

Reviewer comment on the manuscript

Coupling the TKE-ACM2 Planetary Boundary Layer Scheme with the Building Effect Parameterization Model

GMD-2024-205

By Zhang et al.

General considerations

The authors have made great efforts to react to the first reviews' comments. This is particularly true with respect to language and clarity of the presentation. I nevertheless have a number of minor comments that I feel should be addressed before the paper can be recommended for publication.

We truly appreciate your insightful comments and constructive feedback, which have significantly contributed to improving the quality of our work. In response to your comments and suggestions, we have made point-to-point modifications shown below and addressed them in the manuscript correspondingly.

Minor comments

(all lines refer to the 'track changes' manuscript [gmd-2024-205-ATC3.pdf])

1. 65 '...PBLs that are too shallow and moist in the evening...'

The repeated "PBLs" is deleted. The sentence is revised to:

For instance, Coniglio et al. (2013) reported that MYJ produces PBLs that are too shallow and moist in the evening.

eq. (8) and (9). I overlooked this in the first version. There seems to be a mismatch in the use of variable name 'z' and 'z_i'. You have introduced 'z_i' in eq (2) as the level heights in relation to the discrete parameterization for the fluxes. Here, a general profile is defined as a function of the height coordinate 'z'. Therefore, all heights in these equations should read 'z', and not

‘ z_i ’. (this is particularly important because your case is a CBL, where usually the height of the inversion (CBL height) is denoted ‘ z_i ’.

Indeed, the original notation of ‘ z_i ’ could cause confusion with the CBL height. We have removed the subscript ‘i’ in equations 8 and 9.

l. 230 ‘...in Table B1’: here, it must become clear that this table is in Appendix B. → ‘....in Tab. B1 (Appendix B)’.

Thank you for the carefulness. We have revised it as follows:

“The distribution of LCZ 1 to 10 (urban) grids and LCZ A to G (non-urban) grids is depicted in Fig.2c. Each class is defined in Table B1 (Appendix B).”

l. 253 ‘are provided in the supplementary material of Zhang (2024)’. If it is written like this, I assume that this is the supplementary material to <https://doi.org/10.1029/2023JD040432>, which is the paper on the turbulence parameterization TKE-ACM2. However, it seems that here, the supplementary material to the present paper is meant (i.e., the Zenodo publication of the data etc.). If so, the reference should read: ‘...is published on a data hub (Zhang et al 2024)’. Applies also to later occurrences of ‘supplementary material’.....

We indeed intended to refer to the supplementary material to the present paper. Thus, we have revised the wording as:

“The coordinates and LCZ classifications of these surface stations are provided in the supplementary material to the present work, which is published on a data hub (Zhang, 2024).”

Any other instances have also been corrected.

l. 256 same as l.230.

Revised accordingly.

l.320the boundary layer became sightly unstable....

Revised accordingly as follows:

“... and a discrepancy in the Boulac+BEP results relative to the LES results suggests that the

boundary layer became slightly unstable from approximately $\sim 10H$.”

l.322 ‘...was well simulated at roof level in BEP and PALM...’: but PALM is the reference (the truth) here – so, PALM only ‘well represents the expectations...’. Rephrase to the end that ‘BEP quite successfully reproduces the PALM reference’.

It has been revised to:

“Figure 4b suggests that PALM simulated a strong wind shear at the roof level, while such an inflection point in the wind speed profile was successfully reproduced by BEP, in contrast with the Bulk simulations, ...”

l.327 ...in contrast to...

Revised accordingly.

Fig. 6, caption: please replace ‘ground truth’ by ‘reference’.

Revised accordingly.

l.333 ‘...leading to conduction’: I overlooked this is the first review, but heat conduction is likely not the reason for this behaviour. I would rather assume this to be a model artifact...(in the LES). This also concerns the following sentence... (I am not sure whether the PALM model includes conduction). Please clarify.

The sensible heat flux (HF) between the solid surfaces (roof, wall, and streets) and the atmosphere occurs due to their physical contact, which is a means of conduction. However, the parameterization of HF between the horizontal surface (HF_hor) and air is different to that between the vertical surface (HF_vert) and air.

Below the building height, HF_vert calculated in BEP and PALM scales with $-\frac{\eta \Delta T}{c_p}$ where η is a $O(10)$ constant, c_p is the specific heat $O(10^6)$, and ΔT is the potential temperature difference between the air and wall. As a result, HF_vert is on the order of $10^{-5} \Delta T$.

