Response to comments by Referee #1 (H. Bogena) on "Examining the relationship between intermediate scale soil moisture and terrestrial evaporation within a semi-arid grassland" by Jana et al.

> *We greatly appreciate the review comments and thank the reviewer for their effort. We have addressed all of the comments and present our responses below.*
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> *The review comments are in regular typeface, while all responses are in italics and indented paragraphs.*

This MS compares measured and modelled actual evapotranspiration (ETa) fluxes with soil moisture dynamics determined by a cosmic-ray probe for the same site located in Australia to analyse the coupling of these processes.

The MS presents an interesting application of Q–Q plots for comparing the shapes of distributions of soil moisture data and modelled fluxes of actual ET in order to evaluate coupling of land surface processes. The MS is well written and the topic fits well to the scope of this journal.

However, there are several issues regarding the methods and the interpretations of the results (see specific comments). At this stage the results are not sufficient enough to support the interpretations and conclusions. The authors seem to have limited knowledge concerning soil hydrological processes and the CRP method and it would be advisable to add an expert of these topics to the authorship. Additional analysis of the data is needed to support the conclusions.

> *We thank the reviewer for their supportive comments regarding the manuscript. However, we disagree with the reviewer's thoughts on apparent issues related to the methods and interpretation of our results. We consider the results as sound and suspect there has been some misunderstanding of the approach and the analysis undertaken. We have endeavored to make this much clearer in the revised manuscript and in the responses to the specific comments below.*

Chapter specific comments

1) Introduction
The introduction chapter is somewhat confused and includes several repetitions. It needs to be rewritten in a more concise and better structured way. In addition, more appropriate research questions or hypotheses need to be formulated and the structure of the paper should be presented.

> *We have gone through this section with an eye towards readability and comprehension and have rearranged sections as well as more clearly articulating the rationale and motivation behind this work. We have stated the motivation (P3, L6-9, "… identifying complimentary observation sources that can be used to improve upon the evaluation of a variety of hydrological processes is a much needed objective (McCabe et al. 2008). This critical need forms a key motivation of this work where we look for an answer to the question: are independent hydrological data-sets available that can be used to inform upon linked elements of the hydrological cycle?"), the rationale (P4, L6-10, "With improved sensing of the root-zone soil moisture, it is expected that any modelled relationship between evaporation and soil moisture will be more robust. From an observational standpoint, however, it has been challenging to explore these links directly due to the mismatch in data scales. Using CRNP soil moisture data collocated with gridded model estimates of evaporation may provide some insight into these processes and relationships.") and the objective (P4, L12-13, "… the objective of this study is to investigate the potential of using CRNP soil moisture retrievals to*

evaluate modelled evaporation estimates derived from a combination of tower based and remote sensing inputs.”).

There are many different terms related to processes of evaporation are used in the MS with different meanings, which is confusing for the reader. For instance, it should be stated clearly when the process of “total actual evapotranspiration” is meant, e.g. indicated with the acronym “ETa”.

We have updated the manuscript and only use the term “evaporation” to represent land surface evaporation, which comprises of evaporation from soil and canopy, as well as transpiration from vegetation. This is standard and accepted terminology.

Instead of using the acronym “COSMOS”, which is basically the US network of cosmicray neutron probes, the term “cosmic-ray neutron probe” or CRNP is more appropriate (see e.g. Bogena et al., 2015).

We have replaced “COSMOS” with “CRNP” in all cases.

It is wrongly stated that the CRNP have footprint of 300-400 m radius and that the footprint of flux measurements by an EC-tower would be much smaller. In fact the footprint size of a CRNP typically smaller than 300 m radius (see Köhli et al., 2015) and the average footprint of an EC-tower is typically larger, integrating areas larger than 50 ha (e.g. Graf et al., 2014).

We have revised the text to reflect the updated footprint radius of the CRNP sensor, as given by Kohli et al, 2015.

The dynamic footprint of an EC tower depends on the height of the tower and roughness of the surface, as well as wind-speed influences. For a 2 m tower height and pasture, the footprint remains within the range of a few hundred meters, with the standard rule of 1:100 for fetch distance as validated by Leclerc and Thurtell (1990). We have updated the text (P5, L31) to specify the height of the EC tower. In our particular case (and a large number of Fluxnet installations), the fetch of the EC tower is comparable to the scale of the CRNP footprint.

