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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.

F. Tian et al.

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Answers to comments by anonymous referee 3

We would like to thank the anonymous Referee 3 for his valuable comments on our manuscript, which will improve the quality of the paper greatly.

General comments

As pointed out by the Referee, it is true that the differences between our extension and the original formulation by Reggiani *et al.*(1998), and the main objectives are not highlighted in the manuscript. Some of the extension of REW definition and formulation may be possible not clearly expressed, and the manuscript has some redundancy on definition and formulation. Some sentences are apt to be misled due to the language problem. Major improvements have been made in the revised manuscript according to the Referee's comments. Here we would like to explain and discuss the following key points:

(1) The scientific and societal significance of studies on energy related processes, as well as the literatures, are added in the revised manuscript.

As pointed out by the Referee, explicit treatment of energy related processes, especially for cold regions, is the major attention of our manuscript. Therefore, the scientific and societal significance of studies on energy related processes should be discusses, which is missed in the original manuscript. In the revised manuscript, the importance of studies on evaporation/transpiration and special hydrological processes related with cold regions such as melting, freezing, and thawing are highlighted.

(2) Inclusion of subsurface heterogeneity in the REW definition is discussed in the revised manuscript.

In our extensible definition of REW, hillslope is treated with by its flow nature as well as by its evaporation/transpiration nature simultaneously, and is divided into various kinds of land covers which presently include bare soil zone, vegetated zone, snow covered zone, and glacier covered zone. Surface heterogeneity is, therefore, more explicitly considered than in the previous work as pointed out by the Referee. For the subsurface layer, the most striking heterogeneity is the layered strata along the soil profile vertically. Subsurface flow and associated preferred flow (Lei *et al.*, 1999) could be generated in the heterogeneous subsurface zones. To take such heterogeneity into account, not only should the subdivision scheme of subsurface layer and hence the final balance equations be revised, but also the related constitutive relationships such as evaporation/transpiration, infiltration, and seepage outflow well developed by Reggiani and Rientjes (2005) and Lee (2005) should be revised. This will require a lot of endeavor. In the new definition of REW, such heterogeneity is, therefore, excluded

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explicitly in order to avoid over-complexity. However, the additional equations could easily be coupled if we further divide the saturated zone and the unsaturated zone into several strata, which can be done in a way similar to the introduction of the reservoir zone demonstrated in Appendix B in the revised manuscript.

(3) Determination of area fraction of every sub-region in a REW is discussed in the revised manuscript.

To our mind, with the help of field and remote observation technologies the area fraction of every sub-region in a REW can be easily determined. For example, LAI which indicates the vegetated area can be obtained from several satellite productions such as MODIS and Landsat. Also, the location of the snow covered zone can easily be determined by field or remote observation. We discuss the determination of each sub-region's area in detail in the description paragraphs of each sub-region in Sect.3. The main ideas are listed as follows:

Main channel reach: The water course of the main channel reach can be determined either by field observation or by DEM analysis.

Sub-stream-network zone: The location and area of lakes, reservoirs, or other large water bodies can be determined by field or remote observation, and that of the sub-REW-scale network of channels can be determined by field observation or DEM analysis, similar to the main channel reach.

Vegetated zone: The horizontal projected area of vegetated zone changes with the calendar and the cultivation season which could be measured by remote observation or modeled by various crop models (Cong, 2003).

Snow covered zone: The location of the snow covered zone can easily be determined by field or remote observation. Its area and depth are key factors for hydrological modeling in cold regions. However, they cannot easily be recognized and much literature can be found about their measurement and modeling (Maurer *et al.*, 2003; Chen *et al.*,

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1996; Cao and Liu, 2005).

Glacier covered zone: The glacier covered zone is the volume occupied by glacier ice whose location is always fixed.

Bare soil zone: The horizontal projected area of bare soil zone varies with the area of other surface sub-regions.

(4) Where evaporation/transpiration occurs and how is it represented in our approach?

As pointed out by the Referee, we assume that evaporation occurs only from the surface sub-regions in our manuscript (see Assumption 1 in Sect.6.1). The Referee questions such assumption. We agree with the Referee's comment that evaporation from the upper layer of soils is a very important process to deplete soil moisture. To our mind, however, the so called upper layer of soil can be considered as bare soil zone in our defined REW system. When water evaporated from soil surface what actually occurs includes two consecutive processes, the former is capillary rising process from unsaturated zone to soil surface, and the latter is water vaporization process from soil surface into the atmosphere. Commonly the term "evaporation" just indicates the latter process. When water supplied to the soil surface is abundant the potential evaporation occurs, and when water supplied to the soil surface is limited the potential evaporation is restricted. In our extension of REW approach, capillary rising process can be represented by water exchange term between bare soil zone and unsaturated zone, and evaporation can be represented by phase transition term between liquid and vapor in the bare soil zone and other surface sub-regions. For detail the reader can refer to Eq.(34) in the revised manuscript or Eq.(45) in the original manuscript.

