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Discussions

Interactive comment on "Using kinetic energy measurements from altimetry to detect shifts in the positions of fronts in the Southern Ocean" *by* Don P. Chambers

D. Chambers

dchambers@marine.usf.edu

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Dear Dr. Chapman,

I appreciate your review of this paper and the obvious effort you took. Based on your comments and those of the second reviewer, I have extensively revised the paper. I have attached the fully revised paper with track changes added so you can see where I made changes.

Below I also answer your comments and describe how I have modified the paper. I'm sorry that the format that OS requires for inputting comments does not easily allow for

highlighting original comments and responses (I'm afraid I have forgotten al the LaTeX commands I ever knew), but I have tried to differentiate the original comment with REVIEWER COMMENT, my response with RESPONSE, and any additions to the text will be in quotes. Also, please note that there are some Figures with I have a attached to answer your comments.

Cheers,

Don Chambers

REVIEWER COMMENT Major comments: Engagement with prior literature The author argues that what I will call the 'contour' view of Southern Ocean fronts (as advanced by the various papers by Sokolov & Rintoul and used in numerous studies since) gives a spurious rise to a spurious southward movement of the various fronts.

While I have no problem with this interpretation of the evidence (quite the contrary, I support it) this idea is not particularly new, and the discussion in this paper does not place the new results in the context of the extensive work that has been conducted since. While the author does cite the work of Graham et al (2012), Gille (2014) and Freeman et al. (2016), the discussion is cursoary I feel that the current state of knowledge is clearer than is presented in this paper.

For example, Thompson et al. (2010) showed using local histograms of potential vorticity in an eddy resolving model that the ACC jet/frontal structure undergoes rearrangement both spatially (generally in the lee of large topographic features) and temporaly as a result of localised mixing of the PV. The structural rearragement of the fronts complicates their intrpretation as contours of some quasi-conserved quantity. Graham et al (2012) studied in detail the response of fronts defined with contours (à la Sokolov & Rintoul) to shifts in high SSH/ADT regions, finding that the elevated grad(SSH) can shift substantially without a corresponding change in the location of the contour that is supposed to track it. Chapman (2014) showed that the temporal and spatial variability of fronts was strongly influenced by their definition.

Additionally, several relevant studies that directly study the shifts (or lack thereof) are not discussed in this study. For example, Shao et al. (2015) used a method based on higher order statistics to investigate trends in frontal position (and their response to climate modes like SAM and ENSO), finding essentially none. More recently in Chapman (2017) I used my method (described in Chapman 2014) together with a statistical model of the frontal occurrence maps to revisit this problem, once again finding no change.

As such, it seems like a consensus is starting to emerge – shifts detected with the contour method are likely spurious, and there has been minimal observed variability in the locations of the fronts and jets that make up the ACC. This paper would be more useful in the current context by explain where its results fit in, and what it does that other papers do not.

Response: I have updated the Introduction extensively based on the recommendations. Please see the attached revision with track changes on. I apologize for not having seen your 2017 paper in my literature review. I have utilized the results from it in a new figure (Figure 7), which I believe should mitigate some of your concerns about this method being able to track the positions of fronts. I now show that the detected half-power points fall nicely in the probable locations of jets that you have calculated. Also, thanks for pointing out that Shao et al (2015) paper. I completely missed that one in my review.

REVIEWER COMMENT Methodology In order to get the front location from the CKE along the satellite ground tracks, the author defines a kind of centroid, which he calls the "half-power point" (although a pedant might note that this is, actually, the half-energy point, since we're dealing with energy directly and not power), which is defined in Eqn. 2. The location of this "half- power point" is then taken to be representative of the location of the front.

I'm not convinced that this metric measures what the author says it does. For instance,

the locations of jets and their associated fronts are usually take to local maxima of the SSH gradients (corresponding to the highest geostrophic velocities – and in this study, the peaks in the CKE). However, the example calculation presented in Fig. 3 shows that the half-power point is located between two peak in a local CKE minimum. One could argue (and I think the author does try to) that regardless of whether the half-power point is detecting the maxima or not, it's following the (approximate) centroid of the CKE distribution along that ground track, and thus is representative of the frontal "envelope" (for want of a better term). However, I don't see it in this example, where the two major peaks are around 10 degrees and can't really be considered part of the same feature.

