnitude of the differences between the variables. In a model evaluation exercise like this study, we are focused not only on the overall trend of model prediction vs. field measurements, but also the magnitude of difference between model prediction and measurements, which is represented as ε_r. Tables 8 and 11 also show clearly that ε_r can decrease with little (or even opposing) effects on r (or r^2)

How would your results differ if you had segregated model results by environment?

The focus of this work is twofold, to evaluate the OK model and to obtain optimum values of the internal parameters used in the model. The latter objective requires us to use as many as possible number of study sites and that the model is not only sensitive to a particular environment/plant community, therefore, we did not segregate the results by environment.

Was the model calibrated for each site, or did you use single estimates of A, z_0, etc., for all predictions. This may be stated in the text, but I could not recognize it.

The OK and MOK models were calibrated based on all 65 sites, and the optimum values of A, z_o, etc were used for all predictions. Section 2.3 now states: "In the OK model, z_o is set as a constant for all sites. This allowed us to treat z_o as a fitting parameter in our model validation and meant that z_0 would not have to be estimated at each field site. Other model input parameters, including A, C, and $(u_s/u_*)_{x=0}$, were also treated as constant for the purpose of the model validation." In addition, there is now extensive discussion of constant z_o in the Discussion.

Your arguments for the use of RMSEL are logical given the range of observations and predictions. But because most sediment is moved during larger 'events' it would seem that use of RMSE, emphasizing errors in those more meaningful conditions, would be a better approach. A 100% error when the transport rate is 1 gm-1d-1 is insignificant. The same error when the transport is 100 gm-1d-1 is substantial. It's not clear, from an application perspective, why you would seek to give them statistical equivalence.

In response to this point, and in defense of using RMSEL (and ε_r), the following text has been added to section 2.4: "The *RMSEL* was utilized instead of the root mean squared error (*RMSE*, the error calculated without first taking the log) because the horizontal flux estimates spanned two orders of magnitude. The use of *RMSEL* instead of *RMSE* is justified by the purpose of the OK model, which is to estimate horizontal over a wide range of field conditions including those with low flux. The use of *RMSE* would emphasize errors of prediction for larger fluxes considerably more than errors of prediction for smaller fluxes. It is our contention that locations with higher horizontal transport are not necessarily more meaningful in terms of the amount of transport in or dust produced from natural landscapes. The amount of dust produced from landscapes (i.e., the vertical flux in units of M A^{-1} T^{-1}) can be approximated as a linear function of the horizontal flux with the constant of proportionality depending on soil characteristics [e.g., *Gillette*, 1977]. Therefore the amount of dust produced from a landscape proportional to the product of the horizontal flux and the area over which the horizontal flux occurs. That is to say, large areas with relatively low flux may produce as much dust as small areas with higher flux. With this in mind, it would seem necessary to have a model that can estimate both the small fluxes and the large fluxes equally well. Thus, we chose to use as our error metric RMSEL, which emphasizes error for small fluxes and large fluxes equally, over RMSE, which emphasizes error for large fluxes over small fluxes."

There is also a substantial literature on the relationship between vegetation and threshold shear velocity that seems of relevance to your discussion, but which is not cited (e.g., King et al., 2005, in JGR).

We believe that the AE is referring here to the *Raupach et al.* [1993] and *Marticorena et al.* [1997] shear stress partitioning schemes. Insofar as these are used in the SHAO and MAR models, they are now discussed a great deal in the revised manuscript. Indeed, as we have now tried to make more clear in the manuscript, the OK model differs in a fundamental way from these shear stress partitioning schemes (OK changes the distribution of shear stress whilst these schemes change threshold shear velocity of the surface based on vegetation (lateral) cover) which results in significantly different model results. In addition, *King et al*. [2005] is cited.

Reviewer #1 (Comments to Author):

General Comments: This is a valuable, well written and well-formulated documentation of field experiments to validate the new wind erosion model of aeolian transport in rangeland and the optimum model parameters was selected. To my knowledge, this is the first such set of laborious field experiments to test a model. The practical application of the work is if incorporating into other ecosystems, particularly agricultural lands, it may provide reliable estimate of total atmospheric dust loading and the distribution of dust emission in the world. The paper should be accepted with some revision.

