

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: 29 August 2017

Review of Chambers 2017, Ocean Science Discussions

Summary

In this paper the author uses along track altimetry from 1993 to 2015 to compute the geostrophic Cross Track Kinetic Energy (CKE) which is used, in turn, to infer the positions of jets and fronts in the Southern Ocean. The advantage of using the along-track altimetry, as opposed to the gridded products, is the finer spatial resolution and the fact that the optimal interpolation tends to attenuate the SSH gradients and hence the

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calculated EKE. The author asserts (citing evidence from previous work) that the locations of the Southern Ocean jets can be inferred from peaks in the CKE. The author shows (convincingly) that the CKE provides a reasonable approximation of at least the form of the total kinetic energy by comparing the CKE and TKE at cross-over points.

Having established that the CKE is a reasonable approximation of the TKE, the author then uses the CKE to (a) determine the position of Subantarctic Front (SAF) and Polar Front (PF) in the Southern ocean; and (b) determine any changes in the position of the 2 fronts by repeating this calculation over 3 year periods. The author finds time-mean frontal positions that are somewhat in agreement with preceding studies (such as the hydrographic fronts of Orsi et al. 1995) although significant differences exist in certain locations. The author also finds no significant trends in frontal locations, although there are some localised frontal shifts, which is broadly in agreement with a number of recent studies.

Overall, this study is interesting, well written and tackles a thorny problem that is currently a matter of debate. However, I have several problems with the paper as it is currently written. In particular, I am not convinced by the methodology used by the author to convert the CKE profiles into front locations, and the author either skims, or does not engage with entirely, a large amount of recent literature on this subject, and hence, an incomplete picture of the state-of-the-art is presented. My concerns will be detailed below.

The good news is that with a bit more attention to detail, and some demonstration of the methodology, this paper will become a valuable addition to the literature. I don't think this will require too much additional work.

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.

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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 cursory 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 temporally as a result of localised mixing of the PV. The structural rearrangement of the fronts complicates their interpretation 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.

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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.

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.

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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.

Particular comments

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).

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).

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

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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.

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)?

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.

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?

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