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Peer Review File

Oceanic eddies induce a rapid formation of an internal wave continuum

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17th Jul 23

Dear Dr Yang,

Your manuscript titled "Oceanic eddies induce a rapid formation of an internal wave continuum" has now been seen by 3 reviewers, and we include their comments at the end of this message. They find your work of interest, but some important points are raised. We are interested in the possibility of publishing your study in Communications Earth & Environment but would like to consider your responses to these concerns and assess a revised manuscript before we make a final decision on publication.

We, therefore, invite you to revise and resubmit your manuscript, along with a point-by-point response that takes into account the points raised. Please highlight all changes in the manuscript text file. As a guide, in the revised manuscript, please discuss your results in the context of the background literature and bring out the novel contributions, as pointed out by the reviewers.

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

Please use the following link to submit your revised manuscript, point-by-point response to the referees' comments (which should be in a separate document to any cover letter) and the completed checklist:
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We hope to receive your revised paper within six weeks; please let us know if you aren't able to submit it within this time so that we can discuss how best to proceed. If we don't hear from you, and the revision process takes significantly longer, we may close your file. In this event, we will still be happy to reconsider your paper at a later date, as long as nothing similar has been accepted for publication at Communications Earth & Environment or published elsewhere in the meantime.

Please do not hesitate to contact us if you have any questions or would like to discuss these revisions further. We look forward to seeing the revised manuscript and thank you for the opportunity to review your work.

Best regards,

Viviane V. Menezes, PhD
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Communications Earth & Environment
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Heike Langenberg, PhD
Chief Editor
Communications Earth & Environment

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REVIEWER COMMENTS:

Reviewer #1 (Remarks to the Author):

Major comment

My major comment is that it is not clear to me in which way the work presented here goes significantly beyond the original work on the same topic in reference [18], ie Barkan, Winters, and McWilliams 2017. Fundamentally the same dynamics is investigated in both papers and fundamentally the same conclusions are drawn about the crucial importance of eddy-wave interactions for the evolution of the wave field and subsequent energy dissipation. The main novelty seems to be that this is now done with a different numerical model, with somewhat more realistic wind forcing, and with some new diagnostics, but that seems not very much novelty compared to [18]. Whether this is enough for publication in this journal is an editorial decision.

Minor comments

1. It might be worth mentioning in the introduction that the decay time scale of the internal wave spectrum is generally believed to be very short (a few weeks?), which strengthens the case that a fast generation mechanism for the wave continuum is needed.

2. p4: I was surprised that the net dissipation in the eddy-wave and wave-only cases was almost identical. Naively it would appear that the dissipation ought to be stronger in the eddy-wave case, where more wave energy is pushed to smaller scales. Could you discuss this?

3. How did the net, integrated wave energy compare between eddy-wave and wave-only simulations? Some of the approximate theories for this process rely on action conservation and therefore can predict the implied wave energy changes, and it would be interesting to see this diagnosed in this more direct numerical simulation.

4. Are the observed dissipation hot spots in figure 1 correlated with any particular flow features?

5. I found it a little tedious to bear the six different simulations in mind at all times, which also made the figures very busy. I would recommend presenting only the results from the three 500m runs in the paper and relegate the corresponding plots for the other three 2km runs to the supplementary material.

Reviewer #2 (Remarks to the Author):

This is a well written and interesting numerical study finding that the maintenance of the continuum spectrum is inextricably tied to the mesoscale eddy field. The problem of internal wave and dissipation timescales and energetics is very timely and the paper has the potential to be a nice contribution to this.

I think it is ultimately publishable but would recommend major revisions, since as written it is missing and/or under-describing a significant portion of the literature; specifically older numerical studies showing much shorter times than years, and the Pinkel 2014 work addressing Doppler shifting. I also think the paper misses an opportunity to describe the physics and mechanisms of the eddy-IW interactions.

Major comments:

I do not think that a paper on this topic can be written that does not discuss cite Pinkel (2014). Since that paper's focus was whether Doppler shifting can explain the entire continuum spectrum, this is a pretty significant omission.

The authors' finding that Doppler shifting is not important is surely at least partially result to the deceptive description of "Lagrangian" spectra that have been computed using only horizontal motions, not the vertical motions. A significant literature on fine structure contamination exists but is missing in this document. Pinkel (2014) covers both the vertical and horizontal aspects of this.

I think that the inference of a multi-year timescale for formation of the GM spectrum from the spin-up timescale is a significant overestimate that is at odds with the rest of the literature on wave-wave interactions. Even the now-ancient work by Olbers and other pioneers in this field found tens of days rather than years. Hibiya et al (2002) and other uncited papers find still faster time scales. More recent work (e.g. Le Boyer, which is cited) and unpublished work by Giovanni Dematteis seems to suggest much shorter lags. This paper needs to do a better job of addressing the work by others on this topic.

The manuscript refers to "new physical processes" but in fact no mechanism is given for *how* the eddy field catalyzes the transfer rates.

Minor comments:

It will surprise no one that no IW continuum arises when only eddy forcing is supplied. Suggest minimizing those comparisons between eddy-only and wave. The important comparisons are between wave only and eddy-wave.

p4, last para, first line: suggest "lateral" not "spatial."

Section 2.2: last sentence in first paragraph is wordy/awkward.

2.3 first paragraph: please state why the frequency/horizontal wavenumber spectrum is the right quantity to show this.

The peaks at (2,3,4)f are not observed in the thermocline. So this appears to be a model artifact that should be explained.

p9: last sentence before Discussion: a statement on lateral scales of of dissipation is made from the co-spectrum. Strictly that gives coherence scales not scales of dissipation as stated.

Reviewer #3 (Remarks to the Author):

Referee report on 'Oceanic eddies induce a rapid formation of an internal wave continuum'
by Luwei Yang et al

This paper examines the mechanisms causing the energy of wind-driven ocean internal waves to spread in wavenumber and frequency space. Conventional wisdom is that this spreading, which leads to a continuum in both wavenumber and frequency, is the result of nonlinear wave–wave interactions. The paper relies on a series of numerical simulations to demonstrate that in energetic parts of the ocean (here the subpolar Atlantic Ocean) the interactions between waves and eddies provide a much more effective mechanism of wave energy redistribution than wave–wave interactions which act on unrealistically long time scales.

The paper is very interesting and clearly written. The simulations have been carefully set up and analysed. Overall, the paper makes a convincing case for the dominant role of eddies in shaping the internal wave spectrum in the ocean. I think it can play an important part in shifting the views of the physical oceanography community which for many years has overemphasised the importance of wave–wave interactions.

I think the paper could be improved in one respect. Several recent works study how eddies redistribute wave energy through a scattering process associated with triadic catalytic interactions. The paper cites these as [22-26] in the introduction but does not engage further with the results. It would be good to discuss which, if any, of the theoretical predictions are supported by the paper's simulations. These predictions are of a k^{-2} horizontal spectrum and of a frequency spreading that depends on the unsteadiness of the eddies and hence is expected to be much slower than the wavenumber spreading (see also Cox et al 2023, Inertia-gravity-wave diffusion by geostrophic turbulence: the impact of flow time dependence, *J. Fluid Mech.*, **958**, A21). In this respect, I wonder whether the continuum for frequencies greater than f in the eddy-wave simulation (Fig. 3) can confidently be interpreted as an internal wave response. The energy level is much smaller than that of the inertial peak and rather sensitive to resolution and whether the spectrum is Lagrangian or Eulerian. I can imagine various contaminations (e.g. by the eddies themselves) unrelated to wave dynamics leading to such a signal.

In the same spirit of engagement with earlier theoretical work, it would be interesting to discuss further whether resonant wave–wave interactions play any role, e.g. at the smallest scales, and whether predictions for energy transfer rates stemming from kinetic theory (e.g. Ref. [14]) are consistent with the (wave-only) simulations.

I also have the following comments that the authors could address.

1. References [15] and [16] are cited on p2 as 'recent modelling studies' but date back from 2009 and 2015. Omit 'recent'. Are the results of these modelling studies consistent with the direct estimation of energy transfer rates from the kinetic equation, as performed in [14]?

