Anonymous Referee #2

We thank Anonymous Referee #2 for the careful and critical evaluation of our manuscript. We have reread our manuscript in the context of these comments and have thoroughly considered each suggestion. The most substantial revision suggested by Anonymous Referee #2 would be the addition of a dedicated Discussion section to highlight the mechanisms behind the patterns we observed. We believe that these suggestions will greatly improve our manuscript. Specific comments are addressed below.

Summary

This manuscript describes the impacts of marine heatwaves on different regions of the Salish Sea. It examines the relationships between the Northeast Pacific marine heatwave (NEP-MHV; 2014–2017) and larger-scale climate signals such as the SOI and the NPGO. The study investigates the effects of temperature anomalies during these heatwave events on regional ecosystems, highlighting cascading effects across different phytoplankton and zooplankton groups. The results show that both the drivers of these perturbations and their ecological impacts vary among regions. The study combines modeling and observations, a comprehensive and relevant approach that supports the validity of the SalishSeaCast model, which performs well when compared with observations.

Overall, this work contributes meaningfully to our understanding of ecosystem perturbations associated with extreme climate events, an issue of growing importance as marine heatwaves are becoming more frequent and intense under ongoing global warming.

My main concern with the manuscript relates to the interpretation of the results. The processes underlying the contrasted responses observed across regions are complex but crucial to understand and to highlight. In its current form, these processes are not sufficiently interpreted or discussed, and the manuscript would benefit from a more thorough discussion of the mechanisms behind the observed patterns. The manuscript is well structured, clearly written, and highly relevant to the scope of the journal, provided that the following comments are addressed.

General comments

The manuscript does not include a dedicated Discussion section; elements of discussion are integrated in the Results section. However, interpretation of the impacts of the different marine heatwaves on biogeochemistry and lower trophic levels should be addressed in a dedicated section. It is essential to discuss the processes linking large-scale climate signals and local MHWs, as well as the relationships between temperature and nitrate anomalies and phytoplankton and zooplankton responses, including differences among the functional groups or the model.

The Discussion should address the following specific points:

- Why are temperature anomalies in different regions more strongly correlated with certain large-scale climate signals than others (SOI, NPGO). The manuscript would benefit from presenting hypotheses, supported by relevant literature, to interpret these correlations.

Agreed (also see related comments made by Reviewer #1 above). We will do a better job of framing the different dynamics that influence the regions of interest in

the Introduction. Setting the stage for these regional differences will allow for a better discussion of our interpretation of the correlations with each region to the large-scale climate indices in a dedicated Discussion section.

For both sites (JdF and SoG), the manuscript should explain the cascading processes by which temperature and nitrate anomalies propagate through the food web, from phytoplankton to zooplankton, and discuss why/how these pathways differ between sites.

Thank you for this suggestion. We will improve our discussion of the cascading processes throughout the food web and how they differ between regions. If necessary, we will subtitle these sections to highlight these key differences. To summarize here, in the JdF region we observed increased warming during NEP-MHW. It is a "cooler" region relative to the SoG. During the NEP-MHW nitrate concentrations were lower than average but still not limiting (model and observations showed that concentrations were never <5 µM). Both diatoms and nanoflagellates in the model showed an increase in biomass due to the slight impact of temperature dependence on growth. Z1 responded well to this increase due to its feeding preference for both nanoflagellates and diatoms. In contrast, the Central SoG is one of the warmest regions of the Salish Sea. Warming was not as strong here during the NEP-MHW compared to the JdF region and was periodically interrupted by cooler waters during this time period. Nitrate is the most limiting factor to phytoplankton growth in this region with persistent nitrate limitation in summer affecting diatom growth due strong stratification and a lack of replenishment to the surface waters as a result of weaker winds throughout negative NPGO years. Nanoflagellates increased during the NEP-MHW. Z1 in this region followed closely the diatom pattern but the pattern for Z2 was not so clear.

Interpretation and discussion of the zooplankton results presented in Section 3.3.2 (see related comment below).

Detailed response provided below.

Specific comments

Section 2.2 Study Period

- Throughout the manuscript, you refer to extended time periods associated with well-known large-scale climate signals, such as the NEP-MHV (2014–2017), SOI and El Niño events (e.g., 2010, 2015), and NPGO years (post-2014). However, these periods are described using different expressions, which may be confusing for the reader (e.g., L246: "post-2014 period (i.e., during negative or warm-phase NPGO years)"; L382, L453: "negative NPGO years"; L565: "NPGO negative (warm) years").

