Response to reviewer #1:

We thank reviewer #1 for a thorough and constructive review. This review helped us to significantly improve the manuscript particularly the interpretation of the nutrient stress dynamics that is the main scope of this study. All comments have been addressed as described below. Reviewer's comments are italicized, our responses are in normal black font, citation from the manuscript are in blue font, ~~strikethrough~~ text was deleted, and new sentences that were added in the revised version, in response to reviewer comments, are shown in green font text.

GENERAL COMMENTS

This manuscript offers a valuable contribution to understanding island mass effects (IME) in the South Pacific by integrating satellite observations with in situ measurements from the Tara Pacific expedition to examine phytoplankton dynamics across four archipelagos. Its multi-platform approach, inclusion of both macronutrients and trace metals (notably iron), and evidence for reduced phytoplankton physiological stress within IME zones collectively strengthen the case for iron, as well as macronutrients, enrichment from terrigenous/reef inputs or local upwelling as key drivers of observed patterns. However, major revisions are needed to improve clarity, methodological transparency, and data presentation. With focused revisions addressing these issues, the manuscript has strong potential to advance our understanding of nutrient dynamics and phytoplankton ecology in oceanic island systems.

Thank you. We followed the suggested revisions to improve clarity, methodological transparency and data presentation, as discussed below.

SPECIFIC COMMENTS

- I recommend adding a regional chlorophyll-a map that shows the locations of all four study archipelagos to provide geographic context for readers.

We thank the reviewer for this suggestion. We added a map as the new figure 2 showing the average of monthly MODIS-Aqua chlorophyll concentration over the entire study period, overlaid with the area covered by the binned map of each studied region.

- The authors analyze nutrient concentrations across three zones: coastal IME, advected IME-affected waters, and oligotrophic waters. However, the zone definitions (lines 653-658) are currently relegated to an appendix but should be moved to the main Materials and Methods section, as they are key to the study design.

The coastal IME zone was only examined in relation to in situ data to aid with the interpretation of the strong nearshore spatial variability in surface ocean properties observed in the underway measurements and in the macronutrients and iron concentrations. Because satellite observations are impacted by bottom reflectance in shallow waters (30–100 m depth) the number of valid satellite pixels within the coastal IME category is very small. For this reason, satellite data were classified using only two zones, BO and IME.

For the inline data, the separation between IME coastal and IME advected allow us to examine the differences between the processes occurring in shallow coastal IMEs versus in open-ocean advected IMEs, in order to better identify potential sources of nutrient and trace metal enrichment.

We added this paragraph to section 2.6:

"To examine the differences between coastal IMEs and advected IMEs, we categorized all in situ observations (i.e. continuous underway measurements, macronutrient concentrations, and iron concentrations) into three groups: background ocean (BO), coastal IME, and advected IME. In situ data located within an IME contour detected from satellite observations and over bathymetry shallower than 100 m were classified as coastal IME, whereas all remaining in situ data within IME contours were classified as advected IME (see Appendix B). We limited this categorization to in situ data because satellite data in shallow areas are in general not retrieved in clear water conditions due to the impact of bottom reflectance on radiometric estimates."

And to clarify this point in the Appendix B, we added the following two sentences:

"The objective of this analysis is to assess quantitatively the differences between the shallow coastal IME and the open ocean advected IME and identify potential mechanisms involved in their development."

- The nutrient data presentation is confusing and potentially inconsistent. In Figure 3, the beige shading indicates the coastal zone, yet Figure C1 shows macronutrient and iron concentrations for Fiji's coastal IME that do not appear in the transect in Figure 3. Furthermore, the "Other IME" detected around May 29, 5 PM, appears to correspond to a different island not visible in the track shown in Figure 4. Please clarify these discrepancies and ensure consistency between figures. The authors might consider indicating the boundaries of the track shown in Fig 4 using vertical lines in Fig 3.

During the Tara Pacific expedition, the vessel remained at each island stopover for 1–4 weeks for coral sampling and was moored in the coastal zone. Macronutrients were systematically sampled at coastal stations, whereas trace metals were only sporadically sampled there. Surface ocean bio-optical properties, macronutrient concentrations, and iron concentrations in the coastal zone differed from those observed along the inbound and outbound transects (see Figs. B8 and C1). When plotted on the same time series, the variability associated with the coastal stations would dominate the signal and make the IME-related variability along the transects difficult to identify.

For this reason, we chose to focus on the data recorded along the inbound and outbound transects in Figs. 4 and 5 (formerly Figs. 3 and 4) and in Figs. B2–B7 (formerly Figs. B1–B6), that are on gradients between islands and the open ocean (as opposed to temporal variations within a lagoon/coastal zone), which are the main focus of this study. Nutrients and iron concentration data from the coastal zones are shown separately in Figs. B8 and C1 and are also available in the publicly accessible databases.

Similarly, the zoomed maps of the inbound and outbound transects (Figs. B3, B5, and B7) do not display the full spatial extent shown in the corresponding time-series plots (Figs. B2, B4, and B6), as they are intended to highlight the spatial variability detected at the transition from BO to IME in the inbound and outbound transects. In response to the reviewer's suggestion to improve readability and consistency between the time-series plots and the zoomed maps, we added red vertical lines in the time-series figures to indicate the start and end of the transect sections shown in the corresponding zoomed maps.

