Response to major concerns of reviewer 1 (Helge Goessling)

We would like to thank Helge Goessling for his overall positive feedback on our approach, his challenging critics and the detailed suggestions of how to improve the manuscript. Hereafter, we give detailed answers to all comments. The original comments are quoted in italic.

Major concerns

(..) the authors have not yet convinced me that their model does a good job at representing the critical aspects of the system under investigation. (..) I have two main concerns. The latter relate to 1) the far-reaching consequences of the assumption "ξ=const", and 2) the insufficient acknowledgement of the fact that soil-atmosphere coupling is considerably more than moisture recycling.

1) Assumption of constant soil moisture change rate ξ

In the following I try to sketch what I think that ξ=const means for the system behaviour. At any point along the air's trajectory, infiltration (IN:=P(1-α)) is determined by W alone, and S is determined by the constraint that the sum of evaporation (ET) and runoff (R) must balance IN-ξ, i.e. ξ=IN-ET-R. Excluding the special case ξ=0, ξ=const implicates that, for the air following the "first" air parcel, either the assumption ξ=const cannot hold anymore (as soil moisture has changed in time according to ξ), or that mass conservation in the soil compartment is violated, i.e. the meaning of ξ itself is ignored.*

In other words, the constraint ξ=const can only be used to determine the "initial condition" of the soil, but the unconstrained system (i.e. Eqs. (14) and (15)) would have to be used from that moment on if mass conservation shall not be violated. One could now argue that the model shall only capture a "snap-shot" in time, but my feeling is that the model would be much more conclusive if it represented the situation prevailing during more than just a moment in time, e.g. one season (which, I surmise, might be what the authors actually intend with their model).

Another consequence of the constraint ξ=const is, as the authors show formally with Eqs. (31) and (32), that for a substantial fraction of the "plausible" parameter space (compare Tab. 2), W and/or S become unphysical (and with them the fluxes). For example, S(W0) is negative if W0 is just slightly lower than 0.5 with standard parameters (compare Fig. 2, red curves). In "case 3" (slow ET, fast R) with I=0 (as additionally introduced for Eq. (28), using standard parameters otherwise including ξ>0), (W1) and the corresponding R are negative (as follows from W1=sqrt(I)=0 put into Eq. (16) -> S(W1=0)=-tauq*ξ). (..) All in all, the constraint ξ =const seems to turn the causality upside down: Physically, ξ should be the result of infiltration, evaporation, and discharge (as in Eq. (15)) rather than the other way round. (..) I have no simple suggestion for a change of the model. If the authors decide to stick to their model as it is, i.e. including the assumption ξ=const, I would like to read convincing arguments against my objections.*

Answer: We acknowledge this comment but we beg to disagree with the overall conclusion that we turned upside down the causality. In fact, the chosen formulation does not invert the causality since ξ does, as it should, result from infiltration, evaporation and discharge (as the reviewer points out, it holds that *ξ=IN-ET-R).* Our model assumption of ξ = constant (for individual seasons, not for the entire year) simply does not allow it to change differently during subsequent time periods of the same season. This means that we impose a gradual change of the average soil moisture throughout a season.

Our approach of x = constant is similar to linearising a differential equation, where one assumes some variable $z=z(S_0)$, when in fact $z=z(S)$. In our case, to find a solution for $S(t)$, we have $\mathrm{d}S/\mathrm{d}t = \mathrm{d}S/\mathrm{d}t|(S=S_0)$. Of course, the value of ξ should not be chosen arbitrarily but e.g. based on the analysis of real data. We will add a comment on this in the discussion section.

2) "Moisture recycling" versus other mechanisms of evaporation-precipitation coupling.

The model, and the study as a whole, seems to build strongly on the assumption that, at the considered (i.e. continental) scale, moisture recycling (i.e. the effect evaporation has on precipitation via the atmospheric moisture budget) is much more important than other mechanisms through which evaporation affects precipitation. How does this underlying assumption fit together with the view that seems to prevail in the "meteorological community"? Seneviratne et al. (2010) formulate this view as follows: "The key for understanding soil moisture-precipitation interactions lies more in the impact of soil moisture anomalies on boundary-layer stability and precipitation formation than in the absolute moisture input resulting from modified evapotranspiration".

Answer: Moisture recycling can be seen as a rainfall-bringing mechanism (i.e. extra moisture in the atmosphere causing rain) as well as a result of rainfall-bringing mechanisms (i.e. locally or continentally evaporated water simply rains out irrespective of the mechanism that caused rainfall. In the revised version, we will stress that the recycling length scale and ratio is in fact an indicator for when moisture recycling becomes the dominant driver of sustaining (not equal to triggering) rainfall (i.e. when $\lambda(x)/L \cong x/L$ or $\rho\cong0.5$). (see similar discussion hereafter).

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It seems that Schaefli et al. are aware of this argument, since they write in P8317,L11-13 that "From a meteorological perspective, [the limited attention given to moisture recycling] is not surprising since advective moisture fluxes are generally an order of magnitude larger than evaporative fluxes". But the authors leave this marked dissent largely unresolved. Fortunately (for the relevance of this study), I think that the above sentence is only partly correct: advective moisture fluxes are an order of magnitude larger than evaporative fluxes only if the considered spatial scales are sufficiently small.