Response: I have included three new figures and extensive comments in the revised paper to answer your concern. First, the new Figure 4 shows the three-year averages of CKE along this pass as distinct plots to better visualize the problem of variability of the peaks. In no year is a distinct minimum CKE observed that is less than 200 cm2 s-2. Because of this and the variable number of maxima, I am hesitant to track the individual peaks, as they do not always exist.

I have added the following text in the revision, around lines 381 to 397 to justify this:

"The mean CKE profile pictured in Figure 3 has multiple local maxima, most likely associated with the variability in the narrow jets that surround the front. As shown by Chapman (2017a), these jets (evidenced in higher gradients of SSHA) do not occur around a front 100% of the time. At most, they occur about 30% of the time, and more often less than 15% of the time. Figure 4 shows the behavior of CKE along this pass for different 3-year periods. Note that the number of clearly defined maxima ranges from a low of 4 for the 2014-2016 average to 9 in 1993-1995. While other studies have estimated positions of these maxima in SSHA gradients on as short as daily intervals (e.g., Chapman, 2017a), one does not obtain a consistent number of maxima each time, making the determination of shifts difficult. Moreover, note that although there are two general peaks in CKE in the long-term mean profile, the minimum between them is still higher than 200 cm2 s-2. A minimum is also not well defined in several of the shorter averaging periods (for example, 2008-2010). Thus, instead of attempting to track all the maxima of CKE individually – analogous to tracking steepest gradients, as in Thompson et al. (2010), Graham et al. (2012), or Chapman (2017a) – we track an estimate of the center of the envelope of enhanced CKE, as it exists in all averaging periods. The assumption we make in doing this is that the localized maxima are associated with variable jets, but the position of the envelope of high CKE is related to the front."

Figures 5 and 6 in the revised paper show the estimate mean frontal positions with those from other studies, including yours (Figure 6). I feel this shows rather nicely that the positions of the half-power point of enhanced CKE falls within the estimates made by others using different techniques.

REVIEWER COMMENT An additional problem arises due to the different forms of the CKE profiles (Fig. 4). How does the calculation of the half-power point depend on the structure of the CKE profiles? I ask as I can imagine that, particularly in the case of the skewed profile (Fig. 4b) that the half-power point would be strongly biased and I'm not really sure what it would be measuring. On top of this, there's no real attempt to compare the new method with previous studies, save for the very cursory comparison in Fig. 2.

The author could clarify his calculation here by repeating the half-power point calculation on either a) some idealised profiles along the lines of those presented in Fig. 1; or (b) choosing some representative profiles and presenting them as examples in an expanded Fig. 3.

Response: Comment on skewed example, the half-power is biased toward the peak away from a simple mid-point computed from the average of the southern and northern points of the envelope. This is preferred, as it means the location is closer to the peak CKE where one would assume the strongest currents associated with the front are

located.

The following text as been added to the manuscript around lines 398-428 to address this:

"There are many different ways to compute a "center" of the envelope, ranging from the average of the two end points, to a centroid calculation, to computing the point where the integral of CKE over distance is balanced on both sides, which we call the "half-power point." We have selected the latter to use, as it defines a "center" closer to the peak of CKE in the envelope. This is advantageous when the CKE curve is slightly skewed, with less magnitude on one side and more on the other. Assuming that the variability (and hence CKE) would be highest near the front (i.e., what is assumed in studies using the gradient method), finding a center of the envelope that is biased toward peak CKE is a reasonable approach." Additionally, we have included statistics on the various percentage of CKE shapes found (lines 487-494 in the revised text):

"Four general types of enhanced CKE were found (Figure 5). In most regions, the envelope in CKE is more or less symmetrical (52% of cases). Only a few profiles have two distinct regions of enhanced CKE were identified, with a clearly defined minimum below 200 cm2 s-2 between them in all time periods (3% of cases). 20% of the passes have multiple peaks that vary in time (i e., Figure 4), while 25% have a skewed envelope (Figure 5), with a long rise in CKE a long rise followed by a sharp drop-off. In all cases, though, the shape of the CKE envelope closely follows that of EKE, although the amplitude was attenuated, by anywhere from 25-50%. Having closer samples of CKE, however, allows for a better computation of the half-power point."

