Review 1

Response to the review on "Spectral signatures of the tropical Pacific dynamics from model and altimetry: A focus on the meso/submesoscale range"

by M. Tchilibou et al.

We thank the reviewer for taking the time to review our manuscript so thoroughly. We very much appreciate the time, effort, and thought put in to do so. We have found the feedback extremely useful to correct some inconsistencies and imprecisions present in the first version. We are glad that the reviewer finds the paper fundamentally sound to be published after minor revisions. All the comments have been addressed in the last version.

Please find the reviewers' comments below in bold and our responses in non-bold.

Reviewer

The paper is concerned with understanding the dynamical and technical reasons why wavenumber spectra of SSH tend to have flatter spectral slopes in the tropical oceans. It uses primarily analysis of model simulations, with some supporting data analysis. The main conclusion is that the shallow (or flat) spectral slopes at wavelengths less than 200km is mostly due to internal tides and internal waves– this conclusion is supported by the fact that a realistic model simulation with high-frequency winds and tidal forcing has a shallow spectral slope at these large wavenumbers that resembles the observed spectrum from altimetry, whereas a simulation that is similarly realistic except for the inclusion of tidal forcing does not produce the shallow spectral slope (and instead has much less energy at wavelengths below 200km). The conclusion is also supported by various other lines of analysis.

The analysis is sound, and the study provides important information that will be of interest to other researchers. I enjoyed reading the paper and feel the authors did a careful job with the analysis and interpretation. My main criticism is that the "story" is a little complicated and hard to follow; I think this is partly just because it is just inevitably a complicated story, but it may be possible to make changes to the presentation to clarify the arguments. Beyond this, I have minor reservations concerning small details of terminology and interpretation. While I give many critical comments below, I think the paper is fundamentally sound, and that it should be straightforward to address my comments. I recommend that the paper be published after minor revisions.

Major comments:

(1) Lines 114-115: For testing the choices people make in data processing, it would be better to use a model with the most realistic signal possible. Using output from an unrealistic model (no tides) might give misleading guidance about how a particular analysis method should perform when using real data.

We agree that it would be better to make the tests in data processing by using a model with the most realistic signal possible. It corresponds with the R36Th model which is a regional model of the Solomon Sea. Because our study wants to deal with the entire tropical Pacific, we find more appropriate to use the G12d5 model. We may hope that this choice is not bad because the leakage effect concerns mainly the distortion at high wavenumbers of energetic low wavenumbers, and the G12d5 model is well suited to get the large scale motions. Also, the comparison of spectra computed from G12d5 and R36 in Fig. 10 gives confidence on the choice we make.

(2) Line 228: This is the first occurrence of the term "isotropic spectrum". The term is not defined in the paper, but I think the term is being inappropriately used to mean "wavenumber magnitude spectrum". I think that what the authors mean is that it is the wavenumber spectrum after azimuthally integrating the 2D wavenumber spectrum. That is a wavenumber magnitude spectrum. Real data (or GCM data), in general, will not have an isotropic spectrum, which is a spectrum that is the same regardless of what direction a line of samples is taken (e.g., zonal versus meridional).

Yes, we agree. We suppress here the term isotropic, and when it appears again in the section 4.3, we use your terminology. We have used this term because it was been used in different papers instead of wavenumber magnitude spectrum as in Arbic et al. (2014):

> The theory of spectral transfers $T(K, \omega)$ and spectral fluxes $\Pi(K, \omega)$ in the two-layer QG model, where $K = \sqrt{k^2 + l^2}$ is isotropic wavenumber and ω denotes frequency, is summarized here. We let $\hat{A}$ denote the

(3) Line 232: I think this statement must be wrong. That is, I do not believe that the second part of the sentence logically follows from the first. It is true that using shorter segments reduces the maximum energy (because the spectrum is red and the segments are detrended). But, this should only affect the spectral slope via analysis artifacts. One analysis artifact that could affect the spectral slope as record length changes is spectral leakage or sidelobes– reducing the record length should increase the leakage from energetic low wavenumbers to weaker high wavenumbers, thus decreasing the spectral slope. The relationship between record length and the leakage is discussed in the book Random Data by Bendat and Piersol (2000 edition, p. 400).

