We wish to thank the reviewers for their insightful comments, which will substantially improve the revision. In cases where we did not understand the reviewers' concerns or our responses were otherwise inadequate, we ask the editor to correct our misunderstanding and we will, of course, make any changes requested.

We have provided the community with ~10^6 new boundary-layer velocity profiles and complementary high-quality TLS data for microtopography (see Supplement). Our study was comprehensively designed to capture the full range of microtopographic variability present in playas of the western U.S., necessitating field work across a large region (approximately 1000 miles). We took care to obtain data over approximately 2.5 orders of magnitude in height above the ground. We analyzed and augmented this important new dataset with relevant computational fluid dynamics modeling and data analysis. We developed an empirical model for predicting mean z_0 values based on microtopography that predicts z_0 values measured in our study sites with an R^2 value of 0.991. More broadly, it demonstrates how both the amplitude and the slope of microtopographic variations at a range of spatial scales contribute to z_0 values. We hope that the editor sees the value in our contribution.

<u>SAFL Sediment Dynamics Group:</u>
Q: "*...further explanation is needed regarding the known or postulated effects of vegetation and mobile sediment, in eolian or other settings. If one were to repeat the velocity measurements in an evolving or vegetated landscape with microtopography, are there reasons to expect that the linear formulation would be less successful?*"
A: This is a great question, and we thank the SAFL group for their interest in our work. We have tested our model in evolving landscapes (i.e. wind ripples) and found it to work well in the absence of saltation. Figure R1 (so labeled because it is for this reply only) presents a laser scan of a 6 m x 7 m section of a low sand sheet in the Salton Sea dune field at two instants in time: during a lull within a fast wind event, and after the winds slowed down significantly and the wind event ended. Ripples formed during the fast winds (shown in A) have larger wavelengths and are taller (approximately 1 cm) than the ripples formed during slow winds (approximately 4 mm, shown in B). Also shown in Figure R1 are the z_{0n} values (the contribution to the effective roughness length from each Fourier mode) predicted by the model. The model predicts roughness lengths of 0.07 mm and 0.05 m for the instants shown in Figure R1A and R1B, respectively. We measured z_0 values in the range of 0.05-0.1 mm and 0.03-0.07 mm from velocity profiles during times shown in (A) and (B), respectively, when saltation was absent. Figure 1RC quantifies the shift in spatial scales that contribute to roughness (from scales of approximately 30-50 cm for the time shown in A to 10-20 cm for the time shown in B).

Although preliminary, this analysis demonstrates that the model (with no parameter adjustment) does a good job of predicting the measured effective roughness length in evolving landscapes (wind ripples in this case) when saltation is absent. This is encouraging because wind ripples have a distinctively different morphology compared to the playa surfaces studied in the paper. We prefer not to put this figure into the revision because it represents a very different geomorphic setting from the playa surfaces that are the focus of the paper and because these data are being analyzed for another publication.

The contribution of saltation to the effective roughness length is beyond the scope of this paper. Tackling it (which we are doing now) requires collecting and analyzing data for saltation intensity, grain size distributions, etc. in addition to wind velocity profiles and multi-temporal

TLS data. We have not yet attempted to quantify roughness in vegetated landscapes because vegetation (at least in the shrublands we are most familiar with) bends in the wind, a process that impacts the effective roughness length in ways that require sophisticated models to quantify (e.g. see work of Nepf and her colleagues).

