

# ***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***

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Received and published: 12 October 2017

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 all the LaTeX commands I ever knew), but I have tried to differentiate the original comment

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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 attached to answer your comments.

Cheers,

Don Chambers

REVIEWER COMMENT: Clarity and writing style turned out to be of significant issue. Manuscript text is jumpy and mostly written in a narrative tone rather than a scientific one (including a mixture of tenses, writing as though speaking casually, etc.) which made its reading quite difficult and confusing at times. Before publication, I recommend that text be overhauled to ensure the study is translated to the broader community effectively and to avoid losing any specific details and major findings. If text limit is not an issue, I recommend adding text and analysis where appropriate which would greatly improve the overall transparency and reproducibility of the work as well as its placement in the broader literature.

RESPONSE: Writing has been revised to keep the same tense (all present) and to add more details on processing, locations of example tracks, and statistics of data in order to make results reproducible. Please see the revised text with track changes turned on to note changes.

REVIEWER COMMENT: The use of 'over three-year periods' throughout the text (e.g., In 9, Ins 165-167, etc.) is inaccurate, as averages were not consistently taken over three years. As such, I would suggest that the author use 'multi-year periods,' etc. throughout the manuscript and include an explanation for those chosen. Specifically, in Figure 5, the author uses years 2011-2012 as a two-year grouping while the others before and after are three-year groupings. (1) What is the basis for using 3-year periods? (2) Does the choice of x-year groupings affect the mean positions significantly? (3) Would this choice then affect the interpretation of the long-term trend (i.e., are the reported trends/shifts sensitive to the magnitude of groupings)?

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RESPONSE: This was partly a typo. It should have been 2011-2013. But the last year was only 2-years at the time of submission. 2016 data has now been processed and all results have been updated. There is no significant change. Since all groups are now three years, I will use three-year periods. The three-year average was taken to reduce influence of annual and Southern Annular Mode variations, which has been demonstrated previously. I have added text to explain the selection of three-years for the averaging period at the end of Section 2 (lines 439-477 of revised paper):

“A similar procedure was done for CKE averaged over discrete 3-year intervals, starting in January 1993 and ending in December 2016. A 3-year average was used to reduce the influence of individual eddies on determining the envelope, and to reduce interannual variations in the front position, which have been observed in other studies at some locations (e.g., Kim and Orsi, 2014; Shao et al., 2015). In particular, Kim and Orsi (2014) and Shao et al. (2015) found significant correlation with the Southern Annular Mode, which has a quasi-biennial oscillation (Hibbert et al., 2010). By averaging over three years, we found 8 distinct, statistically uncorrelated samples of CKE for each groundtrack from which to deduce shifts in the half-power point.”

I do not believe it is worth the space to evaluate different averaging periods, since all are consistent now. I have tested this, however, and find that results from two-year averages are not statistically different, if one accounts for increased autocorrelation in the residuals to the trend fit.

REVIEWER COMMENT: The author motivates accurate ACC front and jet detection in light of future climate change. However, by failing to make a clear distinction between a front and a jet, the author risks adding to the already existing confusion in the literature by consistently treating them as the same thing. I feel strongly that the author should include more text on the distinction between these two physical and dynamical features, use ‘fronts and jets’ rather than ‘fronts/jets’ throughout the text (e.g., In 31), and strive to make it clear when a front or a jet is being referenced and maintain consistency throughout. Even after completing this review, I am still unsure whether this analysis

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sought to detect shifts in fronts or jets, despite the title.

Moreover, while I agree this method is novel, I feel it is misleading/inaccurate to say that this study locates fronts themselves, but rather, like Gille (2014), identifies and uses a proxy for frontal and jet-like features. Please comment on this distinction. Also see specific in-line (ln 152) comment below.

