

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.

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2a) The article lacks of an explicit “material and method” section, with sound descriptions of the model, the modeling strategy and the catchment, the later being treated as a “study case”. By beginning the introduction with the description of “The Thomas Brook catchment” the authors induce the reader to treat the paper as a “site specific” study. In the introduction the study should be replaced in a more general context, and include a review of other works dealing with the effect on soil heterogeneity modeling

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on water flow at various scale or the way soil heterogeneity is mainly treated in water flow catchment scale modelling studies.

Response 2a: The Introduction has been modified to present a more general context. The first paragraph of the introduction becomes: “The hydrologic response of a small catchment was modelled using a 3D coupled model. The main objective of this study was to investigate the level of complexity required to simulate hydraulic connections and interactions between surface water (springs, overland flow, and streamflow) and groundwater (within unconsolidated sediments and the bedrock aquifer).” The Thomas Brook study area is only presented at the end of the Introduction. The Abstract has been similarly modified. A review of other studies on heterogeneity is already present in the Introduction. Specifically, the paragraph beginning by “The impact of heterogeneity on surface and subsurface processes. . .” is devoted to this topic.

2b) The “Results and discussion” section should be reorganized in order to better illustrate the effect of soil heterogeneity modeling.

Response 2b: This section has been modified. Subsection 5.1 (Model calibration) is now sub-section 4.4 (Model calibration and initial conditions); the section “Results and discussion” now contains: 5.1 Model response 5.2 Effects of heterogeneity (“and other factors” has been removed since the focus is on heterogeneity) 5.3 Catchment behavior for different response variables.

3a) It is unclear in what matter the scenario including snow cover modeling (scenario 9) represents an added value to the issue of heterogeneity modelling and so, unless the authors could discuss it, it should be deleted. Nevertheless, if snow accumulation and melting modeling is a critical issue in yearly water flow modeling, all the scenarios should include these processes.

Response 3a: The addition of snow cover does not represent an added value to the issue of heterogeneity. It was added simply to study the additional effect of the snow accumulation once the model was sufficiently well-represented geologically. The an-

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nual budget is of course the same. We especially wanted to see the effect on monthly recharge. This is why we did not introduce it for each scenario.

3b) Finally, the conclusion of the study would be clearer and stronger if the authors focus their attention on the effect of soil heterogeneity on water flow modeling.

Response 3b: A paragraph has been added in the Conclusions that makes a recommendation on the minimum heterogeneity to be included: “For instance, they [the seven scenarios] showed that the description of subsurface heterogeneity must include, as a minimum, hydraulic conductivity values for the three bedrock formations, because of their strong influence on water table position, and for the various surficial units given the dominant role of the surface cover in rainfall-runoff-recharge partitioning.” 4) Scenarios 4 to 6 are not enough discussed. Some variables seem to be better simulated for high level of soil heterogeneity scenarios but the improvement is less clear for other variables. Could the authors discuss this point?

Response 4: The text of the first paragraph of the new sub-section 5.1 (Model response) has been significantly modified to better describe the various scenarios. This paragraph has been changed to: “The various scenarios show different performance according to the observed (streamflow) or estimated (recharge) variables. In general, model performance based on streamflow improved from scenario 1 to scenario 8, but for recharge, results are variable (see Table 3). Scenarios 4 to 6 produced similar values for hydraulic components, with the best results for mean streamflows compared to observations; nonetheless, recharge was much too high. Scenario 7, with the most realistic representation of the system thus far, nevertheless yielded water levels that were too low in the North Mountain formation and an annual recharge that was too high, at 675 mm (Table 3), likely due to the available local data not being very representative. This was corrected in scenario 8 when regional values were incorporated. When considering recharge, model performance also improved significantly from scenario 8 to scenario 9, after snow accumulation was taken into account (see below).”

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5) Recommendation on the “minimal description” of soil heterogeneity sufficient to assess water flow at the catchment scale would be of great significance and this point should be discussed in the conclusion.

Response 5: See “Response 3b”.

For the specific comments: - part 2 and part 3 should be put together : the “description of the study area” includes the geological context.

We prefer to keep these sections separate.

- p 2755, lines 13-16 : It could be informative to indicate the number of year of surface flow data monitoring.

