Tsypin and colleagues have submitted a manuscript on evolving subsurface thermal regimes, with the key novel contribution being the role of changing recharge. They conclude that advection plays a role in general, but that changing advection (e.g. more cold-season recharge) cannot overwhelm the overall pattern of groundwater warming from diffusion. The study is generally well written and interesting.

We would like to thank reviewer#1 for their positive feedback on our study, as well as for the in-depth review and constructive suggestions. In what follows, we provide our point-by-point responses to each comment (our answers are marked in red).

A few comments, in no particular order:

1. Many times in this manuscript the authors use 'temperature' to refer (I think) to surface air temperature; such examples can often be found when referring to climate data. But in a manuscript focused on groundwater temperature, they should always be clear about where in the earth system domain they are referring to when they mention temperature.

We thank reviewer#1 for this comment. In the corrected version of the manuscript, we will specify the type of temperature being referred to.

Reviewer#1 correctly points out that in some instances, especially in the context of the weather generator and recharge modeling, we refer to near-surface air temperature, measured at 1-2 m above the ground, as simply „temperature". This indeed may be confusing, as the manuscript primarily focuses on subsurface temperature. In our study, we used the same near-surface air temperature both for calculating potential evapotranspiration and as the upper boundary condition for advective-conductive heat transport.
Regarding top boundary conditions, we have come across various approaches: assigning surface air temperature directly (Bense and Kurylyk, 2017), using surface air temperature with an offset correction (Bense et al., 2017) , using shallow soil temperature (Benz et al., 2024), or using land surface temperature from remote sensing (Casillas-Trasvina et al., 2022). We elaborate on our choice for using near-surface air temperature later in this reply (Question 7c).

2. The authors miss some quite related studies from the Netherlands on the role of groundwater flux on subsurface warming. For example Bense et al. (10.1029/2019WR026913) show how *changes to groundwater flow regimes* can be inferred from temperature profiles (a close topic to the present manuscript although this former study did not forecast into the future), and Bense et al. 2017 (10.1002/2017GL076015) showed that recharge influences where the inflection point occurs in evolving temperature profiles (see L542, where I think a conference abstract is cited for this point). Also, Figure 9 of the present study shows deep temperature shifts when the flow regime changes. This was also demonstrated by Bense et al. 2017 (10.1002/ 2017WR021496) who showed that using a linear temperature-depth profile as an initial condition results in deep shifts in temperature with time because that initial condition doesn't have the same groundwater flow regime as the forward modeling (e.g., Taniguchi et al. 1999 solution). Side note: I think the authors could do a better job of explaining that groundwater flow regime shifts are what is driving

the temperature changes at depth in some of their results (which could not happen under heat diffusion alone under the timescales considered here).

We appreciate the suggestions of the related studies. We were surprised to realize that we originally didn't cite any works of Victor Bense, despite being familiar with them.

Bense et al. (2020) is a valuable study, as it includes a rare dataset of repeated deep temperature profiles and demonstrates temperature changes due to shifts in groundwater dynamics rather than due to heat diffusion, though the described groundwater flux changes were caused by groundwater abstraction, not due to climate change (see also recent example by Klepikova et al. (2025)). A higher impact of multi-year pumping than of ongoing climate-driven recharge reduction on groundwater dynamics was also shown for the study region by Tsypin et al. (2024).

Monitoring the depth of the inflection point proved to be a powerful tool for quantifying groundwater flow rates in Bense and Kurylyk (2017). Unfortunately, the vertical resolution of our regional 3D groundwater model does not allow to simulate changes in the inflection point depth with time.

For the calculation of the vertical groundwater flux rates from temperature profiles we cited the classic works of Stallman (1965) and Bredehoeft and Papadopulos (1965). Bense et al. (2017) presents a major advancement, focusing on both transient effects of temperature and groundwater regime. In our study, we account for this by including recharge and surface air temperature history since 1950 to simulate current and future groundwater dynamics.

