

LETTER TO THE EDITOR

Reply

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In our paper (Chatwin and Allen, 1985a), we have tried to point out some *fundamental* points, that seem to be missed by many or misunderstood, both in geophysics and elsewhere. Since Professor Hasse has been moved to write in response to this paper, it is obvious that we have not done as well as we had hoped. We apologise for not being expert geophysicists and therefore missing some of the references quoted in paragraph 1 of Hasse's letter. However, we should like to take the opportunity here to reply to the points raised by Professor Hasse in his letter as well as clarifying in a more general sense the reasons why we originally wrote the note and why we believe our approach to be both valuable and important.

In line 12 of our paper, referred to in the second paragraph of Hasse's letter, we, of course, mean the "same time" relative to the start of an experiment or realization. This is *not* a debatable statement; it is simply a definition of an ensemble average. It is obvious that an ensemble must be properly described (Chatwin and Allen, 1985b, p. 123) so that we can decide whether a single realization or experiment should or should not belong to that ensemble. There appears to be some confusion in Hasse's letter, perhaps partly for the reasons stated as "everybody . . . thinks of, but does not care too much for ergodicity". We agree with Hasse's points in paragraph 4 of his letter regarding different periods in geophysical problems; the use of ensemble means in various cases of geophysical interest has already been discussed by us in Chatwin and Allen (1985b, p. 125).

Hasse discusses spectral analyses and shows how trends can be eliminated. We thought hard about using a spectral approach but decided against it for the following reasons. Such approaches are commonly used in statistically

steady problems of atmospheric and oceanic turbulent flows but, as is well-known, many geophysical flows and dispersion problems are not stationary, so spectral analysis no longer has algebraic advantages. Whereas stationary dispersion implies stationary turbulence, stationary turbulence does not imply stationary dispersion, e.g., a finite cloud of pollutant grows on the average. Accordingly, various different data analysis techniques must be applied to try and produce quasi-stationary series for different parameters, as discussed by Soulsby (1980) and in Hasse's letter. In addition, even if the dispersion is stationary, averaging techniques have to be applied which bring in the errors described in our paper.

We feel that the important points are (a) that workers should be aware of these different sources of error, and (b) to present our argument so as to apply to all cases, stationary and non-stationary.

There is no misunderstanding in the statements below eq. (6) in Chatwin and Allen (1985a). In the definition of $C_T(x, t)$ given in eq. (5), and referred to in paragraph 5 of Hasse's letter, it is arbitrary and irrelevant whether t or t_0 is used (just like choosing the colour of a car). It appears that Hasse has misunderstood this point, and the sentence "In that case, the time average of the deviation from the mean is exactly zero" in his letter is wrong, as our examples show. The analysis and conclusions in Section 2 of our paper are equally valid whether we calculate values at time t or t_0 .

We urge geophysicists, meteorologists etc., like Hasse to re-examine what they are doing from a fundamental viewpoint. It appears that some basic principles are overlooked or ignored in the avalanche of spectral, bispectral, cross-spectral analyses and experimental techniques which are applied in the analysis of geophysical data.

REFERENCES

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- Soulsby, R. L. 1980. Selecting record length and digitization rate for near-bed turbulence measurements. *J. Phys. Ocean.* 10, 208–219.