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Ocean Science Discussions

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Interactive Comment

Interactive comment on "Temporal and spatial characteristics of sea surface heightvariability in the North Atlantic Ocean" *by* D. Cromwell

D. Cromwell

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I thank the anonymous referees and Professor Robin Pingree for their insightful and thoughtful comments. As many of the important points made by them overlap, it would probably be most helpful and efficient to take them together. The following, then, is my combined response:

STUDY MOTIVATION AND OBJECTIVES

The paper is an observation-based study whose primary purpose is to give a detailed description of the spatio-temporal variations in sea surface height variability in the North Atlantic Ocean. The aim is to present state-of-the-art satellite observations of this dynamic and important region and to make at least an initial interpretation. Fully addressing possible physical mechanisms to explain the observations would require

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considerable discussion of process modelling and/or analysis of output of ocean - or, more likely, coupled atmosphere-ocean - general circulation models. This would represent a huge undertaking.

However, I have included significant discussion of possible physical mechanisms by addressing the propagation of baroclinic Rossby waves and/or eddies, and also the possible role of atmospheric forcing as captured by the North Atlantic Oscillation and East Atlantic Pattern climate indices. I note that Prof. Robin Pingree, who has considerable expertise in this region, refers to the study in his interactive comment as "an excellent remote sensing paper" with "new and interesting results" which are "timely and significant" and adds that the paper is "a significant contribution to the remote sensing literature".

Referee 2 commented that more treatment of the higher modes (in particular, 3 and 4) would be expected. I have now expanded the descriptions of modes 1-4 for all three cases (see 'Correlation Analysis' below), with added interpretation drawing on previously published work, and I have provided fuller summaries of these modes in Tables 1-3. Referee 2 remarks in comment Ad 1) (b) that "the added value of this paper over the analysis of Fu (2004) is said to be in the investigation of these higher modes". This is indeed a major factor but it is not the only one as explained in the first paragraph of §3.1. For example, in one of the case studies in my paper I apply filtering that does not remove the propagating baroclinic signals, unlike Fu (2004).

WAVELET ANALYSIS

I have clarified the usage of wavelet analysis. The referees are correct to point out that features occurring below the cone of influence (i.e. longer-period variations) have to be treated with caution. I have added a brief definition of what is meant by 'cone of influence' so that this should now be clearer. I have explained that when the global wavelet spectrum is calculated, many signals, such as 5-year cycles identified in the study, are indeed shown to be significant at the 95 per cent confidence level. (Recall, too, that

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the length of the time series of the data is over 11 years: a 5-year signal will therefore complete two complete cycles and a reasonable identification can thus normally be made.) Likewise, the plots of scale-averaged (roughly equivalent to bandpass-filtered) timeseries show statistically significant variations. Moreover, on occasion the time-evolution of signals with particular periods does appear above the cone of influence, or close to it, so that a reasonable interpretation can be made on that basis. This is particularly so when confirmed by the presence of strong peaks in the global wavelet plot and/or scale-averaged plot. Careful interpretation of a combination of all these plots underpins the mode descriptions in the revised paper. This has been explained in the revised text.

Earlier errors in Tables 1-3 identified by the referees have now been corrected. Indeed, Tables 1-3 have been improved by providing a fuller description of the observed modes, first providing a general description of the pattern and then a clearly delineated description of each signal identified, whether in phase or in amplitude. This also addresses referee 2's concerns that I had not considered the higher modes adequately and that I had made insufficient use of the amplitude time series. As the new cross-correlation results show (see below), the higher modes appear to be climate index-related. Also note, as we point out in the revised paper, a strong signal in amplitude does not necessarily equate to a strong signal in phase and vice versa. Occasionally, however, a periodic signal is pronounced in both phase and amplitude.

