Response to Reviewers

We sincerely thank the referees for their careful reviews and thoughtful suggestions for our manuscript entitled "*The satellite chlorophyll signature of Lagrangian eddy trapping varies regionally and seasonally within a subtropical gyre*". The changes implemented in the revision based on their feedback have improved the clarity and quality of our manuscript.

In summary, in response to the major comments by Referee #1, we added an error analysis to the results shown in Figures 3, 4, and 5 and expanded upon the Conclusion (Section 5). In addition, minor edits were made to Figure 6 of the main text and Figs. B1, B4-B7, B9 of the appendix, and substantial edits to Fig. B8. Table C4 was added to the appendix to describe the total number of satellite chlorophyll-a data points incorporated in the study. As suggested by Referee #2, we added clarifying text to the methods sections and expanded upon several discussion points in Section 4.

The reviewers' comments were copied directly below in black text, to which we responded point-by-point in blue. All line numbers in our response to reviewers reference the "clean" manuscript (which can differ from line numbers in the "tracked changes" documents).

Notification to the authors

Please ensure that the colour schemes used in your maps and charts allow readers with colour vision deficiencies to correctly interpret your findings. Please check your figures using the Coblis – Color Blindness Simulator (https://www.color-blindness.com/coblis-color-blindness-simulator/) and revise the colour schemes accordingly. => Fig. B8

We edited the color scheme of Fig. B8 and Fig. 6a in the revised text to be color-blind-friendly.

Comment from Referee #1

The paper investigates the role of mesoscale ocean eddies in shaping chlorophyll-a distributions within the North Pacific Subtropical Gyre (NPSG) by comparing Eulerian and Lagrangian methods for eddy identification. Using two decades of satellite observations, the authors demonstrate that Lagrangian Coherent Vortices (RCLVs) maintain stronger chlorophyll anomalies compared to Eulerian-identified eddies due to limited lateral dilution. The study reveals significant regional and seasonal variability, highlighting distinct patterns in chlorophyll anomalies among northern, southeastern, and Hawaiian Lee Eddies. This research challenges traditional assumptions about mesoscale eddy trapping and provides valuable insights into their biogeochemical impacts.

The paper presents a well-rounded introduction enriched with relevant references, offering a concise and clear overview of the state of the art. That being said, I suggest the authors reconsider whether it is necessary to split the introduction into three different sections. I believe it would work better as a single section without subsections.

Following the reviewer's suggestion, we removed the subheadings in the Introduction section.

The limitations of each method are clearly stated and well-argued. The results are well-scoped, the figures are of high quality, and future steps are clearly defined. Some results are supported by illustrative sketches, which enhance clarity and help readers visualize complex processes. The discussion is extensive, well-founded, enriched with relevant references, and solid in its argumentation.

We thank the reviewer for their compliments on this work.

My recommendation is acceptance if the [major] revisions are successfully addressed to ensure that the present results are confirmed to be robust against error analysis (see Major comments).

Minor comments:

In the abstract, every key concept is briefly explained; however, non-expert readers may struggle to understand what the authors mention in lines 10–11 unless a brief explanation of the nonlinearity parameter is provided.

We amended the sentence to the following with the changes in bold text, removing the term "nonlinearity parameter": "*Eddy trapping of positive chlorophyll anomalies is most significant in the southern regions of the NPSG, counter to expectations from a* ***commonly used Eulerian metric of eddy trapping***." (Lines 10-11)

In the abstract, line 13: I suggest the authors explicitly state the finding. Is it a positive or negative relationship? The answer seems to be in lines 322–323. Please avoid leaving open questions or ambiguity about your results in the abstract.

We amended the sentence to the following with the changes in bold text: "*We found weak relationships between eddy age and the magnitude of surface chlorophyll anomalies in most long-lived Lagrangian coherent vortices; the strongest exception was in wintertime anticyclones in the Lee of the Hawaiian Islands,* ***where chlorophyll increases throughout eddy lifetimes.***" (Lines 12-14)

Line 128: Which figure are the authors referring to? Please clarify.

This LaTeX compilation error was corrected in the revised text to "Figure B2". (now Line 131)

Lines 380–381: I suggest the authors properly frame the results to the study region in these lines. While the findings are suggestive of broader applications, the authors cannot extend these conclusions globally without demonstration.

