Web links to the author's journal account have been redacted from the decision letters as indicated to maintain confidentiality.

5th Mar 21

Dear Professor Nagai,

Your manuscript titled "The Kuroshio flowing over seamounts and associated submesoscale flows drive 100-km-wide 100-1000-fold enhancement of turbulence" has now been seen by two reviewers, whose comments are appended below. You will see that they find your work of some potential interest. However, they have raised quite substantial concerns that must be addressed. In light of these comments, we cannot accept the manuscript for publication, but would be interested in considering a revised version that fully addresses these serious concerns.

We hope you will find the reviewers' comments useful as you decide how to proceed. Should additional work allow you to
• address these criticisms (that is, either to incorporate the suggestions or provide a compelling argument why the point made by the reviewer is not valid, or relevant to the editorial threshold as outlined below)
AND
• meet our editorial thresholds as outlined below,

then we would be happy to look at a substantially revised manuscript.

In the following, we list our main editorial requirements.

****
****Editorial threshold 1: present compelling and novel insights into generation of turbulence through Kuroshio-seamount interactions, including a fully supported and comprehensive discussion and analysis of possible mechanisms

****Editorial threshold 2: appropriately place your manuscript in the context of the available literature as outlined by the reviewers

Please also note that we operate a FAIR data policy, whereby at the time of publication, the minimal dataset necessary to interpret, replicate and build upon the methods or findings reported in the article should be placed in a publically available repository. We do not allow statements that data are available upon request. (see also here https://www.nature.com/commsenv/journal-policies/editorial-publishing-policies#availability)

****

Please bear in mind that we will be reluctant to approach the reviewers again in the absence of substantial revisions.

If the revision process takes significantly longer than three months, we will be happy to reconsider your paper at a later date, as long as nothing similar has been accepted for publication at Communications Earth & Environment or published elsewhere in the meantime.

We understand that due to the current global situation, the time required for revision may be longer than usual. We would appreciate it if you could keep us informed about an estimated timescale for resubmission, to facilitate our planning. Of course, if you are unable to estimate, we are happy to accommodate necessary extensions nevertheless.

We are committed to providing a fair and constructive peer-review process. Please do not hesitate to contact us if you wish to discuss the revision in more detail.

Please use the following link to submit your revised manuscript, point-by-point response to the referees' comments (which should be in a separate document to any cover letter) and any completed checklist:

[link redacted]

** This url links to your confidential home page and associated information about manuscripts you may have submitted or be reviewing for us. If you wish to forward this email to co-authors, please delete the link to your homepage first **

Please do not hesitate to contact me if you have any questions or would like to discuss the required revisions further. Thank you for the opportunity to review your work.

Best regards,

Heike Langenberg, PhD

Chief Editor
Communications Earth and Environment

On Twitter: @CommsEarth

EDITORIAL POLICIES AND FORMAT

If you decide to resubmit your paper, please ensure that your manuscript complies with our editorial policies and complete and upload the checklist below as a Related Manuscript file type with the revised article:

Editorial Policy Policy requirements

For your information, you can find some guidance regarding format requirements summarized on the following checklist:(https://www.nature.com/documents/commsj-phys-style-formatting-checklist-article.pdf) and formatting guide (https://www.nature.com/documents/commsj-phys-style-formatting-guide-accept.pdf).

REVIEWER COMMENTS:

Reviewer #1 (Remarks to the Author):

Review of “The Kuroshio flowing over seamounts and associated submesoscale flows drive 100-km-wide 100-1000-fold enhancement of turbulence” by T. Nagai et al.

This paper presents the strong and widely distributed turbulences resulting from the near-inertial waves (mostly nearfield) and symmetric instability (mostly far-field) as a result of the Kuroshio-seamount interaction. Turbulence data, hydrographic data, and velocity data were collected from three cruises around seamounts in the Tokara Strait. By utilizing the data, turbulent mixing along the Kuroshio and over the long-range across the Tokara Strait was quantified. High-resolution numerical simulations were performed to help the explanations of the mechanisms responsible for the observed small-scale processes and their resulting turbulence. The structures of high vertical wavenumber and negative potential vorticity characterized the presence of NIW and SI, respectively, which producing the strong mixing. Especially, the SI is evident to be responsible for the turbulent mixing for longer range, which largely exceeds the current advection.

The observations are novel, and the analysis results are very significant. The paper is mostly well written but can be improved in some places. Therefore, the following comments and corrections need to be addressed before I can recommend publication.

General comments

The argument of negative PV-induced enhanced mixing based on the observations and model simulations needs to be clarified. The method proposed in Thomas’s paper is helpful to recognize SI. But other processes can induce turbulent mixing as well, which is evident that strong turbulence can also be observed in the region of positive PV. An alternative mechanism is that the tilting of the horizontal shear layer could enhance the vertical shear and consequently result in enhanced turbulence via KH instability. This must be clarified. Furthermore, the SI can’t produce turbulence directly. The mediating processes leading to turbulent transition are required. The KH instability is a candidate. I suggest incorporating the analysis of Ri. Both the SI and vortex tilting could enhance the turbulence via KH instability. Details can be found in specific comments.

Specific comments

L35-36. The observations and model did not truly reveal the microscale turbulence that “follows” the submesoscale instability but revealed their presence at the same time. Otherwise, the sentence gives an impression that the submesoscale instability (O(100 m - 10 km)) evolves to turbulence (O(<1 cm)) directly. But there must be certain mediating processes such as KH instability.

L40-42. Both the negative PV and NIW were responsible for the significant region of turbulent mixing over 100 km?

