

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

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Thank you for agreeing to be the editor for this paper, and also for these interesting discussion points. We have provided responses to your comments below. Each response begins with your comment.

How can they (Espath et al., 2013) 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.

C766

You are correct. The sixth order method used by Espath et al. (2013) gives additional degrees of freedom and hence will provide Espath et al. (2013) with the accuracy that we gain by having a higher local mesh resolution. We do not believe that the result of Espath et al. (2013) are any less, or more accurate than our results.

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.

As noted above, the number of degrees of freedom in the Espath et al. (2013) model is significantly higher than the number of grid points, and hence Espath et al. (2013) does not have a lower resolution. It is also important to note that it is feasible that Fluidity would be able to model currents at higher Reynolds numbers. The simulation in this paper used a maximum of 512 cores. It is certainly possible to use more cores than this to extend to higher Reynolds numbers in the future.

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.

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.

C767

There are some variations between the results in this paper and those of Espath et al. (2013), but the authors would suggest that these differences are no larger than difference between the results of Espath et al. (2013) and Necker et al. (2002). Notably, our model agrees very well with the model of Espath et al. (2013) 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. (2013), particularly in the later stages of the simulation. However, 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. (2013) 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.

Thank you again for these interesting discussion points. We hope that we have provided adequate responses to your comments and look forward to hearing your response. We strongly believe that this discussion process is resulting in a greatly improved manuscript.

References

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C768

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C769