nature portfolio

Peer Review File

The open ocean kinetic energy cascade is strongest in late winter and spring

Web links to the author's journal account have been redacted from the decision letters as indicated to maintain confidentiality.

Decision letter and referee reports: first round

9th Aug 23

Dear Dr Schubert,

Your manuscript titled "Estimating the Oceanic Kinetic Energy Cascade from Along-Track Altimetry" has now been seen by 2 reviewers, and we include their comments at the end of this message. They find your work of interest, but some important points are raised. We are interested in the possibility of publishing your study in Communications Earth & Environment, but would like to consider your responses to these concerns and assess a revised manuscript before we make a final decision on publication.

We kindly ask that you demonstrate compelling new insights into the seasonality of oceanic spectral kinetic energy fluxes in observations and models, including demonstration that the methodology used to calculate spectral fluxes is robust. You may also consider simplifying your figures as is possible and/or appropriate to aid readers' interpretation.

We therefore invite you to revise and resubmit your manuscript, along with a point-by-point response that takes into account the points raised. Please highlight all changes in the manuscript text file.

We are committed to providing a fair and constructive peer-review process. Please don't hesitate to contact us if you wish to discuss the revision in more detail.

Please use the following link to submit your revised manuscript, point-by-point response to the referees' comments (which should be in a separate document to any cover letter), a tracked-changes version of the manuscript (as a PDF file) and the completed checklist:
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We hope to receive your revised paper within six weeks; please let us know if you aren't able to submit it within this time so that we can discuss how best to proceed. If we don't hear from you, and the revision process takes significantly longer, we may close your file. In this event, we will still be happy to reconsider your paper at a later date, as long as nothing similar has been accepted for publication at Communications Earth & Environment or published elsewhere in the meantime.

Please do not hesitate to contact us if you have any questions or would like to discuss these revisions further. We look forward to seeing the revised manuscript and thank you for the opportunity to review your work.

Best regards,

Rachael Rhodes
External Editor
Communications Earth & Environment

Heike Langerberg, PhD
Chief Editor
Communications Earth & Environment

EDITORIAL POLICIES AND FORMATTING

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Decision letter and referee reports: first round

Editorial Policy: Policy requirements (Download the link to your computer as a PDF.)

Furthermore, please align your manuscript with our format requirements, which are summarized on the following checklist:
Communications Earth & Environment formatting checklist

and also in our style and formatting guide Communications Earth & Environment formatting guide .

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Please refer to our data policies at http://www.nature.com/authors/policies/availability.html.

REVIEWER COMMENTS:

Reviewer #2 (Remarks to the Author):

Review of “Estimating the Oceanic Kinetic Energy Cascade from Along-Track Altimetry” by Schubert et al.

In this paper the authors use a high-resolution model to investigate spectral kinetic energy fluxes.

Decision letter and referee reports: first round

Spectral kinetic energy fluxes are an important descriptor of the scale-dependent energy budget of the ocean. As the authors point out, calculation of spectral fluxes requires information about velocities in two dimensions, and therefore along-track altimeter data—which can be used to estimate geostrophic velocities only in the cross-track direction—cannot be used to directly compute spectral fluxes. The authors use a high-resolution numerical model to show that fluxes computed using information in only one direction from an approximate method (see for instance Equation 1 and Figure 5) are close to those computed from fully two-dimensional information. They then compute spectral fluxes from along-track data and describe seasonal differences seen in the fluxes. The paper is interesting and well written. I recommend publication after consideration of the following points.

Many of the figures are quite technical. I am an expert in the field and I still had to study Figures 3-7 for a while to understand their main point. The average reader of this general-audience journal is going to have a hard time understanding the importance of these figures. I suspect that the main point the authors are trying to make—about seasonality of spectral fluxes—could be made with fewer figures. That might make it easier for a non-specialist to follow the arguments presented.

The authors display results from simulations with and without tides. The average reader would think from reading the paper that simulations with tidal and atmospheric forcing are done all of the time by ocean modelers, but that is not the case. A lot of hard work went into joint tidal/atmospheric forcing runs by a few groups, and then everyone else realized it was important to do add tidal forcing to oceanic general circulation models. In my opinion the authors should consider adding a sentence about the fact that adding tidal forcing to oceanic general circulation models is relatively new, and cite a few example papers from the groups that started doing this first.

Very minor points:

Lines 190-191: “which that takes this time” -- ? Unless I am missing something this is poor grammar. Please rewrite for clarity.

The concept of transition scale between balanced and unbalanced flows is not defined the first time that it is used.