Specific comments and suggestions:

1. As one might predict, shear stress between or within the vegetated "shrub lands" were not only a function of lateral cover and shrub density, but also the structure of the single vegetation and cluster distribution type. These controls will vary with location, thus limiting the exportability of the results. Essentially every area will be unique to some degree based upon shrub structure and the nature of the shrub itself. If possible, the authors should take Dong et al's (2011) recent field evaluation about the equations for the near-surface mass flux density profile of wind blown sediments as reference to gate more comparable datum at different land types.

We agree that shrub structure will have some impact on shear stress partitioning, and therefore on transport, but we disagree about the magnitude of this effect. In response, the following text has been added to section 2.3: “In the OK model, the impact of the shrub structure is accounted for mostly in the $\left(\frac{u*_s}{u*}\right)_{x=0}$ parameter. In reality, to some extent the vegetation structure will impact shear stress partitioning and therefore $\left(\frac{u*_s}{u*}\right)_{x=0}$, but there is in fact a remarkable degree of overlap in shear stress portioning ratio (SSR) amongst solid and porous objects [*King et al.*, 2005]. When examining all available SSR in light of the OK model, there was no clear value of $\left(\frac{u*_s}{u*}\right)_{x=0}$ that separated solid from porous objects, although there was a slight bias toward higher values of $\left(\frac{u*_s}{u*}\right)_{x=0}$ for porous objects. In light of these observations, it is unclear how much $\left(\frac{u*_s}{u*}\right)_{x=0}$ would vary amongst porous objects. In short, there is no compelling reason based on existing data to treat $\left(\frac{u*_s}{u*}\right)_{x=0}$ as anything but a bulk constant. *C*, too, may vary with shrub structure or porosity, but in the absence of experimental or theoretical guidance on this and for the purpose of parsimony, it has been treated as a constant.”

With regard to the author’s second point that shear stress partitioning depends on “cluster distribution type “, we agree emphatically. This is the purpose of the OK model and the reason for the present study and we believe that new text describing the model and how it differs from the MAR and SHAO models in the Abstract, Introduction, Discussion and Conclusion make this clear.

Finally, with regard to the Dong et al. (2011, Equations for the near-surface mass flux density profile of wind-blown sediments, 36, 1292-1299, ESPL) paper, it is unclear what the reviewer is hoping that we would be able to do. This paper examines several different flux density profile equations to determine which work best in four different sites in China. Unfortunately, the paper did not evaluate the Shao and Raupach (1992, JGR, 97:20,559-20,564) equation that we use and has been used by others (e.g. Gillette), so does not provide any guidance as to whether another equation might have been used more profitably. We note, however, that Dong et al.'s (2011) results indicate that a three-parameter modified exponential function worked best. The Shao and Raupach (1992) equation is a three-parameter modified exponential function, albeit different from the one used in Dong et al. (2011). In terms of using "Dong et al's (2011) recent field evaluation about the equations for the near-surface mass flux density profile of wind blown sediments as reference to gate [sic] more comparable datum at different land types," we interpret this to mean that we should have included these four sites in our validation dataset. This is, quite simply, impossible due to because the requisite (esp. gapsize) data were not collected at these sites.

2. If possible, please reduce the length of the introduction of the Okin's (2008) model in part 2.3 from 201-247, it is not necessary to include such a lengthy. Readers can learn more details easily form the origin paper published earlier (Okin, 2008)

We kept the material for the introduction of the Okin's (2008) model, as suggested by the editor.

3. I noted an error: line 97 - "low" should be "high";

The suggested change has been made.

4. Line417: Annotates in the bracket may be omitted, because it easily to understand according to the context. Do you think so?

The annotation in brackets has been deleted.

5. Line 191-193 and 210: It is confused that since "was divided by the inlet area of the trap", why the unit of the horizontal mass flux was g m-1 d-1? It seems that it is a point flux according to the author's definition in text. If it really such meanings, than how can the data in this research be compared with other researcher's?

