

Response to Reviewer 1

for "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, Louise Rousselet, Andrea M. Doglioli, Anne A. Petrenko, and
Thierry Moutin

We thank Anonymous Reviewer 1 for their time and effort in formulating their review of the manuscript. Below we reproduce the reviewer's response and address their concerns along the way.

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 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 can be 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.

Response to Comment

Yes, indeed, the surface layer where one would expect most submesoscale motions is poorly sampled, since the mixed layer, according to our definition, averaged around 20 dBar for both the MVP and CTD datasets. Additionally, some data were cut off from this surface layer so that a direct comparison could be made to Ri using the ADCP data. The problems with the horizontal resolution of these features was mentioned in Sect. 2.4, Pg. 6, lines 26-29, as well as the Discussion. We have added the following changes (in bold) to the latter, where the possibility that the MVP survey did not fully resolve the features of interest is mentioned:

Discussion, Sect. 4.2, Page 13, Line 24:

If most submesoscale structures are expected to be in the mixed layer, which throughout this dataset was near 20 m, then the sub-kilometer R_D (here found to $<200\text{m}$) would not have been resolved by the MVP survey **since MVP horizontal resolution is ~ 2 km. Besides problems of horizontal resolution, the shallow mixed layer also precluded complete vertical resolution by the underway MVP. As a result, though the summertime conditions present during the surveys lend support to reduced submesoscale circulation, the very same conditions make it difficult to state with confidence that the Ri and Ri_g methodology is entirely conclusive.** However, since the bloom of interest in these surveys spanned the top 40 m, and covered hundreds of kilometers in horizontal extent, these small features, should they have existed, would not have impacted the full depth range of the bloom, nor would they have significantly affected the horizontal advection of the entire bloom.

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.

Response to Comment

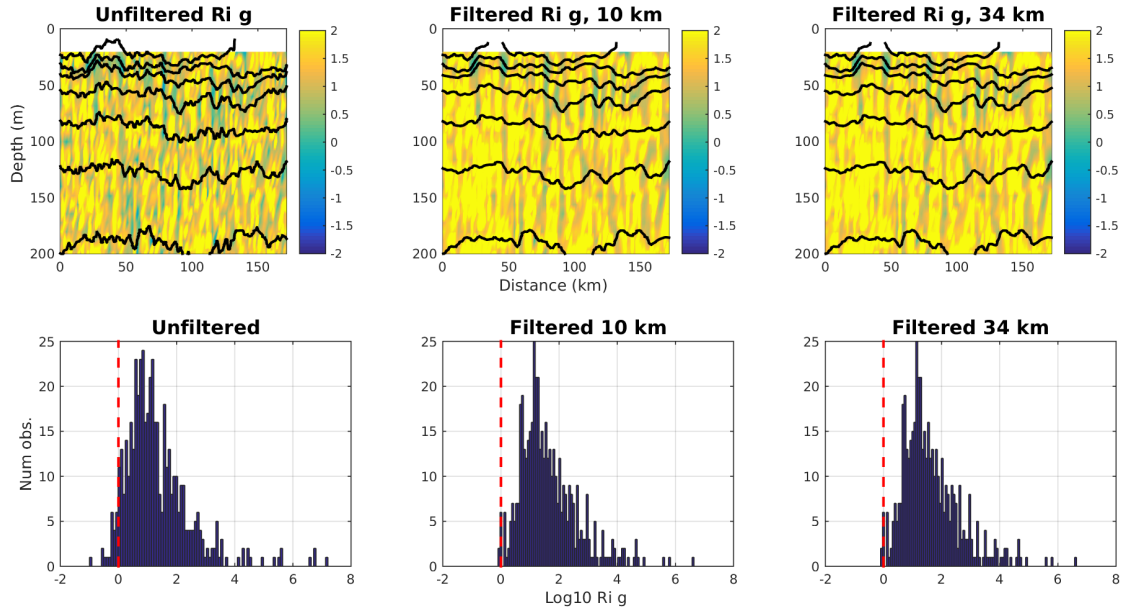
We have changed Fig 5 in accordance with this suggestion.

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?

Response to Comment

Regarding the aside, yes, the striped nature of Ri in Fig. 5 was most likely due to internal waves. In particular, near-inertial oscillations were observed during OUTPACE and are also likely a part of the ADCP data set during the MVP transects. Therefore, we looked into how filtering to remove these effects alters our results, which is summarized below.

The reviewer is astute in noting that by not horizontally filtering the density, an aliased internal wave may increase the horizontal buoyancy gradient, thus biasing Ri_g toward smaller values. For comparison, below is what Fig. 5 looks like when density is horizontally filtered, and we have also provided histograms of Ri_g 's distribution for the unfiltered and filtered treatments over the top 50 m depth. Due to the homogeneity of density, the median decorrelation length scale (as judged by zero-crossings of the auto-correlation function) for density in the upper 50 m of T4 was approximately 34 km (~ 18 observations). Since this length scale is a significant fraction of the full-depth Rossby radius alluded to in the text (and in itself somewhat suggests the natural scale being mesoscale), below we present the 34 km filter (right panels) alongside a shorter 10 km filter treatment (~ 5 observations) for comparison (middle panels).



