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Ocean Science
Discussions

Interactive comment on "Variability in the air–sea interaction patterns and time-scales within the Southeastern Bay of Biscay, as observed by HF radar data" *by* A. Fontán et al.

Anonymous Referee #2

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This manuscript describes the comparison between large-scale reanalysis wind products and low-frequency HFR surface currents off the Basque coast of Spain. The timeseries of winds and currents are analyzed using EOFs and correlations to develop a model of the correlated wind-current variability at time scales longer than 20 hrs. The spatial effects of the sea breeze are also investigated. While this new dataset of HFR currents appears to have much potential - its already been utilized in a number of papers on inertial and seasonal variability - I found that this work did not offer as strong a contribution to increasing our understanding of circulation in the area, or new methods for predictive tools as it could have, and should have to be considered for publication.

(Additional new information) Unfortunately, only after completing my review of the original draft did I noticed the reviewer #1 comments and reply by the author. While, much of the comments i've written below, echo what was said by reviewer #1, I believe they go a step further in the questions regarding the processing steps and the limited analysis completed. Despite reviewer #1's requests, the author's reply provided only an incremental addition to the draft, particularly to the veering angle of the Ekman currents.

It is my believe that significant further investigation is still necessary, and not beyond the scope of the present work. For this reason and the comments given below, I suggest that the manuscript, in its current form, be rejected. I would encourage the authors to proceed with the additional work suggested by myself, reviewer #1, and mentioned by them in the manuscript text and their reply to reviewer #1, and resubmit an expanded, more thorough analysis of the modes of variability present and their implications for circulation and transport over the area in question.

Major comments:

The manuscript requires additional editing for English grammar as there are numerous typos, missing words, and unclear phrases.

A great deal of time is spent describing the methods used to pick the statistically significant EOFs, when (1) simpler methods exist (see below) and (2) the result seems to be that only a few modes are viable. How different is the full result from the North et al or Overland and Preisendorfer short cuts? Does it justify the additional $\sim$page of methodological description?

What type of smoothing is applied to the reanalysis winds? What is the minimum length scale expected? Even if the wind grid points are completely independent (which they are not), a maximum of six grid points exist across the HFR domain. Is the wind to HFR comparisons significantly different than comparing the HFR to a single timeseries of wind (A spatial mean from the reanalysis, or a nearby buoy perhaps)?

Filtering: The text describes that a filter is applied separately in each continuous time segment of radar data? How long are the gaps in question? If not using a forward/backward filter method - not stated - what does this do to the ends of each segment? If short gaps (<20 hrs) could be interpolated before filtering, the resulting timeseries might be more robust for the lower frequencies in question without any significant increase in error. This might ease the issues with spectral calculations as well as the EOF.

p2799:L1-15 Back to the significance testing, I believe you could be a bit more careful about the length/time scales of the data set. How does the cross-validation method handle the temporally-filtered datasets, which have autocorrelation scales of tens of hours? You've made some effort to exclude the 'nearby' (please define, relative to the averaging radius, etc.) locations 'since the data are autocorrelated', but this is true in time as well.

However, I think you are just trying to establish which EOF modes are statistically significant. Since it is the local results you are interested in, why not just test if the local percent variance explained at a given location is statistically significant. The fraction of variance explained by a given mode at a given location is simply the square of the cross correlation between the raw time series at that location and the amplitude time series of the particular EOF. Standard methods to compute the effective degrees of freedom and appropriate level of significance would then show which modes are 'significant' in which locations and allow you to exclude global modes that do not explain enough 'local variance' to be of interest.

The existence of diurnal surface currents driven by seabreezes are a major result of the paper, but it is unclear what the HFR-based results add to the existing literature on the subject. Perhaps if the spatial extent of the diurnal signal within the HFR domain could be examined in more detail. For example, S1 wind amps are strong all along the 200m isobath in fig3, but the surface current amps do not follow this pattern along the spain coast. What drives this difference?

p2803:L10-20: You've stated above that the wind and current eofs/CCAs of both mode one and two are linked, yet are stating that a portion of the mode two eddy must not be related (either directly or indirectly) to the winds. As this type of a feature has significant implications for circulation and exchange, I believe that understanding it's dynamics should be within the scope of this paper.

p2805: L10-20: You conclude that the EOF/CCA methods, as shown here, should be useful as a predictive tool for spills, SAR, etc.. However, the work described focused on timescales longer than 20 hours, ignoring the larger fraction of the total variance that exists at time scales less than 20 hours (your figure 2a). As written, the predictive ability would be limited to a small portion of the total variance. How would the analysis look if the full variance was included? Many of the papers cited as examples of predictive models conclude that these models depart rapidly from 'truth' within a matter of hours, rendering the long-term (10s of hours to days) predictions useless.

Minor comments:

Isn't it expected that diurnal winds are stronger than annual winds?

p2798:L17 Please give more detail why weighting the data values by latitude is necessary or provide an appropriate citation.

P2801:L20: I don't understand what you mean by 'withholding the serially correlated time interval'?

Fig4: I don't find this very helpful. Based on the information in the tables, it is not obvious to me why the shaded box was the one chosen by the method. Is there additional criteria that is not in the 'figure'?

p2802: L11-13: The CCA model show enhanced... I'm not following this point.

p2802: L20-end: The 'Ekman veering' of surface currents relative to the wind varies significantly across the domain. Can the authors discuss this at all?