The authors would like to thank referee #1 for the comments on the submitted manuscript. The revised version of the manuscript takes into account all his/her comments, as described below.

Abstract
Line 6: missing "in" nor accounted for in the primary"
Response: Corrected.

Introduction
Line 29: a powerful mean? Should this be a powerful medium? or means? Line 29: a powerful mean? Should this be a powerful medium? or means?
Response: We changed "powerful mean" for "powerful medium".

Methods
Line 93: Did all the profiles have light depths associated with them. If not how did you decide whether the last depth sampled was at or below the euphotic depth?
Response: Unfortunately, optical property data were not available for each vertical profile of chlorophyll a. Hill et al. (2013) showed that approximately 5% of this large database contained light measurements. In this manuscript, when the optical properties were not available, we choose then to use a fixed depth, relatively deep to avoid missing the potential subsurface chlorophyll maximum (SCM). In lines 20-23, p. 1350, we explained the strict criteria that we used in this manuscript: "(2) the lowermost sample of the profile had to be collected at or below the base of the euphotic zone (1 % of the surface irradiance) __or__, at least at a depth of 75 m or 15 m above the bottom depth (Z_{BOT}) for shallower stations".

Line 94: Is Zbot the depth of the water column?
Response: As written in line 21, p. 1353 and defined in Table 1, Zbot is the bottom depth.

Line 149 to 159: I am a little confused about this paragraph. Are the data separated into pre-bloom, post-bloom and winter purely by the calendar month? Irrespective of the location or ice cover? I ask because a location with complete ice cover in April or May would not be characterized as in the post-bloom phase. Also is there no bloom phase? Later in the paragraph it is stated that the temporal threshold between pre- bloom and post-bloom was defined as the time period when surface Chl a is highest, so were the periods calculated individually for each station? I need a little more clarity in this section.
Response: For clarity, the section ii) of mentioned paragraph was rewritten. Here are the revised sentences: 'ii) The dataset was partitioned into the following three separate time periods: pre-bloom (February-April), post-bloom (May-September) and winter period (October-December). For the development of the empirical model, however, the partitioning of the time periods is based on the conceptual scheme illustrated in Figure 4, which depicts the annual cycle of the $chla_{surf}$ concentration throughout the growing season. The temporal threshold between the pre-

bloom and the post-bloom periods was defined as the annual highest $\mathrm{chl}a_{surf}$ *value when the spring bloom reaches its paroxysm (Fig. 4). Hence, the empirical model does not contain a specific period of spring bloom. When the sea-ice free period is long enough, a fall bloom is expected, owing to nutrient replenishments at the surface layer by forcing events (i.e. convective mixing and upwelling) during the late season. The temporal threshold between the post-bloom and winter period was defined as the time when daylight becomes less than nine hours. This threshold corresponds to the approximate length of photoperiod below which the SCM is no more observed within any bin (data not shown). In addition, a flowchart showing the partition and the use of the database has been added to improve clarity (see new Fig. 3).*

Line 178: Should read 'A spectrally-resolved model" not spectrally-resolve, just missing the "d".
Response: Corrected.

Results
Line 394 to 399: Looking at the plots in fig 6 and comparing them with those time series in fig 8, I don't see the same features. The well-defined SCM at low surface concentration is not there.
Response: This is a very good comment. The parameterizations used in Figure 9 (formerly Fig. 8) are really based on the parameter values presented in Tables 3, 4 & 5 and chl a profiles showed in Figures 5, 7 & 8 (formerly Figs. 4, 6 & 7). The question is why we do not detect the SCM in all the sub-arctic and arctic seas. The well-defined SCM at low surface concentrations are visible, in particular in oligotrophic regions (i.e. Beaufort Sea, Hudson Bay). In the other productive regions, the high chlorophyll a concentration during the spring bloom tends to mask the presence of SCM, which are still present but really weak compared to the spring bloom. This is one of the main conclusions of this paper; the SCM features are intimately related to oligotrophic conditions and will be primordial in these regions. In Arctic regions where the vertical stratification is weak and that surface waters are replenished in nutrients, SCM will represent only a minor part of the annual phytoplankton biomass and production, as precisely underpinned in Figure 9 and Table 6.

Line 421: This sentence is confusing. "Thereafter, the consequences of low during the post-bloom (when >0.7 mg m-3) and winter periods could explain, to a lesser degree, PP overestimations." Can you clarify for me, why low surface Chl leads to overestimations?
Response: We mean that, even for low surface chlorophyll a concentrations (particularly during the winter period), the maximum of chlorophyll a will be present close or at the surface (Fig. 4, see the winter period and the post-bloom period when $\mathrm{chl}a_{surf}$ *>0.7 mg m^{-3}). So, when we compared the assumption of homogeneous vertical profiles with the parameterized vertical profiles (Fig. 5 and Table 3), we observed also overestimations in PP, such as during the pre-bloom period (Table 6). This sentence has been improved in the final version of the manuscript (see section 3.3).*

Line 427: I was unable to find 40% in table 6.

Response: We agree that an underestimation of 40% is not showed in Table 6. The table indicates only the means of under- and overestimations for the different time periods. The underestimations of 40% were only extreme cases (line 24, p. 1365), as the overestimation of 20 % during the pre-bloom (line 15, p. 1365). In the present version of the manuscript, we indicated that these data are not shown. Similar results, in particular the underestimations, were obtained by other studies (e.g. Martin et al., 2010; Arrigo et al., 2011; Hill et al., 2013). The legend of the Table 6 has been improved in the final version of the manuscript.

Line 489: You can also use the PP estimate from Codispoti et al 2013 Progress in Oceanography which is based on nutrient drawn down.

Response: Unfortunately, it is difficult to compare our results with those from the recent study of Codisposti et al. (2013). Their PP values were estimated in both open and ice-covered waters. In our study, we estimated the phytoplankton production in ice-covered waters only. For this reason, we did not compare our PP estimates with those of Codispoti et al.

Line 565: I could not find table 7.

Response: Corrected. We changed "table 7" for "table A1".

Figures

Figure A1: I did not see this figure referenced in the appendix text.

Response: We cited this figure in line 16, p. 1372.

References cited in the Responses section:

Hill, V. J., Matrai, P. A., Olson, E., Suttles, S., Steele, M., Codispoti, L. A., and Zimmerman, R. C.: Synthesis of integrated primary production in the Arctic Ocean: II. In situ and remotely sensed estimates, Prog. Oceanogr., 110, 107-125, 2013.