

Interactive comment on “Parameterization of vertical diffusion and the atmospheric boundary layer height determination in the EMEP model” by A. Jeričević et al.

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Dear Dr. Steeneveld,

Thank you for reading and commenting our paper. You have pointed some valuable issues which will be certainly included in the MS.

C1: What was the motivation to start with a new parameterization for $K(z)$ and the ABL height. It has not been mentioned in the paper that one was unhappy with the EMEP results under the old schemes. If this was the case, please mention. A1: Motivation of improving the mixing scheme was not due to the fact that the old scheme in the model was giving poor results but to examine the impacts and effects of different schemes on

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the ability of the model to simulate transport of different pollutants. Special emphasis was given on the parameterization of stable atmospheric conditions which are of great importance for application in air pollution. It is mentioned in the Introduction that special applications of the model have been recently developed at higher resolutions, and coupled with different meteorological drivers: EMEP4UK (e.g. Vieno et al., 2009a, b) and EMEP4HR (Jericevic et al., 2007; Kraljevic et al., 2008). This development of the EMEP model includes detailed meteorological effects that become progressively more important on the finer spatial scale, such as turbulence and convection generated by a complex terrain. As a first step of the EMEP model development on a finer horizontal scale, turbulence parameterizations, particularly vertical diffusion scheme $K(z)$, need to be tested. It is found that the change of the mixing scheme in the model leads to (slight) improvements and contributions are mainly scientific and technical. However, a few percent of improvement in a meteorological model performance is generally considered both acceptable and important in the meteorological community during the last two decades or so.

C2: P9605: At which height as ϕ been evaluated? A2: Universal functions in the EMEP model are calculated at the height of the surface layer (4% of the boundary layer height).

C3: P9605: Note that Eq. (6b) is only valid for $z/L < 1$ A3: Yes.

C4: Equation 10: Can you provide some physical background on the formulation of Eq.10. It appears it is an interpolation, but why in this way? A4: A gradually-varying function, $K(z)$, was introduced to generalize the classical analytical solution for the Ekman layer flow using the WKB method (after Wentzel. Kramers and Brillouin who popularized this method in theoretical physics). Since there is no explicit relation between the boundary layer profiles and K , a solution which approximates the third order O'Brien profile was defined between the constant K value and numerically derived solution of the Ekman profile (e.g., Grisogono: A generalized Ekman profile with gradually varying eddy diffusivities, QJRMS, 121, 445-453, 1995-G95). Three different $K(z)$ pro-

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files were tested (G95), against numerically determined u and v profiles. Furthermore, in Grisogono and Oerlemans: Katabatic flow: analytical solution for gradually varying eddy diffusivities, *JAS*, 58, 3349–3354, 2001-GO01a, and Grisogono and Oerlemans: Justifying the WKB approximation in pure katabatic flows, *Tellus A*, 54, 453–462, 2001-GO01b and Grisogono and Oerlemans: A theory for the estimation of surface fluxes in simple katabatic flows, *Q. J. Roy. Meteorol. Soc.*, 127, 2725–2739, 2001-GO01c; the Prandtl model for katabatic flows is solved for gradually varying $K(z)$ profile expressed in the exponential form as in Eq(10) of our paper. In GO01a comparison with observations from PASEX, Austria, 1994, showed that new $K(z)$ resembles the data much better than the constant K -solution. This form of $K(z)$ is also examined in Parmhed et al.: An improved Ekman layer approximation for smooth eddy diffusivity profiles, *BLM*, 2004 finding $K(z)$ useful in both theoretical and practical applications offering an elegant and accurate, but still simple and analytic, description of the near neutral, horizontally homogeneous geophysical boundary layer

Q5: P9606: Can you provide also the uncertainty of the coefficients in Eq. 11 and 12.

A5: Averages and standard deviations are presented in Figure 1.

