Dear Mr. Westerhoff,

Thank you very much for taking the time to read the manuscript and commenting on parts of it. We agree with you that the presentation of this research can be clarified and below you will find the answers to the overall and minor comments you made about the manuscript titled "Comparison of measured brightness temperatures from SMOS with modelled ones from ORCHIDEE and H-TESSEL over the Iberian Peninsula" by A. Barella-Ortiz, J. Polcher, P. de Rosnay, M. Piles, and E. Gelati.

OVERALL COMMENTS

The text would be somewhat clearer if input data and methodology would be separated. I now have trouble understanding what part of the input data was processed by the authors and what part of the processing was already provided with the input data set. Also, make sure you are then consistent with the past tense (e.g., we derived TB using this method). It seems like either the 2.1 and 2.2 were written by different persons, or that the authors clearly know more about the 2.2. The text does not look like a unified text. I think distinguishing between used data/models and the methodology could partly solve this. The authors are too elaborate and tend to go into 'discussion mode' in most parts of the paper where they shouldn't. For example, in the data and methods section. They should be more concise and to the point. There should be a clearer explanation of why the temporal pattern seems to match, but the spatial pattern not. There must be some areas then where the temporal pattern also does not match? Please explain clearer. Please consider the topography as one of the candidate for the difference found. Please consider separating discussion and conclusion. Please explain each topic per sub-section in a discussion, which makes it clearer (and hopefully more concise). Sorry, I stopped my detailed textual comments at the EOF analyses text, since it became too unstructured for me to understand.

We propose the following structure to clarify and be more concise in the information given in each section:

Abstract

1 Introduction

2 Data
- 2.1 SMOS retrievals of TB
- 2.2 Modelled TB: CMEM
  - 2.2.1 Input data from Land Surface Models
    - Differences between LSM hydrological schemes and temperature estimation
- 2.3 Precipitation and Land Surface Temperature

3 Methods
- 3.1 Data sampling and filtering processes
- 3.2 Comparison analysis:
  - Spatio-temporal correlation
  - EOF

4 Results
- 4.1 Comparison of measured and modelled brightness temperatures
  - Temporal correlation
  - Spatial correlation.
- 4.2 Temporal and spatial characterization of the TB error
  - 4.2.1 TB error

- Spatial patterns
- Expansion coefficients

4.2.2 LST and precipitation errors

- Spatial patterns
- Expansion coefficients

4.2.3 Analysis of CMEM assumptions

4.3 Annual cycle of brightness temperatures

5 Discusssion

6 Conclusions

Current section "Data and methods"
Following your advice, this section will be divided into two different ones: "Data" and "Methods".

Current section "Results"
We propose to divide the current section 3.1 (Comparison of measured and modelled brightness temperatures) into two sub-sections without numbering: temporal correlation and spatial correlation.

- We believe that the good temporal correlation is due to the temperature's quick response to precipitation and to a lesser extent, to the strong annual cycle of land surface temperature (page 13030, lines 22 - 25). Contrary to the temporal correlation, the spatial one is poor. This result identifies an inconsistency between the spatial structures of measured and modelled TB. To better understand this inconsistency, we performed the EOF analysis of the TB error. In the revised version of the manuscript, we will explain in a clearer way the difference between the results of temporal and spatial correlation analyses, their meaning and the extra information the EOF analysis brings to understanding the spatial inconsistency between measured and modelled TBs. It should be noted that the EOF analysis also allowed us to confirm that the temporal correlation is driven by the TB's high frequency behaviour. Otherwise, since the dominant mode of the TB error is governed by the annual cycle of the TB signal, the values obtained for the temporal correlation would have been weaker than the ones shown in Figure 1.
- The mountainous areas are identified as areas where the temporal correlation does not match and relief effects are given as a possible explanation for this (page 13031, lines 1 – 7).

