Review: Schlenz et al, 2012

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Title: Analysis of SMOS brightness temperature and vegetation optical depth data with coupled land surface and radiative transfer models in Southern Germany

Reviewer's summary:

Brightness temperature observations from ESA's Soil Moisture and Ocean Salinity (SMOS) satellite are analyzed with a coupled land surface model (PROMET) and radiative transfer model (L-MEB). L-band brightness temperature measurements from the SMOS satellite and the EMIRAD radiometer were used for this analysis. Additionally, the Level 2 vegetation optical depth product as derived from SMOS observations was analyzed. The timeframe over which the analysis was performed was a relatively short period (3 weeks) in the spring of 2010 and a longer period (7 months) from spring to fall of 2011. The study area was located in the Upper Danube Catchment in Southern Germany.
Relatively low (to fair) correlation was found for the comparison between the airborne EMIRAD- and SMOS satellite brightness temperature observations, moreover the SMOS brightness temperature observations did not show the expected seasonal cycle. A fairly high correlation (R=0.65) was found between SMOS L2 soil moisture and optical depth, while these two variables are considered to be independent. Furthermore, it was concluded that SMOS L2 vegetation optical depth does not seem to be a reliable source of data.
The main reason for the low similarity between the datasets was explained by RFI issue's of the SMOS satellite observations. This explanation was confirmed by comparing the different observations to outcomes of the L-MEB model.

General comment: Major points

In general the language is of fairly good level however in some cases the language needs improvements. Several times the word 'in' was used incorrect leading to a different meaning of the line. Also the author should pay attention to the use of brackets in the references. Language improvements should be implemented throughout the entire document. *The language has been improved throughout the document. The references have been reworked.*
In many cases the author makes certain statements without any justification, for example 'with good results', 'low to fair', 'less reliable', 'representative portion', 'sufficient quality' and there are several others. Such statements cannot be made without any justification in the form of scientific evidence (e.g. provide basic statistics). *Those statements have either been removed or statistical information has been added.*
Observations of the SMOS satellite and the EMIRAD radiometer were both influenced by RFI. Observations of the EMIRAD radiometer could successfully be filtered for these human induced, point source emissions (Schlenz et al. 2012a). Apparently, RFI contamination in the observations of the SMOS satellite could not (entirely) be filtered. What would be the reason for differences in these two systems? Please, detail a dedicated section, including sensor differences, to this issue since it's very important and it's relevant for the entire manuscript. Consider the structure of Chapter 2 to be: 2.1-Study area and in situ data 2.2-Remote sensing observation 2.2.1-Airborne data 2.2.2-SMOS data 2.2.3-Radio Frequency Interference 2.3-Coupled land surface and radiative transfer modeling and so forth. *Section 2 has been reworked completely, including a new structure. The sensor differences that lead to a different effect of RFI have been added. The main*

sensor differences are the different field of view and the interferometric measurement principle of SMOS which makes SMOS more RFI prone than EMIRAD.
An important message in this manuscript is that SMOS brightness temperature observations over the research area are not in line with airborne measurements and modeled brightness temperatures. Also, SMOS L2 vegetation optical depth values were found to be an unreliable source of information. The cause for the first was considered to be a single problem, related to RFI. In fact, this is a two-way problem since it could also be related to SMOS sensor accuracy, precision, calibration issues (e.g. hot load, cold load, calibration procedure) and even the sensor design itself. This should be added to the manuscript. For the latter, these multiple issues remain but algorithm problems, which were confirmed by the high correlation between soil moisture and vegetation optical depth, may be an additional cause. Most of these issues are discussed and come back several times in the manuscript, unfortunately not very well structured. So, please make sure this is well structured throughout the entire manuscript, since it's a key message in the manuscript. *This has been reworked and restructured to make it more concise. The additional possible causes for SMOS problems have been added.*

General comment: Minor points

The authors are referring to two different timeframes, the first being short term (3 weeks) and the second being long term (7 months). Be more precise (and careful) with these terminology, since in many cases (e.g. climate modelers, algorithm developers) a 7 month period is still extremely short. For such terminology ('a long term comparison') it's often required to have at least a couple of seasonal cycles, while in your so called 'long term comparison' there is not even a single seasonal cycle used. *You are right. I explain clearly now what I mean with longterm analysis. It is only called long term in comparison to the short term analysis that consists of only a few days during the SMOS Validation campaign.*
Please, don not use codes and acronyms like FL_NO_PROD, FL_RFI_Prone_V, DQX in the text since this can be very confusing. *Agreed. I explain these acronyms now.*
Several times it was claimed that there is a known bias between the SMOS satellite and EMIRAD airborne observations, statistics (e.g. RMSE) were calculated without correcting for this known bias. What would be the cause of this bias? Inform the reader about the word 'known' when referring to this bias. Also, apparently the bias is known, so why is there no correction implemented for this known bias (before calculating the statistics). *This issue is treated with more care now. I make clear in the text, that only one author reports that bias, which is the reason why I do not use this information for corrections. It still needs further investigation.*
Some images are not shown however they might contain very interesting information. Consider providing them in a next version of the manuscript. *I considered it, but decided to add no additional images as they do not make things clearer but would extend the manuscript over a readable length.*
It is concluded that RFI is the cause of most of the problems (P. 5415, L.26). Based on the performed analysis and literature, this is an unjustified statement since up to date nobody was able to separate the RFI related- and retrieval algorithm problems. Therefore it is not possible to use the word 'most', so be more considered. *Agreed. I changed the terminology and draw my conclusions with more care now.*

