"A harmonic projection and least-squares method for quantifying Kelvin wave activity"
Response to Reviewer #2

This manuscript attempts to derive meaningful indices for Kelvin wave activity along the equatorial and coastal waveguide in the Indian Ocean. It is a worthwhile goal, and the authors have certainly invested a great deal of effort in the pursuit, but I regret that I cannot recommend the manuscript for publication. There are too many unsupportable and erroneous mathematical manipulations that don't do what the authors intend them to, and I cannot come up with suggestions for straightforward corrections that would fix the problems. Below I will list problems as they arise in the manuscript, but this list is not necessarily comprehensive. By the point in the manuscript where the list stops, I could no longer believe in the product of the manipulations.

General comments: Many thanks to the reviewer for this detailed, thoughtful review. Based on feedback received from both reviewers, the manuscript has been reworked so that the method is clearer and easier for others to replicate. The method itself has also been changed, such that the projections of the "wave functions" (formerly basis functions) in x, y, and t are carried out simultaneously, rather than carrying out the projections of the structure functions in y before projecting the harmonic functions in x and t. The latitude radius of the projections has also been expanded, from 5° to 15°. We hope that these modifications address some of the major concerns that the reviewer had about the physical significance of each step, and the potential for cross-contamination of Kelvin and Rossby wave signals.

In addition, the revised method also enables more direct comparisons between our results and the results of other methods that have extracted the Kelvin wave signal from SSH data. Of particular note: as in earlier work by Delcroix et al. (1994), Boulanger and Menkes (1995; 1999), and others that extracted the signal from Kelvin waves and the lowest meridional mode Rossby waves, the method in the revised manuscript is used to recover signals representing both Kelvin and Rossby waves. Hence, the title of the article has been changed to "A harmonic projection and least-squares method for quantifying equatorial wave activity". It was also decided to exclude the coastal wave part of the decomposition in the revised manuscript and focus on the more straightforward equatorial region to refine our method; it is hoped that coastal waves can be tracked using a similar method in a future study.

Note about the manuscript: All of the text that has been revised in the article (which includes most of the material) is in italics.

1) Line 58: The authors claim to be building on the methodology of Boulanger and Menkes, 1995, 1999, but this is not true. The problem with deducing equatorial wave amplitudes from only Sea Level Anomalies (SLAs) is that the signals of separate wave modes are not orthogonal (as the authors acknowledge). The individual wave amplitudes could only be deduced if the observed meridional profile of SLA were composed of only a finite number of modes. Boulanger and Menkes made the reasonable approximation that MOST of the SLA profile could be described by a large but finite number of modes (21) and then they derived an approximate solution for the singular matrix associated with projections onto all these modes. This manuscript only attempts a projection onto the lowest Hermite function, which if done properly, would not be able to distinguish between the amplitudes of the Kelvin wave and the first meridional mode long-Rossby

wave; both of which can be expected to be present in the SLA signal. Presumably, these can be separated later by separating the eastward from the westward propagating signals, but the ambiguity should be acknowledged up front. It is misleading to call the projection attempted in equation (2) the "Kelvin wave y-projection."

The reviewer is correct about the conceptual framework of our original process: to project the meridional (or cross-shore) Kelvin wave structure onto the SLA data, and then project the meridional projections (the "y-projection") onto propagating harmonic basis functions. Given some of the points raised later in this review, and also in the interest of making the method clearer and more effective, it was decided to eliminate the intermediate step of carrying out the meridional projection in isolation, and instead carry out projections in x, y, and t simultaneously. This is done by projecting onto the SLA data the three-dimensional "wave functions", which are a combination of the meridional structure function in y, the propagating harmonic functions in x and t and the taper functions in x, as shown in equation (1) of the revised manuscript.

