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Interactive comment on "Rainfall-runoff modelling in a catchment with a complex groundwater flow system: application of the Representative Elementary Watershed (REW) approach" *by* G. P. Zhang and H. H. G. Savenije

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The thorough comments and questions raised by Erwin Zehe (Referee) are very much appreciated. We are grateful to the referee, who has spent a large amount of time and effort on the review, which will result in, we believe, a great improvement of the manuscript. We are going to revise our manuscript accordingly. All modifications will be presented in the final version of our manuscript.

In the following text we respond to the referee's comments, in the order of his comments/questions raised.

Abstract

– line 10: Statement "Through flux exchanges among the different spatial domains of the REW, surface and subsurface interactions are fully coupled" doesn't make sense. Does it mean that surface and subsurface processes are described in a fully coupled manner?

We do mean that the coupling of surface and subsurface flow processes in the mathematical model is realised by computing the flux exchanges between surface and subsurface domains of each REW through an implicit computation scheme. We'll rephrase the statement.

Introduction

– Comment on physically based models: The authors should stress that physically based process descriptions include (at least in principle) measurable state variables and parameters. I see the problem of data needs but not the problem of equifinality in this context (which means all model structures are equally likely). This should be discussed in the context of conceptual models.

We will reformulate this part considering this comment. However, the authors recognise that in practice nowadays with current technology, equifinality problems exist even when physically-based models are applied and when calibration is necessary. Physically-based models often contains more parameters than conceptual ones. Hence, equifinality problems can be more serious with physically-based models than with conceptual ones.

– Page 642 line 3: TOPMODEL is not a conceptual model!

We agree that TOPMODEL is regarded as a physically-based approach according to some of hydrologists and modellers. However, others (e.g. Güntner et al., 1999; Seibert et al., 2003; Kavetski et al., 2003) consider it a conceptual one, probably due to the fact that the TOPMODEL has some similarities, as Beven et al. (1988) stated, to the

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Interactive Comment

“stochastic-conceptual” model of Freeze (1980). This is somewhat subject to different point of views or perceptions on the classification of modelling approaches.

– *Page 642 last paragraph: The authors talk about sub domains and closure before they introduce the REW concept. Please define the closure problem before you use these words, because a lot of readers don’t know these terms.*

– *In the context of closure the authors should refer to the work of Lee et al. (2005)*

We shall add a brief introduction on the closure problem in the text of this section and shall make an appropriate reference to Lee et al. (2005).

– *It is of course possible to use conceptual models for estimating impact of landuse changes on water balance, if the model parameters are properly regionalized (e.g. Hundecha and Bárdossy, 2004, Journal of Hydrology)*

In our paper, we mentioned “To address the question of how land use change… affect hydrological… functioning, the model needs to contain an adequate description of the dominant physical processes.” We also mentioned “Although the physically based distributed models are supposed to offer a great potential and utility in predicting the effects of land-use change…” But we did not exclude the possibility of conceptual models for estimating the impact of land-use changes on the water balance. Actually, we wrote “To improve the potential for making use of spatially distributed data, some lumped conceptual models have been extended to be distributed or semi-distributed. Examples are the HBV-96…” Hundecha and Bardossy (2004) stated that their intention of using the semi-distributed HBV model is “to use a simple model whose data requirement is not very high, both in terms of amount and resolution, and yet that is based on a reasonable representation of the major catchment processes.” This is well in accordance with our opinion on the possible use of models for evaluation of land-use changes.

Section 2: Mathematical representation

– Page 644 line 2: Statement is misleading! An REW does only implicitly account for lateral spatial dimensions!

This statement will be modified.

– Page 644 line 10: “averaged” might be more suitable than “upscaled”

We agree to use “averaged”.

– Please define the closure problem a little clearer, why is it difficult to assess the exchange terms in Eq. 1?

Brief discussions on closure problem with respect to Eq. 1 will be implemented.

– Page 644 line 8: Not the domains, the processes in different domains are characterised by different time scales

The statement will be changed according to this comment.

– How are Darcy’s law, Manning’s law and Saint Venant Eq. related to exchange terms or balance equations in the REW? Please explain this to the reader.

We shall provide a table that explains the relations between the flux terms and the according closure functions.

– Page 645: Eq. 1–5: Please specify the dimension of the storage S and the fluxes!

This will be done.

