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Hydrology and Earth System Sciences Discussions

Interactive comment on "Groundwater contribution to river flows – using hydrograph separation, hydrological and hydrogeological models in a southern Quebec aquifer" *by* M. Larocque et al.

Anonymous Referee #1

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General

This paper is of potential interest for HESS. It aims at a better estimation of baseflow amounts. The authors argue that by combining three different methods (a lumped conceptual hydrological model, a groundwater flow model and hydrograph separation) a better distinction between aquifer contribution and shallow subsurface contribution to base flow can be made.

The paper is in general well written and organized.

My main concern is that the principal claim that is made in this paper, is not sufficiently supported by the modeling results. The baseflow which is estimated by the groundwater flow model (and which is clearly lower than the baseflow estimated with the other two methods) depends strongly on the calibrated hydraulic conductivity values. The base flow to the river is the result of hydraulic head gradients and hydraulic conductivities. The authors argue that after calibration the hydraulic heads are reproduced reasonably well, so we might assume that the hydraulic head gradients are also reproduced well. However, the hydraulic conductivity which was calibrated by trial and error, depends strongly on the (fixed, but uncertain) recharge rate. The authors clarify that recharge rate is quite unsure for the area, so we can expect also a considerable uncertainty with regard to hydraulic conductivity. The authors do not consider this uncertainty as their trial-and-error calibration will not provide an uncertainty for the calibrated hydraulic conductivity. A standard tool for automatic calibration with MODFLOW (PEST) could help here. An additional complication is that heterogeneity is not taken into account, which could also affect the results. Altogether, my concern is that the estimated baseflow with help of MODFLOW (around 3 m3/s) is subject to considerable uncertainty. If this is the case, it is also not possible to make such a clear distinction between baseflow and shallow subsurface flow, as the authors claim. Therefore, in my opinion it is necessary that the authors clearly demonstrate that their study is not affected so much by the mentioned uncertainties. This would imply that they would repeat the calibration for different scenarios. One possibility is that they calibrate the groundwater flow model for different recharge values. It is recommended that an automatic calibration is carried out, for example with help of PEST. It would also be desirable that the authors would make some check to see whether spatial variability of hydraulic conductivity could have an important impact on the results.

The paper should also be strengthened on the rationale behind the comparison of the three methods. The discussion of the results of the three methods is rather speculative,

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and this leaves the reader with the feeling that in fact we learn not so much from this comparison.

Main comments

Page 7813, Line 3. Which period for the extremes? Were the extremes also from the period 1990-2004?

Page 7813, Line 5-6. Do both zero temperatures band snow accumulation extend from early December until the end of March? I guess that the authors talk here about snow cover. Please specify.

Page 7815, Line 18. Why was potential evapotranspiration calculated by this method? Not enough data for Penmann-Monteith?

Page 7815 line 18 until Page 7816, line 10. This would be clearer introducing some formulas.

Page 7816, line 25. This is relative. The processes are not represented so physically by this model....

Page 7817, Line 5. Could you specify what this means and be more specific?

Page 7817, line 12-13. Calibration with a trial-and-error procedure has severe disadvantages, one of them is the fact that the best fit will not be found. The uncertainty of the estimates will also not be quantified. It is highly recommended to use a more sophisticated procedure.

Page 7817, line 12-13. The calibration result will be affected by the predefined recharge rate value, which is also unknown. The uncertainty of the recharge rate should be taken into account in the analysis.

Page 7817, line 20-21. Are the ratios of horizontal and vertical hydraulic conductivities also calibrated?

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Page 7818, line 1-7. The authors admit that there is considerably uncertainty with respect to recharge rate. The question is how this affects the calibrated K and baseflow values. It is also evident that a transient groundwater flow model with transient recharge values would be able to reproduce a temporal variable baseflow.

Page 7819, line 10. Introduce NSC.

Page 7820, line 7. I guess this should be mean absolute error instead of mean average error.

Page 7820, line 5-14. This evaluation is only made on the basis of the reproduction of hydraulic head data at the observation locations, without any verification. This limitation should be acknowledged in the manuscript.

Page 7821, line 21-23. This information comes out of the blue. I propose skipping this sentence.

Page 7822, line 17. This procedure is not correct. The calibrated hydraulic conductivity is determined by the input recharge rate. If recharge rate is varied over the period 1990-2004, a good check would be if the groundwater flow model (with the calibrated hydraulic conductivities) would reproduce well time series of hydraulic head values for the period 1990-2004.

Page 7824, line 20-21. I doubt that the distinction can be made with the groundwater flow model.

Minor comments

Page 7811, Line 6. “outcrop in rivers”. Is this a common way to describe exfiltration from an aquifer in a stream?

Page 7819, line 16. (. . .) tends to overestimate (. . .)

Page 7819, line 18. (. . .) and higher air temperatures at the end (. . .)

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Page 7819, line 22-23. Change to: evapotranspiration.

Page 7821, line 19. “Reducing” instead of “reduce”.

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