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Comment on hess-2021-564

Anonymous Referee #2

Referee comment on "Technical note: A revised incoming neutron intensity correction factor for soil moisture monitoring using cosmic-ray neutron sensors" by Magdalena Szczykulska et al., Hydrol. Earth Syst. Sci. Discuss., <https://doi.org/10.5194/hess-2021-564-RC2>, 2022

This manuscript "Technical note: A revised incoming neutron intensity correction factor for soil moisture monitoring using cosmic-ray neutron sensors" deals with the problem of incoming neutron radiation correction for cosmic-ray soil moisture sensors.

The authors claim to derive a "revised correction factor" using an amplitude scaling technique that has already been proposed in the literature before, while suggesting the use of median instead of reference count rates. In the same study, the authors find that the proposed median count rate often is insignificantly different from the reference count rate, that it turns the whole approach to be highly dependent on the individual measurement period, and that it is less generalizable. Good agreement has been found by comparing the overall average at tens of sites with findings from other studies, but the reader is still left with no clear idea about the added value provided by this study.

While the problem statement is relevant for the CRNS community, the novelty of the approach as well as the quality of the explanations are low. However, there is a potential to accept the manuscript with major revisions, particularly by further elaborating on the scientific foundations, on the statistical quality, and on the demonstration of measurement results.

Major concerns

- The HESS manuscript type "Technical notes" requires to "report new developments, significant advances, and novel aspects of experimental and theoretical methods and techniques which are relevant for scientific investigations within the journal scope." The

addressed problem and topic are relevant for studies published in HESS, but it is not clear how to identify the significant advancement and novel aspect of this study. While the manuscript "should be short (a few pages only)", the authors elaborate unnecessarily on less related aspects, such as the importance of soil moisture for hydrology or drying and wetting characteristics of individual sites. I would suggest to provide a more concise document with a strong focus only on the *technical* aspect of incoming neutron correction:

- what is the expected sensitivity of CRNS to these corrections (cite e.g. Baroni et al. 2018 (JoH)),
- provide fundamental derivation of your approach and discuss differences to previous approaches, and
- statistically sound evaluation (using all your individual site data, proper statistical measures, and uncertainty analysis).
- The fundamental explanation of the involved processes and physics have been marginally addressed. The authors seem to treat the correction approach as yet-another function without discussing the meaning of the analytical form or the physical processes involved. For example:
 - Why should detected low-energy neutrons in any way behave proportional to incoming high-energy neutrons? What fraction of the detected signal is direct and indirect radiation and how does this change with site conditions? The authors you are citing have answers to that. This would help readers to actually understand your approach and this may have direct implications on the interpretation of the performance of your results at individual sites.
 - Can we expect this approach to be different for different regions on Earth? What has this to do with latitude or cut-off rigidity? The answers may have implications on the performance of your distributed sensors and should be discussed using the empirically obtained values for G .
 - What could be the physical reason for the necessity of an amplitude scaling in the form of $G \cdot (I/I_m - 1)$? This has implications on the choice of G and its meaning, and could answer whether situations could occur with $G < 1$, too.
 - Also put the whole idea into context and note that it is about correcting for solar activity fluctuations and mention that the scale of those variations can be from years (solar cycle) to days (FDs, GLEs). This has direct implications on the conclusions that you could and could not draw from your analysis, e.g. by choosing a "monthly average", with which one would have no chance to resolve any short-term variations.
- The authors determine γ empirically, while there are high chances of fitting through soil moisture variations. A more "safe" approach would be to calibrate γ on quiet periods (low solar activity) and at sites with constant soil moisture (desert, concrete, or lakes). Some authors (which you have cited) have already traveled along this path and it could be helpful to report on advantages and disadvantages of the various approaches.
- The authors spend two pages to derive their concept, and end up with an equation which is equal to the one from literature which has been presented already in the introduction. It is claimed that the new equation is now "revised", for two reasons:
 - it uses median count rates instead of a reference count rate (although this option has already been suggested by Zreda et al. 2012 and others). Interestingly, in your own study you tell that the difference between the use of I_m or I_{ref} often is insignificant...
 - the scaling factor G now is empirical (although this has been done already by other authors which are cited elsewhere in this very study), while they are hardly compared to values from existing approaches. In addition to that, even the relationships to the cut-off rigidity, as proposed by Hawdon 2014, for instance, are empirical. So instead of empirically building on their theory, you reduce the complexity of finding G even more by applying a purely empirical fit. This comes with the risk of low transferability and generalizability of the results.

- Under these circumstances, I strongly suggest to properly discuss previous literature and not to sell the current development as a "revised correction approach". This study for me looks rather like a case study, where some of the existing suggestions have been tested on the UK network data. It is surely important to do that, but it would require a different storyline.
- L143: If the results are dependent on the variation of the incoming radiation, then it should be easy to evaluate this by testing the approach during high and low solar activity. Something that could be done at the COSMOS-UK Network.
- L147: "Preliminary studies show no simple relationship with ... latitude, altitude, or rigidity"
In the last sentence before the conclusions you are mentioning the most important aspect, in my view, which should be central to this study. Consider elaborating on building a theoretical basis for your approach, which involves confrontation with the mentioned quantities. This would help to develop this approach further towards a generalized and transferable method.

Specific comments:

- Common and vague language is used quite often, rather than clear and factual language. E.g., L17 "perhaps less obvious", L85 "gives a gradient G, not of 1, but 1.5", L122: "change is very much greater".
- L33: the Desilets equation is semi-empirical, the parameters have been derived from observations only. In general, this paragraph can be much shorter. The choice of the conversion function and vegetation or organic carbon corrections are of lesser importance here.
- Fig 3: The comparison of time serieses with different G is not insightful. Please provide reference data, e.g. from the neutron monitor (to demonstrate a changing correlation to their signals) or soil moisture data (to demonstrate better match).
- Fig 4: Now you show TDT data, but only with G=1, so there is no chance to compare the approaches and to evaluate the performance of your idea. Moreover, consider showing a weighted average using different depths, instead of only 10 cm data.