Hydrol. Earth Syst. Sci. Discuss., 6, C1250–C1256, 2009
www.hydrol-earth-syst-sci-discuss.net/6/C1250/2009/

Hydrology and Earth System Sciences Discussions

Interactive comment on "A modeling study of heterogeneity and surface water-groundwater interactions in the Thomas Brook catchment, Annapolis Valley (Nova Scotia, Canada)" *by* M. J. Gauthier et al.

C. Rivard

crivard@nrcan.gc.ca

Received and published: 19 June 2009

1. I wonder what would happen if the order of the scenarios would change i.e. first adding the top layers, then refining the bedrock geology. Please discuss this issue.

Response 1: We did not try changing the order of the scenarios, but in the end the final result would be the same. We did not give any "priority" to bedrock as opposed to surficial geology (indeed we started with surface geology in scenario 2; in scenarios

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper

3 and 4 we incorporated the main features of the bedrock geology; in scenarios 5 and 6 we refined the surface geology; and in scenario 7 we refined the bedrock geology). Our strategy was to first include characteristics that we thought would influence most the model response (e.g., adding a surficial cover, defining three bedrock units). Then, known parameters (e.g., porosity) were added.

2. I also wonder how well the simplified scenarios would work if their parameters were all subjected to calibration. And the opposite question: if no calibration is allowed at all.

Response 2: No calibration was performed on hydrogeological parameters (K, n). Scenarios 1 to 7 use local existing and collected information; scenarios 8 and 9 use regional values. A trial and error approach was used for the model parameters common to all scenarios (described in Table 2). Perfect calibration was not the focus of this paper, as we discuss in the Introduction and Model calibration sections.

Specific comments:

page 2752 / line 14 "to a sufficient degree" : define "sufficient".

This phrase in the Abstract is expanded in the Conclusions: “The simulated heads, aquifer recharge, and streamflow at the outlet for scenario 9 were comparable to observed or previously estimated values.”

/ line 16-17 "North Mountain basalts": is it really necessary to refer to the formation name in the abstract? just "basalt" should do, here.

Done.

page 2758 - line 9 "and other criteria" : like?

This sentence has been modified: “The distinction between grid cells belonging to the hillslope and stream network systems can be made according to three different threshold-based options, based on criteria such as upstream drainage area, local terrain slope, and land surface curvature.”

HESSD

6, C1250–C1256, 2009

/ line 12-13 "D8 scheme ... or by ... methods" : which algorithm is actually used?

An addition has been made to this sentence: “Cell drainage directions can be identified by the simple D8 scheme (the method used for this study, whereby one of eight inflow/outflow directions is taken on each cell) or by more recent nondispersive and dispersive methods (Orlandini et al., 2003).”

/ line 20-22 "two different ... schemes allow ..." : they allow updating. But have they been used? which one? or both?

We have added at the end of this paragraph: “The data assimilation feature was not used for this study.”

page 2759 / line 16 "gOcad" : what kind of software is that?

gOcad is a software for the construction of geological models. For more details, see their website at: http://www.gocad.org/www/

page 2760 / line 9-10 "a high end laptop computer" : in 5 years time, high-end computers will have completely different specs. Please add more info on the processor speed, like as defined in flops.

True. We have replaced this sentence with: “A grid of this size required calculation times of several hours for 1-year simulations run on a laptop computer with a 1.90 GHz processor.”

/ line 15-17 : "the input fluxes are [P-Epot]" : How can P-Epot be used as input fluxes? P-Eactual can, but not Epot, which is not a flux but only the maximum possible flux.

The CATHY model treats both P and Epot as “potential” fluxes, and the boundary condition switching procedure determines whether this flux becomes the actual amount of water that gets infiltrated/evaporated (Neumann boundary condition) or whether the actual flux is less than this amount (Dirichlet boundary condition). We have added the following sentence to clarify this issue: “Switching between specified head and

HESSD

6, C1250–C1256, 2009

specified flux boundary conditions occurs at surface saturation (zero pressure head) in the case of rainfall and at the “air-dry” pressure head value in the case of evaporation.”

page 2764 / line 3 : "model calibration" : which calibration technique was used? Which goodness-of-fit criterion was used?

See Response 2.

/ line 5 "K and n were assigned" : a prior estimation on this scale requires some averaging procedure. Has that been done? Also. why haven’t these parameters been calibrated?

