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

Koen Hilgersom et al.

k.p.hilgersom@tudelft.nl

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We would like to thank Referee #2 for giving a thorough review of the manuscript and expressing his concerns. From the complete list of comments, the referee seems to be most concerned about the following issues: the axisymmetric assumption for double-diffusive phenomena, the application of a RANS model instead of a DNS model, and the lack of comparison with laboratory or numerical experiments. Part of these concerns may have been caused by the fact that the referee approaches the modelling of double-diffusive phenomena from a completely different perspective compared to our approach: the referee is clearly an expert in DNS modelling studies to double-diffusive phenomena whereas our RANS approach concerns a larger scale. In other words, we are interested in how double-diffusive systems behave on a larger scale (e.g., 'does a double-diffusive convective system evolve?', and 'how does the location

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of a sharp interface evolve over time?'), but we are not interested in fine simulations of the smallest-scale perturbations induced by double-diffusion.

In the following, we address the general comments point by point (Section 1), list our reactions to the detailed comments (Section 2), and provide an overview of the changes made to the manuscript based on the comments (Section 3).

1 General comments and answers

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).

This article provides a solution to model density-driven flows in an axisymmetric grid setup. One of the intended purposes of the model is the application in circumstances where double-diffusion potentially occurs. Although an axisymmetric modelling approach is not novel for CFD models (lines 41 to 49 mention examples for a variety of fields of research), it has to our knowledge never been applied in hydraulic free-surface models. The reason why an axisymmetric model can be favourable for our intended application is that we are dealing with a central circular inflow at the bottom boundary, where groundwater of a contrasting salinity and temperature enters the surface water. The axisymmetric modelling approach serves here to closely resemble the volumetric inflow of water and constituents. The aim is to simulate the double-diffusive system that develops, but not the locations of convection cells and salt-fingers, which can never be achieved with the selected model type (as pointed out by the referee in the following comments).

The existence of cases with very local inflows and circular water bodies was already mentioned in the Introduction (lines 50-53). Lines 63-64 mentioned that the developed framework is intended for local saline seepage sources. As I understand from the

comments by Referees 1 and 2, these descriptions are too brief and should provide more information on the intended application. We therefore developed lines 63-64 to a separate paragraph, so that our main objective is clearer: “The development of an axisymmetric variation of SWASH falls in line with our research to localized saline water seepage in Dutch polders. To simulate the effect of a local seepage inflow on the temperature profile of the surface water body, a numerical model is required that accounts for sharp density gradients, a free surface and potential double-diffusive processes. The axisymmetric grid set-up aids in correctly representing the volumetric inflow and modelling the flow processes around the local inflow."

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.

The referee is obviously right that the "salt-fingers" that develop are not real salt-fingers but circles around the centre of the axisymmetric grid. As pointed out before, our and the referees modelling considerations are completely different. It is not our aim to model exact locations of double-diffusive phenomena, but merely the general behaviour of the system: under the given conditions, will a layered system develop? This is a different question from whether we can approach the exact shape of a salt-finger. In the submitted manuscript, we apparently have not stressed this point enough. We will therefore better stress this at the end of the first paragraph, where the topic is introduced: when we inform the reader about the development of an axisymmetric framework, we will add that this is intended to incorporate the larger-scale effects of double-diffusion. The last sentence of the first paragraph (lines 19-21) will be replaced by: “In this article, we present a framework for a quasi 3-D finite volume approach that allows free-surface flow modelling in an axisymmetric grid. The model framework is intended for a shallow water body where salinity and temperature gradients potentially

induce double-diffusive processes. As such, the model intends to simulate larger-scale features of double-diffusion (i.e., interface locations in a stratified system and heat and salt transport)."

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.

The referee here notes that the model is a RANS model. As stressed before, this type of model is used with a different purpose as compared to the DNS models referred to by the referee. We think that the difference in modelling considerations has lead to different insights by the referee on how the model should be set up.

Further, the referee points out his belief that a standard k-ε model is not suitable for these conditions. We would like to ask the referee to better explain why he has this belief for these applications. In our eyes, the RANS approach requires a turbulence model to approach the effect of eddies, which due to the mesh size are not directly incorporated in the simulations. This is a major difference from DNS models, which do not require such turbulence models.