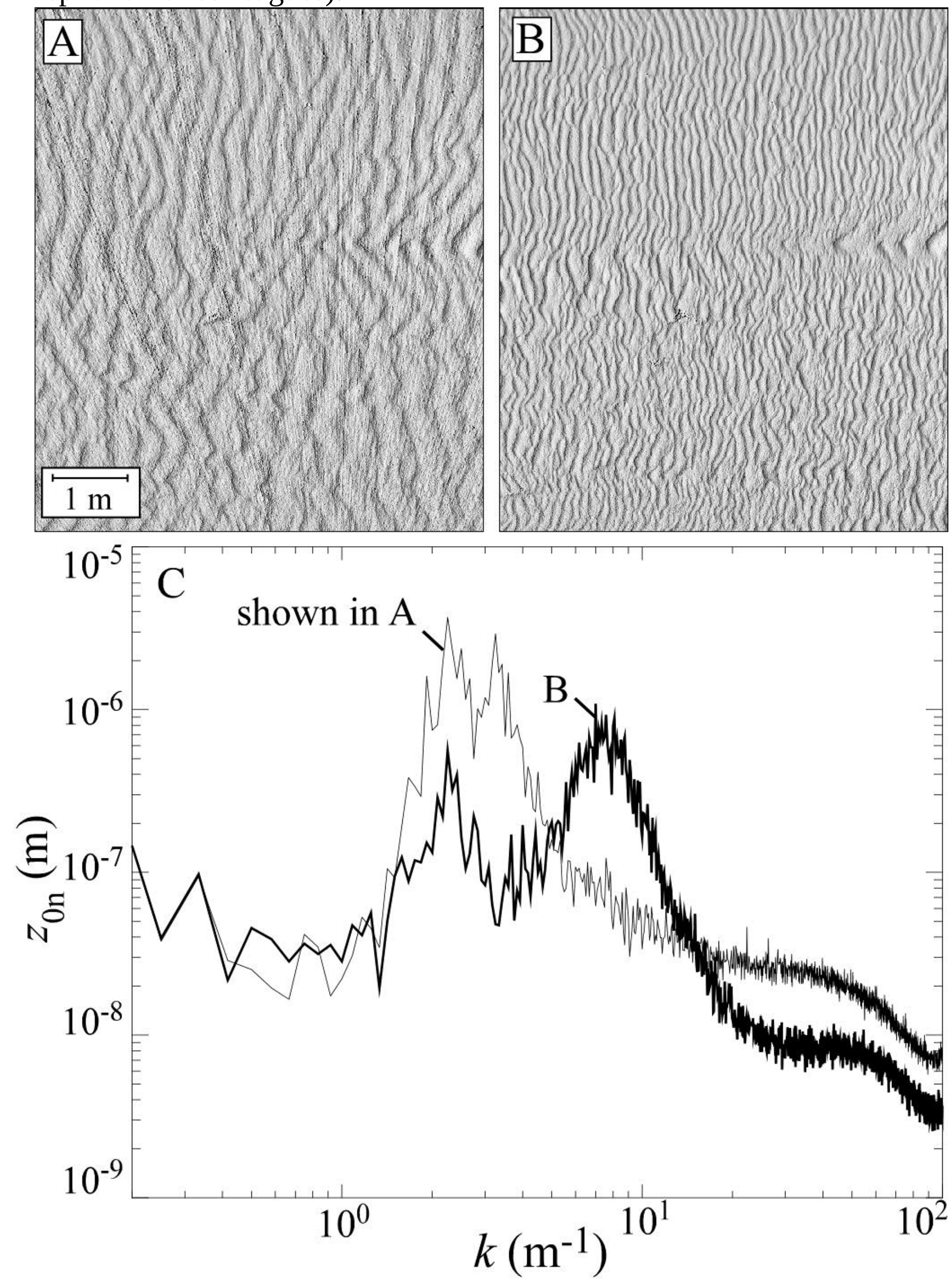

Figure R1. Prediction of the effective roughness length over evolving wind ripples in the absence of saltation. (A)&(B) Shaded-relief images of a wind-rippled landscape at two instants and time corresponding to relatively fast (in A) and relatively slow winds (in B). (C) Plots of the contribution of each Fourier mode to the predicted effective roughness length, z_{0n}, as a function of k for the topography in (A)&(B) (case B shown with thick curve). Effects of saltation (if present) on the roughness length are not included in the model. The predicted z_0 values are 0.07 mm and 0.05 m for the topography shown in (A) and (B) respectively.

Q: "*We would be interested to read more from the authors about the difficulties their linear formulation is likely to encounter in moving to larger scales, and scale ranges.*"
A: This is another great question. We would prefer not to speculate too much in the actual paper (bearing in mind reviewer 2's statement that he/she found our main conclusions to be mere speculation), but we are happy to take advantage of this public forum to discuss this issue. The short answer is that we don't foresee any challenges with applying the linear formulation *per se* to predict the effective roughness length at larger spatial scales than those we considered in our paper. However, there are several points worth making on this issue.

The first point is that the value of the effective roughness length depends, to an extent, on the resolution of the model it is being fed into. This is a subtlety that we did not have the space to go into in the paper but it is important for the issue of how our model might be used at larger spatial scales. In Earth System Models (ESMs) the lowest pixel in the atmosphere is often chosen to be 10 m above the ground or vegetation. Above this height the atmospheric component of the ESM solves the full equations of fluid dynamics (with some simplifying assumptions, of course) and therefore directly models the primary effects of large-scale topography on the flow. Within heights of 0-10 m, the GCM relies entirely on the effective roughness length and the law of the wall to estimate the velocity profile (which controls, and is controlled by, large-scale atmospheric dynamics). As such, the role of the effective roughness length in ESMs is primarily to quantify the effects of topographic obstacles less than approximately 10 m in height (but note that there are limitations associated with the horizontal resolution of ESMs such that many hills significantly taller than 10 m are not resolved by the computational grid or the input data and hence should be implicitly included in the effective roughness length). More research is needed on this issue. The point we made in the paper on p. 1111 was simply that the effective roughness length is function of variability at multiple scales likely extending beyond the range of scales captured by radar (one technique currently used to constrain z0 values for input into ESMs). We think this point is still valid.

