Response to reviewers of the manuscript 2016GL070261 "Seasonality of submesoscale dynamics in the Kuroshio Extension"

Cesar B. Rocha, Sarah T. Gille, Teresa K. Chereskin, & Dimitris Menemenlis

Summary

In this revised version, we have addressed the reviewers comments. The most notable change is the inclusion of a wavenumber-frequency spectral analysis suggested by reviewer 2. This analysis helps us vindicate our claim that inertia-gravity waves, particularly super-inertial waves, account for most of the summertime SSH variance and surface KE at scales between 10 and 100 km. We now also place our study in the context of SWOT and discuss the implications of our findings for the estimation of geostrophic velocity from high-resolution altimeters. We thank the reviewers for the speedy review, constructive criticism, and fine suggestions, which helped us improve our discussion and provide a better context for our study.

1   Reviewer #1

This is a timely investigation into the submesoscale sea surface height (SSH) and velocity variations in the Kuroshio Extension region using global high-resolution OGCM simulations. The main conclusion of the study that the surface ocean submesoscale turbulence and inertia-gravity waves modulate out-of-phase seasonally, is novel and important. Overall, the manuscript is well presented and clearly written and I recommend its publication in GRL after the following comments are addressed.

We thank reviewer #1 for the speedy review. Below we address the reviewer's comments and suggestions.

1. *In the bottom paragraph on P.10, the authors emphasized that "At scales larger than 20km, the April and October spectra based on hourly velocity snapshots are nearly indistinguishable from each other". This emphasis appears to be contradictory to the statements in other places of the manuscript that the seasonality of subinertial submesoscale flows is strong (e.g., those relating to Figure 1b-c and Figure 2).*

    We agree that the emphasis on the similarity of hourly spectra can appear out of contradictory if placed out of context. Those spectra (now Figure 4c and supporting information) are similar because both submesoscale sub-inertial and super-inertial flows undergo a strong seasonal cycle that is out of phase — there is a phase cancellation and the spectra based on hourly spectra is indistinguishable within errorbars. In any event, we removed that sentence and we believe the new Figure 4 will convey the phase cancellation idea more efficiently.

2. *Does the mesoscale eddy variability in the Kuroshio Extension region of the model have a similar energy level as those in the along-track altimeter data? This point is worth men-*

tioning as it impacts on the relative importance between the instability-induced turbulence versus inertia-gravity waves.

3. *P.4, line 2 from bottom: Missing a word after "early".*

   Fixed.

4. *P.6, line 5-7: Should October 15 be part of "late spring/early summer"?*

   We rewrote that sentence to avoid ambiguity.

5. *P.7, line 12: Change "KE spectra" to "KE spectrum"?*

   Fixed.

6. *P.8, bottom: The black line in Figure 2c shows a non-zero divergence value. Shouldn't the gridded SSH data be non-divergent by definition?*

   The convergence of meridians (the β-effect) accounts for the small divergence depicted by the black line in Figure 2c.

7. *P.11, line 4-6: Figure 4a reveals that daily averaging not only reduces the kinetic energy level for scales smaller than 250km, but also those longer than 500km. What part of the large-scale flow signals are being removed?*

   The large-scale super-inertial flows are barotropic tides. The new figure 4 supports this interpretation — there is significant semi-diurnal lunar tidal SSH variance (and KE) at scales larger than 500 km.

2   Reviewer #2

In this paper, the authors describe seasonality of submesoscale (10-100 km) activities in Kuroshio Extension region by using outputs from their new high resolution numerical simulations. Authors found that submesoscale turbulence dominates in late winter and early spring because of mixed layer instabilities, and inertial-internal waves in late summer and early fall because of internal tides and stratification near the surface. This new simulation, which includes tides and a high frequency wind forcing, and authors' attempts to analyze surface submesoscale turbulence and near-inertial waves are potentially important to oceanographic community. However, some discussions seem weak, possibly because it is very difficult to separate sub- and super-inertial motions in the submeso dynamics as authors mentioned.

   We thank reviewer #2 for the speedy review. Below we address the reviewer's comments and suggestions.