To improve clarity and consistency, it would be helpful to explicitly describe these climate events when they are first introduced in section 2.2, and to define a clear terminology that is then used throughout the manuscript. This approach is already well implemented for the NEP-MHV period (L103), and a similar treatment for the other climate signals would greatly

enhance readability. For example: “Monthly values for the NPGO index indicated a shift to the negative (warm) phase in October 2013 (Fig. 2). Hereafter, we refer to the post-2014 period as the ‘negative NPGO years’.”

Thank you for this comment. We agree that clarification in the terminology here will improve readability. As such, we will make the suggested changes in the revised manuscript.

- In several sections, key statements are supported almost exclusively by self-citations and are not sufficiently placed within the broader literature. For instance, in Section 2.2 (L108–108), relationships between NPGO/SOI and physical and biological conditions in the Salish Sea are referenced primarily through the authors’ previous work, and in Section 3.1 (L274), relationships between temperature and large-scale climate indices in the Strait of Georgia are supported in a similar way. While these references are clearly relevant, including additional independent studies would strengthen the manuscript by better situating the results within the broader scientific context.

Fair point. We will provide additional citations where relevant and will better situate some of our key statements within the broader scientific context.

Section 2.3 SalishSeaCast Model

- L152: “Functional light dependence was switched to a potential energy curve but tuned to match the old response closely”.

What do you mean by “tuned”? Please elaborate.

We will revise the text to clarify what we mean by “tuned”. The sentence will be revised to read: "Functional light dependence was switched to a potential energy curve with constants chosen to match the old response closely”.

- L174-187: Could you briefly describe the trophic interactions among the different model components (diatoms, nanophytoplankton, Z1, Z2)? This information would be useful for interpreting the cascading effects observed during marine heatwaves.

Thank you for this suggestion. We are happy to add more detail on the trophic interactions among the different model components in the Methods section of the revised manuscript.

Section 2.4 Model Data

- L195: “We focus our discussion mainly on the surface (0-50m) as this depth layer is most relevant to both phytoplankton and zooplankton.”, and L202: “Model output for phytoplankton (diatoms and nanoflagellates) and zooplankton (Z1 and Z2) biomass were depth-averaged over the 0-50 m depth range to capture the full extent of the euphotic zone across regions (mmol N m-3)”.

The manuscript would benefit from including information on the vertical structure and migratory behavior of zooplankton in the different regions of the Salish Sea. First, this would support the assumption that a 0–50 m layer is appropriate for representing zooplankton. In addition, such information would be valuable for interpreting the results on the impacts of marine heatwaves on lower trophic levels (cf. my comment on Section 3.3.2).

> We use 0-50 m because we wanted to balance where we know the phytoplankton are growing (euphotic zone) and where we know the model Z1 and Z2 are concentrated. For our two regions of focus, Juan de Fuca Strait and Central SoG, zooplankton are concentrated mainly in the upper 50 m (see Suchy et al. 2023). Furthermore, we will clarify that the surface layer is commonly defined by the 0-50 m depth layer in this region to clearly delineate between this layer and the underlying intermediate waters (e.g., Pawlowicz et al., 2007, Ianson et al., 2016). When model and observation data are considered together, we used full water column averages for zooplankton to avoid any confounding influence of the vertical structure and migratory behaviour of zooplankton on our results.

- L204: “the extent to which temperature dependence and light/nutrient limitation was limiting to growth was calculated based on the phototrophic growth rate equations in the model”.

Could you please elaborate on the methodology of this diagnostic?

> Absolutely. We agree with Anonymous Referee #2 that there is a lack of detail regarding the calculations of temperature dependence and light/nutrient limitation, which was evident in comments from both reviewers. The revised manuscript will provide an elaboration of this diagnostic as well as a more careful discussion of these results.

Section 3.1.1 Temperature

- L271: “indicating a longer-term warming signal than can be explained by the NEP-*M_H_W_ _a_l_o_n_e_”._ _C_a_n_ _o_r_ _c_a_n_’t_ _?_ _

> We will clarify this sentence to read: “...indicating a longer-term warming signal that cannot be explained solely by the NEP-MHW.”

