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Atmospheric Measurement Techniques Discussions

Interactive comment on "Comparison of satellite microwave backscattering (ASCAT) and visible/near-infrared reflectances (PARASOL) for the estimation of aeolian aerodynamic roughness length in arid and semi-arid regions" *by* C. Prigent et al.

Anonymous Referee #2

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This article presents a technique to measure the aeolian aerodynamic roughness length based on a joint analysis of microwave and shortwave satellite observations. In this regard, the ASCAT sensor is looking through the atmosphere and would then provide permanent information despite a low spatial resolution (∼25km). On the other hand, PARASOL observations offer a better spatial resolution (∼6km) but are more scarce due to atmospheric contamination (clouds plus aerosols for investigated areas).

I am somewhat surprised that it is being considered for Atmos. Meas. Tech. and as such I would have expected to be directed towards sister journal more devoted to surface processes. But this is certainly not crucial although potentially interested persons may not visit AMTD webpage.

I found the objectives were well stated, the state of art is well reviewed, the methodology is sound and results are promising. Besides, the standard of English is quit correct. Some sentences do not bring new insights and could be removed as suggested hereafter. I provide below some additional comments that may lead to improve the readability of the paper. My overall recommendation is therefore minor revisions of the manuscript. Abstract – It would be already relevant to indicate here the spatial resolutions of the two instruments and the re-sampling strategy adopted. Besides, I believe the respective time of revisit may also be worth mentioning here. The last sentence could be removed.

Section 1. – Some comments here. The aeolian aerodynamic roughness places a regard to dry (or wet) conditions while the aerodynamic roughness length is a more abstractive notion, hard to directly measure in general. The link between these roughness quantities is somewhat the lead of this article and more clearness may be suitable at this stage. Besides, the surface roughness in l.25 seems to be defined as an 'optical roughness' representing the characteristics (height/distance) of dominant obstacles (protrusions). These latter will impact on the signal, this depending on the sensor resolution for sure, as discussed on p.2935 (l.5-9). In p.2936 (l.6), it is discussed the effects of aerosol. I would like to add that following an aerosol event, the dust deposit at the surface may smooth the surface roughness (this depending on wind) while remote sensing studies could still interpret this as aerosols being still in suspension. In this regard, it would be interested to check the occurrence of an aerosol episode considering for instance MACC re-analysis (http://data-portal.ecmwf.int/data/d/macc_reanalysis/) in areas where satellite observations fail to deliver an aerosol product. Incidentally, a soil wetness index may be useful to interpret the roughness variations as a whole.

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Thus, more constraints in the analysis would strengthen the outcomes of the study. L.20 has a typo ('the' appears twice). What do the authors mean by 'practical relationship'? Is it in the sense of 'operational'?

Section 2. – L. 2938, p. 2938: worth mentioning that dunes patterns may not be stationary at the pixel resolution of 6 km. I believe some earlier studies with POLDER (1) were precisely devoted to identify steady targets for vicarious calibration. (1) Cosnefroy, H., M. Leroy, X. Briottet, 'Selection of Sahara and Arabia desert sites for the calibration of optical satellite sensors', Remote Sensing of the Environment, 58, pp. 101-114, 1996. The selected area labeled c) seems to belong to a vegetated zone (see NDVI in Figure 2 for instance). Comment on statement P. 2939, l. 11 : BRDF model of Eq.(1) is well calibrated provided backward and forward observations are handled because of enhancement of shading effects, which yields the baseline for measuring the k1/k0 coefficient. What is in fact the estimated size scale for z0 estimates? P.2940, l. 28: I believe that the sentence "Xian et al. . . . mountain environments" could be removed as it does not bring particular information here.

Section 3. – I wonder the reason to restrain observations to an angle of 45° (P. 2940, l. 2) when angles down to 35° could be also considered. Has the incidence a strong impact on the linear fit between backscattering coefficient and incident angle? In Figure 4, the re-sampling strategy is not quite clear. Does initial ASCAT map at 25 km re-sampled at 6km on PARASOL grid, or conversely? I believe that the benefit of Eq.(2) is that it merges roughness information occurring at different scales. In theory, PARASOL should provide a roughness for coarse scale – driven by shadowing merely – whereas micro-scale roughness seems to be more the focus with ASCAT. I would assume this latter is indeed the more important in regard to the cohesion of particles versus the potential aeolian erosion.

Section 4. – I believe that a topographic map would help to understand the threshold applied to mountainous areas. There, it is likely that k1/k0 would reflect the orography merely. P. 2944, l. 9: I do agree with the authors' comment. Likely, the environment

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factors that could affect the measurement of k1/k0 over bare areas are recent dust deposit, soil wetness and moderate-scale topography.

I suggest two additional papers that also searched on a determination of a roughness parameter from a measurement of the reflectance:

Lettau, H., Note on aerodynamic roughness-parameter estimation on the basis of roughness-element description, Journal of Applied Meteorology, 8, 828-832, 1969.

Roujean, J.-L., D. Tanré, F.M. Bréon, et J.-L. Deuzé, Retrieval of land surface parameters for GCM from POLDER bidirectional measurements during HAPEX-Sahel, Journal of Geophysical Research, 102 (D10), 11,201-11,218, 1997.

Interactive comment on Atmos. Meas. Tech. Discuss., 5, 2933, 2012.