The following document summarises the comments made by the reviewers and the editor, and references changes that have been made in the manuscript to address these comments. Throughout, comments made by the reviewers and editors are formatted in ***italic and bold*** with our response following each comment.

1 First reviewer (RC C680/714) : Major comments

3221-13: Please be accurate on giving references. Which of the cited papers refers to hundred km/hr turbidity current, Heezen and Ewing or Talling et al? The latter is mostly an analysis of turbidites, so I reckon it has to be the former refereeing to the Grand banks event?

This reference is not totally accurate. A similar comment was made in **RC C684** which read:

It is stated in the Introduction that particle-laden currents density currents can occur at scales up to several 1000's of kilometres, yet has this been shown? We know that submarine channels and submarine fans extend over thousands of kilometres and therefore such flows can carry sediment over such distances (as noted by the authors), but it is less clear if the flows at any one time fill the entire channel for such lengths, or whether longitudinally shorter currents traverse these systems over time.

Based upon these comments and the small correction identified in **RC C714** we have corrected the revised manuscript (ln:47–50).

3222-16: Define Grashof number, explain physical meaning, and how it relates to Reynolds number. Gr number is used in lock-

exchange problems, but not very often in other flow environments, where Re is much more popular.

This is a good suggestion that will make the paper more easily readable by a wider audience. We have augmented the paragraph in question (ln:92–97) to describe what the Grashof number is and describe it's relation to the Reynolds number.

3222-21: Is this concentration volumetric or by weight? Define here.

It is volumetric. We have modified the manuscript to clarify this (ln:104)

3223-11: Is a Gr number of 5×10^6 ***turbulent enough? A rough square root conversion yields Re=2236, which can be turbulent but also transitional between laminar and turbulent depending on the circumstances and the problem. This issue deserves an analysis.***

Necker et al. (2002) found the flow to be fully turbulent at this Grashof number, as is the case in this work, so $Gr = 5 \times 10^6$ is high enough to obtain a fully turbulent flow. Specificaly answering the question 'is it high enough?' depends upon exactly what the aim of the work is. Obviously it would be nice to increase the Grashof number if possible, and this will be the aim of future work.

We follow Necker et al. (2002) in using $Gr = 5 \times 10^6$ for the purposes of comparing computational results. It is a highly cited paper in the field, and is a simulation set up that has been used for validation and comparison by both Nasr-Azadani and Meiburg (2011) and Espath et al. (2014) for similar purposes.

We agree that the issue deserves an analysis and there are other papers that look at simulations over a range of Gr numbers (Espath et al., 2014; Cantero et al., 2007, 2008) and analyse the difference in results obtained.

The aim of this paper is not to validate the use of a particular Grashof number. We have modified the last sentence of the paragraph in question (ln:106–109) to clarify this point.

3223-15: Be more specific about what 'large domain'. It is realistic to expect using Fluidity in domains larger than the experiments replicated in this manuscript. But probably field scale modeling-order of magnitude of dozens of kilometers, is still out of reach. Please comment.

We agree in part that this is not adequately explained. It is important to distinguish between higher Grashof numbers and larger domains. This comment is meant to imply larger domains with similar Grashof numbers. This is explained more fully in the conclusion. We have modified the manuscript such that this comment is left to the conclusion where it is more fully explained. Within the abstract (ln:32) and introduction (ln:172) we now state that the model is 'well suited to simulating turbidity currents in complex domains'.

3225-8: Is 'displacement effect' equivalent to 'added mass effets'? Or is it something else?

This is not the same as 'added mass'. This is simply the displacement of fluid by particles, which will increase the fluid volume and invalidate the incompressibility assumption. To make this clearer a slight modification was made to the manuscript removing 'displacement effect' such that (ln:226–229) reads

> Due to the very low volumetric concentrations, the displacement of fluid by the suspended particles can be ignored (Necker et al., 2002). Therefore the velocity field is considered to be divergence-free.

3227-equation 8-line 11: Is η the bed elevation rather than the volume? Otherwise the units in equation 8 do not work well.

