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Letter to the Editor

An experimental study on the evolution and dispersion of a cloud heavier than air, by M. Ayrault, J.-L. Balint and R. Morel, Journal of Hazardous Materials, 26 (1991) 1–26.

Dear Editor,

I read this paper with a considerable amount of interest. There are, however, a few points in it that I think are worthy of comment, viz:

(i) pp. 8–9. So far as the indirect method of calibrating the mean concentration with the grey level is concerned, it is not clear how stable the finally adopted calibration curve is with respect to variation in the predictions of the Mercure Gaz-Lourd (MGL) numerical model.

(ii) p. 9. Estimates of the skewness and flatness of the concentration field are very sensitive to sample size. Assuming a Normal ensemble, the standard deviation (SD) of the skewness is $(6/N)^{1/2}$ and the SD of the flatness is twice as great at $(24/N)^{1/2}$. Substituting $N=34$ as in the paper gives SDs of 1/6 and 1/3, respectively. In practice the SDs are likely to be even larger, under the influence of non-Normality. In practice this means that the skewness and flatness pictures (not shown in the paper) corresponding to the mean and SD pictures in Fig. 21 (pp. 19 and 20) would look very mottled, and would presumably need some auxiliary spatial smoothing to augment any inherent smoothing due to spacial coherence.

(iii) p. 19 bottom. Higher-order movements are not all that useful as indicators of distributional form. The best way of assessing the skewness and flatness of a distribution relative to the Normal case is to look at the odd and even parts, respectively, of the standardised quantile excess function

$$\delta Q_0(u)=Q_0(u)-Q_0{}^N(u), \quad 0<u<1$$

where

$$Q_0(u)=\frac{Q(u)-Q(0.5)}{2(Q(0.75)-Q(0.25))}$$

and

$$Q_0{}^{\mathrm{N}}(u)=\frac{\Phi(u)-\Phi(0.5)}{2(\Phi(0.75)-\Phi(0.25))}$$

Here, $x=Q(u)$ is the inverse of the (sample) cumulative distribution function $u=F(x)$ and $\Phi(\cdot)$ is the corresponding function for the standard Normal distribution. It is also worth considering the effect of grey-level quantization on the collected statistics when the concentration is low — a point not mentioned in this paper.

(iv) p.22. When $2\alpha = 0.05$ and $N = 34$ the half-width of the confidence interval on the mean is only equal to 0.035, as stated in the paper, when $S = 0.1$. Reference to the RMS concentration profiles in Figs. 23, 24 and 25 show that this is generally the case, but the implied condition on S should have been made explicit. It would not be possible on the argument used in the paper, to fix $N = 34$ unless it were already known that $S = 0.1$. Ayrault et al. must have had prior knowledge. On the other hand, the plot of S_{MAX} in Fig. 22 shows that $S_{MAX} > 0.2$! Some explanation is called for.

(v) p. 24. The first sentence should read:
... defined by the ratio $I = \bar{C}S$ ...
for the sake of consistency.

I should very much like to know what Ayrault et al. have to say about the points I have raised.

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Authors' reply

Dear Editor,

We send you some responses to the comments of John Davies.

(i) With the indirect calibration procedure, the numerical results are considered as references. Our first mean image M1 is compared with the corresponding numerical results. The grey-level values, associated with the numerical concentration values provide the calibration curve C1. The aim of the indirect method is to obtain a linear relation between the concentration and the grey-level values. After four iterations, the relation is linear (Balint, Ph.D. Thesis, Ecole Centrale de Lyon, 1982). Unfortunately, each iteration corresponds to a loss of information. This is the main reason we have defined a threshold and considered only two iterations.

(ii) In our case, given the small number of samples, the skewness and the flatness pictures looked very mottled. The accuracy of these statistical moments is too low. A spatial smoothing, which is an image enhancement, cannot increase the accuracy of these results but only the visual perception of the pictures.