Part I

Response to the referees' comments

Part I of this document lists all the referees' comments and our answers. For a list of changes to the manuscript, we refer to Part II.

1 *Referee 1 comments and answers*

First of all, we would like to thank Referee #1 for reading the manuscript carefully and expressing his/her thoughts on where the manuscript should be improved. We hope that our answers and improved submission take away most of the referee's major concerns. In the following, we answer the comments point by point (Section 1.1).

1.1 Comments and answers

1. It is not clear what the main objectives are in the paper. The test cases are 2DV. Why do you have to solve the equations in cylindrical coordinates? The reason for developing a non-hydrostatic model in cylindrical coordinates should be clearly stated.

This article explores the density-driven flow in radial direction around a central seepage source. In two of the test cases (Case 1 and 2), this inflow is absent. However, these test cases also serve to test the functioning of the axisymmetric model set-up.

The introduction of the article already explained why an hydrodynamic model in cylindrical coordinates can be preferential in some specific cases (lines 50-53). We found reason to develop a non-hydrostatic model in cylindrical coordinates, because these axisymmetric cases exist, for example in river deltas with saline seepage. The presented model set-up allows to correctly represent the volumetric (in)flow in the model. In the updated version of our article, we therefore extended the paragraph (lines 50-53) to clearly state the existence of axisymmetric cases. The following text replaces the last sentence of this paragraph: "Examples of such cases are close-to-circular water bodies with uniform boundaries, and the flow around a central point (e.g., a local inflow from a pipe or groundwater seepage). The occurrence of local saline seepage inflows into shallow water bodies of contrasting temperatures has been described by de Louw et al. [2]. Hilgersom et al. [3] have shown how these local inflows can induce thermohaline stratification in the shallow surface water bodies above these inflows." The requirement to correctly represent the volumetric flow in the modelling approach is now better stated in lines 63-64, where we explain why we develop an axisymmetric variation of SWASH (see our answer to Referee #2).

2. To my knowledge, double diffusion is sensitive to turbulence models. Usually largeeddy simulations are conducted to capture the instability. However, no *turbulence model is presented in the paper.*

We agree that the inclusion of turbulence is important when modelling double diffusion, and therefore our simulations did employ a turbulence model. In the manuscript, the inclusion of the standard k-ε turbulence model was briefly mentioned in lines 88-90. As it is definitely relevant to stress the importance of turbulence modelling, we expand more on the inclusion of the turbulence model in a new version of the manuscript. The following new paragraph replaces the sentences about the horizontal and vertical viscosity: "In this RANS model, turbulence is modelled with the standard k-ϵ model [5]. The modelled eddy viscosity is added to the molecular viscosity, yielding a non-uniform vertical viscosity ν_v. For the calculations in this article, the horizontal kinematic viscosity ν_h is set uniform to its molecular value ($\sim 10^{-6} m^2 s^{-1}$). "

3. The sensitivity of the numerical results on grid should also be discussed. Since the numerical diffusion would contaminate the physics.

The referee raises an important issue here, although grid sensitivity is usually more an issue in DNS models. A sensitivity analysis is therefore beyond the scope of this paper. We refer to our answer to Referee #3 for a discussion based on some results for grid sensitivity tests. In the paper, we would like to stick to the presentation of the method and show several test cases to verify and validate the model. We recommend a more thorough sensitivity analysis in a future study, as knowledge of the grid sensitivity of the model results is essential for future applications.

