General comments

The paper is dedicated to a very topical issue, and thus certainly is of interest. However, I find it very poorly substantiated and lacking some important proves of the statements made by the authors. The illustrations are of so small size that they become nearly useless. It is very deplorable because the figures are almost the only presentation of quantitative results in the paper.

REPLY: The figure will be enlarged and the statistical results provided within 4 new tables (see below).

Concrete comments

1. The methodology is given unsatisfactorily. Daily PP model (eq. 1): no reference to a more detailed description of this model. It is necessary to provide a derivation of the model, with an explicit discussion of the boundary conditions, the model sensitivity, to vertical profiles of CHL, and other colour-producing agents.

REPLY: Equation 1 was first proposed by Platt et al (1980) and modified by Arrigo and Sullivan (1994) who replaced PAR by PUR. These references will be added to the revised version. There were many other similar semianalytical models published and validated the past 3 decades (e.g. Platt and Sathyendranath 1988, 1993; Balch et al. 1989, Morel 1991). The form of these algorithms is robust (Campbell et al. 2002, Carr et al. 2006, Saba et al. 2011). What actually make the difference in terms of performance among these different models, or different versions of the same model, are the values adopted for the parameters and, most importantly, the quality of the inputs for variables of the model (chl and light). Regarding model parameters, we used the only available parameterization derived from polar data for EKPUR, and a constant value for PBmax representative of Arctic waters (see below the discussion about the temperature dependence of PBmax). Regarding the inputs, Chl is estimated using the GSM algorithm, which has been validated for Case 2 waters (IOCCG 200?), and Kd(λ) is estimated using the combination of algorithms also validated for Case 2 waters (IOCCG 200?, Lee et al. 2005). Note also that Ardyna et al. (2013 this issue) validate the model using in situ data as inputs. Additionally, our model has been recently compared with other satellite-based PP model used in Polar Regions. This intercomparison will be published in a special report of the International Ocean Color Coordinating Group (IOCCG) on Remote Sensing of Polar Region (publication foreseen in late 2013). The intercomparison included the models of Arrigo et al (2008), Hill et al., (2012), and Hirawake et al. (2011,2012). When using the same input for chlorophyll-a (CHL), our model provided similar estimation of PP than Arrigo et al (2008) model. In the present manuscript, our PP estimation are lower than Arrigo's one due to the fact that we used a different CHL product (i.e. GSM vs OC4) and that the diffuse attenuation of spectral downwelling irradiance (Kd) is estimated using a quasi-analytical method rather than a pure empirical one (see below for more explanations).

Regarding the sensitivity of the model, the work requested by the reviewer is pertinent but is out of the scope of the current paper, which focuses on a trend analysis of PAR, PP and ocean optical properties. We are aware of the limitations of such satellite-based PP model, some of which were listed on page 14001. Two recent studies examine the sensitivity of the satellite-based PP model to vertical profile of CHL, i.e. Arrigo et al. (2011) and, more recently, Ardyna et al (2013; this issue). The later study, which used the same PP model than the one we used, concluded that the annual PP might be slightly overestimated when considering a vertically homogenous water column (see below for details). Our model also accounts for other color-producing agents such as colored dissolved organic matter (CDOM) or total suspended material (TSM). We have actually made several sensitivity analyses of the model inputs to assess their impacts, and some of them will be provided in the revised version. That being said, we are preparing another manuscript, which will be addressing some methodological questions specific of the Arctic Ocean.

The authors neglect the dependence of PmB on SST. What errors might it inflict?

