Atmos. Meas. Tech. Discuss., 5, C2080–C2085, 2012
www.atmos-meas-tech-discuss.net/5/C2080/2012/

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.

C. Prigent et al.

catherine.prigent@obspm.fr

Received and published: 5 September 2012

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. >We will make sure to advertise this paper in the community, outside the reader of Atmos. Meas. Tech. Note that the data set has already been solicited by Atmos. Meas. Tech. readers and has been distributed, since this discussion has been posted.

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.

>Thank you to the reviewer for his positive recommendation.

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.

>The spatial resolutions of the instruments have been added. The revisit time for both instruments is twice a day roughly, but due to atmospheric contamination, usable optical observations are much less frequent. The last sentence is important to us as this dataset is intended to be distributed to a large modelling community.

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.

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’?

>We are aware of the different definitions of the roughness length, and of the difficulty to find a clear and convincing description that can 1) represent what the users need at various scales 2) be consistent with what is measured by the different instruments. We discussed these definitions with dust modellers, as well as with land surface modellers: these discussions proved that it was not easy to reach a consensus between the different users and we tried to report in the paper the summary of these discussions. As in our previous study (Prigent et al., JGR, 2005), we propose a ‘practical’ solution that can be used by dust modellers, for both local and global applications. In that sense, our approach is also operational. Dust events, as well as rainy events can modify the roughness length at short time scale. In this study, the objective is to derive the mean value of the roughness length, as averaged over a full season. We fully agree with the reviewer that detailed analysis of some locations that ae likely to exhibit temporal variability would be very interesting. This will be the object of future studies. The typo

(two ‘the’) has been corrected.

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).

> We fully agree that some variability is expected in the arid and semi-arid zones, both in terms of dune structures as well as in terms of vegetation. Our intent n this study is to provide modellers with averaged roughness estimates at global scale. Detailed studies at regional / local scale could be performed in the future to analyse the impact of these changes.

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?

> The estimation of the roughness length from k1/k0 is based on the shadowing effects that element such as rocked and pebbles can have on the surface. We expect these elements to be of the order of a few centimeters and more.

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.

>This sentence shows the difficulty to find in situ roughness length measurements that are compatible with the satellite observations. We will keep it.

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?

>ASCAT does not observe each location with a given angle frequently enough to get a robust estimate of sigma0 for this angle. We use all observation angles with ASCAT to derive a linear relationship between the sigma0 and the angle. Then, we use only the interpolated value at 45° for practical reasons (selection of another angle would not change the results, as the regression is linear in angle).

In Figure 4, the re-sampling strategy is not quite clear. Does initial ASCAT map at 25 km resampled 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.

>The initial ASCAT maps are resampled at 6 km, on the PARASOL grid. This is mentioned on the paper: 'The ASCAT data is projected onto the PARASOL grid, using distance-weighted means to the closest neighbors.'

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.

>The observation wavelength for ASCAT is roughly 6 cm. It is sensitive to centimetre scale roughness elements. Given the high correlation (0.91) between the z0 derived from PARASOL and ASCAT, it is likely that they are sensitive to similar roughness scales.

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.

>The topography map has not been presented on the study, but we used it in the analysis. It shows that both observations (visible and scatterometer) are very sensitive to the topography, with the major topographic structures clearly observables on the

maps on Figure 1 A and D, and are excluded from the z0 maps on Figure 6, as they are related to high z0 values. A comment has been added in the text, in section 4.

P. 2944, l. 9: I do agree with the authors' comment. Likely, the environment 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.

>These references have been added to the text, in the introduction.

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