Author Response to RC1

Journal: ESSD

Title: A 1-km daily surface soil moisture dataset of enhanced coverage under all-weather conditions over China in 2003-2019

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General Comments:

"This paper proposes fine-resolution surface soil moisture (SSM) data over China. The significance and potential impact are clear, and the novelty and results are promising. However, a major revision is needed to address my concerns."

Response:

All authors greatly appreciate you for your constructive comments that have helped improve our paper. We have paid great attentions on each bullet pointed out by you and have modified our paper carefully based on your comments. Please see the following responses to your specific comments and technical comments.

Response to specific comments

1) *"Line 236: About the LST validation:*

Aqua nighttime passing time can be 1-hour away from 1:30 am LT. Even nighttime LST does have small variations than daytime LST, can you find some sites with minute-level observations in China to prove that using ground observations at 2:00 am introduces little uncertainty to the validation results?

Besides, the 0-cm ground temperature is different from LST physically, especially over vegetated areas, where SSM estimation by LST may have considerable uncertainty. Over these places, LST is closer to vegetation canopy temperature (air temperature).

To address my two concerns above, I would recommend including a brief test in the discussion by using site-measured LSTs that are computed by surface upward longwave radiation and BBE, and it would be convincing to include sites over various land cover types."

Response:

Thanks for your suggestion. We have added a new Section 4.2 to discuss this question as well as your Questions 3 and 5 below. For the current discussion, please see Lines 669-686 for details. Basically, we cannot use flux towers to substitute validation data derived at meteorological stations because the spatial density and temporal coverage of the former dataset are not adequately high to represent the entire China. However, following your suggestion, we have implemented the test to address these two concerns in the added Section Appendix C.

In Appendix C, we selected 4 extra flux towers where long radiation observations are publicly available for a comparison with our 0-cm ground temperature, based on your suggestion. The minute-level LST of these towers between 1:00-2:00 A.M. are stable and consistent with the night-time 0-cm ground temperature at meteorological stations.

But one point we need to stress is that the meteorological sites are all located "under open environmental conditions with relatively lower fraction of tall trees and water bodies"(see Lines 677-679 in Section 4.2), according to the official regulation of the National Meteorological Administration of China. Also, it is difficult to find flux towers paired with meteorological stations over densely vegetated regions. Instead, the 4 towers are all located within grasslands across the country.

Besides, we have also re-checked the overpass time of MODIS LST product. The extreme time deviation from 1:30 A.M. can be about 15-20 minutes in our study period and region, not as large as one hour.

2) *"is there any evidence to prove the rationality of '7x7' and '-5$_{th}$ to 5$_{th}$'? "*

Response:

These two values have been actually determined as the optimal ones based on our test and evaluation against in-situ data from a collection of values. We have revised the paper by adding this description. Please see Lines 360-368.

3) *"Fig 3: Clear bias is still shown in filled LST results (Fig 3b) compared to the clear-sky validation (Fig 3a). Will it affect the SSM estimation when clear-sky (unbiased) and filled LSTs (biased) simultaneously exist in a spatial window using the 'universal triangle feature' or in SEE calculation?"*

Response:

Thanks for this comment. There is indeed inevitable influence for such clear-sky-to-cloud mixed windows when we intend for a dataset of quasi-complete coverage. Based on your question, we have added a brief discussion on this in the new added Section 4.2. Please see Lines 657-668.

In summary, such influence implies that the actual difference between SSM downscaling results at cloudy and at clear-sky conditions may be larger than "0.056 vol/vol VS 0.053 vol/vol". But overall, it should not affect the main features of the proposed product (e.g. the better performance of the STDF-derived LST in downscaling cloudy SSM compared to the bias-adjusted one). Also, such possible sacrifice for accuracy of clear-sky SSM in the clear-sky-to-cloud mixed windows can make the product accuracy more consistent between cloudy and clear-sky conditions. This is beneficial to wider application of the product in future studies.

