Response to Reviewers: Third Round

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Response to Remaining Minor Revisions

Comment TR1: Definition of LERC

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"LERC" (Zonato et al., p. 9) Never defined before. Please define what is this step /
phase
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Response TR1

Manuscript changes.

Revised manuscript, Sect. 2.2, paragraph "CPU I/O and deferred re-compression".

We agree that the term was introduced too abruptly. We have now defined LERC as Limited Error Raster Compression and clarified that this is a post-processing compression step applied to completed GeoTIFF outputs, after the scientific fields have already been computed. We also retained the variable-specific maximum absolute error settings used for the LERC+ZSTD repacking.

Comment TR2: Elevation-correction equation

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"P denotes the vertical temperature operator" (Zonato et al., p. 10) P for profile ???!!
In Equation 3, not sure P is an ideal letter to simply understand the equation. Shouldn't
be T + a correction term ? that takes into account the elevation ? Moreover, P is not
directly related to the rest of the Equations afterward. Maybe it would be simpler to
have the resulting Equation fo deltaH>0 and deltaH<0 below lifting condensation and the
combination of (4) and (5) in the other cases ?
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Response TR2

Manuscript changes.

Revised manuscript, Sect. 2.3, "Elevation bias correction".

We agree. We removed the abstract operator notation and rewrote the correction directly as an elevation-dependent air-temperature adjustment. The manuscript now states the dry-adiabatic expression used when the DEM terrain is below the ERA5 surface, and also when upward displacement remains below the lifting condensation level:

$$T_{2\mathrm{m,corr}} = T_{2\mathrm{m}} + \Gamma_{\mathrm{d}} \Delta H.$$

For upward displacements above the LCL, the text now explains that the profile is first lifted dry adiabatically to the LCL and is then continued with the saturated pseudo-adiabatic integration described by the subsequent equations. We also clarified that the pressure variable is read from the forcing metfile in kPa and converted internally to Pa before the thermodynamic calculation. This makes the relationship between the elevation correction and the later thermodynamic equations explicit.

Comment TR3: Calculation of $z_{0,\mathrm{city}}$

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"domain-average urban roughness length z0,city from buildings and trees" (Zonato et al.,
p. 11) How z0 is calculated ? More specifically, are buildings and trees taken into
account the same way for the calculation ? And how trees are considered ? One tree patch
(containing several trees) = max tree height ?
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Response TR3

Manuscript changes.

Revised manuscript, Sect. 2.3, paragraph "Reference wind".

We have expanded the description of the roughness estimate and now describe it as it is set up in the code. The domain-average roughness length is computed as an area-weighted mean of building cells, tree cells, and all remaining cells. Buildings and trees therefore enter the same domain-mean estimator, but as separate masks with separate mean positive heights. Building cells are identified first, and tree cells are counted only where tree height is positive and no building is present. The roughness components are $z_{0,\mathrm{b}} = 0.1\overline{H}_\mathrm{b}$, $z_{0,\mathrm{t}} = 0.1\overline{H}_\mathrm{t}$, and $z_{0,\mathrm{o}} = 0.03\,\mathrm{m}$. Thus a tree patch is not represented by its maximum height for this roughness-rescaling step; the tree contribution uses the mean positive tree-DSM height over non-building tree cells.

Comment TR4: Use of "wake"

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"wake" (Zonato et al., p. 11) Why using wake here ? In Röckle model types, wake is a
reference to a specific zone located downstream the building. You use directional wind
reduction both for upstream and downstream zones right ? Thus using "Directional wind
reduction model" might be more appropriate ?
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Response TR4

Manuscript changes.

Revised manuscript, Sect. 2.1 and Sect. 2.3.

We agree. The previous wording could incorrectly imply that the whole GLIDE-SOL wind module only represents the downwind Röckle wake zone. We revised the terminology to "directional wind-reduction model" where the model as a whole is described. We now reserve "wake" mainly for explicitly downwind or leeward contexts. We also clarified in the preprocessor description that GLIDE-SOL represents both upwind and downwind attenuation zones before smoothing and clipping the directional coefficient fields.

Comment TR5: Röckle-type terminology

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"the Röckle wake model" (Zonato et al., p. 11) I would rather write "of Rockle type
models such as described in..."
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Response TR5

Manuscript changes.

Revised manuscript, Sect. 2.3, paragraph "Directional wind-reduction model".

We agree and changed the wording accordingly. The manuscript now describes the method as a simplified one-step attenuation scheme inspired by Röckle-type models such as those described in Röckle (1990) and Bernard et al. (2023), rather than as "the Röckle wake model".

