REF#1

Summary

This article summarises the surface circulation in the Eastern Mediterranean as inferred from the deployment of 97 surface drifters over a period of roughly two years.
The sampling strategy involved frequent small deployments in, or across, key circulation regions such as channels and persistent eddies. Good coverage by the drifters is available both spatially and temporally, the latter allowing an analysis of seasonal variability.
The drifter analysis is thorough, and a number of new and interesting features are discussed, such as the branching behaviour of Atlantic Water (AW) after passing through Sicily Channel and a reversing circulation along the southern edge of the Levantine Basin. Overall, this article makes an important contribution to our understanding of the circulation in the Eastern Mediterranean. However, this article would be improved significantly by further investigation and discussion of the possible mechanisms responsible for the seasonal variability identified. For example, changes in wind forcing are hypothesised as a source of seasonal variability, but not even a qualitative discussion of wind patterns during the time period of this study is provided. It would be nice to see some of these suggestions teased out further. The other major concern is that more information needs to be provided regarding error in the drifter velocity fields.

General comments

1) While the primary readers of this article will most likely be familiar with the region and previous studies conducted here, I feel that the manuscript would benefit from the inclusion of a schematic diagram of the circulation, with the key circulation features (e.g. LE1, MMJ, Mersa Matruh, etc.) labelled. If space provided, it would be particularly nice to have a schematic with multiple panels that showed a historical representation of the circulation and what aspects of the circulation has changed based on these (and other recent) studies.
c1. We do not know yet whether we have enough space to include a figure presenting the historical succession of the maps of the circulation in the Eastern Mediterranean. We could include something like Figs. A1 and A2. But, since a previous paper already shows this historical succession through multiple panels, we prefer to add somewhere in the introduction: “multiple panels showing the historical representations of the surface circulation can be found in Hamad et al. (2005)”.

2) I have no question that the processing or analysis of the surface drifter data was completed properly—some of the authors have a great deal of experience in this area and the proper references are cited. However, further information needs to be provided about how reliably the drifters are representing the surface flow. At the very least, an error estimate of the velocity measurements needs to be provided. Also, 40% of the observations were collected while the drifter was without a drogue. This is a large percentage of the observations and I would not expect the error in the velocity estimates from drifters without drogues to be the same as those with drogues.
You might also consider addressing the following: Are the surface drifters showing largely windforced Ekman velocities or geostrophic velocities? Are the surface drifters steered by topography in the different basins?
c2. Poulain et al (2009) (we have updated it) show that the magnitude of the wind-driven currents is about 0.7% and 2% of the wind speed for the drogued and undrogued SVP drifters

respectively. This means that a wind of about 10 m/s induces drogued SVP wind-driven velocity of about 7 cm/s (undrogued: 20 cm/s).
Experimental position errors (due to the Argos system) can produce error of about 3 cm/s on the velocities. Additional errors can be included, such as the errors related to the kriging interpolation and the limited number of samples (standard error). However, we believe that the most important source of error is due to the bias introduced by the bin-average of data non-uniformly distributed in both space and time. The observations included in one bin can span a few days or an entire extended season and the error related to this bias is difficult/impossible to evaluate. To reduce these bias errors, we considered only bins with more than 10 observations and coming from at least 2 different drifters.
The pseudo-Eulerian statistics (mean flow, variance ellipses, EKE and MKE) have been re-calculated excluding undrogued observations with a speed ratio (wind-induced slip/drifter speed) larger than 50%. Results are essentially similar to the maps presented in the original paper.
Geostrophic drifter velocity are evident mainly in the southern Ionian and are confirmed by altimetry products. As evidenced by Poulain et al. (2008), the drifters feel significantly the direct (Ekman like) and indirect (through adjustment of sea level and geostrophy) effect of the surface winds. The indirect effect is more important in coastal waters. For instance, the reversal of the circulation in the southern Ionian claimed to be related to the change in wind forcing, is also evident in satellite altimetry maps.