RESPONSE: Agreed. I have added several comments in the introduction to discuss the difference between fronts and jets, and the confusion that authors refer to them interchangeably. I also comment on the fact that other studies have shown one cannot actually identify a “front” at any specific time, only in the mean sense. In the revision, I now refer “fronts and jets” or “fronts or jets” and never use “fronts/jets.” The revised text is given below (lines 42-58 in the tracked changes document):

“Because of the highly variable nature of jets and the lack of clear observational detection of fronts in some areas, the literature has become muddled over the difference between a front and a jet, primarily because the “front” is rarely observed at any specific time due to the high-variability of jets (Thompson et al., 2010; Thompson and Richards, 2011; Chapman 2014; 2017). However, even in the presence of highly variable jets, methods have been developed to determine mean fronts positions in a probabilistic sense. Thompson et al. (2010) demonstrated one could define fronts in the Southern Ocean by computing probability density functions of potential vorticity in an eddy-resolving general ocean circulation model. Chapman (2014, 2017) later showed this could also be done using localized gradients in dynamic topography (i.e., high geostrophic velocity) using satellite altimeter observations, but again, only as statistical probability. This is because these areas of enhanced gradients and velocity are more reflective of jets, which strengthen and die, appear and disappear, bifurcate and join back together. Because of this, they can only be detected on average 10-15% of the time. However, Chapman (2014, 2017) has demonstrated that, at least in a mean sense, fronts defined by mean dynamic topography contours (commonly known as the “contour method”) do lie within the probability distribution inferred from “gradient”

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methods.”

Then at the end of the Introduction (lines 160-186):

“In this paper, we develop a new method to study variability in the position of the fronts in the Southern Ocean, based on tracking the location of envelopes of kinetic energy measured by altimetry. It is known from modeling studies that the front positions are associated with increased kinetic energy, due to instabilities in the jets and interactions with bathymetry (Thompson et al., 2010; Thompson and Richards, 2011). After demonstrating that kinetic energy computed from along-track satellite altimetry forms relatively wide envelopes of enhanced energy that occur within the probability range of jets and fronts (Chapman, 2017), we track the position of this envelope from 1993 until 2016 to quantify if the envelope has shifted south by a statistically significant amount. This is based on the assumption that if the front and jets around the front has shifted south, then the envelope of high kinetic energy should also move by a comparable amount. Since kinetic energy calculation also depends on estimating gradients of sea level anomalies, this approach is similar to other gradient methods for detecting fronts or jets (e.g., Chapman, 2014; 2017; Gille, 2014; Freeman et al., 2016). It differs from these approaches, however, in that instead of determining individual gradients and tracking these over time, it looks for regions of high gradients (i.e., high energy) surround by regions of low gradient (i.e., low energy). This allows us to detect envelopes for every time-period considered, instead of only a fraction of the time, allowing for better tracking of the change over time.”

Moreover, in the rest of the paper, I am clear on using “fronts” to describe a mean location of the average current or transport (as measured by CKE), while “jets” are used to describe the smaller length-scale, but highly variable CKE peaks. I feel this is consistent with other studies (e.g., Chapman, 2017).

REVIEWER COMMENT: Given the relatively higher resolution of the along-track data, please comment on how the presence of small-scale features (e.g., eddies) might affect

the methodology and/or results, if any?

RESPONSE: I have addressed this in the following text revision in Section 2 (lines 357-371):

“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 cm<sup>2</sup> s<sup>-2</sup> 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 cm<sup>2</sup> s<sup>-2</sup> 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, especially 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).”

REVIEWER COMMENT: Given what we know of the influence of the depth of the ocean on ACC front and jet positioning, please comment on any quantitative assessments relating to seafloor topography? For instance, (1) are identifications of fronts and jets more successful near shallow regions or (2) is the magnitude of the ‘uncertainty’ in the trends shown in Figure 6 influenced by the depth of the ocean?

RESPONSE: In my opinion, discussing the front and jet positions relative to bathymetry is a little off topic to this paper, and so I have chosen not to discuss it in the revised text.

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I think it is more important to discuss the locations relative to other estimates, which I have done.

