Author Response to reviewer comment #1

Improved representation of soil moisture processes through incorporation of cosmic-ray neutron count measurements in a large-scale hydrologic model

Fatima et al.
Hydrol. Earth Syst. Sc., doi:egusphere-2023-1548

RC: ***Referee Comment***, AR: *Author Response*, ☐ Manuscript text

RC:1. ***The reviewer thanks the authors for addressing many of the points raised during the review. There are, however, still some open points which require to be addressed. The authors' reliance on citation of literature appears to be selective, raising concerns that they may be attempting to back up their arguments through simple reference rather than substantive engagement with the cited works. This practice, characterized by citation on a keyword basis without thorough consideration of contextual nuances, risks oversimplifying complex scientific concepts and extrapolating findings beyond their appropriate scope, which may impact the validity of their arguments. Consequently, this approach undermines the integrity of the scientific discourse by potentially perpetuating misconceptions and failing to address or acknowledge inherent limitations or uncertainties within the literature. An illustrative example of this tendency is evident in their persistence regarding the potential influence of speculative factors such as cow activity on model offsets, without adequately substantiating these claims or conducting investigations to validate such hypotheses. Additionally, their insistence on employing a variety of statistical performance measures to evaluate a systematically offset model appears to be a misguided attempt to obfuscate inherent shortcomings rather than directly addressing or mitigating them. This approach not only distracts from the clarity and focus of their analysis but also raises doubts about the overall approach of the model and the transparency of the authors in reporting their findings.***

AR: *Thank you for taking the time to review our manuscript.*

Detailed comments to the reply of the authors:

RC:2. ***"however, our experiments demonstrated that varying soil depths from 3 to 6 layers did not have a substantial impact on the simulated neutron count results in our model setting" (...) The reviewer had raised suspicions about the coarse layer structure as hydrologically the topsoil dynamics happens on a much smaller scale and that could have implications on the CRNS signal. The reviewer suspected that there is a scale mismatch between a layering that might be appropriate for hydrological purposes and CRNS which is sensitive to dynamics on a smaller scale. To be precise: In the way the authors have structured their simulations, they reduce most of the signal dynamics measured by the CRP to two layers. The question of the reviewer may have been misleading as the referring of the authors to literature which used a similar layering for other reasons it would is not directly in favor of their argument. The additional material which is presented by the authors does not support the challenged conclusion. They show, that there are significant deviations for the simulations using 3, 5 or 6 layers. As the residuals are better in the three-layer case, one can assume that the authors have chosen the more coarse representation in order to yield better results. Typically, a more fine layered representation would lead to converging results.***

In case significant deviations or alternating fit qualities, it hints that there are further systematics with respect to that model parameter. As in the original manuscript of the authors the vertical weighting function was presented incorrectly, this was reason enough for the reviewer to suspect a systematic error based on the choice of layers. However, the only conclusion which can be drawn from the material the authors present here is, that the results are (still) biased by the choice of layers. In case the authors chose to model their system less granularly to yield a better fit quality, the unknown system bias might yield systematically wrong representations. Given that using fewer soil layers reduces specifically the residuals in situations where the deviations are surprisingly large, the assumption is that the authors might simply introduce new errors to compensate other model errors. It could be the case that the authors have - involuntarily as due to their arguments - chosen a for CRNS representative reduction of the layering scheme. That, however, would need to be analyzed separately.