REVIEWER COMMENT Additionally, it would help build the author's case if there was a more detailed comparison of the fronts defined in this work with other studies. This comparison needn't be too detailed, but a brief discussion would certainly help build confidence in the author's calculation.

Response: See new Figures 6 and 7, along with the new discussion between lines 495 and 605):

“Figure 6 shows the locations of the half-power points determined from the mean CKE profiles, along with estimate of the front position based on different methods: density gradients from historical hydrographic sections (Orsi et al., 1995), dynamic topography contours (Kim and Orsi, 2014), and the gradient of sea surface temperature (Freeman and Lovenduski, 2016a). There are two estimates of the SAF and SACCF, and three of the PF. One of the PF estimates (from Freeman and Lovenduski, 2016a) includes the standard deviation of the daily estimates.

It is important to note the large differences in the estimates for the same front, which indicates how uncertain these calculations are. For instance, in the Indian Ocean at 50°E, Freeman and Lovenduski (2016a) find the PF at the same location that Orsi et al. (1995) found the SAF, while Kim and Orsi (2014) find it significantly farther south. The SAF determination using the contour method (Kim and Orsi, 2014) is substantially farther north than the one determined from hydrographic data (Orsi et al., 1995) at most longitudes.

Many estimates from the half-power points of enhanced CKE occur between the same front estimated by different methods, indicating they are at least within the uncertainty bounds of frontal detection by any method. Other values are at locations either north or south of the other front estimates by as much as 3°, but it should be noted that the standard deviation of the PF estimated by Freeman and Lovenduski (2016a,b) averages 2-3°, indicating these positions estimated from CKE are within the level of expected frontal variability.

Probably a better method for determining frontal position is to examine the probability of jets occurring (Chapman, 2017a) (Figure 7). The CKE-defined mean front positions lie within the probability envelopes, giving more confidence that the CKE measure is providing a comparable measure of frontal position in many areas. The only location where CKE-defined fronts don’t agree well with the probability field from Chapman

(2017a) is just west of the dateline, where two points lie between levels of high jet (and hence front) probability.

Still, the good comparison is reassuring that the method developed in Section 2 is successfully detecting regions of high energy related to jets around fronts. Since the movement of jet positions has been used to estimate movement of the fronts (e.g., Chapman, 2017a), a comparable calculation with positions of high CKE seems reasonable.”

REVIEWER COMMENT Lines 49-50: "First, it assumes that the average of the position shift of the contours across all longitudes represents the shift at all longitudes” I disagree. Sokolov & Rintoul (2009)b show numerous examples of localised shifts in the contours. The most notable is the approximately 10 degree shift of the contour associated with the PF-N as it traverses the Kerguelen Plateau (see their Fig. 12). The problems with contour type methods are discussed in depth in Graham et al. (2012) and Gille (2014) and Chapman (2014,2017).

Response: Agree. I have revised this section extensively. The revised text now reads (lines 69-99 of revised text):

“Using the contour method and tracking how the dynamic topography contours associated with a front position shift in time, Sokolov and Rintoul (2009b) found that the SAF and PF had both moved south by approximately 60 km over 15 years between 1993 and 2008). Kim and Orsi (2014) recently updated this analysis and found that while the average frontal position across the Southern Ocean indicates a strong southward shift, this is due to primarily to substantial shifts only in the Indian Ocean sector. They found no significant shifts throughout the Pacific or Atlantic Ocean sectors using this contour method.

The primary assumption of these analyses if a contour of dynamic topography shifts south, it is uniquely caused by front moving south. This is not true. Gille (2014) recently demonstrated that all contours in the Southern Ocean have shifted south on average, and that this follows from the observed rise in sea level – as the sea surface height rises, the contours will appear to shift south. While this breaks down at the far south and north of the ACC when dynamic topography gradients are small, these areas are far away from the PF and SAF. Gille (2014) used a different measure to determine the position of the ACC fronts, based on the latitude of the mean surface transport of the ACC measured by altimetry, which is in essence a mean location of all the jets in the Southern Ocean. She found no significant shift on average, but considerable interannual variability, especially regionally."

