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Hydrology and Earth System Sciences Discussions

Interactive comment on "Extension of the Representative Elementary Watershed approach by incorporating energy balance equations" *by* F. Tian et al.

Anonymous Referee #3

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It is very interesting to read this paper, which is about theoretical extension of the REW concept, though I very much look forward to reading the future paper(s) on pursuing constitutive relationships for the balance equations presented in this paper.

This paper tries to extend the REW approach in two aspects: 1) expanding the energy balance equations to describe the processes that are of concerns in cold regions; 2) expanding the division of REW sub-regions into 8 zones. Based on their conceptualization, 24 ODEs for mass, momentum and energy balance equations have been obtained following the averaging approach that Hassanizadeh and Gray (1979a, 1979b) developed and Reggiani et al. (1998, 1999) have applied. This paper is of interest of HESS readership, especially for this special issue. The paper is generally structurally well organized, but needs refinement in English (see some examples listed in the technical corrections). However, I have the opinion that the authors should consider and address the points raised in the following before its possible publication in HESS.

1. The authors should discuss the scientific and societal significance of studies on hydrology of cold regions (snow/ice hydrology), especially of your region under study, and provide more literature studies on such research to better justify your motivation of the extension for the REW approach in that regard.

2. In this work, the authors segregate a REW into 6 zones for the surface area, different from the original REW concept, but keep the subsurface being 2 zones, same as the original the REW concept. The impetus of such division of a REW into 8 sub-regions and associated process descriptions are to remediate, as the authors claimed, the deficiencies of the original REW approach. However, I do have some concerns and worries on this extension:

    (a) Apparently, the surface heterogeneity is somehow more explicitly considered in this work than in the previous work on the REW approach. However, the authors avoided tackling the subsurface heterogeneity by excusing for avoiding over-complexity. Indeed, as the authors realized, it is easier to observe the surface flows than the subsurface flows. That is the reason why subsurface heterogeneity and the complex subsurface processes are so crucial to the understanding of watershed hydrological processes, and so to the physically-based modeling.

    (b) The partition of the REW surface area into fractions for the 6 surface zones and the determination of these area fractions would require large efforts, in addition to searching appropriate parameterizations for the 24 balance equations (although the authors may argue that this is left for future research).

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3. For simplifying the equation sets, the authors assumed that evaporation occurs only in the surface zones. It might be a reasonable assumption particularly for the cold regions during the cold periods. However, in a common sense, evaporation from the upper layer of soils or from the unsaturated zone, which is a very important process to deplete soil moisture, thus affecting runoff generation, is substantial and not negligible, especially in the non- or poorly vegetated regions during warm periods. Such an assumption, which needs better justifications, would restrict your approach/model to some very particular cases. Due to this, the title of this paper does not really reflect what it should do.

4. The derivation of general energy balance equations have already been pursued and presented in Reggiani's et al. (1998). To describe and model the energy processes in a real world hydrological system, one does not need to spend extra efforts on repeating the derivation of the general energy balance equations, but on parameterizing the energy balance equations by relaxing the associated restrictive assumptions. Therefore, the presentation of the general energy balance equations (form Page 474 till Page 479, Eq. (103)) can be largely shortened. It would be an essential extension of the REW approach and new application if the parameterization of the energy balance equations could have been sought.

   To present the energy balance in terms of temperature explicitly, as in Eq. (104), you have applied the 1^{st} law of thermodynamics and assumed that the change of the internal energy of the system is only due to the change of heat energy, whereby the work done by/on the system is neglected. This assumption should be explicitly listed in the paper as the other assumptions applied in your study.

5. For theoretical studies on hydrology, deriving equations is often involved. Therefore, applications of maths, the tool to do the job, are important. However, if we look down to the core of the studies, especially for hydrological processes studies, understanding and proper applications of physics that dictate those processes, and justified physically reasonable conceptualizations of the processes

are the more fundamental issues. Therefore, in my opinion, one should pay more efforts on searching for the physics or pursuing the appropriate physical conceptualizations rather than on basic mathematical manipulations and presentations.

For this paper, due to its lengthiness, I would suggest that it be shortened by reducing and omitting some parts of paper and the appendix, e.g. the definitions of the time-averaged REW-scale quantities that are more or less the same as have been presented in Reggiani et al. (1998), the proofs of "lemmas", the temporal and spatial derivation terms of the physical quantity ϕ, as well as the convective and non-convective terms of ϕ.