AR: *The mHM is a large mesoscale hydrological model and we are showing the compatibility of how can we account for neutron count measurements in such a model while respecting the relevant processes and scales represented by it. We are aware that real world is continuous, but for a hydrological model's conceptualization we need to divide the soil moisture profile (i.e. models' subsurface) into layers to simplify the complex equations governing water flow. This facilitates computationally efficient operation of the model on relevant scales. Moreover, the available soil datasets are very coarse and do not allow for more detailed vertical resolution.*

Hence, we chose the layering based on widely accepted experience, given the available datasets representing soil properties. mHM setups prescribe the soil layering according to the soil dataset available. In global applications we use Soilgrids v2 which prescribe layers as follows: 0–5 cm, 5–15 cm, 15–30 cm, 30–60 cm, 60–100 cm, and 100–200 cm `https://soil.copernicus.org/articles/7/217/2021/`*. In Germany, we use a much accurate soil map provided by the Federal Institute for Geosciences and Natural Resources (BGR, 2020). The dataset (BUEK 200; BGR, 2020 at available at a resolution of 1:250 000) was discretized at 100 × 100 m^2 grid cells with varying soil horizons according to corresponding soil ID. The soil horizons in BUEK 200 vary from 1 to 7. For these reasons, for the German Drought Monitor at 1 × 1 km^2 resolution, we upscale the soil parameters with MPR (Samaniego et al., 2010) and homogenize the soil layering based on end-user's feedback, namely 0–25 cm, and 25–60 cm, and 0–60 cm depths Boeing et al. (2022). The soil layering of mHM is similar to other land surface / hydrological models.*

As we showed in our previous response in different layering we don't have a high sensitivity of our results. ***Probably we were not clear enough as we showed in the last response that there was no significant deviation in neutron counts simulation for different selections of layering in this study.*** *There will be certain degree of dependency of the results depending on this selection but this is inhabitable the focus of this study not about layering. The choice of layering in this study was also made by the availability of the soil datasets, in this case we used (BGR, 2020) which is a global dataset that is not detailed enough to allow for finer vertical resolution.*

Following the new GDM setup presented by Boeing et al. (2022), we adopted the upscaled soil layering because the present study contributes to the improvement of this system with novel soil monitoring sensors more appropriate to the resolution of the model.

We acknowledge that the formula for the vertical weighting function presented in the original manuscript was incorrect. However, we have verified that the corresponding Fortran code in the mHM model is accurate.

RC:3. ***"Thank you for bringing up t at the other N-SM conversion functions (...)" Thank you for providing additional context regarding the selection of N-SM conversion functions in your study. However, the reviewer would like to address a couple of points: The statement regarding the UCF method from Franz et al. (2013) having low experimental performance in the past, as demonstrated by McJannet et al. (2014) and Baatz et al. (2014), may not accurately represent the broader literature where similar issues have been raised for the N_0 method as well. Therefore,***

it would be more appropriate to acknowledge the mixed findings in the literature rather than categorically dismissing the UCF method based on selected studies. Regarding the recent UTS method from Köhli et al. (2021), it is important to recognize that while it may require further validation, it still represents a noteworthy advancement in the field of N-SM conversion functions. Dismissing it solely on the basis of its publication date and perceived complexity may overlook potential benefits it could offer, especially if it proves to outperform existing methods in certain scenarios. Overall, while the reviewer appreciates the thorough explanation provided for the choice of COSMIC and Desilets methods, it is essential to maintain a balanced and nuanced perspective on the various N-SM conversion approaches available in the literature.

AR: *We appreciate your comments on the N-SM conversion functions from the literature. However, we have selected the Desilets method and the COSMIC method for specific reasons. Both methods require information from the soil profiles, which is readily available in the mHM model. In contrast, the Universal Transport Solution (UTS) function couples soil moisture with air humidity in a non-separable way, while no atmospheric information about air humidity is available in the distributed hydrological model mHM. Same holds for the UCF function, which additionally requires a number of parameters that relate to hydrogen pools that are not represented by mHM. We agree that these methods together with additional model parameters would have the potential to improve the results of this study, but the implementation of these additional components into mHM is far beyond the scope of our study.*

We have added this discussion to the manuscript.

> Previous studies, such as McJannet et al. (2014) or Baatz et al. (2014), have noted low experimental performance for the Universal Calibration Function (UCF) method described by Franz et al. (2013). However, we have selected the Desilets method, known as the N_0 method, and the COSMIC method for specific reasons. Both methods require information from soil profiles, which is readily available in the mHM model. In contrast, the Universal Transport Solution (UTS) function couples soil moisture with air humidity in a non-separable way, while no atmospheric information about air humidity is available in the distributed hydrological model mHM. The same holds for the UCF function, which additionally requires a number of parameters related to hydrogen pools not represented by mHM.