As one would expect, the filtered Ri_g has larger values (ie weaker vertical shear due to horizontal density structures), and only a few observations are at the value of 1 (0 in log-scale). The differences between the 10 km and 34 km filtering are virtually non-existent. For the purposes of this manuscript, since even with a biased signal the conclusion was that submesoscale shear was inconsequential, this further substantiates our claims. Therefore, we propose to keep the unfiltered Figure 5, but to add the above figure as part of the Supplementary material as Fig. S3 (Figures S1-S2 will concern the T-S figure requested). Additionally, we have added to the Materials and Methods section to refer to this sensitivity to filtering (changes in bold):

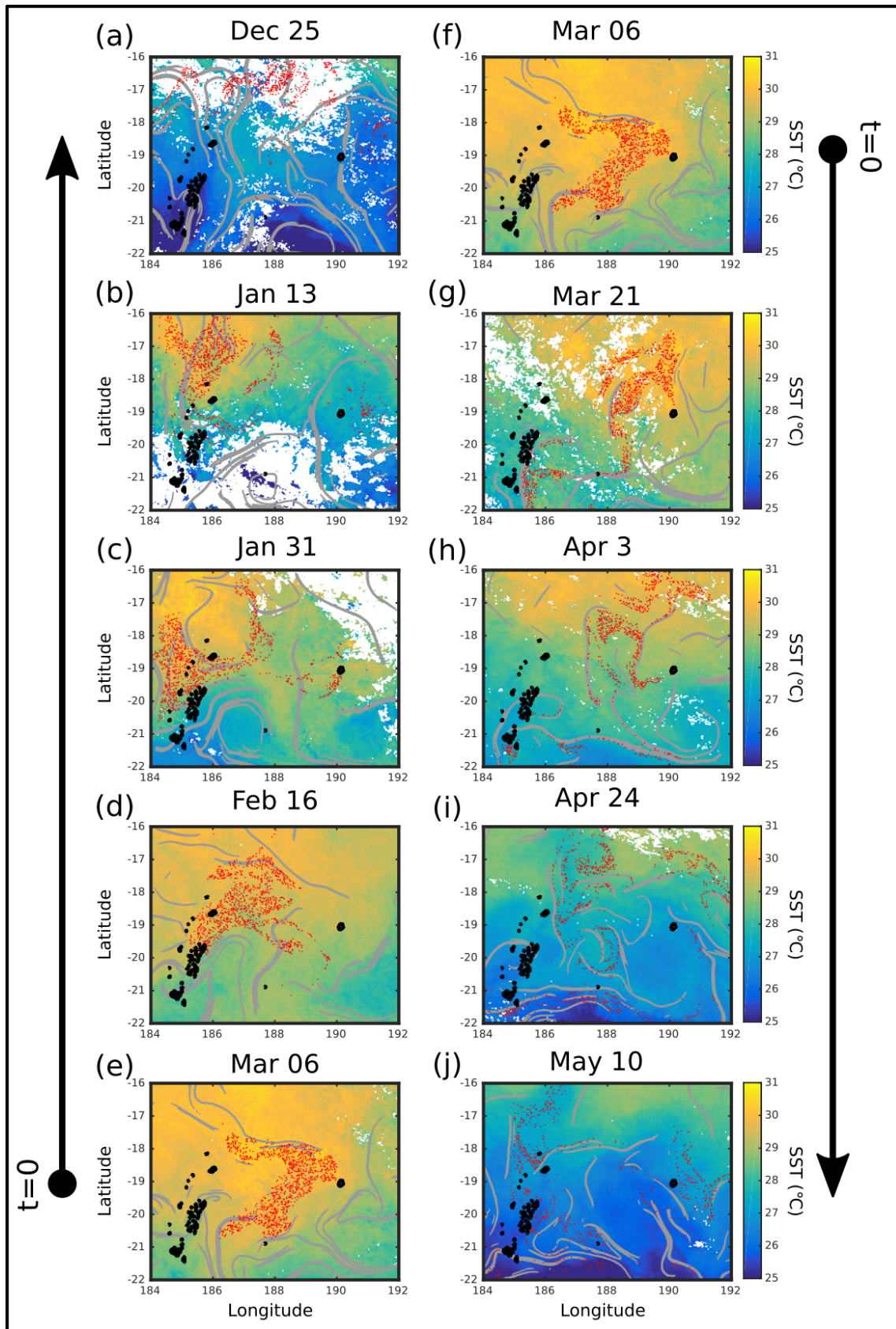
Materials and Methods, Sect. 2.2.2, Pg. 4, Line 30:

As discussed below (Sect. 2.4), the stratification was such that, even at 2 km resolution, density structures associated with balanced currents near the surface might be missed, so no additional filtering was applied to density in the MVP dataset despite possible aliasing of internal waves. **The Ri_g number analysis detailed in Sect. 2.4 can be sensitive to these aliased waves, and possibly bias the results.** While, in general, this processing step should be considered, sensitivity analysis shown in the Supplementary Material (Fig. S3) revealed that the lack of filtering with the current dataset did not affect the resulting conclusions.