To clarify the current section 3.2 (Temporal and spatial characterization of the error), we propose the following changes:

- The EOF analysis will be explained in the new "Methods" section.
- The current sections 3.2.1 and 3.2.2 will be divided into two subsections without numbering: "Spatial patterns" and "Expansion coefficients".
- The text explaining the reasoning behind our focus on LST and precipitation errors will be moved to the section 4.2.2 from the new structure and will also be restructured to make it more concise.
- We will move some parts of the text to the new "Discussion" section. For example, the paragraph discarding the LSMs as responsible for the spatial inconsistency between measured and observed TB (page 13037, lines 19 - 27).
- We will create a new section 4.2.3 "Analysis of CMEM assumptions". It will include the text which is currently at the end of section 3.2.2 about the study of how may assumptions in CMEM affect modelled TBs.

The current section 3.3 (Annual cycle of brightness temperatures) will be rewritten to clarify our

reasoning.

Current section "Discussion and conclusions"
As you suggested, this section will be divided into "Discussion" and "Conclusions". In the former we will discuss the similarities between the results obtained for the TB comparison and those from the SSM comparison (Polcher et el. 2015), which will no longer be in the "Results" section.

DETAILED COMMENTS

Page 13020
1. Abstract Just use ORCHIDEE and H-TESSEL and explain the abbreviations in the input data. I would leave out the "(CMEM)" and introduce the abbreviation in the input data section.
We agree with your proposal of not explaining the abbreviations in the abstract.

2. Line 13: "However, their spatial structures. . .". Considering the sentence before, it is not clear what their points towards.
We propose to modify the text to: "Measured and modelled brightness temperatures show a good agreement in their temporal evolution, but are not consistent when their spatial structures are compared".

Page 13020
3.Line 23: Replace 'nowadays' by 'at present' .
We agree with your proposal.

4. Line 25, use proper reference of WWDR
The reference will be added:
WWAP (World Water Assessment Programme). The United Nations World Water Development Report 4: Managing Water under Uncertainty and Risk. Paris, UNESCO, 2012.

Page 13021
5. "Remotely sensed soil moisture products have brought about new ways to perform data retrieval, adding new observations to data assimilation chains. The optimal combination of these products with modelled ones is expected to provide best estimates of the true soil moisture state." I think that ground-observed soil moisture should play a role here and should be mentioned too. Otherwise, the statement is not correct (after all, modelled ones are a guess, and remotely sensed ones are quite noisy). Or do you have a reference that claims this statement?
Ground-observed soil moisture is not included here because we can not deal with it at the scale at which this study was carried out. However, we propose to change "the best" by "better".

Page 13022
6. Line 2. Please add 'and the relatively large radar wavelength of the L-band' at the end of the sentence.
We do not understand your comment. We are using L-band data from a radiometer, not a radar. What we want to stress in this sentence is the sensitivity of this frequency band to the soil water content. Indeed, L-band is a relatively large wavelength (21 cm aprox.), but we think this is not the point.

Page 13023
7. Line 20: "Furthermore, SSM is a critical variable regarding water resources especially in the Iberian Peninsula, . . .". Why is it critical in the IP?
Surface soil moisture is a critical variable in semi-arid areas and most of the IP is semi-arid.

8. Line 21. Use ‘IP’, not ‘Iberian Peninsula’.
We agree with your proposal.

Page 13024
9. Line 17: “and SMOS TBs at the antenna reference plane were derived”. Did the authors derive these? If yes, this should be clear. Also, if yes, lines 17 and further should be also in past tense (e.g. line 18, “These TBs were first screened out . . ..” etc)
The L1C product was provided by the Barcelona Expert Center (page 13025, lines 1 to 2) and was not part of the work done for this study.

