Interactive comment on “A soil moisture and temperature network for SMOS validation in Western Denmark” by S. Bircher et al.

Anonymous Referee #1

Received and published: 9 December 2011

In the framework of the recently launched Soil Moisture and Ocean Salinity (SMOS) spatial mission a soil moisture and temperature network was established in Denmark. This manuscript reports a complete description of this network as well as a preliminary comparison between the 0-5cm in situ measurements and the SMOS soil moisture (L2). The paper is well written, well structured and clear. The title clearly describes the contents of the paper. The abstract provides a concise and complete summary and the reference list is appropriate. The presentation is clear and the language is fluent and precise. It addresses an important topic that should be of great interest to HESS readers. The results are generally sound and well supported with figures and tables. While the article is not particularly original and mostly a description paper, it is certainly useful to document this effort. I recommend this paper for publication after minor revisions.

P.9962, L.2: ’[...] soil moisture data globally, [...]’ please add the revisit period

We will adjust the sentence accordingly.

P.9962, L.5: ’Decagon ECH2O 5TE’, not appropriate in the abstract

We will adjust the sentence accordingly.

P.9962, L.22- 24:"Consequently,the network performs according to expectations and proves to be well suited for its purpose." Please consider removing/rephrasing this sentence, I believe that the quality of such a network has nothing to do with the result of the comparison between measurements and remotely sensed products. According to this sentence, one may assume that Networks are monitored by satellite data.

Yes, of course you are absolutely right. This sentence is supposed to refer to the network data analysis carried out to check the network performance, which the comparison with remotely sensed products was part of (P.9962, L.13-22). We will adjust the phrasing to make this clearer.

P.9963, L.12: soil moisture of good quality

We will adjust the sentence accordingly.

P.9963, L.14: launched in 2009

We will adjust the sentence accordingly.

P.9963, L.14: update Kerr et al 2001 by Kerr, 2007, Kerr et al., 2010 Kerr, Y. (2007): Soil moisture from space: where are we?, Hydrogeol. J., 15(1), 117–120. Kerr, Y.H., Waldteufel, P., Wigneron, J.-P., Delwart, S., Cabot, F., Boutin, J., Escorihuela, M.- J., Font, J., Reul, N., Gruhier, C., Juglea, S.E., Drinkwater, M.R., Hahne, A., Martín- Neira, M. & Mecklenburg, S., (2010). The SMOS Mission: New Tool for Monitoring Key Elements of the GlobalWater Cycle. Proceedings of the IEEE, 98 (5), 666 - 687.

We agree with you on the article of Kerr et al. 2010 and it has been added. However, in our opinion Kerr 2007 is more a review on soil moisture assessment from space in general, and we believe that Kerr et al. 2001 is the paper describing the mission per se. Thus, this reference has been kept.

P.9964, L.2: 'Spain (Martinez-Fernandez & Ceballos, 2003)' Please add Martinez-Fernandez & Ceballos, 2005. Martínez-Fernández, J. & Ceballos, A. (2005). Mean soil moisture estimation using temporal stability analysis, J. Hydrol., 312 (1-4), 28–38, doi:10.1016/j.jhydrol.2005.02.007.

Thanks, you are right; the article from 2003 does only describe the REMEDHUS network and not the one in the Rinconada catchment. Reference to the 2005 article will be added

P.9964, L.3: 'France (Calvet et al.,2007)' Please add Albergel et al, 2008. Albergel, C., Rüdiger, C., Pellarin, T., Calvet, J.-C., Fritz, N., Froissard, F., Suquia, D., Petitpa, D., Piguet, B. & Martin E. (2008). From near-surface to root-zone soil moisture using an exponential filter: an assessment of the method based on in situ observations and model simulations, Hydrol. Earth Syst. Sci., 12, 1323–1337, doi:10.5194/hess-12-1323-2008.

We will add the reference.

P.9966, L.24: Accuracies according to constructor?

Yes, these are accuracies given by the manufacturer. It will be clarified in the text.

P.9967, L25: SMOS measures Full polarization, not dual polarization. It is not clear to me what is the source of the SMOS level2 data (ESA?).

True, SMOS is measuring in full polarization - this will be updated accordingly.

The used SMOS data is the one reprocessed by the Centre d'Etudes Spatiales de la BIOsphère (CESBIO) using the state of the art L2 prototype algorithm (V4.00). This information will be added to the text.

P.9969, L.18: '[...]and 0.56%', respectively.

We will adjust the sentence accordingly.