Regarding the side note, we think that Figure 9 in the manuscript effectively conveys the relationship between flow regime (expressed as downward groundwater flux) and temperature changes. We agree that heat diffusion alone cannot drive these changes, but the opposite is also true: changes in only recharge/advection rates without surface temperature shifts are also not sufficient to explain magnitude of temperature changes in the subsurface.

We will include some of these references in the revised manuscript.

3. Figure 1b - it is stated that this is a 3D view of the model mesh. However, neither the mesh nor the model parameters are indicated (from what I can see). This is just a geological model (or perhaps geometric model of the model domain). Much is made of the geology in this paper (rightfully so), but I think it would be useful to just indicate the parameters on this model domain (at least k and Ss - or put in a clear table). If a table, the thermal properties for each layer would be useful to list too (in the main text).

Figure 1b was a screenshot of the FEM mesh, colored with different material regions (i.e., stratigraphic units). The low image resolution in the preprint and the small size of mesh elements makes mesh geometry barely visible. Only the mesh refinement along rivers can be recognized. We will make sure that it will have higher resolution in the corrected manuscript (Figure 1).

Instead of indicating individual parameters on the model domain (this would be not practical, given their number), we chose to color the model by stratigraphic layers and provide Table A1 with the thermal and hydraulic properties of each layer, enabling a straightforward mapping. Additionally, the model can be downloaded in the linked data publication.

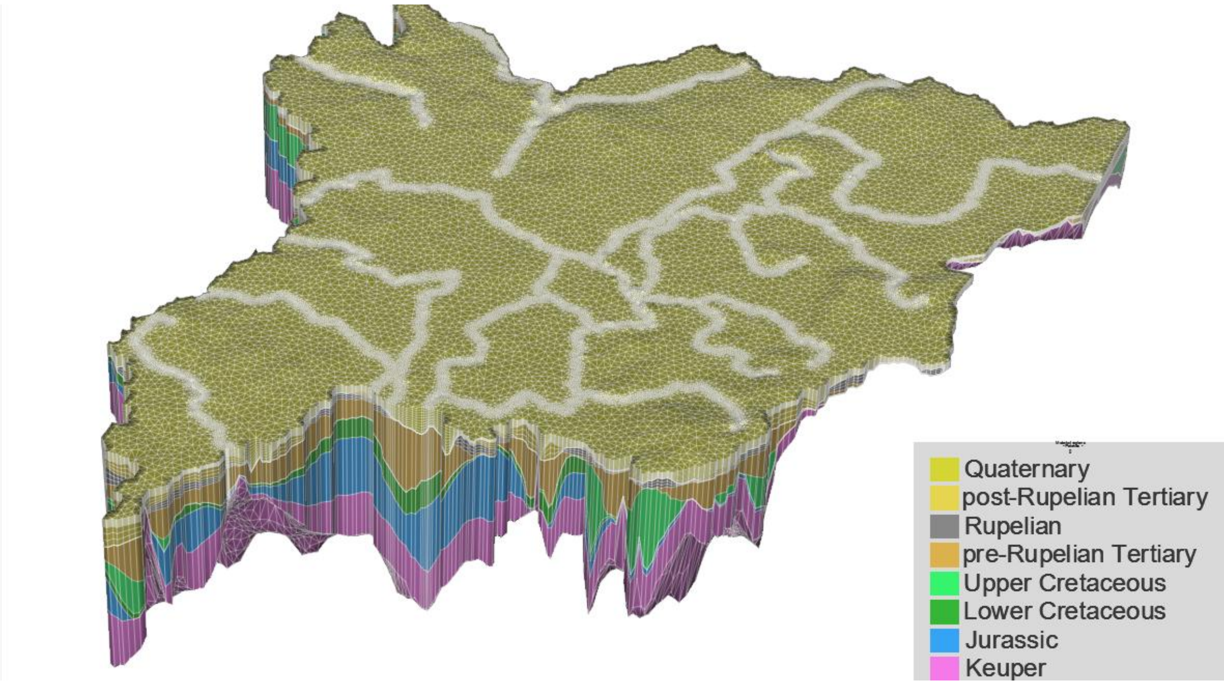

Figure 1: 3-D view of the FEM mesh.