2) The "projections" in (2) and (3) are not really projections, the mathematical forms and the choice of integration limits are puzzling, and the authors provide no justification for their choices. In Fourier analysis it is common to subtract the mean of a data set prior to projecting onto the sines and cosines, but this works because the basis functions all have zero mean. This is not true of the Hermite functions, and it is not true of the exponential profile of the coastal wave. Subtracting the means before integration introduces extra terms that have nothing to do with the desired projection, and limiting the integration limits to 5 degrees latitude ensures that the Kelvin wave structure will not even be orthogonal to structures that it should be orthogonal to. I'll use a simple idealized situation to illustrate the problems with (2). On an unbounded equatorial β-plane, the orthonormal Hermite functions, ψ y), n = 0, 1, 2,···, provide a (ˆ n complete basis. The argument of the Hermite function is the latitude normalized by the q ◦−y = y/L , with L = c/β ≈−3 latitude for c = 2.5 m/s. Even in deformation radius: ˆ e e a bounded basin like the Indian Ocean, a truncated set of these functions does a decent job of describing meridional structures while remaining approximately orthonormal. An exception might be the region just south of Sri Lanka, but this is only a small part of the longitudinal span of the basin. The meridional structure of an equatorial Kelvin wave's SLA is ψ : the Gaussian part of the authors' equation (1), with a normalizing factor of π (so that ψ dˆ y = 1). The −1/2 structure of the mode-1 long-Rossby wave's SLA is (2 ψ + ψ). Suppose the measured 2 0 equatorial SLA contains only a Kelvin wave of amplitude A , a 1st mode long-Rossby wave K of amplitude A and a background of other variability that is orthogonal to the Kelvin 1 ∞−n=1 n n wave: B(y) = Σ b ψ . A true projection onto the Kelvin wave structure would be

$$
\begin{aligned}
K_y &= \int_{-\infty}^{\infty} h_{SLA}\, \psi_0 \, d\hat{y} && (1)\\
&= \int_{-\infty}^{\infty} [A_K \psi_0 + A_1(2^{-1/2}\psi_2 + \psi_0) + B]\, \psi_0 \, d\hat{y} && (2)\\
&= (A_K + A_1)\int_{-\infty}^{\infty} \psi_0^2 \, d\hat{y} + 2^{-1/2} A_1 \int_{-\infty}^{\infty} \psi_2 \psi_0 \, d\hat{y} + \Sigma_{n=1}^{\infty} b_n \int_{-\infty}^{\infty} \psi_n \psi_0 \, d\hat{y} && (3)\\
&= A_k + A_1. && (4)
\end{aligned}
$$

This demonstrates the non-orthogonality of the pressure structures of equatorial waves, but at least we're only left with two modes to be sorted out later. If the integration limits ◦−are reduced to ±10 , the results are essentially the same, with only a small error. At the ◦−authors' chosen integration limit of 5 latitude, however, the Kelvin wave structure is still almost 25% of it's maximum value, and over this interval it is not even approximately orthogonal to any of the other even Hermite functions. In this case, a proper attempt at a projection would yield:

$$K_y = (A_K + A_1)\int_{-5^\circ/Le}^{5^\circ/Le}\psi_0^2\,d\hat{y} + 2^{-1/2}A_1\int_{-5^\circ/Le}^{5^\circ/Le}\psi_2\psi_0\,d\hat{y} + \Sigma_{n=1}^{\infty}b_n\int_{-5^\circ/Le}^{5^\circ/Le}\psi_n\psi_0\,d\hat{y} \quad (5)$$

$$= 0.98(A_k + A_1) - 0.06A_1 + 0.08b_2 + 0.06b_4 + \Sigma_{n=6}^{\infty}b_n\int_{-5^\circ/Le}^{5^\circ/Le}\psi_n\psi_0\,d\hat{y}. \quad (6)$$

The additional terms may be individually small, but a realistic background would contain a large number of them, and they can add up to a significant number that has nothing to do with the Kelvin wave amplitude, all because the integration was not carried to a latitude where the Kelvin wave is truly insignificant. An even worse situation arises when the means are subtracted prior to the integration, as in the manuscript's equation (2). I will continue to integrate in the nondimensional coordinate ˆ y for consistency with the above equations, but note that with the exception of a normalizing constant, the "projection" below is identical to (2) in the manuscript. Integrating over ˆ y, the authors' definition of mean becomes

$$\overline{a} \equiv \frac{1}{2r/Le}\int_{-r/Le}^{r/Le} a\,d\hat{y}, \quad (7)$$

and the "projection" in their equation (2) is

$$K_y = \frac{1}{2}\int_{-r/L_e}^{r/L_e}(h_{SLA} - \overline{h_{SLA}})(\psi_0 - \overline{\psi_0})\,d\hat{y} \quad (8)$$

$$= \frac{1}{2}\int_{-r/L_e}^{r/L_e}h_{SLA}\,\psi_0\,d\hat{y} - \overline{h_{SLA}}\left(\frac{r}{L_e}\right)\frac{1}{2r/L_E}\int_{-r/L_e}^{r/L_e}\psi_0\,d\hat{y}$$
$$- \overline{\psi_0}\left(\frac{r}{L_e}\right)\frac{1}{2r/L_E}\int_{-r/L_e}^{r/L_e}h_{SLA}\,d\hat{y} + \overline{h_{SLA}}\,\overline{\psi_0}\frac{1}{2}\int_{-r/L_e}^{r/L_e}d\hat{y} \quad (9)$$

$$= \frac{1}{2}\int_{r/L_e}^{r/L_e}h_{SLA}\,\psi_0\,d\hat{y} - \frac{r}{L_e}\overline{\psi_0}\,\overline{h_{SLA}}. \quad (10)$$

