nature portfolio

Peer Review File

Topographically driven vigorous vertical mixing supports mesoscale biological production in the Tsugaru Gyre

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This file contains all reviewer reports in order by version, followed by all author rebuttals in order by version.

Version 0:

Reviewer comments:

Reviewer #1

(Remarks to the Author)
GENERAL COMMENT
In this study, Kaneko et al. describe concomitant measurements of turbulence and nitrate along a transect across a sill in the Tsugaru strait, and show elevated levels of turbulence and very large nutrient fluxes (by ocean standards) associated with divergent flow around the topographic feature. They propose that the nutrients fluxed into the surface layer are advected offshore fueling intense primary production in the Tsugaru gyre. They test their hypothesis by assessing the correlation between downstream chlorophyll and flow divergence at the sill, as measured by a surface radar. This is in my view the most interesting achievement of this paper, since it is normally difficult to track the effect of turbulent mixing on primary productivity, which is probably enabled by the very intense fluxes. As such, this is a very interesting study and deserves publication. However, I have a series of major concerns about the research/manuscript that would require some effort before I can accept publication. I outline these in detail below, together with some other minor comments and suggested edits but in general my major points revolve around these issues:

-while the correlation between flow divergence and chlorophyll downstream is very promising, I feel the analysis is still quite shallow and leaves many questions open (some of which I detail below). It would be interesting to understand better the spatio-temporal dynamics of the flow divergence before going further. From Fig. 3 it appears that divergence is highly variable in space and in very short time scales. It left me wondering how the biological response couples with such high frequency fluctuations and whether there are lower frequencies as well that are dominating the divergence variability (I think I am calling for a frequency analysis here). Another key issue, is that no lagged correlations were investigated, and it appears logic to me that there must be some lag between the nutrient supply at one point, and the biological response downstream. Is the flow that fast that this is not relevant? Maybe this is what the authors intend to convey in their last paragraph but it wasn't entirely clear.

-I feel the conclusion in Lines 122-124 that the diapycnal nutrient flux is visible in the nitrate distribution is not entirely convincing as it is presented. I am not saying is not happening, it probably is, but the data could be presented in a more compelling way.

- I find the text a bit confusing in many placing and needs substantial improvement for clarity. I have made a number of suggestions on that regard below.

- There is a very nice discussion on how the enhanced turbulence impacts the productivity in the Tsugaru gyre, but I feel that, in order to publish the manuscript in Nature Communications, further efforts are done to highlight the broader relevance of the paper. There are a couple of very general sentences in the abstract and conclusions, but I feel the message could be made stronger with a more focussed dissertation: highlight more regions that could experience similar dynamics and discuss their relevance for the large scale production of subtropical gyres, e.g. highlight the disproportionate contribution of the Tsugaru gyre to carbon export in the subtropical Pacific with respect to surface area. And also, in the abstract you mention sustainability and marine conservation. This sounds a bit too broad, unless you mention long term changes in the current across the strait and the importance of monitoring it or something like that.

- Some more details would be beneficial about some parts of the methodology

DETAILED COMMENTS

Line 21-24: “Nutrients transported vertically in specific hotspot areas that reach the surface can be conveyed downstream by background currents, thereby augmenting surface biological production over a wider expanse, including robust currents traversing seamounts.”, sentence a bit convoluted/confusing. Suggest rewritting
Line 32. Suggestion: “A surface divergence field” -> “a divergent surface flow field”
Line 34. Suggestion: “Formation area” -> “area”
Line 36. Last sentence is a bit too general and lacks real substance. It would be OK if you explored further the implication for ecosystem management in the manuscript, but not really justified at the moment
Introduction. The first paragraph sets up the general relevance but it entirely lacks of references. Some reference would increase the credibility
Line 52. “[...] the Tsugaro gyre” could be added at the end of the sentence for clarity
Line 61. “higher distribution” -> “higher values/concentration”?
Line 68. In the paper you report nitrate flux in mmol/m2/s, but here is per day. It would be good to make the units more uniform to make comparisons easier
Line 95. “the similar localized increase”, unclear phrase
Line 103 and after. “the isotherm”, which isotherm? All isotherms? In general lines 100-112 seem a bit confusing to me, they could benefit from some polishing
Line 116. It is confusing that you have focused on the temperature description above, and here you emphasize comparing nutrients with density instead of temperature. It would be simpler to pick one, since, I guess, they must be closely related
Line 122 – 124. It is hard to verify this conclusion by looking at the nitrate plot here, since nitrate is not shown together with density, so it is difficult to tell whether there was a significant impact of diapycnal flux on nitrate concentration. Further, “similar depths displaying different nitrate concentrations does not necessarily imply a diapycnal flux, if isopycnals are not flat. You could modify the plots slightly to get the message across more effectively. Perhaps adding a subplot with nitrate+density could help. I also suggest (see below), maybe putting the last panel in an independent figure. Your conclusions may be reinforced by performing some along stream nitrate budget / advection diffusion balance: knowing the flow speeds and the nitrate evolution along the stream, you may infer a diapycnal flux that you could compare with your measured flux. I am thinking of something along these lines: https://doi.org/10.1029/2007GL032785. I am not sure if this is going to work (possibly not, due to unsteadiness), but may be worth having a try.
Line 128-129. This sentence is somewhat repetitive
Line 131. The reporting of the flux values is confusing. Try expanding the sentence.
Line 134-139. You could maybe explain to the readership of NC what are the implications of the existence of a hydraulic jump. It may not be obvious for some of your biologically-oriented readership.
Line 142. I think you should say that the HFR helps you support the assumption that the zonal divergence is representative of the three-dimensional structure of the float.
HFR divergence analysis. Fig 3 depicts very substantial temporal variability of the flow divergence. I am not very familiar with the HFR, it would be good to know how confident you’re with this patterns and what is the instrumental noise level for their detection. If they are robust, you could comment a bit on the significance of the short term variability. Before proceeding to the correlation with chlorophyll it would be desirable to better understand the spatio-temporal dynamics of the flow convergence/divergence: how flow convergence varies in time and space and how consistent and persistent are the patterns described during the cruise. Maybe some EOF or time-series analysis could be presented, in the main text or as SI. It would be useful also, some discussion about what drives those changes.
Lines 158-161. Sentence not clear. The link between radar flow and nutrients is not obvious. Also, what do you mean by the "transport of nutrient supply"? Do you mean the transport of recently upwelled nutrients or something on this line?
Correlation analysis. This is a very interesting promising thing, but I think it should be explored in more depth. First thing, have you tried lagged correlations? I am surprised of this direct correlation with no lag, when your results imply that there is an advective time-scale for this response (the time it takes for the waters with the upwelled nutrients to reach a certain point). In general, I feel that there is a lack of understanding of the underlying spatio temporal dynamics of the physico-chemical coupling. Would something like a wavelet coherence analysis help? Maybe it is too complex. I would encourage the authors to provide some more insights with the approach they consider most useful. This analysis would strengthen conclusions like those in sentences 178-182. Also it is unclear why the convergence zone shows no correlation, I would assume that the convergence-divergence patterns are linked to each other. Could you expand a bit on that? If panel b is not helping you, you may as well dropped, but now that you showed it I am intrigued by it.
Line 183. Confusing sentence. Turbulence does not “uptake” nutrients, it supplies nutrients which are then taken up by plankton.
Lines 185-190. I understand what you mean here, but I think you could use a more direct language to make the point clearer. Something along these lines: “the throughflow advects water that is poorer in nutrient compared with the generally dominant subarctic waters”
Line 192. “uptake” -> transfer or supply
Line 198. Not sure if “carbon flux” is an accurate term here, I would rather say it this the new production of organic carbon that could be sustained with the influx of nutrients (and will potentially become a downward flux of organic carbon)
Line 202-203. you may as well make a, b values explicit there, but it would be important also to say where this model comes from and whether is known to apply locally
Line 205. “about daily advection” is unclear, maybe something like “though horizontal advection over the course of one day”
Line 296. From “reproduction” does not make sense to me. Do you mean regenerated production?
Concluding remark and relevance. The mention to a single other place in the globe feels a bit insufficient here for a generalist journal. Would it be able to expand a bit on the broader relevance? E.g. showcase other locations with potentially similar structures?

-Figure 4. You show potential vorticity contours there, but I can recall seeing a reference in the text as to why those are there. I guess is to highlight the gyre location. Make sure that this is explicit somewhere.
Methods
- You mention that you followed a drift sampling strategy, but without a more detailed description it is not straightforward to guess what it exactly means and what are the advantages of this approach for your study. You could expand on the sampling strategy a bit
-Line 219. What do you mean by modifications in latitude
-Line 221-223. unclear, did you take surface water samples to assess the drift in the SUNA sensor? How did the sensor performed? It would be good to have some more information. Also, could you use these samples to reinforce your message of nutrient upwelling on the lee side of the sill?
-Lines 124 onwards. Some more details would be welcome. How many profiles? How many shear sensors have you got?
-Nitrate density – fits. Do you do the fitting for each profile, or all profiles combined? In the main text you suggest significant nitrate changes along isopycnals, how does that impact this estimation? (probably not a lot, but just checking).
-SADCP. Also some more details could be useful: What was the configuration, in terms of bin size, segment averaging etc?

Reviewer #2

(Remarks to the Author)
This study makes excellent use of a dataset comprising of 1) long-term high frequency radar measurements of surface currents and divergence and satellite measurements of chlorophyll a together with, 2) shipboard measurements of turbulent energy dissipation to reveal that the wide expanse of chlorophyll a within the Tsugaru Gyre, is tied to the extremely high values of vertical diffusive nitrate fluxes caused by the presence of a Shiriya Spur seamount in the Tsugaru Strait. On the basis of statistical analysis the authors were able to show that there was a strong correlation between long-term high-frequency radar measurements of surface divergence downstream of the sea mount and long-term satellite measurements of chlorophyll a. Although the measurements are high-quality, and the findings interesting, I couldn't find anything significantly novel in this manuscript that would merit its publication in Nature Communications. There have been several previous reports on the role of seamounts from elsewhere as contributing to areas of elevated biological productivity. From within the Tsugaru Strait, some of the authors of this manuscript as well as others have shown that presence of sea mounts are responsible for excessive increases in turbulent energy dissipation rates along the path of the Tsugaru current and consequently nutrient injection into the euphotic column and the elevated chlorophyll concentrations downstream in the Tsugaru Gyre. Perhaps the only novelty in this study is the strong correlation between surface divergence immediately downstream of the seamount and surface chlorophyll a concentration. Despite this interesting observation, I found the discussion of the results very weak, in particular, the few lines addressing the lack of correlation between surface divergence upstream of the Shriya Spur seamount and sea surface chlorophyll in the Tsugaru Gyre seemed cursory at best. Although it seems clear that stratification downstream is essential to phytoplankton growth. I also note that the discussion lacked any mention of the potential role of the anti-cyclonic Tsugaru Gyre in accumulation surface chlorophyll. Finally the section describing the extrapolation of shipboard nitrate fluxes over the region of intense divergence to estimates of carbon fluxes, and the estimates of satellite measurements using surface chlorophyll a measurements to be over-simplistic. For a journal of this repute, I would expect a more rigorous evaluation of both carbon fluxes and net primary productivity especially using an approach that views this hot spot of chlorophyll a as a continuous algal culture system. which is receiving pulses of nutrients continuously that sustains the anti-cyclonic Tsugaru Gyre's elevated chlorophyll a. If the authors intend resubmitting, they should restate as to what makes their results different from those reported for other seamounts, what makes the findings in this manuscript different from those reported earlier for the Tsugaru Current, the role of the Tsugaru gyre and stratification in the build up of chlorophyll a

Reviewer #3

(Remarks to the Author)
Review of the manuscript "Vigorous vertical mixing supports biological production in the mesoscale Tsugaru Gyre"

The paper presents a case in which observational data were used to suggest that during the summer, when the Tsugaru Strait throughflow intensifies, turbulence over the Shiriya Spur and the associated nutrient supply rate significantly increase. The supplied nutrients are then transported downstream resulting in enhanced primary production across a broader region within the Tsugaru Gyre. The study highlights the critical role played by the divergence field in nutrient supplies and the transport mechanisms within the Tsugaru Gyre. Since this understanding holds great importance for sustaining fisheries and marine ecosystem conservation, I believe the broader scientific community would benefit from this report including oceanographers, fishery scientists, etc. My comments are as follows.

1. The presentation of results requires substantial revision to enhance clarity and consistency. One prominent issue is the inconsistency in the use of terminology. The terms "rear of the sill", "back of the sill", "behind the sill", and others, were employed without clear specification of whether they refer to the same concept or different concepts, and this causes confusion. To make the research more accessible to a broader audience, I would recommend using simpler and more easily understandable terms, such as "east of the sill." The headings in the results section as well should offer clear and concise descriptions of the content being introduced (please refer to the details).

2. Furthermore, the discussion is notably lacking. While the research introduces several critical figures and concepts, it falls short of providing an in-depth discussion beyond that of correlation maps, which are insufficient to effectively elucidate the proposed mechanisms. The advection of nutrients is controlled not only by surface currents, but also by factors such as phytoplankton assimilation rate, etc. The concept of the Froude number and its association with the idea of an "internal

hydraulic jump" was also introduced. However, no adequate discussion of what these concepts mean in the context of this study was given. A more in-depth discussion of these critical elements is necessary.

3. The methods section lacks essential details. For instance, it is unclear which type of correlation analysis was conducted to examine the relationship between chlorophyll-a and the surface velocity divergence. Moreover, the treatment of chlorophyll-a data remains unspecified. Satellite-derived chlorophyll-a is prone to missing pixels, often due to cloud cover, especially at the temporal scales used in the correlation, but it was not addressed how this was handled in the correlation analysis.

4. The quality of the observations was also not thoroughly discussed. For instance, while calibration data for nitrogen measurements was collected, the paper omits its presentation. Lines 238-239 mention the adjustment of nitrate profile offset with reference to bottle data, but it lacks clarity regarding why this adjustment was necessary and how it was carried out. Probably this section could benefit from a clear separation between data and methods.

Details:
21-24: the sentence needs to be improved to simplify the flow.
95: It does not follow from the above statement that the high chlorophyll-a was caused by turbulent mixing.
110: "...gradually shoaled,..."?
111-115: The sentence needs revision to improve the flow and clarity. I think the point being made is that, “while a similar phenomenon has been observed off Cape Tappi, it is noteworthy that the magnitude of this phenomenon was significantly greater in the case of Shiriya Spur”?
112: It would be more precise to use terms like "west of the summit" or "east of the summit" instead of "behind" in this context, as the latter term is unclear and ambiguous.
119: Near the topography?
125: levels of vertical nitrate flux were immediately behind the sill?
132: In a geographic context, using cardinal directions namely north, south, east, and west is indeed more intuitive and specific compared to the term "rear," which can be vague and open to interpretation. So, to provide clear and unambiguous geographical references I’d advise to stick to the above cardinal direction.
137: How significant was this difference from 1? Notably, this number exhibits a decrease in an eastward direction, coinciding with the areas where intense mixing was observed. It would be beneficial to provide a brief explanation of how it is derived and offer additional context, as it plays a crucial role in supporting the claims made in this paper. Later in the methods section, the paper describes its calculation in more detail, but a concise overview and context would enhance the reader's understanding of its importance.
139: What does “internal hydraulic jump” mean in this context?
147, 166: Should be revised to provide a clear and concise description of these paragraphs.
183: What is meant by the “concept of nutrient uptake” in this context? Typically, it describes the process in which microorganisms, such as phytoplankton, absorb nutrients from their surroundings to facilitate growth and metabolic activities. However, it appears that this definition may not apply here.
195: I would suggest removing the term “back of the sill”. The given longitude and latitude ranges provide more precise context than the above term, which is ambiguous and can lead to potential confusion.
198: It's important to make a distinction since the carbon flux referred to here is potential. The estimates are based on the rate of nitrogen supply and not the actual nitrogen uptake rate by phytoplankton.
199: What is the relevance of the speed or mass advection in the estimate of primary production? It appears only chlorophyll-a concentration is important. It would be more intuitive to clarify what the coefficients “a” and “b” are and how they are obtained.
203: Providing context of background concentration
216: Was water sampling performed only “at the start and at the end”, or was it conducted “from the start to the end”?
222: Surface water samples collected at approximately every alternate location were primarily used to calibrate the nitrate sensor? What other parameters were measured during these water samplings, and at what frequency were they collected?
227: “The calculation method...” for what?
234: Like the other parameters in the equation, Γ should also be accompanied by a brief description.
245: Please provide a context as to why this approach was adopted. Adding the reason help readers understand the motivation behind the methodology.
249-251: The sentence contains a lot of information and is difficult to follow. First, “flux” here refers to nitrate flux? There is no mention of nitrate correction until “corrected nitrate.” What correction is this referring to? Second, was the 30-m running mean filtering applied along the entire transect within the surface region? To improve clarity, please break down the sentence.
253: Oh, so the filtering was along the nitrate vertical profile!
254: What does “downstream of the topography” refer to or mean in this context?
259: Briefly explain what the “correction for misalignment” does. Additionally, it is not clear how the “percent” was estimated in this case.
280: 8-day resolution data was used in Fig. 1c. What was the purpose of 1-day resolution data?
292: What would be considered the "observed" bottom layer in this context? Would this be the bottom layer to which a measurement instrument descended to or simply the water depth.
294: The manuscript referred to appears to be about the “light-saturated photosynthetic rates” and not primary production. Please verify. Also, probably the coefficients were derived in a specific site. Are they applicable to the region of this study?
519: What “reference numbers” are those?
520: indicate the nutrient flux in ... regions?
527: What do the vectors indicate?
532: The criteria for defining the spatial average of the surface divergence is not clear. The area average boxes have

different sizes. How were they defined? Probably a Hovmöller plot of divergence and chlorophyll-a would provide additional insights.

Version 1:

Reviewer comments:

Reviewer #1

(Remarks to the Author)

The authors have made a very substantial effort to improve the manuscript, including a series of new, insightful analyses, as suggested. I deeply appreciate such an effort. The manuscript is on the right track to become much stronger. However, one of the key results of the manuscript (the correlation between localised at the Shiriya spur and productivity in the Tsugaru gyre) is now less clear cut than it appeared to be with the previous, much shallower analysis. There was a substantial effort to still make this point, which is now spread across 6 figures (3 in the main text and 3 as supplementary). The most prominent feature in those figures seems to be the high correlation at 1 day lag in Figure 6, which is (perhaps deceivingly) at odds with the “advective” hypothesis that the authors are putting forward. The answer to this apparent contradiction may be there already but is diluted in the wealth of figures and information. I would recommend taking a step back and thinking about the best way of showing their point more succinctly and convincingly. There are other relevant points I list below. I also attach an annotated PDF with minor comments.

Overall, my perception is still that the manuscript is of high quality and makes an interesting and appealing point. It could eventually reach Nature Communication standards but getting to that point still requires some work to strengthen the main conclusions and the clarity of the arguments.

It is not entirely clear/explicit why you can assume a direct correspondence between the elevated mixing rates during the drift experiment and the flow divergence downstream of the spur (end of page 8). You should provide a more straightforward mechanistic connection between the two. It may relate to the hydraulic jump’s dynamics, but it would be good to see an explicit argument. This weakness could be solved by adding one or two sentences explaining the dynamics that justify the link.

A second weakness pertains to the main idea that mixing of nutrients at the Tsugaru Strait supports productivity over the adjacent gyre. The authors have made a significant effort to justify this point, but the wealth of information becomes a bit overwhelming, and the point is somehow lost. The paper would benefit from a synthesis effort. Right now, you have 3 main figures to make this point, plus several supplementary figures. This feels like too much, and the ideas get diluted.

If I understood correctly, the advection time-scale seems to be too long (~ 1 week) to explain the strong correlations at zero-lag between localised divergence and large-scale chlorophyll distribution in the wider Tsugaru Gyre area (the authors acknowledge that around line 332). If there was an advective connection between divergence and productivity, progressing anti-cyclonically along the gyre’s rim, I would expect to see some evidence of that in your correlation maps in Figure 6, 7. There is some of that in Figure 7, but I feel it could be more convincing. Perhaps, I am distracted by the prominent signal indicating a wide-spread short-lag coherence in Fig 6a. This would rather suggest a common larger-scale driver at ~daily timescales (tides? - can you comment on what this driver may be?). I understand that then you decide to focus on sub-inertial timescales, so perhaps this is your point. If that is the case, you could focus more on this idea and strengthen your arguments. Since the short-term coupling appears more prominent in Figure 6, you need to convince the reader that the coupling at subinertial frequencies matters as well or matters more. For example, you could estimate how much of the chlorophyll variability is contained at subinertial frequencies and how much of it is explained by the covariance with divergence. Other points of improvement are explaining better what point Figure S6 makes and adding some significance contours to the wavelet analysis in Figure 8 and discuss how this analysis compares with the lag correlations in Figure 7.

You could explain up front why you focus on the period August-September. Is it because this is the time of the year when the gyre / the localised divergence are active? Is nutrient limitation in the gyre stronger during this time of the year, so you see a clearer signal when nutrients are supplied? Maybe expanding on that could help you highlight the importance of localised mixing, e.g. if productivity in the gyre is higher than expected for this time of the year. The addition of productivity estimates is a good added element of the revised manuscript. If you could show that productivity is higher than expected for the region in times of the year when the divergence is more active, that could make your argument stronger.

You could elaborate more in the connection between the sub-inertial variability and your previous study on the Tsugaru Warm Current position (line 245-247). Can you demonstrate/illustrate this somehow, with a figures

Line 90. A brief explanation of what the drift sampling strategy means in practice, and how close it is to Lagrangian, would

be useful in the main text.

Lines 119-124. An important feature seen in this figure is that chlA concentrations are maximum east of the spur. You could use this feature to strengthen your argument.

Page 10-11. You should explain why you picked 1 day and 7 day lags in Figure 6.

Figure 7. Correlation subplots have different limits for their y-axis. This makes interpretation unnecessarily complicated.

Supplementary Figures 4,5. I struggle to interpret these 3-dimensional plots. Given that this is is SI and you have plenty of space, I would suggest showing some slices as subplots rather than a 3d figure.

Discussion. You compare the areal nutrient supply at the spur with uptake in the gyre and show that the former is higher. However, enhanced fluxes presumably occur over a small area, while the uptake occurs over a much broader area. When scaled over the respective areas, do supply and uptake still match?

General: although the language has improved, I still feel that some parts of the introduction could be more clearly written (see annotated PDF).

(Remarks on code availability)

Reviewer #2

(Remarks to the Author)
General comments: I would like to commend the authors for incorporating many of the suggestions made by all the reviewers of the previous version of the manuscript. The changes made, including the addition of new datasets and analysis have certainly helped improve the robustness and validity of the results. The manuscript is still in need of editorial changes for improving its clarity.
I have tried to help as best as I can by making many editorial suggestions which can be found in the annotated version of the manuscript attached with my review.

Specific comments:
1) In my previous review, I had requested the authors to comment on the novelty of their work, specifically outlining what sets their work apart from the previous studies by Tanaka et al. 2021 within the same region, and by others who have already described the importance of underwater seamounts and physical mixing mechanisms triggered by these mounts and their role in facilitating nutrient fluxes into and the growth of phytoplankton in the euphotic zone. The response that the studies of Tanaka et al. (2021) were undertaken at a location 100km away from the present study seem to me at best lame. I would have liked more details - for instance whether the mixing, nutrient enhancement, divergence mechanisms differed or were not reported in Tanaka et al. adequately.

