Reviewer #1

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.

We thank the Referee for considering our paper and for raising a number of important points. All the corrections resulting from his/her comments will be included in the final version of the manuscript. As well, as requested, we will describe the processes linked to the formation and depletion of eddies. Additionally, the differences between the applied tracking method and the methods published in previous literature have been better described.

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.

We will reduce the description of the tracking algorithm, moving some details to an appendix as suggested.

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.

The tracking algorithm is unable to recognise events such as merging and bifurcation of eddies, and the continuity routine does not fix this problem but at the same time it does not add errors to the results.
An evidence of failure would be represented by mistakenly joining of two structures, which are not easily detectable by using SLA data. In fact, we did not find any such case.
In our opinion, a good example of the mode of operation of the continuity routine can be shown by the "very long-life eddy" observed by Puillat et al., 2001. The tracking algorithm, even after application of the continuity routine, erroneously detects the termination of the structure near the coast, where the closed contour of Okubo-Weiss bifurcates. Nevertheless, in this case the continuity routine proves to be useful as it joins two separated features (410 and 295 days long respectively) into a single structure 706 days long.
The cases of bifurcation and merging of the structures are complex topics we have not addressed in this work, but which will be the object of detailed study in future. Nevertheless, the tracking algorithm is not the most suitable tool to investigate the interaction between these structures.
In conclusion, we can affirm that the continuity routine we have implemented improves the results, even if it does not resolve the problems of merging and bifurcation of eddies.
The application of the continuity routine over 24 years leads to the decrease of the total number of eddies detected (from 8208 to 6543) with the consequent increase of the mean lifespan from around 66 to 88 days.

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 threshold value of 90 days has been chosen as a function of the distribution of all the lifetimes of anticyclonic eddies. In fact, north of 39° N, the lifetimes within the 97^{th} percentile are shorter than 90 days. Furthermore, we observed that 97% of the cyclonic features (included on the suggestion of Reviewer #2) in the northern part of the basin have lifespans shorter than 90 days.
In general, considering the whole basin, 95% of both kinds of structures have lifespans less than 90 days.
For these reasons we would maintain the threshold value of 90 days to discriminate between short- and long-life eddies.

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

The general intention of our manuscript is to highlight the differences between the structures formed along the Algerian Current and the eddies formed in proximity to the North Balearic Front, while the paper of Escudier et al., 2016 focuses only on the

southern Algerian Eddies. Nevertheless, a further description of differences and similarities with the paper of Escudier et al., 2016 will be inserted in the new version of the manuscript.
In particular, as the new version will include the analysis of cyclonic and anticyclonic structures over a larger spatial domain, it will ease the comparison.

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?).

We changed the structure of the draft as suggested.
SLA data are now available until the end of 2016 (2014 in the previous version). In the new version, we applied the tracking method and elaborated the statistics to the end of 2016.

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.

We changed and integrated the references as suggested.

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

We added references for geometrical methods, and in particular Sadarjoen and Post, 2000 and Nencioli et al., 2010 were usefully inserted.

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

The statement was not clear, so we will better explain which parameters in the detection algorithm can be changed according to the researcher's interests.
In particular this sentence has been changed as follows:
"The tunable parameters in the detection algorithm are three: the interval between the contours of SLA, the maximum radius of a closed contour of SLA detected and the threshold of the Okubo-Weiss parameter.

Conclusions, pg 20 l10: “(15)”????

It indicates the number of the equation, but “Eq.” was missing. Corrected.

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

The sentence has been removed.

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

The paragraph has been removed

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.

We agree with the observation of the Reviewer. In the hope of making the graph clearer we have included the bars of “formation and vanishing” over the bars of the “formation”.

Font size in figures in general should be made larger.

We think that it depends on the Copernicus template, as we used the style suggested in the Latex file, but we will check and eventually we will ask to enlarge the font size in all the figures.

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

We will substitute the figures with a single table as suggested.

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.

We will add more geographical names in order to better describe the study area.

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.oceanscience.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…

The bibliography will be modified according to the correct format.