– Page 647 line 23: It is the average width of the channel.

Agreed.

– Page 648 Eq. 8: To avoid ad hoc solutions, please explain why you choose this approach for interception and the meaning of the threshold. In the spirit of the REW I would expect an approach with parameters that may be somehow linked to measurable

quantities like LAI

The threshold represents the actual amount of precipitation that is held by all interception media (e.g. tree leaves, grass surface and litter etc.) and eventually evaporated within the same day. This threshold is expected to be measurable and there are a couple of research groups are attempting to work on the measurements.

We could have derived equations at shorter time steps describing interception process starting from the water balance and energy balance principles. We foresee that with this approach, another domain would be necessary to accommodate the process description, thus more parameters and more complexity would be introduced into the model. Indeed, this is an interesting and important topic, and is expected to be a future research topic within the REW approach research context. However, at present stage, our intention is to test whether the REW approach in the current form is applicable to evaluating catchment rainfall-runoff relation. Therefore, before making a large leap to extend the model, we would like to keep the model as parsimonious as possible, while the importance of interception can be addressed.

– *Page 648 Eq. 9: This Eq. suggests infiltration is driven by the average pressure gradient across the unsaturated zone, which is certainly not true!*

The flux term expressing the infiltration, as stated by Reggiani et al. (2000), is linearised in terms of the difference of hydraulic potentials between the surface (infiltration-excess overland flow zone) and the unsaturated zone. This is a typical Darcy-type flow approximation, which resembles the Green-Ampt approach for vertical infiltration (e.g. Tindall et al., 1999). We agree that this form, which assumes that the soil has homogeneous properties, a piston-like moisture profile with a distinct sharp wetting front exists etc., is not a perfect one. However, it does provide a simpler but physically sound approximation for infiltration (compared with Philip's method and Horton equation), which is consistent with the descriptions of the unsaturated flow in the REW approach.

– *Page 649 Eq. 12: Evaporation is reduced from potential to actual rate if the average* *soil moisture in the u zone drops below 50% of porosity of the soil. Explain why you use this form and a linear reduction?*

When the soil moisture content is below 50% of the soil porosity, i.e. the saturation degree is smaller than 0.5, it is assumed that the evaporation or transpiration from the unsaturated zone is water-supply (i.e. water availability) limited, leading to a reduced evaporation rate. Due to the unknown catchment-scale reduction function under the water stress condition, and to the simplicity, as a first trial, the linear reduction function is assumed. This equation agrees with the procedure generally used in agricultural engineering (e.g. Rijtema and Aboukhaled, 1975; Doorenbos and Kassam, 1979).

– Page 652 Eq. 22: What is m?

This is a typing error (Eq. 21). It should be m_r, which is the average cross-sectional area of the channel segment under study. In Eq. 20, m_i should be m_{ri}, which is the average cross-sectional area of the ith inflow channel segment.

– Page 652 Eq. 23: What is t in the exponent? The term insight the brackets maybe derived with elementary geometry, so far so good. Please explain why you introduce the exponent, especially t and g?

In Eq. 23, the exponent *tg*γ_o shall be changed to tanγ_o. Apology for the confusion. The introduction of tanγ_o is motivated by the fact that hillslope topography, besides the groundwater table fluctuation, would also play an important role in the variation of the saturated surface area. Further more, we consider that the change of the saturated surface area is nonlinearly related to the groundwater table change due to the variable topographic settings. In our case, the topography is represented by the average hill-slope angle γ_o. Zhang et al. (2005) have analysed the effect of γ_o on the variation of saturated surface areas.

– Page 654 Eq. 32: According to definition (10), u is the volumetric soil moisture i.e. water volume stored in the pore space divided by the volume of the u-zone.

If the groundwater table is rising or falling, the volume of the u-Zone and the volume of water stored in the pore of the u-zone decrease with the same rate dys, thus the volumetric soil moisture stays the same (although the total water volume in the u-zone is changing). Hence the last term in Eq. 23 seems to be wrong!

We agree that the volume of the u-zone and the volume of water stored in the pore of the u-zone change with the same rate d*ys*, under the condition that the unsaturated zone surface area (omega_u) or the saturation overland flow area omega_o does not vary (this can only happen when the slope is completely flat, i.e. y_u+y_s=Z (see also Eq. 26). However, this is not the case. Below the complete derivation of Eq. 32 is given.