The "Other IME" detected around May 29, 5 PM corresponds to an IME patch that was associated with a submerged seamount (located at 15°39'38.2"S, 175°51'59.8"E) that was detected along the inbound transect to Fiji. We added the following sentences in section 3.1 to describe this observation:

"The inbound transect to Fiji crossed an IME episode associated with a submerged seamount north of Fiji that is not connected to the island shelf (15°39′38.2″S, 175°51′59.8″E; the IME is visible in the inbound panel of Fig. 4 but out of boundary in the zoomed maps in Fig. 5). This IME episode was also characterized by a synchronized decrease in [PPC]/[Chla] and an increase in [Chla]/[C_{phyto}], consistent with the entrainment of low–light-adapted phytoplankton into the surface layer. This pattern suggests enhanced vertical mixing around the seamount, likely driven by the interaction between ambient currents and the seamount topography."

- The definition of upwelling-affected zones based solely on surface variables requires clarification. While the authors note that Fiji's shallower nutricline should produce a clearer upwelling signal, Figure 3 shows increased dissolved iron in the IME/upwelling region but not for DIN or phosphate. Given the spatial proximity of iron and macronutrient sampling stations (both seemingly influenced by upwelling) the differential nutrient response warrants discussion. Consider addressing potential mechanisms that may explain these patterns.

We define the upwelling-affected zones based on spatial variation in SST, SST and two independent phytoplankton physiological indices representing their light acclimation, along the inbound and outbound transects. We use phytoplankton light acclimation as an indicator of entrainment of sub-surface, low-light adapted cells to the surface, often associated with upwelling. We have replaced the following sentence in section 3.1 to emphasize this point:

"~~Together, these synchronized trends observed with four independent measurements are consistent with the presence of an upwelling event that occurred in a $\sim$170 km wide band near the coast west of Viti Levu at the time of sampling."~~

with

"Together, these synchronized trends observed with four independent measurements suggest an upward entrainment of water parcels and low-light adapted phytoplankton cells, an observation that is consistent with the occurrence of an upwelling event. Based on the spatial variability of these parameters, we estimate that this upwelling occurred in a ~170 km wide band adjacent to the island shelf west of Viti Levu at the time of sampling."

We do not rely on nutrient and trace metal concentrations to define the upwelling-affected zones because it can be misleading since nutrients and trace metals can be rapidly consumed and we did not measure nutrient fluxes. Once the upwelling-affected zone is defined, we simply analyze the spatial variation of macronutrients and iron stresses, which we argue are better indicative of new input than concentrations (since concentration are not indicative of the actual flux), to see if variations in stress indices coincide with the defined upwelling zone. A decrease in nutrient or iron stress in an upwelling zone is suggestive of a new input in nutrients or trace metals. In addition, the low spatial resolution of our nutrient and trace metal sampling do not capture the high spatial variability between the upwelling affected zone and the island shelf zone while stress indices, that are estimated from satellite data, better capture this spatial variability. As explained in section 3.2 the lowest iron stress was observed closest to shore while the lowest nutrient stress was observed in the upwelling-affected zone which suggests that they have different input mechanisms and sources.

We added the following sentences to section 3.2 to clarify this strategy:

"Differences in spatial variability between macronutrient or iron concentrations and their respective stress indices can result from two (or more) factors. (1) Macronutrient and iron concentrations reflect standing stocks, whereas the stress indices are more indicative of the macronutrient and iron fluxes experienced by cells. In other words, although high macronutrient and trace metal concentrations likely imply increased supply, low concentrations do not necessarily imply limiting flux. (2) The spatial resolution of sampling differs between measurement types: the macronutrient and iron stress indices (respectively η' and ϕ_{sat}) are retrieved at 1 km^2 resolution, while discrete sampling of macronutrients and iron were collected at a much coarser spatial resolution (on average every 290 km). Despite these differences, the combined use of both concentrations and stress indices is informative because disparity between them is suggestive of rapid uptake of macronutrients or iron by local phytoplankton populations."

- I suggest relocating Table 4 to the Results and Discussion section and converted from qualitative to quantitative format. Specifically, include metrics such as: magnitude of biomass increases, relative intensity of upwelling effects between Society Islands and Fiji, extent of iron enrichment, and other quantifiable comparisons that would strengthen the analysis. The current color coding effectively highlights different IME characteristics between islands and should be retained.

Table 4 was supposed to be in the last paragraph of the Results and Discussion, but the LaTeX formatting moved it to the conclusion automatically. We will ensure it goes back to the Results and Discussion in the final formatting.

