Dear Editor Emilio Marañón,

we are pleased to resubmit for publication the second revised version of manuscript entitled "**Response of phytoplankton communities to the onset of the 2020 summer marine heatwave in the Drake Passage and Antarctic Peninsula.**".

We sincerely appreciate the constructive criticisms and the time dedicated to revise our manuscript by the editor and anonymous reviewers. Overall, we have included new statistical analyses in the manuscript (CCA and Wilcoxon tests), re-structured the discussion and included all the comments suggested by the reviewers. We have carefully considered all their comments and have addressed each of their concerns as outlined below. Please note that reviewer´s comments are in **bold,** and our responses in *italics*.

REVIEWER 1

R1C1: *Manuscript Title: Response of phytoplankton communities to the onset of the 2020 summer marine heatwave in the Drake Passage and Antarctic Peninsula*

General Comments

This manuscript provides important insight into phytoplankton community responses to the 2020 summer marine heatwave in a climatically sensitive region of the Southern Ocean. The study combines in situ hydrographic, pigment, and microscopic data with satellite observations to document compositional changes in phytoplankton, notably a shift to smaller phytoplankton.

This is a timely and relevant contribution, especially in light of the increasing frequency and intensity of marine heatwaves due to climate change. However, while the manuscript presents a valuable dataset and is generally well-written, the current version lacks a strong analytical framework, remains descriptive in tone, and would benefit from a more focused hypothesis and clearer ecological interpretations. Integration of pigment and microscopy data is also limited. I believe the paper can be significantly improved with revisions and recommend publication after addressing the comments below.

Authors wish to thank reviewer 1 for his/her valuable time dedicated to review the manuscript and for the positive and constructive comments that greatly help to improve the manuscript. Reviewer comments to authors are listed below in italics and the responses to reviewers are in bold.

R1C2: Abstract. *Add specific quantitative outcomes (e.g., % changes in phytoplankton groups, temperature anomalies etc).*

Corrected according to reviewer's suggestion. In the new version the most relevant findings obtained with the new statistical analyses incorporated in the paper will be included. Please note that quantitative data of the temperature anomaly was already provided in the manuscript.

R1C3: Abstract. *Clarify the main ecological implication of observed shifts - how do they influence productivity or carbon export?*

Corrected according to reviewer suggestion. In the new version of the manuscript the comparisons between algal biomass accumulation (derived from fluorescence), net

primary production (modelled data) and diatom abundance before and during the MHW are provided.

R1C3: Introduction. *The introduction provides a good overview of the context surrounding marine heatwaves and their ecological importance, particularly in polar systems.*

The authors should clarify the rationale behind combining satellite data with in situ pigment and taxonomic data and articulate the research question or hypothesis more clearly. This would provide stronger direction for the reader and better frame the significance of the study's findings.

Corrected according to reviewer 1's suggestion. A more detailed explanation of use of environmental parameters measured during the campaign is presented at the end of the introduction. Moreover, a rationale for merging *in situ* and satellite-derived information is also provided.

R1C4: Methods. *While the general methodological framework is outlined, important details are missing or need clarification.*

The pigment analysis section should specify which marker pigments were used to identify different phytoplankton groups, along with a reference to the methodology.

We believe there has been a misunderstanding in this point. Phytoplankton marker pigments associated to different functional groups were not measured in the present study. Only fluorescence was measured *in situ* from the water samples retrieved from the water intake of the research vessel. This information was combined with satellite-derived chlorophyll-*a* concentration in the first version of the manuscript. These are the only two pigment-related parameters presented in the manuscript. Identification of both diatoms and coccolithophores was performed using microscopy techniques as stated in section "2.4 Phytoplankton analysis" (lines 317-351 of the original version of the manuscript). Detailed explanations on the taxonomic identification of these groups are provided in this section. However, in the new version of the manuscript, in order to avoid the use of two proxies for a similar parameter (algal biomass accumulation), instead of comparing *in-situ* fluorescence with satellite-derived chlorophyll-a concentration, we have replaced chlorophyll-*a* by Net Primary Production (NPP). Net Primary Production, which takes into account not only satellite chlorophyll but also photosynthetically available radiation (PAR) for light availability, sea surface temperature (SST) as a proxy for biological rates, and length of day. We believe that this approach avoids the use two proxies for algal biomass accumulation and adds new information regarding primary production (mentioned by both reviewers) while avoiding the use of two proxies (i.e. fluorescence and satellite-derived chlorophyll-a) for the same parameter (algal biomass accumulation).