RC: 4. ***"We are using the five most established measures in hydrology to evaluate the model performance (...)" Thank you for providing insight into your approach to model evaluation and the rationale behind using multiple statistical measures. However, it is important to note that while employing a variety of evaluation metrics can provide a more comprehensive assessment of model performance, it does not inherently address the systematic uncertainties introduced by modeling choices. The use of various performance measures may indeed offer a more nuanced understanding of model behavior, capturing different aspects such as bias, dynamics, and temporal errors. However, it is essential to recognize that these measures are still influenced by the underlying assumptions and parameterizations of the hydrological model itself. As such, simply presenting results that are consistent across multiple measures does not necessarily guarantee robustness in the face of the evident model uncertainties. In addressing the concerns raised by the reviewer, it would be beneficial for the authors to provide a more transparent discussion of the modeling assumptions, limitations, and potential sources of uncertainty. This would help contextualize the interpretation of the evaluation results and provide a clearer understanding of the model's performance and its implications for CRNS measurement in combination with hydrological modeling.***

AR: *We agree that from the model performance matrices the uncertainty cannot be reduced, but we can check the model performance. The source of uncertainty can arise from model parameterization, specifically the representation of soil moisture in the mHM model across different sites. For further* *details on parameterization, the Supplement provides extensive information on both prior and posterior solutions of each parameter for each site. The mHM currently lacks a vegetation dynamics module and that need to be improved.*

Uncertainties related to model simulation are discussed in the manuscript in section 3.2. These topics are also discussed in the conclusion section of the manuscript.

RC:5. ***"As was demonstrated in a large number of accepted literature, COSMIC is an analytical-based model incorporating key physics-based processes important for CRNS applications in conjunction with models (...)" The authors might mistake an "analytical model" for a "physics-based model". While it is true that COSMIC is an analytical forward-operator which mimics physical processes it does not mean that there are actually representations of physical processes. The authors here are probably subject to a 'non-sequitur' error. Correlation does not mean causality. COSMIC eventually uses an exponential function with empirically determined parameters. These parameters unfortunately do not correspond to the physical quantities they are supposed to stand for. The COSMIC approach is similar to the UCF approach by Franz et al. (2013). Desilets before used a hyperbola for describing the N-SM relation. Köhli et al. (2021) showed that the most realistic representation of the measured CRP intensity can be described by a combination of hyperbola and exponential function. As far as an exponential function can represent the N-SM relation to some extent, it does not mean COSMIC with its exponential function represents physical processes in any way. The be clear on that point: COSMIC does not use in its analytical description any physically correct attenuation lengths, instead, if one would require to use such instead of empirical parameter adaptations the equation would not work. Furthermore: COSMIC is described as focusing solely on the influence of locally and directly transported neutrons to the detector, which suggests it may not adequately represent the complete physics of the CRNS method. This limitation could undermine its claim to be a physics-based model if it neglects important physical processes. COSMIC makes several assumptions, such as the belief that neutrons in the soil are only produced by other high-energy neutrons, which may not accurately reflect the true physics of neutron interactions in the environment. This suggests a potential oversimplification or misunderstanding of the underlying physical principles. Additionally, researchers have raised criticism regarding the accuracy of mathematical formulations and calculations within COSMIC, including errors in integrating equations in cylindrical coordinates and inaccuracies in mathematical expressions. These issues suggest a lack of rigor and precision in the model's implementation, which is crucial for any model claiming to be physics-based. As the authors in a later statement pave the ground for an entirely opportunistic choice "Hence, we consider the 1D model assumptions adequate for the target application.", the reviewer asks the authors to acknowledge this in their manuscript.***