Yes, we agree that this statement is wrong. It lacks the link with the leakage effect. We add the Bendat and Piersol reference. The sentence has been changed in such a way:

"*Using shorter segments than this reduces the maximum energy and should increase the leakage from energetic low wavenumbers to weaker high wavenumbers, thus decreasing the spectral slope (Bendat and Piersol (2000).*"

(4) Lines 267-281: I don't like this paragraph very much. It is a long and convoluted way of saying that the spectral leakage from low to high frequencies is worse with the tk01 taper and with shorter segments, which careful analysts will know or realize for themselves. There are also some statements that seem out of line with the conventional understanding of taper windows and spectral leakage: (a) I think the statement that the 10%-cosine taper causes artifacts at wavelengths around 10% of the record length shows a misunderstanding of the effects of taper windows. Try applying this taper to white noise, and then examine the spectrum. There will be no distortion at short wavelengths. The distortion arises from spectral leakage (side lobes) from more energetic frequencies, and it thus has as much to do with the spectrum of the untapered time series as the taper. The authors seem to believe that the distortion reliably occurs at wavelengths that are about 10% of the domain size for the 10%-cosine taper, but this is just a coincidence (and I do not think Fig 2 actually shows such behavior). The taper window affects all frequencies in the same way– by convolving the Fourier transform of the taper window with the Fourier transform of the input time series (e.g., appendix of Harris, 1978 paper mentioned above). (b) Again, I think the same misunderstanding is reflected in the statement that, "The advantage of Tk05 is in retrieving the large-scale peaks which are smoothed with the Hanning filter window". The Hanning taper window does not preferentially alter low frequencies. (c) The presence of the "unfiltered spectral fluctuations at small scale" in the doubly periodic spectra is a

separate issue, but I doubt that is an inherent problem with the technique– instead, I suspect some numerical issue may have arisen in the way it was implemented. In the above, my tone is critical, but I appreciate the point the authors are making about how the previous analyses with short segments and mild tapers (like 10%-cosine) are potentially (or likely) contaminated by spectral leakage from the energetic low frequencies. For a rectangular taper window (no taper) of length T, the sidelobes of the Fourier transform of the window function (i.e., of sinc(fT)) decay as 1/(fT). So, the sidelobes of a particular spectral peak will decay as 1/(fT)ˆ2, which means that longer records have less severe leakage effects. You can find something similar to this discussed in Bendat and Piersol, Random Data, 2000, p. 440. I think the message the authors are trying to convey is that, to safely avoid leakage in the tropics, it is best to use a long record and an effective taper window. It's a good point, and I commend the lead author for his attention to this detail. This was one of the things I liked most about the paper.

We greatly appreciate your fruitful comment. Yes, we agree that there is misinterpretation of the effect of the X% cosine taper on spectra, and some misleading interpretations of technical issue. We have made a lot of sensitivity tests and checked as most possible our results, but we got a little lost. We thank the reviewer of the reference (Harris, 1978), we have this paper. The text has been changed and we hope the story is now clearer and easier to follow.

"*The particular sensitivity of spectra in the tropics to the spectral segment length and windowing is linked to energetic EKE and SSH signals extending out to longer wavelengths, and illustrates the ability to deal with spectral leakage from low to high wavenumbers. Tk01 is the worst tapering window, and the distortion of spectra is amplified for short data segments.Tk05, and Hann are a good compromise for preserving much of the original signal and reducing leakage, but need to be applied over larger segments.*

So, to safely avoid leakage in the tropics, it is best to use a long record and an effective taper window. We do not advise to use the Tk01 filter window. The Tk05 or Hann filters give convincing results in the equatorial band, with a minimum of 15° to 20° needed in segment lengths. In the off-equator region, 10° data segments or 10°X10° boxes are sufficient. We choose to use the Tukey 0.5 filter in the paper."

(5) Lines 382-397: This whole paragraph strikes me as weak speculation, resorting to an exotic and potentially unnecessary mechanism. Energy at shorter periods and the nondispersive line can also occur as a simple result of westward mean flow, which seems more plausible to me. (Most of the region was mean westward flow, the SEC occupies most of the 10-20S region and the NEC covers a lot of the 10-20N region, and especially the part with intraseasonal SSH variance.) Farrar and Weller (2006, JGR) examined the effect of the NEC on the Rossby wave propagation and instabilities near 10-13N. Note that, while "linear Rossby wave theory" is often taken to mean the quasigeostrophic equations linearized about a state of rest, one could linearize about any background flow state and it would still be linear Rossby wave theory. Thus, a nondispersive ridge arising from the effect of a westward mean flow on the wave propagation would still be consistent with linear Rossby wave theory.

