Reply

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23 September 1998

We thank the author for thoughtful comments on the construction of statistical models of atmospheric variations (SAMs) in midlatitudes. The points he raises deserve consideration in studies that might use such models. We wish to add several comments to Frankignoul's (1999, hereafter F99) note in the following.

As shown in Xu et al. (1998, hereafter XBL) the main results are due to excitation of the Pacific decadal oscillation (PDO) by the wind stress curl. As such, the physics is not unlike those of the ENSO, a situation under which F99 acknowledges that the type of model used in XBL is appropriate. The addition of heat flux, which F99 sees as a potential problem, was shown in XBL to affect the amplitude of the PDO, but not its existence. So the main result in XBL seems little affected by the cautionary comments.

The details of the cautionary note involve numerous assumptions, particularly the partition of a simplified heat balance equation into a number of different elements. We have no way of knowing whether such a partitioning, which is responsible for the result that F99 derives, is meaningful in the real world.

In any event, F99's results depend entirely on a parameter ''n,'' which is stated to be the ratio of the variances of two first-order Markov processes: one representing stochastic forcing (q), and the other atmospheric fluxes that do not include a feedback (m). Based on statements in the text and Eq. (5), we would have thought that q and m were random processes; the assumption of an AR1 process, which needs to be justified for monthly data like that used in XBL, partially determines F99's answer. In any event, the potential bias in the SAMs is shown to be proportional to $n^2/1+n^2$, for example, Eq. (6). If n is large, the bias becomes trivial, while if n is very small, the reverse is true.

The problem is to estimate from observations the approximate value of n. This is not done explicitly in F99's note, and indeed, we suspect it cannot be done in practice. In fact, even the definitions of the key quantities, q and m, are not clear to us from the note. F99 mentions that H, say, the turbulent heat flux in his example, can be estimated. But that gives no clue as to the relative sizes of q and m.

Several other details remain. The coupling coefficient defined in (6) should not have a negative sign in front of it. This error, if corrected, would change the sense of F99's comment entirely. In XBL the wind stress went into the momentum equation, not the heat balance equation as F99 states. Finally, the positive feedback cannot by itself make the system oscillate as F99 states. That requires a negative feedback.

In spite of the above, we feel F99 has contributed an important caution in the use of generalized statistical models. In fact, we wonder if his concerns do not also apply to many of the ''tuned'' aspects of general circulation model codes. Unfortunately, until the partitioned quantities such as q and m are rigorously defined and important factors such as their variance ratio evaluated, it is difficult to decide just how much weight to place in F99's concerns. In any event, we appreciate his interest in our work and the gentlemanly fashion in which he offered his comments.

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