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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.

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The authors sincerely appreciate the time that the reviewer has devoted to the revision of the manuscript, and we would also like to thank the referee for some of his/her constructive comments provided. However, we firmly disagree with some of the suggestions provided, e.g. EOF truncation and our treatment of temporal autocorrelation. In the next lines, we make a point-by-point reply to the referee's comments.

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

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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.

Reply: up to date, this new dataset of HF radar currents has only been used in a single paper about inertial currents: Rubio et al. (2011). With regard to the methodology, the Barnett and Preisendorfer (1987) approach to CCA is a common method in the atmospheric literature but not so common in the oceanic literature. To our knowledge, only two papers apply this approach in neighbouring areas, but not in the Bay of Biscay: Sánchez et al. (2007) and Herrera et al. (2008). Sanchez et al. (2007) performed EOF-CCA approach to study the coupling between ocean winds and SST off the western Iberian Peninsula. Herrera et al. (2008) applied the same approach to study the relationship between local (upwelling index and river run-off) and remote (oceanic meridional temperature gradient) forcings, and high-resolution CTD profiles and meridional residual currents at western Iberian Peninsula. None of them are focused on wind-induced currents. Additionally, these studies do not cover the southeastern Bay of Biscay. Moreover, this study significantly extents previous studies about water circulation over the SE Bay of Biscay: Ibáñez (1979), González et al. (2004), Fontán et al. (2006), Fontán et al. (2009), Rubio et al. (2011), Fontán et al. (2012) and Rubio et al. (2012). Most of them are focused on fixed locations, excluding Rubio et al. (2011), which is the only one that uses the large spatial coverage HF radar measurements in the area. Above all, this contribution justifies that diurnal wind-current coupling is probably affecting wider areas than that covered by the HF radar system. We have rewritten our manuscript accordingly.

(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 reviewer1, 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.

Reply: we do not agree with the reviewer's viewpoint. In the following, we have again tried to clarify all the suggestions or comments made by the reviewer.

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, reviewer1, and mentioned by them in the manuscript text and their reply to reviewer1, 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.

Reply: we do not share the previous two comments of the reviewer. These do not provide any constructive course for action; therefore, we cannot discuss them in detail.

Major comments:

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

Reply: thank you very much for this suggestion; we will get the manuscript proofread by a native English speaker.

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 page of methodological description?

Reply: we would like to make a lengthy comment at this point. We already used in

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our previous version of the paper one of the references the reviewer proposed (North et al., 1982). We will rewrite the text in order to clarify what we did. We think that the methodology used in our manuscript, in order to estimate errors in the EOF-CCA model, must be defended since it is much more advanced than the one proposed by the reviewer. To put it simply, we performed an evaluation of the statistical significance in three major phases: (a) Evaluation of the sampling errors in the EOFs. To this end, we used one of the techniques proposed by the reviewer (North et al. (1982); see page 2798, lines 26-27 and page 2801, lines 3-4). Additionally, we also performed a Monte Carlo test on the spatial structure of the EOFs. (b) The choice of the EOF truncation is just one step to build a CCA model. We selected the optimum number of EOFs that yield the CCA model predictive ability by means of a Monte Carlo method and a cross-validation analysis that considers temporal autocorrelation. EOFs can be unimportant from the CCA point of view even if they are non-degenerated. We checked for this too. (c) The significance of the canonical correlations is tested at the 95% confidence level, by means of a Monte Carlo test that checks the significance against a null hypothesis that considers the temporal autocorrelation of the series.

We would like to discuss in detail the question raised by the reviewer about the truncation rules mentioned by the reviewer: (1) Overland and Preisendorfer (1982): It is a Monte Carlo technique for selection of principal components for which the geophysical signal is greater than the level of noise. The level of noise is simulated by repeated sampling of principal components computed from a spatially and temporally uncorrelated random process. The 95th percentile is used for the selection of eigenvalues. The test proposed by us (Monte Carlo + cross-validation) is more powerful since it not only checks the results from the EOF analysis but it also tests the error by the CCA model by means of the cross-validation approach. Additionally, it guaranties that the error estimation is robust without any assumption of gaussianity. (2) North et al. (1982): the objective of this paper is basically provided in the title: "Sampling errors in the Estimation of Empirical Orthogonal Functions". We quote the authors: An obvious difficulty arises in physically interpreting an EOF if it is not even well-defined intrinsically. This