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 they consistent with the hypothesis?

Response to Comment

As the reviewer has surmised, yes, SST data are available, and indeed SST is a more reliable tracer than Chl. Beyond the biological emphasis of this manuscript, Chl was used instead of SST because SST contained fewer visible gradients. We believe this is due to the fact that regional summertime heating at these latitudes is strong. The SST equivalent of Fig. 6 is provided below:



The FSLEs that are the focus for the current manuscript (in areas of high chl-a) overlap regions of homogeneous SST. However, for certain dates FSLEs can be seen to correspond with SST fronts. Jan. 31, for example, indicates a recirculation of water south of the bloom's exit from the island region with cooler values to the South. Additionally, FSLEs and cooler water to the South are well-aligned in the last three panels (Apr 03 to May 10). Though the SST unfortunately does not contribute to the bloom's narrative, for completeness, we will add the above SST figure as supplementary material Fig. S4, and include the following references in both the Results and Discussion (changes in bold):

Results, Sect. 3.4, Pg. 9, Lines 20-24:

The remotely sensed distribution of surface chl-a, calculated FSLEs and ARIANE Lagrangian particle positions, over a period spanning 25 December, 2014 to 10 May, 2015 are shown in Fig. 6. FSLEs and particles are shaded gray and red, respectively, with 10% of the particles randomly selected for plotting in all subpanels. 25 December (Dec., all months hereafter shortened) was chosen as the starting point by visually examining the chl-*a* dataset for a pre-bloom period, with a bloom 'source' region identified on 13 Jan. centered at 186° E, 20° S. **The temporal evolution of FSLEs and the of Lagrangian particles superimposed on SST is reproduced in Fig. S4 of the Supplementary Material.**

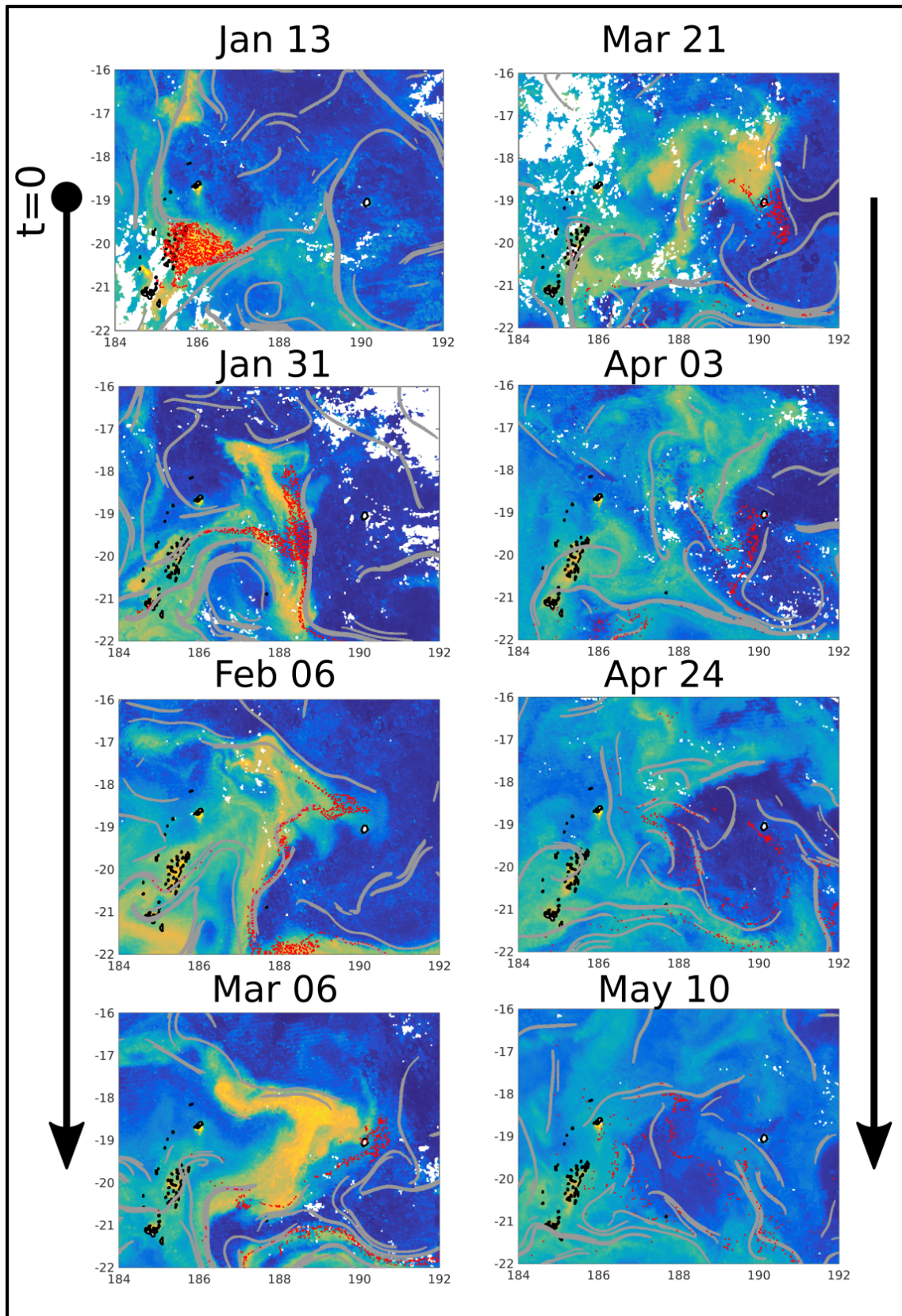
Discussion, Sect. 4.2, Pg. 14, Line 14:

