Response to detail comments of reviewer 1 (Helge Goessling)

This supplement gives detailed answers to all comments of reviewer 1. The original comments are quoted in italic. we only include the detailed comments which require an answer from our side. There is a list of suggestions (e.g. introduction of an additional reference to an equation) which we will follow without repeating them here. All changes will be included in the complete list of changes to be submitted with the revised version.

Detailed comments

P8316,L20-21: "Such an analysis has potential to anticipate the range of possible land use and climate changes [...]." To me this sounds as if the approach presented in this study could somehow be used to infer how land use as well as climate might change in the future, which you apparently do not intend.

Answer: We will reformulate to " Such an analysis has the potential to give first order estimates of the impact of land use and climate changes on the coupling of atmospheric and soil moisture"

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P8318,L3: "The results are [..] at least partially influenced by the model sensitivity". I argue that the results of these studies actually can be equated with "model sensitivity" (to changed soil conditions)– they are sensitivity studies. The context suggests to me that better terms for what you mean might be "inter-model spread", "inter-model variability", or "model uncertainty".

Answer: This sentence is part of the discussion of numerical moisture recycling studies. While we appreciate the comment of the reviewer we do not want to enter here the discussion in as far such models can be of use to analyze the behaviour of nature or whether corresponding results represent merely the model sensitivity. We will reformulate to: "Numerical studies commonly use e.g. month-long integration of regional or global coupled atmosphere-land surface models to analyze moisture feedbacks by varying soil and vegetation parameters and boundary conditions. Studying the sensitivity of such models can give valuable insights into these feedbacks, in particular in the context of multi-model studies (Koster et al., 2004)."

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P8319,L2: "We adopt a Eulerian-Lagrangean modelling scheme (e.g. Huang et al.,1994) to simulate moisture transport along atmospheric stream lines where physical Langrangean quantities (atmospheric moisture, particle paths, dispersion and advection) are computed with Eulerian fluxes (rainfall, evaporation)". I have difficulties with the classification of the presented approach as "Eulerian-Lagrangian". As far as I know the term is commonly used for certain numerical methods that deal with the discretisation of the advection-diffusion equation, which is also the case for the study you refer to (Huang et al., 1994). Eulerian-Lagrangian methods are advantegous where neither the advective term nor the diffusive term dominates, since otherwise a purely Lagrangian or a purely Eulerian method can be used (compare the "Peclet number" e.g. in Neuman 1981). However, your study largely gets along without discretisation. Furthermore, since you eliminate the negligible diffusive term from your equations (horizontal transport in the atmosphere is associated with a high Peclet number), it seems that the remaining problem is rather of purely "Lagrangian" nature. I also do not agree with the classification of quantities into Eulerian and Lagrangian ones. To me it seems that you classify the quantities rather into ones associated with "horizontal" and "vertical" processes.

Answer: We used the term "Eulerian-Lagrangean" in the very general sense meaning that the suggested approach combines quantities referring to a fixed reference system (soil moisture) and quantities referring to a moving, Lagrangean reference system. We are, however, aware that there is a well-defined set of numerical methods and especially finite element methods that are termed "Eulerian-Lagrangean". We will, therefore, omit this term from the revised version since it is not essential. We will, in exchange, introduce the Eulerian coordinates (x,y,z) explicitly in the equations 1 to 3 and the Lagrangean coordinates ($\boldsymbol{x}$,t) from equation 5 onwards to avoid any confusion (see also response to reviewer 3).

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P8319,L11-13: "[..] We assume uniform vertical properties of the atmosphere and model the exchange of moisture with a vertically uniform soil compartment [..]". Making use of the "well-mixed assumption" as you do is not identical with assuming that the atmosphere (and the soil compartment) is completely uniform in the vertical.

Answer: We will reformulate to: "At a given location x, we assume that the atmosphere and the soil compartment are each composed of a single, well-mixed layer, connected by the vertical exchange fluxes of precipitation and evaporation."