Q6: P9607, Eqs 13,14,15: Perhaps it is interesting to check the paper of Vogelezang and Holtslag (1996). They use the same method (also at Cabauw), but they find much better results if u_s and v_s in eq. 14 and 15 are not taken at the surface, but at the 20, 40 or 80 m level. Vogelezang, D.H.P., and A.A.M. Holtslag, 1996: Evaluation and model impacts of alternative boundary-layer height formulations. *Boundary-Layer Meteorol.*, 81, 245–269. A6: The paper of the Vogelezang and Holtslag (1996) is a known and interesting work where different forms of bulk Richardson number method are studied in the stable atmospheric condition and validated against the Cabauw data. They find improvements in correlation for the stable boundary layer of certain type. Their estimated Rig numbers are higher than the corresponding RiB numbers (typically around 30%). This basically shows that estimated atmospheric conditions based on Rig number are more stable and the resulting boundary layer heights are lower. Similar effect can be

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achieved if the critical bulk Richardson number is varied according to stability (Jeričević and Grisogono, Tellus A, 2006). This inevitably leads to a conclusion provided by Zilitinkevich and Baklanov, BLM, (2002) on the existence of the RiBc and a large amount of different formulations based on Ri numbers. However, in this work Ri numbers were differently estimated from the measurements and model, which is emphasised in our revised manuscript. From the measurements RiB numbers are estimated using values at 2 m as the lowest level, while in estimation of RiB from the EMEP model first model level (approximately at 50 m) was used as the lowest level. As a consequence considerably more cases with $H > 200\text{m}$ is found in the measurements than in the model (please check the supplement material) which is in the agreement with findings of Voegezang and Holtslag (1996). It would be also interesting to check variations H values achieved by applying different forms of RiB, but it is beyond the scope of this study.

Q7: A question on DATABASE64. First, does, in your opinion, a 64 cubed LES have sufficient resolution to provide reliable results for the turbulent fields? Second, in the DATABASE64 dataset, the surface sensible heat flux has been prescribed at the surface. However, in the paper below, we show based on theoretical arguments that using a surface heat flux is not a proper boundary conditions for stable conditions (not for LES, no for 1D models). With this information, can you comment on the reliability of the DATABASE64? Basu, S., A.A.M. Holtslag, B.J.H. van de Wiel, A.F. Moene, and G.J. Steeneveld, 2008: An inconvenient 'truth' about using the sensible heatflux as a surface boundary condition in models under stably stratified regimes, Acta Geophys., 56, 88-99. A7: The LES used a dynamic sub-grid scale closure model which parameterizes the TKE dissipation with Smagorinsky closure and a resolution of 643 gridpoints. In the paper of Esau and Zilitinkevich (2006) it is shown based on a few intercomparison and convergence studies, that relatively small 643 mesh is sufficient to keep simulation errors at the level less than 5% of the total turbulent kinetic energy. Furthermore, the authors have provided comparisons for some turbulence statistics resolved on 643 and 1283 meshes finding that the differences in the vertical transport characteristics remains fairly small (e.g. for weakly or moderately stable ABLs). They found this con-

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clusion consistent with the Beare et al. (2006). Since authors provided reliable proofs of LES model performance at 643 mash, we found it suitable for use for our purposes. In your paper Basu et al. (2008) you show based on analytical approach that surface heat flux should be avoided as a lower boundary condition in LES models. Presented dual nature of sensible heat flux is notwithstanding intriguing issue and we are looking forward to see the validation results based on analyses of field observations. In all cases of DATABASE64 an initial temperature profile, neutral or with constant stratification and background geostrophic wind, the surface roughness length and surface heat flux were prescribed. As you state, the surface heat flux can be used for near neutral and weakly stable conditions (in DATABASE64: truly neutral, conventionally neutral and partly nocturnal) while moderately and strongly stable (partly nocturnal and long lived stable) conditions are those you refer to and question in your investigation. We use the DATABASE64 as the state of the art, finding it convenient and reliable for our purposes. However, we think that all new findings which could be used to explain our results based on the LES data should be taken into account, while the evaluation of the LES model itself is left to its developers. Therefore, we have pointed at your findings in a description of the LES model (First paragraph of the section 2.2. LES data.

Q8: P9608: Has the PBL depth in DATABASE64 been determined with the same method as in the newly developed scheme? This should be true for an honest comparison. A8: The PBL height used to calculate $K(z)$ profiles, with the both methods the O'Brien and Grisogono, from the DATABASE64 has been calculated based on the TKE profiles. More details in Jeričević and Večenaj (2009) BLM.