REVIEWER COMMENT Line 53: While Kim & Orsi do find some migration of the fronts in the Indian Sector, it's worth noting a number of studies find no significant shifts (Graham et al. 2012, Shao et al. 2015, Chapman 2017).

Response: Noted and changed in the revision. The new text is (lines 107-155 of revised text):

"Researchers using other methods also find little or no southern migration of the fronts or jets in the Southern Ocean as a whole. Graham et al. (2012) used a high-resolution model to show that the Polar Front and Subantarctic Front are constrained by bathymetry, even in increasing and shifting winds. Gille (2014) found no significant change in the latitude of mean transport of the surface currents in the ACC. Shao et al. (2015) utilized the skewness of sea level anomalies to identify front positions, and found no southward motion, but did find changes in the east Pacific correlated with the Southern Annual Mode. Chapman (2017), using positions of fronts determined from the probability of jet locations, also found no significant southward movement, but high interannual variability. Finally, Freeman et al. (2016) used weekly estimates of the Polar Front position determined from satellite sea surface temperature (SST) gradients to show no significant southward shift between 2002 and 2014 on average, except in the Indian Ocean. They also found a statistically significant northward shift of the PF in part of the south Pacific."

REVIEWER COMMENT Lines 75-78: "Here, we will utilize a new method to study the position of the fronts in the Southern Ocean, based on tracking the location of eddy kinetic energy (EKE) measured by altimetry. It is known from modeling studies that the front positions are associated with increased EKE, due to instabilities in the jets and interactions with bathymetry ... ." I got very, very confused reading this article because of this paragraph. Here, the author states that he will use EKE to track fronts. This set off alarm bells in my head, because although the author is in general correct that EKE is higher along jets (see Hughes 1996), I can show you evidence from both models and altimetry that shows broad, high EKE regions spread over a wide range of latitudes that encompass several fronts, particularly in "storm track" regions. Thus if the author were to use high EKE to detect fronts, I'd be skeptical and would probably need some convincing. However, in section 2, the author appears to be using to the total kinetic energy, as he interpolate the mean MDT from the DTU10 to the ground track. Additionally, the simple examples presented in the Fig. 1 thought experiment seem to be based on the ADT and not the SLA. Some clarification would be welcome here.

Response: I apologize for the confusion and tried to make clear I was using anomalous currents throughout Section 2, and not absolute currents. The MSS from DTU10 is used to compute sea level anomalies, as I was clear to note. I never use MDT except in the one example at the front, which is just used to motivate a way that MDT contours could shift south for other reasons than sea level change.

I have revised section 2 to put the discussion of EKE and CKE closer together to hopefully avoid this confusion, and have added the word "anomalous" more throughout to remind the reader I am only discussing anomalous currents.

While the reviewer is correct that there can be some places with high EKE over large regions, this will be at a lower limit than what I examine. It will also tend to be more episodic, and not consistent from one year to another.

I've revised the section on the calculation of the CKE envelope to discuss this more. The revised text (lines 357-371) is below:

"Several criteria were utilized to quantify where the high CKE values were considered to be associated with fronts. First, we constrained the southern boundary to be 5° south of the Orsi et al. (1995) values of the PF and the northern boundary to be 5° north of the SAF. Secondly, we used a lower-limit for CKE of 200 cm2 s-2 for detection and tested that the width of the envelope of high CKE exceeded the lower-limit for at least 100 km. The requirement that the envelope be greater than 100 km was done to reduce the impact of eddies in an otherwise quiescent region, since the diameter of eddies in the Southern Ocean is about 100 km. The CKE lower-limit was determined via iteration with different limits. For each case, the average center of the CKE envelope averaged over 24-years (based on the mean of the first and last points to exceed the lower-limit) was computed and compared visually to the Orsi et al. (1995) front positions. 200 cm2 s-2 was selected because there were a significant amount of CKE envelope centers clustered around the Orsi et al. (1995) fronts and the envelopes were found for every 10-day repeat cycle. Using a higher limit resulted in fewer detections, espscially when smaller time-averages were used. Using a lower limit, we could find more potential front positions based on CKE, but many were far from the front positions estimated by Orsi et al (1995)."

Moreover, the new Figure 7 nicely shows the positions of fronts estimated from CKE using this method align nicely with the locations found by Chapman (2017) based on the probability of a jet being present.