I have plotted the locations of the mean CKE half-power points along with ocean depth to show the reviewer (Figure R1, below). In general, there is no clear pattern that emerges. In some areas, high CKE is found in very deep water with no significant bathymetry changes around it (i.e., 90°W), in other places high CKE is found in deep water between shallower bathymetry, which likely leads to stronger currents and more turbulence (30-60°E). In other areas, the points follow more moderate bathymetry (3000 – 4000 m depth, 120°E-150°E). I also found no correlation between higher variance about the trend (i.e., uncertainty) and depth.

REVIEWER COMMENT: If possible, please comment on how this newly-developed methodology compares (in skill, accuracy, etc.) to previous front-detection methodologies and the recommendation, if any, for its future use? Increasing the size of Figures 1 and 4 would greatly improve readability of axes.

RESPONSE: I have done this with a two new figures (Figures 6 and 7) and a revised discussion in Section 3, 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

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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  $3^\circ$ , but it should be noted that the standard deviation of the PF estimated by Freeman and Lovenduski (2016a,b) averages  $2-3^\circ$ , 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: Ins 34-37 Please include relevant citations.

RESPONSE: This refers to the statement about the model winds. References are given to Fyfe and Saenko (2006) and Swart and Fyfe (2012). So I don't fully understand this request. The statements that follow are my observations of the figures in the paper, so don't need a reference. I have added a reference to the particular Figure in the paper that shows this:

“It should be noted, however, that the mean position of the southern hemisphere westerlies in the models lies significantly equatorward of the true position (e.g., Figure 2 in Fyfe and Saenko, 2006).”



REVIEWER COMMENT: In 71 I could not find the citation in the References section for Freeman et al. (2016).

RESPONSE: I apologize for the oversight. It has been added.

REVIEWER COMMENT: In 75As the author has developed this new method, they should highlight it (e.g., 'Here, we utilize a new method . . .' should most definitely read 'Here, we develop a new method. . .')!

RESPONSE: Done

REVIEWER COMMENT: Ins 75-79 The motivation behind using KE measurements is presented in a sloppy manner in this last paragraph of the introductory text. This motivation should be made stronger and clearer.

RESPONSE: This has been revised on lines 164-186:

“After demonstrating that kinetic energy computed from along-track satellite altimetry forms relatively wide envelopes of enhanced energy that occur within the probability range of jets and fronts (Chapman, 2017), we track the position of this envelope from 1993 until 2016 to quantify if the envelope has shifted south by a statistically significant amount. This is based on the assumption that if the front and jets around the front has shifted south, then the envelope of high kinetic energy should also move by a comparable amount. Since kinetic energy calculation also depends on estimating gradients of sea level anomalies, this approach is similar to other gradient methods for detecting fronts or jets (e.g., Chapman, 2014; 2017; Gille, 2014; Freeman et al., 2016). It differs from these approaches, however, in that instead of determining individual gradients and tracking these over time, it looks for regions of high gradients (i.e., high energy) surround by regions of low gradient (i.e., low energy). This allows us to detect envelopes for every time-period considered, instead of only a fraction of the time, allowing for better tracking of the change over time.”

REVIEWER COMMENT: In 87 I think the inclusion of the word 'high' is a typo here.

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RESPONSE: Yes. Thanks for catching that.

REVIEWER COMMENT: Ins 83-39 Perhaps some rearranging of text is needed? The author motivates and suggests that the study uses EKE but then immediately throws it out in this section.

RESPONSE: Yes, I agree. I have moved the discussion of the altimetry data specifics (download and processing) to the beginning, then move the discussion of EKE and CKE after that, to keep them together and explain why EKE computed from the along-track data is not as high-resolution, so CKE is used instead. I have also moved up the equations discussing calculating EKE into Section 2 before discussing CKE.

REVIEWER COMMENT: In 104 Please provide the citation(s) for (and/or why) these corrections (are recommended).

RESPONSE: A reference has been added

REVIEWER COMMENT: Ins 100, 105 Please make clearer the explanation for the interpolation method and model used. Here, it reads as if the author uses the DTU10 model to create the interpolated data (In 100) but also that this model is then subtracted from that interpolated data (In 105). Is it a model or model output?