Q9: P9610: Perhaps the paper can be strengthened if the systematic part of the RMSE is used, and the index of agreement instead of correlation coefficient (Willmott, 1981). A9: We are aware that there is no single best performance measure or best evaluation methodology. It is recommended that a suite of different performance measures is applied (Chan and Hanna, 2003). It is definitely not an easy task to find the appropriate approach. Wilmot and Matsuura (2005) state that the RMSE is an inappropriate and

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misrepresented measure of average error and propose the use of MAE which they find to be a more natural measure. In our opinion, the measures used in this paper confirmed their main objectives. Furthermore, another paper is under preparation for the same ACP issue, on evaluating the EMEP model and different vertical diffusion schemes based on correlation coefficients, MAE, MSE and RMSE calculated between the measured and modelled Radon 222 data at different stations in Europe.

Q10: P9611: It is mentioned that Grisogono is less diffusive than Obrien. However, in Fig 2 I see the opposite. A10: What we wanted to say is that the Grisogono method represents surface concentrations in stable conditions better than the operational scheme, and it is generally less diffusive. However, in this particular case it was not less diffusive. We will paraphrase this statement.

Q11: P9615: Can you comment on the quality of radiosounding data. My experience is that wind speed is limited available, and only at coarse resolution. This will impact on RiB. A11: Radiosoundings are convenient for usage due to their spatial coverage, regularity and availability; however we agree that they suffer from some serious shortcomings. Crossing the ABL along a skewed path within a few minutes provides a 'snapshot'-like profile that can be a limiting factor in estimating H (e.g. Parlange and Brutsaert, 1989). If the time response of the radiosonde instruments is too long, some sharp changes in the profiles can be smoothed artificially. Large differences might occur between the depth of ABL inferred from the soundings and the depth of the turbulence especially for neutral and stable conditions. Furthermore, error in radio soundings measurements contains two components: (a) the soundings include horizontal structure in addition to the main vertical structure and (b) the sounding is almost instantaneous and therefore subject to random errors due to fluctuations caused by eddies. The contamination of the sounding by horizontal structure could be significant over complex terrain or urban area where horizontal gradients may be high due to surface heterogeneity. Quality of radiosounding data has been discussed in Jeričević and Grisogono (2006) where H values were calculated from the radiosoundings data based

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on RiB number in an urban area.

Q12: P9618: Which % of data has a PBL height of more than 200 m? In the paper below, a substantial amount of the Cabauw data has $H > 200\text{m}$? So how representative is your evaluation? A12: Uncertainty in the data is now shown and discussed in the revised manuscript. Results are shown here in Fig 2 and Fig 3.

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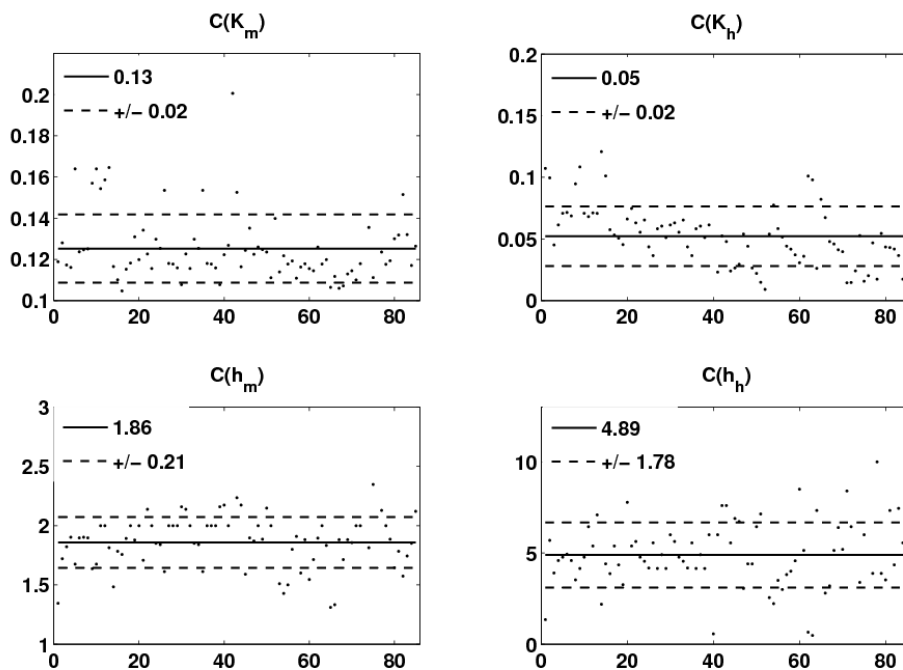
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Fig. 1. Averages and standard deviations of the $C(K)$ and $C(h)$ coefficients determined based on the LES data.

