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Atmospheric Chemistry and Physics Discussions

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. Seibert,

Thank you on your detailed and constructive review. The answers on your comments are the following.

Answers to the Specific Comments

C1. The English is not always good enough, especially with respect to the usage of articles and prepositions. Before publication, language editing is necessary. A1: We have now improved the English in the manuscript (as much as we could since none of

us is a native English speaker).

C2. p. 9602, l. 13: eutrophying pollutants: wouldn't that include ammonia / ammonium? A2: Yes, NH3 and NH4+ are also important eutrophying pollutants. However they are not so frequently measured at all EMEP stations and we wanted to have spatial as well as temporal coverage of the data.

C3. p. 9604, l. 10: It is not clear what is meant with "trapezoidal rule" for the calculation of pressure at higher levels. One would expect that the barometric formula would be used for that, and if one wants to go into detail, then the question would be how measured temperature and humidity have been used. One is also wondering why it is necessary to calculate these pressure values. From later text one can guess that it was used to calculate potential temperature. It would be good to mention this. By the way, for these purposes, a simple conversion such as 1°/100 m would be sufficiently accurate. A3: The "trapezoidal rule" is a common method for numerical integration. Consider y=f(x) over the closed interval [x_0, x_1] where x_1=x_0+h. Assuming f(x) is well behaved in the interval the trapezoidal rule states that TR(f, h)=h/2 (f(x_0)+f(x_1))) is an numerical approximation of the integral of f(x) over the interval [x_0, x_1]. This rule is used in order to derive the pressure at the altitude from the hydrostatic relation which is needed to calculate the potential temperature which is, in turn, needed to calculate the bulk Richardson number. In this way, we do not imply any specific stratification, and the only approximation used is the hydrostatic approximation. If we used simple approximation of 1C/100m, this would mean that we a priori assumed a specific stratification. This would make the calculation of Richardson number meaningless since it would depend on the wind sheer only.

C4. p. 9606, l. 9 ff: I found the presentation including the way the equation was written a bit confusing. Firstly, h is often used for what the authors here call Hs. By analogy, one should call it zmax. Then one may introduce a normalised height _x = z=zmax. The empirical constants should not be called C(K) and C(h) as this implies a function rather than a value, and is misleading as they also have different dimensions.

Better to use subscripts. In Eq. 12 it is a bit strange that the inverse of the constant is actually used, why not zmax = CzH with Cz = 1=3? Finally, one could write Eq. 10 much simpler and more transparent as The sentence might be written something like “Grisogono’s profile combines a linear term, which dominates near the surface, with an exponential decay so that the maximum of K is reached at about 0.3H, similar to O’Brien’s formula.” (I would avoid “proposed new scheme” as it has already been introduced in other articles.) See also comments of Reviewer #2. A4: Choice of h for the height of Kmax is taken in the same way as it was introduced by Grisogono and Oerlemans (2002). Although by analogy it would be possible to take zmax, it wouldn’t be physically true because Kmax is actually maximum value of K(z) while h is not the maximum height. Also we would like to keep the empirical constants C(K) and C(h) as well as Eq. 12 as it is in Jeričević and Večenaj (2009), from now onward JV09, to preserve consistency. Furthermore the empirical constants C(K) and C(h), are in fact scaling parameters dependent on K and h (see Eq. (9) in JV09). The proposed substitution for Eq. 10 is not much different from the present one and the new variable is introduced. Since you propose a sentence, which is adequate and nicely put, we will use it there.

C5. p. 9607, l. 5: The gradient of K at Hs is missing in the list of parameters on which the O’Brien formula relies. It should be noted that in a practical implementation one could use simple approximations for most of these parameters. Typically, one needs to determine only the ABL height and the mentioned gradient, the latter being available from an analytical description of the surface layer. The argument that these parameters are difficult to specify especially in stable conditions is not very valid as even the present EMEP version is using O’Brien only for unstable conditions. A5: We agree that in practical applications there are very simple methods for determination of input parameters in O’Brien equation. However our criticism goes to how good, accurate and reliable these are for determination of surface parameters, especially in stable conditions. Beside the ABL height, one needs to calculate K_Hs, vertical diffusion at the height of the surface layer which is determined based on the Monin-

Obukhov theory (Eq. (6) in the revised paper). Although the O’Brien polynomial profile is not calculated in stable conditions, all stated variables K_H, K_Hs, H and Hs should be provided for K(z) determination in stable atmospheric conditions.

