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Hydrology and Earth System Sciences Discussions

Interactive comment on "A generic system dynamics model for simulating and evaluating the hydrological performance of reconstructed watersheds" *by* N. Keshta et al.

N. Keshta et al.

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Response to Editor comments

1) It is necessary a comparison with at least one other soil moisture model, more widely known and tested than the original SDW, in order to properly assess the improvement allowed by the proposed approach; The system dynamics (SD) approach is based on the understanding of the complex relationships existing among the different elements within the considered system. The main issue in using the SD modeling approach is to understand the system and its boundaries, moreover, to identify its key building blocks, and the proper representation of the physical processes through relatively ac-

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Interactive Discussion

Discussion Paper

curate mathematical relationships (modeling for understanding, not just for prediction). Therefore, it is a different modeling philosophy from the traditional modeling approach. We consider this philosophy to be more suitable for reconstructed watersheds. To show the effect of these modifications, the authors incorporated a preliminary run to compare between the performance of the SDW and the GSDW models. However, the authors will try to make that comparison with one other soil moisture model e.g. SWAT in order to assess the merit of the purposed GSDW model in simulating soil moisture and AET.

Accordingly, an extensive review of the evolution of the soil moisture models presented in hydrological literature (and especially of those including an explicit modelling of the role of vegetation) should be included in the Introduction. Referee #1, who explicitly asked for this kind of review, may perhaps help the Authors suggesting additional appropriate references to integrate the Authors& list.

We will try to locate some additional relevant papers; however, it will be greatly appreciated if referee #1 can help in suggesting additional appropriate references.

2) It is necessary to test the model over a case study that include overland flow generation (either changing the case study or choosing a period in which there is runoff generation in the reconstructed watersheds), in order to analyze the performance of the model in the reproduction of water balance;

One of the criteria of selecting soil covers in reconstructed watersheds in north of Fort McMurray, northern Alberta, Canada, and in other similar semi-arid areas is to increase the infiltrated water and minimize the generated overland flow. Referring to Figure 8, there is a slight value of overland flow generated during snowmelt period and in day 192 there is another value of 9mm of overland flow corresponding to an incident of 40 mm of precipitation. The previous values show that the overland module is functioning in the GSDW model. Moreover, our manuscript is about this case study and similar regions. Models that just aim at predicting runoff are so many in the literature. We did not intend to add one more to such valuable traditional models.

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3) At the current stage of development of spatially-distributed models, the use of a lumped approach on a watershed of almost 1580 km2 (the natural watershed case study) cannot be considered appropriate for reproducing phenomena so variable in space as soil moisture and vegetation storage.

Hydrological models can be classified into two major types, lumped and distributed. The choice between a lumped and a distributed approach is based on the purpose of the model and the natural of watershed. Typically, lumped parameter models provide a general understanding of the system but do not give a complete coverage of the details (which fulfills our aim of modeling for understanding, not just for prediction). The engineering purpose here is to asses the performance in terms of soil moisture and AET. Those need to be assessed as an overall lumped average. Because of this we need to base the assessment on overall lumped average rainfall. Also the area has homogeneous vegetation and soil properties. So, we are not really simulating the 1580 km2, but rather a representative column of it. This is a necessary 1st step in designing reclamation soil cover. In this context, the choice between a lumped and a spatially distributed approach for rainfall-runoff (RR) modeling is not a trivial one. Andréassian et al. (2003) compared both lumped and distributed approaches in RR modeling in “chimera watersheds”, which result from the association of two “actual” watersheds (2200 Km2), similar in size, but located away from each other on the drainage network. Andréassian et al. (2003) concluded that the largest part of the improvement that can be brought by spatial distribution is due to rainfall variability, which is not applicable in our case study where the natural watershed was treated as a lumped system.

References: Andréassian V., Oddos A., Michel C., and Perrin C., 2003. "Chimera watersheds to understand the relative importance of rainfall distribution in semi-distributed rainfall-runoff models". 17TH Conference on Hydrology, Impacts of Water Variability: Benefits and Challenges, The 83rd Annual, Long Beach, CA.

Response to Referee # 1 comments

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The model is a lumped conceptual model, not really innovative because it seems similar to old multilayer models that are present in hydrology text books.

Well, in principle, yes it is similar to some existing models. However, in the building approach, simulation environment, and purpose of modeling, it is drastically different. The application is also unique; include: • Minimizing parameterization. • The model combines both physically based and empirically based equations to describe the hydrological processes. • The model could be was used to simulate a wide spectrum of sites with; different soil types, soil stratification, and inclination relying on a widely available meteorological and soil data. • Incorporating the previous modifications allows the GSDW model to test hypothetical cover alternatives, and the performance of the reconstructed covers for long term periods.

