We wish to deeply thank the referee for his or her review. The comments are particularly valuable given his deep knowledge of the subject of the paper. I will consider with particular care any additional suggestion that the referee may offer. The referee's Comments are in blue, and the author's responses are in black.

General

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.

Reply：The major criticism of the referee is related to a lack of innovation and the referee think the model we devolved "is more or less a simple version of the Land Parameter Retrieval Model (Owe et al., 2001, Owe et al., 2008)". Actually, we think the referee did not fully understand our model partly because of being not good for our writing.

First，let us review the literatures by Owe et al. (2001,2008). A key to their Land Parameter Retrieval Model (LPRM) is deriving surface temperature using from 37 GHz vertical polarization brightness temperature observations. Owe et al. indicated: "Daytime emitting layer temperatures are often more difficult to estimate because of more intense surface heating. While this (here refers to "deriving surface temperature") is a significant problem in arid and semiarid locations." They are right. Moreover, the "significant problem" also HEAVILY occurs during night time over our study area. This is why we did not use the LPEM.

Second, "the other existing models" need more or less ancillary parameters or information, which we have done our best to avoid. We readily met the challenge (using least ancillary parameters). In our model, only global soil texture data (Webb et al.,1991) besides the AMSR-E brightness temperature measurements was used. This is distinctive compared to others.

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).

Reply 1： Thanks! We will check and complete.

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.

Reply 2： It is good suggestion. We will add.

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.

Reply 3： It is good suggestion. It is really important to this paper, and we will complete it.

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.

Reply 4： The "tau" is "solved" in equation 6 (Page 1063) in advance. Please refer to Meesters et al. (2005). It is critical for parameterizing "tau".

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).

Reply 5: Deeply sorry! A serious mistake. "Soil temperature" should be corrected as "soil moisture"

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.

Reply 6： Sorry, it is not a copy of Owe et al., 2001,although there are very similar. They are simulated from different radiative transfer models. For example, Q parameter was considered in Figure 2a but did not in Owe et al .

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

Reply 7: Please see **Reply 4**. We have realized our paper was not well written so led to some serious misunderstandings. We now answer above comments by point to point: 1) Yes. 2) No. We compute h from the *lowest* MPDI values (>0.04) with a fixed soil moisture content of 5.5 %. In this condition, tau naturedly tends to zero. 3) Not right. h value of about 0.73 is the highest h value over bare soil.

But how can you obtain h values for the rest of the region. Do you ignore tau 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.

Reply 8: For the rest of the region, considering large areas of high mountain where soil

moisture can not be estimated due to snow cover etc. and the continuity of the h distribution, the h values is set to 0.6. We tested the h values ranging from 0.2 to 1.3 for the no-bare region for deriving soil moisture, and the value 0.6 for h was best. So 0.6 is empirical in sense. Although we have realized that uniform value for the rest region is unrealistic, there is no other better method to address this issue. Any suggestion? Given relative small area contributing to entire soil moisture calculation, we think this assumption (h=0.6) is acceptable.
Tau is never neglected though our paper.
The sand and clay content info was used in our paper (see Page 1068 Line 10-12) .

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?

Reply 9 : Page 1062 equation 1 is based on this assumption: surface temperature is equal to canopy temperature. MPDI used here is calculated from equation 1 (dual polarizations). Nighttime soil, canopy are usually quite stable, in other word, there are considered equal, meeting equation 1.

Page 1068 In the implementation it is not clear how you are dealing with tau. 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.

Reply 10 : Please see **Reply 4** and **Reply 8**.

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.

Reply 11 : Good suggestion. We agree with the referee and try to find a good data set to value our methodology. This new methodology uses least ancillary information, and can reach relatively high precision.