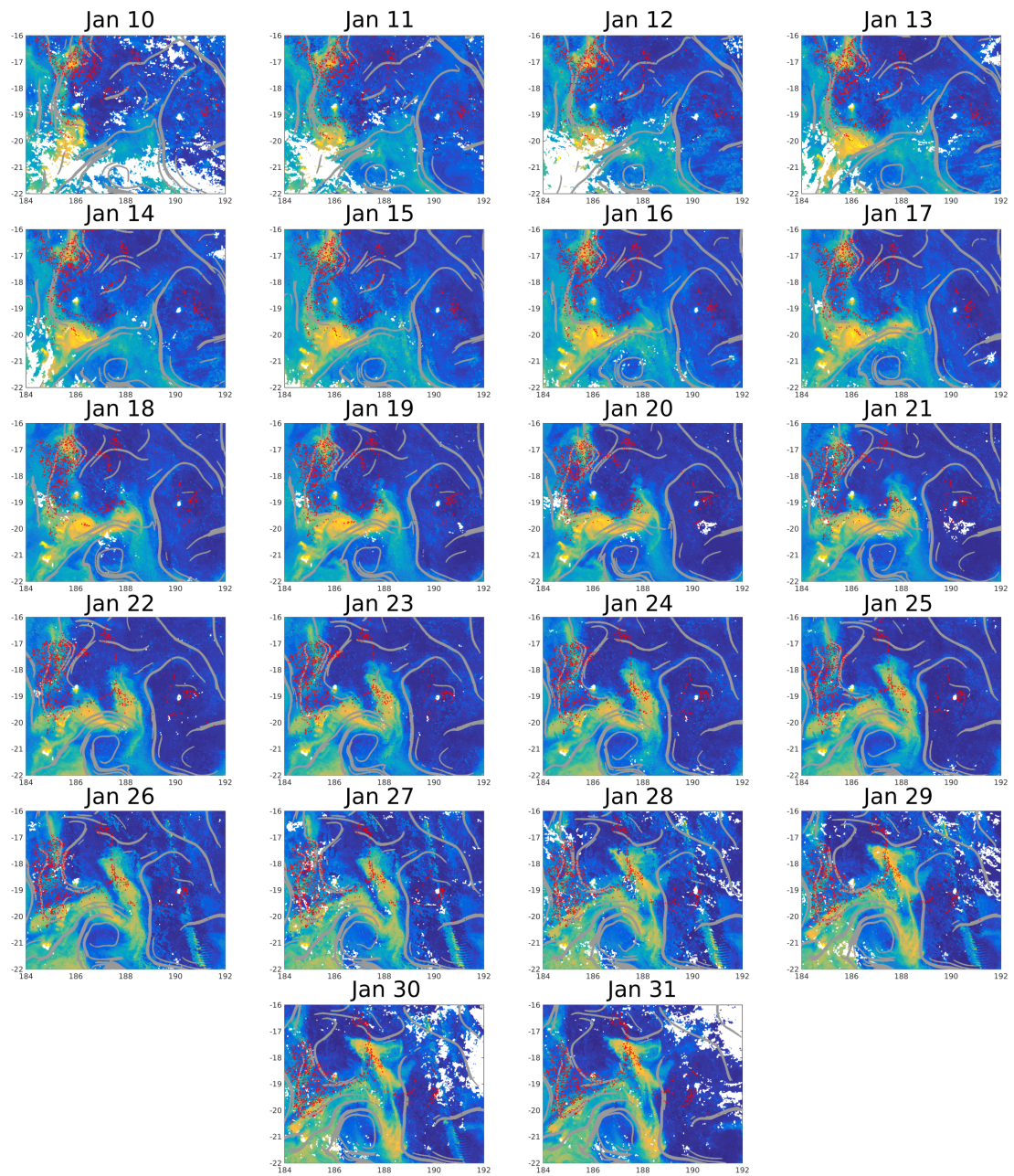
Evidently, chl-a is a reactive tracer and undergoes its own evolution, as shown by the shaded areas in Fig. 7b. **By contrast, the temporal evolution of SST, which does not suffer from this deficiency, did not display enough variability in the bloom region to confirm the efficiency of FSLEs or ARIANE particles in representing its advection (Fig. S4 in the supplementary material). This is most likely due to the strong, regional summertime heating that occurs at these latitudes.**

I find the particle tracking back to 25 December unconvincing, 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.

