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LETTERS TO THE EDITOR

Reply to Wiin-Nielsen's comment on simple climate models with periodic and stochastic forcing

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In his comment on our paper "Stochastic resonance in climatic change" (Wiin-Nielsen, 1983), Wiin-Nielsen states that to satisfy the requirement of three steady states in a range of 10 K around the present mean global temperature, "radical" changes must be made in the physical formulation of a globally integrated energy balance model. To us it seems very interesting to consider what radical change we have made to the physics of any Budyko-Sellers model (globally averaged) in constructing our own. As noticed by Wiin-Nielsen, these changes are in the albedo parameterization. It may be illuminating to compare our formulation of the albedo with those traditionally considered, for example $A + BT$, where A is of order 1, $B < 0$ is of order 10^{-3} K^{-1}, and T is the temperature in degrees Kelvin (see Fraedrich, 1979). As clearly stated and often restated in our paper, our formulation is required to be realistic only in a temperature range of about 10 K. Let us see how much we do change the albedo, taking as a measure the percentage variation of the albedo in the temperature range considered. Using obvious calculations, we find that the albedo changes by only 1%. We cannot see how such a change can be considered radical or the formulation which produces it any more unphysical than the usual monotonic functions. We are not alone in invoking a different parameterization of the albedo in this range (see Battacharya et al., 1982 and the physical reason explained there), and there are several physical mechanisms that may change the albedo even more, such as cloud distribution, dust, geographical distribution of vegetation and snow cover and so on. Our albedo formulation is not less physical within our stated temperature limit than others currently used.

Moreover, the fact that such a change would explain features of the climatic record is the predictive value of our theory that otherwise would be merely curve fitting. However, we recognize how difficult it might be (with today's knowledge) to discern such small changes from any existing observational network. Hence we did not (and do not) claim so much. Wiin-Nielsen's other remark, concerning the singularity in the albedo formulation, at this stage appears irrelevant since the value of temperature for which the divergence occurs is well outside the range considered as useful. However, the same divergence problem was found by Fraedrich (1979). In avoiding unphysical divergence for low temperature, he introduced an arbitrary cut-off of the albedo. The problem resides in the impossibility in a globally integrated model to incorporate a non-linear feedback mechanism such as that in Seller's model.

In conclusion, we do agree that it is difficult to reproduce climatic transitions, but for a reason opposite to that given by Wiin-Nielsen. There would be less difficulty in understanding and verifying the climatic changes ranging over 100 K, as for instance is allowed in the paper of Nicolis' (1982). In that case the accompanying albedo changes would be large enough to be detectable even with present day knowledge. But in that case, a very large noise would be needed to produce a quasi-periodic transition if a realistic heat capacity were used, as pointed out by Wiin-Nielsen.

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