L50. 1TW -> 1 TW.

L127. Peaks?

L130. h ~ 200-400 m.

Fig. 1f. The lines of legs b and c are not easy to identify because they are almost connecting.

L155-156. Transects b and c are mostly parallel to the orientation of Tokara Strait. The location of Tokara Strait should be labeled in Fig. 1f.

L162, L163. m2s-1 -> m2 s-1
L167. Wkg-1 -> W kg-1; ms-1 -> m s-1, and maybe some other places.

L168. How was the value of downstream extent estimated?

Fig. 3a. The black areas indicate isobath (at what depth)?

L186. It was confusing to make the comparison. It seems the red line in Fig. 3c is the track of leg c (northwest-southeast running) in Fig. 1. However, the bathymetry shown in Fig. 3c looks like that in leg b. It would be helpful to add the observational survey tracks in Fig 3a.

L198-200. Consider adding the illustrations for the generation and formation of the positive and negative PVs. The modeled PV showed the von Karman-like vortex street, which needs some illustrations here as well.

L202-207. The argument of negative PV-induced enhanced mixing based on the model result is fragile. As mentioned, the mixing scheme in the model is based on the Richardson number. The SI occurring within the negative PV is one of the possibilities to lower the Ri. Other processes that could increase the vertical shear profile and/or reduce the stratification can induce model mixing, same with the observations. Therefore, more careful analysis and elaboration are needed.

L212-215. Could the authors assess the generation of NIW using the present model? For example, the relationship between the NIW magnitude and the model diffusivity (if it could be outputted).

L239-240. It is difficult to identify their coincidence. I suggest overlapping the contour line of PV = 0 in figs 5c and 5d.

L242-244. An estimate of Ri was not shown here. The corresponding variations between Ri and negative PV would provide useful insight. The SI can't produce turbulence by itself. The presence of mediating processes leading to turbulent transition is required. Numerical modeling of Taylor and Ferrari (2009; JFM) suggested the secondary KH instability as a mechanism by which energy is transferred from the symmetric instability to microscale turbulence. I will suggest adding a plot like Figure 9 but with the x-axis representing Ri.

L240-242. As I have mentioned, the mechanism of SI is not necessary the (only) source of turbulent mixing. Occasionally, the strong turbulence can be observed in the region of positive PV, which may partly support this point of view. Alternatively, observations in Farmer et al. (2002; Dyn. Atmos. Oceans.) suggested the tilting of the horizontal shear layer, commonly seen downstream, could enhance the vertical shear and consequently produced KH instability. Therefore, the turbulent mixing was enhanced due to the collapse of KH billows. This idea was further examined numerically by White and Helfrich (2013; JFM). The process does not require the preferred occurrence in the region of negative PV. The authors should clarify the possibility.

Farmer, D., Pawlowicz, R. & Jiang, R. (2002). Tilting separation flows: a mechanism for intense vertical mixing in the coastal ocean. Dyn. Atmos. Oceans. 36 (1–3), 43–58.

Taylor, J. & Ferrari, R. (2009). The role of secondary shear instabilities in the equilibration of symmetric instability. J. Fluid Mech. 622, 103–113.

White, B., & Helfrich, K. (2013). Rapid gravitational adjustment of horizontal shear flows. Journal of Fluid Mechanics, 721, 86-117.

Reviewer #2 (Remarks to the Author):

Please see attachment.

THE KUROSHIO FLOWING OVER SEAMOUNTS AND ASSOCIATED SUBMESOSCALE FLOWS DRIVE 100-KM WIDE 100-1000-FOLD ENHANCEMENT OF TURBULENCE

In this manuscript the authors describe measurements from several cruises taken in the Kuroshio where direct microstructure turbulence measurements showed enhanced turbulent mixing in the wake of seamounts. These observations are interpreted in terms of several possible mechanisms, including the generation of lee waves, near-inertial waves, and submesoscale instabilities due to boundary layer generation of vorticity. Additional context is provided using a high-resolution numerical model. The topic is interesting and timely, with much focus currently in the physical oceanography community on flow-topography interaction and submesoscales—and their implications for ocean energetics and mixing.

Given the unique nature of these observations, I think a manuscript based on them is certainly worth publishing. However, while I found the description of the microstructure observations well done and thorough, the dynamical interpretation of the mechanisms responsible for the observed enhancement of turbulence is quite speculative, and not entirely convincing. I detail some of my concerns in this regard below. Given this, I suggest an editorial decision of major revisions.

1. Major Comments

(1) As a general comment I don't think the authors completely situate their findings in the recent literature. I will try to point out a few of these instances in specific cases below. One of which is that the authors claim in the abstract that 'this submesoscale mechanism...has been elusive without any observational evidence.' This is not true, and in fact similar microstructure measurements have been reported, and interpreted in terms of submesoscale instabilities generated near the bottom, in Naveira Garabato et al. (2019). Other less direct measurements have also recently been interpreted in terms of submesoscale mixing, for example Ruan et al. (2017).

(2) The analysis of the observations in terms of a diffusivity using the Osborn method is almost certainly quantitatively incorrect for submesoscale SI-Inertial instability, and very well may be qualitatively incorrect as well. The root of this is that the mixing efficiency for these instabilities is not well-constrained at the moment, and for example many simulations of symmetric instability have tended to suggest mixing efficiences near 0. Given this, these results (and the interpetation in terms of nutrient fluxes) seems to be quite speculative. Despite the relatively coarse resolution, which does not directly resolve SI scales and smaller, I imagine that some analysis of the numerical simulation could have provided some evidence of the role of enhanced mixing due to the low PV wakes.

