

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

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### **General comments**

The paper presents an extension of the Representative Elementary Watershed concept to cold regions. Do to so, the initial REW model was modified in order to include explicitly the energy balance equations. It also required a new organisation in the REW zones in order to be more appropriate to glacier and snow covered regions. Further, it helps in improving the representation of evapotranspiration in the REW model which was previously poor. This last point could be more highlighted in the text as it a major process in most hydrological catchments, even in "hot" regions. The paper presents

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the theoretical equations, for each of the 6 surface zones and 2 subsurface zones. The key point of the REW concept, which is the closure relations, is left for another paper, which could be discussed. As such, the model could not be applied to a real catchment. This limits the conclusions of the development, as always with the description of a new model. In this presentation of the extension of the REW concept, I would suggest that the authors highlight, in the manuscript structure, what was adapted from the Reggiani's model structure and definitions, and in parallel what is totally new.

This paper addresses relevant scientific questions within the scope of HESS, as the REW concept is a new hydrological modelling approach which could be a way to capitalize the knowledge of each "traditional" approach. The paper presents novel ideas, by adapting this concept to cold regions, which was never done before, nor published in any other journal. The mathematical methods are clearly exposed and sometime over-developed. The assumptions allowing simplifications of the equations are clearly explained. Nevertheless, more attention should be paid to explain clearly what was already available in the first version of the REW model and what is totally new, especially concerning the various definitions. The abstract provides a concise and complete summary of the study. The paper is well structured. The appendix is hard to read and follow for non mathematician readers. On the contrary, the nomenclature list is very useful. The language is quite fluent and precise.

### Specific comments

1. Title (page 427): Concerning the title of the paper, I would suggest changing "incorporating" by "considering" or "including"; as the energy balance equations were already incorporated in the work of Reggiani et al.(1998, 1999). The major contribution of the present paper is a real application of the equations to each zone, and the context of cold regions. I would suggest "Extension of the Representative Elementary Watershed approach by considering energy balance equations for cold regions".

2. Page 429 lines 7 to 11: I agree with the authors that there is a problem of com-

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patibility between the physical meaning of the equations and the scale at which we use them. In my mind, the ways proposed to solve this problem are not opposed, and Beven (1989, 2002) did not present them as such. On the contrary, I would say that they are complementary and need to be conducted at the same time. This is one of the major stakes of research in hydrological modelling, nowadays. And this is particularly true with the REW concept. This complementarity should be addressed in the text.

3. Page 430 line 10: It seems to me that freezing and thawing are elementary processes of the hydrological cycle, only for cold regions. On the contrary, the authors could highlight their improvement of the representation of evaporation and transpiration that is much more universal.

4. Page 431 lines 1 and 2: as pointed out by the authors, the derivation of the closure relations is the key point of the REW concept. So, it's a shame that it could not be presented in the article with the balance equations. Nevertheless, I agree it would have been a difficult and too long article. At least, some ideas of investigation could be provided in the text for some of the processes. Will it be done under a general form as it was proposed in Reggiani et al. (1999)? Any modelling approaches could be helpful to derive these closure relations?

5. Pages 431 to 433: As the paper includes many equations, I would suggest not to present equations 1 to 11 which are the ones developed by Reggiani et al. (1998, 1999, 2000)

6. General comment on part 3: the authors should work on a better organisation of this part of the manuscript, which is long and not clearly structured. For instance, a way to do it would be to explain more clearly what is kept from the Reggiani et al. model, what was modified in the new version, and what is totally new. Another structure of this part should be a presentation by processes: vegetation, stream network, surface runoff, spatial discretisation of the surface, etc.

7. Page 434 lines 5 to 13: The paragraph does not reflect exactly the way the transpi-

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ration is taken into account in the model. Even if transpiration occurs at the leaf level of the plant, the water comes from the soil layers by the way of root extraction. It does not appear as such in this part of the text, which is quite misleading. Furthermore, it seems to me strange to omit evaporation, which could be higher than transpiration when the vegetation is scarce (i.e. low LAI). According to the manuscript, you totally omit it, even in the bare soil surface zone. Is it specific to cold region? How could it be included for catchment where evaporation is an important part of the water budget? If the paragraph is kept as such, the last sentence should be "the vaporization of water within the soil is small compared with *transpiration* (Jacobs et al. 1999)...".

8. Page 435 lines 9-14: Even if the two ways of runoff generation are sometimes uneasy to distinguish, the separation between the saturated overland flow zone and the concentrated overland flow zone has the major advantage to inform on the saturated fraction of the REW surface, which could be an interesting internal variable.