2) Although I had not pointed this out earlier, and for the sake of their readers, I would like the authors to provide more details as to whether the area of enhanced chlorophyll downstream of the TWC is a year-round feature? This is especially important given the importance of this process in sustaining this productive ecosystem. I assume that this mixing is a year round feature, albeit with slight changes in strength given the uni-directional flow of this current through the Tsugaru Strait and perhaps slight changes in current velocities over the year. If this is not a year round feature and the direction and strength of the TSW flow does not change much, what is the reason for seasonal variability of Chl-a within the gyre. Is it related to weaker divergence, light limitation, grazing pressure, lack of water column stability etc?

As I have stated above - I have made several edits in the annotated version of the manuscript. I would like the authors to have a look at edits to the abstract which currently lacks clarity and conciseness. I also made some edits to the introductory section which would benefit from clear statements as to the motivation for this study. In the other sections of the manuscript, I have also made some edits and have made some notes about the figures some of which appear to be labeled inadequately

(Remarks on code availability)
I have not had a chance to look at the code, but I believe it has been submitted.

Reviewer #3

(Remarks to the Author)
Comments on "Vigorous vertical mixing supports biological production in the mesoscale Tsugaru Gyre”

The authors have shown responsiveness to the comments provided by this and other reviewers. However, I remain concerned about the lack of structure and depth in the discussion section. This weakness persists in the current version. Additionally, the language requires further consideration.

Key aspects covered by this manuscript include (1) nutrient supply via turbulent mixing, (2) nutrient transport to the downstream Trugaru Gyre, and (3) enhanced productivity resulting from this nutrient supply. However, these elements, aside from correlations, lack in-depth discussion. Even the correlations themselves require further interpretation to discern their significance. I recommend enhancing the discussion to address these points more comprehensively, particularly focusing on the implications of the observed positive correlation between nutrient supply and chlorophyll-a levels. Additionally, the insights provided by the lag correlations are noteworthy and deserve further elaboration.

I'm uncertain whether the newly added wavelet figures add significant value to the main text or if they might be more suitable for supplementary materials. Additionally, there seems to be an abundance of introduced materials without subsequent discussion. It's crucial that the discussion begins with an exploration of nutrient supply and transport mechanisms, followed by an elucidation of their effects on biology. This sequential approach would enhance the coherence and depth of the manuscript.

Details
L25: "…the impacts of localized nutrient supply on…?"
L27: add comma after Pacific Ocean.
L27-29: a lot of unnecessary words that can be removed to give space to more critical information in the abstract.
L34: the significance of the study can be made more impactful.
L52: on long-term, how? Maybe include references. I thought this study was trying to demonstrate this control over broad-scales?
L88: This should mean surface water mass?
L92: "A previous study" can be name since it is repeatedly referenced.
L109: what timescales?
L179: since VGPM is referred to by name, the other model too can be referenced by name.
L357: very long sentence and difficult to follow.
L396: another long sentence.
L562: Since no significant differences were found, I would suggest to keep it simple in the manuscript and show the absolute value rather than ratio. Also, the y-axis should have the same scale for all subplots. This makes comparison easy and straightforward.
L611: area cannot support a concept.
L626: extrapolated vertical mean?
L627: range -> flux? This comparison is a bit strange. Is the nitricline in the same depth range as the euphotic layer?

(Remarks on code availability)

Version 2:

Reviewer comments:

Reviewer #1

(Remarks to the Author)
I want to than the authors for the efforts they devoted to address my comments and craft a very thorough response letter and a much improved manuscript. The authors have now introduced new analysis, based on modelling efforts, to clarify my questions and I am happy to say that they dissipated my concerns, which regarded the advective time-scales of the nutrient signatures around the gyre. I would like to praise again their effort to introduce new clever analysis. I am now happy with the science, it is very interesting and the arguments are now well supported by different threats of evidence.

I still have some substantial editorial recommendations to improve the clarity and conciseness of the manuscript. I list the main points here, but I also attach an annotated PDF with more details and additional editorial comments (I am not listing those comments separately here for the sake of returning this review more quickly, I hope the authors don't mind that).

The clarity of the manuscript is much improved now, however, I have the feeling that some parts of the manuscript may be a bit difficult to understand for a reader that hasn't have access to the previous versions of the article. I am struggling to judge the clarity with full confidence, given that I read already the previous versions already, but I'd have some concrete suggestions:

- Rewrite the first paragraph of the introduction, with a clear, simpler message, perhaps following this thread: (1) Mixing is key to supplying nutrients to plankton communities; (2) mixing is enhanced at topographic features (cite some examples); (3) the effects of topographic mixing on productivity remain poorly understood due to the sparsity of observations and the

difficulty of linking small-scale mixing measurements with larger-scale productivity; (4) here we address this issue for a case study region.

- I struggled to follow section "Spatial and temporal changes in the surface velocity field around the Shiriya Spur", although it is really promising. Try to bring the focus back to your main messages here, and guide the reader at the start, explaining why you look at different scales of variability (in particular the subinertial variability of the jet. This variability is key in subsequent analysis but I don't feel it has been introduced properly) and why this matters for divergence/nutrient supply. I would invite the authors to reconsider whether all the information in Fig. 5 is really needed in the main text. I am happy if they decide to leave everything, I am just mindful of conciseness and clarity.

-Lines 362-371. I also invite the authors to reconsider whether this paragraph adds something new and relevant to the main text, or it can be dropped.

-For a series of other editorial recommendations, see attached PDF.

(Remarks on code availability)
I have quickly checked the code and the data in a previous, all seems fine in principle

Reviewer #2

(Remarks to the Author)
I am very happy that the authors have incorporated many of the changes recommended at every stage of the review process. The result is that they have a very very strong and compelling manuscript " "*Vigorous vertical mixing supports mesoscale biological production in the Tsugaru Gyre* The work holds substantial significance for both the field and related disciplines" that meets the high standards for publication in Nature Communication. The work holds substantial significance for both the field of physical and biological oceanography and is richly deserves to be published

(Remarks on code availability)
I do not have the time right now to go through the code

Reviewer #3

(Remarks to the Author)
Comments

Overall, I feel the text has improved, but I strongly encourage the authors to reconsider the narrative, particularly by reducing the reliance on general model outputs. The core issue, as noted in the previous round of reviews, is that the story is becoming less clear. The combination of observations with general model results is not compelling. If the model outputs are included, they should be more focused on explaining specific events, such as in the case of Fig. 7. Several figures could be streamlined to enhance focus, for instance, simplifying Fig. 1 and moving several others to supplementary material. A simple and clear narrative can be achieved by focusing on parts of Fig 1 and 5, as well as Figs 3, 6, and 7. The remaining figures add too much material without significantly enhancing the manuscript.

I also notice a tendency to force connections between observations and phenomena without providing clear, logical explanations (e.g., L168-171), which weakens the manuscript. As I mentioned in the previous review, the section titles need to be more informative to help readers understand the content. For instance, "General situation at the time of the survey" is vague, general situation of what? A more specific title would improve clarity. Furthermore, from the current title, one would expect it detailing the conditions associated with the survey. Instead, it discusses chlorophyll-a enhancement and the implied impact of vertical mixing on nutrient supply near the spur. This makes the section feel misplaced. As such, it would be better positioned after "Turbulence over the spur and associated diffusive nitrate flux", which details the mechanisms of nutrient supply.

Overall
-Text clarity remains an issue: despite revisions, convoluted passages remain and should be streamlined for better flow. Try using shorter sentences to isolate ideas. I am inclined into thinking text clarity is not merely a language problem but more about the narrative focus.

-Too many figures and study focus remains unclear: the number of figures have increased considerably but they did not significantly improve the clarity or focus of the manuscript, particularly in balancing observation-based findings with general model outputs. Fig. 7 from model outputs is useful, but other figures dilute the focus.

-Section order and titles: The structure needs revision for better flow. The results are mixed with discussion and this adds to the confusion. Section titles should be more specific to guide the reader.

-I suppose high-resolution current data and turbulence measurements should be the main focus of the manuscript.

In summary, I recommend focusing on simplifying the manuscript's narrative, rather than expanding it with new material.

Details

L27 "outputs derived from an ocean data assimilation model" makes the sentence dense and shift focus away from the main point. While I agree that model simulations add valuable context, I understood that the primary focus of this study was on field observations, in particular HFR currents and turbulence measurements. The simulations should serve as supporting materials, otherwise it becomes unclear whether the study is centered on modeling or observations? It would be reasonable to develop a specific modelling study to test a given hypothesis, however, the use of model results here only suggest that it is to support the general pattern of the ocean.

L46: convoluted and contradicting sentence (functional interplay vs. may exert)

L50-1: What is the point of the sentence? Reviewer #2 of previous versions has repeatedly pointed out this weakness. I'm not entirely sure it's accurate to say "this interplay is poorly understood". From this draft, it seems the poorly understood aspect is how this interplay supports non-local, large-scale biological enhancement, which this study aims to demonstrate (L56). Again, elaborate on the novelty of this work.

L54: Another convoluted text. I struggle to get the point. Is "this system downstream" referring to the nutrient supply source region or the environment downstream receiving the nutrient supply? If it is the latter, the "environment downstream" would be more appropriate than "system downstream". What is meant by "continuous phytoplankton culture".

L55: The placement of this sentence makes it confusing and unclear. It might be beneficial to move it before L53 or rephrasing it to suggest that similar analyses in other regions could be insightful. Otherwise, what is the purpose of this study? More specifically, "pivotal processes" is quite ambiguous. What specific processes are being referred to?

L60: For context, it would be beneficial to add the magenta line to (e) or make (e) and (f) share the same scale on x-axis.

L69: What is the significance of the simulation figure in the main text?

L74: In Fig. 2 the bottom panels are sections from the top, but the horizontal range is different, why?

L77: This statement is quite ambiguous, echoing the initial concerns raised by the reviewer of the previous version. I believe the authors intend to say "normalized by its pixelwise standard deviation", as is evident from the computation code. Additionally, from Fig.1e it can be inferred that the data follows a log-normal distribution. In such case, the ratio of geometric mean (10^mean(log(data)) to the standard deviation in the log space (std(log(data)) would be more accurate metric to produce Fig 1e and possibly the spatial pattern will be different.

L82-91: I tried to revise the paragraph to make more sense of it but the flow and connectivity of the sentences is not clear. Further, how does "Fig. 1e indicates that there may be a nutrient supply point at the Shiriya Spur that more directly impacts biological production within the Tsugaru Gyre"?

L93: Again, it is not clear what is the intended meaning of "continuous phytoplankton culture".

L107-9: I do not understand why model outputs would be needed to capture the general pattern already available from satellite observations.

L110: In the previous revision, there was a comment about the difficulty in comparing sub-figures with different scales. I notice that in Fig. 1a-b, the x-axis range differs from that of the bottom Fig. 1c-d. This hinder readers from making a simple and straightforward connection between the surface (top figures) and vertical profile (bottom figures). Aligning the x-axis range across these sub-figures would facilitate easier comparison and clarity overall.

L119: "...approximately 41.6°N–41.7°N, 141.5°E–141.7°E" -> a marker indicating this location on Fig. 1a-b would improve the presentation.

L135: Lines 639-644 are the only place where CBEM is discussed in the Methods section. I suggest moving this information earlier in the text to provide readers with the necessary context without requiring them to search for details presented in just six lines, especially because the following text provides more detail for the VGPM.

L129-49: I find it difficult to follow the text here. The introduced PP values seem inconsistent with observed summer estimates. So, what is the point of including these values? Further, paragraph L147 abruptly shifts to a different topic, leaving the previous PP discussion unresolved. It is not immediately apparent how the Shiriya Spur connects to the idea of sustained nutrient delivery that bolster production within the Tsugaru Gyre. Please help the reader by making the connection and logical progression of paragraphs' ideas more seamless.

Additionally, does "This estimate of PP..." refer to the VGPM, or to both VGPM and CBEM? L143 suggests it refers to CBEM. Regardless, it seems both estimates yield similar results However, considering that 16°N–24°N is far south, and KNOT and

K2 are far north, outside the current study area, I question whether these comparisons are reasonable given the significant differences in environmental conditions. That is to say, are the limiting factors for PP similar across all these regions? The text is quite convoluted and should benefit from clarification.

L168: “Therefore,...increase in chlorophyll-a east of the Shiriya Spur implies a nutrient supply near the spur...”? Why and how an increase in chlorophyll-a east of the Shiriya Spur necessarily imply a nutrient source near the spur?

L185: Where to look for dissipation rate east of 141.68°E in Fig. 3b?

L206: "...including the Kuroshio region". Why should the reader care about the Kuroshio region? What specific phenomenon observed there is similar, and how does it relate to this study's findings? Providing this connection would help clarify its importance.

L219: Is Fig.3b the right reference here?
L220: Color should mean contour in nitrate gradient of Supplementary Figure 2?

L376-7: So far, there has never been any mention or definition of subarctic water. So, the text here will certainly confuse any reader readers.

L378: The phrase "The sensitivity to the selected area was also notable." is also vague and unclear.

L380: It is better to be more specific about "high-frequency component".

A few editorial suggestions
L1: Topographically-driven vigorous vertical mixing supports sustained mesoscale biological production in the Tsugaru Gyre?

L30: These processes are caused by the presence of a distinct topographic feature—an underwater seamount—within the path...

L35: The results of this study emphasize the importance of re-evaluating nutrient supply processes in respect to submesoscale interactions with underwater seamounts for large-scale enhanced biological productivity.

L41: Among the different nutrient supply mechanisms to the euphotic zone, tidal-driven flows over seamounts are noteworthy for their enhanced vertical mixing.

L44-9: “...thus affecting nutrient supplies from greater depths” -> thus positively affecting nutrient supplies from greater depths to the surface?

Additionally, nutrient supply through horizontal advection by mesoscale eddies has been demonstrated to influence phytoplankton distributions.

The functional interplay among topography, strong currents, and eddies exerts control over chlorophyll-a distribution on a long-term and broad-scale through continuous or repeated nutrient supply processes near seamounts and subsequent strong horizontal advection.

L52: a potent -> a strong

L50-8: In this study, we investigated the interaction between a strong current and local topography, revealing that this interplay fosters a vigorous nutrient supply within a confined area at the outlet of the Tsugaru Strait in the North Pacific, Japan. We suggest that this steady supply of localized nutrients supports sustained phytoplankton growth in the downstream environment. Consequently, studying these mesoscale processes (O(100) km) can provide significant insights into ecosystem dynamics while being essential for establishing key observation and monitoring sites.

L59: Move this paragraph right after L50.

L65-9: The seasonal variations in the horizontal flow path are accompanied by significant changes in the vertical outflow characteristics over the topographic feature known as Shiriya Spur, located near 41.4°N–41.7°N, 141.5°E–141.6°E, and extending northward from Cape Shiriya (Fig. 1e).

L76-9: The long-term (2014-2022) mean distributions of surface chlorophyll-a concentrations in August and September,

normalized by its pixelwise standard deviation, revealed...

L82-91: The study by Tanaka et al. reported a vertical turbulent nutrient flux over
a sill located at approximately 140.35°E (off Cape Tappi region) in the western part of the strait (Fig. 1f). Their reported flux of 1 mmol N m^2 day^{-1} [O(10^{-5} mmol N m^{-2} s^{-1})] (at the surface?) is significantly greater than values observed in the open ocean. While this nutrient supply off Cape Tappi is substantial, the sustained productivity in the Tsugaru Gyre suggests the presence of an additional nutrient source, given the distance from Cape Tappi. The Tsugaru Current, which passes over the Shiriya Spur, may induce disturbances that more directly impact the biological production within the Tsugaru Gyre. However, the nutrient supply capacity at this topographic feature, the Shiriya Spur, has not been assessed, nor has there been a study linking turbulence intensity (over several hours to days) with large spatiotemporal variations in biological production.

L92-9: To better understand the drivers of the sustained phytoplankton productivity downstream of the Tsugaru Current, we conducted drift turbulence measurements following
an advected water column in the eastern part of the strait, crossing the Shiriya Spur. In this area, continuous surface current velocity data from a high-frequency radar (HFR) system have been collected since 2014. Through a combination of turbulence observations, HFR
monitoring, and with the aid of ocean data assimilation model (JCOPE-T DA) outputs, we suggest that nutrient supply at the submesoscale around the Shiriya Spur sustains biological productivity downstream.

L141-6: The above PP estimates exceed the reported values for summer in the western North Pacific subtropical gyre. However, they are within the range of reported values for the summer to fall seasons in the subarctic region of the western North Pacific.

L159: During the turbulence measurements, conducted on 26th August 2021, the eastward current flowed... Concurrently, relatively high surface chlorophyll-a concentration were observed from the eastern outlet of the strait (approximately 141.75°E, i.e., in the area immediately adjacent to the east side of the Shiriya Spur) to approximately 142.4°E downstream, based on 16-day composite from 21st August to 5th September (Fig.1f).

L208: The magnitude was also approximately 100 times greater than that estimated for the Cape Tappi in the western strait topography.

L229-35: The obtained value for Frl was greater than 1 at both 141.56°E and 141.58°E, where intense eastward flow and an undulating isopycnal surface were observed (Fig. 3e). This suggests the presence of an internal hydraulic jump. Specifically, when the background flow velocity approximates the propagation speed of the internal gravity wave, stagnation and the superposition of internal gravity waves near the critical point ($Fr_1 = 1$) are expected. This condition leads to significant isopycnal displacement, resulting in heightened turbulence due to instability.

(Remarks on code availability)

Version 3:

Reviewer comments:

Reviewer #3

(Remarks to the Author)
Comments

I would like to commend the authors for their responsiveness to reviewer comments and their considerable effort to make the manuscript more accessible. It is now significantly easier and more pleasant to read and grasp the ideas being introduced. The text has clearly improved in clarity and conciseness. While I still find a few sections in need of further revisions, which I detail below, I believe these are minor issues that can be resolved in preparation for publication.

Due to time constraints, I have not separated my commentary from editorial suggestions; please see specifics below.

Specifics

I might be missing the point, but “vertical mixing sustained” and “vertical mixing supports” convey different nuances. The latter seems to better capture the manuscript’s intent to emphasize that “vertical mixing supports” high biological productivity in Tsugaru Gyre.

L32-35 blends general and specific information resulting in a diluted message that doesn't clearly articulating the manuscript's main point. Please consider the following:

“Our long-term datasets revealed a positive correlation between recurring submesoscale divergence related to the topographic feature and the downstream distribution of chlorophyll-a. This finding reinforces the significant role of the underwater seamount in enhancing the productivity of the downstream anticyclonic gyre.”

Integrate L48 into L49 for a seamless flow
“In this study we introduce a case study to address the above issue by investigating the interaction between a strong current and local topography, revealing that...”

L54: Fig. 1b and c show seasonal changes in Tsugaru Gyre characteristics, yet the text discusses “the flow path of the current...”, in this case, the Tsugaru Warm Current. This reference to Fig. 1b and c may be misleading or confusing to readers. Additionally, is the text implying that the Tsugaru Warm Current (Fig. 1a) and the Tsugaru Gyre (Fig. 1b and c) are the same thing? Please clarify the reference to Fig. 1b and c.

L68-80: “To quickly test this hypothesis, we normalized the long-term (2014–2022) seasonal (August and September) mean surface chlorophyll-a concentration (mg m^–3) by its pixelwise standard deviation (mg m^–3) and denoted the normalized data as Ichl (unitless). This index (Ichl) revealed elevated values (>2) within the gyre compared to those outside the gyre (Fig. 1e). Assuming that the chlorophyll-a concentration at each grid is normally distributed during the season, the elevated value of Ichl within the gyre supports the notion of enhanced chlorophyll-a levels with small temporal variation during that season. The Ichl exhibited a horizontal maximum in the region corresponding to the area immediately east of the spur (approximately 41.7°N, 141.6°E–141.7°E; Fig. 1e). This suggests the existence of a nutrient source around the spur that supports stable chlorophyll-a levels, as shown by Ichl.”

I believe the assumption of normality for chlorophyll-a data can be tested and may not be readily accepted by biological oceanographers. It would be beneficial to address this assumption more critically in the text.

L75: “As chlorophyll-a concentrations within the gyre remain above zero during the season” What does this mean? Is there any period when chlorophyll-a levels are below zero?

L86: “...given the distance from Cape Tappi and the spatial distribution of Ichl...”

L106: I believe that including a paragraph to introduce the results would greatly enhance the reader's experience. Currently, there is a significant jump from the introduction to the results, which may disrupt the flow.

L105: “Firstly, we briefly present the seasonal variability characteristics of the Tsugaru Warm Current and Tsugaru Gyre, and associated topographically driven upwelling and surface velocity divergent field, on the east side of the Tsugaru Strait, using outputs from the JCOPE-T DA (Supplementary Fig. 1, with the 2019 output as an example; see the Methods section). This analysis is motivated by the challenges in continuously and repeatedly acquiring the spatial structure of surface currents, including the vertical velocity of the ocean interior. We then introduce results based on field observations using HFR and turbulence measurements. Finally, we discuss the implications for productivity in the Tsugaru Gyre using long-term chlorophyll-a data and empirical estimates of primary productivity.”

Seasonal characteristics of velocity divergence from the JCOPE-T DA model
“The outputs from the JCOPE-T DA model reasonably reproduce the seasonal variations in the current and the formation of the Tsugaru Gyre (Supplementary Fig. 1a and b). For the vertical east–west section of each season at 41.65°N...”

L130: Adding a reference would help support this claim and strengthen the argument.

L133 could be moved after the results of turbulence and nitrate fluxes.

L135-137: This information is redundant and can be removed, as it has already been discussed in the introduction.

L153-156: I am not clear what this statement is trying to convey. It feels too speculative and should be removed from the results. A clear statement of the rationale behind this comparison would enhance clarity.

Alternatively, it would be more reasonable to focus on the estimates of nutrient input resulting from the interaction between the Tsugaru Warm Current and the spur, assessing its impact on primary production during August and September against background conditions. This approach would provide a clearer understanding of the contribution.

L157 feels like the introduction of L169.
L164: “The R/V’s travel distance”?
L180: indicating→associated with?
L200: “...western strait topography...?”
L205: “This result suggests that the nutrient content at the surface on the east side of the spur is greatly influenced by vertical nitrate fluxes.”
L266: “...from→during...”
L906: The concept of normalized chlorophyll-a is becoming unnecessarily complex. The notation 〈chl(x,y,t)〉 is not clear, as the “t” inside the brackets makes it difficult to interpret the average. What exactly does it convey? If we are to calculate the pixelwise mean at a given time, shouldn’t it simply correspond to the original value?
L951: “from” July to November 2021, or for July “and” November 2021?

(Remarks on code availability)
The code does provide a README file. However, I have not installed or tried running the application.