Table 4 is intended to offer a 'digestible' summary of the observed patterns. We understand the interest of showing quantitative results however the amplitude of change of a given parameter associated with an IME is quantifiable relative to its associated BO value resulting in positive IME – BO differences for some parameter such as biomass, but can also resulting in negative IME – BO differences for stress indices such as η' and ϕ_{sat}, which can create confusion. We would like to keep this table as simple as possible so it can serve its purpose. However, we added quantitative metrics for the satellite variable observed in the form of a new figure in the supplementary material (figure B1) showing the distribution of each satellite variable studied and displayed in the PCA (figure 3 – formerly figure 2) for BO and IME zones. We added a short introductory text in Appendix B describing the analysis:

We aggregated satellite data from all IME and BO pixels of each studied archipelago across the 6-month time series to quantify differences between IME and BO regions and compare these differences among archipelagos. We assessed normality independently for each category using a Lilliefors test. When all categories satisfied the normality assumption, differences were evaluated using a parametric analysis of variance (ANOVA); otherwise, a non-parametric Kruskal–Wallis test was applied. Because each violin plot represents a very large number of observations (i.e. between 2.5 and 12 million data points), statistically significant differences were detected for all variables between IME and BO regions; however, the magnitude of these differences varies among parameters and regions and should be interpreted accordingly.

This new figure shows that over the 6-month period, the macronutrient stress was in fact lower in the Rapa Nui's IME relative to BO. We modified Table 4 accordingly and adjusted the results and discussion section

- Please carefully review all references to figures and appendix materials to ensure they are accurate and consistent throughout the manuscript (see examples in "Technical corrections").

We have reviewed all figure and appendix references.

TECHNICAL CORRECTIONS

Line 1: Remove "([Chla])" because the acronym is not used later in the abstract.

"~~([Chla])~~" was removed

Line 3: The IME acronym is already defined in line 1; remove the redundant definition.

"~~IME~~" was removed

Lines 4–7: Consider combining the two sentences, for example: "Here, we study IMEs associated with four South Pacific subtropical archipelagos over six-month periods. We use a combination of satellite-derived physiological indices and in situ bio-optical data collected during the Tara Pacific expedition (year) to further our mechanistic understanding of IME."

The two sentences were combined as suggested.

Line 4: Replace "four different island groups" with "four South Pacific subtropical archipelagos."

"~~four different island groups~~" was replaced with "four South Pacific subtropical archipelagos"

Line 6: Italicize in situ here and throughout the manuscript.

The manuscript composition guidelines of Biogeosciences specifically requires to not italicize common Latin phrases (including "in situ").

Lines 6–7: Insert the year of the Tara Pacific expedition.

The years were inserted

Line 8: Replace "on average" with "typically"

"~~on average~~" was replaced with "typically"

Line 10: Replace "in some cases" with "In specific regions"

"~~in some cases~~" was replaced with "In specific regions"

Line 15: Change "indicates" to "indicate."

"~~indicates~~" was changed to "indicate"

Line 36: Provide a reference for the statement "variations in [Chla] can result from changes in phytoplankton biomass, community composition."

The reference Langdon, 1988 was added to this statement and Geider, 1987 was added to the following reference.

Line 37: Define the SST acronym at first use (it was not defined in line 24).

SST was defined formerly line 24

Line 43: The phrase "or its inverse" is unclear. Since Table 3 uses the Chla/Cphyto ratio, define that ratio directly.

The suggested change was made

Lines 54–57: Split this sentence into two to improve clarity.

We replaced the sentence "~~We integrate high-resolution in situ bio-optical proxies with satellite data to assess covariance between nutrient stress responses, increases in biomass, and potential changes in community composition to investigate the type of nutrient enrichments associated with IME, their origin, and their ecological consequences on surface plankton communities~~" with "We integrate high-resolution in situ bio-optical proxies with satellite data to assess the covariance between nutrient stress responses, biomass increases, and potential shifts in phytoplankton community composition. This integrated analysis allows us to generate hypotheses related to the type and origin of nutrient enrichments associated with the IME and to evaluate their ecological consequences for surface plankton communities"

Line 59: Replace "island groups" with "archipelagos" and "latitude and longitude" with "geographical position."

The suggested change was made

Line 60: Replace "and consequently" with "which."

The suggested change was made

Lines 61–62: "Approach" is repeated; replace one instance with a synonym.

The second "~~approach~~" was replaced with "method"

Line 67: Consider adding a semicolon after "2016–2018"

A semicolon was added after "2016–2018"

Line 84: Replace "gradients of changes in community" with "gradients in community."

We decided to change "~~gradients of changes in community composition~~" to "gradients in optical properties" in response to reviewer 2 comments.

Line 97: Add the missing semicolon: "i.e., 550 nm; Chase et al., 2013."

The suggested change was made

Line 100: Remove the semicolon after "particle size."

The text following the citation was supposed to be located before. We replaced "~~(Boss et al., 2001, i.e. γ_{cp} which is inversely proportional to mean particle size;)~~" with "(i.e. γ_{cp} which is inversely proportional to mean particle size; Boss et al., 2001)"

Table 2: Remove "only" from the table title.

"~~only~~" was removed

Line 102: PAR should be "photosynthetically active radiation," not "available."

The suggested change was made

Line 104: Define the NASA SeaBASS acronym or simply refer the reader to Bourdin and Boss (2016). Also consider introducing the sentence with "Finally, we collected…".

Bourdin and Boss (2016) was referred to and "Finally" was added to the following sentence.

Line 112: "four six-month-long sequences of satellite images": are the dates identical for the 4 islands? What are they? Temporal resolution? (8-day mentioned but not until section 2.4 and mostly 2.5).