10. Line 23: “These TBs have been first screened out for Radio-Frequency Interferences (RFIs) (strong, ... from the Icosahedral Snyder Equal Area (ISEA) 4H9 grid to a 0.25 regular latitude–longitude grid, which is easier to manipulate.” Did the authors do this? If so, put it in past tense, so it is clearer whether that has been done or not. Further, is there any study known that describes the effect of the topography on incidence angle (i.e. local incidence angle)?
As in the previous comment, the L1C product was provided by the Barcelona Expert Center. The data treatment of this product has been given in this section to inform the reader about its physical characteristics. For example, the fact that TBs have been screened out for RFIs is important to know when studying the main source of the error between measured and modelled TBs. The following studies describe the effect of topography on the incidence angle of microwave radiometers: Talone et al. (2007), Pulvirenti et al. (2011), and Utku et al. (2011). To our knowledge, this effect is not corrected for in SMOS operations, they have a topography flag and do not estimate soil moisture in the locations where it is raised.

Page 13025
11. Line 23 - “The reason being that Wilheit (1978) was chosen in. . .” Please correct this to correct English into something like: The methodology of Wilheit (1978) to compute. . .. was chosen, because. . ..”. This also goes for all other times Wilheit is mentioned in the journal.
Following your advice, we will rephrase the text (see the next comment).
We do not understand what do you mean when you refer to the other times Wilheit is mentioned in the manuscript. Do you propose to replace every time we refer to Wilheit (1978) by “the methodology of Wilheit (1978)”? Should we accompany “Wilheit” by the concept “methodology”? If it is so, we believe that even if it is not written in the phrase, it is clear that we refer to it. In fact, it is said in most of the phrases where “Wilheit” (and thus the methodology of Wilheit) appears: “Wilheit parametrization” (page 13026, line 2), “the parametrization proposed by Wilheit (1978)” (page 13038, lines 24 to 25) , “the parametrization of Wilheit (1978)” (page 13045, lines 10 to 11).

12. Line 23: - “because it is more physically based..”. More physically based than what?
The expression “more physically based” is used here as we believe that the methodology proposed by Wilheit (1978) is based on sounder physical grounds than the application of the Fresnel law for estimating the surface emissivity. We will modify the text as follows: “For TB_{HT} the reflectivity of the flat soil surface is computed following the Fresnel law, so it is expressed as a function of the soil dielectric constant and the observation incidence angle. This formulation considers the emission at the soil interface. As it is simple and affordable in computing time it is commonly used for microwave emission modelling and soil moisture retrieval, as well as for operational applications (e.g. Wigneron et al., 2007, de Rosnay et al., 2009). It assumes an a priori soil moisture sampling depth, which in this study corresponds to the first soil layer of the land surface model (7cm for HTESSEL). For TB_{OR}, the multilayered soil hydrology of ORCHIDEE opens the possibility to consider the soil moisture profile and the resulting volume scattering effects on the soil emission. Therefore the reflectivity of the flat soil surface is computed using the Wilheit (1978) parameterization.”

13. Line 25: - Fresnel's law. Do you have a reference? Also, this part, up to page 13026, line 18, looks somewhat like a discussion. I think this text should be in, but please be to the point and clearer. For example: "The methodology of Wilheit (1978) was used to calculate TBor. It considers the soil . . ... etc . Fresnel's Law (ref) was used to calculate TBht. The differences between the Wilheit and Fresnel's law are: . . ."
The following reference will be added:
"Ulaby, F. T., Moore, R. K., and Fung, A. K.: Microwave Remote Sensing (Active and Passive), vol. 2. Reading, MA: Addison-Wesley, 1986."
The text referred to in this comment explains the choices made for the configuration of CMEM to model the smooth surface emissivity for TB_{OR} and TB_{HT}. Therefore, we believe that it should remain in this section.

Page 13026
14. Line 20: 'Several differences van be identified between. . .' You do not start an explanation of models like this. First start with the introduction of the two models, then very concise explain some differences, but refrain from stepping into the discussion mode. For example, start with line 24.
Thank you for this advice. The main objective of this section is not the description of models, but the difference in their hydrological scheme, as well as in their estimation of land surface and soil temperature. This is key because these differences allow to discard LSMs as the main cause of the spatial inconsistency between measured and modelled TBs. The title of the section may, however, not be clear enough and thus, we propose to change it to "Differences between LSM hydrological schemes and temperature estimation". As exposed in the response to the "overall comments", we propose to change this section to section 2.2.1 (Input data from Land Surface Models) where both LSMs will be briefly introduced. It will include a subsection without numbering (Differences between LSM hydrological schemes and temperature estimation), where the differences will be explained.