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Reviewer #1 (Remarks to the Author):

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GENERAL COMMENT

In this study, Kaneko et al. describe concomitant measurements of turbulence and nitrate along a transect across a sill in the Tsugaru strait, and show elevated levels of turbulence and very large nutrient fluxes (by ocean standards) associated with divergent flow around the topographic feature. They propose that the nutrients fluxed into the surface layer are advected offshore fueling intense primary production in the Tsugaru gyre. They test their hypothesis by assessing the correlation between downstream chlorophyll and flow divergence at the sill, as measured by a surface radar. This is in my view the most interesting achievement of this paper, since it is normally difficult to track the effect of turbulent mixing on primary productivity, which is probably enabled by the very intense fluxes. As such, this is a very interesting study and deserves publication. However, I have a series of major concerns about the research/manuscript that would require some effort before I can accept publication. I outline these in detail below, together with some other minor comments and suggested edits but in general my major points revolve around these issues:

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R01-G01:

-while the correlation between flow divergence and chlorophyll downstream is very promising, I feel the analysis is still quite shallow and leaves many questions open (some of which I detail below). It would be interesting to understand better the spatiotemporal dynamics of the flow divergence before going further. From Fig. 3 it appears that divergence is highly variable in space and in very short time scales. It left me wondering how the biological response couples with such high frequency fluctuations and whether there are lower frequencies as well that are dominating the divergence variability (I think I am calling for a frequency analysis here). Another key issue, is that no lagged correlations were investigated, and it appears logic to me that there must be some lag between the nutrient supply at one point, and the biological response downstream. Is the flow that fast that this is not relevant? Maybe this is what the authors intend to convey in their last paragraph but it wasn't entirely clear.

Response:

Thank you sincerely for your thorough and constructive feedback. In light of your comments, we have incorporated additional analyses to address your inquiries. We have included details on the spatiotemporal distribution of the flow field (refer to newly added Fig. 3) and conducted a wavelet analysis of the surface current divergence in the area of interest (see newly added Fig. 4). Additionally, we have performed a lag correlation analysis of surface divergence and chlorophyll-*a* (Figs. 6 and 7). We have also included wavelet relationships between this divergence and the area-averaged chlorophyll-*a* concentrations in 18 subregions within the Tsugaru Warm Current (Fig. 8).

The updated manuscript now features an illustration of the mean surface current velocity distribution during the summer period (August to September) from 2014 to

2022. It confirms that the background current (Tsugaru Warm Current) flowing through the Tsugaru Straits consistently exceeds the seafloor topography just offshore of Cape Shiriya as an average state over time. We used this background as the basis for selecting the area in which to calculate the divergence field of surface current velocities (41.65°N–41.70°N, 141.60°E–141.65°E).

The wavelet analysis (as depicted in the newly added Fig. 4) clearly illustrates diurnal and semidiurnal variations in the divergence of surface current velocities, which we believe correspond to tides. Furthermore, the findings suggest that subinertial frequency variations (ranging from a few days to 10 days) tend to intensify during the summer and fall seasons. This phenomenon may align with the north–south variation in the transit latitude of the Tsugaru Warm Current, as demonstrated in our previous study (Kaneko et al., 2022; https://doi.org/10.1186/s40645-022-00509-z). Specifically, the Tsugaru Warm Current typically flows over the terrain on average but exhibits avoidance behavior on the mentioned time scales with periodic north–south shifts.

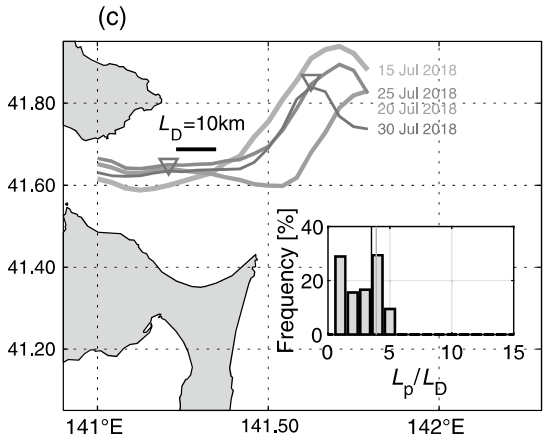

Fig. 9 in Kaneko et al. (2022). Example of shorter-term variation in “the axis” (indicated as lines) of the Tsugaru Warm Current estimated from the high-frequency radar during summer.

Using high-pass filtered data, we further explored the correlation between surface velocity divergence and chlorophyll-*a* distribution. We observed that, in this case, the correlation was notably significant only in a narrow region adjacent to the topography (refer to Supplementary Figure 6). Building upon these results, we theorized that the subinertial periodic variation of the divergence field, synchronized with the advection time scale (approximately one week for a half-circumnavigation), could be linked to the positive correlations observed over a broad area in the Tsugaru Gyre (as shown in Fig. 6a). The detection of remarkable wavelet coherence in subinertial frequencies within the Tsugaru Gyre during August and September (Fig. 8) would be construed as evidence supporting this notion.

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R01-G02:

-I feel the conclusion in Lines 122-124 that the diapycnal nutrient flux is visible in the nitrate distribution is not entirely convincing as it is presented. I am not saying is not happening, it probably is, but the data could be presented in a more compelling way.

Response:

Thank you very much for your suggestion. We have incorporated the information regarding the distribution of nitrate on the isopycnal surface as shown in Fig. 2e.

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R01-G03:

- I find the text a bit confusing in many placing and needs substantial improvement for clarity. I have made a number of suggestions on that regard below.

Response:

Thank you for bringing this to our attention. We have addressed and corrected individual revisions based on the comments provided. Additionally, since the authors are not native English speakers, we have utilized an English proofreading service (AJE, https://www.aje.com/) to ensure that the wording is more suitable. However, we would greatly appreciate it if you could reiterate any points that may still be difficult to understand. Thank you once again for taking the time to review and provide feedback.

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R01-G04a:

- There is a very nice discussion on how the enhanced turbulence impacts the productivity in the Tsugaru gyre, but I feel that, in order to publish the manuscript in Nature Communications, further efforts are done to highlight the broader relevance of the paper. There are a couple of very general sentences in the abstract and conclusions, but I feel the message could be made stronger with a more focussed dissertation: highlight more regions that could experience similar dynamics and discuss their relevance for the large scale production of subtropical gyres, e.g. highlight the disproportionate contribution of the Tsugaru gyre to carbon export in the subtropical Pacific with respect to surface area.

Response:

Thank you for your valuable comments. In response, we have included an estimation of production within the Tsugaru Gyre in the second paragraph of Results section. As previously mentioned in our correspondence with the editor, we have conducted two estimations: one using an empirical equation based solely on surface chlorophyll-*a* concentrations, and the vertical generalized production model (VGPM, Behrenfeld & Falkowski, 1997; https://doi.org/10.4319/lo.1997.42.1.0001) that considers additional factors such as local light environment and sea-surface water temperature. We believe that both estimates are comparable in magnitude and closely aligned. We have also compared these estimations with values from previous studies conducted in other ocean regions, such as the western North Pacific subtropics by Shiozaki et al. (2009) (https://doi.org/10.3354/meps07837) and the subarctic by Imai et

al. (2002) (https://doi.org/10.1016/S0967-0645(02)00198-4). These comparisons indicate that primary production within the Tsugaru Gyre is relatively high. Furthermore, we contend that production within the Tsugaru Gyre can be characterized as stable, exhibiting minor fluctuations from year to year (refer to the newly added Fig. 1d). Therefore, we are confident that we have effectively addressed your feedback.

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R01-G04b:

And also, in the abstract you mention sustainability and marine conservation. This sounds a bit too broad, unless you mention long term changes in the current across the strait and the importance of monitoring it or something like that.

Response:

Thank you for bringing this to our attention. We have made significant revisions to the abstract to accommodate the word limit, which was imposed because the manuscript you previously reviewed was transferred from another Nature journal. As a result of the word limit in Nature Communications, we were unable to address the issue of marine conservation directly in the present abstract. Instead, we have briefly summarized it within the context of the impact of submesoscale nutrient supply.

While it is possible to speculate that variations in the intensity and changes in the path of the background flow (i.e., Tsugaru Warm Current) might also play a role in the divergence east of the spur, the precise nature of the different timescale variations of the Tsugaru Warm Current remains uncertain. We are currently exploring other studies, such as investigating the impact of decadal-scale variations on the Tsugaru Warm Current. Looking ahead, we aim to investigate whether the diverse timescale variations might affect production within the Tsugaru Gyre through the key area of nutrient input.

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R01-G05:

- Some more details would be beneficial about some parts of the methodology

Response:

Thank you for your comments. We will provide detailed explanations of the modifications in separate comments below. We have included the number of turbulence sensors in TurboMAP, sampling details for the correction of the SUNA nitrate sensor (additional information has also been added in Supplemental Table 1), and details of the SADCP data acquisition settings.

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DETAILED COMMENTS

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Abstract

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R01-D01:

Line 21-24: “Nutrients transported vertically in specific hotspot areas that reach the surface can be conveyed downstream by background currents, thereby augmenting surface biological production over a wider expanse, including robust currents traversing seamounts.”, sentence a bit convoluted/confusing. Suggest rewriting

Response:

Thank you for your feedback. As noted in the general comments section above, we have significantly revised the abstract to align with the new word limit for abstracts, following our transition from another Nature Journal. We appreciate your understanding.

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R01-D02:

Line 32. Suggestion: “A surface divergence field” -> “a divergent surface flow field”

Response:

We adjusted the wording as per your instructions initially, but during the subsequent reorganization, some of the phrasing had to be altered due to the character limit mentioned above. We appreciate your understanding, again.

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R01-D03:

Line 34. Suggestion: “Formation area” -> “area”

Response:

Due to the aforementioned word limit, we have omitted this sentence in our organization. We trust you comprehend our decision.

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R01-D04:

Line 36. Last sentence is a bit too general and lacks real substance. It would be OK if you explored further the implication for ecosystem management in the manuscript, but not really justified at the moment

Response:

Thank you for your feedback. As mentioned in the general comments section above, we have substantially revised the abstract to streamline the text in accordance with the updated word limit for abstract. We appreciate your understanding.

In the revised abstract, we have refined our assertion to propose that the submesoscale nutrient injection identified in this study could instigate a reevaluation of nutrient supply routes in similar locations. Investigating such injection at the submesoscale level could prove valuable as a monitoring point for potentially bolstering mesoscale primary production.

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Introduction

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R01-D05:
Introduction. The first paragraph sets up the general relevance but it entirely lacks of references. Some reference would increase the credibility

Response:
Thank you for your highly valuable and significant remarks. We have incorporated the pertinent references and made revisions to the first paragraph of the Introduction section.

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R01-D06:
Line 52. “[...] the Tsugaro gyre” could be added at the end of the sentence for clarity

Response: We have made the correction as you suggested.

Please refer to the 2nd paragraph of the Introduction.

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R01-D07:
Line 61. “higher distribution” -> “higher values/concentration”?

Response: We have addressed the issue as you indicated.

Please refer to the 2nd paragraph of the Introduction.

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R01-D08:
Line 68. In the paper you report nitrate flux in mmol/m2/s, but here is per day. It would be good to make the units more uniform to make comparisons easier

Response:
Thank you for bringing this to our attention. We have updated the description to ensure clarity in both units whenever feasible. However, we feel it is important to acknowledge that the observed values in our study exhibit significant variability, and translating them into average values over a day is an assumption. We have articulated this notion in the "Discussion" section, contrasting it with primary production as daily fluxes.

Please also refer to the 2nd paragraph in the “Turbulence over the spur and associated diffusive nitrate flux”, and the 1st paragraph of the Discussion.

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Results

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R01-D09:
Line 95. “the similar localized increase”, unclear phrase

Response:
Thank you for bringing this to my attention. I have clarified the area and removed the word, "SIMILAR".

Please refer to the final sentence in the 1st paragraph of the “General situation at the time of the survey”

R01-D10:
Line 103 and after. “the isotherm”, which isotherm? All isotherms? In general lines

Response:
The expression “the isotherm” has been revised to emphasize isopycnal surfaces with specific values for their description.

Please refer to the “Turbulence over the spur and associated diffusive nitrate flux“ Section

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R01-D11:
Line 116. It is confusing that you have focused on the temperature description above, and here you emphasize comparing nutrients with density instead of temperature. It would be simpler to pick one, since, I guess, they must be closely related

Response:
As mentioned earlier (R01-D10), the description has been adjusted to concentrate on individual isopycnal surfaces, while also incorporating specific values from the isotherms.

Please refer to the "Turbulence over the spur and associated diffusive nitrate flux" section.

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R01-D12:
Line 122 – 124. It is hard to verify this conclusion by looking at the nitrate plot here, since nitrate is not shown together with density, so it is difficult to tell whether there was a significant impact of diapycnal flux on nitrate concentration. Further, “similar depths displaying different nitrate concentrations does not necessarily imply a diapycnal flux, if isopycnals are not flat. You could modify the plots slightly to get the message across more effectively. Perhaps adding a subplot with nitrate+density could help.
I also suggest (see below), maybe putting the last panel in an independent figure. Your conclusions may be reinforced by performing some along stream nitrate budget / advection diffusion balance: knowing the flow speeds and the nitrate evolution along

the stream, you may infer a diapycnal flux that you could compare with your measured flux. I am thinking of something along these lines: https://doi.org/10.1029/2007GL032785. I am not sure if this is going to work (possibly not, due to unsteadiness), but may be worth having a try.

Response:

Thank you for your valuable feedback. In light of your comments, we have initially included a figure panel as Fig. 2e depicting the distribution of nitrate on the isopycnal surface along the east–west observation line. We anticipate that this figure will enhance our comprehension of elevated nitrate concentrations on isopycnal surfaces to the east of the topography.

We have also taken into account the comments and the previous study you recommended, investigating the balance between advection and diffusion in the east–west direction, specifically $u\partial N/\partial x$ and $\partial/\partial z(K_z\partial N/\partial z)$. Regrettably, we did not achieve conclusive results indicating closure in these terms alone. As previously mentioned by you, the imbalance primarily will stem from issues related to unsteadiness and the challenge of assessing advection effects in the north–south direction using the outcomes of this observation. Additionally, the Sharples method is tailored to extract nutrient gradients closer to the background field [i.e., ideally, $\partial/\partial z(K_z\partial\langle N\rangle_t/\partial z)$], hence it cannot fully assess the impacts of short-term changes in nutrient gradients [i.e., $\partial/\partial z(K_z\partial N'/\partial z)$].

To more accurately evaluate such a balance, conducting more targeted observations (such as numerous repetitive measurements of turbulence intensity on parallel sidelines) is crucial. However, resolving phenomena with significant temporal variability (like turbulence and tides) in a rapidly flowing area poses considerable challenges. We hope you appreciate this aspect.

Instead, we utilized the average nitrate turbulent vertical flux on the isopycnal surface $\langle F\rangle_\sigma$ and the average depth of the isopycnal surface $\langle z\rangle_\sigma$ to assess the trend of vertical convergence of the fluxes ($\partial\langle F\rangle_\sigma/\partial\langle z\rangle_\sigma$), and to compare the distribution pattern of the east–west gradient of nitrate on the isopycnal surface ($\partial N_\sigma/\partial x$). This comparison has been illustrated in Supplementary Figure 2.

While we did not consider the advection velocity of the background flow, we observe a trend towards a positive nitrate gradient (indicating higher concentration in the eastern, downstream area) just east of the summit of the topography (24.5–25.0σ_θ, 141.59°E–141.62°E, particularly in the flux convergence region (Supplementary Figure 2).

The previous Fig. 2f has been reassigned as Supplementary Figure 3 due to the number of panels.

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R01-D13:
Line 128-129. This sentence is somewhat repetitive

Response: Thank you for your comment. We have revised this sentence.

Please refer to the 2nd paragraph in the "Turbulence over the spur and associated diffusive nitrate flux" section.

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R01-D14:
Line 131. The reporting of the flux values is confusing. Try expanding the sentence.

Response:
Thank you for your comment. We have also addressed the issue as you indicated.

Please refer to the 2nd paragraph in the "Turbulence over the spur and associated diffusive nitrate flux" section.

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R01-D15:
Line 134-139. You could maybe explain to the readership of NC what are the implications of the existence of a hydraulic jump. It may not be obvious for some of your biologically-oriented readership.

Response:
Thank you for your significant comment. We have included explanations of the hydraulic jumps.

Please refer to the 3rd paragraph in the "Turbulence over the spur and associated diffusive nitrate flux" section.

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R01-D16:
Line 142. I think you should say that the HFR helps you support the assumption that the zonal divergence is representative of the three-dimensional structure of the float.
HFR divergence analysis. Fig 3 depicts very substantial temporal variability of the flow divergence. I am not very familiar with the HFR, it would be good to know how confident you're with this patterns and what is the instrumental noise level for their detection. If they are robust, you could comment a bit on the significance of the short term variability. Before proceeding to the correlation with chlorophyll it would be desirable to better understand the spatiotemporal dynamics of the flow convergence/divergence: how flow convergence varies in time and space and how consistent and persistent are the patterns described during the cruise. Maybe some EOF or time-series analysis could be presented, in the main text or as SI. It would be useful also, some discussion about what drives those changes.

Response:

Thank you for your valuable feedback.

Firstly, we noted the prevailing dominance of east–west flow velocities. However, we have also included the following statements: *"In reality, the north–south velocity distribution may also affect the divergence field and the vertical structure of the density surface. Therefore, we investigated the two-dimensional divergence field of the surface velocities obtained by the HFR system"*. (Please refer to the final three sentences in the "Turbulence over the spur and associated diffusive nitrate flux" section).

A Hovmöller spatiotemporal analysis (Fig. 3) was employed to characterize the east–west flow and the divergence of the surface flow within the area of interest. Additionally, a wavelet analysis (Fig. 4) was utilized to provide further insight into the spatiotemporal variability in the divergence.

We believe that these additional results provide the reader with more compelling insights into why we focused on the divergence of surface velocities at this location. Furthermore, it appears evident that the diurnal and semi-diurnal variations due to tides are significant. In other words, the substantial variation in divergence observed during the drift period could be attributed to these tides rather than noise from the HFR. The mean signal-to-noise ratio of the HFR system at the observation site during the study was greater than 5, surpassing the threshold proposed by previous studies (Kirinchich et al., 2012, https://doi.org/10.1175/JTECH-D-11-00160.1; Hains et al., 2017, https://doi.org/10.1175/JTECH-D-16-0203.1).

Please note that the comparison between the field exhibiting short-term variations and the internal structure of the ocean is aimed at illustrating a systematic relationship pattern between turbulence intensification and divergence field of the surface current velocity.

In addition to these short-term variations, we contend that the behavior of the longer-period Tsugaru Warm Current and the heightened divergence of surface currents at subinertial frequencies, as outlined in the general comment, aligns with our assertion (Figs. 4 and 8). Specifically, when the Tsugaru Warm Current intersects the Shiriya Spur at this frequency, it boosts surface nutrient supply in the submesoscale region, leading to increased divergence of surface currents on the eastern side of the topography, consistent with the relationship (spatial pattern) between the current and the topography during the shipboard expedition. We believe this synchronicity at the frequency further substantiates the validity of our hypothesis, indicating elevated nutrient supply in the submesoscale region with divergence of surface current east of the topography.

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R01-D17:

Lines 158-161. Sentence not clear. The link between radar flow and nutrients is not obvious. Also, what do you mean by the "transport of nutrient supply"? Do you mean the transport of recently upwelled nutrients or something on this line?

Correlation analysis. This is a very interesting promising thing, but I think it should be explored in more depth. First thing, have you tried lagged correlations? I am surprised of this direct correlation with no lag, when your results imply that there is an advective time-scale for this response (the time it takes for the waters with the upwelled nutrients to reach a certain point). In general, I feel that there is a lack of understanding of the underlying spatio temporal dynamics of the physico-chemical coupling. Would something like a wavelet coherence analysis help? Maybe it is too complex. I would encourage the authors to provide some more insights with the approach they consider most useful. This analysis would strengthen conclusions like those in sentences 178-182. Also it is unclear why the convergence zone shows no correlation, I would assume that the convergence-divergence patterns are linked to each other. Could you expand a bit on that? If panel b is not helping you, you may as well dropped, but now that you showed it I am intrigued by it.

Response:

Thank you very much for your invaluable comments. We clarified the concept that the divergence field of surface current velocities east of the Shiriya Spur, as detected by the HFR system, could serve as an indicator of vertical nutrient supply through vertical mixing, supported by the observed correspondence (as mentioned in the final paragraph of the "Spatial and temporal changes in the surface velocity field around the Shiriya Spur" section). Although horizontal transport through advection is not addressed here, we discuss its effects in a later section concerning lag correlation analyses in the revised manuscript.

The correlation analysis in the previous manuscript was considerably simplified due to space constraints. However, in response to reviewer comments, we have endeavored to enhance the analysis by incorporating lag correlations. Initially, we included 7-day lag correlation maps as shown in Fig. 6c (and Fig. 6d). Subsequently, we partitioned the Tsugaru Gyre into 18 subregions and showcased their lag correlations in Fig. 7. This division into subregions along with spatial averaging, aims to mitigate the impact of potential chlorophyll-*a* data gaps. Furthermore, we investigated the distribution of correlation coefficients derived from the high-pass filtered data (see Supplementary Figure 6). We have also included wavelet relationships between the divergence and the area-averaged chlorophyll-*a* concentrations in the 18 subregions within the Tsugaru Warm Current (Fig. 8).

With the inclusion of the lag analysis shown in Fig. 7, we observe that correlations tend to be high in regions with short lag times (~ 0 day) in the waters east of the Shiriya Spur (e.g., R04 and R05 in August). Additionally, peaks or notable correlations with lag are noted in R08 and R11 at and 6–7 days in August, and in R08 and R11 at 5–6 days in September. Additional high-passed correlations (Supplementary Figure 6) revealed that the region of notable correlation was predominantly centered around the Shiriya Spur, with no widespread correlations evident in the region of 41.0°–41.5°N, 142°E–143°E of Fig. 6a. These outcomes lead us to infer the lag attributed to advection.

We included wavelet analysis for the divergence of surface current velocities in the area east of the Shiriya Spur (Fig. 4). Considering that signal enhancements in the subinertial range (spanning from a few to 10 days) typically occur during summer and

autumn (Fig. 4c), the broad range of correlations at small lag (Fig. 6a) is attributed to synchrony between the timescale of advection (~7 days assuming a 150 km diameter for the Tsugaru gyre and a mean current velocity of 0.4 m s^{-1}) and the periodic enhancement of divergence in the immediate eastern region of the spur. We believe that this increased divergence at subinertial frequencies is a result of the north–south movement of the Tsugaru Warm Current, as previously noted. The notable wavelet coherence signals between the divergence and chlorophyll-*a* observed in the Tsugaru Gyre at subinertial frequencies, as depicted in Fig. 8, will further substantiate this concept.

We are unsure about the specific time scale at which the biological response initiates due to the absence of observations on this cruise, but we anticipate it to occur between one and several days after the nutrient supply takes place.

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Discussion

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R01-D18:
Line 183. Confusing sentence. Turbulence does not “uptake” nutrients, it supplies nutrients which are then taken up by plankton.

Response:
Thank you very much. You are absolutely right; we have amended it to "supply" along with R01-D20.

Please refer to the 1st paragraph in the Discussion section.

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R01-D19:
Lines 185-190. I understand what you mean here, but I think you could use a more direct language to make the point clearer. Something along these lines: “the throughflow advects water that is poorer in nutrient compared with the generally dominant subarctic waters”

Response:
Following your comments, this section has been revised and moved to the 1st paragraph of the "Divergence east of the spur and chlorophyll-*a* variation in the summertime Tsugaru Gyre" section in order to make the text more concise.

