Response to editor for Biogeosciences Discuss., 11, C2753-C2755, 2014:

Overview

Your revised paper clearly demonstrates the correlations between winds, SST, and Ekman upwelling in the Kerguelen region, and now includes appropriately expanded information as requested by both reviewers on the partial correlation and filtering techniques applied to the data to examine these links. However, the further links to the distributions of satellite chlorophyll, and the mechanisms that are put forward to underpin them (upward supply of chlorophyll from a deep maximum and/or iron to enhance chlorophyll production) remain relatively speculative and undeveloped. No quantitative examination of the feasibility of these links was explored using in-situ data from the KEOPS campaigns, and while this is beyond the scope of this paper, it does mean that there is a need to be more careful with citations of other papers on these issues (as requested by Reviewer 1), and with ensuring that appropriate caveats are prominently displayed in the final manuscript. Needed changes to address these concerns are itemized below.

Thank you for this supportive evaluation and for the constructive suggestions.

1. The revised paper cites works by Charrasin et al. 2010, Guinet et al. 2013, Carranza and Gille, submitted, and Carranza et al., submitted as evidence that there are strong subsurface chlorophyll maxima in the Southern Ocean and that their vertical mixing could explain links between wind fields and chlorophyll fields near the Kerguelen plateau. However, these first two papers do not actually demonstrate the presence of subsurface chlorophyll maxima. Rather they show that fluorescence profiles which exhibit subsurface maxima derive from aliasing of the fluorescence-chlorophyll relationship by fluorescence quenching. When corrected for this effect, the subject on which these papers focus, the occurrence of chlorophyll subsurface maxima is very infrequent (e.g. $<$9% of profiles in the large data set examined by Guinet et al., 2013, with the maximum relatively small $\sim$30% and at most 200% of surface values, and thus dispersion by vertical mixing is also unlikely to be able to explain the multi-fold surface chlorophyll variations). Thus the statement in the revised manuscript (on page 5, line 5) that subsurface chlorophyll maxima are ubiquitous (and associated inference that these are likely to contribute to spatial variations in satellite surface chlorophyll) appears unjustified, and this portion of the paper needs to be corrected. If you feel the submitted work from Carranza and Gille, and Carranza et al., (or other work) does demonstrate a subsurface chlorophyll maximum in the Kerguelen region, in opposition to this published work, then this difference needs to be acknowledged and discussed. Attention to this issue is also needed at page 12 line 15, and page 14 line 24.

This is a good point. The Carranza et al (2014) analysis is based on night-time only profiles to avoid quenching effects, so we think the results are robust, but the results are new and still under review. We agree that there is no need to confuse this discussion by injecting new results that are still being finalized. We have removed the sentence referencing the sub-surface Chl-a maximum from the introduction. On p. 12, we have rewritten the text to remove reference to the Carranza et al study and to focus primarily on upwelling of Fe (while acknowledging that Chl-a upwelling may also play a role.)

> KEOPS-1 observations indicate the presence of a sub-surface iron maximum at the depth of the shelf around 500 m (Blain et al., 2008). We suggest that the persistent wind-induced upwelling may bring sub-surface iron-rich or may help retain water that

> is high in Chl-a at the ocean surface, particularly in the region of the shallower plateau to the northeast of Kerguelen. Lateral advection may then carry this iron-rich (and possibly Chl-a-rich) surface layer into the deeper regions downstream, along the northern flank of the Polar Front or within the surface Ekman layer.

On p. 14, we have removed discussion of the sub-surface Chl-a maximum. The text now states:

> The magnitude of this wind-induced upwelling remains weak, with wind-stress curl of 2 to 5 $\times 10^{-7}$ N m^{-3} implying vertical Ekman pumping velocities w around 2-4$\times 10^{-6}$ m s^{-1} giving a change of the thermocline of only 4-11 m month^{-1}, which is too slow to bring iron-enriched water to the surface either from the shallow 200-m plateau or from the sub-surface iron maximum that occurs near 500 m depth (Blain et al., 2008; van der Merwe et al., 2014) but could help bring water to the surface from the base of the spring or summer mixed layer or could help to retain water high in Chl-a at the ocean surface. Individual storm events could permit stronger short-duration upwelling events. The specific mechanisms governing this will require further investigation.