Mass balance equation of the unsaturated domain reads,

$$\frac{\mathrm{d}}{\mathrm{d}t}\left(\rho y_u \theta_u \omega_u A\right) = \underbrace{\min\left[\left(i - i_{dc}\right), \frac{K_{su}}{\Lambda_u}\left(\frac{1}{2}y_u + h_c\right)\right]}_{\text{infiltration}} \rho\omega_u A - \underbrace{\min\left[1.0, \left(2\frac{\theta_u}{\varepsilon_u}\right)\right]\left[e_p - \min\left(i, i_{dc}\right)\right]\rho\omega_u A}_{\text{transpiration}} - \underbrace{\alpha_{us}\frac{K_u}{y_u}\left[\left(\frac{1}{2} - \frac{\theta_u}{\varepsilon_u}\right)y_u + h_c\right]}_{\text{percolation/capillary rise}}\rho\omega_u A \quad (1)$$

The left hand side of Eq. (1) can be rewritten by applying the chain rule:

$$\frac{\mathrm{d}}{\mathrm{d}t}\left(\rho y_u \theta_u \omega_u A\right) = \rho y_u \omega_u A \frac{\mathrm{d}\theta_u}{\mathrm{d}t} + \rho\theta_u A \frac{\mathrm{d}}{\mathrm{d}t}\left(y_u \omega_u\right) \quad (2)$$

$$\rho y_u \omega_u A \frac{\mathrm{d}\theta_u}{\mathrm{d}t} + \rho\theta_u A \frac{\mathrm{d}}{\mathrm{d}t}\left(y_u \omega_u\right) = \underbrace{\min\left[\left(i - i_{dc}\right), \frac{K_{su}}{\Lambda_u}\left(\frac{1}{2}y_u + h_c\right)\right]}_{\text{infiltration}} \rho\omega_u A - \underbrace{\min\left[1.0, \left(2\frac{\theta_u}{\varepsilon_u}\right)\right]\left[e_p - \min\left(i, i_{dc}\right)\right]\rho\omega_u A}_{\text{transpiration}} - \underbrace{\alpha_{us}\frac{K_u}{y_u}\left[\left(\frac{1}{2} - \frac{\theta_u}{\varepsilon_u}\right)y_u + h_c\right]}_{\text{percolation/capillary rise}}\rho\omega_u A \quad (3)$$

Rearrange Eq. (3) by moving the second term of the left hand side to the right hand side, we have

$$
\begin{aligned}
\rho y_u \omega_u A \frac{\mathrm{d}\theta_u}{\mathrm{d}t} = & \underbrace{\min\left[(i - i_{dc}), \frac{K_{su}}{\Lambda_u}\left(\frac{1}{2}y_u + h_c\right)\right]}_{\text{infiltration}} \rho\omega_u A \\
& \underbrace{- \min\left[1.0, \left(2\frac{\theta_u}{\varepsilon_u}\right)\right][e_p - \min(i, i_{dc})]\rho\omega_u A}_{\text{transpiration}} \\
& \underbrace{- \alpha_{us}\frac{K_u}{y_u}\left[\left(\frac{1}{2} - \frac{\theta_u}{\varepsilon_u}\right)y_u + h_c\right]}_{\text{percolation/capillary rise}} \rho\omega_u A - \rho\theta_u A \frac{\mathrm{d}}{\mathrm{d}t}(y_u\omega_u)
\end{aligned}
\tag{4}
$$

Then we can arrive at

$$
\begin{aligned}
\frac{\mathrm{d}\theta_u}{\mathrm{d}t} = & \underbrace{\min\left[\frac{(i - i_{dc})}{y_u}, \frac{K_{su}}{y_u\Lambda_u}\left(\frac{1}{2}y_u + h_c\right)\right]}_{\text{infiltration}} \underbrace{- \min\left[1.0, \left(2\frac{\theta_u}{\varepsilon_u}\right)\right]\frac{[e_p - \min(i, i_{dc})]}{y_u}}_{\text{transpiration}} \\
& \underbrace{- \alpha_{us}\frac{K_u}{y_u^2}\left[\left(\frac{1}{2} - \frac{\theta_u}{\varepsilon_u}\right)y_u + h_c\right]}_{\text{percolation/capillary rise}} - \frac{\theta_u}{\omega_u y_u}\frac{\mathrm{d}}{\mathrm{d}t}(y_u\omega_u)
\end{aligned}
\tag{5}
$$