RESPONSE: I have clarified the discussion. One can either interpolate the SSH data to a mean track using the gradients of the MSS (in bilinear interpolation), or interpolate the MSS to the SSH location using bilinear interpolation. The results are the same. The model is an average of nearly 2 decades of satellite altimetry data, so not a numerical model output. The new text is (lines 215-223):

“We utilize the 1-Hz along-track SSH data from the four altimeters and compute sea level anomalies by interpolating the DTU10 mean sea surface model (Andersen and Knudsen, 2009; [http://www.space.dtu.dk/english/Research/Scientific\\_data\\_and\\_models/downloaddata](http://www.space.dtu.dk/english/Research/Scientific_data_and_models/downloaddata)) to the SSH location using bilinear interpolation. The DTU10 mean sea surface model

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is based on SSH from multiple altimeters averaged over 17 years in a rigorous and consistent manner (Andersen and Knudsen, 2009). T/P, Jason-1, and Jason-2 data were all included. All recommended geophysical and surface corrections (e.g., water vapor, ionosphere, sea state bias, ocean tides, inverted barometer, etc) have been applied, to correct for biases introduced by atmospheric signal refraction and sea state effects (e.g., Chelton et al., 2001).”

REVIEWER COMMENT: In 136 Please provide the longitude of the south Indian Ocean track used throughout the study. (If a reader were to attempt to reproduce the method, this would provide a perfect case study to check their progress.)

RESPONSE: Done. We have identified the specific satellite pass and also highlighted the track on the revised Figure 2.

“An example of a detected high CKE envelope is shown in Figure 3, based on the average of CKE computed from T/P-Jason satellite pass 207 in the south Indian Ocean, starting at 64.3°S near the prime meridian and going to 41.2°S and 41°E longitude between 1993 and 2015.”

REVIEWER COMMENT: Ins 137-143 Was there a particular optimization technique used to hone in on 200 cm<sup>2</sup>s<sup>-2</sup>? Further, please comment on to what extent this method may ‘miss’ the parts of fronts and jets that lose energy and disappear or weaken? In other words, please comment on the limitations of this choice of threshold.

RESPONSE: The level was determined in an ad hoc procedure to find a level where all centers found CKE envelopes were in a location near Orsi et al. (1995) front positions. I also see I neglected to mention an additional criteria, that the envelope was larger than 100 km. This was done to minimize the impact of individual eddies. The revised text is (lines 357-371):

“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

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of the Orsi et al. (1995) values of the PF and the northern boundary to be  $5^{\circ}$  north of the SAF. Secondly, we used a lower-limit for CKE of  $200 \text{ cm}^2 \text{ s}^{-2}$  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.  $200 \text{ cm}^2 \text{ s}^{-2}$  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, especially 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).”

REVIEWER COMMENT: Ins 146-148 Are there any more plausible explanations for the varying number of local maxima other than ‘due to the instability of jets around the front’ and as such, I’m not sure if I understand the author’s meaning here - please explain or provide a relevant citation.

RESPONSE: I cannot think of another possibility, and others have shown the jets around the fronts are highly variable, as referenced earlier. In the revised text, I have added a new Figure (new Figure 4) showing the CKE for this pass for different 3-year averages. I have also added more discussion on this, referencing studies that have looked at these jet positions separately and making the argument why I only examine the whole envelope.

The relevant new text is between lines 381 and 397 of the revised text:

“The mean CKE profile pictured in Figure 3 has multiple local maxima, most likely

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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  $\text{cm}^2 \text{ s}^{-2}$ . 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.”

REVIEWER COMMENT: Ins 147-150 Has the author performed any analyses that would serve to ‘ground-truth’ the assumption that the ‘mean of the region of high CKE followed the front position’ (i.e., using data to confirm)? Or is this purely motivated by a previous study that has already shown this but is not included as a citation?

RESPONSE: I hope that the 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) demonstrate that these assumptions appear valid, in that the half-power points align with locations that other studies have detected fronts.