On the other hand, something on the derivations of the energy balance equations is missing in this paper: how did you arrive at those heat energy balance equations for each phase of each zone from the general energy balance equation (Eq. 32 or Eq. 104)? To be precise, how did you parameterize the first term (the external energy supply term) of the left hand side of Eq. 32 or Eq. 104 to be the second terms (the latent heat) or and third terms (the radiation term) appearing on the right hand sides of Eqs. 38, 39, 44, 49, 51, 53, 56, 59, 62, 65, respectively? At least, the assumptions or physical reasoning for such parameterizations should be presented.

Moreover, how did you come up with those "k"s for the energy exchange terms in these equations, and how would you define them (although, again, the authors might argue that this question is beyond the scope of this paper)?

6. In this paper, the authors argued that it is not necessary to differentiate the overland flows (between the infiltration-excess overland flow (Hortonian flow) and the saturation-excess overland flow). The authors justified this argument by stating that the water body has no infiltration capacity (impermeable), and so the saturation-excess runoff can be generated as long as the rainfall is greater than evaporation, and thus the saturation-excess runoff can be seen as a subset of infiltration-excess runoff. However, this argument and statement are fundamen-

tally conceptually flawed. Firstly, strictly speaking, water body and soil are different media when rainfall infiltration is regarded. Secondly, more crucially, the two runoff generation mechanisms are very different. The former is a top-down process (the saturation starts from the top surface layer) while the latter is a bottom-up process. As a result, the runoff generated by the former mechanism has a much shorter time scale with which stream hydrographs exhibit sharp flood peaks (e.g. flash floods in arid and semi-arid regions almost immediate after rainfall evens due to the facts that the surface soil layer is compact and so impermeable, or rainfall intensity is too high) while the runoff generated by the saturation-excess mechanism often show a longer storm-to-peak time lag (e.g. delayed high stream flow evens in humid vegetated regions due to the fact that soil saturation has to be first met).

Specific Comments/Technical Corrections

Page 430, Line 6: "The REW approach, however, cannot... because of..." This statement is incorrect. As a theory, the REW approach has already taken energy balance into account. However, the resulting balance equations and their closure relations in the current forms (indeed due to the assumptions for the initial applications) are not able to describe the energy processes.

Page 430, Line13/Line 21: The word "factoring"/ "re-configure" should be "factorizing"/ "re-configurate", respectively.

Page 431, Line 13: "Based on...into catchment zones (REWs) and sub-regions...". It would be better to revise the sentence as "...into catchment units/elementary units (REWs) and sub-regions (zones)...".

Page 433, Line 11-12: I have difficulty to understand the statement: "In Reggiani et al's formulation, energy balance equations are considered as identical equations and omitted due to their isothermal assumption". This statement is, to my understanding, the

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main motivation of this work. Please make the statement clear on “identical equations and omitted”.

Page 434, Line 2: “phases such as ice and snow. . . ”. Ice and snow both are solid phase of water but in different crystal structure.

Page 435, Line 10-14: Indeed, in the blueprint of Freeze and Harlan (1969), no separation of the mechanisms for overland flow was made. However, I don’t see much literature that supports the abandonment of the differentiation of the overland flow mechanisms in “the current generation of physically-based models”. At least, please provide stronger or more references to back up your statement.

Page 438, Line 2-11: the term “subsurface flow” is quite ambiguous, a clearer definition or explanation should be provided.

Page 439, Line 11-13: Is it true that, of all the surface zones, ONLY the main channel reach CAN exchange water, momentum with the neighboring REWs or the external world? Presumably it is your assumption. If it is an assumption, it would be better to explicitly describe it and justify it.

Page 441, Line 23: “. . . which is denoted by $S'^{T}(K)$.” T is reserved for a superscript indicating “atmosphere”, but here$S'^{T}(K)$ is used for denoting land surface. It is confusing and misleading to use T as a superscript in this context.

Page 445, Line 1-4: This is the definition of the time-averaged main channel length per unit REW surface area (projected), in the unit [L^{-1}]. However, in the list of symbols, ξ^r (Page 485) is given a unit [L], which is wrong. I suggest that all units should be thoroughly checked, and units for some quantities (e.g. b and L) that are missing should be added. In addition, L is reserved for symbolizing the entropy production. But L is used also for the instantaneous length of the main river channel, and for superscripts indicating neighboring REWs too. It would be better to try to avoid such confusion.

Page 474, Line 8 and Page 476, Line 7, the Eqs. for I_α^{jP}: the domain of the spatial integral (the second integral) should be S^{jp}. In your Eqs., you put a lower limit as S and upper limit as *jP* for the second integral, which are probably typos.

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