General Comments:
This paper uses an idealized simulation with prescribed soil moisture gradients to derive a simplified algorithm that represents the amount of precipitation generated by local evaporation and advection terms. The authors note that previous studies have qualitatively shown how soil moisture gradients and atmospheric profile influence precipitation, and state that their goal is to quantitatively isolate the primary drivers of precipitation. I believe their methods, i.e. using an idealized model with prescribed soil moisture gradients, are sound, and their results are relevant. Overall, I find that the paper convincingly demonstrates the relative important of soil moisture gradients over the absolute magnitude of soil moisture, which makes sense physically, but it glosses over some other important points that deserve more explanation, such as the importance of the atmospheric profiles. Also, the derivation of the algorithm they use seems fine, but needs some clarification in order for the reader to be able to completely recreate their results. The second stated goal of the paper is to determine "what is the relative role of the atmosphere, or in other words the efficiency in converting these potential moisture sources into precipitation." Terms that represent the efficiency of advection and evaporation are derived, but there is no discussion of how the actual atmospheric profile impacts those terms, which then detracts from the significance of these findings. Also, while the authors cite publications that use the two atmospheric profiles utilized in the model simulations, they do not display them in a figure or discuss them in any way. This leaves the reader wondering what the difference is between them, what the profiles are like, and how these profiles could affect the results. For example, a profile that is more unstable could increase convection and strengthen the circulation, however there is no context like this provided in the paper. Also, I looked up the two profiles in the cited publications and found it difficult to compare them because they are presented in different formats. Because of these oversights, the reader is left unsure why the authors included two different profiles in the first place, and how the atmospheric profile impacts the authors' findings.
Response:
We thank the reviewer for his/her comments which helped us to revise the parts of the manuscript which were not clear. As suggested by the reviewer we clarified why the particular soundings employed in the study were chosen and decided to add a figure with two skew-t diagrams relative to the different atmospheric soundings. Furthermore, we elaborated more on the physical meaning of the efficiencies and why the atmospheric profiles have different ones. In the following we present the responses to the reviewer specific comments and technical corrections, which also answer the questions presented in the general comments.

Specific Comments:
1. page 3 line 10-12: "the change of precipitation with soil moisture does not depend on the soil moisture content itself and that the most efficient way to increase precipitation consists in increasing the surface wetness gradient.", but page 1 line 8-9: "these changes surprisingly do not depend on soil moisture itself but instead purely on parameters that describe the atmospheric initial state." — is it the atmospheric state or the soil moisture gradient that is most important? Also, see my other comments about the importance of addressing the atmospheric state more thoroughly in the paper.
Response: we agree with the reviewer that the presence of both sentences was misleading. For this reason we modified them in the manuscript and added some clarification notes on the dependency of precipitation on both soil moisture and the atmospheric state. We revised section 4.3 and stressed the conclusion that, although the derivative of precipitation does not depend on soil moisture but just on the atmospheric state through the efficiencies and the B parameter, the absolute value of precipitation does depend on soil moisture as shown in Eq. 15.

2. Page 10, Line 11: "In order to test the validity of the theory proposed in section 2" is confusing. This is stated in section 2, and I'm not sure what the theory is. Suggest repeating what the theory is or otherwise clarifying here.

Response: We apologise for the wrong reference: it should have been Section 3 instead. We corrected this in the manuscript.

3. Page 4: Please clarify why the "dry-soil advantage profile of Findell and Eltahir 2003" is used and why it is appropriate for this investigation.
Response: This particular sounding was observed on 23 July 1999 in Lincoln, Illinois (USA) and was chosen as a typical example by Findell and Eltahir 2003 for cases when a strong heating of the surface forces the triggering of convection. Given that on the dry patch low soil moisture availability causes strong sensible heat fluxes to heat the air above we though that using this sounding would produce the strongest response in the atmosphere.

4. Please include an additional figure with the two atmospheric profiles (from Findell and Eltahir 2003 and Schlemmer et al. 2012).
Response: we added an additional figure in the manuscript with a skew-t diagram of the two soundings (see Fig.1 of this document).

