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Ocean Science Discussions

Interactive comment on "Simulations of ARGO profilers and of surface floating objects: applications in MFSTEP" *by* C. Pizzigalli and V. Rupolo

Anonymous Referee #2

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In this paper the authors present some Lagrangian simulations performed in the framework of MFSTEP, including both ARGO-like floats and surface particles. The paper is clear and some of the results are interesting. I think it is suited for publication especially in a dedicated special issue, even though it needs some revisions as detailed in the following.

- The simulations are based on a 1/8 degree MOM daily average outputs, and I think this poses some significant limitations on the results. Disperson, especially at relatively short time scales as considered here, is likely to be influenced by small scale velocity features which are not resolved in the model. Also, the simulations for ARGO floats

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show that the velocity field at 350 m. is often very weak, an aspect that I suspect is not realistic in the model. I think this point should be discussed and pointed out clearly in the Introduction and/or Summary sections.

- The authors refers to two other papers where their MFSTEP simulations are presented, Pizzigalli et al, (JGR submitted) and Poulain et al (OS submitted). It is not always clear how the results are partitioned and what is new in the present paper. This should be clarified more in the text.

- The authors often refer to their results as part of different WPs in the framework of MFSTEP. Personally, I do not like this type of presentation, since the article will have to stand on its own and will be read by a greater audience than just the MFSTEP one, so that the reference to WP's will not mean much. I suggest the authors change this aspect of the presentation.

More in detail: - The authors mention that a proxy for the number of independent measures is the number of cycles which are separated by a distance X greater than the Rossby radius R (see Table 1). I am not sure I quite understand this. Are they talking about distance between consecutive positions? Even if consecutive profiles are not independent, $X<R$, they are independent from the following profiles, taken at greater distance and/or different times.... The authors should clarify this point. Also, the number of cycles when floats reach the bottom seems very high. How does it compare with realistic values?

- In Table 3 and 4 the authors show values of mean errors Delta and s.d, which are quite puzzling. Clearly the mean has no value, since the s.d. is so high, and it just reflects the fact that many floats move very little during Tdrift, as mentioned by the authors, I think these values are misleading and they do not need to be included especially in Table 4. A better approach, in my opinion, would be to clearly discuss why these values occur, also as a consequence of model limitation, and then limit the statistics to distances greater than a cutoff value, as recognized also by the authors. An important

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point to clarify is how realistic (or unrealistic) is the high value of slow moving cycles. The best thing would be to compare the percentage of cycles with distance less that a cut off occurring in the simulations versus the occurrence in real ARGO floats.

- In Fig.6 and 7 there seem to be two spots of low error in the vicinity of the Sicily and Sardinia Channel. Could the authors comment on them?

- I found the comparison with the oil spill image (Fig.8) not very useful and almost misleading. I think it should be improved or removed. First of all, the Modis image depicts the oil distribution at day 17 after the spill. Why do the authors compare it with the trajectories of the center of mass of the simulations (red lines)? It seems to me that the comparison should be with the simulated concentrations at the same day. Also, the total center of mass (black line) is not very clear graphically, does it stop before Byblos? Finally, the difference between the real and simulated release point is significant, and the model clearly does not have a correct coast line, as recognized also by the authors. The only positive result is that the simulations show in average a correct direction of propagation, but I am not sure that this is enough to motivate the comparison.

- In 3.2.1. the setting should be explained more clearly. Are the authors solving an advection equation with a fixed decay rate?

- The paper has many typos. It should be re-read and edited carefully.

Interactive comment on Ocean Sci. Discuss., 3, 1747, 2006.