No, the dates are not identical. Each 6-month period is centered temporally on the date of in situ sampling with Tara of its respective archipelago. A reference to section 2.5 where all this information is presented was missing. It was added as: "(see section 2.5 for additional information on temporal coverage and resolution)".

Line 114: Revise citation format to "Steinmetz et al., 2011; last access: 2023-12-22" (remove the second "Steinmetz").

This citation parenthesis includes two different citations, Steinmetz et al., 2011 refers to the publication presenting the atmospheric correction while the second citation refers to the version of the software used. This second citation was requested by the production team of Biogeosciences for the publication of the "Part I" of this study (Bourdin et al. 2025). The level 2 products of this study are the same ones as the ones used in "Part 1" to compute [Chla] (Bourdin et al. 2025), therefore, for consistency and to anticipate the production team request, we have added the software citation in this study where it is needed.

Line 122: Rephrase as "Following Bourdin et al. (2025),".

The suggested change was made

Line 123: The BO acronym was not defined at first use.

The BO acronym was defined, and we refer to section 2.6 for IME and BO definition.

Line 131: At line 102, iPAR was defined as "radiation," not "radiance."

"~~instantaneous photosynthetically available radiance (iPAR)~~" was replaced by iPAR

Line170-176: to clarify. Should there be hats on the min/max values?

There are hats on min/max values however they are narrow hats only on the symbols η. To improve clarity the narrow hats were replaced with wide hats that now cover the whole symbols.

Lines 178–181: Consider merging the two sentences, as the idea that satellite estimates were calibrated with in situ measurements from the underway system is repeated.

The main purpose of the second sentence in question is to inform the reader these calibrations were performed using the relations between in situ data and the calibration dataset, to avoid possible confusions with the study dataset. We have replaced "~~SST, [Chla], and iPAR~~" with "these variables" and deleted "~~from the continuous underway system~~" to avoid repetitions.

Line 203: in section 2.6 clarify that the IME is detected on 8-day maps. Where does 2025 individual IME realizations come from? 8-day over 6 months for 728 islands+reefs (table 1) gives 16,744 realizations; if 4 islands only 92.

We added "on each 8-day median map" in the first sentence of the section 2.6 to clarify that IMEs were detected on the 8-day maps. We thank the reviewer for pointing out the inconsistency regarding the 2,025 IME realizations implying that section 2.6 did not clearly present the method. The 2,025 IME realizations represent all the detected IME events associated with the island groups studied in all 4 regions. The groups of islands studied in each region include all islands that were in contact at least once (over the 6-month period studied) with the IME associated with the main archipelago/island studied (i.e. Rapa Nui, Society Islands, Samoa, and Fiji-Tonga). Therefore, the studied island groups would neither include all islands of each domain nor just the 4 main islands. We added the following sentences to clarify this point in section 2.6: "We focused our analysis on archipelagos located near the center of each study region. We excluded IMEs detected near regional domain boundaries from the analysis because one of the stopping criteria of the detection algorithm could be triggered prematurely near the domain boundaries, thus underestimating the area of that IME (Bourdin et al., 2025). Large IME patches—such as those associated with the Society Islands, Samoa, and Fiji—frequently stem from the combined effects of multiple islands each of which may generate its own local IME superimposed on the broader signal of the main island (Bourdin et al., 2025). These large IMEs commonly encompass numerous smaller islands, each of which may generate its own local IME superimposed on the broader signal of the main island. To account for this, we identified all islands that were included at least once within the IME associated with each main archipelago during the analyzed 6-month period and selected all IME events linked to this set of islands for each region. Using this approach, we detected a total of 2,025 individual IME realizations associated with the four studied archipelagos over the six-month study periods."

And adjusted the following sentence accordingly.

Line 215: Replace "along" with "during."

The suggested change was made

Line 228: The difference in centroids appears to indicate only that the IME is stronger; the connection to iron is unclear.

We have rephrased the sentence in question to clarify this point:

In each region studied, the difference between the IME and BO centroids is aligned with the vector of ϕ_{sat} and is larger in the western basin (i.e. Samoa, and Fiji and Tonga case studies) than in the eastern basin of the SPSG (i.e. Rapa Nui and Society Islands case studies; Fig. 2), suggesting that iron enrichment in IME zones is higher in the western Pacific compared to the eastern basin.

To clarify the impact of IMEs on surface ocean properties detected from satellite observations, we added a figure to the Supplementary Material showing violin plots that contrast each parameter included in the Fig. 2 PCA between IME and BO zones (Fig. B1).

Lines 242–243: Figure 2 does not distinguish which Rapa Nui IME points correspond to austral summer versus winter; please clarify.

We added two statements between parenthesis "(i.e. circles located at the top part of the scatter plots)" referring to the austral winter, and "(i.e. circles located in the middle of the scatter plot)" referring to the austral summer.

Line 246: The statement that all IMEs except Rapa Nui's show moderate increases in bbp443 and thus biomass is not visible to me; please verify. I think some of the Fig 2 conclusions are overblown like this one (even if corrected) or "characterized by reduced n'" etc. Fig 2 is only the first 2 components so "suggests" this but doesn't "show" it. To check if this is true overall the authors should need not a PCA retaining only the first 2 modes, but box plots contrasting IME/BO for n' or bbp433 for instance.