2.1   Major points

1. *First of all, generation mechanism of submesoscale inertial-gravity waves in summer and early fall is unclear to me. In the paper, authors attribute the mechanism to internal tides (X-12, section 6, last sentence) and emphasize using the tidal forcing in their model (X-5, section 2, 2nd paragraph). However, authors do not explain or show any interaction mechanism of internal tides (please at least add some reference). With the fixed frequencies of tidal constituents and existence of the critical latitude for the diurnal tides, I am not sure what mechanism can represent the surface near-inertial wave generation within the whole analyzed region by internal tides (with the horizontal scales of 10-100km). If authors compare Figures 2 and 4 with those estimated from the non-tidal forcing run in the Supplemental material, authors' conclusion could be strengthened.*

The generation mechanism generation of internal waves are changes in the wind (NIWs) and barotropic tides sloshing over topographic features — we are not claiming that there exists a different generation mechanism in summer compared to winter; neither are we claiming that surface NIWs are generated by internal tides. What is different is the near-surface expression of IGWs, which is very sensitive to the near-surface stratification (D'saro 1978). This is particular true for higher vertical-mode waves (supplemental material).

To reference potential mechanisms of how energy can quickly be transferred to high wavenumbers by interaction with mesoscale-submesoscale turbulence and PSI, we added new references to the discussion (Ponte & Klein 2015; Mackinon & Winters 2004; Alford et al 2016).

The simulation without tides (ECCO2 Adjoint) has much coarser horizontal resolution and barely resolves the submesoscales; its grid spacing is 18 km, while its effective resolution larger than 60 km.

2. *I also wonder why authors ignore wind-induced near-inertial waves (X-13, the first line). It is well studied that wind-induced near-inertial motion in mixed layer in back ground flow fields disperse quickly and propagate into the ocean interior (e.g., Young and Ben-Jelloul 1997, J. Mar. Res., 55, 735-66). The distribution of those waves depends on the distribution of vorticity (or Laplacian of vorticity) of background flows (Klein and Llewellyn Smith 2001, J. Mar. Res., 59, 697-723; Klein et al. 2004, Q. J. R. Meteorol. Soc., 130, 1153-1166). Therefore, those wind-induced near-inertial waves could have the horizontal scales of 10-100km. Note that those studies are based on geostrophically balanced or quasigeostrophic turbulent field. However, authors may need to study interactions between submeso dynamics regime (Rossby number and Richardson number are close to 1) and near-inertial waves in winter and spring cases where I guess that non-linear terms may play a role. I don't think that authors need to explain all internal wave dynamics in this GRL paper. I would rather suggest to remove all discussions related to submesoscale inertial-gravity waves and focus on describing differences between October and April.*

We are now citing the recent review on NIWs by Alford et al. (2016) that contain a section dedicated to mesoscale-turbulence-NIW interactions, citing all the aforementioned papers. We believe the new discussion together with Figure 4 and Figure S7 will help clear any ambiguity. Those figures unambiguously show that most of the IGWs with horizontal scales between 10-100 km are mostly super-inertial (not near-inertial); the refracted/dispersed NIWs propagate into the interior and only weakly project onto SSH.

3. *Finally, I don't understand why authors define 10-100 km as a submesoscale and used this scale for the spatial filtering (X-7, section 3). Authors should discuss Figure 4 first and then give a reason why they define 10-100 km as a submesoscale and use this scale for the filtering. In X-11, section 5, author point out that a part of the dynamics around 250 km scale should be explained by near-inertial waves. Then, I wonder why authors decide to discuss internal waves in 10-100 km scale.*

Following reviewer's #3 suggestion, we have added paragraphs about SWOT to the introduction and conclusion. This adds context to the manuscript and help justify why we focus only on 10-100 km scales. Our definition of submesoscales have pros and cons (see reply to reviewer's 3 minor comment #1) but is consistent with the definition used by (at least part of) the SWOT community (Fu & Ferrari 2008). Giving

the definition, which we present in the first sentence of the introduction, then there is no ambiguity in using 100 km as a cut-off scale for the filter. We prefer to present bulk statistics first, and then discuss details of different scales — this makes even more sense now with the new wavenumber-frequency analysis.

