

Interactive comment on “Joint analysis of coastal altimetry and HF radar data: observability of seasonal and mesoscale ocean dynamics in the Bay of Biscay” by Ivan Manso-Narvarte et al.

Anonymous Referee #2

Received and published: 30 April 2018

the paper focuses on the validation of two different altimetry products and aims at demonstrating the complementary with high-frequency radar observations of surface currents in the bay of Biscay. the manuscript has a few flaws, that should be accounted for properly before it is accepted for publication. details are given below for the Authors and the Editorial Board.

Abstract: - define "surface currents"; - correct the spatial and temporal resolution: depending on the HFR system, they can be as low as 300m to 12km, 5 min to 3 hours; same corrections apply within the text - line 20: what variability is the Author referring to? why is it so? it would be useful to have these details in the abstract - line 25: is this

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correlation increase statistically significant? is there a real benefit in including a simplified Ekman current model to the data, given the amount of processing the dataset already go through? - check grammar and break sentences to improve readability (mainly within the manuscript) - lines 20-25: I personally would rephrase this sentence in order to focus on benefits first and limitations after; for instance, something like: "Both HFR and altimetry capture the main oceanographic features in the region (the IPC and the mesoscale eddies), however performances reduce in the areas closer to shore because of", or similar.

Introduction: - page2, line4: references to Jerico and Jerico-Next should be added; - page2, line6: cit: "...best possible quality indicators..." of what? - page2, line19: definition of "HF" is missing; the guess is, it means High-Frequency spectral components, but it confuses with the acronym HFR; - page2, line20: HFR do not measure "inertial waves" but can resolve "inertial currents" if the proper grid resolution is set up; - page3, line 12: missing network after HFR - page3, line 15-18: one of the major motivations of this present work - that is, the comparison of the two products - is not stressed out properly in my opinion; this is actually te added value to this manuscript.

Section 2.1.1: This section should be rephrased and detailed more because as it is now it contains a series of significant errors. - radial velocities are not measured directly; they are derived from the inversion of the 1st order scatter from Bragg-matching waves; - operational range is usually frequency and bandwidth dependent; low-frequency systems have usually narrower bandwidths thus boosting range but with inversely-proportional range resolution - 40KHz bandwidth should provide a radial range resolution of ~ 3.7 km, not 5km as stated; - "noise to signal ratio" should be the opposite: signal to noise ratio; - is it the RT HFR product being used, or the reprocessed DM data set used instead? - "receipt antenna pattern" should be "receive antenna pattern"; - bi-annual calibration: performed every two years, or twice per year? - page 4, lines 5-13: this section is confusing. Gurgel (1994) and Lipa and Barrick (1983) proposed the unweighted least-square fit for the WERA and the Codar systems. the

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first does a 1-1 match of radials from two stations, the second uses a spatial search radius. The OMA analysis has nothing to do with this. OMA was developed by Kaplan and Lekien (2007). My understanding is the following: radials in polar coordinate systems from the two separate stations were mapped to currents on a cartesian grid using the HFR_Progs Matlab package; then, an OMA analysis was performed for gap-filling purposes. Since the results of the conventional least-squares approach were similar to the OMA output, it was decided to use the OMA products for the following analysis. If that is the case, there is at least one motivation for me to ask 1, if there is any quantitative comparison between the OMA and the LS fit with any other data set (see for instance Cosoli et al., 2015, who tested the EOF interpolation versus the conventional LS fit in the Malta Channel); 2, to have at least a map of the comparison metrics between the OMA - LS fit products. The reason being: OMA is fitting a limited number of modes which will inevitably lose some observed structures, and most likely adds some spurious structure that needs to be accounted for properly.

Section 2.1.2 This section also should be checked thoroughly and detailed more. - page4, lines28-32: details should be provided about this data processing approach, especially in relation to the spatial filtering approach. References should be added to the Loess filter because it needs to be understood properly in order to avoid biases at ranges from the coastline within the filter spatial cut-off length. if the filtered products are used to derive the along-track geostrophic currents, I would expect a systematic bias between HFR and satellite in the coastal regions; this would explain for instance the biases documented in Figure 5 for the first 4-5 bins; also, it would most likely explain why correlation is maximised between km 40 and 50 (3rd panels, figure 5 a and b) - page5, lines1:5: same considerations as above apply to this dataset

Section 2.2 - page5, lines17-24: while the moving-average filter is probably fine in removing the low-frequency components from the HFR data set, it would be useful to have also some quantitative results of the sensitivity study about the 2, 5, 10, 15 d windows. How was the phase shift introduced by the MA process handled, for instance?

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Given the spatial smoothing the altimetry data goes through, I believe a similar thing should be done for the HFR data set, so that to avoid any processing bias. - page 6: I there is something wrong with eqtn. 3; this applies to a standard orthogonal Cartesian x-y plane with x axis pointing eastwards, y axis pointing northwards and z axis pointing to the opposite direction of gravity; not clear in the text if the geostrophic velocities are computed in this coordinate system. assuming it is so, however, the derivative should be computed along y if one wants the across-track velocity, not x: $u = -g/f \cdot DSLA/Dy$; -page 6, lines 18-31: more details are needed in regard to this. I assume that the comparison is performed after projecting the geostrophic currents in the direction of the radar stations, so to have a "true" comparison between the radial currents. That would be fine if the radars was error-free, which is not the case. Usually, the direction-finding radars suffer from systematic and unpredictable errors in the determination of the incoming signal, which results in statistically significant bearing offsets (see Emery et al., 2004, for additional details). I think this analysis should be extended to a few more angular sectors or the potential limitations properly acknowledged in the text - page 7, eqtn. 5: the bulk-flux formula described here has no references-it should be added; is the stress computed at the standard 10-m height? what formulation is used for the drag coefficient? is it wind speed dependent or independent? - page 8, lines 7-8: HF again I suspect stays for "high-frequency"; so, the Ekman currents are computed then low-pass filtered with the same 10-d moving average filter. same considerations as before apply also to this product - a spatial filtering should also be applied

Section 3 - I would like to see the actual 95%-99% CL to correlation and statistics; are changes in correlation statistically significant? based on Table 1, the high standard deviations do compensate for any changes in mean values, and as such I would be cautious in interpreting similar variations - it is stated that in general adding the Ekman currents decreases rmsd but adds variability; it would be interesting to see a plot of these terms and try understand if the added variability reflects in the intrinsic variability of the Ekman term

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- Figure 5 needs some additional analysis and comments: interestingly, HFR-altimetry correlation is maximised at around 40-60 km which is comparable to the size of the altimetry spatial filtering window; the HFR dataset shows an inversion at the edge of the grey-marked area (which corresponds to the 1000m isobath); but neither the CMEMS or the CTOH products follow that pattern. why is it so? what are the sources of a similar disagreement?

- While in general there is an agreement between the mesoscale patterns (Figure 8 for instance), comparison is poor in the region close to shoreline where the altimetry products are often in opposite direction to the HFR data. In this sense, it would be interesting: 1, to investigate a bit further the assumptions of geostrophic balance in the boundary regions; 2, try to merge the altimetry and HFR data so to correct and in this way maximise the two products

Interactive comment on Ocean Sci. Discuss., <https://doi.org/10.5194/os-2018-33>, 2018.

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