REVIEWER COMMENT: In 152 I’m not convinced that this method is identifying par-

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ticular fronts, or at least distinguishing them from one another, as suggested (but it's possible that lack of clarity is influencing my interpretation). The author details Figure 3 as if there's only one front represented by the two peaks contained within the 'one bump' (where CKE > 200 units). However, the two peaks shown in Figure 3 could in fact be two distinct fronts, the PF (at 52S?) and the SAF (at 49S?), given the large latitudinal differences between them. Perhaps finding the mid-point in this example is really just finding the energetic space (possibly filled with weaker fronts and/or jets as suggested) in between these two major fronts. If so, this study is more like Gille (2014) than suggested (in lns 150- 152): if close enough to one another, this study as presented often finds the latitude of mean CKE regardless of major front position (i.e., frontal and jet-like features, including the possibility of multiple fronts and jets of the ACC) and not the 'mean CKE around a particular front' as stated. Please comment (and elucidate the text).

RESPONSE: I hope the revised manuscript and new Figures 6 and 7 alleviate these concerns. As shown more clearly in the new Figure 4, only the more northerly "peak" in that CKE profile is consistent from 3-year period to 3-year period. The southerly peak is often replaced by multiple smaller peaks (i.e., 2008-2010), suggesting these are more jets than a front. I have looked at the fronts as defined by Orsi, Kim and Orsi, and Freeman and Lovenduski at this track (Figure R2, below). As you can see, the southern bump is not associated with the PF – it is the more northerly one. In fact, the Orsi front positions put the PF and SAF nearly on top of each other at this point, whereas Kim and Orsi suggest the SAF is farther north here, and find a PF position nearly identical to Freeman. I don't believe discussing the front positions for a single profile is necessary in light of the new Figures 6 and 7, and the discussion of them. But if the reviewer and editor feel this Figure is worthwhile, I can add it.

REVIEWER COMMENT: Also, over what time period does Figure 3 represent? Please provide temporal averaging information.

RESPONSE: Average of 1993-2015 for this illustrative purpose. The figure caption has

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been revised to reflect this.

REVIEWER COMMENT: Ins 170-172 Do these calculations require the same ‘simplifying assumptions’ that the author refers to (and therefore avoids) earlier in the text (Ins 112-114)?

RESPONSE: No. I have clarified this in the revised text where the crossover and along-track velocities are discussed together, instead of separately.

REVIEWER COMMENT: In 174 Please provide support for the author’s ‘reasonable assumption’ conclusion.

RESPONSE: The following text has been added to answer this on lines 251 to 299:

“This formulation assumes that the velocity field has not changed significantly between the times of the groundtracks. At high latitudes, the majority of crossovers ( $> 78\%$ ) have a time separation of less than 3 days. At  $40^\circ$ , the average propagation speed of an eddy is about  $3 \text{ cm s}^{-1}$  [Chelton et al., 2007], meaning the eddy would have only been displaced by 8 km at most over this period. At higher latitudes, this is even less. Considering the diameter of eddies at these latitudes are of order 100 km [Chelton et al., 2007], the movement is not large enough to cause a significant change in velocity at the point.”

REVIEWER COMMENT: Ins 195-196 Please elaborate on or discuss the science behind the (apparent) greater number of sites of enhanced CKE found along the SAF than the PF (e.g., is the SAF known to have more KE?).

RESPONSE: I don’t really have a good explanation for this, and choose not to speculate for the reason. These are the regions with enhanced CKE as found by the relatively conservative criteria we use. If I use lower CKE limits, I can find more points along the PF, but I also find many more between the PF and SACCF. Thus, I try to be conservative in the limits used in the algorithm.

REVIEWER COMMENT: Ins 197-198 What is meant by ‘changes since the hydro-

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graphic data used in that study were collected?’

RESPONSE: That phrase has been deleted as the discussion (lines 495-595) now focuses on the wide spread among different estimates.