In section 4.1 (Model implementation; now section 4.2), the following sentence was modified to make it clearer that only one year was used in the model. “Only one year, 2005, was retained because it was the only one containing a nearly complete stream-flow time series on record.”

- p 2755, lines 26-27. For how many year is this average? Could the authors indicate whether the year 2005 is representative of the mean climatic and/or hydrological behavior of the study zone?

Climate statistics from Environment Canada are currently provided for 1971-2000 (30-year period). 2005 is quite typical, with total precipitation for 2005 being 1201 mm/y, while the average over 1971-2000 is 1211 mm/y.

- p 2758 Please indicate the resolution method of the Richards equation : finite difference? Finite element ?

This specification is now provided in section 4 (Hydrological model of the Thomas Brook catchment): “The three-dimensional Richards equation, solved by the finite element method, represents variably saturated flow in porous media, . . .”

- p 2759: lines 1-3 : Could the authors justify your choice?

The 20 m DEM was coarsened to a resolution of 60 m in order to keep the simulation turnaround times reasonably short. .

- p 2759 lines 5-9: figure 5 should be introduced and commented in part 3 (geological context)

Done. Figure 5 is now called Figure 4 and the sentence introducing it has been moved accordingly.

- p 2759 lines 18 – 28. I believe the authors chose a flat base for the bottom of the flow domain to limit mesh complexity. How was assigned the thickness of 50m at the outlet of the catchment? Could the authors discuss these choices and indicate the possible consequences? Wasn't it possible to decrease the total thickness and decrease the mean thickness of the vertical layers? The bottom layer is the only one to have a nonunique thickness. Therefore the maximal thickness of this layer may reach 200m. Is this thickness not too high to support the assumptions of classical Richards equation resolution scheme? In particular it seems from fig 15 that this layer is not completely saturated near the North Mountain cuesta (between latitude 4.9955 and 4.995 106 m). More generally, the quality of the mesh should be evaluated with for example an aspect ratio ($\Delta x/\Delta z$), Calver and Wood (1989), Paniconi and Wood (1993)).

A very thick bottom layer is not normally a problem for simulations that are driven by surface inputs (rainfall/evaporation), provided the total profile being simulated (soil + aquifer) is deep enough relative to the horizontal scale of the catchment being simulated. As for the quality of the mesh, mesh aspect ratios could indeed be important to examine in more detail. However, once again we should expect that the bottom layer should not be too affected by the aspect ratio factor. This factor will be more influential in the near-surface layers, where very fine vertical layers (relative to horizontal mesh dimensions) are typically needed to resolve surface/subsurface partitioning. This issue has not been examined in detail in this study.

- p 2760, lines 11-13: It means that surface catchment and ground catchment have the

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same limit and the same outlet. Is this hypothesis supported by experimental data or observations?

This is a very relevant comment. Indeed, surface and subsurface flows probably very rarely coincide in their flow boundaries, but integrated groundwater/surface water studies must by necessity impose the same “domain” for the entire flow system. Even within the same flow domain, however, this limitation can be overcome, for example by imposing source/sink boundary conditions along selected points or regions of the lateral catchment boundaries. The problem is that it is exceedingly difficult to obtain reliable estimates of water fluxes across these boundaries, as was the case also with our study.

- p 2760, lines 13-22 : To my opinion, Fig 6 and its comments should be included in the description of the study site, as an illustration of the hydrological behavior of the catchment.

Although this would be a good idea for two of the curves, we prefer to keep this figure in Section 4 since the graph of Figure 6 also includes some results (simulated streamflows).

- p 2760, line 24 to p 2761 line 29: These are mostly general information concerning the model CATHY and not specific information relative to your study. This general information should be included in a 4.1- model description and only specific information (flow domain geometry and discretization, boundary and conditions, material properties...) should be kept in a 4.2- model implementation.

Done. A section 4.1 (Description of the coupled model) was created.

- p 2762, lines 2-6: Is it “response parameters” or “response variables”. To my mind, soil properties such as porosity or conductivity are parameters whereas discharge, saturation dynamics, . . . are variables since they are calculated from state variables such as pressure head or soil moisture content.

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“Response variables” is now used in the text.