While our findings are limited to the NPSG, other studies consistently corroborate in various domains that the nonlinearity parameter is a poor predictor of Lagrangian coherency. In the revised manuscript we clarify our statement and explicitly describe the locations for which this has been tested:

> "*Previous studies have shown that the nonlinearity parameter is not predictive of Lagrangian coherency for eddies in other regions, including the Agulhas leakage (Beron-Vera et al., 2013), Gulf of Mexico (Beron-Vera et al., 2019), East Australian Current (Cetina-Heredia et al., 2019), North Brazil Current (Andrade-Canto and Beron-Vera, 2022), and South China Sea (Liu et al., 2022). We also find that the nonlinearity parameter is insufficient to determine eddy trapping strength in the NPSG. Given the consistency of these findings across various oceanic systems, we do not recommend using the nonlinearity parameter criterion alone to infer effective eddy coherency without further testing. Although Lagrangian metrics are more involved and computationally expensive, they are frame-independent and more informative.*" (Lines 424-431)

Figure 2: The caption could benefit from more details about the zoomed-in view shown in the right-hand side panel.

We added the following text to the end of the caption for Figure 2: "*The right-hand panel shows the relative difference between the eddy and background probability density distributions of δcclim, demonstrating the difference in the likelihood of a given chl-a anomaly to occur in the eddy type compared to the background ocean.*"

Figure 6: Increase the grid resolution to improve the readability of values in the composite subplots.

Figure 6 in the revised manuscript has higher grid resolution and smaller spaces between subplots to improve readability.

Figures B6, B7, and B9: The percentages are difficult to read due to overlapping text. Adjust the layout to improve clarity.

Figures B5, B6, B7, and B9 have been edited so that the quantile labels are above the subplots instead of inside to improve readability.

Major comments:

There is an important aspect that the authors have omitted in their work and that requires attention. To increase the robustness of the results and the significance of the observed distinct patterns, I suggest the authors include an error analysis to better assess the magnitude of the differences discussed. On many occasions, the differences presented are very small, yet they are claimed to be significantly distinct. Since the values discussed here are not straightforward or intuitive for routine daily use, readers need background information about the errors to properly interpret and contextualize the differences being reported. Please provide information on this. The current version lacks on any information about error analysis.

Briefly, our initial efforts to measure statistical significance were not helpful since p-values converge to zero with such large sample sizes. In the revision, we used a bootstrap analysis to provide clear confidence intervals to provide some measure of uncertainty. We expand on these points below.

Regarding statistical significance:

We did not present tests of statistical significance due to the large volume of data (Table C4 was added in the revision to show the total number of data points analyzed, including 342,639,336 total chl-a measurements). Initial attempts to compare the probability density distributions always returned p-values of 0, but these can be ascribed to the convergence of p-values to 0 with increasing sample size.

In the revision, we:

1. Added the sentence: "*Tests of statistical significance are uninformative when comparing the distributions of large datasets because the p-value converges to zero (e.g., Lin et al., 2013), as is the case here when aggregating two decades of satellite chl-a data.*" (Lines 182-183)
2. Added information about these sample sizes: "*82.7% of chl-a measurements were collected in the background ocean (i.e., outside of all eddy types; n = 283,319,424), while 17.3% were in an Eulerian and/or Lagrangian eddy (n = 59,319,912; Table C4)..*" (Lines 196-198)
3. Reworded and replaced the word "significant" in one case, which now states: "*Anomalies are rarer during spring and summer when southeastern cyclones are only more likely than the background to have positive δcclim up to a threshold magnitude.*" (Lines 295-296)

Regarding "error analysis":

We agree that the study would benefit from an error analysis. To this end, in the revised manuscript we report confidence intervals computed from a bootstrapping analysis described in the new Section 2.4:

"*...we computed confidence intervals for f(δcclim) (as suggested in Hubbard and Armstrong (2006)) using a nonparametric bootstrapping method (Efron, 1979). First, the numpy.random.choice() Python function was used to randomly resample the δcclim datasets with replacement a number of times equivalent to the original sample size (sample sizes range from 411,546 to 25,960,826; Tables C2 and C3). "With replacement" means that each data point could be sampled in every draw, even if previously chosen. Next, we calculated f(δcclim) from the probability density distribution of the bootstrap dataset. These steps were repeated 1000 times for each eddy type. Finally, we estimated 95% confidence intervals for every δcclim value from the 2.5 to 97.5 percentiles of the 1000 bootstrap f(δcclim) distributions.*" (Lines 184-190)

Figures 3, 4, and 5 now show the confidence intervals for every x-value (*δcclim*). The relatively narrow confidence intervals provide support for the inferences originally reported in the manuscript.