In our manuscript, we already focussed on the importance of the model grid selection when discussing the major disadvantage of 3-D models: they are highly computational expensive for the fine meshes required to correctly approach the salt and heat transport. We agree that the modelled physics can be highly influenced by the model grid and that we can better highlight the issue of grid sensitivity in this paper. We therefore decided to add a sentence to the Conclusions that pays attention to the fact that numerical results, and especially those for double-diffusive systems, can be sensitive to the selection of the model grid. This sentence is included in a new final paragraph of the Conclusions that sets out the applicability of the model and future recommendations: "Although the model is already able to show expected behaviour in the double-diffusive regime, we recommend a further exploration of its limitations and possibilities. For example, a grid convergence study should indicate whether the selected mesh size yields a convergence of results for all diffusion and advection dominated cases. Further, a comparison with DNS model results would support the validation of the model. In future applications, we stress that this model approach should be employed as a RANS model that simulates thermohaline stratification processes on a larger scale. As such, the model can be favourable in applications that allow an axisymmetric approach."

4. eq. (12): in 2DV and 3D models, bottom friction is usually accounted for through a bottom roughness. Chezy coefficient is often used in 2DH models. Why do you choose Chezy coefficient instead of bottom roughness? How does this coefficient affect your results?

We completely agree that the inclusion of a Chézy bottom friction is an unusual approach for a multilayer model. In fact, the presented model code provides the option to calculate with a logarithmic wall approach including the Nikuradse roughness height to determine the bottom friction, which is a far more common practice. The bottom friction is incorporated in the presented cases to slightly impede the high flow velocities that can locally occur, and not to approach a specified level of bottom roughness. Due to familiarity and simplicity, the authors had therefore selected a Chézy coefficient. Instead of what was presented in Eq. 12, the Chézy bottom friction was already scaled to the flow profile in the bottom layer and should have actually been presented as follows:

$$\nu_v \frac{\partial u}{\partial z}\bigg|_{z=-d} = \frac{g}{C^2} \cdot U^2 \cdot \frac{u_{k=1}}{|u_{k=1}|} \tag{1}$$

To assess the effect of the Chézy bottom friction compared to the law of the wall, we repeated Case 3 for both bottom friction boundary conditions (Figure 1). For these calculations, we applied a horizontal mesh size of 5 mm and an inflow velocity of 1 mm/s)[1]. The results show how much the Chézy boundary description affects the flow patterns in the model, especially near the grid centre. The improper flow description near the bottom boundary yields an improper friction of the local friction and in the end yields a far more turbulent flow. Figure 1 shows that the Chézy friction causes a lot more turbulent mixing of heat compared to the logarithmic wall description with a Nikuradse roughness height of 0.1 mm. Also for a roughness height of 10 mm (not shown here), the logarithmic wall law yields a steady growth of the bottom layer without a lot of turbulent mixing.

To conclude, we would also like to stress that the application of the law of the wall is the most common practice for multilayer flow modelling. For this reason, and because of the results that we have shown, we recommend that the users of the model follow this approach instead of using the Chézy bottom friction. In a new upload of our dataset, we disallowed the use of other friction coefficients which are intended for depth-averaged calculations for the axisymmetric case in the model code. In the article, we do not mention the possibility to use a Chézy coefficient anymore, and we formulate the bottom boundary condition for u-momentum for our simulations with the logarithmic wall law. We thank the referee for pointing this out.

5. theta is used for the tangential direction in section 2.1. However, this becomes alpha in section 2.3. Please make it consistent throughout the paper.

We thank the referee for making us aware of the inconsistent use of theta. We replaced theta in Section 2.1 by alpha, when introducing the cylindrical

[1]It should be mentioned here that the result plotted in Figure 8 of the manuscript was actually calculated for an inflow of $2 \cdot 10^{-4}$ m/s, and not $1 \cdot 10^{-3}$ m/s as was indicated in Table 1 (we repeated all simulations with a different bottom friction, so this is corrected in the new manuscript).