2. Figs. 3 and 4: the text and captions refer to the power laws ω^{-2} and k^{-2} as consistent with GM. As mentioned above, k^{-2} is also the prediction for scattering by eddies. Since the authors have access to the full fields, would it be possible to assess the consistency with GM by a comparison between the full (ω, k) spectra and the GM spectrum?

3. Fig. 4: In the eddy-wave simulation, it is striking that the peaks at $\omega = 2f$ and $3f$ also have a specific spatial scale? Is there an explanation for this?

4. p7, 1st para: I am confused by the statement attributing the elevated Lagrangian spectrum to sweeping by currents. Isn't this the kind of Doppler shifting effect that Lagrangian averaging removes?

5. p8, penultimate sentence: I don't follow the comment. The dissipation is localised in frequency but not in wavenumber. How is that the result of wave energy being redistributed across frequencies? Do you mean between harmonics? The impression I get is that wave–wave interactions generate harmonics of f and that there is little other frequency spreading (but a good deal of wavenumber spreading).

6. p10, l9: There is a potential for confusion about what is meant by 'Doppler shifting' here. The scattering of wave energy by eddies can be traced back to the Doppler shift term of the dispersion relation: this introduces spatial inhomogeneity and hence refraction and scattering. Doppler shift is meant here in a narrower sense, I think, but I am not sure which.

7. p12, 3rd full para: I think the statement that the total Lagrangian energy is equal to the total Eulerian energy is false (averaging and taking the square do not commute), independently of the area preservation issue mentioned. I certainly have never seen an argument showing they are equal.

Oceanic eddies induce a rapid formation of an internal wave continuum

L. Yang, R. Barkan, K. Srinivasan, J. C. McWilliams, C. J. Shakespeare, A. H. Gibson

We thank three anonymous reviewers for providing very helpful and constructive comments, which have been taken into account in the revised manuscript. In response to these comments, we have highlighted the novelty of this study, further discussed our results in the context of the background literature, and made other corrections as suggested.

Please find responses to the individual suggestions below. Each of the points raised by the reviewers is reproduced in blue, with our response following, and the modified text included in italics with quotation marks. The line numbers in our response refer to those in the revised manuscript.

Response to Reviewer 1:

Major comment:

My major comment is that it is not clear to me in which way the work presented here goes significantly beyond the original work on the same topic in reference [18], ie Barkan, Winters, and McWilliams 2017. Fundamentally the same dynamics is investigated in both papers and fundamentally the same conclusions are drawn about the crucial importance of eddy–wave interactions for the evolution of the wave field and subsequent energy dissipation. The main novelty seems to be that this is now done with a different numerical model, with somewhat more realistic wind forcing, and with some new diagnostics, but that seems not very much novelty compared to [18]. Whether this is enough for publication in this journal is an editorial decision.

In this work we combine realistic submesoscale-permitting and resolving ocean simulations (that have been extensively validated against remote sensing and in situ observations; see Barkan et al. (2021)) and a *single* idealized spatially-uniform inertially-resonant storm to study the rapid emergence of the internal wave continuum as an initial value problem. As such we believe this hybrid realistic-idealized configuration represents a novel paradigm on the intersection of geophysical fluid dynamics and numerical ocean modeling. We also specifically examine the impact of submesoscales on the continuum emergence by performing this methodology across two different resolutions, one of them submesoscale-resolving, to show that submesoscales aid in reducing the presence of the higher harmonic peaks and in the formation of the continuum; we specifically ensure that these Eulerian spectra are not a consequence of horizontal Doppler shifting by computing Lagrangian spectra.

To highlight novelties discussed above, we have modified the sentence in L90-93 to say that,

> "The first set (the eddy-wave case) *is a novel hybrid idealized-realistic computational paradigm that* simulates a realistically forced oceanic eddy field and a wind-forced internal wave field generated by *a single* idealized wind event characteristic of a winter storm in the study region."

We have also added a sentence in L96-101,

> "*Each of these three sets of simulations was computed at two different horizontal resolutions, the first at a mesoscale-resolving 2 km horizontal grid spacing and the second at a submesoscale-resolving 500 m horizontal grid spacing (i.e., Srinivasan et al., 2023); this choice explicitly allows us to evaluate the role of submesoscale currents in the formation of the internal wave continuum* (further details...)."

We have revised the sentence in L316-322 to provide details for the low-frequency wind forcing used in the eddy-only case,

> "*The eddy-only case is forced by the daily realistic low-passed wind stress and initialized with an eddy field. The daily low-passed wind stress (Supplementary Fig. 1) is produced by a simulation forced by daily low-passed wind velocity from the NCEP Climate Forecast System Reanalysis (CFSR) (Saha et al., 2010) with hourly temporal resolution (Barkan et al., 2021). The eddy field used as the initial and boundary conditions is taken from the smooth forcing simulation in Barkan et al. (2021) that has been validated against the AVISO dataset, Argo- and mooring-based measurements collected in the study region.*"

The study by Barkan et al. (2017) on the other hand employed a highly idealized ACC-like channel model that lacked a realistic surface mixed layer parametrization and consequently lacked a realistic submesoscale field (it is dominated by a few isolated submesoscale coherent vortices and a single dominant front instead of the sea of mixed-layer eddies and fronts representative of a realistic winter mixed layer seen here) and is forced by a continuous stochastic white-noise forcing to generate internal waves in a forced-dissipative way. The assertion that the Barkan et al. (2017) framework is only marginally different from our present paradigm feels like an unfair and inaccurate comparison. Unlike the white noise forcing in Barkan et al. (2017), our choice of a purely inertial storm ensures that near-inertial internal waves (NIWs) must emerge entirely through self-interactions or through interactions with eddies; this paradigm takes inspiration from recent QG-NIW idealized modeling studies (Rocha et al., 2018) which force an eddying homogeneous QG turbulence model with a single inertial oscillation (which is only possible because in those theories, they derive separate equations for waves and the balanced flow). This choice of a single resonant storm, i.e. an initial value problem, allows us to accurately characterize the time-scales of emergence of the internal wave continuum which is to our best knowledge, the first demonstration of this kind in a realistic ocean model configuration.

We also note specifically that the Doppler shifting effects in Barkan et al. (2017) are likely to be strong due do the strong currents in their ACC-like configuration and the Eulerian spectra they show can be difficult to interpret in that light, something we explicitly account for in our present study.

Finally, and somewhat incidentally, we should mention that the focus of the two studies are entirely different and reflected in the choice of the analysis - Barkan et al. (2017) was primarily interested in evaluating the effect of internal waves on catalyzing eddy energy dissipation; the natural follow up of that study was actually Barkan et al. (2021) which examines this issue in realistic setting in the same region as our present study. This present study instead attempts to address how eddies catalyze the formation of an internal wave continuum, a process that we closely relate to the spatio-temporal energy dissipation cospectrum.

Minor comments:

1. It might be worth mentioning in the introduction that the decay time scale of the internal wave spectrum is generally believed to be very short (a few weeks?), which strengthens the case that a fast generation mechanism for the wave continuum is needed.

   We thank the reviewer for the suggestion. We acknowledge this point and indeed, the characteristic internal wave energy dissipation time scale increases from about 20 days at the surface to 50 - 100 days in the interior (McComas and Müller, 1981a,b; Müller et al., 1986; Polzin and Lvov, 2011), which needs a formation mechanism for the internal wave continuum transferring wave energy on a similar timescale to balance. However, as Reviewer 2 pointed out, the yearly time scale is an overestimate for the formation of the continuum due to wave–wave interactions. There are studies that have suggested much shorter time scales for the formation of the continuum due to wave–wave interactions (e.g., Hibiya et al., 2002; MacKinnon and Winters, 2005; MacKinnon et al., 2013). We therefore decided to remove the discussion of contrasting time scale here.

2. p4: I was surprised that the net dissipation in the eddy-wave and wave-only cases was almost identical. Naively it would appear that the dissipation ought to be stronger in the eddy-wave case, where more wave energy is pushed to smaller scales. Could you discuss this?

