Geosci. Model Dev. Discuss., 7, C707–C709, 2014
www.geosci-model-dev-discuss.net/7/C707/2014/

Geoscientific
Model Development
Discussions
Open Access

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

P. A. Ullrich (Referee)

paullrich@ucdavis.edu

Received and published: 2 June 2014

1 General Comments

The manuscript "Direct numerical simulations of particle-laden density currents with adaptive, discontinuous finite elements" by S. D. Parkinson, J. Hill, M. D. Piggott and P. A. Allisson describes a modern numerical framework that utilizes cutting-edge numerical techniques for the simulation of a turbidity current in 2D and 3D. The paper is well written, with a focus on the technical description of the underlying model. Numerical results have been provided to assess the quality of model simulations, although the

discussion of these results is only addressed very briefly. I am happy to suggest this manuscript for publication once the following issues are addressed.

2 Specific 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.
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?
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 unwinding, adaptive remeshing is then the third source of diffusion in the simulation.
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?

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?
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.
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?

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