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P8319,L14-15: "Lateral transport through advection and turbulent diffusion is modelled only for atmospheric moisture [: : :]". I suggest to omit the consideration of turbulent diffusion more or less completely from the study, also from Eq. (1). A notice that this term can be neglected for horizontal moisture transport in the atmosphere (if, as in your case, only boundary-layer eddies and not larger-scale (synoptic) eddies or longer lasting temporal fluctuations are included in the dispersion term) should be sufficient. The sentence suggests that you actually account for turbulent diffusion as well, which you don't as you explain later. Leaving this issue out of the paper would make it more concise.

Answer: Thanks for this suggestion; we will omit it from equation 1.

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Eq.(1): You might add a comment on how u_x, which in this 1D-formulation apparently is an effective wind speed, relates to the vertically sheared 3D wind- and moisture-field (compare Goessling and Reick, 2011, Eq. (6)).

We will modify the text as follows:

" (..) the wind speed in the flow direction, which should be seen as an effective wind speed (i.e. moisture weighted), which is for example used by Goessling and Reick (2011, Eq. 6).

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Eq.(4): For my taste this equation is superfluous.

Answer: From the comments of reviewer 3 it appears that our assumptions behind the solution we present are not entirely clear, especially not how the stationarity assumption comes into play. We will, therefore, certainly keep this equation here and make clear that the stationarity is introduced by assuming that u_x = cst in time.

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*P8321,L4-6: "In the case of convergence, the narrowing of the control width results in an increased concentration of water in the control volume, which results in an apparent inflow of moisture". You describe convergence as a lateral convergence of neigbouring trajectories (as shown in Fig. 1) and translate this into an apparent inflow of moisture. If I am not mistaken you do not change x (such that x*b is constant), meaning that the wind speed does not increase as the trajectories converge. This would mean, however, that surface pressure p has to increase accordingly (dp/p=-db/b) because more mass per area resides above the surface, which you probably do not intend. The point seems to be that what you are describing is not a lateral convergence of the whole vertical column, but a lateral low-level convergence associated with a compensating high-level divergence, resulting in a lateral moisture convergence because the low levels carry most of the moisture. So, I think that your final expressions are OK (latest when you replace the explicit treatment of the column width b by the* *apparent moisture inflow I), but you should clarify the description of convergence. (I have not thought it through if changes in the text might be sufficient, or if changes of the equations are necessary to arrive at a solid description).*

Thanks for this comment. While certainly correct from a physical point of view, a detailed discussion seems to complicate things unnecessarily from a hydrological perspective. We will address this point by

a) adding after equation 3 "Note that the control volume ($\Delta x c_m b$) refers to the moisture carrying part of the tropospheric column only."
b) reformulating after equation 5 to: "The last term in the above equation encodes the net change of the moisture carrying streamline's shape (of its width) along x (..)"

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P8321,L20-22: "Precipitation on a daily timescale can be assumed to depend (linearly) on the atmospheric moisture above a certain threshold (e.g. Trenberth et al., 2003; Savenije, 1995b)". It is clear that strongly simplifying assumptions such as this one have to be employed when one wants to come to simple mathematical expressions that can be investigated analytically. Therefore I think it is acceptable that you use this simplification, but to me the phrase "can be assumed" is somewhat too strong.

Answer: We will reformulate this to tone down the statement: "The formation of precipitation in the atmosphere is known to be a non-trivial physical process; in order to make the model analytically tractable we adopt here the simplifying assumption that precipitation depends linearly on the atmospheric moisture above a certain threshold (e.g. Trenberth et al., 2003; Savenije, 1995b)".

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Eq.(8): The reader can guess that c_t and w_t are the threshold-values of C and W, but I recommend to state this explicitly in the text. Also, it would help to state somewhere the relation between C and M, which I assume to be $M = x * b * C$, meaning that $C = c_m * W$ (as can also be seen from the x-axis label in Fig. S1).

Answer: Thanks for pointing this out. We will introduce explicit explanations for all these quantities.