   Please note that even in the 500 m model we use the actual internal wave dissipation mechanism is not properly resolved and is dominated by the vertical dissipation parameterized by K-profile parameterization (KPP). Once the simulated internal wave field reaches a small enough vertical scale KPP kicks in because a bulk Richardson number threshold is violated and mixes momentum vertically. Based on this comment, however, we now emphasize more clearly the enhanced dissipation in the eddy-wave solution below the mixed layer.

   To highlight the enhanced dissipation below the mixed layer, we have added a sentence in L138-140,

   > *"However, the energy dissipation averaged between 400 m and 800 m in the eddy-wave case (1.7×10^{-9} W kg^{-1}) is one order of magnitude larger*

than that in the wave-only case (1.4×10^{-10} W kg^{-1})."

In the event that the referee is still referencing the Barkan et al. (2017) study, it should be noted that they examined the decrease in eddy energy of the eddy-only case relative to the eddy-wave case and did not explicitly compare the super-inertial dissipation in the eddy-wave case with their wave-only case as we do here so it is difficult to reference them on this aspect. As explained in the answer to the major comment by the referee, this stems from the explicit focus of Barkan et al. (2017) on the eddy kinetic energy dissipation.

3. How did the net, integrated wave energy compare between eddy-wave and wave-only simulations? Some of the approximate theories for this process rely on action conservation and therefore can predict the implied wave energy changes, and it would be interesting to see this diagnosed in this more direct numerical simulation.

   It is challenging to define wave action and track its evolution in realistic numerical simulations. In theory, the initial distribution of wavenumbers generated consequent of the storm forcing can be determined using a Wigner transform (Ryzhik et al., 1996; Savva et al., 2021), which in practice, however, is beyond the scope of the current work but something we plan to address in future studies. The subsequent evolution of wavenumbers can be tracked by a ray tracing approach, which assumes a slowly varying medium and relies on spatial scale separation in the WKBJ regime. However, the WKBJ assumption is violated in our simulations especially during the winter season marked by deep mixed layers of weak stratification; i.e. the wave scales are not much smaller than the flow scales. In spite of this, we did explicitly attempt such a ray-tracing approach for our simulations partly with the hope of addressing questions raised by the referee but abandoned them due to inconsistent results and numerical challenges. We therefore leave the diagnostics suggested by the reviewer to a future study.

4. Are the observed dissipation hot spots in figure 1 correlated with any particular flow features?

   This question is not answered in the current study because it is the focus of a forthcoming publication, which diagnoses a complete energy budget of NIWs including the dissipation term. In this forthcoming paper, we will investigate the correlation between the dissipation in the near-inertial band, computed using the band-pass coarse-graining approach (Eyink and Aluie, 2009), and flow structures in detail.

   A brief answer to the reviewer's question is that the dissipation hot spots shown in Fig. 1 (i.e., super-inertial energy dissipation) in the manuscript are correlated with anticyclonic mesoscale features, which is illustrated in Figures i and ii. The upper panel in Figure i shows the Hövmoller diagram of the correlation between the energy dissipation in the near-inertial band and the daily low-passed vorticity, whereas the lower panel shows the Hövmoller diagram of the correlation between the super-inertial energy dissipation and the daily low-passed vorticity. The correlation is positive below the mixed layer, indicating that the dissipation (negative by definition) correlates with regions of negative vorticity. The spatial comparison of energy dissipation and vorticity shown in Figure ii demonstrates that the dissipation hot spots overlap with regions of strongly negative vorticity, most evidently in anticyclones.

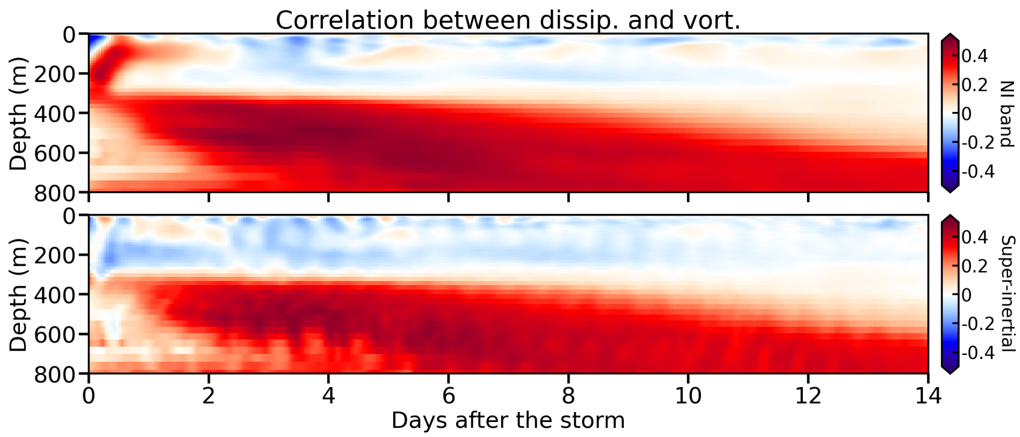

Figure i: Hövmoller diagram of the domain-averaged correlation between energy dissipation and vorticity. The dissipation is shown for the near-inertial band in (a) and for the the super-inertial band in (b). The near-inertial band is defined between the frequencies 1/24 cph and 1/12 cph. The super-inertial frequency band includes frequencies larger than 1/14 cph. The vorticity shown here is low-passed with a cutoff frequency of 1/24 cph.

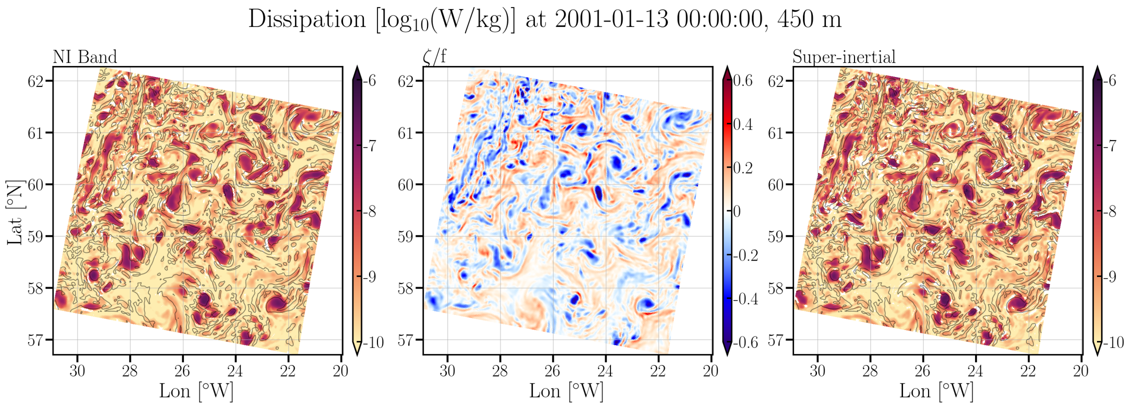

Figure ii: A snapshot of (a) energy dissipation in the near-inertial band, (b) daily low-passed vorticity, and (c) super-inertial energy dissipation 4 days after the storm at 450 m depth from the 2 km resolution eddy-wave case.

5. I found it a little tedious to bear the six different simulations in mind at all times, which also made the figures very busy. I would recommend presenting only the results from the three 500m runs in the paper and relegate the corresponding plots for the other three 2km runs to the supplementary material.

We have considered the reviewer's suggestion and decided to keep figures as they were. Although six simulations may seem like a lot for a first read, it is necessary to show results from both 2 km resolution and 500 m resolution simulations in the main body text. By presenting both 2 km resolution and 500 m resolution simulations, we demonstrate that the eddies, including both mesoscale and submesoscale eddies, are important for the formation of an internal wave continuum.

While mesoscale eddies are resolved in both 2 km and 500 m resolution simulations, submesoscale eddies are only resolved in the 500 m resolution simulations. The role of submesoscale eddies for the formation of the internal wave continuum is demonstrated by the comparison between 2 km and 500 m resolution simulations. For example, the frequency spectrum for the eddy-only case in the 500 m resolution simulation is more energetic at super-inertial frequencies than that in the 2 km resolution simulation at both depths (Fig. 3), which indicates the presence of submesoscale eddy energy at these frequencies. The presence of super-inertial submesoscale eddies is likely the reason why the continuum is better filled up in the 500 m resolution eddy-wave simulation than in the 2 km resolution eddy-wave simulation.