The footprint area cited by the reviewer from the article by Graf et al. (2014) is based on a tower height of 38m, which is much higher than the tower at this study location.

Leclerc, M.Y., & Thurtell, G.W. (1990). Footprint prediction of scalar fluxes using a Markovian analysis. Boundary-Layer Meteorology, 52, 247-258

It is wrongly stated that a large number of point measurements are not feasible. However, recently established critical zone and terrestrial observatories provide exactly this kind of data (see e.g. Bogena et al., 2015; Qu et al., 2015)

While a limited number of observatories are providing fine resolution soil moisture data, they involve significant outlay of finance, physical effort and time, as compared to, for example, utilizing scaling schemes, or installation of intermediate scale sensors. Although such observatories are invaluable in providing data to understand the underlying processes, it remains impractical to implement a large number of sensors across any and every field of interest. The statement remains very much correct in this context, but we have adjusted the text to include the observatory concept.

2) Data and Methodology

The three models are only described very rudimentary. The basic equations and flowcharts of the algorithms should be presented to better demonstrate the differences in the methods. This information could be added as a chapter “supplementary materials”.

The three models used in this study are well-established and commonly used models, with extensive coverage in the recent literature. We have provided a number of key references to articles in which they are described in more detail. We also provide references to articles where the three methods are compared to each other. Given their extensive appearance in the literature, we feel that it is not necessary to repeat the description of these models in intricate detail in this paper.

In addition, the input data used for each method should be presented separately. For instance it would be very important to know which soil moisture data was used for the modelling. It is unclear for which reasons the TDR measurements are used in this study.

There seems to be some misunderstanding in relation to the evaporation models used here. No soil moisture observations were used in any of the three models mentioned in this study, as it is not a requirement. To make this clearer to the reader, we have mentioned the inputs required for each in the model descriptions.

The ancillary TDR measurements were used to confirm the validity of the CRNP soil moisture time series.

3) Analysis and Discussion

Comparing the change in root zone soil moisture with changes in ETa on a daily time scale is not appropriate, given the large differences in temporal dynamics, i.e. soil moisture changes much slower and with time lags compared to ETa, which responses to short-term changes of the meteorological forces.

Fundamentally, changes in soil moisture will ultimately be reflected in changes in the evaporation response: the issue of time scale is of course critical in this mass balance approach. For this setting, examining changes at the daily scale seems to be an appropriate resolution, as borne out by the results. It is precisely because of this temporal mismatch that the quantities are compared at the distribution level, rather than point to point. As expressed in Figure 2, the day to day changes in the soil moisture and ET follow very similar distributions.

Arguing that CRP and EC measurements are “rather inferred than measured” is not appropriate. To argue that these measurements a less accurate than model results is a strong statement and needs quantitative proof. Please provide measures for the accuracy of both measurements as well as for the model results.

The reviewer appears to have misunderstood the statement in P7, L20. We mention that the observations of soil moisture and evaporation are “inferred” as in they are indirectly measured. For example, field sensors do not directly measure the soil moisture, but other quantities such as the dielectric constant or the neutron counts. These are then converted to the quantity of interest, i.e., the soil moisture, using a conversion algorithm. Uncertainties and errors are bound to be introduced at each step, thus reducing accuracy. This is an entirely appropriate (and well accepted) rationale.

That being said, nowhere in the manuscript do we imply or insinuate that the observations are less accurate than model results. Without particular reference, we are not sure where the reviewer gets this impression in the text.

It is argued that the CRP shows higher variability compared to TDR because it integrates over greater penetration depth. This is wrong for several reasons. First, the integral measurement of soil moisture over a profile should be less dynamic than a point measurement near the surface (e.g. 10 cm). Second, the CRP shows more dynamics, because the measurement sensitivity decreases exponentially with depth. That means the variations of the first cm below the surface are most important. In addition, the CRP is also sensitive to water stored above the surface, e.g. intercepted by leaves and litter layer (see e.g. Bogena et al., 2013).