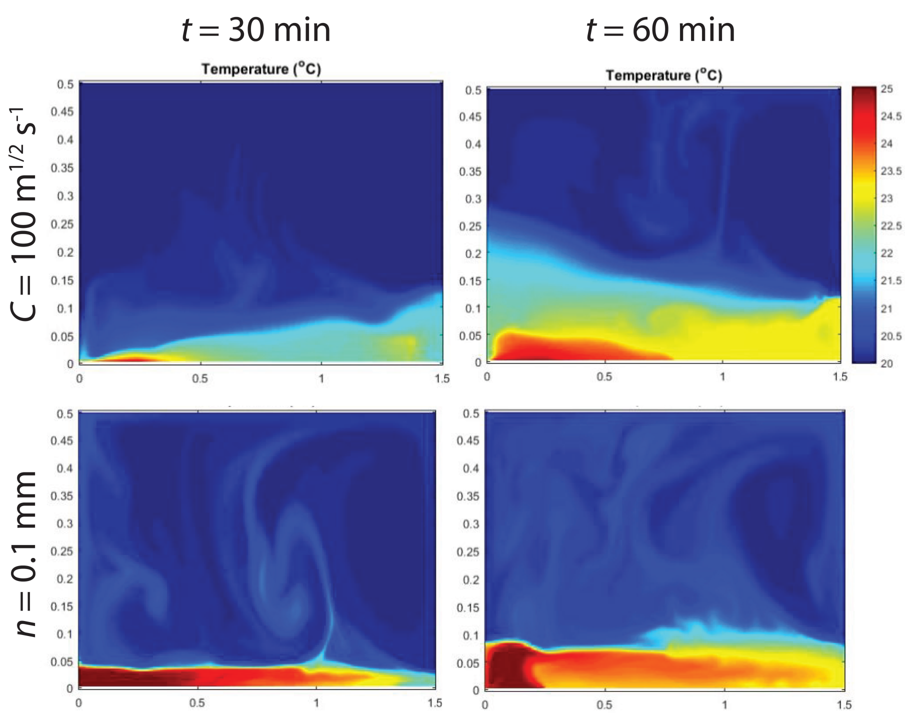

Figure 1: Comparison between bottom friction boundary conditions described by a Chézy coefficient C and the logarithmic wall with a Nikuradse roughness height n at 30 min. and 60 min. after the start of the run (horizontal mesh size: 5 mm; inflow velocity: 1 mm/s).

coordinates. In the updated version of our article, theta is only employed as an implicitness factor in the theta scheme.

2 *Referee 2 comments and answers*

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 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 2.1), list our reactions to the detailed comments (Section 2.2).

2.1 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 therefore better stress this at the end of the first paragraph of the new manuscript, where the topic is introduced: when we inform the reader about the development of an axisymmetric framework, we added that this is intended to incorporate the larger-scale effects of double-diffusion. The last sentence of the first paragraph (lines 19-21) is 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 [5] 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 added more validation cases that test the following based on flux laws published in Carpenter et al. [1] and Radko and Smith [6]:

- 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).

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. [3] 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 2.2).

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 (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 extended 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 are part of this.

As explained in my answer to the fourth general comment, the papers by Radko do 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.

2.2 Answers to detailed comments

Line #	*Explanation: how are the suggestions applied / why are they ignored?*
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 [5] (see also our answer to the third general comment in Section 2.1). 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 removed this sentence.
l. 71	See our answer to the fourth general comment (Section 2.1).
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 changed this sentence as follows: "To account for vertical turbulent diffusion, D_v is calculated by adding the molecular diffusivity and turbulent diffusivity: $D = D_{mol} + D_{turb}$."

Line #	*Explanation: how are the suggestions applied / why are they ignored?*
Eq. 6	We thank the referee of making us aware of the mistake in this equation. The equation is 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. [1], 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 is now mentioned in the extended literature review and this way supports the importance of variable diffusion rates.
l. 136	We reformulated 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 defined the variable as such and refer to the mathematical definition of ω in Equation 16 of Zijlema and Stelling [7].
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 are 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 2.1). The introduction now better states 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 changed 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 2.1). As explained in our answer, this is not an issue in our opinion.

Line #	*Explanation: how are the suggestions applied / why are they ignored?*
Fig. 7	The interface was not yet plotted in this figure. In a newer version we plotted 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 is 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 is 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 are now 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.

