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Hydrology and Earth System Sciences Discussions

Interactive comment on "A dynamic approach for evaluating coarse scale satellite soil moisture products" *by* A. Loew and F. Schlenz

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We would like to thank the two reviewers (Dr. Piles and an anonymous referee) as well as Dr. Crow for their very constructive comments on the first version of the manuscript. We give answers to their questions in the following. Significant changes in the manuscript are indicated in bold.

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Interactive Discussion

Discussion Paper

1 Comments by Dr. Piles

1.1 General comments

Q: This paper deals with the comparison of in situ soil moisture samples and satellite derived soil moisture estimates at coarse resolution. This study is a timely contribution, particularly for the validation of SMOS and upcoming SMAP soil moisture products. It compares the time stability and the triple collocation (TC) methods, and proposes the use of the TC method at a shorter timescale so as to adapt to temporally stable soil moisture patterns. The authors are presenting a nice work; the approach is original and interesting. Nevertheless, I would recommend them to complete the study with a detailed comparison to the TC method in Miralles and Crow, 2010, and to include an analysis of how to choose the most suitable timescale, given a specific case study. I would also suggest them to introduce the different sections in the introduction and be more concise in the conclusions. I believe these changes are needed to strengthen the paper and help its readability. Hence, I suggest the authors to address the comments hereafter before publication.

1.2 Specific comments

Q: Line 2, page 7265. You should clarify that SMAP will also provide a soil moisture product at 10 km spatial resolution.

Done

Q: Lines 15 to 17, 7265. Since Scipal et al. 2009 were the first ones to apply the TC method to soil moisture, it would be appropriate to cite them in the Introduction.

Done.

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Q: Lines 22 to 28, page 7265. It is not clear from this paragraph how this paper will "contrast the potential" of the two validation methods introduced in the previous paragraph (Cosh et al. 2004, 2006 and Miralles and Crow 2010). Differences with respect to these two approaches should be outlined to better understand the novelty of the method proposed.

This sentence seems to have been misleading, as it is actually not the objective of the paper to contrast the two methods of Scipal and Miralles et al. We did extend on the methodology of Scipal et al. and investigated it's potential on much shorter timescales and apply it to the soil moisture validation problem. We therefore rephrased the sentence.

Q: Line 29, page 7265. "rather short timescales" is a vague expression, would it be possible to indicate an approximate value?

Done.

Q: Line 8, page 7266. I would recommend a change of notation: Root Mean Square Error (RMSE) instead of Root Mean Square Deviation (RMSD). RMSE is more frequently used within the soil moisture remote sensing community. Also, note that the D in RMSD stands for deviation and not for difference.

We agree that the term RMSE is often used to characterize the deviations between two data sets and that this is common used term in the field of soil moisture validation. However, we think it is a bit difficult to speak about an error in that context, as this terminology implies that you actually know to "truth", which is typically not the fact in case of the soil moisture validation problem, where the deviation between the two data sources is a function of the uncertainties (random errors) of each of the data set and the uncertainty in representing an area from a large footprint with often very local measurements. We therefore prefer the term "deviation", instead of error. We changed from difference to deviation in the manuscript, as suggested by the reviewer.

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Q: Lines 24 to 25, page 7266. I would suggest to add the following reference for the Australian Airborne Cal/Val Experiment for SMOS (AACES) dedicated campaign: Peischl, S., Walker, J.P., Allahmoradi, M., Barrett, D., Gurney, R., Kerr, Y., Kim, E. LeMarshall, J., Rüdiger, C., Ryu D., and Ye N., 2009. Towards Validation of SMOS Using Airborne and Ground Data Over the Murrumbidgee Catchment. In Anderssen, R.S., R.D. Braddock and L.T.H. Newham (eds) 18th World IMACS Congress and MODSIM09 International Congress on Modelling and Simulation. Modelling and Simulation Society of Australia and New Zealand and International Association for Mathematics and Computers in Simulation, July 2009, pp. 3733-3739.

We did add this reference to the list. Thanks!

Q: Lines 1 to 4, page 7267. To complete the list of regular soil moisture networks I would suggest to include a reference to OZnet (www.oznet.org.au)

We added the reference to OZNet. Thanks!

Q: Lines 13 to to 15, page 7267. You should make clear in the beginning of the sentence you are addressing soil moisture spatial variability, i.e. ": : :soil moisture spatial variability within the footprint of a satellite sensor: : :".