(3) One of the mechanisms proposed here for the enhanced turbulence associated with the low PV regions is the trapping of near-inertial waves and the subsequent generation of secondary instabilities. This is interesting, but I'm not personally aware of any prior exploration of this, as related to topographic wakes, in the literature. For example, did this mechanism show up in the recent idealized simulations of flow over seamounts by Srinivasan et al. (2019)? And if not, what is different about the Kuroshio setting?

(4) The pattern of dissipation as a function of PV and the Thomas angle, Φ, given in figure 6 is hard to interpret. There is enhanced turbulence for small Φ at all values of the PV.

I suspect this reflects simply the dependence of Φ on the inverse Richardson number, such that the plot is just saying that the Richardson number tends to be small where there is turbulence and vice versa. It is true that there is an apparent enhancement of dissipation for very negative values of PV when averaged (second panel), but this is also a very undersampled part of parameter space, and this effect would be much less pronounced if different averaging bins were chosen. It is also evidently not entirely robust across surveys, as shown in figure 9. Given this, I'm somewhat unconvinced that there is robust enhancement of turbulence in low PV regions in these surveys.

(5) The Thomas angle is a useful metric for considering instability types, however the $-45°$ criteria does not always delineate the transition between symmetric and centrifugal modes, and was derived for the special case that $f + \zeta = 0.4f$. This is somewhat obscured in the Thomas et al. (2013) paper, but can be seen in the appendix of that work. Note that the delineation between symmetric and inertial modes is there defined using the ratio of lateral to geostrophic shear production (LSP/GSP, their equation 42), which is function of the relative vorticity field. They then assume that $f + \zeta = 0.4f$ —as is appropriate for their *specific* case considering the Gulf Stream — from which the ratio $LSP/GSP \approx 1$ for $\Phi \approx -45°$. In contrast, figure 4 suggests significant spatial variations of ζ in your observations. Sections of this work that use this to delineate instability types may therefore not be correct.

(6) My final major comment relates to the data availability statement, where it is noted that '*The datasets generated during and/or analysed during the current study are available from the corresponding author on reasonable request.*' I am sympathetic towards the desire to maintain some level of ownership over a unique dataset such as this, which undoubtedly represents significant hard work in acquisition. However, making data available only upon request (and further only on those deemed 'reasonable') falls short of open reproducible science. On this point I would just urge the authors to consider providing the data in an online repository (for example zenodo.org) such that others can reproduce their analysis, and to make sure that it remains accessible far into the future.

2. Minor Comments

(1) Line 267: missing space in 'leg 1'.

(2) Line 355, and related to comment (5) above: You might consider recent work by Wenegrat and Thomas (2020), who show that the inertial-symmetric transition may not be very important for BBL problems, as both modes behave similarly except for a transition between lateral and vertical geostrophic shear production.

(3) Equation 7: Add brackets to delineate the scope of the arctan operator.

References

Naveira Garabato, A. C., and Coauthors, 2019: Rapid mixing and exchange of deep-ocean waters in an abyssal boundary current. *Proceedings of the National Academy of Sciences*, **116 (27)**, 13 233–13 238, doi:10.1073/pnas.1904087116, URL http://www.pnas.org/lookup/doi/10.1073/pnas.1904087116.

Ruan, X., A. F. Thompson, M. M. Flexas, and J. Sprintall, 2017: Contribution of topographically generated submesoscale turbulence to Southern Ocean overturning. *Nature Geoscience*, **10 (11)**, 840–845, doi:10.1038/ngeo3053, URL http://www.nature.com/doifinder/10.1038/ngeo3053.

Srinivasan, K., J. C. McWilliams, M. J. Molemaker, and R. Barkan, 2019: Submesoscale Vertical Wakes in the Lee of Topography. *Journal of Physical Oceanography*, **49 (7)**, 1949–1971, doi:10.1175/JPO-D-18-0042.1, URL http://journals.ametsoc.org/doi/10.1175/JPO-D-18-0042.1.

Thomas, L. N., J. R. Taylor, R. Ferrari, and T. M. Joyce, 2013: Symmetric instability in the Gulf Stream. *Deep Sea Research Part II: Topical Studies in Oceanography*, **91**, 96–110, doi:10.1016/j.dsr2.2013.02.025, URL http://linkinghub.elsevier.com/retrieve/pii/S0967064513000829.

Wenegrat, J. O., and L. N. Thomas, 2020: Centrifugal and Symmetric Instability during Ekman Adjustment of the Bottom Boundary Layer. *Journal of Physical Oceanography*, **50 (6)**, 1793–1812, doi:10.1175/JPO-D-20-0027.1, URL https://journals.ametsoc.org/jpo/article/50/6/1793/346599/Centrifugal-and-Symmetric-Instability-during-Ekman.

Reply to reviewers

Thank you for all the useful comments and suggestions. Here, we reply to each of the comments provided by the reviewers. The reviewers' comments are in blue.

Reviewer #1 (Remarks to the Author):
Review of "The Kuroshio flowing over seamounts and associated submesoscale flows drive 100-km-wide 100-1000-fold enhancement of turbulence" by T. Nagai et al.

