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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 comments of anonymous referee #2:

First, we would like to express our deep appreciation to the time and efforts that referee #2 has put in the reviews. The manuscript aims at showing the use of the generic system dynamics watershed (GSDW) model to simulate the various hydrological processes in different reclaimed watersheds. Along this line, the GSDW model was used to simulate a wide spectrum of sites with different soil types, soil stratification, and inclination relying on available meteorological and soil data. Even though we still want to keep the paper within “testing the merit of the developed model to simulate different watersheds” context, we have made significant changes and modifications in the manuscript to address the comments of the reviewer. We do believe and hope that the changes are satisfactory.

1. Conclusions reached are that the model simulates ET and soil moisture "reasonably well" although this isn’t necessarily substantiated by the figures or even the stats. Modelling is a very difficult process and thus, admitting that the modelling is not done all that well does not diminish the utility of this work. But modelling papers should in general show that the incorporation of concepts and upgrades to equations, etc., do or do not improve the representation of hydrological processes. While the simulation of ET and soil moisture were pushed to the forefront, the concepts involved in those parts of the model output are the same in both GSDW and its predecessor SDW. Thus, because this is a modelling paper, it should show how the "upgrade" between SDW and GSDW improved the simulations. Thus, the authors must include the simulations by SDW and show an improvement by GSDW.

Response: The main advantage of the GSDW model is to simulate different reconstructed soil covers, and even testing hypothetical covers to allow for a comparison of different alternatives, of textures, gradients, and thicknesses. There is an on going research now to investigate the potential of the GSDW model for testing hypothetical cover alternatives, and the performance of the reconstructed covers for long term periods. Our aim here was not to make a comparison between the pre-existing SDW model and the upgrade. On the other hand, Elshorbagy et al. (2005; and 2007) tested the SDW model just on the three D-covers, and the simulations where for 2001 as a calibration year and 2002 as a validation year (the covers were newly constructed in 1999). Elshorbagy et al. (2007) noted that large changes in the soil properties (e.g. porosity, hydraulic conductivity) will have occurred during the year 2000. Such an expectation was corroborated by the measured field values of saturated hydraulic conductivity e.g. the saturated hydraulic conductivity for the peat layer increased by 400% between 2000 and 2001, moreover, the vegetation cover started to be established in year 2001. For

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the previous reasons, we tried to simulate the hydrological performance of those reconstructed watersheds when they become well established. This was the main reason for choosing years 2005 and 2006 for the simulation purposes using the GSDW model instead of using the same years previously modeled with the SDW model. Furthermore, the GSDW model includes a canopy module, which did not exist in the SDW model; this makes the use of the SDW model for the simulation of well established sites infeasible. With regard to the use of the expression “reasonably well”, it was not meant to be “shying about the results” but it means that the authors are happy with the results knowing that there is no perfect model. But to completely address the reviewer’s comment, The SDW model was recalibrated on 2005 and validated for cover D3 on year 2006, the SDW model MRE was 16% and 6% for validation 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. Clearly, the GSDW model performs better than the SDW model.

2. Not only should the SDW simulations be conducted and the validation statistics compared to GSDW, but water balance components for each model in the simulations should be provided. Especially the canopy interception model which the authors claim is the primary upgrade. Once these are included, the paper will be an excellent contribution

Response: The water balance components cannot be compared because there are no direct measurements of runoff and lateral flow and thus comparison will not yield any valuable information on the effect of the modifications on the other simulated water balance components. However, a table was incorporated to the revised manuscript of the total ET, and canopy interception values calculated by the GSDW model to show the significance of canopy module in intercepting precipitation for the validation years.

Site Evapotranspiration (mm) Interception(mm) D1 285 42 D2 320 69 D3 285 47 SWSS 360 50 SBH 294 45 OA 385 116

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3. The literature review is fair but leaves something to be desired. On page 1445 the authors describe what Yanful and Aube (1993) did in their lab tests including a "comparison of results"; however, they neglect to state what the results were and how they contributed to the development of the current study research objectives (as all literature reviews should).