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P8322,L1-3: "On longer timescales, a squared relationship between P and W appears to capture their relationship reasonably well (see supplement, Fig. S1)". If I am not mistaken, you only use Eq. (9) for the precipitation term in your model, so why did you introduce Eq. (8) at all? But why, on the other hand, should the annual timescale be more appropriate for your model? As far as I understand the objective of the model is to track the development of air columns over the continental scale in a Lagrangian manner, so, wouldn't the daily timescale be more appropriate (since it takes air typically a few days to travel continental distances)? Do you have some kind of process-based explanation why on the annual timescale a squared relation between P and W should hold? Why shouldn't there be a threshold-behaviour anymore? When looking at Fig. S1 I also wonder why the relation is seemingly much closer in case of the Congo and midUS regions compared to the other two regions. Again referring to Fig. S1, I guess that the multiple points for each region represent the single grid cells contained in the regions, is that right? I am asking because it's not clear to me how the picture would change if one plotted different years for one single grid cell instead, and which of these two possibilities is more appropriate to support Eq. (9).

Answer: As stated above, the solution of our model is a snap-shot in time, however we parameterize the model with a parameterization that holds on the time scale of weeks to months and also interpret the results on this time scale. We consider it more appropriate to assess the influence of land-use or climate changes on such time-scales rather than on the daily time scale. Therefore using (where local coupling might be dominant) the threshold behavior would not necessarily make sense.

This different relation when upscaling to a larger time scale is analogous to upscaling the daily threshold function for interception to a monthly or annual function (De Groen and Savenije, 2006; Gerrits et al., 2009).

In the figure hereafter, one can see that also on a two-weekly scale $P \sim W^2$ works reasonably well in some areas whereas in others the relationship seems more threshold like. However, since we are pursuing an analytical solution we have decided to stick to the $P \sim W^2$ relation.

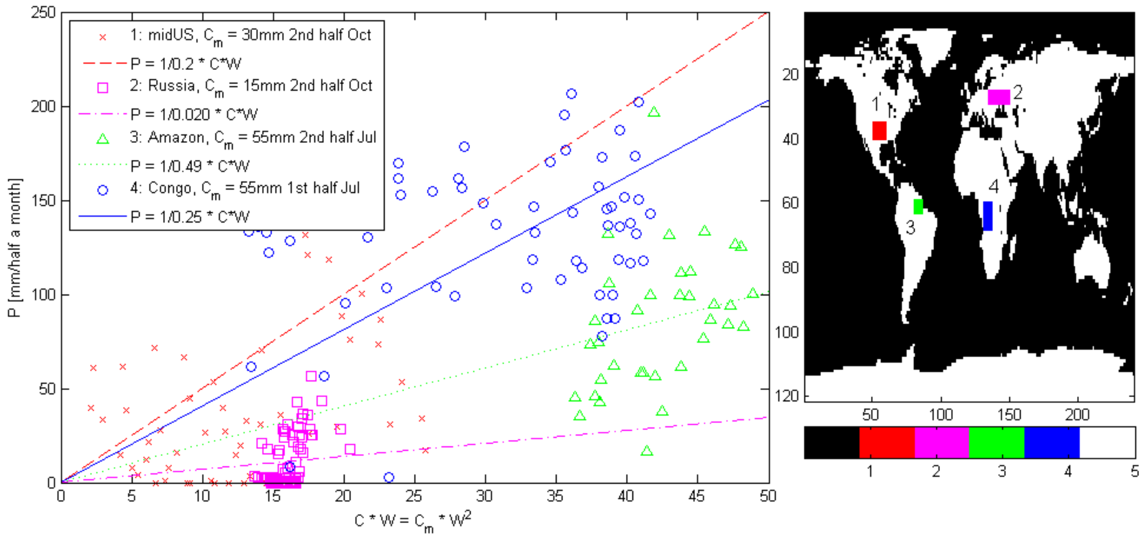

Fig. A1: Plot of half-monthly precipitation *P* versus squared atmospheric moisture *W* from re-analysis data (grid cell size 1.5° x 1.5°) to support the assumption of $P \sim W^2$ (as figure S1 of the HESSD manuscript but different time step).

