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Interactive comment on "Seasonal effects of photophysiology and chlorophyll *a* abundance on phytoplankton group-specific primary production in the Kuroshio region as revealed by SeaStar/SeaWiFS" *by* Takafumi Hirata and Koji Suzuki

Anonymous Referee #2

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This manuscript "Seasonal effects of photophysiology and chlorophyll a abundance on phytoplankton group-specific primary production in the Kuroshio region as revealed by SeaStar/SeaWiFS" proposed a new approach to estimate group-specific primary production, along with group-specific quantum yield and chlorophyll a specific absorption, by integrating satellite image processing/statistics with bio-optical models. The method is for the first time developed, yet looks not so solid, partially due to lacking of evaluation of the results. Meanwhile, it is not clear why it is limited to Kuroshio region,

and if the main point is the seasonal effects of photophysiology and chlorophyll a abundance on phytoplankton group-specific primary production, or it is the method, and why that seasonal effects are of concern. In a word, more efforts are needed to make the method more convincing, and to clarify the logic in the manuscript.

Some specific comments: 1. To my knowledge, the target region is actually not only Kuroshio area (see Fig. 1, and the satellite images). Some of the sampling stations are located in the Oyashio region. Almost all diatoms revealed by SeaWiFS are also located in that water (Oyashio). It would be a better choice to focus on Kuroshio-Oyashio frontal region, where water is dynamic and rich of nutrients and various groups of phytoplankton. 2. L28-30: I would suggest to cite references for this sentence "Despite the oligotrophy of the current, higher fishery production has been recognized in and around the current, which is a paradox of the Kuroshio ecosystems." Or data should be provided, proving that higher fishery production was found in the Kuroshio Current compared to other warm currents such as the Gulf Stream. 3. L107-110: Why used CbPM PP, not VGPM or others? Did you do a comparison? 4. Because the method to derive group-specific PP, quantum yield and absorption coefficient is new, a separated section providing details of the method and its evaluation, would be of much more help. 5. Section 3.1, how to understand higher diatom than haptophyte Chl (0.21 vs 0.11) is consistent with higher haptophyte than diatom absorption (0.0026 vs 0.0016), and the notion of more abundant haptophytes in this region? Also, I would say a mean value is not meaningful for this kind of water. 6. In addition, the authors appear careless while preparing this manuscript. For example, Line 404-405 is an incomplete sentence.

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