We thank the referee for his/her valuable comments and suggestions, which will improve the paper. The responses to your comments are marked in blue.

**General comments**

1. I like the concept of distinguishing between random changes in precipitation and changes that result from soil moisture, but I have some concerns about the representativeness of the results. So far, I am not convinced that the simulation of one day and the evaluation on two comparably small evaluation domains is the right concept. For a more robust conclusion, more cases are necessary and the analysis of one evaluation area alone might be more meaningful. As the authors already mention in the Conclusions, further case studies are needed. If this paper is intended to be a proof-of-concept, the authors should clearly state that in the manuscript and be cautious with any general conclusions.
   As far as we know the concept of shifting the model domain has not been conducted in any model study so far. Furthermore, effects from soil moisture changes are rarely compared to other modifications to rate the soil moisture effects. Thus this study aims on proofing the concept of shifting the model domain as an estimate on model uncertainty and on comparing the concept to physically meaningful changes such as modifications in soil moisture. We recommend on enlarging the study to further cases as we could show that the shifted model domain is a useful tool for model uncertainty estimates and that this is necessary to validate the effect of soil moisture. However conducting further case studies is beyond the scope of this work. We will stronger emphases that the major goal was to introduce this concept. Before performing more case studies, one should be aware that the choice of evaluation domain might be crucial for the results and therefore this is a first necessary step before applying the concept to further cases.
2. My main criticism is due to the fact that nothing is being said about the physical processes that are responsible for these differences. The different model runs are compared to each other with the SAL method, but reasons for the differences remain unclear. As the paper is comparably short, I recommend to add a section on the physical processes responsible for the differences. For example, domain averaged time series of convection-related parameters could be shown here.
   We included a discussion of several quantities that react systematically on the soil moisture changes in contrast to precipitation.
3. When performing a sensitivity study, the control run has to be evaluated first to assure that it serves as a good basis for the sensitivity runs. I believe you need to insert a subsection on the synoptic controls, the observed precipitation and the results of the control run.
   This study aims on the comparison of simulations with different soil moisture and focuses on the differences between various simulations rather than finding the simulation that best fits observational data by model tuning.
   We included a section to describe the synoptically conditions and compared it to radar observations. A more detailed evaluation of the model in this case is beyond the scope of this work and would not improve the results of this work.
4. In many operational forecasting centers, soil moisture is already perturbed in their ensemble prediction systems. Some information about that and most importantly, the differences to the method used in this paper, should be added to the manuscript.
   Using soil moisture perturbation for ensemble prediction one need to know confidentially that this modification can generate an ensemble spread that is sufficiently large to capture possible realizations. That is exactly what this study addresses by comparing the effect from soil moisture perturbation to perturbation, which does not change physically meaningful parameters.
   We included this in the introduction and present some methods of soil moisture modification as conducted by weather services. However, they are not comparable to our soil moisture modification and the comparison to a different uncertainty estimate as they themselves present the uncertainty estimate/model spread.

**Specific comments**

1. What do you mean by model uncertainty? Please clarify. Again, later on: "Only drastic soil moisture changes can exhibit the model uncertainties..." Probably you mean similar uncertainties as in other ensemble systems where e. g. stochastic perturbations are inserted, tuning parameters changed, or different initial and boundary data from another model are used. This has to be made clearer at several locations in the manuscript
   We rewrote the abstract to make this clear

2. P1, L4: "...but the systematic behaviour is still complex..." Up to now, there is no consent about the existence of a systematic relationship of soil moisture to precipitation. I would rather write: "...but the response of precipitation to soil moisture changes is still complex..."
   included
3. P1, L6: Some details about the ensemble approach used in this work should be given here.
   included
4. P1, L23: Surface temperatures are dependent on the sensible heat flux, not the latent heat flux. Please rephrase.
5. You mean the water content of air? Then it's probably better to write: "Secondly, soil moisture strongly influences the low-level humidity via the latent heat flux."
   included
6. "...react on the soil moisture." Better: "...depend on soil moisture due to its effects on low-level temperature and humidity."
   included
7. What do you mean with the synergy of soil moisture-precipitation feedbacks?
   The role of orography in the soil-moisture feedback
8. Is shallow convection still parameterized? Which COSMO version do you use?
   Yes, Version 4.4
9. P3: Is the total drying of the soil the respective permanent wilting point?
   With the 50% increase in soil moisture, did you assure that you don't have larger values than the porosity allows?
   Values of soil moisture are set to zero in the initial conditions and multiplied by a factor of 1.5 independent of wilting point and porosity, respectively
   You state in the manuscript that you want to show the full range of soil moisture influence. So why did you use just a 50% increase and not the maximum value possible for the respective soil type?
   It is right that an enhancement of 50% does not show the full range of possible soil moisture influence. We corrected this statement. Anyhow, the increase by 50% exceeds the increased that is reached by using the relatively wet soil moisture pattern from another day so that this state a large change as the realistic modifications. We included a figure to show this.
   Did you change all levels in the soil in the same way?
   Yes
   Did you make the changes at the model initialization time?
   Yes, we added this in the text.

10. P4, Figure 2: This figure is too pixelated, the text is hardly readable.
    Concerning your band pattern: Does the soil moisture changes from 1 grid point to the other or is there a smoother transition over a couple of grid points? Do these strong gradients introduce any thermal direct circulations?
    We do not have a smooth transition zone, but the initial soil moisture conditions also include strong gradients.
11. Which moist simulation do you refer to? I don't agree with the statement that in the moist simulation, precipitation occurs mainly at places that are free of precipitation in the CNTRL run. At least, I don't see that in Figure 3.
    We included a more detailed description of where precipitation occurs at the shown timestep
12. Figure 3: Instead of showing one time of day, a 24-h accumulated precipitation would be much more meaningful. Soil moisture may also influence the timing of cloud formation, so

one snapshot might not be enough to show the overall effect. In addition, time series of domain-averaged precipitation should be shown as well
We added the timeseries of accumulated precipitation.

13. Random perturbations are introduced by shifting the domain boundaries. Please explain in more detail, why you consider this as random perturbations. One way to prove that would be to insert stochastic perturbations e. g. in the initial temperature field. The authors should comment on that.
With this method we can ensure, that we do not generate any other patterns, that are overlayed and do not change meaningful quantities such as temperature. We described this more detailed.

**Technical comments**

We corrected the manuscript as suggested in the technical comments