Turbulence models do require calibration. One of the reasons why we selected the standard k-ε model is that it has been applied for decades (Launder and Spalding, 1974) and has become the most popular turbulence model. The model's constants have therefore been confirmed in numerous studies. Like with any model, one should always keep a critical view when applying the k-ε model, but the historical experience with this model supports its apparent effectiveness.

On the other hand, it is not completely true that DNS models do not require any calibration at all. In the case of DNS models, a certain calibration comes back in the assessments of appropriate mesh sizes and the selection of numerical schemes.

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.

The referee is right that the quantitative case study with the central inflow is not dominated by double-diffusion, as it concerns a system of unconditionally stable layering. The other case studies consider the double-diffusion dominated systems merely qualitatively, but show that the model functions quite well near critical points where the stability regime changes. Considering the purpose of the model (see answers 1 and 2), these results do support the applicability of the model for its purpose. We agree that a comparison with DNS is recommendable to further test the modelling framework, and we added this as a recommendation to our Conclusions section. For the resubmission of our manuscript, we will add more validation cases that test the following based on flux laws published in Carpenter et al. (2012) and Radko and Smith (2012):

- salt and heat flux across a double-diffusive convection interface;
- interface thickness for salt and heat interfaces in the double-diffusive convection regime;
- salt and heat flux across a salt-fingering interface.

In comment 7, the referee suggests a comparison with the Radko articles about thermohaline staircases. These staircases usually have thicknesses of 20 - 50 m and occur in deeper waters. In contrast to the suggested literature, the intended application of our model typically concerns waters of maximum a few meters deep. Trying to apply the

model to deeper waters would a) surpass the aim of the model, b) would make the free-surface approach irrelevant, and c) would require a too large mesh in the vertical to be conveniently modelled within our model framework (as both a larger depth and a high resolution over this depth are required).

The additional simulations will require more time, and we will therefore ask the editor for an extension of the usual time until resubmission.

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.

This remark underlines the completely different starting points of the referee and us. The referee is right if the context would be the ocean but we disagree that this is the only context, as our study was initiated based on other cases. As pointed out in the introduction, double-diffusive phenomena also occur on smaller scales like boreholes and solar ponds (lines 36-39). Moreover, Hilgersom et al. (2016) have recently shown that double-diffusive phenomena like salt-fingers can also occur in small drainage canals. In such canals, but for example also solar ponds, the inclusion of a free surface is relevant, and potentially even crucial. We therefore present this method specifically for water bodies at these smaller scales.

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

We thank the referee for his critical view and address the comments in our answers to the detailed comments (Section 2).

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

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(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.

We thank the referee for these suggestions, which indeed form an important addition to the literature review. In the new version of the paper we will extend the literature review and pay more attention to especially also the DNS work (2-D and 3-D) that has been performed in this field of research. The suggested references will be part of this.

As explained in my answer to the fourth general comment, the papers by Radko will not fulfil to validate the model, as it concerns a problem in a deep water body. The papers by Carpenter et al. (2012) and Sommer et al. (2014) provide better comparable cases.

2 Answers to detailed comments

- l. 10: With “expected density and diffusivity driven flow and stratification", we referred to the flow resulting from the sharp density gradients, which result from the double-diffusive processes in these systems. We are changing this part of the sentence to “the expected double-diffusive processes and the resulting density-driven flows".
- l. 57-58: We here refer to the selection of a staggered grid. The staggered grid keeps the conservation of momentum and mass intact, and this conservative property is required for a proper salt and heat transport modelling. This is what we referred to with the sentence “the momentum and mass conservative grid setup allows accurate modelling of transport processes". Because this sentence was not clear, we are changing it to: “the staggered grid allows a momentum and mass conservative solution of the governing equations, which is required for accurate salt and heat transport modelling".

