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Interactive comment on "Surface currents and upwelling in Kerguelen Plateau regions" *by* M. Zhou et al.

Anonymous Referee #2

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Review of "Surface currents and upwelling in Kerguelen Plateau regions" by M. Zhou et al. for publication in Biogeosciences.

I was very interested to see the analysis of the drifter data, but was disappointed that many opportunities for cross-comparison with other data sets were overlooked. For example, the stream function fitting produces a sea surface height field that could be compared with altimetric sea surface height anomalies. Ekman velocities are determined as a residual from the drifter velocity and the streamfunction based geostrophic velocities. These could be compared with Ekman drift calculated from scatterometer wind fields. Alternatively, the wind stress curl field estimated from Equation 11 could have been compared with the wind stress field shown in Fig. 11. Or again, the vertical velocities calculated could be compared with Ekman pumping from scatterometer

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winds.

The mixing estimates are possibly not reliable because of problems with the choice of method used and the short time series from the drifters. The other reviewer describes the problems in detail and I believe he/she is quite right. The authors could build some confidence in their estimates by applying the “state of the art” methods described in LaCasce et al 2014, and the other references provided by the other reviewer. The error bars may still be too large. Very little attention is given to the errors associated with any of the fields presented, and this should be rectified before the manuscript is published. Also, there are other global estimates of mixing that the authors should refer to in order to show consistency with their results (e.g. Sallee et al. 2011).

Even though the mixing estimates may not be robust, there is a lot of other information that is valuable from these data. In particular, the separation of drifter velocities into geostrophic and Ekman components is very clever. And a lot more can be made in the discussion about the Ekman currents, which are so hard to measure. Backing out the wind stress curl also provides a useful cross-comparison with scatterometer products. I think there is value in discussing this. And of course the vertical velocity field is important for interpreting the impact of the Kerguelen Plateau on the productivity of the region, as well as understanding the physics of the meandering flows.

There are many instances where the grammar is a bit confused and the point is hard to see. This should be corrected.

One final point is that the Discussion and conclusions seem superficial and rushed, and I think could be improved with more consideration of the relationships between the different fields calculated from the drifter data, and available from other remotely sensed data.

I believe the manuscript may be suitable for publication after major revision, which should include addressing the specific comments listed below.

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Specific comments:

Line 11 and elsewhere – “nature iron fertilization” should be “natural iron fertilization”

Page 6841

In Table 1 it would be useful to see the length of record from each drifter in the study area. You say the positions are interpolated to every hour, but how often do they actually sample?

Page 6850

1)Sentence beginning “Time-averaged ..” is not clear.

2)Line 14. You say zero crossing is independent of filter window. Fig 3 says this is not true. The low-passed curves have zero crossings near 10-12 days. I guess this is what the red-dashed lines are marking.

3)Line 25. Kappa-infinity is an unnecessary complication. You don’t actually use it, it’s a theoretical concept that makes the paper more complicated than it needs to be. You only need to talk about the estimated Kappa. I suggest you delete Eq. 3 and any reference to Kappa-inf.

Page 6851

The Lagrangian timescale of 4 days lies in the negative lobe of the autocorrelation function in Fig. 3. This is not a good choice of timescale to integrate out to. Klocker et al. 2012 discuss the need to integrate beyond the negative lobe.

Page 6853

1)You assume a 50m mixed layer depth. This seems quite arbitrary when you have access to CTD data from your voyage. You should calculate from data along the ship track and then compare it with global mixed layer atlases to see how it varies beyond the ship track throughout the rest of your region.

2) Eq 13 should actually be integrated over the Ekman layer, which may not correspond to the mixed layer. You should state this explicitly. If you have estimated the geostrophic velocity reliably then the Ekman velocity should go to zero below the Ekman depth and then a deeper integration to the mixed layer depth won't affect the estimate of the vertical velocity. However, if there is residual geostrophic velocity in your Ekman velocities, then vertical velocity will be affected. Some discussion of the sensitivity of the w calculation from (13) should be included.

Page 6854

You use Eq 2 to calculate diffusivity. As pointed out by the other reviewer, LaCasce et al 2014, discuss different methods and show that your method produces very noisy results. You should try the other methods and see whether you get any convergence on estimates of the diffusivity.

Page 6855

1) You say you can't separate surface gradients and wind stress, but you actually do this through the steps you follow. By fitting the geostrophic streamfunction you isolate the Ekman velocities and then find the wind stress curl.

2) Change "called as" to "referred to"

3) change "in the eastern slope" to "along the eastern slope"

4) Does the streamfunction fitting procedure produce an error estimate? It should, and the errors should be discussed.

5) near line 15, you talk about a divergent-convergent pair. I don't see this. Of all the features apparent in Fig. 9 this region along 48.5S shows the least variation. Maybe the positions are wrong?