2.2 Minor points

1. *X-4, section 1, 2nd paragraph and X-13, section 6 last: Authors claimed that they analyze the Kuroshio Extension region. However, their study region does not include between 140E and 155E where the Kuroshio Extension is strongest in Figure 1(a). Please explain why you exclude this region in the paper. Since those longitudes includes the Izu-Ogasawara ridge (internal tide generation) and formation region of Subtropical mode water (strong mixed layer front), I would expect that including these latitudes would strengthen authors conclusions. I would also like to point out that positions of KEO and KESS moorings in the supplemental material are outside of the study region.*

    The upper bound on the size of the domain is chosen for lateral homogeneity purposes, necessary for the spectral analysis. We decided to focus in this subdomain to avoid the strong lateral inhomogeneities associated with the Kuroshio upstream and near its separation from Japan's coast. We have consider larger and smaller domains and found insiginificant quantitative differences.

    The supporting information explicitly states that both KEO and KESS mooring were outside our chosen domain. We analyzed data from those mooring simply to assess the skill of numerical model in representing high-frequency modes. As discussed in the supporting information, the comparison uses model output in the grid points closest to the mooring.

2. *X-4, section 1, 3rd paragraph: "submesoscale inertial-gravity waves". I feel that this nomenclature is too sudden. It might be better to write "inertial-gravity waves with a horizontal scales of 10-100 km (hereafter submesoscale inertial-gravity waves)"?*

    We included the definition — thanks for pointing that out.

3. *X-9, section 4, 2nd paragraph: I wonder what we could learn from the sentence "In other words, even in April, when submesoscale turbulence prevails, only the daily-averaged fields are largely in geostrophic balance". This suggests that the balanced and upward cascade motion (originated from the submesoscale instability) scenario in Sasaki et al., (2014, nature comm. 5) could be wrong because they use the daily averaged data (X-11, section 5, last of 1st paragraph)? Or it just means that the balanced regime is actually the submeso-dynamics regime?*

    This simply means that there is significant ageostrophic components, even in April. Of course, daily-averaging the fields filters most of ageostrophic. This does not imply that Sasaki et al's mechanism is incorrect; it does suggests, however, a more complicated picture, with likely small energy leak towards small scales. In any event, to avoid ambiguity, we have rewritten that sentence and excluded repeated information.

4. *X-10, section 4, 2nd paragraph, the last sentence: Please add some reference.* The sentence has been rewritten.

3   Reviewer # 3

This manuscript provides model-based evidence of a seasonal cycle in submesoscale motions in the western North Pacific (Kuroshio Extension). The manuscript summarizes the results of a study in which two years of moderate-resolution (1/24th degree or 4.6 km) and one year of high-resolution (1/48th degree or 2.3 km) model output were examined for evidence of submesoscale activity. Measures of submesoscale activity include vorticity, divergence and strain rate and the authors demonstrate that these quantities are enhanced in winter. Kinetic energy and sea surface height (SSH) spectra also show seasonal trends. These results are consistent with an existing suite of studies reporting seasonality to submesoscale flows.

What is relevant for the present study and distinguishes it from previous studies is that the authors report an increase in internal wave activity during summer and which (they suggest) projects onto the SSH variance. Because of the relevance of this study to challenges facing the upcoming Surface Water Ocean Topography (SWOT) mission—which aims at measuring submesoscale flows from space from nearly-balanced flows—this study will be of interest to readers of GRL. The major error that should be addressed prior to publication is a more direct connection of divergence and increased SSH variance to inertia-gravity waves. At present, this connection is entirely speculative since any ageostrophic flows can give rise to divergence and SSH variance. Edits should be made to the text and figures, as well.

We thank Christian Buckingham for the speedy and thorough review. Below we address Buckingham's comments and suggestions.

3.1   Major comments

1. *The authors must clearly demonstrate that increased inertia-gravity waves (IGWs) are responsible for the seasonal signal in horizontal divergence and SSH spectra at high wavenumbers. The authors argue that this is so but it is not demonstrated with sufficient clarity. It is true that a portion of this energy/variance will be attributed to internal waves modified by rotation but other flows give rise to divergence (and thus, SSH variance). Thus, the seasonal changes reported by the authors might be the result of seasonal changes in these other flows. Given that this is the main message of the paper, the authors must make this modification.*

   We have included wavenumber-frequency spectra of SSH variance (new figure 4) and KE (figure S7). Plotting bounds for the dispersion relationship of free IGWs, as suggested by the reviewer, unambiguously help us make that IGWs account for most of the super-inertial variability at scales between 10-100 km. Thanks for the excellent suggestion!

