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Hydrology and Earth System Sciences Discussions

Interactive comment on "Thermal management of an urban groundwater body" *by* J. Epting and P. Huggenberger

Anonymous Referee #1

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Epting and Huggenberger state in the title of their manuscript that they intend to deal with a management concept for the sustainable thermal use of an urban ground water body. "The concept is designed to be applied for shallow thermal ground water use and is based on (1) a characterization of the present thermal state of the investigated urban groundwater body; (2) the definition of development goals for specific aquifer regions, including future aquifer use and urbanization; and (3) an evaluation of the thermal use potential for these regions." In order to support their concept, they conducted various investigations: a) analysis of existing head and temperature data; b) installation and one-year measurements of 4 new multilevel observation wells; c) modeling. Finally, "management strategies for minimizing further groundwater temperature increase, targeting "potential natural" groundwater temperatures for specific aquifer regions and

exploiting the thermal use potential are discussed".

The concept as described is of relevance and would be suited for publication in HESS.

However, I have a series of comments to Epting and Huggenberger:

Although the authors stated that "a preliminary analysis of existing temperature time series revealed that meaningful data interpretation is difficult", they analyzed, presented and interpreted data of old observation wells (OW1 to OW5 in Fig. 5). The reader is confused.

The authors write that "The effect of lost heat from the canalization and the district heating network was neglected as these objects mainly lie in the unsaturated zone". It is not clear why these heat sources could not potentially influence ground water temperature.

The authors describe longitude and latitude of their site as "7°35' N, 47°32'W". This point lies within the Atlantic Ocean.

The calculation of the areal ground water recharge rate by percolating meteoric water as presented looks extremely crude. Is there any justification for the approach?

Boundary conditions are described for the river boundary, the lower boundary (quasi-impermeable stratum), the North-Western boundary (no flow and Cauchy type), Southern boundary (Dirichlet boundary for head and temperature), injection of warm water. The thermal river boundary condition remains unclear or completely undefined. For the upper boundary only the recharge rate is prescribed. In an unclear section about thermal input of heated constructions only degree-day-factors are evaluated, which is calculated only for large buildings. Nothing is written about thermal input rates from buildings. Is there any rate introduced in the model? This point remains unclear. Moreover, nothing is described about a seasonal thermal boundary condition at the complete soil surface. Can it be that the authors did not consider a thermal boundary condition at the soil surface, which is, from their data, not very far away from the ground water table? Can it be that the soil surface is modeled as a thermal insulator except may be the thermal input by large constructions? From the text I have to assume so. Anyway, I would expect a precise justification for the procedure. Otherwise I would not trust the model results.

The flow and heat transport model was calibrated for the year 2010 using all head and temperature data (old and new observation wells). Apparently, the authors did just data fitting without testing the reliability of their model with independent data. How the authors calibrated their model and which parameters they calibrated remains unclear. For thermal parameters literature data was taken and reference to pumping test data is given. From the calibration results we can see that errors are in the order of the yearly fluctuations of the temperature. Obviously, the heating effect by the buildings is approximately met in the four new observation wells. However, one of these wells is located at the Southern (Dirichlet) boundary and should therefore meet the measurements anyway. Nevertheless, the modeling section remains to a large extent unsatisfactory and unclear.

An important point in their modeling is the evaluation of the so called “potentially natural state”. However, this important point is not treated in this manuscript at all and it remains unclear how it is determined. The authors refer to a submitted other article, which is not (yet?) available. Obviously, according to the local regulation in their country the deviation from this “potentially natural state” is limited to 3K, which represents key information for any thermal ground water management. Anyway, this limit is already exceeded today in certain areas of the aquifer. Where is now the management strategy as stated above? It looks that there is mismanagement concerning the thermal use of the aquifer, despite the fact that temperature time series were available already back to 1994.

From the simulated maps in Figure 5 it can be strongly presumed that the thermal distribution is dominated to a large extent by Dirichlet-type boundary condition for head and temperature at the Southern boundary, which is obtained from interpolated data.

Therefore, it is not astonishing at all that the model results are relatively close to measurements. The results do not clearly prove that the model works properly. Already the measured data would obviously violate the local regulation about the “potentially natural state”.

The authors mention several times that they also investigated river-ground water interactions. However, this is restricted to the interpretation of one new multilevel observation well relatively close to the river. For the reader it is difficult to assess this point since no river data are shown. Moreover, no comparison with modeling results is presented.

Based on the model new locations of thermal use are introduced. How these locations were selected is not clear. Again I miss a clear management strategy as promised above. Nevertheless, the impact of the new facilities is modeled and discussed, again with respect to the temperature change compared with the (unclear) “potentially natural state”. The authors see possibilities for a substantial thermal use for space heating, since some of the investigated aquifer domain is already now too warm. This is obvious, already from looking at the data. They estimated the heat potential accordingly. What I miss is the evaluation of a long term energy rate (per year) which can be used, not just heat mining considerations.

From the management concepts the authors as described in their abstract point (1) (characterization of the present state) is met by the measurements and their presentation. However, point (2) (definition of development goals) is to a large extent missing in the text. Point (3) (evaluation of the thermal potential for the region) is restricted to the estimation of the amount of energy stored in the obviously ‘too warm’ aquifer. From the tools the modeling part is quite unsatisfactory.

After all, I wonder what is really new in the contribution. The model is very conventional (and to a large extent unsatisfactory and unclear) and the ground water management part is quite poor.

Therefore, I would not accept this contribution in this form.

Further specific remarks:

* p. 7191, 7214, 7215: A very funny expression: “Garbage incarnation facility”. Obviously this is a typo.

* Fig. 5: Since it the result of a 3D model it is not clear what is shown in Fig. 5, mean temperature or temperature at a specific level? The same holds true for Fig. 6 to 10.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 9, 7181, 2012.