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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 #2

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General comments: This is a detailed and high quality paper looking at the use of adaptive unstructured mesh techniques and discontinuous discretisations for modelling particulate gravity currents using DNS. The authors show that these techniques can reduce the element count by ∼2 orders of magnitude. Even allowing for the associated costs of remeshing, the overall computational saving is at least an order of magnitude. The results are tested against a variety of other DNS modelling and physical modelling results, and typically show excellent agreement with these. Starting conditions are not identical, so exact comparisons are not possible, suggesting that the minor differences in many of these comparisons may be the result of starting conditions and boundary conditions.

Specific 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.

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. . .?

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.

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?

Technical comments: 1. P3227 line 18, should be 'turbidity currents' rather than 'turbidity current' 2. P3246 line 28. De Rooij and Dalziel has the wrong date, this should be 2001, not 2009. See also caption of figure 11. 3. Acknowledgements, line 2, should be 'their' not 'there' 4. Can details be added of the book title and editors for the De Rooij and Dalziel reference?

Interactive comment on Geosci. Model Dev. Discuss., 7, 3219, 2014.