Response to Comment

We performed the requested Lagrangian analyses and below we provide the corresponding figures. First, Fig. 6 is re-created with a new particle initialization at the bloom's position near the island group on Jan 13 and advected in a forward time integration. Hereafter, we will refer to this as "Fig-6-Forward". Next, we provided the timeseries of Chl/FSLE/particle figures, starting from Jan 10 (a few days earlier than Jan 13) to Jan 31 with the Mar 06 particle seeding from the manuscript to fill in the gaps not presented in the original Fig. 6.





The additional Chl/FSLE/particle images between Jan 10 to Jan 31 document the bloom's exit from the island group in an eastward direction. The importance of the North-South FSLE barrier is evident during this period. The particles seeded in the bloom on Jan 13 (Fig-6-Forward) are largely absent from the bloom on Mar 06 (the seeding date in the original Fig. 6) and on Mar 21, the MVP sampling date. While disappointing for our argument, it is not surprising that chaotic particle trajec-

ries are not strictly accurate two months after their initialization. This can be due to the reactive nature of chl-a, but also to the motions unresolved by our satellite data. The reactive nature of chl-a may be important, in particular for the blooms of this region that are sustained over long time periods by N_2 fixation. Symmetric to the lack of particles inside the bloom in Fig-6-Forward after two months, the backward integration shown in the original Fig. 6 produced few particles inside the bloom on Jan 13 near the island source region, also almost two months prior to seeding. If the errors result from unresolved physical motions, these results imply a temporal window beyond which particles have deviated from their "true course", and this is clearly less than two months. The particles in both scenarios, despite their limitations, both show eastward advection over time, which was one of the main results in our analysis. Additionally, while the particle positions are sensitive to the errors in the velocity field, the FSLE structures are relatively robust to these errors (mentioned in Sect. 2.4, Pg. 7, Line 7 with reference), and both particle seeding experiments show their role as flow barriers.

The original Fig. 6 was chosen to show the temporal evolution of the bloom both before its appearance on Jan 13 and after its decline starting in April. While the Dec 25 integration of particles may be unconvincing, the FSLEs are still relevant. Additionally, though there is much interest in following the bloom from its source and identifying its causes, one of the focuses of this work is in comparing which circulation regime creates biological gradients. In order to exemplify the ability of the mesoscale regime to form the gradients observed both by remote sensing and in situ data (e.g. MVP Transect 4), we feel the Mar 06 seeding is more relevant for the exposition of gradient formation (see original Fig. 6g). To prevent figure clutter but to also still show the results shown above, we propose adding both figures to the Supplementary Material as Figures S5 (Fig-6-Forward) and S6, respectively. We have modified the manuscript in the following ways:

Materials and Methods, Sec. 2.4, Pg. 7, Line 18 (additions in bold):

Lagrangian particles were spaced $1/50^\circ$ ($\sim 2\text{km}$) apart within the chl-*a* contour of 0.3 mg m^{-3} . **An additional forward particle experiment initialized on Jan 13, with a localization of the bloom near an island group, was also conducted, and these results are shown in supplementary material Fig. S5.**

Results, Sect. 3.4, Pg. 9, Lines 20-24 (previous changes repeated in italics, additions in bold):

The remotely sensed distribution of surface chl-*a* with a bloom 'source' region identified on 13 Jan. centered at 186° E , 20° S . *The temporal evolution of FSLEs and the Lagrangian particles superimposed on SST is reproduced in Fig. S3 of the Supplementary Material. **Additional chl-*a* data, between January 10 and 31, 2015, are also provided in Fig. S6.***

Discussion, Sect. 4.2, Pg. 14, Line 14 (previous changes repeated in italics, additions in bold, deletions with a strikethrough):

Evidently, chl-*a* is a reactive tracer and undergoes its own evolution, as shown by the shaded areas in Fig. 7b. *By contrast, the temporal evolution of SST, which does not suffer from this deficiency, did not display enough variability in the bloom region to confirm the efficiency of FSLEs or ARIANE particles in representing its advection (Fig. S3 in the supplementary material).* **Moreover, the particle positions are not reliable over long timescales. Few particles can be found in the bloom on Jan 13, when it was localized near a group of islands. Conversely, a second initialization experiment on Jan 13 failed to produce many particles in the bloom for Mar 06 and Mar 21 (Supplementary Material, Fig. S5). This limitation may be a result of chl-a being a reactive tracer or of unresolved motions in the altimetry-derived flow field. Therefore, while the ability of the Lagrangian particles to both remain in the region of interest and to accurately represent elevated chl-a values (mostly above or near the 75th percentile) provides strong positive evidence that mesoscale flows were indeed advecting the bloom water, after around two months the accumulated errors due to unresolved flows make direct inspection of particle position uninformative. The FSLEs, in comparison, do not suffer from this sensitivity.**