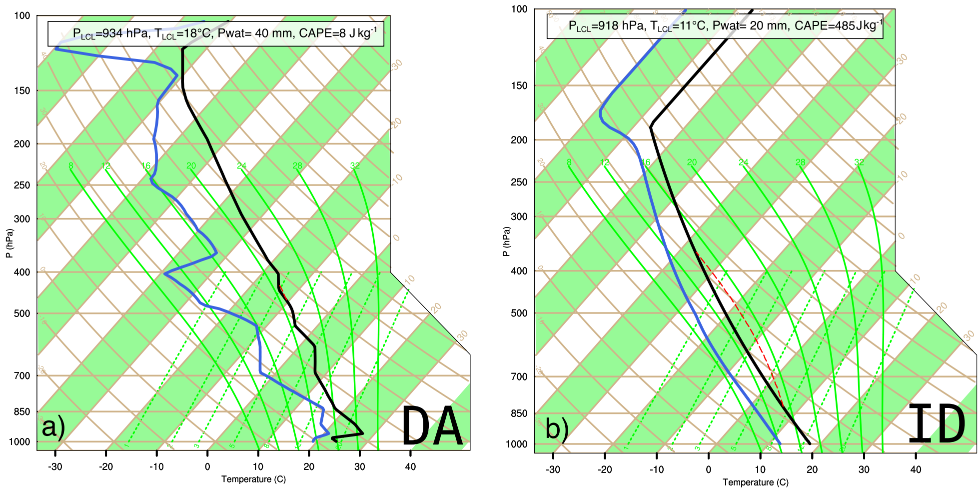

Figure 1: Skew-t diagrams for the sounding used in the simulations.

5. Page 4: Please clarify why the Schlemmer et al. (2012) profile is used over a different one, what question is answered by including it in the study, and how it differs from the profile from Findell and Eltahir 2003.
Response: we used the profile of Schlemmer et al. (2012) for one main reasons, namely that it greatly differs from the sounding of Findell and Eltahir (2003). In particular it has a lower surface temperature and lower integrated water vapour content although having a larger initial instability. This allows us to test the idealized model and show that efficiencies and the B parameter do depend on the atmospheric state. We performed additional simulations using the second sounding presented in Findell and Eltahir 2003 but, as the results were similar to the DA case, we didn't include those in the manuscript. We added a few sentences in the manuscript to justify our choice.

6. Figure 2: This figure takes some time and effort to interpret. It would be easier for the reader if vectors were used in place of windspeed contours and if the "dry" and "wet" sides are labeled. Also, please add a sentence to the text explicitly stating which side in Figure 2 is warmer (and why) and which direction the front is propagating. This all may seem obvious, and is stated more explicitly later in the text, but to the first-time reader it takes time to put it all together while examining figure 2.
Response: We changed Fig. 2 in order to ease its interpretation. We used vectors instead of contours to indicate zonal winds and used explicit labels for the dry and wet patches. We think now it is clear

where the patches are located so that there is no need to add another sentence to say which side of Figure 2 is warmer and in which direction the front is propagating.

7. Page 8 Line 24: Clarify what "the fact that one efficiency doesn't match well" means. Which efficiency? And it doesn't match well with what?
Response: We meant that one efficiency is not enough to describe the variations of precipitation. As shown in Fig. 6 when using a single efficiency the decrease of precipitation with increasing value of soil moisture on the dry patch cannot be captured. We rephrase this sentence in the manuscript.

8. Page 8 Line 29: which sounding is "another sounding?". Also see previous comments about soundings. This would be a good place to spend some time discussing what it is about the two profiles that result in efficiencies that are higher than with the first sounding.
Response: We were referring to the sounding of Schlemmer et al. (2012) so we added this explicit reference in the text. We think that the higher efficiencies obtained with the Schlemmer et al. (2012) sounding are due to a combination of different effects. One of those is the different convection triggering. With the sounding of Schlemmer et al. (2012) convection is triggered almost 1 hour before than with the sounding of Findell and Eltahir (2003). This allows the atmosphere to fully exploit the instability caused by the morning heating and to develop a stronger front propagation, as shown in Fig. 6 of the manuscript. Although the maximum advection of moisture over the dry patch in the ID cases is smaller than the one of the DA cases, the atmosphere is able to efficiently convert it into precipitation, thus leading to larger efficiencies. This is also evident in Fig. 2 of this

