Review of: *Contrasting physical drivers of chlorophyll-a responses to marine heatwaves in the South China Sea*

This manuscript investigates chlorophyll-a (Chl-a) responses to marine heatwaves (MHWs) in the South China Sea using satellite observations, reanalysis products, and a Synergistic-Unique-Redundant Decomposition (SURD) framework. The authors identify contrasting summer Chl-a responses in the Pearl River Estuary (PRE) and Vietnam Upwelling Area (VUA), and propose distinct physical mechanisms linking MHWs to these biological responses.

Overall, this is a well-organised and generally well-written manuscript. The study addresses an important question, namely whether ecosystem responses to MHWs are driven primarily by temperature anomalies (or heat stress) themselves or by associated physical changes in the marine environment. The application of SURD provides an interesting and novel approach for investigating interacting drivers.

I found the results for the VUA relatively convincing, with the proposed mechanism linking weakened upwelling, reduced nutrient supply and negative chlorophyll anomalies appearing physically consistent. However, I have more concerns regarding the interpretation of the PRE results, where the proposed plume-retention mechanism is more speculative and appears less directly supported by the available observations (e.g. nutrients). I also think the manuscript would benefit from a more careful discussion of the limitations of using satellite-derived surface chlorophyll as a proxy for phytoplankton biomass and ecosystem productivity.

The manuscript addresses an exciting and important research question that is highly relevant to current oceanographic research, particularly given the increasing occurrence of marine heatwaves and growing interest in their ecosystem impacts. The study fits well within the scope of *Ocean Science* and presents a novel application of the SURD framework to a challenging physical-biological problem. Subject to the comments below, the manuscript is potentially suitable for publication following revision.

Major comments

One of my major issues with the manuscript is that surface chlorophyll does not necessarily represent biomass changes, throughout the manuscript, changes in Chl-a are frequently interpreted as changes in phytoplankton biomass or productivity. However, increases in chlorophyll concentration can arise through several mechanisms such as increased phytoplankton biomass, increased chlorophyll content within existing cells through physiological light acclimation (Masuda et al, 2021, or through changes in mixed-layer depth and vertical redistribution of existing phytoplankton biomass (e.g. concentrating or diluting existing chlorophyll). For example, shoaling of the mixed layer may increase surface chlorophyll concentrations simply by

concentrating existing phytoplankton into a thinner layer, without any increase in biomass or production. Conversely, entrainment of low-chlorophyll water can dilute surface concentrations. The manuscript should discuss these possibilities more explicitly. In particular, do the authors consider whether the positive PRE anomalies or negative VUA anomalies may partly reflect changes in vertical redistribution rather than changes in biomass? Chlorophyll (from the reanalysis data product) integrated over depth might support the analysis.

Additionally, the authors should outline the typical vertical structure of chlorophyll in summer as marine heatwaves have been shown to alter the vertical structure of chlorophyll (Li et al, 2025: Vertical structure of chlorophyll-a during marine heatwaves in the California Current Ecosystem | Communications Earth & Environment). The introduction contains little discussion of the vertical chlorophyll structure of the region.

My other concern is that the interpretation of the PRE mechanism requires strengthening, the VUA interpretation is relatively straightforward, with weakened upwelling providing a plausible link between MHWs and reduced chlorophyll concentrations. I found the PRE mechanism less convincing, the authors argue that weakened winds promote retention of nutrient-rich plume waters, resulting in positive chlorophyll anomalies. While the hydrographic evidence for plume retention is plausible, the nutrient supply pathway remains inferred rather than demonstrated. The conclusions regarding nutrient-enhanced phytoplankton growth in the PRE should therefore be expressed more cautiously. I suggest clearly distinguishing between the evidence supporting plume retention and the more speculative inference that plume retention enhanced nutrient availability and phytoplankton growth.

Additionally, seasonal climatology is used to define chlorophyll anomalies during MHWs, but it is currently difficult to assess how unusual these anomalies are relative to normal summer variability. The authors should clarify whether the reported anomalies exceed the typical variability observed during the season and whether the high-chlorophyll belt described in the VUA represents episodic bloom activity.

The conclusions primarily summarise the results but provide relatively little discussion of their broader implications, what are the implications of reduced chlorophyll within the Vietnam upwelling system? Could these changes influence fisheries productivity? How might this related to regional carbon cycling and export? Expanding the discussion of wider ecological implications would strengthen the significance of the study.

Minor comments

- The manuscript would benefit from a brief discussion of chlorophyll as a proxy for phytoplankton biomass, including important limitations.

- Sect. 2.3.1: Please explain why the 1982-2011 climatological baseline was selected for MHW detection. It would also be useful to note that the fixed baseline approach follows Hobday et al. (2016) and much of the MHW literature, but does not account for long-term warming and shifting temperature baselines.
- Line 188: Consider changing to "both the PRE and VUA exhibit *significantly* higher median annual total MHW days and cumulative intensity…"
- Line 200: Consider specifying "increases exceeding 40% relative to the climatological summer chlorophyll concentration."
- Line 212: Why was cumulative SST intensity selected as the sole thermal-stress metric? The introduction contains little discussion of thermal stress mechanisms affecting phytoplankton physiology, despite this variable playing a central role in the SURD analysis.
- Lines 157-160: Please consider presenting the lag sensitivity results in supplementary material rather than only describing them in the text.
- Figure 11: The schematic is helpful and effectively summarises the proposed mechanisms; however, the font size and labelling could be improved to aid readability.

Recommendation: Major revision

The manuscript addresses an important topic and introduces an interesting analytical framework. The VUA results are compelling and the paper is clearly written. However, stronger justification is needed for the PRE interpretation, particularly regarding the inferred nutrient pathway, and the use of satellite derived surface chlorophyll observations as a biological indicator. Addressing these issues would substantially strengthen the manuscript.