Discussion, Sect. 4.2, Pg. 14, Line 18-20 (additions in bold, deletions with a strikethrough):

Firstly, the ~~passage of particles~~ **location of the bloom** near an island group **on Jan 13 (Fig. 6b, S5)** ~~before and at the beginning of the bloom (Fig. 6A-b)~~ suggests a possible island effect in the ignition of the bloom. **Despite the fact the bloom's beginning was not captured by in situ data, we still suggest a mechanism responsible for causing the bloom.** Considering that N₂ fixation drove new production, and nearby stations SD12 and SD13 had detectable phosphate levels, alleviation of another necessary and limiting nutrient, iron, was possibly at work.

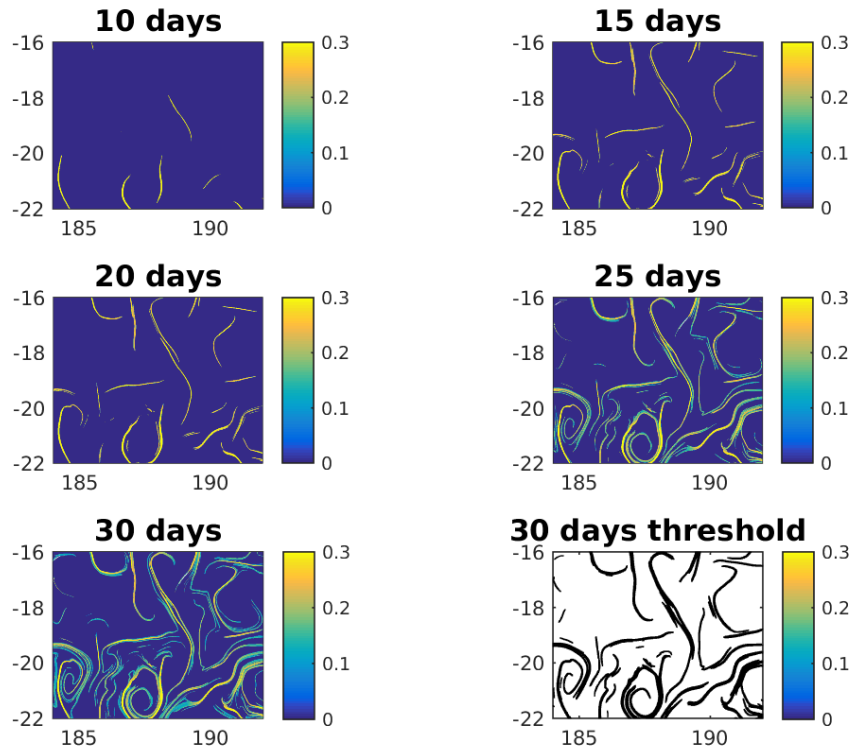
Discussion, Sect. 4.2, Pg. 14, Line 24-25 (additions in bold, deletions in strikethrough):

Secondly, the shifting FSLEs and **both** Lagrangian particle **experiments (Figs. 6, S5, and S6)** ~~tracks~~ demonstrate the general eastward advection of the bloom from its localized island source in Fig. 6b until its easternmost position in Fig. 6g.

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?

Response to Comment

The position of FSLEs, especially the strongest features, are fairly robust to the integration timescales. Long integration periods, in general, resolve smaller-scale features. Below we provide the FSLEs using 10, 15, 20, 25, and 30d integrations for Jan 31, when the bloom advected away from the island group and experienced strong North-South shear.



The largest change is between 20 and 25 days. Past this point, at 30 days, weaker structures appear, but are removed by the 0.15 d^{-1} threshold. Hence, in our case, there is no point in integrating with longer periods than 30 days, and the sensitivity of the FSLEs in our dataset should be minimal. We have added mentioned these sensitivity tests in the materials and methods:

Materials and Methods, Sec. 2.4, Pg. 7, Lines 6-7 (additions in bold):