To the extent that measured roughness lengths are generally lower than 1 m, even in topography with variations much greater than 10 m, our method suggests a possible reason, i.e. that the slope of topography generally decreases with increasing spatial scale. If topography is modeled as a Brownian walk (simplified, but consistent with power-spectral analyses of many topographic datasets), the amplitude of topographic variations increases as $L^{1/2}$ but the mean slope decreases as $L^{-1/2}$ (where L is the spatial scale). Since the effective roughness length is a strongly nonlinear function of slope in our model, our model predicts a diminishing importance of large-scale topographic variations on the effective roughness length. This could be an avenue of investigation for future work.

If the SAFL group knows of datasets that are available for us to test our method at larger spatial scales we would be eager to try it out.

<u>Reviewer 1:</u>
Q: "*1. I am not sure I understand the authors' goal. My point is that the estimate of the relevant value of z_0 depends on the scale of the considered problem. Take for instance a wind flow over a rough sinusoidal topography. z_0g = surface roughness; a = amplitude of the sinus; lambda = wavelength of the sinus. Assume scale separation z_0g « a « lambda. Assume also that the mean wind shear velocity u_* is such u_* z_0g/n nu » 1, i.e. the flow is turbulent even at the scale of the surface roughness. Then, for heights z « lambda, there is a law of the*

wall with a roughness z_0g, and this could be relevant for an estimate of the basal shear stress (which is modulated by the surface slope). However, for heights z » lambda, there is another law of the wall with a roughness that depends on the amplitude of the sinus, and its slope lambda/a (and probably still on z_0g as well), as mentioned by the authors. But I would say that this is more relevant for the description of the larger scale wind circulation rather than for surface processes. For profiles with values of z on the order of lambda, I'm not sure what we can deduce. I appreciate that the small and the large scales can interact: the large undulation can slow down the wind due to an increased surface friction, and thus reduce the basal shear stress. But, when studying and discussing the z_0 issue, one should make clear what is kept constant, e.g. the wind shear stress far from the surface u_*. So, I don't think that 'what is the roughness length of turbulent flows over a given topography?' is a well-posed question in general: one has to specify the related physical problem of interest first."

A: We certainly agree that the velocity profile over multi-scale topography is governed by multiple length scales. Indeed, in a recent paper (doi: 10.1002/2014JF003314) one of us highlighted the importance of the composite, piece-wise logarithmic nature of wind velocity profiles on sediment transport over ripples and dunes (a point that has been made by others too, of course). However, the *effective* roughness length of an aerodynamically rough turbulent boundary layer flow is reasonably well defined: it is the z_0 value obtained by a fit to the velocity profile starting from the top of the roughness sublayer (i.e. the height above the bed significantly larger than (usually taken to be twice) that of the dominant roughness elements) up to as high within the boundary layer as can practically be measured. Every Earth System Model requires a global map of effective roughness length as input. It is a reasonably well-defined quantity and every Earth System Model makes a decision about what its value should be above every spot on Earth's surface. Defining the dominant roughness element can be tricky and assumptions of scale separations (of the kind invoked by the reviewer above) can be useful. More generally, however, one has to use the approach we outline in the paper, i.e. compute the spectrum of z_{0n} values in order to quantify the impact of each scale of variability on the effective roughness length.

We agree with the reviewer that the primary importance of our paper is more atmospheric than geomorphic at this time, in part because the paper does not quantify how the component of the bed shear stress that drives sediment transport relates to the effective roughness length. Our work does allow anyone to quantify the effective roughness length, from which the drag of the surface on the atmosphere can be calculated using velocity data at just a single height above the surface (i.e. the most common velocity data available). The atmospheric scientists we speak to believe this is a significant result.

Follow-up studies we are currently conducting will, when published, demonstrate the importance of our results for sediment transport. First, we are using a combination of CFD models and field measurements to determine the empirical relationship between the skin friction component of the bed shear stress (that which drives sediment transport) and the shear velocities and effective roughness lengths, which are necessarily measured some distance above the bed. We think even the most skeptical reviewer will agree that some type of measurement of the velocity profile is needed in order to predict sediment transport, and our follow-up work will relate measurable aspects of the velocity profile to the component of the bed shear stress that drives sediment transport in the presence of microtopography (which is almost always present). Second, we are currently using our method to isolate the component of the effective roughness length that is due to microtopography and that which is due to saltation when saltation is present. The total roughness length is a combination of these two components when beds are erodible and saltation is active, and previous studies have not been able to isolate the relative importance of each. These follow-up studies will clearly demonstrate that the results of this paper are important for both atmospheric sciences and geomorphology. We considered including a paragraph in the discussion section on this current/future work. However, we also need to minimize the type of speculation that reviewer 2 criticizes. Therefore, we propose to make no changes to the paper on this point.