can happen for instance if two or more EOFs have the same eigenvalue. It is easily demonstrated that any linear combination of the members of the degenerate multiplet is also an EOF with the same eigenvalue. The authors of the paper continue: This ambiguity for degenerate multiplets is the base for understanding the sampling theory in this paper. We continue quoting the original authors: Any estimate of the EOFs from a finite number of realisations will be subject to sampling errors. It is important to establish some estimate of the sampling errors involved before interpreting sample EOFs. ... From this error estimate, we derive a rule of thumb determining whether a sample EOF is expected to be a faithful representation of the true EOF. The rule of thumb is explained by authors as follows: In other words if the sampling error in the eigenvalue is comparable to the distance to a nearby eigenvalue, then the sampling error in the EOF will be comparable to the nearby EOF. These estimations of sampling error are very important and this is the reason that explains that this method is often used as a rule of thumb, particularly when the EOFs have to be physically interpreted. However, there are several reasons that justify that we did not use this method alone, because we have sound reasons not to rely on this method alone (we used it but we also used additional methods). See Figure A. Degeneracy according to North et al. (1982) rule of thumb for current (top) and wind (bottom) EOFs.

First of all, we are not interested in interpreting EOFs individually. We are only interested in using the EOFs as a data compression tool (a very efficient one). Then, the EOFs are used to build the CCA model. As this is very common in literature (Barnett and Preisendorfer, 1987), we decided to use this technique. From the point of view of data compression, the degeneracy of EOFs is not a serious problem. Even if some effective degeneracy appears, the retention of a whole multiplet solves this problem, as we have quoted from the original authors above. That is, the fraction of variance associated with that multiplet is still expanded by the subspace spanned by the degenerate multiplet (North et al., 1982). We explicitly consider this option in the EOF-based CCA model. Thus, if two EOFs form an effectively degenerated multiplet, they must be selected in order to decrease the cross-validation error. In contrast, if too much

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truncation is performed in the EOF phase before performing CCA, important information could be discarded (page 519, Wilks, 2006). Therefore, we are confident that our method based on a Monte-Carlo generation of subsamples and error estimation yields better and much more robust estimations of the overall algorithm errors from all the phases (not only the initial EOF-based data compression phase but also the CCA phase).

We agree with the reviewer that the North's rule of thumb is often used. We did indeed use it as a pre-screening technique (lines 26-27 in page 2798 and line 5-8 in page 2801). However, due to the fact that the apparent sample size is very high, this technique yields results that are too liberal. The first EOF showing degeneracy is 16th for the wind and 18th for the current (at the D1 spatial domain). Consequently, we finally relied in the use of Monte Carlo + cross-validation. The EOFs retained by our estimation (6 for the currents and 2 for the winds) are not degenerated as can be seen in Figure A (not to be included in the final version of the manuscript, unless required). In addition, we have performed a schematic illustration of the methods discussed above: North's rule of thumb (North et al., 1982), Overland and Preisendorfer (1986) and Monte Carlo + cross-validation (See Figure B. Comparison of the steps in the estimation of errors according to the suggestions by the reviewer (left) and our strategy (right). Not to be included in the paper unless requested).

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)?

Reply: we will clarify this issue in the revised version of the manuscript. On the one hand, we do not apply any spatial smoothing to the reanalysis winds. The Barnett-Preisendorfer approach to CCA consists of initially computing the EOFs in the original fields (Barnett and Preisendorfer, 1987; Wilks, 2006). So, we applied EOF analysis to

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all the wind grid points, and as stated in our manuscript (from page 2801, line 28 to page 2801, line 2), we selected only two EOFs to represent the truncated wind field anomalies. This means that we are actually using two degrees of freedom to represent the wind field and not all the grid points in the reanalysis. This is the basis of the Barnett-Preisendorfer approach to CCA. On the other hand, the reviewer said that a maximum of six grid points exist across the HFR domain, but there are at least sixteen reanalysis points (see Figure 1 in the original submission). In any case, we think that the effect of both, local and remote winds, on subtidal currents has to be considered and tested. For this purpose, we performed our analysis on two domains (D1 and D2 spatial domains) and we obtained better results within the smallest domain (D1). Finally, the reviewer suggested performing the analysis with a spatial average of the reanalysis winds or a single buoy. In this particular case, the covariance matrix cannot be of a rank higher than 1 and consequently, we would only be able to get a single CCA pattern and, thus, we would inevitably miss the second CCA pattern. The second CCA pattern is also an important result of our study as stated by the reviewer him/herself in the first paragraph of page C1324: "This type of feature has significant implications for circulation and exchange; I believe that understanding its dynamics should be within the scope of this paper". We disagree with the suggestion of averaging the reanalysis winds, because we think that this would lead to less robust and seriously truncated results.