R1C5: *Similarly, the taxonomic identification protocols need further elaboration: how were small flagellates and cryptophytes distinguished under the microscope? What criteria or taxonomic references were followed?*

These groups are mentioned in the introduction with reference to previous works in the study area. These previous studies suggest that diatoms are being replaced by cryptophytes in the Southern Ocean. However, our analysis unfortunately did not

cover soft-tissue phytoplankton which is the reason why small flagellates and cryptophytes are not mentioned in the material and methods, results or discussion.

R1C6: *Additionally, the sampling design lacks clarity. The number of stations, depth profiles, replication strategy, and temporal frequency should be explicitly stated. A table summarizing each station, its coordinates, and associated environmental and biological parameters would be highly informative.*

Corrected according to reviewer's suggestion. In the new version of the manuscript an excel file (supplement file) containing detailed information of each station, including coordinates, date, time, environmental parameters and diatom and coccolithophore species abundances is presented. Please note that information about the sampling depth and sampling strategy is provided in sections 2.1, 2.3 and 2.4. For example the following text can be found in section "2.1. The POWELL-2020 campaign": "seawater samples were collected from the ship's continuous intake at 5 meters depth. During the southbound transit, surface seawater samples were collected every 3–4 hours to capture key changes across the different ACC fronts. On the return (northbound) transit, sampling was conducted every 2 hours. Within the Bransfield Strait and Powell Basin, sampling intervals were generally every approximately 4 hours, adjusted according to other ongoing research activities".

R1C7: *The statistical treatment of the data is underdeveloped. At present, community-environment relationships are not quantitatively analyzed. Application of multivariate techniques such as Principal Component Analysis (PCA), or Canonical Correspondence Analysis (CCA) would help strengthen the interpretation of community structure and its drivers.*

Corrected according to reviewer's suggestions. In the new version of the manuscript a Cannonical Correspondence Analysis (CCA) has been implemented to evaluate relationships between species composition and environmental variables. CCA is an extension of Principal Component Analysis (PCA) that incorporates external variables, allowing for the exploration of how these variables influence species distribution patterns. The CCA is presented in a new Figure in the manuscript, and it is now mentioned in material and methods, results and discussion. Moreover, the non-parametric Wilcoxon test was used to determine differences in key environmental parameters, diatom and coccolithophore cell concentrations and the relative abundance of key diatom species before and during the marine heat wave in the Drake Passage (a new figure is presented in the new version of the manuscript showing this information). For this comparison, sampling stations of the Drake Passage were organized into four groups: stations north of the Polar Front sampled before (i) and during the onset of the MHW (ii) and stations south the Polar Front before (iii) and during the onset MHW (iv). Stations within the same region before and during the MHW are compared in the new version of the manuscript.

R1C9: Results. *The authors describe shifts in phytoplankton composition and pigment concentrations across stations but do not substantiate these changes with statistical comparisons. Including statistical testing e.g., ANOVA, regression, or clustering would support the observed trends and improve scientific rigor.*

Corrected according to reviewer 1's suggestion. As mentioned in the previous answer to the previous comment, in the new version of the manuscript a Canonical

Correspondence Analysis has been implemented to evaluate the influence of the measured environmental parameters on the distribution of diatom species. As coccolithophore assemblage composition is limited to two species and their distribution is almost limited to the SAZ and PFZ, no CCA was included for coccolithophores. Additionally, the non-parametric Wilcoxon test was used to assess possible differences in key physical and chemical parameters, diatom and coccolithophore cell concentrations and in the relative abundance of key diatom species before and during the marine heat wave in two regions of the Drake Passage (north and south of the Polar Front). This information is now part of material and methods, results and discussion in the new version of the manuscript.

R1C10: *Figures could be greatly improved. Stacked bar charts illustrating group-level phytoplankton abundance by station or region would provide a clearer visualization of community shifts. Overlaying pigment concentrations on satellite SST or chlorophyll maps would also help to connect in situ and remote observations.*

Corrected according to reviewer's suggestion. As mentioned in the answer to the reviewer in the previous comment, in the revised version of the manuscript we compare cell abundances of diatom and coccolithophores (among other environmental and biological parameters) before and during the MHW in two regions of the Drake Passage (north and south of the Polar Front). This information is presented in a new figure in the manuscript, and we believe that substantially contribute to the visualization of changes in physical, chemical and biological parameters during the MHW. Please note, that we have not included more maps as we believe there are enough figures already and because the data is better reflected numerically in the box plot comparison (one of the two new figures included in the manuscript=.