Q: "*2. What is the exact status of Eq. 3? It looks purely empirical, but is there a model that suggests the form of this fitting function? Is it new or already proposed in this or similar context? How does it compare to other proposed fits in the literature?*"
A: Equation (3) is empirical but, as Figure 10 demonstrates, allows a nearly perfect fit to CFD model results. We know of no theoretical model that suggests equation (3), but that does not mean that one does not exist. We simply plotted the data and imagined a function that would fit it well.

It is customary in the scientific literature to include a reference when one is using the results of others and not to include a reference when one is presenting original results. Since we did not include a reference, equation (3) should be interpreted as being original to this paper. We propose to make no change to the paper on this point.

Q: "*3. The CFD parameters are presented in a dimensional way, which is problematic. Rather than lengths and velocities, Reynolds numbers are relevant, and ratios, e.g. z_0g, height, and amplitude of the sinusoid in comparison to the viscous depth nu/u_*. Also I don't understand why the authors do not use periodic boundary conditions, in order to avoid an undesired fetch effect. How is modelled the micro-scale roughness z_0g in the k-epsilon code?*"
A: Reynolds numbers are relevant, but the reader has been provided all of the information to calculate them. The height of the viscous sublayer is approximately 0.4 mm at threshold, decreasing to smaller values with increasing u* (Kok et al., 2012). Therefore, it is clear that the viscous sublayer is much smaller than the roughness elements assumed in the models. The micro-scale roughness is included in the model as a boundary condition on the gradient of the turbulent flow velocity (if turbulence is present) within the cells nearest the ground. Flow within this cell is assumed to be logarithmic in form with micro-scale roughness length z0g if the flow is turbulent, otherwise a laminar profile is used based on the viscosity of air. We have not experimented with periodic boundary conditions in the PHOENICS CFD model but will pursue this in the future – thanks to the reviewer for this excellent suggestion. We propose to add the following sentence to section 2.3: "In the CFD model the ground surface is treated using a wall-function approach, i.e. the velocity profile within the first cell is assumed to be logarithmic with a microscale roughness length equal to z0g if the flow is turbulent, otherwise a laminar profile is used based on the viscosity of air"

Q: "*4. The authors should give a precise definition of what they call the 'roughness sublayer'. Is it the same as what is usually called the viscous sublayer, of thickness nu/u_*, where the wind profile is not logarithmic, as described by e.g. van Driest (1956)?*"
A: The viscous sublayer and the roughness sublayer are not the same. Both are regions where mixing-length theory breaks down, but in the first case it is because flow is laminar and in the second case because the structure of the flow is strongly influenced (e.g. via flow separation) by individual roughness elements of the surface.

The roughness sublayer is defined by the American Meteorological Society (http://glossary.ametsoc.org/wiki/Roughness_sublayer) as “The lowest atmospheric layer immediately adjacent to a surface covered with relatively large roughness elements such as stones, vegetation, trees, or buildings. The roughness sublayer extends from the surface up to about two to five times the height of the roughness elements. Within the roughness sublayer the flow is three-dimensional, since it is dynamically influenced by length scales of individual roughness elements and surface layer scaling cannot be expected to apply”. In the discussion paper we defined the roughness sublayer as “the range of heights above the ground comparable to the height of the largest roughness elements.” In the revised manuscript we propose to include the following two sentences to clarify this issue: “The roughness sublayer is the layer where the mean velocity profile deviates from the law of the wall as the flow interacts with individual roughness elements. This layer is typically considered to extend from the ground surface to a height of approximately twice the height of the tallest roughness elements.”

Q: “*5. Fitting z_0: Adjusting a log-profile on the direct data, or fitting a straight line on the log of the data is not equivalent, and the later gives more weight on the data close to the surface. What was the choice of the authors? How were error bars in velocity measurements (or data dispersion due to fluctuations) accounted for in the fitting process?*”
A: Our procedure for fitting the data was described on p. 1116, i.e. “least-squares fitting of the wind velocities to the natural logarithm of the distance above the ground. The shear velocity is equal to the slope of u vs. ln z multiplied by kappa. The roughness length is equal to the exponential of the following: minus the intercept divided by the slope.” This procedure precisely follows Bergeron and Abrahams (1992), equations (6) and (7). We propose to add the following sentences to the revised paper to clarify this issue: “To extract a z_0 value from the velocity profile data, we followed the procedure of Bergeron and Abrahams (1992), who emphasized the need to regress u on ln z rather than ln z on u. The shear velocity is equal to the slope of the regression of u on ln z multiplied by kappa (equation (6) of Bergeron and Abrahams (1992)) and the roughness length is equal to the exponential of the following: minus the intercept divided by the slope (equation (7) of Bergeron and Abrahams (1992)).”