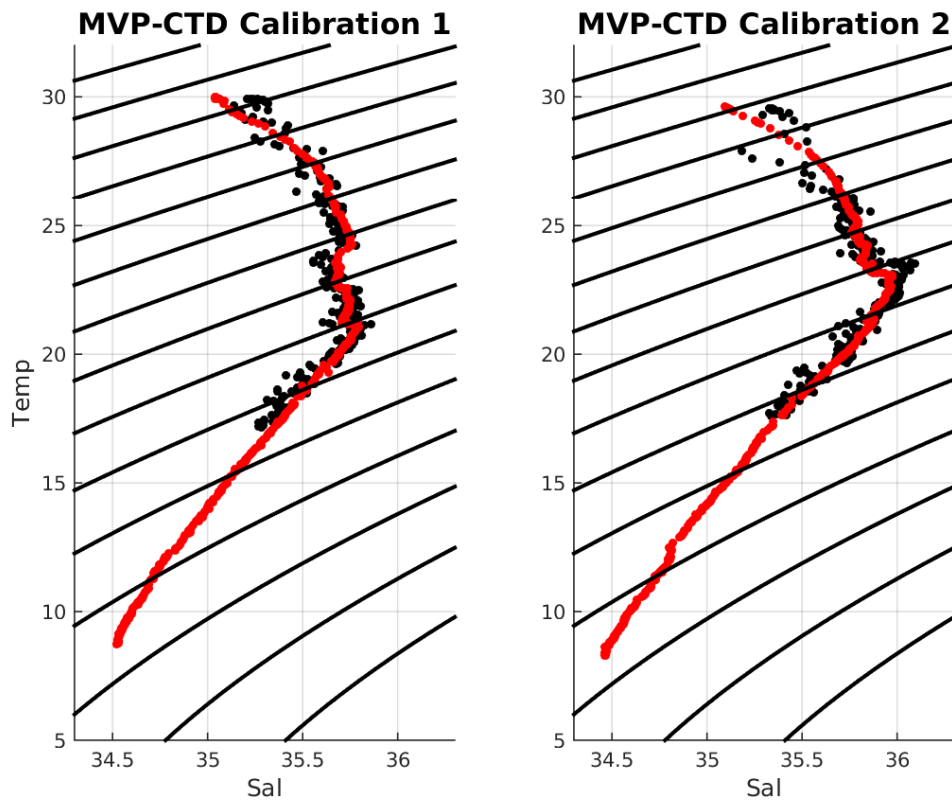
The robustness of FSLE calculations to small-scale errors in velocity fields has been previously studied (Cotte et al., 2011). **The 30 day integration timescale we have chosen is likewise robust. Sensitivity analyses (not shown) indicate the strongest features are resolved with 10-15 day integrations, with finer detail emerging over 25-30 day integrations. The smallest structures are removed by a 0.15 day^{-1} threshold for the analysis in this study.**

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

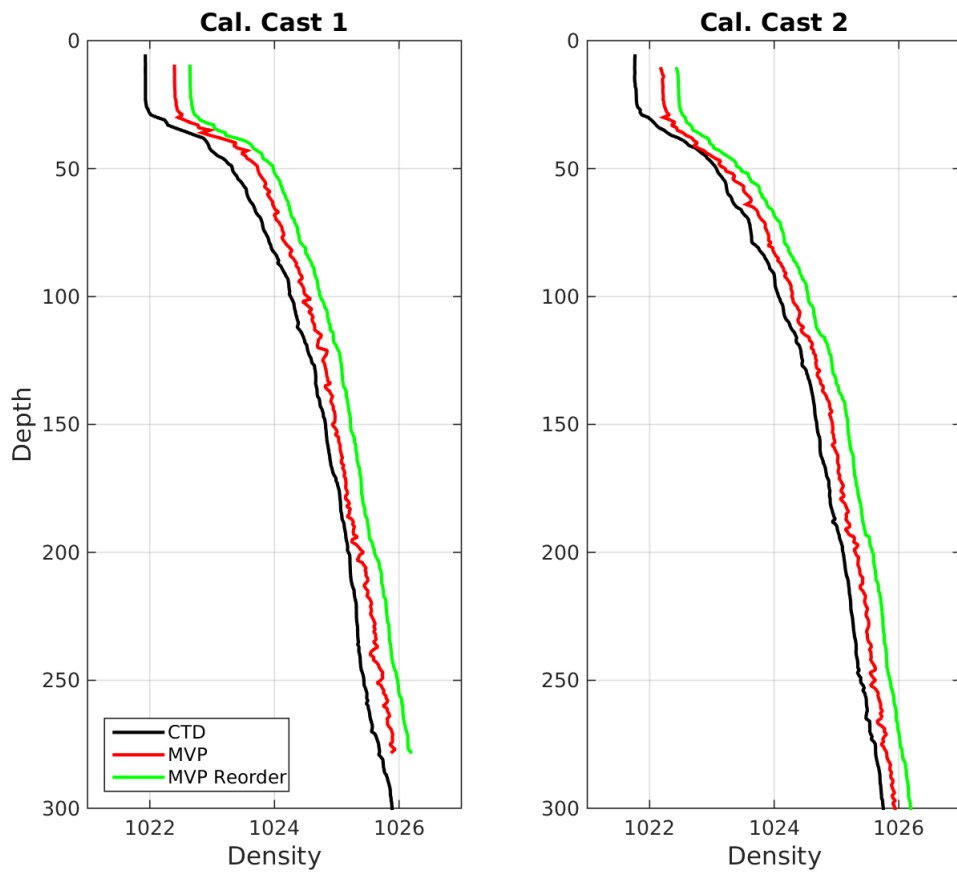
- Given the issue with the salinities from the CTD I think there needs to be a T/S diagram using MVP data in Supplementary material to reassure the reader that density comparisons between Fig. 2 and 3 are reliable.