- L275: “In contrast, surface temperature anomalies in the JdF region were significantly correlated to the SOI ($r = -0.44$, $p < 0.001$), but not to the NPGO ($r = -0.11$, $p = 0.12$; Table 1).” Could you provide an interpretation of this result, or propose hypotheses to explain it? See the general comment regarding the need for a dedicated Discussion section.

> Thank you. This comment is similar to a comment made by Anonymous Referee #1. We will provide an interpretation of this result in the revised manuscript. Our hypothesis is that the JdF region, with its direct connection to the open ocean is more strongly influenced by the shorter term (1-3 years) influences of SOI effects on the upwelling/downwelling of nutrients into the region. In comparison, the deep basin of the Central SoG is semi-enclosed and affected by regional scale forcing (from all

of the atmosphere, open ocean, and rivers) that is integrated over longer time scales such as the decadal time scales associated with the NPGO.

Section 3.3.1. Phytoplankton

- L411: “slightly less light limitation”.

Please consider providing a metric or quantitative indicator to better support the interpretation of Figure 9. Moreover, why was this analysis/diagnostic performed only for diatoms? Applying a similar diagnostic to all four model components (diatoms, Z1, Z2) would help to interpret the processes underlying the variation patterns observed across the different trophic levels studied.

We focussed on diatoms with this metric because they are the most variable prey item for Z1 and Z2 in the model. A similar metric will be added for nanoflagellates to the supplemental material as light, temperature, and nutrients are less limiting to their growth in the model compared to diatoms; yet, they are still an important component of the Z1 and Z2 diet. To address Anonymous Referee #2’s concern about the interpretation of this metric, we will include a better explanation of the figures. For example, Figure 9a is directly comparable to Figure 9b but this is not obvious to the reader (we separated the plots to make them less busy). A direct comparison of the two plots shows that temperature and light limitation are much larger in the JdF compared to either nutrient. In addition, we will add subplots to the right-hand side of Figure 9 showing the climatology for each limiting parameter (not the anomaly) to help the reader understand the magnitudes of the baselines to which anomalies are being calculated.

- L426: “Also evident was that diatoms during the post-2014 period (i.e., during negative or warm-phase NPGO years) peaked earlier, and for shorter durations, before switching to nanoflagellate dominance compared to the 2007 to 2014 period (Fig. 8b, Supp. Fig. S6a). »

Unclear, please clarify or rephrase. In particular, what is meant by “peaked earlier”, earlier relative to what?

Here we mean that diatoms peaked earlier in the season relative to the pre-2014 period. This will be clarified in the text.

Section 3.3.2. Zooplankton

- In the JdF region, the marine heatwave has been described as being linked to the NEP-MHV and SOI, with the end of positive temperature anomalies and negative nitrate anomalies around 2020. From 2020 to 2022, negative temperature anomalies reappear, along with positive nitrate anomalies. A similar shift is observed for both nanophytoplankton and diatoms, with predominantly negative anomalies over the 2020–2022 period. Consequently, negative anomalies are also observed for Z1, but we do not observe the same response of Z2 (except for a peak in 2022). How can these contrasting responses between Z1 and Z2 be explained?

Z1 biomass in the model follows closely with the diatom and nanoflagellate patterns because this class freely evolves based on model dynamics. Z2 is more constrained by the model's settings as it represents the model closure term. We will add this important clarification to Section 3.3.2 to avoid confusion.

More generally, providing information and relevant literature on the trophic structure and/or taxonomic composition in the different regions studied would greatly help to interpret these results. In particular, differences between the SoG and JdF sites, as well as communities' changes across the different periods within each site, should be discussed. Overall, this part of the results would benefit from being accompanied by interpretation in the Discussion section.

Thank you for this comment. We will add information regarding the trophic structure and/or taxonomic composition where possible (i.e., in the Central SoG). Similar information for the JdF is more limited given the historical lack of zooplankton sampling in the region (see Figure 10c).

Section 3.3.4. Model and Observation Comparisons

- L467: "model zooplankton (Z1 and Z2 combined) values were always within the range of observed values".

Figure 10c shows that the model does not capture the maximum observed zooplankton values. Please clarify.

This statement will be revised to read: "That said, model zooplankton (Z1 and Z2 combined) values were always within the range of observed values although the model failed to capture some of the extreme values which we expect in the observation data due to the patchiness of zooplankton."