Table 1: Hydraulic and thermal properties of the groundwater model units: K – hydraulic conductivity, φ – porosity, S_y – specific yield, S_s – specific storage, C – volumetric heat capacity, λ – thermal conductivity, and Q_r – heat production. Properties were kept identical across scenarios to isolate climatic controls and were taken from previous studies (Scheck and Bayer, 1999; Noack et al., 2013) except Quaternary and Tertiary post-Rupelian units, which were subject to calibration. Note that the original manuscript wrongly had a minus sign shown for thermal conductivity values.

Unit	K_{xy} [m s^{-1}]	K_z [m s^{-1}]	φ	S_y / S_s [m^{-1}]	C [J m^{-3} K^{-1}]	λ [W m^{-1} K^{-1}]	Q_r (W m^{-3})
Quaternary	2.1×10^{-4}	1.2×10^{-7}	0.23	0.1 / 1×10^{-4}	1.7×10^{6}	1.5	7.0×10^{-7}
post-Rupelian Tertiary	5.8×10^{-7}	5.8×10^{-8}	0.23	/ 1×10^{-4}	1.7×10^{6}	1.5	7.0×10^{-7}
Rupelian	1.2×10^{-9}	1.2×10^{-10}	0.2	/ 1×10^{-4}	1.8×10^{6}	1	4.5×10^{-7}
pre-Rupelian Tertiary	1.2×10^{-4}	1.2×10^{-8}	0.1	/ 1×10^{-4}	1.7×10^{6}	1.9	3.0×10^{-7}
Upper Cretaceous	1.2×10^{-6}	1.2×10^{-7}	0.1	/ 1×10^{-4}	2.3×10^{6}	1.9	3.0×10^{-7}
Lower Cretaceous	1.2×10^{-6}	1.2×10^{-7}	0.13	/ 1×10^{-4}	2.3×10^{6}	2	1.4×10^{-6}
Jurassic	1.2×10^{-6}	1.2×10^{-7}	0.13	/ 1×10^{-4}	2.2×10^{6}	2	1.4×10^{-6}
Keuper	1.2×10^{-7}	1.2×10^{-8}	0.06	/ 1×10^{-4}	2.3×10^{6}	2.3	1.4×10^{-6}
Water					4.2×10^{6}	0.65	0

For reviewer's #1 reference we show the 3D distribution of some properties below (Figure 2).