2. *There is very little introduction in this manuscript. The authors must address this shortcoming prior to publication. The obvious method of addressing this issue is to discuss the seasonality of IGWs in the context of SWOT and why this might matter. The introduction should, therefore, place your study in the greater context and answer the question, "Why does this study matter?"*

   We agree that the planning of SWOT is perhaps the most immediate application of our results. We have added a paragraph dedicated to SWOT (new paragraph 2 of section 1). We have also added a discussion about the implication for SWOT in section 6. Thanks for making us provide a better context for our study.

3. *There needs to be a discussion about the effective resolution of your model. The fact that*

the model has a resolution of dx = 2.3 km and 4.6 km does not mean it can resolve oceanic phenomena at this scale. Its effective horizontal resolution is closer to 8 times the grid resolution (Soufflet et al. 2013). This corresponds to 18 km and 35 km for LLC4320 and LLC2160 output, respectively. [A more exact value could be obtained by estimating zonal and meridional spectra (of u, v or SSH) and identifying the high wavenumbers / low wavelengths at which the spectral energy falls precipitously.] This means the model is not resolving a number of submesoscale coherent vortices (SCVs) that may occur as a result of ML baroclinic instability; these have diameters close to 5 km. So the follow-up questions from a dynamical point-of-view become (1) where does the unresolved energy come from or go to? and (2) how does this affect your results? These questions need to be addressed or at least discussed if the main topic of your paper is submesoscale dynamics.

We are aware of the this issue — the effective resolution based on the KE spectrum, was already mentioned in the supporting information to the first draft. In fact, the smallest scale discussed in our paper (10 km) was set by the effective resolution of highest-resolution simulation. To be more explicit, we have now included the effective resolution together with the nominal resolutions in the main text (Section 2).

The "unresolved energy" does not come from anywhere; it is unresolved. To the extend that 1-km-scale are associated with a forward energy transfer, then their effects on the scales concerning our study (10-100 km) are likely energetically insignificant. Also important to this discussion is that higher resolution simulations will resolve 1-km-scale eddies and high-wavenumber IGWs that are generated through wave-wave interactions and wave-vorticity interactions.

Buckingham et al. (2016) have a very good point that models and observations to date may not resolve the mixed-layer deformation scale in summer; this is no different for our simulations. But summer is also the season when lateral buoyancy gradients (and thus available potential energy) are minimum within the mixed layer, so the reviewer's conjecture that this is the main source of seasonally varying eddy kinetic energy is not obvious.

For geostrophically balanced mixed-layer instabilities (which we call shallow baroclinic instabilities), linear theory predict maximum growth rates at about $2.6 \times R_{ml}$, where R_{ml} is the mixed-layer deformation scale. But in equilibrated QG turbulence driven by ML instabilities (Callies et al. 2016) the deformation scale has only a catalytic role, where eddy baroclinic energy is converted into (mixed-layer) eddy barotropic energy; as in interior baroclinic geostrophic turbulence, most of the baroclinic conversion occurs at larger scales (Callies et al. 2016).

Still, we recognize that this issue may be further investigated and we included a paragraph to the discussion.

4. *If the authors could estimate the magnitude of the sea surface height signature within a particular band as a function of season that might be helpful. For example, the authors could integrate SSH spectra (Figure 4b) from hourly fields between 10 km and 33 km (i.e., wavenumber = 3 x 10-2 km-1 which appears to be the cross-over point for the two curves) to obtain SSH variances within a particular wavenumber band. Additionally, under the assumption that SSH variance in winter is dominated by balanced motions while SSH variance in summer is the sum of balanced + unbalanced motions, the difference between these two quantities yields a bound1 for the SSH variance due to unbalanced motions.*

We have integrated the wavenumber-frequency spectrum of SSH (Figure 4) to estimate variance in a particular frequency bands. We have included a table with the standard deviation of SSH for super-inertial and sub-inertial flows at scales smaller than 100 km in different seasons. Thanks for the great suggestion!

