We thank the reviewers for their comments on our manuscript. Please find below the reviewers' original comments (in plain text) and our responses (**in bold**).

Reviewer #1:

I think authors have successfully addressed my concerns expect they have misunderstood my comment "Line340-342: this is not that obvious visually. I would suggest authors to improve figure 7 by also making sure they are readable in white and black". I was not suggesting them to change figure 7 in white and black, but to make sure it is still readable if it is printed in white and black. It is a minor issue but worth paying attention to.

Thank you for this helpful comment. We have changed the figure to use a divergent color scale. This color scale employs white in the center (values from -1 to 1) to represent minimal soil moisture deviations, light red on the left (values from -4 to -1) to indicate soil moisture below the average, and dark blue on the right (values from 1 to 4) to indicate soil moisture above the average. The red and blue are in light and dark, respectively, which can make the color scale readable when it is printed in black and white. In addition, we have given the symbols a black outline so that the points with a light color are more distinct from the gray background.

Reviewer #3:

The authors presented a long-term dataset of soil moisture in a small catchment on the Chinese Loess Plataea. The manuscript is easy to read. Although no reviewers have posed serious questions per se during the open discussion phase, the novelties of this study are not still very clear. Here are few comments that, I hope, can be of any use for the authors.

Thank you for these comments.

1. The effect of North- and South-facing slopes (on soil moisture or other processes) is well studied in the past, particularly in eco-geomorphology. There is nothing really surprising about the results. The authors can refer to some of the refs I listed below. In my opinion, those studies performed much better analysis than the current study.

Of course, we are also not surprised that south-facing slopes have a lower soil moisture content. However, we were surprised (and others may also be surprised) that the effects of aspect are largest during the winter, when ET rates are lowest. Regarding the first study mentioned by the reviewer, Gutiérrez-Jurado et al.'s results are potentially confounded by variations in soil characteristics. One major advantage of our loess study site is that there is almost no variation in soil properties from one location to the next; thus we are better able to isolate the effects of topography on soil moisture. This is particularly important for low moisture contents when texture has a relatively large effect on soil moisture content. We now also cite Gutiérrez-Jurado et al. (2007) at line 401, and explicitly make this point. Regarding the second study, Srivastava et al. (2021)'s

results are based purely on a modeling exercise. We do not understand how this can be considered a "much better analysis" than real-world data.

Note that in response to this comment, we have also changed the brightness of the aerial photograph in Figure 1b.

2. The authors raised the question regarding local vs. nonlocal factors. First, the authors need to specify what local and nonlocal factors are. Second, it seems the authors did not really address this question in the end.

In fact, we did specify what local and nonlocal factors are, at lines 55-63 of the previous version, but we have clarified the text to make this more obvious. We have also mostly removed the local/nonlocal distinction, in favor of a more explicit contrast between aspect-controlled ET variations (a local control) and downslope flow controlled by hillslope convergence (a nonlocal control) but now also refer back to these terms explicitly on L419 and 421 in the discussion.

3. For the topographic effect, the authors need to consider what are the spatial and temporal scales that under consideration. Similar to the spatial scale effect, the topographic impact can be very different at different temporal scales (e.g., storm event vs. mean annual), as the controlling processes can vary significantly.

We agree. However, we hope it is obvious that we have presented a seasonal analysis based on monthly data, so we have no information about soil moisture responses at the storm-event timescale. Similarly, it should be clear that we focus on the hillslope and catchment scale. However, we now state this explicitly on L111 above the research questions. Note that we addressed this difference in scale explicitly by looking at the hillslopes and gully sites separately, and also in the discussion on L424-431. We prefer not to comment on spatial or temporal scales for which we have no data.

4. For deep soil moisture analysis, another important issue is the traveling time between surface and depth, particularly under dry conditions (due to low unsaturated hydraulic conductivity). Sometimes, it may take several months or years for infiltrated water to reach deep soil layers. This should be considered in the analysis.

We agree that deep infiltration can take long time spans, particularly in arid regions. However, there is so little deep infiltration below 2-3 meters at our site (see Figure 3a) that we have no practical way to observe these long timescales in our data.

5. Given the long-term data presented here, I highly recommend the authors to go deeper than the current analysis, which basically provides no new information other than another case study and does not really provide very important implications as claimed by the authors.

We strongly disagree with the characterization of our work as just "another case study". We have analyzed an unusually dense network (89 locations in a 43 hectare catchment) of unusually deep measurements (up to 5 m depth), in a loess catchment whose very uniform substrate allows us to analyze topographic controls on seasonal soil moisture patterns. This work yields surprising results, such as that soil moisture is highest during the months of least precipitation, and vice versa. Another interesting result was that the topographic wetness index was a very poor predictor of

moisture patterns, despite its widespread use in models. We now highlight these novel aspects more clearly in the abstract, introduction and conclusion (L16-20, L91-94, L477, and L481).