Also, how is the porosity accounted in equation 8? In this equation - which is also known as Exner equation, porosity is usually accounted on the Left Hand Side modifying $d\eta/dt$.

This is correct. η is the depth of deposited sediment, not the volume. This is a mistake which has been fixed in the revised manuscript. We do not consider porosity. Porosity is not usually considered when calculating deposit depth from models of this type. See Equation (21) in Necker et al. (2002), Equation (14) in Espath et al. (2014). The comparative experimental results of De Rooij and Dalziel (2001) are given in g/cm^3 such that an estimation of porosity is not necessary. It is also worth noting that the maximum deposit in the experiments of De Rooij and Dalziel (2001) is 0.018g/cm^3 which gives a depth of $\approx 68\mu$m. This is only just equal to a single layer of particles and hence porosity does not have any impact on the result.

However, this is an interesting comment. We have agreed that this omission of porosity should be noted in the manuscript and have added a comment at (ln:292).

3229-15: I don't think the authors have explained what discontinuous means in the context of the Galerkin FEM scheme. A brief explanation of highlighting the differences of Continuous vs Discontinues Galerkin scheme no longer than a short paragraph will help.

We have added adjusted the manuscript to provide a brief explanation (ln:356–362)

3229-16: are velocity and sediment concentration discretized in different fields?

We use the term 'field' to refer to a scalar or vector field within the domain. As such, velocity and sediment concentration are different fields. They do, however, both use the same discretisation. No adjustment was made to the manuscript.

3231-9:15: A flow chart with pictures of grid examples at each stage is need to follow the complex adaptivity system described in 2.3 with metric formation, new mesh generation and data transfer.

We agree that a flowchart may help to illustrate the adapt algorithm and have included a flow chart describing the adapt process (Figure 1). This is referenced in the introduction text to the adaptivity section (ln:439).

Following this addition the reviewer suggested that some snapshots of the adapted mesh were added. We have added an additional figure (Fig. 8) and also added a reference to (Piggott et al., 2008) (ln:441) which explains the adapt process verbosely and has excellent images describing each step of the process.

3235-5: Why are velocity free-slip b.c. used at the wall sides? Is it for the sake of comparison with other papers or to minimize the number of elements?

The reason behind using free slip boundary conditions for the side walls is to keep the simulation set up comparable with the other modellers simulations. We feel that this question is already answered at the end of the paragraph in question.

3237-21: 'However, one important quantity does show convergence.' Which one? Say it here.

The quantity that shows convergence is ϵ_d. We have modified the start of this sentence (ln:674–676) to make this clear.

3237:24:28: The combination of upwind flux terms and slope limiting in the discretisation dissipates energy at scales that the mesh cannot resolve. Additionally, adapting the mesh requires a data transfer operation which produces errors in conservation of energy, although these are minimal compared to the numerical dissipation from an under-resolved mesh. This paragraph is a bit dark.

Are you saying that the errors in energy conservation due to data transfer from mesh to mesh are negligible compared to errors due to the mesh not being small enough?

There will be some error due to data transfer using 'Galerkin projection', but these errors are very small. An under-resolved mesh has the capability to introduce huge errors comparatively. Obviously the goal is to get both sources of error to be small enough such that they are negligible.

We have re-worded the paragraph (ln:680–684) to make it clearer.

3244-3: Why this high resolution clustered on the left hand wall and not on the right hand wall? Shouldnt it be more or less symmetric?

An interesting point. Referring to Fig. 9 in the revised manuscript. At $t = 8$ the current is very close to the left-hand wall. The fluid in front of the current has a very wide area to recirculate, the eddy is much larger, and hence the curvature in velocity is much less. At $t = 20$ the head of the current is beyond half-way along the domain but the body still extends back to $x = 2$. The area available for recirculation behind the current is still significantly smaller than the area in front of the current. The manuscript has been edited to include this discussion (ln:923–932).

