

Interactive comment on “Assessing the uncertainty of soil-moisture impact on convective precipitation by an ensemble approach” by Olga Henneberg et al.

Anonymous Referee #1

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General comments

This paper aims at assessing the soil moisture-precipitation feedback for one case in northern Germany with numerical simulations using the COSMO model. Besides a control run, several sensitivity simulations were performed with reduced/increased soil moisture, with a banded soil moisture distribution, and realistic soil moisture values from other days. These model runs are compared to runs with shifted model domains and different initialisation times to distinguish between random changes in precipitation and changes that result from soil moisture.

Although the concept itself is promising, I have a number of major concerns with respect to the representativeness of the results and the applied method. Some of my con-

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cerns are probably caused by clumsy English phrases that will need to be addressed before the paper is suitable for publication. I expand on some of these concerns below and outline additional major and minor comments. My recommendation is major revisions.

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.
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.
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.
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.

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Specific comments

1. P1, L2: 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.
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..."
3. P1, L6: Some details about the ensemble approach used in this work should be given here.
4. P1, L23: Surface temperatures are dependant on the sensible heat flux, not the latent heat flux. Please rephrase.
5. P2, L1: 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."
6. P2, L5: "...react on the soil moisture." Better: "...depend on soil moisture due to its effects on low-level temperature and humidity."
7. P2, L16: What do you mean with the synergy of soil moisture-precipitation feedbacks?
8. P3, L3: Is shallow convection still parameterized? Which COSMO version do you use?

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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? 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? Did you change all levels in the soil in the same way? Did you make the changes at the model initialization time?
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?
11. P5, L4: 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.
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.
13. P7, L25: 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.
14. P8, L9: I don't understand what is meant with "reversed direction", please clarify.

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15. P13, L13: "... show a positive feedback for decreased soil moisture." This is misleading, in their paper they find a positive feedback for increasing soil moisture but only for relatively dry soils. Please rephrase.

Technical corrections

1. I suggest to change the title to: Assessing the uncertainty of soil-moisture impacts on convective precipitation by an ensemble approach.
2. P2, L16: "...over complex terrain", orographic is not needed when using complex terrain
3. P5, L10: ...will be introduced...
4. P6, L2: You name a modified model simulation **mod**, why not name the control run **ctrl** instead of **comp**?
5. P7, L6: fore should be for
6. P11, L2: convective inition should be convection initiation
7. P12, table caption: ...mean of the uncertainty-ensemble**after** after...
8. References: Please use the abbreviations used from the respective journals:
Atmos. Res.
J. Atmos. Sci
Meteorol. Z.
J. Hydrometeor.
Q. J. R. Meteorol. Soc.
Geophys. Res. Lett.
J. Climate
Mon. Wea. Rev.

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Geosci. Model Dev.
Boundary-Layer Meteorol.
Bull. Amer. Meteor. Soc.
Atmos. Chem. Phys.

9. P13, L31: Something is wrong with the last sentence, please rephrase.
10. P14, L9: The expression "systematics of precipitation" is unusual, perhaps better: "especially concerning the existence and strength of the soil moisture-precipitation feedback."
11. P15, L11: Here are \LaTeX commands in the link: $\backslash T1$ etc., please remove them.
12. P15, L20: Germany, not germany
13. P16, L13: Please remove the brackets from {E}, {GPU}, {COSMO}
14. P16, L21: Something is wrong with the entry for the pages: 407–430+341?
15. P16, L22: Muhlbauer, A.
16. P16, Reference Schättler et al.: The latest version of the COSMO user guide is from the year 2016, please update your entry.

Interactive comment on Atmos. Chem. Phys. Discuss., doi:10.5194/acp-2016-1049, 2017.

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