Line #	*Explanation: how are the suggestions applied / why are they ignored?*
l. 310	For this, we refer to our answer to the first general comment (Section 2.1). The introduction now better states 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 2.1), we do agree that a quantitative validation was still lacking. In the new version of the article, a quantitative validation approach is included.
l. 321	The referee is right that no double-diffusive processes are validated in the article. Such validation is added in this 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 *Referee 3 comments and answers*

We would like to thank Referee #3 for reading our manuscript and expressing his/her thoughts. We thank the referee for the compliments for the paper structure and the model development. The major concern seems to be that the robustness of the model is insufficiently proven and that a comparison with real world data would support the presented framework. In the following, we answer the general comments point by point (Section 3.1), list our reactions to the detailed and technical comments (Sections 3.2 and 3.3).

3.1 General comments and answers

1) It would be good to elaborate on use of SWASH vs a completely new model. Was the primary reason to take advantage of computational infrastructure? It seems like this may have been more work than starting fresh and it would be nice to include further details for this design choice.

We thank the referee for highlighting this point. In our eyes, the extension of SWASH is advantageous for multiple reasons, which we already described in the article. In our approach, we want to show how a normal 2-DV model can be easily extended to an axisymmetric model by adding few terms. Moreover, SWASH has several features that were required for our intended model application (i.e., a local groundwater inflow into a shallow water body, where the groundwater has a different salinity and temperature). These features were: calculation of the free surface, the non-hydrostatic component, the staggered grid (mass and momentum conservation), and easy extendability of the freely available code.

2) Please justify this choice of method as opposed to alternative methods, e.g., advanced mesh refinement.

Our study focuses on the development of a framework for an axisymmetric modelling approach for free-surface models. The methods that the referee refers to are advanced techniques that allow models to quicker find an accurate solution. These are very interesting techniques, but the objective of the current article is to present the derived framework.

3) Please provide additional discussion of applications and uses for this code.

Based on comments by Referee #2, we concluded that the intended applicability has not sufficiently been explained. We therefore extended the paragraphs in the Introduction (lines 50-53 and 63-64) that introduced the applications for which we developed this modelling framework.

3.2 Answers to specific comments

Line #	*Explanation: how are the suggestions applied / why are they ignored?*
l. 146	Based on a comment by Referee #2, we better formulated this in the Introduction, where we list the advantages of SWASH (lines 56-59). Instead of just mentioning that SWASH has a momentum and mass conservative grid setup, we now write: “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. 150-175	In our opinion, the presentation of the derived numerical framework is one of the major objectives of the article. Therefore, we keep this part of the manuscript in the main text. Furthermore, it is not uncommon in papers in the field of fluid mechanics to present the numerical framework in the text.
l. 181	The sentence was not correctly formulated: only the horizontal time integration of the transport equations is explicit. This is not expected to cause problems in the solution of the transport equations given the small time steps employed. The horizontal momentum terms are solved with MacCormack’s 2^{nd} order predictor-corrector scheme.
l. 198	We have increased the white space between the dots and the variables in the equation to increase the readability.
Fig. 4	Although we do not see directly which lines should get a different width to make the figure clearer, we increased the width of the diffusive flux arrows to distinguish them from the water fluxes at the inflow and outflow. We hope that this meets the expectation of the reviewer. The color maps of all color plots are changed to viridis.