(5) Why should the general energy balance equations be reformulated?

Some new definitions, which are different from Reggiani *et al.* (1998), are introduced in our manuscript such as time averaged and fluctuation value of the generalized in-

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ternal energy (see Definition 19 and Definition 20 in the revised manuscript), the time averaged value of the generalized external energy (see Definition 21), and the generalized energy exchange term across the interfaces (see Definition 22 and Definition 23). These definitions help clarify the formulation and are, therefore, necessary, which are not explicitly defined in Reggiani et al.'s (1998) formulation. Also, as pointed out by the Referee, the energy balance equations in terms of temperature explicitly are proposed in our manuscript, which will be convenient for the equations' application. In spite of its necessary, the formulation procedures for the energy balance equations are greatly simplified to shorten the length (which is reduced from more than 6 online pages to 4 online pages or so) by following the Referee's comments. Furthermore, some of the formulation procedures are removed or simplified in order to shorten the length but not at any cost of interpretation in Appendix A. For example, the proof of Lemma1 and Lemma2 is removed, and the formulation procedures for temporal and spatial derivation terms, convective and non-convective terms, and the general form of energy conservation equation are largely simplified.

(6) The derivation of the energy balance equations.

The Referee questions the logicality of derivation procedure of energy balance equations. However, we couldn't understand the Referee's question. To our mind, each term in the equations has been explained in detail, and no confusion emergences especially in Eq.(32) or Eq.(104) which expresses the heat balance relationship. The term on the left hand side of Eq.(32) represents the derivation of heat storage of α phase in j zone due to the variation of the temperature (which is not the external energy supply term as argued by the Referee), and on the right hand side the first term accounts for heat generation rate of α phase in j zone, the second term represents heat transfer rate from j zone to its environment, and the third term accounts for the heat transfer rate from the α phase to the remaining phases within j zone.

(7) The fraction coefficient "k"s for the energy exchange terms

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The fraction of heat exchange terms absorbed by water, ice, and soil matrix are used in the derivation of the heat balance equations. However, they are removed in the final results. That's to say, these coefficients do not appear in the final equations (see Eq.(33) in the revised manuscript or Eq.(44) in the original manuscript).

(8) Partition of saturation excess flow and infiltration excess flow

As pointed out by the Referee, partition of saturation excess flow and infiltration excess flow really has the major advantage to inform on the saturated fraction of the REW surface. The "abandon" argument in the original manuscript is not suitable. In the revised manuscript, we emphasize that runoff generation can be modeled physically even without such partition, and that different types of runoff generation have the underlying unified mechanism (Rui, 2004). The fact that the hillslope is divided into two different overland flow zones does help to represent various flow processes conveniently, but still cannot represent evaporation/transpiration occurring from various kinds of land cover such as water, vegetation, bare soil, snow, and glacier. Hillslopes, which are the primary regions for runoff generation as well as water dissipation, must be treated with by its flow nature as well as evaporation/transpiration nature simultaneously. Therefore, the original hillslope division scheme containing saturated overland flow zone and concentrated overland flow zone, which is intended to account for various flow processes, is inadequate for hydrological modeling physically. In our new definition, the hillslope is divided into various kinds of land covers which presently include bare soil zone, vegetated zone, snow covered zone, and glacier covered zone.

The original statement that the saturation excess runoff can be seen as a subset of infiltration excess runoff is incorrect and is removed and revised in our new manuscript.

We agree with the Referee's argument that the two runoff generation mechanisms are much different. The infiltration excess runoff is a top-down process, while the saturation excess runoff is a bottom-up process. But we still argue that the two different types of runoff generation have the underlying unified mechanism. For detail readers can

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refer to Rui (2004) or the brief description in Sect.3 in our manuscript. To our mind, the differences between two runoff generation mechanisms are the results of the combined effects of rainfall and soil with different characteristics, which can be modeled physically without such partition as in most current physically-based hydrological models such as SHE (Abbott *et al.*, 1986a, b) and GBHM (Yang D. *et al.*, 2000, 2002a, 2002b).

The authors' answers to the specific comments are listed below:

Comment 1

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.