9. Page 435 lines 15-22: The authors should clarify in which cases the sub-REW-scale network needs to be described explicitly, as it does not seem to me it was the objective of the REW model. On the contrary, Snell and Sivapalan (1995) worked on the simplification of the representation of the river network and introduced the conceptualisation of the river in the REW model.

10. Page 436 lines 2-3: The surface and subsurface layers were already separated in the Reggiani's version, weren't they?

11. Page 436 line 3 and 29: The authors should be more careful concerning the "fundamental land surface types". The one chosen are characteristic of cold regions but not universal ones. They are not suitable, for instance, for any catchment in Africa, Southern Asia, Oceania, etc.

12. Page 437 lines 1-5: This comment is linked to the previous one. As the authors suggest that "new sub-regions can easily be added", I suggest that they better explain how to do this. An extension to this sub-REW discretisation could be to consider

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various types of vegetation as it is done in several distributed models such as SWAT (Arnold et al. 1998), or HYDROTEL (Fortin et al., 2001).

Fortin, J.-P. et al., 2001. *Distributed watershed model compatible with remote sensing and GIS data. 1. Description of model. Journal of Hydrologic engineering*, 6(2): 91-99.

13. Page 438 lines 1-12: The first paragraph of this page should be written more concisely, more directly

14. Page 438 lines 17-21: The explanation of the upper limit of the saturated zone sounds strange to me as the level of the water table is a mean value for the whole REW. As the REW is conceptualised only with mean values of altitude, the limit should not vary across the REW, except if the saturated zone is now considered at the surface sub-regions scale and not at the REW scale. Please, explain if you changed anything in the saturated zone conceptualisation. page 438 line 21-23: The use of the word "isolated" sounds like if the saturated zones could not exchange any flux with the external world or the neighbouring REWs. But, according to the balance equations for the saturated zone (equation 34), it does not seem to be the case. Please, clarify this.

15. Page 438 line 29: Why is gas considered as a phase material in the unsaturated zone, if the evaporation and the transpiration are not considered in this zone?

16. Page 439 lines 22-27: I wonder that no evaporation at all is considered, not even in the bare soil zone. Is it because the adaptation of the REW concept is specific to cold regions?

17. Page 439 lines 28 to page 440 line 4: I wonder whether or not the snow covered area may vary in time. Is it possible in the model structure?

18. Page 440 lines 5-8: The glacier covered area could probably be seen as invariant in some parts of the world, but not in the Andes or in the Alpes. Is this assumption specific to the authors' site of study?

19. Page 441 §4.1: The authors should explain what is identical to the previous version,

and what is new, in the geometric description of the REW.

20. Page 444-445 §4.3.: The authors should explain that these definitions are extensions of the definition of Reggiani et al. (1998), to the new zones, or at least highlight the novelty of their definitions.

21. Page 451 line 22: Table 6 is not really useful because it's impossible to read it without the nomenclature list. It should be drawn again by incorporating the meaning of the terms (e.g. with a crossed table) or avoided.

22. Page 462 line 23: The last sentence should be "spatial scale of the REW" or "elementary watershed". The equations are not applied at the whole watershed scale.

23. Page 463 lines 14-17: The way the runoff is represented in the model is not really described before the conclusion. It should be presented elsewhere (e.g. part 3) and only summarised in the conclusion.

24. Page 463 line 21: The last sentence of the paragraph is misleading as it is explained in page 435 that "it is unnecessary to separate the saturation excess runoff from the infiltration excess runoff any more".

25. Page 464 lines 1-7: The procedure to include another sub-region is an important point for the applicability of this new version of the REW concept by other researchers. It should be extended to more general situations, such as considering various vegetation types, or cities, etc. It should be discussed in the text and reminded more concisely in the conclusion.

26. Page 464 lines 13-16: Could the authors give any ways of investigation for the development of the constitutive relationships, which are the key point for the application of the model to a real catchment?

### Technical corrections

27. Page 434 line 13: (Adrie, 1999) should be replaced by (Jacobs et al. 1999). There

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was confusion between the first name and the last name. The changing should be also done in the references list.

28. Page 435 line 7: "which" should be added at the beginning of the parenthesis: which cannot exceed...

29. Page 438 line 5: Write "subsurface flow can *be generated*".

30. Page 438 line 21: Write "be considered as *part of*..."

31. Page 439 line 2: Write "capillary *rise*".

32. Page 445 line 14: "Definition 10" should be added before equation 23.

33. Page 463 line 21: Replace the sentence by "total surface runoff can be divided into infiltration excess and saturation excess, according to..."

34. Page 464 line 11: "Reggiani et al. 2005" should be replaced by "Reggiani et Rientjes, 2005"

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