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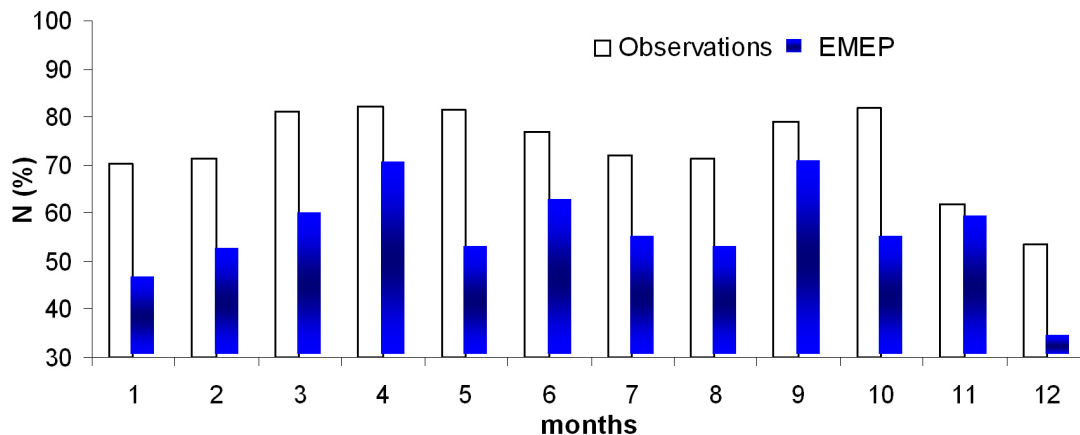


Fig. 2. Number of hourly H values higher than 200 m, N (%), determined from the observations (white bars) and from EMEP model (blue bars) per month during 2001 at the Cabauw tower.

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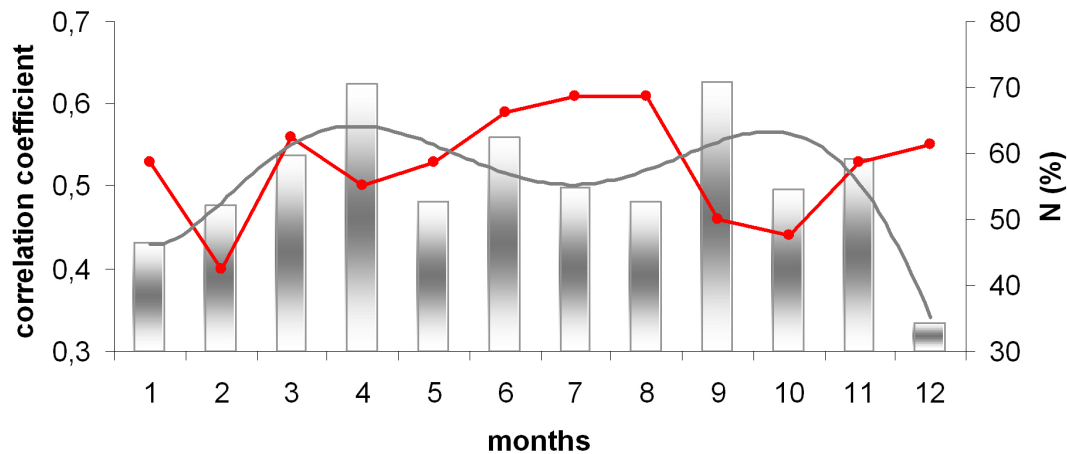


Fig. 3. Number of hourly $H > 200$ m values, N (%) determined from the observations (bars, right axes) and the corresponding monthly correlation coefficient (red line, left axes) at the Cabauw tower during the

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