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R01-D20:
Line 192. “uptake” -> transfer or supply

Response:
Thank you for your feedback. This has also been rectified, along with R01-D18.
Please refer to the 1st paragraph in the Discussion section.

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R01-D21:

Line 198. Not sure if “carbon flux” is an accurate term here, I would rather say it this the new production of organic carbon that could be sustained with the influx of nutrients (and will potentially become a downward flux of organic carbon)

Response:

Thank you for your feedback. We consider that over an extended period, it could be viewed as a sinking flux (carbon sequestration from the surface), but we suspect that it is likely advected near the surface within a certain timeframe. However, at the present stage, assessing the quantitative aspects of new production, export, and advection seems challenging. In the revised manuscript, our initial approach involves estimating primary production based on surface chlorophyll-*a* concentrations using two different models. Subsequently, we convert it to nitrogen content using the Redfield ratios and compare this to the vertical turbulent nitrate fluxes estimated through observation.

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R01-D22:

Line 202-203. you may as well make a, b values explicit there, but it would be important also to say where this model comes from and whether is known to apply locally

Response:

Thank you for your invaluable feedback. It is mentioned in the Method section that these coefficients are empirically determined. Additionally, another model (VGPM) was employed to estimate primary production in the Tsugaru Gyre, taking into account local sea-surface water temperature and light conditions. We have also rectified the incorrect reference cited in the previous manuscript, as it should have been Berger (1989). Our apologies.

In light of your comments, we have introduced these models and the level of primary production within the Tsugaru Gyre in the second subsection in the Results, comparing it to other oceanic regions.

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R01-D23:

Line 205. “about daily advection” is unclear, maybe something like “though horizontal advection over the course of one day”

Response:

Thank you very much for your feedback. Concerning the discussion section, we found that the previous manuscript became overly complex due to numerous assumptions regarding quantification of the vertical turbulent nitrate fluxes, estimation of the area where nutrient inputs to the surface layer occur, and distances due to advection. As a result, the revised manuscript adopts a simpler approach, employing a comparison method based on per unit area values. Consequently, this portion has been omitted. Thank you for your understanding.

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R01-D24:

Line 296. From “reproduction” does not make sense to me. Do you mean regenerated production?

Response:

We have identified the part you mentioned as L206. While we have made substantial revisions to the "Discussion" section as outlined earlier, we have also addressed the language you highlighted, changing it to "regenerated production".

Please refer to the 2nd paragraph in the Discussion.

==========

R01-D25:

Concluding remark and relevance. The mention to a single other place in the globe feels a bit insufficient here for a generalist journal. Would it be able to expand a bit on the broader relevance? E.g. showcase other locations with potentially similar structures?

Response:

In accordance with your suggestion, we have updated the text by incorporating findings from other oceanic regions, such as the Mediterranean Arboreal Gyre, the Kuril Islands eddy, the Kuroshio and Kuroshio Extension currents and their recirculation, and the Antarctic Circumpolar Current.

Please refer to the 3rd paragraph in the Discussion.

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R01-D26:

-Figure 4. You show potential vorticity contours there, but I can recall seeing a reference in the text as to why those are there. I guess is to highlight the gyre location. Make sure that this is explicit somewhere.

Response:

We have included a description and rationale for relative vorticity in the Methods section. Furthermore, we have incorporated a velocity vector into this figure, which corresponds to Fig. 6 in the revised version.

Please also refer to the 1st “Divergence east of the spur and chlorophyll-a variation in the summertime Tsugaru Gyre” section.

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Methods

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R01-D27:

- You mention that you followed a drift sampling strategy, but without a more detailed description it is not straightforward to guess what it exactly means and what are the advantages of this approach for your study. You could expand on the sampling strategy a bit

Response:

We have included explanations of the significance of the drift sampling in the text.

Please refer to the 4th paragraph in the Introduction and the 3rd paragraph in the "Chlorophyll-a distribution and estimated primary production within the Tsugaru Gyre" section.

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R01-D28:

-Line 219. What do you mean by modifications in latitude

Response:

Explanation has been added in the Method section (3rd paragraph). This entails adjusting the starting latitude of the drift to be more northerly than originally planned, based on information obtained from the Tsugaru Warm Current Axis Latitude Survey (Fig. 1c will also help your understanding).

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R01-D29:

-Line 221-223. unclear, did you take surface water samples to assess the drift in the SUNA sensor? How did the sensor performed? It would be good to have some more information. Also, could you use these samples to reinforce your message of nutrient upwelling on the lee side of the sill?

Response:

As you pointed out, we utilized it to assess sensor offsets, so we have included that information. Details such as sampling latitude and longitude have been summarized in Supplementary Table 1. Analysis of surface water samples revealed a more pronounced trend of increasing SiO2 concentrations east of the summit, while the distribution of NO3 exhibited a consistent trend but with a weak signal at the surface, which is not elaborated upon in this study.

Furthermore, while this manuscript does not delve into it, the root mean square error between the six CTD water samples and the offset-corrected profiles obtained from SUNA near the Shiriya Spur was approximately 1.2 uM during the same voyage.

In the preceding manuscript, details concerning the spatial dispersion of nitrate from the carousel water sampling (41.60°N, 141.41°E and 41.69°N, 141.72°E) were

inadvertently omitted due to the replacement of panels in Fig. 2 during our writing process. We believe this may have led to some confusion, and we apologize for any inconvenience caused. We have included this transect information as Supplementary Figure 8.

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R01-D30:

-Lines 124 onwards. Some more details would be welcome. How many profiles? How many shear sensors have you got?

Response:

Details regarding the number of turbulence deployments during drift have been included in Supplementary Table 1. The TurboMAP is equipped with two shear sensor probes.

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R01-D31:

-Nitrate density – fits. Do you do the fitting for each profile, or all profiles combined? In the main text you suggest significant nitrate changes along isopycnals, how does that impact this estimation? (probably not a lot, but just checking).

Response:

In this study, we conducted the fitting for each profile based on the concept of estimating the time-averaged nitrate concentration gradient relative to the density field. We believe that this approach helps mitigate the impact of spike contamination and the influence of short-term disturbances on the nitrate gradient estimation. However, since the distribution of nitrate on the isopycnal surface varies at each observation point along the flow direction of the Tsugaru Warm Current, fitting was performed individually at each point.

Upon examining the fitted cases on the isopycnal surface using all profiles, we observed that the flux estimates tended to be approximately half of the fitted cases for each profile.

However, because the fluxes vary spatially due to the intermittent nature of turbulence intensity, we believe that enhancing the accuracy of turbulence intensity estimation is a more critical issue for achieving more precise flux estimation.

Please note that during the drift observation, priority was given to turbulence intensity observation, so only surface water sampling was utilized for correction purposes. This was done to rectify the offset of SUNA.

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R01-D32:

-SADCP. Also some more details could be useful: What was the configuration, in terms of bin size, segment averaging etc?

Response: We have included additional information.

Please refer to the 5th paragraph in the Method.

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Reviewer #2 (Remarks to the Author):

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This study makes excellent use of a dataset comprising of 1) long-term high frequency radar measurements of surface currents and divergence and satellite measurements of chlorophyll a together with, 2) shipboard measurements of turbulent energy dissipation to reveal that the wide expanse of chlorophyll a within the Tsugaru Gyre, is tied to the extremely high values of vertical diffusive nitrate fluxes caused by the presence of a Shiriya Spur seamount in the Tsugaru Strait. On the basis of statistical analysis the authors were able to show that there was a strong correlation between long-term high-frequency radar measurements of surface divergence downstream of the sea mount and long-term satellite measurements of chlorophyll a.

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R02-G01:

Although the measurements are high-quality, and the findings interesting, I couldn't find anything significantly novel in this manuscript that would merit its publication in Nature Communications. There have been several previous reports on the role of seamounts from elsewhere as contributing to areas of elevated biological productivity. From within the Tsugaru Strait, some of the authors of this manuscript as well as others have shown that presence of sea mounts are responsible for excessive increases in turbulent energy dissipation rates along the path of the Tsugaru current and consequently nutrient injection into the euphotic column and the elevated chlorophyll concentrations downstream in the Tsugaru Gyre. Perhaps the only novelty in this study is the strong correlation between surface divergence immediately downstream of the seamount and surface chlorophyll a concentration. Despite this interesting observation, I found the discussion of the results very weak, in particular, the few lines addressing the lack of correlation between surface divergence upstream of the Shriya Spur seamount and sea surface chlorophyll in the Tsugaru Gyre seemed cursory at best.

Response:

Thank you sincerely for your invaluable comments. As you rightly indicate, it is widely acknowledged that topography significantly influences biological production. Our co-authors' prior investigations in the Tsugaru Strait (Tanaka et al., 2021; https://doi.org/10.1007/s10872-020-00588-w) further underscore the significance of nitrate transport mechanisms facilitated by topographical features. However, as you astutely highlight, our study leverages long-term observations of surface currents with submesoscale resolution (~3 km). Utilizing this data, we can now demonstrate that the divergence of surface currents within a relatively confined area, situated at the eastern outlet of the Tsugaru Strait over simple, straightforward topography, correlates with the mesoscale distribution of chlorophyll-*a* in the Pacific Ocean. We argue that this finding is of paramount importance, as long-term monitoring of surface current velocity distributions at such spatiotemporal granularity is only realistically feasible within a limited geographical area.

We consider the concept of the "continuous algal culture system," as indicated in your previous comment, to be truly invaluable. We further assert that the Tsugaru Warm Current, the Shiriya Spur, and the Tsugaru Gyre within the eastern Tsugaru Strait

present exceptionally suitable sites for examining these relationships as in ideal model of this system. We posit that our findings hold promise for reassessing the impact of specific disturbances in various oceanic regions.

Moreover, as elaborated below, we think that the inherent characteristic of the periodic north–south oscillation of the Tsugaru Warm Current at subinertial frequencies, including its occasional temporary bypass of the Shiriya Spur (Kaneko et al., 2022; https://doi.org/10.1186/s40645-022-00509-z), serves to bolster our aforementioned assertion regarding the impact of submesoscale nutrient injection at the topography, constrained by the spatial scale of the Tsugaru Warm Current in the latitudinal direction, on downstream chlorophyll-*a* distribution.

We have enhanced our manuscript to reinforce these arguments and have incorporated the "continuous algal culture system" concept, as mentioned above, by introducing the term "isolated phytoplankton incubator" (please refer to the first paragraph of the Introduction).

Regarding the previous study around the strait (Tanaka et al., 2021; https://doi.org/10.1007/s10872-020-00588-w), it is pertinent to note that the focused observation points were situated more than 100 km away from the Pacific Ocean. To address this consideration, we have incorporated the observation points from Tanaka et al. (2021) into Fig. 1c, for instance. In the figure, on the western side of the strait (approximately 140.20°E–140.47°E), a local increase in chlorophyll-*a* concentration was observed east of the saddle; however, this enhancement does not exhibit significant continuity with the Tsugaru Gyre (e.g., Fig. 1c). Possible explanations include the observed fluxes being smaller in Tanaka et al. (2021) compared to the present study, and the likelihood of nutrient consumption during the advection process.

Additionally, we have included the distribution of mean chlorophyll-*a* concentration normalized by standard deviation around the Tsugaru Gyre for the months of August and September, spanning from 2014 to 2022, as depicted in Fig. 1d and newly introduced Supplementary Figure 1. This normalized chlorophyll-*a* distribution underscores the role of the Tsugaru Gyre as a stable "culture pod," characterized by relatively high concentrations and minimal variation (Fig. 1d). Within the same figure, the presence of a horizontal maximum near the Shiriya Spur (41.6°N–41.8°N, 141.6°E–141.8°E) further suggests the significance of this topography in fostering this culture pod.

We believe it is very plausible to point out that our previous manuscript presented only basic results for correlation analysis. One explanation for this is that the manuscript was suggested for transfer from another Nature Journal and was constrained by the size limitations of the initial journal where it was submitted.

Given the additional comments from other reviewers regarding the correlation analysis, we have incorporated several supplementary analyses into the revised paper. Specifically, we have included lag correlations (depicted in Figs. 6 and 7), correlations

utilizing high-passed data (Supplementary Figure 6), and spatiotemporal analysis of surface current velocity and its divergence on the east flank of the Shiriya Spur (newly added Fig. 4), including wavelet analysis (newly added Figs. 5 and 8). Through these analyses, we believe that we have compiled valuable information elucidating the rationale behind selecting the target area and the impact of advection.

In the high-passed lag correlations (Supplementary Figure 6), we did not observe a significant positive correlation downstream of the Tsugaru Gyre as seen in Fig. 6. Therefore, the correlation downstream is likely influenced by advection. From the wavelet analysis, we noted an intensification of subinertial periodicity (1- to 10-day) in relation to the divergence of surface current velocities from summer to autumn (present Fig. 4). Regarding variations in the subinertial periodic band, we attribute them to north–south fluctuations in the flow path of the Tsugaru Warm Current during the season, induced by periodic atmospheric disturbances (Kaneko et al., 2022; https://doi.org/10.1186/s40645-022-00509-z). We speculate that one reason for this could be the synchronization of such periodic variations with the advection timescale into the downstream area (approximately 7 days to go half way round, assuming 150 km diameter and 0.4 m s^{-1} velocity). This potential synchronization may represent an inherent characteristic of the Tsugaru Gyre.

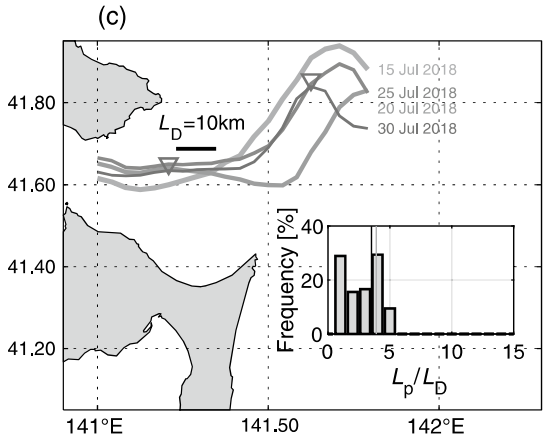

Fig. 9 in Kaneko et al. (2022). Example of shorter-term variation in “the axis” (indicated as lines) of the Tsugaru Warm Current estimated from the high-frequency radar during summer.

We considered the possibility that the north–south movement of the Tsugaru Warm Current could influence the distribution of chlorophyll-*a* within the Tsugaru Gyre by controlling nutrient supply to the east of the Shiriya Spur at the subinertial frequency. The recently incorporated wavelet analysis, investigating the divergence in the area adjacent to the eastern flank of the spur and chlorophyll-*a* concentrations within the Tsugaru Gyre, reveals an increased signal in this subinertial frequency range during August and September within the Tsugaru Gyre (Fig. 8). We argue that this discovery bolsters the aforementioned hypothesis.

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R02-G02:

Although it seems clear that stratification downstream is essential to phytoplankton growth. I also note that the discussion lacked any mention of the potential role of the anti-cyclonic Tsugaru Gyre in accumulation surface chlorophyll. Finally the section describing the extrapolation of shipboard nitrate fluxes over the region of intense divergence to estimates of carbon fluxes, and the estimates of satellite measurements using surface chlorophyll a measurements to be over-simplistic. For a journal of this repute, I would expect a more rigorous evaluation of both carbon fluxes and net primary productivity especially using an approach that views this hot spot of chlorophyll a as a continuous algal culture system. which is receiving pulses of nutrients continuously that sustains the anti-cyclonic Tsugaru Gyre's elevated chlorophyll a. If the authors intend resubmitting, they should restate as to what makes their results different from those reported for other seamounts, what makes the findings in this manuscript different from those reported earlier for the Tsugaru Current, the role of the Tsugaru gyre and stratification in the build up of chlorophyll a

Response:

Thank you greatly for your invaluable comments. We have endeavored to address the effects of advection through lag correlation analysis, among other methods, as outlined earlier. Regarding the estimation of primary production, in addition to empirically estimating it solely from surface chlorophyll-*a* concentration, we have also employed the vertical generalized production model (VGPM, Behrenfeld & Falkowski, 1997; https://doi.org/10.4319/lo.1997.42.1.0001), utilizing local photosynthetically available radiation (PAR) and sea-surface water temperature data, among other factors. Based on these estimated primary productions, we have compared the nitrogen uptake by phytoplankton to observed nitrate fluxes, assuming the Redfield ratio. Additionally, we have included information pertaining to primary production and nitrate uptake rates around the Tsugaru Gyre (Shiozaki et al., 2020; https://doi.org/10.1016/j.dsr.2020.103304).

As mentioned earlier, the present study offers increased certainty regarding the notion that nutrient supply to the surface in a relatively confined area may exert a wider-ranging impact. This finding represents a novel contribution not adequately demonstrated in previous studies conducted in the Tsugaru Strait. It is also worth noting, as you rightly pointed out, that the Tsugaru Gyre serves as an algal culture and accumulation system, connecting to such a robust nutrient source. Additionally, we have included information indicating that primary production within the Tsugaru Gyre during August and September surpasses that of the central subtropical circulation of the North Pacific during the same season, as detailed in the second paragraph of the results section.

The findings of the current study underscore that even submesoscale nutrient sources, when robust, can exert a significant impact on a broader spectrum of phytoplankton production. This highlights the potential for a reassessment of nutrient supply pathways in marine areas with similar structures.

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Reviewer #3 (Remarks to the Author):

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1. The presentation of results requires substantial revision to enhance clarity and consistency. One prominent issue is the inconsistency in the use of terminology. The terms "rear of the sill", "back of the sill", "behind the sill", and others, were employed without clear specification of whether they refer to the same concept or different concepts, and this causes confusion. To make the research more accessible to a broader audience, I would recommend using simpler and more easily understandable terms, such as "east of the sill." The headings in the results section as well should offer clear and concise descriptions of the content being introduced (please refer to the details).

Response:

We apologize once again for any confusion caused and appreciate your patience. As per your feedback, we have updated the description to incorporate directions: east, and west. Thank you for bringing this to our attention.

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2. Furthermore, the discussion is notably lacking. While the research introduces several critical figures and concepts, it falls short of providing an in-depth discussion beyond that of correlation maps, which are insufficient to effectively elucidate the proposed mechanisms. The advection of nutrients is controlled not only by surface currents, but also by factors such as phytoplankton assimilation rate, etc. The concept of the Froude number and its association with the idea of an "internal hydraulic jump" was also introduced. However, no adequate discussion of what these concepts mean in the context of this study was given. A more in-depth discussion of these critical elements is necessary.

Response:

Thank you sincerely for your suggestion. We have incorporated additional detail into the correlation analysis, and we have included an explanation and conducted a confidence interval analysis for the Froude number. Furthermore, we have expanded upon the discussion of the uptake rate information, drawing from the literature values presented in Shiozaki et al. (2020) (https://doi.org/10.1016/j.dsr.2020.103304). Additionally, we have provided a more thorough examination of the effect of advection.

Other reviewers have also raised this point (i.e., lack of discussions), and we found it to be highly valid. Consequently, we have included some analyses addressing it, as mentioned above. Specifically, we have incorporated a wavelet analysis of the divergence of surface currents on the eastern side of the terrain (please refer to newly-added Fig. 4) and a correlation analysis utilizing high-pass time-series data (see Supplementary Figure 6). These analyses suggest that the short-lag broad-scale correlations depicted in Fig. 6 encompass the effects of advection. Furthermore, the enhancement of the variations in divergence at subinertial frequency range (Fig. 5) and advection time scales of the Tsugaru Gyre appear synchronized over a period of approximately one week.

The variation in subinertial frequencies observed in the divergence of surface current velocities may correspond to the north–south shift of the Tsugaru Warm Current, as illustrated in my paper by Kaneko et al. (2022) (https://doi.org/10.1186/s40645-022-00509-z). In essence, during the long-term average, the Tsugaru Warm Current flows at a latitude crossing the Shiriya Spur (Fig. 3 and c). However, during the summer to autumn period, it exhibits a characteristic shift to circumvent this topography. We believe such a feature may be unique to the Tsugaru Straits.

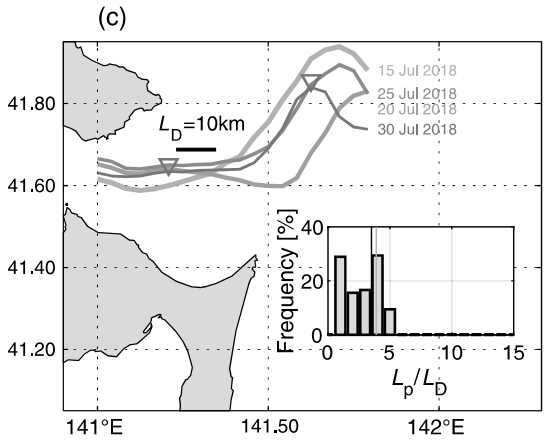

Fig. 9 in Kaneko et al. (2022). Example of shorter-term variation in “the axis” (indicated as lines) of the Tsugaru Warm Current estimated from the high-frequency radar during summer.

We entertained the notion that the north–south movement of the Tsugaru Warm Current could impact the distribution of chlorophyll-*a* within the Tsugaru Gyre by regulating nutrient supply to the east of the Shiriya Spur. The recently included wavelet analysis examining the divergence of the surface current in the region adjacent to the eastern flank of the spur and chlorophyll-*a* levels within the Tsugaru Gyre (Fig. 8) demonstrates an intensified signal in this subinertial frequency range during August and September within the Tsugaru Gyre. We contend that this finding lends support to the aforementioned hypothesis.

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3. The methods section lacks essential details. For instance, it is unclear which type of correlation analysis was conducted to examine the relationship between chlorophyll-a and the surface velocity divergence. Moreover, the treatment of chlorophyll-a data remains unspecified. Satellite-derived chlorophyll-a is prone to missing pixels, often due to cloud cover, especially at the temporal scales used in the correlation, but it was not addressed how this was handled in the correlation analysis.

Response:

Thank you for your valuable feedback. We have included a description of the correlation analysis in the Methods section. As you highlighted, there is an issue with missing data for chlorophyll-*a*. However, we have aimed to enhance the robustness of

the correlation analysis by extending the data length from 2014 to 2022. Additionally, in Figs. 7 and 8, we have tackled this issue further by averaging the subareas spatially.

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4. The quality of the observations was also not thoroughly discussed. For instance, while calibration data for nitrogen measurements was collected, the paper omits its presentation. Lines 238-239 mention the adjustment of nitrate profile offset with reference to bottle data, but it lacks clarity regarding why this adjustment was necessary and how it was carried out. Probably this section could benefit from a clear separation between data and methods.

Response:

The description of the CTD water sampling has been reorganized for clarity. Additionally, we have included the latitude and longitude of the surface water sampling data used to calibrate the SUNA nitrometer in Supplementary Table 1. This surface water serves to calibrate the SUNA's surface offset. However, during drift observations, only surface water samples are utilized for SUNA calibration, as the aim is to gather frequent turbulence data.

In the previous manuscript, information regarding the spatial distribution of nitrate from the carousel water sampling locations (41.60°N, 141.41°E and 41.69°N, 141.72°E) was accidentally left out due to the replacement of panels in Fig. 2 during the writing phase. We suspect this oversight may have caused confusion, and we apologize for any inconvenience it may have caused. We have rectified this by incorporating the transect information as Supplementary Figure 8.

Additional information is provided in the individual responses.