This is an error and it has been corrected, the time averaged horizontal mass flux at a specific height (i.e. *q(z)*) should have units of g m^{-2} d^{-1}. The total horizontal flux, Q_r, is the integral of *q(z)* from the ground surface to 1-m and has units of g m^{-1} d^{-1}. The paper compares total horizontal flux in combination with flux-shear velocity functions presented by other researchers.

6. On Figure 1, the symbols in the diagram should be redefined again, and the vertical axis's sand flux unit should be the same for ease read and comparably. Furthermore, the (a), (b) and (c) text should be add in the figure, these are listed in the text and caption, but not marked in the diagram. Also, the title of the horizontal axis in figure 1 should be "lateral cover, λ " as described in the caption for reader ease.

This figure has been redrawn. Please see my reply to the editor.

7. I have found that this paper should also mix centimeters, meter, and millimeters in text and figures-use SI units only for convenience compare with each other.

We've changed to use meters exclusively.

Reviewer #2 (Comments to Author):

This manuscript titled "Modeling wind erosion on rangelands in the United States: Application and model validation" describes the application of a sediment transport model in mainly vegetated landscapes based on a series of five various field measurement campaigns. The model is measured against the measured data using a quantitative estimate of the model performance and compares this performance to other model validation manuscripts using other models (but does not compare other models to the model used in the manuscript). The manuscript is well written with good methodology and appropriate amount of discussion.

Other than the listed comments below, the other concerns with the manuscript include: the lack of description in the methods and results of the field data (BSNE and wind field data) and that there was no comparison of its performance compared to other existing models for the same field datasets. The field dataset is the basis for which the model performance is based on and therefore more time should be spent on describing this data; including BSNE collection times, Q time series, wind fields.

The revised manuscript is substantially longer than the original due to the new model comparisons. In order to not further bloat the manuscript, these data were provided in the Supplemental Material Table S1.

A more detailed description of the layout of the BSNE traps and the collection times was added to section 2.1 Description of the Sites. This new text is "At non-JER sites, all measurements were conducted along three 50-m transects oriented at 100, 220, and 340 degrees from due north and set up beginning 5 m from the BSNE stem. At the JER sites, measurements were conducted along three 50-m transects oriented in the direction of the prevailing wind."

We also created a new section "3.1. Characteristics of the Model Input Data" in the Results to provide more information on the field data and model inputs. We moved Table 2 to this section, which served to present the characteristics of vegetation, threshold shear velocity, and wind. In addition, in this section, two new figures, Figure 4, frequency distribution of wind, and Figure 5, BSNE monitored horizontal mass flux were also included.

Model performance is compared to other models by way of estimating how well the other models performed within previously published dataset comparisons. Although this does give some estimate of the range of acceptable error within the model performance, a direct comparison of the *Okin [2008]* model to the other models using the previously published datasets or the dataset presented in the manuscript would provide a better evaluation. It is for these two reasons I will suggest to accept the manuscript with major revisions.

The model comparison has been done, resulting in extensively rewritten text throughout the paper, including new sections in Methods and Results. These changes are too extensive to be enumerated in detail.

Line 97: Figure 1. The scales and units of the variables on these figures are all different. Consider revising the axes to improve visual comparisons. Additionally, the field experiment data are represented by continuous lines although they are data points, consider representing points with symbols.

This figure has been redrawn. Please see my reply to the editor.

Line 121: "(Courtright and Van Zee, 2011)" - missing in bibliography.

This paper has been added to the references.

Line 140: Recommend changing "was" to "were".

The recommended change has made.

Line 170: It is unclear what the purpose of the Frisbee is and why it needs to be dropped from a height of 10 cm above the vegetation - consider revising. Also, what is the width of the line-intercept method?