As suggested, we added a figure to the Supplementary Material (Fig. B1) showing violin plots that contrast each parameter included in the Fig. 2 PCA between IME and BO zones. This new figure supports more clearly the interpretation of the results and highlights the statistical significance in surface properties between IME and BO zones in the studied regions and periods.

Line 257: Specify "High concentrations of what?"

"~~high concentrations~~" was replaced with "zones of higher biomass, macronutrient, and iron concentrations"

Line 262: Instead of directing the reader to Gardner et al., 2006; Behrenfeld and Boss, 2006, please reference Table 2, where descriptions and citations are already included.

We replaced "~~(i.e. proxy for particulate organic carbon concentration; Gardner et al., 2006; Behrenfeld and Boss, 2006)~~" with "(i.e. proxy for particulate organic carbon concentration; see Table 2)"

Lines 264-265: should not be a decrease in SST and SSS but decrease in SST and increase in SSS.

Indeed, the sentence was modified accordingly.

Line 264: Rephrase as "Fig. 6 in Bourdin et al. (2025)."

The reference was rephrased as suggested.

Line 265: A change of 0.2 °C and 0.1 PSU may be insufficient evidence for two distinct water masses; reconsider this claim.

We replaced "~~change of water mass~~" with "change in seawater physical properties" and "physical properties" was added to the following sentence.

Line 291: Consider rephrasing as "2025); however, bbp was not due…".

The end the sentence in question was rephrased to increase clarity: "however, bbp was not adjusted because of the low correlation between in situ and satellite data (see Method 2.5)."

Line 306: Clarify what "subsurface population" refers to—subsurface phytoplankton?

"phytoplankton" was added: "subsurface phytoplankton population"

Lines 311–317: The text first states that upwelling is the main macronutrient source west of Fiji, then that terrigenous inputs are significant. Consider revising if both sources are important. The fact that N and P are higher at coastal regions does not indicate that terrigenous inputs are a significant source, or this should be better explained (upwelling occurs a little further offshore perhaps?)

We revisited this sentence to avoid confusion. The new sentence is:

"*η'* was also consistently lower in the IME zone relative to the BO, with lower values detected as far as ~150 km offshore on the outbound transect"

Line 319: Figure B1 corresponds to Rapa Nui, not Fiji.

We corrected the figure reference to reflect Fiji's

Line 328: Figure B5 corresponds to Samoa, not Fiji.

We corrected the figure reference to reflect Fiji's

Lines 351–353: The longitudinal gradient in total iron concentration is stated to be shown in "Fig. 3, B1, B3, and B5," but this pattern is only clearly visible in Fig. C1. Please correct the figure reference.

The figure reference was corrected

Lines 377–380 and Fig. D1: Comparisons between zones are difficult because the y-axes differ between panels; consider standardizing axes.

The introduction sentence of this paragraph was inaccurate. This paragraph is meant to show patterns in each region relative to their respective BO zones. We added "when compared to their respective BO zones" to the end of the first sentence to improve clarity. Because the objective of Figure D1 is to compare the difference between IME and BO properties between regions we want to maximize the range of variation on the y-axis to highlight the difference between IME and BO in each region study, therefore, we think it is important to keep the relevant range for each region.

Lines 380–389: Appendix D contains only Figure D1; refer directly to the figure rather than "the figure and the appendix."

We modified the references to only refer to the figure.

Lines 390–405: Figure 5 is referenced seven times in 15 lines; consolidate where possible.

This paragraph was revised to refer to the figure only once at the beginning of the paragraph and to improve flow.

Line 415: The authors state that delta phy sat (between IME and BO) decreased over time and approached 0, but line 421 says that phy sat was significantly lower in IME relative to BO even during the bloom demise. Unless I'm missing something, this is contradictory.

We understand why these two statements may appear contradictory, and we appreciate the reviewer's comment. This issue illustrates a limitation of presenting results as differences between IME and BO properties, which can indeed be confusing (see also our response to Specific Comment 5 regarding the summary table). In this case, $|\Delta\phi_{sat}|$ decreased toward zero but remained sufficiently large relative to its uncertainty to be significantly lower in the IME than in the BO (i.e. $\Delta\phi_{sat} > SEM^{f}\Delta\phi_{sat}$). In other words, $\Delta\phi_{sat}$ increases but $|\Delta\phi_{sat}|$ (which represents how ϕ_{sa} is different between the IME and BO) decreases. We have clarified this point in the revised text:

We replaced "~~Given that the difference in Φ_{Sat} between the IME and BO decreased (i.e. $\Delta\phi_{sat}$ approached 0), the net iron enrichment associated with the IME relative to the BO decreased over this period despite an increase in iron enrichment in the IME. Therefore, the iron stress in the BO decreased more than the iron stress in the IME~~

~~over the period studied, suggesting that the processes controlling phytoplankton iron stress in the IME had less seasonal variability than those in the BO~~" with "The difference in ϕ_{sat} between the IME and BO (i.e. $\Delta\phi_{sat}$) remained statistically significant ($|\Delta\phi_{sat}| > SEM^{f}_{\Delta\phi_{sat}}$), indicating persistent iron enrichment within the IME associated with Fiji–Tonga. However, $|\Delta\phi_{sat}|$ decreased over the six-month period, approaching but not reaching zero, implying that Φ_{Sat} values in the IME became more similar to those in the BO. Because Φ_{Sat} within the IME also decreased over this period, this trend suggests that Φ_{Sat} decreased more strongly in the BO than in the IME. This pattern is consistent with an increase in iron availability in the BO, which reduced the contrast in Φ_{Sat} between the IME and BO. These results indicate the presence of seasonal variability in iron stress within the BO and suggest that the processes controlling this variability differ from those governing iron enrichment within the IME".