Re: 'jumps' in phase. Referee 2 correctly points out that these require care in the interpretation. Note that, as the reviewer indicated, wavelet analysis of a phase timeseries does indeed capture the periodicity of a signal: when a propagating feature moves periodically through 0 to 360 degrees then back to 0 before rising once again to 360 degrees, wavelet analysis tracks this phase evolution with time. There are a very small number of occurrences where a slight change in phase angle causes a spurious jump between 360 to 0 and back to 360. However, care has been taken in the revision to exclude those events from the descriptions provided in the tables and text. The con-

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clusions remain robust.

CORRELATION ANALYSIS

I have redone the correlation analysis of the two climate indices (NAO and EAP) versus the four strongest principal components for each of the three cases studied:

(i) North Atlantic data, lowpass filtered at 18 months; (ii) Azores subtropical frontal region, lowpass filtered at 18 months; (iii) Azores subtropical region, lowpass filtered at 30 days.

Referee 2 rightly queried the issue of statistical significance of the correlations found. To address this, I have now calculated the effective degrees of freedom based on the coherence and autocorrelation of the data, as explained in 'Data Analysis Methods in Physical Oceanography' by William J. Emery and Richard E. Thomson (§3.15, pp. 257-261). I also corrected an earlier error in my code to calculate the cross-correlation coefficients. The results are different from the previous version of the paper: most are now null results, but there are two statistically significant anti-correlations involving the higher modes. I have made some interpretation of this, including reference to previously reported results.

OTHER POINTS

* I agree with the reviewers that the figures as published in Ocean Science Discussion were rather small and awkward for the reader to see clearly. I have rectified this by, for example, plotting two of the spatial modes at a time, rather than compressing four subplots into one plot as in the previous Figure 1. I would also request that Ocean Science reproduce the wavelet plots (e.g. Figure 6) at a slightly larger scale, perhaps by plotting in portrait mode as originally produced.

* Although mention of the MOC is relevant to the introduction, in terms of providing added justification for interest in SSH variability, I agree that its repeated mention at the end of the paper is perhaps extraneous. This has now been removed. However, I

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have added a related point in discussing previous work by Pingree in §3.1.4.

* On re-inspecting the results, it is difficult to justify my comment about signals propagating along the eastern Atlantic boundary (Table 1, mode 1) and this has been removed. The point remains, however, that there is a ridge of high SSH variability here. I have added a short discussion as to why this might be so.

* Referee 2 asked about how the start and end of timeseries are treated. As is standard in wavelet analysis, zero padding to the next higher power of two is performed prior to the wavelet transformation (Torrence and Compo, 1998). I have added a note to that effect in the relevant section (§2.3).

* In §2.1, as requested by Referee 3, I have added a note that the inverse barometer correction was applied to the altimeter data.

* I have explained in the revised text how I estimated the trajectory of propagation in mode 3 (§3.2.2). A similar trajectory was found in mode 4 (though somewhat less prominent: the propagation of baroclinic Rossby waves and/or eddies appears stronger in mode 3 than in mode 4).

* Referee 3 asks whether Rossby waves or pulses of mesoscale eddies are being observed. This is a good question that has been raised in the literature and it remains open. I have added some comments about this in the paper.

* External tick marks have been added to the maps to enable easier reading of the latitude and longitude.

* Figure captions now indicate that the contour interval is regular, optimising the dynamic range between min and max values.

* It was not our intention to compare relative amplitudes of modes in §3.2.2 (30-day lowpass filtered data in the subtropical front), for example, with the other two cases studied (annual cycle suppressed).

* It is not clear to me why referee 3 expects westward propagation at the annual cycle Westward propagation predominantly occurs at subannual periods in the mid-latitudes of the North Atlantic. In any case, a propagating signal would not be "buried" by a non-propagating component at the same period: they would normally be distinguishable. Any travelling feature would appear as a propagating phase signal, observed in the wavelet spectrum of the phase part of the PC time series. A non-propagating signal would not display a peak in the phase wavelet spectrum, though it would be observable in the amplitude wavelet spectrum. Hopefully this is clear in the revised paper.

Once again, I thank the reviewers for their helpful and insightful remarks.

Interactive comment on Ocean Sci. Discuss., 3, 609, 2006.