4.1) "*Fig 5: After readers notice the clear differences between two data at some locations (Fig 5a&b), they may want to know which data is more accurate.*
In order to address this concern, you may need to focus on the sites over these regions, where the proposed data have considerable differences with SMAP-Sentinel (e.g. far northeastern, northern west, southern provinces near the sea), specifically and separately, rather than just over entire China (Line 499). "

Response:

We had actually carried out such analysis which is consistent with your suggestion. In this regard, we produced a map for demonstrating all available validation sites in terms of the direct ubRMSD difference (at each site) based on ubRMSD of SMAP-sentinel data minus that of the proposed product. From the map (see the Fig.1 below), however, we cannot find significantly different regional (e.g. between the northeast and the southwest) patterns of the "ubRMSD difference". As a consequence, we decided to maintain the current validation strategy for our paper. Detailed reasons are as follows:

(1) First, from the map below we can see that the validation sites are not evenly distributed across the country, especially considering the much smaller number of sites in the southwestern part. This makes it difficult to make a fair comparison for different sub-regions.

(2) Second, it is important to notice that validation of remote sensing soil moisture based on site measurements actually evaluates the similarity of the "trends" in both the spatial and the temporal dimensions between remote sensing and in situ data. But for the sub-regional validation, we can only evaluate the site-based temporal trend or spatial trend at a much smaller spatio scale but have to abandon the national-scale spatial trend which is especially important. This indicates that the overall validation across the country can be a more comprehensive and more fair validation strategy.

(3) The main object of our paper is to develop a product of higher temporal resolution, higher coverage and higher accuracy than current data (SMAP-Sentinel). As with comparing the detailed qualities of different data products in different sub-regions, the map (in Fig.1 below) indicates that the inconsistent performances of the products cannot be simply ascribed to their differences on geographical locations or climatic regions. In reality, as the basic theories, data inputs, mathematical algorithms, and uncertainty sources differ completely between SMAP-Sentinel combined and PM-optical-data combined downscaling frameworks, the complexity of this issue may be beyond the center topic of current study and need to be investigated specifically in the future.