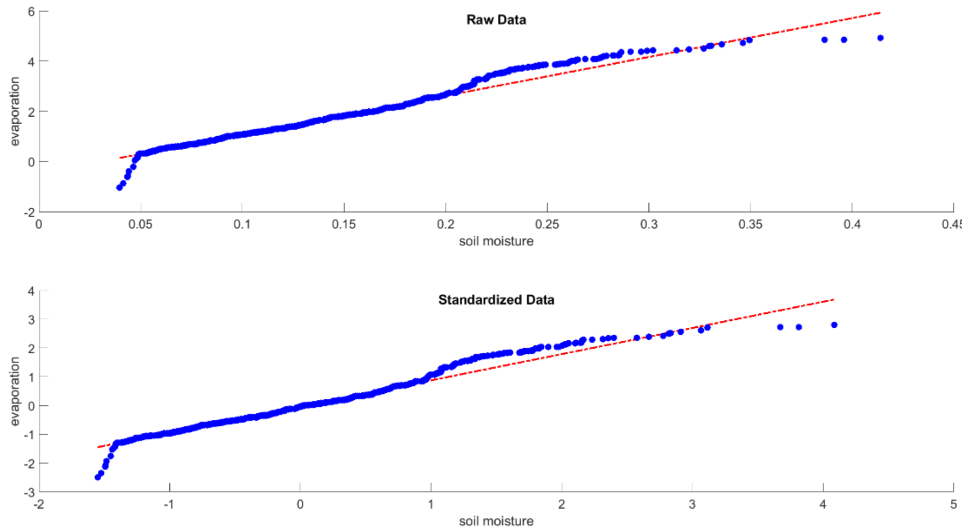
This is certainly valid reasoning and we have adjusted the text to better reflect our meaning. It should be noted that the CS616 TDR used at the site also provides a (vertically) depth integrated measurement (0-30 cm), not a truly point scale estimate. The text now reads as:

"While it might be expected that the TDR data should display greater variability in response, the CRNP measurements have higher variability in soil moisture values. This could be due to factors such as the variability in the measurement depth of the CRNP with change in the saturation, and higher sensitivity of the CRNP to near-surface moisture as compared to deeper layers (Bogena et al. 2013). High frequency variations at the soil surface may also be attenuated in the TDR signal since it is integrated over the 30 cm probe depth."

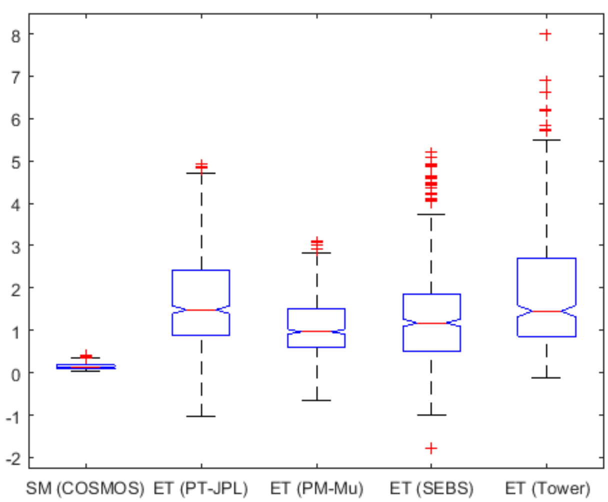
Regarding the sensitivity of the measurement, the soil moisture data has been corrected for other sources as explained by Hawdon et al., 2014. At this semi-arid rangeland site, interception and litter are not factors of great influence.

It needs to be checked if the data standardisation has an effect on the Q–Q plots. I might be possible that the agreement is partially due to this procedure. I suggest to add an ANOVA test using the non-standardized data including the p-value.

Standardizing the data has no effect on the shape of the Q-Q plots, since the plots depend on the residuals. The only difference is in the numerical value of the axes. An example is shown below with CRNP soil moisture and PT-JPL evaporation data from the entire period of record. The first panel is the Q-Q plot for the raw data, while the lower panel is for standardized data. As can be seen, there is no difference in the shape of the plot. Standardizing the data has just scaled the values differently.



As mentioned in the text (P8, L16-17), the p-value is an indicator of the similarity of the mean values of the two quantities being compared. In our case, the two quantities, soil moisture and evaporation, have vastly different ranges and units, and thus, mean values. Performing an ANOVA test between two such datasets can only provide a p-value of 0, as shown in the example below. The ANOVA is performed on the raw dataset for the entire period of record used in the study. As can be seen, the boxplot for the soil moisture is crushed due to the range of the evaporation data, and the p-value is essentially 0.