REVIEWER COMMENT: Ins 198-199 Please provide the longitudinal location of this anomalous/southerly finding so that the reader does not have to search within the figure for it

RESPONSE: All locations of specific deviations discussed in the paper have now been added.

REVIEWER COMMENT: Here, the author presents the possibility that the method identified the SACCF to the south - please include discussion on the known high variability of the region (e.g., work by Ansorge et al., 2014)?

RESPONSE: I really don't think it is relevant to this discussion to cite that paper, considering there is only one point that might be in the SACCF. The revised paper does not explicitly highlight this point.

REVIEWER COMMENT: Ins 200-206 This paragraph is the perfect opportunity to provide much-needed quantitative information. For example, in addition to referencing REVIEWER COMMENT: Figure 5 to show variability, the author could provide relevant quantities that would give the reader an idea of the ‘spread’ about the average across the Southern Ocean. Mean, standard deviation, etc. This information would also help to contextualize the work

RESPONSE: This section has been extensively revised, with a new figure showing the variability (Figure 4) for each 3-year period. The discussion is on lines 381-392.

“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

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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 cm<sup>2</sup> s<sup>-2</sup>. A minimum is also not well defined in several of the shorter averaging periods (for example, 2008-2010).”

REVIEWER COMMENT: In 201 Re: ‘compared to the mean,’ please provide temporal information here.

RESPONSE: This information has been added to the figure caption.

REVIEWER COMMENT: In 203 Re: ‘suggesting jets.’ Why not fronts? Again, this goes back to the issue I have with the clarity of the study text. Is the author detecting fronts or jets or both with this method and if both, how are they making that distinction?

RESPONSE: I have revised this section and added new text to clarify my argument that we are detecting fronts as defined by the envelope of enhanced CKE driven by variable jets that surround the fronts (lines 393-397):

“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.”

REVIEWER COMMENT: In 205 While the author deems it ‘impossible’ to report on jet movements, the author could still provide the reader with some quantitative information

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here, such as specific comments on any temporal trends in these local maxima (e.g., their number, magnitude, etc.).

This has been done in the revised text lines 381-392.

“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  $200 \text{ cm}^2 \text{ s}^{-2}$ . A minimum is also not well defined in several of the shorter averaging periods (for example, 2008-2010).”

REVIEWER COMMENT: In 210 What is meant by ‘formal error?’

RESPONSE: Formal error is the error that comes out of the covariance matrix of ordinary least squares when it has not been scaled by the variance of the residuals. In the ordinary computation, this assumes the variance has been normalized to 1, so does not represent the true variance of the residuals. Hence one should scale this by the variance of the residuals to the fit (at a minimum) before estimating the standard error.

Since this is a standard definition, I don’t feel any more detail or references are required in the text.

REVIEWER COMMENT: In 212 Please write more mathematically. For example, instead of  $\sqrt{8/6}$ , ‘ $\sqrt{n/(n-2)}$ , where  $n$  is the degrees of freedom,’ or the like . . . .

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RESPONSE: This has been changed to:

“This was also scaled up to account for the degrees of freedom lost by estimating the trend by  $\sqrt{n/nEDOF}$ , where  $n = 8$ , and  $nEDOF = 6$ .”

REVIEWER COMMENT: In 217 Re: ‘which can be seen somewhat in Figure 5,’ please remove this kind of qualitative language.

RESPONSE: “Somewhat” has been removed, as the new figure (Figure 4), shows this more clearly.

REVIEWER COMMENT: In 223 Re: ‘there is no significant change,’ I feel this is too strong of language. Perhaps, ‘there is no statistically indistinguishable change.’ The use of ‘statistical’ when referring to significant change is required here.

RESPONSE: this has been changed to:

“For the majority of points (76.8%), there is no statistically significant change – no movement of the front is as likely as either a southward or northward shift due to the high variability in 3-year positions.”

REVIEWER COMMENT: Ins 227-234 No information is provided to the reader on the time periods analyzed in the referenced studies so as to make clear whether the author is making a direct comparison (also in reference to Ins 239-241).

