Comment on os-2021-88
Emmanuel Boss (Referee)

Referee comment on "Global distribution and variability of subsurface chlorophyll a concentrations" by Sayaka Yasunaka et al., Ocean Sci. Discuss., https://doi.org/10.5194/os-2021-88-RC1, 2021

Review of ‘Global distribution and variability of subsurface chlorophyll a concentration’ by Yasunaka et al.

Reviewer: Emmanuel Boss, University of Maine.

This paper is focused on describing the salient features associated with the ubiquitous chlorophyll sub-surface maxima in the world’s ocean. The depth of this maxima is contrasted with a variety of other horizons (associated with physical stratification, light, nutrient, and oxygen). Both annual and seasonal variation are provided.

This paper is of interest to the readers of OS (as well as Biogeosciences) and I support its publication. While relatively clear, I think this paper could benefit from editing by a native English speaker as some sentences do not read well. I have some comments that I believe if answered could significantly improve the impact of this paper.

Major comments:

Introduction: The introduction mostly avoids discussing the link between Chl-a and phytoplankton biomass. Sometimes the sub-surface chlorophyll maxima is a biomass maxima but often it is mostly a photo-acclimation response of phytoplankton (e.g. Kiefer and Zaneveld, 1991, JGR), e.g. an increase of pigmentation with decreasing light, just as you see with your house plants. This is important to note as many still use Chl-a as an indicator of biomass without understanding the associated bias, particularly when it comes to vertical distribution.
Methods: Chlorophyll concentrations are NOT normally distributed in the surface and likely at depth. Thus, it would be good if before applying any QC steps the authors look at the frequency distribution of the data they downloaded and use non-parametric statistics (median, percentiles) rather than averages and standard deviation for QC.

The data types used each has its own source of error (varying from non-photochemical quenching to changes in Fchl/Chl with depth and differences between HPLC and fluorometrically determined Chl). It would have been good if those were detailed. Some of the uncertainties may have consequences on the profile of Chl. One can see this issue with some of the transects in Fig. 1e where it seems that methodological issues cause a specific cruise line to be different than most other measurements in a given region.

Euphotic layer depth, e.g. 1% light level, is a bad light horizon to compare Chl distribution to as phytoplankton care about absolute light levels, not relative ones (e.g. Letelier et la., 2004). In a ML, cells cycling up and down are exposed to, on average, the median light level in that layer. A fixed depth horizon is meaningless in such a case. Also, satellite products of Z_eu do not take into account the vertical chlorophyll distribution but are only based on surface estimate and statistical correlations between those and z_eu, similar to the vertical profiles of Uitz or Westberry. I strongly urge the authors to not use the z_eu but actually attempt to compute the actual light level at the Chl maxima depth (e.g. using a validated model such as Xing and Boss, 2021 that requires PAR at the surface (available from satellite) and a chlorophyll profile).

Results:

The relationship between a chlorophyll maxima and the ML depth depends on the criterion used to define the ML (and there are many). If the ML is an active mixing layer, there cannot be a maxima within it. If the criterion is such that it describes a longer time scale formation, gradients can form within it (e.g. Zewada et al., 2004, compared optically defined and physically defined ML depth). These can be due to a variety of processes spanning from lateral restratification to phytoplankton photo-acclimation, to name a few.

Line 155-160- the explanation provided does not take into account the actual light level and hence is correlative at best.

Line 175-180- If I am not mistaken, iron is undetectable in the subarctic N. Pacific all year.

Line 180: In the discussion explain how and why subsurface fluorescene maximum may differ from Chl-a maximum and how it may affect your results. You should attempt to contrast your distribution based on floats and those based on water sample analysis to see if there are significant differences between them.
Minor comments:

Line 19: replace ‘suggestive results’ with ‘has implication to’.

Line 35: If I understand correctly from Park’s paper, Chlorophyll responds faster than SST to the wind change associated with El Nino. The way this sentence is written suggests that chlorophyll change is a necessary condition for the onset of El Nino which it is not. This is likely due to the rapid Chl response to local MLD change while the SST change is mostly due to advection (from west to east) which takes months.