3) In the discussion a number of seasonal differences in the surface circulation are identified. In most cases only a brief suggestion of the mechanism for the variability is given. The manuscript would be greatly improved by providing the reader with further information about these differences. For example, on Page 539, line 2, "This clockwise elongated circuit seems to be related to the wind forcing that influences the circulation at seasonal time scales as explained by Pinardi and Navarra (1993)." Were there big seasonal differences in the wind velocities during these years? Similarly, further discussion of the reversal in the Levantin Basin between 20_E and 25_E is needed.
Are the eddies here (LE1, LE2, EE) permanent features? Is there any evidence that this seasonal reversal is a persistent feature of the surface circulation? What are the implications for the seasonal reversal in the circulation here, is it primarily a transport issue, or will the reversal also lead to changes in the modification processes of AW?
c3. Maps of mean wind vectors have been added to the revised paper. Considering the extended winter (nov05-apr06) and the extender summer (may06-Oct06), one can note that mean winds are stronger in summer but that the variability is larger in winter.
There is no significant change of wind prevailing direction in the Levantine. In contrast, in the Ionian, in winter the prevailing winds are from west, while during summer they turn to north or even east (see Figs. B and C).
LE1, LE2 and the EE are not permanent features. No eddies are permanent, but they occur continuously, as shown by the detailed 4-year analysis of Hamad et al. (2006). The EE area is characterized by eddies that tend to accumulate and/or merge. On the other hand, between ~20-25°E, generally the (LE) eddies propagate at a few km/day. While Hamad et al (2005, 2006) observed an eastward (downstream) propagation of the eddies in this area, EGITTO/EGYPT drifter data show a westward drift. For example the three drifters trapped in the LE2 described anticyclonic rotations that progressed westward from April to September 2006. The westward drift velocity was about 1-2 km/day and this LE2 eddy travelled more than 250 km during those six months (see also Sutyrin at al, 2009).

One consequence is that the mean current statistics computed in such an area where the current path is modified by propagating eddies can be biased (see also c4). There is no seasonal general reversal of the circulation in the Levantine Basin. The anticyclonic rotation associated with LE2 can induce a westward current along a small portion of the slope, but the flow is eastward between ~21-23°E and between LE2 and LE1. The impact of the positions and the propagations of the eddies on the drifters trajectories for each deployment can be verified on the animation of the trajectories visible on http://doga.ogs.trieste.it/doga/sire/egitto/database_egitto/movies/sep05-oct07.avi.
We do not have enough data to affirm that the seasonal reversal in the southern Ionian is a permanent feature or not, but it was already pointed out in Poulain and Zambianchi (2007) using a different drifter database.
Due to the seasonal reversal, AW can delay its transport to the east and can modify its characteristics through the mixing in the southern Ionian.

Sutyrin G., Stegner A.. taupier-Letage I. and Teinturier S.: Amplification of a Surface-Intensified Eddy Drift along a Steep Shelf in the Eastern Mediterranean Sea, J. Phys Oceanog, 39 (7), 1729–1741, 2009.

4) A key conclusion of this manuscript is that the Mid-Mediterranean Jet (MMJ) arises from the contributions along the outer edge of the major anticyclonic eddies in the Levantine Basin (what the authors refer to as the paddle-wheel effect). This eastward flow arises in Eulerian maps of the mean flow because of persistent eddies. However, from a Lagrangian standpoint, if all the drifters simply become entrained in the major eddies, the MMJ would be an ineffective mechanism for transporting AW eastward.
The authors should discuss how frequently drifter trajectories follow the MMJ path into the eastern Mediterranean (it seems to be relatively infrequent from Figures 5 and 6). If trajectories following the MMJ path do exist, then the significance of there being two different paths to the eastern side of the Levantine basin should be discussed. For example, would AW undergo different modification processes along these two different paths?
c4. We have made clear (in the introduction) that the surface circulation is not necessarily the circulation of the water of Atlantic origin, which means that we do not differentiate between AW and Levantine Surface Water.
Only 4 drifters spanned the longitude from 22°E to 29°E. Three of them, after being transported in the central part of the Levantine successively by LE1, LE2, IE and/or EE at least, ended up on the coast close to Alexandria (see Fig. 5 of Taupier-Letage, 2008) and only one reached Cyprus (see Fig.D). As already said in the paper, the paddle-wheel effect plays a fundamental role in the offshore transport of the surface water and this means that, thanks to the eddy presence, surface waters (possibly of Atlantic origin) can cross the basin reaching its centre and even Cyprus.
This is one of the possible path that surface waters can follow, the other one is (due to Coriolis) all along the slope. While the alongslope path is well defined in the drifter tracks and in the drifter mean flow, since only a few drifters went offshore thanks to the alongslope instabilities (one of them stranding in Cyprus) we can only affirm the existence of a (meandering) path that can cross the Levantine basin. This is not a definite strong jet but more like the contribution of several paths that result from the local and temporal distribution of the eddies.
Moreover, as we said in c3., the statistics computed for the mean current in the area of the LE1 and LE2 eddies can be biased. Caution must be taken when we look at the mean flow maps (Figs. 7b, 8b and 9b). We have compared the amplitudes of the N, S, E and W