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Details:

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Abstract

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R03-D01:

21-24: the sentence needs to be improved to simplify the flow.

Response:

Thank you very much for your feedback. The abstract has undergone significant revisions to align with the revised word limit for abstracts (due to transfers from another Nature Journal). We appreciate your understanding.

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Results

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R03-D02:

95: It does not follow from the above statement that the high chlorophyll-a was caused by turbulent mixing.

Response:

We have revised our descriptions to indicate that there is a nutrient supply, with one potential source being vertical flux. Thank you.

Please refer to the final sentence in the 1st paragraph of the "General situation at the time of the survey"

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R03-D03:

110: "...gradually shoaled,..."?

Response:

Thank you very much for bringing this to our attention. We have eliminated the confusing sentence. Thank you for your understanding.

Please refer to the 1st paragraph of the "Turbulence over the spur and associated diffusive nitrate flux"

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R03-D04:

111-115: The sentence needs revision to improve the flow and clarity. I think the point being made is that, "while a similar phenomenon has been observed off Cape Tappi, it is noteworthy that the magnitude of this phenomenon was significantly greater in the case of Shiriya Spur"?

Response: We have made the correction based on your comment.

Please refer to the 1st paragraph of the "Turbulence over the spur and associated diffusive nitrate flux"

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R03-D05:
112: It would be more precise to use terms like "west of the summit" or "east of the summit" instead of "behind" in this context, as the latter term is unclear and ambiguous.

Response:
Thank you very much for your significant input for the readers. Essentially, the description has been revised to align with east–west orientation through the manuscript.

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R03-D06:
119: Near the topography?

Response:
While specifying the longitude and latitude, we have also included east–west notation for clarification.

Please refer to the 2nd paragraph of the "Turbulence over the spur and associated diffusive nitrate flux"

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R03-D07:
125: levels of vertical nitrate flux were immediately behind the sill?

Response:
As mentioned above, the description was essentially revised to incorporate directions: east, and west.

Please refer to the 2nd paragraph of the "Turbulence over the spur and associated diffusive nitrate flux"

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R03-D08:
132: In a geographic context, using cardinal directions namely north, south, east, and west is indeed more intuitive and specific compared to the term "rear," which can be vague and open to interpretation. So, to provide clear and unambiguous geographical references I'd advise to stick to the above cardinal direction.

Response:
Thank you very much for your valuable feedback. We apologize for the recurring inconvenience in several sections. As previously mentioned, we have fundamentally revised the description to consistently utilize east and west orientations throughout the manuscript.

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R03-D09:

137: How significant was this difference from 1? Notably, this number exhibits a decrease in an eastward direction, coinciding with the areas where intense mixing was observed. It would be beneficial to provide a brief explanation of how it is derived and offer additional context, as it plays a crucial role in supporting the claims made in this paper. Later in the methods section, the paper describes its calculation in more detail, but a concise overview and context would enhance the reader's understanding of its importance.

Response:

The distribution has been adjusted to include the confidence interval estimated by the bootstrap method in Fig. 2f. The critical point, denoted by $Fr = 1$, signifies the stagnation of internal waves induced by background flow, and it was estimated to be around Station 15 (41.684°N, 141.583°E).

Please refer to the 3rd paragraph of the "Turbulence over the spur and associated diffusive nitrate flux" and Fig. 2f.

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R03-D10:

139: What does "internal hydraulic jump" mean in this context?

Response: We have included a brief description of ours as depicted in R03-D09.

Please refer to the 3rd paragraph of the "Turbulence over the spur and associated diffusive nitrate flux".

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R03-D11:

147, 166: Should be revised to provide a clear and concise description of these paragraphs.

Response:

In response to your feedback, we have revised these paragraphs by incorporating an analysis of what we found inadequately explained in the previous draft. A concise summary is provided below.

To provide further clarity on the rationale for selecting the domain (41.65°N – 41.70°N, 141.60°E–141.65°E) from paragraphs L147 onward, we have included information regarding the average summer north–south structure of the east–west flow velocity (see Fig. 3). From this figure, we demonstrated that this latitudinal zone (41.65°N–41.70°N) corresponds to the area with the fastest flow axis of the eastward Tsugaru Warm Current as a temporal average for August and September (Fig. 3b, c). Additionally, we presented evidence of the divergence of surface current velocities, highlighting it as the region of maximum north–south divergence within the mean divergence field (Fig. 3d, e). We believe that these findings clarify our rationale for

selecting the region and demonstrate the correspondence between the chosen area and the region of enhanced turbulence during turbulence observations (Fig. 4, previously Fig. 3).

Paragraphs from L166 were further revised to enhance the correlation analysis. The mean area of divergence of surface velocities has been modified (see Fig. 6), and lag correlations have also been taken into account (see Fig. 7). We anticipate that these analyses will address the inquiries raised in the comments.

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Discussion

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R03-D12:

183: What is meant by the "concept of nutrient uptake" in this context? Typically, it describes the process in which microorganisms, such as phytoplankton, absorb nutrients from their surroundings to facilitate growth and metabolic activities. However, it appears that this definition may not apply here.

Response:

Thank you for your valuable comment. It was meant to address the transport of nutrients to the surface through local vertical mixing in the previous manuscript, which has now been rectified. Furthermore, we have made substantial revisions to the Discussion section. We now employed two distinct models to estimate primary production as already mentioned, then utilized the Redfield ratio assumption to gauge the nitrogen uptake rate by production and compare it to the magnitude of the vertical turbulent nitrate flux estimated through observation.

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R03-D13:

195: I would suggest removing the term "back of the sill". The given longitude and latitude ranges provide more precise context than the above term, which is ambiguous and can lead to potential confusion.

Response:

Apologies for the confusion. We have rectified it throughout the manuscript accordingly.

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R03-D14:

198: It's important to make a distinction since the carbon flux referred to here is potential. The estimates are based on the rate of nitrogen supply and not the actual nitrogen uptake rate by phytoplankton.

Response:

Thank you very much for your valuable feedback. Firstly, we have strengthened our method for estimating primary production, as previously mentioned. In addition to the empirical estimation method utilized in the previous manuscript (incorrectly referenced

as you pointed out; Berger, 1989 is more appropriate. Our apologies), we have also incorporated the vertical generalized production model (VGPM) method (Behrenfeld & Falkowski, 1997; https://doi.org/10.4319/lo.1997.42.1.0001), which considers local sea-surface water temperature and light conditions.

With these estimated primary productions, we have revised the comparison from focusing on carbon to focusing on nitrogen taken up by the phytoplankton, based on the Redfield ratio and the observed nitrate flux in the present study.

Please also refer to the 2rd subsection of the Result, "Chlorophyll-a distribution and estimated primary production within the Tsugaru Gyre".

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R03-D15:

199: What is the relevance of the speed or mass advection in the estimate of primary production? It appears only chlorophyll-a concentration is important. It would be more intuitive to clarify what the coefficients "a" and "b" are and how they are obtained.

Response:

Thank you very much for your comments. We think that the added lag correlations (Figs. 6 & 7) may have indicated a more noticeable impact on advection. However, due to the significant uncertainties in the estimation methods discussed in the previous manuscript's Discussion section, such as estimating advection speed and the area where nutrient injection occurs. Thus, in the revised manuscript, we have chosen to discuss nitrate fluxes and nitrogen uptake rate determined from the estimated primary production per unit area.

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R03-D16:

203: Providing context of background concentration

Response:

In the revised manuscript, the area (40.75°N–41.75°N, 141.75°E–142.75°E) where the mean surface chlorophyll-*a* was measured is clearly specified.

Please refer to the 2nd paragraph of the "Chlorophyll-a distribution and estimated primary production within the Tsugaru Gyre" and Fig. 1d.

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Method

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R03-D17:

216: Was water sampling performed only “at the start and at the end”, or was it conducted “from the start to the end”?

Response:

Thank you very much for bringing that to our attention. We apologize for the confusing wording; the CTD observations with water sampling were conducted twice, both before and after the drift observations, but it was not used for calibration due to its distance from the SUNA observation point. We have revised the text to clarify this point. Additionally, for surface water sampling, we have included details about the locations where the observations were carried out in Supplementary Table 1.

Please refer to the 3rd paragraph of the “Method” section, and Supplementary Figure 8.

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R03-D18:

222: Surface water samples collected at approximately every alternate location were primarily used to calibrate the nitrate sensor? What other parameters were measured during these water samplings, and at what frequency were they collected?

Response:

The information above was utilized to adjust for the SUNA offset. Detailed information regarding the sampling points for surface water has been included in Supplementary Table 1.

Please refer to the 2nd paragraph of the “Method” section and Supplementary Table 1.

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R03-D19:

227: “The calculation method...” for what?

Response:

Additional information has been provided regarding the calculation target for estimating the dissipation rate (ε).

Please refer to the 1st paragraph of the “Method” section and Supplementary Table 1.

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R03-D20:

234: Like the other parameters in the equation, Γ should also be accompanied by a brief description.

Response:

Thank you very much. We have included an explanation (the mixing coefficient); it is a technical term associated with turbulence intensity.

Please refer to the 1st paragraph of the "Method" section.

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R03-D21:

245: Please provide a context as to why this approach was adopted. Adding the reason help readers understand the motivation behind the methodology.

Response:

Thank you greatly for your insightful comments. The aim of this fitting method is to estimate the time-averaged vertical gradient distribution of nitrate. This is crucial because estimating the vertical nitrate gradient is prone to contamination by sensor noise and short-term fluctuations. We have now included this clarification in the Method section.

Please refer to the 2nd paragraph of the "Method" section.

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R03-D22:

249-251: The sentence contains a lot of information and is difficult to follow. First, "flux" here refers to nitrate flux? There is no mention of nitrate correction until "corrected nitrate." What correction is this referring to? Second, was the 30-m running mean filtering applied along the entire transect within the surface region? To improve clarity, please break down the sentence.

Response:

We apologize for any earlier confusion. Firstly, when referring to flux, we specifically mean vertical turbulent nitrate flux.

As for nitrate calibration, it entails adjusting the offset value of SUNA through surface water sampling (see Supplementary Table 1).

The process of zeroing the nitrate flux near the surface is a correction that addresses terminal extrapolation during the fitting process of nitrate concentration on the isopycnal surface. We employed a moving average to mitigate spike errors introduced by SUNA, thereby selecting a 0 value near the surface layer in the profiles. Please note that while we empirically selected a width of 30 meters, we also tested another scenario with a width of 10 meters. The estimated nitrate fluxes at the nitricline (Fig. 2d), were almost within the same range.

Secondly, the 30-meter filtered nitrate profiles employed in the terminal processing of fluxes and in estimating the depth of the nitricline (to be discussed in the subsequent individual response, R03-D23) are not directly associated with the flux estimates. This is because the vertical gradient information, transferred onto the isopycnal surface

through fitting, was utilized for actual flux estimation, as explained earlier. In essence, we believe that transferring to isopycnal surfaces has its merits in circumventing such vertical filtering issues.

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R03-D23:

253: Oh, so the filtering was along the nitrate vertical profile!

Response:

Since the smoothed profile is solely utilized to just determine the depth of the nitricline, rather than for the flux estimation itself, the flux estimate remains largely unaffected. For instance, even with a 10-meter smoothing for estimating the depth of the nitriclne, the average flux in the nitricline east of the summit (141.55°E) stands at 1.2×10^{-4} mmol N m^{-2} s^{-1}. We are confident that this (30-meter smoothing) dose not significantly influenced the magnitude presented in the "Discussion" section.

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R03-D24:

254: What does “downstream of the topography” refer to or mean in this context?

Response: Apologies for any confusion. It should be corrected to "east."

Please refer to the 2nd paragraph of the “Method” section.

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R03-D25:

259: Briefly explain what the “correction for misalignment” does. Additionally, it is not clear how the “percent” was estimated in this case.

Response:

Thank you so much for bringing this to our attention; it is a somewhat technical term related to the ADCP installation on the ship. Additionally, we have included an explanation of "the percent good."

Please refer to the 4th paragraph of the “Method” section.

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R03-D26:

280: 8-day resolution data was used in Fig. 1c. What was the purpose of 1-day resolution data?

Response:

Thank you for bringing that to our attention. We utilized the 8-day resolution data to provide a general overview of the relatively long-term chlorophyll-*a* distribution during the voyage from late August to early September 2021 (Fig. 1c), whereas the 1-day resolution data were employed for the correlation analysis. We have included additional explanations about it.

Please refer to the 7th paragraph of the "Method" section

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R03-D27:

292: What would be considered the "observed" bottom layer in this context? Would this be the bottom layer to which a measurement instrument descended to or simply the water depth.

Response:

Thank you so much for your suggestion. That is the depth to which the measurement instrument descended. TurboMAP conducts free-fall observations, but due to the sensor's fragility, it needs to be halted with a certain margin to prevent collisions with the seafloor. Hence, it is challenging to gather information right above the seafloor. We have made revisions to our manuscript.

Please refer to the 9th paragraph of the "Method" section

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R03-D28:

294: The manuscript referred to appears to be about the "light-saturated photosynthetic rates" and not primary production. Please verify. Also, probably the coefficients were derived in a specific site. Are they applicable to the region of this study?

Response:

Thank you for your valuable remarks. We have rectified the reference to Berger (1989). Our apologies for your any confusion. Additionally, we have incorporated estimates of primary production from VGPM to account for local conditions, such as sea-surface water temperature and light conditions, as mentioned already. While there are some discrepancies between the two models, they are not substantial enough to markedly impact the discussion of the order of magnitude.

We have also included a statement indicating that the model coefficients in Berger (1989) are empirical.

Please refer to the 10th paragraph of the "Method" section

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R03-D29:

519: What "reference numbers" are those?

Response:

References and their corresponding numbers have been provided. Corrections have been implemented, and explanations have been added. It appears that the numbers were not aligned with the references in the previous manuscript. Our apologies for any confusion caused.

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R03-D30:
520: indicate the nutrient flux in ... regions?

Response:
In the newly added Supplementary Table 3, we have included the references we cited along with a supplement indicating the location of the region.

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R03-D31:
527: What do the vectors indicate?

Response:
This is the surface velocity observed by the HFR system. A reference scale has been added. Thank you.

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R03-D32:
532: The criteria for defining the spatial average of the surface divergence is not clear. The area average boxes have different sizes. How were they defined? Probably a Hovmöller plot of divergence and chlorophyll-a would provide additional insights.

Response:
Thank you very much for your invaluable comments. Including details on the spatiotemporal variation of surface zonal velocity and their divergence (Hovmöller diagram, Fig. 3, and wavelet diagram, Fig. 4) should provide the reader with a better understanding of why we selected this area. Please also see our response to your comment R03-D11. Additionally, we have standardized the size of the boxes for comparison (Fig. 6a and c).

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Reviewer #1 (Remarks to the Author):

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The authors have made a very substantial effort to improve the manuscript, including a series of new, insightful analyses, as suggested. I deeply appreciate such an effort. The manuscript is on the right track to become much stronger. However, one of the key results of the manuscript (the correlation between localised at the Shiriya spur and productivity in the Tsugaru gyre) is now less clear cut than it appeared to be with the previous, much shallower analysis. There was a substantial effort to still make this point, which is now spread across 6 figures (3 in the main text and 3 as supplementary). The most prominent feature in those figures seems to be the high correlation at 1 day lag in Figure 6, which is (perhaps deceivingly) at odds with the "advective" hypothesis that the authors are putting forward. The answer to this apparent contradiction may be there already but is diluted in the wealth of figures and information. I would recommend taking a step back and thinking about the best way of showing their point more succinctly and convincingly. There are other relevant points I list below. I also attach an annotated PDF with minor comments.

Overall, my perception is still that the manuscript is of high quality and makes an interesting and appealing point. It could eventually reach Nature Communication standards but getting to that point still requires some work to strengthen the main conclusions and the clarity of the arguments.

Response:

Once again, we thank you for the significant amount of time you have dedicated to the peer review of our paper. To address your points, we have decided to use the outputs of the ocean data assimilation model (JCOPE-T DA), as mentioned at the beginning of this document. We believe that this addition has enhanced the information on the interactions between turbulence and the divergence of surface velocities, as well as on the spatiotemporal scale of nutrient-enriched surface water advection by the surface current.

We aimed to simplify the manuscript as much as possible, but please note that we could not reduce the number of figures and tables. However, we believe that the revisions make our arguments more concise and compelling.

We hope you understand, and we thank you again for your time and effort in reviewing our paper.

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R01-G01:

It is not entirely clear/explicit why you can assume a direct correspondence between the elevated mixing rates during the drift experiment and the flow divergence downstream of the spur (end of page 8). You should provide a more straightforward mechanistic connection between the two. It may relate to the hydraulic jump's dynamics, but it would be good to see an explicit argument. This weakness could be solved by adding one or two sentences explaining the dynamics that justify the link.

Response:

Thank you very much for your essential and important comments. Due to the limited observational data on the ocean's interior, we have decided to use the outputs of the ocean data assimilation model to address this issue. Figures 2, 4, and 7 show the results from the model outputs. In particular, Figure 4e and f provide results that more directly suggest a link between turbulence (internal hydraulic jump) and surface divergence processes.

Please also refer to "Internal hydraulic jumps, upwelling and surface divergence as suggested by JCOPE-T DA data" in the Results section.

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R01-G02:

A second weakness pertains to the main idea that mixing of nutrients at the Tsugaru Strait supports productivity over the adjacent gyre. The authors have made a significant effort to justify this point, but the wealth of information becomes a bit overwhelming, and the point is somehow lost. The paper would benefit from a synthesis effort. Right now, you have 3 main figures to make this point, plus several supplementary figures. This feels like too much, and the ideas get diluted.

Response:

Thank you very much for your valuable input. Regarding our ideas, the new version of Fig. 1e is a good illustration, as you suggested (R01-D02). To reinforce this, we have added another figure as Fig. 5f. Figure 5f shows the ratio of chlorophyll-*a* concentration when divergence is positive to the chlorophyll-*a* concentration when divergence is negative (including lag due to daily advection).

Through these additions, we have aimed to present our findings in a more integrated manner, although the current manuscript still contains a large amount of figures.

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R01-G03:

If I understood correctly, the advection time-scale seems to be too long (~ 1 week) to explain the strong correlations at zero-lag between localised divergence and large-scale chlorophyll distribution in the wider Tsugaru Gyre area (the authors acknowledge that around line 332). If there was an advective connection between divergence and productivity, progressing anti-cyclonically along the gyre's rim, I would expect to see some evidence of that in your correlation maps in Figure 6, 7. There is some of that in Figure 7, but I feel it could be more convincing. Perhaps, I am distracted by the prominent signal indicating a wide-spread short-lag coherence in Fig 6a. This would rather suggest a common larger-scale driver at ~daily timescales (tides? - can you comment on what this driver may be?). I understand that then you decide to focus on sub-inertial timescales, so perhaps this is your point. If that is the case, you could focus more on this idea and strengthen your arguments. Since the short-term coupling appears more prominent in Figure 6, you need to convince the reader that the coupling at subinertial frequencies matters as well or matters more. For example, you could estimate how much of the chlorophyll variability is contained at subinertial frequencies and how much of it is explained by the covariance with divergence. Other points of

improvement are explaining better what point Figure S6 makes and adding some significance contours to the wavelet analysis in Figure 8 and discuss how this analysis compares with the lag correlations in Figure 7.

Response:

Thank you for your extremely important remarks. Regarding our previous manuscript, we realized that the discussion was obscured by a lack of information on the spatiotemporal scale of advection. To address this, we have conducted a particle tracking experiment with an ocean data assimilation model (Fig. 7). This experiment suggests that the timescale of advection due to surface current velocities is much shorter than that we had previously assumed from long-term averages of velocity: the advection can complete a circuit in about two to three days.

We now believe that this is consistent with the results of the lag correlation analysis (~3 days; e.g., R05, R08, R10–R11 in Supplementary Figure 7 in the present manuscript). In the current manuscript, for ease of discussion, we initially describe this circulating advection effect as creating a lag of around 3 days. Subsequently, we also highlight the effect of this recirculation on the lag correlation (e.g., Fig. 6b) of around 7 days.

Furthermore, we discuss the spatial bias of the positive correlation (in the southeast direction of the Tsugaru Gyre; 41.0°N–41.5°N, 142°E–143°E; Fig. 6b) and another effect of the advection of the large clockwise circulation (Fig. 7; 39.5°N–41.5°N, 142°E–144°E). The advection from this larger circulation appears to be more related to subinertial frequency fluctuations (Fig. 8c–f). We will continue to refine the details of this analysis in future studies.

We have added significance to the wavelet analysis in Fig. 8, as you pointed out. Regarding the previous Supplementary Figure 6 (now Supplementary Figure 8), the explanation has been organized in relation to the ~3-day circulation.

Please also refer to "Divergence east of the spur and chlorophyll-a variation in the summertime Tsugaru Gyre" in the Results section.

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R01-G04:

You could explain up front why you focus on the period August-September. Is it because this is the time of the year when the gyre / the localised divergence are active? Is nutrient limitation in the gyre stronger during this time of the year, so you see a clearer signal when nutrients are supplied? Maybe expanding on that could help you highlight the importance of localised mixing, e.g. if productivity in the gyre is higher than expected for this time of the year. The addition of productivity estimates is a good added element of the revised manuscript. If you could show that productivity is higher than expected for the region in times of the year when the divergence is more active, that could make your argument stronger.

Response:

Thank you very much for your fundamental and important comments. Quite simply, this is due to seasonal variations in the Tsugaru Warm Current system. We have added Fig. 1c, d, and Fig. 2 to visually reinforce this point. In particular, we believe that the

JCOPE-T DA results in Fig. 2c, and d effectively illustrate how the vertical velocity changes during the stratification period.

We have also added Fig. 5f to address the latter point of your comment. Please refer to our previous response (R01-G02) as well for Fig. 5f.

Please also refer to the 2nd paragraph in the Introduction, and "Seasonal variability characteristics of the Tsugaru Warm Current and Tsugaru Gyre" in the Results section.

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R01-G05:

You could elaborate more in the connection between the sub-inertial variability and your previous study on the Tsugaru Warm Current position (line 245-247). Can you demonstrate/illustrate this somehow, with a figures

Response:

We appreciate your important comments. To reinforce this point, we have added Fig. 4a (JCOPE-T DA outputs) and Fig. 5 (observed HFR data). Figure 4a shows the results from JCOPE-T DA, which should help visually illustrate the variation in the position of the Tsugaru Warm Current axis. Fig. 5c shows the seasonal variability and the correspondence between the position of the Tsugaru Warm Current axis and the surface current divergence. Fig. 5d shows the inverse relationship between them during the stratification period (June–November).

Figure 5e (wavelet coherence) is a revised version of the previous Fig. 4 (wavelet transform of the divergence) and serves as a useful illustration of the significance of their subinertial frequency fluctuations.

Please also refer to "Spatial and temporal changes in the surface velocity field around the Shiriya Spur" in the Results section.

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DETAILED COMMENTS
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R01-D01:
Line 90. A brief explanation of what the drift sampling strategy means in practice, and how close it is to Lagrangian, would be useful in the main text.

Response:
Thank you for your comment. We have added a sentence stating that the advection velocity is roughly estimated from the ship's position and time (~1.4 m s^{-1}). This velocity is consistent with the surface current velocity of the Tsugaru Warm Current.