- l. 70: Despite the theoretical requirement to calibrate the turbulence model, the model parameters for the standard k-ε model model have been found consistent in numerous studies since it was first published by Launder and Spalding (1974) (see also our answer to the third general comment in Section 1). Because we are not aiming to promote this method as a method that does not require calibration (in this sense, it is no different from most other RANS models), we will remove this sentence.

- l. 71: See our answer to the fourth general comment (Section 1).

- l. 88-90: In contrast to DNS simulations, the horizontal scales are generally different from the vertical scales in our applications. For these applications, it is assumed that in the horizontal plane less shear will occur compared to the vertical plane. This causes anisotropy, and explains why we select an advanced turbulence model for the vertical eddy viscosity, compared to a constant horizontal viscosity. Due to the relatively fine mesh, we believe that the horizontal viscosity can be approached by its molecular value. Our simulations, which show examples of the intended model applications, also indicate that vertical mixing is generally larger than horizontal mixing.

- l. 108-109: The referee correctly points out that our sentence improperly suggested that the horizontal diffusivity also includes turbulent diffusion. In fact, the same anisotropy in diffusion was assumed in the transport model as was done in the flow model. Therefore we will change this sentence as follows: “To account for vertical turbulent diffusion, D_v is calculated by adding the molecular diffusivity and turbulent diffusivity: $D = D_{mol} + D_{turb}$."

- Eq. 6: We thank the referee of making us aware of the mistake in this equation. The equation will be reformulated to cylindrical coordinates where Q represents the depth and width integrated velocity, as it was actually employed in our framework: $y \cdot \partial\zeta/\partial t + \partial Q/\partial r = 0$, $Q = UHy$

- l. 117: We disagree with the suggested unimportance of molecular heat and salt diffusion rates, because there are cases where the molecular diffusivities are the main drivers of the salt and heat transport. For example in Carpenter et al. (2012), who are referred to by the referee, it is concluded that the major transport mechanism of salt and heat over an interface in the double-diffusive convective regime is molecular diffusion. This specific case will now be mentioned in the extended literature review and this way supports the importance of variable diffusion rates.

- l. 136: We will reformulate the horizontal mass boundary condition as follows: : $\partial cr/\partial r = 0$

- Eq. 13: ω is the relative vertical velocity. In the new version of the article, we will define the variable as such and refer to the mathematical definition of ω in Equation 16 of Zijlema and Stelling (2005).

- l. 164: The anti-creepage terms are used to better approach the horizontal diffusive fluxes over the top and bottom cell-interfaces, which are often not horizontal. The terms are derived from a further expansion of the transport equation on a depth-varying vertical grid. As a further explanation and derivation of the anti-creepage terms can be found elsewhere, we have decided that our current explanation in the article suffices.

- Eq. 20, Eq. 21: The units will be included at the introduction of the variables (line 198 for α_V and β_V, line 203 for S and T).

- l. 214-215: For this, we refer to our answer to the first general comment (Section 1). The introduction will now better state the purpose of our model, which makes the relevance of a central inflow clearer to the reader.

- Fig. 5: The asterisk refers to the shape of the marks of the data points that

are defined relative to depth. We will change this part of the caption to: “The cell depths that are defined relative to the local water depth (as marked by *) are..."

- l. 261-262: The numbers that support this hypothesis were already in the paper: for Case 1, the density ratio $R_\varrho = 2.04$, and for Case 2, $R_\varrho = 1.19$. When R_ϱ approaches 1, a higher mass transport is expected, as we explained in lines 212-214. We are changing this sentence to: “Based on the difference in density ratios, the salt-fingers in Case 2 are expected to transport more salt and heat (Section 2.4)."

- l. 266-269: For this, we refer to our answer to the second general comment (Section 1). As explained in our answer, this is not an issue in our opinion.

- Fig. 7: The interface was not yet plotted in this figure. In a newer version we will plot the interface at the times t=0 and t=5400, as the interface indeed moves upward over time (most notably in Case 2). The depth profiles are averages over the complete horizontal domain, which will be mentioned explicitly in the caption.