P.9970, L.10: Please add acronym of GIS.

We will adjust the sentence accordingly.

P.9970, L.13: above sea level => a.s.l.

We will adjust the sentence accordingly.

P.9974, L1: How does it affect the continuity of the measure, do cultural practices impact the soil structure?

Yes, temporarily removing the stations placed in the agricultural fields of course leads to gaps in the measurements. On the other hand, after plowing/seeding and harvesting the soil structure may be changed. Thus, we believe it is more important that the investigated soil corresponds to the actual encountered conditions in order to acquire representative measurements – especially, given that 80% of the studied SMOS pixel is covered with agricultural land. A sentence with the reasoning for the station removal will be added.

P.9975, L.23: ’RMSE’ In situ data contain errors (instrumental and representativeness), so they are not considered as ‘true’ soil moisture, particularly when they are not calibrated. It should be underlined by using the RMS Difference terminology instead of RMS Error.

We take your point that none of the two data sets being compared is the ‘truth’ while the other error prone. As suggested, we will adapt the RMS Difference terminology instead.

P.9977, L.22: typo, ’of both’

Thanks, this will be corrected

P.9980, L.24: typo, ’Ruediger’ is ’Rüdiger’ (also in references list).

Thanks, this will be corrected

P.9980: More emphasis should be put on RFI as Denmark was particularly affected in 2010.

It is true that RFI contamination turned out to constitute a problem in Denmark and this issue is certainly of high importance. However, as the main objective of this article is to present the design and implementation of the network and it is already quite long, we feel it is out of the scope to include analysis on RFI. This first comparison between SMOS and network data is merely shown to confirm a clear trend between the two, while it is stated that further investigations are necessary. These are subject of a subsequent study (submitted in a separate article to Transactions on Geoscience and Remote Sensing): SMOS data was filtered based on quality parameters contained in the level 2 product, which showed very good correspondence with an RFI detection scheme based on level 1A data (Anterrieu 2011). Comparing the filtered SMOS data with the in situ average improved the correlation significantly, while the bias only decreased slightly. We assume that through the filtering overpasses of heavy RFI contamination are removed, while the remaining bias could at least partly be due to permanent low energy RFI pollution (soft RFI) still present in the data. Investigations on this subject are ongoing. A short paragraph will be added to section 5.3.3 to comment on this issue.

Interactive comment on “A soil moisture and temperature network for SMOS validation in Western Denmark” by S. Bircher et al.

Anonymous Referee #2

Received and published: 13 December 2011

Overview
The study describes the set up of a soil moisture and soil temperature network in the Skjern River Catchment, Denmark. The network is mainly addressed to the validation of the soil moisture product obtained by SMOS. Additionally, a first comparison between in situ observations and the SMOS soil moisture product for the year 2010 is carried out.

General Comments
As already underlined by Referee 1, the paper is well written and structured and addresses an important topic, i.e., the optimization and establishment of a soil moisture network to be used as benchmark for the validation of coarse-resolution satellite soil moisture products. I also agree with Referee 1 that the paper is mostly a technical descriptive document. However, some interesting points are investigated in the paper as, for instance, the optimization of station locations according to land use and soil features. Therefore, it may deserve to be published. However, I have several concerns that should be addressed before the publication. Reading the paper it is not clear the reason why the approach described in the paper would provide an enhanced in situ soil moisture observation. Obviously, this is theoretically expected but some information to demonstrate that the employed procedure provides a more reliable soil moisture estimate at SMOS pixel scale should be given. For instance, I suggest showing some analysis that highlights that the average of the whole soil moisture network data provides a significantly different soil moisture time series than using a more limited number of observations.

The problem when implementing such a network is always where to place the individual stations. The most optimal would be long-term pre-studies to investigate temporal and spatial patterns of the involved environmental variables. But then again, one would have to think about how to set these up and optimally choose a dense sampling scheme to come as close as possible to reality – for this, labor and financial costs are too high. Also, the data is needed now. So, our idea was an attempt to a priori try to think of as many potential impacts as possible and consider all available information on the most soil moisture influencing variables rather than choosing a random solution, in order to enhance the likelihood of a representative set-up. However, it is true that this hypothesis remains unanswered. However, a study has been carried out where the network stations were grouped after a) soil types, b) land cover classes, and c) composite class numbers. The respective averages were compared to corresponding SMOS L2 soil moisture data. It turned out that amongst all of these subgroups only the soil type sand class achieved as good statistical results as the entire network average. As the sand class includes about 80 % of the stations, it is not further surprising that it behaves very similarly. In any case, the fact that none of the subgroups performs significantly better than the network average enhances our confidence in the representativeness of the chosen network setup. We considered including these findings in this article. However, as the main objective here is the presentation of the design and implementation of the network and the paper is already quite long, we decided to submit them together with other more detailed SMOS validation work in a subsequent article (Bircher et al., submitted to Transactions on Geoscience and Remote Sensing). A short paragraph pointing to the main conclusions of this analysis and the corresponding article will be added in section 5.3.3.