3.2 Minor comments

1. *(Section 1, Paragraph 1) The authors use the term "submesoscale" and "mesoscale" as descriptive of motions having (1-100-km) and (100-300 km) horizontal scales, respectively. I find these definitions misleading.*

   One way to define mesoscale and submesoscale is to define what scales of motion a particular sensor will resolve. While this would not be optimal, it is objective. This is probably the motivation for the definition used by Callies and Ferrari (2013), since the swath altimeter (as initially proposed) would resolve scales greater than 1 km but smaller than the present-day, profiling satellite altimeters – i.e., eddy-like features with diameters greater than 80-100 km (see Chelton et al. 2011, Progress). Unfortunately, this definition has the adverse effect of changing for each sensor.

   A better definition of the submesoscale might be founded on dynamical arguments. Wunsch and Stammer (1998) introduce the geostrophic equations and note that the lower limit for which this approximation is strictly valid is 30 km. This conveniently defines the lower limit of the mesoscale regime. I would therefore define the submesoscale regime as anything smaller than this but one for which the hydrostatic approximation remains valid (e.g., 1 km).

   The motivation for wanting to use the term "submesoscale" in the title is clear – submesoscale motions are a hot topic within the community and it catches individuals' eyes. This is fine. But what must be made clear within the manuscript is what scales of motion and what force balances (i.e., dynamical regimes) are being described.

   We respect the reviewer's opinion about the definition of submesoscales. But there seems to be no clear definition in the literature — the Stammer & Wunsch paper states that their definition of mesoscales is "very rough" and "there is no generally agreed upon definition". There is also irritation that oceanographers decided to term mesoscales the flows with dynamics analogous to the meteorological synoptic scale motions.

   Some investigators define mesoscales as flows with scales near the 1st baroclinic deformation radius. But there is significant confusion owing to factors of 2π — e.g., some investigators would use $R_1 = 30$ km, others would argue for $L_1 = 2\pi \times 30 \approx 190$ km (Wunsch & Ferrari 2004; Ferrari & Wunsch 2009). To add yet another controversy, Larichev & Held 1995 argue that the deformation length is not the panacea as linear theorists suggest. In baroclinic geostrophic turbulence, the bulk of the energy production occurs on scales much larger than the deformation radius; the energy-containing scales of equilibrated baroclinic geostrophic turbulence are also larger than the deformation radius (e.g., Larichev & Held 1995).

   According to our definition the mesoscales are the energy-containing scales of the flow — the eddies that contain most of the eddy kinetic energy in the ocean (Ferrari & Wunsch 2009). On observational grounds, this scale varies roughly from 100-300 km (from the centroid of KE wavenumber spectrum from current altimeters for example). We then *define* the energetically subdominant scales, the scales smaller

than about 100 km. Our lower wavelength definition is limited by the effective resolution of our higher resolution simulation (~10 km). This definition is consistent with Callies & Ferrari 2013 (they did not define mesoscales/submesoscales based on scales that a sensor resolves as the reviewer suggests).

We understand that our definition is imperfect — we may amuse some readers but irritate others. But besides the unsettled definitions in our community and consistency with our recent work (Rocha et al. 2016), there are at least two reasons for sticking to our definition: (i) this is the definition used by the SWOT community (Fu & Ferrari 2008); incidentally current altimeters do not resolve scales smaller then about 100 km; and (ii) our goal is to determine the flows (geostrophically balanced turbulence, inertia-gravity waves, etc) that dominate the 10-100 km horizontal scales and their seasonality. In any event, readers that make to the first sentence of our abstract will find our explicit definition of submesoscales.

2. *(Abstract) The abstract could be rewritten to emphasize its connection to SWOT.*

   We now describe that there are implications for the accuracy of high-resolution altimeters.

3. *(Section 1, last paragraph) Adding "with implications for SWOT" at the end of the introduction would also help this article.*
   Added.

4. *(Section 1, Paragraph 1) The authors mention a suite of papers documenting seasonality at the submesoscale but fail missed a few: Ostrovskii (1995) and Brannigan et al. (2015). These are observation- and model-based studies, resp.*

   Thanks for pointing that out.

5. *(Section 1, Paragraph 2) Model resolutions: it would be helpful for readers to place in parentheses nominal horizontal resolutions corresponding to these spatial resolutions.*
   We have added both nominal and effective resolutions.