R1C11: *Integrate pigment and microscopy-based data to corroborate group dominance.*

Please, note that marker pigments of specific phytoplankton groups were not assessed in the current study, and therefore we are unable to implement this suggestion in manuscript.

R1C12: Discussion. *The discussion is informative but lacks depth in its ecological interpretations. The authors should explore the mechanisms underlying the observed shift from diatoms to cryptophytes and flagellates. What biological or ecological traits allow cryptophytes to flourish during warm, stratified, possibly low-nutrient conditions? Discussion of motility, mixotrophy, or cell size could offer explanations.*

Please note that cryptophytes and flagellates are both mentioned in the introduction, but these groups were not identified in the current study. This is the reason why we did not deepen into their ecology in the discussion. However, following reviewer's suggestions, new information about the ecological affinities of cryptophytes can be found in the introduction, while the whole discussion has been rewritten and reorganized with more insights into the ecology of the targeted phytoplankton groups.

R1C13: *The authors should also contextualize their findings within the broader literature. Have similar shifts been reported during other MHWs in polar or temperate systems? Drawing comparisons with other studies would strengthen the generalizability of the results.*

We appreciate the point made by the reviewer. We have carefully considered the available literature and there is little information on the impact of marine heat waves on the phytoplankton groups addressed in the present study. However, we have included new insights in the discussion in reference to the impact of marine heat waves on Southern Ocean ecosystems, such as Behrenfeld et al. (2016) which is mentioned in section 4.3 of the discussion: "chlorophyll-*a* variations do not always reflect changes in phytoplankton concentration, particularly in Southern Ocean environments that are co-limited by nutrients and light. In these areas, phytoplankton can adjust their C:Chl-*a* cellular ratios in response to transient climatic events such as MHWs (Behrenfeld et al., 2016)." Also, Peña et al. (2019) which report responses of phytoplankton to a marine heat wave is mentioned in the discussion.

R1C14: *Importantly, the biogeochemical implications of these community changes are not discussed. How might a shift toward flagellates affect carbon export efficiency, nutrient recycling, or trophic transfer? Finally, the discussion should acknowledge the limitations of the current study, including the absence of nutrient data, limited temporal coverage, and potential sampling biases.*

Please note that our study does not allow us to determine the effect of the MHW on the biological pump with certainty as stated at the end of the abstract and in the discussion. However, we speculate about the possible effect of an increase in the number of small diatom species on higher trophic levels and on the biological pump. This information can be found in the last paragraph of the discussion. Moreover, we agree with the reviewer in that it is important to underscore the limitations of our study. Consequently, in the new version of the manuscript, we have included the following text in the last section of the discussion: "Before extrapolating our results into a broader context, it is important to acknowledge the limitations of our study, including the discrete sampling of the water column before and during the MHW. Multi-day monitoring of the water column in the areas affected by the MHW would be required to evaluate the evolution of phytoplankton communities through time (e.g. Landry et al., 2024). Also, it should be acknowledged that only surface water phytoplankton assemblages (i.e. 5 m depth) were reported in the current study but it is possible that phytoplankton communities below the surface mixed layer may have responded differently to the MHW. Moreover, our study focussed on two major phytoplankton groups, but to be able to evaluate shifts in the phytoplankton community future studies should also address other relevant algal groups, including important soft-tissue phytoplankton, such as, cryptophytes and *Phaeocystis*. Identification and quantification of chlorophyll-*a* and marker pigments of the main phytoplankton taxonomic groups would complement well microscopy-based methods providing a more robust picture of the response of phytoplankton communities to MHWs." Please note that we did not mention "absence of nutrient data" because this information is presented in the manuscript.

R1C15: Conclusion. *The need for continued monitoring and integration of ecological and biogeochemical data in a changing Southern Ocean should be included.*

Corrected according to reviewer's suggestion. The following statement has been included at the end of the Conclusions section: "While our study has provided valuable insights into the intricate relationship between Antarctic phytoplankton and environmental change, it is evident that further research is required to fully comprehend their complex response. This requires regular monitoring of key Southern Ocean regions to identify shifts in phytoplankton

composition and structure under extreme climatic conditions like MHW. Achieving this ambitious goal will require international cooperation and data sharing among nations, as no single country can accomplish it alone."

R1C16: Figures and Tables. *Several figures are of low resolution and should be revised to meet publication standards.*

We apologize for the low resolution of some figures in our manuscript, which resulted from copying and pasting images into the Word document. We assure you that the original figure files maintain optimal quality. We'll submit the original high-resolution files to the editorial office if the manuscript is accepted for publication and we will do our best to improve the quality of the figures in the reviewed version of the manuscript. We appreciate your attention to this matter and look forward to ensuring our publication meets all standards of quality and clarity.

