The manuscript by Danhui Su et al. proposed a method to determine unsaturated soil water content based on thermodynamic equilibrium, which is independent of the water containing media and with the absolute error less than 1%. The manuscript presented the relationship between soil water content and the mass ratio and density ratio of water to vapour at thermodynamic equilibrium, conducted sandbox experiments to validate the function of the mass ratio and density ration of water to vapour.

This study is of potential application in the monitoring soil water content, even for the rocks. However, there are some concerns from my perspective. First, regarding the validation of the proposed method with the measured soil volumetric water content, the measured ”truth” is obtained using a soil water content sensor, with an accuracy of 2%. I am not sure how much you believe in the measurements of the selected soil water content sensor. Have you calibrated the used soil water content sensor in terms of different soil textures (medium sand and fine sand in this manuscript)? Second, there are a wide range of soil textures, why only selected the medium sand and fine sand, how about the clay soils and organic soils?

Overall, the idea of this manuscript is interesting, but the validation part is not that convincing from my perspective. My specific comments are as follows.

Abstract:

Line 10-11: you mentioned the current challenges of the measurement methods of soil water content, this manuscript dealt with all of them or not. Please clarify.

Introduction:

Line 66-68: “Furthermore, the mass ratio function is independent of the nature of the water-containing media, according to theoretical analysis and statistical results. The absolute error of soil water content between the calculated and measured is less than 1% and is positively correlated with temperature.”
This is the findings of your work, should not belong here.

Results:

“When the temperature is low, the intensity of water evaporation is weak, and the water conversion rate into vapor is slow, resulting in low vapor content in the pore and a non-equilibrium condition.”
here you mean, the temperature is low corresponds to a non-equilibrium condition?? Please clarify.

Line 302: “volumetric water content sensor, which has a 2% error.”
here the measured volumetric water content is obtained using the sensors?
the absolute error, is assumed that the used sensor is the truth, which is not according to your words (2% error).
Please give more evidence demonstrating the validity of the selected soil water sensor, e.g., calibration work.

Line 308-311: “This is because although the soil volumetric water content fluctuates with temperature, it has no significant correlation. In contrast, the volumetric water content obtained by this study’s method is positively correlated with temperature, resulting in a more significant absolute error at higher temperatures.”
here you mean that it is the nature that soil water content should be positively correlated with temperature? Please clarify.

Line 357: “When the geotechnical system is in equilibrium or near equilibrium, …”
how to determine whether the geotechnical system is in equilibrium or near equilibrium or not in practical?

Figure 4. Please give the dimensions in details and the position of soil sensors.
Figure 5:
legend: what is the difference between two "water"?
Figure 5 and figure 6 can be merged into one figure.

Figure 9:
From this figure, the determination coefficient is 0.998, largely due to the two sets of data with different values. and the fitting trend line indicate that the calculated water content is overall overestimated. Thus, although the determination coefficient is high, the validity of the calculated method is not convincing. Please explain how do you deal with the overestimation and dispersion of results?

Table 4 and Table 5:
In these two application cases, the values of water content are relatively stable, i.e., around 27.8% and 0.612% for the soil and rock, respectively. Please explain why the water content is kind of stable at different depths.

I suggest the authors add the limitations of the proposed method, e.g., whether it can be applied in frozen soils, not suitable for measuring soil water content at the surface soil layers (usually non equilibrium), etc.

Although I am not the native English speaker, I think the manuscript can be improved from the perspective of English