Reviewer 1

The authors have addressed my previous comments properly and the clarity of this manuscript is greatly improved. I only have some additional minor comments as in the following, mostly suggestions for clarification in the introduction.

Thank you again for your input, it has been really useful.

Specific comments:

L3: “waves” -> “surface waves”? Fixed

L9: “two-way coupled model” -> “two-way coupled ocean-wave model”? Fixed

L10, and following lines in the abstract: “wave mixing” -> “wave-driven mixing”? Changed

L69-70: Its effects, in particular in driving Langmuir turbulence and thus enhancing vertical mixing, is parameterized in ocean models. See, e.g., Li et al., 2019. Added a line to this affect

L79-80: I think the terminology is confusing here. Even with explicitly coupled wave model, the effect of surface waves on the mixing is still parameterized in an ocean model — wave phases are not resolved in ocean general circulation models. I think what the authors meant here is parameterized wave variables, not wave-induced mixing (which is always parameterized in the ocean model regardless of where the wave information comes from).Amended

L82: Two-way coupling itself does not induce enhanced vertical mixing — there has to be a vertical mixing parameterization that depends on wave variables provided by the two-way coupling. Changed ‘induce enhanced’ to ‘enhance’ to be more reflective

L85-86: As alluded here, there are two components of incorporating the effects of wave-driven mixing in an ocean model. (1) a turbulence parameterization scheme that depends on wave variables and (2) a source of wave information — from a wave model in the two-way coupled wave-ocean setup and some empirical relations in the ocean-only setup. So, the wave-driven mixing parameterization is as important as wave-ocean coupling, which should probably be discussed a bit more in this introduction. At least, it should be clarified what specific mechanism of wave-driven mixing is considered here. Is it wave breaking, Langmuir turbulence, or something else? The additional processes considered are detailed in section 3.2, we have added them into the introduction at line 102

L97: See comments above. I think the authors meant explicit representation of wave statistics here, not wave-driven mixing effects. Fixed

L102: It might be useful to make it clear in the introduction what specifically are these wave-induced processes? See above

L220-222: I think the depth integrated Stokes drift is also needed in addition to the surface Stokes drift in order to use the method in Breivik et al., (2016) to reconstruct the full Stokes drift profile? True, this paragraph focuses on what is added by the wave model that makes it different from the ocean only case rather than a full discussion of the schemes.

L239-241: Also Langmuir turbulence enhanced vertical mixing, which is more relevant to the GLS turbulence scheme here. See, e.g., Li et al., 2019. Noted and reference added

Reviewer 2

Major comments:
The revised manuscript addresses the previous comments and demonstrates notable improvements in the interpretation and discussion of the results. However, the current version suggests that the model may overestimate temperature and chlorophyll concentrations, potentially leading to an exaggerated assessment of wave and storm impacts on net primary production (e.g., comparison of Figure 4 and Figure 14). Clarifying this issue would strengthen the credibility of the study's conclusions regarding the biogeochemical effects of wave processes.

Thanks for the comment. In the text we acknowledge that the impact is larger in the model due to the overestimation, but there is enough to suggest that the patterns of the response are similar. The use of a twin experiment in this case allow us to draw conclusions about processes and effects if not the scale. We have added text into the conclusions to clarify this.

Detailed comments:
Figure 9: To better account for regional differences in mean concentrations, it would be helpful to plot the time series of the percentage change relative to the OCN case. This would also help to more clearly highlight the significance of wave effects. Due to low concentrations over the winter period the percentage change amplifies values during that time and detracts from the differences in the relevant months

Line 350: I would expect the warm bias in the model to lead to an earlier bloom onset. The bloom onset is dictated by many factors and BGC models often struggle to predict the timing in free running simulations. Previous studies that did not have a warm bias have also seen an onset later than observations suggest.

Line 355: This paragraph should be revised to after the removal of the observed bloom onset figure. Consider either including the figure in the supplementary materials or explicitly noting that the figure is not shown. Removed the reference to the removed figure

Figure 12: Although it is not the primary focus of this study, what might explain the higher winter temperatures with enhanced mixing shown in the WAV case? In the autumn and winter the surface of the ocean cools down. Enhanced mixing brings water that is now warmer than the upper layers towards the surface, effectively dampening the cooling of the top 100m of the ocean. It is the opposite of what happens in the spring when the surface is warming. The whole annual cycle of warming and cooling is dampened by enhanced mixing.

Figure 13: Should be "in the upper 100m" and this figure is not necessary in the main text of the manuscript. Fixed the caption. Whilst this figure could be removed we feel

demonstrating the monthly variation to the stratification difference between the two model runs highlights the impact.

Line 488: The increase in summer production is less pronounced than off-shelf. Fixed