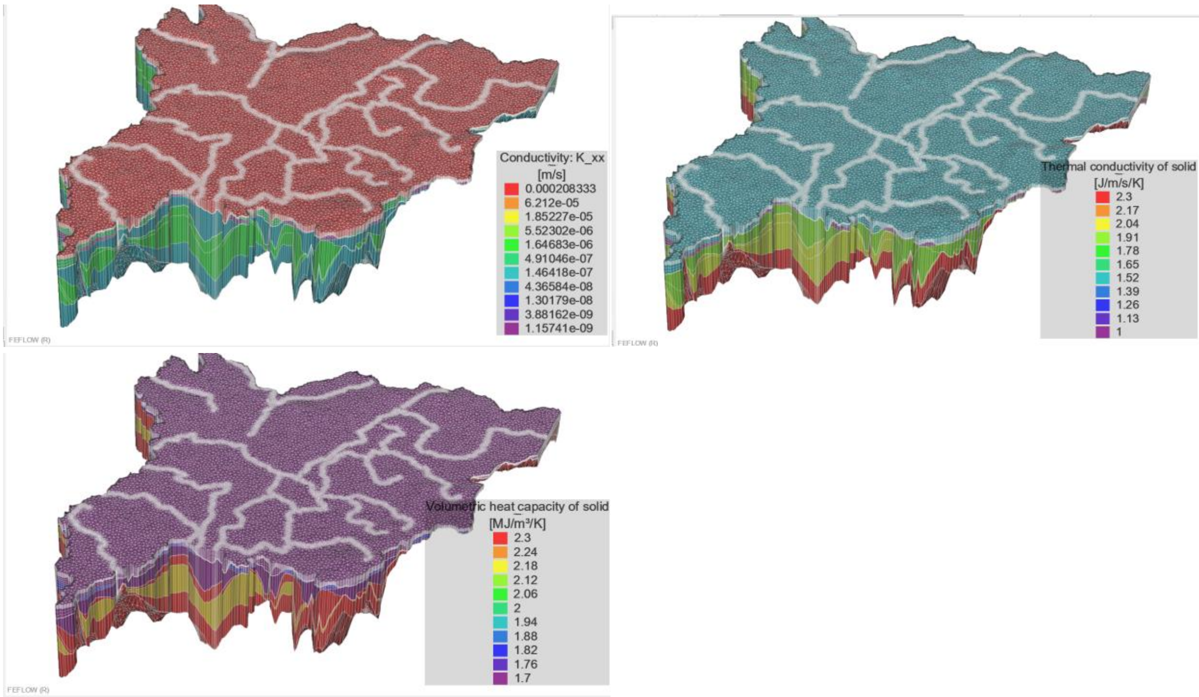

Figure 2: Distribution of hydraulic conductivity, thermal conductivity of solid, and volumetric heat capacity of solid in the model.

4. The recharge modeling is impressive - it might be good to be more explicit that all of these recharge realizations do not translate into equivalent model runs for groundwater temperature. I only figured that out later, but I admit to reading this in a moving car with children yelling in my ear etc. The authors calibrate this to streamflow. How does the model performance compare for just baseflow? That might be more comforting to know, since that is a better indicator of whether the recharge and discharge processes are being captured reasonably.

Reviewer#1 correctly points out that for groundwater simulations, we used not every 750 recharge realizations, but a mean, p90, p10 ensemble members for each of the two emission scenarios.

Most of the hydrological model calibration was presented in an earlier German-wide study by Guse et al. (2024). The model performs well for high-flow periods, as its original focus was flood analysis. The highest Nash–Sutcliffe model efficiency coefficient was obtained for gauges on major rivers such as Elbe and Rhein, indicating that regional trends are well captured.

We have analyzed the calculated baseflow, and in the corrected version of the manuscript we will comment on the model performance for this variable within the study region.

5. Eq. 1 - units don't work - is Recharge in units of 1/s (like a volume flux per unit volume)?

Yes R should be [s^{-1}]. We thank reviewer#1 for the correction.

6. This is a big numerical model domain with many elements. Was this solved with high-performance computing or anything - or just run on a 'regular' computer? My simulations with this many nodes took weeks to run, but that was 10 years ago.

This question was also raised by reviewer#2. Below we provide detailed information about the simulations and will include a concise description in the updated manuscript.

Computational experiments for the climate and hydrological components—including the non-stationary weather generator, the mHM hydrological model, and the workflow connecting both—were performed on the GLIC high-performance computing (HPC) system at the GFZ German Research Centre for Geosciences. The system uses the SLURM workload manager, a standard HPC scheduling environment that assigns resources and coordinates parallel workloads. This setup allowed us to run the modelling chain efficiently across 50 compute nodes in parallel. Each node provided 40 GB of RAM, and each task used four CPU cores. With this configuration, the complete workflow—from the nsRWG simulations to the mHM impact modelling—required roughly 10 days of wall-clock time.

