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Interactive comment on "The Fate of a Southwest Pacific Bloom: Gauging the impact of submesoscale vs. mesoscale circulation on biological gradients in the subtropics" *by* Alain de Verneil et al.

Anonymous Referee #1

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The topic of how primary production may be sustained by mesoscale and submesoscale circulations, particularly in the low nutrient subtropical gyres, is one of considerable broad interest. Yet it is an area where observations remain relatively few, particularly compared to modelling studies despite several of the latter indicating a major role for such physical processes. Motivated by encountering an unexpected phytoplankton bloom in such a region, the authors use a combination of in situ data and remote sensing to explore the potential role of the physical circulation. Their conclusion - that vertical nutrient flux from submesoscale processes is an unlikely control and that it is largely a 2D phenomenon with the mesoscale field advecting water against the

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Discussion paper

mean flow – is one that seems best supported by the data. This is a very interesting result, particularly with the corollary that the mesoscale flow may be drawing iron away from the islands to trigger the bloom. However, there are a few aspects of the paper that I think need addressing before the manuscript can be published.

The paper essentially considers two possibilities: submesoscale vertical movement and mesoscale horizontal movement. The reader needs to have faith that both options have been thoroughly tested before the conclusion has been reached.

For the submesoscale, the largely horizontal structure of density and discontinuities between surface and deep Chl are pretty convincing. I find the Richardson number argument less so given that the majority of submesoscale motions are confined in the surface boundary layer which is poorly sampled judging by Fig. 5. The authors should acknowledge this. For clarity, Fig 5 should also have its colour scale changed so that the blue-yellow transition is centred on zero – the value of interest. Additionally the MVP data being unfiltered will have internal waves (as acknowledged) which may misleadingly increase the lateral buoyancy gradient used for Ri_g. As an aside, might the striped nature of Fig. 5 be due to internal waves?

For the mesoscale, the argument largely rests on advection using the altimetry derived flows and the Lyapunov exponents. Presumably if the sky was clear enough for such Chl images then SST is also available. This should also be shown in Fig. 6 (or in an equivalent new figure) as it gives greater faith in the analysis. SST does not have the complication of being a reactive tracer. i.e. does the SST field show the same/different matches with the FSLEs? Are theye consistent with the hypothesis? I find the particle tracking back to 25 December uncompelling, particularly given the very interesting idea in the Discussion of island iron being a factor. At first glance, the most striking feature of Fig 6 is a high chl patch on the east side of the island which seems about to be drawn away by the mesoscale flow. This is the basis of the authors' iron suggestion and they need to do it more justice. It would be interesting to see the results of seeding particles over the patch of high Chl next to the island on Jan 13 and running this forward to see how these waters correspond to those found later in the patch hosting LDB. If there are more satellite images available between Jan 13 and 31 in particular these should also be shown. On a more technical note the authors should discuss the consequences of using 30d integrations for FSLEs given the extent to which the 2d circulation is apparently changing in Fig. 6. How does the match up to tracers change if shorter integrations are used?

I additionally have a number of more minor comments/suggestions:

- Given the issue with the salinities from the CTD I think there needs to be a T/S diagram using MVP data in Supplementary material to reassure the reader that density comparisons between Fig. 2 and 3 are reliable

- The location of the CTDs taken at LDB needs to be indicated on Fig. 2 and 6

- Fig. 3 looks like there might be an issue with quenching as the increase and decrease in surface Chl up to Mar 18 seem to have the expected daily cycle.

- This isn't really relevant to the main question behind the paper but why do the NO3 and PO4 profiles have a maximum around 120m?

- If the hypothesis is of P controlling N fixation which drives the bloom it might be worth doing a scatter plot of PO4 versus Chl in surface waters (taking care with quenching) as a negative correlation would support this.

- There is no Section 2.3.1 – first line , p7

- Figs 2 and 3 need the same colour scale

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