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Hydrology and Earth System Sciences
Discussions

Interactive comment on "A simplified model of precipitation enhancement over a heterogeneous surface" *by* Guido Cioni and Cathy Hohenegger

Anonymous Referee #2

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Review of manuscript (hess-2017-547): A simplified model of precipitation enhancement over a heterogeneous surface By: G. Cioni and C. Hohenegger Recommendation: Major Revision Anonymous: Yes

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.

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).

2. The simplified model 1) Assumptions According to Section 4, the authors assumed that "Ewet does not depend on ÏŢdry" (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 ÏŢdry or ÏŢwet 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?

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).

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?

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.

2. 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.

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).

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?

3. Pg. 3, line 25: Why a rectangular domain can limit computational cost?

4. Pg. 9, lines 6-7: Please provide relevant evidences on ". . . several secondary events develop due to the waves propagating away from the collision".

5. Fig. 2: It can be better to show wind as vectors, rather than contours.

6. Fig. 9: Change "∂Pdry/ÏŢdry" as "∂Pdry/∂ÏŢdry".

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

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