Q: “*6. A few missing references that could be relevant: Taylor et al., Boundary-Layer Met. 1989 Raupach et al., Appl. Mech. Rev. 1991 van Rijn, J. Hydraul. Div. 1983.*”
A: These references will be added to the revised paper.

Q: “*7. Error bars are missing in Fig. 6.*”
A: In the revision we will plot the error bars, but they will be barely visible since the accuracy of the sensors is 4% of the measured value. There is variability in wind speed computed within each 12 s interval, certainly, but the literature indicates that one should not attempt to measure a mean velocity for the purposes of estimating mean velocity profiles using any time interval less than approximately 10 s (e.g. Namikas et al., 2003), as discussed in the paper.

Q: “*8. As the authors discuss, I have a problem with a Fourier analysis of a non-linear problem, and to me it would make more sense to extract a relevant length (for a given problem) from the Fourier spectrum of the bed elevation, and apply a formula like (3), rather than summing up over the whole spectrum like in (4).*”

A: If one accepts that both the amplitude and slope of microtopographic variations influence the effective roughness length (which we showed in Figure 10 for the case of a sinusoid), it follows that there is no single Fourier mode that controls the effective roughness length, unless the topography is a perfect sinusoid (which it never is). This is because the slope is a high-pass filter of the topography (i.e. the slope is proportional to $k*a_n$ where a_n is the Fourier coefficient) and hence is more sensitive to high-wavenumber components of the topography than the amplitude is.

We think our paper demonstrates that one must quantify the amplitude and slope of topography at multiple scales in order to most accurately estimate z_0. Having quantified the amplitude and slope at multiple scales (e.g. using necessarily time-intensive methods such as TLS, structure from motion, etc.), why would one want to throw out most of the data by forcing this multi-scale problem into a mono-scale framework? We appreciate the desire for simplicity (and for honoring the linear nature of the Fourier transform) but the fact remains that the effective roughness length does depend on the shape of bedforms (not just on their amplitude, or wavelength, or maximum slope) (e.g. Lefebvre et al., Flow separation and roughness lengths over large bedforms in a tidal environment: A numerical investigation, doi:10.1016/j.csr.2014.09.001), so it follows that some type of multi-scale representation of the topography is needed to properly estimate the effective roughness length. Also note that our study sites clearly include cases (e.g. Willcox Playa) in which the effective roughness length is not controlled by a single scale of microtopography but rather where two very different length scales contribute equally to the effective roughness length.

Reviewer 2:

Q: "*1- Multi-scale analysis: There is no evidence in the paper that the fact a topography has multiple scales affects the roughness length in any meaningful way. The CFD simulations are run with single scale sinusoidal surfaces, while there is no clear way to measure the contribution to the roughness length from the different scales within the topography. I suggest the authors to use the CFD code to test this hypothesis and run simulations with multi-scale synthetic data (with few sinusoidal modes to get a better picture). That way they could compare the simulations with predictions using either Eq. 3 with an effective amplitude and slope or Eq. 4. Without this basic information any discussion of the effect of multiples scales is merely speculative.*"

A: If one accepts that both the amplitude and slope of microtopographic variations influence the effective roughness length (which we showed in Figure 10 for the case of a sinusoid), it follows that there is no single Fourier mode that controls the effective roughness length, unless the topography is a perfect sinusoid (which it never is). This is because the slope is a high-pass filter of the topography (i.e. the slope is proportional to $k*a_n$ where a_n is the Fourier coefficient) and hence is more sensitive to high-wavenumber components of the topography than the amplitude is.

However, it is straightforward to demonstrate using the CFD model that the effective roughness length is a function of the multi-scale variability of the microtopography. In the revision we propose to use one of the playa profiles (we chose the Soda Lake smooth site but similar results are obtained for other profiles) to compute the effective roughness length with and without smoothing of the topography (see text below). This analysis shows that the effective roughness length decreases as the high-wavenumber variations are removed, i.e. that the roughness length depends on the microtopography at multiple scales.