REVIEWER COMMENT Line 118-120: Part of the author's justification for using the along track altimetry is that the gridded product attenuates the EKE due to the optimal interpolation used. So does optimally interpolating along the ground cause the same attenuation (albeit somewhat reduced)?

Response: Optimally interpolating ANY data set can lead to attenuation of signal, but

this is not something that is considered by the 99% of users who utilize the gridded AVISO products. Those products uses data from multiple passes (and altimeters) as far as 30-days away and several hundred km away (albeit weighted by a temporal/spatial covariance function) to create "1-day" grids. My OI filter utilizes data from only a few minutes and up to 200 km away along the track to optimally interpolate the data to reduce the effect of noise. As we discussed in the Hogg et al. (2015) study, the difference between the EKE from the along-track OI versus that from the gridding OI was about a 60-70% attenuation in EKE.

How to quantify the attenuation of the along-track OI filter? One way to do this is to compare OI filtered data with non-filtered data, along with an estimate of the expected noise based on the estimation from 1-Hz SSH anomaly data. Below, I show a figure of the average of OI CKE compared to CKE computed from unfiltered SSH data (Figure R1, left) for one particular pass. As can be seen, the CKE from the unfiltered data is significantly higher, but I will show this is mainly due to noise in the SSH measurement. Recall that to compute the CKE, one has to take the numerical derivative of SSH data. If the data is "noisy", the noise in the derivative will be even higher. This is then multiplied by g/f to get velocity, then squared to get CKE, increasing the noise even more. Note that since f is proportional to sin(latitude), the CKE noise should get higher as one approaches the equator. This can be seen in Figure R1 (left) as the slight slope from south to north in the CKE based on unfiltered CKE.

In a 2003 paper (Chambers D. P., J. C. Ries, and T. J. Urban, Calibration and Verification of Jason-1 Using Global Along-Track Residuals with TOPEX, Marine Geodesy, Special Issue on Jason-1 Calibration/Validation, Part 1, Vol. 26, 305-318, 2003), I estimated the RMS error in 1-sec sampling Jason-1 data by comparing to coincident TOPEX measurements and tide gauges. I estimated an error in the 1 Hz data of 3.7 cm. However, I also showed a portion of this error is correlated over distances > 100 km – due to orbit errors, dry water vapor correction errors, sea state bias correction, etc. All of these will nearly be common between adjacent 1-sec bins, so will cancel out when velocity is computed. Thus, we have to only be concerned with the uncorrelated part, which I estimated to be about 2.9 cm. Assuming this is uncorrelated between adjacent bins will increase the error in the difference to 4.0 cm, which means an error in SSH anomaly gradient of 2.8 x 10-6 radians (on average). If this is converted into CKE error (Figure R1, right) one sees this noise model nicely fits the residuals between the unfiltered and OI-filtered data. This suggests the majority of the difference in the filtered and OI filtered data is noise, although there may be some attenuation of very low-signal variability (i.e., between 56°S and 54°S), but this is not considered in this study.

If one removes the noise model from the unfiltered CKE (Figure R2), it lies on top of the CKE based on the filtered SSH anomalies, suggesting the attenuation is minimal. It is definitely much smaller than the 60-70% (on average) in the EKE computed from the gridded AVISO products. The residual differences are as likely to be from noise as any real signal.

In my opinion, a long discussion of this in the paper is not warranted. However, if the reviewer and editor feel it is necessary, I would be happy to add this discussion and the figures as an appendix to the revised manuscript. At the moment, the following has been added to the revised manuscript in the middle of Section 2 (lines 224-239), after describing where to obtain data and basic processing:

“We utilize this record rather than the gridded products based on mapping SSH from multiple altimeters (e.g., Ducet et al., 2000; Pujol et al., 2016), because the along-track data have a finer resolution in space (6.9 km along the groundtrack) and we recently demonstrated that the mapped altimetry data underestimated eddy kinetic energy (EKE) throughout the Southern Ocean compared to using along-track data by as much as 60-70% (Hogg et al., 2015). While the along-track sea level anomalies are filtered to reduce noise and thus may attenuate some signal, the filtering used (described later in this section), is less than that used for the mapped data, which uses observations from as long as 20 days and 200 km away to influence the mapped value.