Reviewer #3 (Remarks to the Author):

This manuscript describes a calculation, from along-track altimetry, of the kinetic energy transfer at various scales at midlatitudes and high latitudes, comparing it with high-resolution ocean models. It also describes the seasonality of this transfer, which is particularly important as according to one of the reigning theories of submesoscale energization, the scales (< 200km) discussed become energized by a mixed layer instability which peaks in teh winter months. The authors find results broadly consistent with this, and they find broad consistency between observations and models.

This would certainly be an important and high-impact result, and the paper is well written, but I have concerns about the methodology and an additional comments, all of which need to be seriously addressed before this paper is suitable for publication.

(1) The authors cite the classic work of Scott and Wang which measures kinetic energy transfer from the AVISO gridded product. Any discussion of this should also take into account the later paper by Arbic et al (J Phys. Oceanography, v. 43 (2013), p 283-300). This paper compares satellite results to models, and finds that the limited horizontal resolution of the AVISO dataset causes a misestimate of the kinetic energy transfer. To be fair, the result is that a *forward* cascade observed by Scott and Wang is a misrepresentation; and the current authors simply use

along-track data rather than the AVISO data, but this seems like a cautionary tale.

(2) On the other hand, the authors explicitly *force* their measure of the kinetic energy transfer to be always negative, by taking the absolute value of the last term in brackets in equation (1). This is assuming the answer, presumably because it is consistent with their models. However, this reviewer feels the dayta should be taken at face value and compared with models. Why do they not let the sign float and see what they get?

(3) The authors use an assumption to local isotropy to justify computing the kinetic energy transfer from the cross-track velocity. However, the larger signals are near major current systems. Particularly at the larger scales, is it clear that isotropy is a good assumption near large background currents?

(4) In general filtering along-track data at some length scale potentially allows for the mixing in of shorter-scale features. As an example, if one measures along-track wavenumber, a given wave could have a large and unmeasured cross-track component to the wavenumber meaning the *actual* wavenumber is much higher, and a measurement using along-track scale would be contaminated by much shorter distances. For power law measurements, the spectrum is usually red at these scales and this might not matter, but I note from Figure 1 that the transfer seems to be larger at shorter distances. This issue needs to be addressed squarely.3, no unceratinty range.

Dear reviewers,

thank you very much for your constructive feedback!
We provide below a point-by-point response to your remarks in blue. We received two reviews numbered #2 and #3 and keep this numbering.

Reviewer #2:

Review of "Estimating the Oceanic Kinetic Energy Cascade from Along-Track Altimetry" by Schubert et al.

In this paper the authors use a high-resolution model to investigate spectral kinetic energy fluxes. Spectral kinetic energy fluxes are an important descriptor of the scale-dependent energy budget of the ocean. As the authors point out, calculation of spectral fluxes requires information about velocities in two dimensions, and therefore along-track altimeter data—which can be used to estimate geostrophic velocities only in the cross-track direction—cannot be used to directly compute spectral fluxes. The authors use a high-resolution numerical model to show that fluxes computed using information in only one direction from an approximate method (see for instance Equation 1 and Figure 5) are close to those computed from fully two-dimensional information. They then compute spectral fluxes from along-track data and describe seasonal differences seen in the fluxes. The paper is interesting and well written. I recommend publication after consideration of the following points.

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Thank you for this comment. Our comparison of the cascade in the two simulations with and without tides and the estimate from the observations shows that the estimate is in general very close to the non-tidal run. This is what we expected, since a large part of the tidal signal in the observations has been removed by the phase-locked tidal-correction. Furthermore, we can explain differences from the expected patterns by the transition scale being too high at the respective scale, time, or location. We only included the comparison to the tidal run because there is a small tidal signal left in the observations that could not be corrected. We wanted to be sure and wanted to show that the estimation works similarly well with and without tides. In the revised document, we have now moved the tidal run completely to the supplementary information. This greatly simplifies the Figures and text in the main body. Moreover, we have removed the difference plots between the original and estimated flux as these differences can be seen by eye in the Figure pair. Finally, we have also removed the comparison of the original and estimated seasonal cycle, as the corresponding plot for the non-tidal run has the same sign for very large parts of the domain and the main additional information left, that there is a slight reduction in the amplitude of the seasonal cycle in the estimate, can also be seen in the Hovmoeller plot (bottom panels in the new Figure 4c and 4d).