REPLY: If we exclude the Barents sea, SST in the Arctic varies between -1.8 to ~4°C. Within this temperature range, it is not clear whether or not there is a relationship between PmB and SST in that range. In the PBopt vs. T relationship published by Behrendfeld and Falkowski (their fig.7; L&O, 1997), there is apparently no significant relationship within this temperature range. In fact, there is no in situ data from the Arctic that would justify the use of a relationship between PmB and SST. Our own data from the Arctic did not show any significant relationship between PmB and SST (see Huot et al., Biogeosciences Discuss., 10, 1551-1576, 2013, this issue). So, how much error would introduce a PBmax vs SST relationship that has never been validated in the Arctic?
We acknowledge the fact that our PmB may be underestimated in the Barents Sea or in the southern Bering Sea where SST can reach 10°C. But most of the production in the Arctic occurs in spring or early summer at the ice edge when and where low temperature prevail (see Perrette et al., BG, 2011; Ardyna et al. 2013). That being said, the major objective of this paper is the assessment of trends in PP or in Ocean optical properties. Our PP trends are thus temperature-independent and may be very slightly underestimated if SST has increased between 1998 and 2010 and only if PBmax is temperature-dependent, which remains to be proven at the Arctic scale.

Results of verification of the model with in situ determinations are mandatory, but they are absent.

REPLY: As mentioned above, the model yield comparable estimate of PP relative to other satellite-based models found in the literature. None of the published models have been validated using in situ data. Pabi et al. (2008) presented a verification of their model with the assumption that Chl-a input is free of error. Strictly speaking, this is not a validation either.

The model simulates daily PP rates. The spaceborne Chl, IOPs and cloud and ice concentration data employed are monthly. Calculated spectral Rrs values are monthly. How is all reconciled?

REPLY: This is true that CHL and IOPs are monthly, but it is not true for cloud (3h) and sea ice (daily) (see PAR model section 2.1). Indeed PP calculations were made at 3-hour intervals, which corresponds to the ISCCP time resolution. This was not clearly stated in the section 2.2 and we will modify the text accordingly.

So far, we have employed monthly ocean color data because the data coverage is extremely sparse in daily or weekly composite. Even in the monthly data, there are still gaps in the ocean color data coverage due to cloud or sea ice (see the figure below). This is an inherent limitation of ocean color observations in Polar Regions. Our strategy to fill the gaps in ocean color data is described on page 13993, lines 16-25. In brief, we fill the gaps using the 13-years climatology of ocean color data.
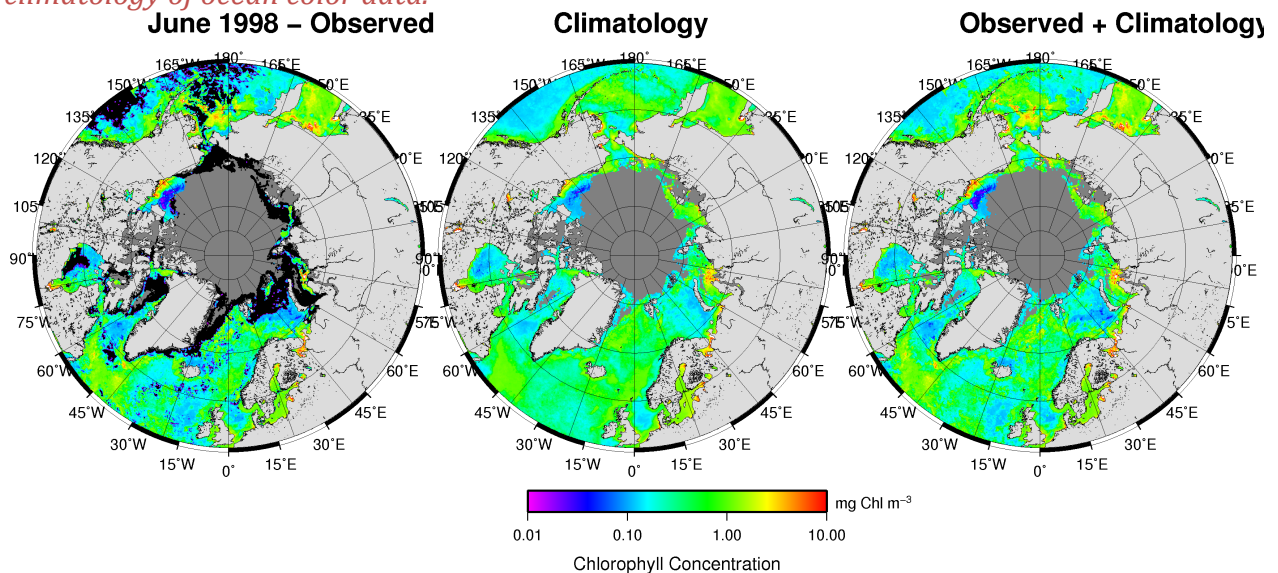
Figure - Example of a monthly CHL map, retrieved by applying the GSM01 algorithm on SeaWiFS data of June 1998. Gaps in the data were filled in with climatological data that were computed over 13-years. Gaps are due either to persistent cloud cover or sea ice.