Done

Q: Lines 18 to 19, page 7267. It would be helpful if you could indicate in what specific sections you are introducing every approach and their potential. The same way, it would greatly increase readability if you could include a short summary of what each section of the paper is devoted to at the end of the introduction.

Done

Q: Lines 22 to 25, page 7269. Please, clarify differences between the approach in Scipal et al. 2009 and the one in Miralles and Crow, 2010. What soil moisture data is used in Miralles and Crow, 2010?

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We did clarify that Miralles et al are using an anomaly approach, while Scipal et al used original time series of soil moisture observations. The remote sensing observations of Miralles are based on a product, generated by the USDA (Jackson et al, 2009)

Q: Lines 6, page 7271. My understanding is that the final expression for var (ez) should be <(z)2 -<x*z*> (beta2/beta0)>. Please, check.*

The reviewer is right. Thanks for correcting this typo.

Q: Lines 15 to 20, page 7271. This paragraph is confusing; could you please detail (I) why rescaling is typically performed over satellite and modeled soil moisture retrievals, and (ii) why the estimation of the errors is independent of the chosen model parameters?

From equation (9), it becomes clear that the estimated errors are only dependent on the anomalies of x,y and z. They are in fact independent of the model parameters alpha and beta, if one of the data sets serves as a reference (alpha0 = 0, beta0 = 1). It seems that the appearance of the beta terms in Eq (9) might lead to the conclusion that the errors are still dependent on beta, which they aren't. We therefore removed the second part of the equations in the revised form of the manuscript.

Q: Line 17, page 7272. The ISEA4H9 has an inter-cell distance of 14.989 km. Please correct.

Done

Q: Line 9, page 7275. From Fig. 2, I would say that the dynamical range of soil moisture is from 0.02 to 0.5 [m3/m3].

We guess, that the reviewer meant 0.2 to 0.5. We agree with that and did change the text.

Q: Line 20, page 7275. I would recommend adding an explicit equation showing how $e_{station}$ *is estimated.*

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Done

Q: Lines 20 to 21, page 7277. You should indicate both in Table 3 and in the text what the subscripts x, y, and z stands for in the correlation notation. I assume x stands for station, y for model and z for satellite. The correlations in year 2009 are significantly lower than in 2008, and there are six cases wit r < 0.3. Is this correlation still significant? Consider revising.

It was mentioned on page 7226, line 14 ff that the stations are used as a reference (x), the model simulations as y and the remote sensing data set as z. We therefore did not change the text of the manuscript. However, we agree that it makes sense to clarify this separately for the Table caption. The shown correlations are all statistically significant on a 99% level.

Q: Figure 6. Do you have a hypothesis of what's causing the high gain value observed in some of the days/runs?

The high gains are observed in cases where no correlation is existing. For instance, run 18 in year 2008 shows a gain of $\beta_1 = 8.3$ in June 2008. Here the correlation between the station (x) and the AMSR-E data (z) is 0.04 only. Thus, one can not expect reliable results from the methods in these cases. The calculation of the significance level of the correlations might help the interpretation of the results in these cases.

Q: Lines 9 to 10, page 7279. Figure 8 is showing results for year 2009, and the text is referring to year 2008. I guess there is some inconsistency here, consider revising.

Yes, it should read 2008. We corrected the paper. Thanks!

Q: Lines 1 to 6, page 7281. Here, also more information on the Miralles and Crow approach are needed to assess the results obtained with the two approaches, their advantages and disadvantages (see comment 10 above).

In response to comment 10), we did clarify that the approach of Miralles et al is based on an anomaly approach. We don't think that it is necessary to clarify this here again.

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Q: Line 16, page 7280. It might be interesting to show the degradation in TC estimation of representativeness error as a function of dt. In your analysis you used dt = 30 days, 60 days and 1 year, do you think it would be advantageous to include a shorter timescale? is it feasible to find an optimum dt for the case studied?

In response to the reviewers comment, we made a sensitivity study for dt=[15,30,60,90]. However, we were not able to find a clear dependency of the obtained representativeness error as a function of dt. The error varies as function of dt, but also as function of the year, which indicates that an optimum size of dt is actually dependent on the soil moisture dynamics. It was thus not possible to identify an optimum dt using the present data set. In general we agree that this is an interesting aspect for further research.

Q: Figure 9. I would recommend coloring each station differently in this plot. Also, the caption should indicate that significant correlation stands for 95% confidence or r2 > 0.3.