This paper presents the strong and widely distributed turbulences resulting from the near-inertial waves (mostly nearfield) and symmetric instability (mostly far-field) as a result of the Kuroshio-seamount interaction. Turbulence data, hydrographic data, and velocity data were collected from three cruises around seamounts in the Tokara Strait. By utilizing the data, turbulent mixing along the Kuroshio and over the long-range across the Tokara Strait was quantified. High-resolution numerical simulations were performed to help the explanations of the mechanisms responsible for the observed small-scale processes and their resulting turbulence. The structures of high vertical wavenumber and negative potential vorticity characterized the presence of NIW and SI, respectively, which producing the strong mixing. Especially, the SI is evident to be responsible for the turbulent mixing for longer range, which largely exceeds the current advection.

The observations are novel, and the analysis results are very significant. The paper is mostly well written but can be improved in some places. Therefore, the following comments and corrections need to be addressed before I can recommend publication.
Thank you for the positive comments on the manuscript. We have thoroughly revised the manuscript taking all the comments into consideration.

General comments

The argument of negative PV-induced enhanced mixing based on the observations and model simulations needs to be clarified. The method proposed in Thomas's paper is helpful to recognize SI. But other processes can induce turbulent mixing as well, which is evident that strong turbulence can also be observed in the region of positive PV. An alternative mechanism is that the tilting of the horizontal shear layer could enhance the vertical shear and consequently result in enhanced turbulence via KH instability. This must be clarified. Furthermore, the SI can't produce turbulence directly. The mediating processes leading to turbulent transition are required. The KH instability is a candidate. I suggest incorporating the analysis of Ri. Both the SI and vortex tilting could enhance the turbulence via KH instability. Details can be found in specific comments.
Thank you for the useful comments. We have added the discussion on the possibility of the tilting and stretching of the horizontal shear to enhance the vertical shear.

Specific comments

L35-36. The observations and model did not truly reveal the microscale turbulence that "follows" the submesoscale instability but revealed their presence at the same time. Otherwise, the sentence gives an impression that the submesoscale instability (O(100 m - 10 km)) evolves to turbulence (O(<1 cm)) directly. But there must be certain mediating processes such as KH instability.

Thank you. We have modified this sentence to "On the other hand, recent numerical studies revealed that geostrophic flows over steep slopes can change the signs of potential vorticity (PV) causing submesoscale instability followed by intense irreversible mixing processes".

L40-42. Both the negative PV and NIW were responsible for the significant region of turbulent mixing over 100 km?

Because many different processes could occur simultaneously, it is difficult to ascertain the most dominant mechanism to induce the turbulence over 100 km. In the region close to the seamounts, lee waves and other mechanisms such as tilting and stretching of the lateral vorticity become important. Therefore, we have modified this sentence as follows.

"In this study, using a state-of-the-art tow-yo microstructure profiler, and a numerical model, we show here for the first time that the Kuroshio flowing over steep seamounts generates streaks of negative PV and near-inertial waves. With these long-persisting mechanisms in addition to other near-field mixing processes, intense mixing hotspots are formed over a 100-km scale with the elevated energy dissipation by 100- to 1000-fold."

L50. 1TW -> 1 TW.

Thank you. This has been corrected.

L127. Peaks?

We have changed "peaks" to "jumps".

L130. h ~ 200-400 m.

Thank you. This has been corrected.

Fig. 1f. The lines of legs b and c are not easy to identify because they are almost connecting.

We have added circles to indicate stations.

L155-156. Transects b and c are mostly parallel to the orientation of Tokara Strait. The location of Tokara Strait should be labeled in Fig. 1f.

We have added the location of the Tokara Strait.

L162, L163. m2s-1 -> m2 s-1

Thank you. This has been corrected.

L167. Wkg-1 -> W kg-1; ms-1 -> m s-1, and maybe some other places.

Thank you. This has been corrected.

L168. How was the value of downstream extent estimated?

The 100 km is estimated based on the depth averaged dissipation rates that show elevated values >O(10^{-8} W kg^{-1}) in supplementary material (Fig S1k-o). However, we acknowledge that the previous description on this is not accurate enough. We have modified this to "*The average dissipation rates below 100 m are >O(10^{-7} W kg^{-1}) over 65 km and >O(10^{-8} W kg^{-1}) over 100 km in the Tokara Strait (supplementary materials Fig. S1 l-m).*" at P9L11.

Fig. 3a. The black areas indicate isobath (at what depth)?

It is at 200 m. We have added this to the caption.

L186. It was confusing to make the comparison. It seems the red line in Fig. 3c is the track of leg c (northwest-southeast running) in Fig. 1. However, the bathymetry shown in Fig. 3c looks like that in leg b. It would be helpful to add the observational survey tracks in Fig 3a.

We have added the observation track in Fig. 3a.

L198-200. Consider adding the illustrations for the generation and formation of the positive and negative PVs. The modeled PV showed the von Karman-like vortex street, which needs some illustrations here as well.

Thank you. We have added these model illustrations in this part as follows at P12L1.

"*The modeled potential vorticity shows the von Karman-like vortex street ~~after~~ having streaks of negative and positive values along the stream over a 50-km width, originating from the northeastern and southwestern slopes of the seamounts, respectively, in the Kuroshio in the regions near the in-situ observation lines (Fig. 3a, d and e). Although the model cannot resolve the inertial–symmetric instability[5-9,34] and associated secondary Kelvin Helmholtz instability[31], the modeled streaks of negative PV near the observation sites would explain why the turbulence in the observations is strong and long-persisting (supplementary information Fig. S7).*".

L202-207. The argument of negative PV-induced enhanced mixing based on the model result is fragile. As mentioned, the mixing scheme in the model is based on the Richardson number. The SI occurring within the negative PV is one of the possibilities to lower the Ri. Other processes that could increase the vertical shear profile and/or reduce the stratification can induce model mixing, same with the observations. Therefore, more careful analysis and elaboration are needed.