15. Line 24: "The hydrological scheme used by ORCHIDEE is based on the model of the Centre for Water Resources Research (CWRR)" Please cite the model of the CWRR model if possible.
We propose to replace CWRR by the term "multi-layer scheme". In addition, this will also clarify the text.

Page 13027 and 13028
16. The whole text is based on the difference between the two models. I think this is a wrong approach, as it makes the text very confusing. First state the two models, then concisely explain their differences. For example, for three subsections of ORCHIDEE, H-TESSEL, and DIFFERENCES would be clearer.
Following your previous comment (number 14) the main objective of the section is to provide information about the LSMs' hydrological scheme and temperature estimation, establishing that each LSM deals with them in a different way. In our opinion, a description of the models is not necessary. As explained before, we propose to restructure this section as section 2.2.1 (Input data from Land Surface Models) with a subsection without numbering (Differences between LSM hydrological schemes and temperature estimation).

Page 13029
17. Line 6-7: "Since H-TESSEL's surface state variables consist of a value each 6 h" This should have been mentioned in the explanation about the models and I cannot find it.
In section 2.2.2 (Land surface models) it is said "TB_{HT} is output at 6 hourly time steps (at 0, 6, 12, and 18)" (page 13028, lines 23 to 24). We propose to rephrase the text from page 13029 to "However, TB_{HT} consists of a value each 6 hours, and an hourly sampling resulted in data being neglected because TB_{HT}'s hours did not always correspond to those from SMOS's observations".

P13030

18. 3 Results. The author step into comparison straight away. I think they could be better off and clearer if they first present the results. So the separate results of TBSM, TBOR and TBHT with some clear explanations. Then add a section 4. Analyses of the results. Starting with 4.1 Comparison of modelled and brightness temperatures, then 4.2 Temporal and spatial characterisation of the error and 4.3. Annual cycle of brightness temperatures.

We understand your concern. However, we believe that with the modifications proposed in the "overall comments" the results will be more clearly set appart, since the information will be divided into more subsections and the discussion moved to the new "Discussion" section. In our opinion with this change there is no need of making a distinction between the presentation and the analysis of the results.

P13042.

19. Line 5: "This study complements a previous one where modelled Surface Soil Moisture (SSM) from the ORCHIDEE Land Surface Model (LSM) was compared to retrieved SSM from SMOS (Polcher et al., 2015)." This sentence states clearer that this work complements earlier work. Please use this clarity in a sentence in the introduction as well. I am sorry, I stopped at the EOF analyses, since it became too unstructured for me to understand. Please restructure first.

The "Introduction" section contains the following phrases: "The main objective of this paper is to extend the analysis of these discrepancies by comparing brightness temperatures measured by SMOS (Level 1C, L1C, product) with modelled ones obtained from the coupling of ORCHIDEE's state variables and a RTM. In addition, a second set of modelled TBs using state variables from the Hydrology – Tiled ECMWF Scheme for Surface Exchanges over Land (HTESSEL), is included in the comparison." They appear after the results of Polcher et al. (2015) are discussed (page 13023, lines 9 to 14). This is also reminded at the beginning of the "Results"section (page 13030, lines 8 to 12). We will rewrite this text to make it clearer.

We would like to end this document by thanking you for the comments made about the manuscript. However, we are sorry that you could not provide us with your comments up to the end of the manuscript as they are pertinent and help us clarify our discourse. We hope you will see a clear improvement in the presentation of the results in the revised version of the manuscript.

Yours sincerely,

Anaïs Barella-Ortiz