Done

Q: Line 8, page 7281. I would recommend mentioning the approaches you are comparing in this first sentence of the conclusions. Also, it should be clearly stated in the conclusions that you propose to combine the static TC method and the dynamic TC method at a shorter timescale, and that for your particular study the time stability approach is not applicable. Please, consider adding a short paragraph to clarify.

We did add a small additional paragraph.

1.3 Technical corrections

Q: Lines 19 to 21, page 7266. My understanding is that "of" should be replaced by "or", as you are citing two examples, is that it?

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Done, thanks!

Q: Line 15, page 7275. It should be "least squares"

Done

Q: Table 2. The table caption should detail the specific parameters calculated and its notation: gain, offset, correlation, $e_{station}$.

We did adapt the caption of the table

2 Comments by Dr. Crow

Q: This is a very nice paper that makes at least two important contributions to satellite soil moisture validation activities: 1) it's the first paper (to my knowledge) to look at time stability and triple collocation validation approaches simultaneously and 2) it makes novel points about our ability to apply both approaches over variable time periods.

Q: Introduction: I think a reference to Scipal et al. (2009) would be appropriate in the second paragraph. They were the first to apply to triple collocation to soil moisture. However the particular triplet they used (i.e. passive microwave soil moisture /active microwave soil moisture /model-based soil moisture) is different than the triplet used in Miralles and Crow (2010) and here (i.e. passive microwave soil moisture/sparse ground-based observations/model-based soil moisture). This could be clarified.

We have added the reference to Scipal et al. (2009) and did clarify the differences between the papers.

Q: Line 10, P. 7271. Clarify which of the three data sets is assumed to provide the unbiased reference here.

Done

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Q: Line 20, P. 7271. I could not follow the sentence "However, the estimation of the errors in independent of the chosen model parameters as can be seen from Eq. (9)." "Beta" variables do appear in Eq. (9): : :so it seems like the choice of a reference data set DOES impact the recovered errors. I'm probably missing something: : :considered clarifying.

This seems to be a misunderstanding. Taking Eq.8 and Eq. 9 together, the betas cancel out and the error are only depended on the measurements if $beta_0$ is given. Thus the errors are in fact independent of the chosen model parameters. We clarified that sentence in the manuscript. As we had the impression that the equations seem to puzzle the reader, we have remove the second part of the equations, so it becomes clearer that the errors are in fact independent of $beta_x$.

Q: Lines 20 to 25, P. 7275. This is a very good point. The "stable" nature of time stability approach is only relative and will break down as it is evaluated at progressively longer time scales. The same thing is likely true as you increase the spatial extent of the time stability analysis.

Thanks for this comment. We agree that a further spatial extent might also result in a decrease of the applicability of the temporal stability concept.

Q: I'd recommend an explicit equation somewhere to define how the benchmark $e_{station}$ error is estimated. Also, make sure to clarify the difference between $e_{station}$ and e_{stat} whenever the two are compared (in e.g. the caption for Table 3).

Thanks, one of the reviewers made a similar comment and we adapted the paper.

Q: Figure 9 – My understanding is that the points plotted in the "2008" and "2009" graphs represent a mixture of stations and monthly temporal periods. So that it is difficult to establish whether the observed correlation is due to TC capturing temporal or spatial patterns in spatial representativeness errors. Given that a key point in the analysis is that the proposal dynamic approach can capture monthly temporal trends

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in errors, the authors should consider coloring each particular station individually so the reader can see whether temporal variations at a given site accurately track real variations (i.e. are relative periods of high and low representativeness error actually reproduced in the TC analysis?...or is the observed correlation due to just capturing station-to-station differences?). It is difficult to answer this question using the current plot.

We did colorize the plot as suggested. It can be seen, that the temporal dynamic is captured.

Q: Along these same lines, it might be interesting to explicitly compare the degradation in TC estimation of representativeness error as a function of dt: : :how much more scatter is introduced as you proceed to short temporal averaging windows (i.e. dt = 1 year to dt = 60 days to dt = 30 days)?

Dr. Piles made a similar comment. We made a sensitivity study for different values of dt, but could not find a clear dependency of the representativeness error as function of dt.