ANOVA Table

Source	SS	df	MS	F	Prob>F
Groups	1230.01	4	307.502	330.09	9.21512e-236
Error	2802.19	3008	0.932		
Total	4032.2	3012			

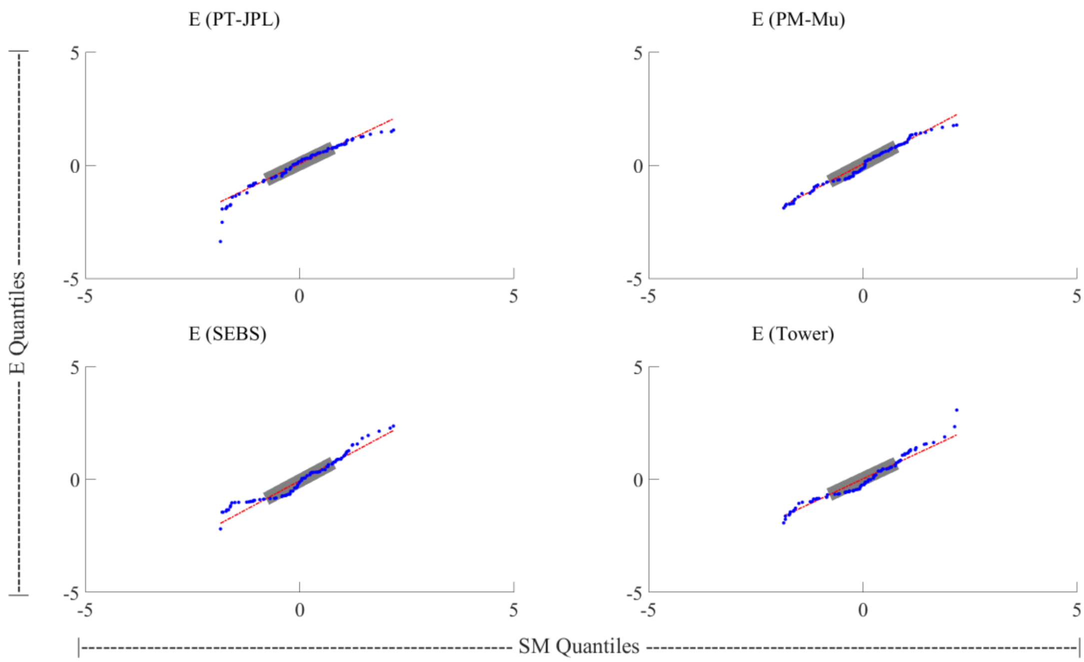
p-value

Why does the SEBS model produce more outliers?

The SEBS model is more sensitive to uncertainties in land surface temperature as compared to the other two models. This could make the model outputs behave in a different manner, especially at the extremes. (P11, L19-21)

The reasoning behind the selection of the subperiods is not well visible in the data presented in Figure 3. Why is the highly dynamic and thus interesting period between subperiods 1 and 2 not included?

The period between sub-periods 1 and 2, while highly dynamic, was close to being simply a scaled version of the total period of record. This is borne out in the Q-Q plots below for that period (DoR 230-320).



Hence, it was felt that analyzing periods of distinctly different behaviors, as described in the text for the four chosen sub-periods, would provide a better understanding of the correspondence between the soil moisture and evaporation signals under different situations.

I have difficulties with the statement the similar distribution as shown by the Q-Q-plots alone demonstrate that ETa is driven by rot zone soil moisture. The low correlation of the raw data is telling us a different story. Therefore, this statement needs to be substantiated with further analysis.

Q-Q plots are a commonly used tool to assess similarity of distributions. Correlation is a point-to-point statistic. We have shown in the manuscript (and referred to other studies with similar deductions) that such point-to-point statistics are not necessarily the best way to evaluate correspondence between two stochastic variables, especially those whose process time scales are different. In such a scenario, distribution matching is a better option. Since the line plots of the soil moisture and evaporation do not match, but the distributions do, it is a logical inference that the two quantities are behaving similarly at the distribution level. Hence, the deduction that the soil *moisture (root zone since the CRNP is measuring over depth) is still driving the evaporation process.*

The statement that low temperatures have decoupled soil moisture and air humidity duing period during period 4 needs to be better explained.

