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Interactive comment on "Spectral signatures of the tropical Pacific dynamics from model and altimetry: A focus on the meso/submesoscale range" *by* Michel Tchilibou et al.

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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 ex-

cept 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.

(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).

(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).

(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.

(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.

(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)."

(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.

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.

(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.

(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.

(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".

(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.

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

(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.

(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.

(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.)

(10) Figure 7, use of the term "isotropic spectra"– see 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.

(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.

(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.

(14) Lines 471-472: I believe the so-called equatorial geostrophic approximation is of limited validity (only valid at low frequencies).

(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.

(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.)

(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).

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

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

(20) Line 612: delete "eddy"

(21) Lines 612-613: Also, the inertia-gravity waves examined by Farrar and Durland had very large zonal wavelengths.

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

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

(24) Line 648-649: How do we know there is a spectral cascade?

(25) Lines 662-664: I find this confusing. There is no way using geostrophic currents changes the SSH spectrum.

(26) Line 678: I do not understand what a "turbulent spectral slope" is.

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

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

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