Line #	*Explanation: how are the suggestions applied / why are they ignored?*
Model convergence	We have tested for grid sensitivity in the dynamical case of the double-diffusive convective layer that develops from the central inflow in the streambed (Case 3 in the article). Here, we focus on the sensitivity to the horizontal grid size, since this size is expected to be most influential in the axisymmetric approach. We have performed these tests for inflow velocities of $2 \cdot 10^{-4}$ m/s and $1 \cdot 10^{-3}$ m/s[2], and for horizontal mesh sizes of 2.5, 5, and 10 mm (the former for the period of 1 hour, the latter two for 2 hours). For the inflow of $1 \cdot 10^{-3}$ m/s, Figure 2 presents the results for temperature after 1 hour and 2 hours for the different mesh sizes (the salinity profiles show similar results). In this case where advection dominates, the flow near the seepage inflow, no real differences are seen for the different mesh sizes. For the inflow of $2 \cdot 10^{-4}$ m/s, something interesting happens (Figure 3). When the bottom layer is still thin, diffusion dominates this case: the larger diffusion of heat warms the boundary layer of the surface water on top of the inflowing groundwater at a larger rate than that salt is transported upwards. This makes the boundary layer locally unstable and leads to a sudden breaking of the develloping bottom layer. This effect is seen for a horizontal mesh size of 2.5 mm, but not for larger mesh sizes, and displays a sensitivity to the grid for cases where the effect of diffusion is dominant for a thin layer near the central inflow. These effects are not seen once the bottom layer has grown further. Based on these results, we therefore recommend applying a fine mesh near the central inflow in case the model is applied for very small inflows in combination with the development of a very thin (initial) layer.
Simplifications	This issue was also raised by Referee #2. The article does not aim to model double-diffusive features in detail (e.g., the shape of salt-fingers), but rather their main effect on stratication and salt and heat transport on a larger scale. Whether the model is succesful in resembling these patterns on a larger scale is something that needs to be validated, and we agree that the validation was still lacking in the submitted article. To meet the concerns of the referees about the unclear presentation of our purpose (i.e., the larger scale), we better stress this in the Introduction.

[2] It should be mentioned here that the result plotted in Figure 8 of the manuscript was actually calculated for an inflow of $2 \cdot 10^{-4}$ m/s, and not $1 \cdot 10^{-3}$ m/s as was indicated in Table 1 (based on comments by Referee #1, we repeated the simulations with a different bottom friction, so this *is* corrected in a new manuscript).

Line #	*Explanation: how are the suggestions applied / why are they ignored?*
Returning to the research question in conclusions	We agree that the Conclusions section does not clearly reflect the ultimate intentions of the article: the modelling of a central seepage inflow at the bottom boundary of a surface water body, where contrast in salinity and temperature can lead to the occurrence of double-diffusive phenomena. In a new submission, we partly rewrote the last two paragraphs of the Conclusions to make the presented conclusions supportive to the ultimate goal. The paragraphs are replaced by the following: "For our purpose of studying shallow water bodies, three aspects were important: 1) the inclusion of a free surface, 2) the efficient solution of a circular seepage inflow, which makes the problem three-dimensional, and 3) a proper simulation of density driven flow and double-diffusivity driven salt and heat transport. The former aspect was already fulfilled by employing the SWASH framework. The second aspect was solved by assuming axisymmetry for the Navier-Stokes equation in cylindrical coordinates. The derived numerical framework is presented as a Cartesian 2-DV description with few additional terms and width compensation factors. Our implementation of these terms in the non-hydrostatic SWASH model demonstrates the opportunity to easily expand a 2-DV model towards the presented quasi 3-D model. The third aspect was fulfilled by extending SWASH with a new density and diffusivity module. The case studies demonstrate explainable behaviour for density driven flow and double-diffusivity driven salt and heat transport. The formation of convective layers and salt-fingers are in accordance with the theory of double-diffusivity. A quantitative validation method was presented to evaluate the model's performance for a cold and saline inflow developing a dense water layer near the bottom. For laminar flow conditions, the numerical model showed a similar radial expansion of the bottom layer as expected from analytical results."

