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Ocean Science
Discussions

Interactive comment on "The instability of diffusive convection and its implication for the thermohaline staircases in the deep Arctic Ocean" *by* S.-Q. Zhou et al.

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In this paper the authors have applied the classical linear stability results for a double-diffusive stratification of the diffusive type to the deep thermohaline staircase of the Canada Basin, Arctic Ocean. There are a number of arguments made in the application of the theory to the observations that I have found difficult to follow. I list some of the difficulties that I have had below.

(1) The last sentence of the paragraph around line 20 on page 1350. I don't understand what assumption is being referred to here. As far as I understand the authors have used the linear stability theory developed for a layer of height L, with constant gradients of

T and S inside (i.e., linear T and S profiles) that is bounded by rigid free-slip surfaces above and below. This has been applied in this section to the interfaces themselves, i.e., neglecting the fact that there are mixed layers on either side and assuming rigid boundaries. According to the theory this system predicts that an (oscillating) instability will occur when R_rho<1.15 (approximately). If this very idealized setup is assumed to apply to the interface region only then there is no instability because we have all interfaces with R_rho>2. Is this a correct interpretation?

(2) If I have understood correctly, the authors next seem to apply the same linear stability model to the boundary layer. They make a choice for the salinity Rayleigh number that is based on the mean background gradient averaged over many of the staircase steps, and cite the work of Turner (1968). I don't see how this is justified since the boundary layer of an interface within a much larger staircase certainly can not know what the large-scale mean gradient is. In the Turner experiments he studied the formation of a staircase into a uniform salinity gradient by heating from below – in this case the argument of using the background gradient is valid because that is the gradient in the boundary layer. Can the authors please comment on this interpretation of their analysis?

(3) In general, as far as I can see, the linear stability theory that has been applied is for a different type of instability than is present in a double-diffusive staircase, i.e., at an interface. The theory used is for the classical oscillating instability of smooth (linear) gradients of T and S, in which the density ratio 1 < R_rho < 1.15, or if R_rho<1 then a salinity stabilized convection. Carpenter et al. 2012 (see references in the discussion paper) have showed that the instability that is present at a diffusive interface (without rotation) is in agreement with the Linden & Shirtcliffe (1978) view, namely, that the T interface grows much faster than S due to the larger diffusivity, and produces unstable thermal boundary layers. The instability is a direct convective mode of the diffusive boundary layers, not of the oscillating diffusive convection type. The analysis shows that the boundary layer length scale can not be taken as the interface thickness

because the salinity stratification is overwhelmingly stable (otherwise you would not have a staircase, it would all be completely mixed), and can not be considered as the mean background gradient. On top of all of this is the fact that the instability at a diffusive interface is due to the thickening T interface (relative to S), and this changing background state violates the assumptions of a stability analysis.

For the authors additional information, the influence of rotation on the heat flux across double-diffusive interfaces has been addressed by Kelley (1987), and very recently by Carpenter & Timmermans (2013). They propose that the relevant control on rotation influencing the diffusive convection is the relative thickness of the thermal interface and the Ekman length.

References:

Carpenter, J.R. and M.-L. Timmermans (2013): Does rotation influence double-diffusive fluxes in polar oceans? J. Phys. Oceanogr., early online release, doi: 10.1175/JPO-D-13-098.1

Kelley (1987): The influence of planetary rotation on oceanic double-diffusive fluxes. J. Mar. Res., 45, 829-841.

Linden, P.F. & T.G.L. Shirtcliffe (1978): The diffusive interface in double-diffusive convection. J. Fluid Mech., 87, 417-432.

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