Atmos. Chem. Phys. Discuss., 8, S3340–S3343, 2008
www.atmos-chem-phys-discuss.net/8/S3340/2008/

Atmospheric Chemistry and Physics Discussions

Interactive comment on "Ship plume dispersion rates in convective boundary layers for chemistry models" *by* F. Chosson et al.

Anonymous Referee #1

Received and published: 4 June 2008

In this work a Lagrangian particle dispersion model (LPDM), driven by large eddy simulation (LES) winds, is used to simulate the the evolution of a ship plume for a series of marine convective boundary layer scenarios. It is demonstrated that the dispersion follows three regimes, an initial one for timescales shorter the large eddy turnover time in the boundary layer (approx. 20 mins.), during which the plume spreads to fill the vertical extent of the boundary layer, a transition regime, and a regime for later times during which the plume primarily diffuses horizontally. The effect of a realistic buoyancy flux at the ship stack is considered, and it is demonstrated that unless the buoyancy flux is very high and the stratification at the top of the boundary layer is weak (i.e. the BOMEX scenario), the buoyancy flux is insufficient for much plume material to penetrate above the level of the inversion. Plume entrainment rate functions are estimated as a function of time from release, and constant value approximations to this function (for use in chemistry transport model paramtrizations) are also given.

The paper seems to me to be technically sound and the results both contribute to understanding of ship plume dispersion and point towards ways in which this dispersion might be parametrized in large-scale models. I am not sufficiently familiar with the subject area to comment on the originality of the modelling approach, but the authors have taken some care over applying their work directly to the specific problem of buoyant ship plumes. I would therefore be happy to see the work published in ACP after some consideration of the comments below.

Specific Comments

- I found the approach of Lamb (1978) taken in this paper to be an excellent choice in reducing the dimensionality of the system, and its use seems entirely appropriate for the ship scenarios considered. However, my feeling is that the utility of this approach might be much better described in the paper. Convolution-like integrals such as (6) commonly appear in Green's function solutions of PDEs in various branches of applied mathematics. However, comparison with their use in similar problems suggests several problems with (6) as written.
  - The same 'current position' coordinates (either (x, y, z) or (x_p, y_p, z_p)) should appear on both sides of the equation. Otherwise it does not make sense.
  - The time-like argument of p_1 should be $t - t'$ (not t'). This is because the integrand represents the influence of the source emitted at time t' on the concentration at time t, i.e. a time $t - t'$ later. The correct formulation preserves the convolution-like form of the integral.
  - The expression (6), with (U, V) as the mean wind, is the solution for a sta-

tionary source. It seems to me that the solution for a moving source is exactly identical (under the assumptions for which (6) is valid). In this case one need simply intepret (U, V) as the velocity of the frame advevted by the mean wind, relative to the moving source, i.e. $(U, V) = U_{\mathrm{wind}} - U_{\mathrm{ship}}$. I believe a chance has been missed to compare plume evolutions at different relative wind velocities (i.e. different ship directions). It is not necessary to recalculate p_1 for this, merely to apply (6) at several different relative wind velocities. Perhaps the authors could consider adding such calculations in a revised version?

There should also be a more explicit statement of some of the assumptions underpinning (6). Its validity must follow from assumptions about the (statistical) self-similarity of the convective turbulence in the marine boundary layer (MBL) under translations in both (x, y) and t, together with independence of the turbulent statistics from the mean wind velocity. The latter assumption cannot be entirely accurate as the influence of the sea surface must presumably be felt by the MBL to some extent (e.g. due to turbulent momentum fluxes from the surface).

- Beginning of section 4.1. More details are needed here describing exactly how $F(t)$ is obtained from the results obtained from (6). The implementation of the slender plume approximation might be made more quantitative, e.g. is p_1 calculated only as a function of cross-stream and height coordinates, in addition to time from the source?

- The values of (U, V) used in the calculation of $C(x, y, z, t)$ do not seem to be explicitly stated anywhere. Is it the case that they not enter the calculation as a consequence of the slender plume approximation?

- Pg. 6801 l. 20. Define PANAMAX.

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Interactive Comment

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- Regarding equation (12), it might be worth commenting that isotropic Fickian diffusion (which leads to a Gaussian plume model) in N-dimensions leads to $b = 1$ and to $at_* = N/2$. The best fits in Table 2 suggest that the present results are close to $b = 1$ and $at_* = 1$, suggesting that two-dimensional diffusion in the slender plume approximation is a fairly accurate description of the plume evolution for the data being approximated here.

- Which boundary layer scenario does Table 2 refer to?

- Pg. 6799, l. 25. 'identify' $\rightarrow$ 'identified'.

- Pg. 6802, l.21. 'However, the heat flux at...'. This sentence is repeating a point made clearly in the preceding paragraph.

- I am unconvinced by the authors' argument concerning the advantage of approximating (12) by a single timescale as in (13). The expression (12) is hardly complicated, and I don't see any obvious reason why a parametrization of plume effects, for use in a large-scale model could not be derived based directly on (12). The result stated on pg. 6808, l. 8, that a constant τ gives a good model of far field plume dispersal, seems in direct conflict with (12), which seems to me a more physically plausible result. Also, it should be highlighted as clearly as possible in the text that the estimates given in Fig. 8 are for the plume evolution for $t >> t_*$ only.

- Abstract. There are several missing definite and indefinite articles here.

Interactive comment on Atmos. Chem. Phys. Discuss., 8, 6793, 2008.

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