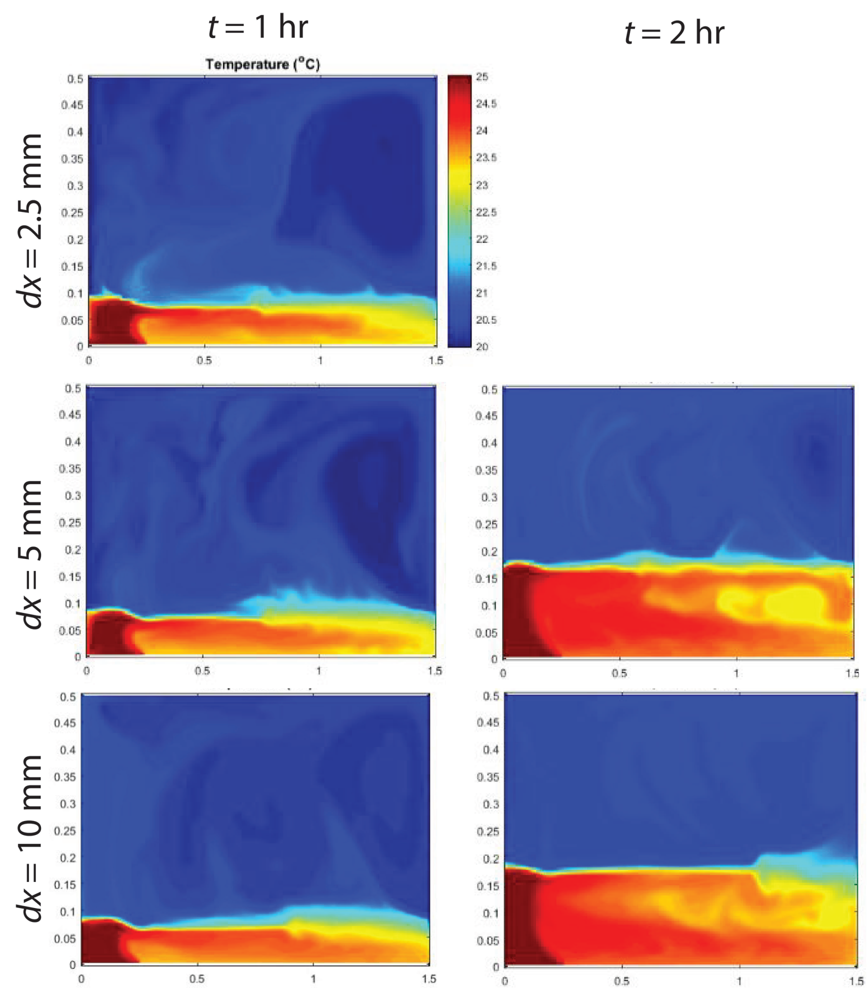

Figure 2: Development of a double-diffusive convection layer for an inflow velocity of $1 \cdot 10^{-3}$ m/s.

