Review of "On modulation instability in a system of jets, waves and eddies off California" by Ivanov et al. (2014)

Zonal jets are persuasive features in rotating planets' atmospheres, but only in recently years have quasi-zonal jets been observed broadly in the Earth's oceans. But how the quasi-zonal jets in the ocean are generated is still the subject of an ongoing discussion. This paper studied the generation mechanism for the observed quasi-zonal jets off Central and Southern California and concluded that the quasi-zonal jets are formed by near-resonance interactions, predominately by quartet (modulation) instability.

The study applies a double spectral approach to seperate temporal and spatial variabilities. The M-modes approach successfully considers the irregularity of the coastlines, and the discrete wavelet transform efficiently seperates six frequency bands. While the method is novel and superior in dealing with spatio-temporal complexity of the observed altimetry, I have some concerns about the linkage between theories and the diagnose. Most of all, it is not clear to me to what extent one can directly apply existing theories in Fourier space to the M-modes in this paper. More discussions on the modulation instability theory and more details about the data processing will significantly improve the manuscript. I would recommend a major revision before publication. My specific comments are listed as follows.

1 Major comments:

1. More discussions are needed in order to better connect the theories with the diagnose in this paper. There is a family of modulation instabilities. A detailed explanation of the modulation instability used in this study will be helpful. Is the modulation instability in the paper only referred to the quartet resonance, such as stated in P110L6?

2. The M-modes approach has advantages in dealing with non-regular domains, but also imposes difficulty in interpreting results. Majority of the existing theories about the modulation instability are derived in Fourier space. The formulation of resonance in an $(M - mode, \omega)$ space is not common and needs clarification. For example, the authors used the mode index of the M-modes as an equivalence of Fourier wavenumber (P106L20). Readers would benefit from a reference or some discussions on the validity.

3. The paper argues that all structures observed off California were generally weakly nonlinear (P106L4). But one may think it is highly nonlinear by looking at any ocean/modelling data with a higher horizontal resolution than the one in the AVISO data. It is questionable how well the gridded AVISO data represent the near-boundary ocean dynamics.

4. The oceanic eastern boundary current off California is a baroclinic system. What is the influence of the baroclinicity on the modulation instability and the interpretation of the SSH signals?

5. Can we define all signals within 4-18 months frequency band as Rossby waves (P102L12-13)? Can you discuss it in terms of the Rossby wave dispersion relation?

6. The method of identifying triads and quartets needs more clarification (P108L9-14).

2 Minor comments:

1. Introduction needs more details. What are the similarities and discrepancies of the quasi-zonal jets defined in this study compared with the jets found in other studies within the same region such as Centurioni et al. (2008), or the jets observed in the open oceans and in the atmospheres on the Earth and other planets? Why is the modulation instability expected to dominate in generating zonal jets in this region? Wang et al. (2013) and Qiu et al. (2013) indentified that quasi-zonal jets can result from triad-resonance. Is the triad-resonance in their studies a special case of the modulation instability? Can you comment on the relevance of your findings with theirs?

   (a) Wang, J., Spall, M. a., Flierl, G. R., & Malanotte-Rizzoli, P. (2013). Nonlinear Radiating Instability of a Barotropic Eastern Boundary Current. Journal of Physical Oceanography, 43(7), 1439–1452. doi:10.1175/JPO-D-12-0174.1

   (b) Qiu, B., Chen, S., & Sasaki, H. (2013). Generation of the North Equatorial Undercurrent Jets by Triad Baroclinic Rossby Wave Interactions. Journal of Physical Oceanography, 43(12), 2682–2698. doi:10.1175/JPO-D-13-099.1

2. The abbreviations in P100L10-12 look odd.

3. Please explicitly calculate the representative frequencies of the six frequency bands. It is also easier remembered and understood if the six frequency bands are referred to as $\omega_1 \cdots \omega_6$.

4. The authors argued that the M-modes with mode index less than 30 are not important. However, the 16th M-mode in Figure 1 looks more interesting to me in a context of quasi-zonal jets. I would like to see more details and discussions on those modes, at least for completeness.

5. The argument regarding the role of flow dissipativity (section 7) is not very convincing. It lacks direct evidence.

6. The label (f) should be (g) in Fig3.

7. The caption of Fig.4 reads "(a) longer than 6 months". Do you mean "longer than 36 months"?

8. In general, the fonts in the figures are too small. The labels for the time-axis, i.e., the x-axis in Fig. 4-9, are not consistent.