It is extremely difficult to measure the height of vegetation that the wind might "see". Does one use the topmost blade of grass, even though it will probably flatten in the wind? If not, what else can one use? If not the topmost blade, how can a replicatable height measurement be found? For height, as with all field measurements, decisions need to be made to ensure that results are internally consistent and can be replicated by others. Even though it is the easiest, our decision was that using the topmost height doesn't make sense for studies of aeolian processes because, in practice, often a seedhead, leaf, blade, or branch that sticks significantly above the bulk of the canopy but will not have much influence on windflow either because it is very small or very flexible. The Frisbee standardizes a measurement of height that isn't the topmost height. By dropping from a standard distance and using a standard, easily obtained object (a regulation Ultimate Frisbee) measurements of height, we ensure that the same force is use to push down the topmost piece of canopy in every case. The use of a dropped disk for vegetation measurements has a long history. As a brief response to this question, the following text has been added to Section 2.2: "This empirical approach was used to approximate the effect of wind shear stress bending the top of the plants and to eliminate the effect of small/thin leaves or stems that may protrude significantly from the main canopy, but which probably have little impact on airflow."

In terms of the width of the line-intercept method, we do not understand the question. A line-intercept is the intercept of the canopy along a line, usually delineated by one side of a stretched meter tape and therefore has no width.

Line 218: Parenthesis of density units is unnecessarily superscripted.

This typo has been corrected.

Line 231: Extra word "to" should be removed.

The “derived from to” has been changed to “related to” according to the editor’s suggestion.

Line 235: Extra word "of" should be removed.

Suggested change has been made.

Line 245: It should be explained why the authors deem it acceptable to hold these parameters constant for model validation as they have provided (Table 3) information that suggests they can vary considerably.

z_o: With the inclusion of the Modified OK model, z_o is not necessarily considered constant, and an extensive discussion of z_o is now provided in the Discussion.

A is an empirical constant that corrects for the form of the flux equation and the units used. All of the original manuscripts from which we pulled flux equations include an empirical constant of some type or another.

$(u_{*s}/u_*)_{x=0}$ and *C*: see response to Reviewer #1 above.

Line 337: The model parameters were bound by these values when solved (Table 5, Line 259-260), so it is no surprise that they are within this range. Suggest revision.

Agreed. This text has been deleted.

Line 339: "roughness height zo" - revise by choosing one.

With deletion of the text due to the above comment, this comment no longer pertains.

Line 348: This paragraph is confusing as it describes past work involving dust emission schemes, while the following paragraph describes (and the model validation given in the results) only discusses horizontal flux transport models. Although related in many environments, the measurement of dust transport and therefore the validation of dust emission models are more complicated than solely sand transport models. Suggested revision is to omit this paragraph.

This paragraph has been deleted and related references have been excluded.

Line 365: Replace "sand-alone" with "stand-alone"

This typo has been corrected.

Line 416: Missing or additional word(s). Revise.

“have been estimated flux” was revised as “have estimated flux”.

Line 360: Model estimates not much better than WEPS or RWEQ? It would have seemed that this model would have performed better, perhaps using the cropland models to estimate the field data collected in this study can provide a better comparison between the performance of the different models.

Additional models have been evaluated here and substantial revisions have been made throughout. Nonetheless, we disagree that the take-home message of this section (which has been moved to appear later in the Discussion Section) should be that the model estimates aren’t much better than WEPS or RWEQ. To highlight this we have added the following text: “It is critical in these comparisons to note that all of the studies referenced above were from agricultural fields, many of them bare, and on which soil parameters could be measured in detail. All of the studies cited above except *Fryrear et al.* [1998] were also for individual storms. Bare soil or homogenous crop plantings and single events are arguably much simpler systems for modeling aeolian transport than the structurally and spatially heterogeneous rangelands used in this study. In addition, the fact that there were several cases in which aeolian activity was not modeled, even though it was observed, constitutes a significant failure of these models. There are no such cases in the present study for the OK model and we believe that these comparisons show that the OK model performs well above benchmarks set by previous studies.”

Table 2: Revise U*t to standard units [m/s]. It also should be noted in caption that these are estimated.

The units for u*t have been changed to m s^{-1} throughout the paper. The suggested change for Table 2 has been made.

