Geoscientific Model Development Discussions

Geosci. Model Dev. Discuss., referee comment RC1
https://doi.org/10.5194/gmd-2021-141-RC1, 2021

Review of gmd-2021-141

Anonymous Referee #1

Referee comment on "Influence on the temperature estimation of the planetary boundary layer scheme with different minimum eddy diffusivity in WRF v3.9.1.1" by Hongyi Ding et al., Geosci. Model Dev. Discuss., https://doi.org/10.5194/gmd-2021-141-RC1, 2021

The manuscript investigates the effects of different minimum eddy diffusivity on the near surface temperature using the Weather Research and Forecasting (WRF) model for 2 winter periods. Overall, the manuscript is well structured and investigates an important topic. The research approach is adequate, but can still be imporved. The analysis of the model results is lacking depth and would greatly benefit for a more careful assessment of the physical process within the nocturnal boundary-layer and the link between the state of the boundary layer during daytime and night-time. The manuscript would also greatly benefit from a throgough model evaluation and some polishing of the english language.

Overall comments:

1. One of my major concerns is the lack of a proper model evaluation. The evaluation of the modelling setup is currently spread throughout different sections and is mainly focused on 2m temperature from one observational point (section 2.1), and a few vertical temperature profile (Fig 7) . I would highly recommend to perform an extensive evaluation for 2m temperature and 10m-wind speed at different observational stations across the domain. This could offer insight on the bias in near surface temperature and atmospheric stability during the night. Ideally an evaluation of the modelled surface energy balance and surface exchange fluxes should be provided for the observational point where the physical process analysis is conducted (in section 3.1). Such an evaluation would help the reader to understand how the model performs at the default Kzmin setting, before the effects of the changes in Kzmin are investigated.

2. The physical process analysis in section 3.1 lacks depth. The authors claim that shortwave radiation is unimportant for the deviations in Tskin during nighttime, because shortwave radiation in negligible during night. Still the amount of shortwave incoming radiation during the day affects the daytime evolution of air temperature and atmosphreric stability in the boundary layer, which can impact the nocturnal temperature gradience and the atmospheric stability during the night. This might result in different deviations for the

Tskindue to changes in Kzmin under sunny and cloudy conditions. Have the authors investigated the effect of Kzmin during different meteorological conditions? I would recommend that the authors discuss in more detail potential daytime carryover effects on the nocturnal near surface temperature differences.

3. Have the authors investigate the importance of temperature advection on the changes in near surface temperature gradience and the sensible heat flux? Although it makes sense that changes in Kzmin affect HFX, the authors do not report if advection of temperature is present over the observational site and whether it contributes to changes in near surface temperature gradience. Considering that figure S.2 shows substantial spatial variability in Kz it is reasonable to assume that there will be spatial differences in near surface temperature, which could lead to substantial temperature advection. Thus, I would recommend that the authors either discuss the importance of temperature advection or show that it is negligible at the location of the analysis.

4. The analysis on the spatial differences in 2m temperature and the impact of Kzmin (section 3.2) is brief and not well substantiated. Why do area with complex terrain produce larger eddies? Are there spatial differences in near surface wind speed throughout the domain that could explain the differences in Kz? The near surface wind speed during night-time could be very important for the nocturnal turbulent mixing, but is not discussed at all in the manuscript. Why is the T2m over urban areas strongly affected by Kzmin? I would expect that over urban areas there is more turbulent mixing during night-time and therefore the Kzmin values would be less relevant, as their effect is mainly dominant during very stable atmospheric condition. Did the authors use an urban canopy model to parameterize physical processes of the urban surface? If not (seem to be the case based on Table 1), what value can the Kzmin analysis provide over urban areas if the physical processes responsible for the surface energy balance and turbulent exchange fluxes over the cities are not properly parameterized?

5. A discussion on the limitations of the current research approach and an comparison with results from previous studies, on the effects of eddy diffusivity in the nocturnal boundary layer, is missing.

Specific comments:

6. Section 2.2 Have the authors allowed for any model spin-up time to ensure that the atmospheric state and soil temperatures are properly a spun-up before the analysis is conducted?

7. Equation 7. Here PURB seem to be dependent only on the urban and water fractions. What happens when the landuse fraction is neither urban nor water (e.g., vegetated areas)?

8. Line 225. Please note that differences in the surface outgoing longwave radiation, could affect the cooling/heating rates of temperature at the different model levels. The WRF model does allow for the output of temperature tendencies due to changes in net-shortwave and net-longwave (at all model levels). It might be worth utilizing these tendency terms to identify if there are indirect effects on the near surface temperature due to the differences in net longwave radiation between the experiments.

9. Line 248. The turbulence within the boundary layer cannot be resolved (only parameterized) with the current WRF setup as the horizontal grid spacing is too large. Kzmin is more important during nighttime, because under very stable conditions the modelled diffusivity (Kz) is lower than Kzmin, in which case the boundary layer scheme will replace Kz with Kzmin is the calculation of exchange coefficients and heat fluxes.

10. Line 255. The increase in entrainment is very much depended on how the entrainment is parameterized in the ACM2 scheme, information which the authors do not provide. For instance, If the modelled entrainment is proportional to the surface sensible heat flux (this is common in some PBL schemes), then is barely any change in entrainment as Fig. 3d shows minimal change in sensible heat flux during daytime. In any case, it would be important that the authors describe how they concluded that entrainment increases for higher Kzmin and how they calculated it.

11. Line 259 Is the difference between the daytime temperature profiles caused only by effects of Kzmin on the turbulence during daytime or is it purely due to the already large temperature differences during night-time?

12. Line 265. The 2m temperature in WRF is not interpolated based on Tskin and the temperature at the first model level, but is rather calculated from the Tskin temperature, the surface sensible heat flux and the echange coefficient of temperature at 2m (as seen in Li and Bou-Zeid, 2014).

13. Fig.5 would greatly benefit from the addition of subplots with the actual 2m temperature, Tskin, and HFX 2D fields for the default Kzmin value in of the ACM2 scheme. Moreover, there is no definition of the exact period that the authors consider as daytime and night-time.

Technical corrections:

14. The manuscript would benefit from a through editing check.

References:

Dan Li and Elie Bou-Zeid (2014) Quality and sensitivity of high-resolution numerical simulation of urban heat islands. Environ. Res. Lett. 9 055001