Response to reviewer 2 - detailed response

This supplement gives detailed answers to all comments of reviewer 2. The original comments are quoted in italic.

Detailed comments

The authors consider the atmosphere as a box with vertically uniform properties that is being advected along a stream line. I do not see any justification for this assumption. Moisture is not well mixed in the atmosphere and wind speeds are not constant with height, which means that the origin of the moisture varies with height as well. An illustrative example is the West-African monsoon system. Here, at lower levels moisture is being advected from the Atlantic ocean towards the continent, whereas the large scale flow patterns at higher levels are in the opposite direction. Such a situation, which is not uncommon in locations with strong coupling between the land surface and the atmosphere, seems to be hard to describe using the model in this paper.

Answer: This comment is well taken. In presence of an extremely layered system as the one mentioned in the above comment, it will indeed be difficult to use our model, especially on the local scale (<~500 km). However, on the continental scale (>~500) we can imagine that we might still try to describe the larger scale system, i.e. the higher part involving advection of moisture from the East-Africa. Whereby we could treat the Atlantic moisture as an lateral influx (I(x,t)) or have a more complicated double system. However, this application lies beyond the scope of this paper. In the revised version, we will add a comment on the limitation of our model for the specific case of layered systems

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Then, as the first reviewer already suggested, the assumption of having a constant rate of change of soil moisture in time takes away the applicability of your model in regions where soil-atmosphere feedbacks are most exciting, namely those where the soil moisture content is a strong function of the local precipitation rate and vice versa, and not of the season. A fixed rate of change of soil moisture represents a non-coupled system and limits the applicability of the model to regions where the soil moisture is determined by large scale meteorological systems.

Answer: The soil acts a reservoir which transforms the rainfall input pulses into the much smoother signals of transpiration and runoff (here we use signal as a synonym for a time series). Therefore, the temporal evolution of soil moisture at any given place will always show a low frequency signal (seasonal filling and emptying), overlain by small scale fluctuations (see schematic in Fig. 1). Both components of the soil moisture signal are directly related to the local precipitation rate; we simply chose to describe the low frequency component by a constant rate of change and to neglect the high frequency fluctuations. We agree, that there are regions where the high frequency components might be more important than the low frequency (seasonal) component, e.g. in arid or very humid climates where there is complete absence of a seasonal component. We will add a note on this in the revised version.

In the revised version we will also make more clear that the purpose of this model is to understand what happens on the continental scale (>~500 km) rather than on the local scale (<~500 km), where indeed local coupling processes might be dominant (e.g. Findell et al., 2011;Taylor et al., 2011).

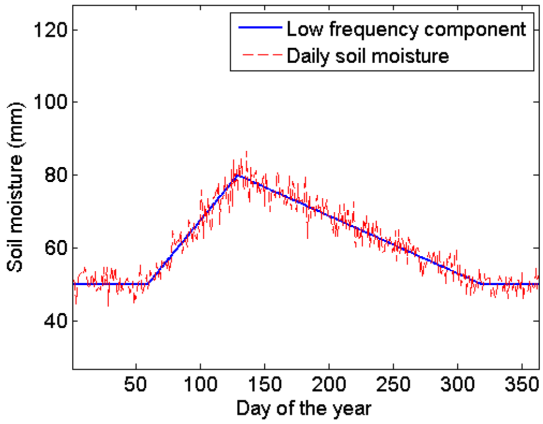

Fig. A1: Schematic illustration of the temporal evolution of soil moisture with a seasonal cycle and daily fluctuations

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Furthermore, balancing soil moisture in a Lagrangian framework seems rather odd to me as the soil column should not advect together with the air column. Streamlines are variable in time, and have a much shorter time scale than soil moisture, so I do not directly see how soil moisture can be balanced along a stream line. Therefore, I would like the authors to clarify this decision.

Answer: We acknowledge this comment but we do not see the problem of using a moving reference system for a quantity that is not moving itself. The parameterized exchange between the atmosphere and the soil only depends on the location but not on the advection. The reviewer is of course right when he/she points out that streamlines are variable in time, however we would like to argue that this is a necessary simplification to start understanding E-P feedbacks better on the continental scale. This is e.g. in line with the schematics of Goessling and Reick (2011, Figure 8) and Savenije (1995, Figure 1).

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Then, the authors do not justify the assumption that the parameters u_x, τ_q, τ_p, τ_e, e_m, s_m and I are constant in space.

Answer: Assuming that these parameters are constant in space (at least piece-wise along portions of the trajectory) corresponds to the assumption that the large-scale recycling phenomenon depends on large scale ecosystem patterns and average climatic conditions rather than on local scale heterogeneities.

In the theoretical framework of the model presented here, we keep them constant, however in a real case scenario these parameters do not have to be (certainly they will change due to e.g. orographic effects), but this “problem” can easily be overcome by discretizing the system where each spatial unit can have different parameters.

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To make their model convincing, I suggest that the authors take reanalysis data to back up their assumptions and to show that their framework at least reproduces the general trends of soil moisture and atmospheric moisture when moving over streamlines, before they apply the model to explain the behavior of the land-atmosphere system under different regimes.

Answer: As we discussed in the manuscript, the assumptions about the climatic variables are based on re-analysis data. We agree that a detailed comparison of our model output to data would be extremely interesting. For atmospheric moisture, such evidence can hardly be provided. In fact, observed soil moisture data (e.g. through remote sensing) refers to only the first few centimeters of the soil; corresponding re-analysis data is the result of this data assimilation into a land-atmosphere model, i.e. reanalysis data basically reflects the assumptions in the model rather than the nature. Such a comparison is underway, but we believe that presenting such results should be the topic of another paper. For atmospheric moisture, we believe that presenting the results of such an analysis should be the topic of another paper.

References

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