Regarding interpretation and contextualization of differences:

We made additions to the text in the revision to aid the reader in interpreting the results:

1. Added an interpretation of the relative difference in probability density distributions: "*$f(\delta c_{clim}) = 1$ corresponds to a 100% increase in the density of observations of δc_{clim} in the eddy compared to the background.*" (Lines 165-166)
2. Added subplot (b) to Figure B8 that shows the average chl-a concentration by region and season so that readers can more easily contextualize the magnitude of the chl-a anomalies.
3. Added references to the average chl-a values and % increase in density distribution throughout the text:
    - "*To contextualize these anomalies, the average surface chl-a concentration in the NPSG from 2000 through 2020 was 0.068 mg $\cdot m^{-3}$ (although this varies regionally and seasonally (Appendix Fig. B4)). Thus, anticyclonic RCLVs are 59.7% more likely than the background ocean to contain chl-a blooms with concentrations roughly 1.5 times the average ($\delta c_{clim} = 0.031$ at $\max(f) = 0.597$), but not likely to double the average concentration.*" (Lines 205-209)
    - "*Cyclonic RCLVs are **up to 20.4%** likelier to have positive chl-a anomalies than the non-eddy background ocean.*" (Lines 218-219)
    - "*Occurrences of positive δc_{clim} are **up to 46.5%** more common in all types of anticyclones …*" (Line 251)
    - "*...the average chl-a concentration is highest in the northern winter, so the northern anticyclones are likely to elevate chl-a by relatively similar amounts: up to 56.5% of average concentrations in the fall versus 59.9% in the winter (Appendix Fig. B8b).*" (Lines 255-256)
    - "*...wintertime southeastern cyclonic RCLVs are 118.3% more likely than the background to contain $\delta c_{clim} = 0.077$ mg $\cdot$ m^{-3}, or a doubling of the background average chl-a concentration (Appendix Fig. B8b).*" (Lines 290-291)
    - "*For context, the average chl-a concentration in the southeast spring is 0.063 mg $\cdot$ m^{-3} and 0.060 mg $\cdot$ m^{-3} in the summer (Appendix Fig. B8b), so RCLVs are likely to enhance chl-a by up 41.3% and 35.0% of spring and summer average concentrations, respectively.*" (Lines 297-299)
    - "*...they have a 933.0% higher likelihood than the background ocean to contain $\delta c_{clim} = 0.032$ mg $\cdot$ m^{-3}, or a 51.6% increase in chl-a concentration compared to the regional and seasonal average (Appendix Fig. B8).*" (Lines 316-317)

I believe the authors should expand the conclusions section. It should be self-contained and comprehensible when read in isolation. In its current form, it omits many relevant results, limitations, and future perspectives. Please enrich the conclusions with more details (drawn from the discussion section) to provide a comprehensive closure to the study.

Following the reviewer's recommendation, we expanded the Conclusion (Section 5) with more details.

Comment from Referee #2

This paper investigates the chlorophyll-a signature of ocean eddies in a subtropical gyre using combined eulerian/lagrangian eddy atlases and analyses based on conditional Probability Density Distributions (PDD) of climatological chlo-a anomalies, both applied on satellite data. It is clearly written and presented in an intelligible and didactic manner that made the reading very enjoyable. I also appreciated the honesty and integrity of the presentation. It introduces a well-thought and powerful methodology (that would allow exploring further these biophysical interactions in many different oceanographic settings) containing plenty of (useful for reproducibility) details. Finally, it reports several novel and synthetically-presented results that exemplify the complexity of detecting ocean eddies and their biological responses (yet, common patterns emerge), which reveal the sensitivity of the results to the methodology used, which (importantly) show that the non-linearity parameter is not predictive of Lagrangian coherency, and which typify ocean eddies in terms of their dynamics and associated chlo-a signatures. While the results were already reasonably well discussed, I suggested some developments below. All in all, I recommend a minor revision to address the points raised here.

We thank the reviewer for their compliments on the manuscript.

Major points

Note: We broke up some of the reviewer's longer points to address the individual components.