The authors display results from simulations with and without tides. The average reader would think from reading the paper that simulations with tidal and atmospheric forcing are done all of the time by ocean modelers, but that is not the case. A lot of hard work went into joint tidal/atmospheric forcing runs by a few groups, and then everyone else realized it was important to do add tidal forcing to oceanic general circulation models. In my opinion the authors should consider adding a sentence about the fact that adding tidal forcing to oceanic general circulation models is relatively new, and cite a few example papers from the groups that started doing this first.

We agree and have added the following sentence to the part on the tidal impact in the supplementary information: “Including tides in ocean general circulation models is a rather new development (Arbic et al. 2010, Mueller et al. 2012).”

Very minor points:

Lines 190-191: “which that takes this time” -- ? Unless I am missing something this is poor grammar. Please rewrite for clarity.

Thanks, we removed “that” from this sentence.

The concept of transition scale between balanced and unbalanced flows is not defined the first time that it is used.

Thanks, we shifted the introduction of the transition scale to the beginning of the result section and the respective discussion on Figure 2 to the new section 2.2.

Reviewer #3:

This manuscript describes a calculation, from along-track altimetry, of the kinetic energy transfer at various scales at midlatitudes and high latitudes, comparing it with high-resolution ocean models. It also describes the seasonality of this transfer, which is particularly important as according to one of the reigning theories of submesoscale energization, the scales (< 200km) discussed become energized by a mixed layer instability which peaks in teh winter months. The authors find results broadly consistent with this, and they find broad consistency between observations and models.

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Thank you for this comment. We absolutely agree, added the reference to Arbic et al. (2013), and added the following sentences to the manuscript: “The results showed an inverse cascade (from smaller to larger scales) at scales larger than about 75 km and a forward cascade (from larger to smaller scales) at smaller scales. While the forward cascade was later found to be an artifact of the filtering and interpolation technique onto the regular grid, the inverse cascade at larger scales was found to be robust to filtering (Arbic et al. 2013, Qiu et al. 2014).”.

(2) On the other hand, the authors explicitly *force* their measure of the kinetic energy transfer to be always negative, by taking the absolute value of the last term in brackets in equation (1). This is

assuming the answer, presumably because it is consistent with their models. However, this reviewer feels the dayta should be taken at face value and compared with models. Why do they not let the sign float and see what they get?

Thank you for this remark. You are right, the estimated flux is globally forced to be inverse. When we started working on the study, we first noted that the Leonard stresses for u*u and v*v (below named Tuu and Tvv) are globally positive and that it is the sign of the respective horizontal derivative multiplied by these stresses that determines the direction of the cascade in the respective first and third terms on the right hand side in equation (2). However, this is not the case for the second term. Furthermore, in the case of the geostrophic flux, the horizontal non-divergence of the geostrophic flow (u_x=-v_y) allows to rewrite the sum of the first and the third term as (Tuu-Tvv)u_x, where again, the term in the brackets is associated with changing signs. Thus, for the flux, both the Leonard stresses and the horizontal derivatives impact the resulting direction of the cascade. This, together with the fact that there are many cancellations between the terms that are not identifiable from along-track data, indicated that it was not possible to estimate the sign of the flux correctly. We tried, but found absolutely no way to estimate the sign of the flux reasonably well from along-track data. For example using equation (1) without taking the absolute value of the along-track derivative and without the minus sign produces estimated fluxes that bear no resemblance to the original fluxes in our simulation, or to previously published results from simulations, or at very large scales to AVISO-derived fluxes.

However, away from the shelf regions (and when L>T), both our simulations very rarely show forward geostrophic fluxes, when averaged over 5x5 degree subdomains. This is in agreement with recent theoretical studies that have shown by applying Helmholtz decomposition prior to coarse-grained flux computation, that the balanced part of the flow is usually associated with inverse fluxes. Moreover, at larger scales, this is in agreement with studies based on AVISO. Finally, we show that the time- and area-averaged cascade is indeed in very good agreement with the original one when we estimate it with equation (1), and thus that our estimation works well, although in the case of the original flux the sign has been left floating.

In the revised manuscript, we have added further justifications for equation (1) to chapter 3.5. In the same chapter, we have extended the discussion of what the prescribed sign implies for the interpretation of the estimated fluxes.

(3) The authors use an assumption to local isotropy to justify computing the kinetic energy transfer from the cross-track velocity. However, the larger signals are near major current systems. Particularly at the larger scales, is it clear that isotropy is a good assumption near large background currents?