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Eqs.(14)&(15): Please indicate which equations exactly are used to arrive at these expressions. This is particularly important to make clear that you are using Eq. (9) instead of Eq. (8), although I did not find this choice explained (see comment on P8322,L1-3).

Answer: We will reformulate to: "After substitution of Eqs 9, 10 and 13 into Eq. 5 and into Eq. 6, the coupled water balance model becomes "

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P8324,L1: "time steps". I suggest to replace this term by one that is not commonly associated with numerical discretisation, e.g. "time scales" or "time spans". Eq.(16): If I am not mistaken you are using the constraint "δ=const" leading to Eq. (16) not only within Sect. 2.2 to allow for an analytical solution, but for all following considerations as well. I therefore suggest to attach this whole paragraph (P8323,L19-P8324,L7) to the end of the preceding Sect. 2.1.

Answer changed to "time spans"

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P8325,L18-19: "physically realistic". I would prefer e.g. "physically meaningful", because the actual realism of the model is a different issue.

Answer changed to "physically meaningful"

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P8325,L20-21: "[: : :] if there is real equilibrium moisture W1 [: : :]". I suggest to write something like "if W1 is a real number" to avoid ambiguity.

Answer: will be modified as suggested.

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P8328: The authors write for "case 4" that "The assumptions behind the above solution will break down at large x, because the atmospheric moisture content W cannot exceed unity" (P8328,L17-18). I wonder if the logic of this sentence shouldn't be the other way round: Problematic assumptions (like ξ=const) cause W to exceed unity. (comment moved to here from the general comments).

Answer: We acknowledge that the assumption of ξ=constant can result in unphysical solutions. We would like, however, to emphasize that, in any case, the equation becomes unphysical for large *x* and positive lateral inflow, independent of assumptions about the soil moisture variation ξ. This simply follows from the fact that very fast evaporation and very slow discharge lead to "too much water in the atmosphere", which can only be evacuated through lateral flow. We will change the text to reflect the above.

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P8331,L7-9: "For runoff and storage, the effect depends on the location along x; it decreases close to the coast and increases inland, as visible in the linearly related runoff profile (Fig. 5c)". It took me a while to understand what you mean here. It would help if you (I) incorporate into this sentence again that changes due to faster evaporation are meant, and (II) mention that with the "inland increase" you mean that very small difference that is hardly visible in the figure.

Answer: The sentences were indeed ill-formulated. They should read as: "For soil moisture storage *S*, the effect depends on the location along *x: S* decreases close to the coast and increases inland; this is illustrated in Fig. 5c, which shows the runoff profile along x (which depends linearly on *S*)".

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P8331,L15+17+19: "W(x'|Theta)". You have already defined that for x<x' the parameter set Theta is applied and that from x' onwards Theta' is applied. So, I don't see why you should include the conditional statement ("given Theta") in this term. In contrast, in Eq.(33) the conditional statement makes sense in "dW(x|Theta)", though again not in the bracketed factor.

Answer: $W(x)$ or $W(x')$ does not convey direct information about the used parameter set, only about the location; since we are discussing here the effect of a switch of parameter values from x' onwards, we need to specify a condition on W at the switching location x' under the old parameter set, i.e. $W(x'|\theta)$.

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P8331,L15-17: In the first sentence of this paragraph you mention that the length scale L also varies with changing Theta, but in L15-17, it seems that you ignore potential changes in L that would, as far as I understand, also impact dW/dx. Isn't this relevant for the validity of the cases i and ii?