velocities in the box 20-24°E and 32-34.5°N, and we show that the northward and southward mean velocities are of the same order of magnitude as the E and W velocities. The E component is obviously greater, which accounts for the eastward general circulation, but the fact that amplitudes of the N and S components are similar, shows that the strong eastward current represented in the mean flow maps is an artefact due to anticyclonic moving eddies. The fact that the eddies moved (along-basin) imply that, in the statistics, the N and S components cancel each other, while the E (offshore) and W (alongslope) components result in a zonal mean flow. Indeed, if the centre of the LE eddies would have been steady, the resulting mean flow maps would reveal two distinct eddies (like IE and EE) and no eastward offshore flow (westward alongslope) in between.
Additionally, since here we do not study the water masses properties, we cannot say anything about the modification of the AW along the different paths.

5) Drifters are drogued at a constant depth, and therefore do not follow individual water masses. Since the surface circulation is uniformly to the east across Sicily Channel, it must be returned at depth. Is there any indication from the drifters (or using other data), where the fluid is sinking to be returned to the western Mediterranean (likely to occur in areas of convergence or divergence). Drifter trajectories will not represent paths of water mass transport in this regions.
c5. This is absolutely true. The drifters are meant to study the surface circulation. And the aim of this paper is, in fact, the study of the surface current in the Eastern Mediterranean. The intermediate and deep circulations can be examined only through a water masses analysis (mainly with CTDs and moorings).

Specific comments
The following are minor comments and suggestions to improve the manuscript.
Page 526, line 11, “completes" should be complements. “Several veins are evidenced", not sure what is meant by veins.
c6. Ok this is clarified in the revised text.

Page 527, line 6, “lighter Atlantic water (AW)." Some more details should be given about the signature of AW—is it warmer and fresher or is its reduced density due to a single component. How easy is it to track the flow of AW throughout the eastern Mediterranean?
See c4.

Page 527 and throughout, it would be preferrable if circulation features and eddies were referred to as either cyclonic or anti-cyclonic rather than using clockwise and counter-clockwise.
c7. We intentionally used these terms to differentiate the topographically-controlled permanent features (clockwise and counter-clockwise) from the more variable ones (cyclonic or anti-cyclonic) (see terminology on http://www.ifremer.fr/lobtln/OTHER/Terminology.html). But, actually, this can create some confusion. We have modified the revised text using cyclonic/anticyclonic only.

Page 527, first paragraph, Figure 1 and potentially a new figure showing a schematic of the circulation should be referenced here and throughout the introduction to aid the reader in understanding the circulation features discussed.

See c1.

Page 528, line 2, “displaying a new feature: a clockwise circuit in the northern part of the Ionian." Is this in addition to the counterclockwise circulation mentioned on Page 527, line 17, or is the sense of the circulation in this part of the basin reversed from historical representations? This should be made clear in the text.
c8. The historical maps, due to the limited data at that time, can represent only the picture of the surface circulation in a certain time. Our intention here is only to go over the various schema presented in the years to make clear that the surface circulation in the eastern Mediterranean is already not well defined especially in the southern part of this basin. In particular, on page 528, line 2, we are describing the Malanotte-Rizzoli et al. (1997) schema. However, it is known that this clockwise circuit was observed for ~10 years in the 1990s (decadal variability) and it vanished from 1998, as proven by Hamad et al. (2005) at least. The EGITTO/EGYPT drifter data show that it re-appeared, but we have not enough data to study the evolution of this feature.

Page 528, line 23, “described in the POEM diagram", please add reference.
c9. Ok done.