We understand the criticism of the reviewer about the interpretation of the non-dispersive line as the signature of coherent vortices. It was a subject of debate during several years. Because, these spectra look like spectra in mid latitudes, the discussion was to suggest the presence of eddies to discuss on a possible inertial range. But we mention the weakly nonlinear regime of the region. Also, we didn't know your reference (Farrar and Weller, 2006) that demonstrate how the dispersion curve of Rossby waves follows similar non dispersive line. Therefore, this paragraph has been rewritten to take into account your comments.

"*Although linear Rossby wave theory provides a first - order description of the EKE spectra, in both hemispheres energy extends to higher frequencies (Fig. 4a), and as the wavenumber and frequency increases, significant deviations from the baroclinic dispersion curves occur (Fig. 5a,c). Much of the energy lies approximately along a straight line called the 'non dispersive line' in wavenumber–frequency space as it implies non-dispersive motions. The wavenumber dependencies along the 'non dispersive line' could be the signature of non-linear eddies (Rhines, 1975). The westward propagation speed is estimated at ≈ 10 cm/s, close to the eddy propagation speed found in this latitudinal range by Fu (2009) and Chelton et al. (2007). But these regions are defined as a weakly nonlinear regime (Klocker and Abernathey, 2014). In this region of mean zonal currents the dispersion curves experience Doppler shifting by the zonal flow which makes the variability nearly non dispersive (Farrar and Weller,2006) . So, the non-dispersive line could account both for coherent vortices and more linear dynamics such as Rossby waves or meandering jets propagating westward (Morten et al., 2017).* "

(6) Line 422, "[The fact that the meridional EKE spectrum has larger values than the zonal one] reveals the energetic meridional perturbations due to instabilities of the larger-scale zonal currents". This isn't obviously true, and I don't see any support given for this interpretation. It is widely appreciated that scales of variability near the equator tend to be larger in the zonal direction than in the meridional direction. This is true of many kinds of variability (mean currents, inertia-gravity waves, Kelvin waves, Yanai waves, TIWs). Instead, one might say that "[The fact that the meridional EKE spectrum has larger values than the zonal one] is consistent with the widely held notion that scales of variability near the equator tend to be larger in the zonal direction than in the meridional direction for many kinds of variability (mean currents, inertia-gravity waves, Kelvin waves, Yanai waves, TIWs)."

You are completely right, and it was also our interpretation. This sentence is a big mistake, and we correct it using your suggestions.

"…the meridional EKE spectrum has a higher level of energy than the zonal one (Fig. 7b). This reflects a shift of energy towards the smaller scales in the meridional direction that is consistent with the widely held notion that scales of variability near the equator tend to be larger in the zonal direction than in the meridional direction for many kinds of variability (mean currents, inertia-graviy waves, Kelvin waves, Yanai waves, TIWs)."

(7) Lines 437-438, "So, poleward of 10◦ the hypothesis of isotropy seems to be relevant for scales up to 250 km even if the flow is supposed to be weakly nonlinear". I don't understand the logic. I also really doubt this statement is true. The beta effect is still relatively strong on 10-20N, so I would expect the flow to still be sensitive to beta and to not be isotropic on at least 100-200km scales. Maybe I am misunderstanding what is meant by isotropic, in which case the term should be defined.

Our definition of an isotropic spectrum is the same than yours that you give in the remark 2: It is a spectrum that is the same regardless of what direction a line of samples is taken.

The spectra on Fig. 7ac are similar whatever the direction: zonal, meridional, and with the magnitude spectrum. So it argues for isotropy. We were a little bit surprised because isotropy is the signature of nonlinear eddies in mid latitudes, at first. So it is the reason why we have written this sentence: " So, poleward of 10◦ the hypothesis of isotropy seems to be relevant for scales up to 250 km even if the flow is supposed to be weakly nonlinear". We change a little this paragraph.
"In the 10°x10° off-equatorial boxes, the energy at long wavelengths is greatly reduced compared to the equatorial band. The peak of the EKE spectra corresponds to a wavelength of 300 km. Yet the zonal, meridional and magnitude EKE spectra are similar for wavelengths up to 250 km (Fig. 7a,c). So, poleward of 10° the hypothesis of isotropy seems to be relevant for scales up to 250 km even if the flow is supposed to be weakly nonlinear, and sensitive to beta effect (Klocker and Abernathey, 2014). The EKE slope over the redefined mesoscale range from 100 to 250 km is between -2 and -3 which lies between the prediction of SQG and QG turbulence."