We disagree with the reviewer's contention that there is "no clear way to measure the contribution to the roughness length from the different scales within the topography." This is precisely what Figure 11 shows, i.e. how different scales within the topography contribute to z0. New text and figures we propose to add:

"To demonstrate the suitability of PHOENICS for modeling atmospheric boundary-layer flows and to establish that the effective roughness length depends on the microtopographic variability at multiple scales, we performed a numerical experiment using the central microtopographic profile measured at the Soda Lake smooth site as input (plotted in Fig. XA). We measured a mean z_0 value of 4.6 mm from velocity profiles at this site. Figure XB presents the velocity profiles predicted by the PHOENICS model for 2D flow over the profile, following the procedures detailed in the Methods section. PHOENICS predicts an effective roughness length of 2.4 mm based on a least-squares fit of the velocity to the logarithms of distance above the ground from a height equal to twice the height of the dominant roughness elements to the top of the model domain. As such, the PHOENICS model predicts a z_0 value similar to the value we measured in the field (relative to the four order-of-magnitude variation in z_0 values we measured across the study sites).

To demonstrate that the z_0 value depends on microtopographic variability at multiple scales, we filtered the Soda Lake smooth profile diffusively to remove some of the small-scale (high-wavenumber) variability while maintaining the large-scale variability (i.e. the root-mean-squared variability of the filtered and unfiltered profiles is identical). Figure X plots the original profile, the filtered profile, and their amplitude and z_{0n} spectra. The z_0 values for the unfiltered and filtered cases are 2.4 mm and 0.15 mm, respectively, based on fitting the velocity profiles predicted by PHOENICS. That is, the filtered profile has a z_0 value more than an order of magnitude smaller than the original profile despite the fact that the amplitude of the large-scale microtopographic variations is the same as the original profile. Equation (4) predicts 2.8 mm and 0.25 mm, respectively, for the z_0 values. The z_0 value decreases in the filtered case because steep slopes that trigger flow separation are significantly reduced at a wide range of scales by filtering, lowering the z_0 value.

The results of this numerical experiment demonstrate that z_0 values depend on variability microtopographic variability at multiple scales. There is also a general theoretical argument that supports this conclusion. If one accepts that both the amplitude and slope of the microtopography influence the effective roughness length (which we demonstrated in Figure 10 for the case of a sinusoid), it follows that there is no single Fourier mode that controls the effective roughness length, unless the topography is a perfect sinusoid (which it never is). This is because the slope is a high-pass filter of the topography (i.e. the slope is proportional to $k*a_n$ where a_n is the Fourier coefficient) and hence is more sensitive to high-wavenumber components of the topography than the amplitude is."