Comment TR6: Replace generic "wake" wording

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"wake" (Zonato et al., p. 12) same as before, replace by something like "wind-reduction
models" ?
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Response TR6

Manuscript changes.

Revised manuscript, Sect. 2.3 and Results captions/text.

We revised the generic uses of "wake" where they referred to the complete diagnostic wind module. In particular, we changed the section heading to "Directional wind-reduction model", replaced "wake-based attenuation" with "directional wind-reduction", and changed several references to overlapping "wakes" into overlapping reduction or attenuation zones. We retained "wake" only where the text specifically refers to leeward/downwind behavior or to the terminology of Röckle/URock.

Comment TR7: URock vegetation comparison

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"with specific windfactor formulations" (Zonato et al., p. 30) You may add that
currently, there is no building-lie upwind/leeward influences zones
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Response TR7

Manuscript changes.

Revised manuscript, Appendix A, wind-model comparison table.

We added this clarification to the vegetation row of the comparison table. The URock column now states that vegetation is represented with dedicated vegetation zones and wind-factor formulations, but without the building-like upwind/leeward influence zones used in GLIDE-SOL.

Other Reviewer Comments Not Yet Answered

Comment TR8: UHI diagnostic downscaling

"This should be interpreted as a diagnostic downscaling of a city-scale daily UHI amplitude according to local morphology, not as a claim that atmospheric UHI variability is physically resolved at exactly 2 m. Coarser or spatially smoothed UHI fields remain a future sensitivity test" (Zonato et al., p. 10) In my opinion, it is not only that it is a diagnostic downscaling but a limited diagnostic downscaling since it might take into account that air temperature does not vary as much as it can vary with the proposed model (you might for example use a moving average of about 500 m to limit this local effect). In areas with a single tree pixel (which is quite unrealistic I confess), the SVF of all your pixels will be close to 1 while the difference between the pixel with the tree and the other pixels will be quite big. Thus the result is that air temperature between 2 close-by pixels can be high which does not reflect the reality. In the added text, it is not clear that the limitation of the 2m air temperature is mainly related to the vegetation fraction which is a very local calculation.

Response TR8

Manuscript changes.

We revised the UHI-cycle description to state explicitly that the limitation is linked to the local vegetation indicator used in the modulation term and to the absence of horizontal turbulent diffusion and advection in this diagnostic calculation. The manuscript now states that the actual 2 m air-temperature response would be expected to be spatially smoother than the diagnostic field. We also clarified that the 2 m output should be interpreted as a limited diagnostic downscaling. In the limitations, we added that future development should introduce a stability-dependent representation of intra-urban UHI variability, following evidence from urban green-space cooling studies such as Haeffelin et al. (2024), while also clarifying that wind remains the dominant contributor to the all-hour UTCI improvement in the Dortmund evaluation.

Response.

Thank you for pointing this out. We agree that the previous wording did not make the main limitation clear enough. The issue is not only that the UHI field is diagnostically downscaled from a city-scale amplitude, but also that one of the local modifiers, the vegetation fraction, is currently evaluated at the native SOLWEIG grid scale. Since horizontal turbulent diffusion and advection are not explicitly represented in this local diagnostic modulation, real 2 m air temperature would be expected to be spatially smoother than the diagnostic field.

We have therefore revised the text to describe the approach as a limited diagnostic downscaling and to identify the lack of flow-dependent horizontal blending as a specific limitation. In future work, the intra-urban UHI variation should include a dependence on atmospheric stability, turbulence, wind, and advection. Haeffelin et al. (2024), for example, show experimentally that the cooling contrast associated with urban green spaces depends strongly on boundary-layer stability and turbulent mixing. We therefore now state that stability-dependent smoothing or neighborhood-scale vegetation metrics should be tested in future work. We also clarified that this temperature correction is only one contribution to UTCI; in the Dortmund evaluation the dominant improvement comes from the wind correction.

Comment TR9: Additional literature for tree wake treatment

"This differs from URock, where vegetation drag is represented without adding the same building-like upwind and wake zones used here. The additional upwind and leeward zones are a diagnostic GLIDE-SOL extension, motivated by the physical expectation that porous tree canopies induce upstream adjustment and coherent downstream wakes, as also shown in recent isolated-tree wake experiments (Grandoni 350 et al., 2026)" (Zonato et al., p. 13) I think slightly more literature references might be added here. QES-wind has its own wake model for isolated trees (Margairaz et al. - 2022). They only appy it when the tree is outside the upstream or downstream cavity zones and only consider the tree height as impacting the tree wake zone. They show that a cavity zone might also be added since it has been shown in several observational studies. I think some of these informations might be added, if not here at least in the discussion. Margairaz, Fabien, Hanieh Eshagh, Arash Nemati Hayati, Eric R. Pardyjak, et Rob Stoll. "Development and Evaluation of an Isolated-Tree Flow Model for Neutral-Stability Conditions". *Urban Climate* 42 (mars 2022): 101083. https://doi.org/10.1016/j.uclim.2022.101083.