The groundwater model was simulated separately on a local workstation equipped with an Intel Core Ultra 7 155H processor (16 cores) and 32 GB of RAM. Each of the six scenarios required ~3,500 computational timesteps to cover a 145-year period with monthly boundary conditions, resulting in simulation times between 13 and 26 hours.

7. Boundary conditions questions: a) is there any vertical hydraulic gradient on the vertical boundary conditions at the sides? b) Does the model allow for exfiltration at the surface if the water table rises too high? c) I'm surprised the surface air temp and ground surface temperature are so similar. Is there no snow here? The mean annual air temp of 9C suggests there would be.

We would like to clarify our choice of boundary conditions:

a) No, the hydraulic head was constant with depth along the sides of the model. Therefore, a vertical pressure gradient according to the gravitational effect is present, which does not generate an additional vertical flow component. This may imply that groundwater flow / discharge into rivers at the boundaries is underestimated. While in the presented model the side edges were kept open, in a previous work we demonstrated the effect of no-flow boundaries for the same model domain (Tsypin et al., 2024). An additional complication is that flow in the deep aquifers is controlled more by the structural dip of the sedimentary layers than by local drainage configuration. In follow-up studies, a more granular boundary condition assignment could be tested, combining no-flow, river, and in/out flux boundary conditions.
b) In the "phreatic" model definition of the unconfined aquifer, the water level may theoretically rise above the ground surface. This would have two consequences. (1) Along all major river valleys, a Cauchy boundary condition was prescribed, allowing

for groundwater exfiltration. (2) In other areas, the initial water table was normally several meters deep. Therefore, even in periods of highest recharge, the amount of unaccounted outflow due to discharge into ephemeral streams and/or direct evapotranspiration from the water table is expected to be low.

c) Yes, the small difference between surface temperature and near-surface air temperature also surprised us (left image below). However, this difference refers to modeled temperatures from the GCM within the IPCC6 report. We have looked more carefully at historical data since then (right image). The mean difference is still relatively small, 1.2°C, which can be explained by following:
   a. According to the German Weather Service, the annual mean number of snowing days in 1985-2015 was 21, and the number of days with snow cover >1 cm was 24. Only January has long-term mean temperature slightly below zero.
   b. The study uses monthly temperatures, while the highest difference between air and surface temperatures occurs on the diurnal cycle
   c. The analysis was performed for natural land cover. In urban environments, using air temperatures likely underestimates the degree of surface warming, as discussed in Appendix B of the original manuscript.

