**Review Comments**

The study by Wu et al., uses time-series data collected from a sediment trap deployed in the southern South China Sea to provide a year-round assessment of fecal pellet–mediated carbon fluxes and to evaluate how these fluxes respond to upper-ocean dynamics. The findings highlight that, in addition to the seasonal monsoon-driven pattern, other physical processes—such as typhoons, eddies, and episodes of high wind speed—also contribute significantly to the variability of deep fecal pellet carbon fluxes.

Overall, the manuscript is well written and logically organized. The figures are presented effectively, and the results strongly support the main points. I do not have major concerns. My suggestions are mainly minor, relating to figure design, methodological description, and expansion of the discussion to improve clarity and depth. Please see my detailed comments below:

## Specific Comments

**Line 40**: Please add relevant references after each study approach cited, to demonstrate how these methods have improved our understanding of fecal pellet–mediated carbon fluxes.

**Line 75**: The abbreviation *SCS* has already been defined in a previous section; repetition is unnecessary.

**Lines 165–176**: Cite the corresponding figures or tables when describing results.

**Line 130**: Provide more details (including model data source, configuration, and validation) for the models used to derive the biogeochemical parameters (e.g., Global Ocean Eddy-Resolving Reanalysis, CMEMS Global Ocean Low and Mid Trophic Levels). Many biogeochemical models are available; please justify why these particular models were selected and comment on their performance in the SCS.

**Figure 1**: Consider embedding a small map showing the relative location of the SCS within the broader Pacific Ocean. This would help readers unfamiliar with the region.

**Line 80**: The description that the mixed layer depth (MLD) is "relatively deep" in the SCS may not be appropriate. As shown in your results, there is a significant seasonal cycle, and the maximum MLD remains shallower than 60 m.

**Line 90**: It is difficult to believe that diatoms contribute ~70% of the phytoplankton biomass in the open-ocean upper layer. Do you mean in the sunlit surface water, or are you referring to aggregates/sinking particles in the mesopelagic zone? Please clarify.

**Line 120**: Please add methodological details on how POC flux was quantified from the sediment trap, since the FPC:POC flux ratio is discussed extensively later. In addition, the observation that the FPC:POC ratio remains relatively constant despite increases in total POC flux is intriguing. Does this reflect a true ecological signal— i.e., that different components contributing to POC flux increase proportionally—or could it be an artifact of the methodological approach? Some clarification on this point would strengthen the interpretation.

**Line 125**: The *a* in *Chl a* should be italicized. Please modify throughout the manuscript.

**Line 140**: The *p* in *p-value* should be italicized. Please modify throughout the manuscript.

**Figure 3**: Use different color schemes to denote the two monsoon seasons to improve visualization.

**Figure 5a**: Please clarify which direction the positive values represent.

**Figure 5h**: Report depth-integrated NPP in *mg C m⁻² d⁻¹* to align with the units of POC flux. Also, the time unit is missing.

Precipitation data may be redundant and contribute little to your analysis; consider removing it.

**Figures 6g–6h**: Place surface nitrate on the x-axis, since the aim is to examine how zooplankton respond to upper-ocean dynamics.

**Line 295**: Please clarify how you estimated the ~22-day time lag at your study site.

**Line 335**: Replace *CMD* with the more standard term *deep chlorophyll maximum (DCM)*.

**Line 410**: What is the typical fractional contribution of zooplankton fecal pellets to total POC flux in oligotrophic oceans? Additional discussion and comparison with previous studies would strengthen this section. yibin