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Discussions

Interactive comment on "Reducing soil moisture measurement scale mismatch to improve surface energy flux estimation" *by* Joost Iwema et al.

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1 General comments

The manuscript by Iwema et al. addresses the use of new Cosmic-Ray Neutron Sensor (CRNS) data in land surface model calibration. By using CRNS and in situ sensor data from 12 sites across the U.S., the authors systematically investigate to which degree calibration against this novel data leads to an improvement in the simulation of observed surface fluxes. Unfortunately the results do not show a clear improvement of using CRNS data over in situ data, but this does not in any way affect the quality of the research. Overall, I have a positive impression of the manuscript which I believe would make a good contribution to HESS, but it will need improvements on several

aspects. In particular, some of the main conclusions do not seem to follow from the results (a problem also identified by the other referee), the authors should make clear that the in situ data is not used to its full potential so the study is not a clean comparison between datasets but rather a comparison of scale, and the authors seem to have been somewhat selective in the selection of references to identify the knowledge gap. These issues are discussed in more detail below. However I believe these comments can be addressed by minor (mostly textual) revisions.

A first remark concerns the title, which does not seem to reflect the contents of the manuscript. The study really looks at calibration using soil moisture observations at different scales, and it does not look at measurement scales as such. I suggest a title along the lines of "Land surface model performance using cosmic-ray and in situ soil moisture data for calibration". Also, the use of "reducing" is seemingly at odds with the results, which does not show an improvement when using observations at the "model" scale.

As was also pointed out by the other referee, the conclusion that JULES has a weak coupling between soil moisture and ET does not follow from the results. In fact, JULES has a strong coupling by definition, and the coupling in reality can only be less strong. The fact that ET estimates do not improve with improvements in soil moisture is likely because ET is not so sensitive to changes in soil parameters, even for different climate conditions. This is for instance shown in a paper I wrote in 2009 (Teuling et al., 2009), in which I investigated effects of soil parameters on soil moisture and ET. In short, effects of soil parameters on soil moisture are generally large, but effects on ET are small. This is primarily caused by the main effect of soil parameters which is a shift in the mean, rather than the dynamics (this is also consistent with the strong contribution of bias to MSD as reported by the authors). This should be discussed better.

Related to this is the question why rooting depth was not optimised along with the soil parameters. If the model rooting depth does not reflect the actual root profile, optimization of soil parameters along will not lead to a better estimation of available soil water. These choices should be better motivated.

In making a case for the validity of their research question, the authors miss out on an important body of literature on spatio-temporal characteristics of soil moisture fields. It has been shown by numerous studies that while soil moisture generally shows a large spatial variability, individual points maintain their rank while the mean changes. This insight started with the “classical” Vachaud et al. study, but numerous other studies (for instance Teuling et al., 2006, Mittelbach and Seneviratne, 2012, among others) have reported similar behaviour. This behaviour implies a relatively small spatial variability of fluxes, which was explained from a theoretical perspective by Albertson and Montaldo (2003) and explored Teuling and Troch (2005), among others. In effect, based on these studies, the hypothesis could also be formulated more neutral in the form of a null-hypothesis: “A reduced scale mismatch does not lead to LSM flux estimates closer to eddy covariance observations..”. The results could subsequently be interpreted as insufficient evidence to reject this hypothesis. In any case the introduction should be changed to include a discussion on the relation between point-scale and large-scale soil moisture. This can then also be used to explain the reference to Franz et al. (2012), which now conflicts with the hypothesis (if CRNS and in-situ soil moisture have already been reported to be similar, how can the authors still expect to find a difference in fluxes?). These comments do not make the research any less relevant, but the phrasing of the hypothesis should be in line with the “state-of-the-art”, and not a convenient selection thereof.

In applying CRNS and PS soil moisture products, the authors only consider the shallower PS soil moisture observation in order to comply with the CRNS observation depth. While this makes sense given the goal of the study (scale mismatch and not a comparison of observation techniques), it is not sufficiently recognized that this introduces an unfair disadvantage to the PS data. There might be important information in deeper PS observations, in particular during soil moisture-limiting conditions, that is not being considered in this study. It is thus crucial to make a clear distinction between the

scale aspect and the observation technique, and acknowledge in the discussion in the discussion that PS soil moisture might give better results when all available observations are used. A better alternative would be to redo the analysis using all observation depths in addition to using only the shallow observations, but this would likely require a substantial amount of work.

2 Detailed comments

Page 1, Line 27: it is not the coupling between soil moisture and flux partitioning that is strong (this is similar across climates), but the soil moisture control on temperature at seasonal timescales.

Page 3, Line 15: If the CRNS data is known to give similar values as the in situ data, how can this be consistent with the hypothesis posed later on? Please change the line of reasoning, but also take into account my previous comments on missing references on soil moisture temporal stability.

Page 5, Line 28: Richards equation -> Richards’ equation

Page 13, Line 13: “We could think of...” ->All arguments are backed up by references, so it might be better to say “Several reasons have been proposed...”

Figures: Generally ok, but I agree with the comments of the other referee that the number can be significantly reduced without sacrificing the main message.

3 References

Albertson and Montaldo, 2003. Temporal dynamics of soil moisture variability: 1. Theoretical basis, Water Resour. Res., 39, 1274.

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