Minor comments:

(1) Lines 59-60: It is not true that the deformation radius is theoretically infinite at the equator. (Or perhaps one could say that the deformation radius is finite in the most commonly used theoretical approaches to equatorial dynamics.) Look for the "equatorial Beta-plane approximation" or the "shallow water equations on the equatorial Beta-plane". It must be in most textbooks on oceanic or atmospheric dynamics.

Yes, we agree. We just referred to the definition for the off equatorial region but it is not correct. We suppress this part in the text.

(2) Line 70: The phrase "representativeness of SSH to infer the tropical dynamics" doesn't have any clear meaning to me. The SSH (pressure) field is a fundamental dynamical variable in itself. I think the point you are making is that, in the tropics, the assumption of geostrophic balance is much more questionable than in midlatitudes.

Yes, it is what we mean. The text has been changed for clarity.
"Also, the tropics are characterized by strong ageostrophic flow, and the representativeness of geostrophic balance from SSH to infer the tropical dynamics needs to be checked."

(3) Line 85: "Unresolved" has a couple of technical meanings (related to sampling and modeling), and I am not sure that either one of these is the intended meaning here.

Thank you for your checking. We have changed by "unsolved"

(4) Line 89: It is imprecise phrasing to say that "tidal and supertidal signals... greatly exceed the internal dynamics at scales less than 300 km wavelength". First of all, I do not understand why internal waves are not considered internal dynamics. Second, and less importantly, it would be clearer to say "supertidal SSH signals... greatly exceed the signals from internal dynamics".

We agree that "internal dynamics" is confusing here. It was a wrong way to separate low and high frequency motions. We change it by "subtidal dynamics", and the text has been changed to highlight the signature of supertidal signal in accordance with Savage et al. (2017).

"*Recent results from a high-resolution 1/48° model highlight that the tidal and supertidal signals in one region of the equatorial Pacific greatly exceed the subtidal dynamics at scales less than 300 km wavelength, and supertidal phenomena are substantial at scales approximately 100 km and smaller (Savage et al. 2017).*"

(5) Line 238: Isn't a 50% cosine taper (Tukey) a "full cosine taper" or a Hann window? The authors should be specific about whether they mean 50% at each end or 25% at each end when they say 50%. (If it is the latter, I agree the two are not the same.) Since the authors seem interested in taper windows, they may be interested in this paper: Harris, F.J, 1978. On the use of windows for harmonic analysis with the discrete Fourier transform. Proceedings of the IEEE., vol 66, p.51.
It is a 50% cosine taper. It means that there are 50% of the coefficients that are smaller than 1 (25% at each end). We thank the reviewer for the reference. We know it.

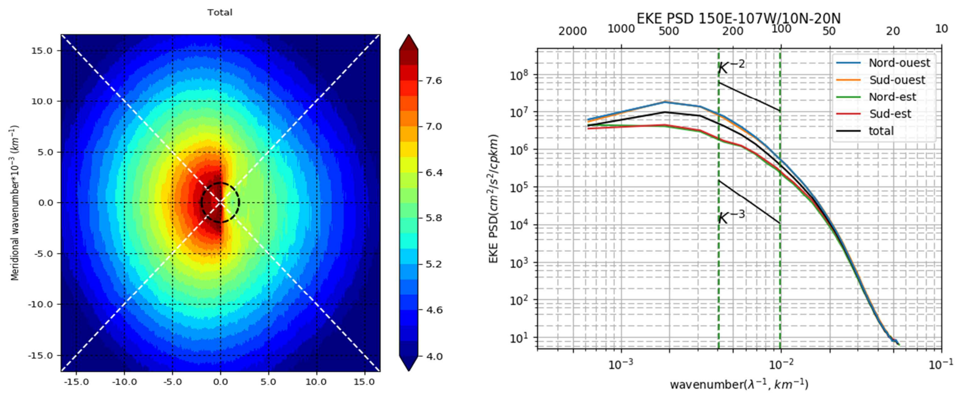
(6) Line 236: "Han" should be "Hann"... or is this just meant to be an abbreviation for "Hanning"?
Okay, we change Han by Hann

(7) Line 349: I think Lee et al. (2018) is a useful reference for this statement (perhaps better than Willet, 2006). By the way, it should be Willet et al. (2006). T. Lee, J.T. Farrar, S. Arnault, D. Meyssignac, W. Han, and T. Durland. Monitoring and interpreting the tropical oceans by satellite altimetry. In D. Stammer and A. Cazenave, editors, Satellite Altimetry Over Ocean and Land Surfaces. CRC Press, Taylor and Francis
Group, 2018.
It is clearly a good reference. I have asked for the book and I have added this reference.