Response to Reviewer 2:

This is a well written and interesting numerical study finding that the maintenance of the continuum spectrum is inextricably tied to the mesoscale eddy field. The problem of internal wave and dissipation timescales and energetics is very timely and the paper has the potential to be a nice contribution to this.

I think it is ultimately publishable but would recommend major revisions, since as written it is missing and/or under-describing a significant portion of the literature; specifically older numerical studies showing much shorter times than years, and the Pinkel 2014 work addressing Doppler shifting. I also think the paper misses an opportunity to describe the physics and mechanisms of the eddy–IW interactions.

We appreciate the reviewer for the positive feedback and constructive comments, which have been addressed below.

Major comments:

I do not think that a paper on this topic can be written that does not discuss cite Pinkel (2014). Since that paper's focus was whether Doppler shifting can explain the entire continuum spectrum, this is a pretty significant omission.

The authors' finding that Doppler shifting is not important is surely at least partially result to the deceptive description of "Lagrangian" spectra that have been computed using only horizontal motions, not the vertical motions. A significant literature on fine structure contamination exists but is missing in this document. Pinkel (2014) covers both the vertical and horizontal aspects of this.

Responding to the points raised by the reviewer on Doppler shifting, we have cited Pinkel (2008) and Pinkel (2014) in L180, we have also added a paragraph in L287-297 in the Discussion section to further discuss our results in the context of previous studies,

> *"As part of our analysis, we find that horizontal Doppler shifting of wave frequency by currents plays a negligible role in distributing wind-forced internal wave energy across timescales. Although, it may be more important in regions with strong currents like those found in western boundary currents and the Southern Ocean (Nagai et al., 2015; Rama et al., 2022). We emphasize that we only consider the effect of horizontal Doppler shifting since the Lagrangian spectra are obtained from advecting water parcels using the horizontal velocity. We acknowledge that the high wavenumber part of the continuum spectrum may be contaminated by vertical Doppler shifting (Sherman and Pinkel, 1991; Alford, 2001; Pinkel, 2008), especially in the 500 m resolution simulations where the frontal vertical circulations are present (Thomas, 2019). Addressing the effects of vertical Doppler shifting through particle advection in three dimensions will be the subject of future studies."*

I think that the inference of a multi-year timescale for formation of the GM spectrum from the spin-up timescale is a significant overestimate that is at odds with the rest of the literature on wave–wave interactions. Even the now-ancient work by Olbers and other pioneers in this field found tens of days rather than years. Hibiya et al (2002) and other uncited papers find still faster time scales. More recent work (e.g. Le Boyer, which is cited) and unpublished work by Giovanni Dematteis seems to suggest much shorter lags. This paper needs to do a better job of addressing the work by others on this topic.

We thank the reviewer for the very relevant critique and for pointing out the various studies. In the current manuscript we no longer claim that eddy-wave interactions are a more efficient mechanism than wave-wave interactions for the formation of the internal wave continuum.

To address this comment, we have removed the sentences in L25-28 in the Abstract, in L57-61 in the Introduction, and in L238-242 in the Discussion to avoid the imprecise inference (the line numbers here refer to those in the original manuscript). Instead, we have added Olbers (1976), Pomphrey et al. (1980), and Hibiya et al. (2002) to the sentence in L52-54 in the Introduction,

> *"The development of an internal wave continuum has traditionally been attributed to wave–wave interactions that transfer energy from the limited forced scales to a wide range of internal wave scales (Olbers, 1976; McComas and Bretherton, 1977; Pomphrey et al., 1980; McComas and Müller, 1981a; Müller et al., 1986; Hibiya et al., 2002; Polzin, 2004; Lvov et al., 2012; Eden et al., 2019)."*

We have also added Hibiya et al. (2002) to the sentence in L279-281 in the Discussion,

> *"It is evident that wave–wave interactions are expected to increase when internal tides are also forced (Polzin and Lvov, 2011; Nelson et al., 2020; Arbic, 2022), and that they can independently lead to the formation of a GM-like spectrum (Hibiya et al., 2002; Arbic et al., 2018; Chen et al., 2019; Nelson et al., 2020; Peddada and Chalamalla, 2023)."*

In upcoming studies performed by us in the California Current System (CCS) and the same region as this current study, we analyze cross-scale energy fluxes of eddy-wave triadic interactions and find that in realistic scenarios when both eddies and waves are present, wave scattering by eddies is the dominant mechanism and far stronger than wave-wave interactions indicating that eddy-wave interactions might be the preferred dynamical pathway. The question of how these results change in weak eddying regions or strong tidally forced regions like Luzon Strait remains open and is part of our planned investigations.

The discussion of the relevant time scale and upcoming studies have been included in L274-286,

> *"In this study internal tides are intentionally eliminated to focus on the interaction between eddies and wind-forced internal waves, which are believed to be a major driver of turbulent mixing in the upper ocean (Ferrari and Wunsch,*

2009; Alford et al., 2016). In the time scale considered here, wave–wave interactions produce super-harmonics across a wide range of wavenumbers but are not sufficient to form an internal wave continuum in both frequency and wavenumber space. It is evident that wave–wave interactions are expected to increase when internal tides are also forced (Polzin and Lvov, 2011; Nelson et al., 2020; Arbic, 2022), and that they can independently lead to the formation of a GM-like spectrum (Hibiya et al., 2002; Sugiyama et al., 2009; Chen et al., 2019; Nelson et al., 2020; Peddada and Chalamalla, 2023). The dominant mechanisms at play may thus vary regionally and depend on the relative magnitudes of the near-inertial waves, the internal tides, and the eddy field. We note in passing, that in upcoming work we find that in eddy rich regions corresponding to the north Atlantic and the California Current System (CCS), scattering of waves by eddies is found to be far more important than wave-wave interactions."

The manuscript refers to "new physical processes" but in fact no mechanism is given for *how* the eddy field catalyzes the transfer rates.

We have discussed the underlying processes by adding the following paragraph to the Discussion section in L262-273.

"*Our results can be related to recent theoretical studies which suggest that the scattering by the geostrophic eddies is responsible for the diffusion of wave kinetic energy in wavenumber space (Savva and Vanneste, 2018; Kafiabad et al., 2019; Savva et al., 2021; Cox et al., 2023) whereas the refraction by eddies leads to the energy diffusion in frequency space (Dong et al., 2020, 2023).However, whether the physical mechanisms responsible for the eddy-induced effects in our simulations can be attributed to these diffusion theories is unclear, because the underlying weak refraction approximation (Kafiabad et al., 2019; Savva et al., 2021) and spatial scale separation assumption (Dong et al., 2023) do not hold in the numerical simulations presented here. The time scale for the formation of an internal wave continuum in the current study is consistent with that suggested by Kafiabad et al. (2019) (tens of days) to form an GM-like spectrum in wavenumber space. Although the frequency diffusion is predicted to occur on a longer time scale than the wavenumber diffusion (Dong et al., 2023), an internal wave continuum in frequency space is also formed in our simulations within three weeks following the storm.*"

We have also added a detailed discussion of mechanisms suggested by previous studies in L58-73,

"By assuming weak and slowly evolving background flow and linear internal waves with relatively small spatial scales (i.e., the WKB approximation), Kafiabad et al. (2019) showed that a prescribed three-dimensional, homogeneous and stationary eddy field acted as a diffusion operator, leading to the diffusion of internal wave action across wavenumbers, but along surfaces of constant

frequency. Idealized numerical simulations revealed that this 'wavenumber diffusion' mechanism can lead to the formation of an internal wave wavenumber spectrum in tens of days with the same slope as the commonly observed Garrett-Munk (GM) spectrum (Kafiabad et al., 2019). Savva et al. (2021) relaxed the WKB approximation to show that wavenumber diffusion can be explained by a scattering process whereby a stationary geostrophic mode catalyzes the energy transfer between two internal wave modes with slightly different frequencies. Dong et al. (2020) extended these earlier theories and showed that, under the WKB approximation, if the prescribed eddy field was allowed to vary in time it can act as a diffusion operator across frequencies as well. This internal wave 'frequency diffusion' was shown to act on longer time scales than 'wavenumber diffusion' (Dong et al., 2023; Cox et al., 2023)."

