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Ocean Science
Discussions

Interactive comment on "Mesoscale Eddies in the Algerian Basin: do they differ as a function of their formation site?" *by* Federica Pessini et al.

Anonymous Referee #1

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The paper makes use of altimeter data (delayed time gridded maps of SLA distributed by CMEMS) from 1993 to 2015 to track eddies in the Algerian Basin. For tracking, the authors implemented an hybrid tracking algorithm spawn from the work of Halo et al. 2014 adapted to the region and with some modifications. The authors then were able to classify sectors within the Algerian basin based on eddy generations/depletions, preferred tracks and so on, evidencing a differences between the eddies associated to the Algerian current and the eddies associated to the North Balearic front. Overall the paper has a clear logical flow and the conclusions (but one statement) reflects the results presented. The title is OK.

The paper however is substantially descriptive, as processes behind the formation/depletion of eddies in the sectors considered, are not tackled. At the same time,

the paper is not methodological, as the methodology implemented is substantially based on previous literature. These considerations somewhat degrades the relevance of the paper for the international community.

Major remarks

Section 2: The tracking algorithm spawn from the work of Halo et al., 2014, while modifications by Pessini et al. are described in section 2.5. The description of the tracking algorithm is very long (also considering that, say 90%, was developed by somebody else) and many details that can be skipped pointing to the existing literature. I also suggest to move regional values adopted and other in-depth details to an appendix to enhance readability.

Section 2.5: In order to prove robustness of the "eddy continuity routine", the authors discuss the successful example of the eddy described by Cotroneo et al. 2016. I wonder if there are cases of failure of this routine and why. Also, I believe the authors should discuss what is the impact of this routine on the results presented later. In the conclusions the authors state that this modification is an improvement, but I cannot judge. In general, there is no attempt by the authors to provide a measure of uncertainty of the methodology.

Results: my main concern is about the threshold between short-life and long-life eddies (90 days). The choice of the threshold does impact the results presented (in particular figs.8-10-11-13 and associated conclusions). The choice of 90 days seems arbitrary and can alter the inference on "longer-life" short life eddies or "shorter-life" long life eddies. To make the analysis robust, I really think the choice of the threshold should be at least inferred from statistical or dynamical arguments.

The reasons behind discrepancies with Escudier et al., 2016 should be discussed.

Minor and editorial remarks:

The dataset used (SLA) should be presented in a separate sub-section of the MM section, not buried inside the descriptions of the tracking algorithm. The authors specify that the dataset begin in 1993, but not the end (2015?).

Pg 3, l9: Instead of fusco et al 2008, a better references can be Rixen et al GRL 2005 and Schroeder et al SciRep 2016. Also, as detailed in Schroeder et al., 2016, WMDW experiences relatively "short-term" (few years) changes, not long-term (decadal) only. Budillon et al 2009 is grey literature. I would consider dropping it.

Pg4 last para: physical vs. geometrical methods. Many references are listed for physical methods, while none for geometrical methods . . .

Pg 6, l 6: "the number of tunable parameters is thus reduced to three". This statement comes out of the blu

Conclusions, pg 20 l10: "(15)"????

Conclusions, Pg 22 l3-5: I do not agree with the downgrading of the relevance of short-life eddies. Unless the authors support this sentence with some references, I would just erase this statement.

The last paragraph in the conclusion section ("In the past [. . .]") is not justified by any result presented and should be dropped.

Figure 8, 11: If I understood correctly, blue and green bars include also eddies formed and terminated in the same sector (equivalent to yellow bars). My suggestion would be to show in blue only eddies formed in the sector and terminated in a different sector and accordingly for green. In this case, the figure would clearly visualize the dominance of the yellow bar in fig 8 at least.

Font size in figures in general should be made larger.

Fig 10 and 13 are not very high quality figures and results can be easily summarized in one single table instead.

Figure 1: missing many geographical names as well as oceanographic features (e.g.,

Gibraltar, AC, AG, NBF. . .). All names cited in the text have to be presented in figure 1 for readers unfamiliar with the region.

The way references are managed in this draft may be an academic example on how NOT manage citations and bibliography. I strongly recommend the authors to read OS citation guidelines (https://www.ocean-science.net/Copernicus_Publications_Reference_Types.pdf) and, why not, to give a try to one of the reference management software available on the market . . .

(a) Many places in the text: citations should not include authors' first names (e.g., Isern-Fontanet and E. Garcia-Ladona 2003, Pasquero and A. Provenzale, 2001). Besides, the latter should be Pasquero et al . since the authors are three. . .

(b) Pinardi et al., 2013 is indeed 2015

(c) Pg 5. L13-14 and Pg 6, l9: websites should not be included in the main text, while listed in the references following OS rules

(d) Penven and Echevin 2005: there 3 missing authors. Accordingly, in the text it should be cited as Penven et al. . ..

(e) Volume number is generally not mandatory, but page numbers are.

(f) Puillat et al 2002: is the title incorrect?

(g) Pasquero et al. 2001. As said, missing one author (A. B. should be A. Babiano. . .)

I may have missed other errors. . .

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