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Earth Surface Dynamics Discussions

Interactive comment on "Predicting the roughness length of turbulent flows over landscapes with multi-scale microtopography" *by* J. D. Pelletier and J. P. Field

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The authors discuss how to estimate the effective roughness length z_0 involved in the logarithmic law of the wall. This work is based on field data recorded in ten playa sites, as well as on numerical data obtained from computational fluid dynamics (CFD). The field data are composed of measurements of the topography on a 10m x 10m area, with a vertical precision on the order of a mm, and of time series of wind velocities at different heights above the surface (from 0.01 to 2.8m). In the CFD, turbulent flows modelled by a k-epsilon closure are run over a sinusoid of given wavelength, amplitude and surface roughness. The authors show that z_0 can be estimated by an empirical formula (Eq. 3).

This work is interesting and the combination of field and numerical approaches is valuable. I have, however, a certain number of reservations, listed below, that prevent me to recommend publication of the manuscript at this stage.

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/\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.

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?

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?

4. The authors should give a precise definition of what they call the 'roughness sub-layer'. Is it the same as what is usually called the viscous sublayer, of thickness $\sim$10 nu/u_*, where the wind profile is not logarithmic, as described by e.g. van Driest (1956)?

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?

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

7. Error bars are missing in Fig. 6.

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).

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