

LETTER TO THE EDITOR

Reply

By ARNE M. BRATSETH, *Institute of Geophysics, University of Oslo, P.O. Box 1022, Blindern, Oslo 3, Norway*

(Manuscript received December 12, 1985)

In their comments on my paper "A note on CISK in polar air masses", Huang and Källén suggest using the structure function (3) to describe the diabatic heating. Close inspection reveals that there is no reasonable physical correspondence between this function and the vertical velocity field (2). The rising branches are treated differently. Some are heated and some are not! It is not surprising that this peculiar choice does not satisfy the perturbation equation (1). If the heating is modeled by using $\cos(kx)\cos(l y)$ in the rising branches and 0 in the sinking branches, then the leading terms of the Fourier-expansion are $2/\pi^2 + 0.5 \cos(kx)\cos(l y) + \dots$. This corresponds well with the treatment in my paper.

I have never claimed to have incorporated all

aspects of the horizontal asymmetry of the horizontal heating. The purpose of the reduction factor γ was to compensate for the obvious overestimation of the average differential heating which will take place if the sinking air is cooled at the same rate as the rising air is heated. My Fig. 1 and the reference to the Fourier-expansion $1/\pi + 0.5 \cos x + \dots$, was only meant as a motivation for introducing such a device. Certainly, this correction does not lead to any qualitative change of the model. In the polar lower literature, however, similar models have been used for quantitative purposes without paying any attention to this problem. My intention was to warn against this practice.

REFERENCE

- Bratseth, A. M. 1985. A note on CISK in polar air masses. *Tellus* 37A, 403–406.