Atmos. Chem. Phys. Discuss., 9, C1600–C1606, 2009
www.atmos-chem-phys-discuss.net/9/C1600/2009/

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.

Anonymous Referee #2

Received and published: 9 June 2009

The objective of this manuscript is to test new parameterisations for the vertical eddy diffusion coefficient and the boundary layer height within the Unified EMEP chemistry-transport model. The parameterisations are tested against data from Large Eddy Simulations, radiosoundings and the Cabauw tower. In addition, their effect is evaluated based on a comparison of modelled concentrations of some air pollutants with observed data. The new parameterisations show a better agreement with measurements and LES data than those routinely used in the EMEP model. For the concentrations, the overall improvement is relatively small, but it is obvious that the model performance depends on various factors, and any attempt to improve it should be encouraged.

The manuscript is suitable for the scope of Atmospheric Chemistry and Physics, but I would recommend a major revision before publication.

Major comments

1. Grisogono parameterisation

1.1. By inserting Eqs. 11 and 12 into Eq. 10, one obtains K(z) = C(K).C(h).u*.e^$\frac{1}{2}$ .z.exp[-0.5(z/h)^2], which can be written as K(z) = C.u*.z.exp[-0.5(z/h)^2], where C is a constant. This does not depend on the boundary layer height H, even though the authors state that H is explicitly included (p. 9607), as H cancels out when introducing K_max and h. It should be clarified why Eqs. 11 and 12 are needed and why C and h are not fitted directly.

1.2. Based on the formula derived above, if the eddy diffusivity within the surface layer (z « h) can be approximated by K(z) = C.u*.z, i.e. by a linear profile (cf. Fig. 2 for z < 50 m). By using C(K) = 0.1 and C(h) = 3, one obtains C = 0.49, which is not very different from the van Karman constant. Thus the surface-layer K(z) profile effectively corresponds to the profile derived from Monin-Obukhov similarity in neutral conditions. It is thus surprising that this would provide a better fit than a model that includes stability effects. Please discuss.

2. K(z) evaluation based on LES data

2.1. Why did you compare the Grisogono scheme against the O'Brien profile, even though the latter is not used in the EMEP model in stable conditions?

2.2. The data presented in Section 3.1 are taken from a previous study (Jericevic & Vecenaj, 2009, ref. in MS). Instead of showing just two examples, it would be more useful to present a more complete summary of the results of that study.

2.3. Why did you choose to show only stable cases, even though neutral cases were also included in the LES dataset? In fact, the K(z) values (and their vertical extent) shown in Fig. 2 seem quite large for stable conditions. If these examples are shown, it

ACPD

9, C1600–C1606, 2009

would be useful to report the basic flow variables associated with them.

2.4. Why did you compare K_m rather than K_h that is used for scalar diffusion in the EMEP model?

3. K(z) evaluation based on EMEP model performance

3.1. Why did you not include nitrate concentrations in the comparison? I know there are fewer stations measuring these compounds, but these would provide information on the balance between primary and secondary compounds (cf. sulphur with both SO_2 and sulphate included).

3.2. I am unable to follow the discussion in Section 3.3.1. Firstly, the authors explain that the model overestimates NO_2 concentrations at the SE02 station and that overestimation is larger in 2000 than 1990, and much larger in 2001. I do not understand how this trend is related to the exclusion of the measurement stations in the North Sea area. Secondly, the authors state that the "Grisigono method is less diffusive than O'Brien in stable conditions" (repeated on p.9613), but Section 3.1 demonstrates just the opposite. (See also the comment about the use of O'Brien scheme in the EMEP model.)

4. BL height comparison with observational data

4.1. The same Ri_B-based criterium for the estimation of boundary layer height is used both within the analysis of observations (both radiosounding and tower data) and the proposed new modelling scheme. Thus the modelled and observation-based H values cannot be considered fully independent. I would like to see a comment on this.

4.2. The Cabauw tower provides measurements up to a height of 200 m. Figure 15 indicates that the estimated H exceeds this for most of the daytime, which significantly limits the possibilities for model evaluation. Thus the conditions to which the presented results actually apply should be described and the limitations of these data quantified. It would be useful to discuss whether the seasonal variation in the agreement (Fig. 16)

ACPD

9, C1600–C1606, 2009

is related to the availability of data.

5. Presentation

5.1. The authors should improve the presentation. The description of the EMEP model, in particular, is insufficient and partly confusing. The most recent and detailed documentation of the K(z) and H parameterisations used in the EMEP model (Simpson et al., 2003, EMEP Report 1/2003) should be used as the key reference.

