

## ***Interactive comment on “Application of the adjoint approach to optimise the initial conditions of a turbidity current” by Samuel D. Parkinson et al.***

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We would like to thank the reviewer for her/his constructive comments.

Below, we provide answers for each of the reviewer's comments:

> 1. The objective function can be considerably improved. Figure 5 shows that the > Bed 1.1 profile has a depth around 0.5 cm at the right boarder. But in the simulated > results the deposit depth is always zero at the right boundary ( $y=1$ ) according > to (17). This can also be noticed from Figure 7. So the target profile used in > the objective function (53) does not represent a complete profile, while the simulation > always provides a complete one.

The simulated profile is indeed always zero on the right boundary. However, the model

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allows for profiles that go beyond the observed target profile. Hence the simulated profile may take non-zero values where the target profile stops, and the functional allows a match of the simulated and target profiles.

> One possible way for a better comparison > is to extrapolate the target profile further to the right until the depth becomes zero, > and then define the functional on the range  $[0, x_{\text{E}}^{\text{N}}]$  instead of  $[0, x_{\text{E}}^{\text{Qmax}}]$

We see two problems with this approach:

1. Extrapolation is generally a "dangerous" process. For example, if one only had a very small target profile, maybe over 1 km, one would be faced with extrapolating this profile to  $x^{\text{N}}$  - and depending on the extrapolation strategy one would probably obtain different reconstructions.

2. The simulation end time, and the length of the simulated profile  $x^{\text{N}}$ , are not known a priori. More precisely, the simulation is terminated once all sediment in the fluid has is settled to the sediment, which also means that the simulated profile does not change further after this point.

> This will make the calculation easier and avoid the difficulty with discontinuities. > Originally discontinuity may arise when  $x_{\text{E}}^{\text{N}} > x_{\text{E}}^{\text{Qmax}}$  because the model profile is > truncated at  $x_{\text{E}}^{\text{Qmax}}$  in the integral evaluation and then the functional depends on > only part of the system states. Furthermore, there would be no need to use the > scaled filers and the nasty approximation of max and min functions.

We agree that it would be nice to simplify the functional - but we do believe that our functional yields a more robust reconstruction, even if it means that we have to handle the additional complexity.

> 2. It may be worth trying different end times for the simulation. The optimal modeled > profile found in this work looks much shorter than it should be compared to the > target profile, leaving a significant gap towards the end. An ideal model profile > that

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matches well with the observation would be expected to have longer length  $>$  than the extent of measured data ( $x^N > x^{\max}$ ). Is it possible to get a longer  $>$  profile by increasing the end time  $t_f$ ? If so, the quality of the solution would be  $>$  improved

Extending the simulation would not change the length or the shape of the profile further. The reason is that the simulation is terminated only after nearly all particles are settled into the sediment. This means that further simulation time will not have significant impact on the sediment profile. The conclusion of the manuscript discusses a number of other reasons why the optimised profile does not match the target profiles more precisely - most likely our numerical model is missing some important physical details, which could be an interesting future direction of this research.

$>$  3. More evidence should be provided to support the claim that the gradient-based  $>$  optimization methods are powerful for solving the inverse problem of find turbidity  $>$  current parameters. Why not generate a reference profile with a known initial condition  $>$  and run the same code with different initial guesses to see if the reference  $>$  profile can be reproduced in the optimal solution? This artificial test will give more  $>$  insight on the performance of the optimization techniques. And hopefully this will  $>$  also make it clear if there are local minimums that lead to low-quality solutions.

We appreciate your comment, and it is a very good point. However, we feel that this is already covered in the paper as it stands now. Although this very simple model could feasibly be optimised manually, a more complex, and complete model would be very difficult to optimise by manually tweaking the input parameters. Specifically lines 315-322 cover this, and at numerous locations in the paper we discuss how improvements to the model are required to better model the flow - many of which would greatly increase the number of input parameters leading to big efficiency gains by using a gradient-based optimisation.

$>$  4. There are not enough instructions on how to run the code. For example, FEniCS  $>$  is suggested to be run with Docker, however, it is not clear how to get all  $>$  the

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components work with Docker, or without Docker. The specific commit of  $>$  libadjoint described on the bitbucket page does not compile with gfortran 5.3 on  $>$  my mac, but the latest commit compiles without any problem. It would be beneficial  $>$  for the readers who want to play with the code if the workflow could be  $>$  simplified (e.g. wrap everything into a FEniCS Docker image) and described in  $>$  more detail. Based on the current available resource, I am not able to confirm the  $>$  reproducibility of the results, although I have no doubt with them.

Installation is indeed a problem, and the FEniCS Docker idea is great. We have now created a Docker file that should simplify the installation significantly. The details are describe on the official bitbucket webpage: [https://bitbucket.org/simon\\_funke/adjoint-turbidity/overview](https://bitbucket.org/simon_funke/adjoint-turbidity/overview)

Specific comments ————— All specific comments have been addressed. In particular, we now to use  $\eta_T$  rather than  $\eta^T$  throughout the paper (it was inconsistent before). We decided on this notation to be in line with  $\eta_0$  (the initial sediment profile).

Please also note the supplement to this comment:  
<http://www.geosci-model-dev-discuss.net/gmd-2016-136/gmd-2016-136-AC2-supplement.pdf>

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