

Interactive comment on “Temporal and spatial characteristics of sea surface height variability in the North Atlantic Ocean” by D. Cromwell

Anonymous Referee #3

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Review of

Temporal and spatial characteristics of sea surface height variability in the North Atlantic Ocean

By D. Cromwell.

Submitted to Ocean Science.

General Comments

This paper presents careful analyses of sea surface heights as measured by altimeters on several satellites between 1992 and 2004. The analyses show statistical modes of features of the North Atlantic Ocean. Further analysis focuses on the Azores subtropi-

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cal frontal region, finding the annual signal to dominate, with a much smaller contribution from westward propagating Rossby waves.

I prefer to see more interpretation of the spatial patterns noted in the analyses. For example the northward propagation of mode 1 signals along the European and North American coasts is noted, but is not related to other studies or observations to determine the physical significance. It could have been a significant result in this paper, yet these two regions are specifically excluded from more detailed analyses.

The techniques of complex empirical analyses and wavelet analyses are familiar to me but I have not used them myself, so I am not able to determine if they are computed properly.

Reviewers are asked to comment on specific items as listed below:

- 1) Does the paper address relevant scientific questions within the scope of OS? Yes
- 2) Does the paper present novel concepts, ideas, tools, or data? Yes, novel results from application of existing analysis techniques to recent data
- 3) Are substantial conclusions reached? Yes. Westward propagating Rossby wave(s) are observed, but their relative significance seems small.
- 4) Are the scientific methods and assumptions valid and clearly outlined? Yes.
- 5) Are the results sufficient to support the interpretations and conclusions? Yes, but I hoped to see more interpretations
- 6) Is the description of experiments and calculations sufficiently complete and precise to allow their reproduction by fellow scientists (traceability of results)? Yes
- 7) Do the authors give proper credit to related work and clearly indicate their own new/original contribution? Yes
- 8) Does the title clearly reflect the contents of the paper? Yes

- 9) Does the abstract provide a concise and complete summary? Yes.
- 10) Is the overall presentation well structured and clear? Yes
- 11) Is the language fluent and precise? Yes
- 12) Are mathematical formulae, symbols, abbreviations, and units correctly defined and used? Some abbreviations are not defined. Otherwise Yes
- 13) Should any parts of the paper (text, formulae, figures, tables) be clarified, reduced, combined, or eliminated? Most figures are too small relative to the size of their captions. Otherwise, No
- 14) Are the number and quality of references appropriate? Yes
- 15) Is the amount and quality of supplementary material appropriate? N/A

Specific comments

1 Introduction This section describes many processes not discussed later. Even the propagating signals, mentioned in lines 20-21, are investigated later for only one of the three regions in which they are observed. Perhaps, for example, include a comment later in this paper on the application of this paper's findings to the Hakkinen and Rhines (2004) results.

2.1 Data description. A note on the 10- and 354-day sampling intervals of satellites would give perspective on the 7-day interval in the data source. Almost all products from satellite altimetry include correction of inverse barometer effect, so most authors do not mention it. However, in this paper, where comparison to North Atlantic air pressures is made, the text should mention that the inverse barometer correction has been applied to the DUACS dataset.

3.1 Interannual sea surface $\bar{\xi}$. North Atlantic Ocean This section begins with a useful summary of the novel aspects of this paper. However, the propagating signals along the NE and NW coasts are mentioned, but not investigated. Are these signals inves-

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tigated previously in other studies? Are they new to this study? Are they physically meaningful? The northward propagation in the NW seems unusual to me. Some comments here on these two signals would improve the impact of this paper. Figure 1 (and most other figures) is far too small relative to the text in the caption. Figure 1 (and Figures 4 and 7) requires contour labels. If the contour interval is regular, the intervals could be listed in the caption to this table, to preserve clarity in the figure. External latitude and longitude ticks on the unlabelled axes would enable to reader to find latitude and longitude ranges along these axes. The zero crossings of phase in Figure 2 appear to be 6 years apart, yet the period of this signal is given as 5 years in the text. In Figure 3b (and 6b and 8b since they are similar) the description of the cone of confidence is somewhat confusing. Later discussion indicates that “below” implies toward the bottom of the page or plot. However, the period increases toward the bottom of the page or plot. If much of the red zone lies on the unconfident side of this line, this should be noted in the text. The x-axis label for Figure 3a is too close to the title of Figure 3b, and similarly for other panels of these three figures.

3.2.1 Interannual variability: lowpass filtering at 18 months Is it correct to conclude from this section that propagation in space is not convincingly observed in the 20 to 40 N, 50 to 10 W region, when the data are low pass filtered at 18 months? This would seem worthy of mention.

3.2.2 Subannual baroclinic propagation: lowpass filtering at 30 days. This section describes analyses of data that include the annual cycle, which was eliminated prior to the analyses of previous sections. It is therefore difficult to compare the relative amplitudes of these modes in 3.2.2 with previous modes. It might be reasonable to relate modes 3 and 4 to the non-annual cycle modes (subtract mode 1 from the total variance). I would expect the annual cycle of SSH variability to display some westward propagation. This propagating part of the signal might be buried in the non-propagating component at this period, but could still be present. Is this possible? Can the westward propagating annual signal have been missed for this reason? How was the observed

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trajectory of the Rossby waves determined in Figure 7? It seems to be a smoothed line through the peaks in amplitude in this figure. Did these analyses really find Rossby waves, or pulses separated by several years?

Minor points on this paper: Most modern scientific papers use the active voice. Is it time for single authors to themselves as “I” instead of “We” ? If abbreviations or acronyms are to be used in the paper, please introduce them where the long name is first written. (For example, insert MOC in line 8, page 612. Try to not write url addresses spanning two lines.

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