A Review of "Semi-Lagrangian advection in the NEMO ocean model"

By Christopher Subich, Pierre Pellerin, Gregory Smith, and Frederic Dupont

This paper describes the implementation of a semi-Lagrangian advection scheme for momentum and tracers in the NEMO ocean model. The algorithms that have been adapted or developed are well presented and clearly explained in useful detail. Some preliminary results showing that the methods "work" are also presented for idealised and real-world configurations.

The adaptation for ocean models of semi-Lagrangian algorithms is a significant contribution to the ocean modelling literature and the paper deserves to be published for that reason alone. Similarly to the other reviewer, my view is that the implementation is a major achievement for which the authors deserve congratulation. I am also very supportive of the algorithms being honestly presented, warts and all. The initial results on the quality of the simulations are also interesting.

The first reviewer has produced a very thorough review so my review is less lengthy than it might have been. I refer to the first review where it seems relevant.

From my point of view the main weakness of the paper is in the introductory discussion. The motivation for using a semi-Lagrangian advection scheme needs more discussion. As the first reviewer mentions, Lemarie et al (2015) and Shchepetkin (2015) show that the time step in NEMO-like ocean models of ORCA025 resolution can be limited by vertical advection in hot-spots near the bathymetry (those papers should be referenced). As the authors mention ice-ocean drag can also limit the time-step. In the ocean, horizontal advection by internal gravity waves and currents is usually considered to be the next limitation on the time-step. In atmospheric models, semi-Lagrangian schemes usually calculate the horizontal pressure gradients as well as the Lagrangian advection semi-implicitly (Staniforth & Cote 1999) so that the time-step is not limited by the horizontal currents or the gravity wave speeds. The rationale for this is that external gravity waves (and sound waves) in the atmosphere travel at about 300 m/s whilst the winds themselves are typically around 100 m/s. In the ocean, internal gravity waves travel at about 3 m/s whilst ocean currents are not much more than 1.5 m/s. So one might anticipate that semi-Lagrangian schemes for the ocean would similarly calculate the (baroclinic) pressure gradients semi-implicitly. Unfortunately that would necessitate inversion of a 3D Helmholz problem. This problem would be much better conditioned than the free surface solver for the external mode but its efficient solution would be technically challenging. The second paragraph of section 1.1 highlights the point that the horizontal grid spacing in ORCA025 in the Canadian Archipelago is only 3-4 km. On first reading I did wonder whether the internal gravity waves may be significantly slower in the regions where the grid spacing is small and whether that would make semi-Lagrangian advection efficient for this grid. But the authors do not mention that possibility.

I think there needs to be some recognition and discussion of the points made in the previous paragraph. It would be best for this to be in the introduction if good reasons can be given as to why semi-Lagrangian advection without semi-implicit calculation of pressure gradients is expected to allow long timesteps. Otherwise the issue should be, perhaps briefly, discussed in the concluding section.

The introduction should also mention the concerns that a) SL schemes usually do not conserve quantities in the same way that flux formulations do and b) their upstream nature introduces a biharmonic damping (see the suggestion relating to Figure 8 below).

More detailed comments

Title:

I agree with the other reviewer that there could usefully be a few more words in the title. Perhaps implementation [or development] of a Semi-Lagrangian algorithm [or scheme] …

Abstract

I am uncomfortable with the first paragraph of the abstract because it gives the impression that the semi-Lagrangian scheme described in this paper is very similar to the SISL algorithms used in atmospheric models which, for the reasons explained earlier, does not seem to me to be really the case.

Introduction

Lines 15-27: I know it is difficult to start papers but the discussion of coupled models here seems to be rather tangential.

Lines 29-39. The pole problem is of course much more severe in atmospheric models. Are the currents in these narrow straits as strong as those in the Gulf Stream? I ask because tidal currents can be very strong and CFL depends on u/dx. Is the water shallow in the Canadian archipelago where the grid spacing is smallest? The currents might then be much faster than the internal gravity waves in these regions (see earlier comment). This would make the SL method suitable for the ORCA025 domain – but not for some other domains.

Lines 40-43. The references given by the other reviewer should be mentioned here.

Section 2

The other reviewer asks questions about the details of the leapfrog scheme. NEMO is in the process of transitioning to an RK3 time-stepping scheme. Perhaps it is beyond the scope of the paper to discuss this but it might be easier (and more forward looking) to consider how to adapt the SL scheme for an RK3 scheme.