- l. 271-272, Fig. 8: Because the bottom layer develops from below, the double-diffusive convective layered structure needs to first build up. However, the layer displayed in Figure 8 already displays double-diffusive convective properties at its interface: the bottom layer only has not extended yet to the outer boundary. It has to be noted that the inflow velocity for this specific case was $2 \cdot 10^{-4}$ m/s, and not $1 \cdot 10^{-3}$ m/s, as was mistakenly written in Table 1 (this will be corrected in the new version of the manuscript). Due to this smaller inflow velocity, it took longer for the bottom layer to develop.

- l. 290-291: The sentence before explains that we experienced difficulties to define our inflow parameters so that the flow will be laminar at once. Therefore, the flow was turbulent at first (because of the very shallow bottom layer), and a laminatisation occurred after the bottom layer had further grown.

- l. 297: The method applied is probably misunderstood. The flow was definitely turbulent during the first 6000 s of the model simulation. Afterwards, a laminarisation occured which is visible from the resulting radial expansion of the salinity and temperature interface. The latter case is what we based the analytical benchmark test on, and the numerical results fairly agree (without the 0.5 % turbulent diffusion). Then we wondered if the slight deviation of the numerical results could be explained by the flow not being completely laminar throughout the domain and at each moment after t = 6000s. In that case, the effect of turbulence would be incorporated in the numerical results. For the benchmark, the effect of turbulence would come back in the diffusivity that is then enhanced by the turbulence. Our calculations for a slightly increased diffusivity better fitted the shapes of the curves for the numerical results in Fig. 8. We therefore concluded that the slight deviation of the numerical results from the benchmark *might* be caused by turbulence.

- l. 299: We will be more precise: "the turbulent diffusion was calculated by dividing an assumed kinematic viscosity $\nu = 10^{-6}$ m2s-1 by the Prandtl-Schmidt number (Equations 8 and 9)." Of course, the assumed kinematic viscosity is not turbulent, but the Prandtl-Schmidt number is also not constant (especially for low turbulence values). We therefore do not claim something like that the flow was turbulent 0.5 % of the time. The calculated 'turbulent diffusivities' only serve as a proxy to study whether turbulence could have been of influence here.

- l. 310: For this, we refer to our answer to the first general comment (Section 1). The introduction will now better state the purpose of our model, which makes the relevance of a central inflow clearer to the reader.

- l. 320-321: We disagree with this: our verification methods show an accordance with double-diffusive theory as it comes down to the expected onset of double-diffusive layering and the occurrence of salt-fingers (which is demonstrated by

calculating the density ratios and Turner angles for the simulated systems). As written in our answer to the fourth general comment (Section 1), we do agree that a quantitative validation was still lacking. In a new version of the article, a quantitative validation approach will be included.

- l. 321: The referee is right that no double-diffusive processes are validated in the article. Such validation will be added in a future submission. On the other hand, the model is intended for density-gradient systems that can either be subject to stable stratification and double-diffusion. For this reason, a validation for a stable stratification is equally relevant. The fact that the comparison with the benchmark was done with a very specific interface definition (35 % of the step change) was not just cherry picking: it was the interface definition that provided the sharpest image of the interface location, where using other fractions of the step change did not provide a real sharp interface shape (and therefore probably compared less to the analytical benchmark).

3 Changes to the manuscript

The following lists the changes that we will make to the manuscript based on the referee's comments:

- We will better define the purpose of the model and the article, by modifying and extending:
  - lines 19-21, better explaining that our model focuses on larger-scale features of thermohaline stratified systems and that we do not simulate exact locations and shapes of the salt-fingers;
  - lines 50-53, better explaining that situations of seepage inflows in shallow waters, causing thermohaline stratification, actually exist;

- lines 63-64, better explaining why we choose for an axisymmetric approach over an 2-DV approach (to better simulate the volumetric inflow of the central seepage source).

- We will provide a better validation based on salt and heat transport across a interface for the salt-fingering and double-diffusive convection regime and the apparent thickness of the interfaces for salt and heat in the double-diffusive convection regime.

- We will extend and update the literature review with the suggested references. Moreover, the literature review will make a clear distinction between DNS models (both 2-D and 3-D) and RANS models.

Further, the changes listed in Section 2 were applied based on the detailed comments.

References

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