AR: *We agree that calling the COSMIC model "physics-based" might be a misleading term, although it has been used in previous literature to describe COSMIC. For an analytical approximation of natural phenomena, it is just natural that every single physical process cannot be represented in great detail. Hence, it is also not logical to assume that the parameters of the model are actual physical attenuation lengths. Instead, COSMIC only mimics the overall picture of the neutron-soil interaction phenomenon, and their parameters are effective representations of the processes. In particular, neutron attenuation lengths do not only depend on the material, but also on the neutron energy itself. Since the neutron constantly changes its energy during the path through the soil, one would have to use an infinite number equations and attenuation lengths to mimic this process. Instead, the equations in COSMIC average out all the different processes and resemble the average attenuation in the material with effective parameters.*

That said, COSMIC is rather an analytical model than a physical model. We have changed this terminology in the manuscript to avoid further confusion.

In this study, we refer to COSMIC as an "analytical model" or "emulator" because it simulates neutron counts based on simplified, empirical relationships rather than explicitly representing the *complex physical processes. As acknowledged by Köhli et al. (2021), these simplifications result in a model that is not entirely physics-based but serves as an approximation.*

> Here, we test three approaches, (i) the direct calculation of neutrons from the equal-averaged SWC profiles based on (Schrön et al., 2017), (ii) the same with ~~a~~ weighted-average ~~profile SWC~~ soil moisture profiles based on Schrön et al. (2017), and (iii) the ~~physics-based model~~ neutron forward operator COSMIC by Shuttleworth et al. (2013) .

> Two empirical and one ~~physical model~~ the forward operator a COSMIC approaches are evaluated for deriving ~~neutrons~~ neutron counts from the soil moisture profile.

> The COSMIC operator also accounts for the full soil moisture profile, ~~but in a more physically behaved manner,~~ following the track and attenuation of the neutrons in and out of the soil column.

> On the one hand, it is a ~~physics-based approach incorporating a comprehensive representation of the neutron counting process~~method that aims at mimicing the physical processes of neutron transport in the soil in detailed way, but on the other hand, it relies on the detailed representation of the site characteristics in the hydrological model.

RC:6. ***"Thank you for this remark. The site-specific nature of the N_0 parameter is a well-recognized aspect within the Cosmic-Ray Neutron Sensor (CRNS) community (...)" That response unfortunately contains several logical shortcomings:***

- ***The authors cite various studies to support the assertion that N_0 is site-specific. However, the mere mention of previous research without providing specific evidence or logical reasoning does not sufficiently substantiate the claim.***
- ***Additionally, the observation of non-identical N_0 values across different sensors, as noted in the referenced studies, does not inherently establish the site-specificity of N_0, especially as the authors also cite studies which have shown the opposite of consistent N_0 values.***
- ***The author suggests that because the mHM model does not explicitly incorporate site-specific influences on N_0, the value of N_0 is inferred solely through the calibration procedure.***
- ***This oversimplified inference overlooks potential complexities involved in determining the site-specific nature of N_0 and assumes that the model's omission of certain factors implies their negligible impact on the N_0 determination.***
- ***As the authors state in their response: Other factors beyond site characteristics may influence the calibration process. This neither means that the N_0 parameter should be site-specific nor theoretically is site-specific.***

AR: *Thank you for your detailed feedback on the site-specific nature of the N_0 parameter. I appreciate your observations and agree with the points you raised about the N_0 value. Therefore in the revised manuscript, we have simulated neutron counts across the sites using the field measurement N_0 values. The N_0 was taken from Bogena et al. (2022), the COSMOS Europe paper.*

RC:7. ***"We are here referring to equation 8 as mentioned in the text. As was explained, it resembles an integral of the vertical neutron transport, geometrically projected to the vertical axis." The authors probably mean "projected integral". The term "geometric integral" is already used in mathematics for a different type of calculation procedure.***

AR: *Thank you for the correcting the terminology used. We have updated and revise our manuscript to use the word "projected integral" instead of "geometric integral."*

> The ~~regional~~ original formulation of the COSMIC method has been ~~revised to include the θ_{lw}~~ further extended by the inclusion of layer-wise lattice water content ~~as well. Besides the addition of lattice water to the code, the original version of COSMIC has also~~ and bulk density. Furthermore, COSMIC inside mHM has been numerically optimized to substantially increase the computational performance. This includes the calculation of the ~~geometric~~ projected integral (Eq. 7) based on lookup tables.