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.

Reply: we agree that we have to clarify this issue in the text. The method consists

of a digital filtering using a 137-hour weighted and centred moving average (see filter weights in Figure C, not to be included in the revised version unless requested). This filter is specially designed to suppress unwanted components, semidiurnal and inertial periods in our case (see Thomson (1983), for clarification). The weights (w) satisfy the condition w0+sum(2*wk)=1, where k=1,..,N. In each 137-hour segment, the filter is applied if and only if abs(1-(w0+2*sum(wk)))<0.4 as other authors suggested for gappy data (Pugh, 1987). This is a condition that is easily accomplished if the neighbouring data are present as the weights are higher for the nearest values (see Figure C). Therefore, this means that short gaps are interpolated as the reviewer suggested. In fact, we lost 2% of the total data, at the most, by applying the filter. As can be seen in Figure 2, the digital filter permits to eliminate the unwanted frequencies efficiently and without numerical complexity. On the other hand, both, spectral calculations (Lomb, 1976; Scargle, 1982) and EOF analysis, deal with gappy data. In fact, one of the advantages of the proposed approach as a predictive tool is that it can be used with unevenly sampled data without any requirement of reconstruction of the HF radar data. We will adequately rewrite this issue in the revised version accordingly.

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.

Reply: we think that the paragraph wording is not very clear and this has probably misled the reviewer. The cross-validation approach is performed on the time domain and not on the spatial domain. We will rewrite this paragraph in the revised version of the manuscript in order to clarify this issue. The sentence "the neighbouring values are also excluded since the data are autocorrelated" means that the neighbouring values in the temporal domain or the neighbouring time steps are also eliminated since the

data are temporally autocorrelated or serially correlated. Briefly stated now, firstly, we calculated the temporal time-scale of autocorretlation of wind and HF radar currents. The cross-validation approach consists of eliminating some temporal observations and testing a model (EOF-CCA model or any other since is a nonparametric procedure) on the skipped time observations. That is, every time step (ti), a time interval of length 2*tau+1 [ti-tau ti+tau] (where tau is the decorrelation time-scale) is omitted to build the model. Finally, the model is tested on excluded observations, that is, on the time interval [ti-tau ti+tau]. In this procedure, we have considered the autocorrelation time-scale as not to distort our cross-validation error estimations. Therefore, the suggestion made by the referee, about the autocorrelation in the time domain, was already considered in the first version of the manuscript. However, we will rewrite the paragraph to explain it clearly in the revised version of the paper.

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.

Reply: we have used the EOF analysis as a method for compressing the information; that is, we use only 6 (currents) and 2 (winds) uncorrelated variables (principal components) to build a CCA regression model, instead of using all the grid points. The 6 and 2 uncorrelated variables represent as much as the 87% and 86% of the current and wind variability. We are not interested in the local structure of any EOF. We just take advantage of the principal components' ability to create the best possible CCA model by testing it with the cross-validation method (Feddersen, 2003; Feddersen and

Andersen, 2005; Xoplaki et al., 2003; among others). Therefore, we think that this comment refers to the spatial structure of the EOFs and we have not shown, not even mentioned, anything about the spatial structure of the EOFs in our manuscript. Perhaps if the reviewer could tell us what he/she meant we would be able to understand his/her suggestion.

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?

Reply: thank you very much for this suggestion; we will clarify this issue in the revised version of the manuscript. As stated by the authors (page 2799, lines 18-19), it is well known that the land and sea breezes are one of the most prominent mesoscale phenomenon in coastal locations around the world (Kumar et al., 1986). Pielke (1981) provided a nice review of the physical interactions between land and sea breezes and coastal waters. Simpson (1994) also performed an overview of this mesoscale feature, which has implications for coastal physical oceanography, coastal meteorology, and atmospheric pollution. However, to our knowledge there is no research undertaken about land-sea breeze interaction on offshore waters (continental shelf and slope) in the Bay of Biscay and particularly in the SE Bay of Biscay. Consequently, our contribution provides new insights: (1) into the offshore water response to the land and sea breezes in the SE Bay, and (2) for future research about the potential effect of the breezes in the neighbouring areas (northern Iberian and Armorican shelves). With regard to the difference between diurnal wind and current amplitudes along 200m isobaths in the northern coast of Spain, we can say that diurnal wind amplitudes are obtained for the period 1979-2010, whereas diurnal current amplitudes correspond to the period 2009-2010. Therefore, the diurnal wind and current amplitudes are not comparable. We

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recognise that this may be confusing for the reader and we will clarify this issue in the next version of the manuscript.

