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.

- ➔ One reason for also interpreting the data from the conventional old observation wells was to make some general conclusions of the regional thermal settings and discussing long-term thermal development observed in different regions within the investigated groundwater body. Long-term data sets only are available from conventional old observation wells.
- ➔ As the installation of multilevel observation wells is expensive only four wells could be installed. Here we focused on investigating some relevant processes in detail.
- ➔ We could include the following in the final revised paper: *For the interpretation of groundwater temperature two types of data sets were available: (A) temperature data that are measured additionally to groundwater head within 27 conventional observation wells; and (B) high-resolution data from four multilevel observation wells. Although data of type A allow to describe the regional thermal settings and to discuss long-term thermal development in different regions of the groundwater body, the data are not collected systematically. Furthermore, the conventional observation wells are constructed and instrumented non-uniformly which impedes the comparability of the different temperature time series. In contrary data of type B derive from uniformly constructed and instrumented observation wells, which provide more consistent temperature data. However, in respect of financial expenses only for four locations data could be monitored and to date no long-term data sets are available.*

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.

- ➔ We agree with the reviewer: In the current model setup, the effect of dissipated heat from the public sewer systems and the district heating network was not accounted for. As most of these objects are mainly located in the unsaturated zone, a direct and instantaneous influence on thermal groundwater regimes was neglected. As local effects on the groundwater temperatures have to be expected, more sophisticated model setups will incorporate such objects to investigate the sensitivity on the system in future. However, currently the data are not readily available (network locations and especially depth).
- ➔ One conclusion we draw in our JH paper, in which we present a complete analysis the data of the multilevel observation wells, states: "…natural and anthropogenic disturbances dominate thermal groundwater regimes within groundwater saturated zones of heterogeneous gravel aquifers and high groundwater flow velocities. Preferential thermal propagation is very heterogeneous and intensified in more conductive coarse fluvial deposits. This implies that standardized tautochrones which describe seasonal penetrations of temperature fluctuations by subsurface temperature profiles have to be handled with care within groundwater saturated zones".

The authors describe longitude and latitude of their site as "7_35' N, 47_32'W". This point lies within the Atlantic Ocean.

- ➔ Should be changed in the final revised version to *"47°33' N, 35°33'E*

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?

- ➔ Quantitatively areal groundwater recharge by percolating meteoric water is compared to other boundary conditions (areal groundwater inflow, river) negligible. This also could be observed during the calibration process and the sensitivity of this parameter. For details of percolating meteoric water in the region of Basel visit:
  http://www.lubw.baden-wuerttemberg.de/servlet/is/18644/
  (p. 38-41, in French and German)
  Currently this is the best data set available for groundwater recharge. The values of 1/3 and 1/30 derive from this report.

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.

- ➔ The thermal river boundary conditions are defined as Cauchy boundary conditions by daily values of river water temperature and a calibrated heat transfer rate (see p. 7190 l.15-21; table 1). Unclear what further information is needed.

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.

- ➔ Information about construction depths was only available for large buildings. Temperatures derived from the degree-day method were included as Dirichlet boundary condition for the building structures in the subsurface. The following text could be included in a final revised version at the end of section 3.2.7: *The derived progression of heating temperatures (upper imbedded subplot Fig. 9) was included as Dirichlet boundary condition at the building locations.*
- ➔ Furthermore the sentence in l.3-4 could be amended by: *The areal extent of buildings was considered for the mesh generation and construction depths are represented by selecting the appropriate layers.*

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 authors agree that some information is missing (section 3.2.1, l. 8-10): *Areal groundwater recharge by percolating meteoric water and the upper thermal boundary condition at the soil surface (air temperature measured 5 cm above ground) are derived from the Basel-Binningen meteorological station.*
- ➔ Through the soil water and heat transport from the surface to the groundwater table and vice versa is controlled by the calibrated heat transfer rates which were calibrated (Fig. 4).

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.