Note that the right-hand wall is not actually shown in the Fig. 9. The total domain length in x is 19. We have added this detail as a note in the caption of Fig. 9 to make it clear.

3246-3:4: The addition of erosion of the bottom surface in this simulation will increase the concentration of the head of the flow and may lead to a faster head speed. If this happens it is only an initial transient effect due to the inertia of the dense flow collapsing as the lock opens. With zero bed slope no flow can sustainable pick up sediment and self-accelerate (Sequeiros et al. 2009). The fact that deposition steadily increases from the very beginning until

beyond t=10 (Figure 10) hints that self-acceleration is not occurring.

We agree with the comment and have removed this sentence.

3246-16:24: This paragraph is misleading. A better agreement with previous studies does not imply that the outcome is real. It just points to the numerical models doing the same thing. Artificially vanishing entrainment does not make it happen in the real world.

This paragraph is not intended to imply that anything happens in the real world. We are simply trying to show that the deposition rate results from the Fluidity model match well with other models. This requires removing the effect of the erosion rate. We have modified the paragraph (ln:1031–1045) make this clearer.

3247-1:4: This comment on Figure 11 is also misleading. The other models do not have erosion capabilities (as stated in 3246-13:15) and their outcomes in Figure 11 are similar to Fluiditys. It seems to me that the three models simply cannot match the experimental results. This is regardless of the algorithm for erosion included or not. Ergo the problems seem to be something intrinsic in the models themselves beyond them having capacity to model some degree of erosion with empirical equations or not.

We can appreciate how there is some confusion about what is being said here. We do not aim to say anything about a match with De Rooij and Dalziel (2001) other than that the peak deposit is in approximately the right place. The comments mainly focus on comparison with the other computational models.

This part of the paper (ln:1046–1078) was largely reworded following a number of comments. We believe the modification is clearer in regard to this comment and addresses the misfit between all of the models with the experimental data.

2 First reviewer (RC C680/714) : Minor comments

We do not agree that 'scale' should be changed to 'work'. We believe that 'scale' is the correct word to use here. Apart from this, We agree with all of the minor comments and have made the changes requested.

3 Second Reviewer (RC 684/769) : Major comments

1. ***It is stated in the Introduction that particle-laden currents density currents can occur at scales up to several 1000's of kilometres, yet has this been shown? We know that submarine channels and submarine fans extend over thousands of kilometres and therefore such flows can carry sediment over such distances (as noted by the authors), but it is less clear if the flows at any one time fill the entire channel for such lengths, or whether longitudinally shorter currents traverse these systems over time.***

Covered in section 1 of this document.

2. ***It is stated in the introduction that density currents play an important role in the global carbon cycle. Can references be added to support this statement? Is the role of density currents in the carbon cycle sufficiently well known or should this statement be modified by adding a caveat along the lines of 'may' play an important role. . .?***

This sentence is intended as a leader into the next. Through being one of the key processes for moving organic matter from the continental shelf to the deep ocean we propose that turbidity currents do play an important role in the global carbon cycle.

We do not feel that this sentence is hugely important in the context and have adjusted the sentence (ln:62) to clear up any ambiguity such that it reads as

> Turbidity currents are also a key process for the movement of sediment around our planet (Talling et al., 2012). They form ...

The next two questions are answered together

3. ***Can further comparison be made between the modelling of sedimentation and the model of De Rooij and Dalziel (2001)? The model here is nominally closer to De Rooij and Dalziel (2001) as it incorporates erosion, yet it is considerably worse than either of the two models that do not incorporate this aspect.***

and

4. ***Can a fluidity model without erosion be added on to figure 11 by way of comparison? This may help show if there is a problem with the erosion model. It does look at present as if the erosion model does not replicate reality, why is this?***

The three-dimensional simulation presented in this paper is expensive. The reviewers agree that it would be useful to be able to directly compare results from the Fluidity simulations with and without erosion, but it is not felt that the information gained by doing this is essential for this paper. This reponse was questioned by the editor in **EC C747**:

It is a shame that you feel that you cannot repeat the test case without the Fluidity erosion algorithm. There are so many differences between your model and that of Espath that I would be very interested to see confirmation That These differences are down to the erosion model.