Answer: We agree that the cited statement should have been put into, e.g. a spatial context. We will change the text to: "From a meteorological perspective, this is not surprising since advective moisture fluxes are often an order of magnitude larger than evaporative fluxes, especially at small spatial scales."

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Actually, the "recycling length scale" reveals at which spatial scale the integrated evaporative flux measures up to the advective flux. However, the fact that moisture recycling becomes relevant at continental scales does not imply that moisture recycling is necessarily more important than local coupling. Finally, when large-scale land-surface modifications are considered, changes in the large-scale circulation play an important role in determining the overall hydrological response as well. We discuss these scale aspects in some detail in our own recent study (Goessling and Reick, 2011, particularly Sect. 2.4).

Answer: Local influences (in time and space) of e.g. soil moisture on precipitation (e.g. Findell et al., 2011; Hohenegger et al., 2009; Taylor et al., 2011; Van Heerwaarden et al., 2009) usually prevail over moisture recycling as rainfall-triggering mechanism (Seneviratne et al., 2010), however the recycling length scale and recycling ratio in fact reveal the scale at which moisture recycling becomes the dominant driver of sustaining (not equal to triggering) continental rainfall: i.e. when $\lambda(x)/L \cong x/L$ or $\rho\cong0.5$. At this scale the evaporation that occurred along the streamline outweighs the advective flux. Based on an extreme land-use change scenario Goessling and Reick (2011), however conclude that moisture recycling becomes significant well before ρ≅0.5, but also note that large scale circulation changes might play an important role in determining the overall hydrological response as well.

We will modify the text to: "From a meteorological perspective, this is not surprising since advective moisture fluxes are often an order of magnitude larger than evaporative fluxes (e.g. Schär et al., 1999), especially at small spatial scales. Furthermore, the focus is often on precipitation triggering mechanisms (e.g. the effect of soil moisture conditions on boundary layer stability and precipitation, Seneviratne et al., 2010) rather than on mechanisms that sustain rainfall as in the present paper."

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Partly it seems to me as if Schaefli et al. equate "moisture recycling" with "local coupling". This is for example the case in P8338,L13-16, where the authors talk about "moisture recycling hotspots" and then refer to Koster et al. (2004). However, I would argue that the "hotspots" presented in the latter study are probably to a larger extent "local coupling hotspots" than they are "moisture recycling hotspots". A simple and strong argument why local coupling and moisture recycling must actually be different things is that, while moisture recycling can result a priori only in positive evaporation precipitation coupling, local coupling can be both positive or negative (see e.g. Hohenegger (2009) for a demonstration of the latter).

Answer: We agree that it is useful for the introduction and the discussion to explicitly separate between coupling effects induced by feedback of the soil system on the boundary layer and on precipitation triggering and purely quantitative coupling effects due to moisture recycling. To avoid any amalgam of the two mechanisms, we will modify the introduction (see previous answer) and also the discussion section to which the above statement refers to. The new text will read as: " (..) we anticipate that a detailed analysis for seasonally dominant moisture trajectories on different continents could give valuable indications on how different the effect of climate or land use changes can be in regions that play a crucial role for moisture recycling, especially in regions that are moisture suppliers (van der Ent et al., 2010)."

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I do not suggest that the authors should change the model to account for local coupling and circulation effects: such additional aspects, so far as it's possible to incorporate them at all, would increase the complexity and the uncertainties associated with the resulting model, probably making the model much more difficult to analyse, at least analytically. What I would really like to see, however, is that Schaefli et al. pay tribute to the fact that soil-atmosphere coupling is more than moisture recycling, and that the question whether moisture recycling is the "major player" at the considered scale is not settled yet.

Answer: We will explicitly mention that " *soil-atmosphere coupling is more than moisture recycling* " in the introduction (see one of the previous answers) and reformulate sentences mentioning the "soil-atmosphere feedback system" such as to make clear that we present a model of the moisture fluxes between the two compartments rather than of other coupling mechanisms. We will namely modify the start of the discussion section on p. 8338:

"We presented an analytical, fully coupled model of the moisture fluxes between the soil and the atmosphere. This feedback model, has the potential to give insights into nonlinear moisture recycling mechanisms along dominant moisture trajectories, which is an important aspect of soil-atmosphere moisture coupling. The model can distinguish between interception (fast feedback of moisture) and delayed feedback through the soil by way of transpiration and soil evaporation, two major advantages over existing analytical approaches (..)".

Furthermore, we will change the first sentence of the conclusion to: "We presented a feedback model of the moisture fluxes between the soil and the atmosphere and derived its analytical solutions (..)"

Additionally, we will incorporate at an appropriate location the following piece of text:

Our model is meant to study how the influence of (land-use) changes propagates downwind on the continental scale (on weekly to monthly time scales), i.e. where the spatially integrated evaporation along the streamline reaches the same order of magnitude as the advective flux. The value of such an analysis is to complement the various modeling studies that analyze the influence of land use changes on the circulation.

References

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