Page 533, Figure 4, The drifter trajectories in January 2007 are quite different from all the other deployments in this figure. It would be good to add a discussion of what forcing might be responsible for this discrepency. In particular, it does not appear to be a seasonal issue because trajectories from the February 2006 deployment are very different.
c10. Right, it is not a seasonal issue. The drifters were deployed upstream of the entrance of the Sicily Channel. This is an area characterized by a complicated surface dynamics as already revealed by Poulain and Zambianchi (2008). Here, the surface flow can enter the Channel or can move to the north-east missing the entrance.
Anyway, following the suggestion of the referee #2, we decided to shorten to the minimum the description of the initial trajectories.

Page 533, line 28, “The curves and sharp bends in trajectories indicate the presence of other eddies . . . and meandering structures." Please be clear that you mean smaller scale eddies, or mesoscale eddies from your definition in the introduction.
c11. We have added that they are mesoscale eddies.

Page 535, line 10, “but between 14_E and 16_E where there are more than 100 observations in bins", this is an incomplete sentence.
c12. It has been rephrased.

Page 536, line 22, “It’s signature is less intense during winter since drifters were entrained only around [IE]." Is there any other evidence that this feature has seasonal variability. Since it is stationary, is it localised by a topographical feature, or is it related to wind forcing?
c13. Ierapetra is created or reinforced every year in summer by the northerly Etesian winds, it can be stationary for a couple of years or can markedly move as south as the Egyptian slope or west of Crete. It can live several years and therefore co-exists with the newly generated eddy. Its signature (from drifter tracks) is less intense in winter only because in summer several drifters were released inside it, while during the winter releases drifters went around it

(paddle-wheel effect). In the mean flow maps (to be separated from the MKE/EKE info) Ierapetra is well represented in both seasons.

Page 537, line 14, “In the Levantine sub-basin, the winter circulation computed is similar to the total average." It would be good to be more quantitative here: what measure of the circulation are you using?
c14. Has been rephrased in the revised text.

Page 538, first two paragraphs of Section 4, Most of this is already covered in previous sections and does not need to be repeated.
c15. Right. It is not repeated in the new text.

Page 538, line 26, “It can be compared to the mean circulation already described by Malanotte-Rizzoli et al. (1997)." It would be more useful here to state how this data differs from the circulation picture described in the referenced paper.
c16. Has been rephrased. We have added that we have confirmed the referenced circulation picture providing information on the southern part.

Page 539, line 12, “comforted" should be “confirmed".
c17. Corrected.

Page 539, line 23, “This reversal is confirmed by Poulain and Zambianchi (2007) and presumably related to the forcing by the prevailing southeastern winds." Please add some further information about the winds here. Can you give some suggestions for why the flow reverses (see also point 3 above).
See c3.

Page 545, Reference Figure 1 in the caption of Table 1.
c18. Ok

Page 548, Figure 2, It would be nice to use different color trajectories for those that are entrained in the various labelled eddies.
c19. We would like to exclude colour figures from this paper. We believe that using different gray shades for the tracks would not help to identify better the eddies.

Page 552, Figure 6, This is a nice figure! Filtering like this really pulls out the key structures and their propagation through the basin.
Figures 7-9, It is difficult to make out both the colours showing MKE/EKE with the arrows/ellipses overlaid. You could consider adding more panels to make this clearer.
c20. Right, MKE and EKE maps have been separated from the mean flow and variance ellipses plots.

Also, it might be interesting to reduce bin size in interesting regions where you have a large number of observations (such as in LE1 and LE2) to improve the resolution of these eddy features.
c21. We prefer to keep a constant bin size or scale separation throughout the paper. Focusing on the description of some eddies using smaller bin size is outside the scope of this

paper. Furthermore since the eddies are moving, the pseudo-Eulerian description might not be appropriate.

