The manuscript explores the effects of subsurface water infiltration systems (WIS) on vertical soil movement and land subsidence across a collection of peat meadows. The study is data-driven and is based on very detailed experimental campaigns. The sites investigated cover a variety of settings typical of the targeted regional scenario. On one side, I do find the experimental study to be interesting and informative. On the other side, I do think the hydrological component of the study is still not fully developed, the experimental approach and techniques being mostly associated with geotechnical and soil mechanics areas. In this sense, I find the discussion to be focused mostly on the description of the results encapsulated in the figures rather than providing clear interpretations linking fundamental hydrological processes. Hence, I would suggest expanding this element, which is important for the Journal and its readership, and perhaps relegating some more technical and descriptive parts (e.g., local geological/sedimentological settings) to Appendices.

The focus of this paper is on vertical land movement and driving soil deformation processes, and not on hydrological processes. Hydrology is however an important driver of vertical land movement in peat soils, which is explained in sections 4.1.2 and 4.1.3. The authors think the topic fits very well in HESS, as this is not only about hydrology but also about earth system sciences. For your information, a more in-depth scientific paper on groundwater level measurements at the same study sites is currently being written.

Depending on the final review, we would be fine with moving parts of the descriptions of the sites to an appendix. E.g. the maps and the introductory text for each site could be moved. This could for example be replaced by an overview table with some of the main characteristics of the 5 sites. The tables with info on anchors should remain in the main text in our opinion.

I also found the focus on climate change to support the importance/impact of the study to be too much highlighted and more oriented towards practical applications, rather than uncovering/analyzing fundamental hydrological processes. I would suggest diminishing the emphasis on such element and highlighting more clearly the importance and collocation of the study in the context of the current literature associated with fundamental processes. In essence, the Authors should be clear about whether their contribution is more application-oriented or geared toward providing enhanced understanding of hydrological processes and system functioning. Since I do see a lot of potential in this sense, I would then suggest de-emphasizing the application-oriented aspect.

Climate change was only mentioned in the introduction. We rephrased the paragraph where it was mentioned, but we still mention climate change because it is one of the reasons this research has been carried out (reduce CO_2 emissions from drained / subsiding peat soils. Measures like water infiltration systems are also applied for optimizing water management and reducing land subsidence in times of climate change. We do already make the link between soil deformation processes and their main drivers in the introduction. Moreover, the main focus of this paper is on vertical land movement in peat soils, of which hydrological processes are one of the drivers.

In terms of quality of results, I did not find too many comments about data uncertainties. For example, I am assuming that groundwater levels are associated with some uncertainties. How are uncertainties associated with all of the data types analyzed impact on potential relationships between processes? Is there a way the Authors can provide some insights on these aspects?

The spirit levelling and extensomery techniques measure at millimeter scale accuracy. It should be realized that we do present averages of about 100 levelling measuring points. If desired, we could include standard deviations of the mean (standard deviation of changes in soil height relative to the first measurements, otherwise it would be an indication of how irregular the surface is). The uncertainty of the groundwater level measurements is somewhat larger, maybe up to a few centimeters (uncertainty of the device and possible difference between groundwater level in the soil and in the monitoring well). In any case, considering the dynamics of both vertical movement and groundwater level, we expect that these *uncertainties will not have any effect on the outcomes and conclusions of the paper. We could include above mentioned notes about uncertainties in the final manuscript.*

Are some of the results (for example, the results depicted in Figure 14) to be expected? If so, is there a rationale underlying such expectation? Or do they come as unexpected? These are some examples of insights that Authors could provide to enhance the potential impact of their work.

Yes, this is explained in section 4.1.2. We will refer to this section in section 4.1.1.

Additionally, are these types of results typical of the context they Authors analyze? Or can they be somehow transferred to other settings?

Yes, this is further explained at the end of section 4.1.2.

The Authors attempt providing a fit to the data. Why do they expect a linear trend? Is this simply to identify a trend or can this be employed to do something more, e.g., to provide some interpretive model. In any case, when performing a model calibration, I assume the Authors have also evaluated uncertainties associated with parameter estimates. I was not able to see bounds of uncertainty around the plotted linear trends. I would suggest an in-depth analysis of this element together with a clarification of the actual purpose of providing a linear trend line. This is also in line with the statement made by the Authors regarding obtaining an improved quality fit with more data (the Authors refer to a manuscript which is still in the writing phase). Why should the reader be interested to what the Authors define a better fit? How do the Authors quantify the terminology better fit? Simply in terms of R2? Model parameter uncertainty? Model predictive power? The reader would benefit from this kind of discussion, in my view.

We used linear fits because we want to assess of the average subsidence (rate) for a certain period. We would need a much longer time frame to be able to detect other types of trends. Also, other studies that did have longer time series (a few decades) show that a linear trend is best to use for assessing an average subsidence rate (e.g. Massop et al., 2024, Schothorst, 1977). Moreover, the trendlines give a first indication of the long term subsidence trend (further, the paper focuses on seasonal vertical land movement). Longer time series will give more reliable estimates because yearly differences in land movement dynamics are increasingly filtered out more, and the R^2 will increase. We believe an in-depth uncertainty analysis will not add much to the paper at this stage, the time series are still too short. The paper demonstrates methods used and first (promising) results, and is likely to have a follow up paper with more in-depth analyses at a later stage. These lines of reasoning could be highlighted more in the final manuscript.

When discussing about temporal data series, did the Author observe any trend/drift associated with measurement accuracy? Any induced correlation among data?

No, this has not been observed

On these bases, I would suggest a series of revisions that I would define as ranging between moderate and major.