Reply to comments by S. B. Feldstein and W. A. Robinson on 'Spatial structure of ultra-low frequency variability of the flow in a simple atmospheric circulation model' (October 1992, **118**, 1211–1233)

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SUMMARY

The note by Feldstein and Robinson (hereafter referred to as FR) raises two questions. The first is whether the EOF1 of zonal wind in the numerical integrations that we carried out is correlated with the atmospheric angular velocity or with the mean angular momentum. The second is whether the ultra-low-frequency fluctuations which we observed have any dynamical significance or are merely 'climate noise'. We reply to each of these issues in turn, and are grateful to FR for raising some interesting questions about our results.

1. ANGULAR VELOCITY AND EOF1

We accept their claim that the correlation between angular velocity and EOF1 is not necessarily established by the coincidental similarity of their spectra. However, in the particular runs that we described, this correlation is larger than that found by FR from their runs. Indeed, this correlation is very sensitive to the details of the model formulation. This is immediately clear from the form of EOF1, which consists of alternate easterly and westerly jets, and so its projection onto the atmospheric super-rotation involves a great deal of cancellation. Small changes in the structure of EOF1 lead to different projections onto the atmospheric super-rotation, and indeed can easily change its sign.

Our EOF analysis was not intended to explore the behaviour of atmospheric angular momentum as such, but rather to determine objectively the most important flow structures involved in the ultra-low-frequency variability of our model. By chance, ultra-low-frequency variability was first noted by examining a time series of atmospheric angular velocity. However, it turns out that these flow structures are not concentrated in the tropics (as might be suggested by FR's argument) but in the winter mid latitudes. We suggest that the reason for this is that eddy torques in the tropics are small for a terrestrial type of circulation with tropical easterlies; the other source of tropical variability would be Coriolis torques acting on the meridional circulation, which, as FR point out, are also small. Both these sources of torque in our simple model are linked to eddy events originating in the mid latitudes. If EOFs are sought not of $[u]$ but of $[u] \cos \phi$, which is proportional to the specific angular momentum, then very similar structures, which are confined to the winter mid latitudes, are found.

A mechanism which could induce greater tropical variability in the $[u]$-field could be provided if there were strong feedbacks between the large-scale tropical flow and the subgridscale convection. Our model contains no representation of moist convection, and so this might give undue weight to the resolved, sloping convection of the mid latitudes.

A curious result is found for our PC2 which appears to have more significant relationships with specific angular momentum. The correlation of PC2 with ξ_0^1 is -0.37, whereas the correlation with the mass distribution terms in the global atmospheric angular momentum is $+0.52$. When PC2 is correlated with the total global atmospheric angular momentum, the two effects cancel each other nearly exactly, so that the correlation coefficient is very close to zero—for which we have no explanation.

2. CLIMATE NOISE

FR draw a distinction between the dynamically driven, low-frequency fluctuations and the stochastic climate noise. They suggest that the low-frequency fluctuations which we found were simply climate noise.

In a model as simple as ours, such a distinction is unclear. Since external forcing is fixed, and there is no coupling between the atmosphere and any other system, all fluctuations are ultimately the result of internal, nonlinear dynamics. Whether one terms this 'noise' or not is a matter of

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degree, not quality. If the fluctuations could be modelled by some low-order set of approximations to the full dynamics, presumably they would not be termed 'simply noise'. If the fluctuations appear only when the full set of equations is retained, then perhaps random stochastic noise is a useful parametrization of the effects of unstable, high-frequency transients on the time evolution of the system.

Results to be reported in a subsequent paper (James *et al.* 1994) suggest that the low-frequency fluctuations of the zonal wind in our model are not purely random. When the evolution of the zonal wind is represented by a trajectory on a PC1–PC2 plane, a systematic circulation is found, rather than a random walk through the populated parts of the PC1–PC2 plane. This circulation results from a number of discrete, large-amplitude events which occur sporadically in time. Each event has a great deal in common with the variations of the zonal flow produced by individual, large-amplitude baroclinic life-cycle events of the type discussed by Simmons and Hoskins (1978) and many other authors.

Following on from the work reported by James *et al.* (1994), we would now suggest that the low-frequency fluctuations of our simple atmospheric circulation model have both stochastic and low-order elements. Whether or not these fluctuations are to be regarded as random noise, requires that the nature of the mechanisms underlying them should be understood. Then the relative roles of internal dynamics and external forcing in generating observed and predicted low-frequency circulation fluctuations may be separated in a quantitative manner.

REFERENCES

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