Minor comments:

1. It will surprise no one that no IW continuum arises when only eddy forcing is supplied. Suggest minimizing those comparisons between eddy-only and wave. The important comparisons are between wave only and eddy-wave.

   We agree with the reviewer that the key comparisons are made between the eddy-wave and wave-only cases. However, the eddy-only case is also important to be included in the results because this set of simulation shows the energy level of submesoscale eddies that overlap with internal waves at super-inertial frequencies (Fig. 3), i.e., not all super-inertial motions are internal waves. In addition, as we respond to a comment from reviewer 3, the comparison between eddy-only and eddy-wave cases demonstrates that the filling up of a continuum and the energy level of this continuum, although sensitive to resolution, can be confidently attributed to the presence of internal waves.

2. p4, last para, first line: suggest "lateral" not "spatial."

   Thanks. The sentence has been updated in L144-145, which now reads,

   > "In addition to the differences in vertical extent, the *lateral* distributions of internal wave energy dissipation below the mixed layer are strongly affected by the eddy field."

3. Section 2.2: last sentence in first paragraph is wordy/awkward.

   We have rewritten this sentence (L154-155) as follows,

   > *"These depths represent locations within and beneath the mixed layer, respectively."*

4. 2.3 first paragraph: please state why the frequency/horizontal wavenumber spectrum is the right quantity to show this.

   The referee is right that the segue between sections 2.2 and 2.3 wasn't quite right. We have reworked the first sentence of section 2.3 (L196-199) to motivate this section as

"To examine the impact of the eddy field on the distribution of internal wave energy across spatial scales at each frequency band, we show the frequency-horizontal-wavenumber spectra of internal wave energy in the 500 m solutions for the eddy-wave and wave-only cases at 33 m and 355 m depths (Fig. 4)."

5. The peaks at (2,3,4)f are not observed in the thermocline. So this appears to be a model artifact that should be explained.

   Although the distinct harmonic peaks at $\omega = 2f$, $3f$, and $4f$ may be partly attributed to imperfect resolution (discussed below), these peaks are observed in the ocean, see for example Kawaguchi et al. (2020) and Barkan et al. (2021). The comparison between the 2 km and 500 m runs suggests that the amplitude of super-harmonic peaks decreases with resolution; further increasing the model resolution may lead to the smearing of these peaks and make the model simulations more consistent with observations (Barkan et al., 2021). However, the simulated peaks that extend above the continuum is not an artifact and is in fact supported by observations in this region (Barkan et al., 2021). We have added the discussion of the emergence of these peaks in the thermocline in L245-252,

   "Specifically, we demonstrate that in the submesoscale-permitting 2 km resolution eddy-wave model, the simulated continuum is not perfectly smooth and marked by spectral peaks at higher inertial harmonics i.e. $2f$, $3f$, and $4f$. In the 500 m submesoscale-resolving eddy-wave model, however, we observe a pronounced decrease in the amplitude of these peaks and a more realistic internal wave continuum is observed (Barkan et al., 2021), highlighting the importance of interactions between smaller, faster submesoscales and internal waves in accurately simulating the continuum."

6. p9: last sentence before Discussion: a statement on lateral scales of of dissipation is made from the co-spectrum. Strictly that gives coherence scales not scales of dissipation as stated.

   We should have been more explicit about what we are computing. Specifically we are referring to the equations of energy in spectral space. In other words

$$\hat{u}_t + \ldots = \hat{\tau}_z \tag{0.1}$$

$$\Rightarrow \hat{u}^* \hat{u}_t + \hat{u}\hat{u^*}_t + \ldots = \hat{u}^* \hat{\tau}_z + \hat{u}\hat{\tau_z^*}, \tag{0.2}$$

$$\Rightarrow \frac{\partial |\hat{u}|^2}{\partial t} + \ldots = \partial_z(\hat{u}^* \hat{\tau} + \hat{u}\hat{\tau^*}) - (\hat{u}_z^* \hat{\tau} + \hat{u}_z \hat{\tau^*}) \tag{0.3}$$

   Here $\hat{u}$ refers to the Fourier transform of u, $\hat{u}^*$ denotes its complex conjugate, and $\tau = \nu_{kpp} u_z$ is the parameterized turbulent stress. The first term on the final equation above on the right hand side is wind work at each band around (k, ω) while the second term is the dissipation cospectrum. The orthogonality of Fourier modes allows us to interpret the cospectrum as the dissipation in that frequency-wavenumber band. Based on the reviewer's comment we have added this derivation in L442-453.

Response to Reviewer 3:

This paper examines the mechanisms causing the energy of wind-driven ocean internal waves to spread in wavenumber and frequency space. Conventional wisdom is that this spreading, which leads to a continuum in both wavenumber and frequency, is the result of nonlinear wave–wave interactions. The paper relies on a series of numerical simulations to demonstrate that in energetic parts of the ocean (here the subpolar Atlantic Ocean) the interactions between waves and eddies provide a much more effective mechanism of wave energy redistribution than wave–wave interactions which act on unrealistically long time scales.

The paper is very interesting and clearly written. The simulations have been carefully set up and analysed. Overall, the paper makes a convincing case for the dominant role of eddies in shaping the internal wave spectrum in the ocean. I think it can play an important part in shifting the views of the physical oceanography community which for many years has overemphasised the importance of wave–wave interactions.

We thank the reviewer for the positive comments.

I think the paper could be improved in one respect. Several recent works study how eddies redistribute wave energy through a scattering process associated with triadic catalytic interactions. The paper cites these as [22-26] in the introduction but does not engage further with the results. It would be good to discuss which, if any, of the theoretical predictions are supported by the paper's simulations.

Based on the reviewer's suggestion a more complete description of these earlier theoretical studies is now provided in L58-73. We further discuss what aspects of theses earlier theories agree with our realistic solutions in L262-273.

We have added Cox et al. (2023) to the sentence in L54-58,

> *"Recent theoretical and idealized numerical studies suggest that the interactions between ocean eddies and the internal wave field can potentially contribute to the formation of the internal wave continuum spectrum through direct energy transfer from eddies to internal waves (Taylor and Straub, 2016; Barkan et al., 2017; Rocha et al., 2018; Taylor and Straub, 2020; Barkan et al., 2021) and through the redistribution of internal wave energy across scales by eddies (Barkan et al., 2017; Savva and Vanneste, 2018; Kafiabad et al., 2019; Dong et al., 2020; Savva et al., 2021; Cox et al., 2023; Dong et al., 2023)."*

The detailed description of the cited papers in L58-73 reads,

> *"By assuming weak and slowly evolving background flow and linear internal waves with relatively small spatial scales (i.e., the WKB approximation), Kafiabad et al. (2019) showed that a prescribed three-dimensional, homogeneous and stationary eddy field acted as a diffusion operator, leading to the diffusion*

of internal wave action across wavenumbers, but along surfaces of constant frequency. Idealized numerical simulations revealed that this 'wavenumber diffusion' mechanism can lead to the formation of an internal wave wavenumber spectrum in tens of days with the same slope as the commonly observed GM spectrum (Kafiabad et al., 2019). Savva et al. (2021) relaxed the WKB approximation to show that wavenumber diffusion can be explained by a scattering process whereby a stationary geostrophic mode catalyzes the energy transfer between two internal wave modes with slightly different frequencies. Dong et al. (2020) extended these earlier theories and showed that, under the WKB approximation, if the prescribed eddy field was allowed to vary in time it can act as a diffusion operator across frequencies as well. This internal wave 'frequency diffusion' was shown to act on longer time scales than 'wavenumber diffusion' (Dong et al., 2023; Cox et al., 2023)."

We have also put our results in the context of previous theoretical studies by adding the following paragraph to the Discussion section in L262-273.