Response: Amended throughout the revised manuscript. The following phrase was added: “Yanful and Aube (1993) deduced that long term predictions for evaporation and no evaporation showed clay layer would remain saturated even under prolonged dry periods.”

4. With regard to the paragraph starting on line 10, page 1445, it is noted that Elshorbagy et al applied SDW to "inclined" reconstructed watersheds. It would be useful if the authors detailed the significance of terrain in their model, or perhaps the need to refer to the watershed as "inclined" at this stage and how SDW was extended to simulate "other inclined watersheds". From examining equation 11, it seems to be an empirically based expression and not a physically-based one or even a conceptual one similar to that in TOPMODEL.

Response: The word inclined is omitted in the revised manuscript. However, the need to refer to the watershed inclination is to mention that the model is capable of handling both inclined (e.g. the three D-covers, and the SWSS site), and horizontal terrains as in the SBH, and the OA sites. It should be clear that the SDW model was built as a site-specific for inclined watersheds, but the GSDW model can handle inclined and flat watersheds. For equation 11 it is an empirically based expression and it is a modification of what is found in Elshorbagy et al. (2007).

5. It would be useful if the authors could connect all the model equations to the loops described in figure 2.

Response: The rationale for the causal loop diagram is to illustrate the mutual interaction between different factors affecting the watershed hydrological processes and the signs illustrate what affects what. Also, Figure 2 is already complicated, adding model equations to the loop will add to this complexity.

6. With regard to the natural watershed description, what is the size of the area and the resolution of the digital terrain model used to obtain the slopes? How was information obtained at the sub-daily time step used in the modelling process which seems to be at a daily time step? What were the saturated hydraulic conductivities and pore-size distributions? Since the AET model is an important part of this paper, the primary equation showing the lambda terms should be provided somewhere on page 1454.

Response: For the first part, the OA site is almost 1580 km2. The information was averaged for the modeling process because the GSDW model is one-dimensional (vertical balance) lumped model. The following part was added to the revised manuscript to cover the inquiry about the values of saturated hydraulic conductivity and pore-size distributions: “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”. Also, the equation showing the lambda term is incorporated in the revised manuscript.

7. While the calibration coefficients are listed, it would be useful to list other non calibration parameters that were implemented in the model and affect soil moisture distribution and AET.

Response: The reason of not listing the other parameters because the model sensitivity to them was not significant.

8. The authors speak of "depth-averaging" at the top of page 1461. Please elaborate.

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Response: Every moisture content measurement is multiplied by the corresponding depth (of a particular sensor) to obtain water depth in a layer, total water depth is calculated for all layers/horizons, and the total water depth is divided by the depth of layers/horizons.

9. If SM is as important as AET, why not report AET as daily values like they do with soil moisture. Can they show the daily graphs as opposed to the cumulative graphs?

Response: Daily graphs of AET could be shown as in the case of SM; however, the predictability of the fluctuations in AET in a daily scale is a daunting task because of the land-atmosphere interaction. The variability of simulating AET is influenced by the soil moisture state. Also, numeric models are nonetheless limited to the extent that the parameterizations of physical processes are only approximations of the true action (Entekhabi et al., 1996). AET values are highly dynamic process through the entire day, for the sake of modeling the AET values, the hourly measured eddy covariance (EC) values were aggregated in a daily scale which adds to the data uncertainty. Moreover, as mentioned in the manuscript that the EC method, have an accuracy range ś8 to ś20% (Eichinger et al., 2003; Strangeways, 2003). On top of the uncertainty associated with the model structure, all together make the daily simulation of AET values not always giving realistic values on a daily scale. Moreover, what really affects the long term simulation is the total AET value in each year. Also the soil moisture deficit is a function of both cumulative moisture and AET.

10. Why aren’t the same statistics used to validate the models for soil moisture used to validate the models in terms of AET outside of rain events?

Response: Amended in the revised manuscript.

11. The authors should consider using more than equations 13, 14 and 15. While the authors recognize that certain equations are more affected by peaks than others, they should also include stats that only focus on peaks as long extended drying periods will often improve the values of statistics that simulate the entire time period. The authors

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should also make a comparison of extreme/peak values or values immediately after rain events.