Thank you for this useful comment. We are not claiming that the model enhanced dissipation is caused by SI with the negative PV, as the model does not resolve SI. But we agree that this gives some impression that the SI with the negative PV provides the energy dissipation in the model. Therefore, we have removed this part from the manuscript and added more analyses on the other mechanisms such as the tilting the lateral vorticity in the observations and the model.

L212-215. Could the authors assess the generation of NIW using the present model? For example, the relationship between the NIW magnitude and the model diffusivity (if it could be outputted).

Thank you for the useful comment. We agree that the generation mechanism of the near-inertial waves is certainly a very important topic. We have tried to assess this problem by extracting internal waves from subinertial flow and comparing them with the model dissipation rates. Because the nested model domain is too small to employ the Lagrangian temporal filter, the extraction of internal wave is done by first subtracting geostrophic shear from the total shear, which is then demeaned, detrended, and integrated vertically to obtain internal wave velocity. However, it is found to be very difficult to determine how the extracted internal wave kinetic energy is related to the dissipation rates (Fig. A). First, the extracted internal waves may include any non-geostrophic flows, not just the internal waves. Also, internal waves can induce energy dissipation instead, as other mechanisms do. Accordingly, we have mentioned that the generation mechanisms of the near-inertial internal wave in the Tokara Strait are still unclear as follows at P24L22.

"*Although detailed generation mechanisms of the near-inertial waves in the Kuroshio near the seamounts of the Tokara Strait are still not clear, the combined set of the multiscale mixing processes near the bottom of the ocean illustrated by this study shed light on how the subinertial flow over seamounts leads to the turbulent mixing in addition to that caused locally by lee waves alone and demonstrate how the Kuroshio over the seamounts creates large and efficient mixing hotspots.*"

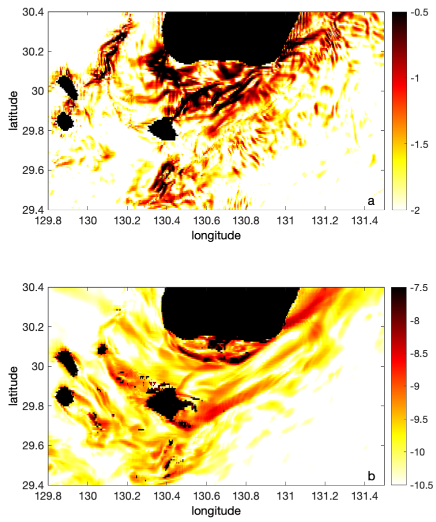

Fig. A (a) Deduced internal wave kinetic energy and model energy dissipation rate through vertical shear at 200 m depth

Also, as the descriptions about the possibility of trapping and dissipation of generated and/or amplified near-inertial waves are somewhat speculative, we moved this to Discussion in the revised version.

L239-240. It is difficult to identify their coincidence. I suggest overlapping the contour line of PV = 0 in figs 5c and 5d.

Thank you. We have added contours at PV=0 in Fig. 5c and d.

L242-244. An estimate of Ri was not shown here. The corresponding variations between Ri and negative PV would provide useful insight. The SI can't produce turbulence by itself. The presence of mediating processes leading to turbulent transition is required. Numerical modeling of Taylor and Ferrari (2009; JFM) suggested the secondary KH instability as a mechanism by which energy is transferred from the symmetric instability to microscale turbulence. I will suggest adding a plot like Figure 9 but with the x-axis representing Ri.

Thank you. We have added the new Figure S8-9 in the supplementary information to show ADCP Ri for November 2017 and June 2018 cruises. Also, we have added a panel to show TKE dissipation rates as a function of Ri and PV, and a panel for bin-average TKE dissipation rates as a function of Ri in the new Figure 6. It is shown in the vertical section of Ri (Figure S9) that the relatively low Ri of O(1) is found with large positive PV and TKE dissipation rates. The additional panel in Fig. 6b shows that the extremely large dissipation rates of O(10^{-5} W kg^{-1}) are found with negative PV and O(1) Ri, rather than smallest Ri (Fig. 6b), although both the tendencies of low PV and Ri associated with the large TKE dissipation rates are found (Fig. 6c-d). We have added the following sentences at P13L12.

"*The bin-average TKE dissipation rates as a function of PV and Ri show that both the low PV and low Ri are associated with the large TKE dissipation rates (Fig. 6c-d). However, it is also shown that extremely large dissipation rates of O(10^{-5} W kg^{-1}) are accompanied by the negative PV and Ri~1 rather than smallest Ri, while slightly more moderate but still very large dissipation rates of O(10^{-6} W kg^{-1}) can also be associated with positive PV but with relatively small Ri (Fig. 6b). These results suggest that the extremely strong turbulence occurs even with Ri~O(1) in the streak of the negative PV extending from the northeastern slope of the seamount, while strong turbulence can also be generated when Ri is small enough even with positive PV on the southwestern slope of the seamount.*"

L240-242. As I have mentioned, the mechanism of SI is not necessary the (only) source of turbulent mixing. Occasionally, the strong turbulence can be observed in the region of positive PV, which may partly support this point of view. Alternatively, observations in Farmer et al. (2002; Dyn. Atmos. Oceans.) suggested the tilting of the horizontal shear layer, commonly seen downstream, could enhance the vertical shear and consequently produced KH instability. Therefore, the turbulent mixing was enhanced due to the collapse of KH billows. This idea was further examined numerically by White and Helfrich (2013; JFM). The process does not require the preferred occurrence in the region of negative PV. The authors should clarify the possibility.