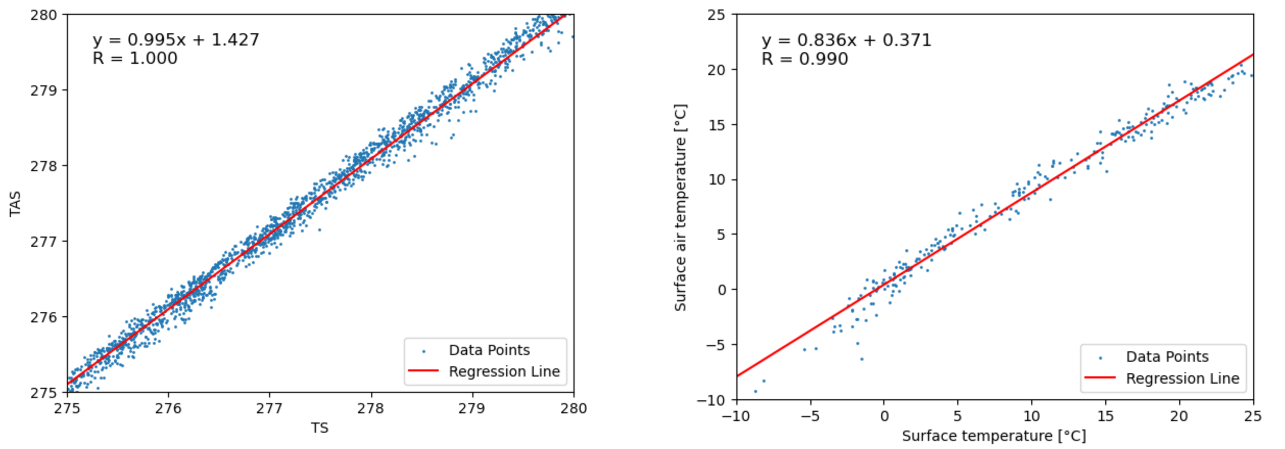

Figure 3: relationship between simulated surface air temperature (TAS) and surface temperature (TS) in Brandenburg during 2015-2100 period from the climate model Amon_MPI-ESM1-2-HR_ssp585_r1i1p1f1_gn (in Kelvin) (left); historical data from Potsdam secular station (1994-2024) (https://opendata.dwd.de/) (right).

8. Using mesh size for characteristic length for Peclet number is arbitrary (but maybe common) - how do the authors know what the threshold is for this Pe formulation for which advection matters? Later on they present Pe values and use these to highlight the role of advection, but this is not a formulation for which Pe =1 implies that advection = conduction (like say the Bredehoeft and Papadopulos 1965 formulation does - if I remember right). So what is the threshold for when advection matters? Maybe that is hard to say.

According to the classic approach, where characteristic length is defined by travel distance, Pe values greater than 1 indicate a dominance of the advection, while Pe values smaller than 1 indicate diffusion-dominated transport (Bredehoeft and Papadopulos (1965), DOMENICO and PALCIAUSKAS (1973) for heat transport or Ogata and Banks (1961) for mass transport). Using mesh element size as the characteristic length for Pe is common in numerical simulators (FEFLOW, GOLEM, OpenGeoSys) with a primary relevance for assessing numerical stability of advection-dispersion: spatial discretization in advection dominated systems often leads to artificial oscillations and numerical dispersion (Huysmans and Dassargues, 2005).

Caution is required when interpreting the role of advection, because element size may vary with mesh discretization and between layers of different thickness. Therefore, we should limit interpretation of Pe to a qualitative level. For example, in a S-N model cross-section in Figure 4 below, a relatively higher role of advection is characteristic of shallower, more permeable aquifers, as well as for glacial plateaus with higher hydraulic gradients.

After considering the reviewer's comment, we decided to exclude the paragraph on Pe interpretation in the revised manuscript.

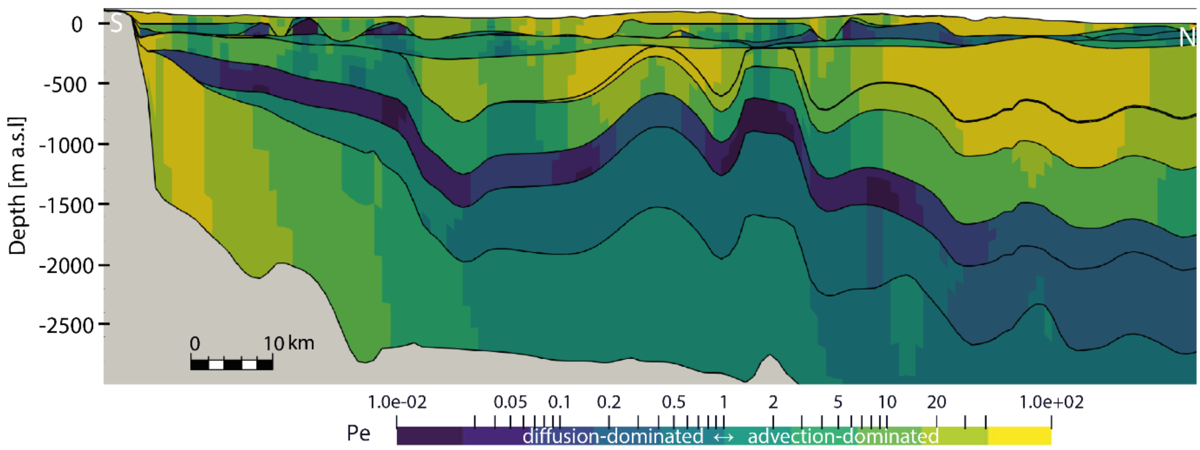

Figure 4: Model S-N cross-section with *Pe* numbers.