(8) Lines 351-352. Having equal amounts of energy propagating in opposite directions ('balanced northward and southward propagation') is a hallmark of standing modes–the TIWs largely take the form of standing meridional modes, as seen from other perspectives in Lyman et al, (2005) and Farrar (2008, 2011) and earlier work.
Yes, we thank the reviewer for their constructive comments. The text has been changed:
"*have a meridional propagation with northward and southward motion roughly balanced that is a hallmark of standing meridional modes for TIWs as seen from others perspectives in Lyman et al. (2005) and Farrar (2008, 2011) and earlier work*"

(9) Lines 400-401, about the steep spectral slopes being consistent with an inertial subrange. OK, but isn't there a difference between positive and negative values of zonal wavenumber? How does that fit with an inertial subrange? (I don't know the answer, but I suspect it isn't so simple.)
Yes, we agree that the sensitivity of the spectra to positive and negative values of zonal wavenumber is not discussed here, and that is a question of interest in our region. In this paper, we present only wavenumber spectra that are commonly discussed in most of the literature. This question is analysed as part of my Phd thesis where I give more details of the 3D spectra as the one below for the corresponding region showing meridional and zonal propagation. As you mentioned, there is a strong anisotropy in the zonal direction with dominant westward propagations for wavelengths higher than

200 km. It affects the horizontal wavenumber spectra as shown below. Considering the westward propagation only, steeper the slope that looks like a strait line with a -3 slope.



(10) Figure 7, use of the term "isotropic spectra"– see major comment 2.
We have changed the terminology in all the text according your major comment 2.

(11) Lines 443-453: I don't feel this paragraphs adds a lot, and I think it makes tenuous connections to midlatitude dynamics. (a) It is a trivial truth that the change in slope of the ridge is related to the change in wave speed (these are essentially the same thing if the ridge roughly makes a line through k=0, omega=0). In addition, different equatorial wave modes and TIW modes, which have different meridional structures and extents, should feel the equatorial currents differently. The strong equatorial currents are almost surely an important factor influencing the propoagation of variability having phase speeds less than 1 m/s, so I do not think it is a good idea to try to explain the latitudinal change in the w-k spectrum as being due to changes in Beta and the deformation radius alone.
The comment here is close to the major remark 5 where the reviewer doubts on the relation with mid latitude dynamics. We agree that we need to be aware of the limit of our spectra and the text has been changed in consequence. This paragraph is a summary of the previous discussion in regard to the literatures.
"Our modeled zonal frequency-wavenumber spectra differ strongly across the equatorial and off equatorial regions. They show a good representation of the tropical wave and TIW/TIV dynamics. The slope of the ridge of westward variance in the zonal k-ω spectrum in Fig. 5 increases towards the equator. As the slope becomes steeper, more power is concentrated at lower wavenumbers. The change in slope of the ridge itself is mainly related to the change in deformation radius, and expresses linear or non-linear variability propagating non-dispersively (Wortham and Wunsch, 2014). The equatorial region differs from the off equatorial regions in having strong anisotropy with mainly zonally oriented structures (Fig. 7), higher energy at long wavelength due to the strong activity of long equatorial waves, and an overlap between geostrophic turbulence and Rossby wave time scales that produces long waves and slows down the energy cascade to eddies with scales consistent in the tropics with a generalized Rhines scale (Lr) (Theiss, 2004, Tulloch et al., 2009; Klocker et al., 2016; Eden, 2007)."

(12) Lines 454-462: After several readings of the paragraph, I think I understand the intended point: (i) Geostrophic balance is still an important factor near the equator, but the validity of geostrophic turbulence near the equator is questionable; (ii) The model spectra show contrasts between the equatorial and off-equatorial regions... Maybe this kind of rewording would help make it clearer.
We are sorry, part of this paragraph is not at the right place: Discussion about geostrophy/ageostrophy is in the next section 5.
The text has been corrected.

