## *Interactive comment on* "Vertical distribution of chlorophyll *a* concentration and phytoplankton community composition from in situ fluorescence profiles: a first database for the global ocean" *by* R. Sauzède et al.

**Anonymous Referee #2**

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**General comments**:

Being a non-native English writer, I am not going to make any comments on the language quality. From a strictly scientist point of view, the paper is well written and the objectives are clear. The authors have used an interesting and ambitious methodology (i) to harmonize chlorophyll profiles from different sensors and platforms, and (ii) to provide size-based estimates of the community composition. The data set is well presented, appears extensive and the processing method is innovative. Figures are

also clear and well done. The paper thus merits publication.

The authors need however to provide more information on the method limits that are not listed clearly. In my opinion, the reading of Sauzede et al. 2015a was required and then several questions appeared after that. An efficient synthesis of the main limitations of the methodology without going back to the previous article is thus required. For example (like all methodologies using a training set), the authors indicate some limitations for applying it on profiles prior 1991. It was assumed that the relationship between the phytoplankton biomass and the community composition with the fluorescence profile is similar to that one after 1991. In a context of global scale, climate change and validation of long simulations, I'm thus wondering how some modifications of the phytoplankton composition over time would be detected from the algorithm? The authors indicate also that the method "is robust with slightly less accurate results for the Arctic basin and the Indian Ocean which are two areas known for data scarcity". I think the authors should provide a specific quality index for this data base for helping to weight correctly their estimates for some comparison with model outputs. Are the authors able to produce some potential errors or a quality index in the classification according to the number of HPLC samples available for one period and one area? This step is very relevant because, at least in [ChlT], this data base could be reuse by a large community of modelers by providing a higher resolution that WOA13.

I'm also wondering if the authors could provide some recommendations on the sampling protocols to improve FLAVOR results (exp : not sampling at fixed depth but according to the shape of the profile) and to be consistent over time for the next decade (could you recommend some HPLC measurements with a minimal frequency for one area?). This one could be tested by under sampling (spatially and temporally) different size of training sets, to evaluate the impact on the estimates quality.

**Specific comments** : P8: How did you manage temporal trends due to fouling for autonomous in-situ Chl sensors? Did you use some quality indices associated with these platforms? P8, lines 22-23: How can you have less than five values per profile

with at least ten values per profile ???  If condition-4 was satisfied, the condition-5 should be fine? Not clear P9, line 6: Have you really checked 48 600 profiles visually?? P11: First sentence of section 3.1 is a repetition of the previous section and can be removed. P11, line7 : Is the "FLAVOR use" a real **calibration** process ? Probably not, in spite of the good quality of the algorithm.

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