By filtering only alongtrack data, the time differences are small (a few minutes at most), and the spatial influence is less than 100 km. Tests with unfiltered data accounting for estimated random noise in the sea level anomaly data suggests attenuation of kinetic energy is minimal with this approach and, more importantly, that the shape of the kinetic energy envelope does not significantly change.”

REVIEWER COMMENT Line 146-148: “We initially tried tracking each of the maxima, but that quickly became complicated because sometimes the four or local maxima would become five, or even just one. This is likely due to the instability of the jets around the front.” While tracking maxima is complicated, it’s not impossible to do. After all, it’s been done in Thompson et al. (2010), Graham et al. (2012) and Chapman (2017) who showed a variable number of jets around the ACC. While this complication certainly could justify moving to the centroid method, I’d still like to see some stronger justification that it does pick up the frontal locations (or, at the least, their envelopes) - see major comments

Response: I have modified this discussion to acknowledge that while others have examined the position of the steepest gradient, these positions are far more variable than looking at the centroid of the enhance CKE envelope. I have also included arguments as to why I have chosen to look at the position of the centroid over time instead of the specific jets. I have also added a new figure (new Figure 4) showing the CKE for this pass for different 3-year averages to show how the local maxima vary from period to period.

The relevant new text is (lines 381-397):

“The mean CKE profile pictured in Figure 3 has multiple local maxima, most likely associated with the narrow jets that surround the front. As shown by Chapman (2017), these jets (evidenced in higher gradients of SSHA) do not occur around a front 100% of the time. At most, they occur about 30% of the time, and more often less than 15% of the time. Figure 4 shows the behavior of CKE along this pass for different 3-year periods. Note that the number of clearly defined maxima ranges from a low of 4 for the 2014-2016 average to 9 in 1993-1995. While other studies have estimated positions of these maxima in SSHA gradients on as short as daily intervals (e.g., Chapman, 2017), by doing this one does not obtain a consistent number of maxima each time, making the determination of shifts difficult. Moreover, note that although there are two general peaks in CKE in the long-term mean profile, the minimum between them is still higher than 200 cm2 s-2. A minimum is also not well defined in several of the shorter averaging periods (for example, 2008-2010).

Thus, instead of attempting to track all the maxima of CKE individually – analogous to tracking steepest gradients, as in Thompson et al. (2010), Graham et al. (2012), or Chapman (2017) – we compute the center of the envelope of enhanced CKE and track that, as it exists in all averaging periods. The assumption we make in doing this is that the localized maxima are associated with variable jets, but the position of the envelope of high CKE is related to the front."

Finally, I include new figures showing the location of the half-power points relative to other front estimation methods (Figure 6) and relative to the probability functions of Chapman (2017) (Figure 7). I hope these demonstrate that these assumptions appear valid, in that the half-power points align with locations that other studies have detected fronts.

The figures and discussion of them has been added to Section 3 and can be seen on lines 495 and 605 of the attached revised document with track changes.

REVIEWER COMMENT Line 158 (Eqn 2): The author calls this a centroid, but generally a centroid is defined as the first moment (or center of mass) and not the half-power point. Has the author attempted to use the standard definition? Does the half-power point have any advantages?

Response: This is true. I have removed the word "centroid" in the revised text, and use "half-power point" throughout. I have computed the centroid using the standard

definition and compared to the "half-power point" in several cases. For most examples, they are nearly the same (1-2 6.9 km bins away). Only in the skewed CKE example is there a major difference, and in that case the half-power point is closer to the peak of CKE. In this case, I would argue the half-power point is better, as it is closer to what one would determine using maximum gradient methods. I have added text in Section 2 to describe this argument.

The relevant text is:

"Thus, instead of attempting to track all the maxima of CKE individually – analogous to tracking steepest gradients, as in Thompson et al. (2010), Graham et al. (2012), or Chapman (2017) – we track an estimate of the center of the envelope of enhanced CKE, as it exists in all averaging periods. The assumption we make in doing this is that the localized maxima are associated with variable jets, but the position of the envelope of high CKE is related to the front.