- definition of the studied region (sect. 2 lines 90-97): I understand the rationale (and usefulness when studying such complex coupled mechanisms) of the restricted region of study chosen by the authors. Yet, I wonder if a slightly larger domain (in both latitude and longitude, or essentially in longitude considering the predominant zonal eddy trajectories) would have allowed the authors to track eddies for longer time, hence providing more statistics and potentially slightly affecting the overall results (relying on PDDs). Could you please comment on this? The other reason for which I'd be curious to see similar analyses performed over a larger domain would be to investigate how such bio-physical coupled dynamics would evolve depending on the oceanic provinces considered. The 'domain choice' was essentially based on "physical oceanography" arguments; conversely, it could have been made using "biological arguments" and/or following "biogeochemical provinces" (see for instance Reygondeau et al. 2013 GBC).

We did choose the spatial bounds of the domain based on average biological gradients in the region that are set by environmental or physical phenomena. We clarified this in the revision: "*These spatial bounds reduce the degrees of freedom associated with large-scale environmental* ***and biological*** *gradients from the ultra-oligotrophic western NPSG* ***(Polovina et al., 2008)****, the Transition Zone Chlorophyll Front* ***that seasonally oscillates between 30-40◦N (Glover et al., 1994), the productive*** *California Current System to* ***the east,*** *and* ***a thermocline ridge located between 3-13◦N that supports higher nutrient concentrations (Pennington, 2006)****.*" (Lines 86-90)

Even without considering such level of details, one could split, at first order, the North Pacific basin into two regions: an "eutrophic" domain (e.g. coastal and equatorial upwellings as well as shelf areas) and an "oligotrophic" domain (e.g. the rest, excluding the very high latitudes). Previous studies that analysed the interplay between eddies and chloro-a (primarily in major upwelling systems, but also considering the link with the nearby oligotrophic gyres, such as Rossi et al. 2008 GRL, Rossi et al. 2009 NPG, Gruber et al. 2011 NatGeo, Hernandez-Carrasco et al. 2014 DSR) discussed several coupled mechanisms that hold over different oceanic settings. Previous work also tried to contrast and reconcile both positive (enhancement) and negative (depletion) effects that have been documented in the literature; such perspective at basin-scale would surely bring new insights. Indeed, a larger studied region would have allowed the authors to expand their regional/seasonal analyses (sect. 3.2, which I found very interesting) to larger portion of the ocean surface, including several oceanic biomes and potentially supporting distinct coupled mechanisms. I suggest to add a paragraph of discussion (with refs) in sect. 4 and a perspective (sect. 5) along these lines.

We agree that the choice of a larger domain would have introduced additional richness to the analysis through inter-region interactions, as the cited studies have shown. However, it is also interesting that despite our choice to select a somewhat homogeneous physical/biogeochemical domain of study, there are still distinct sub-regimes (North, South, Lee regimes). Extending the domain therefore represents an interesting prospect, but beyond the scope of the current study. To flag this for the reader, we added a closing sentence in Section 5 regarding the future expansion of this work to other domains: "*Our reproducible methodology can be applied to other ocean regions to reveal regional complexities and unifying patterns in the chl-a signatures fostered by eddy trapping and dispersal.*" (Lines 491-492)

We chose not to add a paragraph speculating about the expectations from such a study in order not to distract the reader from the investigation of regional and seasonal variability within a single biogeochemical province, which is the focus of this work.

- Parameters of the algorithm parameters ("32 days" time-scale): I understood that the youngest eddies contained in the original Lagrangian Atlas were 32 days old, and that this has been modified here using new Lagrangian particle experiments to allow tracking eddies sooner, i.e. from when they are 8-day old onward (sect. 2.1.2). I wonder however how the physical and geometrical connections are ensured for a given eddy when associating earlier images (from 8-days to 32 days, deduced from one set of particle trajectories) with later images (32 days and older, deduced from another set of particle trajectories, if I understood well Fig. B2).

The eddies were tracked from many overlapping 32-day Lagrangian simulations in Jones-Kellett & Follows, 2024 (doi.org/10.5194/essd-16-1475-2024), not one set of particle trajectories (see Fig. B1). The contours from eddy formation at ages 24, 16, and 8 days were then drawn from the same Lagrangian trajectories used to derive the first 32-day contour of each tracked RCLV. We reworded and added more details in Section 2.1.2 to clarify the steps taken to build the Lagrangian atlas.

Lagrangian models are sensitive to initial conditions, so would the results be the same if the atlas would have been computed upfront from a single set of trajectories covering all affordable eddy's lifetimes from 8-day onward? Would this affect global statistics of eddy lifetimes?