This will be of great use for design and reclamation decision making for the oil sands industry.

A previous model version published by the authors in 2005 and 2007 is not clear. I cannot read those papers, but the authors explained that the only new thing of this model version is the canopy storage module. The authors proposed two approaches. However it is not clear if they used both the approaches, they did not provide a comparison of the two methods, and more important they do not have data for testing the approaches..

We do believe that we did not mention in the manuscript that the only modification was the canopy storage module. The previous model versions were extremely site specific models where one has to build his/her own model to simulate each site individually. Whereas, the GSDW model allows the user to select: soil stratification, thickness, number of soil layers and their inclination. The canopy storage was computed in the GSDW model using two techniques based on the available data; Valente et al. (1997) conceptual model, and van Dijk and Bruijnzeel (2001) analytical model. The main practical drawback of the Valente et al. (1997) model lies in its extensive data requirements which are not always available. While, The van Dijk and Bruijnzeel (2001) model; based on a modification of Gash et al. (1995) interception model, elegantly retains some of the simplicity of the empirical approaches. The previous two approaches and a third option of no canopy were provided in the GSDW model to switch between them based on the available canopy data for the site. This will be made clear in the revised manuscript. We agree with the referee that there are no data in the reconstructed sites for testing the approaches; however, we do believe that those approaches were tested and validated in literature before, and testing other components of the water balance also implicitly validate the interception component. Finally the model at watershed scale is not really tested, because in the case study the runoff is absent.

One of the targets of designing the soil covers in the reconstructed watersheds in Fort McMurray, northern Alberta, Canada, is to increase the infiltrated water and to minimize the runoff, as the study areas are in semi arid regions where PET exceeds the precipitation. This is the main reason that model does not show considerable runoff values except in snowmelt periods and some rare events during the growing season and not because a malfunction of the model structure and formulations. The runoff in this case is of much less importance compared to soil moisture and evapotranspiration.

1) Introduction section. It must include a literature review on hydrologic models for soil moisture prediction, and more important for runoff prediction at watershed scale. The authors must explain what is the new contribution of their model and why they are not using existing models. At least they should compare their model with existing models.

The Introduction section will be re-edited to cover the referee comment. The results of a preliminary run to compare between the SDW and the modified version GSDW model will be incorporated to the revised manuscript to cover the 2nd referee comment. Moreover, the authors will try another existing model (e.g. SWAT model) to compare with the GSDW model.

2) Page. 1447 and 1448. cut these two pages. It is enough the scheme of Figure 2.

They are just describing a typical hydrological model.

Will be amended in the revised manuscript.

3) Page 1449 and 1450: The canopy interception model seems too complex. It compute the evaporation rate from the canopy as the sum of both the trunk and the canopy evaporation. You do not have data to test the model, and it seems to me that the contribution of the trunk evaporation is not really important. You are just increasing parameterization. Again I cannot see in the paper any data for testing this canopy module and I cannot find any comparison between different canopy models.

As mentioned previously, two canopy modules were used in the GSDW model; Valente et al. (1997) conceptual model, and van Dijk and Bruijnzeel (2001) analytical model. The authors agree that Valente et al. (1997) conceptual model is to some extant complex in structure and need a lot of data to run the module, that was the main reason that the authors implemented van Dijk and Bruijnzeel (2001) analytical model. In the GSDW model structure, the authors allowed the user to choose between them; moreover, we added another option which is the case of no canopy in case of newly reconstructed sites. The selection was left to the user depending upon the available data.

4) Page. 1451: SW is the surface water storage. Can it saturate? I cannot see any saturation parameter. Why?

Equation 7 shows only the change in the surface water storage (a simple water balance at soil surface) not the way the water will be distributed between each hydrological process, that is the reason that the referee can not see any saturation parameter. The logical procedure of the way the model handles saturation is described in page 1453, line 10-14, and represented in Fig. 2 page 1472.

5) Page 1454, equation 11. It needs more explanations. Is it derived mathematically? How did you estimate it?

It is a modified empirically based equation used in the previous SDW models, however we added another parameter to cope with different inclination of soil layers (Cslope) to compute the interflow component.

6) Page 1455. equation 12. again, explain the equation derivation.

It is a modified empirically based equation from the previous model to cope with the overland flow component. Since the model structure uses reservoir-based mechanisms to simulate the different hydrological processes, water in excess of the infiltration capacity of the first layer is directed as overland flow in the summer.

7) Page 1458. it is not clear the size of the natural watershed in the area of BOREAS. There is written that the area is 1000 km x 1000 km, which is huge. I do not think that you are simulating the whole area with the lumped model. Furthermore details on runoff measurements are not provided. What is the instrument? Finally details on basin characteristics are not provided. What is the basin area? What is the average altitude? Etc.