RESPONSE: The time periods have been added.

REVIEWER COMMENT: Ins 235-241 The Discussion section would greatly benefit from comments on the science behind the reported/consistent northward and southward shifts over their 23-year time period.

RESPONSE: I have added a comment on this, which has been addressed in the Kim and Orsi (2014) study:

“Kim and Orsi (2014) suggest that the shift of the fronts in the Indian Ocean were

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not directly related to shifts in winds, but instead were caused by an expansion of the Indian subtropical gyre. They linked the shift in the southeastern Pacific to wind changes related to mainly the Southern Annular Mode in that region (Kim and Orsi, 2014).”

REVIEWER COMMENT: Ins 244-246 I agree. However, the clarity of the manuscript requires improvement.

RESPONSE: I hope the substantially revised manuscript is clearer now.

REVIEWER COMMENT: Ins 246-247 This is such an important statement but more content (or a rephrasing to really ‘hit it home’) is needed. What IS happening now, during this time of no shifts? Has there been any warming in the past 23 years? Any other changes in forcing? Please discuss more science.

RESPONSE: I have added a paragraph on what changes have been observed in the Southern Ocean in the last two decades, and I have also rearranged some of the text in those lines. The revised and new text is:

“Overall, this study supports the recent studies by Kim and Orsi (2014), Gille (2014), Freeman and Lovenduski (2016a), and Chapman (2017). All find that, while the frontal positions of the ACC are highly variable in time, there is no statistically significant shift in the fronts to the south on average. This study utilized a novel technique to reach this conclusion, which adds to the robustness of evidence that there has not been a shift in the frontal positions. Thus, while the fronts may eventually shift south in a warming climate, there is no strong evidence that it is happening at the moment.

Other studies have shown significant positive trends in the Southern Ocean that have been connected to the warming climate. These include changes in the ocean heat content in the upper ocean since the between the 1930s-1950s and 1990s (e.g., Böning et al., 2008; Gille, 2008), increases in the heat content of deep water between the 1990s and 2005 (Purkey and Johnson, 2010), and increases in eddy kinetic energy in

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the Indian and Pacific Oceans since 1993 (Hogg et al., 2015). Observational evidence of shifts in the winds, however, indicates that while there may be a slight southward shift in winds during the southern hemisphere summer, the overall yearly average shift is not significant (Swart and Fyfe, 2012). Thus, the growing consensus that fronts have not shifted to the south, on average, is consistent with no significant shift in the yearly averaged winds.”

REVIEWER COMMENT: In 256 I feel the word ‘flawed’ is too strong here. From what I can make of it all, the studies that use the contours have results that cannot be interpreted without the caveat of sea level rise, whereas this study and the other independent studies listed do not use methods influenced by sea level rise. Therefore, instead of ‘flawed’ I would suggest the use of ‘sensitive’ to sea level rise, as Gille (2014) uses.

RESPONSE: the text has been changed to read:

“...one has to conclude that the method of using dynamic topography contours to detect changes in front position is too sensitive to sea level rise to be useful for determining shifts in frontal positions, although it may prove useful for determining the mean position as Chapman (2017a) has argued.”

REVIEWER COMMENT: In 262 Heads up: missing grant number.

RESPONSE: Thank you. I left that out because at the time of submission, NOAA had not established the new grant number for this research. NOAA has established the funding, but as a subaward to a larger award handled through the University of Miami, so I have just revised to “a grant from NOAA”.