Moreover, some analysis dealing with the spatial and temporal variability of the in situ collected data could give to the reader a more clear picture of the soil moisture behavior in the study area.

While we clearly see the importance of the issue of spatial and temporal variability, we believe that we have already addressed it in several ways. In Figure 8 (Section 5.2), soil moisture and temperature profile data is shown of selected stations to cover the range of encountered conditions throughout time and space. Furthermore, Figure 10 depicts a comparison of the environmental conditions in the south-west and north-east of the studied SMOS pixel. The results are discussed in Section 5.3.2. Finally, we show the average over all network stations together with the standard deviation in Figure 11 and discuss them in Section 5.3.3, P 9980, L 9-15. Thereby, all mentioned plots span an entire year and we think this should give a good impression of the temporal behavior. In the revision we will do our best to make this stand out more clearly.

I was also surprised to see that the agreement between the initial guess soil moisture (derived by the ECMWF model) and in situ observations is higher than the one retrieved by SMOS. This means that the soil moisture product derived by the ECMWF model (the first guess) is better than the SMOS one, at least in the investigated area. I think this aspect should be analyzed and discussed.

Yes, this is certainly a good point. However, in this respect, we think it is important to keep the two following things in mind: First of all, when looking at the worldwide findings (Section 5.3.3, P. 9980-9981, L.23-28,1-3), there are also SMOS validation sites where the retrieved SMOS soil moisture already shows a similar R^2 as the ECMWF model, while RMSE/bias are sometimes smaller for retrieved SMOS soil moisture than ECMWF. Second, SMOS has only been launched two years ago while the ECMWF model has become well established in the course of many more years of research. Looking at the progress SMOS data quality has made since launch, we believe that by means of persistent feedback from validation activities, it is very likely that it will continue improving in the near future. In this respect, several studies are underway at the Danish study site concerning the cleaning of soft RFI, difference in sampling depth, and inaccuracies in the retrieval algorithm. A short paragraph will be added to section 5.3.3 to comment on this issue.

Finally, in a paper dealing with remote sensing of coarse-resolution soil moisture product, I believe that the other satellite sensors, besides SMOS, providing a soil moisture product at global scale could be at least mentioned: ASCAT, AMSR-E and Windsat. Moreover, a comparison of the results with the ones obtained with the other sensors, for instance in Europe (e.g. Brocca et al., 2011), might be given.

Yes, you are right, the other sensors should be mentioned in the introduction and we will do so. However, as said before, the main goal of this article is to introduce the design and implementation of the network. Thus, we think that it is out of scope to address any results of comparisons.

In the specific comments, I report a number of further changes and clarifications that should be required. On these bases, in my opinion, I find that the paper may become worthy of publication on HESS after a moderate revision.

Specific Comments/ Technical Corrections (P: page, L: line or lines)
P9962, L4: For sake of clarity, please specify that soil temperature data are measured by the network. At a first reading, I believed that air temperature data are collected.

We will adjust the sentence accordingly.

P9962, L22: Actually, soil temperature data is a product derived by the ECMWF model, it is not retrieved by the SMOS radiometer. I believe it should be specified better in the abstract because it seems that also these data are retrieved by SMOS. Moreover, it is not clear in the abstract what the initial/retrieved products represent.

Sorry for that. We will adjust the respective sentences to increase comprehensibility.

P9964, L21-25: Also Brocca et al. (2010) applied temporal stability and random analysis for the optimization of the number of soil moisture sensor within an area of 60 km^2. Additionally, Famiglietti et al. (2008) analyzed also the number of soil moisture samples as a function of spatial scale; this aspect could be discussed in the Introduction.

We will add a short paragraph in the introduction taking these articles into consideration.

P9965, L13-15: At this point of the paper this sentence is not clear, please revise.

Sorry for that. We will revise the sentence to increase its comprehensibility.

P9966, L13-14: It is not clear to me what is "the shelter correction factor". Moreover, as rainfall observations are only used for data visualization it is not needed to give too much details for them.

We will remove this information from the article.

