Ocean Science
Discussions

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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

Anonymous Referee #2

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Chambers, Don P. Using kinetic energy measurements from altimetry to detect shifts in the positions of fronts in the Southern Ocean. Ocean Science Discussions

Summary

Chambers develops a new method for the identification of the major ACC fronts, the Subantarctic Front and Antarctic Polar Front; while the ACC is comprised of additional fronts, these two fronts are known to carry the majority of ACC transport and are therefore deemed most important in this study. The ultimate goal of the presented work is to determine whether these fronts have shifted north-south in recent decades (using altimetry from 1993-2015) and place the reported results in the context of recent frontal

studies, both observational and modelling.

Given the previously documented relationship between elevated Kinetic Energy and ACC fronts and jets, this study uses Cross Track Kinetic Energy (CKE) to infer their locations in the Southern Ocean. First, the author provides justification for the use of along-track data over a gridded/mapped product to infer locations of ACC fronts and jets to ensure greater spatial resolution and lesser noise. Second, the author shows that temporally-averaged CKE-derived front locations are generally found near the traditional Orsi et al. (1995) front locations, less some gaps where the Chambers methodology is unable to locate them. Finally, the author presents a trend analysis of the CKE-estimated front locations and finds that these two fronts have not shifted south on average, in contrast to previous studies using methods that are influenced by a sea level rise signal, albeit some north-south shifts in regions of the South Indian and South Pacific, consistent with recent studies (that avoid the sea level rise issue).

Comments

General

Overall, I found the study relevant for publication in and within the scope of Ocean Science. Chambers presents novel methodology in order to independently confirm results of previous ACC studies (but offers no insight on the longevity of the method for use in future studies). While the abstract text is the most clear section of the manuscript and does a nice job of summarizing the methods and results, I take issue with the rest of the manuscript text, which lacks clarity and fluent and consistent language and voice, and in turn limits overall transparency and reproducibility of the work. Also, I'm not convinced that the author provides enough analysis of the CKE-derived front locations in order to merit its publication as is. However, with additional attention and focus on clarity and content and inclusion of a few more quantitative results, I believe this study will positively contribute to the broader literature.

Specific

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.

The use of 'over three-year periods' throughout the text (e.g., ln 9, lns 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)?

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

<div align="center">C3</div>

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.

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?

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?

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.

While it is not a new idea that the methods of the previous studies tracking contours are affected by sea level rise, I like the inclusion of Figure 1 which nicely illustrates the described sensitivity.

In-line

lns 34-37
Please include relevant citations.

ln 71
I could not find the citation in the References section for Freeman et al. (2016).

ln 75

<div align="center">C4</div>

As 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...')!

lns 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.

ln 87
I think the inclusion of the word 'high' is a typo here.

lns 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.

ln 104
Please provide the citation(s) for (and/or why) these corrections (are recommended).

lns 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 (ln 100) but also that this model is then subtracted from that interpolated data (ln 105). Is it a model or model output?

ln 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.)

lns 137-143
Was there a particular optimization technique used to hone in on 200 cm^2 s^{-2}?

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.

lns 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.

lns 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?

ln 152
I'm not convinced that this method is identifying particular 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).

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

lns 170-172
Do these calculations require the same 'simplifying assumptions' that the author refers to (and therefore avoids) earlier in the text (lns 112-114)?

ln 174
Please provide support for the author's 'reasonable assumption' conclusion.

lns 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?).

lns 197-198
What is meant by 'changes since the hydrographic data used in that study were collected?'

lns 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.

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)?

lns 200-206
This paragraph is the perfect opportunity to provide much-needed quantitative information. For example, in addition to referencing 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.

ln 201
Re: 'compared to the mean,' please provide temporal information here.

ln 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?

ln 205
While the author deems it 'impossible' to report on jet movements, the author could still provide the reader with some quantitative information here, such as specific comments on any temporal trends in these local maxima (e.g., their number, magnitude, etc.).

ln 210
What is meant by 'formal error?'

ln 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 . . . .

ln 217
Re: 'which can be seen somewhat in Figure 5,' please remove this kind of qualitative language.

ln 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.

lns 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 lns 239-241).

lns 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.

lns 244-246

I agree. However, the clarity of the manuscript requires improvement.

lns 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.

ln 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.

ln 262

Heads up: missing grant number.

References

Ansorge, I. J., Jackson, J. M., Reid, K., Durgadoo, J. V., Swart, S., and Eberenz, S. Evidence of a southward eddy corridor in the south-west Indian ocean. *Deep-Sea Res. Pt. II*, doi:10.1016/j.dsr2.2014.05.012, 2014.