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 its dynamics should be within the scope of this paper.

Reply: we did not mean that the second mode is not related to the winds. We said that this mode appears to be wind-driven. Additionally, we stated that we checked two additional CCA models (coastal waters and deep waters separately) in order to be absolutely sure that we could reject the hypothesis that the result was an artifact. We obtained and explained accordingly (page 2803, lines 15-21) that it was not an artifact due to the methodology. Additionally, Pingree and Le Cann (1989) studied the wind driven residual currents by means of the response of a barotropic model forced by steady winds from a set of varying directions. This set of wind forcings does not cover all the available wind directions. In particular, they did not use the NE winds, such as the ones showed in our Figure 5b. However, considering that CCA searches for linear patterns, we can compare the reverse phase of our Figure 5b (corresponding to negative phases of the second mode expansion coefficient) with Figure 4 in Pingree and Le Cann (1989). In this case, they forced their model with a SW wind and they get a cyclonic response over the area covered by our study (their Figure 4). This result is fully consistent with our results in Figure 5b and gives a physical support to the results obtained by means of the statistical analysis of the observed data. These authors (Pingree and LeCann, 1989) found that the response of the shelf waters to the forcing by a steady wind is very fast, they obtained a steady response of currents within 4 days. This result is also in agreement with our results. An estimation (Mudelsee, 2002) of the average lifetime of our second canonical correlation coefficient yields an average lifetime of 2.1 days, so that it can be expected that the surface currents respond to the

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forcing within this time scale based on previous results of Pingree and Le Cann (1989). We will add this paragraph to the discussion of the results in the revised version of the manuscript.

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.

Reply: Perhaps we did not make it clear enough in the text of our first version of the manuscript. We already mention (page 2798, line 5) that we have removed the effect of inertial and semidiurnal tidal currents, since they cannot be predicted with an EOF-CCA linear model using the wind field as a predictor. It is a general rule in physics that different models must be used to predict different phenomena. Our intention is to use the CCA-based model as a brick in a more complex system; the CCA model would only predict the response to the wind forcing. Afterwards, the prediction of tidal and inertial components would be added to derive the total currents. Obviously, the CCA-EOF model is just a part of a more complex system. We will clarify this issue within the next version of the manuscript.

Minor comments:

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

Reply: we did not expect that. In any case, the answer is in Fig. 3c: the diurnal component exceeds the annual one when the ratio S1/Sa is above 1, and this only happens in a portion of the French shelf and of the Cantabrian shelf and slope. In fact, most of the domain shows ratios below 1; consequently, the annual component is

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stronger that the diurnal one in a large part of the area.

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

Reply: this is stated in North et al. (1982), page 701, equations 11 to 14. The spatial domain D1 is quite small and this correction has practically no effect; it is more important for D2 spatial domain. We will include the reference in the next version of the manuscript.

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

Reply: We mean that in order to avoid the role of the temporal autocorrelation (or serial correlation) in the error estimates from the combined EOF-CCA model, in each time step, we eliminate or remove not only one time step but also a block of 2*tau+1 time steps (where tau is the temporal decorrelation scale) (von Storch and Zwiers, 2003). Note that this has to do with your comment above, in page C1323 (second paragraph). Please, see our extended response there.

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 an additional criterion that is not in the 'figure'?

Reply: we agree with the reviewer that this may not be clearly written in the manuscript and it is difficult to understand unless the reader is used to the technique. We will rewrite the paragraph in order to explain Figure 4 more clearly. A cross-validation test is used to select the number of elements (EOFs in this particular case) of the model. The general rule is that EOFs are added until over-fitting appears. This means that the addition of a new element results in a worse model (based on skill scores) for predicting the part of the sample that was not used in the formulation of the CCA model. In our particular case, we have selected a model with 2 EOFs for wind and 6 EOFs

for currents (see Figure 4). The skill scores are R(2,6)=0.76, BIAS(2,6)=-0.04 and RMSE(2,6)=0.66. If we select 3 EOFs for the wind, the skill scores are: R(3,6)=0.65, BIAS(3,6)=-0.03 and RMSE(3,6)=0.76. Overall, the CCA model gets worse skill scores by including an additional EOF for the wind. By including the 7th EOF for currents, the skill scores for the model are: R(2,7)=0.76, BIAS(2,7)=-0.04 and RMSE(2,7)=0.66. This means that the addition of a new EOF does not lead to a better CCA model. Therefore, the simplest model (according to Occam's razor) with the best skill scores is obtained by using 2 and 6 EOFs for the winds and currents, respectively.

