General Comments
This study aims to identify the most important parameters that impact rainfall variations over spatially drier patches. Relying on idealized simulations and a simplified model, the authors concluded that precipitation changes over a heterogeneous surface do not depend on soil moisture, but the initial atmospheric state. The research is interesting. However, the manuscript needs to be substantially clarified and the formulation of the simplified model should be further justified.
Response: We thank the reviewer for his/her precious comments which helped us to revise many sections of the manuscript which were not clear. In the following we will answer all the major and minor comments of the reviewer.

Major Comments
1. The simulations
As described in Section 2, the simulations were conducted using an atmospheric model (ICON-LEM) coupled with a land surface model (TERRA-ML). Accordingly, it appears that soil moisture over both the dry and wet patches evolves as the model integrates forward and soil moisture in each experiment is only specified at the initial time. I am not completely sure about whether this is the case, because assumptions in the simplified model are more consistent with simulations using constant soil moisture values throughout the model run. Please clarify. Also, please briefly describe the purpose of reducing dynamic contributions of advection on precipitation when setting up the size of the simulation domain (Pg. 3, lines 26-28).
Response: The referee is right: soil moisture in our setup is prescribed at the initial time and then freely evolves during the day as a response to the atmospheric forcing (precipitation, evaporation...) and to the soil model. Thus, whereas in the formulation of evaporation of the simplified model (Eq. 4) we used the value of soil moisture at initialisation time and assumes it stays constant, soil moisture, and thus evaporation, will change in the simulations. However changes in soil moisture over one diurnal cycle are not expected to be so strong to significantly feed back on evaporation and precipitation. We investigate this by computing the daily average value of soil moisture in the simulations and comparing it to its initial value (see Tab. 1 of this document). Except in the

Case	Initial soil moisture (dry patch average) [$m^3\ m^{-3}$]	Diurnally-averaged soil moisture (dry patch average) [$m^3\ m^{-3}$]
DA_20_100	0.0908	0.109
DA_50_100	0.227	0.227
DA_65_100	0.2951	0.270
DA_100_100	0.454	0.291

Table 1: Prescribed initial value of volumetric soil moisture and diurnally-averaged value over the entire period of the simulation for different cases (first column). All values are considered averaged over the dry patch, although for the initial soil moisture we prescribe the same value everywhere over the dry patch (see manuscript).

DA_100_100 case, the values are fairly similar. The big difference between initial soil moisture and diurnally-averaged soil moisture in DA_100_100 is due to the fact that the soil cannot stay saturated and thus the soil model will produce an instantaneous runoff to bring back soil moisture to the field capacity. However this has no effect on the evaporation as it does not change for soil moisture values larger than the field capacity (see Fig.7 of the manuscript). We will clarify this aspect in the manuscript.
When deciding the domain size we wanted to reduce as much as possible the dynamical contribution of advection on precipitation, that is the spurious effect of the front collision on precipitation. As showed in Fig. 6 the collision of the two fronts in the middle of the dry patch,

because of the periodic boundary condition, enhances convergence, uplift and thus precipitation. Since such effect can alter the interpretation of the results we wanted to delay the diurnal front collision as much as possible, while keeping the computation costs affordable for running several sensitivity experiments. That's why we settled on a domain which is 400x100 km^2 big.

2. The simplified model

1) **Assumptions:** According to Section 4, the authors assumed that "Ewet does not depend on Ï ¸Tdry" (Pg. 13, line 1) and "evaporation over the dry patch does not depend on the soil moisture of the wet patch" (Pg. 14, line 1). These two assumptions are needed to get the key results (Eqs. 10, and 15), but not clearly justified. When either Ï ¸Tdry or Ï ¸Twet varies, should not precipitation over the wet or dry patches change, which in turn impact Ewet or Edry through the impact on soil moisture therein? On Pg. 11 (lines 2-7), it is assumed that the advection of water vapor and hydrometeors is mainly constrained in the boundary layer. As shown in Fig. 2, however, the return flow at ~1-3 km is not negligible. Could you please justify this assumption further?

Response: There is no dependency of E_{wet} on ϕ_{dry} because in Eq. 4, and more generally in the Budyko formulation, evaporation over a surface depends on the local soil moisture, in this case ϕ_{wet}. Furthermore, our formulation of the evaporation considers a soil moisture constant in time, as explained in the answer to the previous comments. This is well justified as we only consider one diurnal cycle: over this period precipitation is expected to change soil moisture only marginally and thus not to change evaporation appreciably. Regarding the return flow it should be noted that in our simulation this branch of the circulation has a much weaker intensity (in terms of zonal velocities at least 50% less) and lasts just for a few hours. For these reasons we consider it as negligible when developing the idealized model.

2) **Derivations:** Please provide more details on how to approximate Eq. 5 to get Eq. 6, and how Eq. 7 is obtained. To get Eq. 8, it seems that one has to assume the vertical extent of moistening process due to latent heat flux, Hmoist, is the same over the dry and wet patches. Is Hmoist related to turbulent eddies? If so, this assumption can be problematic because low level temperature differences are up to ~4 K between the dry and wet patches (Fig. 2), where sensible heat flux differences can reach 280 W/m2 (Pg. 5, lines 5-6).