RC:8. ***"Evidence for the influence of crowding cows at this site" As the authors themselves state in their reply that there is no evidence that this is a relevant influence factor, the reviewer asks the authors to remove such distracting assumptions.***

AR: *Crowding cows have been mentioned by Schrön et al. (2017) as a likely influencing factor on the neutron variability at this site. This statement was based on protocols from the land owner. However, we do not have access to exact time tables and number of cows per hour. Therefore, we agree that this statement is vague and we have deleted the sentence to avoid any speculation.*

> ~~Another factor is the time-variable effect of crowding cows near the station, which may influence the CRNS signal, but is challenging to correct in the CRNS measurement (Schrön et al., 2017)~~

> ~~Moreover, in the middle of September, many cows had been present at this site, which could have led to a non-negligible variation of the neutron signal and thus to a non-meaningful expression of correlation-related measures (Schrön et al., 2017)~~

RC:9. ***"Here the consistency in simulating neutron count variability means that mHM has the capability of capturing the general trend and pattern of the simulated data (...)" Thank you for the clarification provided regarding the interpretation of the statement regarding the consistency in simulating neutron count variability. However, the explanation provided does not fully address the concern raised by the reviewer regarding the potentially misleading nature of highlighting only the top 1% of model runs in combination with potential systematic biases. While the reviewer acknowledges that the top 1% of model runs may demonstrate in some cases a stable performance in terms of overall trend and pattern capture or extentension to a large subset of model runs, the reviewer lacks the understanding of the statistical deviations and variations the authors present. With each subset of model runs showing an inconsistent variability it is not easy to follow the arguments brought up by the authors based for example on seasonality. As brought up earlier, by selectively highlighting only the best-performing model runs, there is a risk of overlooking potential weaknesses or limitations in the model performance across a wider range of conditions and scenarios.***

AR: *Thank you for remark concerns regarding our selection of the top 1% of model runs in our analysis. In our study, we focused on the top 1% of model runs to demonstrate the potential of the model under optimal parameter sets. This choice was the need to identify where the model most effectively captures the variability and trends in neutron counts, which are the aims of our research. Depending on the objective of the study, the hydrological community commonly chooses a certain number of parameter sets (e.g., (Smith et al., 2019; Borriero et al., 2022; Demirel et al., 2024)). The parameter prior and posterior solution is shown in the Supplement material showing which parameters most significantly influence mHM performance for each sites.*

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Author Response to reviewer comment #2

Improved representation of soil moisture processes through incorporation of cosmic-ray neutron count measurements in a large-scale hydrologic model

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RC: ***Referee Comment***, AR: *Author Response*, □ Manuscript text

AR: *We appreciate the editor and reviewers' time and insightful feedback, which have improved the manuscript and enhanced the clarity of our research.*

RC:1. ***I am still troubled by a potential flaw in this manuscript. As I understand the work, the link between true field soil moisture and modelled comparisons has been lost, although Fig. S5 may show that this is not the case (do Fig. 5 measurements use the fitted $N_{0_{Des}}$ or a field calibrated N_0?).***

