Response to Reviewer 2 for the manuscript:

Assessing Soil and Potential Air Temperature Coupling Using PALM-4U: Implications for Idealized Scenarios

Manuscript number: egusphere-2024-1234

In the following text the original comments by the reviewers are given in black, our answers are blue. Line numbers refer to the unmarked manuscript (i.e., no tracked changes).

Reviewer #2 (Remarks to the Author):

In their article titled “Assessing Soil and Potential Air Temperature Coupling Using PALM-4U: Implications for Idealized Scenarios,” the authors aim to answer the important question of how underground temperature extremes impact atmospheric temperatures. More specifically, the authors formulate the following three research questions:

- How to depict a realistic but idealized domain in PALM-4U?
- Do heat or cold extremes in the soil modify potential air temperatures?
- What parameters affect these modifications?

The authors are moving in a new direction by investigating the effect of subsurface temperature extremes on air temperatures (and not vice-versa). In the introduction, the authors succinctly address the relevance of this novel perspective. With its interdisciplinary view of the interactions between multiple spheres of the Earth system, this research is of interest to the scientific community and beyond, making it a suitable contribution to ESD. The authors present a well-written and concise manuscript in most parts and provide a good introduction to the general relevance of the topic as well as typical approaches and limitations in understanding and implementing a thermal coupling of the subsurface and the atmosphere.

Reply: We thank the reviewer for their careful, comprehensive and constructive feedback which helped us to improve the manuscript significantly. Thank you for (also) highlighting the strength of the paper, this is very much appreciated.

However, I find the manuscript difficult to follow for the following reasons: First, the introduction provides a good overview of the relevance and state of knowledge, but it seems decoupled from the rest of the manuscript. For example, I assume the choice of boundary conditions is self-evident to the authors, but it may not be to every interested reader. In that way, a brief explanation of boundary conditions used in temperature simulation at interfaces (e.g., in atmospheric research) would help the reader understand the choice of boundary conditions, their pros and cons, and why they were considered for the investigation.

Reply: We have addressed your concerns in the following way:

> The decoupling between the introduction and the rest of the manuscript: We believe the reason for this was that the introduction mainly discussed the second part of our results, the coupling of subsurface and atmosphere and only briefly mentioned the challenges we address in the first part of the results section: how to model this coupling in an idealized domain. This has now been addressed by adding a more detailed problem description and by explaining (the importance of) boundary conditions and their meaning in idealized and realistic domains. We added a new paragraph on page 2, line 70:

[In our study we ask the reverse: do alterations in soil temperatures impact potential air temperatures?]. "Due to a lack of usable real-world data, this study approaches this question numerically in an idealized domain. As such it is intended as a proof of concept, laying the groundwork for future research. Idealized domains are not yet defined in PALM-4U. Before conducting experiments, it is essential to thoroughly understand and characterize the processes in our "area of investigation".

as well on page 6, line 133:

"Within the PALM-4U model there are several options for LBCs such as Dirichlet, cyclic, those mentioned in conjunction with one radiation boundary, Neumann, turbulence recycling, etc., which can be looked up here: (https://palm.muk.uni-hannover.de/trac/wiki/doc/app/initialization_parameters#bc) (Initialization Parameters). Further, the detailed explanation of the LBCs and how they calculate the flow is given here: (https://palm.muk.uni-hannover.de/trac/wiki/doc/tec/bc) (Boundary Conditions). We decided using the Dirichlet/radiation LBC and entire cyclic LBCs because these options are plausible for our use case. In this way the system can unfold without allowing too many degrees of freedom. The advantages and disadvantages we faced with those options are depicted in the discussion."

Second, there is a very detailed presentation/explanation and interpretation (rather than discussion) of the obtained results. My impression is that the authors focus on these highly detailed results, but the general evaluation of the model performance/suitability is not prominently discussed and/or limited to the plausibility check of the results. Therefore, the question arises of how the results and model performance can be evaluated and checked. Can available datasets or data from experiments be used to validate the results?

Reply: As of right now our numerical model cannot be validated to data from experiments as there is no data on the impact of subsurface heat or cold anomalies on atmospheric temperatures. Because of the complexity of atmospheric temperatures and air movements and the many diverse drivers of local climates, such data is more or less impossible to achieve in an experimental setup. Hence, as an initial test of our hypothesis (i.e., can subsurface heat or cold sources impact atmospheric temperatures?) we decided to focus on a quasi-idealized experiment. This is in no way meant to represent the real world, but rather intended to answer questions in the fundamental sciences, using well established numerical models to do so. We are currently working on a numerical model of a real-life domain, which will give more insight into how subsurface urban heat islands may contribute to atmospheric urban heat. However, we believe that this first step is significant enough to both climate sciences and geosciences warrant its own publication. This point was also added in the manuscript (discussion 4.1):

"A general evaluation of the model performance to check quality of the digital representation of reality cannot be assessed as there are no reliable observational data facing the question how heat sources in the soil affect ground-level atmospheric temperature. Thus, our purpose was to test our hypothesis for the first time using quasi-idealized experiments."