Line 424: Consider referring readers to Figure E3 here.

A reference to Figure E3 was added

Line 443: Delete ", where Σ(30 m isobath area) is plotted against IMEarea" and simply reference Fig. 7A.

The suggested change was made.

Lines 445–449: Rephrase for clarity. Suggested: "The Σ(30 m isobath area) effectively predicts the potential minimum IME area using a second-order polynomial model (dashed blue line in Fig. 7A), whereas island size alone is an unreliable predictor due to large variability in IME area for a given Σ(30 m isobath area)." Consider adding the equation directly to Figure 7.

We replaced "~~The Σ(30 m isobath area) is a good predictor of the potential minimum IME area using a second-order polynomial model (i.e. dashed blue line in Fig. 8.A):~~" with "The Σ(30 m isobath area) effectively predicts the potential minimum IME area using a second-order polynomial model (dashed blue line in Fig. 8A):" and replaced "~~Island size alone is not a good predictor of the strength and extent of the IME given the large range of IME area for a given Σ(30 m isobath area) (Fig. 8).~~" with "However, it is insufficient to accurately predict the mean strength and spatial extent of the IME, as evidenced by the large variability in IME area observed for a given Σ(30 m isobath area) (Fig. 8.A)." We retained the equation in the text rather than in the figure because its length prevented it from fitting within the figure width while maintaining a readable font size.

Lines 450–451: The word "enrichment" is repeated three times; revise to avoid repetition.

This sentence was reworked to avoid repetition of the word "enrichment"

Lines 483–487: This sentence is too long; divide it into two for clarity.

This sentence was divided in two and slightly reworked to improve clarity.

Line 488: using islands as natural nutrient enrichments that "could be studied", this has been done quite a bit already. Cf PlumeEx for Galapagos (eg https://doi.org/10.1016/S0967-0645(98)00019-8) or KEOPS for the Kerguelen (eg https://doi.org/10.1016/j.dsr2.2008.01.002).

> This sentence was modified to acknowledge previous studies of natural nutrient enrichment associated with IME and include the two citations suggested.

FIGURES AND TABLES:

Table 1: Some percentages in pie charts are unreadable, please consider showing only major components. In the caption, consider rephrasing "Island geomorphic types were determined following Nunn et al. (2016)."

> The percentages <= 5% were hidden and caption was modified as suggested

Table 2: Remove "only" from the caption.

> "~~only~~" was deleted from the caption

All tables: Consider bolding the first row to highlight column headers.

> The column headers were bolded across all tables

Figure 4: The authors should consider keeping the same colorbars left and right where possible (ie except for SST where values are too different between south and northwest of Fiji). Being able to compare both sides of the island would be valuable.

> We have considered using the same colorbar ranges on Figure 5 (formerly Figure 4), Figure B3 (formerly Figure B2), Figure B5 (formerly Figure B4), and Figure B7 (formerly Figure B6) and tried it before the initial submission. After comparison of the maps with the same colorbar and without we decided to keep the different colorbar to maximize the contrast between the IME and BO zones for each transect (inbound and outbound) to highlight the spatial variability within each transect more than compare the inbound and outbound transects. Alternatively, the inbound and outbound transects can be compared in Figure 4, B2, B4, and B6 (formerly Figure 3, B1, B3, and B5) in which the inbound and outbound transect share the same y-axis.

Figure 6: Define in the caption what the shaded area represents. Same for Figures E1-E3

> The shaded areas were defined in Figure 6, E1, E2, and E3.

Several figures: would be good to remind the reader what variables represent in the caption. This is done well in Fig. 3 but less in Fig. 4 (remind that quantum yield is iron stress) and not at all in Fig. 5 (eg "Chl normalized by Chl a", add "indicative of red algae" etc).

> The missing variable descriptions were added to the figures.

Figures B7 and D1: Please clarify what the asterisks represent. Consider adding this information to the figure legend or caption.

The asterisks description was added in the caption.

Figure C1: The error bars are not defined. Please specify what they represent (e.g., standard deviation, standard error, 95% confidence intervals).

The error bars represent the standard error, and it was added to the caption.

General recommendation: Ensure all figures include complete information needed for interpretation, including clear definitions of symbols, error bars, statistical indicators, and any other notation used.

We reviewed all figures and their captions to make sure they include all the information needed.

At last, we want to thank the reviewer again for the thorough review and hope that both the reviewer and editor agree that the above revisions significantly improved the clarity of the paper.