AR: *Thank you for your inquiry on Fig. S5 in our manuscript. We relized that the N_0 is a very sensitive parameter toward the conversion to soil water content from neutron counts using Desilets equaction, we now fixed the N_0 value and did the analysis again, Now in the* ***Fig.5*** *, the N_0 parameter value is taken from field calibrations, which is documented for each site, including Grosses Bruch, Hohes Holz, Hordorf, and Cunnerdorf, we utilized the measurement data from the COSMOS Europe data paper by (Bogena et al., 2022), where they converted neutron counts to soil moisture, $\theta(N)$, using the methodology from Desilets et al. (2010).*

$$\theta(N) = \frac{0.0808}{{}^{N}/_{N_0} - 0.372} - 0.115 \qquad (1)$$

We have updated Fig.9 on soil moisture in the revised manuscript. This includes a left panel with default parameter runs in mHM,and right panel with the calibrated parameter runs using the $N_{Des,U}$ method that include Figure 1 across all the site, The updated a figure for the other methods of soil water content i.e., $N_{Des,W}$ and N_{COSMIC} and has been added to the supplementary materials.

To clarify the statement regarding field soil moisture, we added this explanation to the revised manuscript.

> *In these figures, the grey dots represent the CRNS soil moisture measurements. The N_0 parameter values, taken from field measurement, are documented for each site, including Grosses Bruch, Hohes Holz, Hordorf, and Cunnerdorf. We utilized measurement data from COSMOS Europe (Bogena et al., 2022), where neutron counts were converted to soil moisture, $\theta(N)$, using the methodology from Desilets et al. (2010).*

Detailed comments to the reply of the authors:

RC:2. ***Note although this paper is about improving soil moisture (process) representation in models (the title), there is only one plot showing calibrated modelled soil moisture, and this is in the Supplementary. In the main paper, only neutron count comparisons are shown. Since both the relationship between neutron counts and soil moisture and the predicted soil moisture are both calibrated, it would seem to me that there is no longer necessarily a representation of true field soil moisture. It is noted that the authors (in previous reply) have chosen to focus on neutron count agreement, rather than soil moisture agreement; however, I would urge the authors to consider how this method can still be traced back to the absolute soil moisture measurement.***

AR: *Thank you for your feedback regarding the focus of our manuscript on neutron count comparisons rather than directly on modeled soil moisture, as neutron counts are a reliable measurement for soil moisture. In response to the reviewer concern, we have revised our approach by fixing the* N_0 *parameter based on field measurements. We then reanalyzed the neutron counts and soil moisture data, and the results for the time series of soil water content using the* $N_{Des,U}$ *method are now included in the manuscript as shown in Figure 1. Additionally, the results for the* $N_{Des,W}$ *and* N_{COSMIC} *methods have been added to the Supplementary Material. Now this analysis ensures that our analysis more directly reflects the true field soil moisture.*

RC:3. ***To elaborate on this point: as the authors describe, the CRNS method for field soil moisture (SM) measurement has a free calibration parameter*** N_0***, or more specifically here*** $N_{0_{Des}}$***. Through careful field calibration, normally by collection and moisture analysis of field soil samples, the value of*** $N_{0_{Des}}$ ***is determined. This provides the crucial link or traceability of the measured neutron counts to soil moisture content – the quantity which hydrologists are actually interested in knowing.***

However, in this paper, the field calibration value of $N_{0_{Des}}$ ***is not used. Instead as part of the model calibration period (line 292) the*** $N_{0_{Des}}$ ***is optimised – presumably by minimising the neutron count rate or soil moisture error of the model (it is not stated what objective function was used). This model calibrated*** $N_{0_{Des}}$ ***will be different to the field calibration, giving different soil moisture content for a given neutron count – thus the true site-specific calibration of neutron counts to soil moisture has been lost. Whilst the calibrated model may have better agreement with the observed neutron counts, the model output calibrated soil moisture does not necessarily have a similar improvement i.e. the soil moisture could be biased high or low, and that bias accounted for in terms of neutron counts by the model calibrated*** $N_{0_{Des}}$***.***

Seeing Fig. S5, I actually do not think this flaw really exists – but the detail of the N_0 ***model calibration versus soil moisture calibration needs to be clearer to explain how this potential issue has been dealt with. The authors should justify their approach of a model calibrated*** N_0 ***versus using the value already known from site specific field calibration of the CRNS. And it may be of value to compare these.***