9. The recharge change runs in the groundwater model are interesting (see Table 1), but I'm still not clear where some of these came from (esp. the discrete synthetic scenario). I guess at least one is more of a sensitivity run rather than an expected scenario? Also, it would be helpful if Table 1 were more precise about timelines rather than "present" and "late century". What years are the before/after runs here for?

The information on the exact time ranges and derivation of the synthetic scenario is given earlier (L245-249) and later (Section 4.2) in the text, respectively. We agree with reviewer#1 that it is needed to be marked in the table as well.

The corrected table is given in Table 2 below.

Table 2: Summary of the tested groundwater modeling scenarios.

Scenario	Description	ΔT (surface)	Δ Groundwater recharge	Comment
		Present (2002-2021) to late-century (2081-2100) periods		
		Ensemble scenarios derived with nsRWG and mHM (Section 4.1)		
SSP245	Moderate emission pathway	+1.5 °C	-10 %	Ensemble mean
SSP585	High-end emission pathway	+3 °C	-20 %	
		Synthetic and reference scenarios (Section 4.2)		
M1	Recharge increase, moderate warming	+1.5 °C	+50 %, higher winter recharge ratio	after Marx et al. (2021)
M2	Recharge increase, strong warming	+3 °C	+50 %, higher winter recharge ratio	
M3	Recharge as SSP585, present-day temperature	0 °C	-20 %	Reference cases to isolate the recharge effects
M4	Recharge increase, present-day temperature	0 °C	+50 % Winter	

10. Model calibration - consider presenting the RMSE normalized to head range. I think that would do a better job of showing the fit. 6.6 m sounds high, but the head is highly variable, so it is not a bad fit at all.

Figure 5 below shows the updated plots with added RMSE normalized to the range, as well as bias value.

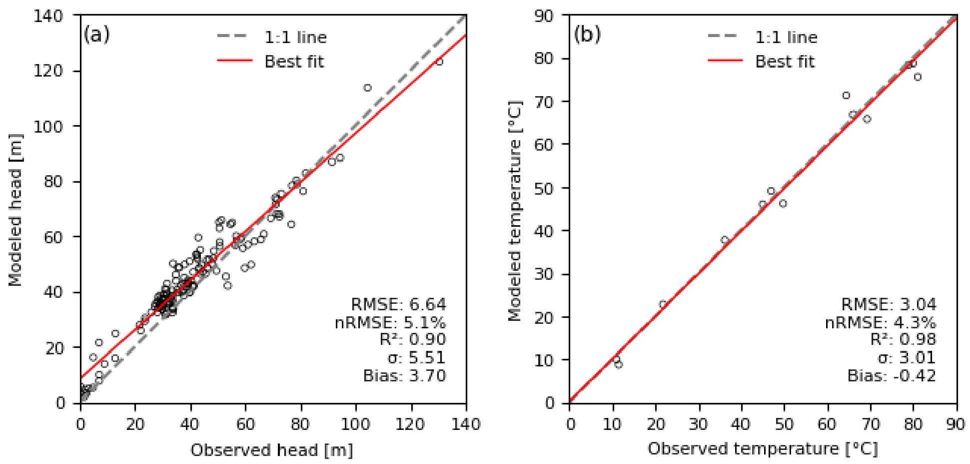

Figure 5: Steady-state groundwater model calibration results: simulated versus observed hydraulic head and temperature at monitoring points (locations shown in Error! Reference source not found.**c). RMSE – Root Mean Square Error, nRMSE – normalized to value range; R^2 – coefficient of determination; σ – standard deviation.**

11. Where do groundwater temp measurements come from for calibration? Case borehole temp profiles? Pumped groundwater with temp recorded? Open boreholes? Large diameter? Are convection or seasonal bias concerns?

