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Interactive comment on "The wind-driven overturning circulation of the World Ocean" *by* K. Döös

Anonymous Referee #2

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Review of "The wind-driven overturning circulation of the World Ocean" by K. Doos

This manuscript describes a simplified and nearly analytically tractable model of the global-scale overturning circulation. Thermodynamic processes are implicitly included by specifying the outcrop latitudes of the interfaces, the depths at the eastern boundary of those interfaces, and specifying the zonally integrated transports across each basin of each layer. The spatial structure of each layer can then be found by integrating equations similar to those of Luyten et al. (1982). The similarity of these structures to the ocean and the sheer ability of this very simple model to reproduce some aspects of the overturning circulation is offered as a demonstration that the overturning circulation can occur without interior ocean diapycnal mixing.

I fully accept the idea that upper branch of the overturning circulation can be closed without resorting to interior ocean diapycnal diffusivity. But this idea has been in the literature for years [e.g. Toggweiler and Samuels (1993, 1995, and 1998) or McDermott (1996)]. This manuscript does very little in my judgment to advance the credibility of this well-established and widely accepted idea.

I also have difficulty finding the predictive value of this new model for understanding the dynamics of the ocean. The specification of a long list of outcropping latitudes, depths, and transports would seem to allow the model to reproduce almost any more sophisticated ocean model. The fact that the simple model can reproduce OCCAM appears to me to be built into the parameter choices for the model.

The net meridional transports of various layers are determined from the Ekman transport at key latitudes. For the Southern ocean, this follows ideas developed by Toggweiler and Samuels (1993; 1995; 1998 - none cited here) and later tested in coarse resolution GCM sensitivity studies by Gnanadesikan and Hallberg (2000). The validity of this assertion that the Ekman transport must match the net transport in the upper layer has been refuted in numerous studies of the role of eddy form drag or bolus fluxes in the dynamics of the Southern ocean (see, e.g. Marshall et al, 1993 or Olbers, 1988).

At other latitudes (for example in the Pacific, p. 485, line 27), the identification of net transport in a layer with an Ekman transport stems from an overly rigid assertion that the outcrop lines are exactly zonal and static, with unrealistic delta-functions of buoyancy forcing implied. These outcrop lines would coincide with the fronts associated with western boundary currents, and there is every reason to think that there can be significant eddy fluxes of the lightest water here that balances the Ekman transport.

In summary, the manuscript presents a very simple model that, with a given choice of key parameters, reproduces the vastly more complex OCCAM's overturning circulation. However, these externally set parameters (which really should be governed by ocean thermodynamics) appear to be sufficiently important that I am dubious of the predictive value of this very simple model. Moreover, the central idea that this paper advocates, that an important component of the global overturning circulation is largely driven by

Southern Ocean winds, is already very well established, and this paper does little to further develop this idea. I can not recommend publication of this manuscript.

Additional comments:

The Rayleigh drag coefficient \Gamma, really should not be described as a "bottom friction coefficient" (line 8, p. 477). Were it so, it would only apply in the (motionless) layer 4. Relatedly, the western boundary layer is not appropriately described as "Stommel layer" (line 13, p. 476), which would be governed by a proper bottom drag. I realize that this Rayleigh drag closure is mathematically convenient, but it should not be confused with the traditional Stommel boundary layer.

The vorticity and potential vorticity equations on lines 16 and 19 of p. 490 are inconsistent with Eqs. (2)-(4). These use part of a Laplacian viscosity instead of the Rayleigh drag in (2)-(4). So the last term on line 16, "... + A\frac{\partial^3 v_n}{\partial x^3}" should instead be "... - \frac{\partial \Gamma v}{\partial x}", with a similar change to the first term on the right had side of line 19. Additionally, the Ekman pumping term on the right hand side of line 19 is dimensionally inconsistent and should be divided by h^2.

The sentence on lines 6-7 of p. 291 is missing a verb; I cannot figure out what was intended here.

References:

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