Please also refer to the last paragraph in "Chlorophyll-a distribution and estimated primary production within the Tsugaru Gyre" in the Results section.

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R01-D02:
Lines 119-124. An important feature seen in this figure is that chlA concentrations are maximum east of the spur. You could use this feature to strengthen your argument.

Response:
Thank you very much for your very important point. We have reinforced this point in the Introduction. Please refer to our comment on R01-G02.

Please also refer to the 2nd paragraph in the Introduction and Fig. 5f.

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R01-D03:
Page 10-11. You should explain why you picked 1 day and 7 day lags in Figure 6.

Response:
We appreciate your important comment. We believe that the addition of the results from the particle tracking experiment has made it easier to understand. We also think that Supplementary Figure 6 (particularly Supplementary Figure 6b) in the current manuscript helps clarify the relationship between the timescale of the Tsugaru Gyre recirculation and the correlation.

Please also refer to "Divergence east of the spur and chlorophyll-a variation in the summertime Tsugaru Gyre" in the Results section.

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R01-D04:
Figure 7. Correlation subplots have different limits for their y-axis. This makes interpretation unnecessarily complicated.

Response:

We apologize for any inconvenience. We have reduced the amount of information in the figure and included only the correlation for August and the Pearson's linear correlation. Due to space limitations, we have moved the correlation figure to a supplementary section. For the y-axis, two types of ranges were used: a wide range (–0.2–0.4) and an extended range (0.15–0.3) to emphasize lag correlations, with each range unified in the respective sub-area (R01–15).

==========

R01-D05:

Supplementary Figures 4,5. I struggle to interpret these 3-dimensional plots. Given that this is is SI and you have plenty of space, I would suggest showing some slices as subplots rather than a 3d figure.

Response:

We apologize for any inconvenience. Following your comment, we have chosen to present it as a planar plot in Supplementary Figure 6. Additionally, only Pearson's linear correlation was discussed.

==========

R01-D06:

Discussion. You compare the areal nutrient supply at the spur with uptake in the gyre and show that the former is higher. However, enhanced fluxes presumably occur over a small area, while the uptake occurs over a much broader area. When scaled over the respective areas, do supply and uptake still match?

Response:

Thank you very much for your extremely important comments. In the previous manuscript, the spatiotemporal scale of advection was unclear, which made the discussion difficult. We believe that the addition of results from the particle tracking experiments has made it easier to visually grasp the spatiotemporal scale.

Based on this scale, we have added a discussion comparing integrated values by area per day: the input is 0.1° latitude (~11 km) and 0.1° longitude (~8.3 km), and the consumption is 0.2° latitude (~22 km) and 1° longitude (~83 km). The estimated ratio (the former to the latter) is quite large, suggesting that turbulent fluxes have a significant impact. However, other nutrient sources, such as nitrogen fixation and the influence of regenerated production within the euphotic layer, must also be considered in future work.

Please note that due to the large temporal variability of turbulence, there is a possibility that the fluxes are larger than observed (e.g., Supplementary Figure 5), highlighting the importance of more detailed in-situ observations in the future. As this study indicates that a relatively limited area is important (e.g., Figs. 1e, 5f, and Supplementary Figure 8), this area will be intensively investigated in the future. We expect that the efficiency of observation, such as the allocation of ship time resources, can be greatly improved by the present study.

Please also refer to the 2nd paragraph in the Discussion section.

==========

R01-D07:
General: although the language has improved, I still feel that some parts of the introduction could be more clearly written (see annotated PDF).

Response:
Thank you very much for directly correcting the wording. We have used an English Language Proofreading service in previous reviews, but it seems that issues such as long sentences tend to persist. We apologize for the inconvenience and would be grateful if you could point these out to us.

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Responses on the attachment file:
(Please refer to the yellow highlighted parts in the tracked changes)

==========-

R01-RA01:
L22: why?

Response:
This section has been removed based on direct corrections from another reviewer. Please note that we are unsure if we have accurately captured the exact intent of the question, but one possible answer may be expressed by the second sentence of the introduction in the revised version. Thank you.

==========-

R01-RA02:
L41-43: a bit repetitive not clear what the second part of the sentence adds

Response:
We apologize for any inconvenience caused. We have revised the text to make it more concise.

Please refer to the 1st paragraph in the Introduction section.

==========

R01-RA03:
L47-49: long. could be made clearer

Response:
We apologize again for the inconvenience. We have also revised the text here to make it more concise.

Please refer to the 1st paragraph in the Introduction section.

==========

R01-RA04:
L48: (a threshold-shaped topography) Not sure I visualise this

Response:
Thank you for your comment. We apologize for any inconvenience. We are working to express this clearly and have amended the wording. We meant the terrain jutting north from Cape Shiriya (i.e., sill-like topography in the east–west direction).

Please refer to the 1st paragraph in the Introduction section.

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R01-RA05:
L90: It would be useful to explain how you achieve this "Lagrangian" drift Sampling and if you can quantify "how" Lagrangian it is

Response: Thank you for your comments. Please also see our response to R01-D01.

Please refer to the last paragraph in "Chlorophyll-a distribution and estimated primary production within the Tsugaru Gyre" in the Results section.

==========
R01-RA06:
L97-99: long and unclear

Response:
We apologize again for the inconvenience. We have also revised the text here to make it more concise.

Please refer to the last paragraph in the Introduction section.

==========
R01-RA07:
L106: You could give some value so one knows where to look at on the map

Response:
Longitude, and the location of Cape Shiriya have been added to the text and Fig. 1f, respectively.

Please refer to the 1st paragraph in "General situation at the time of the survey" in the Results section.

==========
R01-RA08:
L143-145: Are Here alternative explanations (nutrient sources) that you need to rule out before conclusively saying this?

Response:
Thank you for your important comment. While we do not deny the existence of other sources, we believe that our additional analyses (e.g., Fig. 5f) positively highlight the impact of turbulent nutrient supply in this confined area. We also think that the results from the JCOPE-T DA output (Fig. 2) support the importance of this area, as a natural sequence. Nevertheless, we have made some amendments to the text to address the points you have raised.

Please refer to the last paragraph in "Chlorophyll-a distribution and estimated primary production within the Tsugaru Gyre" in the Results section.
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R01-RA09:

L222: Maybe you could explain why this flow divergence is relevant to nitrate fluxes (is it a metrics for hydraulic jump ?) to justify why you carry on the analysis using HF radar data

Response:

Thank you for your important comment. As noted earlier, we have used the JCOPE-T DA output to address this point. Please refer to our responses to R01-G01 and others.

Please also refer to "Internal hydraulic jumps, upwelling and surface divergence as suggested by JCOPE-T DA data" in the Results section.

==========

R01-RA10:

L257-260:

I think this connection should come before the long-term analysis, so we know what this is for

But also, importantly what is the mechanistic connection? Is this a signature of hydraulic jump?

Response: Please also see the responses to R01-G01 and others here.

Please also refer to the 3rd paragraph in "Spatial and temporal changes in the surface velocity field around the Shiriya Spur" in the Results section.

==========

R01-RA11:

L273:

Why 1 day and not zero days or any other value? Did you try several? What is the lag with higher correlation? If the correlation is so high at such short leg over such a large area to me this suggests a common large scale driver rather than a causal relationship

Response:

Thank you for your fundamental question. Please see Supplementary Figure 6a for the lag 0 case. We believe that particle tracking (Fig. 7) has provided information on the spatiotemporal scale of the advection, making it easier to interpret the periodic strengthening of the correlation in an integrated way.

Please refer to "Divergence east of the spur and chlorophyll-a variation in the summertime Tsugaru Gyre" in the Results section.

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R01-RA12:

L314-316: I see 0-lag correlations for R08 and R10 in August

Response:

Thank you for your important comment; please also see comment R01-RA11. This does not imply that there is no correlation at lag 0. Note that the correlation characteristics differs slightly between August and September, so the lag 0 correlation region may appear different in Supplementary Figure 6a (August–September) compared to Supplementary Figure 7 (only August).

Please refer to "Divergence east of the spur and chlorophyll-a variation in the summertime Tsugaru Gyre" in the Results section.

==========

R01-RA13:

L361: I am confused. Isn't this contradictory with lag correlations?

Response:

In the previous manuscript, we assumed a longer advection timescale (~7 days) based on the long-term mean velocity, which made the discussion somewhat confusing. For a more detailed understanding of the 7-day lag, it is necessary to consider the characteristics of complex systems (e.g., the presence of large gyres). Nonetheless, a simpler approach would be to say that within the Tsugaru Gyre, advection and circulation on a 2- or 3-day scale strengthen the correlations in this region.

Please refer to the 1st paragraph in the Discussion section.

==========

R01-RA14:

L412: Do they observe a notable biological response there?

Response:

Yes, for example, their Figure 2 shows that the presence of a gyre affects chlorophyll-*a*. Please note that in the eastern part of the Tsugaru Strait, the northern boundary is open (~41.8°N, >141.2°E), and the Tsugaru Warm Current often bypasses the terrain (e.g., Fig. 4a). Fluctuations in the divergence of surface current velocities associated with this bypass are a major difference.

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Reviewer #2 (Remarks to the Author):

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General comments:

I would like to commend the authors for incorporating many of the suggestions made by all the reviewers of the previous version of the manuscript. The changes made, including the addition of new datasets and analysis have certainly helped improve the robustness and validity of the results. The manuscript is still in need of editorial changes for improving its clarity.

I have tried to help as best as I can by making many editorial suggestions which can be found in the annotated version of the manuscript attached with my review.

Response:

We sincerely thank you for your valuable time and insightful comments, including direct corrections. Following your suggestions, we have substantially revised the Abstract and Introduction. In addition, we have considered the suggestions of other reviewers and decided to incorporate the results of the ocean data assimilation model. Although the inclusion of these results may extend the peer review process, we believe it is crucial to strengthen our arguments. We appreciate your understanding.

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Specific comments:

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R02-S01:

1) In my previous review, I had requested the authors to comment on the novelty of their work, specifically outlining what sets their work apart from the previous studies by Tanaka et al. 2021 within the same region, and by others who have already described the importance of underwater seamounts and physical mixing mechanisms triggered by these mounts and their role in facilitating nutrient fluxes into and the growth of phytoplankton in the euphotic zone. The response that the studies of Tanaka et al. (2021) were undertaken at a location 100km away from the present study seem to me at best lame. I would have liked more details - for instance whether the mixing, nutrient enhancement, divergence mechanisms differed or were not reported in Tanaka et al. adequately.

Response:

Thank you for your valuable comment. The direct mechanism we are considering here is similar to the process proposed by Tanaka et al. (2021) (shear instability from internal hydraulic jumps). These processes are universal. In this study, we identified a new candidate site for a more direct nutrient supply into the Tsugaru Gyre at the Shiriya Spur (Fig. 1e). There have been no reported observations of turbulent vertical mixing in this area, which is discussed more simply in the Introduction.

A novel aspect of this study is the use of surface divergent flow as a proxy for nutrient supply, leading to a long-term correlation analysis. No other such observations have been made in the Tsugaru Strait. Through this analysis, we were able to extend the discussion to include lag correlations between surface divergence and chlorophyll-*a*.

This demonstrates more clearly that the Shiriya Spur is a significant area of influence among the various possible nutrient supply sources (e.g., Fig. 5f).

The characteristics of the Tsugaru Strait, where the axis of the Tsugaru Warm Current fluctuates from north to south, either overcoming or bypassing the Shiriya Spur over periods of a few to ten days, provide an ideal situation for such a characterization analysis. The newly added Fig. 5f illustrates this clearly.

Please also refer to the 2nd and 3rd paragraph in the Introduction.

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R02-S02:

2) Although I had not pointed this out earlier, and for the sake of their readers, I would like the authors to provide more details as to whether the area of enhanced chlorophyll downstream of the TWC is a year-round feature? This is especially important given the importance of this process in sustaining this productive ecosystem. I assume that this mixing is a year round feature, albeit with slight changes in strength given the uni-directional flow of this current through the Tsugaru Strait and perhaps slight changes in current velocities over the year. If this is not a year round feature and the direction and strength of the TSW flow does not change much, what is the reason for seasonal variability of Chl-a within the gyre. Is it related to weaker divergence, light limitation, grazing pressure, lack of water column stability etc?

Response:

Thank you for your important comments. A distinct seasonality exists in this area (Fig. 1c, d in the present manuscript), associated with seasonal variations in stratification strength and the flow path of the Tsugaru Warm Current. To further aid the reader's intuitive understanding, we have added information on these seasonal changes, including the JCOPE-T DA output (Fig. 2). For example, Fig. 2d shows that significant upward velocity occurs east of the Shiriya Spur (141.6°E–141.7°E) during this season. Fig. 4d, another JCOPE-T DA result, clearly shows the correspondence between this upwelling and the surface divergent flow. Therefore, we believe that, the stability of the water column is one of the background factors controlling seasonal variations in chlorophyll-*a* in a broader sense.

Please also refer to the 2nd paragraph in the Introduction, and "Seasonal variability characteristics of the Tsugaru Warm Current and Tsugaru Gyre" in the Results section.

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R02-S03:

As I have stated above - I have made several edits in the annotated version of the manuscript. I would like the authors to have a look at edits to the abstract which currently lacks clarity and conciseness. I also made some edits to the introductory section which would benefit from clear statements as to the motivation for this study. In the other sections of the manuscript, I have also made some edits and have made some notes about the figures some of which appear to be labeled inadequately

Response:

We appreciate you spending your valuable time revising our text. We have revised the text to present our arguments more concisely, especially in the introduction, including our response to your comment above (e.g., R02-S01). We always strive to ensure the quality of the English text by using an English language review service, but we would be very grateful if you could point out any issues you notice.

==========

R02-S04:

Reviewer #2 (Remarks on code availability):

I have not had a chance to look at the code, but I believe it has been submitted.

Response:

Thank you very much. As we are revising under time constraints, we are submitting the codes as a supplementary file rather than uploading them to a server. Thank you for your understanding.

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Responses on the attachment file:
(Please refer to the green highlighted parts in the tracked changes)

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R02-RA01:

This sentence needs to rewritten to flow with first line.

Response:

Thank you for your comment. We have revised it as you suggested. Please let us know if this is not what you intended.

Please also refer to the 1st paragraph in the Introduction.

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R02-RA02:
L47:
What is treshold shape?

Response:

Thank you for your comment. We apologize for any inconvenience. We are working to express this clearly and have amended the wording. We meant the terrain jutting north from Cape Shiriya (i.e., sill-like topography in the east–west direction).

Please refer to the 1st paragraph in the Introduction section.

==========

R02-RA03:
L75:
What is the purpose of the reference to Tanaka et al 2021 shown in the figure?

Response:

This is intended to show the difference in chlorophyll-*a* concentration between the western (relatively lower; the region of Tanaka et al. 2021) and eastern (relatively higher) sides of the Tsugaru Strait (Fig. 1f). We have removed the text from the figure.

Please refer to the 3rd paragraph in the Introduction section.

==========

R02-RA04:
L120:
Standard deviation of individual pixes that were used to create the mean or the standard deviation of the chlorophyll disribtion over the study location

Response:

Thank you for your comment. We have incorporated the wording you suggested and revised it accordingly.

Please refer to the 2nd paragraph in the Introduction section.

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R02-RA05:
L135-141:
This sentence is very difficult to understand. Please break it up for clarity. I got comlpletely lost and was not sure what was being compared against what?

Response:
We apologize for any inconvenience caused. The sentence has been split and amended to clarify the meaning.

Please refer to the 2nd paragraph in "Chlorophyll-a distribution and estimated primary production within the Tsugaru Gyre" the Results section.

==========

R02-RA06:
L143:
Please provide the coordinates and also show in Figure

Response:
Longitude, and the location of Cape Shiriya have been added to the text and Fig. 1f, respectively.

Please refer to the 1st paragraph in "General situation at the time of the survey" in the Results section.

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R02-RA07:
L207:
Wasnt' this reported in Tanaka et al. (2021)?

Response:
Thank you for your important comment. Please refer to comment R02-S01.

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R02-RA08:
L250-253:
Panels have not been labeled

Response:
We apologize for the inconvenience. The figure has been labeled. Please note that it has been moved to Supplementary Figure 5.

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R02-RA09:
L261-262:
What is the reason for this variability. Is it due to tides. If so please mention this.

Response:
Thank you for pointing this out. We have noted that this is likely due to tidal effects.

Please refer to the 1st paragraph in "Spatial and temporal changes in the surface velocity field around the Shiriya Spur" in the Results section.

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R02-RA10:
L273-284:
The Fig does not have proper labels a-d for the panels and hence it is diffciult to follow the text. Also have the panels been mislableled as the positive correlation can be seen also for the left bottom panel which is lableled as 7 day lag?

Response:
We apologize for the inconvenience. The panel placement of Fig. 6 has been corrected.

==========

R02-RA11:
L412-418:
I feel this is all irrelvant.

Response:
Thank you very much for your valuable comments. We have removed the relevant sections in response to your remarks.

Please see to the last paragraph in the Discussion section.

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Reviewer #3 (Remarks to the Author):

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The authors have shown responsiveness to the comments provided by this and other reviewers. However, I remain concerned about the lack of structure and depth in the discussion section. This weakness persists in the current version. Additionally, the language requires further consideration.

Response:

We sincerely thank you for your valuable time and comments on our manuscript. Following your comments (e.g., R03-G01, 02), we have made efforts to improve the structure of the manuscript. To achieve this, we decided to use outputs from the ocean data assimilation model (Fig. 2, Fig. 4, and Fig. 7).

We are confident that these outputs will enhance our understanding of the seasonal variability of the Tsugaru Warm Current and the Tsugaru Gyre (Fig. 2), the connection between internal hydraulic jumps, turbulent processes, vertical currents, and surface current divergence (Fig. 4), and the spatiotemporal scale of advection (Fig. 7). We believe they will also aid in visual comprehension.

Although the inclusion of this information may increase the demands on the peer review process, we believe it will strengthen our arguments. Once again, we thank you for your valuable time.

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R03-G01

Key aspects covered by this manuscript include (1) nutrient supply via turbulent mixing, (2) nutrient transport to the downstream Trugaru Gyre, and (3) enhanced productivity resulting from this nutrient supply. However, these elements, aside from correlations, lack in-depth discussion. Even the correlations themselves require further interpretation to discern their significance. I recommend enhancing the discussion to address these points more comprehensively, particularly focusing on the implications of the observed positive correlation between nutrient supply and chlorophyll-a levels. Additionally, the insights provided by the lag correlations are noteworthy and deserve further elaboration.

Response:

Thank you very much for your summary of key points and important comments. In light of comments from reviewers, we have addressed these issues by using ocean data assimilation model outputs as mentioned above.

In particular, we believe that the particle tracking experiment example (Fig. 7) provides material on a specific spatiotemporal scale relevant to your question (2). The presence of a Tsugaru Gyre recirculation lasting a couple of days (Fig. 7) reinforces the interpretation of the lag correlation results using long-term data (e.g., R05, R08, R10–R11 in Supplementary Figure 7).

We also believe that the newly added Fig. 5f strengthens our argument by connecting Fig. 1e with the lag correlation results (Fig. 6a and Supplementary Figure 6).

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R03-G02

I'm uncertain whether the newly added wavelet figures add significant value to the main text or if they might be more suitable for supplementary materials. Additionally, there seems to be an abundance of introduced materials without subsequent discussion. It's crucial that the discussion begins with an exploration of nutrient supply and transport mechanisms, followed by an elucidation of their effects on biology. This sequential approach would enhance the coherence and depth of the manuscript.

Response:

Thank you very much for your valuable comments. We have decided to keep the wavelet analysis as Fig. 7 in the main text, although the areas we address have changed to Areas 1–6.

In line with your suggestion, we have reviewed the structure of the paper and revised it to make our arguments more straightforward, using the data assimilation model outputs (e.g., Figs. 2 and 4) as described above.

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Details:
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R03-D01
L25: "…the impacts of localized nutrient supply on…?"

Response:
Thank you for pointing this out. This part has been removed in accordance with direct correction instructions from another reviewer. We appreciate your understanding.

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R03-D02:
L27: add comma after Pacific Ocean.

Response:
Thank you very much. We have made the corrections.

==========
R03-D03:
L27-29: a lot of unnecessary words that can be removed to give space to more critical information in the abstract.

Response:
Thank you for your comment. As noted above, the abstract has received direct revision instructions from another reviewer and has been substantially revised in accordance with their suggestions.

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R03-D04:
Introduction
L34: the significance of the study can be made more impactful.

Response:
The Introduction has also been extensively revised in accordance with direct revision instructions from other reviewers. Thank you for your understanding.

==========
R03-D05:
L52: on long-term, how? Maybe include references. I thought this study was trying to demonstrate this control over broad-scales?

Response: Thank you for your comment. We have added the description.

Please refer to the 1st paragraph in the Introduction section.

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R03-D06:

L88: This should mean surface water mass?

Response:

Thank you very much for your comment. In accordance with the revision of the introduction, this section has been removed. Thank you for your understanding.

==========

R03-D07:

L92: “A previous study” can be name since it is repeatedly referenced.

Response:

The section related to the relevant paper has been simplified in the revised introduction. Thank you for your understanding.

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R03-D08:

L109: what timescales?

Response:

Thank you for your suggestion. We have added specific timescales.

Please refer to the 3rd paragraph in the Introduction section.

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R03-D09:

L179: since VGPM is referred to by name, the other model too can be referenced by name.

Response:

Although not a common name, it was denoted in this study as CBEM (surface Chlorophyll-Based Empirical Model). The literature has also been updated (Falkowski et al., 1998 has been added).

Please refer to the 1st paragraph in “Chlorophyll-a distribution and estimated primary production within the Tsugaru Gyre” the Results section.

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R03-D10:
L357: very long sentence and difficult to follow.

Response:
We apologize for the text being difficult to read. We have revised this part.

Please refer to the last paragraph in "Turbulence over the spur and associated diffusive nitrate fluxe" the Results section.

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R03-D11:
D11
L396: another long sentence.

Response:
We apologize for the difficulty in reading this section. It has been removed following a revision. Thank you for your understanding.

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R03-D12:
L562: Since no significant differences were found, I would suggest to keep it simple in the manuscript and show the absolute value rather than ratio. Also, the y-axis should have the same scale for all subplots. This makes comparison easy and straightforward.

Response:
Thank you very much for your comments. Following your suggestion, we have unified the y-axis for each subplot (using two ranges to emphasize the lag correlation peaks). To reduce the amount of information, we used only the August correlations and the Pearson's linear correlations. Please note that this figure has been moved to Supplementary Figure 7.

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R03-D13:
L611: area cannot support a concept.

Response: Thank you for pointing this out. The text has been revised.

Please refer to the 1st paragraph in the Discussion section.

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R03-D14:
L626: extrapolated vertical mean?

Response: Thank you for pointing this out as well. The text has been revised.

Please refer to the 2nd paragraph in the Discussion section.

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R03-D15:
L627: range -> flux? This comparison is a bit strange. Is the nitricline in the same depth range as the euphotic layer?

Response:
Thank you very much for your important comments. As you noted, the nitracline does not necessarily coincide with the euphotic layer depth. Nonetheless, as the euphotic layer depth at the time of this observation is estimated to be around 60 m (please see the second paragraph from the end in the Methods section), the discussion on the intensity of turbulent fluxes in this region (east of the Shiriya Spur; 141.6°E–141.65°E; Fig. 3c) should not be significantly affected in terms of order of magnitude (see also the discussion on water column averages of the turbulent nitrate flux). The wording has been amended to clarify this point.