The very technical nature and a high degree of detailed explanation of the results make it difficult to follow the common theme and line of argumentation and really understand the work's contribution and novelty – also regarding the accuracy of the results and validity beyond the model domain. The guidance of the reader is missing or at least not apparent to me. Hence, I encourage the authors to revise the manuscript accordingly (moderate to major revision required).

Minor comments include:

1. Lines 20-27: Check the syntax for suitability in a scientific journal

Reply: As non-native speakers it is unclear to us what you are specifically referring to. Would you be able to point out examples? Thank you very much in advance.

2. Line 41: How relevant is the impact of an individual construction? The accumulation is probably relevant, and the examples given appear somewhat random.

Reply: We changed the manuscript to clarify that not only accumulation but individual constructions alone can impact groundwater and soil temperatures significantly.

"For example, ground temperatures near underground parking garages can be up to 10 K warmer. This appears within and outside the urban environment and is an addition to the accumulation of urban waste heat".

4. Line 46: Delete "However"

Reply: Done: "The thermal coupling between the underground and the atmosphere is complex."

5. Line 64: Please specify "near surface atmosphere"

Reply: Done in the manuscript: "… (until 4000 m height but with a special focus on the lowest 35 m) …"

6. Line 87: What about heat transport via percolating water? Should be addressed.

Reply: Advective heat transport is typically not considered dominant for subsurface heat transport, particularly in an urban environment where sealed surfaces are omnipresent. We added a short paragraph about this in the manuscript.

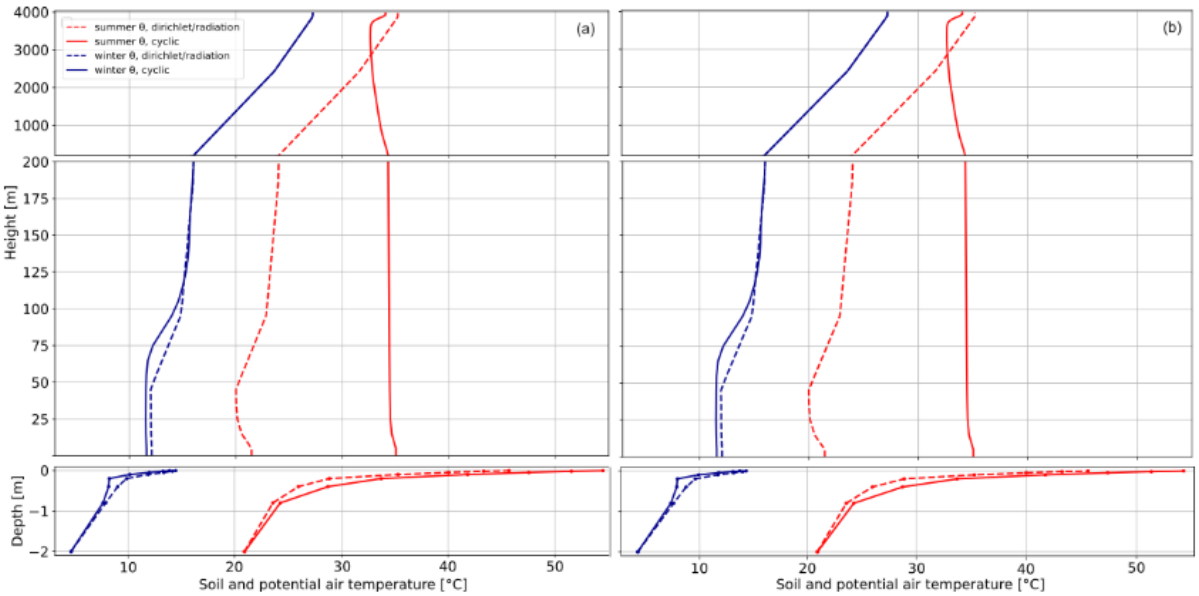
"Particularly in an urban environment the advective heat transport can be neglected due to the wide occurrence of sealed surfaced."

7. Line 158 and others: Is "right side" the best terminology to refer to the orientation in the model?

Reply: Thank you for this comment, indeed it is confusing. We changed left/right to inflow and outflow boundary in the text.

8. Line 197: You mention Fig. 2 and then Fig.5. Should Fig. 5 then be renamed as Fig. 3?

Reply: Very good point. Due to our storyline, we could not move Figure 5 forward. We have now changed Figure 2 in a way that we added the soil temperatures already at this point. Thus, the offset is already visible here.


summer θ, dirichlet/radiation
summer θ, cyclic
winter θ, dirichlet/radiation
winter θ, cyclic
(a)
(b)
Height [m]
Depth [m]
Soil and potential air temperature [°C]
Soil and potential air temperature [°C]