R1C17: *A figure showing phytoplankton community composition by station, using bar plots or maps, would add clarity.*

We have carefully considered the figure suggestions of both reviewers and tried to find a balanced solution for their requests/suggestions. Please note that we have not converted our diatom and coccolithophore counts into biomass estimates, and therefore, plotting both groups together in the same graph is not possible. Also, due to the high number of stations is difficult to plot all the available data in maps. However, we believe that the new figure with the boxplots, provides a good summary of the information requested by reviewer 1 in his/her suggestion. In this figure, we present average data of key phytoplankton and environmental data collected in the Drake Passage organized into four groups: stations north of the Polar Front sampled before (i) and during the onset of the MHW (ii) and stations south the Polar Front before (iii) and during the onset MHW (iv). We believe the graphs presented in the new figure illustrate well the general trends in abundance and composition of the most relevant phytoplankton groups and species.

R1C18: *A table including station metadata, environmental variables, and phytoplankton abundances would greatly assist readers in interpreting spatial patterns.*

We completely agree with the reviewer that it is essential to make the data of the manuscript freely available for the scientific community. Please note that as stated in Data availability section (after conclusions), supplementary file with all the information per station, including phytoplankton data and environmental data, will be made available upon publication.

R1C19: *If zooplankton data were collected during the cruise, these should be reported and briefly discussed, as they could influence phytoplankton standing stocks through grazing.*

In our current study, we focused exclusively on phytoplankton and did not collect or analyze any zooplankton data from the oceanic stations under investigation. We apologize for any confusion caused by our omission of this information in the manuscript. To address the reviewer's suggestion, we have added a brief statement at the end of section "2.4 Phytoplankton analysis" to clarify that our study does not include zooplankton data and their potential influence on phytoplankton standing stocks through grazing. We appreciate the insightful comments provided by the reviewer and will consider incorporating zooplankton analysis in future studies if resources permit.

R1C20: *Language. The manuscript is well-written overall, with only a few grammatical errors.*

We appreciate the reviewer's positive feedback on our manuscript's overall quality and clarity. We have carefully reviewed the text to address the minor grammatical issues mentioned, ensuring that the writing remains clear and concise throughout the paper.

REVIEWER 2

R2C1: *Review to the manuscript entitled "Response of phytoplankton communities to the onset of the 2020 summer marine heatwave in the Drake Passage and Antarctic Peninsula" by Andrés S. Rigual-Hernández et al., submitted to EGUsphere*

General comments: I read this contribution by Andrés S. Rigual-Hernández et al. with great interest, as it addresses the important question of species-specific phytoplankton responses to marine heatwaves (MHWs) in the Southern Ocean — a region as climatically relevant as it is sensitive to climate change. The study is based on an exceptionally valuable in-situ dataset collected along the Drake Passage, one of the most critical ocean gateways on the planet and a notoriously challenging region to sample. The transect cuts across key physical, chemical, and biological gradients and fronts that structure the Southern Ocean, providing an opportunity to examine how climate forcing — in this case warming and stratification — impacts nutrient availability and phytoplankton communities across boundaries between Subantarctic, Polar, and Antarctic shelf-influenced waters. The focus on diatoms and coccolithophores, two functional groups central to food webs and biogeochemical cycles, further highlights the importance of this work.

The authors assembled a comprehensive multiparametric dataset, combining in-situ, satellite-derived, and reanalysis products, which enables exploration of the drivers of productivity and species composition in relation to hydrographic and air–sea exchange processes. I particularly value the sampling design, which captured conditions both before and during the MHW, allowing for an assessment of potential community shifts linked to warming. The manuscript is well written, and the study has notable potential to advance our understanding of diatom and coccolithophore responses to warming in the Southern Ocean, not only along the Drake Passage but also in adjacent areas such as the Northern Antarctic Peninsula.

That said, I have a few comments and suggestions for strengthening the manuscript:

The authors would like to express our gratitude to reviewer 2 for dedicating precious time in evaluating the manuscript. The provided feedback, both positive and

constructive, has significantly contributed to enhancing the overall quality of the paper. Reviewer comments to authors are listed below in italics and the responses to the reviewer are in bold.