5.2. The authors should describe the methods more accurately. On p. 9599, for example, it is stated that K(z) is based on Monin-Obukhov similarity theory in stable conditions, while Section 2.4 indicates that this is only true for the surface layer in unstable conditions. In Section 3.1, the Grisogono scheme is compared against the O'Brien profile. This appears puzzling, as the LES data used as a reference are for stable conditions, while according to Section 2.4 (and Simpson et al., 2003) the O'Brien profile is used in the EMEP model in unstable cases only.

5.3. The language should be revised. There are also many technical inaccuracies; see the detailed comments below.

Detailed comments

p.9598, line 5, "especially in the stable conditions": Only stable conditions were tested.

p.9598, line 14 (and elsewhere), "ABL schemes": The ABL height is only considered. Please rephrase.

p.9599, line 13, "vertical diffusion scheme K(z)": Please define the variable K (and z) properly.

p.9599, line 17: "Deardorf" should read "Deardorff".

p.9600, line 7 (and elsewhere): Remove 'a' from the reference.

p.9604, Eq. 1: The use of K_min is not consistent with Simpson et al. (2003).

ACPD

9, C1600–C1606, 2009

p.9604, Eq. 1: Please explain how the mixing length is obtained.

p.9604-5, Eqs. 1 and 2: These equations should be written in terms of finite differences rather than derivatives.

p.9605, Eq. 3: This is not consistent with Simpson et al. (2003).

p.9605, lines 6 and 12: Please use different symbols for different model layers.

p.9605, line 8: Why "recalculated"?

p.9605, line 12: As K_H is a constant, dK_H/dz = 0 by definition. Please reformulate to indicate that you mean dK(z)/dz = 0 at z = H.

p.9605, Eq. 5: z/L should be replaced by H_S/L.

p.9606, line 20: These values are not taken directly from Jericevic and Vecenaj (2009). That study presents values for both K_m and K_h; C(K) = 0.13/0.06 and C(h) = 1.52/3.73 for m/h. Please clarify.

p.9607, line 8: "ABL" should read "ABL height".

p.9607, Eq. 13: Ri_B depends on the level j, so you could write Ri_B,j.

p.9608, line 5: But K_H is rather unimportant and K _H_S is obtained from Monin-Obukhov similarity theory.

p.9608, lines 7-10: In the EMEP model, a Ri-based method is used in stable conditions.

p.9608, lines 10-12: Repetition.

p.9608, line 13: Why "vertically integrated"?

p.9609, line 5, "both schemes": Unclear which schemes are referred to.

p.9609, line 11: Please define r.

p.9609, lines 21-24: This conclusion is not justified. It is of course possible that the

ACPD

9, C1600–C1606, 2009

Interactive Comment

differences could result from the processes mentioned, but there are a number of other equally plausible explanations.

p.9610, lines 10-11: The caption of Fig. 10 states that all stations are included.

p.9610, line 12: The title is misleading, as most of this section is about trends and the problems around the North Sea.

p.9611, line 10-11: Please explain why the coarse horizontal resolution in this case results in the overestimation of concentrations, while typically it overestimates horizontal dispersion and thus would underestimate concentrations.

p.9612, line 4, "used": For what?

p.9612, lines 17-21: RD(BIAS) (where BIAS actually means the absolute value of BIAS) is a bit difficult to perceive. Please explain why the maximum value of this metric is 100% (cf. Fig. 7).

p.9613, line 12: How did you define the "no change" case? Its frequency obviously depends on the number of significant figures recorded or a tolerance criterium applied.

p.9614, lines 7-8: All the stations that did not indicate improvement were not shown to have a "higher uncertainty in measurements". Please rephrase.

p.9614, lines 21-22: I would assume that the seasonal variation of emissions also plays a significant role here.

p.9614, lines 28-29: If this refers to Fig. 10, then it was previously explained that the stations with highest uncertainties were removed. Anyhow, this sentence can be removed.

p.9615, line 17: typo

p.9616, lines 12-13: All the values shown in Fig. 11c are not within these ranges.

p.9616, line 18: According to Simpson et al. (2003), a minimum H of 100 m is enforced

ACPD

9, C1600–C1606, 2009

in the EMEP model.

p.9617, lines 19-20: Unclear which model is referred to.

p.9617, lines 26-: Methods should be described in Section 2.

p.9619, line 2: Fig. 15m does not exist.

p.9619, lines 9-10: How do you compare the boundary layer height "in the surface layer"?

p.9620, line 15: It is not clear why a less diffusive scheme would be an "important preference". Please rephrase.

p.9620, lines 23-24: Unclear what is meant by this sentence.

p.9623, line 7: A wrong page number.

Figs. 9, 12, 14 and 16: The data points should be indicated and connected by direct lines instead of a curve based on an unspecified function.

Fig. 10: Units missing. The lines should be explained. I am not sure if this figure is needed (or could perhaps be enhanced by including some statistics).

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

ACPD

9, C1600–C1606, 2009

Interactive Comment