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Hydrology and Earth System Sciences
Discussions

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Interactive comment on "Estimation of subsurface soil moisture from surface soil moisture in cold mountainous areas" *by* Jie Tian et al.

Anonymous Referee #2

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General comments: This manuscript describes a wide-variety of approaches to estimate subsurface and profile soil moisture from surface soil moisture data in the Qilian Mountains of China. Most of the conclusions are well-supported, but the manuscript suffers from statistical inconsistencies. A consistent set of statistical measures should be maintained throughout the manuscript (NSE, RSR, and R). The only exception could be Fig. 10 where the aim is to compare the SMAP ubRMSE to the mission's accuracy requirement. Also, 30% of the data should be withheld for validation for all three methods, not only the ANN method. The performance of the SMAP-based ExpF method for estimating profile soil moisture (i.e., SWI) is overstated, and including the NSE and RSR statistics will likely provide a much more objective view. The manuscript also suffers from poor organization in some places and is not well-written.

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Discussion paper

Specific comments: 1. line 126. Clarify the meaning of “to consider the assumption of uniform vertical profiles of soil temperature and soil dielectric properties”.

2. Table 1. Results are being reported with too many significant figures. I doubt that the laboratory is able to measure sand and silt content with adequate precision to justify four significant figures. Reduce to a more appropriate level, perhaps three significant figures.

3. Section 3.4. Provide more explanation. Was a separate ANN model developed for every depth combination and every site?

4. Equation 6 is the wrong equation.

5. line 196. not “persistent” but “consistent”

6. line 205-208. This should be moved to the methods section.

7. line 257. You have not provided any convincing evidence that “For most hydrological researches, the correct temporal variation of SM is more crucial than the exact value, suggesting that more emphasis should be given to R when selecting the most appropriate estimation method.” You have presented three statistical measures to evaluate these methods (RSR, R, and NSE). For two out of the three statistical measures (RSR and NSE) the ANN method had the best performance. Therefore, you should include a clear statement that the results from the ANN method were statistically superior to those from the other two methods. You are still free to prefer the ExpF approach if it is simpler to apply than the ANN method. Just don’t try to justify that choice on a statistical basis.

8. Figure 7a is unnecessary and should be deleted. Your results show that “Year” does not have a significant effect, so the data should be presented including all years as done in Fig. 7b.

9. line 290-297. This should be moved to the methods section.

10. line 298-305. This section is not convincing. How strong is the correlation between ln-transformed LAI and precipitation? Perhaps the apparent relationship between Topt and LAI is a spurious result of the correlation between LAI and precipitation.

11. line 319-322. Move to methods.

12. line 319-322. What steps were taken to prevent problems due to collinearity of the predictor variables?

13. Table 4. Is “ln ln (sand)” correct in the last row?

14. Fig. 9. Present RSR instead of RMSE to be consistent with the rest of the manuscript.

15. line 380. Not “persistent” but “consistent”.

16. Section 4.4.1. You should note an important limitation of this analysis. There is a huge scale mismatch between the 9 km SMAP data and the in situ sensors which measure at a single point. This will likely degrade the agreement between the two data sets.

17. line 394-399. Move this to methods.

18. line 394-399. Why did you even bother all the effort to determine Topt from the in situ stations in the prior sections? Now you are not using those Topt values but instead finding new ones based on comparison of the SMAP data with the in situ data. This does not make sense in the flow of the manuscript.

19. line 400-406. Include NSE and RSR measures here. They are crucial for quantifying the mismatch between the SMAP SWI and the observed SWI values as shown in Fig. 11.

20. line 425-428. This point should also have been made in Section 4.4.1.

21. Tables 5 and 6. Replace RMSE with RSR. Add NSE.

22. line 436. Your results (Fig. 11) show that the performance of the SMAP profile SWI estimates is relatively poor. This is being partly obscured by the omission of the NSE and RSR statistics.

23. line 451-452. Here on the 22nd page of the manuscript a completely new data set is introduced. This is inappropriate. If this section is important to the manuscript, then take the time to justify it in the introduction and describe it in the methods.

24. line 465. Interpolated how? What evidence do you have that the interpolation is statistically valid? What is the associated uncertainty? This again should be justified in the introduction and described in the methods.

25. line 465. Also, why bother to spatially interpolate Topt? You have just argued that Topt defined in one region (Heihe) is valid in another region (Maqu).

26. line 495. The data in Fig. 11 show that the accuracy is relatively poor. Relying on the R value alone is clearly misleading in this case where there is a substantial bias. Including the NSE and RSR as suggested above will likely show that the performance is not very good.

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