This idea from the reviewer is akin to the methods deployed by Liu & Aberntheу, 2023 (https://doi.org/10.5194/essd-15-1765-2023), where they advect particles forward in time for discrete non-overlapping 30, 90, and 180-day intervals. This strategy is inflexible and does not reveal the exact lifespan of features; rather, it identifies fluid masses that happen to be coherent over the simulation period. Such a method would also result in particles converging to specific regions and thus does not capture the evolving birth and decay of features.

The Jones-Kellett & Follows 2024 (doi.org/10.5194/essd-16-1475-2024) RCLV tracking method is more sophisticated due to the flexibility to track features over their full lifetimes by consistently re-initializing particles and not using a single set of particle trajectories. This produces an eddy atlas that is directly comparable to SLA eddy atlases and captures the biologically relevant evolution of the coherent features as they grow and shrink over time.

- The authors generated a Eulerian atlas by imposing a minimum lifespan of 32 days to match the original version of the Lagrangian Atlas and then perform a neat cross-comparison. They used daily input SLA data but that were first reduced to an 8-d frequency to match other input data, which make sense to me. It is worth noting that the daily gridded CMEMS products have already been numerically interpolated in space and time, and that the mean frequency of altimeters revisiting the same oceanic location is of the order of a week or more. A short discussion point could be added to emphasize the fact that upstream numerical treatments applied on input data may affect downstream physical analyses such as transport estimates and eddy detection (Capet et al. 2014 GRL).

We added the following in the revision to acknowledge this point: "*The CMEMS and OC-CCI products spatiotemporally interpolate information from all available altimeter and ocean color products, respectively. We note that the numerical treatment of these data affects the accuracy of mesoscale feature tracking (Capet et al., 2014; Ballarotta et al., 2019) and the detection of their chl-a signatures.*" (Lines 94-97)

Similar biases could arise from the missing data in chlo-a maps; this has been already acknowledged in Appendix C but I suggest to emphasize it in the main text.

Missing data in the chl-a maps was acknowledged in the last paragraph of the Discussion in the submitted manuscript: "*Another limitation of satellite chl-a observations is missing data from cloud coverage including during storms, which can stimulate phytoplankton blooms in eddies..*" (now Lines 471-472).

- Another methodological interrogation concerns the parameter called "eddy disappearance", set on 3 days here. Would other values affect substantially the overall eddy statistics? I do not understand well how this parameter could (or not) affect statistically the number and ages of detected eddies. One could consider adding basic statistics to explore the sensitivity of the Eulerian atlas to a few values of the "eddy disappearance" parameters. Could you please comment on this?

The disappearance parameter is needed to account for satellite altimetry data gaps that can sometimes miss mesoscale features temporarily. We used the same tolerance as in Chelton et al. 2011 (dx.doi.org/10.1016/j.pocean.2011.01.002), and added this reference to the text in the revision to support our choice in parameter (Line 107). The META3.1 eddy tracking product uses a more liberal, but similar, tolerance of 4 days (Pegliasco et al. 2022; doi.org/10.5194/essd-14-1087-2022).

Too conservative of a disappearance parameter will cause the tracking algorithm to prematurely "kill" an eddy and then consider it a new feature after it reappears. Too liberal of a disappearance parameter may merge two separate features into one eddy track. Therefore, this parameter choice most directly affects the recorded SLA eddy age. Our results do not rely on SLA eddy age (chl as a function of eddy age was only evaluated for long-lived RCLVs in Section 3.3), so the SLA eddy disappearance parameter would affect the overall statistics of the chl-a results.

More generally, it would be useful for readers to show simple statistics (to be reported in Appendix C for instance) derived from the total numbers of detected eddies (by eddy type) and their lifetime in the different atlases (e.g. eulerian, lagrangian v1, lagrangian v2). I wonder for instance if the min/mean/max ages are statistically similar when comparing Eulerian and Lagrangian approaches.