There are some variations between the results in this paper and those of Espath et al. (2014), but the authors would suggest that these differences are no larger than difference between the results of Espath et al. (2014) and Necker et al. (2002). Notably, our model agrees very well with the model of Espath et al. (2014) on head speed, whose results did not agree well with those of Necker et al. (2002). Concerning deposit rate, the Fluidity results show better alignment with the results of Necker et al. (2002) than do Espath et al. (2014), particularly in the later stages of the simulation. However, saying that, the authors would argue that all three models are generally in good agreement.

Note that there are several variations in the boundary conditions for velocity and sediment, and the initial condition for sediment, between the Fluidity simulation and the models of Espath et al. (2014) and Necker et al. (2002). The inclusion of erosion is a notable variation, but is certainly not the only one. The authors do not believe that the differences in results from these models is due solely to the erosion model.

We do agree that more comment can be made as to the success of the erosion algorithm and about how the deposit profile matches against the experimental results of De Rooij and Dalziel (2001). A major revision has been made to the this part of the paper (ln:1046–1078) and we believe that this section answers the points raised by these reviewer comments.

4 Second Reviewer (RC 684/769) : Minor comments

We agree with all of the technical comments made and have made the changes requested.

5 Third Reviewer (RC 707) : Major comments

1. ***Page 3222, line 6-9: Small scale laboratory experiments can provide useful insight into the dynamics of these currents, but are limited by scaling issues and the available measurement techniques (Kneller and Buckee, 2000). Is there any particular reason why model simulations are not compared directly to experiments? Validation by comparison to other model results is generally insufficient, unless you can be certain that the numerics correctly represent the underlying physical processes. More succinctly, the authors should address the question: With the advanced numerical formulation presented in this paper, has progress actually been made on representing the underlying turbidity currents? If not, it should instead be pointed out that the emphasis of this article is on providing a framework for addressing deficiencies in the current physics formulation.***

The model is directly compared to experimental data of deposit depth. However, this is the only direct comparison with experimental data and we agree that comparison with experimental results is not a major focus of this paper.

The principal focus of this paper is validation of novel computational methods for this application. For this reason we choose to compare the model against the previously published, well regarded three-dimensional DNS models of Necker et al. (2002) and Espath et al. (2014). The equations upon which this model is based, and variations upon them, are well established for modelling sediment-laden density currents of this type. They have been validated against experimental results in a number of different situations using a range of diagnostics (Sequeiros et al., 2009; Necker et al., 2002; Espath et al., 2014; Huang et al., 2007; Georgoulas et al., 2010).

The authors agree that this could be made more clear. The end of the introduction now contains text to clarify this (ln:153–166).

2. ***Page 3230, line 26: Advection and diffusion are coupled using a first-order coupling strategy. Can you comment briefly on the validity of this approach? Is there any influence on the results by a more frequent application of diffusion?***

Diffusion is only applied once. A reference to the fluidity manual is included which described the process in detail (ln:426). The authors agree that the description in the manuscript was a little unclear and have modified the sentence describing this (ln:418–423).

For information, we have described the process in detail below.

The discretised advection-diffusion equation for a scalar, c, can be written in matrix form as

$$\mathbf{M}\frac{c^{n+1} - c^{n}}{\Delta t} + \mathbf{A}\left(\mathbf{u}^{n+1/2}, u_s\right) c^{n} + \mathbf{K}c^{n+1/2} = 0 \;, \tag{1}$$

where:

- $\mathbf{M}$ is the mass matrix,
- c^{n} is the known value of c from the end of the last timestep,
- c^{n+1} is the unknown value of c that we are solving for,
- $c^{n+1/2} = (c^{n} + c^{n+1})/2$ is the Crank-Nicolson time-discretised value of c,
- $\mathbf{u}^{n+1/2} = (\mathbf{u}^{n} + \mathbf{u}^{n+1})/2$ is the Crank-Nicolson time-discretised velocity,
- $\mathbf{A}\left(\mathbf{u}^{n+1/2}, u_s\right)$ is the advection matrix, which is a function of the velocity and sinking velocity,
- $\mathbf{K}$ is the diffusion matrix.

