

## ***Interactive comment on “An analytical model for soil-atmosphere feedback” by B. Schaefli et al.***

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General comments:

In their manuscript, Schaefli et al. present a hydrological soil-atmosphere model that describes the evolution of the atmospheric moisture budget along trajectories of air passing over continents. External fluxes (runoff and lateral moisture convergence) and internal fluxes (precipitation and evaporation) are parameterised in a simple way that allows to analyse the system behaviour in an analytic manner.

I very much appreciate approaches like this one that are aimed at understanding the behaviour of a very complex system by reducing the complexity to a set of "most important" aspects. Further, the model suggested by Schaefli et al. contains very interesting aspects that are usually neglected in approaches as simple as this one, e.g. the rep-

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resentation of lateral moisture convergence. Schaepli et al. present an elaborate analytical evaluation of their model and provide links to a number of important hydrological quantities.

However, despite these promising ingredients, the authors have not yet convinced me that their model does a good job at representing the critical aspects of the system under investigation. Beside a number of medium and minor comments, I have two main concerns. The latter relate to 1) the far-reaching consequences of the assumption " $x_i = \text{const}$ ", and 2) the insufficient acknowledgement of the fact that soil-atmosphere coupling is considerably more than moisture recycling.

1) " $x_i = \text{const}$ ".

Eqs. (14) and (15) represent a system of two coupled differential equations for the relative atmospheric moisture ( $W$ ) and the relative soil moisture ( $S$ ). By approximating the time rate of change of  $S$  (i.e.  $x_i$ ) with a constant, the authors reduce the system to one differential equation of first order, Eq. (17) (which is not of second order, as stated in P8324,L8). Hence,  $S$  is not a "prognostic" variable anymore, but a "diagnostic" one.

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

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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  $x_i = \text{const}$  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,  $S(W_0)$  is negative if  $W_0$  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  $x_i > 0$ ),  $S(W_1)$  and the corresponding  $R$  are negative (as follows from  $W_1 = \sqrt{I^*} = 0$  put into Eq. (16)  $\rightarrow S(W_1=0) = -\tau_{\text{atm}} * x_i$ ). 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  $x_i = \text{const}$ ) cause  $W$  to exceed unity. All in all, the constraint  $x_i = \text{const}$  seems to turn the causality upside down: Physically,  $x_i$  should be the result of infiltration, evaporation, and discharge (as in Eq. (15)) rather than the other way round.

More or less the whole analysis presented by Schaefli et al. involves the assumption  $x_i = \text{const}$ . Therefore, 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  $x_i = \text{const}$ , I would like to read convincing arguments against my objections.

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

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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".

It seems that Schaeffli 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. 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).

Partly it seems to me as if Schaeffli 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).

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 Schaeffli 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.

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Specific 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.

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".

P8319,L2: "We adopt a Eulerian-Lagrangian modelling scheme (e.g. Huang et al., 1994) to simulate moisture transport along atmospheric stream lines where physical Lagrangian 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 advantageous where neither the advective term nor the diffusive term dominates, since otherwise a purely

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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.

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.

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.

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)).

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

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

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inflow of moisture". You describe convergence as a lateral convergence of neighbouring trajectories (as shown in Fig. 1) and translate this into an apparent inflow of moisture. If I am not mistaken you do not change  $\Delta x$  (such that  $\Delta x \cdot b$  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).

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.

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  $M = \Delta x \cdot b \cdot C$ , meaning that  $C = c_m \cdot W$  (as can also be seen from the x-axis label in Fig. S1).

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

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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).

P8322,Eq.(9): I suggest to also include  $P=P(C)$ , i.e.  $P=C^2/(\tau_{\text{ap}} \cdot \text{cm})=W^2 \cdot \text{cm}/\tau_{\text{ap}}$  (see comment on P8332,L4-5).

P8322,L5: "residence time for precipitation". This sounds strange since precipitation is not a reservoir but a flux. Can't you call it again "time scale of the precipitation process", as before?

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).

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 " $x_i=\text{const}$ " 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.



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

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.

