Response to Fredrik

Andrea Zonato and co-authors

30 June 2026

In preparing this revision, we regenerated all model outputs after updating the wind-zone length coefficients in the preprocessor and after adding new global building-data sources to the Dortmund input workflow. The revised simulations, figures, and skill metrics therefore reflect the updated coefficient settings and building morphology inputs.

Response to Fredrik

Dear Fredrik,

Opening Comment

> Thank you for an interesting read and use of the SOLWEIG-model. A full workflow using GPU-enabled technology for Tmrt calculation is proven to be very efficient for this type a 2.5D-model. Many of you know me as the creator and maintainer of the SOLWEIG-model and based on that, I would like to add some comments regarding the setup and results produced. I must confess that I have only done one read-through, so some of my comments might be explained in the text already. Sorry for that.

Response to Opening Comment

Thank you very much for reading the manuscript and for these important comments. We agree that the SOLWEIG setup and the observational reference for Tmrt needed to be described more explicitly. We have revised the manuscript accordingly.

Comment F1

> Please improve the description on the settings using the model as this have large implications on your results and the interpretation of your findings. You state that you are using v2022a and this includes anisotrophic schemes for the long- as well as the diffuse shortwave sky. Did you implement these settings? Also, did you use the Reindl et al method to partition diffuse and direct shortwave radiation from global or did you get that from ERA5? More details are needed! Especially since the settings could be an explanation to your bias in Tmrt shown in e.g. fig 7, especially in dense urban areas because of the anisotrophic skies (Wallenberg et al. 2020; 2023)

Response F1

Manuscript changes.

Revised manuscript lines 154–204.

We agree. We expanded Sect. 2.1 ("Overview of the SOLWEIG core") to state the exact radiative schemes used in the Dortmund experiments. The simulations use the SOLWEIG v2022a anisotropic diffuse shortwave scheme, where diffuse shortwave radiation is distributed over sky-vault patches using the Perez et al. (1993) sky-luminance formulation rather than treated as an isotropic hemispheric term. They also use the v2022a anisotropic longwave scheme, where sky longwave emission is resolved over sky patches rather than prescribed as a spatially uniform hemispheric contribution.

We also clarified the treatment of radiative forcing in the meteorological-forcing description. ERA5 downward surface shortwave radiation is used as global horizontal shortwave irradiance. We do not take separate direct and diffuse shortwave components from ERA5 for these runs. Instead, SOLWEIG partitions the global shortwave irradiance into direct and diffuse components using clearness-index-based atmospheric transmissivity and the Reindl et al. (1990) relations. We additionally clarified that ERA5 downward longwave radiation is retained for reproducibility and optional experiments, but was not prescribed in the Dortmund simulations; longwave exchange was computed by SOLWEIG and distributed with the anisotropic longwave scheme.

Comment F2

> Using this version of the model requires the correct referencing which should include Wallenberg et al. 2020 and 2023.

Response F2

Manuscript changes.

Revised manuscript lines 140–160 and 617–620; reference list lines 810–846.

We added the requested references to the SOLWEIG setup description and to the discussion of remaining Tmrt uncertainties: Perez et al. (1993), on the sky-luminance distribution used for diffuse shortwave anisotropy; Wallenberg et al. (2020), on anisotropic diffuse shortwave radiation; and Wallenberg et al. (2023), on anisotropic longwave irradiance. We also cite Reindl et al. (1990) where the diffuse/direct shortwave partitioning is described.

Comment F3

> You are using black globe temperature sensors to derive Tmrt which needs to be discussed further. Did you calibrate these against more accurate observations and if not, do you think the bias in e.g. fig 7 could be explained based on this rather that my points in bulletpoint 1?

Response F3

Manuscript changes.

Revised manuscript lines 470 and 691–697.

We agree that the observational derivation of Tmrt should be discussed more carefully. The Dortmund biometeorological network uses Decentlab DL-BLG black-globe sensors, from which Tmrt is derived. The resulting Tmrt observations were not independently calibrated against 3-D net radiometers within this study. We therefore added a statement in the case-study section clarifying that no calibration correction was applied to the derived Tmrt observations.

We also added a short paragraph in the Discussion noting that the Dortmund comparison should be read as an evaluation against the operational biometeorological network rather than as a radiometer-level calibration experiment. We further clarified that the Dortmund simulations already use the anisotropic shortwave and longwave sky schemes, so the remaining Tmrt bias cannot simply be attributed to an omitted isotropic-sky approximation.

References Provided By Reviewer

Wallenberg, Nils, Lindberg F, Holmer B, and Thorsson S. (2020) "The Influence of Anisotropic Diffuse Shortwave Radiation on Mean Radiant Temperature in Outdoor Urban Environments." Urban Climate 31 (2020). `https://doi.org/10.1016/j.uclim.2020.100589.`

Wallenberg, N., Lindberg, F., Holmer, B., and Rayner, D. (2023) An anisotropic parameterization scheme for longwave irradiance and its impact on radiant load in urban outdoor settings. International journal of biometeorology. `https://doi.org/10.1007/s00484-023-02441-3.`

Both references are now present in `references.bib`, cited in the revised manuscript, and visible in the manuscript diff.

Best wishes,
Andrea and co-authors