"*Our results can be related to recent theoretical studies suggesting that the scattering by geostrophic eddies is responsible for the diffusion of wave kinetic energy in wavenumber space (Savva and Vanneste, 2018; Kafiabad et al., 2019; Savva et al., 2021; Cox et al., 2023) whereas the refraction by eddies leads to wave energy diffusion in frequency space (Dong et al., 2020, 2023). However, whether the physical mechanisms responsible for the eddy-induced effects in our simulations can be attributed to these diffusion theories is unclear, because the underlying weak refraction approximation (Kafiabad et al., 2019; Savva et al., 2021) and spatial scale separation assumption (Dong et al., 2023) do not hold in the numerical simulations presented here. For example, although the time scale for the formation of an internal wave wavenumber continuum in our simulation is consistent with that suggested by Kafiabad et al. (2019) (tens of days), we also obtain an internal wave frequency continuum over the same time scale; much more rapidly than predicted by the 'frequency diffusion' mechanism (Dong et al., 2023).*"

These predictions are of a k^{-2} horizontal spectrum and of a frequency spreading that depends on the unsteadiness of the eddies and hence is expected to be much slower than the wavenumber spreading (see also Cox et al 2023, Inertia-gravity-wave diffusion by geostrophic turbulence: the impact of flow time dependence, J. Fluid Mech., 958, A21).

We agree with the reviewer that in Kafiabad et al. (2019) and Savva et al. (2021) frequency conserving wavenumber diffusion should occur over time scales $\epsilon^2 t = O(1)$, with ϵ proportional to the Rossby number of the flow ($\approx$ 3Ro in Kafiabad et al. (2019)). Dong et al. (2023) indeed shows frequency diffusion occurs over longer time scales than that. In our realistic solutions however, the local Rossby number can be $O(1)$ (Fig. 1b in the manuscript), and so the assumption of weak background flow and time-scale separation often break down. We find frequency diffusion to occur very rapidly in our solutions (within 3 weeks following the storm). One can argue that if the Kafiabad et al. (2019) theory holds beyond its regime of validity then for Ro $\sim O(1)$ frequency diffusion and wavenumber diffusion can occur on similar time scales, but we refrain from making such a bold statement. Instead, and largely based on the reviewer suggestion, we discuss the similarity and differences between our results and the earlier theories in L262-273.

In this respect, I wonder whether the continuum for frequencies greater than f in the eddy-wave simulation (Fig. 3) can confidently be interpreted as an internal wave response. The energy level is much smaller than that of the inertial peak and rather sensitive to resolution and whether the spectrum is Lagrangian or Eulerian. I can imagine various contaminations (e.g. by the eddies themselves) unrelated to wave dynamics leading to such a signal.

This is precisely why we also have an eddy-only simulation, which demonstrates the high-frequency content of the eddy field across resolutions (dotted lines in Fig. 3 of the manuscript). It is evident that at both resolutions the high-frequency energy content in the eddy-wave solutions is 1-2 orders of magnitude larger than in the eddy-only solutions. It is therefore unlikely that the high-frequency energy content in our eddy-wave solutions is associated with the eddy field, even if it plays an important role by 'diffusing' wave energy.

The super-inertial energy level is indeed sensitive to the resolution and reference frame. The sensitivity of the internal wave energy level to model resolution reflects the importance of diffusion of wave energy by submesoscale eddies (simulated in the 500 m resolution simulations but not the 2 km resolution ones) for the formation of the continuum. The sensitivity of the internal wave energy level to reference frame reflects the importance of (horizontal) Doppler shifting by eddies.

In the same spirit of engagement with earlier theoretical work, it would be interesting to discuss further whether resonant wave–wave interactions play any role, e.g. at the smallest scales, and whether predictions for energy transfer rates stemming from kinetic theory (e.g. Ref. [14]) are consistent with the (wave-only) simulations.

We have added a sentence in L276-279 to discuss wave–wave interactions and their role for forming the continuum in our simulations,

> *"In the time scale considered here, wave–wave interactions produce the super-harmonics across a wide range of wavenumbers but are not sufficient to form an internal wave continuum in both frequency and wavenumber space."*

Quantifying whether these are resonant exchanges goes well beyond the scope of this paper. We do like to point out that a recent study (`https://arxiv.org/pdf/2207.02758.pdf`) that shows that wave-wave interactions do not seem to lie at all on the resonant curves, suggesting that these early kinetic equation theories may not be accurate for the ocean.

I also have the following comments that the authors could address.

1. References [15] and [16] are cited on p2 as 'recent modelling studies' but date back from 2009 and 2015. Omit 'recent'. Are the results of these modelling studies con-

sistent with the direct estimation of energy transfer rates from the kinetic equation, as performed in [14]?

Responding to a similar comment raised by Reviewer 2, we have deleted this sentence to avoid the confusion on the time scale for the formation of an internal wave continuum due to wave–wave interactions. The reason is that the spin up time scale (on the order of years) reported by these modeling studies (Sugiyama et al., 2009; Müller et al., 2015) is not representative of the formation of an internal wave continuum following an external perturbation that generates waves at a certain wavenumber and frequency, such as the resonant storm in our simulations. Previous studies that evaluated resonant kinetic equation estimated a time scale on the order of tens of days for the evolution of stationary GM spectrum due to wave–wave interactions (Olbers, 1976; McComas and Bretherton, 1977; Pomphrey et al., 1980; McComas and Müller, 1981b). More recently, Lvov et al. (2012) demonstrated the evolution time scale predicted by the resonant kinetic equation in these early theoretical studies was in fact not slow and contradicted with weak interaction theory; the authors suggested that including nonresonant interactions in the kinetic equation improved its self-consistency and decreased the rate at which the (stationary GM) spectrum evolved. The conclusions in Lvov et al. (2012) implied that the evolution time scale for the smooth GM spectrum was longer than the early estimate of tens of days.

2. Figs. 3 and 4: the text and captions refer to the power laws ω^{-2} and k^{-2} as consistent with GM. As mentioned above, k^{-2} is also the prediction for scattering by eddies. Since the authors have access to the full fields, would it be possible to assess the consistency with GM by a comparison between the full (ω, k) spectra and the GM spectrum?

   We have computed the GM spectra for the 500 m resolution simulations at 33 m and 355 m depths (using the horizontally averaged stratification at each depth) and compared them with the frequency-horizontal wavenumber spectra shown in Fig. 4 (Figure iii). The modeled frequency-horizontal wavenumber spectra are overall consistent with the smooth GM spectra in terms of their structure and magnitude: both of them show an increasing frequency bandwidth as horizontal wavenumber increases and are characterized by peaks on the order of 10^{-3} to 10^{-2} m^2 s^{-2}.

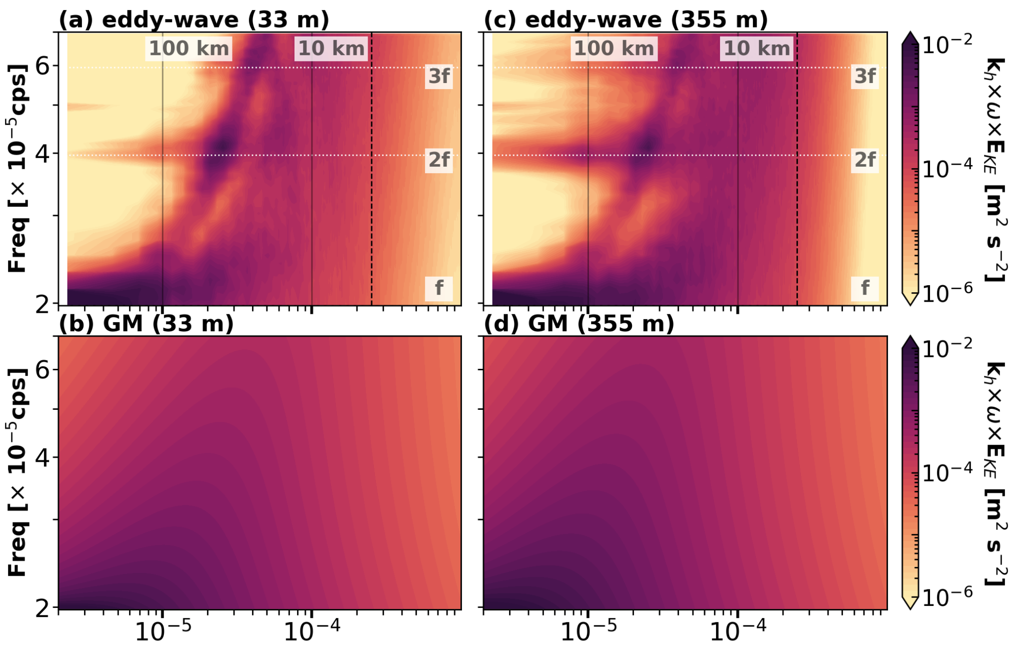

Figure iii: The frequency-horizontal wavenumber spectra at (a) 33 m and (c) 355 m depths in the 500 m resolution eddy-wave simulation. These two panels are identical to Figs. 4a and 4e in the manuscript. The GM spectra computed using the horizontal-averaged stratification at (b) 33 m and (d) 355 m depths in the 500 m resolution eddy-wave simulation.