The differences in the physical manifestation in the eddies between the Eulerian and RCLV atlases were explored in depth in our previous manuscript (Jones-Kellett & Follows 2024; doi.org/10.5194/essd-16-1475-2024), including the eddy lifespans. Therefore, we do not provide a detailed comparison in this manuscript. We added brief explanations in the revision to give the reader some context about the eddy lifespans:

- "*The median lifespan of the tracked SLA eddies was 55 days, but one survived as long as 503 days in this domain.*" (Lines 110-111)
- "*The median lifespan of the RCLVs is 40 days, and the maximum is 413. RCLVs typically persist for shorter timescales than their SLA eddy counterparts in this domain, except in the Lee of the Hawaiian Islands (Jones-Kellett and Follows, 2024).*" (Lines 134-136)

Note that the eddy age statistics do not differ between v1 and v2 of the RCLV atlases, as they track the same features over their lifetimes and v2 only adds information about the genesis of the RCLVs. The minimum eddy age is the same in all datasets since this was a chosen parameter set to 32 days.

- The temporal resolution of most input data ("8 days" frequency) would prevent the authors to analyse fast biological dynamics (such as fast-reacting but short-lived blooms of pico-/nano- phytoplankton, sub-daily photo-acclimatation, intense grazing events, viral shunts, etc…), despite the fact that such processes have been documented in the ocean. Physically talking, it may also be missing rapid (sub-weekly) changes of coherency (or trapping ability) of the detected eddies, as the dynamics of some eddies may evolve fast due to wind-events and/or eddies merging, etc… (see for instance Fig. 4 of

Froyland et al. 2015 Chaos). I suggest to add a few lines of discussion in a potential new sect. “BGC discussion” (see below) and in sect. 4.3, respectively.

Indeed there are both physical and biological phenomena present and acting timescales that are not resolved by the datasets or are integrated over by the 8-day time averaging of the chlorophyll data set. Our current study is, of course, subject to the limitations of the observational platforms. Both the Eulerian and Lagrangian eddy atlases were constructed from daily altimetry data, so they do incorporate sub-weekly dynamics including eddy splitting/merging, or loss of coherency at any point along the trajectory. We essentially sub-sample the eddy trajectories every 8 days, co-locate the 8-day average chl-a data, and aggregate the information over two decades to perform a statistical analysis. This subsetting does not bias against any particular sub-weekly dynamics.

Wind events are an interesting topic and may cause vertical mixing in the eddy, or generate ageostrophic currents that would cause an eddy to lose coherency in the Ekman layer, for example. We added a sentence to acknowledge this in the Discussion section: “*Our methods are based on geostrophic currents, so dispersive ageostrophic currents in the surface Ekman layer generated from wind events could transport waters out of the bounds of the geostrophic RCLVs (e.g., Johnson et al., 2024).*” (Lines 469-471)

- Somewhere in sect. 4, I suggest to refer to and discuss the results of Hernandez-Carrasco et al. 2018 SR. The main point that seems relevant to discuss here is the fact that hydrodynamics may enhance/deplete phytoplankton through active mechanisms (e.g. affecting vertical dynamics so that it would fuel the euphotic zone with new nutrients, or conversely deplete surface layers, then affecting plankton growth rates) but it could also just re-organise spatially (e.g. passive horizontal aggregation, vertical deepening/shoaling of chlo-a patches without growth, etc…) the surrounding chloro-a standing stocks.

We added a reflection on this point in Section 4.2: “*Understanding elevated chl-a in RCLVs is also nontrivial because, at any given life stage, a coherent eddy could passively advect a phytoplankton patch or actively stimulate growth via vertical processes (Calil and Richards, 2010; Jönsson et al., 2009; Hernández-Carrasco et al., 2018). Distinguishing active and passive plankton dynamics in eddies is essential to quantify mesoscale contributions to primary production (Jönsson et al., 2011; Jönsson and Salisbury, 2016).*” (Lines 409-412)

- In sect. 4, I suggest to discuss the fact that ocean colour data sensed by satellites might be missing a non-negligible part of the biological responses in these oligotrophic gyres (especially in these eddies in which the pycnocline deepens) as phytoplankton maximum are sometimes organized a deep chloro-a maximum. I suggest the authors to slightly more develop this point (already written in Appendix A) and transfer it to the main Discussion sect. 4 (introducing a new “BGC” subsection?).