Response: We decided to revise section 4.1 and 4.2 and specifically to remove Eq. 6 since it contained an ambiguous notation. Instead we decided to include the approximation of specific humidity (eq. 7) and then proceed to explain how to obtain Eq. 8. The latter equation assumes that the vertical extent of the moistening process H_{moist} is the same as the vertical extent of the breeze circulation H_{front}. We agree with the reviewer that the two are not exactly the same. To check the validity of this assumption we computed these two heights from the simulations. H_{moist} was computed as the height of the PBL over the wet patch, diagnosed with the bulk Richardson number method (see Seibert et al., 2000). H_{front} instead was computed as the height at which the zonal pressure anomaly ahead of the front reaches 0 (see Rochetin et al., 2017). Although slight differences up to 300-500 m were present at some time instants, the two variables showed similar values. As the goal is to develop a simplified model that only retains the main drivers of precipitation variability, we think that the assumption is well justified. Furthermore, it should be noted that, since H_{moist} does not depend on ϕ_{dry}, including its effect won't change the results, i.e. the fact that the derivative of precipitation does not depend on ϕ_{dry}. We included this information in section 4 of the manuscript.

3) **Comparison to Lintner et al. (2013)** As noted in the article (Pg. 10, lines 26-30), feedbacks between the land-surface and atmosphere are neglected in the simplified model after taking evaporation as Eq. 4. Consequently, it is not unexpected that soil moisture can be irrelevant to

precipitation change in the simplified model. If the formulation of evaporation in Lintner et al. (2013), where land-atmosphere interactions are considered, is used in the derivation, will the theoretical model proposed here still be valid?
Response: We have to disagree with the reviewer. Within the framework of our simplified model precipitation changes are independent of soil moisture as the derivative of precipitation with respect to soil moisture does not depend on the latter. As explained in the manuscript this is an effect not only of the linear dependency of evaporation on soil moisture but also of the constant front velocity. Note that the idealized model does not consider explicit land-surface interactions but is in good agreement with the results of the simulations which are coming from a coupled land-atmosphere model. Thus, we don't think that the results would differ much in case the land-atmosphere interactions would be considered as long as one diurnal cycle is considered (see also our response to comment 1 above).

4) **Precipitation efficiency associated with evaporation and advection** Could you please elaborate further on why precipitation efficiency is independent of soil moisture? Although the authors showed that precipitation efficiency associated with advection is independent of evaporation using the extreme case DA_20_100, where Edry is negligible, it is not clear on why precipitation efficiency associated with evaporation is independent of soil moisture. Overall, it is hard to evaluate the simplified model according to how it is presented. The conclusion that precipitation change over a heterogenous surface is independent of soil moisture can be an artifact that land-atmosphere interactions are eliminated in the theoretical model.
Response: From the physical point of view, soil moisture controls directly evaporation but not precipitation. The control of soil moisture on evaporation is already included in Eq. 4. The efficiencies then describe the processes that take place in the atmosphere which convert moisture sources (advection and evaporation) into precipitation. They are used to represent the fact that two different atmospheric states will produce different precipitation amount, even though the soil moisture and evaporation can be identical. For this reason, the efficiencies should not be defined as functions of soil moisture.
The conclusion that precipitation changes over a heterogeneous surface are independent of soil moisture is related to the assumptions made in the idealized model: as long as the front propagation velocity does not depend on soil moisture and the evaporation is a linear function in soil moisture our results won't be affected. Moreover, as shown in Figs. 8 and 9 of the manuscript, our assumptions are justified as our model, despite its simplicity, is able to reproduce the simulation results fairly well. We clarified these aspects in the conclusions and in section 4.

3. Writing
The manuscript requires an editorial revision to correct wording issues. Some sentences are either awkward or redundant. For example, ". . ., in a nutshell, . . ." (Pg. 6, line 4), ".. thanks to the previous section . . ." (Pg. 12, line 12) and etc. can be removed.
Response: we removed these ambiguous sentences.

Minor Comments
1.Are equations 5 and A2 written correctly as advection? It is also unusual to have dot product between a scalar (qtot) and a vector (ufront or v).
Response: We modified the equations by removing the dot and using vector notation. Otherwise the equations are correctly written.

2.Pg. 2, lines 16-17: Please clarify further on Guillod et al. (2015), what does ". . . a negative spatial coupling coexists together with a positive temporal coupling" mean and indicate?
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"

3.Pg. 3, line 25: Why a rectangular domain can limit computational cost?
Response: This sentence was meant to represent a comparison between a squared domain (which in our case would be 400x400 km^2 big) and our chosen rectangular domain (400x100 km^2) which contains less grid points and is thus less expensive to run simulations on.

4.Pg. 9, lines 6-7: Please provide relevant evidences on "... several secondary events develop due to the waves propagating away from the collision".
Response: We agree that using the term"wave" was not appropriate so we rephrased this part to read "In the ID_20_100 case strong precipitation events with local maxima of 10 mm h^{-1} are produced in the center of the patch after the fronts' collision and several secondary events develop due to the fronts propagating again away from the collision. "

5.Fig. 2: It can be better to show wind as vectors, rather than contours.
Response: We revised Fig. 2 to ease its interpretation. Now winds are displayed through vectors and not contours.

6.Fig. 9: Change "∂Pdry/Ï ¸Tdry" as "∂Pdry/∂Ï ¸Tdry".
Response: We thank the reviewer for spotting this typo, which we promptly corrected.

7.Table 1: Change "Name" as "Experiment".
Response: Corrected