6) What are the criteria for choosing the 2 circled regions. Is it based on water depth? I don't see the value in calculating an average over these regions which contain fluc-

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tuating positive and negative values that will average out to a small number. Looking at figure 9, there are far more interesting things to discuss. There is a beautiful correspondence between the flow field and the estimated vertical velocities that appear to be linked to the phase of the meandering. Lindstrom and Watts discuss such a link for the Gulf Stream, and Tracey et al. for the Subantarctic Front. This phase locking is a distinct feature of your results and should be described.

7) You say “The mean currents are weak. . .”. Are you referring to the mean currents coming off the shelf, or across the whole region?

8) You describe the non-divergent/geostrophic currents in detail but you don’t mention the size of the residual Ekman currents at all. I think it would be interesting to know how big they are in relation to the geostrophic currents and what the spatial pattern looks like. Does it reflect the wind stress field from scatterometer? Your method provides a consistency check between wind stress curl computed from Eq 11 and that from scatterometer. This is worth investigating.

9) The domes and depressions in isopycnals don’t necessarily correspond to upwelling and downwelling. Fig 10 needs some more work if you want to identify such a relationship. See my comments on Fig. 10.

Page 6856

Line 5 – As mentioned above, compare wind stress curl from Quikscat with your estimate.

page 6857

1) Line 14 – You infer a slope current of 30 cm/s from mass balance, but you don’t say how fast it is from the SVP data.

2) In Eq 14, is the wind stress curl from quikscat or from Eq 11?

3) I assume the meridional transport of 5.6 m2/s is calculated from Eq 14. How does

this compare with transport estimated by taking meridional velocity from the drifters and multiplying by an approximate area of the shelf/slope over which the current flows?

4) Sentence beginning “Because . . .” at line 24 is not clear.

Page 6859

1) You say that the mean transport and eddy mixing are of similar magnitude. It would be helpful if you discussed the implications of this, compared with the case where one or other dominates.

2) You say Rossby numbers are generally small. How small? How did you calculate them? This isn’t mentioned in the methods section.

Page 6860

Line 8. Doesn’t the 1.7Sv include mixed layer transport?

Page 6861

Line 7. Line beginning “Considering that . . .” needs a reference.

Table 1

Add column indicating length of each record in study region

Fig. 1

It would be useful to see the location of the CTD stations from Fig 9 on this figure as well. Are there other CTD data? Are there any shipboard velocity estimates to compare with?

Fig. 3

What are the red dashed lines?

Fig. 6

It would be good to add more value to this figure, otherwise you could just add the vectors to the background of Figs 4 and 5. You could include altimetric height at deployment, from which could you show the position of branches of the ACC from Sokolov and Rintoul's SSH criteria.

Fig 7.

Use the same colour bar in each panel.

Fig. 9

I'm not sure why you say "false colors". I would just say colors.

Fig 10.

The association between up and down welling with isopycnal fluctuations is very sketchy. You could easily add a lot of value to this figure and do a much more convincing job at making the association. For a start, delete the bottom panel and overlay density contours on the temperature and salinity shading. It's not clear what the dotted lines are in each panel at the moment. What are the vertical lines in the top panel? For vertical velocity, mark the transition from down/up welling objectively by calculating the position of zero crossings in w along the ship track through Fig. 9. I would also add another panel with a line plot of w along the ship track. It would also be useful to see a line plot of horizontal speed or some representation of the horizontal velocity as an additional panel in this figure. And possibly even SSH anomaly from your streamfunction, and from altimetry.

Fig 11.

Make the axes the same as for Fig 9 so one can compare directly. Add topographic contours so the shelf break can be identified. Add CTD locations.

References.

Klocker, A., R. Ferrari and J. H. LaCasce. 2012b. Estimating suppression of eddy

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mixing by mean ïňĆows. Journal of Physical Oceanography. Vol. 42. pp. 1566-1576.

LaCasce, J.H., R. Ferrari, R. Tulloch, D. Balwada and K. Speer, 2014. Float-derived isopycnal diffusivities in the DIMES experiment. J. Phys. Oceanogr. (44), 764-780.

Lindstrom, S. S. & Watts, D. R. Vertical motion in the Gulf Stream near 68$ˆo$∼W J. Phys. Oceanogr., 1994, 24, 2321-2333

Sallee, J.-B., K.Speer and S. Rintoul, 2011. Mean-ïňĆow and topographic control on surface eddy-mixing in the Southern Ocean. Journal of Marine Research, 69, 753–777, 2011

Sokolov, S. & Rintoul, S. R. Circumpolar structure and distribution of the Antarctic Circumpolar Current fronts: 1. Mean circumpolar paths J. Geophys. Res., 2009, 114

Sokolov, S. & Rintoul, S. R. Circumpolar structure and distribution of the Antarctic Circumpolar Current fronts: 2. Variability and relationship to sea surface height J. Geophys. Res., 2009, 114

Tracey, K. L.; Watts, D. R.; Meinen, C. S. & Luther, D. S. Synoptic maps of temperature and velocity within the Subantarctic Front south of Australia J. Geophys. Res., 2006, 111

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