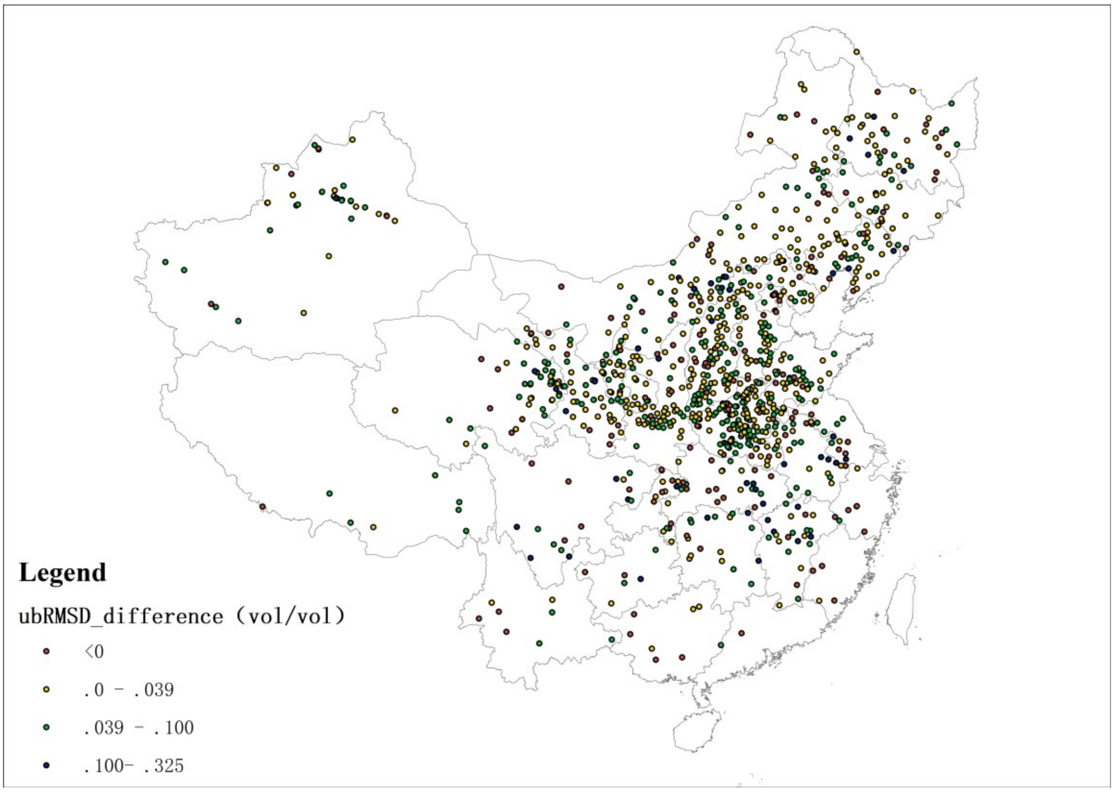

Fig. 1 The spatial distribution for Difference of single-site-based ubRMSD of SMAP-sentinel data minus that of our proposed product ($ubRMSD_{sentinel\text{-}smap}$ - $ubRMSD_{proposed}$), corresponding to Fig.5 in the paper. Sites with samples less than 20 for one year are excluded.

4.2)*"Besides, SMAP shows a very good accuracy (Fig A1a) while the downscaled SMAP-Sentinel (Fig 5c) has large (nearly doubled) ubRMSD. Can you explain why the accuracy is considerably decreased after downscaling?"*

Response:

According to the authors of the SMAP-Sentinel product (Das et al., 2019), uncertainty of this product includes that from its ancillary datasets, the optimization process on its model coefficients, as well as the increased speckle noise introduced when the spatial resolution of Sentinel-1 data is enhanced from 9 km to 1 km. Therefore, the authors of Das et al. (2019) comment that there is "tradeoff between adding spatial resolution with C-band SAR data and noise-levels". This can explain why SMAP-Sentinel has larger ubRMSD than SMAP data. This result is also supported by another previous evaluation study (Mohammad et al., 2018).

On the other hand, we understand that you may have concern on the result that the SMAP-Sentinel based ubRMSD is nearly doubled after downscaled. However, it is important to notice that the analyses in Appendix A and in Fig.5 are not based on the same numbers of validation sites. In Appendix A we only employed quite a small portion of the sites in only 53 microwave 36-km grids because only these sites have the qualified distribution density for representing the microwave grids. As these sites are mostly distributed in plain regions (see Fig. A1), there is a chance to further enlarge its performance difference with the SMAP-Sentinel based result because the latter is evaluated based on a much larger number of validation sites. As a conclusion, we can compare the performance difference between SMAP-Sentinel and our proposed data in Fig.5 as they are based on the equivalent sampling size, whilst it is more or less not fair to quantitatively analyze the decreased ubRMSD of

SMAP-Sentinel data against that of SMAP data between Fig.5 and Fig.A1.

5) *"Appendix B: It's strange that filled LST with considerable bias (-1.7 K) can achieve better SSM accuracy (0.058 vol/vol) than the SSM (0.064 vol/vol) from more accurate/realistic cloudy-sky LST in Fig. A3, and such accuracy difference is even larger than its difference with the clear-sky SSM (0.053 vol/vol, LST is unbiased). If that is the case, the logic behind it is that SSM is not sensitive to the LST, which is not right.*

Besides, the LST bias explanation in Lines 309-311 is not convincing: if the filled LST has clear bias compared to site observations, it only means it cannot reflect the realistic surface condition."

Response:

We basically accept your comment that Lines 309-311 is not convincing enough. Now we have modified and moved these sentences to Lines 634-668 in the new added Section 4.2 as a better and more open discussion. In brief, the STDF-derived LST under cloud with clear bias may not be suitable for all cloudy conditions, especially we agree with you that it is not suitable for rainy cloud. However, we argue that it can explain at least a substantial part of the non-rainy cloud condition. For the bias-adjusted cloud gap-filled LST, although it is better in reflecting the realistic surface condition, such mechanical relationships among cloud, LST and SSM can be beyond what has been described by the UTFS theory which was originally proposed for clear sky only (see the Fig.2 below for illustration).