2. The possibility of contributions to the control of surface chlorophyll levels by upwelled iron supply needs more prominent display of its associated caveats. At present, the paper does acknowledge that magnitude of upwelling is too small to deliver iron to the mixed layer, at page 14 line 20. It then suggests in the Conclusions at page 15 line 5, without any development, that the apparent link between wind distributions and chlorophyll levels must therefore derive from interactions of the wind induced mixing and upwelling with other processes. This leaves the overall connection rather under-developed and uncertain, and thus the abstract needs to acknowledge that the mechanism for the links is not yet understood, so that this state of play is very clear to all readers. For example, the abstract sentence:
"Chl-a values tend to be more elevated in places where wind-stress curl induces Ekman upwelling than in locations of downwelling."
could be expanded to read:
"Chl-a values tend to be more elevated in places where wind-stress curl induces Ekman upwelling than in locations of downwelling, although the estimated upwelling rates are too small for this relationship to derive from direct effects on upward iron supply, and thus other processes, which remain to be determined, must also be involved in the establishment of these correlations."
The suggested modification has been made to the abstract. A similar modification has been made in the Summary and Discussion section. As indicated in the final quoted passage in response to (1) above, the text now notes, "The specific mechanisms governing this will require further investigation."

Additional details:
Page 4 line 15: citations that you could consider adding to the Sanial reference to justify this statement about iron supply from Kerguelen include:

> *Bucciarelli, E., Blain, S., Treguer, P., 2001. Iron and manganese in the wake of the Kerguelen islands (Southern Ocean). Marine Chemistry 73, 21-36.*
> *Chever, F., Sarthou, G., Bucciarelli, E., Blain, S., Bowie, A.R., 2010. An iron budget*

> *during the natural iron fertilization experiment KEOPS (Kerguelen Island, Southern Ocean). Biogeosciences 7, 455-468.*

(Notably these papers actually measured Fe gradients away from the islands, whereas the Sanial paper is limited to Ra isotope distributions with hypothesized links to Fe delivery.)
Thanks for suggesting these papers, which are both very interesting. The citations have been added as suggested.

Page 10, line 20: citation of the Gaube et al., 2013 paper is rather poorly done. This paper did not address the role of Fe, rather it focused on macronutrients, and also acknowledged that vertical mixing of subsurface chlorophyll maxima may have been the dominant effect (for this last point see the paragraph in their paper about the profiling floats; this view is also supported by subsequent work in review including co-authors from the Gaube et al paper). So, a more nuanced citation is needed, e.g. that the mesoscale link has been shown for the Indian Ocean where macronutrient supply and vertical mixing seem to be the important processes, or something along those lines.
This sentence has been rewritten to avoid misrepresenting the Gaube et al. findings:

> As Gaube et al. (2013) demonstrated in the eastern Indian Ocean, eddy-scale processes can modulate Chl-a in a manner consistent with eddy-induced Ekman upwelling; assuming similar processes occur in the KEOPS-2 region, they could be responsible for exchanging macronutrients, iron, and even Chl-a, between subsurface and surface layers.

Page 13 line 3 and possibly elsewhere , the Zhou et al., this volume, citation should be augmented by the already published Park et al., 2014 paper:
Polar Front around the Kerguelen Islands: An up-to-date determination and associated circulation of surface/subsurface waters: Young-Hyang Park1,, Isabelle Durand1,Elodie Kestenare2, Gilles Rougier3, Meng Zhou4, Francesco d'Ovidio5, Cédric Cotté1and Jae-Hak Lee6, DOI: 10.1002/2014JC010061*
This is an excellent suggestion. We read the Park et al manuscript while we were doing revisions, but because it hadn't been sent to Biogeosciences, we weren't sure how quickly it would be published and opted for a conservative approach to citing it. Now that the paper is in press, we've added the reference.

Figure 1 caption:
the phrase:
(f) for October-November 2011, as in (f).
should read:
(f) for October-November 2011, as in (c).
Corrected.

References

Blain, S., Sarthou, G., and Laan, P.: Distribution of dissolved iron during the natural iron-fertilization experiment KEOPS (Kerguelen Plateau, Southern Ocean), Deep-Sea Res. II, 55, 594–605, 2008.

Gaube, P., Chelton, D. B., Strutton, P. G., , and Behrenfeld, M. J.: Satellite observations of chlorophyll, phytoplankton biomass, and Ekman pumping in nonlinear mesoscale eddies, J. Geophys. Res. - Oceans, 118, 6349–6370, 2013.

van der Merwe, P., Bowie, A. R., Quéroué, F., Armand, L., Blain, S., Chever, F., Davies, D., Dehairs, F., Planchon, F., Sarthou, G., Townsend, A. T., and Trull, T.: Sourcing the iron in the naturally-fertilised bloom around the Kerguelen Plateau: particulate trace metal dynamics, Biogeosciences, in preparation, 2014.