Anonymous Referee #2

We thank the anonymous referee #2 for its constructive remarks. Specific responses to the reviewer' comments are found below:

The title describes the main focus of the manuscript. It is specific to surface currents since only these can be estimated (assuming geostrophic balance) from the altimetry.
"Focus" above is the correct word. There is some argument for the merits of the approach, but otherwise the approach is very much "we used this method for the Black Sea and here are the results". The merits are vis a vis using drifters and altimetry separately, or an alternative way to combine them, but there is little assessment of how good absolutely are the results beyond qualitative statements about consistency (or not) of some features with earlier findings. Some quantitative assessment would help, and also some discussion of what factors are likely to make this approach perform well (or not) in other contexts. Many more potential readers will have interests in other areas than the Black Sea.

I wonder if this approach makes full use of the drifter data. They seem to be used only in a one-off regression for the whole time period as a "calibration" of the altimetry data which then becomes the main data source. This should be a point for discussion.
That's correct! Eq. 2 can be viewed as a correction of the altimeter data using the drifter measurements.

The presentation is generally straightforward / logical, and the English generally good although there will be changes when copy-edited.

Detailed comments

Page 1506. The Introduction should begin with some motivation about why Black Sea currents are particularly interesting.
We have added the following introductive paragraph: "Surface currents contribute significantly to the distribution of hydrological properties, nutrients, pollution and other kinds of dissolved and suspended particles, among adjacent coastal regions and between coastal and open-ocean regions. Their assessment assume an important role in the densely inhabited coastal areas and semi-enclosed basins, where pollutants and contaminants are produced and discharged in large amounts. In this contest the Black Sea (BS), with a catchment area that cover large parts of Europe and Asia (Stanev et al., 2005), represents a good example of marginal semi-enclosed basin, characterised by a limited water exchange with the other basins and an intense anthropic activity and rivers discharge. The upper-layer dynamics of the BS is conventionally described as the sum of a basin scale cyclonic boundary current over the continental slope (e.g. Oguz et al., 1992), cyclonic gyres in the basin interior (Korotaev et al., 2001) and quasi-stationary or recurrent anticyclonic eddies along the coast (Korotaev et al., 2003). The interaction among these different and multi-scaled features contribute to the mixing of coastal water with the open sea and results in a high spatio-temporal variability of the current field."

The first paragraph discusses the merits of the approach relative to drifters and altimetry separately. What about comparison with other methods of estimating (Black Sea) currents?
Data of BS currents derived from other methods (like current meter moorings) are not readily available. Even if they were any, they are representative of a limited number of specific locations and generally exclude the near-surface layer. So comparison is not obvious. Additional drifter data could have been used for validation purposes but here we have decided to use all the drifter data for the combination method instead of selecting some of them for comparison with the results.

Page 1507 line 23. "spikes" (maybe more than one).

Ok, done.

Page 1508 lines 6-7. I do not understand “used with the drifter velocities” if the coefficients “_ and _ are drawn from the results obtained in the Mediterranean Sea from Poulain et al. (2012)” (lines 16-17). Was a regression carried out for the Black Sea data or were the results of Mediterranean Sea regression used?

We have clarified this issue in the revised version of the manuscript (section 2) and we have shown the regression results in Table 1:

" The model was applied to the different drifter designs separately, over the whole BS, and the results are presented in Table 1. "

Page 1509 equation (2). I guess that this regression is for each spatial bin separately but over all the daily data. Please clarify here (rather than later suggestions) and also that the velocities and B (also in line 12) are vectors. Is A just one scalar or could it reflect different scalings for two vector components as hinted in line 8 and later references to |A|?

We have clarified all these issues in the revised version of the manuscript; in particular we have added the following description:

"Drifter geostrophic velocities and satellite altimetry data were averaged in non-overlapping geographical bins of 0.25° x 0.25° x 1 day and, within each spatial bin, they were combined using the following regression model (according to the method described in Poulain et al., 2012):

$$\boldsymbol{U_{DG} = A\, U_{SLA} + B + error,} \qquad \textbf{(2)}$$

where the vector U_{SLA} includes the bin-averaged anomalies of surface geostrophic velocities, concurrent with the bin-averaged drifter geostrophic velocities U_{DG}. The unknowns A and B are complex number or 2D vectors. The slope A is the local adjustment of amplitude of U_{SLA} (Niiler et al., 2003). Over the time period considered for the definition of SLA (1993-1999), the mean of U_{SLA} is zero and B coincide with the time-mean drifter circulation; for any other period, the mean of U_{SLA} is not zero and B is the offset between U_{SLA} and U_{DG} (Menna et al., 2012). "

and also:

" The veering angle introduced by A (not shown) is not significantly from zero."

Page 1509 lines 14 to 17. These additional motions only affect |A| if they are somehow correlated with SLA but not with the “correct” magnitude.

Yes, otherwise they would be in 'error'.

Page 1509 line 24. Does this definition of UG imply that the values are derived from satellite altimetry only? Please clarify.

As explained in section 2, once the unknowns in Eq. 2 (A and B) have been estimated for 11-year of concurrent drifter and altimetry data (1999-2009) they can be used to estimate the 'unbiased' mean $\langle U_G \rangle_u$ from the satellite altimetry data for any time period, e.g.:

$\langle U_G \rangle_u$ over the period 1999-2000 can be estimated from the Eq. 3 as:

$$\left\langle U_G \right\rangle_{99-00} = A\left\langle U_{SLA} \right\rangle_{99-00} + B$$

where A and B derived from a combination between drifter velocities and satellite SLA over the period 1999-2009. Actually U_{DG} from Eq. 2 (without the error term) can be used at any time to get the best estimate of surface geostrophic velocity, from which unbiased statistics can be calculated.

Page 1509 line 26. “less” implies a comparison – with what current estimate in this case?

We have substituted with the following sentence:
The pseudo-Eulerian statistics computed with these geostrophic currents are defined as 'unbiased' because they are less biased with respect to those calculated directly with the drifter data (Menna et al., 2012; Poulain et al., 2012).

Page 1510 equation (6) needs (. .) around the terms after the factor 1/2.
Ok, done.

Page 1511 line 5. Please define BE, SE at first use (here?) – Batumi Eddy, Sebastopol Eddy?
Ok, done.

Page 1512 lines 5-6. If speeds are less than 10 cm/s then the MKE should be less than 50 cm2/s2.
Ok, done.

Page 1513 line 5 end. Either "can move" or "moves".
Ok, done.

Page 1514 lines 6-7 and 9-11. These periods of more intense activity should be stated all together; they need some rationalising as there seem to be two thresholds for "more intense" and overlapping times when they occur.
Ok, done.

Several figures, especially figure 5, would benefit from larger lettering for any printed copy.
Ok, done.