Answer: A change in L will only scale the moisture profile in the x-axis direction but not modify its shape. We will modify the text to: "A given hydroclimatologic parameter set corresponds to a particular moisture profile (in the atmosphere and in the soil) that is characterized by the length scale *L* and the equilibrium moisture W_1. If the parameters change to a new value at a given point of the flow path, this can first of all modify *L*, which only modifies the characteristic length scale of the profile but not its shape. The effect of a parameter modification on W_1 can create three different situations in an increasing regime:"

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P8332,L1-2: "A special case is the situation where I = 1: it holds that W1(I*=1) = 1 and no regime switch is possible". For this to be true, I* would have to be excluded from the list of changeable* *parameters. Also, similar "special cases" would hold for cases where W1 = 0 (as e.g. in case 1a). For my taste you could just drop this sentence.*

Answer: We will follow the suggestion and drop this sentence.

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P8332,L4-5: "Mountain ridges can decrease the precipitation time scale, modify lateral convergence or induce very different evaporation time scales". I would argue that the main effect of a topographic obstacle is to decrease cm, the atmospheric water holding capacity. The effect on precipitation (in your model) would be the same as decreasing τ_p, as one can see from Eq. (9) when W is replaced by C/cm. However, you probably did not intend to have cm changeable along x, since then W wouldn't behave continuous along x (while C would).

Answer: We agree that an important effect of mountain ridges could be to modify c_m (see modified text, next answer). For the discussion here, it is not necessary that W remains continuous since any parameter change will directly affect it (see fig. 6).

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P8332,L10: "a decrease of the evaporation time scale". Don't you mean an increase (i.e. less evaporation), as you say later?

Answer: Thanks for pointing out the mistake here, for which we apologize; the text, in fact, referred to an earlier version of the figure (it showed τ_e in a single location (left) and the effect of changing τ_p at two different locations (right). The corrected figure is shown hereafter. The new text of the entire paragraph will read as:

"Mountain ridges can decrease the water holding capacity of the atmosphere or the precipitation time scale, modify lateral convergence or induce very different evaporation time scales, potentially creating regime switches (fig. 6, left). Particularly interesting are potential regime switches due to land use changes. A common question is to anticipate the impact of a modification of the evaporation process on runoff. Considering the feedback system rather than the isolated hydrologic system suggests that the expected response depends on the moisture regime and on the lateral convergence. For example, a decrease of interception could cause a regime switch further downstream if an increasing moisture regime is dominating close to the coast (Fig. 6, left). An increase of the evaporation time scale τ_e could either slow down the increasing regime or lead to a regime switch, depending on the values of all other parameter values and on the location of the land use change (see Fig. 6 right where a modification of τ_e in two different locations is illustrated)."

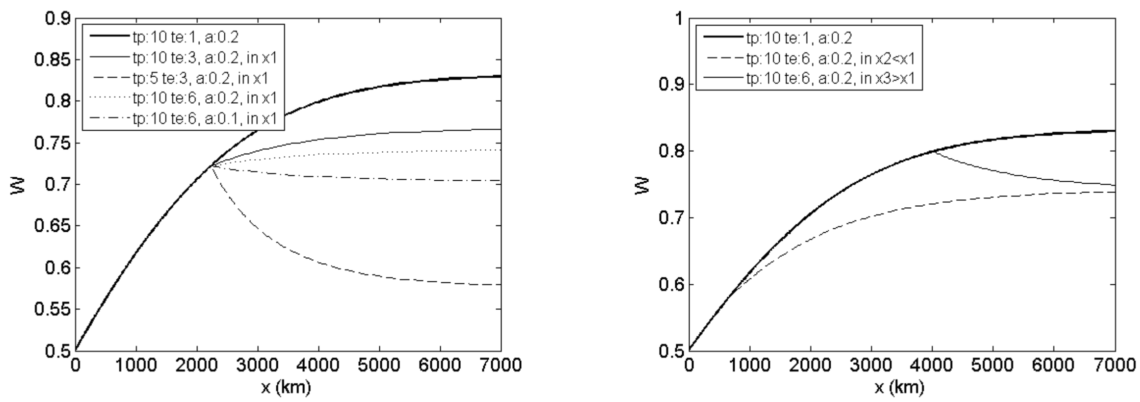

Fig. A2 (manuscript Fig. 6): Moisture profile modifications and regime switches due to parameter changes (parameter units and default parameter values as in Table 2 of the manuscript), left: for changes of τ_p, τ_e, α in x_1, right for a change of τ_e in two different locations $x_2<x_1$ and $x_3>x_1$; note the role of α in the switch induced with the parameter set, τ_p = 10 days, τ_e =6 months, l =1.2mm−1 (left figure).