The last 3 sentences of Appendix A that touch on these points were ingested into the Discussion section in the revision: “*A deep chlorophyll maximum (DCM) layer persists in the NPSG where phytoplankton grow at a depth that efficiently balances nutrient and light availability. Elevated chl-a concentrations have been observed at the DCM in cyclones relative to anticyclones north of the islands (Seki et al., 2001;*

Xiu and Chai, 2020; Barone et al., 2022), in contrast to the surface observations reported here. Thus, the biological response to eddies at depth may differ from the surface (Huang and Xu, 2018; Zhao et al., 2021). " (Lines 461-465)

- In sect. 4.2, I suggest to add a discussion point on a possible reason that could be invoked to explain why the biological signatures of Hawaiian Lee eddies differ quite substantially from the rest of the gyre: it consists in the biogeochemical enrichments (of macro and/or micro-nutrients) and/or biological seeding (e.g. bringing new coastal species into open-ocean waters) of the depleted open-ocean waters when they flow over the (narrow) continental shelves and at a close proximity of the islands' coastlines. This effect to-date has been essentially studied in the Southern Ocean (the lee of the Crozet archipelago for instance, where macro-nutrients are in excess) but it is possible that the Hawaiian archipelago would also release some elements/plankton community in the water column that affect downstream responses. This would fit nicely in the new suggested BGC discussion subsection.

There are many potential mechanisms driving the differences found in the Hawaiian Lee eddies compared to elsewhere in the gyre, including the island mass effect as described by the reviewer. We added this and other possible mechanisms in the discussion, moving this piece on the Hawaiian Lee Eddies into Section 4.1 ("Regional Variations and Mechanisms") of the revision:

"*The Lee Eddies of either polarity may act as carriers of blooms stimulated by the "island mass effect" (Hasegawa et al., 2009; Cardoso et al., 2020), where the coastal upwelling of subsurface nutrients or run-off from islands acts as fertilizer that enhances chl-a concentrations and marine primary production (Doty and Oguri, 1956; Messié et al., 2020). This effect has been recognized in the Lee of the Hawaiian Islands (Gilmartin and Revelante, 1974; Messié et al., 2022; Feloy et al., 2024), but separating its influence from other eddy-driven phytoplankton stimulants is challenging. For example, the high angular velocities of the Lee cyclones may support more extreme isopycnal doming and eddy pumping of nutrients compared to eddies generated to the north or southeast of the islands (Friedrich et al., 2021). Ekman pumping from eddy-wind interactions likely drives blooms in the anticyclonic Lee Eddies (Gaube et al., 2014, 2015), consistent with our finding that chl-a is most elevated in wintertime Hawaiian Lee anticyclones, with trapping further enhancing local concentrations (Fig. 5) that linearly increase over their long lifetimes (Fig. 6). Nitrogen-fixing diazotroph blooms could also contribute to elevated chl-a in the Lee anticyclones, as found in a regional model study (Friedrich et al., 2021) and similarly observed in anticyclones north of Hawaiʻi (Church et al., 2009; Fong et al., 2008; Wilson et al., 2017; Cheung et al., 2020; Dugenne et al., 2023). However, these observations were primarily in the summer, which is the season with the least extreme likelihood of enhanced satellite chl-a compared to the background ocean (Fig. 5e). In situ investigations of microbial communities in Lee anticyclones are currently lacking but may be necessary to understand possible seasonal variations in plankton groups that contribute to increased chl-a in these features.*" (Lines 383-398)

Minor points

- The three first sentences of Sect. 1.3 need to be supported by one (or more) references. Otherwise they should be rewritten.

We removed the subheadings of the Introduction following the suggestion of Reviewer #1. Given that change, the first two sentences of what was 1.3 were also removed since they repeated ideas from the previous paragraph (which are supported with references). The third sentence that the reviewer refers to was rewritten and a reference was added: "*The chl-a signature of Lagrangian trapping vortexes versus leaky eddies has not previously been quantified, but they likely differ because trapping is theorized to preserve local anomalies of chl-a* ***(Gaube et al., 2014).***" (Lines 71-72)

- p. 6 line 128: refer to Fig. B1

This LaTeX compilation error was corrected in the revised text to "Figure B2". (now Line 131)

- p. 22, suggest harmonizing the wording ('backward-in-time")

Good eye! To be consistent with the main text, we changed the wording to "backward-in-time" in Fig. B1.

Refs cited:

https://doi.org/10.1038/s41598-018-26857-9

https://doi.org/10.1029/2008GL033610

https://doi.org/10.1002/gbc.20089

https://doi.org/10.5194/npg-16-557-2009

https://doi.org/10.1038/ngeo1273

https://doi.org/10.1016/j.dsr.2013.09.003

https://doi.org/10.1063/1.4927830

https://doi.org/10.1002/2014GL061770