Reviewer #3 (Comments to Author):

The authors present a careful validation of the *Okin [2008]* model of wind erosion using a wide range of field data from the USA. I recommend accepting the paper following some minor corrections:

l. 52 - explain what 'a factor of 3.0' means (here and in the text)
This terminology has now been removed from the manuscript and we use the term ε_r, which is approximately relative error.

l. 79 - Nickling and Gillies, 1993 - missing in the references

This paper has been added to the references.

l. 94 - Marticorena

Corrected.

l. 94 - Mahowald et al., 2002 - missing in the references

This paper has been added to the references

l. 121 - Courtright and Van Zee, 2011 - missing in the references

This paper has been added to the references.

l. 166 - why were these values (100, 220, 340 degrees) chosen? Is it related to dominant wind directions, or simply to represent different directions?

There is nothing special about these directions for the non-JER sites, for clarification the following text has been added: “At non-JER sites, all measurements were conducted along three 50-m transects oriented at 100, 220, and 340 degrees from due north and set up beginning 5 m from the BSNE stem. At the JER sites, measurements were conducted along three 50-m transects oriented in the direction of the prevailing wind.”

l. 206 - I assume this should be gaps upwind and not downwind

The model simulates how plants impact shear stress in the gap immediately downwind of plants. No changes have been made.

l. 238 - the authors state that z0 is supposed to be independent of plant parameters. However z0 is influenced by upwind topography and vegetation to a distance of dozens-hundreds of meters (see for example Levin et al., 2008, ESPL, Estimation of surface roughness (z0) over a stabilizing coastal dune field based on vegetation and topography).
l. 312 - z0 values between 1-9 cm are characteristic more with upwind vegetation or upwind topography than to bare surface microtopography.

With the addition of the MOK model and the associated discussion, we believe these comments have been addressed.

l. 344 - missing in the references

The paper Minvielle et al. (2003) has been added to the references.

l. 359 - TX stands for Texas?

Yes, and TX was changed to Texas, USA to make it more clear.

l. 379 - r=0.71 was not statistically significant, whereas on l. 396 r=0.41 was statistically significant?

Correct. The r_{crit} of 0.81 is for a lower sample number (n= 6) than for our study (n=65).

l. 395 - r correlation coefficient values not shown in Table 7

They are now included.

l. 399 - Namikas and Sherman, 1997 - missing in the references

This paper was added to the references.

l. 416 - have estimated flux

Corrected.

Table 2 - the title states 'unvegetated soils', however in the table fractional plant cover reaches even 78%. Please explain this.

This terminology is adopted to differentiate the threshold shear velocity of the soil, in areas not actually directly under plants, from the threshold velocity of the surface with vegetation present. The OK model treats the threshold fundamentally differently from other models (e.g. Marticorena et al. [1997] and Shao [2008]) that use the vegetation cover to adjust the threshold of the soil to come up with a threshold for the vegetated surface. In the OK model, the soil threshold shear velocity is never adjusted and there is no "surface" threshold. Instead, the distribution of shear stress on the soil is adjusted based on plant parameters. This is a subtle, but critical, distinction that actually is the main impetus for the OK model, as has been discussed throughout the revised manuscript. As a matter of terminology, the distinction is between the threshold of the "soil" (as it is measured in between plants, where present) and the threshold of the (vegetated) "surface". Because "soil" and "surface" could, to some readers be interpreted as the same, we adopted the terminology of the threshold of the "unvegetated soil" to get across the idea that it is for the soil and not the vegetated surface. This is a very difficult distinction to get across in a single adjective (i.e., "unvegetated"), and we could think of nothing better. The following text was revised in section 2.2: "Threshold shear velocity (u_{*t}) for unvegetated soils (i.e., for the soil itself rather than the vegetated surface as a whole) was estimated using a method newly developed by Li *et al.* (2010)."

Table 3 - Minivielle et al. (2003) - missing in the references

This paper has been added to the references.

Table 4 - some symbols do not show in the PDF, and are seen as black squares

This table has been checked all symbols are now correct.

Tables 7 and 9 - add r correlation coefficient values

r has been added to Tables 7, 8, 10, 11, and 12 (new numbering).