R2C2: Baseline context*: The paper would benefit from a clearer discussion of baseline conditions in the study region, situating results within the existing literature. Explicitly distinguishing which findings align with or diverge from established knowledge would sharpen the argument for local MHW impacts.*

Corrected according to reviewer's suggestion. The discussion has been reorganised and rewritten following the recommendations of the two reviewers. In the first section of the discussion, we describe the distribution of species according to the CCA suggested by reviewer 1 and also we provide a description of the usual/background nutrient conditions, comparing our observations with previous studies (i.e. Freeman et al. 2019). Note that the usual/background environmental nutrient conditions are presented in Table 1 of the paper. Subsequently, in section 4.3, we describe the changes in physical (SSTs), chemical (nutrients) and biological (phytoplankton) properties during the MHW. Moreover, diatom and coccolithophore cell concentrations and species composition documented our study with previous studies are compared throughout the discussion. For example, comparisons of bloom intensity with previously recorded diatom concentrations in the study area are mentioned in section 4.3.: "The diatom bloom observed in the SZ and AAZ during the MHW reached cell concentrations of up to 2 x 10^5 to 1.8 x 10^6 cells L^{-1} respectively (Fig. 4a). These values were one to three orders of magnitude greater than previous reports in the same zonal systems during summer (Villafañe et al., 1995; Olguin et al., 2006; Cefarelli et al., 2010)". Comparisons of coccolithophore cell concentrations are also presented later in the same section (4.3): " Notably, coccolithophore concentrations were also substantially lower than previous reports during the austral summer in both the SAZ (23 x 10^4 coccospheres L^{-1}, Charalampopoulou et al. 2016; 15 x 10^4 coccospheres L^{-1}, Saavedra-Pellitero et al. 2019) and PFZ (58 x 10^4 coccospheres L^{-1}, Charalampopoulou et al. 2016; 11 x 10^4 coccospheres L^{-1}, Saavedra-Pellitero et al. 2019)."

R2C3: Integration of results: Rather than structuring the discussion separately as "nutrient distributions" and "phytoplankton abundances/species distribution," I recommend integrating these aspects. Both are interdependent and currently discussed across subsections, which leads to some redundancy and confusion. An integrated analysis would better highlight the mechanistic links the authors aim to establish.

Corrected according to reviewer's suggestion. Following the reviewer's suggestion the whole discussion has been reorganized. In a first section " 4.1 Environmental variability and phytoplankton species distribution in the Drake Passage and Antarctic Peninsula in summer 2020" the distribution of the two phytoplankton groups and key species is described together with the distribution of nutrients and other environmental parameters. Please note that despite differences in the relative contribution of some species before and after the MHW, their geographical distribution did not change significantly during our survey (with eh exception of *Chaetoceros* RS). In the second part of the discussion, entitled "4.2 Processes and implications on surface water properties of the 2020 marine heatwave" it is explained the origin of the MHW. In this section we have added evidence of the possible advection of *Chaetoceros* RS into the core of the Antarctic Circumpolar Current advected from the outermost zonal systems of the Southern Ocean. Lastly, in the third section, entitled: "4.3 Influence of the onset of the marine heat wave on phytoplankton communities and nutrient

distributions of the Drake Passage" the impact of the MHW on the abundance and distribution of the two phytoplankton groups and key species together with their influence on environmental parameters is addressed.

R2C4: Hypothesis clarity*: In some instances, the proposed mechanisms could be more fully elaborated to guide the reader through the reasoning. A schematic diagram could be very effective in summarizing the hypothesized processes.*

The discussion has been thoroughly reorganized and rewritten in order to add clarity following the recommendations of both reviewers. We have considered the possibility of adding an schematic diagram, however, we finding very difficult to summarize all the information provided in the discussion in a single diagram.

R2C5: Sampling depth limitation*: The exclusive use of surface (5 m) samples should be acknowledged more explicitly as a limitation. While surface-based studies are common, especially in remote regions such as the Drake Passage, phytoplankton communities below the surface mixed layer may respond differently, and this should be discussed in terms of implications for the study's conclusions.*

We appreciate the point made by the reviewer and we agree that the sampling depth is a limitation in our study. In the new version of the manuscript a detailed list of limitations of our study is presented before extrapolating our findings into a broader context in section 4.3 of the discussion.

