A review of

Comparative Mesoscale Eddy Dynamics under Geostrophic versus Cyclogeostrophic Balance from Satellite Altimetry

by

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In this study the authors have explored how mesoscale eddy properties differ when considering cyclogeostrophic balance compared to geostrophic balance. They argue that the long-standing geostrophic balance assumption does not fully describe mesoscale eddy currents and a recalibration towards cyclogeostrophic balance should be considered. The authors consider separate regions across the North Pacific, motivated to improve their understanding of mesoscale currents across the Kuroshio Extension and wider afield. To conduct their analysis, the authors make use of satellite altimetry data at a horizontal resolution of 0.25 degrees. This data product is then used to compute the geostrophic and cyclogeostrophic eddy field, of which they acquire from Cao et al., (2023). Thereafter, a series of figures and analyses are presented that give the reader a strong sense that this change in balance can be important.

As the reader progresses through the paper it is clear that there are a number of things that could be improved. Although the results do demonstrate changes to eddy properties such as EKE and eddy distributions, the authors provide limited discussion on the processes that cause these changes. I think the authors could do a better job to tie in wider significance to these findings e.g. through process studies and the implications for say Earth system modelling - tuning of models through surface EKE, for example.

Overall, I believe that a study of this scope does fit into the Ocean Science journal and would be of interest to the wider community. However, there are a number of things (see below) that need addressing before it can be considered for publication. I recommend major revisions before publication. My comments are in no particular order.

Comments

- The reader could be helped by the inclusion of the geostrophic and cyclogeostrophic balance equations, a discussion of the differences, and a schematic to illustrate this. This could then be used to explain some of the differences in the results e.g. why the geostrophic assumption is suitable for the ACE EKE in CC region (Fig 1) but not in the KE region. Are there larger pressure/centrifugal differences? And on line 196 ‘Thus the discrepancy…’ could give further insight here by including equations and discussion.

- The ‘Discussion’ section appears to be a continuation of results, focusing on case studies of ACE and CE in each region. Should a separate section within the results be made called ‘Case Studies’? I would not put new results in a discussion section.

- In the abstract there is mention of a few process changes but no actual discussion of this in the paper. Please consider what is written in the abstract should be discussed in the paper. For example, ‘possess stronger potential energy’. I think this line refers to the ‘Discussion’ section and Figure 18 (?), but there is no discussion of this in the paper. I would disagree that the eddy possesses stronger potential energy. Is the eddy barotropic, is it baroclinic? The sea level anomaly does not change, and no subsurface examination is carried out, so it is hard to say this.

- There are a number of places throughout the paper that could benefit by further explanation. Some examples, ‘eddy generation enhanced due to upwelling’ (line 297), and ‘AE influenced by curvature dissipate more readily’ (line 607). Are there any process based studies that show these to be the case?

- It is interesting that the Kuroshio Extension has the largest eddy radii (Fig 5), could there be a reason for this?

- The inclusion of the case studies is to try and draw some physical understanding from the results? Maybe this needs to be clear.

- At the end of the Introduction section, you could outline your paper e.g. ‘In section X we will discuss the results, and in section Y …’. This will solve my above point.

- Stegner and Dritschel (2000) [A Numerical Investigation of the Stability of Isolated Shallow Water Vortices] looked at the stability of ACE and CE in cyclogeostrophic balance; maybe this paper will help to add further support and locate more citations.

- I somewhat understand the inclusion of strain rate and enstrophy, but feel it should probably be omitted. In 2D turbulence, dissipation is proportional to enstrophy, and so you could get some understanding from this. The strain rate similarly suggests dissipation but in 3D turbulence, which the 0.25 degree altimeter is never going to capture. Have there been studies that have looked at turbulence using satellite altimeter data? They are probably heavily constrained by resolution. An alternative could be to include the wind stress curl and Ekman pumping, but you then need a wind stress data product… you could look at spatial changes in EKE?

- The results are often quite long winded, could they be broken up and focus given to the most significant finding?

- Could other eddy census studies be discussed further e.g. Chelton et al (2011), Chen et al (2019) 10.1029/2019JC014983

- How do you think your results would differ if you chose an alternative eddy detection algorithm? Would eddy radii change? Just something to think about.

Specific comments

- Line 22-23: eddy kinetic energy and Eddy Kinetic Energy - decide what to use.
- Line 24: ‘Eddies predominantly …’ this isn’t new.
- Line 27: ‘Vorticity is more stable’ - the eddy is more stable by virtue of a smaller Rossby number?
- Line 31-32: Where do you look at eddy-eddy and eddy-current interactions?
- Line 50: space between ‘kilometers(Chelton’.
- Line 76: ‘where leeward… shows significant periods…’ do you cycles/variability?
- Line 108: source for satellite data?
- Line 112: put ‘data are accessible …’ in data availability, and cite in text.
- Line 121: what is the velocity fluctuation? Spatial, temporal - 5,10 days?
- Line 123: ‘percentage difference…’ between geos and cylco?
- Eq 5 is the dynamical Rossby number.
- Line 166: ‘andSCS’
- Line 180: ‘This study compares…’ say this earlier?
- Line 183: ‘Satellite altimetry overestimates…’ using geostrophic assumption?
- Line 190: ‘Spatially, the difference…’ could be reworded to clarify cyclo is more positive than geos over the domain?
- Figure 1: Label each figure panel with the region (save reader from scanning caption and back to panels), remove legends from all but panel b.
- Figure 2: Agulhas Insurgence?
- Figure 2: Why are there differences across regions?
- Figure 3: Regions are quite small, can these be made bigger?
- Line 360: Any studies showing why these eddies are larger? Can you cite any.
- Figure 9 and 10: Less eddies generated and less dissipated in cyclo. All regions are fairly similar, can the results be compressed into one figure, side by side with generation and dissipation? Feel free to change or not, just a suggestion. Could any other figures be condensed? Show total and then most significant finding?
- Line 468: Why are there less eddies generated and dissipated under cyclo balance?
- Line 486: Fig 11b and d should be Fig 12?
- Figure 12: More of a uniform dissipation of geos eddies in KE region but not in cyclo, why?
- Figure 13: Label left column CE and right column ACE? Seems the biggest fluctuations are in SCS, any idea why?
- Figure 15: Could be consistent with labelling? E.g. KE region is sometimes presented first, but here it is in panel d. Why are there such differences, even in geos and cyclo, this is where other literature could come in use.
- Figure 16: You could almost say the vorticity and strain are inversely related?
- Line 580: Change name of discussion? Do you need to include all five case studies here?
- Line 732: Could additional sources and references be included here to tie up your findings? What are the implications, bigger picture, takeaway for the reader?
- Line 780: Where do you discuss eddy-eddy interactions and how do you define this?