Q: The comparisons to Miralles and Crow (2010) are interesting. However, there are at least three differences between the two approaches that should be discussed: a) Miralles and Crow's analysis was based on errors in soil moisture ANOMALIES calculated relative to a soil moisture climatology based on applying a 31-day moving average window to multi-year data while this analysis is based on errors after only a long-term mean has been removed from the data. In this sense, this analysis is more ambitious than Miralles and Crow (Miralles and Crow show that TC is actually more difficult if you do not remove a seasonal cycle). b) As already discussed in the paper, Miralles and Crow attempt to recover only longterm errors while this analysis attempts retrievals over a shorter time period. c) On the other hand, Miralles and Crow's analysis did not filter out periods of low crosscorrelation between observations (as was done here) and applied TC to the entire time series of available data. In this sense, Miralles and Crow

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are attempting a more ambitious retrieval. So this analysis is more ambitious (i.e. attempting a more difficult estimation problem) on two counts while Miralles and Crow is more ambitious on a single count. Direct comparisons between the two approaches all still meaningful but should briefly discuss all three differences.

We agree that the two approaches are different on a technical level. We have therefore added an additional paragraph in section 5.2.4 to further explain the differences. However, as we also apply a moving window in case of the dynamic TC approach, the data is automatically rescaled within each time window (new alpha and beta parameters are obtained). One might think of this rescaling also as a kind of a removal of typical temporal dynamics within the analysis window. In such a sense, the results might be somehow comparable to the anomaly approach by Miralles and Crow (2010).

Q: The updated citation for "Miralles and Crow, 2010" is: Miralles, D.G., W.T. Crow and M.H. Cosh, "A technique for estimating spatial sampling errors in coarse-scale soil moisture estimates derived from point-scale observations," Journal of Hydrometeorology, in press, 2010.

We did update the reference

2.1 Some minor points regarding figures and tables:

Q: Table 1 – Why does the experiment numbering start with 16? It might be clearer just to refer to each experiment based on the location name used to represent the "single station" soil moisture observation (i.e. Engersdorf, Locheim, etc, : : :.).

The reason for the numbering is due to technical reasons. As we always mention the experiment number together with the name of the station, we don't see a problem with using the experiment as an additional 'key index' throughout the paper. The only place, where we do not explicitely mention the name of the station is in Table 3, where there is not enough space for the long station names. We did therefore not follow the

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recommendations.

Q: Figures 5 to 7 - In the figure captions, clarify what the difference is between plotted results presented in the left and right columns of the figures.

The differences between the two columns is just the year. We changed the caption.

Q: Figure 8, second column - Add axis labels to the scatterplots.

Axis labels are there its [m**3/m**3]

3 Comments by Referee 2

Q: The title/abstract of the paper suggests that a novel evaluation approach for coarse resolution soil moisture data is presented. In fact the paper presents an analysis of the triple collocation (TC) error model which was first published in the 1990s. The only novel aspect of the study is the application to temporal subsets of the analysed data to account for temporal varying biases.

Q: The paper is still worth publishing as it provides some interesting insight into the temporal and spatial sampling characteristics and representativness of different soil moisture products. In addition the analysis confirms the robustness of TC when applied with in-situ data. TC can account for scaling errors, which could be an interesting prospect of the method.

We would like to emphasize that it was not the intention of the paper to introduce a new kind of TC analysis, but use this well established method in the specific context of the soil moisture validation problem. The paper makes novel contributions in multiple senses to that highly important research field by a) comparing TC method to the temporal stability approach b) extend the TC method to the soil moisture validation problem on variable timescales

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3.1 Following issues need to be clarified:

Q: A relatively small set of data is used in the analysis, specifically when applying TC in a temporal adaptive way. The question arises if the observed differences in the observed gain parameters and errors are significant. This could easily be tested.

We are fully aware that the use of data from a rather short time period has an impact on the significance of the correlations between the data sets. We did therefore clearly indicate the periods where the relationships between the different data sets are not significant at the 95% level (see Figures 5-7). As this had already been included in the manuscript, no changes were applied.

Q: The low number of observations could also result in biased error estimates as the sample size might not allow deriving a stable pdf. Consequently the conclusion that the observed lower errors for the adaptive approach (compared to the static one) are due to higher accuracy of the satellite product during different periods may not be fully true. The lower errors can also be an artefact caused by the limited sample size.

We agree the reviewer, that the lower errors might theoretically be also an artefact of the sample size. However, the major improvement basically comes from the temporal adaptive method, as the scaling coefficients (alpha and beta) are calculated on shorter timescales. This adaptive scaling results in a better correspondence of the satellite data set to the reference data set as compared to longer timescales and thus to a smaller random error.