C6. p. 9607, new method for determination of H. I agree with the comment of GJ Steeneveld that improvements of the shear term by Vogelezang and Holtslag (1996) should have been taken into account. Not only do they suggest to replace the surface winds by the wind at some level representing the surface layer top, they have shown that adding a term representing the shear production in that layer through the friction velocity is beneficial (see also the COST 710 Report, WG 2 Mixing height determination, online at http://www.boku.ac.at/imp/ envmet/finalreport_cost710-2.pdf). A6: Please check our answer to the Dr. Steeneveld on this question. In our formulation of the RiB number (Eq. 14) we have also used winds at the top of the layer instead of surface winds which is especially convenient in the cases of the low wind speeds. It would be also interesting to check variations of RiB and H values achieved by varying the lower boundary layer in different atmospheric conditions as well as with additional term in the RiB number; however, this is beyond the scope of this paper.

C7. All figures suffer from lossy compression and insufficient resolution. I don’t know whether this is due to the author’s files or introduced by the ACPD production. In any case, this should be avoided. A7: We have now improved the quality of most of the figures.

C8. Fig. 6 etc.: I am wondering why relative differences in correlation coefficients have been used. I think that is not appropriate. A8: Relative differences can be used for almost any quantity. However since Rewiever2 also commented on appropriateness of relative differences of BIAS, we present now only differences. Now spatial distribution of differences between the correlation coefficient and BIAS values are represented instead of relative differences (Figure 9).

C9. Fig. 9: It is not good to use these smoothed curves, especially without symbols for

every month (I presume the figure presents monthly values). A9: Curves are now not smoothed.

C10. Fig. 10: The correlation coefficients and possibly other parameters should be provided. A10.: Figure 10 is now excluded and correlation coefficients and BIAS values are described in Sec. 3.3.3.

C11. Fig. 11 and 13: I am wondering why 00Z and 12Z values are lumped together. Stable and unstable conditions should be treated separately to provide more insight. Several stations show worse results with the new formulation. The reason for this should be clarified (and, if possible, improvements be made). Separation of day and night might help for that. I think stations have been ordered according to geographical latitude. Maybe ordering them according to a MH- or performance-related parameter would be more useful. A11. We have also analyzed mixing height at 00 and 12 UTC. It is a detailed study of the ABL height, however it does not contribute to the conclusions of this paper. Your suggestion to order stations according to statistical parameter is accepted.

Answers to the Technical comments

C1. Acronyms such as EMEP4UK, DATABASE64, etc. should be explained. A1. Done.

C2. p. 9600, Schafer et al. should be Schäfer et al. (cf. List of references!) A2: Done.

C3. p. 9604, eq. 1: I presume that Ri and V /z are to be taken at z – please make that clear. Also, one can print vectors as bold letters or with an arrow but one does not normally combine these two notations. One is also wondering why derivatives are used here when these formulae will be used only in a discrete form. Presenting them in this form would be more clear. A3: Done.

C4. p. 9605, eq. 4: It is not good to use _z as an abbreviation of H − Hs as _z is always used for height increments. A4: Changed.

C5. p. 9608, l. 6: What is meant by “higher order K(z) schemes”? A5: It is rephrased

in: ‘and development ofschemes based on a higher order closure is a subject of current and future research’.

C6. p. 9605, l. 8: “recalculated” should probably be “calculated”. A6: Changed

C7. p. 9610, l. 11: r is not explained and the way how a formula is mixed into the running text is not nice. A7: Formula for r is now added.

C8. p. 9626, l. 28, polution should be pollution A8: Corrected.

C9. p. 9627, l. 1, title words should not be capitalized A9: Done.

C10. Fig. 1 and Fig. 8, coloured dots are too small to be recognised well, the orange colours are too similar A10: Those results are now represented as spatial distribution (Figure 9).

C11. Fig. 2, the parameters leading to the given shapes should be given. A11: The basic flow variables are now presented in Figure 3.

C12. Fig. 4 caption, Ilmitz should be Illmitz, and Fig. 5 caption, Vielle should be Vieille (both are given correctly in the figure legend – why are they misspelt in the caption? A12: Corrected.

C13. Fig. 12, curve is clipped. Torshavn or Torshaven? A13: Changed in the text to Torshaven.

C14. Fig. 15, region from RiB between .2 and .25 is not coloured. A14: Now that is corrected.

Interactive comment on Atmos. Chem. Phys. Discuss., 9, 9597, 2009.