Using the manuscript's values of r and c, the equation is

$$K_y = \frac{1}{2}\int_{-5^\circ/L_e}^{5^\circ/L_e}h_{SLA}\,\psi_0\,d\hat{y} - 0.85\,\overline{h_{SLA}}. \quad (11)$$

The first term on the right-hand-side is half of the true projection of the SLA onto the Kelvin wave structure (which includes the mode-1 Rossby wave amplitude), except that it ◦−will contain the extraneous terms noted above because the value r = 5 is too small. The second term is a fraction of the mean SLA and has nothing to do with the Kelvin wave. Furthermore, the factor in front of h SLA e asymptotes to 0.94 as r/L →−∞. amplitude.

This term cannot be removed by choosing larger integration limits. It is the consequence of erroneously subtracting the means of h and ψ0 in (2).

Similar issues arise with the "projection" in (3) onto the coastal Kelvin wave structure, but I won't go into detail. Even without the problem of subtracting the means, the exponential decay of the coastal wave would project onto just about anything. K would y likely include contributions that have nothing to do with the Kelvin wave or even the Rossby waves represented by their limited set of x −−t "basis" functions. To repeat, the "projections" in (2) and (3) are not really projections onto Kelvin wave structures, and they are not reliable measures of either the Kelvin wave or low-mode Rossby wave amplitudes.

To paraphrase the core issues with this comment, it seems that the reviewer was concerned that (1) the original meridional/cross-shore structure functions being projected have nonzero means, (2) the integration limits are insufficient to distinguish between Kelvin and Rossby wave meridional structures, and (3) removing the means from the basis functions and the SLA fields introduces a spurious term (proportional to the product of the mean values of the Kelvin wave structure and the SLA). Issue (1) is why the means of the structure functions are removed prior to the projection being carried out (lines 80-83 in the revised manuscript). It is the meridional shape of the structure function (not the absolute SLA values) that we are trying to recover from the data; if the mean were not removed, then the projection would yield a term that is proportional to the mean value of the SLA along that cross-section.

Issue (2) is a very valid point; the integration limits of 5°S and 5°N were initially chosen to reduce the impact from off-equatorial waves and eddies, as well as to avoid the landmasses of India and Sri Lanka. However, we acknowledge that the benefits (and arguably the necessity) of expanding the integration range outweigh the earlier concerns about off-equatorial contamination of the signal. Hence the integration limits have been expanded in the revised manuscript:

"Each wave function is projected onto the SLA data between latitudes 15°S and 15°N, to resolve the meridional structures of equatorial Kelvin waves and the first 5 meridional Rossby wave modes." (lines 100-102)

Additionally, we think the risks from off-equatorial contamination are mitigated by the modification to our method, in which we are projecting the meridional structure and zonal phase propagation simultaneously. This is because off-equatorial waves propagate more slowly than the lowest equatorial wave modes, and because projecting our wave functions across the basin in x, y, and t reduces the impact from landforms in one small part of the basin.

Issue (3) may result from a confusion about what we are trying to represent. For example, if the meridional structure of an equatorial Kelvin wave were projected onto an SLA field with essentially infinite integration limits in y, then it would not be necessary to subtract the mean of the meridional structure; the mean would be essentially zero anyway, since the peak in the structure at the equator would be outweighed by its near-zero values on the flanks. However, in the real ocean we need to project with finite integration limits. In this case, the first term on the right-hand side of the reviewer's equation (11) does not represent the "true" projection value; it is a weighted mean of the SLA value along the meridional transect.