3. Fig. 4: In the eddy-wave simulation, it is striking that the peaks at $\omega = 2f$ and $3f$ also have a specific spatial scale? Is there an explanation for this?

   The energy peaks at $\omega = 2f$ and $3f$ approximately indicate the horizontal scale of mode-1 internal waves. The horizontal scale of mode-1 internal waves is set by the dispersion relation, which is provided in the Supplementary Information (S5, n=1) and plotted in Figs. 4c and 4d, given their frequencies ($2f$ or $3f$ for the harmonics) and vertical wavenumber (π/H). However, it is not clear to us why the mode-1 internal waves contain more energy than waves of other scales at these frequencies.

4. p7, 1st para: I am confused by the statement attributing the elevated Lagrangian spectrum to sweeping by currents. Isn't this the kind of Doppler shifting effect that Lagrangian averaging removes?

   The shape of frequency spectrum in the eddy-only case in Fig. 3c indicates that some low-frequency motions are shifted to higher frequencies in the subinertial range in the Eulerian reference frame. The sweeping effect refers to the scenario whereby the slowly evolving (low frequency) submesoscale currents are swept past an Eulerian observer and therefore appear to be changing faster than they actually are. This sweeping effect occurs in the Eulerian reference frame but not in the Lagrangian frame, which is why the energy is restored to lower frequencies in the Lagrangian spectra and an elevated energy level is seen there.

   The same frequency shift in the sub-inertial range in the Eulerian frame can take place if small-scale (large wavenumber) low-frequency submesoscale currents are Doppler shifted by the mean flow, which will alter the intrinsic frequency of submesoscale currents by $\mathbf{U} \cdot \mathbf{k}$, where $\mathbf{U}$ is the mean flow velocity and $\mathbf{k}$ the wavenumber of submesoscale currents.

To make this sentence (L186-190) less confusing, we have written it as follows.

> *"The elevated sub-inertial energy levels in the Lagrangian spectra indicates a return of sub-inertial energy to lower frequencies in the Lagrangian reference frame; the shift of this part of energy to higher sub-inertial frequencies in the Eulerian reference frame may potentially be explained by the sweeping of slowly evolving submesoscale currents by the mean flow past an Eulerian observer, as discussed in Callies et al. (2020)."*

5. p8, penultimate sentence: I don't follow the comment. The dissipation is localised in frequency but not in wavenumber. How is that the result of wave energy being redistributed across frequencies? Do you mean between harmonics? The impression I get is that wave–wave interactions generate harmonics of f and that there is little other frequency spreading (but a good deal of wavenumber spreading).

   We apologize for the typo. The comment in L215 (in the original manuscript), "presumably as a result of distribution of wave energy by eddies across frequencies (Fig. 3)" is for the eddy-wave case and was misplaced in the manuscript. We have moved it to L234-235 following the sentence describing the eddy-wave case, which now reads,

   > *"In the eddy-wave case, however, the internal wave energy dissipation occurs across a wide range of internal wave frequencies (Figs. 5b, 5e), presumably as a result of distribution of wave energy by eddies across frequencies (Fig. 3)."*

6. p10, l9: There is a potential for confusion about what is meant by 'Doppler shifting' here. The scattering of wave energy by eddies can be traced back to the Doppler shift term of the dispersion relation: this introduces spatial inhomogeneity and hence refraction and scattering. Doppler shift is meant here in a narrower sense, I think, but I am not sure which.

   We agree with the reviewer that the Doppler shifting term in the dispersion relation ($\mathbf{U} \cdot \mathbf{k}$) does introduce scattering, as shown in Kafiabad et al. (2019). Although under the weak flow assumption, the intrinsic frequency in these theories is always assumed much larger than the Doppler shifting, and so we view these theories to be in the weak Doppler shifting regime. We were instead concerned with 'strong' Doppler shifting, as discussed for instance in Sherman and Pinkel (1991), Pinkel (2008) and Pinkel (2014). We now make that more explicit in line 180. Please note that the differences between the Eulerian and Lagrangian spectra show that there are some weak Doppler shifting effects in the internal wave frequency range, which is potentially responsible for the observed scattering and refraction.

   We have also added a paragraph in the Discussion section (L287-297) to clarify the meaning of Doppler shift,

   > *"As part of our analysis, we demonstrate that horizontal Doppler shifting of wave frequency by currents plays a negligible role in distributing wind-forced internal wave energy across timescales. Although, it may be more important in regions with strong currents like those found in western boundary currents and the Southern Ocean (Nagai et al., 2015; Rama*

et al., 2022). We emphasize that we only consider the effect of horizontal Doppler shifting because the Lagrangian spectra are obtained from advecting particles using the horizontal velocity. We acknowledge that the high wavenumber/frequency part of the continuum spectrum may be contaminated by vertical Doppler shifting (Sherman and Pinkel, 1991; Alford, 2001; Pinkel, 2008), especially in the 500 m resolution simulations where the frontal vertical circulations are present (Thomas, 2019). Addressing the effects of vertical Doppler shifting through particle advection in three dimensions will be the subject of future studies."

7. p12, 3rd full para: I think the statement that the total Lagrangian energy is equal to the total Eulerian energy is false (averaging and taking the square do not commute), independently of the area preservation issue mentioned. I certainly have never seen an argument showing they are equal.

   We argue that the domain-integrated Lagrangian and Eulerian energy are equal, for a 3D non-divergent flow, assuming infinite resolution and closed boundaries. Imagine that we were to advect particles in 3D and that we would start with one particle per grid box. Because the Eulerian flow is non-divergent we would always have, on average, one particle per grid box and so one would expect that, in the limit of infinite resolution, the probability density functions of the Lagrangian and Eulerian velocities be the same (and so will the total energy). Because we only advect particles in 2D they, in fact, tend to accumulate on convergent frontal regions, and so the normalization we carry out is to correct for this sampling bias.

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18th Oct 23

Dear Dr Yang,

Your manuscript titled "Oceanic eddies induce a rapid formation of an internal wave continuum" has now been seen by our reviewers, whose comments appear below. In light of their advice we are delighted to say that we are happy, in principle, to publish a suitably revised version in Communications Earth & Environment under the open access CC BY license (Creative Commons Attribution v4.0 International License).

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

Please note that it may still be possible for your paper to be published before the end of 2023, but in order to do this we will need you to address these points as quickly as possible so that we can move forward with your paper.

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Best regards,

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Heike Langenberg, PhD
Chief Editor
Communications Earth & Environment

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REVIEWERS' COMMENTS:

Reviewer #1 (Remarks to the Author):

The authors have revised the paper substantially and my impression is that this is as far as it will go. I have no further comments.

Reviewer #2 (Remarks to the Author):

The authors have done a good job of addressing my comments, and I think they have also carefully addressed those of the other reviewers. I am content with publication.

Reviewer #3 (Remarks to the Author):

Referee report on the revison of 'Oceanic eddies induce a rapid formation of an internal wave continuum'
by Luwei Yang et al

. I have read the revised paper and the authors' responses to my comments on the original manuscript. I am satisfied with the changes made and am happy to recommend the paper for publication.