Also, it is typical to leave titles off the plot panels since the information is given in the caption.
c22. Right

REF#2

Overall comments:
The article describes the main surface circulation features of (the southern part of) the Eastern Mediterranean Sea depicted from tracks of 97 drifters deployed in the area during 2005-07. The manuscript includes important information, valuable for describing the spreading of Atlantic Water (AW) in the Ionian and Levantine basins and the regional dynamics of the surface circulation. This kind of information is important for understanding the pattern of the regional circulation and its seasonality, especially in an area where observations are very sparse in space and time, but the results are mostly descriptive and of statistical character. The discussion about mechanisms involved is minimal (and in some places not really convincing). I find the paper important and interesting for publication, but the authors can greatly improve it through reorganizing and extending the discussion of regional dynamics.
c23. The paper has been improved by extending the discussion and listing the mechanisms involved in the dynamics of the surface circulation. As pointed out by the referee, the drifter observations used are very important and self-pertinent and, most of all, they are the first of this kind in this poorly-known area: one of the aims of this paper is to deliver quickly this new information, and provide modellers with new hypotheses to test.

General comments:
The description of the atmospheric forcing pattern (during the experiment or at least the climatological pattern) is absent from the manuscript. The wind effect is mentioned in several places in the manuscript without describing the wind pattern (and its seasonality) in the area. Other processes affecting the surface circulation pattern, such as topographic effects (obvious in some cases), is not discussed.
See c2. and c3.

The discussion about the mechanisms involved in the eddy generation and evolution is very weak. This is also evident in the discussion of the role of eddies in the Mid Mediterranean Jet region. Although I tend to agree the mechanism of eddies detached from the boundary current is very important for carrying AW northwards, the article does not give more information about eddy translation and variability (through the overall and individual drifter tracks).
c24. The mechanisms involved in the eddy generation and evolution are outside the scope of this paper. There are other papers already (or about to be) published (Sutyrin at al., 2009 and Taupier-Letage, 2008) or in preparation on the data acquired during these EGYPT/EGITTO experiments (not only drifters) and other complementary databases.

Comments about "current reversal/fluctuation" are not thoroughly proved in the article, as opposed to eddy variability/eddy field. The part describing the first ten days trajectories (page 533/line 12 – page 534/line 14) does not seem to contribute to the discussion of the surface circulation. The authors should either remove this part or explain the necessity of this analysis and the major findings.
See c10.

Did the drifters deployed record other parameters (e.g. surface temperature)? If so, it would greatly benefit the article to include the recorded water characteristics, especially since the main goal of the article is tracking the spreading of AW in the Eastern Mediterranean. If not, the reader should not be directed to a dataset (page 532/line 26), but a plot of the regional water characteristics should be included.
c25. Indeed the drifters recorded also SST, as mentioned in the paper. The measurements are taken only at the drifter positions, this imply that what we observed from the SST drifter data is temperature variability due to the heating/cooling of the surface water during the year following the drifter and from this information it is not easy to describe the water mass characteristics and estimate their motions. Anyway, satellite SST prove that in general drifters move keeping the warm waters to the right (see Fig.D). As said in c4., we have made clear (in the introduction) that the surface circulation is the circulation of the water of Atlantic origin, which means that we do not differentiate between AW and Levantine Surface Water. The dataset cited was used only to find the beginning months of the extended 6-month season so as the extended winter (summer) corresponds to the most homogeneous (stratified) conditions. From this analysis, the periods Nov-Apr and May-Oct resulted optimal.

The effect of missing drogues is not clearly established. Although a correction is applied (and I know the authors are experts in this field), it should be mentioned somewhere in the manuscript what are the overall differences in the results after removing this part of the data set.
c26. This has been explained in the new text, see also c2.

A map of the geographical nomenclature and the general surface circulation patterns described in the introduction (and later in the text) could be very helpful.
c27. The geographical nomenclature is already depicted in Fig.1, while for the general circulation historical schema see c1.

Finally, the Conclusions section is very weak (it is actually a "future work" section).
The authors could summarize what they mean by "improved or novel description of the surface circulation", by pointing out improved/novel findings in the region.
c28. Right, the conclusions have been restructured.

Specific comments:
In the description of Ierapetra and Pelops eddies (Page 528/line 21) please add reference.
Please provide reference for the "POEM diagram" (Page 528/line 24).
c29. ok

The authors mention that "in the Levantine sub-basin, no clear seasonal signal was expected to dictate periodic releases" (page 531/line1). Can they explain and prove that?

c30. The EGYPT-EGITTO data are the first ones, and the results we obtained confirm that there is no seasonality in the south from ~20°E eastward (at basin scale). Additionally, wind pattern (Figs. B and C) also shows that there is no strong seasonality in the prevailing wind direction in that region. We change the text adding " expected from historical measurements and wind data".