- p 2762, lines 13-17 and Figure 7: It is not necessary to include conductivity and porosity value in the text. It would be clearer to report those values in a table than to indicate it on figure 7. In Figure 7, soil number should be reported and the boundaries between soils better marked (by black line?).

We prefer keeping the values in Figure 7 so that the figure stands by itself.

- p 2764 §1 and 2: These 2 paragraphs (and part of the following paragraph p2764-2765) mainly present the method of model initialization and calibration and should therefore be included in the 4th part of the article (4.3. Model calibration).

Done. These two paragraphs now belong to a distinct section (4.4 Model calibration and initial conditions).

-part result and discussion : The last paragraphs of 5.1 and section 5.2 should be re-arranged in order to better illustrate the effect of heterogeneity on surface and ground-water flow (the title “effects of heterogeneity and other factors” is not appropriate). The authors should better describe the results of all the scenarios. In particular, scenarios 4 – 5 -6 simulate better the mean outlet streamflow than scenario 8. It could be interesting to plot in a graph the stream-flow simulated by all the scenarios. The section “Catchment behavior for different response variables” is interesting but does not lead to new informations/ideas about the effect of heterogeneity on flow modeling. This section should be removed OR you should compare the difference in “saturation zone” location for the 8 scenarios.

The last paragraphs of the old section 5.1 have been put into the new section 6.1 (Model response for scenario 9). The title of section 6.2 has been changed to “Effects of heterogeneity (“and other factors” has been removed since the focus is on heterogeneity). Scenarios 4 to 6 are now better described (see Response 4). We believe that the section “Catchment behavior for different response variables” (now section 6.3) is

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important to the paper as it provides examples of results obtained for two important variables related to SW/GW interactions: saturation and the water table position.

- p 2765 lines 14-16 / 23-25 : How many well measurements were used? In particular, were all the data for well 1 and well 2 used ? If not, why?

30 wells were visited for GW level measurements over the study area (8 km²). The complete records of wells #1 and 2 were only used for comparison with simulated daily data at the same location in Figure 9. Figure 8 only uses mean water level measurements (observed and simulated). Therefore, Wells # 1 and 2 are contained in only one point each in Figure 10.

- p 2766 lines 6-8: How fluctuate the groundwater level for scenarios 1 to 7? It would be interesting to examine “groundwater variables” especially for scenarios 4 to 6 for which the mean outlet streamflow was well reproduce.

GW levels were much too low in the North Mountain Formation for scenarios 1 to 7. Decreasing K to the regional value helped raising them to an acceptable value. Differences with observations usually decreased towards the southern part of the catchment.

- p 2766 lines 22 – 26: the sentence is difficult to understand and need to be clarified.

This sentence was perhaps too long and has been split in two (and a comma added) to make it clearer.

- p 2767 lines 25-27: Did the authors include recharge value for January in the annual value reported in table 3. If simulated value January recharge are too high because of initial conditions, January should be included in the “initialization period” and the comparison between simulated and observed data should be made from February to December.

Recharge for January is indeed included in the annual value reported in Table 3. We prefer to present annual (12-month) estimates, even if we have to explain that the first month may be affected by initial conditions.

- p 2768 lines 1 – 10: This part of your paper should be enhanced (since it was mentioned in the introduction results in terms of numerical performance.)

Initially, a section on numerical performance was included in the paper. However, the paper was too long and we decided to remove it. We have now removed this last part of the sentence (“...as well as on the numerical performance of the model.”) from the Introduction, as it was a relic from the initial version of the paper.

- Table 3 : correct the number of “*” for the first note and in the formula.

Done.

- figures 2 to 4: roads should be removed. Figure 2 is not clear: what was drawn :altitudes of soil surface as indicated in the text or potentiometric map as indicated in fig.2 caption?

Figure 2 presents GW elevations (a potentiometric map). The reference to Figure 2 was removed from the sentence discussing the topography. We do not think that roads represent a problem in these figures, at least not if the figures are reproduced in colour. They show that the wells are mostly located along roads in this area, and they convey that data was not available in less accessible areas.

- figure 9 : it would be informative to plot daily net atmospheric forcing and/or outlet streamflow above this graph.

This information is already provided in Figure 6.

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