R2C6:Warm-pool mechanism*: The argument that temperature anomalies arose from warm pools advected southward by anticyclonic eddies is compelling. However, if this mechanism is correct, why is there no evidence of concomitant advection of coccolithophore-enriched waters in the northbound compared to the southbound transit? This apparent discrepancy requires clarification.*

We appreciate the point raised by the reviewer. The lack of concomitant advection of coccolithophore assemblages into the southern Drake Passage remains puzzling. However, in the new version of the manuscript we provide new insights, such as the possible degradation of coccospheres during transport from lower latitudes (coccospheres are less resistant to degradation than *Chaetoceros* RS). These new insights have been included in new section 4.2. and read as follow: "It could be argued that the advected *Chaetoceros* RS should have been accompanied by the transport of subantarctic coccolithophores south of the Polar Front. However, it should be noted *Chaetoceros* RS are highly resistant to degradation (Rembauville et al., 2016; Rembauville et al., 2018) while coccospheres disarticulate rapidly after cell death. Therefore, it is possible that the advected waters transported a signal of subantarctic coccolithophores but in the form of detached coccoliths, which were not assessed in the current study. "

R2C7: Specific comments: *L25: replace "are" by "is".*

Corrected according to reviewer's suggestion.

R2C8: *L35: when you say: mirrored the physical and chemical properties" is not fully clear whether you refer to "background" conditions or in relation to the MHW.*

We appreciate the point raised by the reviewer. In the new version of the manuscript, it clarified that both the diatom and coccolithophore assemblages mirrored the environmental conditions before and during the marine heat wave. In the new version of the manuscript the sentence reads as follows: "…mirrored the physical and chemical properties of the water masses delineated by the Southern Ocean fronts before and during the onset of the MHW."

R2C9: L39-40: (..) abundance reaching bloom concentrations (…)"

Corrected according to reviewer's suggestion.

R2C10: *L45-46: Wouldn't surface waters advected from lower latitudes be relatively enriched in coccolithophores, even in the presence of a small diatom bloom?*

We agree with the comments in that the lack of coccolithophores transported from lower latitudes could be considered puzzling. In the new version of the manuscript, we provide a possible explanation for this feature, included in section 4.2 of the manuscript. The following explanation has been included" It could be argued that the advected *Chaetoceros* RS should be accompanied with the transport of subantarctic coccolithophores south of the Polar Front. However, it should be noted *Chaetoceros* RS are highly resistant to degradation (Rembauville et al., 2016; Rembauville et al., 2018) while coccospheres disarticulate rapidly after cell death. Therefore, it is possible that the advected waters transported a signal of subantarctic coccolithophores but in the form of detached coccoliths, which were not assessed in the current study."

R2C11: *L64-66: I would reframe to: "little information exists about the effects of marine heatwaves on phytoplankton in the Southern Ocean (…)*

Corrected according to reviewer's suggestion. The sentence has been rephrased and now it reads as follows: "However, little information exists about the effects of marine heatwaves on phytoplankton in the Southern Ocean, which represents the base of its marine food webs and regulate its biogeochemical cycles."

R2C12: *L83-84: could you expand on the reasons for this ecological shift in response to sea ice decline?*

In the new version of the manuscript, it is clarified that the cause of the ongoing shift from diatoms to cryptophytes is most likely the lower salinities resulting from meltwater events. The new sentence reads as follows: "This shift in the dominance is considered to be driven by the higher physiological tolerance of cryptophytes to lower salinity waters by melt-water events (see Moline et al., 2004 and references therein)."

R2C13: *L97-99: is the reduced mineral ballast availability the main issue about this compositional shift? Or is it also – if not mostly – about the lesser carbon sequestration by smaller-sized phytoplankton (i.e., lower carbon content)?*

This is an interesting point highlighted by the reviewer. The text has been revised explaining the main factors responsible for the weaker carbon pump. In the new version of the manuscript the text reads as follows: "This is because the organic content of particles lacking mineral ballast (such as cryptophytes) remineralizes at shallower depths than those associated with biominerals such as opal produced by diatoms. Moreover, while diatoms form fast-sinking algal aggregates and are an

important component of faecal pellets produced by zooplankton (Green et al., 1998; Smetacek et al., 2004), cryptophytes are not grazed efficiently by Antarctic krill which most likely results in a weaker carbon pump".

R2C14: *L109: delete "of" before "temperature".*

Corrected according to reviewer's suggestion.

R2C15: *L109-110: are referring to temperature as a key factor affecting the physiology of the cells, or by controlling the dynamics of the mixed layer depth?*

We appreciate the point highlighted by the reviewer. Following reviewer's suggestion, the role of temperature controlling metabolic rates is now specifically stated in the text. The new sentence reads as follows: "This information is particularly important because temperature is one of the main factors controlling phytoplankton growth by directly influencing metabolic rates (Eppley, 1972)".