Response: We mentioned in the manuscript that the RMSE tends to produce high error values because it is biased towards the peak values. However, at the same time we used the MARE which is less sensitive to the peak values. To address the referee comment about the focus on peaks, the percent error measurement in the peak (PEP) is incorporated in the revised manuscript.

12. Can the authors please show which years used in the calibration/validation are wet or dry years.

Response: The values of total precipitation for the D-covers, and the SBH site are 341.2 mm and 294.3 mm for years 2005 and 2006, respectively. For the SWSS site, the value of total precipitation is 285.9 mm, and 366.3 mm in the years 2005–2006, respectively. Finally, the same values for the OA site were 479 mm, and 483.7 mm in the years 1999–2000, respectively. Apparently, year 2006 for the three D-covers and the SBH sites was a dry year, while, year 2005 was a dry year for the SWSS site.

13. The title does reflect the contents of the paper but the abstract is neither concise nor complete. The abstract does not include any of the results of the model performance. It sounds like an introductory paragraph followed by a paragraph from the conclusions section. Please revise the abstract.

Response: The abstract has been modified to accommodate this comment

14. Consider the statement made on page 1443 starting with line 15 which reads: "This key role, of both processes, is pronounced in the evolving hydrological behaviour of reconstructed watersheds resulting from the mining industry." While the reconstructed watershed may evolve in terms of the spatial and temporal distribution of AET and SM, I don’t believe it is "pronounced" as compared to the processes in any other type of watershed. The authors should eliminate this word or describe how it is pronounced.

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Response: Amended, the word is eliminated.

15. How is depression storage incorporated?

Response: The GSDW is a lumped model, and the depression storage was not incorporated in the model.

16. Page 1444 line 1 states that "...tool that facilitates the assessment of the sustainability of various reconstructed watersheds." The word "sustainability" has multiple meanings both in the public sector and within the scientific community. The authors need to define what they mean by a sustainable watershed in this context.

Response: Amended throughout the revised manuscript. The following phrase was added as a final goal of the sustainability: “and to develop an interaction between the local flora and fauna as in natural watersheds”

17. The authors continue on Page 1444 (line 6) with statements such as "complex hydrological processes of the reconstructed watersheds". The impression conveyed is that these reconstructed watersheds are somehow more complex than any other watershed. The authors need to describe what is complex about them in relation to any other watershed more specifically, they should detail why reconstructed watersheds in the Alberta Oil Sands have mostly failed and what success, sustainability and failure mean.

Response: In those regions land reclamation is affected by the local climates where potential evapotranspiration is greater than the annual precipitation. Subsequently, the designed soil covers should have the ability of minimizing runoff, and retaining soil moisture for the growing season. So the main target is to design a cover capable of holding water and releasing it as a sponge to fulfill the vegetation needs.

18. On page 1464 the authors state on line 8: "As expected in....the GSDW model shows that the AET process and soil moisture content play the dominant role in the hydrological processes of the watersheds" Dominant over what other processes?

Response: The two processes are dominant in arid and semi-arid regions with insignificant runoff except in the snowmelt period and potential evapotranspiration greater than the annual precipitation. The phrase was edited in the revised manuscript.

19. If canopy interception was the primary upgrade to GSDW, what recommendations would the authors make regarding the role of canopy interception?

Response: The canopy interception module which is the main upgrade from the SDW model, provide the GSDW model the merit of simulating the future performance of the reconstructed watersheds and long term simulation scenarios. Furthermore, adding the canopy module will allow users to compare different vegetation alternatives/scenarios for future reclaimed covers and suggest the best alternative for vegetation based on the soil moisture deficit.

20. Technical comments: There are numerous typographical errors detailed below.

Response: Amended throughout the revised manuscript. The revised manuscript has been edited carefully to address sentence structure and other editorial problems.

21. Page 1452 Equation 9: Should the sign before the fraction term of equation (9) be positive instead of negative?

Response: Positive sign

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

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