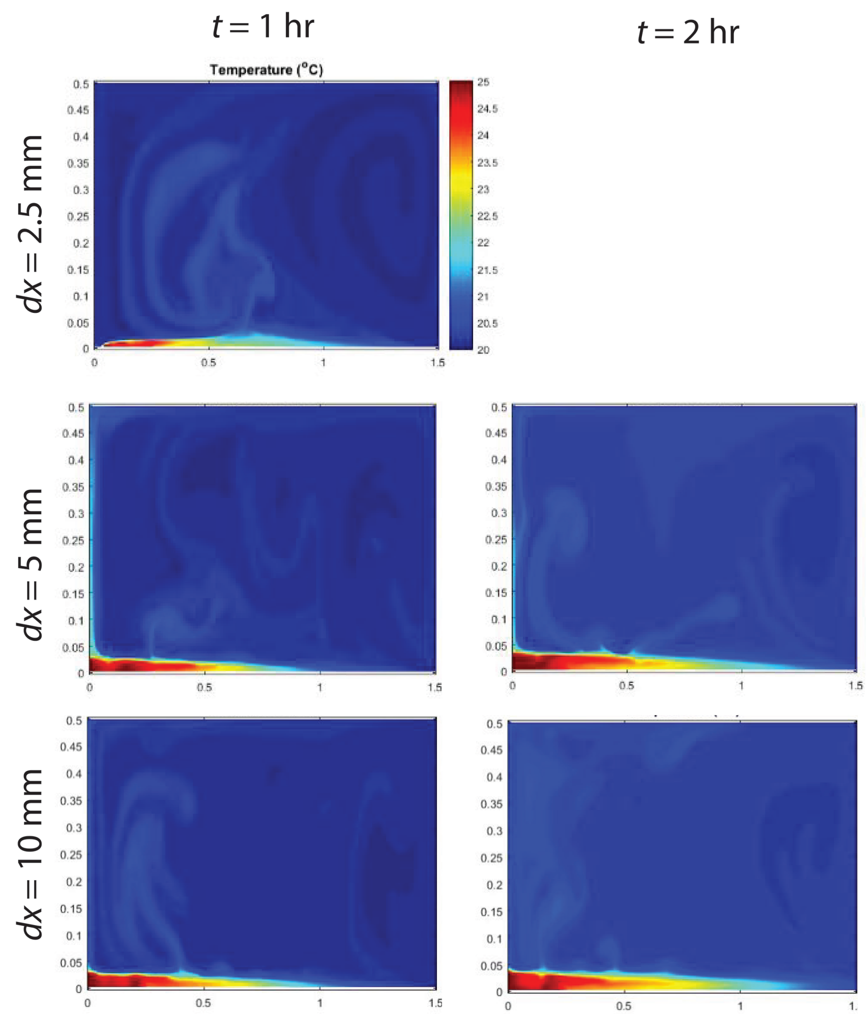

Figure 3: Development of a double-diffusive convection layer for an inflow velocity of $2 \cdot 10^{-4}$ m/s.

3.3 Answers to technical corrections

Line #	*Explanation: how are the suggestions applied*
l. 64	*axi-symmetric* is replaced by *axisymmetric*
Eq. 6	Equation 6 is split over two lines
l. 140	*the* is added between *in* and *tangential direction*

Part II
Changes to the manuscript

The following sections list the changes made to the manuscript based on the comments by each referee. Because of the vast amount of changes, we did not add a copy of our manuscript in which we track all the changes (this would be unreadable).

Referee 1

Based on the comments of Referee #1, we applied several changes to the manuscript:

- We better defined the purpose of the model and the article, by modifying and extending:
  - 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 made clearer that this RANS model does employ a turbulence model (the standard k-ϵ model), by modifying and extending lines 87-90.
- We have tested for numerical grid convergence and we added a sentence to the conclusions to focus on the issue of grid sensitivity of the model.
- The simulations are performed again and uploaded as a new dataset [4], where:
  - the simulations are now done with a logarithmic wall approach where bottom friction is determined by a Nikuradse roughness height instead of a Chézy coefficient;
  - the published code does not allow the option anymore to incorporate Chézy, Manning or any other coefficient that applies for depth-averaged calculations, as soon as the axisymmetric option is selected;
  - the results for Case 3 (double-diffusive convection) is now presented for an inflow velocity of 0.001 m s^{-1} (in the previous manuscript, we accidentally added the results for a simulation with an inflow velocity 0.0002 m s^{-1}, which was not in accordance with Table 1 and made the results less comparable to Case 4).
- The tangential direction of the axes in cylindrical coordinates is now defined as alpha throughout the article.

Referee 2

The following lists the changes that we made to the manuscript based on the comments by Referee 2:

- We better defined 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 provided 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 extended and updated the literature review with the suggested references. Moreover, the literature review makes a clear distinction between DNS models (both 2-D and 3-D) and RANS models.

Further, the changes listed in Section 2.2 were applied based on the detailed comments.

Referee 3

Based on the comments of the referee, we applied the following changes to the manuscript:

- We better defined the purpose of the model and the article, by modifying and extending:
  - 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).

Further, the changes listed in Sections 3.2 and 3.3 were applied based on the specific and technical comments.

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