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Interactive
comment

Interactive comment on "Recent trends of groundwater temperatures in Austria" *by* Susanne A. Benz et al.

Anonymous Referee #1

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This paper addresses an important and interesting topic regarding the influence of atmospheric warming on groundwater temperature (GWT) in shallow systems. The authors used temperature records from 229 wells located in Austria and climatic data from weather stations installed nearby the wells. The positioning of the paper within the framework of studies devoted to the impact of climate change on hydrological system is well presented. The authors found that nationwide temperatures of groundwater increase and correlate statistically well with surface air temperature (SAT). Additionally, authors have used linear and step-wise models to describe the evolution of temperatures. Based on the step-wise approach (which seems to be more accurate than the linear model) the authors have identified that groundwater respond to climate regime shifts with sudden increase in temperature. This paper has been carefully prepared

and is well written. The conclusions will definitely trigger the attention of the scientific community and the readers of HESS. Nevertheless I believe that some points need to be clarified before publication.

General comments:

I. Some aspects of the methodology are not clear or absent. More details on how the 229 wells investigated in this study have been selected is required. More information regarding the type of sensors used to monitor GWT would be helpful to appreciate the quality of the data analyzed. More information regarding the regression approaches is also needed. How the shifts in regimes are determined in the step-wise model (mathematically speaking)? I also raise some additional points regarding the methodology in the specific comments.

II. I believe that there is a discrepancy between the original objective of the paper, which aims at highlighting impact of climate change at regional (country) scale (Line 12), and the description of potential local effects for (some) specific wells and locations. Indeed, the authors describe potential factors which could explain uncorrelated data locally. Local information that are made available to the reader are to my opinion not sufficiently detailed to support the arguments. The conclusions are consequently difficult to trust. I would recommend to separate the description of local factors from the result of the regional statistical analysis (which to my opinion constitutes the novelty of this study). The local impacts could be introduced in a separate discussion section. In this specific section, the authors could provide an exhaustive list of potential factors that could explain uncorrelated data along with some examples from specific sites to illustrate the hypothesis.

III. I believe that the conclusions of this paper could be strengthened by performing a more robust multivariate statistical analysis (Principal Component Analysis for example) considering more factors which might have an influence on GWT, integrating not only SAT but also geology, land cover evolution, water level variation, precipitation,

population dynamic, length of the temperature time series. . .

Specific comments:

Line 68: ". . .over decades". Please be more precise here.

Line 73: ". . . step-wise increases between the regimes". This is not clear to me. What regimes? Please clarify.

Figure 1 b. needs clarification. The presence of 3 curves is confusing. Could you, for example, make the inner percentile filled with transparent colors?

Line 98: How the wells have been selected? What proportion of wells has been excluded from the database? See general comment.

Line 128. Please clarify why you choose 1994 as initial time for fitting.

Line 129: Knowing which software you used is not informative here. . .

Line 132: Please justify the choice of using the Spearman correlation coefficient and provide references.

Line 133: Taking annual mean values calculated with 8 months of data only may introduce some bias. . . Considering only years with full year of data would be more robust to my opinion. Otherwise, please discuss the limitations in the text. It is also not clear why yearly averages are used in the correlation analysis while the linear regressions are performed on monthly mean temperature (Line 129).

Line 132-136: It would be interesting to perform complementary correlation analysis accounting for other parameters such as depth of the wells, depth to the water table, geology, vegetation and land use. This could be assess with multivariate methods such as PCA. This could add valuable picture of the factors influencing the results.

Line 145: "Breaks within the data were filled using linear fit". This is not clear. . . Please provide more information why you have to fill gaps for this analysis (and not for the

other analysis?).

Lines 160-164 and Figure 2a and b: This part require clarifications. As the authors stated, it seems that the shape of Austria (political boundary) might influence the results. Also the topography, with E-W strike orientation, might also have an influence. It is not so surprising that the correlation is better E-W that N-S (same latitude and orientation of topography). I am wondering if the figures are really informative. . .the decreasing correlation with distance in the figure a) is not obvious with the sharp increase at 550 km. . . Does this distance correspond to a decrease of the number of wells considered in the calculation?

Figure 3 is interesting but difficult to read. Would it be clearer if you display the relative change in temperature for all the wells? What are the p values here (not introduced in the text)?

Line 175 -176: To what coefficient are you referring to? The p values in the figure 3?

Lines 187-190: Here it seems that the length of the time series is critical in the interpretation of the correlation analysis. . . Please discuss this point.

Table 1: What does p-value mean here? Not introduced in the text or the caption. . ..

Lines 205-206: Reference to table is missing. It is actually not a big difference of correlation coefficient 0.36 vs 0.24. . . The comparison with population density is not obvious to me from these values. Please clarify. The influence of city center and development of urban area is actually critical. Could it be possible that the increases in temperatures are partly related to urban development? Identifying the correlation with such factors could be assessed with a multivariate correlation methods (PCA).

Line 214-215: This difference in average changes in temperature with higher values for GWT than SAT is surprising. . . Could it reflect the effect of urban development or other anthropogenic activities (pumping, injection, heating system. . .).

Line 226: Please provide a reference to the figure supporting the statement that spatial

pattern of temperature changes is visible. . .

Lines 226-235: Too few information are available on the effect of this flood event. What was the difference in temperature between the river and GW during the event? Did it cover the entire well area? Estimated volume? Please provide more information or I would recommend to remove this paragraph.

Line 236-249: It is somehow surprising and confusing how local effects are introduced again. . . I believe that it should be discussed in a dedicated section discussing potential hypothesis that may explain uncorrelated data with eventually some examples of local factors from specific sites as examples.

Line 247-249: Do you mean that the hot springs appeared suddenly?... I imagine that they were active before and constitute a constant temperature boundary. . .

Line 262: I do not understand what the authors mean by "spatial median annual mean". . . please clarify.

Lines 263 - 266: I am confused here. How do you explain that the shift in GWT occurs earlier than for the SAT? If the "CRS method (do you mean step-wise method) cannot be used to determine the precise timelag between GWT and SAT" why do you use it?

Technical corrections:

Line 29-31: Reference is missing.

Line 58: Reference style for Menberg et al. (2014).

Line 72: Check reference style.

Line 128: should be "Equivalent to the work by Lee et al. (2014)".

Labels of figure 2b could be changed by Northing and Easting.

Figure 6. Please add legends to your figures.

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Discussion paper