Please refer to the 2nd paragraph in the Discussion section.

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Reviewer #1 (Remarks to the Author):

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I want to than the authors for the efforts they devoted to address my comments and craft a very thorough response letter and a much improved manuscript. The authors have now introduced new analysis, based on modelling efforts, to clarify my questions and I am happy to say that they dissipated my concerns, which regarded the advective time-scales of the nutrient signatures around the gyre. I would like to praise again their effort to introduce new clever analysis. I am now happy with the science, it is very interesting and the arguments are now well supported by different threats of evidence.

I still have some substantial editorial recommendations to improve the clarity and conciseness of the manuscript. I list the main points here, but I also attach an annotated PDF with more details and additional editorial comments (I am not listing those comments separately here for the sake of returning this review more quickly, I hope the authors don't mind that).

The clarity of the manuscript is much improved now, however, I have the feeling that some parts of the manuscript may be a bit difficult to understand for a reader that hasn't have access to the previous versions of the article. I am struggling to judge the clarity with full confidence, given that I read already the previous versions already, but I'd have some concrete suggestions:

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R01-G01:

- Rewrite the first paragraph of the introduction, with a clear, simpler message, perhaps following this thread: (1) Mixing is key to supplying nutrients to plankton communities; (2) mixing is enhanced at topographic features (cite some examples); (3) the effects of topographic mixing on productivity remain poorly understood due to the sparsity of observations and the difficulty of linking small-scale mixing measurements with larger-scale productivity; (4) here we address this issue for a case study region.

Response:

Thank you for your very constructive comments. We have revised the opening paragraph to be as straightforward as possible, following the outline you provided. Please note that some sections do not strictly follow points (1)–(4), as they also incorporate suggestions from R#3. However, we believe that the revisions based on your suggestions have effectively emphasized the importance of mixing and associated nutrient flux in a simpler and more direct manner for the reader.

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R01-G02:

- I struggled to follow section "Spatial and temporal changes in the surface velocity field around the Shiriya Spur", although it is really promising. Try to bring the focus back to your main messages here, and guide the reader at the start, explaining why you look at different scales of variability (in particular the subinertial variability of the jet. This

variability is key in subsequent analysis but I don't feel it has been introduced properly) and why this matters for divergence/nutrient supply. I would invite the authors to reconsider whether all the information in Fig. 5 is really needed in the main text. I am happy if they decide to leave everything, I am just mindful of conciseness and clarity.

Response:

Thank you very much for your constructive comments. While we have kept the first paragraph brief, we have added an explanation of why we are focusing on the analysis of subinertial frequency ranges. Additionally, the JCOPE-T DA figures have been moved to Supplementary Fig. 5 and 7, with a brief explanation added in the caption of Supplementary Fig. 7. The previous Fig. 5f has been reorganized as a standalone figure (new Fig. 4).

Please also refer to "Intra-seasonal spatial and temporal changes in the surface velocity field around the Shiriya Spur" in the Results section.

==========

R01-G03:

-Lines 362-371. I also invite the authors to reconsider whether this paragraph adds something new and relevant to the main text, or it can be dropped.

Response:

Thank you for your comment. Following your suggestion, we have decided to delete this paragraph and add (move) some additional text to the captions of the Supplementary Fig. 9.

==========

R01-G04:

-For a series of other editorial recommendations, see attached PDF.

Response:

We would like to thank you for the detailed comments attached to the PDF. Our responses to the comments are provided below as AFXX. Please note that the positions commented on in the PDF may be open to varying visual interpretations, and it's possible that the part we understood may not be exactly where you intended. Additionally, due to the use of the English review service (AJE), some of the final sentences have been revised slightly differently from those you commented on. Please keep these points in mind.

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Comments to the attached file (AF)

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Abstract:

R01-AF01:
L27: derived

Response:
Sentences related to this model have been deleted in accordance with R#3's comments.

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R01-AF02:
L31: suggestion "Enhanced mixing and nutrient fluxes" instead of "These differences"

Response: Corrected as per the comment.

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R01-AF03:
L33?: at the topographic feature

Response: We have revised it according to the comment.

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R01-AF04:
L33?: dowstream

Response: Corrected as per the comment.

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R01-AF05:
L37: where?

Response:
Thank you for your comment. In accordance with the revisions from R#3 (R03-ES03), we believe that the last sentence in the Abstract has been amended.

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Introduction:

R01-AF06:
L41-42:
"Nutrient supply to the euphonic zone, enhanced vertical mixing due to tidal driven flows over seamounts for instance, represents one such unique mechanism"

strange sentence

Response:
In accordance with the comments from R#3 (R03-ES04), we believe the text has been amended to address this.

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R01-AF07:
L53-58: “continuous phytoplankton culture”

This sentence is unclear unless you know the full context: that mixing at the topographic ridge supplies nutrients to the gyre downstream. Why don't you phrase it directly like this instead of using a "metaphor"

Or maybe just leave this for later, after properly introducing the system

Response:
Thank you for your comments. This metaphor was included in response to comments from R#2 during the previous review. We believe it has been improved by incorporating the specific revisions suggested by R#3 (R03-ES07).

==========

R01-AF08:
L59-61: "The Tsugaru Strait is a vital conduit connecting the Sea of Japan and the Pacific Ocean..."

I think you're making your life a bit too difficult with this paragraph. Why don't you make it more simple. Mixing is key to supplying nutrients to plankton communities, mixing is enhanced at topographic features (cite some examples), yet the effects of topographic mixing on productivity remain poorly understood due to the sparsity of observations and the difficulty of linking small-scale mixing measurements with larger-scale productivity.

Response:
Thank you very much for your comments. The text from L59–61 in the previous manuscript has been moved to the second paragraph in the present manuscript following the rewording of the introduction, in line with the comments from R#3.

==========

R01-AF09:
L77-79: "normalized by the standard deviation of individual pixels ..."

I think it is fine if you leave those details for methods or Fig caption

Response:
This part was also addressed by another reviewer (see comment, R03-D09). In response, the text has been revised. The term has been defined as an index, I_{chl}, and additional details have been added to the caption including an equation (Fig. 1). The text has also been supplemented with an interpretation of the index and figure (Fig. 1e).

Please refer to the 3rd paragraph in the Introduction.

==========

R01-AF10:
L85: "O(10−5) mmol N m−2 s−1"

Response: Standardized to day^-1 throughout the manuscript.

==========

R01-AF11:
L92: “continuous phytoplankton culture” system
I would remove this analogy. I think it is not particularly helpful

Response:
We believe that this section has been corrected by rewriting the text according to the specific revisions proposed by R#3 (R03-ES12).

Please refer to the last paragraph in the Introduction.

==========

R01-AF12:
L96-99: "Through combinations of turbulence observations and HFR ..."

Maybe I got a bit lost and missed the information, but would it be good to report the experiment dates here? I think it is useful for later.

Response: The dates of the drift observations have been included in this paragraph.

==========

R01-AF13:
L98: "the nutrient supply in the submesoscale domain"

Response:
R#3 suggested a specific revised text (R03-ES12), so we first rewrote the text accordingly and then added detailed sub-mesoscale spatial scale information.

Please refer to the last paragraph in the Introduction.

==========

R01-AF14:
L99:
in the mesoscale (>X km) gyre

Response:
Please see also the comment above (R01-AF13). R#3 proposed a specific revised text (R03-ES12), so we first rewrote the text accordingly and then added detailed mesoscale spatial scale information.

Please refer to the last paragraph in the Introduction.

==========

Results: “Seasonal variability characteristics …”

R01-AF15:
L115 "another"

Response: Deleted in line with your comment.

==========

Results: “Summertime chlorophyll-a distribution and primary production within the Tsugaru Gyre”

R01-AF16:
L129-131: "Fig. 1e also implies that..."

Can you explain in more detail which of the features in this figure support your statement?

Response:
The index I_{chl} has been defined and modified to represent more specific content. Please also refer to R01-AF09.

==========

R01-AF17:
L147-149: "recognizing it as one of the pivotal sites for the sustained delivery of nutrients..."

It is unclear whether this is a hypothesis at this stage (the one you want to investigate), if so, make it clear with your wording

Response:
At this stage, the wording was modified to more clearly convey what we believe to be the hypothesis.

Please refer to the 2nd paragraph in the section.

==========

Results: “Turbulence over the spur and associated diffusive nitrate flux”

R01-AF18:
L184: "approximately 25.5-26.0 sigma-theta"
homogeneous?

Response:
While we’re not certain if the intent of the comment was fully captured, the sentence

has been amended to clarify that it does not imply water masses of completely identical nature.

Please refer to the 1st paragraph in the section.

==========

R01-AF19:
L185: "east of 141.68E"

Response:
Thank you very much for your comment. During the revision process, this section was considered redundant and has been removed. We appreciate your understanding.

==========

R01-AF20:
L206:
you probably don't need to report the flux in two different units, just choose one

Response:
Thank you very much. We have standardized to day^-1 throughout the manuscript.

==========

R01-AF21:
L207: "The magnitude was also approximately 100 times greater than that estimated over the western strait topography"

maybe report a range for these fluxes?

Response: Specific values have been added.

==========

Results: “Internal hydraulic jumps, …”

R01-AF22:
L247-249: "Internal hydraulic jumps, upwelling and surface divergence as suggested by JCOPE-T DA data" section

maybe add small phrase in your first sentence to explain the reader why you're doing this analysis

Response: An explanatory sentence has been added at the beginning.

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Results: “Intra-seasonal spatial and temporal changes …”

R01-AF23:
L299-302?: "The correspondence between the north–south shift of the current axis..."

This is a very nice analysis, but I would suggest that the authors work on its clarity and conciseness, linking it back to your main thread, and how the different pathways of the jet impact divergence. Do you need all panels in this figure within the main text?

Response:
We understood the comment to be referring to the analysis in L299–302 (JCOPE-T DA). First, taking into account R#3’s comments, we decided to move the JCOPE-T DA analysis to the Supplementary Figs. Then, we also chose to split this figure (Supplementary Figs. 5 and 7).
We also explain the relationship between flow paths and divergence in Supplementary Fig. 7 for the cases of 10th and 30th August. A brief interpretive text has been added to the caption of Supplementary Fig. 7 to address this.

Please refer to the 2nd paragraph in the section, and Supplementary Figs. 5 and 7.

==========

R01-AF24:
L309-312: "Focusing on the daily mean div_ES ..."

Consider splitting this sentence into two for clarity

Response: Divided and organized in accordance with the comments.

Please refer to the last paragraph in the section.

==========

R01-AF25:
L312-315?: "Then, we computed and mapped ..."

not clear if you're introducing the next section here or pointing the reader to some additional analysis

Response:
We have added a sentence afterward to clarify that it serves as a transition to the next section.

Please refer to the last paragraph in the section.

==========

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Results: “Divergence east of the spur …”

R01-AF26:
L336?: "Area 1 and Area 2"

The areas haven't been introduced in the text

Response:
Immediately afterward, we added a reference to a diagram (Fig. 6) illustrating the location of these areas.

Please refer to the 2nd paragraph in the section.

==========

R01-AF27:
L345-346: "The correlation region then shrinks once, ..."

shrinks once sounds unclear to me

Response: We have made the revision. Thank you.

Please refer to the 3rd paragraph in the section.

==========

R01-AF28:
L368-370?: "In addition to the short timescale lags (~3 days)..."

I suggest consider whether this description, which is only based on supplementary materials, needs to be included. Is there something essentially new here?

Response:
The paragraph containing this sentence has been deleted, and a brief explanation has been added to the Supplementary Fig. 9 caption.

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Reviewer #2 (Remarks to the Author):

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I am very happy that the authors have incorporated many of the changes recommended at every stage of the review process. The result is that they have a very very strong and compelling manuscript " "Vigorous vertical mixing supports mesoscale biological production in the Tsugaru Gyre The work holds substantial significance for both the field and related disciplines" that meets the high standards for publication in Nature Communication. The work holds substantial significance for both the field of physical and biological oceanography and is richly deserves to be published

Response:

Once again, we would like to sincerely thank you for the valuable time you spent reviewing our manuscript and for your constructive comments. Two other reviewers also provided several comments, and we have revised the manuscript to address them. However, there are no major changes to the analysis or the overall narrative. The revisions include 1) reorganizing the introduction for clarity, 2) moving the model figures to the Supplement to better highlight the contrast with the observations, and 3) making other textual improvements. We trust these revisions will not significantly affect your earlier decisions. Thank you for your understanding.

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Reviewer #3 (Remarks to the Author):

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Remarks for the reviewer:

For convenience, we have divided the general comments, starting with Overall, into several parts labeled as R03-OXX. We have also separated the Details as R03-DXX and "a few editorial suggestions" as R03-ESXX.

R03-O01:

Overall, I feel the text has improved, but I strongly encourage the authors to reconsider the narrative, particularly by reducing the reliance on general model outputs. The core issue, as noted in the previous round of reviews, is that the story is becoming less clear. The combination of observations with general model results is not compelling. If the model outputs are included, they should be more focused on explaining specific events, such as in the case of Fig. 7. Several figures could be streamlined to enhance focus, for instance, simplifying Fig. 1 and moving several others to supplementary material. A simple and clear narrative can be achieved by focusing on parts of Fig 1 and 5, as well as Figs 3, 6, and 7. The remaining figures add too much material without significantly enhancing the manuscript.

Response:

Thank you very much for your important comments. We have presented the model results in the supplementary material, except for the particle tracking figure (formerly Fig. 7). The model description has been removed from the abstract. The description of the model results in the main text has been adjusted with simple modifications to avoid complex re-reading. Thank you for your understanding.

Regarding the simplification of Fig. 1, we enlarged the area of Fig. 1e, particularly on the western side. We have removed old Fig. 1f.

We believe that these adjustments have made it easier for the reader to focus on the key observational findings as the main results.

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R03-O02:

I also notice a tendency to force connections between observations and phenomena without providing clear, logical explanations (e.g., L168-171), which weakens the manuscript. As I mentioned in the previous review, the section titles need to be more informative to help readers understand the content. For instance,

"General situation at the time of the survey" is vague, general situation of what? A more specific title would improve clarity. Furthermore, from the current title, one would expect it detailing the conditions associated with the survey. Instead, it discusses chlorophyll-a enhancement and the implied impact of vertical mixing on nutrient supply near the spur.

This makes the section feel misplaced. As such, it would be better positioned after "Turbulence over the spur and associated diffusive nitrate flux", which details the mechanisms of nutrient supply.

Response:

Thank you for your comments. Firstly, regarding the section you specifically commented on ("General situation at the time of the survey"), we have decided to delete it. This part was included from the first draft, but its relevance became unclear during the revision process. We once considered placing it after the "Turbulence over the spur and associated diffusive nitrate flux" section, as you suggested. However, we felt this would be redundant due to the overlap with the following section. Thank you for your understanding.

The titles of the other sections were also revised to provide more information.

Furthermore, we believe we have addressed the tendencies you pointed out by through this time revision. For example, we have made similar part (e.g., the 2nd paragraph in the "Summertime chlorophyll-*a* distribution and primary production within the Tsugaru Gyre") clearer to the reader that we regard that statement as an assumption at the stage.

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R03-O03:

Overall

-Text clarity remains an issue: despite revisions, convoluted passages remain and should be streamlined for better flow. Try using shorter sentences to isolate ideas. I am inclined into thinking text clarity is not merely a language problem but more about the narrative focus.

Response:

Thank you for your very important comments. To address this, the first paragraph of the introduction has been revised to be simpler and to make the point clearer by using shorter, more concise sentences, also taking into account R#1's comments.

Other revisions include rewriting sections based on your suggestions and splitting some of the text into several sentences for clarity.

==========

R03-O04:

-Too many figures and study focus remains unclear: the number of figures have increased considerably but they did not significantly improve the clarity or focus of the manuscript, particularly in balancing observation-based findings with general model outputs. Fig. 7 from model outputs is useful, but other figures dilute the focus.

Response:

Thank you very much for bringing this to our attention. The figures of the model have been moved to the supplementary section (except for the figure of particle tracking figure), as mentioned in the comment at the beginning of this article.

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R03-O05:

-Section order and titles: The structure needs revision for better flow. The results are mixed with discussion and this adds to the confusion. Section titles should be more specific to guide the reader.

Response:

Thank you for your valuable comments. We have aimed to simplify the structure and make it as concise as possible. In doing so, we removed some sections and paragraphs (please also refer to SC03).

We also think that overly fine-tuning the sentences in certain sections could unnecessarily increase the peer review workload. For this reason, we did not make detailed changes to the sections and paragraphs describing the model outputs (e.g., "Seasonal variability characteristics of…", and "Internal hydraulic jumps,…") but just moved the figures to the supplementary section to distinguish them from the main results. Thank you for your understanding.

==========

R03-O06:

-I suppose high-resolution current data and turbulence measurements should be the main focus of the manuscript.

Response:

Thank you very much for your comments. As mentioned earlier, we have distinguished the model outputs by moving them to the supplementary section. We believe the revised manuscript is now more focused and highlights the observed findings more clearly.

==========

R03-O07:

In summary, I recommend focusing on simplifying the manuscript's narrative, rather than expanding it with new material.

Response:

In line with your comments, we have revised the content to make it more organized and concise. Consequently, the introduction has been almost entirely rewritten to incorporate your feedback. We have also reflected your suggestions ("a few editorial suggestions") as much as possible in other parts of the document. Again, we sincere thank you for your very constructive and valuable suggestions.

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Details

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Abstract:

R03-D01:

L27 "outputs derived from an ocean data assimilation model" makes the sentence dense and shift focus away from the main point. While I agree that model simulations add valuable context, I understood that the primary focus of this study was on field observations, in particular HFR currents and turbulence measurements. The simulations should serve as supporting materials, otherwise it becomes unclear whether the study is centered on modeling or observations? It would be reasonable to develop a specific modelling study to test a given hypothesis, however, the use of model results here only suggest that it is to support the general pattern of the ocean.

Response:

Following your comments, this sentence has been removed. As mentioned earlier, the model outputs, except for particle tracking, have been moved to the Supplementary Figs.

==========

Introduction:

R03-D02:

L46: convoluted and contradicting sentence (functional interplay vs. may exert)

Response:

In accordance with the proposed amendments, R03-ES05 (L44-9), we believe this section has been improved.

==========

R03-D03:

L50-1: What is the point of the sentence? Reviewer #2 of previous versions has repeatedly pointed out this weakness. I'm not entirely sure it's accurate to say "this interplay is poorly understood". From this draft, it seems the poorly understood aspect is how this interplay supports non-local, large-scale biological enhancement, which this study aims to demonstrate (L56). Again, elaborate on the novelty of this work.

Response:

We believe the text has been revised in line with the specific suggestions from R#1 (R01-G01), and that this is an appropriate response to the comment. Thank you for your understanding.

Please refer to the 1st paragraph in the Introduction.

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R03-D04:
L54: Another convoluted text. I struggle to get the point. Is "this system downstream" referring to the nutrient supply source region or the environment downstream receiving the nutrient supply? If it is the latter, the "environment downstream" would be more appropriate than "system downstream". What is meant by "continuous phytoplankton culture".

Response:
We have amended it in line with your comment on R03-ES07 (L50–8). Thank you very much for your suggestion.
The term "Continuous phytoplankton culture" was used based on an earlier comment from R#2, but R#1 also noted that it was unclear. We believe that we have addressed this by rewriting it more specifically, as indicated above.

Please refer to the 2nd paragraph in the Introduction.

==========

R03-D05:
L55: The placement of this sentence makes it confusing and unclear. It might be beneficial to move it before L53 or rephrasing it to suggest that similar analyses in other regions could be insightful. Otherwise, what is the purpose of this study? More specifically, "pivotal processes" is quite ambiguous. What specific processes are being referred to?

Response:
First, following the proposed text in R03-ES07 (L50-8), we have rewritten the relevant parts accordingly. Additionally, we adjusted the structure within the Introduction and finally decided to delete some sentences that were difficult to understand. We hope you will understand.

Please refer to the 2nd paragraph in the Introduction.

==========

R03-D06:
L60: For context, it would be beneficial to add the magenta line to (e) or make (e) and (f) share the same scale on x-axis.

Response:
Thank you for your comment. In response to your suggestion (R03-O01), we have decided to delete Fig. 1f. This decision was made because the explanation for this figure closely overlaps with the explanation for the new Fig. 4.
Additionally, as per your recommendation, we have added a magenta point to Fig. 1e to indicate the location of the observation point. We have also extended the western portion of Fig. 1e to include the observation area on the western side of the strait (Tanaka et al., 2021).

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R03-D07:
L69: What is the significance of the simulation figure in the main text?

Response:
Thank you for your comments. Regarding the JCOPE-T DA outputs, we have decided to move them to the Supplementary section, except for the particle tracking figure.

==========

R03-D08:
L74: In Fig. 2 the bottom panels are sections from the top, but the horizontal range is different, why?

Response:
Thank you very much for your comments. The *x*-axis (longitude) ranges have been standardized.

==========

R03-D09:
L77: This statement is quite ambiguous, echoing the initial concerns raised by the reviewer of the previous version. I believe the authors intend to say "normalized by its pixelwise standard deviation", as is evident from the computation code. Additionally, from Fig.1e it can be inferred that the data follows a log-normal distribution.

In such case, the ratio of geometric mean (10^mean(log(data)) to the standard deviation in the log space (std(log(data)) would be more accurate metric to produce Fig 1e and possibly the spatial pattern will be different.

Response:
Thank you very much for your extremely valuable comments. Regarding the statistical technique of normalizing the lognormal distribution, we fully agree with your feedback.

However, we believe the frequency distribution of chlorophyll-*a* time series data across the study area's grids does not consistently follow the same pattern. In some pixels, it tends to be more normally distributed, while in others, it is significantly skewed. For example, some grids show a more normal distribution, even when the absolute values are not particularly high, whereas others experience occasional sudden blooms (e.g., >10 mg m^{-3}). The logarithmic scale representation in the old Fig. 1e also reflected these spatial differences in the regimes as background information.

Therefore, we believe that applying uniform logarithmic normalization carries the risk of introducing biases when considering meaningful time-averaged regimes in real marine environments. From this perspective, our approach in this study focuses on the former regime (i.e., a more normally distributed regime in terms of time variability).

In light of this, we have defined the value as an index, I_{chl}, and clarified the explanation, while avoiding terms that implicitly assume a normal distribution, such as standard deviation and normalization. In response to R#1's comments, we have provided

a simpler explanation of I_{chl} in the main text, with a more detailed explanation included in the caption of Fig. 1.

Please refer to the 3rd paragraph in the Introduction.

==========

R03-D10:
L82-91: I tried to revise the paragraph to make more sense of it but the flow and connectivity of the sentences is not clear. Further, how does "Fig. 1e indicates that there may be a nutrient supply point at the Shiriya Spur that more directly impacts biological production within the Tsugaru Gyre"?

Response:
Thank you very much for your comments and specific suggested revisions. For this section, we believe that rewriting the text as you suggested (R03-ES11, L82-91) has significantly improved it. We further emphasized this point by referencing I_{chl} in the sentence.

Please refer to the 4th paragraph in the Introduction.