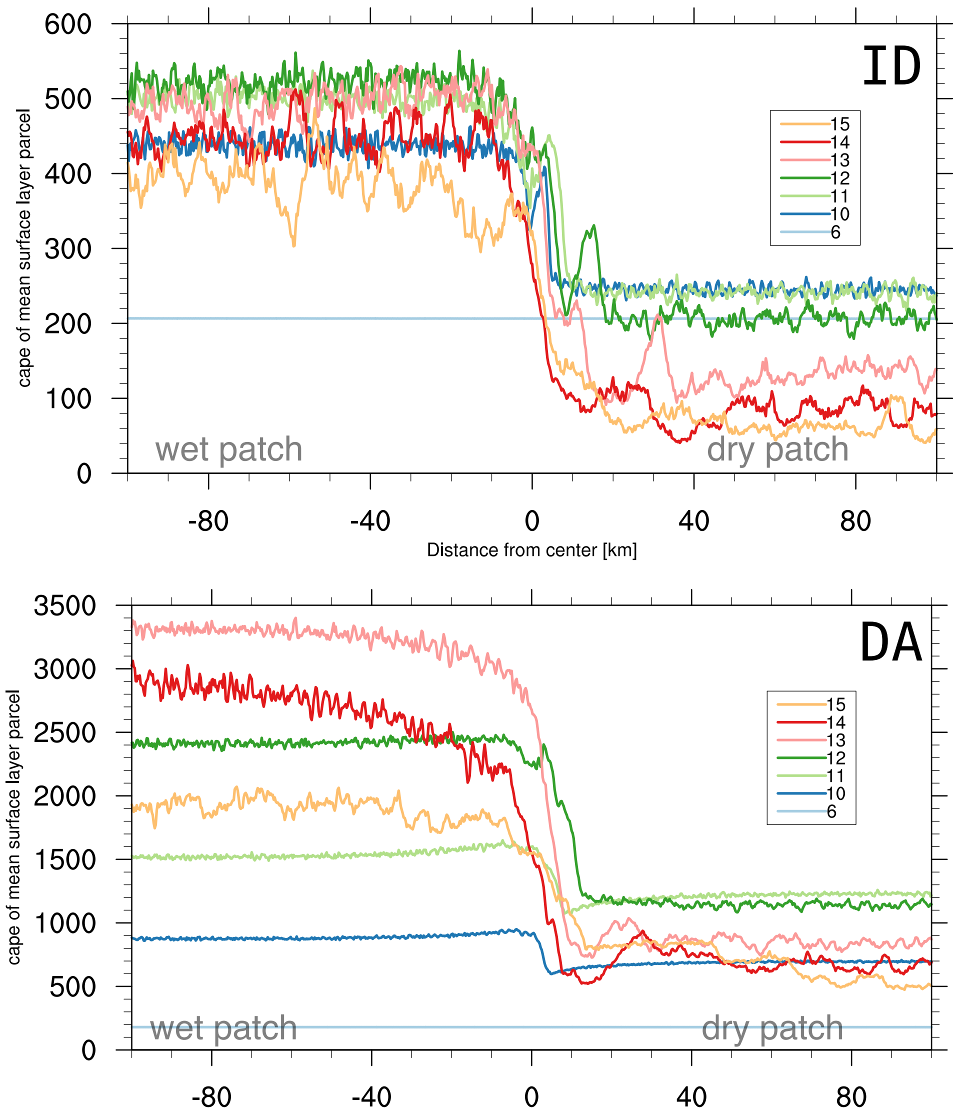

Figure 2: meridional average of convective available potential energy (CAPE, J/kg) for the simulation with the ID sounding (upper panel) and the DA sounding (lower panel). For both simulations the extreme case with a dry patch at 20% saturation is used. The lines indicate the value at different hours during the day (6 LST, initialization time, to 15 LST).

document where the value of CAPE is plotted as a function of time and x-dimension only. When using the DA sounding it can be seen that convective potential instability at 15 LST is larger than the one at the initial time over both patches, while in the ID sounding the opposite happens. Furthermore, the Findell and Eltahir (2003) sounding was not prone to the development of intense rain events, as shown in Cioni & Hohenegger (2017), thus the smaller efficiencies. We added this brief discussion to the manuscript.

9. Page 9 Line 2: "a weaker sensitivity of that particular atmospheric state" . . ..see above comments about the atmospheric profiles. This reference is too vague, and needs more explanation.
Response: We meant that with this sounding precipitation amounts seem not to depend much on the advected moisture, as the estimated B parameter is smaller. We corrected the reference on the manuscript.

10. Page 10 Lines 18-19. This is the first point that the soil type is referenced. The data and methods should include a sentence stating the soil type used in the simulation, the reason why it is used, and its field capacity.
Response: We added a sentence stating the soil type used (5, loam) and the reason why it was chosen, specifically because it is the most frequent soil type over Germany. Also the field capacity and the wilting point were added to the manuscript.