Response TR9

Manuscript changes.

We added Margairaz et al. (2022) to the tree-wake paragraph. The revised manuscript now briefly notes that QES-Winds uses a dedicated isolated-tree flow model, applied outside building cavity and street-canyon zones, and clarifies that this supports representing tree-induced sheltering separately from building wakes while the GLIDE-SOL treatment remains a simplified diagnostic approximation.

Response.

Thank you for this useful suggestion. We agree that the previous text relied too heavily on a single reference. We therefore added Margairaz et al. (2022) and explicitly mentioned the QES-Winds isolated-tree flow model. The revised text now uses this literature to frame the GLIDE-SOL approach more carefully: tree canopies can generate sheltering zones that are not identical to building wakes, and existing diagnostic wind models have introduced dedicated tree-wake treatments. At the same time, we kept the statement conservative by clarifying that GLIDE-SOL does not implement the QES-Winds isolated-tree model; its tree upwind and leeward zones remain a pragmatic first-order diagnostic parameterization.

Comment TR10: Multiplication of overlapping tree wake factors

"Within each contribution, overlapping wake factors are multiplied" (Zonato et al., p. 14) Multiplying building wakes seems relevant but I wonder about multiplying tree wakes since then tree input data may have a quite big impacts if trees are separated by a single pixel ? Not sure there is a need to consider this comment however... =)

Response TR10

Manuscript changes.

No change was made to the manuscript for this point.

Response.

Thank you for pointing this out. In the implementation, trees are first read from the tree-height raster and, for each wind direction, the tree field is rotated into the wind-aligned coordinate system. Connected positive-height canopy cells are then identified as tree components in that wind-aligned grid, and the upwind and leeward attenuation zones are built from each component geometry. Therefore, if two tree patches are separated by a single pixel, they may be treated as two separate components for that wind direction. However, this should have little practical impact on the final coefficient field: the reduction factors are bounded, overlapping contributions are clipped to the prescribed range, and the final field is Gaussian-smoothed over a neighborhood-scale length. A one-pixel gap therefore mainly changes the bookkeeping of the intermediate components, while the final smoothed wind-reduction pattern remains controlled by the broader canopy distribution rather than by isolated pixel-scale separations.

Comment TR11: LCZ legend and station colors

`"Station labels are colored according to the observed Local Climate Zone" (Zonato et al., p. 16) The legend overlap. And it seems the circles for LCZ are not colored while they are in the legend`

Response TR11

Manuscript changes.

We regenerated the Dortmund domain figure. Station markers and station labels are now both colored using the observed LCZ class, and the LCZ legend has been moved and consolidated to avoid overlap.

Response.

We agree. In the previous version, the labels were colored according to the observed LCZ class, while the station circles were drawn with a single station-marker color. This made the LCZ legend ambiguous. We updated the plotting routine and regenerated the figure so that both the station markers and the station labels use the observed LCZ colors, and we repositioned the legend to remove the overlap.

Comment TR12: High-vegetation cover on wind-reduction figure

`"Example wind-reduction coefficient field" (Zonato et al., p. 17) Is adding the high-vegetation cover alter a lot the readability ? Because I suspect it might explain many suspicious wind reductions in some areas.`

Response TR12

Manuscript changes.

We regenerated the wind-reduction coefficient figure for $\theta = 240°$ by marking positive tree-height pixels from the tree-height raster with a yellow overlay labelled "Trees", with a dark halo added for readability. We also revised the caption and the accompanying text to explain that some locally low wind-reduction coefficients outside the densest building blocks are associated with canopy sheltering.

Response.

We agree that showing the trees makes the figure easier to interpret. We replaced the previous subtle point markers with a yellow tree-height overlay labelled "Trees", applied to positive tree-height pixels in the plotted area and outlined with a dark halo for contrast. This keeps the wind-reduction coefficient field readable while making tree-covered areas clearly visible. This helps explain low-coefficient regions that are not obviously associated with dense buildings alone, because the diagnostic wind-reduction field includes both building and tree attenuation contributions.