Our goal is to obtain the same projection value for a Kelvin wave of amplitude 10 cm, regardless of whether the background SLA field it is superimposed on has a mean value of 0 cm or 50 cm. To achieve this, either the structure function or the SLA field (or both) must have the mean removed prior to the projection. Which of these have their mean removed does not make a difference to the value obtained from a projection the structure function onto a 10 cm-amplitude Kelvin wave (due to the cancellation of terms as shown in the reviewer's equations 9 and 10). However, it does make a couple of other important differences. By removing the meridional means of the SLA data, zonal variations in the background SLA values are minimized, which is desirable for equatorial long waves defined dynamically by their meridional pressure and current variations. Removing the means of the structure functions makes the structure functions more orthogonal to one another, and thus better conditions the matrix that must ultimately be inverted. So it is considered desirable to remove the means in both the structure functions and the data. Moreover, the meridional *trends* in both the structure functions and data were also removed; if the integration limits were not expansive enough this would have adverse consequences for the projection of anti-symmetric Rossby wave modes. However, the integration limits of 15° on either side of the equator include almost all of the SLA variability of the first 5 Rossby wave modes, and hence removing these trends excludes the effect of large-scale steric height gradients that we are uninterested in.

3) Lines 124-127: The authors note that the amplitude of a coastal Kelvin wave increases as the wave propagates poleward in the absence of dissipation, and this is correct. They claim, however, that the integral of the Kelvin wave's SLA structure is thus a better measure of the wave, and this is not true. What remains constant with changing latitude is the energy flux, and this is proportional to the integral of the squared SLA structure (which obviously includes the squared amplitude).

As the focus of the revised manuscript is now solely on equatorial long waves, this comment is no longer relevant. However, if an attempt is made in the future to apply this method again to coastal waves, we would certainly consider defining the wave functions so that they conserve energy (rather than the integral of the SLA structure) as they propagate.

4) Equation (1) is not quite correct for Kelvin waves propagating meridionally. This may not matter given my next comment, but the authors should at least note the approximation and cite a reference when they present the equation.

Again, as the revised manuscript focuses on equatorial long waves, this comment is no longer relevant. We acknowledge that the structure of propagating coastal Kelvin waves is more complex than that indicated by the earlier paper's equation 1 (see also the response to the next comment).

5) For a harmonic solution of given frequency, ω, coastal Kelvin waves as described by (1) in the manuscript do not technically exist equatorward of the turning latitudes q 2 2 Y = ±−(ω/β) + (c/2ω) . (12) t 3 Within these latitudes, Rossby waves can propagate freely, any meridional motions on an eastern boundary will shed long Rossby waves, and the transfer of energy flux from an equatorial Kelvin wave incident upon an eastern boundary to the outgoing coastal Kelvin waves poleward of the turning latitudes is more complicated than the simple scenario described by (1). For c = 2.5 m/s and a period of 45 days, the turning latitudes are at ◦−±7 , which is about the latitude of the southwestern tip

of Java. For periods this long or longer, it is not worth trying to describe a Kelvin wave propagating down the coast of Sumatra from the equator. The actual Kelvin waves are pulse-like and so contain a spectrum of frequencies, some of which maybe high enough to sustain a true Kelvin wave along the coast of Sumatra, but even if you could describe the alongshore energy flux of the entire pulse as a pseudo-Kelvin wave, its energy flux would be decreasing steadily poleward as energy is shed westward into Rossby modes. This loss of along-shore energy flux would be much different than that produced by dissipation. The tapered pseudo-basis functions would not apply equally well to both situations.

The complicated dynamics that occur when a Kelvin wave reaches an eastern boundary, resulting in both the reflection of Rossby waves and coastal Kelvin waves, was not fully considered in the earlier version of this article. This issue likely explained why there was a deterioration in correlation between Kelvin wave coefficient and SLA values along the coast of Sumatra. In the revised manuscript focused on equatorial waves, we confine ourselves to considering wave reflection by comparing the incoming Kelvin wave amplitudes with the outgoing Rossby wave amplitudes (e.g., Figures 5-7). In a potential future application of this method to coastal waves, the frequency dependence of the behavior of Kelvin waves impinging on the eastern boundary could be accounted for: either directly by making the meridional/cross-shore structure a function of frequency, or indirectly by tuning the cost function so that the $\mathbf{\Lambda}$ diagonal matrix values are a function of the frequency of the associated wave functions.

6) The $x - t$ "basis" functions presume that all signals are moving along the coast in one direction or the opposite, and the suite of functions is truncated to waves moving in the Kelvin wave direction for a range of appropriate phase speeds and waves moving in the opposite direction for a range of Rossby wave speeds. If a Kelvin wave could be described along the coast of Sumatra, the Rossby waves would not propagate in the opposite direction from the Kelvin wave. The basis function would be trying to isolate signals propagating northwestward at the Rossby wave's westward phase speed. The Rossby wave's amplitude is embedded in the K vector, but the amplitude of the waves shedding westward at one y latitude does not match the amplitude of the wave shedding westward at a different latitude in a way described by the basis function.