(13) Lines 466-468: I don't see the point of including this first sentence. The first part of the sentence is contradicted by the second part of the sentence. The SSH is a measure if the surface pressure field, an important dynamical variable, which may play a role in both geostrophic and ageostrophic motions.
Yes, we agree with your comment. This sentence is used to introduce the SSH variable that is the main purpose of this section. We change it by:

"The SSH is a measure of the surface pressure field, an important dynamical variable, which may be balanced in the tropics by both geostrophic and ageostrophic motions. The ocean circulation is classically inferred from altimetric SSH through the geostrophic equilibrium. Here, we consider how the wavenumber spectra of geostrophic currents (EKEg) differ from that of the total currents analyzed in section 4."

(14) Lines 471-472: I believe the so-called equatorial geostrophic approximation is of limited validity (only valid at low frequencies).
We think that it could be valid consideringthe G12d5 simulation. Picaut et al. (1989) have shown that the equatorial geostrophic approximation is not valid for periods shorter than 10 days, and most of these frequencies are filtered when using the G12d5 model.

(15) Lines 478-479: Something seems illogical about the statement that the total EKE is weaker than the geostrophic EKE. This must mean that the geostrophic EKE is not a useful concept in this case, ie, there must be a lot of variability that is not in geostrophic balance.
We were also surprised by this result but Ponte et al (2013) describe such behavior as an effect of wind-driven mixed-layer dynamics. We change the reference in the text that was not the good one.
"*However, in all regions, the total EKE is steeper than the geostrophic EKE at scales from 250 km down to the 20 km resolved by the model. In mid latitude regions Ponte et al. (2013) also noted stronger geostrophic EKE at small wavelengths (and weaker spectral slopes) compared to upper ocean EKE spectra associated with wind-driven mixed layer dynamics*."

(16) Lines 493-494, "Due to the strong ageostrophic component in the equatorial region, SSH spectra exhibit lower spectral power than in the off-equatorial region."–>(a) I don't understand the link here. (b) Could it also have to do with the fact that f is small? (The authors don't need to respond to this.)
We want to say that althrough the EKE signal in the equatorial region is higher than in the off equatorial region, it is the opposite for the SSH signal. We agree that we have done a misinterpeatation. The sentence has been changed.

"*It is notable that althrough the level of energy is higher in the equatorial region than in the off-equatorial regions, the SSH variability is lower for wavelengths smaller than 500 km. The reduced SSH variability of the low frequency motions (> 10 days) in the G12d5 model is not in agreement with the higher small "scale" SSH levels in altimetry to be discussed in the next section (section 5.2)*"

(17) Lines 551-552: Ok, but here "high frequency" means periods <48 hours. However, atmospherically forced internal waves in the equatorial region can have periods much longer than this (like 3, 4, 5, 7, and 14 days).
Yes, we agree

(18) Line 592: It is odd phrasing to say a "flat spectral peak". I assume the authors meant a "flat spectral slope".
Yes, corrected

(19) Line 608: El Nino or La Nina?
Yes!! El Nino!

(20) Line 612: delete "eddy"
Done

(21) Lines 612-613: Also, the inertia-gravity waves examined by Farrar and Durland had very large zonal wavelengths.
Yes, this reference is wrong, and has been deleted

(22) Line 636: It should say "structures tend"
Corrected

(23) Line 640: it should say "spectra are".
Corrected

(24) Line 648-649: How do we know there is a spectral cascade?
Yes, this term is not appropriate. The text has been changed
"*In the equatorial band from 10°S-10°N, the total EKE is more energetic than the off-equatorial region, and the EKE spectral slope approaches k^{-3} over a large wavenumber range, from 100 to 600 km …*"

(25) Lines 662-664: I find this confusing. There is no way using geostrophic currents changes the SSH spectrum.
We agree that the last sentence is confusing. Here we want to make the link with SSH, the text has been changed.
"So using SSH and geostrophic currents slightly flattens the EKE wavenumber spectra, but the modeled SSH wavenumber spectra maintain a steep slope that doesn't match the observed altimetric SSH spectra"

(26) Line 678: I do not understand what a "turbulent spectral slope" is.
We agree, and delete this term

(27) Line 709: I think "predominate" may not be the right word choice here. (I'm not sure what is intended.)
We change the word by"major"

(28) Line 420: Just to be precise, this should say "wavelengths" instead of "scales".
Done