At the roof, HF_hor in BEP and PALM follow the M-O parameterization, where HF_hor scales with $-\left[\frac{k}{\log\left(\frac{z}{z_0}\right)}\right]^2 U \Delta T$, where $k=0.4$, z is $\frac{1}{2}\Delta z_1$, U the wind speed. Eventually, HF_hor is on the order of $O(10^{-3})\Delta T$. Clearly, the heat flux is more negative at the roof compared to that within the canyon. Therefore, the drastic reduction of heat flux was observed at the roof level.

The revised texts are shown in lines 262 to 264.

1.349 ‘....to a minimum (maximum magnitude) value’. Please carefully review the use of maximum/minimum and decrease/increase in the entire paragraph (given the fact that the momentum fluxes are negative).

We have revised the wordings such that the magnitude of momentum fluxes was discussed in lines 274 to 277.

1.361 The two PBL schemes...

Revised accordingly.

1.400 Figures C1 and C2 in Appendix C.....

We have revised the texts so that readers are referred to Appendix C.

Fig. 9, caption: ‘.... for the grid point of the observational sites (Fig. 2) USTSS (LCZ5), HAT (rural), and KP (LCZ1) from ...’

Revised accordingly.

1.434 In fact, L is most commonly referred to as ‘Obukhov length’ (Obukhov has published the length scale already some six years before the joint paper with Monin in 1954. Should be corrected in the entire manuscript.

“Monin-Obukhov length” has been revised to “Obukhov length” throughout the manuscript.

Fig. 13, caption: again ‘reference’ instead of ‘ground truth’

Revised accordingly.

l. 498 as l. 400

We have revised the texts so that readers are referred to Appendix C.

l.523 ‘remained positively skewed’. I don’t think we see the result of a skewed distribution here. They remain positively biased.

The word “skewed” has been replaced by “biased”.

Fig. 20 why not the same colors as in Figs. 6 and 13?

Figure 20 is now using the same color scheme as Figs.6 and 13 for consistency.

l. 535were considerably smaller...’

Revised accordingly.

l.571 TKE-ACM2+Bulk

Revised accordingly.

“Coupling the TKE-ACM2 Planetary Boundary Layer Scheme with the Building Effect Parameterization Model” by Wanliang Zhang, Chao Ren, Edward Yan Yung Ng, Michael Mau Fung Wong, and Jimmy Chi Hung Fung

Recommendation: Minor revisions

General comments

This manuscript introduced an approach coupling the recently developed TKE-ACM2 PBL scheme with the multi-layer BEP UCM in the WRF model (hereafter, TKE-ACM2+BEP) and evaluated the performance of the TKE-ACM2+BEP approach in comparison to idealized LES for two convective PBL (CBL) cases as well as 1-month real-case observations. For both idealized and real cases, the simulations using the TKE-ACM2+BEP approach were compared with the TKE-ACM2+Bulk to investigate impacts of UCM (BEP vs. Bulk), with the Boulac+BEP to investigate impacts of PBL, as well as with Boulac+Bulk that differs in both PBL and UCM.

The coupling between the TKE-ACM2 PBL scheme and the BEP UCM scheme was made by 1) adding the forcing term computed by the BEP UCM to the rhs of TKE-ACM2 PBL tendency terms for prognostic variables including TKE, and 2) modifying the length scale in the TKE-ACM2 closure by considering the length scale comparable to the building height (l_{build}). The TKE-ACM2+BEP was verified in comparison to idealized LES for two CBL cases focusing on mean temperature and wind profiles and their corresponding vertical flux profiles. The real-case verifications in comparison to lidar wind profiles and near-surface meteorological parameters (U_{10} , T_2 , RH_2) were made for different land use categories, including 10 LCZ urban categories, water, and non-urban categories.

This manuscript is well written and organized providing details of the implementation that are essential to understand the coupling approach and verification results confirming that TKE-ACM2+BEP was properly implemented, showing great potential of TKE-ACM2+BEP to improve the WRF simulations for urban PBLs. I have several minor comments and

suggestions.

We would like to express our sincere gratitude for the time and effort you dedicated to reviewing our manuscript. In response to your suggestions and comments, we have made several improvements to the manuscript.

Minor comments

Lines 114–115, “ A_i and B_i ”: Could you mention here that A_i and B_i are outputs from the BEP UCM? You mentioned it later at Line 125 for TKE, but I suggesting mentioning it to here as well.