End of review

Response to reviewer #2:

We thank reviewer #2 for raising important points regarding satellite data processing and the use of pigment ratio as indicator of phytoplankton community composition. All comments have been addressed or answered as described below. Reviewer's comments are copied below and have been italicized, our responses are indented and in normal black font, citation from the manuscript are in blue font, ~~strikethrough~~ text was deleted, and new sentences that were added in the revised version, in response to reviewer comments, are shown in green font text.

The manuscript investigated the physiological responses of phytoplankton assemblages by inland mass effects (IME) utilizing both satellite and in situ data. The paper presented a comprehensive and valuable dataset collected during the Tara Pacific expedition and evaluated the impact of IME on physiological responses primarily through satellite analysis. This paper offers valuable insights into IME, contributing to a better understanding of the coastal marine ecosystem. However, I have the following concerns that the authors should address:

Major comment No.1

It seems to me that this study did not sufficiently evaluate changes in phytoplankton community composition. While pigment ratios are indeed investigated through HPLC analysis, such as the ratio of photo-protective carotenoid concentrations or photosynthetic carotenoid concentrations relative to chlorophyll a concentrations, this analysis does not constitute a comprehensive evaluation of phytoplankton community composition. These ratios fluctuate in response to light, nutrient, and iron availability, even in the absence of significant changes in the phytoplankton community itself. Consequently, the sentences representing phytoplankton community composition in the Abstract of Line 5 and the Section 3.3 Characteristics of phytoplankton communities from bio-optical signals are not sound in their current form.

We agree with the reviewer that optical measurements do not provide a comprehensive assessment of phytoplankton community composition. While pigment ratios provide limited information on phytoplankton community composition and are subject to biases, there is a big body of literature using HPLC pigments for this purpose (e.g. Kramer et al., 2024). We adjusted the wording throughout the manuscript to clarify that the community composition was only investigated through pigment ratio analysis. Changes in phytoplankton composition were not the central objective of this paper, which focus on physiological stress indicators, and was only used in this study to complement the interpretation of the detected nutrient dynamics.

To clarify that we are investigating pigments ratios and not directly community composition we added the following sentence in section 2.2:

"Phycoerythrin concentrations were not measured as part of the HPLC analysis, thus we estimated the relative concentrations of phycoerythrin using the ratio of the Gaussian absorption function centered on the phycoerythrin absorption peak (i.e. 550 nm; Chase et al., 2013) to the Gaussian absorption function centered on 676 nm, the [Chla] absorption peaks (i.e. agauss550/agauss676). Although not a central objective of this study, we tracked variations in ratios of these accessory pigments to [Chla] (e.g. [PSC]/[Chla]; Table 2) as a crude indicator of changes in phytoplankton community composition. To quantify changes in the mean size of suspended particles, we computed the slope exponent …"

Furthermore, we added a sentence at the end of section 3.3 stating; "While pigment ratios provide limited information on phytoplankton community composition and are subject to biases, this analysis points to possible changes in community composition in association with the IME, a hypothesis that will need to be confirmed with more direct methods (e.g., imaging and metabarcoding), but beyond the scope of this study."

Finally, we revised the relevant sentence in the abstract as follows:

We also show that IME is often associated with changes in pigment ratios, which is suggestive of ~~indicates~~ changes in phytoplankton community composition.

Major comment No.2

The rationale behind utilizing POLYMER for atmospheric correction in this study remains unclear. In recent years, other studies have demonstrated that OC-SMART (Fan et al., 2012) offers superior atmospheric correction in optically complex coastal waters. Therefore, it would be a good idea to explain the specific reasons that led to the selection of POLYMER as the atmospheric correction method.

We chose to keep the technical details regarding the satellite data processing method to a minimum because we followed the same strategy as for [Chla] in the first part of this study (Bourdin et al. 2025), and because this study is already quite detailed and long. The rationale behind utilizing POLYMER is explained in Bourdin et al. (2025). In short, the IME detection algorithm used in this study—and developed in Bourdin et al. (2025)—relies on gap-less maps of [Chla] to function properly. The objective is to reduce the binning time to a minimum while still recovering smooth and gap-less binned products. Therefore, we needed to maximize data recovery to ensure enough realizations for each pixel over the binned period for the median to produce smooth maps.

We used the atmospheric correction POLYMER because it has been reported to retrieve high-quality data in areas impacted by sun glint and the adjacency effect (i.e., nearshore and around clouds), which can greatly impact tropical and subtropical regions. The choice of POLYMER versus l2gen to retrieve [Chla] maps was evaluated against *in situ* HPLC data and described in Bourdin et al. (2025); therefore, it was also applied in this study for consistency. OC-SMART would probably have been a good alternative, especially for its advantages in optically complex waters. Nevertheless, optically complex waters are not very common in the studied regions. The turbidity and CDOM concentrations are generally low compared to coastal ecosystems of continents or higher-latitude regions. Moreover, pixels with bathymetry shallower than 30 m (plus one extra 1 km pixel) were discarded in this analysis to avoid the potential impact of bottom reflectance on the retrieved parameters. This further limit the potential occurrence of optically complex waters and reduces the benefit of using OC-SMART over POLYMER in this study.