This can be reformulated as

$$\mathbf{M}\frac{c^{n+1} - c^{*}}{\Delta t} + \mathbf{M}\frac{c^{*} - c^{n}}{\Delta t} + \mathbf{A}\left(\mathbf{u}^{n}, \mathbf{u}^{n-1}, u_s\right) c^{n} + \mathbf{K}c^{n+1/2} = 0 \ , \tag{2}$$

and then split into two equations as

$$\mathbf{M}\frac{c^{*} - c^{n}}{\Delta t} + \mathbf{A}\left(\mathbf{u}^{n+1/2}, u_s\right) c^{n} = 0 \ , \tag{3}$$

$$\mathbf{M}\frac{c^{n+1} - c^{*}}{\Delta t} + \mathbf{K}c^{n+1/2} = 0 \ . \tag{4}$$

Equation (3) is subcycled in n steps with a CFL criteria one order of magnitude tighter than that implied by Δt, such that we solve n equations of the form

$$\mathbf{M}\frac{c^{\dagger} - c^{\dagger-1}}{\Delta t/n} + \mathbf{A}\left(\mathbf{u}^{n+1/2}, u_s\right) c^{\dagger-1} = 0 \ , \tag{5}$$

where $c^{\dagger-1} = c^{n}$ for the first subcycle, $c^{\dagger} = c^{*}$ at the end of the last subcycle.

3. ***Page 3236, line 13-26: The effect of mesh adaptivity appears to be to add numerical diffusion to the simulation via the regridding procedure. This is likely the reason why more frequent adaptation leads to improved stability in the boundary layer. Is there any way to quantify the effect of this diffusion? In addition to explicit diffusion and upwinding, adaptive remeshing is then the third source of diffusion in the simulation.***

It is very hard to separate the diffusion introduced by the adapt process from the diffusion introduced by the slope limiting and upwinding. It is not possible to do this for the simulation in this paper. However, the Galerkin projection is used which is minimally diffusive (Farrell et al., 2009), and hence the diffusive effect of adapts is small. We can also state from experience, and based on results that will hopefully form part of a follow on paper, that the diffusion introduced by adaptivity and data transfer is insignificant compared to the other sources of diffusion.

Although it is possible that the small amount of diffusion introduced by adapting more frequently may have improved stability, it was clear from our analysis that the main reason adapting resulted in a more stable simulation was that small instabilities were not allowed to grow. As a small instability developed, an adapt led to increased resolution in the region of the instability, which stabilised the problem, as stated at ln:635.

4. ***Page 3237, line 16-20: Although direct convergence of these quantities is not expected due to the chaos of the underlying system, one may still anticipate that statistical convergence occurs. That is, over an ensemble of simulations one would expect that the mean head speed, etc. would be convergent with resolution. Can the authors comment briefly?***

This is an interesting discussion point. The turbulence is acutely sensitive to small perturbations in the initial conditions, parameters, and computational grid, and hence may produce quite a different result for any small perturbation. Therefore, it may be possible to obtain an ensemble average by making small perturbations to an initial condition, or very small changes to the mesh, around each of the mesh resolutions of interest. The authors agree that this has not been considered. The authors are not certain how many perturbations would be required to create a good ensemble average, how large these perturbations should be, and which parameters should be perturbed. This could turn in to an extremely complex, and interesting, but very expensive analysis.

The paper states that 'due to the turbulent nature of the flow, which is very sensitive to small changes in the mesh, it is very hard to show convergence of [many] quantities' (ln:671–673)). Based upon this discussion, the authors believe that the statements made in the paper referring to this are correct. We don't propose to make changes to the paper concerning this point, although believe that it is an interesting discussion topic.