Please also note the supplement to this comment:

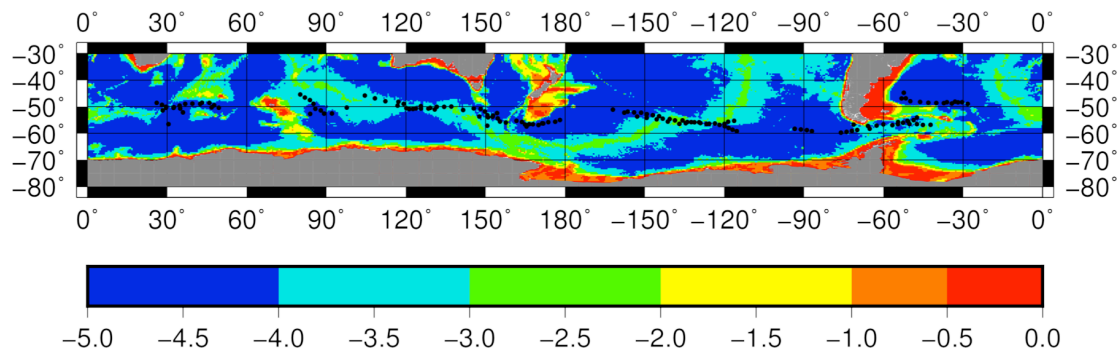
<https://www.ocean-sci-discuss.net/os-2017-57/os-2017-57-AC2-supplement.pdf>

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

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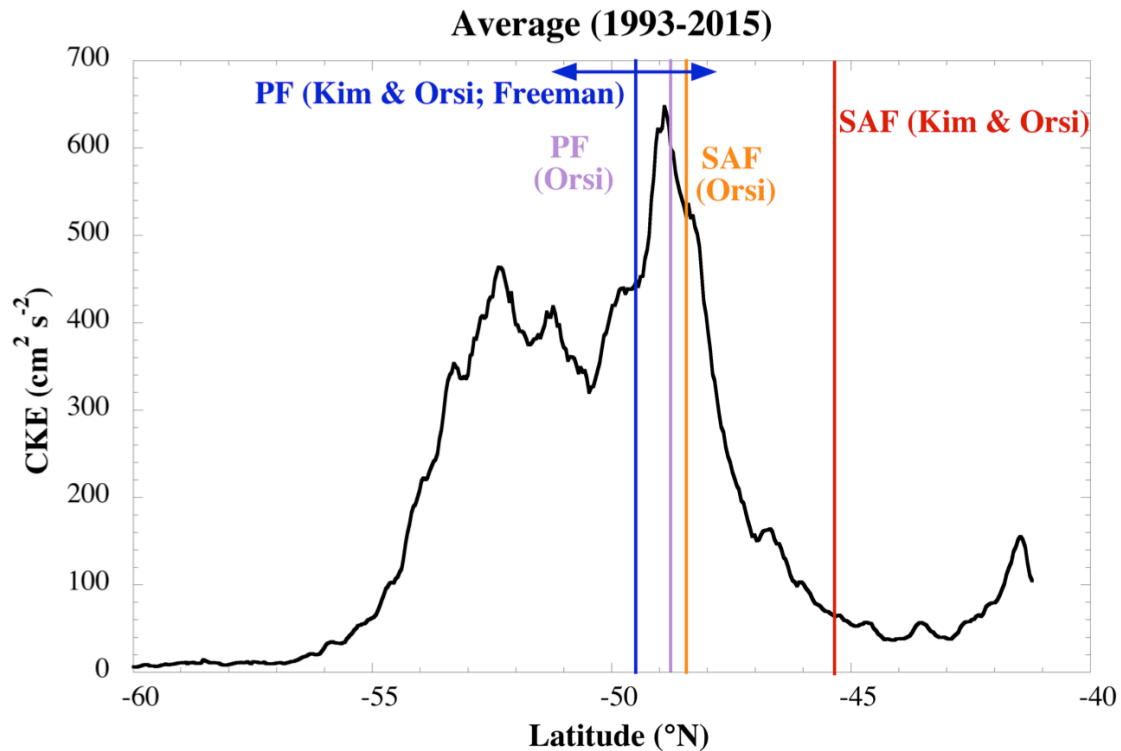
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**Fig. 1.** Positions of mean CKE half-power points along with ocean depth (in km). Bathymetry data from ETOPO5C.

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**Fig. 2.** CKE along pass 207 (same as Figure 3 in revised paper), but with front positions estimated by different groups.

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