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P8333,L8-9: "for higher α, the equilibrium moisture is reached further inland and the same relative moisture is reached at a shorter distance inland". The first half of the sentence makes sense to me, but the second part seems to have the wrong sign: I would expect that the same relative moisture is reached at a "longer" distance inland. The subsequent sentence would have to be changed accordingly.

Answer: Thanks for pointing out this not well formulated statement. We will correct the text to: "Interception also determines the length scale of the feedback system *L,* which increases for increasing α, implying that for higher α, the equilibrium moisture is reached further inland. Given the joint effect of α on *L* and the equilibrium moisture, it can also be shown (Eq. 16) that for a higher α, the same relative moisture is reached at a shorter distance inland in an increasing regime, at a longer distance inland for a decreasing regime. This results in both cases in an increasing moisture at a given location x for a higher α."

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Eq.(38): Considering a complete year (such that S(t0+1yr)=S(t0)) does of course not always mean that ξ=0 during the whole year, but e.g. ξ>0 during a wet season balanced by ξ<0 during a dry season. The two seasons would also correspond to different W. I wonder in how far the applicability of Eq. (38) is affected by this.

Answer: The point we are making here is that the Horton index is usually estimated with meteorological quantities referring to yearly time steps where we can assume that $\xi(t_{yr})=0$ even if ξ within the year is not 0. If the Horton index computed e.g. at monthly time steps with different $\xi(t_m)$ for which it holds that they sum up to 0, then the resulting Horton index averaged over the year would be slightly different from the result we would obtain with ξ=0 directly at a yearly time step. For the range of plausible parameter values, this difference is of the order of magnitude of 1% (can be shown numerically). We will add a note on this.

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P8334,L4-5: "This relationship only depends on the parameters of the hydrologic system [...] and the climatic parameter e_m and is independent of the functional relationship between P and W". But isn't the latter affecting W and, ultimately, W1, and hence also affecting the Horton index via W?

Answer: Of course, the value of *W* is influenced by the assumptions about P~W but the analytic relationship between the Horton index and *W* is independent of the chosen relationship between P and *W* because *P* cancels out from the equation. We will change to text to: "This relationship only depends on the parameters of the hydrologic system [...] and the climatic parameter e_m , i.e. the analytic relationship between *H* and *W* does not depend on the chosen relationship between *P* and *W* (because P cancels out from the equation)."

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*Eq.(44)-(46): I wonder if one cannot go without these discretisation issues, because I consider them rather distracting. As far as I am concerned one could just introduce the "instantaneous" recycling length scale lambda(x)=u_x*C(x)/E(x), which is the distance air travels until the integrated evaporation flux measures up to the atmospheric moisture content given C and E remain constant. Eq. (47) suggests to me that, after all, you indeed have this definition in mind.*

Answer: As can be observed, *C* and *E* do not remain constant along the streamline, so we still see value in computing the recycling ratio. Because the "local" λ changes along the streamline, the behavior of the continental recycling ratio along the streamline is more complicated (Fig. 8) than what might be expected from Eq. (43). We will add a comment on this behaviour in the text.

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Eq.(44)-(46): I also consider the notation style suboptimal, e.g. in Eq. (46), where on the left-hand-side the argument of lambda is "δx" (but lambda is not really a function of the discretisation step, or is it?), while on the right-hand-side the arguments of <C>, <E>, and <W> are two points in x (which are δx apart from each other).

Answer: λ is indeed a function of average quantities over $\delta x = x_i - x_{i-i}$, i.e. it is a function of the discretisation step. We will try to simplify / better explain this part but we will stick to the definition of the recycling length scale λ. We will furthermore specify that in Fig. 8, $\lambda(\delta x)$ is shown (computed according to Eq. 46).

