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Geoscientific Model Development Discussions

Interactive comment on "An axisymmetric non-hydrostatic model for double-diffusive water systems" *by* Koen Hilgersom et al.

Anonymous Referee #2

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The paper describes a 2-DV formulation of the equations governing double-diffusive problems. The paper is focused on mathematical details, and the physics of the investigated problem is not properly considered. In my opinion, the paper cannot be accepted for publication in the present form.

Major remarks

1) What is novel in an axisymmetric model? As pointed out also by Referee #1, the main objectives are not clear. Moreover, practical applications are not discussed (see also next comment).

2) The necessary assumption to formulate the 2-DV model is axial symmetry. However, there is no discussion whether such a symmetry exists in real double-diffusive cases. For instance, the axial symmetry implies that salt fingers are not real "fingers", but

"circles" that develop around a central location. Is this reasonable? This is probably the major limitation of this work.

3) The model is not a DNS model, but a standard RANS model with a k-epsilon model. This means that results are dependent on the parameterization of turbulence, and that the model requires calibration and validation. This is an even more demanding issue in double-diffusive phenomena, which are at the transition between laminar and weakly turbulent flows. I believe that a standard k-epsilon model is not suitable for these conditions, so the whole model formulation is questionable. At least, it cannot be sold as a model that does not require calibration.

4) No comparison is provided with laboratory and/or numerical experiments. Only qualitative analogies are discussed, apart from the case of the central inflow (which is likely dominated by advection and not double diffusion). The authors should try to validate their results at least against DNS.

5) One of the major advantages of this formulation is the consideration of the free surface. However, I cannot see where this is a crucial aspect in double-diffusive problems. To my knowledge, these phenomena occur in deep water and are typically not influenced by the dynamics of the free surface, so the authors should explain why this characteristic is important.

6) The formulation contains some errors (see comments below).

7) The literature review is incomplete and, especially for double diffusion in the diffusive regime, outdated. For instance, no reference is given to recent DNS work, both 2D (e.g., Noguchi & Niino, 2010a,b) and 3D (e.g., Kimura & Smyth, 2007; Carpenter et al., 2012, Sommer et al., 2014). Moreover, papers that analyze the thermohaline staircase (e.g., Radko et al., 2014a,b) could be used to find cases to compare with.

Detailed comments

- l. 10, what is a "diffusivity driven flow"? Double diffusion is due to differential diffusivity, but the flow is always driven by density gradients.

- l. 57-58, “the momentum and mass conservative grid setup allows accurate modelling of transport processes”: not clear.

- l. 70, “model code does not require the calibration”: this would be true for a DNS, but certainly there are parameters that are not exact in the turbulence description used in this model (see also one of the major remarks).

- l. 71, “validated”: I cannot see any real validation of the model in this paper.

- l. 88-90, “the horizontal kinematic viscosity nu_h is set uniform to its molecular value ($\sim$ 10−6 m2s−1). The non-uniform vertical viscosity nu_v includes the local eddy viscosity, as calculated by the standard k-epsilon model”: why the horizontal viscosity should be characterized by its molecular value and the turbulent component added only to the vertical viscosity? I cannot see any reason why local turbulence (if present in this small-scale problems) should be accounted only in one direction.

- l. 108-109, “To account for turbulent diffusion, D_h and D_v are calculated by adding the molecular diffusivities and turbulent diffusivities: D = D_mol+D_turb. The turbulent diffusivities are calculated by dividing the eddy viscosity nu_turb by the turbulent Prandtl number”. Does this sentence imply that the eddy viscosities are isotropic? (this is different from the previous point)

- eq. 6 is wrong. Not only there is a typo, i.e. “dQ/dt” is “dQ/dr”, but also the structure of the equation is wrong, inasmuch it is not formulated the cylindrical coordinate system.

- l. 117, “molecular heat and salt diffusion rates, which in turn are highly dependent on temperature and salinity”: I agree that there is a clear dependence on temperature, but the authors should explain why the dependence is relevant in double-diffusive problems that are typically characterized by very small temperature differences. In this respect, the dependence of density on temperature should be more important.

- l. 136, “dc/dr=0”: this boundary condition is wrong in cylindrical coordinates.

- eq. 13: “omega” is not defined; why “dy” and not “dr”.

- l. 164, “anti-creepage terms”: explain what are these terms.

- eqs. 20 and 21: units are missing.

- l. 214-215, why is the “central inflow” a representative case? (apart from being an axisymmetric case)

- fig. 5, what does the asterisk means in the caption “depth (*)”?

- l. 261-262, “the salt-fingers in Case 2 are hypothesized to transport more salt and heat”: this is an example of vague statements that are common in this paper. Why “hypothesized”? Can we see some numbers?

- l. 266-269: here the authors seem to be aware that there is a problem in describing salt fingers using axial symmetry. As I already pointed out, a field of salt fingers is not axisymmetric (a single finger can be, but not a number of them). This is one of the major drawback that needs to be clarified.

- fig. 7, where is the interface located? In a single point? Is it an average?

- l. 271-272 and fig. 8: where is the layer structure typical of double-diffusive processes?

- l. 290-291, “laminarisation”: I do not understand what the authors refer to. Which kind of flow exists before being laminar?

- l. 297, “diffusivity was on average for 0.5 % influenced by turbulent diffusion”: this is a very strange approach. If the turbulence model is working properly (which is the implicit assumption to be confident in using it), why reducing the calculated value of turbulent diffusivity?

- l. 299, “by applying the Prandtl-Schmidt number”: the procedure is not clear.

- l. 310, “whenever a situation is modelled that can be approximated by axisymmetry around a central location": please provide evidences that this situation exists and is relevant.

- l. 320-321, "The formation of convective layers and salt-fingers are in accordance with the theory of double-diffusivity": this statement is not demonstrated.

- l. 321, "quantitative validation method": I cannot see any general method for validating the model results in this paper. If the authors refer only to the case for the central inflow and to the fact that "the numerical model showed a similar radial expansion of the bottom layer as expected from analytical results" (l. 324), the result is not enough to justify the publication of this paper, considering also that the comparison is satisfactory only for a specific definition of the interface (l. 304-306).

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