P8330,L13: "The relationship between the two state variables depends on all hydroclimatic parameters". Would it make sense to refer to Eq. (16) after this sentence?

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.

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.

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?

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.

P8332,L4-5: "Mountain ridges can decrease the precipitation time scale, modify lateral

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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  $\tau_p$ , 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).

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

P8333,L8-9: "for higher  $\alpha$ , 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.

Eq.(36): " $H_I$ ". I suggest to omit the index "I", or replace it by some other symbol, because the index "I" suggests correspondence to the index of " $E_I$ " (which is not intended, I suppose).

Eq.(38): Considering a complete year (such that  $S(t_0+1yr)=S(t_0)$ ) does of course not always mean that  $x_i=0$  during the whole year, but e.g.  $x_i>0$  during a wet season balanced by  $x_i<0$  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.

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,  $W_1$ , and hence also affecting the Horton index via  $W$ ?

Eq.(44)-(46): I wonder if one can not go without these discretisation issues, because I consider them rather distracting. As far as I am concerned one could just introduce the

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"instantaneous" recycling length scale  $\lambda(x) = ux \cdot C(x) / E(x)$ , 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. I also consider the notation style suboptimal, e.g. in Eq. (46), where on the left-hand-side the argument of  $\lambda$  is " $\Delta x$ " (but  $\lambda$  is not really a function of the discretisation step, or is it?), while on the right-hand-side the arguments of  $\langle C \rangle$ ,  $\langle E \rangle$ , and  $\langle W \rangle$  are two points in  $x$  (which are  $\Delta x$  apart from each other). 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  $\rho(x)$ .

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

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  $\tau_p$  in Eq. (47) only because you bring  $B_u (=E/P)$  into it, and the effect  $\tau_p$  has on  $B_u$  nullifies the illusive effect of  $\tau_p$  on  $\lambda$ . 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  $\tau_p$  has on  $W$  (we have a short comment on this very issue in Goessling and Reick (2011), Sect. 3.2, penultimate paragraph).

P8338,L16: "moisture recycling hotspots (Koster et al., 2004; Van der Ent et al., 2010)". Besides what I already criticised 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 mea-

suress).

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  $x_1$  and  $x_2 > x_1$  with  $x_2 - x_1$  being small compared to the spatial domain. If this local change produces a regime switch at  $x_1$ , I would think that at  $x_2$  a "back-switch" must occur (with  $W_1(x > x_2) = W_1(x < x_1)$ ). This would mean that regions further downstream only "feel" much if the anomaly introduced between  $x_1$  and  $x_2$  was very strong (which cannot be if  $x_2 - x_1$  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  $x_1$  onwards.

P8338,L26: "how close the actual processes are to a potential regime switch". Similar to what I am criticising 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 losing 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),  $W_1$  in your model tends to change gradually in response to a parameter change.

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  $x/u_x$ )? 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  $x_i = \text{const}$ . By dropping this assumption one could perturb soil moisture directly (see general comment 1).

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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. "xi=const" along the whole trajectory)?", and "How important is the omission of other processes?" could contribute to improve the manuscript.

Technical comments:

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).

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

P8319,L21: " $\Delta x b$ ". You suggest to introduce a multiplication sign in order to enhance the readability of this expression. The "x" can otherwise be mistaken to be a multiplication sign itself (making the expression " $\Delta$  times b").

P8319,L21-22: "Consider the control volume V, a tropospheric column of area  $\Delta x b$  [ $L^2$ ] 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).

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 incorpo-

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rated such that  $V$  can not 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).

P8325,Eq.(23): Period missing after the equation.

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

P8329,L22: " $I=5/\text{month}$  with  $cm=20\text{mm}$  corresponds to a lateral influx of  $100\text{mm}$ ". Shouldn't the latter be  $100\text{mm}/\text{month}$ ?

P8331,L16: Here, you have one excess closing paranthesis.

P8331,L20: " $\Theta_i$ ". Here you are referring to a single parameter, but in Eq. (33) again just to the set of parameters  $\Theta$ . Although not exactly wrong because the set of parameters includes the single parameters, this is somewhat inconsistent.

Eq.(33): Period missing after the equation.

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