The MKE/EKE fields presented in page 538/line 4-9 should be interpreted and discussed.
c31. ok, done.

The authors mention that the circulation pattern "can be compared to the mean circulation already described by Malanotte-Rizzolli et al.". What are the findings from this comparison?
See c16.

Figures 7-9 are "very busy" and difficult to follow. A suggestion could be to split the figures in subareas, where the reader could follow easier the results and comments.
See c20.

REF#3

GENERAL COMMENTS:
The paper makes a significant contribution to the knowledge of surface circulation in the Eastern Mediterranean. The drifter data set used is unique and robust enough to draw some conclusions for the time period of study for some of the sub-regions of the area. The authors need to be careful however not to draw too many conclusions in regions or times where data are scarce, or imply that these data represent some kind of mean pattern. That would require a more rigorous study in combination with a dedicated remote sensing and modelling analysis and possibly another in situ data set over the same period. While the paper's results were interesting, the description of the various features was difficult to read, and many times the punch line was diluted as a result. With the proper clarifications and simplifications this paper could improve greatly, since the data it contains are so powerful. I would encourage the authors to follow up this observational paper with a joint study with numerical and remote sensing components and even perhaps other in situ hydrographic data.
c32. As the referee said, the dataset is unique. This is the first drifter dataset that explore also the southern part of the Levantine sub-basin. We are aware that we must be very careful with the conclusions and this is why the paper is mostly descriptive and of statistical character. It is important to consider that one of the aims of this paper is to deliver quickly this new information, and provide modellers with new hypotheses to test.
Other papers using also model products, satellite SST data and other *in-situ* data are already in progress.

SPECIFIC COMMENTS:
A couple of scientific issues mentioned above need further explanation. One is the care that must be taken in interpreting scarce data without a clear tie-in to other data sets. To stay within the scope of this observational paper, this could be limited to a few remote sensing images and an independent source of in situ data that could add credibility to the circulation

that emerges. Although a great data set, there are still (as the authors state) many gaps and many pitfalls in interpretation.
c33. A figure has been added (Fig. D) with the trajectory of the drifter that reaches Cyprus superimposed on a temporal composition of satellite SST images. This figure shows that, in general, drifters move keeping the warm waters to the right. We will not add other remote sensing images because they can be found in other paper already cited and because a detailed paper is in preparation.

Another issue is that of Atlantic Water. It is assumed that any surface current towards the east transports AW, but no evidence of water properties has been given. We do not have information on water mass transports from drifters directly. Some connection to other observational or model studies that show to what extent a surface current in this region represents the subsurface flow of AW would be an excellent addition.
See c4. and c25.

TECHNICAL COMMENTS:
It is not valuable to list all the corrections at this stage, but there are a number of places where English was not used properly (veins, informations, superficial, individuated, well evidenced, comforted by), Also text was confusing in some places. Most critically, the results and discussion sections were too similar, both getting bogged down in cataloguing features and even individual drifter tracks. The first two paragraphs of the discussion could be scratched and the following 3 or so trimmed down to one by not repeating results. This only leaves about 3 paragraphs, which is not nearly enough make important conclusions (although, as mentioned without another source of data, definite conclusions are difficult to make).
At least there should be a stronger tie to the literature mentioned in the introduction, and a few references could be added. (such as Ozsoy/Hecht for POEM data, Melanotte-Rizolli/Bergamasco, Wu and Haines, Zavaterelli and Mellor, Zodiatis et al. for circulation studies, and perhaps an XBT study, Manzella et al.)
There are a few places where broad statements are made that don’t help much the paper, like "data set is widespread" and "interactions between eastward slope current and eddies are well-evidenced". They could be removed or re-worded to say something with more impact.
c34. Thank for all these comments, they have been implemented in the paper.

Finally, it would help if the QC method and method for calculating EKE and MKE were briefly stated. Presuming they are relatively simple, it helps the reader to see them (especially when the reference is a CD).
c35. The QC method is well referenced on page 532 (Poulain, 2001 and Emery and Thomson, 2001). The MKE and EKE computation is quite simple and the definitions have been added in an Appendix.