- L470: "Both model nitrate and chlorophyll a showed better agreement with the observed values in the Central SoG".

Computing a comparison metric, such as the RMSE, for all variables in the observation–model comparison would help support this statement. This is not obvious for chlorophyll-a

We agree that this statement is not obvious for chlorophyll *a*. In this paper we do not include point-by-point comparisons, thus making calculations of RMSE and other statistics less meaningful. Instead, we compared our model and observations over broader spatial scales. SalishSeaCast model has previously been evaluated extensively using point-by-point comparisons (depth and time matched) for nutrients (Olson et al., 2020) and chlorophyll *a* (Olson et al., 2020, Jarníková et al., 2022). While this sentence requires rewording, we will also add a section summarizing previous model evaluations in the Methods, which will include region-specific statistics (RMSE, bias, etc.).

- L486: Please reconsider the scale of Figure 11c, using a range of approximately 0–300 mg C m^{-3}. This range differs from the zooplankton values at JdF shown in Figure 10c; this difference can be noted in the figure legend.

> Fair point and easy fix. The scale in Figure 11c will be adjusted and noted in the figure legend.

- L507: "(F(2, 13) = 4.59, p < 0.05)".

Please provide more details about this metric, either here when it is first mentioned or in the Materials and Methods section.

> Important comment. We will clarify in the methods that this metric presents results for the one-way ANOVA comparison of the mean values for pre-MHW, MHW, and post-MHW periods.

- L528: "while the results presented here show similar patterns between model and observation data for certain parameters, these results varied depending on the depth ranges considered."

The analysis underlying this statement should be clarified, with an explicit reference to the supporting figure or table.

> Thank you for this comment. We now realize this statement is somewhat misleading and also confusing. Choosing different depth ranges (e.g., 0-25 m vs. the 0-10 m for nitrate presented in this study) naturally affected the magnitude of the results presented. For example, nitrate concentrations averaged over the 0-10 m layer are lower than those averaged over 0-25 m. While the majority of the patterns we observed held true regardless of the depth layer chosen, there were some instances where significant differences between pre-MHW, MHW, and post-MHW were found when we used a different depth range even if significant differences were not found for the depth range presented in our results, and vice versa. Ultimately, the depths presented here were chosen so that we could maximize the number of observation samples available for comparison with the model. Nevertheless, this statement will be clarified in the revised manuscript.

- Figures 12 and 13: Calculating this p-value over the periods pairwise (i.e., pre-NEP-MHW vs. NEP-MHW, and NEP-MHW vs. post-NEP-MHW) would have allowed quantification of the shifts between regimes. It is possible that in cases where the p-values over the entire period are not significant, a pairwise-period-specific p-value corresponding to a regime shift could have been significant.

> The statistical analyses in Figs. 12 and 13 are one-way ANOVAs which test whether the means of the three groups are statistically different. The alternate hypothesis of this analysis is that at least one of the means is statistically different, but does not tell us which one(s) of means differ. Similar to what Anonymous Referee #2 is suggesting, a post-hoc pairwise corrected t-test can be used to tell us which set of means is different. Running these post-hoc t-tests confirmed the results from the

one-way ANOVA. i.e., when there was a significant difference between the three groups, the pairwise test confirmed which periods (pre-MHW vs. MHW or MHW vs. post-MHW) was visually different on Figs 12 and 13. We will include the post-hoc paired t-test results in our revised manuscript.

Minor comments

- L47: The acronym SST (sea surface temperature) is defined later in the manuscript (L114). Please define it at its first occurrence (L47).

  Thank you. Sea surface temperature (SST) will be defined on L47.

- L86: Please correct “JDF” to “JdF.”

  Thank you. Will be corrected.

- L100: In the legend of Figure 1, a punctuation mark is missing between “boxes” and “Bathymetry.” Please revise accordingly (i.e., “blue boxes. Bathymetry”).

  Thank you. This will be corrected.

- L109: Punctuation “Suchy et al. ,2022 »

  Thank you. Will be corrected.

- You may use the acronym NEP-MHV that you previously introduced in the figure legends instead of spelling out “the Northeast Pacific marine heatwave” (e.g., L120: Figure 2; L256: Figure 3; L336: Figure 6), for consistency with the terminology used in the main text.

  Thank you. Will be corrected.

- L414: Fig. 9b, not 9c.

  Thank you. This will be corrected.