R2C16: *Figure 1: You mention 51 stations for the nutrient and phytoplankton data selected for this study, but I counted only 48 in the figure. Perhaps consider changing the color of the black labels for improving its visualization.*

We appreciate for the point raised by the reviewer and apologize for the lack of clarity in this point in the manuscript. Please note that stations 13, 14, 15, 16 and 17 were taken around the same location and therefore are represented in a single spot in the map now labelled as 13-17 (please note that this station was wrongly labelled as 12-14 in the first version of the manuscript). Moreover, following reviewer's recommendations the colour of the black labels has been changed into white colour to improve visualization.

R2C17: *L352 – Results: several parts of this section read like "discussion of results", especially sub-section 3.1, which leads to some repetition along the ms. Already the in the first sentence, you start by saying that MHWs and anticyclonic eddies are related, the latter reinforcing the first, before presenting the results. I would leave this kind of interpretation for thew discussion.*

Corrected according to reviewer's suggestions. Section 3.1 has been fully revised and the interpretations have been moved to the discussion in order to avoid repetition along the manuscript.

R2C18: *L353-354: please keep just one title for sub-section 3.1; I would recommend choosing "Satellite-derived and model data".*

Corrected according to reviewer's suggestion.

R2C19: *Figure 3 – I see a station number 17 which is not in represented in Figure 1; and a station number 12 in Figure 1 which is not in represented in Figure 3. Please indicate to which parameters correspond the colors in Figure 3a. There is no easy way in representing the data but I find this figure somewhat challenging to related with Fig. 1 as well as in terms of time. Please consider representing the the two transits along the Drake Passage in a way that facilitates visualizing the changes in the region over time (i.e., before and during the MHW) and space (oriented from north to south). I would recommend using latitudes instead of the stations numbering to have a clearer persective on the meridional extent of the observed changes.*

We appreciate the all the points highlighted by the reviewer and we apologize for the typos identified. Please note that the sampling of stations 13 to 17 were different environments on the same site (Papagaya glacier, Johnson glacier and proximal sample to an iceberg). This information is now clarified in the map of Figure 1 and also in figures 3, 4 and 5. Please also note that in the new figure 3 it the colours of Figure 3a are clarified following reviewer's suggestion.

Regarding Figure 3, we agree with the reviewer that there is no easy way of plotting this data. While we have tried to divide the graphs, we believe that this format reduces clarity. However, following reviewer's guidance in the new figures 3, 4 and 5 we have included a new axis with latitude, while in figures 4 and 5, the graph of the temperature anomaly has been included following comment R2C21.

R2C20: *L460-461: By only addressing the mean fluorescence and satellite Chl-a instead of the spatial variation pattern you might be losing relevant information for discussing the imoacts of the eddy-entrapped MHWs on surface productivity. For example, the northbound transit appears to have higher fluorescence and lower satellite Chl-a compared to the southbound. These differences may hold relevant information which is not discussed in the paper.*

We thank the reviewer for these insights. In the new version of the paper, we have replaced the use of satellite chlorophyll-a with modelled Net Primary Production, which takes into account not only satellite chlorophyll but also photosynthetically available radiation (PAR) for light availability, sea surface temperature (SST) as a proxy for biological rates, and length of day. We believe that this approach avoids the use two proxies for algal biomass accumulation and adds new information regarding primary production (which by the way is mentioned by both reviewers).

R2C21: Figures 4 and 5 – I would add a curve representing the temperature anomaly at the top of the plots, to facilitate the visualization of how phytoplankton responded before and during the MHW.

Corrected according to reviewer's suggestion.

R2C21: Line 501: add "in" after "concentrations".

Corrected according to reviewer's suggestion.

R2C22: Line 510-511: not clear, please refrase.

Corrected according to reviewer's suggestion. The sentence has been re-written to add clarity.

R2C23: Line 516: add "community" after "diatom".

Corrected according to reviewer's suggestion.

R2C24: *L543: Discussion: I would not start with the characterization of the MHW; I think the discussion would be clearer if you would start with an integrated discussion of the region's "background conditions" (i.e., the southbound transit), including both biological and chemical (nutrients) data. This section would provide a contribution to the existing knowledge about this remote region, in comparison to previous studies. Then, in a second*

section, you could discuss the mechanisms behind the onset of the MHW, and discuss its associated effects on chemistry and biology.