We have added extra texts to remind readers that A_i and B_i are outputs from BEP.

Line 146, “anisotropic”: I think in general turbulence closure models in LES compute impacts of isotropic turbulence (assuming model resolution is in the inertial subrange), not anisotropic turbulence. Could you double check this for the PALM model?

Indeed, the turbulence closure model used in the PALM setting is isotropic according to Equation 13 in Maronga et al. (2015).

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Line 254, “observations”: I guess you mean “LES”.

Revised accordingly.

Lines 293–294, “Boulac+BEP seemed to largely underestimate the momentum flux”: Due to the underestimation of the momentum flux, I expected Boulac+BEP would overestimate u/ug compared to TKE-ACM+BEP, like TKE-ACM2+Bulk and Boulac+Bulk underestimate the momentum flux and overestimate u/ug , for both 10WC and 24SC cases. However, Boulac+BEP rather underestimates winds compared to TKE-ACM+BEP. Could you explain why the momentum flux and the wind profiles aren’t consistent for Boulac+BEP? It shows momentum flux profiles closer to TKE-ACM2+Bulk and Boulac+Bulk, but u/ug profiles closer to TKE-ACM2+BEP.

Not only the magnitude of $\overline{w'u'}$ affects the u/u_g profile, but also the gradient of $\overline{w'u'}$ ($\partial\overline{w'u'}/\partial z$) is critical because the tendency term ($\partial u/\partial t$) is balanced by $-\partial\overline{w'u'}/\partial z$ as shown in Eqn.1.

$$\frac{\partial u}{\partial t} = -\frac{\partial\overline{w'u'}}{\partial z} \quad \text{Eqn.1}$$

Below $z/H = 1$, Boulac+BEP produced significantly larger absolute value of $\partial\overline{w'u'}/\partial z$ than Boulac+Bulk and relatively smaller $|\partial\overline{w'u'}/\partial z|$ than TKE-ACM2+BEP, consistent with the considerably smaller u than Boulac+Bulk and slightly larger u than TKE-ACM2+BEP. Yet, the relationship between momentum flux and wind speed is not always linear as $\overline{w'u'}$ is computed mainly by the eddy viscosity times $\partial u/\partial z$, and the feedback mechanisms in the boundary layer can influence how wind profiles respond to changes in momentum flux.

Lines 414–415, “The four simulations generated T2 diurnal cycles with much lower amplitude than observations at water surfaces”: Could you confirm if the observations were also made over water surfaces (e.g., buoy) or they are over land surfaces surrounded by oceans? The observations show quite strong diurnal cycles of T2, which is not typical over water surfaces. If the observations were made over land surfaces while the simulations were at ocean grid points, this could be the reason why the T2 diurnal cycle amplitude is much lower in the simulations.

A few stations were identified as located on a water grid point in simulations at the horizontal resolution of $\Delta x = \Delta y = 1$ km, yet they are in fact situated on land. For example, the Green Island station (GI_AWS, Fig.S46 in the supplement) is placed on a small island surrounded by ocean, leading to that WRF recognizes the grid point as water surface. Other mismatch is found at SHL_AWS (Fig.S59) and SHW_AWS (Fig.S60) stations which are placed at the coast. The mismatch between the actual land cover type of the station location and the model landuse is rather common (e.g., Ribeiro et al. (2021)) and inevitable, considering that we needed to manually check the identified landuse of the stations when the simulation resolution and grid setting change.

Extra explanation has been added to line 420 in the revised manuscript.

References

- Maronga, B., Gryschka, M., Heinze, R., Hoffmann, F., Kanani-Sühring, F., Keck, M., Ketelsen, K., Letzel, M. O., Sühring, M., & Raasch, S. (2015). The Parallelized Large-Eddy Simulation Model (PALM) version 4.0 for atmospheric and oceanic flows: Model formulation, recent developments, and future perspectives. *Geoscientific Model Development*, 8(8), 2515–2551. <https://doi.org/10.5194/gmd-8-2515-2015>
- Ribeiro, I., Martilli, A., Falls, M., Zonato, A., & Villalba, G. (2021). Highly resolved WRF-BEP/BEM simulations over Barcelona urban area with LCZ. *Atmospheric Research*, 248, 105220. <https://doi.org/10.1016/j.atmosres.2020.105220>