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Interactive comment on "On the mesoscale monitoring capability of Argo floats in the Mediterranean Sea" *by* Antonio Sánchez-Román

Anonymous Referee #1

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1 Overall assessment

The paper entitled "On the mesoscale monitoring capability of Argo floats in the Mediterranean Sea" by Sanchez-Roman et al presents a topic relevant to the journal. This work would suggest the optimal ARGO network coverage needed to sample the mesoscale activity of the Mediterranean Sea. It must be pointed out that in the framework of E-aims project this aspect has been already addressed. All the participant members agreed that a continuous increase of ARGO coverage in term of horizontal/vertical and temporal resolution is desirable but not easily maintained. In this work the authors used Observing System Simulation Experiment (OSSE) (Arnold and Day, 1986; Kindle, 1986) a well-known procedure oriented to evaluate the impacts of input parameters and sampling scheme in an existing observing system. Observations for

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these experiments are simulated so they represent ***optimistic real data*** (Griffa at al, 2007). In this way can be assessed the impact of synthetic data since truth state is known in advance (and everywhere needed). This pioneering investigation through OSSE could give information on the best floats coverage and an estimate of the ***ideal maximum error reduction***. Argo profiling depth and cycles length are crucial parameters in optimizing the energy storage of the floats. A detailed study performed trough OSSE, varying those quoted parameters, was lead by P.-M. Poulain and M. Solari in 2009, and focused to monitor the thermoaline variability in Mediterranean Sea. An estimate of the optimal time cycling for MEDARGO floats was defined by C.Pizzigalli and V.Rupolo (2007). In this work they maximized independent observations and minimize the velocity error at the parking depth. They also analyzed the interannual variability of Lagrangian transport in Western Mediterranean from 2000 to 2004. In this paper the final 3cm RMSE value obtained with 75km box side can be considered as the maximum skill that could be achieved increasing the Argo coverage. For this reason I think that the authors should stress that 75km box side would increase the actual network cost of 6 times in order to have a theoretical maximum reduction of the 40% of the actual RMSE. The scientific methods are not clearly outlined and assumptions are not always valid. Traceability of results is very difficult. Authors give proper credit to referred work but not clearly indicate their own contribution. The overall presentation is not clear. The mathematical background is not explained and not well referred (see also specific comments). There is a lack of supplementary data. I also recommend a flow chart for the methodology developed, since this is the main point of the paper. Which data are employed in which point of the methodology. Looking forward for an improved version of the paper.

2 Specific comments

The paper describes in section 2.1 the ARGO dataset, one would ask why "only profiles with a quality position flag of 1 were employed. The major restriction, however, comes from the salinity data close to the sea surface. Profiles exhibiting salinity flags of 3-4 in the first 5 meters of the water column were removed before DH computation". The quality flag for position is for sure a good way to remove gross bad data, but a second quality check should be done on pressure, temperature and salinity, if one of the corresponding flags is different from 1 the data both T/s at that pressure should be neglected (not only salinity in the first 5 meters). Do the authors check the stability of the ARGO profile? Do the authors check the spikes? In our operational system more than the 20% of input data in NRT are neglected for quality check procedure. In section 2.2 the authors describes the altimeter measurements used referring to Pujol et al 2013 and Pujol et al 2016. Here the time period spans from 1993 to 2014 while in the previous section it seems that ARGO are available from 2003 to 2015. And than the authors do a sort of time homogenization removing a mean altimetric map from 2003 to 2011. I think that some more attention should be paid in this operation or at least it should be better explained. Moreover the heuristic value of 3.54cm coming from data should be well described. In section 3 there isn't a clear description, reference or formula that authors used to evaluate the steric effect from Argo float. Moreover at the beginning it seems that "deeper the reference level (of null velocity), the more information form T/S profiles is consider" but the standard deviation evaluated at 400dbar and 900dbar contradict this sentence (the correlation doesn't, instead). This result is discussed in section 5, but proofs aren't provided. For example if the problem is the different time reference of the dataset a study should be done in order to have them referred to the same value. Conversely in the second hypothesis the authors suggest that velocity at 400dbar or 900dbar are not zero as expected by theory. Why don't they evaluate this term from an operational ocean model? Section 4.1 that should describe the experiment design is a mess. In OSSEs there are some basic step that should be

well described:

- “TRUTH” - the Nature Run or observation;
- What PERTURBATION is used to simulate our incomplete knowledge of the Sea;
- How “synthetic observations” come from the Nature fields
- The synthetic observations are then used in the Perturbation quantify improvements or whatever the aim is.

I think that truth is SLA map at 1/8 resolution. The authors add a random value that corresponds to the sum of instrumental and representativeness error (proof?).

1. The instrumental error for satellite is well defined and is around 2-3cm and depends on satellite (jason1-2 are better for med sea compared for example with cryosat or altika) it can be provided by CLS;
2. Representativeness can be evaluated as Oke and Sakov 2008, re-gridding SLA map at different resolution.

A good way to proceed would be to evaluate the sum of instrumental error and representativeness, if this value is in agreement with the 4.92cm evaluated from author the OSSE can represent a good test bed, otherwise more careful should be paid. Moreover there are some areas in the Mediterranean Sea more dynamic than the others, there I doubt that OSSE is a good approximation. Finally, the authors describe different time window for ARGO and SLA but the OSSE is referred to the whole 2014. So the authors use a perturbation evaluated in a period and apply it in another period. Is it right? According to me this is not a good scientific way to proceed.

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3 Other:

- Numbers should be avoid in the abstract
- Page 2, lines 30-31: The Sicily channel separates the eastern and western basins [Criado- Aldeanueva et al., 2012] This sentence doesn't have meaning. The authors should say the Sicily strait being only 300-400m meter deep divide Med sea in 2 sub-basin circulation patterns. The western basin is influenced by Gibraltar inflow, the eastern is driven by winds and the consequently LIW formation.
- Page 3, line 17:" Argo and satellite altimetry are entirely complementary" This is not false, but not fully truth. They are different type of measurement in situ and remote sensing and sample different aspect and quantity of Mediterranean Sea.

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