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Eq.(44)-(46): Regarding the computation of rho, I also think that one can go without the discretisation exercises, and even the whole detour via lambda: One can just use the left part of Eq. (43) (and discretise and iterate this one) to obtain rho(x).

Answer: The discretisation is necessary because the change in E and C along the streamline is not a straightforward computation. The 'detour' via λ is a matter of taste.

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Eq.(47): "λ (δx)". Again, I think that lambda should be a function of "x" rather than "δx", as the right-hand-side of the equation confirms.

Answer: As we tried to make clear earlier in this section, the recycling coefficient is defined for a portion of the trajectory ***x*** and not for a point; we will therefore keep the notation λ (δx). Eq. 47 simply states that it can be approximated with the state variables in x_i, assuming that the average moisture over $\delta x = x_i - x_{i-i}$ equals approximately $W(_{xi})$.

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Eq. 47: Also, after the middle equal sign, you just drop the arguments from the variables. Finally, it would help if you could refer to the equations you use to get the middle equality, which I think are Eq. (9) and (41).

Answer: We will specify that we dropped the arguments for simplicity. The middle equality is obtained by replacing E_I with eq. 9 combined to eq. 10 and E_T with eq. 13. The next step is to isolate S/cm and to replace it with eq. 16. We will specify this in the revised version.

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P8337,L16: "As expected, [...] the precipitation time scale directly influences the recycling length scale". Are you sure that this is correct? I think that you have τ_p in Eq. (47) only because you bring B_u (=E/P) into it, and the effect τ_p has on B_u nullifies the illusive effect of τ_p on λ. And why would you expect a direct influence at all? In agreement with the left part of Eq. (47), I would expect no direct effect, but only an indirect (slow) one via the effect τ_p has on W (we have a short comment on this very issue in Goessling and Reick (2011), Sect. 3.2, penultimate paragraph).

Answer: We have τ_p in Eq. 47 because we express transpiration and interception in terms of *W* and S; this results in an expression including (S/cm)^(-1), which directly depends on τ_p (see Eq. 16). This means that τ_p comes into play here because of the coupling between the two moisture compartments. We will reformulate and omit the formulation "directly influences the recycling length scale".

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P8338,L16: "moisture recycling hotspots (Koster et al., 2004; Van der Ent et al., 2010)". Besides what I already criticized in the general comment 2, I have one minor point: Why do you refer to van der Ent et al. (2010) instead of van der Ent et al. (2011)? To me it seems that the recycling length- and time-

scales (which more or less are "instantaneous" measures) might be more suitable to detect "moisture recycling hotspots" than the continental precipitation/evaporation recycling ratios (which are integrative measures).

Answer: We thank the reviewer for pointing this out, and he is absolutely right, but with the new text (see above) the reference to Van der Ent et al., (2010) makes more sense than the reference to Van der Ent and Savenije (2011).

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P8338,L19-21: "Such a regime switch at a given location would cause a major modification of the hydrologic cycle further downstream, possibly resulting from some minor local change of process time scales e.g. due to vegetation change". To me, "local" means that the parameter change (e.g. vegetation change) takes place between x1 and x2>x1 with x2-x1 being small compared to the spatial domain. If this local change produces a regime switch at x1, I would think that at x2 a "back-switch" must occur (with W1(x>x2)=W1(x<x1)). This would mean that regions further downstream only "feel" much if the anomaly introduced between x1 and x2 was very strong (which cannot be if x2-x1 is really small, i.e. the change really "local"). I think that a persistent regime switch can only occur if the parameter change is introduced permanently from x1 onwards.

Answer: We are indeed talking about a parameter switch which is persistent from x1 onward and will reformulate the text.

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P8338,L26: "how close the actual processes are to a potential regime switch". Similar to what I am criticizing in the last comment, to me this sentence suggests that what you are calling a "regime switch" is something like a bifurcation ("tipping point"), the latter meaning that a small parameter change results in a very different system behaviour (e.g. a steady state loosing its stability, and the system approaching a qualitatively different steady state instead). However, even if a parameter change is not local but persistent along x (see last comment), W1 in your model tends to change gradually in response to a parameter change.