We assessed the impact of 1) using monthly Rrs to caculate IOPs rather than 2) using daily Rrs to calculate daily IOPs and then averaged them to get the monthly IOPs. The average difference between both methods is <5%. The maximum error was ~10%. We did the same exercise for monthy PP and both methods yield difference <10%. We concluded that the time resolution of ocean color data have little impact on the monthly PP estimation.

(We did not calculate spectral Rrs? I don't understand the reviewer's statement)

 The authors refer to an excellent performance of the algorithm retrieving the total absorption and backscattering coefficients. However, they apply the algorithm to gigantic oceanic tracts. Are there proves that the algorithm performs well across all oceanic waters covered by the research? No attempts of this sort are explicitly undertaken by the authors.

The analysis of errors of the algorithm performance. Analysis of the PP model sensitivity to total a and bb retrieval errors is absolutely mandatory, and it is absent as well.

REPLY: This is an important comment and we thank the review for it. Here the reviewer is referring to the IOPs retrievals, which are then used to get an estimate of spectral Kd.

In this study we have chosen to use the QAA algorithms to retrieve total IOPs (Lee et al. 2002) and a radiative transfer-based model to estimate Kd from IOPs (Lee et al. 2005a). The QAA algorithm and the latter Kd model have been validated in a very wide range of oceanic and coastal conditions (Lee et al., 2005b; 2007; Doron et al. 2006). In these validation exercises, the QAA was evaluated in both case 1 and case 2 waters. Its performance was very good. In addition, the QAA was the best algorithm for the retrieval of the total a and b_b according to the IOCCG report #5 (2005).

Here the reviewer is asking for a validation of the QAA in the Arctic waters, for which we don't have the data to achieve it. In fact, in situ dataset that includes Rrs together with total IOPs are extremely rare in the Arctic. Fortunately, the recent field programs CASES (2004), MALINA (2009) and ICESCAPE (2010-2011) programs did collected such data. An evaluation of the QAA performance using these data sets shows very good agreement between retrieved and measured bulk IOPs (i.e., ~5% and ~10% for a and bb in the blue wavelengths) (G. Zhang, R Reynolds and D. Stramski, Pers. Comm., 2012). This work is not published yet and that's why I have referred to my own evaluation of the QAA in the Beaufort Sea, published in my PhD thesis (p. 237-239; PDF available on line), which is within the same error range.

In other words, bulk IOPs retrieval using QAA is robust in a wide range of oceanic conditions based on the available literature. More importantly, satellite-based PP models used in the Arctic Ocean so far employed empirical relationships between CHL and Kd (or IOPs) or the depth of the euphotic zone (Arrigo's method; Behrenfled and Falkowski, etc). These methods are unable to account for other color-producing agents such CDOM or TSM if they vary independently. Theses compounds contribute significantly over most the Arctic shelves waters.

To illustrate the importance of Kd estimation on PP, a sensitivity analysis on Kd algorithms has been performed. To our knowledge, none of those Kd algorithms has been validated in the Arctic, but the analysis is insightful. Briefly, we have tested four different methods to estimate spectral Kd from SeaWiFS imagery and found that it is an important parameter for the PP estimate. The Kd methods tested were:

1. *IOPs from QAA and the Kd model of Lee et al 2005 (LEE; this is the one we have chosen for our study)*
2. *IOPs from CHL-based relationships from Wang et al (2005) and Kd = (a+bb)/mud (WA05; this method is analog to the one adopted in Pabi et al. 2008 and Arrigo's paper)*
3. *IOPs from CHL-based relationships from Matsuoka et al (2011) and Kd = (a+bb)/mud (MAT11)*

The authors write that they fill the gaps in monthly means of the IOPs and Chl with monthly climatological values of IOPs and Chl? What errors does this procedure induce?