AR: *Thank you for your detailed observations concerning the calibration of* $N_{0_{Des}}$ *in our study. Based on the reviewer's suggestion, we conducted an experiment by fixing the* N_0 *values from the measurement sites for each location and run the model by taking snow, soil moisture, and neutron parameters for 100 000 simulation we took the best 10 parameterset based on the objective function* $KGE_{\alpha\beta}$*. We found that by fixing the* N_0 *values, the observed neutron counts matched well for the agriculture sites* Cunnerdrof, Hordorf *and grassland site* Grosses Bruch*. However, a larger discrepancy was noted at* Hohes Holz*, a dense forest site. This difference could be attributed to the Leaf Area Index (LAI), biomass and* *vegetation dynamics, which are not currently integrated into mHM. Recent efforts by Bahrami et al. (2022) aim to address vegetation dynamics in mHM, but this integration is still incomplete. Our study suggests that future research should focus on regionalizing these parameters i.e., N_0, particularly in ungauged locations, to enhance model accuracy and applicability.*

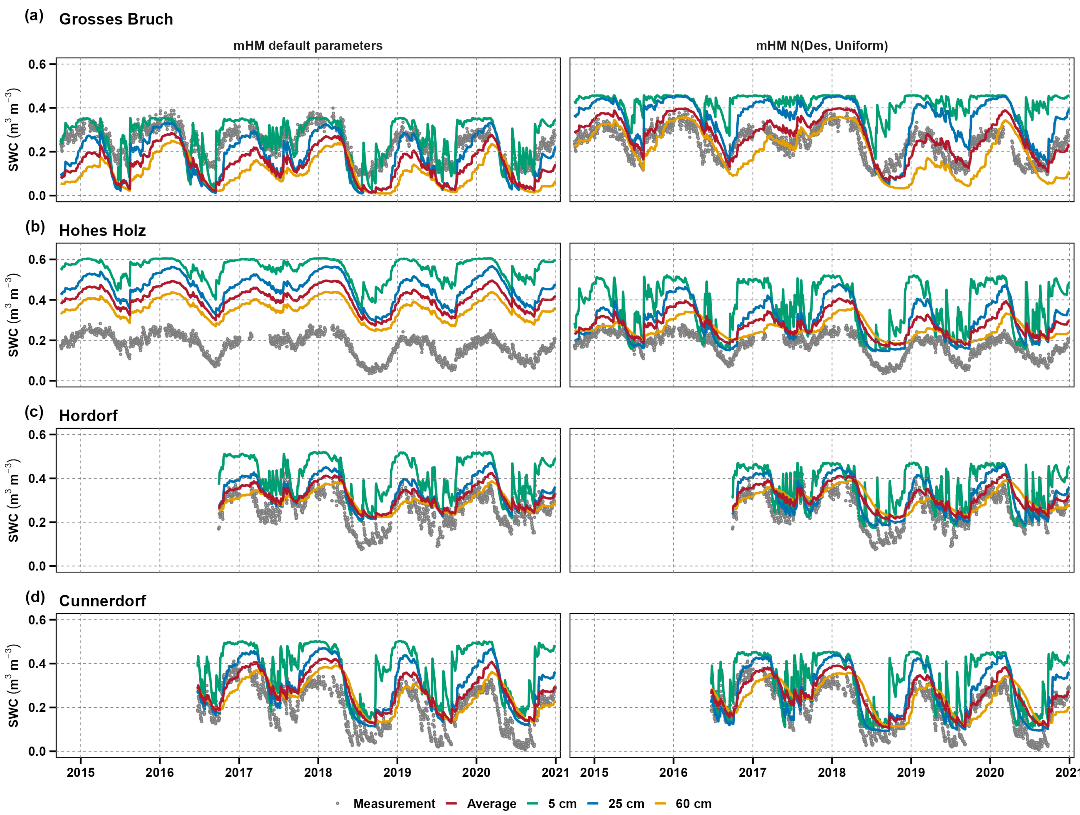

Figure 1: Daily $SWC_{Des,U}$ time series at four sites, comparing observed SWC (gray dots) with mHM-derived SWC at 5 cm (green), 25 cm (blue), and 60 cm (orange) and the average (red). The left panels use default mHM parameters, while the right panels use parameters calibrated with $N_{Des,U}$ method. Selection of N_0 values was based on COSMOS Europe data.