5. ***Page 3242, line 27: Throughout the simulation the number of processor cores that were used was varied between 36 and 512 to keep the number of elements per core in the region of 20 000. Why? Does this have the effect of normalizing the wall-clock time of the simulation?***

The parallel efficiency of Fluidity increases as the number of elements per core increases. However, this relationship is not linear. It is generally accepted that the parallel efficiency of the program is good when there are more than $\approx 10^4$ elements per core.

Conversely, it is beneficial to have fewer elements per core as the simulation runs faster and we obtain results more quickly.

We do not want to run a very inefficient model and waste computer resource, but we do want the simulation to complete as fast as possible. The number of elements varies by approximately an order of magnitude over the course of the simulation. The number of cores that the simulation is run on is varied over a similar magnitude simply to optimise the computer usage.

Referring to the wall-clock time. All processing times given in the paper are processor hours e.g. a simulation running on 2 cores for 1 hour = 2 processor hours.

6. ***Section 6: I suggest the authors include an image depicting a snapshot of the adapted mesh near the gravity current head at an intermediate simulation time. Such an image would provide a better visualization of the effect of refinement on the mesh.***

The authors agree that this addition would improve the manuscript. We have included an extra paragraph in the 'benefits of adaptivity' section (ln:891–905) that describes a new figure (Fig. 8)

7. ***Page 3247, line 24: Can the authors provide a brief discussion on the type of adaptive mesh refinement chosen for this model and how it compares to other, perhaps computationally cheaper techniques, such as a octree-based refinement or block-adaptive refinement?***

A mesh optimisation algorithm (Pain et al., 2001) is used in Fluidty for adapting the mesh. This algorithm offers the most flexibility in producing an optimised mesh, allowing for node movement as well as node insertion or deletion and edge/face swapping. Many dynamic features of the gravity current simulated are anisotropic e.g. the boundary layer and the density interface. The mesh optimisation algorithm used can easily provide a mesh with anisotropic elements such that these features are resolved efficiently. Therefore, this method of adaptivity is likely to produce a well optimised mesh for this application.

However, the adapt process takes a significant portion of the simulation time, hence it is a useful to consider the use of other, cheaper techniques. So long as the problem of hanging nodes can be addressed a hierarchical approach could be used, and may prove a suitable alternative. We have added a sentence at ln:963–970 to highlight this.

6 Editor Comments EC C747/782/793 and RC C779

How can they (Espath et al., 2014) get away with resolution so much lower than what you say is needed (10^9 ***elements)? Are their results less accurate? Or does the 6th order accuracy give them back the accuracy that you obtain with much higher (local) resolution.***

I appreciate your honesty when discussing the cost of your model, explaining why you need adaptivity to counteract the cost of the model. However, with the adaptivity, you should be able to get to higher Reynolds number than Espath for a similar number of degrees of freedom. However Espath also do simulations of Re=10,000 with 5×10^8 ***grid points. They seem to be able to do bigger simulations with higher Reynolds number but at lower resolution.***

The 6-th order method increases the accuracy of the method. The FD model is therefore more accurate for the same dx, although increasing the order of the FD method does come at a cost.

Each model has its strengths and weaknesses. Finite-elements are relatively expensive. But the use of finite-elements and an unstructured grid is very useful for modelling density currents in complex domains, which is very difficult to do using other methods. The authors believe that this is where the strengths of the model lie and that this should be the main focus of modelling efforts using this model.

This point is addressed in a couple of areas of the revised manuscript. The introduction is augmented to discuss the methods that are used by other modellers in more detail (ln:126–142). We then highlight where our model is different and discuss the benefits, and also the increased cost implications of the proposed method (ln:143–149).

(ln:855–861) explicitly compares the number of elements used in the Espath model to our model and comments on how they are able to use a larger dx whilst obtaining the same accuracy.

It is a shame that you feel that you cannot repeat the test case without the Fluidity erosion algorithm. There are so many differences between your model and that of Espath that I would be very interested to see confirmation That These differences are

down to the erosion model.

This has been covered in 3 of this document.

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