Answer: We will stick to the wording "regime change" since, as discussed in the text, a change of W1 can result in a switch from a decreasing to an increasing moisture regime which is to our view a rather different system behaviour.

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P8339,L2-3: "how long it takes for a step change in moisture at the coast to propagate to some distance inland". Do you mean a change in soil moisture (could also be atmospheric moisture, but that would probably just take x/ux)? Supposing the former, I think that this kind of experiment would tell something about soil moisture-atmosphere feedback more directly than the present approach involving the assumption ξ=const. By dropping this assumption one could perturb soil moisture directly (see general comment 1).

Answer: This comment was intended to give an outlook on investigating the temporal, rather than only the spatial behavior of the coupled system. This experiment will probably require a numerical analysis, that's why we suggested that this is beyond the scope of this paper. Concerning the assumption of constant ξ, *please refer to our response on this topic.*

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Sect.(4): I think that the manuscript could benefit from a more extensive discussion of potential shortcomings and limitations of the model. Comments on questions like "How robust are the conclusions against different process representations (e.g. "P propto W-W_t" versus "P propto W^2")?", "How realistic are the applied assumptions (e.g. "ξ=const" along the whole trajectory)?", and "How important is the omission of other processes?" could contribute to improve the manuscript.

Answer: We agree that a few comments on the main assumptions would certainly be welcome here. In view of the comments of all reviewers, we will especially comment on the "ξ=*const*" assumption; regarding missing processes, we will shortly mention the missing hydrological flux "direct runoff" and the implications of assuming that the amount of water entering the soil system does not depend on the soil moisture state.

Furthermore, after incorporation of all changes that follow from the discussion with all the reviewers, this Section 4 will automatically become more extensive.

Technical corrections

We give here after some comments to the suggested technical corrections; all other corrections will be integrated into the revised version as suggested by this reviewer.

- *P8316,L6 (and elsewhere): "Langrangean". At several places in the text you have one "n" too much. Furthermore, the more widely used spelling is "Lagrangian" (despite Lagrange's final "e" in the common French version of his name).*

  We will correct Langrangean but keep the spelling "Lagrangean".

- *Eq.(1): I perceive it as suboptimal to mix continuous and discrete variables in one dimension (dx and Δx) in one equation. Shouldn't it be possible to replace Δx by dx?*

  We will replace Δx by a different parameter name.

- *P8319,L21-22: "Consider the control volume V, a tropospheric column of area Δxb [L^2] and of mass M=VW, where W [–] is the relative atmospheric moisture filling". Volume (V, L^3) times a dimensionless number (W) gives again volume, not mass. Since, however, the units of the terms in Eq. (1) are seemingly volume per time, I speculate that what you call V is total air mass, not volume, but expressed in "length of liquid water equivalent", such that the units become the ones of a volume. Furthermore, to me it seems that M should be termed e.g. "water-mass" rather than just "mass" (though, again, with volume-like units).*

  We made indeed the mistake to call "mass" what in reality is a quantity expressed in "volume of liquid water equivalent". We will correct by adding the density of water and revise the wording.

- *P8320,L16-17: "Expressing the control volume height in terms of the water holding capacity c_m [L] of the tropospheric column". Now the water holding capacity is incorporated such that V cannot be "total air mass", but seems to be "water-mass capacity", again expressed in volume-units. This seems to be inconsistent (see also previous comment).*

  We will make sure that all units / wording are consistent.

- *P8326,L19-20: "ξ<< 0, i.e. D* << 0". These relations do not make much sense because the involved order of magnitude cannot be deduced. But isn't what you are intending to say already contained in the earlier expression "D* < -W^2/(κ(1-W))"?*

  We will reformulate, simply saying that dW/dx > 0 for *D* < -W^2/(κ(1-W)) $\leq$ 0.*

References

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