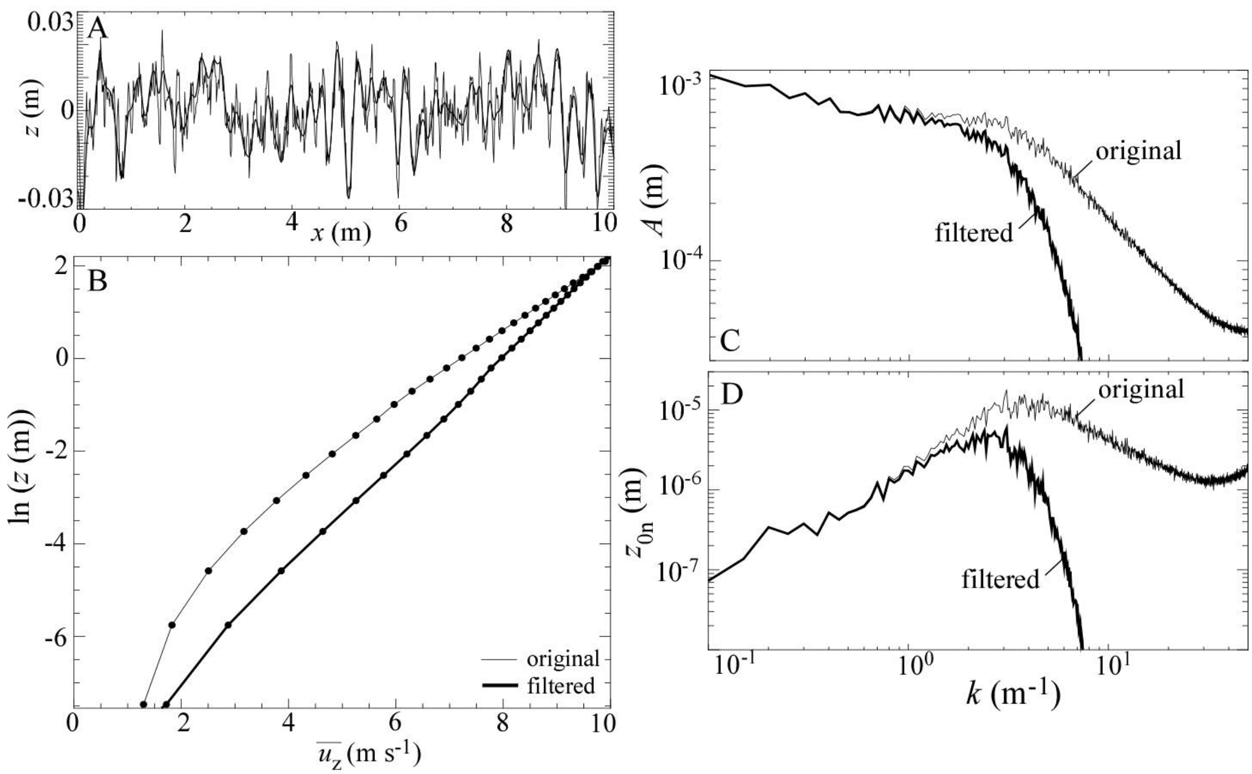

Figure X. Demonstration of the dependence of z_0 values on the multi-scale nature of microtopography. (A) Plot of a profile through the Soda Lake smooth site (thin curve). Also shown is the same plot with diffusive smoothing (thicker curve). Smoothing maintains the amplitude of microtopographic variations at large spatial scales (i.e. the amplitude spectrum is unchanged at large scales) but removes some of the small-scale (high-wavenumber) variability. (B) Plots of the mean velocity profiles predicted by PHOENICS over the original and filtered profile. (C) Amplitude spectra of the two plots in (A). (D) Contributions of each Fourier mode to the z_0 values for the two plots in (A).

Q: "2- *Comparison with field data and interpretation: it is very confusing to use Fourier analysis to make statements about multiple scales because for non-sinusoidal patterns there is no one-to-one relation between the scale of the pattern and a peak in the Fourier spectra. In fact, I don't think it is possible to discuss any 'potential' effects of multiples scales from Figs. 5 and 11, which renders Eq. 4 meaningless when applied to non-sinusoidal surfaces. I suggest using different techniques such as wavelet analysis for this.*"

A: In climate science there is a long history of using Fourier analysis to identify the dominant time scales of climate variability (ENSO, Milankovitch bands, etc.). Similarly, in geomorphology there are many papers that successfully use the discrete Fourier transform to identify the dominant spatial scales of topography (e.g. Perron et al., 2008; Spectral signatures of characteristic spatial scales and nonfractal structure in landscapes, JGR).

The reviewer's point could be that bedforms with the same amplitude and wavelength but different shape have somewhat different power spectra. Specifically, bedforms with higher maximum slopes have larger high-wavenumber coefficients than bedforms with lower maximum slopes of the same amplitude and wavelength. This fact is, however, a strength of our method, not a limitation. Our method predicts higher effective roughness lengths over bedforms with higher maximum slopes (for bedforms of otherwise similar amplitude and wavelength), as indeed they should since the roughness length increases with the extent of flow separation, which is a function of topographic slopes as discussed on p. 1109, lines 24-25.

We also appreciate that the discrete Fourier transform of any finite data set yields an imperfect spectrum (e.g. because spectral power can spread to neighboring frequencies in finite datasets). This limitation can be mitigated by mirroring the data as we discussed on p. 1114, line 27, by collecting more data (e.g. longer profiles), and/or by various tapering procedures.

The primary advantage of the wavelet transform over the discrete Fourier transform is that it handles nonstationarity in data, i.e. it provides a spectral or wavenumber-based representation of the topography in which the magnitude of the coefficients are allowed to vary spatially. The roughness of our study sites is quite homogeneous, however. As such, we don't understand the reviewer's basis for believing that the wavelet transform is inherently superior to the discrete Fourier transform (although clearly it may be superior in cases with significant nonstationarity in microtopographic roughness). In any case both the wavelet transform and DFT involve a linear superposition of basis functions (be they sinusoids, Slepian sequences, Daubechies wavelets, or any other basis function), so the reviewer's concern about the technique as a superposition of a basis function would not appear to be addressed by the use of wavelets.

