Reviewer #3 comment round 2: Physics-informed machine learning for understanding rock moisture dynamics in a sandstone cave; Kai-Gao Ouyang et al.; https://doi.org/10.5194/hess-2022-403

I appreciate the considerable efforts made by the authors to enhance the manuscript since the previous round of reviews, and their diligence in addressing all the questions raised.
However, I regret to say that I remain unconvinced by their response to the central point of my critique, namely the impact of sensor sensitivity to temperature. This remains a point of uncertainty for me.

By presenting two instances of two-week periods with elevated FDR readings despite no observable temperature changes (within the figure), the authors draw the conclusion that the sensor's sensitivity can be considered insignificant. First of all, I suggest changing this figure so that the scale of the y-axis for temperature reflects the range of the data which is shown in the figure, currently the difference between minimum and maximum value is too large.

I would like to support my argument again by referring to the state of literature. It is well known that the electrical permittivity of soils (for which the TDR and FDR sensors have been developed) is influenced “by temperature (Roth et al., 1990; Wraith and Or, 1999; Owen et al., 2002; Rosenbaum et al., 2011), (soil) texture (Ponizovsky et al., 1999) and organisation of thin water film layers (Wang and Schmugge, 1980)”. (p 648, Jackisch et al. 2018). Jackisch et al. (2020) also show in their Figure A3 that most soil moisture probes have a high correlation with temperature.

Even with soils, it is considered ideal to calibrate the sensor under controlled conditions for the respective application. As this study relates to a single sensor in a new application and calibration under controlled conditions requires few resources (sensor, stone, scale, thermometer) I would recommend the authors to perform this calibration. In my eyes, this simple measurement would essentially improve their study.

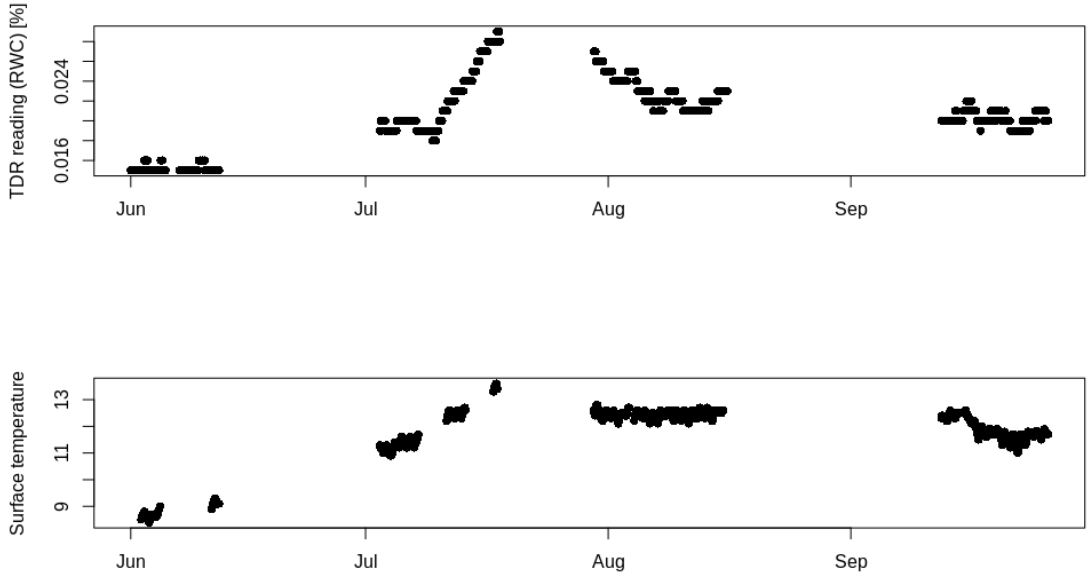
Without this calibration the potential temperature effect needs to be a central point of the discussion and needs to be more prominently addressed in every section of the manuscript, including the abstract, introduction, and the conclusions.