P9968, L7-10: In my opinion, more details regarding the soil moisture and soil temperature products provided by the ECMWF model should be provided. Which products are used? The operational product or the reanalysis one. Please specify.

The presented ECMWF soil moisture and temperature products stem from direct model outputs of the operational forecast products (available at 3-hourly intervals) based on the 00:00 and 12:00 Coordinated Universal Time (UTC) data. This information will be added to the text and reference will be made to the respective document.

P9970, L22: The sentence seems truncated, something is missing, please check.

Thanks for the hint. We will correct this.

P9977-9978: In my opinion, this description of the profile soil moisture and soil temperature data is too long and could be summarized. It seems more appropriate for a technical document.

While we believe that it is of importance to demonstrate that the measured soil moisture and temperature patterns are relatable to encountered environmental conditions, however, we agree that this section is a bit lengthy. We will try to shorten it appropriately.

P9979, L18-27: I am surprised to see that the determination coefficient between soil moisture time series (R^2=0.57) is lower than the one between rainfall time series (R^2=0.86). Usually, soil moisture time series are more smooth than rainfall ones thus showing higher correlation values. I suggest analyzing more in-depth this interesting aspect also considering other soil moisture stations.

A data check of the mean daily precipitation at the DMI 10 km grid nodes contained within the studied SMOS pixel (not shown in the article) indicated that in this area the spatial variability of precipitation is low. This can be explained by the fact that precipitation is often arriving in fronts from the Atlantic, passing rather homogeneously over the area. More local convective events are not very frequent and there are no mountains to disturb the flow of the currents. Meanwhile, the soil moisture patterns in the fields are additionally influenced by irrigation which is applied spatially variable in terms of timing and amount of water. A short paragraph will be added to section 5.3.2 to comment on this issue.

P9980, L3-4: It is not clear to me why these results highlight that the variability between the two areas is in the same order as within them. I suggest specifying better.

Sorry for this. On one hand, we here give the average deviation of the behavior in one area to the other (RMSE) to check if there is a clear difference in the respective trends. On the other hand, we give the standard deviations of the respective averages of the three selected stations in the two areas, averaged over the entire year, indicating the spatial variability within the two areas. As the RMSE and the standard deviations are in the same range, we suggest that the variability between the two regions is in the same order as within them. In the revision we will do our best to write this clearer.

Figure 2: A topographic map of the area should be provided.

As we state in Section 4.1.3, P9970, L12-18, the area is basically flat with about 60 m height difference between the western to the eastern boundary of the studied SMOS pixel. Thus, we do not believe that adding contour lines would provide additional valuable information.

Additional Reference

Brocca, L., Hasenauer, S., Lacava, T., Melone, F., Moramarco, T., Wagner, W., Dorigo, W., Matgen, P., Martínez-Fernández, J., Llorens, P., Latron, J., Martin, C., Bittelli, M. (2011). Soil moisture estimation through ASCAT and AMSR-E sensors: an intercomparison and validation study across Europe. Remote Sensing of Environment, 115, 3390-3408, doi:10.1016/j.rse.2011.08.003.

Interactive comment on "A soil moisture and temperature network for SMOS validation in Western Denmark" by S. Bircher et al.

Anonymous Referee #3

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Overview
This paper serves as a SMOS validation overview using a network dataset in Western Denmark. While this paper provides substantial background for the Skjern watershed, it also compares the network data to the SMOS soil moisture data products. I agree with most of the comments of the other reviewers, though I think RMSE is a necessary metric for analysis.

Yes, we agree with you that this is an important metric. Reviewer 1 suggested adjusting the terminology as it might be misleading and we intend to adapt the RMS Difference terminology instead. Please see our comment to reviewer 1 concerning this topic.

I recommend minor revisions to help clarify some points below. The big conclusion of the paper is that the network performs according to expectations and is well-suited for its purpose. However the SMOS soil moisture product needs further correction. This conclusion is supported by the text adequately and therefor I would recommend acceptance after minor revision.

General Comments
It is unclear to me if the network installations were in the actual crop and land cover types quoted in the tables, or were they installed in grass-patches near these landcover types. Since these appear to be long term installations, I will assume they are in non-tilled/planted station locations which would mean it doesn't matter what the local land use is. Soil texture is significant and this portion is adequately covered. The land use is important for overview, but these stations are not 'in' those land cover types.