Q: The authors removed satellite observations that were affected by precipitation. To address the study objectives this is a valid approach. The authors should however be careful in providing error figures for the satellite product as they are idealized error estimates which may not be representative in practice.

We fully agree with the reviewer on that point and are therefore now discussing this point in the manuscript.

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Q: Use SI units throughout the manuscript.

We have adapted the units.

Q: In Eq 7 I assume the alpha terms should be dismissed. As alpha is a constant it should be 0 after removing the mean.

The reviewer is correct with that comment. The equations have been adapted.

Q: p 7276 line 23: The authors state that TC relies on a significant correlation. This is not entirely true. TC presumes that the three data sets used in the analysis observe the same geophysical quantity. This can be tested with the correlation analysis. It could however be that the correlation is low, for example if the SNR is low under constant dry conditions, and that TC would still provide correct error estimates.

Thanks a lot for this very useful comment. We did add this discussion to the paper.

Q: Table 3: the subscripts are confusing. In the capture sat, model sat is used. In the table station, stat, model, sat and for the correlations x, y and z. Use the same notation and explain it in the text.

Another reviewer made a similar comment and we adapted the Table.

Q: Fig 2: It would be informative to plot the soil moisture evolution for each station to better see the spread/differences.

We disagree that the additional information on each station would result in more clarity about dynamics of the data. We attach a plot of the original data below. The purpose of Fig.2 is to provide a general impression about the temporal dynamics of soil moisture and their spatial variability as well as the differences between the two years. We are still convinced, that this information can be best presented in the way it was made in the paper.

Q: Fig 4: replace x, y, z with model, station and sat. (same for other figures)

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It has been clearly defined in the text that x corresponds to the station, y to the model and z to the AMSR-E data. We explicitly decided to keep the notation x,y,z in the figure to be consistent with the notation introduced in section 2.2. However, to make the figures also readable without a further explanation of the text, we did include the definition of x,y,z also in the figure captions, where it was missing.

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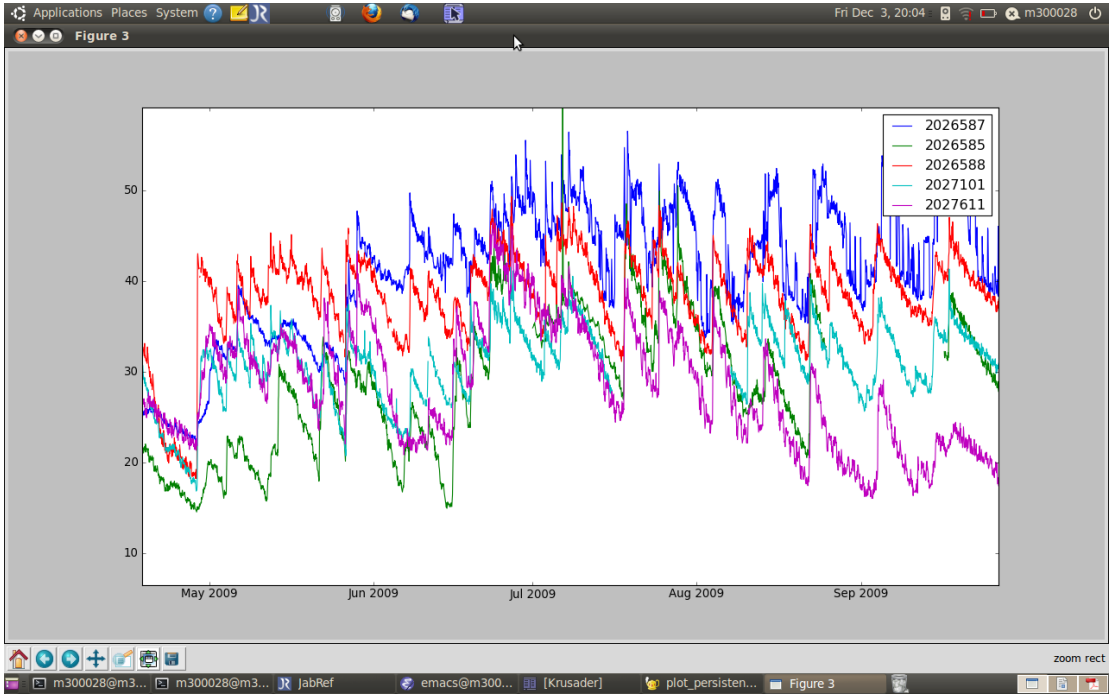

Fig. 1.

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