- ➔ The groundwater flow model and the distribution of horizontal and vertical hydraulic conductivities as well as river bed conductivities were calibrated and optimized several times during the different construction phases of the tunnel highway (Epting et al., 2008a, b). As the calibrated values resulted in very good to good modeling results (observed and calculated heads and water budgets) even at considerably different hydraulic (flood and drought events) and operational (massive construction site drainages) boundary conditions the authors are quite confident in the calibrated hydraulic parameters.
- ➔ Unlike hydraulic parameters, thermal parameters do not vary in magnitude. Therefore literature data were considered. Heat transfer rates in the unsaturated zone and in the river bed were inversely calibrated.
- ➔ Concerning observation well IV: Yes, this well is located near to the southern boundary which is defined as Dirichlet boundary, consequently the validity is limited. We included

well IV for a complete picture of the four multilevel wells. Should we include a remark in the text or take out subplot IV (editor decision)?

➔ Only a few measurements at the model boundary were transferred to boundary conditions (3 conventional + 1 multilevel) and used for calibration. The remaining measurements (24 conventional + 3 multilevel) independently show calibration results by comparing observed and measured groundwater head and temperature data.

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.

➔ See general comments.

➔ In our JH paper we present how we derive a potential natural state: “The calibrated “present state” model (2010) served as the baseline for scenario calculations. To obtain a “potential natural state” all anthropogenic boundary conditions were removed from the “present state” model (2010), leaving only the natural boundaries (atmosphere, including groundwater recharge and surface temperatures; the River Rhine, including river head and surface water temperatures; as well as the basal heat flux). The “potential natural state” represents the thermal groundwater regime under undisturbed (pre-exploitation) conditions and is comparable to the situation in undisturbed regions outside of the city”.

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.

➔ This is right and also formulated in the discussion. Currently the urban groundwater body is mismanaged; further extraction of groundwater for cooling should be avoided. However, there is a big potential for using the additional heat from groundwater. Sustainable management strategies should focus on using this thermal potential.

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.

➔ Why? Measured data are completely independent of the defined model boundary conditions. Only a few measurements at the model boundary were transferred to boundary conditions (see above).

Already the measured data would obviously violate the local regulation about the “potentially natural state”.

➔ This is right (see above). The now available tools allow quantifying and localizing these violations.

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.

➔ River-groundwater investigations include:
  - The discussion of results from multilevel observation well I including the stratification of infiltrated river water.
  - The derivation and discussion of transfer rates for the river bed.
  - Zonation of the river bed according to riverbed sediment depositions and influence of the sheet pile wall at the river board.

➔ What is meant by: No comparison with modeling results is presented? Calibration results are discussed in Fig. 4.

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.

➔ Concerning the location of new thermal groundwater use: The whole investigated groundwater body is covered. Further emphasis was placed on locations up- and down-gradient of existing groundwater users.
➔ Management strategies are discussed in 4.6.

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.
➔ See comments above.

What I miss is the evaluation of a long term energy rate (per year) which can be used, not just heat mining considerations.
➔ The thermal energy rate that arrives at the southern model boundary (down-gradient of the southern strongly urbanized areas) represents a long-term renewable energy resource. In case no other thermal use takes place up-gradient flow and thermal budgets (calculated through defined transects; Fig. 5) are comparably constant. The calculated nominal geothermal heating resource (Table 3) is representative for the whole investigated groundwater body. At other locations within the groundwater body temperature elevations or groundwater flow velocities (zone C) are too low.

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.
➔ See comments above.
➔ Point (2) and (3) could be merged and relativized to: (2) *the evaluation of potential mitigation measures and the provision of tools for the future thermal management of specific regions within the investigated groundwater body.*

Further specific remarks:
* p. 7191, 7214, 7215: A very funny expression: “Garbage incarnation facility”. Obviously this is a typo.
➔ Should be changed to: *waste incineration plant*

* 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.
➔ The modeling results from Layer 11 are shown, this layer lies approximately in the middle of the groundwater saturated zone and therefore should be most representative. This should be included in the various captions.