Equation (3) and line 95: I think x(t) should be x(t_0) (three occurrences two in equation (3)).

I am inclined to agree with the other reviewer that extending the notation in equations (1) – (6) would be helpful. Some ocean model readers might find a schematic figure helpful too. Equations (1)-(4) read very easily but on first reading it took me some time to understand that the first identity in (5) is just (1) with x written as x(t_0 + \Delta t). This was partly because I could not see what the comment on the line before (5) meant. That would be clear if it was written as "noting that x in (1) corresponds to x(t_0 + \Delta t) and that the evolution in (4) is due solely to advection so that RHS_f becomes RHS_{f,adv}, gives"

Section 3

Algorithm 1 and its description are very neat. Algorithm 2 is also clearly described. In the description of case 1 it is (9) rather than only (9a) that applies. If these algorithms have been presented in previous papers references to the original papers should probably be given.

Lines 199-202: I found it difficult to understand this paragraph and hence confusing on first reading. On second reading I think it does not say very much.

Section 3.2

Line 215: I was puzzled on first reading that the derivation for horizontal interpolation previously derived was discontinuous. Is the point simply that the derivatives are continuous in grid point space but not in physical space? It would be less disconcerting and easier to read if this point were made more directly.

Lines 262-265: "Imagine …" I found these two sentences difficult to follow. Is the point that the vertical grid locations vary in the horizontal because of the partial cells so that vertical interpolation to departure points is necessary even though w=0?

Section 3.3

The solution (12)-(14) of (11) is rather neat. Is this a standard test case in the semi-Lagrangian community? A reference for it would be appropriate.

Section 4

Lines 358-373 give a very helpful simple example motivating the solution proposed. The short paragraph in lines 374-376 is less clear. What follows is again very clear and helpful.

The calculation in lines 390 – 406 is admirably clear and concise.

Section 4.3

The use of corner solutions to supplement bi-linear interpolation looks quite novel to me. I'm impressed by it.

Section 5.1

Lines 483-484: This is a useful test case, but one which allows internal gravity waves to propagate faster than the advective velocities would be more relevant to most of the ocean.

Line 514 and Figure 5: "there is an inconsistency which grows with \delta t". I think the authors are pointing out that in (3) the terms are "centred" about the middle of the trajectory (half way between x_a and x_d) whereas in (5) and (6) only the Lagrangian advection term is centred at that point, the RHSE_f term being calculated solely at x_a. I think this point should be made near the end of section 2. Does it affect the order of accuracy of the scheme?

Footnote 9: Ducousso et al (2017) supports this statement

Lines 550-554: I hope the authors will sort out the ice-ocean coupling before re-submitting the paper so that integrations using longer time-steps are presented in the revised manuscript.

Line 557 and Figure 6: The MOC appears to be weaker in the SL integrations. The differences are at least comparable with those typically obtained between 1^o, $1/4^o$ and $1/12^o$ simulations. As this is only one integration it is difficult to be sure how significant the difference is. But more should be said than just that they are comparable. The ACC appears to be stronger in the SL simulations. Similar variations can be obtained for ORCA025 with Eulerian schemes by adjustment of model parameters (though I'm not sure whether this is documented in the literature).

Lines 559-562: Much larger variations than those between the curves in Figure 7 are obtained by varying the model resolution from 1^o to $1/4^o$ to $1/12^o$

Lines 563-567 and Figure 8: NEMO is used extensively for climate change simulations. The implied change in ocean heat content could be significant. Erosion of the thermocline by numerical mixing is a major issue in ocean simulations for climate. So the authors should plot the global mean vertical temperature profile at the start and end of the integration and compare with results with other papers. See for example Adcroft et al https://agupubs.onlinelibrary.wiley.com/doi/full/10.1029/2019MS001726 and Megann https://doi.org/10.1016/j.ocemod.2017.11.001

Section 5

Lines 577-579: A slightly longer summary of what has been achieved in the methods section would probably be helpful to the reader. New algorithms and arguments should be highlighted here or in the introduction.

Lines 590-593: The authors should mention the need to increase the halo size for communication between processors and the possibility of load imbalances when some iterations are slow to converge.