The OA site is almost 1580 km2, with an elevation of about 600 m. The 1000km x 1000km refers to the area of the former Boreal Atmosphere Exchange Study (BOREAS), covering a large portion of Saskatchewan and Manitoba,. In the reconstructed watersheds, runoff where measured in the inclined covers using a channel to collect the runoff water from each cover and weirs for measuring its values. Details on basin characteristics will be provided in the revised manuscript. This portion will be added; “The soil properties are as follows: the saturated hydraulic conductivities are 25 (cm/day), 5.76 (cm/day), and 4.8 (cm/day), and the porosity values are 0.51, 0.45, 0.46 for A, B and C horizons, respectively (Cuenca et al., 1997). The forest canopy is dominated by trembling aspen (Populus tremuloides) with an average height of 21 m and about 2 m high hazelnut (Corylus cornuta) understory interspersed with alder (Balland et al., 2006). Based on the data from an Environment Canada meteorological station nearby Waskesiu Lake (53.92 N, 106.07 W), the mean annual precipitation was 467 mm”. The modeling purpose here is to

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asses the performance of those watersheds in terms of soil moisture and AET. Those need to be assessed as an overall lumped average, base on the overall lumped average rainfall. Also the area has homogeneous vegetation and soil properties. So, we are not really simulating the 1580 km2, but rather a representative column of it

8) Page 1458. cut equations 13, 14 and 15 (definition of rmse, mean absolute relative error and correlation coefficient). They are redundant and obvious.

We agree and equations will be removed from the revised manuscript.

9) Table 1 lists the model parameters, but I cannot see soil parameters. Saturated hydraulic conductivity? Saturation soil moisture? Residual soil moisture? Why? Are you not using soil parameters in the model? Table 1: again, I can not see parameters of the Penman equation for evapotraspiration. Why?

Table 1 is only for the calibration parameters for the GSDW model. Physical parameters were not incorporated in the manuscript because of the limited space. We will add another table in the revised manuscript for the physical parameters used in the model.

10) Page 1460, row 4. the authors wrote that the case studies are in arid and semi-arid regions. Please, can you provide more details on climate for demonstrating it?

The study areas are located in the semi-arid region of Canada where potential evapotranspiration exceeds the precipitation values. Based on the climate data from an Environment Canada meteorological station at Fort McMurray (56o 39 N, 111o 13.2 W), a 30-year period (1971-2000) mean annual temperature was 0.7 °C, and the mean annual precipitation was 455.5 mm.

11) Page 1460, row 21: the authors wrote that "RMSE of the GSDW model is lower than the values obtained from the site-specific SDW model". But I cannot see results of the SDW model in this paper.

Amended in the revised manuscript. The SDW model was recalibrated on 2005 and validated for cover D3 on year 2006, the SDW model MRE was 16% and 6% for valida-

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tion year for peat and till, respectively. The RMSE were 13.5 and 17.3 mm for peat and till, respectively and the simulated cumulative AET was 253.09 mm for the validation year compared to 276 mm of measured AET.

12) More explanation on the systematic errors in soil moisture predictions during DOY 90-160 for all the case studies. The error is large. If the problem is the snow melt, the snow module is not working.

The problem is not the snowmelt module; the reason for this error is that the model does not account for preferential flow. And during the snowmelt period where soil is still frozen, melted snow infiltrates to the soil layers through cracks. This leads to an increase in soil moisture especially in the first layer.

13) I suggest to the authors to use others case studies for testing the overland flow. Indeed they used a case study without overland flow (see Fig. 8). In conclusion the watershed hydrologic model is not really tested. The use of a case study without overland flow was not intentionally selected. In fact this was one of the criteria that were selected in designing the reconstructed site to select a cover that allows for increasing infiltration and minimizing runoff. Referring to the figure 8, the model shows a response of 9 mm of runoff corresponding to 40 mm of precipitation which means that the overland flow module is working. The model is built for semi-arid regions.Wwe could modify the name of the model to reflect that it is for soil covers and for semi-arid regions if this is deemed appropriate. Final note on the contribution of the manuscript: The previous papers (2005 & 2007) were site-specific and only for the SBH site. A strong recommendation from Reviewers of those manuscripts indicated that it will be very interesting to allow the model to handle natural forested sites so that future comparison between natural and reconstructed watersheds is possible. The GSDW model achieved this, simulated a natural sites as well as other reconstructed sites with various properties. Through the leaf area index, the GSDW model is also able to handle different vegetation in terms of type and age. Therefore, the proposed GSDW model is well suited to become an important design, management, and decision-aid tool for watershed recla-

mation in semi-arid regions. In our opinion, the contribution of this manuscript over the previous 2005 & 2007 papers is significant, from both scientific and practical point of views.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 5, 1441, 2008.