My strong recommendation about this aspect is based on the fact that the range measured by the sensor reported in this study is very low (0.010-0.030 over the whole period of observations) while the change in temperature is not negligible (-15 to 14 °C according to Fig. 5; Tsurface 8.4 – 13.6 °C ; Tair 10.9 – 21.4 °C between June and September 2021 from the downloaded data)

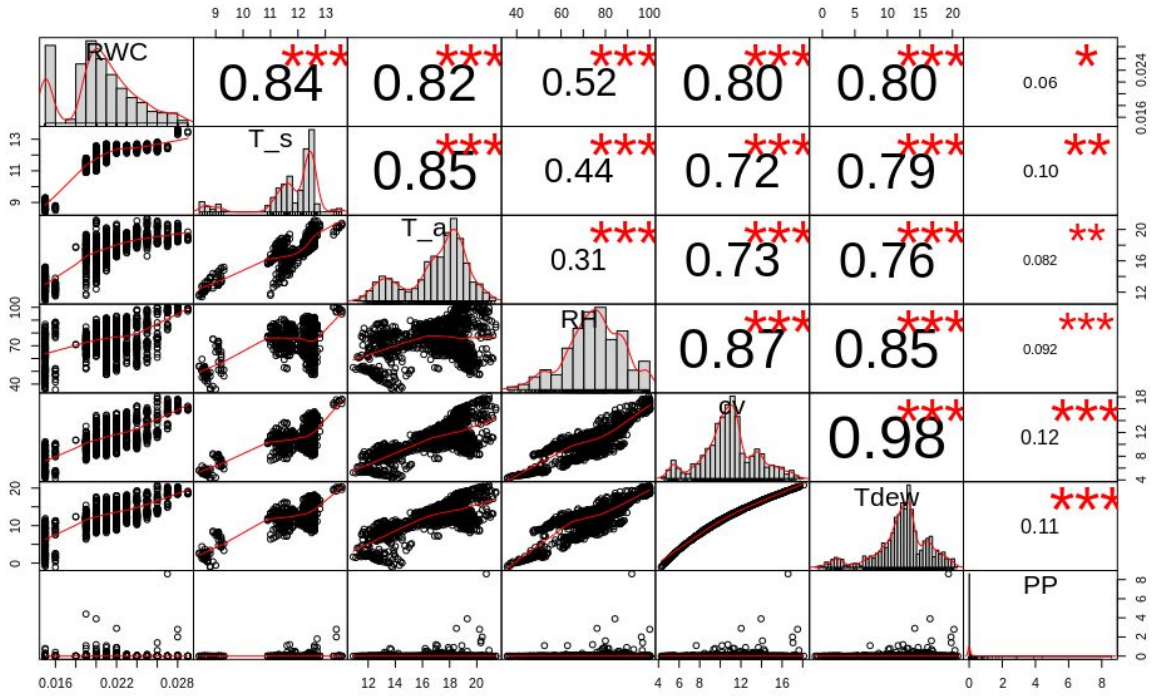
Additionally, I was suggesting in the first review round to the authors to include a more simple method to measure the relationship between the predictor variables than the LSTM to give a baseline to which the performance of the more sophisticated method can be compared. The authors provided a table with the covariance between the signal of the dielectric permittivity (interpreted as RWC) and the normalized atmospheric conditions in 2020 and 2021.
Although this response goes into the right direction, in this context the correlation coefficient is more meaningful than the covariance. Additionally, since my question refers to the necessity of the LSTM, I argue that the same period of time should be considered (June – September). I copy pasted some data out of the pdf provied by the authors (covering summer 2021, please provide .csv ot .txt format in the

future when providing data) and calculated the correlation coefficient between predictor variables and the response variable (RWC) (see below). Although the LSTM outperforms a linear model in this study the linear model would already be doing a very good job (RWC ~ T_surface, $r^2 = 0.71$). Additionally, the strong correlations between RWC and both temperature measurements (0.84 & 0.82) underline the point that a proper validation (under controlled conditions) that the sensor is not mainly influenced by temperature would be recommendable (or this aspect needs to be highlighted).

I think the authors can still substantially improve the quality of their research and this manuscript by addressing the above mentioned points.



correlation chart of all provided variables for the above shown period (pearson correlation coefficient with degree of significance):



RWC = rock water content, T_s = wall surface temperature, T_a = air temperature cave, RH = relative humidity, cv = vapor concentration, Tdew = dewpoint temperature, PP = Precipitation.