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# *Interactive comment on* "Direct numerical simulations of particle-laden density currents with adaptive, discontinuous finite elements" *by* S. D. Parkinson et al.

**Anonymous Referee #1**

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Comments to authors: Main comments: Overall it is a good numerical study on the use of adaptive finite element methods to study turbidity currents. The simulations presented in the manuscript are of the lock-exchange variety only. Some comparison with experimental results is also carried out. I have pointed out major comments the authors must address before I can recommend this manuscript for publication. Minor comments are also included below. Page-line: Comment. 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? 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. 3222-21: Is this concentration volumetric or by weight? Define here. 3223-11: Is a Gr number of 5x10ˆ6 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. 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. 3225-8: Is "displacement effect" equivalent to "added mass effets"? Or is it something else? 3227-equation 8-line 11: Is eta 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 deta/dt. 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. 3229-16: are velocity and sediment concentration discretized in different fields? 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. 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? 3237-21: "However, one important quantity does show convergence." Which one? Say it here. 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? 3244-3: Why this high resolution clustered on the left hand wall and not on the right hand wall? Shouldn't it be more or less symmetric? 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. 3246-16:24: This paragraph is misleading. A better agreement with previous studies does not imply that the outcome is real. It just point to the numerical models doing the same thing. Artificially vanishing entrainment does not make it happen in the real world. 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 Fluidity's. 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.

Minor comments: 3220-17: Replace "scale" with "work"? 3241-4: Do you mean equation (34) rather than (38)? 3244-8: Replace "an parallel" with "a parallel". 3253-Table 1: Explain in the caption what are all the parameters in the table columns. 3255-Figure 2: Explain what ED is in the caption

References: Sequeiros, O. E., H. Naruse, N. Endo, M. H. Garcia, and G. Parker (2009), Experimental study on self-accelerating turbidity currents, J. Geophys. Res., 114, C05025, doi:10.1029/2008JC005149.

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