REPLY: We agree that this is an important problem. But as mentioned before, gaps in ocean color observations are inherent to optical remote sensing. Different strategies could be adopted to fill the gaps. Arrigo et al. calculate the PP rates using the available data in a given region, and apply this rate to ALL open waters in that region. For example, in September when sea ice is minimum and OC data are extremely scarce and unavailable at latitude >71°, PP rates is obtained from a few available pixels in the southern portion of the region (and in several regions, this corresponds to coastal waters). Then this rate is apply to all open waters in the given region, all from 66.67°N to the north pole. How much error this procedure induces? We don't know either.

Here we decided simply not to attempt to calculate PP at pixels that have never been documented by SeaWiFS (for example, see the above figure for the month of June). So our PP estimates are, strictly speaking, only based on observations and no spatial extrapolation was made. This is another reason explaining our lower estimation of annual PP.

Clearly, we cannot provide an absolute error for our method. The only thing we can do is to compare methods. Although a very important issue, we think that this paper is not the place to perform such a methodological comparison.

Please, provide the methodology of quantifying PAR(0-) in more detail.

REPLY: We will provide more details in the revised manuscript.

Could the authors specify the values of PP in the areas under clouds? What is the ratio (outer limits) between the PP values under cloudy and cloud-free atmospheric conditions? May be you can provide a statistically substantiated table of such ratios for different provinces you are discussing in your paper? It is known that the retrievals within the areas immediately contouring the projection of clouds on the water surface are inaccurate. How did the authors combat this problem?

REPLY: Yes, PP is calculated under clouds. The cloud fraction and the cloud optical thicknesses are updated every 3-h, as provided by the ISCCP. We calculate PP assuming clear sky

conditions to assess the impact of clouds on annual PP rates for the different Arctic regions. The light attenuation by clouds, as translate in terms of PP (i.e. 1 – PP/PP_clearky), reduced the total annual PP from 18 to 34% depending on regions. The results for each region will be presented in a table. We also calculate the trends of this factor, which is increasing overtime at a rate of 0.35% per year.. A figure will be presented as well in the revised version. .

It is not clear what the reviewer mean by "It is known that the retrievals within the areas immediately contouring the projection of clouds on the water surface are inaccurate". If it as about the ocean color products retrieval over cloud shadows, it should be known that those pixels are flagged during the Level 2 processing and removed at the Level 3 processing (see http://oceancolor.gsfc.nasa.gov/VALIDATION/flags.html ; Flag 15 Very low water-leaving radiance (cloud shadow)). So clouds contours should not be a problem.

Eq. 2. Again a derivation of this expression is necessary.

REPLY: The eq. 2 is the definition of PUR given by Morel 1978 (Morel, André. 1978. "Available, Usable, and Stored Radiant Energy in Relation to Marine Photosynthesis." Deep-Sea Res. 25: 673–688.). The reference will be added. In addition, we will make Eq 2 more explicit, including the spectral diffuse attenuation coefficient:

$$PUR(z,t) = \int_{400}^{700} E^0(\lambda, 0-, t) \cdot \exp[-K_d(\lambda) \cdot z] \cdot \left(\frac{a_{ph}(\lambda)}{a_{ph}(443)} \right) d\lambda$$

The expression for the mean cosine of downwelling irradiance employed by the authors has its limits of application; they are not specified. It would be much better to use the expression suggested by H. Gordon. The authors do not provide the reasons of choosing the expression by Sathyendranath et al.

REPLY: The mean cosine (mud) is needed to converted downwelling planar irradiance (Ed) to scalar irradiance (E0). Neglecting the upwelling irradiance, then E0 ~ Ed/mud. The mean cosine varies within a relatively narrow range from 0.7 to 0.95 in the upper ocean (Morel and Gentili 2004). Under overcast conditions, which is the dominant situation, mud = 0.84 (Gordon 1989; Morel and Gentilli 2004). So mud variations are important only under clear sky conditions. Relative to other sources of errors (CHL), mud errors are relatively small.