Thank you for this useful comment. We have added the analyses on this possibility in the discussion. The results suggest that this could be a cause of the strong turbulence with positive PV in the region close to the seamount. We have added one figure in the supplementary information, which shows tilting and stretching of lateral vorticity, and tilting of the vertical vorticity could enhance the vertical shear to reduce the Ri. We have added the following sentence at P15L7.

"*Similar to the observations in June 2018, the averaged dissipation rates however show a slight* *increasing trend toward larger positive PV too (blue line of Fig. 9), reflecting the strong turbulence found even with the large positive PV on the cyclonic side of the seamount. In the region very close to the topographic features, the previous studies pointed out that vorticity tilting can result in enhancement of the vertical shear and associated turbulence[37-38]. By computing the magnitudes of stretching and tilting terms of vorticity, it is shown that the vorticity stretching and tilting can enhance or reduce the vertical shear to change Ri rapidly (supplementary information Fig. S12). The magnitudes of these terms are especially large at 100 m depth both on the northeastern (anticyclonic) and southwestern (cyclonic) slopes of the seamount, whereas the relatively larger magnitude is found only on the cyclonic side at 200 m depth. The deeper extent of the vertical shear strengthening or weakening on the cyclonic side of the seamount caused by the vorticity stretching and tilting is consistent with the large dissipation rates with the low Ri but large positive PV on the cyclonic side. Note, however, that the largest dissipation rates are more associated with the negative PV rather than smallest Ri (Fig. 6a-b).*"

Farmer, D., Pawlowicz, R. & Jiang, R. (2002). Tilting separation flows: a mechanism for intense vertical mixing in the coastal ocean. Dyn. Atmos. Oceans. 36 (1–3), 43–58.

Taylor, J. & Ferrari, R. (2009). The role of secondary shear instabilities in the equilibration of symmetric instability. J. Fluid Mech. 622, 103–113.

White, B., & Helfrich, K. (2013). Rapid gravitational adjustment of horizontal shear flows. Journal of Fluid Mechanics, 721, 86-117.

Reviewer #2

THE KUROSHIO FLOWING OVER SEAMOUNTS AND ASSOCIATED SUBMESOSCALE FLOWS DRIVE 100-KM WIDE 100-1000-FOLD ENHANCEMENT OF TURBULENCE

In this manuscript the authors describe measurements from several cruises taken in the Kuroshio where direct microstructure turbulence measurements showed enhanced turbulent mixing in the wake of seamounts. These observations are interpreted in terms of several possible mechanisms, including the generation of lee waves, near-inertial waves, and submesoscale instabilities due to boundary layer generation of vorticity. Additional context is provided using a high-resolution numerical model. The topic is interesting and timely, with much focus currently in the physical oceanography community on flow-topography interaction and submesoscales— and their implications for ocean energetics and mixing.

Given the unique nature of these observations, I think a manuscript based on them is certainly worth publishing. However, while I found the description of the microstructure observations well done and thorough, the dynamical interpretation of the mechanisms responsible for the observed enhancement of turbulence is quite speculative, and not entirely convincing. I detail some of my concerns in this regard below. Given this, I suggest an editorial decision of major revisions.

Thank you for the positive comments on the manuscript.

1. Major Comments

1. (1) As a general comment I don't think the authors completely situate their findings in the recent literature. I will try to point out a few of these instances in specific cases below. One of which is that the authors claim in the abstract that 'this submesoscale mechanism...has been elusive without any observational evidence.' This is not true, and in fact similar microstructure measurements have been reported, and interpreted in terms of submesoscale instabilities generated near the bottom, in Naveira Garabato et al. (2019). Other less direct measurements have also recently been interpreted in terms of submesoscale mixing, for example Ruan et al. (2017).

Thank you for these important references. We have added these to the revised version.

2. (2) The analysis of the observations in terms of a diffusivity using the Osborn method is almost certainly quantitatively incorrect for submesoscale SI-Inertial instability, and very well may be qualitatively incorrect as well. The root of this is that the mixing efficiency for these instabilities is not well-constrained at the moment, and for example many simulations of symmetric instability have tended to suggest mixing efficiences near 0. Given this, these results (and the interpetation in terms of nutrient fluxes) seems to be quite speculative. Despite the relatively coarse resolution, which does not directly resolve SI scales and smaller, I imagine that some analysis of the numerical simulation could have provided some evidence of the role of enhanced mixing due to the low PV wakes.

We disagree with the reviewer. Although the SI occurs along the isopycnal at most and may not be efficient for mixing, the secondary induced K-H instability and turbulence can break the stratification and diffuse up the nutrients. As we have FP07 thermistor data, we have computed the mixing efficiency factor from the observations. The results show that the mixing efficiency is not zero, although it shows a slight decreasing trend toward smaller PV and Φ. We have added the following explanation at P25L21.

"Although the symmetric instability grows along isopycnal at most and is considered to be inefficient to mix the water, a recent study[42] suggests that the secondary induced systematic ageostrophic flows can extract geostrophic potential energy as much as that for the kinetic energy. Also, the secondary K-H instability can produce vertical billows, inducing diapycnal mixing. Using observed microscale thermal dissipation rates with the simultaneously measured TKE dissipation rates, the mixing efficiency factor Γ can be directly computed (see Methods). The obtained mixing efficiency factor shows typical range 0-0.5 even with the negative PV, although there is a tendency of low Γ with relatively low PV and Thomas angle (supplementary information Fig. S13a), suggesting that strong turbulence in the Tokara Strait can destroy stratification and mix the water and tracers vertically."