We have updated the text with additional explanation. The section now reads as:

"It is also likely that low temperatures and additional hydro-meteorological factors could have caused a de-coupling of the soil moisture from the air humidity. Due to the low temperatures, the air humidity would be lower, while the frequent precipitation ensures high soil moisture content. This creates a steep gradient for the moisture at the soil-air interface. In such a scenario, regardless of the presence of abundant soil moisture for evaporation, the models which use air humidity as a surrogate for soil moisture may report lower estimates compared to those observed from the eddy-covariance tower."

It is argued that long periods with no rainfall lead to a disconnection of soil moisture and ETa due to non-monotonic variations in soil moisture. I cannot follow this reasoning. Please explain in greater detail.
A soil moisture profile does not become heterogeneous. Do you mean that soil moisture gradients increase?

The soil moisture profile is said to become heterogeneous since the surface and deeper layer moistures are driven by different processes, and are not linked to each other. We have updated the text with some additional explanation for the terminology. The section now reads as:

"However, there were also long periods with no rainfall events. Combined with the higher temperatures of summer, this leads to greater non-monotonic variations in the soil moisture signature, thus creating a disconnect with the evaporation patterns. There are more switches between moisture-constrained and energy-constrained conditions during this season. It has been demonstrated previously that the occurrence of hot and dry periods leads to de-coupling of soil moisture and evaporation (Pollacco and Mohanty 2012). The soil moisture profile in such situations becomes heterogeneous in that the process driving the surface soil moisture variability (mainly soil evaporation) no longer influences the deeper layer soil moisture variability (mainly due to transpiration). Further explanation of this de-coupling process can be found in the article by Pollacco and Mohanty (2012)."

The statement that ETa models should be validated using soil moisture data is absurd since soil moisture is an important variable of ETa models.

This seems a case of a misunderstanding by the reviewer regarding the evaporation models, rather than an absurdity on the part of the authors. In the range of models examined, and in the vast majority of satellite based evaporation models, soil moisture does not feature as an input variable. Here we exploit the physical mechanism that makes soil moisture a key driver of the evaporation process. As such, it makes perfect sense to validate models using observations that govern or influence that process to a significant extent. It is the same rationale that one might use to evaluate spatially distributed soil moisture maps by using rainfall fields. Indeed, it is this reasoning that is at the heart of the approach explored here. Given the general lack of observation data concerning any specific process, it is important that independently observed, yet physically linked variables, be used to aid in the evaluation process.

Literature

Bogena, H.R., R. Bol, N. Borchard, et al. (2015): A terrestrial observatory approach for the integrated investigation of the effects of deforestation on water, energy, and matter fluxes. Science China: Earth Sciences 58(1): 61-75, doi: 10.1007/s11430-014-4911-7.

Bogena, H.R., J.A. Huisman, C. Hübner, J. Kusche, F. Jonard, S.Vey, A. Güntner and H. Vereecken (2015): Emerging methods for non-invasive sensing of soil moisture dynamics from field to catchment scale: A review. WIREs Water 2(6): 635–647, doi: 10.1002/wat2.1097.

Bogena, H.R., J.A. Huisman, R. Baatz, R., H.-J. Hendricks Franssen and H. Vereecken (2013): Accuracy of the cosmic-ray soil water content probe in humid forest ecosystems: The worst case scenario. Water Resour. Res. 49 (9): 5778-5791, doi: 10.1002/wrcr.20463.

Graf, A., H.R. Bogena, C. Drüe, H. Hardelauf, T. Pütz, G. Heinemann and H. Vereecken (2014). Spatiotemporal relations between water budget components and soil water content in a forested tributary catchment. Water Resour. Res. 50(6): 4837–4857, doi: 10.1002/2013WR014516.

Köhli, M., Schrön, M., Zreda, M., Schmidt, U., Dietrich, P. and Zacharias, S.: Footprint characteristics revised for field-scale soil moisture monitoring with cosmic-ray neutrons. Water Resour. Res., 2015.

Qu, W., H.R. Bogena., J.A. Huisman, J. Vanderborght, M. Schuh, E. Priesack and H. Vereecken (2015): Predicting sub-grid variability of soil water content from basic soil information. Geophys. Res.Lett. 42: 789–796, doi:10.1002/2014GL062496.