6. *(Section 1, Paragraph 3) What types of horizontal and temporal scales for IGWs are we talking about here? How would this fall onto the Garrett-Munk (GM) internal wave spectrum? (Recall: the GM spectrum contains both balanced and unbalanced flows, not just internal waves, despite that the name suggests this.)*

   We explicit state that those are submesoscale IGWs. Using our definition of submesoscales, these are IGWs with horizontal scales about 10-100 km[1]; IGWs have periods The GM spectrum is not accurate close to the surface (most of our results concern surface currents and SSH, and it does not account for tidal and inertial peaks. We believe that the new plot of the wavenumber-frequency spectrum of the SSH variance (new Figure 4) — following the reviewer's suggestion — will more clearly convey the idea that there is no horizontal scale separation between submesoscale IGWs and submesoscale turbulence.

   If the reviewer is using "balanced" as a short to "geostrophically and hydrostatically balanced", then the comment about the GM spectrum containing both balanced

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[1] Bill Young suggested the term submesoscale IGWs.

and unbalanced flows is incorrect. GM used linear theory of IGW to synthesize observations, mostly mooring data — they fitted an analytic model to (super-inertial) frequency spectra and different vertical modes (see Walter Munk's chapter in EPO 1981 and references therein). One could argue that other flows project onto similar temporal scales, but those motions are super-inertial (e.g., stratified turbulence), not (sub-inertial) balanced flows.

7. *Discretization of the underlying equations of motion. How do you expect the combination of vertical and horizontal resolution modifies internal wave properties that exist in your model? Might you be attributing characteristics to these waves that would be different in the real ocean?*

8. *There should be some discussion of the seasonality of the mixed layer deformation radius. Buckingham et al. (2016) point out that all modeling and observational studies to date necessarily introduce seasonally varying energy by virtue of the deformation radius changing with season and falling below the grid resolution in certain seasons. This might be worth a comment.*

9. *(Section 2, Paragraph 1) "The LLC4320 simulation is an extension of the 3-month long output used by Rocha et al. (2016)." I think this is good to mention, here, so please keep. In contrast, in (Section 5, Paragraph 2) the authors note a consistency between the results in the Kuroshio Extension and those in the Drake Passage. This is not a consistency as it is a different location, different time. The author has already made the reader aware of the previous study so there is no reason to cite again.*

   Removed.

10. *(Section 5, Paragraph 1) eliminate "spectral" after Hanning window. Also, no need for quotation marks.*

    Removed.

11. *(Section 5, Paragraph 1) "the projection of these flows onto different horizontal scales ..." Have you thought about computing scale-dependent vorticity, strain rate and divergence? This is really what you would be doing if you did a breakdown of the aforementioned flows into spectral space.*

    Following the reviewer's suggestion, we have replaced the old section 5 with a discussion of the wavenumber-frequency spectrum of SSH variance. To better illustrate the projection of different flows across different horizontal scales, figure 5c shows the integral of the wavenumber-frequency spectrum over frequencies — the result is very similar to calculating the wavenumber spectrum directly from hourly and daily-averaged fields (old figure 4b).

12. *(Section 5, Paragraph 3) Remove the exclamation mark.*

    Removed.

13. *(Section 4, first paragraph) I understand the motivation for examining the Laplacian of sea surface height but the average reader will not understand the connection.*

    Any reader familiar with basic geostrophy concepts is likely to understand the connection.

14. *(Section 4, second paragraph) Last sentence. This is good. I understand this but to make this legible for the general audience it should be explained why we expect a near one-to-one line for the joint-PDF of vorticity and the Laplacian of SSH.*

    As above.

15. *(Section 4, last paragraph) "consistent with linear inertia-gravity waves". It would be helpful to have a reference after this statement.*

16. *(Section 4) I find "jPDF" odd. Try "joint-PDF" or "PDF" in general.*

    We changed jPDF with joint-PDF; it does look much better.

17. *(Section 4, last paragraph) "...whereas divergence is moderately, negatively skewed as predicted by ..." Specifically mention convergence/downwelling.*

18. *(Figure 1) ** Place panels (d) and (e) before panels (b) and (c) since this is the order in which you refer to them in the manuscript. ** Difficult to see contours in (d) and (e). ** Consider overlaying mixed layer depth on these transects since it is mentioned in the text.*

    We changed the order of the panels. All figures are provided in high-quality resolution. We avoid plotting the mixed-layer depth because it is noisy, hindering an important detail we would like the judicious reader to note, particularly in figure 1c: the wiggles in the mixed-layer base indicate internal waves.