Mean values were obtained for K using medians while mean porosities for bedrock were provided by a parallel study using thin sections and image manipulation and analysis techniques (penetration of the dyed mounting medium) and from the literature for surficial units. Their calibration was not the purpose of this paper. We only wanted to see the effect of increasing the geological complexity and of the assignment of local versus regional values.

/ line 7 : "an adequate agreement" : define adequate.

We were interested in obtaining a good or satisfactory visual agreement; the quest for an optimal calibration was not the purpose of the study.

/ line 9-10 : "once obtained, the parameter values ... where kept fixed for all subsequent scenarios" : is this allowed? The parameters are effective parameters, and as such coupled to the model schematisation. When this latter changes, so should the effective parameters.

This would be the case if we sought, for each scenario, an optimal calibration, but this was not our procedure (see also Response 2).

/ line 20-21 : "the match was obtained after 1-2 months" : figure 6 suggests a response time of 3-4 months (recession during july-august-september)

HESSD

6, C1250–C1256, 2009

Interactive Comment

After 1-2 months of summer recession the flow is around 0.1 m3/s, while after 3 or 4 months the flow is about 0.05 m3/s. This provides a rough estimate of the baseflow range (an estimation using hydrograph separation provided a value of 0.08 m3/s).

/ line 27 : "model performance" : how is this quantified?

We compared mean observed and simulated values.

/ line 27 " improves slightly from scenarios 1 to 8" : according to table 3, the streamflow error is smallest for scenario's 4, 5, and 6.

The intermediate scenarios are now discussed in more detail (see Response 4 to Reviewer #1).

page 2766 / line 16 "mismatches could be reduced" : is seems to me that the peaks in the simulated hydrographs are both too large and of the wrong shape. Perhaps part of the catchments is reacting flashy (small but sharp peaks in the data) and other parts are buffering water, reacting much slower (higher baseflow).

We do not think that the peaks have the wrong shape, although the variations within a peak are not always captured. As already mentioned, we did not pursue intensive calibration to improve individual peaks (or other features) for any scenario. An "adequate" match was obtained for scenario 1 and then the parameters described in Table 2 were kept fixed in order to see the evolution on mean annual streamflow, annual recharge, and position of the water table (the three variables for which observed or estimated values were available) as complexity was added to the geology.

page 2767 / line 23 : "adjusting to initial conditions" : why then is the model initialised for january with conditions that resemble summer baseflow?

It may indeed have been better to start with a higher streamflow, but winter is also usually a period with low streamflow.

/ line 29 "suggesting a much greater sensitivity to k" : this is circumstantial evidence,

HESSD

6, C1250–C1256, 2009

and should be tested directly.

We do not think that this is circumstantial evidence; scenarios 7 and 8 for example provide a good illustration of this.

page 2768 / line 26 "what is labeled as Horton ... may arise from shallow subsurface flow ... that saturate" : Sounds like Dunne OF.

What we are describing here is return flow that can saturate near-surface cells while underlying cells remain unsaturated. This is not Dunnian runoff, where the entire vertical profile becomes saturated. It is not exactly Hortonian runoff either (the “classical infiltration excess” mentioned in the paper), but we lump this contribution with Horton runoff because its manifestation is the same (i.e., both result in a saturated surface).

Table 3 : check the use of * ** marks. there are some mistakes. Also in the **100.

Done.

Figures 6, 10, 11: the x-axis tick labels are in French. Why are they on the 5th day of the month?

“05” refers to the year (2005). This has been removed (the year is given in the figure caption) and the labels now give the months in English.

Figure 8: The good fit is partly a result of the topographic gradient. Please provide a similar plot based on groundwater depth (with respect to the surface). (m, ASL) should be (m ASL).

It’s true, the fit would not be as good using GW depths, but an error of 2 m over 5 m is much worse than over an elevation of 200 m. An RMSE threshold in hydraulic head equal to 10% of the difference between highest and lowest heads is often deemed to be quite acceptable.

Figure 9: "Time (days)" should be "Time (day)"

HESSD

6, C1250–C1256, 2009

Done.

Figures 12-14: Longitude and latitude are measured in degrees, not in meters. If you want to use meters, then the labels should be "easting (m)" and "northing (m)"

Done.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 6, 2751, 2009.