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

Reply: we mean that the results are better for the D1 domain than for the D2 domain. See page 2799, lines 12-13 of our manuscript: "Finally, the sensitivity of the results to the spatial scales selected for the winds was also tested by repeating the analysis for two domains (D1 and D2 in Fig. 1)". We obtain better results for the smallest domain rather than for the largest one. We will rephrase this sentence in order to clarify 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?

Reply: we have already answered this point in the reply to reviewer1. We will expand the discussion of this result in terms of other results about this topic such as Kirincich et al. (2005).

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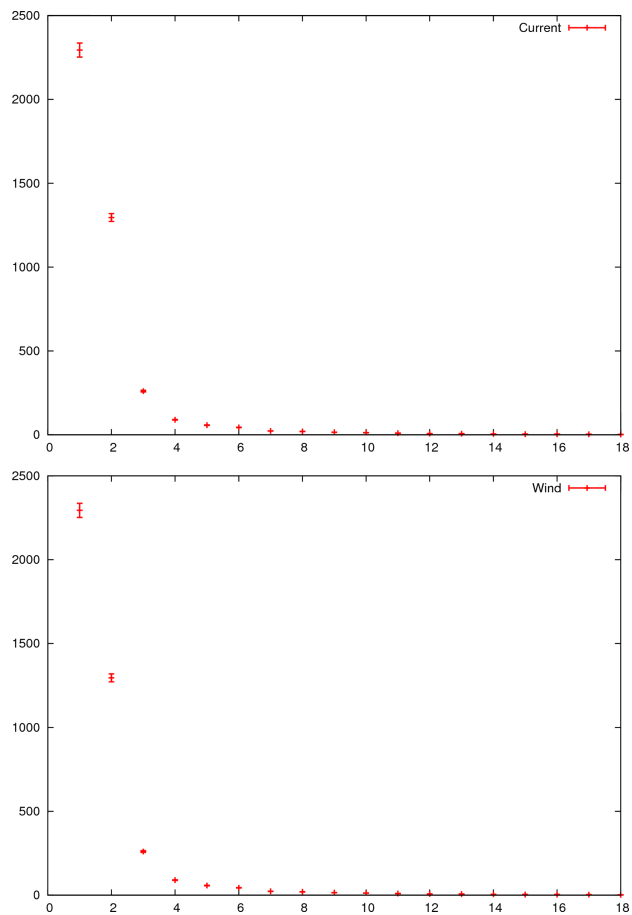
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Fig. 1. Figure A. Degeneracy according to North et al. (1982) rule of thumb for current (top) and wind (bottom) EOFs.
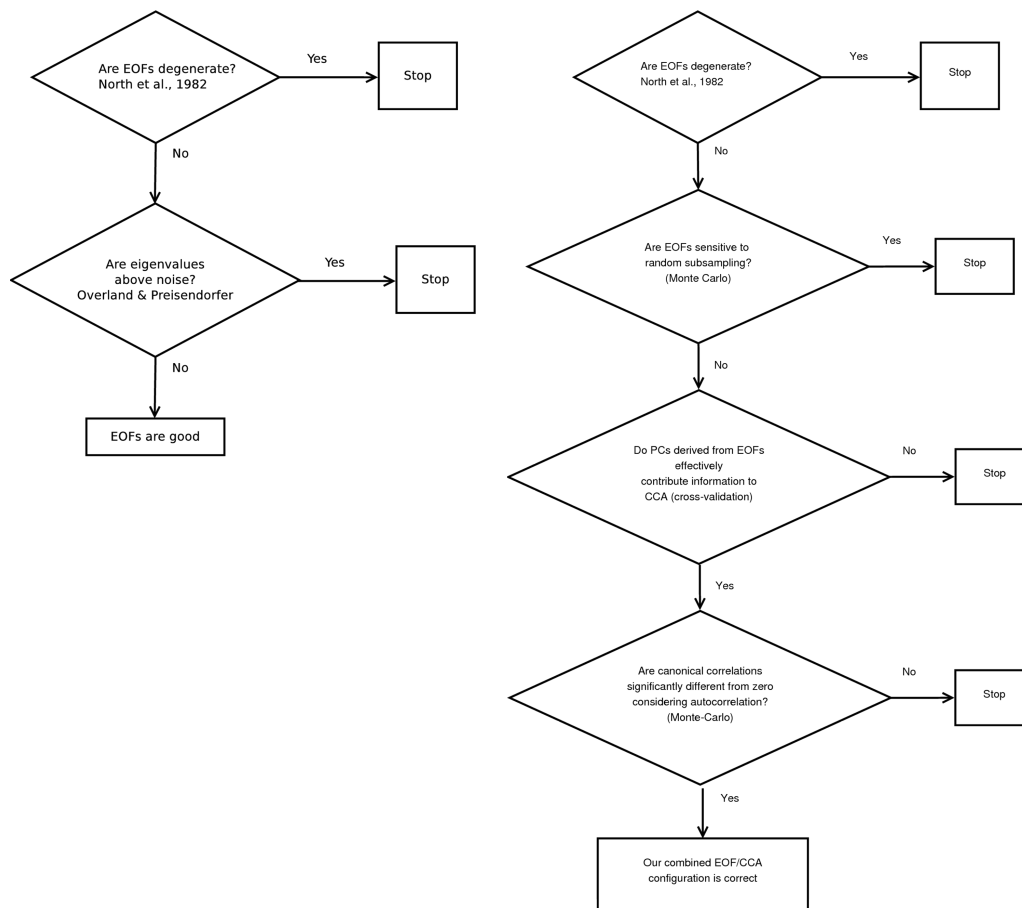
Fig. 2. Figure B. Comparison of the steps in the estimation of errors according to the suggestions by the reviewer (left) and our strategy (right).