19. *(Figure 2) Is the mean of the strain rate zero? Is the mean of the divergence zero? The variance should be used rather than the root-mean-squared (RMS).*

20. *(Figure 2) What is the sensitivity of the estimates to the stencil type? See Arbic et al. paper describing this sensitivity for satellite altimetry.*

21. *(Section 3 and Figures 2 & 3) At what depth are you computing these quantities?*

    All quantities are computed at the surface as indicated in the title of the section. We now explicitly indicate that in the caption of figures 2 & 3.

22. *(Section 2, last paragraph) It is not just solar radiation that enters the surface buoyancy flux calculation.*

    We rewrote that sentence.

23. *(Section 2, last paragraph) "as shallow as 40 m" – I bet that the mixed layer depths get even shallower than this. At midlatitudes, 20-30 m is not uncommon.*

    We rewrote that sentence.

24. *24. (Section 3, Paragraph 1) "These diagnostic highlight the submesoscale structures in the flow." It might help the authors to motivate their calculations using the following:*

    "Any horizontal velocity field, uh, can be expressed in terms of vorticity, divergence, strain rate and mean flow. This can be seen by expressing uh as a Taylor series expansion and then decomposing the velocity gradient into symmetric and anti- symmetric parts [Landau and Lifshitz, 1987]. Because vortices, vertical motion and fronts are ubiquitous at the submesoscale, this is a natural decomposition and variances of these quantities are apt descriptors of submesoscale turbulence."

Adding the elementary motivation above is yet another controversy: some readers may appreciate it but others may find it condescending. We argue that most physical oceanographers interested in our paper will likely know this material. Even readers from other areas, who did not take basic fluid mechanics, will have the intuition that if the flow peaks at mesoscales (energy-containing), then second-order statistics of the velocity gradient will better highlight the submesoscales than the KE (second-order statistic of the velocity).

25. *(Figure 3) Would be nice to include a third row that contains the collapsed (i.e., one- dimensional) PDFs of normalized vorticity to illustrate the skewness of hourly and daily-averaged flow. I believe you already have this information in the supplementary material but would be nice to illustrate to the reader. Also, in your normalization of vorticity, are you accounting for the fact that the Coriolis parameter changes with latitude? Your domain spans considerable changes in f and the relevant dynamical parameter is the gradient Rossby number.*

We have experimented with a new figure 3, including a third row to show the vorticity PDFs. But the extra plots add little extra, if any, information and take a lot of space. For what it is worth, we explicitly give the relevant statistic (skewness) for each month, etc. As the reviewer mentions, the collapsed PDFs for vorticity and divergence are shown in Figure S2, which is a plot that compares April/October and hourly/daily-averaged in a single plot.

Yes, as we mention in the text, we have normalized by those quantities by the *local* inertial frequency, not the inertial frequency at mid-latitude.

26. *(Figure 4b) Strictly speaking, the label on the y-axis should not be SSH variance. SSH variance would be the integral of this curve. The y-axis should be labelled as the power spectral density of sea surface height (SSH), or SSH Spectra.*

We labelled the y-axis of the SSH variance spectrum as *SSH variance density* — short for *SSH variance spectral density* — not *SSH variance* as the reviewer suggests. We chose to use the short term to avoid cluttering the label since there is no ambiguity because we include the units of SSH variance per unit wavenumber. We further disagree that the y-axis should be labelled *power spectral density of SSH* or *SSH spectra* [*sic*]. On physical grounds, *power* adds unnecessary ambiguity, because SSH variance density has nothing to do with *power*.[2] And "SSH [wavenumber] spectrum" is the plot of SSH variance spectral density as a function of wavenumber, not the quantity in the y-axis.[3]

27. *The authors may find the work of Brüggeman and Eden (2015) helpful in addressing some of the aforementioned questions.*

Thanks.

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[2]The term *power spectral density* appears to have been introduced for frequency spectrum of various quantities in signal processing (Rob Pinkel, personal communication).

[3]This confusion is similar to the pervasive axis mislabelling in plots of probability distribution function (PDF) — the best label is *probability density* not PDF.