

Interactive comment on “Surface soil moisture estimates from AMSR-E observations over an arid area, Northwest China” by L. Wang et al.

Anonymous Referee #1

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General

With great interest I reviewed the paper on “Soil Moisture Estimates from AMSR-E observations over an arid area, Northwest China” by Wang et al. Unfortunately the paper is not well written and it takes quite some time to fully understand the methodology. As far as I understand the concept of the approach, it is more or less a simple version of the Land Parameter Retrieval Model (Owe et al., 2001, Owe et al., 2008), where they calibrate the roughness over the bare soil sites and implement it over the rest. What worries me is the simple parameterization of the vegetation optical depth and the lack of explanation why we should use this new model in stead of using soil moisture from one of the other existing models. The authors should at least demonstrate why we should use their model, and how their model compares to the existing ones. Therefore

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I do not recommend this paper for publication, but encourage the authors to resubmit the paper after major revisions.

Details,

Page 1057 There are many quotes in the introductions without references, please check this (eg.page 1058 line 10: The effects..... the surface. and at line 20 At present. . .some conditions).

Page 1058 Jackson et al uses ancillary vegetation data (LAI) in their single channel algorithm. This should be added, because this is an important difference compared to the other algorithms

Page 1059 Line 7. Here you should start another section Line 14; It is not clear why you should develop a specialized model. We already have a series of models, so please explain why you want to develop a new one.

Page 1064 Line 20 In equation 11 there are three parameters unknown: two roughness (Q, and h) and soil moisture. But what about tau ????. Is this because you only want to apply it for bare soils? Please explain.

Page 1065 Line 2: Do you really mean soil temperature?? Or do you mean soil moisture. If you mean soil temperature, please explain how you obtain this, and why (because suddenly you have an additional unknown parameter).

Line 20: Figure 2a is a copy of figure 5 of the paper of Owe et al., 2001. Please refer to this paper at this figure.

Page 1066/7 This part is really not clear and should be rewritten; This is what I understand so far; 1) You make a minimum MPDI map of 2005 2) You compute h from the highest MPDI values (>0.04) with a fixed soil moisture content of 5.5 Vol. % and I assume that you neglect the optical depth ($\tau = 0$??) 3) This gives you an h value of about 0.73

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But how can you obtain h values for the rest of the region. Do you ignore τ for the rest of the region as well? And it is also ill posed to assume a fixed soil moisture content for the entire region. It might be better to use the sand and clay content info to get a better estimate of the spatial distribution of your low soil moisture values.

Page 1067 Line 25. "greater stability of night time surface temperatures" I don't understand this quote, because you use MPDI and this should work at daytime as well because the MPDI minimizes the temperature effect. So what is the real reason to choose for night time observations?

Page 1068 In the implementation it is not clear how you are dealing with τ . Do you solve for it (which might be difficult because it is an extra unknown) or do you give it a fixed value. Please explain.

Page 1069 The validation activity would become much stronger if other satellite data sets would be involved. This would value your methodology. Now we basically don't know how good this new methodology is.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 6, 1055, 2009.