

The authors would like to thank Mr. Narayan for carefully examining our paper and providing us a number of important comments. We will consider with particular care any additional suggestion that Mr. Narayan may offer. We would like to reply to his comments on some points.

Major concerns

1. The reviewer thinks the paper is difficult to understand as the key element of treatment of vegetation optical depth in the actual soil moisture retrieval step is unclear. He suspects that vegetation opacity in our paper has been coupled with the surface roughness parameter 'h'. So he concludes "it is incorrect to take 'h' as annually invariant since the authors have focused on the growing season and during the growing season vegetation varies considerably in its structure, leaf area index and spatial extent. Thereby a uniform 'h' value for the entire year does not make sense".
2. It is wrong that a fixed value 0.6 of h parameter is set for steep slopes.
3. The validation is insufficient.
4. The vegetated / bare region classification using MPDI should be validated using MODIS or such other land cover datasets.
5. Some specific errors

Reply

1 & 2. We think the reviewer did not fully understand our model partly because of being not good for our writing. Vegetation opacity (τ) in our paper expressed in equation 6 (Page 1063) is function of soil moisture and h, according to equation 5 and 8. In other word, if h and soil moisture are known the τ is solved.

For bare soil ($MPDI > 0.04$), we derive h from the lowest MPDI corresponding to assumed soil moisture 5.5% according to equation 12 by a iterative program. For a pixel, we think h may be regarded as fix value for a pixel during a year (April-October), but τ is variable because soil moisture is varying from in equation 6 (Page 1063) no matter for bare soil or for vegetated soil. This point is critical for our paper. The reviewer can refer to Meesters et al. (2005).

For no-bare soil ($MPDI < 0.04$), 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.

3. Yes. We will use other satellite data to revalue our product.
4. It is a really important suggestion. We will validate the vegetated / bare region classification using MODIS or other land cover datasets.
5. In the revised manuscript, several technical corrections will be done.