Response to Comment

We have plotted a T/S diagram of the MVP data for the calibration cast with SD13, shown below. Another CTD cast taken mid-way during other MVP sampling not presented in this manuscript is also shown. The first calibration casts were 3 km and 41 min apart, and the second set was 0.5 km and 53 min. apart.



The variability in salinity is obvious, especially near the surface. This appears as a consequence of the fact that the sound speed is largely sensitive to temperature, and not salinity. As a result, the sound speed inversion to get salinity is relatively indeterminate. One would also expect variability to be greater at the surface, so this is not entirely surprising, but highlights that while the MVP allows for rapid surveying, this comes at the cost of increased signal variability vis a vis CTD rosette sampling. However, since the greater part of stratification was due to temperature, the variability due to salinity presents itself as small-scale salinity spikes in density that, while minimized in our post-processing, are still present (see density profiles below). The three profiles are off-set by 0.25 kg m^3 each.



Once the density is re-ordered, the mis-fit between the calculated density of the MVP and the CTD is small enough (r^2 of 0.99) that the reader should be reassured of its utility for the plots and calculations. We propose to add the two above figures in the Supplementary material, and have made reference to them in the manuscript:

Materials and Methods, Sect. 2.2.2, Pg. 4, Lines 22-24 (additions in bold):

Due to technical difficulties onboard the ship, the conductivity sensor was swapped for a sound velocity sensor. In order to calculate salinity, the roots of the sound speed equation from Chen and Millero (1977) were matched with Mackenzie's linear approximation (1981). Sound speed and temperature data were lag-corrected to reduce salinity spiking. **The variability in calculated salinity from the soundspeed is larger than that calculated with conductivity (Fig. S1), but due to the greater contribution of temperature to stratification the resulting density profiles compare well with the CTD (Fig. S2, $\rho = 0.998$, $r^2 = 0.996$).**

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

Response to Comment

We have added the LDB CTD locations to both Fig. 2 and 6. However, due to the small spatial range of the CTD locations in relation to the area mapped, these locations all overlap and appear as one point.

- 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.

Response to Comment

The reviewer's observation of a daily cycle, most likely involving non-photochemical quenching, was noted by the authors as well. This effect would be problematic if the conclusions relied more sensitively upon the quantitative values of Chl observed. However, due to the large-scale change in surface Chl after Mar 18, the authors feel that the uncorrected contribution due to quenching does not impact the results. Nevertheless, to acknowledge the Reviewer's point, the quenching effect has been noted in the Materials and Methods, Results, and Discussion, as follows:

Materials and Methods, Sect. 2.2.1, Pg.3, Lines 26-27 (additions in bold):

Chl-*a* fluorescence was calibrated to chl-*a* extractions taken from the bottle samples throughout the cruise. **No corrections were made for the daily oscillations due to non-photochemical quenching.**

Results, Sect. 3.1.2, Pg. 8, Lines 12-13 (additions in bold):

From 18 March onward, the surface concentration of chl-*a* also decreased, whereas the chl-*a* max concentration increased and began to resemble a typical DCM distribution. ***Oscillations in the surface chl-*a* during the first half of the timeseries appear, likely due to non- photochemical quenching.***

Discussion, Sect. 4.1, Pg. 12, Line 17 (additions in bold):

Station LDB's CTD timeseries also showed the decrease of surface chl-*a* and the new formation of a DCM near 80 m. ***Fluctuations in the surface chl-*a*, a possible artifact of non-photochemical quenching, were small in relation to the large change in chl-*a* that occurs between the first and second halves of the timeseries.***

- *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?*

Response to Comment

The maximum in nutrients in SD12 was noticed by the authors, as well. This local maximum in nitrate and phosphate is also reproduced in silicate. Additionally, a local oxygen minimum is also found at this depth. As a result, it is not likely a measurement error. Conversely, the temperature and salinity for SD12 don't show any discontinuities or anomalous trends. At depths greater than the 200m limit shown in Fig. 4, the usual trends of increasing nutrients are re-established for SD12. We interpret this local departure from a "classic" nutrient profile to be an intrusion at this depth of an another water mass, and without further data it is difficult to interpret its source. The water mass analysis of the entire OUTPACE

transect will be the scope of another paper from the special issue (Fumenia et al., this issue). As the reviewer notes, this does not directly impact the conclusions of the paper concerning the upper layer, but indeed it is not a mistake.

- 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.

Response to Comment

The author's suggestion to test P control on N fixation, and thus growth, by plotting PO₄ and Chl for a negative correlation is a good one. Unfortunately, the extremely low values of PO₄ in the surface waters of LDB were below the measurement threshold, and so no reliable scatter plot can be made.

- There is no Section 2.3.1 first line, p.7

Response to Comment

Thank you for noting this, it has been corrected to just say Sect. 2.3 in the text.

- Figs 2 and 3 need the same colour scale

Response to Comment

Thank you again for seeing this, the change has been implemented.