That being said, there is one aspect of the results which struck me on second reading and that the authors may want to discuss or highlight. It is clear from the simulation results that the spreading of wave energy is very different in wavenumber and in frequency. In wavenumber, there is formation of a strong continuum that completely removes the peak (at $k_h = 0$) of the wave-only simulation. In frequency, the continuum is much weaker. The peak at the forcing frequency is the dominant feature of the spectrum in both the eddy-wave and wave-only simulations. The frequency continuum is much larger with eddies than without, but very weak nonetheless (10^2 rather than 10^4 times smaller than the inertial peak). Because of this, I think it is fair to say that the simulations results are consistent with the leading-order theoretical predictions that wavenumbers diffuse and frequencies stay put. This does not emerge as clearly from as it should, partly because the authors often refer to "the continuum" with distinguishing between wavenumber and frequency (e.g. on l258). It would be good to highlight the distinct behaviours more clearly.

I have two more small points:

1. l68: The statement is correct but I think it could easily be misinterpreted as saying that wave diffusion leads to the GM spectrum both in wavenumber and frequency. It would good to explain that, according to the diffusion approximation, the frequency spectrum is set by the forcing.

2. l276: I don't understand the distinction that appears to be made between "scattering by geostrophic eddies" and refraction by eddies". Scattering is the result of refraction. The different effect – diffusion of wavenumnber or frequency – depends on whether they eddies are (almost) time dependent or not.

Oceanic eddies induce a rapid formation of an internal wave continuum

L. Yang, R. Barkan, K. Srinivasan, J. C. McWilliams, C. J. Shakespeare, A. H. Gibson

We thank three anonymous reviewers for reading the revised manuscript again and Reviewer 3 for providing additional comments. In response to these comments, we have further discussed the difference in wave energy diffusion in frequency and wavenumber space, clarified the sentence, and explained the terminologies.

Please find responses to the individual suggestions below. Each of the points raised by the reviewers is reproduced in blue, with our response following, and the modified text included in italics with quotation marks. The line numbers in our response refer to those in the revised manuscript (with changes highlighted).

Response to Reviewer 3:

Major comment:

I have read the revised paper and the authors' responses to my comments on the original manuscript. I am satisfied with the changes made and am happy to recommend the paper for publication. That being said, there is one aspect of the results which struck me on second reading and that the authors may want to discuss or highlight. It is clear from the simulation results that the spreading of wave energy is very different in wavenumber and in frequency. In wavenumber, there is formation of a strong continuum that completely removes the peak (at $k_h = 0$) of the wave-only simulation. In frequency, the continuum is much weaker. The peak at the forcing frequency is the dominant feature of the spectrum in both the eddy-wave and wave-only simulations. The frequency continuum is much larger with eddies than without, but very weak nonetheless (10^2 rather than 10^4 times smaller than the inertial peak). Because of this, I think it is fair to say that the simulations results are consistent with the leading-order theoretical predictions that wavenumbers diffuse and frequencies stay put. This does not emerge as clearly from as it should, partly because the authors often refer to "the continuum" with distinguishing between wavenumber and frequency (e.g. on l258). It would be good to highlight the distinct behaviours more clearly.

We agree with the reviewer that the spreading of wave energy due to eddies is different in wavenumber and frequency space. In wavenumber space, the formation of the continuum is accompanied by a smoother spectra where the smallest wavenumber (or the spectral peak) does not stand out as much. In frequency space, however, the inertial peak remains distinct. However, our simulation results do not completely agree with the leading-order theoretical predictions that "wavenumbers diffuse and frequencies stay put". Although the energy level of the internal wave continuum is about 2-3 orders of magnitude lower than that of the inertial peak in frequency space, there is a significant difference in the wave energy level, up to 2-3 orders of magnitude, between the eddy-wave and wave-only cases. This large energy difference is the evidence that the wave energy is spread in frequency space, which can not be explained by the leading-order wavenumber diffusion theory (Savva and Vanneste, 2018; Kafiabad et al., 2019; Savva et al., 2021; Cox et al., 2023). Therefore, even though the energy level of the resulting continuum in frequency space is much lower than that of the inertial peak, it is not fair to say that the "frequencies stay put" based on our simulations. Furthermore, the presence of an energetic inertial (and tidal) peak, two orders of magnitude larger than the continuum, is expected in the study region (see, for example, Figure 2 in Barkan et al., 2021; and Figure 3 in Vic et al., 2021) and is common to many other locations in the ocean (e.g., Figure 1 in Ferrari and Wunsch, 2009). To summarize, the distinct behaviors mentioned by the reviewer are anticipated and the resulting spectra are consistent with observations; our simulations demonstrate the wave energy spreading in both wavenumber and frequency space.

To emphasize the fact that the frequency spectrum in the eddy-wave case is consistent with observations we have added a sentence in L174:

> " *Furthermore, the overall spectral shape is consistent with the observed spectra in the study region (e.g., Barkan et al., 2021; Vic et al., 2021) and many other locations in the ocean (e.g., Ferrari and Wunsch, 2009)*."

Minor comments:

I have two more small points:

1. l68: The statement is correct but I think it could easily be misinterpreted as saying that wave diffusion leads to the GM spectrum both in wavenumber and frequency. It would good to explain that, according to the diffusion approximation, the frequency spectrum is set by the forcing.

   Thanks. We have modified this sentence (L63-66) to avoid the confusion.

   > "Idealized numerical simulations revealed that this 'wavenumber diffusion' mechanism can lead to the formation of an internal wave wavenumber spectrum in tens of days with the same slope as the commonly observed Garrett-Munk (GM) *wavenumber* spectrum (Kafiabad et al., 2019)."

2. l276: I don't understand the distinction that appears to be made between "scattering by geostrophic eddies" and "refraction by eddies". Scattering is the result of refraction. The different effect – diffusion of wavenumber or frequency – depends on whether they eddies are (almost) time dependent or not.

   The reviewer is correct. The scattering of wave energy can result from both advection and refraction by the background flow. The scattering of wave energy is the result, whereas the advection and refraction are the processes (and also the terms in the wave action equation).

The diffusion theories always assume the weak refraction limit. When the weak refraction assumption holds, the scattering of wave energy in wavenumber space results from the advection by both the time-independent flow (Kafiabad et al., 2019; Savva et al., 2021) and the time-dependent flow (Cox et al., 2023). Under the weak refraction assumption, the diffusion of wave energy in frequency space results from the advection by the time-dependent flow (Dong et al., 2020). When the weak refraction assumption is violated, the scattering of wave energy in frequency space can result from the refraction by the background flow, over a much longer time scale (Dong et al., 2023).

Since the Rossby number in our solution is on the order of 1, the weak refraction assumption and other assumptions (e.g. WKB) do not necessarily hold in our simulations and we refrain from using the term "diffusion" for the energy spreading in frequency or wavenumber space. We therefore replace the "wave energy diffusion" with "wave energy spreading" throughout the manuscript to avoid the confusion.

We have modified the sentence in L264,

> "*Our results can be related to recent theoretical studies suggesting that the spreading of wave energy in wavenumber space results from the advection by a weak background flow (i.e., wavenumber diffusion; Kafiabad et al., 2019; Savva et al., 2021; Cox et al., 2023), whereas the spreading of wave energy in frequency space results from the advection by a (weak) time-dependent background flow (i.e., frequency diffusion; Dong et al., 2020) or, alternatively, from refraction over a longer time scale when the weak flow assumption breaks down (Dong et al., 2023).*"

and updated the sentence in L273-277,

> "For example, although the time scale for the formation of an internal wave wavenumber continuum in our simulation is consistent with that suggested by Kafiabad et al. (2019) (tens of days), we also obtain an internal wave frequency continuum over the same time scale; much more rapidly than predicted by the *refraction* mechanism (Dong et al., 2023)."

We have also modified the sentence in L71:

> "*Dong et al. (2023) further demonstrated that over longer time scales the weak background flow assumption of Kafiabad et al. (2019) can break down and that refraction can be important for the scattering of wave energy in frequency space.* "

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