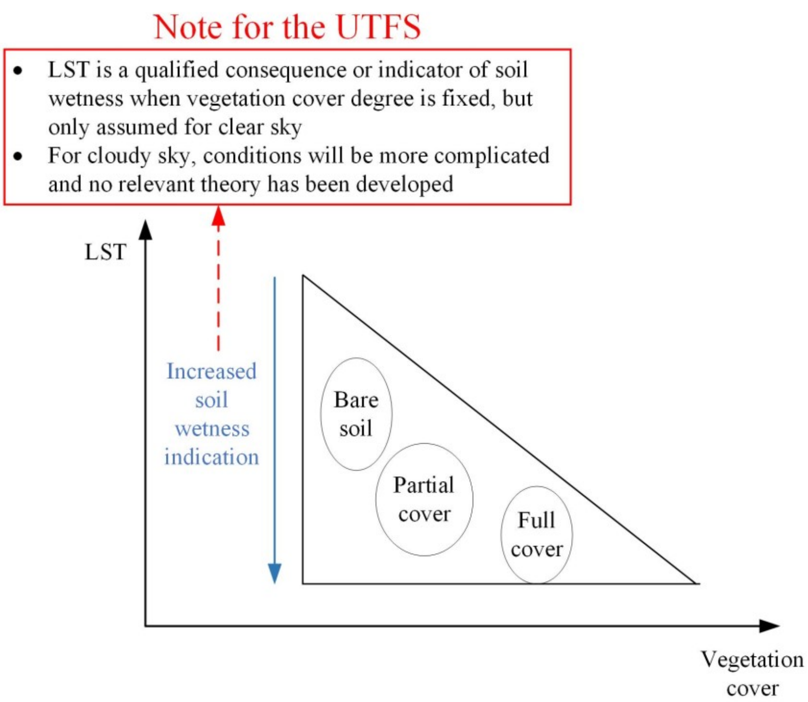

Fig.2 Illustration of the UTFS theory under clear sky

In our revised discussion, therefore, the higher ubRMSD of STDF-derived LST compared to real clear-sky data (0.056 vol/vol VS 0.053 vol/vol) suggests such a gap-filling strategy (based on STDF alone) is not 100% perfect (especially for rainy weather which is the most difficult for the entire community of land surface remote sensing) and further improvements are encouraged, whilst the even higher ubRMSD of non-bias or bias-adjusted LST under cloud (0.064 vol/vol) suggests that the STDF-derived LST is at least a better alternative compared to its bias-adjusted counterpart.

Meanwhile, we also need to stress that the results in Appendix B do not indicate "SSM is not sensitive to LST", because if it is not sensitive, all three groups of SSM should not have difference in their validation performance. The results just indicate the difference of "LST-SSM" interaction mechanisms between clear-sky and cloudy conditions.

Response to technical comments

Comments	Response
Lines 50-56: references are necessary for the background knowledge introduction, especially for the potential application examples	Accepted. See the revised beginning of the Introduction. (Lines 54-58)
Line 87: "universal triangle feature (UTF)" or "triangle feature space (TFS)"?	Accepted. We revised and unified the term as "universal triangle feature space (UTFS)" through the text.
Line 91: please define the acronym UCLA	Done as suggested. (Line 93)
Line 109: ',' should be removed	Done as suggested.
Lines 78-80, 112-113: references, please.	For Lines 78-80, we have accepted your advice and added references. For Line 112-113, "the above-mentioned optical/infrared-data-based downscaling methods" have had their references listed above when each method was firstly described. So there is no need to repeat here.
Line 117: 'whilst .. even inferior' is not appropriate here. There is no such logic in the context unless you mean 'UTF-based methods are found even inferior to the DISPATCH in a typical humid region'	We have altered it to another formulation that weakens such a logic. See Lines 120-122 ("*As far as the UTFS-based method is concerned, a poorer performance was obtained compared to the DISPATCH in a typical water-limited region in North America*"). The main idea we intend to convey is that universality for both of the methods is not perfect enough currently.
Lines 124: the objectives you mentioned here are more like broad impacts or potential significance while the objective of a study should be specific.	Accepted. We have changed the term as "potential significance", because the objective has been described just in the current paragraph (to produce the data product, Lines 123-127)
Line 144: 'after' or 'in the'?	Accepted. Changed to "in the".
Lines 179, 360: 'high resolution' -> 'fine-	Done as suggested.