RC:4. ***Line 303 "Estimated values of $N_{0_{Des}}$ and $N_{0_{COSMIC}}$ obtained in our study are close to optimal values" – how do you know that? What are the optimal values? And the inference drawn is not sound – model simulation of dry conditions is not a prerequisite to obtaining accurate N_0 values.***

AR: *Thank you for your comment. Upon reviewing the evidence and addressing the concerns highlighted in your comment, we acknowledge the issue regarding the closeness of the estimated values of $N_{0COSMIC}$ to the observed values in our study. This discrepancy arises because N_0 functions as a free parameter, varying significantly depending on the chosen method COSMIC or Desilets as also different N_0 values depends upon the method by Iwema et al. (2015); Baatz et al. (2014). However, since we do not have field measurements for $N_{0COSMIC}$, we calculated using the Shuttleworth et al. (2013) method where the* *observed neutron counts and soil moisture from different profile depth are known the only unknown value was N_0.*

To address your concern, we have removed this sentence from the manuscript to maintain accuracy and avoid unsupported claims.

> ~~The estimated values of $N_{0,\mathrm{Des}}$ and $N_{0,\mathrm{COSMIC}}$ obtained in our study are close to the optimal values, indicating that the model has the potential to generate accurate cosmic-ray soil moisture estimates even under dry conditions.~~

RC:5. ***Results – Fig. 5 Also show plots of SWC (as per field calibration – observations) and calibrated modelled SWC.***

AR: *Thank you for your feedback and for suggesting the inclusion of plots showing the traceability of the measured neutron counts to soil moisture content. Also inclusion of SWC estimated from mHM neutron counts. We have showed the result of the time series of SWC in Figure 1. This figure now includes the results where the N_0 value was fixed based on field measurements, and the analysis was conducted accordingly. The new plot provides a comparison between observed SWC (from field calibration) and the SWC derived from the calibrated mHM.*

RC:6. ***Discussion – I would question the soundness of discussing model performance, when it appears that neutron count comparisons rather than SWC have been calibrated. As the authors have chosen to present neutron count data, then they need to be careful as to what is claimed with regard to soil moisture modelling, or to provide evidence to support those claims.***

AR: *Thank you for your feedback. We understand that our explanation of model performance in relation to SWC calibration has to be clarified. Accordingly, we will revise the discussion section to focus on the calibration of neutron counts and their implications for soil moisture estimation. As mentioned in response to above question 3, we have already addressed the estimation of SWC.*

RC:7. ***Line 440 " ... improved not only soil moisture estimation" – NO improvement in soil moisture estimation is shown in the main paper!***

AR: *Thank you for highlighting the need for clearer representation of soil moisture estimation improvements. To address this, we will include Figure 1 about SWC into the Discussion section. This figure shows the soil water content of different layer along with the total average soil water content of mHM based on the calibration of neutron counts.*

RC:8. ***Conclusion – Line 507 ... evaluation with soil moisture observations has not been shown.***

Several statements in the conclusion are not supported by the paper (at least not without digging into Supplementary material) e.g. Line 525 "improved the soil moisture performance of the model.

AR: *It is noted in the conclusion that the evaluation of soil moisture performance was not adequately illustrated in the main paper. We have therefore chosen to include Figure 1, which presents the soil moisture performance of the mHM, in the paper's main discussion section in order to correct this omission. This inclusion of Figure 1 along with a performance matrix table from all sites will make sure that the conclusions are easily verified by readers .*

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