Temperatures from deep aquifers (>500 m) that we used to validate the initial thermal field, are a mix of (a) bottom-hole temperatures (BHT) obtained in wells shortly after drilling, thus representing perturbed borehole temperatures and (b) continuous temperature logs, mostly measured after the boreholes were cased (Förster, 2001). All BHT temperatures were previously corrected by Horner-plot correction and the exponential integral method based on a model simulating the temperature build-up during shut-in time of a well (Förster, 2001). The estimated error of the corrected measurements was ±3°C or ±10°C. Some temperature logs were also corrected using a simple empirical correction, with the error in final estimates of ±3°C (Förster, 2001).

Shallow groundwater temperature monitoring is available from a handful of wells in Berlin and is limited to 80 m below ground (SenStadt, 2020). Unfortunately, continuous vertical profiles and Distributed Temperature Sensing (DTS) are only available for the urban area. We chose to present them separately (Appendix B), due to strong land cover overprint.

12. Figure 7 - it would be interesting to also see changes in discharge presented somehow. The authors talk about changes in recharge (input) and head (storage), but what about output (relevant for some of the reasons groundwater temperature is relevant)

Following the reviewer's suggestion, we will comment on discharge changes in the revised manuscript. A more in-depth analysis would require more thorough parametrization of rivers (colmation layer conductivity, streambed geometry), and is better suited to a catchment scale study (e.g., Haacke et al. (2018)). We can recommend a recent paper by Li et al. (2025), who used similar modeling tools to infer climate-driven river network contraction.

13. L386 "Profile A, located on a glacial plateau, has a shallow seasonal envelope, entirely above the water table, suggesting no advective transfer of the seasonal thermal signal into the saturated zone" - I don't think you have to be saturated for heat advection to matter. It depends on the water flux, not the saturation per se. if you have recharge through the vadose zone, advection can still theoretically matter.

We completely agree with this comment. Our language is surely confusing here. We did not want to say that there is no heat advection through the vadose zone. The point is that the vadose zone on plateaus can be so thick that even with the vertical water flux (relatively slow), the seasonal temperature fluctuations are fully attenuated above the water table. This results in stable year-round temperatures of the phreatic aquifer, though still subject to a long-term warming.

Saturation levels are relevant here because the numerical code scales down hydraulic conductivity proportionally to saturation above the water table.

Minor comments

L60-65 - I think the controversy over the role of advection is overstated here. In the Benz et al. study, the authors used a parsimonious approach in a global study and justified this using 2D numerical models of advection and conduction. They basically found that for typical hydrogeological systems and recharge rates, heat advection is not a primary driver of groundwater temperature change. However, they note that is not the case for some basins. Other studies cited here are in steeper topography or with much higher recharge, and so it makes sense that they found that advection mattered.

We thank reviewer#1 for this comment. It would indeed be more precise to call it a spectrum of findings rather than a controversy. However, we also think the contrasting conclusions on the role of heat advection came not only due to basin-specific geology, recharge and topography, but also an applied approach (e.g., inclusion of lateral groundwater flow, unsaturated zone assumptions, and boundary conditions).

Figure 11 a - label missing for scale bar

The color bar and the vertical axis of Figure 11 b have the same range and share the same label. The same is true for Figures 10 and 7. In order to avoid any misinterpretation we are going to mention this in captions of the corrected manuscript.

Conclusions _ 'depth limit of advection overprint' sounds a bit jargony for the conclusions

Agreed. Proposed change: The first regional aquitard imposes the lower limit on downward advective heat transport, except where it is eroded.

In general, these comments are all easy to address I think, and I look forward to seeing this article published.

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