

Interactive comment on “A revised ocean glider concept to realize Stommel’s vision and supplement Argo floats” by Erik M. Bruvik et al.

Anonymous Referee #2

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General comments

This is an interesting paper, looking into the concept of a smaller, slower glider from a practical sampling and operational perspective. It is clearly shown that the idea has potential from this perspective, but it has not been adequately shown that scientifically or economically, it would be a better choice than the current one. Attempts to justify the Eulerian roaming were not convincing, especially for single missions. Using a much larger number and different analysis techniques would help, but it was not justified exactly how this would work. What scientific questions beyond the lucky detection of an episode or feature would such a large scale network address that ARGO floats do not already address? What is so special about 1000 gliders exactly? Are the reasons Stommel used to justify that number still relevant today? Is a global coverage of gliders

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really the most effective use of resources, or should they be used for specific purposes regionally, but in a scaleable, interoperable way? None of this is addressed. Central to this question is cost, which was essentially brushed aside.. Presently, gliders cost about 10 times more than floats to purchase. Operating costs are much higher because pilots are needed, even when the available autopiloting and fleet-type behavior is used. This will get better eventually, but at present the gross value of hardware in the water is not that different: 4000 floats vs 40 gliders is a factor of 10 of higher investment in float hardware cost compared to glider hardware, a gap which is nearly closed when considering piloting costs, increased data transmission costs, boat recovery costs, and the greater land infrastructure requirements. None of these are reduced with a smaller glider, only in the sense of economy of scale as is currently the case as well. The point is that investment in gliders globally is not far behind that of floats, but it is not coordinated globally yet. To have a fleet of 1000 would require serious increases in investment, and thus very strong scientific justification.

Specific comments

While the general comments above essentially critique the introduction and premise of the paper, some specific comments are laid out for the scientific content that follows. Section 2 Fundamental considerations: this section is scientifically sound and well written. On page 7, line 10: it may be stated that those glider manufacturers now have different designs and that performance may differ (e.g. Seaglider ogive fairing or larger Slocum G3 hull). It would be interesting to update the results for those and to run more simulations for reduced volume versions, rather than just one. page 9, line20-25: It is not clear if a CTD-only glider will best serve the global observing system: there are many more Essential Ocean Variables that gliders can (and soon will) be able to measure. This flexibility is one of the strengths of current gliders. Some examination of what payloads would be possible compared to what is normally done now would be interesting, and I think not outside the scope of the paper. Later in the paper, microstructure is mentioned. That paragraph could be expanded to include

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other potential payloads for the small glider. I am not sure why detailed power budgets and engineering calculations should be excluded from the paper. It seems to me that would strongly support the main point of the paper. More details about the strengths and weaknesses of Eulerian roaming are necessary if the reader is to believe this is a viable alternative. The simulations following help, but no indications are given on how such data could be/have been handled other than a simple citation (Todd et al., 2016). This section 2.6 seems out of place, and fits better in the next section.

Section 3. Clear, but should be merged with 2.6.

Section 4. Results and Discussion. The hypothetical case studies are interesting and show the potential, but are not convincing in terms of scientific value. An attempt is made in 4.4, but the analysis from the mission is oversimplified in my opinion. Separating temporal and spatial variability on these year long missions over large horizontal gradients would be very difficult and it is not always possible with one long track to collect data "useful in understanding the role" or that will "capture the properties and variability". The section about altimetry begins to touch on what could be the scientific goal of such a fleet: the surface topography problem. The number of gliders needed to reduce the current errors in the altimetric eddy field (number, phase and intensity) could be quantified in this paper and justify the existence of the fleet.

Section 5. Specific methods of piloting large numbers should be cited (optimal fleet mission planning) as well as the scientific objectives one might achieve with this (e.g. optimized for data assimilation for altimetry or some other objective). This was very briefly touched upon in the conclusions and future work, but really this should provide a solid background to why the reader should even dig into the paper. Clearly this concept is most valuable in a complex large fleet sampling context and some work has been done already.

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