==========

R03-D11:
L93: Again, it is not clear what is the intended meaning of "continuous phytoplankton culture".

Response:
Thank you for your comment. For this part as well, we believe it has been clarified by rewriting the draft text as you suggested (R03-ES12, L92-9). Thank you very much for your specific guidance.

Please refer to the last paragraph in the Introduction.

==========

Results: “Seasonal variability characteristics…”

R03-D12:
L107-9: I do not understand why model outputs would be needed to capture the general pattern already available from satellite observations.

Response:
In the previous draft, the sentences were added to demonstrate that the model represents the seasonal change well. Based on your comments, we felt our intention would be clearer by only including the sentence “represent”, so we have removed the comparison with OSCAR.

Given the time and effort required for another review, we have been careful not to make the revision too detailed or complex. Instead, we have addressed this by moving the figure to the Supplementary section.

==========

R03-D13:
L110: In the previous revision, there was a comment about the difficulty in comparing sub-figures with different scales. I notice that in Fig. 1a-b, the x-axis range differs from that of the bottom Fig. 1c-d. This hinder readers from making a simple and straightforward connection between the surface (top figures) and vertical profile (bottom figures). Aligning the x-axis range across these sub-figures would facilitate easier comparison and clarity overall.

Response:
Thank you for your comment. We understand that you were referring to Figure 2. We have moved this figure to the supplementary section and corrected the points you mentioned, including aligning the *x*-axis (longitude).

==========

R03-D14:
L119: "...approximately 41.6°N–41.7°N, 141.5°E–141.7°E" -> a marker indicating this location on Fig. 1a-b would improve the presentation.

Response:
We understand that this comment refers to the previous Fig. 2, which has been highlighted in a yellow rectangle based on your feedback. Please note that this figure has been moved to the supplementary section.

Please refer to the Supplementary Fig. 1.

==========

Results: "Summertime chlorophyll-*a* distribution…"

R03-D15:
L135: Lines 639-644 are the only place where CBEM is discussed in the Methods section. I suggest moving this information earlier in the text to provide readers with the necessary context without requiring them to search for details presented in just six lines, especially because the following text provides more detail for the VGPM.

Response:
The description in the Methods section has been relocated to the appropriate part of the text.

Please refer to the 1st paragraph in the section.

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R03-D16:

L129-49: I find it difficult to follow the text here. The introduced PP values seem inconsistent with observed summer estimates. So, what is the point of including these values? Further, paragraph L147 abruptly shifts to a different topic, leaving the previous PP discussion unresolved. It is not immediately apparent how the Shiriya Spur connects to the idea of sustained nutrient delivery that bolster production within the Tsugaru Gyre. Please help the reader by making the connection and logical progression of paragraphs' ideas more seamless.

Additionally, does "This estimate of PP..." refer to the VGPM, or to both VGPM and CBEM? L143 suggests it refers to CBEM. Regardless, it seems both estimates yield similar results However, considering that 16°N–24°N is far south, and KNOT and K2 are far north, outside the current study area, I question whether these comparisons are reasonable given the significant differences in environmental conditions. That is to say, are the limiting factors for PP similar across all these regions? The text is quite convoluted and should benefit from clarification.

Response:

Thank you for your valuable remarks. We believe these have been partly addressed following the proposed text in R03-ES13 (L141-6).

We considered that the difficulty with this sentence stemmed from making assumptions without explaining the differences in productivity between the subtropical and subarctic zones. We also recognized that comparing the productivity of the subarctic region with that of the subtropical region could complicate the discussion, as you pointed out. Therefore, in the revised version, we have limited the comparison to information within the subtropics.

We believe that this revision clarifies the significance of the comparison and improves the connection to the following paragraph.

Please refer to the 1st and 2nd paragraphs in the section.

==========

Results: "General situation at the time of the survey" in the previous manuscript (removed section in the revised manuscript)

R03-D17:

L168: "Therefore,...increase in chlorophyll-a east of the Shiriya Spur implies a nutrient supply near the spur..."? Why and how an increase in chlorophyll-a east of the Shiriya Spur necessarily imply a nutrient source near the spur?

Response:

Thank you very much for your valuable comments. It was suggested that this paragraph be moved to follow the results of the drift observations (R03-O02), and that Fig. 1 be simplified (R03-O01).

After careful consideration, we have decided to delete this paragraph entirely. If it had been moved, it would have overlapped with the following section "Intra-seasonal spatial

and temporal changes in the surface velocity field around the Shiriya Spur," and with the content of the new Fig. 4.

This paragraph was included in the first draft but became somewhat out of place during the revision process. Thank you for your understanding.

==========

Results: “Turbulence over the spur and associated diffusive nitrate flux”

R03-D18:
L185: Where to look for dissipation rate east of 141.68°E in Fig. 3b?

Response:

Thank you very much. The correct figure was the previous Fig. 3a. Please note that the observation result, previously Fig. 3, has been renumbered as Fig. 2 because the JCOPE-T DA figure has been moved to the Supplementary section, as per the comment.

==========

R03-D19:
L206: "...including the Kuroshio region". Why should the reader care about the Kuroshio region? What specific phenomenon observed there is similar, and how does it relate to this study's findings? Providing this connection would help clarify its importance.

Response:

Thank you for pointing this out. This was meant to illustrate a case of a strong current overcoming the terrain. To improve clarity and coherence, we decided to generalize the section on the Kuroshio as a case study for the area distinguished from the open ocean, including the western boundary region, and to explain it in a separate sentence.

Please refer to the 2nd paragraph in the section.

==========

R03-D20:
L219: Is Fig.3b the right reference here?

Response:

Apologies for the lack of clarity. The information on density was originally included in the old Fig. 3b, and we aimed to align with that.

However, this sentence was considered redundant during the revision process and has since been removed. We would like to thank you in advance for your understanding.

==========

==========

R03-D21:
L220: Color should mean contour in nitrate gradient of Supplementary Figure 2?

Response:
The vertical gradient (divergence) of the flux corresponds to the colors in the Supplementary Fig. 3, with the contours also colored for clarity.
However, this sentence has been deemed redundant and has been removed during this revision. Thank you for your understanding.

==========

Results: “Divergence east of the spur and chlorophyll-*a* variation…”

R03-D22:
L376-7: So far, there has never been any mention or definition of subarctic water. So, the text here will certainly confuse any reader readers.

Response:
Thank you for your valuable comments. In response, and in line with our goal of maintaining brevity throughout, we have deleted the sentences before and after this one, including the mention of subarctic water.

==========

R03-D23:
L378: The phrase "The sensitivity to the selected area was also notable." is also vague and unclear.

Response:
Thank you for pointing this out. We have revised the sentence to better emphasize the comparison between the topography of the east and west.

Please refer to the last paragraph in the section.

==========

R03-D24:
L380: It is better to be more specific about "high-frequency component".

Response:
Thank you for your comment. In conjunction with the following sentence, we have modified the part.

Please refer to the last paragraph in the section.

==========

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A few editorial suggestions (ES)

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Thank you very much for your specific suggestions for revised text. We have incorporated many of these suggestions into our own text. Due to the use of an English review service (AJE), some wording has been slightly modified. Please keep this in mind.

==========

Title

R03-ES01:
L1: Topographically-driven vigorous vertical mixing supports sustained mesoscale biological production in the Tsugaru Gyre?

Response: The title has been revised in accordance with the comments.

==========

Abstract

R03-ES02:
L30: These processes are caused by the presence of a distinct topographic feature—an underwater seamount—within the path...

Response: Revised to take into account the comments from R#1 as well.

==========

R03-ES03:
L35: The results of this study emphasize the importance of re-evaluating nutrient supply processes in respect to submesoscale interactions with underwater seamounts for large-scale enhanced biological productivity.

Response: Revised according to your suggestion.

==========

Introduction

R03-ES04:
L41: Among the different nutrient supply mechanisms to the euphotic zone, tidal-driven flows over seamounts are noteworthy for their enhanced vertical mixing.

Response: We have made the revisions as you suggested.

==========

R03-ES05:
L44-9: "...thus affecting nutrient supplies from greater depths" -> thus positively affecting nutrient supplies from greater depths to the surface?

Additionally, nutrient supply through horizontal advection by mesoscale eddies has been demonstrated to influence phytoplankton distributions.

The functional interplay among topography, strong currents, and eddies exerts control over chlorophyll-a distribution on a long-term and broad-scale through continuous or repeated nutrient supply processes near seamounts and subsequent strong horizontal advection.

Response:
We have revised it according to your suggestion and have also included comment R#1. Thank you for your understanding.

==========

R03-ES06:
L52: a potent -> a strong

Response:
This sentence was removed during the revision process. Thank you for your understanding.

==========

R03-ES07:
L50-8: In this study, we investigated the interaction between a strong current and local topography, revealing that this interplay fosters a vigorous nutrient supply within a confined area at the outlet of the Tsugaru Strait in the North Pacific, Japan. We suggest that this steady supply of localized nutrients supports sustained phytoplankton growth in the downstream environment. Consequently, studying these mesoscale processes (O(100) km) can provide significant insights into ecosystem dynamics while being essential for establishing key observation and monitoring sites.

Response:
It has been revised according to your comments. During the reorganization of the overall structure of the introduction, the last sentence was further revised. Please note this.

Please refer to the 2nd paragraph in the Introduction.

==========

R03-ES08:
L59: Move this paragraph right after L50.

Response:
Thank you for your comment. During the revision of the Introduction, the relevant section has been moved to the second paragraph.

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R03-ES09:
L65-9: The seasonal variations in the horizontal flow path are accompanied by significant changes in the vertical outflow characteristics over the topographic feature known as Shiriya Spur, located near 41.4°N–41.7°N, 141.5°E–141.6°E, and extending northward from Cape Shiriya (Fig. 1e).

Response: Revised according to the suggested text.

Please refer to the 2nd paragraph in the Introduction.

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R03-ES10:
L76-9: The long-term (2014-2022) mean distributions of surface chlorophyll-a concentrations in August and September, normalized by its pixelwise standard deviation, revealed...

Response:
Thank you very much for your suggestion. Please also refer to our comments in R03-D09.

Please refer to the 3rd paragraph in the Introduction.

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R03-ES11:
L82-91: The study by Tanaka et al. reported a vertical turbulent nutrient flux over
a sill located at approximately 140.35°E (off Cape Tappi region) in the western part of the strait (Fig. 1f). Their reported flux of 1 mmol N m^2 day^{-1} [$O(10^{-5}$ mmol N m^{-2} $s^{-1})$] (at the surface?) is significantly greater than values observed in the open ocean. While this nutrient supply off Cape Tappi is substantial, the sustained productivity in the Tsugaru Gyre suggests the presence of an additional nutrient source, given the distance from Cape Tappi. The Tsugaru Current, which passes over the Shiriya Spur, may induce disturbances that more directly impact the biological production within the Tsugaru Gyre. However, the nutrient supply capacity at this topographic feature, the Shiriya Spur, has not been assessed, nor has there been a study linking turbulence intensity (over several hours to days) with large spatiotemporal variations in biological production.

Response: We have made corrections based on the comments we received.

Please refer to the 4th and 5th paragraph in the Introduction.

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R03-ES12:
L92-9: To better understand the drivers of the sustained phytoplankton productivity downstream of the Tsugaru Current, we conducted drift turbulence measurements following
an advected water column in the eastern part of the strait, crossing the Shiriya Spur. In

this area, continuous surface current velocity data from a high-frequency radar (HFR) system have been collected since 2014. Through a combination of turbulence observations, HFR
monitoring, and with the aid of ocean data assimilation model (JCOPE-T DA) outputs, we suggest that nutrient supply at the submesoscale around the Shiriya Spur sustains biological productivity downstream.

Response:
Thank you for your suggestion. We have revised the text in accordance with the comments received, taking into account the feedback from R#1 and including specific values in the meso- and sub-meso-expression.

Please refer to the last paragraph in the Introduction.

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Results: "Summertime chlorophyll-a distribution and primary production…"

R03-ES13:
L141-6: The above PP estimates exceed the reported values for summer in the western North Pacific subtropical gyre. However, they are within the range of reported values for the summer to fall seasons in the subarctic region of the western North Pacific.

Response:
Thank you for your comments. We have incorporated and amended the first half of this proposal. We have omitted the second half, which compared it to the subarctic zone, in order to keep the content concise. Please also refer to comments R03-D16.

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Results: "General situation at the time of the survey" in the previous manuscript (removed section in the revised manuscript)

R03-ES14:
L159: During the turbulence measurements, conducted on 26th August 2021, the eastward current flowed... Concurrently, relatively high surface chlorophyll-a concentration were observed from the eastern outlet of the strait (approximately 141.75°E, i.e., in the area immediately adjacent to the east side of the Shiriya Spur) to approximately 142.4°E downstream, based on 16-day composite from 21st August to 5th September (Fig.1f).

Response:
We have removed this section. Please see also our comments in R03-O01, and R03-O02. Thank you for your understanding.

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Results: “Turbulence over the spur and associated diffusive nitrate flux”

R03-ES15:

L208: The magnitude was also approximately 100 times greater than that estimated for the Cape Tappi in the western strait topography.

Response: We have amended the wording based on your suggestion.

Please refer to the 2nd paragraph in the section.

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R03-ES16:

L229-35: The obtained value for Frl was greater than 1 at both 141.56°E and 141.58°E, where intense eastward flow and an undulating isopycnal surface were observed (Fig. 3e). This suggests the presence of an internal hydraulic jump. Specifically, when the background flow velocity approximates the propagation speed of the internal gravity wave, stagnation and the superposition of internal gravity waves near the critical point ($Fr_l = 1$) are expected. This condition leads to significant isopycnal displacement, resulting in heightened turbulence due to instability.

Response: Thank you for your suggestion. We have revised that section as you suggested.

Please refer to the 3rd paragraph in the section.

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Dear Reviewers,

Once again, we would like to express our gratitude for your valuable time, as well as your helpful comments and suggestions on our previous submissions. In this round, we have only received the review comments from Reviewer #3, so we did not create a general list. Instead, we will respond directly to Reviewer #3's comments individually. Thank you for your understanding.

Please note that in the history file (TrackedChanges), the yellow highlights indicate revisions primarily based on individual comments from R#3.
Light grey highlights represent other changes made based on our overall judgment.
The history reflects revisions made by the English editing service (AJE).
Dark grey highlights indicate revisions made by the authors after the English editing.

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Reviewer #3

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I would like to commend the authors for their responsiveness to reviewer comments and their considerable effort to make the manuscript more accessible. It is now significantly easier and more pleasant to read and grasp the ideas being introduced. The text has clearly improved in clarity and conciseness. While I still find a few sections in need of further revisions, which I detail below, I believe these are minor issues that can be resolved in preparation for publication.

Due to time constraints, I have not separated my commentary from editorial suggestions; please see specifics below.

Response:

We sincerely appreciate all of your essential, valuable, and insightful comments and suggestions. We will address each point individually below. This process has been an incredibly valuable experience for me. Thank you once again.

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Title:

R03-01:

I might be missing the point, but “vertical mixing sustained” and “vertical mixing supports” convey different nuances. The latter seems to better capture the manuscript’s intent to emphasize that “vertical mixing supports” high biological productivity in Tsugaru Gyre.

Response:

Thank you very much for your valuable comments. We have made the corrections as suggested.

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Abstract:

R03-02:

L32-35 blends general and specific information resulting in a diluted message that doesn't clearly articulating the manuscript's main point. Please consider the following:

“Our long-term datasets revealed a positive correlation between recurring submesoscale divergence related to the topographic feature and the downstream distribution of chlorophyll-a. This finding reinforces the significant role of the underwater seamount in enhancing the productivity of the downstream anticyclonic gyre.”

Response:

Thank you very much for your detailed suggestions. We have made the revisions as you recommended.

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Introduction:

R03-03:

Integrate L48 into L49 for a seamless flow

“In this study we introduce a case study to address the above issue by investigating the interaction between a strong current and local topography, revealing that...”

Response:

Thank you very much for this as well. We have made the revisions as you suggested.

Please also refer to the 2nd paragraph in the Introduction section.

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R03-04:

L54: Fig. 1b and c show seasonal changes in Tsugaru Gyre characteristics, yet the text discusses “the flow path of the current...”, in this case, the Tsugaru Warm Current. This reference to Fig. 1b and c may be misleading or confusing to readers. Additionally, is the text implying that the Tsugaru Warm Current (Fig. 1a) and the Tsugaru Gyre (Fig. 1b and c) are the same thing? Please clarify the reference to Fig. 1b and c.

Response:

Thank you very much for your comments. We have addressed this issue by adding the flow path information to Figure 1a as a schematic and including an additional explanatory note in the text.

Please note that Fig. 1a has also been revised to differentiate the design from the figures in our previous paper, as there was a query in the Author Checklist stating, “The use or adaptation of previously published images is strongly discouraged.” Thank you for your understanding.

Please also refer to the 2nd paragraph in the Introduction section.

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R03-05:

L68-80: “To quickly test this hypothesis, we normalized the long-term (2014–2022) seasonal (August and September) mean surface chlorophyll-a concentration (mg m^−3) by its pixelwise standard deviation (mg m^−3) and denoted the normalized data as Ichl (unitless). This index (Ichl) revealed elevated values (>2) within the gyre compared to those outside the gyre (Fig. 1e). Assuming that the chlorophyll-a concentration at each grid is normally distributed during the season, the elevated value of Ichl within the gyre supports the notion of enhanced chlorophyll-a levels with small temporal variation during that season. The Ichl exhibited a horizontal maximum in the region corresponding to the area immediately east of the spur (approximately 41.7°N, 141.6°E–141.7°E; Fig. 1e). This suggests the existence of a nutrient source around the spur that supports stable chlorophyll-a levels, as shown by Ichl.”

Response:
Thank you very much for your valuable suggestions. I have rewritten the text based on your recommendations.

Please also refer to the 3rd paragraph in the Introduction section.

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R03-06:

I believe the assumption of normality for chlorophyll-a data can be tested and may not be readily accepted by biological oceanographers. It would be beneficial to address this assumption more critically in the text.

Response:
Thank you very much for your substantive comments. In response to your feedback, we have clearly stated that normality is an assumption in this study, rather than a general condition for all oceans. Additionally, we decided to strengthen our argument that assuming a bell-shaped distribution is reasonable in the study area by adding a histogram of chlorophyll-a for August and September near the Shiriya Spur, with Fig. 1f as a sub-panel.

Please also refer to the 3rd paragraph in the Introduction section and Fig.1f.

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R03-07:

L75: “As chlorophyll-a concentrations within the gyre remain above zero during the season” What does this mean? Is there any period when chlorophyll-a levels are below zero?

Response:
Thank you for your comment. This anticipates the reader's question regarding the denominator issue (mean chl → 0). We believe we have addressed this by providing an example of a specific chlorophyll-a concentration distribution, as shown in R03-06.

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R03-08:

L86: “...given the distance from Cape Tappi and the spatial distribution of Ichl...”

Response: We have rewritten the text based on your suggestion. Thank you.

Please also refer to the 4th paragraph in the Introduction section.

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Results:

R03-09:

L106: I believe that including a paragraph to introduce the results would greatly enhance the reader's experience. Currently, there is a significant jump from the introduction to the results, which may disrupt the flow.

Response:

Thank you very much for your constructive suggestions. We have modified the structure as you recommended, along with R03-10.

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R03-10:

L105: “Firstly, we briefly present the seasonal variability characteristics of the Tsugaru Warm Current and Tsugaru Gyre, and associated topographically driven upwelling and surface velocity divergent field, on the east side of the Tsugaru Strait, using outputs from the JCOPE-T DA (Supplementary Fig. 1, with the 2019 output as an example; see the Methods section). This analysis is motivated by the challenges in continuously and repeatedly acquiring the spatial structure of surface currents, including the vertical velocity of the ocean interior. We then introduce results based on field observations using HFR and turbulence measurements. Finally, we discuss the implications for productivity in the Tsugaru Gyre using long-term chlorophyll-a data and empirical estimates of primary productivity.”

Seasonal characteristics of velocity divergence from the JCOPE-T DA model

“The outputs from the JCOPE-T DA model reasonably reproduce the seasonal variations in the current and the formation of the Tsugaru Gyre (Supplementary Fig. 1a and b). For the vertical east–west section of each season at 41.65°N...”

Response:

Thank you very much for your helpful suggestions. We have revised the structure and text as you recommended."

Please also refer to the 1st paragraph in the Results section, and the subsequent paragraph in the “Seasonal variability characteristics of the topographically relevant upwelling and surface velocity divergence of the Tsugaru Warm Current - implications of numerical modelling”

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R03-11:
L130: Adding a reference would help support this claim and strengthen the argument.

Response:
Thank you very much. We have decided to add Isada et al. (2017) as a reference.

Please also refer to the 2nd paragraph in the "Seasonal variability characteristics of the topographically relevant upwelling and surface velocity divergence of the Tsugaru Warm Current - implications of numerical modelling" section.

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R03-12:
L133 could be moved after the results of turbulence and nitrate fluxes.

Response:
Thank you very much. Following your suggestion, we have moved the first half of this section, including the first paragraph, to the Discussion.

Please refer to the 3rd paragraph in the Discussion section.

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R03-13:
L135-137: This information is redundant and can be removed, as it has already been discussed in the introduction.

Response: Thank you for your comment. We have removed it as you suggested.

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R03-14:

L153-156: I am not clear what this statement is trying to convey. It feels too speculative and should be removed from the results. A clear statement of the rationale behind this comparison would enhance clarity.

Alternatively, it would be more reasonable to focus on the estimates of nutrient input resulting from the interaction between the Tsugaru Warm Current and the spur, assessing its impact on primary production during August and September against background conditions. This approach would provide a clearer understanding of the contribution.

Response:
Thank you for your comment. As you suggested, we have revised the information to remove it.
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R03-15:
L157 feels like the introduction of L169.

Response:
Thank you for your comment. We have moved this paragraph to the beginning of the next section.

Please refer to the 1st paragraph in the "Turbulence over the spur and associated diffusive nitrate flux" section.

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R03-16:
L164: "The R/V's travel distance"?

Response: We have made the correction according to your comment.

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R03-17:
L180: indicating→associated with?

Response: We have modified it as you suggested. Thank you.

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R03-18:
L200: "...western strait topography...?"

Response: Thank you for your comment. We have revised it.

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R03-19:
L205: "This result suggests that the nutrient content at the surface on the east side of the spur is greatly influenced by vertical nitrate fluxes."

Response: Thank you for your comment. We have made the corrections as you suggested.

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R03-20:
L266: "...from→during..."

Response: We have corrected it as you indicated. Thank you.

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R03-21:

L906: The concept of normalized chlorophyll-a is becoming unnecessarily complex. The notation 〈chl(x,y,t)〉 is not clear, as the "t" inside the brackets makes it difficult to interpret the average. What exactly does it convey? If we are to calculate the pixelwise mean at a given time, shouldn't it simply correspond to the original value?

Response:

Thank you very much. Based on your suggestion, we have removed the *t* in the bracket.

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R03-22:

L951: "from" July to November 2021, or for July "and" November 2021?(Remarks on code availability)

Response: Thank you very much for pointing out that it is "from" July to November.

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R03-23:

The code does provide a README file. However, I have not installed or tried running the application.

Response:

Thank you very much for your comments. We understand that testing our code in the same environment we use may not be easy, but we will eventually upload it to the repository.

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