

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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Please see the attached PDF file.

Please also note the supplement to this comment: http://www.ocean-sci-discuss.net/10/C726/2013/osd-10-C726-2013-supplement.pdf

Interactive comment on Ocean Sci. Discuss., 10, 1343, 2013.

C726

Response to the comments of Dr. Brown.

Dear Dr. Brown,

Thanks for your efforts in reviewing our manuscript. We appreciate your strong recommendation for publication. Below are our responses to your comments.

recommendation for publication. Below are our responses to your comments. We agree with you that there is no much difference to use Ran₁ or Ran₁ to represent the thermal Rayleight number of boundary layer at the current carcural yeal est also win in Figure 5. There is only a factor of 2 herveen them. In most of the previous studies (e.g. Palanna & Dillon (1989)), the interface has been directly treated as the boundary layer. Our concern is minity based on the theoretical reasons. As shown in Figure 1 of the manuscript, there are convective rolls in each mixed layer. From the classical fluid mechanism, it is known that the boundary layer has its own characters. It is relatively hand to imagine the covarcive rolls in the neighboring mixed layers could share the same boundary layer. In fact, we do find that the horizonal velocity is zero at aroand micpoint of interface in the laboratory experiment. Similar observation can be found in numerical simulation (Carpenter et al. 2012). This may econvecting layers in some sense. That is why we treat a half interface as the boundary layer thickness of one convecting layer, so does the corresponding Rayleigh number.

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J. R. Carpenter, T. Sommer and A. Wiest (2012). Simulations of a double diffusive interface in the diffusive convection regime. Journal of Pluid Mechanics, 711, pp 411436 doi:10.1017/ jfm.2012.399

Best regards.

The Authors