3. (3) One of the mechanisms proposed here for the enhanced turbulence associated with the low PV regions is the trapping of near-inertial waves and the subsequent

generation of secondary instabilities. This is interesting, but I'm not personally aware of any prior exploration of this, as related to topographic wakes, in the literature. For example, did this mechanism show up in the recent idealized simulations of flow over seamounts by Srinivasan et al. (2019)? And if not, what is different about the Kuroshio setting?

Thank you. Although we do not know if this process can be found in Srinivasan et al. (2019), as we think that the near-inertial wave trapping mechanism is too speculative to be in Results, we moved this to the Discussion section. The difference between Srinivasan et al. (2019) and our study could be their use of a simple barotropic flow over the seamount examined with a small Rossby number.

4. (4) The pattern of dissipation as a function of PV and the Thomas angle, Φ, given in figure 6 is hard to interpret. There is enhanced turbulence for small Φ at all values of the PV.

The large dissipation rates with small Φ at all values of the PV suggest that not only inertia symmetric instability is responsible for the strong turbulence but also some other processes such as lee waves, and vorticity tilting could have also contributed to it. However, after averaging it as a function of PV, it is found that lower PV and especially negative PV tend to have larger dissipation rates. We have also added a panel of averaged dissipation rate as a function of PV and Richardson number in the new Figure 6. It shows that the large dissipation rates are found more with the negative PV even with Ri~1. We have added the following explanations at P13L11.

"*The bin-average TKE dissipation rates as a function of PV and Ri show that both the low PV and low Ri are associated with the large TKE dissipation rates (Fig. 6c-d). However, it is also shown that extremely large dissipation rates of O(10^{-5} W kg^{-1}) are accompanied by the negative PV and Ri~1 rather than smallest Ri, while slightly more moderate but still very large dissipation rates of O(10^{-6} W kg^{-1}) can also be associated with positive PV but with relatively small Ri (Fig. 6b). These results suggest that the extremely strong turbulence occurs even with Ri~O(1) in the streak of the negative PV extending from the northeastern slope of the seamount, while strong turbulence can also be generated when Ri is small enough even with positive PV on the southwestern slope of the seamount.*"

REVIEW

(5)

I suspect this reflects simply the dependence of Φ on the inverse Richardson number, such that the plot is just saying that the Richardson number tends to be small where there is turbulence and vice versa. It is true that there is an apparent enhancement of dissipation for very negative values of PV when averaged (second panel), but this is also a very undersampled part of parameter space, and this effect would be much less pronounced if different averaging bins were chosen. It is also evidently not entirely robust across surveys, as shown in figure 9. Given this, I'm somewhat unconvinced that there is robust enhancement of turbulence in low PV regions in these surveys.

We have tested several different bins to average the observed dissipation rates as a function of PV and found similar tendencies. Under sampling typically occurs at negative PV. Nevertheless, the larger dissipation rates with negative PV are found on average. The new Figure 6 incudes TKE dissipation rates as a function of PV and Ri, which shows that extremely large dissipation rates of O(10^{-5} W kg^{-1}) are accompanied by the negative PV and Ri~1 rather than smallest Ri, while slightly more moderate but still very large dissipation rates of O(10^{-6} W kg^{-1}) can also be associated with positive PV but with relatively small Ri (Fig. 6b).

The Thomas angle is a useful metric for considering instability types, however the −45à criteria does not always delineate the transition between symmetric and centrifugal modes, and was derived for the special case that f + ζ = 0.4f. This is somewhat obscured in the Thomas et al. (2013) paper, but can be seen in the appendix of that work. Note that the delineation between symmetric and inertial modes is there defined using the ratio of lateral to geostrophic shear production (LSP/GSP, their equation 42), which is function of the relative vorticity field. They then assume that f + ζ = 0.4f—as is appropriate for their specific case considering the Gulf Stream — from which the ratio LSP/GSP ≈ 1 for Φ ≈ −45à. In contrast, figure 4 suggests significant spatial variations of ζ in your observations. Sections of this work that use this to delineate instability types may therefore not be correct.
Thank you for the useful suggestions. We have added this discussion to the revised version and added explanations as follows at P13L21.
"*The Thomas angle*[34] *Φ (Methods) shows that the strong turbulence with negative PV_{2D} is associated with a range of Φ (Fig. 6a), suggesting that the energy dissipation occurs through both the lateral and vertical shear of the Kuroshio. However, the absolute vorticity in the vicinity of the seamount is much smaller ~ -2f (Fig. 4) compared with the case discussed in the previous study*[34] *~ +0.4f, implying that lateral shear of the Kuroshio is a very important source of the instability in our case. The slope Burger number*[35]*, - v_x/f Ri is positive with v_x/f ~ -3, and Ri~1, implying also the dominant source of the instability to be lateral shear*[18]."