Recommendations:

It would be very interesting to also run the SMOS retrieval algorithm to the airborne EMIRAD observations, if possible. It might be out of the scope for this manuscript, but are there any plans

for doing so? *I agree that it would be interesting to do so. But it is beyond the scope of this manuscript. At the moment there are no plans to do so, because my colleague has done soil moisture retrievals from EMIRAD data with the LPRM model and I have performed soil moisture retrievals from another radiometer carried on board of the same aircraft, the HUT-2D. The results were interesting but I do not think it would deliver new insights concerning SMOS data at the moment.*
Another interesting suggestion would be to apply the same approach (model Tb's vs. satellite observed Tb's) to satellite observations in a different frequency of which we are sure they are not contaminated over Southern Germany (e.g. C-band observations from the WindSat radiometer onboard the Coriolis satellite). This would allow quantifying the impact of RFI in the L-band frequency. Again, this could be out of the scope for this paper, but are there any plans for doing so? *I agree that this would be interesting, but it is beyond the scope of this paper. I do not have plans to do that in the near future.*

Technical corrections:

P. 5390, L.15: Don't use R<0.5 since this could be very misleading, instead give for example the range or mean. *Done*
P. 5390, L.6: Be careful with the word 'temperature', since this could be confused with the brightness temperatures. This could be done by using 'land surface temperature' instead. *Done*
P. 5394, L.19: It was stated that it 'might be' necessary to optimize… Instead, state that it 'is' necessary to optimize… *Done*
P. 5395, L.23: Change terminology 'long term comparison'. *Done*
P. 5397, L.5-7: Quantify the heterogeneity/homogeneity of the Vils site using basic statistics, for example the standard deviation from the mean brightness temperature over the entire site. *Done*
P. 5397, L.15-16: Quantify the term 'representative portion' using basic statistics. *Done*
P. 5397, L.16: Provide reference for the statement 'EMIRAD is a thoroughly validated radiometer'. *Done*
P. 5397, L.27: Round these numbers to two decimals, what is the significance of the third digit? *Done*
P. 5398, L.8-10: Clarify. *Done*
P. 5398, L.17: Clarify how the signal and noise could be separated. *Done*
P. 5400, L.8-12: Don't use acronyms (see previous comment). *Done*
P. 5400, L.10-12: Clarify how the signal and noise could be separated. *Done*
P. 5400, L.25: Provide the two publications as reference. *They are cited in the following lines.*
P. 5406, L.17-22: Re-write this section, since the meaning of this part is not very clear. *Done*
P. 5407, L.17-22: Remove, since this information was already provided. *Done*
P. 5409, L.8-12: Please clarify this line, since this is related to the overall motivation of this entire research field. Distinguishing between dry and wet periods is essential for retrieving soil moisture from remotely sensed observations, in particular the wet periods. Also, radiative transfer modeling can be very suitable to recognize the (extremely) wet periods if the analysis is executed over a longer timeframe (several seasonal cycles) and using soil moisture anomalies rather than absolute values. *Done. I think you misunderstood the whole section. I am not writing about the difference between dry and wet periods but only about the problems of soil moisture remote sensing shortly after rain events due to standing water, interception and high soil moisture gradients in the upper centimetres of the soil.*
P. 5409, L.21-23: Remove, since this was already discussed. *Done*

P. 5410, L.28: Don't use R<0.45 since this could be very misleading, instead give for example the range or mean. *Done*
P. 5413, L.5-7: This is not a matter of 'or' but likely a combination of the two. *Agreed. Changed.*
P. 5413, L11-12: See previous comment about heterogeneity/homogeneity and basic statistics to prove this. *Done*
P. 5414, L.2: Don't use acronyms (see previous comment). *Done*
P. 5414, L.25-26: Again, this is not a matter of 'or' but likely a combination of the two. *Agreed. Changed.*
P. 5416, L.7-9: This is a major finding in this manuscript and should also be placed in the abstract. *This sentence is already in the abstract*
P. 5428, Fig.3: Provide the statistics of this timeseries (e.g. R_2, RMSE, Bias). *Done*
P. 5434, Fig.9: This image needs major format improvements. *Done*
P. 5436, Fig.11: The text provides the correlation coefficient (R), while the image shows the coefficient of determination (R_2), please line-up. *Done*