Since $\omega_u = \omega_c$ (Reggiani et al., 1999), and make use of Eq. 28, we finally have

$$
\begin{aligned}
\frac{\mathrm{d}\theta_u}{\mathrm{d}t} = & \underbrace{\min\left[\frac{(i - i_{dc})}{y_u}, \frac{K_{su}}{y_u\Lambda_u}\left(\frac{1}{2}y_u + h_c\right)\right]}_{\text{infiltration}} \underbrace{- \min\left[1.0, \left(2\frac{\theta_u}{\varepsilon_u}\right)\right]\frac{[e_p - \min(i, i_{dc})]}{y_u}}_{\text{transpiration}} \\
& \underbrace{- \alpha_{us}\frac{K_u}{y_u^2}\left[\left(\frac{1}{2} - \frac{\theta_u}{\varepsilon_u}\right)y_u + h_c\right]}_{\text{percolation/capillary rise}} + \underbrace{\frac{\theta_u}{\omega_u y_u}\frac{\mathrm{d}y_s}{\mathrm{d}t}}_{\text{water table change}}
\end{aligned}
\tag{6}
$$

– Page 655 Eq. 35 and 36: It is a very good idea to introduce a layering in the soil. however, you use only the porosity of the different layers to calculate the effective porosity in 10. For this purpose you only need Eq. 35. If the porosity in different soil layers is different, Ksu will be also different in the different layers, which affects infiltration and percolation! This approach would make much more sense, if you treated the upper layer as separate zone to calculate infiltration. Please comment on this.

Having realised that the heterogeneity of soil properties is crucial in determining the unsaturated flow, we attempted to take it into consideration by dividing the soil column into two layers of which the soil porosity and hydraulic conductivity are different from each other. Thus, the effective soil porosity of the two subsurface domains should be calculated by the Eq. 35 and Eq. 36. Indeed, when the soil porosity is different, the hydraulic conductivity is different. Hence we applied different hydraulic conductivity for the unsaturated domain (K_{su}), affecting the infiltration, and the saturated domain (K_{ss}) that determines groundwater seepage. Also, the effective conductivity (K_u), which affects the percolation, varies while the soil porosity changes. We fully agree that it would be ideal to explicitly account for the heterogeneity by dividing the soil into more layers, which act as separate domains (zones). However, in modelling practice, a trade-off between model complexity and efficiency has been made.

Section 3: Model application

– Please comment on the scaling factors in Eq. 23, 24, 13 ¡ E did you use them for calibration?

From modelling practice with the REW approach in the current stage, we realised that a large part of runoff production is sustained by surface runoff, which is generated on the saturated surface area. As a result, ω_o being a function of groundwater level and surface slope, acts, in fact, as a surface runoff coefficient. From Eq. 23, we observe that in the right hand side of the equation, the entire term within the brackets and its exponent varies between 0 and 1. Therefore, α_{sf} should not be larger than the

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catchment runoff coefficient. For a first estimation of α_{sf}, the runoff coefficient, and preferably, the surface runoff coefficient can be assigned. In our case, the α_{sf} is fixed throughout the modelling.

In Eq. 13, α_{us} is the scaling factor as a result of linearisation of the dependent function of the mass exchange between Uzone and Szone (see Eq. 25 of Reggiani et al., 2000). In our modelling work, we used the unsaturated soil porosity for α_{us} (similar to Reggiani et al., 2000), implying that the flow (recharge/capillary rise flux) is conducting only through the pores, which is logical and physical.

– *Please give information how you calculated potential evaporation which is input to Eq. 12.*

The potential evaporation data is provided, which is computed based on the Penman-Monteith equation.

– *Why did you use precipitation only from a single rain gauge?*

The data for other stations are not fully available to this study.

– *Page 657 line 13: Please give a reference where you took the values for the realistic guess from?*

We shall add information in the text to indicate how we assigned the values for the parameters.

Section 5

– *Maybe you can give an outlook on a) the most important improvements within the REW approach and b) promising approaches for assessing closure relations*

It is indeed a good idea to address these two points. We shall do so in the final paper, addressing, for instance, the importance of interception and rapid subsurface flow, and the need of inclusion of new domains for both processes respectively.

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