Corrected according to reviewer's suggestion. In the new version of the manuscript, the discussion commences with a general description of the physical and chemical properties of the water masses in the study region. Then the biogeographical distribution of diatom species is discussed referring to figures 4 and also the new figure containing the CCA. Please note that the distribution of diatom species did not change remarkably between the southbound and northbound transits which is the reason why we use all samples available for the CCA analysis. Following reviewer's suggestions a second section is dedicated to the characterization of the marine heat wave. Lastly, in the third section of the discussion the possible influence of the marine heat wave over nutrients and phytoplankton is addressed.

R2C25: *L551: When you say "This patern is characteristic of the Drake Passage", it is not clear whether you are just referring to eddy formation or to MHWs being trapped in such eddies in the region.*

We agree with the reviewer that there is lack of clarity in this sentence. We are referring to the intrusion of mesoscale eddies. Consequently, in the new version of the manuscript the text has been rephrased as follows: "Mesoscale eddy formation is characteristic of the Drake Passage, where the proximity of major circumpolar fronts enhances eddy activity relative to other sectors of the Southern Ocean (Rintoul et al., 1997; Beech et al., 2022), resulting in pronounced horizontal and vertical gradients in water properties."

R2C26: *Current sections 4.1, 4.2 and 4.3 read a bit disconnected and the titles don't fully capture their content. It is not clear what is "background" and what is "anomaly-driven".*

We appreciate the reviewer's comment. The discussion has been fully reorganized, substantially re-written and the titles for all sections have been modified. In the new version of the manuscript we discuss the species distribution during the whole survey using the new CCA plot. This first section is entitled: "4.1 Environmental variability and phytoplankton species distribution in the Drake Passage and Antarctic Peninsula in summer 2020". Please note that the geographical distribution of nutrients and most of the species was similar before and during the marine heat wave (the main difference was only the *Chaetoceros* RS distribution), but with some noticeable changes in the total diatom abundance and relative contributions of some species. The second section entitled "4.2 Processes and implications on surface water properties of the 2020 marine heatwave" is dedicated to the description of the marine heatwave and the last section, which is entitled "4.3 Influence of the onset of the marine heat wave on phytoplankton communities and nutrient distributions of the Drake Passage" is dedicated to effect of the marine heat wave on nutrient and phytoplankton distributions.

R2C27: *L643: delete "in the"*

We appreciate the correction highlighted by the reviewer but we believe that if this change is implemented in the text, the sentence would be grammatically incorrect. Perhaps the reviewer is referring to a different line?

R2C28: *L770: "seem" instead of "seems".*

Corrected according to reviewer's suggestion.

R2C29: *L779: reframe to: "algal biomass accumulation between early January and early February during almost did not change"*

Corrected according reviewer suggestion: The sentence has been rephrased as follows: "…reveals that average algal biomass accumulation between early January and early February almost did not change"

R2C30: *L800-803: why would the drop in nitrates be so "damaging" for coccolithophores and not for small diatoms?*

We appreciate the point made by the reviewer as the drop in nitrate would not only affect coccolithophores but also diatoms. Please note that in the new version of the manuscript we indicate that the diatom bloom may have been responsible, or significantly contributed, to the nutrient depletion. However, we also acknowledge the limitations of our study and indicate that there might other explanations for the coccolithophore numbers such as zooplankton grazing control. The new text reads as follows: "However, it should be acknowledged that this interpretation remains speculative and there are of course other possible explanations for the low coccolithophore cell numbers, including zooplankton grazing control, which was not assessed in our survey."

R2C731: *L802-812: I am not fully convinced by these culture-based arguments to explain to reduced abundance of coccolithohores, foremost of E. huxleyi, during the warming event…*

We agree with the reviewer's comment. Our interpretation of the decline in coccolithophore abundance in relation to low nitrate levels is speculative as we don't have enough data to reach a solid conclusion. Therefore, in the new version of the manuscript, this part of the discussion has been rewritten clearly indicating that our interpretation is a possibility and that there are other alternative explanations that we have not been able to evaluate in our work, such as a top-down control of zooplankton on coccolithophores.

R2C32: *L813-816: As much valuable as this contribution is, I would recommend more moderation in doing such far-reaching speculations given that this is data from one single expedition, and only the surface part of the photic zone of a rather dynamic ocean region.*

We agree with the reviewer and consequently the last paragraph of the discussion has been substantially modified in order to we even more cautious with our statements and acknowledging the limitations of our study as indicated by the reviewer.

R2C33: L836: delete "of the".

Corrected according to reviewer's suggestion.

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EXTRA CHANGES

We have revised the values of Table 1 and include an explanation in materials and methods (end of section 2.2.) about the comparability of nutrient concentrations between our results (in micromolar per L) and previous research in the area by Freeman et al. (2019) (in micromolar per Kg).