The focus of this Part 2 study is to demonstrate the potential of satellite data for understanding the physiological stress of phytoplankton relative to macronutrient and iron limitations, in order to improve our understanding of the different enrichment processes involved in IME formation. Therefore, we did not further test any atmospheric corrections other than those tested in Part 1 (POLYMER and l2gen) and kept the method consistent between Parts 1 and 2 of the study.

Major comment No.3

In this study, ocean color sensors from MODIS/Aqua, MODIS/Terra, VIIRS/Suomi-NPP, VIIRS/JSPP, and OLCI/Sentinel3A&B were utilized. However, each sensor possesses a distinct resolution. MODIS, VIIRS, and OLCI have resolutions of 1 km, 750 m, and 300 m, respectively.

As explained in Bourdin et al. (2025), we adopted the spatial resolution of the satellite with the coarsest resolution among those merged: MODIS, at 1 km. We downloaded Level 1 data for OLCI, VIIRS, and MODIS at this 1 km resolution and produced corresponding Level 2 (L2) products.

A mention of the spatial resolution for these derived Level 2 products has been added to Section 2.3.

"We applied a polynomial-based atmospheric correction (POLYMER version v4.17beta2; Steinmetz et al., 2011; Steinmetz, last access: 2023-12-22) on both datasets to compute 1-km spatial resolution L2 remote sensing reflectance data (Rrs) using ancillary data from the European Centre for Medium-Range Weather Forecasts reanalysis model version 5 (i.e. ERA5)."

For the match-up analysis, it is unclear which relation was evaluated. Were the 1 km resolution data after merging evaluated? If so, the process of merging satellite data to achieve a 1 km resolution is not explicitly detailed in the provided information.

The matchups were performed between in situ underway data and level-2 satellite data of each satellite processed.

We added "Level-2" at the beginning of the section 2.5:

"Level-2 satellite estimates of SST, [Chla], and iPAR (different from the daily PAR used in η' computation) were individually calibrated against in situ measurements obtained from the underway system of the ship to minimize inter-sensor variability and biases."

We added a statement in Appendix A2 to clarify this point:

"Match-ups between the calibrated in situ data collected from the underway system and level-2 satellite data of each satellite processed were performed following recommendations from Bailey and Werdell, 2006."

The Level-2 1 km spatial resolution products are evaluated and calibrated against *in situ* data; however, the merged products themselves are not evaluated. Section 2.5 details the adjustment procedure for each Level-2 satellite product and the 8-day median calculation used to generate the binned/merged products.

We have kept the technical details regarding the merging method to a minimum, as the methodology is already described in detail in Part 1 of this study (Bourdin et al. 2025). This approach was chosen to maintain focus, as the subsequent analysis is highly technical and the study is already quite extensive. More detailed explanations of the merging method and the rationale behind these choices are available in Bourdin et al. (2025).

Additionally, only OLCI and VIIRS were employed for bbp analysis. Given the high-resolution requirements in coastal regions, only OLCI, which is equipped with fluorescence sensors, was deemed suitable for this purpose.

The 1-km spatial resolution products we computed offered a reasonable compromise of spatial resolution and quantity of data to process to retrieve to 6-month long detections of IME in the 4 studied regions.

Our method already significantly improved the detection compared to the previous studies that relied on 4-km spatial resolution products and yearly averages. Moreover, our analysis relies on having the same spatial resolution between all studied parameters, therefore, we used the spatial resolution of the satellite with the coarsest spatial resolution of all the merged satellites, 1-km.

To enhance the clarity of the study, it would be beneficial to include a diagram of chlorophyll in the Appendix's Figure A3 because the accuracy of chlorophyll propagates to subsequent parameter estimation. Additionally, it is recommended to cite Brewin et al. (2015) for statistical analysis.

We added a panel (c) to Figure A3 showing the regressions of [Chla] estimated from the different satellite data versus calibrated [Chla] estimated from the underway system. We adjusted the caption and reference in the text and added "The statistics of these match-up relations were subsequently used to select the atmospheric correction and [Chla] algorithm performing best in our case (Brewin et al., 2015) and the normalized root mean square errors (*nRMSE*) were used as uncertainty estimates and were propagated to subsequent products (see comparison in Appendix B in Bourdin et al., 2025, and uncertainty propagation in Appendix A4 of this study)." to Appendix A2 which includes the citation suggested.

At last, we want to thank the reviewer again for the important points raised and hope that both the reviewer and editor agree that the above revisions and response address these points and improved the clarity of the paper.

References:

Bourdin, G., Karp-Boss, L., Lombard, F., Gorsky, G., and Boss, E.: Dynamics of the Island Mass Effect – Part 1: Detecting the Extent, Biogeosciences, 22, 3207–3233, https://doi.org/10.5194/bg-22-3207-2025, 2025.

Kramer, S. J. and Siegel, D. A.: How Can Phytoplankton Pigments Be Best Used to Characterize Surface Ocean Phytoplankton Groups for Ocean Color Remote Sensing Algorithms?, Journal of Geophysical Research: Oceans, 124, 7557–7574, https://doi.org/10.1029/2019JC015604, 2019.

End of review