And

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

As pointed out by the Referee, Reggiani *et al.*'s (1998) formulation has already taken energy balance equations into account. In spite of it, energy related processes cannot be represented physically. For example, Reggiani et al. (1999) has already pointed out evaporation/transpiration cannot be modeled physically. Furthermore, due to the restrictions of the Reggiani *et al.*'s (1998) REW definition, the hydrological processes occurring in the immense cold regions such as melting, freezing, and thawing, which are intensively coupled with energy supply and transfer processes, cannot be modeled at all. These key points are clearly expresses in the revised manuscript.

Comment 2

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

To our mind, two phases composed of the same type of molecule but in different crystal structure are called different phases.

Comment 3

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

The term "subsurface flow" indicates flow generated in the unsaturated zone, which is heavily used in the hydrological literature. If we search the key word "subsurface flow" by google, perhaps 6,490,000 results may be found.

Comment 4

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.

Owing to the definition of REW, which is actually a sub-watershed, the surface water could not flow across the division boundary of the REW except watershed outlet. To our mind, no more words are worthy to explain such facts.

Comment 5

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.

We agree with the Referee's comment, T should be reserved for indicating "atmo-

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sphere". To our mind, land surface is the interface between REW and the "atmosphere". We just use T to indicate the "atmosphere".

Other comments, concerning spelling and style were taken into account in the revised manuscript.

References

Abbott, M. B., Bathurst, J. C., Cunge, J. A., O'Connell, P. E., and Rasmussen, J.: An introduction to the European Hydrological System - Systeme Hydrologique European, SHE, 1: History and philosophy of a physically based, distributed modeling system, J. Hydrol., 87, 45-59, 1986a.

Abbott, M. B., Bathurst, J. C., Cunge, J. A., O'Connell, P. E., and Rasmussen, J.: An introduction to the European Hydrological System - Systeme Hydrologique European, SHE, 2: Structure of a physically based, distributed modeling system, J. Hydrol., 87, 61-77, 1986b.

Cao, Y. and Liu, C.: The development of snow-cover mapping from AVHRR to MODIS, Geography and Geo-Information Science, 21, 15-19, 2005.

Chen, X., Li, X., Lu, A., and Li, W.: Progresses on quantitative remote sensing of snowcover, Remote Sensing Technology and Application, 11, 46-52, 1996.

Cong, Z.: Study on the coupling between the winter wheat growth and the water-heat transfer in soil-plant-atmosphere continuum (dissertation), Tsinghua University, China, 2003.

Lee, H., Sivapalan, M. and Zehe, E.: Representative Elementary Watershed (REW) approach, a new blueprint for distributed hydrologic modelling at the catchment scale: the development of closure relations, in: Predicting Ungauged Streamflows in the Mackenzie River Basin: Today's Techniques and Tomorrow's Solutions, edited by Spence, C., Pomeroy, J. W., and Pietroniro, A., Canadian Water Resources Association (CWRA), Ottawa, Canada: 165-218, 2005.

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Lei, Z., Hu, H., Yang, S.: A review of soil water research, Adv. Water Sci., 10, 311-318, 1999.

Maurer, E. P., Rhoads, J. D., Dubayah, R. O., Lettenmaier, D. P.: Evaluation of the snow-covered area data product from MODIS, Hydrol. Process., 17, 59-71, 2003.

Reggiani, P., Sivapalan, M., and Hassanizadeh, S. M.: A unifying framework for watershed thermodynamics: balance equations for mass, momentum, energy and entropy, and the second law of thermodynamics. Adv. Water Resour., 22(4), 367-398, 1998.

Reggiani, P., Hassanizadeh, S. M. and Sivapalan, M.: A unifying framework for watershed thermodynamics: constitutive relationships, Adv. Water Resour., 23, 15-39, 1999.

Reggiani, P. and Rientjes, T. H. M.: Flux parameterization in the representative elementary watershed approach: application to a natural basin, Water Resour. Res., 41, 1-18, 2005.

Rui X. F.: Principles of Hydrology, China Water Power Press, Beijing, China, 143-152, 2004.

Yang, D., Herath, S., and Musiake, K.: Comparison of different distributed hydrological models for characterization of catchment spatial variability, Hydrol. Process., 14, 403-416, 2000.

Yang, D., Herath, S., and Musiake, K.: Hillslope-based hydrological model using catchment area and width functions, Hydrol. Sci. J., 47(1), 49-65, 2002a.

Yang, D., Oki, T., Herath, S., and Musiake, S.: A geomorphology-based hydrological model and its applications, in: Mathematical Models of Small Watershed Hydrology and Applications, edited by: Singh, V. P., Frevert, D. K., Water Resources Publications, Littleton, Colorado, Chapter 9, p. 259-300, 2002b.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 3, 427, 2006.