Yes, the network stations are placed in the actual crop and land cover types as given in Table 6. As we state in Section 4.2.2, P9974, L1-3, stations placed in crops have to be temporarily removed during cultivation practices (seed/plantation and harvest) – twice for summer crops (spring and fall) and once for winter crops (late summer). Reviewer 1 raised the issue of continuity in the measurements, please check our response to his comment.

Specific Comments/ Technical Corrections (P: page, L: line or lines)

Table 1: I do not understand what this table is trying to say.

This table is providing information of the topsoil (0-20 cm depth) conditions encountered within the studied SMOS pixel as well as in the corresponding working area, given in percent. The soil types are classified from 10 to 90. These numbers are then used in the sum-up to composite classes as explained in Section 4.1.3, P9971, L11-18. For each soil type the textural fractions of clay, silt, fine and total sand as well as the humus content is given (in percent).

The term working area needs to be clarified.

The working area is the total area of SMOS signal contribution of approximately 123x123 km, see Section 3.2, P9967, L23-24 and Figure 1).

When a figure has multiple plots, please label (a), (b), (c) etc and refer to them as such in the caption.

Good suggestion, we will do this in the revised version.

The authors frequently reference composite classes by number. This seems unnecessarily complex for the reader and it would be better to refer to descriptive terms. Figure 4 especially. A table with descriptive terms would better serve the reader.

Sorry for that. Our suggestion is to add a column in Table 4 with descriptive terms (e.g. ASC for land cover: arable land, topsoil: coarse sand, subsoil: clay) and then use these terms in the text instead of the numbers.

Figure 9, 2nd figure. Figure 10,3rd figure. Both of these Pcorr figures seem to have some significant rainfall events which are not apparent in the soil moisture plots. And there are also significant soil moisture increases with no corresponding precip measurements. Please explain.

That is a good point. The significant increases in the soil moisture not reflected in the precipitation measurements in the first half of March are due to snow melt. Further such increases during the growing season can be ascribed to irrigation. With respect to significant rain events not apparent in soil moisture, we believe you are mostly referring to Figure 9. In that case, soil moisture of single stations is plotted while the shown precipitation is an average over all DMI 10 km grid nodes contained within the studied SMOS pixel. Thus, it is possible that the area on average received a considerable amount of precipitation while a single station was not struck by a certain precipitation event. Short paragraphs will be added in sections 5.3.1 and 5.3.2 to comment on this issue.

Interactive comment on “A soil moisture and temperature network for SMOS validation in Western Denmark” by S. Bircher et al.

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Dear authors,
overall your paper has been well received by the reviewers. As indicated by the reviewers, althought the paper is very descriptive, it is definitely of use for the hydrologic community. I definitely wish to thank the reviewers for their input. However, the second reviewer makes an important statement with respect to "why the approach described in the paper would provide an enhanced in situ soil moisture observation". I believe that this should be very well addressed in the revised version of the paper. I follow his concern, and I was also a little puzzled about the approach.

We have tried to answer this in the response to reviewer 2.

Actually, I have some questions with respect to the design of the monitoring network. Some choices seem to be arbitrary (e.g. why was chosen for 3 clusters? and why was chosen for the locations of these three clusters? -it seems to be confined by some already existing experimental sites-).

The main reason for clustering was to limit the maintenance costs, as it is very time consuming to drive around within such an area. With respect to the chosen locations of the three clusters, we believe that we have addressed this issue in detail in Section 4.1.2, P9969 L21-23 to P9970 L1-8. However, in the revision we will make sure that the method is conveyed more clearly for better understanding.

Furthermore, a GIS study was applied to identify the different land uses where monitoring points would be required, ensuring that the different land covers are well represented. But I wonder, is this the ’optimal’ design methodology with respect to getting an average soil moisture estimate which corresponds to what SMOS ’sees’, or is it ’optimal’ for getting the best spatial average soil moisture?

We believe that this ties back to the concern on why the approach would provide enhanced in situ soil moisture observations (see response to reviewer 2). Considering the most soil moisture influencing variables in the choice of network station locations, we believe to obtain a representative spatial soil moisture average. And this is what SMOS ‘sees’ (weighted by the antenna pattern). Furthermore, the validation area (for which the network is spatially representative) is chosen in the area of major SMOS signal contribution around one node for which soil moisture is retrieved by means of SMOS measurements. Thus, no error-prone interpolation of SMOS data is necessary before the comparison with the network data.

I hope that the authors are able to address the different comments of the reviewers and that they may also provide more information on why this design procedure was selected for setting up a monitoring scheme for SMOS validation. I am looking forward to receiving the revised version. Kind regards, N. Verhoest