

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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General Comments

This paper introduces two changes of the turbulence parameterisation for the EMEP Eulerian air pollution model: The replacement of the Blackadar (stable) / O'Brien (unstable) turbulence formulations with an analytical K profile from Grisogono, and a different mixing height determination, based on a bulk Richardson number formulation. The new schemes have been introduced already, the innovation of the present paper is the evaluation of their impact in EMEP model simulations and a comparison of modelled mixing heights with radiosonde-derived ones.

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I would agree that the new formulations have a potential for improving the model, but it is questionable whether the present form is already good enough.

I agree with many findings of anonymous reviewer #2 (my list of minor issues is probably not as comprehensive). I would like to add that compared to the scientific innovation the paper is relatively long. There is some overlap with the Jeričević and Večenaj (2009) paper. The presentation of the model formulations should be more concise, using also references to other published material, and the comparisons should be concentrated towards the key findings.

Specific Comments

1. The English is not always good enough, especially with respect to the usage of articles and prepositions. Before publication, language editing is necessary.
2. p. 9602, l. 13: eutrophying pollutants: wouldn't that include ammonia / ammonium?
3. 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.

4. 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 H_s . By analogy, one should call it z_{max} . Then one may introduce a normalised height $\zeta_x = z/z_{max}$. The empirical constants should not be called $C(K)$ and $C(h)$ as

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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 $z_{max} = C_z H$ with $C_z = 1/3$? Finally, one could write Eq. 10 much simpler and more transparent as

$$K(z) = K_{max} \zeta_x \exp\left(\frac{1 - \zeta_x^2}{2}\right).$$

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.

5. p. 9607, l. 5: The gradient of K at H_s 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.
6. p. 9607, new method for determination of H . I agree with the comment of GJ Steeneveld that improvements of the shear term by Voegelezang 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).

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7. 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.
8. Fig. 6 etc.: I am wondering why *relative* differences in correlation coefficients have been used. I think that is not appropriate.
9. Fig. 9: It is not good to use these smoothed curves, especially without symbols for every month (I presume the figure presents monthly values).
10. Fig. 10: The correlation coefficients and possibly other parameters should be provided.
11. 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.

Technical comments

1. Acronyms such as EMEP4UK, DATABASE64, etc. should be explained.
2. p. 9600, Schafer et al. should be Schäfer et al. (cf. List of references!)
3. p. 9604, eq. 1: I presume that R_i and $\left|\partial\vec{V}/\partial z\right|$ 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

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derivatives are used here when these formulae will be used only in a discrete form. Presenting them in this form would be more clear.

4. p. 9605, eq. 4: It is not good to use Δz as an abbreviation of $H - H_s$ as Δz is always used for height increments.
5. p. 9608, l. 6: What is meant by “higher order $K(z)$ schemes”?
6. p. 9605, l. 8: “recalculated” should probably be “calculated”.
7. p. 9610, l. 11: r is not explained and the way how a formula is mixed into the running text is not nice.
8. p. 9626, l. 28, polution should be pollution
9. p. 9627, l. 1, title words should not be capitalised
10. Fig. 1 and Fig. 8, coloured dots are too small to be recognised well, the orange colours are too similar
11. Fig. 2, the parameters leading to the given shapes should be given.
12. Fig. 4 caption, Illmitz 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?)
13. Fig. 12, curve is clipped. Torshavn or Torshaven?
14. Fig. 15, region from RiB between .2 and .25 is not coloured.

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