Thank you for that comment. We agree that whenever information from a two-dimensional field is cut along one-dimension, information about the other direction is missing. This is indeed the case in our analysis. However, we partially include information from the other direction by using both descending and ascending tracks of the satellite, which are associated with a large angle between them (Fig. 1a). By averaging over 5x5 degree subdomains, we ensure that information from several descending and ascending tracks is included. This technique contributes greatly to the success of the estimation. However, it is indeed the region of the quasi-zonal core Gulf Stream extension, where the estimation shows the largest deviations from the original flux. This can be explained by the extremely non-isotropic time-mean flow in this region. We have added the following sentences to section 3.5:

“This technique is associated with the inclusion of information from both horizontal directions, as the angle between ascending and descending tracks is large (Fig. 1a), and thus reduces the effect of

the isotropy assumption that is necessarily made when one-dimensional data is cut from a two-dimensional field and then used to make a statement about that field." and
"The former may be due to the extreme non-isotropy of the core Gulf Stream extension and indicates that the fluxes are overestimated in similar energetic non-isotropic flows, such as the western Kuroshio or the Agulhas retroflection.".

(4) In general filtering along-track data at some length scale potentially allows for the mixing in of shorter-scale features. As an example, if one measures along-track wavenumber, a given wave could have a large and unmeasured cross-track component to the wavenumber meaning the *actual* wavenumber is much higher, and a measurement using along-track scale would be contaminated by much shorter distances. For power law measurements, the spectrum is usually red at these scales and this might not matter, but I note from Figure 1 that the transfer seems to be larger at shorter distances. This issue needs to be addressed squarely.

Thank you for this comment. We agree and would add that a feature can also appear smaller based on the track information. For example, if a track cuts through a circular eddy but does not cross its center. The fact that the estimate from one-dimensional data agrees with that of the two-dimensional original shows that the estimation procedure corrects for this issue. We hypothesize that, on average, the erroneous imprint of smaller scale features dominates at larger scales and that it is the decreasing estimation coefficient with increasing scale that compensates for this effect, since this means that the pre-estimate (eq. (1) without C) is reduced more at larger scales, where shorter scale contributions erroneously imprint.

We have added the following to section 3.5:

"The horizontal scale of a SSH feature in the two-dimensional field may differ from that identified from the SSH along a track that cuts through the same feature. For example, if waves do not propagate in the direction of the track, they will appear to be associated with a larger wavelength along the track. Or if a track cuts through a circular eddy and does not cross its center, the eddy will appear to be smaller from the track information. The fact that the flux estimated from the one-dimensional SSH data agrees with that from the two-dimensional original shows that the estimation procedure corrects for this issue. We hypothesize that, on average, shorter scale features erroneously imprint at larger scales, and that the decreasing C with increasing scale corrects for this by reducing the pre-estimate (eq. (1) without C) more at larger scales."

Decision letter and referee reports: second round

2nd Nov 23

Dear Dr Schubert,

Your revised manuscript titled "Estimating the Oceanic Kinetic Energy Cascade from Along-Track Altimetry" has now been seen by our reviewers, whose comments appear below. In light of their advice we are delighted to say that we are happy, in principle, to publish a suitably revised version in Communications Earth & Environment under the open access CC BY license (Creative Commons Attribution v4.0 International License).

We therefore invite you to revise your paper one last time to address the remaining concerns of our reviewers. At the same time we ask that you edit your manuscript to comply with our format requirements and to maximise the accessibility and therefore the impact of your work.

Please note that it may still be possible for your paper to be published before the end of 2023, but in order to do this we will need you to address these points as quickly as possible so that we can move forward with your paper.

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Decision letter and referee reports: second round

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We hope to hear from you within two weeks; please let us know if you need more time.

Best regards,

Rachael Rhodes
Editorial Board Member
Communications Earth & Environment

Heike Langenberg, PhD
Chief Editor
Communications Earth & Environment

On Twitter: @CommsEarth

REVIEWERS' COMMENTS:

Reviewer #3 (Remarks to the Author):

To this reviewer's eye, the authors have addressed the issues the reviewers brought up and have ensured that their paper is transparent about the open questions which remain. I think these changes also made the paper clearer. It's a good paper, and I support its publication in this journal..

Author Responses: Second Round

Dear reviewers,

thank you for your time to go through our manuscript a second time.
We thank Reviewer #3 for the positive feedback (see below).

Reviewer #3:

To this reviewer's eye, the authors have addressed the issues the reviewers brought up and have ensured that their paper is transparent about the open questions which remain. I think these changes also made the paper clearer. It's a good paper, and I support its publication in this journal.