My final major comment relates to the data availability statement, where it is noted that 'The datasets generated during and/or analysed during the current study are available from the corresponding author on reasonable request.' I am sympathetic towards the desire to maintain some level of ownership over a unique dataset such as this, which undoubtedly represents significant hard work in acquisition. However, making data available only upon request (and further only on those deemed 'reasonable') falls short of open reproducible science. On this point I would just urge the authors to consider providing the data in an online repository (for example zenodo.org) such that others can reproduce their analysis, and to make sure that it remains accessible far into the future.
Thank you for the comment. We have added the related dataset to the online repository at: https://ocg.aori.u-tokyo.ac.jp/nextcloud/index.php/s/ygPkzcZwQ8jcGAp

2. Minor Comments
Line 267: missing space in 'leg 1'.
Thank you. This has been corrected.

Line 355, and related to comment (5) above: You might consider recent work by Wene- grat and Thomas (2020), who show that the inertial-symmetric transition may not be very important for BBL problems, as both modes behave similarly except for a transition between lateral and vertical geostrophic shear production.

Thank you. This has been added to the revised version.

Equation 7: Add brackets to delineate the scope of the arctan operator.

Thank you. The brackets have been added.

References

(6)

(1) (2)

(3)

Naveira Garabato, A. C., and Coauthors, 2019: Rapid mixing and exchange of deep-ocean waters in an abyssal boundary current. Proceedings of the National Academy of Sciences, 116 (27), 13233–13238, doi:10.1073/pnas.1904087116, URL http://www.pnas.org/lookup/doi/10.1073/pnas.1904087116.

Ruan, X., A. F. Thompson, M. M. Flexas, and J. Sprintall, 2017: Contribution of topographically generated submesoscale turbulence to Southern Ocean overturning. Nature Geoscience, 10 (11), 840–845, doi:10.1038/ngeo3053, URL http://www.nature.com/doifinder/10.1038/ngeo3053.

Srinivasan, K., J. C. McWilliams, M. J. Molemaker, and R. Barkan, 2019: Submesoscale Vortical Wakes in the Lee of Topography. Journal of Physical Oceanography, 49 (7), 1949–1971, doi:10. 1175/JPO-D-18-0042.1, URL http://journals.ametsoc.org/doi/10.1175/JPO-D-18-0042.1.

REVIEW 3

Thomas, L. N., J. R. Taylor, R. Ferrari, and T. M. Joyce, 2013: Symmetric instability in the Gulf Stream. Deep Sea Research Part II: Topical Studies in Oceanography, 91, 96–110, doi:10.1016/ j.dsr2.2013.02.025, URL http://linkinghub.elsevier.com/retrieve/pii/S0967064513000829.

Wenegrat, J. O., and L. N. Thomas, 2020: Centrifugal and Symmetric Instability during Ekman Adjustment of the Bottom Boundary Layer. Journal of Physical Oceanography, 50 (6), 1793–1812, doi:10.1175/JPO-D-20-0027.1, URL https://journals.ametsoc.org/jpo/article/50/6/1793/346599/Centrifugal-and-Symmetric-Instability-during-Ekman.

13th Jul 21

Dear Professor Nagai,

Your manuscript titled "The Kuroshio flowing over seamounts and associated submesoscale flows drive 100-km-wide 100-1000-fold enhancement of turbulence" has now been seen by our reviewers, whose comments appear below. In light of their advice I am delighted to say that we are happy, in principle, to publish a suitably revised version in Communications Earth & Environment under the open access CC BY license (Creative Commons Attribution v4.0 International License).

We therefore invite you to revise your paper one last time to address the remaining concerns of our reviewers. At the same time we ask that you edit your manuscript to comply with our format requirements and to maximise the accessibility and therefore the impact of your work.

EDITORIAL REQUESTS:

Please review our specific editorial comments and requests regarding your manuscript in the attached "Editorial Requests Table". Please outline your response to each request in the right hand column. Please upload the completed table with your manuscript files.

If you have any questions or concerns about any of our requests, please do not hesitate to contact me.

SUBMISSION INFORMATION:

In order to accept your paper, we require the files listed at the end of the Editorial Requests Table; the list of required files is also available at https://www.nature.com/documents/commsj-file-checklist.pdf .

OPEN ACCESS:

Communications Earth & Environment is a fully open access journal. Articles are made freely accessible on publication under a CC BY license (Creative Commons Attribution 4.0 International License). This license allows maximum dissemination and re-use of open access materials and is preferred by many research funding bodies.

For further information about article processing charges, open access funding, and advice and support from Nature Research, please visit https://www.nature.com/commsenv/about/open-access

At acceptance, the corresponding author will be required to complete an Open Access Licence to Publish on behalf of all authors, declare that all required third party permissions have been obtained and provide billing information in order to pay the article-processing charge (APC) via credit card or invoice.

Please note that your paper cannot be sent for typesetting to our production team until we have received these pieces of information; therefore, please ensure that you have this information ready when submitting the final version of your manuscript.

Please use the following link to submit the above items:
[link redacted]
** This url links to your confidential home page and associated information about manuscripts you may have submitted or be reviewing for us. If you wish to forward this email to co-authors, please delete the link to your homepage first **

We hope to hear from you within two weeks; please let us know if you need more time.

Best regards,

Heike Langenberg, PhD

Chief Editor
Communications Earth and Environment

On Twitter: @CommsEarth

REVIEWERS' COMMENTS:

Reviewer #1 (Remarks to the Author):

The author has done an excellent job of responding to my review. I recommend that the paper be published.

Reviewer #2 (Remarks to the Author):

The authors have satisfactorily addressed my comments, and I recommend publication.

Reply to reviewers

We appreciate that both the reviewers are satisfied by the revision we made. Thank you very much for your time for the useful suggestions during the review processes.

Reviewer #1 (Remarks to the Author):

The author has done an excellent job of responding to my review. I recommend that the paper be published.

Reviewer #2 (Remarks to the Author):

The authors have satisfactorily addressed my comments, and I recommend publication.