There are many different ways to compute a "center" of the envelope, ranging from the average of the two end points, to a centroid calculation, to computing the point where the integral of CKE over distance is balanced on both sides, which we call the "half-power point." We have selected the latter to use, as it defines a "center" closer to the peak of CKE in the envelope. This is advantageous when the CKE curve is slightly skewed, with less magnitude on one side and more on the other. Assuming that the variability (and hence CKE) would be highest near the front (i.e., what is assumed in studies using the gradient method), finding a center of the envelope that is biased toward peak CKE is a reasonable approach."

Please also note the supplement to this comment:
https://www.ocean-sci-discuss.net/os-2017-57/os-2017-57-AC1-supplement.pdf

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Interactive comment on Ocean Sci. Discuss., https://doi.org/10.5194/os-2017-57, 2017.

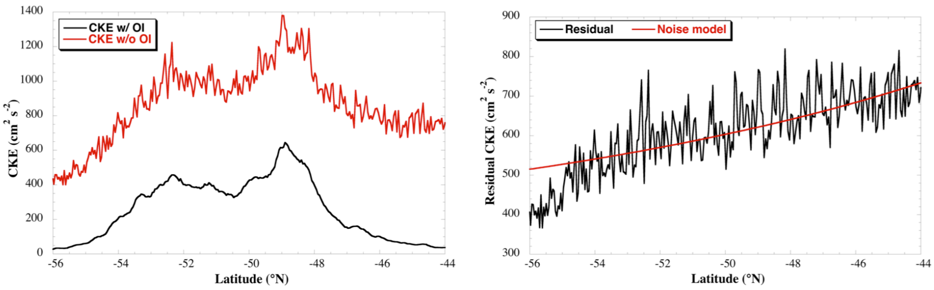

Figure R1. (left) CKE averaged from 1993-2016 along a part of a satellite pass in the Southern Ocean for SSH anomalies that have been optimally interpolated (black line) and from the raw SSH anomalies with no filtering (red line). (**right**) residuals between unfiltered CKE and CKE based on OI-filtered data, along with a noise model based on estimate uncorrelated error in Jason-1 SSH anomalies between 1-sec bins.

Fig. 1. (left) CKE averaged from 1993-2016 along a part of a satellite pass in the Southern Ocean for SSH anomalies that have been optimally interpolated (black line) and from the raw SSH anomalies with no fi

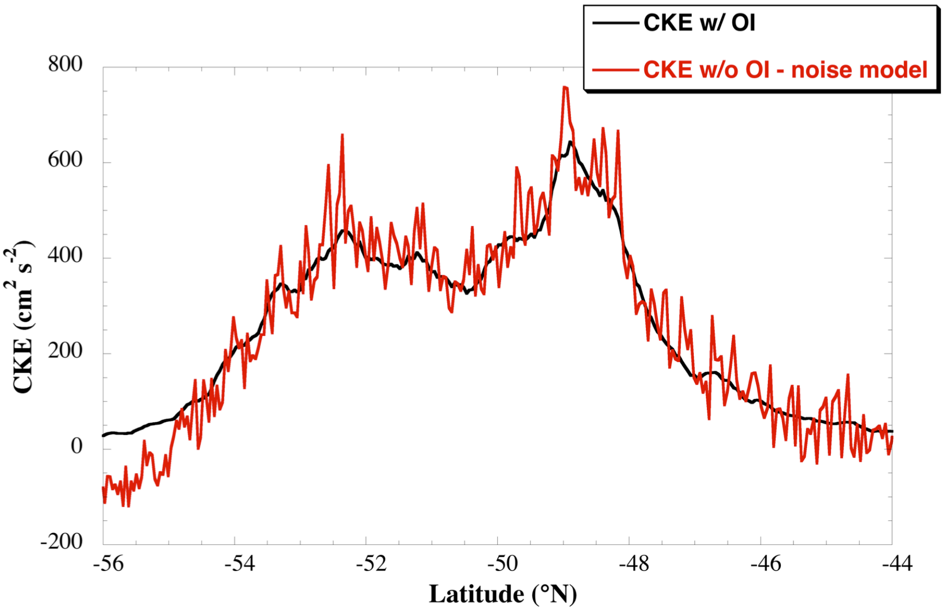

Fig. 2. CKE averaged from 1993-2016 along a part of a satellite pass in the Southern Ocean for SSH anomalies that have been optimally interpolated (black line) and from the raw SSH anomalies with no filtering