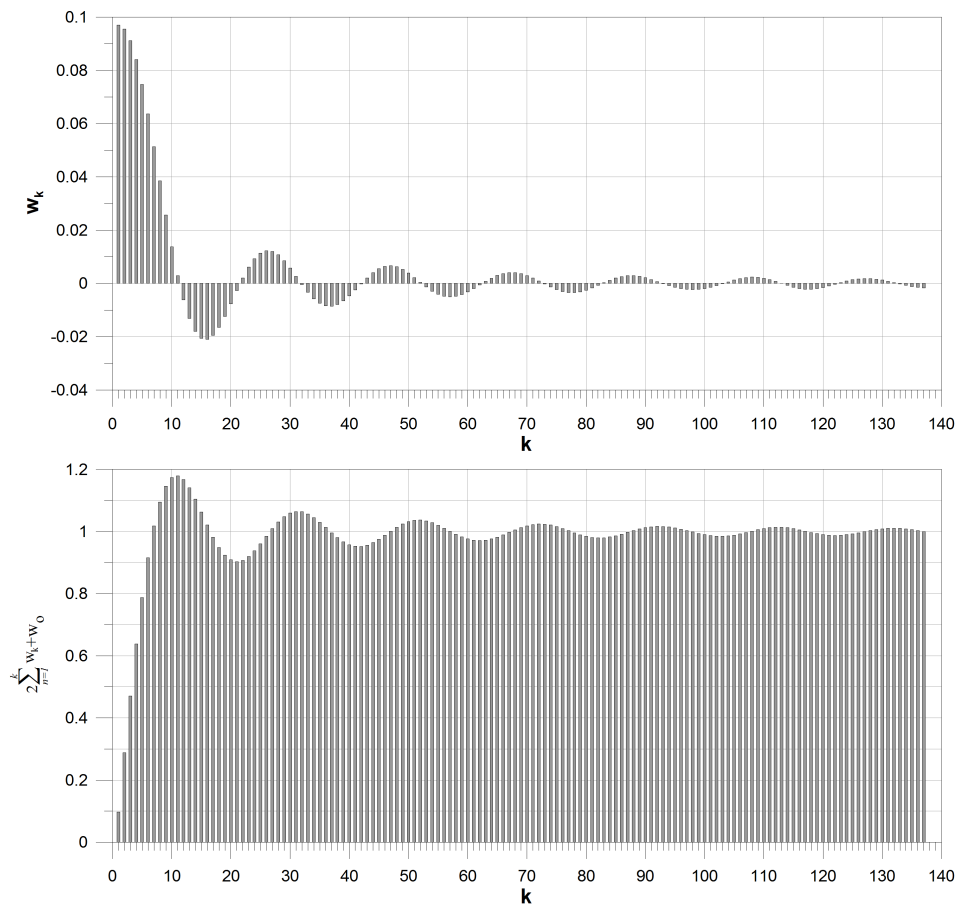**Fig. 3.** Figure C. Filter weights.