11. Page 12: The derivation of beta needs some more explanation. Was it derived using a best fit method from Figure 3? I'm not sure.
Response: as explained in Lines 11-20 the parameter B is obtained through a best fit of the values of advection and evaporation, where evaporation is approximated through Eq. 4. We further clarified this aspect in the manuscript.

12. As a reader, it was difficult to get through sections 4.2 and 4.3. There were some jumps in the logic between equations that were hard to follow, and not all terms were defined (see above). I think if the authors revisit these sections and provide more explicit explanations even where they think the transitions should be obvious, it will help the reader finish the paper.
Response: We agree with the reviewer. For this reason we revised section 4.2 and 4.3 by expanding all the steps used in deriving Eq. 8 and 9 from Eq. 5 and Eq. 15. We added a definition for all the missing variables and further expanded the explanations in the text.

13. Page 15, Line 20: "these parameters depend solely on the atmospheric state." See above comments.
Response: see answers above.

14. Figures 9 and 10: These are important figures. More explanation of these figures is needed, particularly the significance of n_a < n_b (and visa versa) and of beta, and what that means physically. As a reader, I found myself quite bogged down by this point and it was difficult to extract what the authors were hoping to convey with these figures.
Response: We agree with the reviewer: these are the most important figures of the paper and deserve more explanation. We think that the significance of n_a < n_e (and vice-versa) is related to the different way advection and evaporation sources are used by the atmosphere to produce precipitation. In our simulations we always find that the efficiency of advection is larger than the efficiency of evaporation which would mean that the atmosphere is somehow able to use more of the advected moisture than of the evaporated one to produce precipitation. The B parameter, instead, appears to be an additional parameter which describes the importance of advection. We think that this is related to the strength of cold pools, which enhance the advection processes. We revised the explanation of these figures.

Technical Corrections:
Page 1 Line 1-2: For clarity, I suggest rewording the first sentence of the abstract to read "Soil moisture heterogeneities influence the onset of convection and subsequent evolution of thunderstorms producing heavy precipitation through the triggering of mesoscale circulations."
Response: Corrected.

Page 1 Line 6: Suggest rewording to read "A key element of the model is the representation of precipitation as a weighted sum"
Response: Corrected.

Page 1 Line 18: Suggest rewording to read "and which can then affect the distribution of precipitation."
Response: corrected

Page 2 Line 17: Please clarify what is meant by "a negative spatial coupling coexists together with a positive temporal coupling."
Response: We added a clarification sentence: " That is, areas drier than their surrounding (spatial component) but wetter than the climatological value (temporal component) may receive more precipitation than other ones"

Page 3 Section 2.1 heading: Is the subheading "2.1 Experimental Design" needed here? There are no other subsections in Section 2.
Response: corrected.

Page 3 Line 3: "overt" should be "over"
Response: corrected.

Page 6 Lines 6-8: This sentence is difficult to understand. I suggest rewording it.
Response: We added further clarification when describing the algorithm: "More specifically, at the first two time instants the maximum is searched over the entire dry patch while from the third time step onward the maximum search is performed in a box centered on a first guess obtained from a simple linear extrapolation of the previous time instants. "

Page 7 Line 11: "It is immediate to verify" is awkward. I suggest rewording.
Response: we changed the sentence to "it can be verified".

Page 8 Line 5: "firstly" should be "first"
Response: corrected.

Page 8 Line 23: The text states n_a = 0.15 and n_b = 0.10, but Figure 5 states that they are 0.16 and 0.11, respectively.
Response: This was not a typo and deserve some explanation. Both efficiencies can be estimated either from the extreme case DA_20_100 and DA_100_100, assuming that evaporation or advection, respectively, are negligible, or from fitting Eq. 2 to the value obtained in the simulations (as done in Fig.5). The estimate of the efficiencies obtained with these two different methods are just slightly different (0.15 vs. 0.16 and 0.10 vs. 0.11), thus the confusion. We clarified this aspect in the manuscript to make clear that one could use both methods to estimate the efficiencies.

Page 10 Line 19: what is "the expected one"? Please clarify.
Response: we meant the wilting point of the particular soil employed in the simulations. This is now clarified in the manuscript, also in light of the previous comment about the soil characteristics.

Equation 6: I couldn’t find a definition for L_front anywhere in the text. Please include a definition here.
Response: L_front represents the penetration lenght of the front. We added a definition for L_front in section 4.2.