resolution’	
Table 1: url -> URL	Done as suggested.
Line 196: ‘be’ -> ‘being’	Done as suggested.
Line 205: ‘the’	Accepted and removed.
Lines 217-232: Please also include some literature to prove that these involved sites are spatially representative at km scales or have been widely used in SSM validation.	Accepted. Please see Lines 231-232 for the added literatures showing it has been widely used.
Line 224: ‘2014)’	Done as suggested.
Line 262: is the ‘10-cm-depth’ different from the ‘0-10 cm’ like you mentioned in Line 229?	Accepted. They are the same in effect. We have revised the former as “0-10 cm”.
Line 285: Do you mean that one set of coefficients a-d will be used for all pixels of the whole country on t1?	Yes. We have actually tested different solutions including sub-region-based coefficients and the common set of coefficients for all of the country. The data outcome based the common set of coefficients for a certain date has the best quality (obtaining both high accuracy and high coverage).
Lines 309-311: I agree that STDF is enough for the accuracy requirement of soil moisture estimation. However, this explanation here is weird because the atmosphere does have interactions with the surface at cloudy-sky: cloudy conditions may also indicate it is raining or the atmosphere is wet. Such LST and ET disturbance signals, which can be captured by PM-based LST but not by STDF, will impact the soil moisture. In other words, the atmospheric condition cannot be simply separated by using such an explanation.	We appreciate your agreement on our methodology. For the discussion, you can see our detailed response to your Specific Comment 5 above. Basically, we agree with you that atmospheric condition can bring extra uncertainty when we use STDF-derived LST as input, and we have better discussed it in the new added Section 4.2. However, this uncertainty can be smaller compared to using the bias-adjusted LST which is also not suitable for the downscaling theory we base as the theory was actually developed for clear-sky condition.
Line 326: One or two sentences for briefly summarizing the downscaling methodology in Song et al. 2021 are necessary.	Accepted and added. Please see Lines 334-338
Line 329: SEE, “soil evaporative efficiency”	Done as suggested.
Line 346: “All pixels were utilized within … centered at … ” would be better	Done as suggested.
Line 369: can you explain what “spatial averaging disaggregation” is	Sorry for this mistake. We have revised it as “spatial averaging operator for…”
Line 417: why the bias caused by heterogeneity is negative?	We actually mean the effect is “not beneficial”, but not mean it has a “negative-sign bias”. Now we have

	revised it to "disadvantageous effects" (Line 438)
Line 431: why RMSD_diff is important and focused? Maybe both clear-sky and cloudy-sky LSTs have higher uncertainty at some locations but the difference is small.	The main technical issues to tackle for generating this SSM product include gap-filling of LST under cloud, but not include retrieval of LST under clear sky. In other words, our work relies on the general accuracy of the existing LST product under clear sky (MODIS 1-km LST), which has been generally evaluated by Fig.3-(a) for the overall situation. As the primary purpose is to obtain LST of complete-coverage with consistent accuracy for all-weather conditions, the RMSD_diff is most important compared to other metrics. Based on the above concern, we made an extra site-based analysis for RMSD_diff in Fig.3-(c), while for the absolute RMSD values, the all-site analyses in Fig.3-(a) and –(b) are sufficient.
Fig 3: the absolute accuracy numbers of Fig 3(a) and (b) are better to be listed in the figure	Done as suggested.
Line 436: I feel 1.9 K is not small, and the RMSD difference can be ~70% of the clear-sky LST absolute accuracy [*Xu and Cheng*, 2021; *Zhang et al.*, 2021], especially for the nighttime LST. The word 'only' is too strong.	Accepted. We removed the word "only". We also changed the following sentence from "small uncertainty" to "uncertainty is not very significant". (Lines 458-460)
Fig 5, Line 663: please unify the ubRMSD or ubRMSE in the context.	Done as suggested.

Reference

Das, N. N., Entekhabi, D., Dunbar, R. S., Chaubell, M. J., Colliander, A., Yueh, S., . . . Thibeault, M.: The SMAP and Copernicus Sentinel 1A/B microwave active-passive high resolution surface soil moisture product, Remote Sens. Environ., 233, 111380, https://doi.org/10.1016/j.rse.2019.111380, 2019.

Mohammad, E. H., Nicolas, B., Mehrez, Z., Nemesio, R. F., Jean, W., Amen, A. Y., . . . Jean-Christophe, C.: Evaluation of SMOS, SMAP, ASCAT and Sentinel-1 Soil Moisture Products at Sites in Southwestern France, Remote Sens., 10, 569, 2018.