Hydrol. Earth Syst. Sci. Discuss.,
https://doi.org/10.5194/hess-2019-549-SC1, 2020

Hydrology and Earth System Sciences
Open Access
Discussions

Interactive comment on "Blending SMAP, Noah and In Situ Soil Moisture Using Multiple Methods" *by* Ning Zhang et al.

Alan Frank

ar_tis1943@outlook.com

Received and published: 12 January 2020

I thought this paper would be interesting while I saw the title. But after reading the entire article, the value is not apparent. In particular, two major things are lacking. First is any type of theoretical motivation. Second, the empirical motivation is not sufficient to demonstrate that this idea is particularly valuable. The latter is because none of the current multi-source blended soil moisture products uses either of the methods in the article for soil moisture blending. At least in this study, the authors failed to demonstrate their "simple method" can produce one dataset with higher (even similar) accuracy than any current dataset.

This article lacks any meaningful science: there is no hypothesis statement (why should

we expect these simple methods to be interesting or useful?), and the control method is insufficient.

In line 136 to 149, the authors listed four knowledge gaps and they claimed that they addressed all four gaps.

(1) "of the lack of in-situ 137 soil moisture inclusion in product blending. Current studies mainly focus on combining modeled and RS soil moisture, rather than combining all three sources (modeled, RS and in-situ). In-situ measurements can be useful for improving the accuracy of hybrid soil moisture datasets"

Some widely used soil moisture products, such as ESA CCI Soil Moisture (https://www.esa-soilmoisture-cci.org/) and SMAP L4 (https://smap.jpl.nasa.gov/data/), merged satellite, in-situ and model data. If the authors want to demonstrate in-situ soil moisture is critical to improve the data quality, they need to at least compare the results with those using satellite and model data only. If they want to demonstrate their simple methods has better (or even similar) performance than more advanced data assimilation approaches, they need to include at least one data assimilation approach, such as 3D-Var. Comparing those "simple methods" with simpler method (averaging) does not make sense at all.

(2) "There is no comprehensive evaluation of different data blending methods . . . compare the accuracy of different blending methods to identify the optimal approach for soil moisture"

This is a red flag. Regardless whether there is any document comparing different blending methods, being the first to compare some methods is not justification for publishing a study on these methods. Simply, I don't care if you are the first to do something; I only care if you have an interesting idea. It is implied that you are the first to do whatever you are doing, because otherwise it is not publishable (with certain rare exceptions). So instead of stating the obvious (that you think what you are doing is novel), tell me what problem you want to solve or what question you want to answer.

The conclusion from this article cannot convinced me the approach is optimal. It can only indicate a best one among these four approaches. Even so, in the abstract, the authors stated "no significant differences between blended soil moisture datasets using errors estimated from TC, REV or RK. Moreover, the LSW did not outperform AVE", which means nothing.

(3) The impact of measurement units (e.g., volumetric water content, soil moisture anomalies, and percentiles) is unknown. For example, is it better to convert all of the soil moisture measurements to anomalies or percentiles before blending?

This might be the only question that is answered in this article. However, the answer to this question seems cherry-picked. Through standardizing volumetric water content, you will get a better result. When the length of record is short (which is in this study), the standardized anomalies or percentiles would be a proxy of ranking.

(4) A simple and operational methodology is still needed for accurate daily soil moisture mapping with high spatial resolution. Current methods to generate gridded soil moisture data products cannot produce data with sufficient spatial resolution for many agricultural and hydrological applications.

Both remote sensing and model simulation are discussed in the Introduction. Regarding remote sensing observations, it is true that the resolution is poor. In this article, all the evaluations are performed at station-level. If you have in-situ data available and assume that that is the most accurate data, why do you need to blend it with other datasets? If the authors aim to create a dataset with higher spatial resolution than remote sensing and model datasets, please show it. It is unfortunate that I did not see any improvement in terms of spatial resolution. What I saw is that the spatial resolution is determined by Noah or SMAP L4.

I also have some other serious concerns: 1. SMAP L4 is an assimilated product, which includes some model simulations. When this article does the blending with Noah model, do you think you double weighted the model simulations?

2. Three-year data is too short to sufficiently calculate soil moisture anomalies and percentiles.

3. I wondering how this work can be applied in other regions with sparsed soil moisture sites. Figure 10 shows that MAE is larger over those regions with less sites (which is a common situation over the world).

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., https://doi.org/10.5194/hess-2019-549, 2019.