Q: "*2a- More than a signature of a multi-scale phenomena, the good correspondence between measured values and fitted ones using Eq. 4 in Fig. 12, could be just result of the underlying correlations of the roughness length with H_RMSE and S_av as shown in Fig. 8. The authors could check if Eq. 3 fits the field data with an effective amplitude and slope. Eq. 3 could then represent an improvement over existing formulas but without all the complexities of a multi-scale approach.*"

A: We agree that H_RMSE and S_av are correlated in our study sites but we disagree that this correlation can be used as a basis for developing a mono-scale predictive formula for z0. We believe we clearly addressed this issue on p. 1123 when we wrote "in the playa surfaces we studied, playas with larger microtopographic amplitudes are systematically steeper. We would not expect such a correlation between amplitude an steepness to apply to all landform types because, as microtopography transitions into mesotopography and H_RMSE increases from 0.1 to 1 and higher, slope gradients do not continually steepen without bound. If our goal is to understand the controls on z0 values in landscapes generally, the data in Fig. 8 suggests that it is necessary to quantify the separate controls of amplitude and slope on z0 values."

Another problem with the reviewer's suggested change is that S_av is a function of scale. We quantified S_av at the resolution of our data (0.01 m), but the results would be different if S_av were computed at the resolution of 0.03 or 0.1 m instead. Future users of our method might not have data at 0.01 m resolution, hence we would have to come up with empirical formulae for S_av values computed at different resolutions in order to propose a general method. In short, it is impossible to avoid the multi-scale nature of the problem. We propose to make no change to the paper on this point.

Q: "*3- Validation of the CFD model: It is not clear to me that the CFD code is actually able to reproduce the real trends of the measured roughness length on different surfaces. Why not use the measured microtopography to run the model and compare? Even a qualitative comparison will strengthen the argument of the CFD model as a tool to develop expressions for the roughness length.*"

A: The PHOENICS model has been the source of many CFD studies of atmospheric boundary layer flows since its original formulation c. 1974. PHOENICS is the engine of WindSim, which is a leading code for boundary-layer flow modeling in wind engineering applications. As such, the model has been successfully used for atmospheric flows for over 40 years.

That said, it is straightforward to include the results of CFD model runs over one of our actual surface profiles to demonstrate that it predicts a similar roughness length to that we measured. See Figure X (proposed new figure for the revision) and associated text above.

Q: "*Figure 6: please plot wind velocity u vs elevation z in a semilog plot without rescaling the wind velocity. That way the interpretation is straightforward: slope is proportional to u* and the virtual crossing of the z-axis at u=0 by the prolongation of the log-profile is z_0.*"
A: We believe that normalizing the velocity in the way that we did in Fig. 6 best fits the goals of our paper, which is to quantify z0 not u*. The problem with plotting the data without velocity normalization, as we clearly stated on p. 1121, lines 18-20, is that the absolute velocity depends on the windiness of the days we happened to be in the field at various sites. As such, plotting the data without normalization introduces variability into the plots that has nothing to do with z0 but instead is primarily a function of the weather on those days. For this reason, we propose to make no change to the paper on this point.

z0 is defined as the distance above the bed where the velocity goes to zero. Since zero times any number is zero, clearly the velocities can be scaled up or down with no consequence whatsoever for values of z0.

Q: "*End of page 1121: The description of Fig. 6 is wrong, a steeper slope doesn't necessarily correspond to a smaller z_0. Please correct with the new version of Fig. 6 (see above).*"
A: A steeper slope does necessarily correspond to a smaller intercept (and hence z0) when the velocity data are normalized as in Figure 6. To see this, consider the equation for a line: y = mx+b. If the values of y are normalized such that y(x0) = 1, where x0 is a constant, it follows that the slope of the line, m, is equal to (1–b)/x0. That is, a steeper slope necessarily corresponds to a smaller intercept (hence z0) value.

We agree that the two sentences on p. 1121, lines 22-23, are not precisely correct as written. We wrote "The law of the wall predicts a constant slope when u is plotted vs. ln z. A steeper slope corresponds to a smaller z0 value." We should have made clear that the plots of u vs. ln z we were referring to were those normalized as in Fig. 6. In the revision we propose to write "The law of the wall predicts a constant slope when u is plotted vs. ln z. When the velocities are normalized as in Figure 6, a steeper slope corresponds to a smaller z0 value."

Q: "- *In Eq. 4, is the constant z_0g fitted to the field data?*"
A: As stated on p. 1118, line 8, "The value of z0g was chosen based on the measured value of z0 at the two flattest sites (Lordsburg smooth and intermediate), both of which yield z0 = 0.002 mm as described in Sect. 3.2." We believe the discussion paper was clear on this point, so propose to make no change to the revised paper.

Q: "- *The definition of S_av is not clear, why not use the RMS of the gradient of the topography or an alternative definition that doesn't not involves defining any thresholds?*"

A: We defined S_av on p. 1114, line 18, as “the average slope of the topography computed at the 0.01 m scale” (i.e. the scale of the DEMs created from the TLS data). No thresholds are involved. We believe this definition is clear, so we propose to make no change on this point.