This is a good point, which will have to be considered should this method be applied in the future to coastal waves. Using three-dimensional wave functions (as we have done in the revised manuscript) allows much more flexibility in what types of signals can be projected and extracted from the SLA data. Thus, perhaps a future application could resolve the shedding of Rossby waves propagating zonally at different latitudes, as distinct from the Kelvin waves propagating obliquely meridionally down the coast.

7) There is often an energetic eddy field south of Java and the Indonesian Islands that is close enough to the coast to be captured by the K integration in (3). This is a place where y the Kelvin and Rossby waves do propagate in opposite directions, but the manuscript limits the westward propagating basis functions to phase speeds -1.2m/s< c < -0.4 m/s. m,n The eddies propagate at -0.15 to -0.2 m/s (Feng and Wijfells, JPO, 2002), so even

though their SLA signal will be included in the K vector, the basis functions will not be able to y filter them out through their propagation characteristics.

Similarly to the response given to comment 6, this issue could be resolved in the future by projecting wave functions that are bolus-like in the shape of eddies (and propagating in the appropriate latitude and speed ranges), and then removing this signal. There may be more efficient ways to extract the eddy signal though (perhaps by filtering out signals in this latitude band that have short zonal wavelengths).

8) The "basis" functions are not truly basis functions - they do not constitute a complete set. I would have to see more details of the mathematic involved in the "projections" onto the tapered functions to asses the value of this step.

The "basis" functions that are used are not a basis in the traditional mathematical sense: a set of functions, orthogonal to one another, that can be summed up (with coefficients) to explain all of the variability in a numerical field. However, even though our "basis" functions are not orthogonal to one another, their application in our method is to achieve a similar goal: to represent as much variability as possible in the field using a set of functions that represent intrinsic modes of variability. To avoid confusion with the mathematical meaning of "basis", these functions have been renamed "wave functions".

It is not clear what aspect of the mathematics the reviewer would need to see to assess the value of the tapered functions. We have found in the revised manuscript that by adjusting the relative weighting *w* of the misfit to the "taper difference" (the change in the value of the projections as a function of taper location) that we can optimize the correlation values of the mode SLA to the SLA data (Figure 2), and minimize the correlation to the SLA residual (Figure 3). But perhaps the reviewer is seeking a clearer assessment of the improved results that tapered wave functions provide compared to non-tapered wave functions? This would be an interesting analysis, but is beyond the scope of our manuscript at present.

Unfortunately, at this point I have lost confidence in the authors' understanding of precisely what each of their calculations does. It feels like a series of flawed mathematical manipulations that produces an answer of dubious value. The fact that their figures qualitatively resemble the raw SLA plots is probably due to the robustness of the Kelvin wave in this part of the world, but I can't believe that the final K product has more value than an amplitude derived by simply tracking alongshore time-longitude plots. The mathematical complexity of the procedure implies a rigor that is not really justified. The authors have obviously put considerable effort into this procedure and it is possible that with the above comments in mind and a more careful approach, they could rework the procedure into something more meaningful. I can't suggest an obvious way forward, however, so I regret that I cannot recommend the present manuscript for publication.

We understand the reviewer's earlier reservations about this method, particularly considering the complex wave structures that are present at the eastern boundary as Kelvin waves impinge on it. The reviewer points out that the resemblance between the Kelvin wave coefficient in the earlier paper and the raw SLA is likely due to the robustness of the Kelvin wave. This may indeed have played a role, since the correlations between the Kelvin wave coefficient and the SLA data were highest where the Kelvin wave signal was strongest (in the eastern equatorial Indian Ocean and along the Java coastline), and significantly weaker in some other areas. Our original goal was to recover a signal that has both the cross-wave structure and the propagation characteristics of a Kelvin wave: we still consider this goal both important and achievable. The modifications made to our method have improved our ability to parse equatorial wave signals, by expanding the integration limits of the projections, and by projecting the meridional structures and zonal propagation characteristics of the waves simultaneously. We also focus on the equatorial region in the revised manuscript, deferring the challenges of the coastal wave regime so that the method can be refined by application in the extensively-studied equatorial band. Lastly, we apply our method to specifically extract the signals of the first 5 meridional Rossby wave modes, as well as Kelvin waves. This is done to facilitate comparisons to earlier decompositions of equatorial wave activity, especially Boulanger and Menkes (1995; 1999) who also used only SSH data to extract wave amplitudes. We hope that the reviewer finds these modifications sufficient to reconsider their recommendation.