All changes are highlighted in red in the revised version of manuscript

RESPONSE #1

On review of "On modulational instability in a system of jets, waves and eddies off California" by L.M. Ivanov et al.

Thank you very much for your revision of our paper. We indicated our response by red line.

1. "Improve overview of the ocean QZJ ……………………………………………………."
    It was done. See ,p1, ln 28; p2. Ln4; p2., ln 24-31, p3., lm 9-10.

2. "Page 3. I would suggest extending of Connaught et al. (2010)'s results……"
    Done. P. 3, ln 14-22.

3. "Provide more details on how you analyze altimeter observations……….."

    It was done. See, Section 2.

4. "More details should be provided in the Abstract"
    We have revised our Abstract and changed the statement of "The total number of quartets ….."
    See, p.1, ln 15.

5. "lines 25-26, p.98…."

    Done. See, p.2, ln5-7.

6. "After line 25, p. 99………………………………"
    Q is wave steepness. See p. 3, ln 10. The wave steepness cannot be ~2 for weakly nonlinear waves. if q<1 , then a structure is weakly nonlinear wave, if q>1 corresponds to a nonlinear structure. See explanation on p. 2

7. "Explain what is meant by………"
    Done. See p. 4, ln1-2.

8. "Figure 4…………………….."
    Biannual oscillations in Fig. 4 don't depict biannual periodicity because those are quadratic values.

9. "Figure 9…….."
    We have revised the legend of Fig. 9 and discussed how we identified two different types of quartets. See, p.17, ln 15-16, ln 21-22.
    Some discussion about different types of quartets has been added. See, p. 17, ln 15-16, ln 21-22.

10. Thank you very much for noted typos.

<div align="center">**RESPONSE #2**</div>

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

Thank you very much for your review and especially for additional references of Wang et al. (2013) and Qiu et al. (2013). Our revision is given by red line in the manuscript text

1. **Major comments**
   1. "More discussion are needed in order to better connect ………………………………………."
      We have revised the manuscript on p1, ln28; p2, ln3-4, ln18, ln23-31, p3, ln9 – 10, ln14-22.

   2. "The M-modes approach has advantages in dealing with non-regular…"

      See, revision on p.6, ln5-9.

      We do not see any problem with interpretation of modulational instability in (M-mode, ω) space.   M-modes are the same Fourier modes but generalized on non-rectangular areas only. We published more than five papers where these modes were used. Your comments about non-commonly of M-modes seem to be surprise for us. If possible, could you please give us a more specific criticism?

   3. "The paper argues that all structures observed off California were generally weakly…"

      First, the degree of linearity depends on horizontal resolution which was larger than 50-100 km for the AVISO data. Second, we argued that nonlinearity of observed structures change with time. A large part of time q<1 (that can be interpreted as weakly nonlinear structures) but for small part of time q>1 (these are nonlinear structures).

   4. " The oceanic eastern boundary current off California is a baroclinic……………"

      We explained how the baroclinicity influences the modulation instability on p.15, ln 15-23.

   5. "Can we define all signals within 4-18 ……………………………………."

      We do not completely agree with this idea. Of course, we cannot discuss it in terms of the Rossby wave dispersion relation because we do not know the Rossby wave dispersion relation.

   6. The method of identifying triads and quartets needs more clarifications".

This question is not clear because we explained how we select triads and quartets on p.9, ln 4-9.

2. **Minor comments**

1." Introduction needs more details………………………………………………………………………………"

Thank you very much for additional references of Wang et al., 2013 and Qiu et al., 2013 which are cited now.
However, in both the references a simple model of dynamics is used. For example, a barotropic quasi-geostrophic model in an idealized meridional channel is used in Wang et al. 2013. Qiu et al. 2013 discussed the region below 18^0 N which does not coincide with our studying area. Therefore we cannot compare our calculations to results obtained in both the papers.
Now about the Centurioni et at 2008 paper. One of the results was obtained by averaging ROMS output over the whole computational series and produced some quasi-zonal jets. These jets cannot be compared to our results because the latter are time-dependent.

2."The abbreviations in ………….."

We do not see a mistake. We would prefer to keep the abbreviations.

3." Please explicitly calculate ……………."

It was done. See p. 5, ln 13-19.

4.   "The authors argued   ".
Any signals which are a combination of modes with numbers from 1 to 30 cannot propagate westward. For explanation and details please see Ivanov et al.2009 and Ivanov et al.2010.

5.   "The argument regarding the role of flow…."
We demonstrated herein that no quasi-zonal jets can be generated if viscosity is less than some value.

6.   "The label (f) should be (g) in Fig. 3."
Done.

7."The caption of Fig. 4   ……"

Revised.

8.   " In general, the fronts ……………………………………………………

This will be done in a final version of figures with high-resolution. Thank you for the remark.