

Interactive comment on “Closing the water balance with cosmic-ray soil moisture measurements and assessing their spatial variability within two semiarid watersheds” by A. P. Schreiner-McGraw et al.

Anonymous Referee #2

Received and published: 2 July 2015

Overview

The authors carried out an interesting validation study on the cosmic ray sensing (CRS) technique using two experimental semi-arid watersheds in southwest US. The validation of CRS measurements is carried directly using in situ data from two dense in situ soil moisture networks and by using a “closing the water balance approach” in which the coherence of the different measured and estimated water balance components is analyzed and discussed. The soil moisture (SM) spatial variability and its relation with

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SM measurements (from in situ and CRS) is also investigated.

General Comments

The topic of the paper is well suited for the HESS journal. Indeed, CRS may offer a great benefit to many fields of hydrology (e.g., rainfall runoff modelling, satellite SM validation studies, etc. . .) hence, their validation and the test of their reliability is very welcome in the scientific literature. The authors present a number of interesting results and try to discuss them in a context of a “closing the water balance approach” which I found very interesting. Overall the paper is satisfactorily written but my thinking is that it could be significantly improved by a better organization and a better explanation of the related concepts (some paragraphs are really hard to follow). Moreover, it presents some major issues that should be addressed before it can be considered for publication. Specifically: 1) The paper contains a lot of material and information which is good from a scientific perspective but that forced the authors to not go into much details of some important concepts and descriptions. I suggest to focus on the most important results (the authors could follow the suggestions of Reviewer 1).

2) Related to the previous point, I would emphasize and improve the “closing the water balance approach” through a better description of the concepts behind it. There a number of assumptions and contradictions that need to be addressed and justified otherwise it is difficult to follow this part (e.g., $L=0$ assumption and its consequences on the subsequent analysis should be better explained). Establishing a clear method of analysis prior to present the results, other than improving the comprehension of the paper, would add value to it and to its alternative approach.

3) The role of z^* and its relation with z_m and the maximum measurement depth of the probes requires a more clear description. The authors should deserve at least a brief paragraph to this issue. Indeed, it looks like that z^* has a strong influence on the results so I suggest for instance to analyze the z^* time series and discuss further the limitations associated to its time variability and its potential effects on the result interpretation.

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4) With so much information and analyses, it is not easy to follow the manuscript. I would deserve some space in the discussion section for a table summarizing the most important points (especially those associated with the water balance approach) or include a smaller conclusion section highlighting the main findings along with the limitations associated with the CRS method. I list below that main issues I found in the paper in the order of appearance:

1) P5345 L. 14-18. Many satellite SM missions are now available and include not only passive sensors (Kerr et al. 2001) but also active (Bartalis et al. 2007) and combined (passive plus active, Entekhabi et al. 2010) sensors. I suggest to add this references to the manuscript.

Bartalis, Z.; Wagner, W.; Naeimi, V.; Hasenauer, S.; Scipal, K.; Bonekamp, H.; Figa, J.; Anderson, C. Initial soil moisture retrievals from the METOP-A Advanced Scatterometer (ASCAT). *Geophys. Res. Lett.* 2007, 34, L20401.

Entekhabi, D.; Njoku, E. G.; O'Neill, P. E.; Kellogg, K. H.; Crow, W. T.; Edelstein, W. N.; Entin, J. K.; Goodman, S. D.; Jackson, T. J.; Johnson, J.; Kimball, J.; Piepmeier, J. R.; Koster, R. D.; Martin, N.; McDonald, K. C.; Moghaddam, M.; Moran, S.; Reichle, R.; Shi, J. C.; Spencer, M. W.; Thurman, S. W.; Tsang, L.; Van Zyl, J. The soil moisture active passive (SMAP) mission. *Proc. IEEE* 2010, 98, 704–716.

Kerr, Y. H.; Waldteufel, P.; Wigneron, J. P.; Martinuzzi, J. M.; Font, J.; Berger, M. Soil moisture retrieval from space: The Soil Moisture and Ocean Salinity (SMOS) mission. *IEEE Trans. Geosci. Remote Sens.* 2001, 39, 1729–1735.

2) P5346 L5. I would move Eq. (6) and its description to the method block. It is ok to say something but putting details in the method avoids to jump from one page to another and improves the readability of the manuscript. Moreover, in this case the “closing the water balance approach” will be presented in a more consistent and general manner.

3) P5350L19: could you provide a brief justification for these choices? (i.e. the method

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used for averaging SM time series). I guess some info is contained in the references added but it would be beneficial to have something in the manuscript since the spatial mean of the probes is used as a benchmark for the comparison.

4) P5352 L12: boxcar filter. Please add a reference.

5) P5352 L23: Could you provide a clearer justification for limiting the analysis to the 50% of the source area? Which are the effects of considering smaller or larger contributions?

6) P5354 Section 2.4. This section is particularly important and should be explained better. Examples are:

- $L=0$ for short rainfall events could be reasonable but later in the manuscript L is supposed different from zero in many cases. If understand well this refers to a longer analysis period, however, I found this a bit confusing. Could you improve this part and make the text more clear?

-P5354 L8-10. $Z_m=40$ cm. Which are the potential consequences of this assumptions?

-P5354L15-18. Describe the performance metrics in separate section and remove them from the caption of Table 3.

-Can you explain $\min(z^*, z^*t-1)$ in Eq. (6)?

-If this is true that $f_{CRS}>0$ implies infiltration, it cannot be generally said that $f=I$. Indeed, at daily temporal resolution the effect of others water balance components cannot be neglected, e.g., the effect of the deep percolation, especially for JER site.

-It is not sufficiently clear how the authors compare the two SM measurements (CRS and probes) with the water balance components. For instance it is said at P5354 that Q can be derived from $P-I$ when $f_{CRS}>0$ and can be compared with Q measured but I did not find any of this comparison in the result section.

-I cannot well understand from this part how the “closing the soil water balance ap-

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proach” is finally used. I think the authors should significantly improve this part.

7) P5356 L13-19. “ Relative. . .at JER”. I would move this part from the results to the section 2.2.

8) P5356 L21-26. Why not using the SSE to quantify the seasonal differences?

9) P5358 L10. “This suggest that the three approaches. . .”The sentence is not clear, consider revising. Three approaches?

10) P5358 L14. “A closer. . . revealing”. Remove this sentence it is not clear.

11) P5359 L 1-30 – P5360 L1-8. I found this part really hard to follow. It is overall clear that the two sites show strong ecosystem differences but I am expecting a larger discussion on whether θ_{CRS} is able to close the water balance or not with respect to θ_{SN} . (in the title the authors claim this). Something is provided at P5360 L1-8 but I think it has to be expanded.

12) Table 2 information can be put in Table 1.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 12, 5343, 2015.