The expression of Sathyendranath et al. 1989 (Kd ~ (a+bb)/mud) comes from the quasi-single-scattering approximation (QSS) of the radiative transfer equation (see Gordon et al. , 1975; Computed relationships between the inherent and apparent optical properties of a flat, homogeneous ocean, App. Optics). This approximation assumes that forward scattering can be considered as transmitted photon. Gordon (1989) found a similar expression from Monte Carlo simulations (Kd ~ 1.0395 (a+bb)/mud). Despite its limitations, we chose this expression to get an estimation of mud because we obtained Kd from a + bb and the sun zenith angle with the

expression of Lee et al (2005; their Eq. 10). This gives us an approximation of mud from the euphotic zone that is based on RTE simulations.   The impact of this choice for mud relative to the expression of Gordon (his Eq. 12) is expected to be small because 1) of the prevailing cloud condition and 2) the chosen expression accounts for sun zenith angle. More importantly, the potential errors in mud are insignificant for an accurate estimation of E0 relative to the error in Kd (see discussion of Lee et al 2005). If some errors were to be introduced by the E0 and mud calculation, it would be systematic and have no effect on PP trends, which is the main focus of this study.

The empirical dependence between Chl and aph employed by the authors was obtained for a very limited geographical province. Application of it to the entire area of research is certainly a stretch, and an assessment in depth of errors arising from it are mandatory, but they are absent.

REPLY: Only the spectral shape of aph is important for the calculation of PUR in Eq. 2. The statistics used is from a data set from Chukchi and Beaufort Seas from different seasons (N=382). They did not find a very large difference with the global data set published by Bricaud et al (1995;1998) and relatively small seasonal variations. A recent study found similar absorption properties of phytoplankton in the Canadian Arctic including Baffin Bay, Hudson Bay, the Canadian Archipelago and the Amundsen Gulf (Brunelle et al 2012).

We tested the statistics of Bricaud et al (1998), for case 1 water, in our PP calculations to examine the impact of the shape of aph on the annual PP.   For the year 2007, the total annual PP for the Circum-Arctic is increased by ~12% when using Bricaud et al (1998). This result will be provided in Section 3.3. The impact of aph on PP trends is not very large and is insignificant in terms of the PP trends.

2. In order to draw and analyze trends in PAR and PP it is necessary to assess the error bars, and only after that to draw the trends: the error bars might be as high as +/- 30% - 50% or even more, and drawing trends is a procedure requiring special investigations. Without such assessments the trend significance determined by the authors (regardless of the sophisticated procedures employed by the authors) is unclear.

REPLY: The error bar on satellite-based estimation of PP is more likely around 50% considering that the main variable used to calculate PP, i.e. CHL, has itself an error bar not better than 30-35%. On top of the error on Chl, errors on Kd and photosynthetic parameters are probably within the same range. Despite the large error bars that we have to deal with, one can still use remote sensing to assess trends in the environment, as long as the satellite calibration of the radiance has been corrected for instrument drift. Calibration of SeaWiFS has been rigorously maintained by NASA during its 13-years of operation, making this sensor an useful tool to detect trends in the Ocean biogeochemistry (ex.: Behrenfeld et al., 2006; Antoine et al. 2005; Martinez et al., 2009). For example, Vantrepotte and Mélin (2009) found significant positive and negative trends in the SeaWiFS Chl-a products (1998-2007) ranging between +/- 5% yr^{-1}, despite a global error of 35% on Chl-a.   The reason why the used of satellite allows trends analysis is the systematic way the data are collected and processed. The

noise in Ocean Color data and the relatively short time series are more important factors limiting the trends analyses than the bias in the data. Here, we assess the significance of the trends using non-parametric Mann-Kendall test. The figure below only shows trends with statistically significant values (p < 0.05) :

A) Climatology   B) Absolute trends   C) Standardized trends
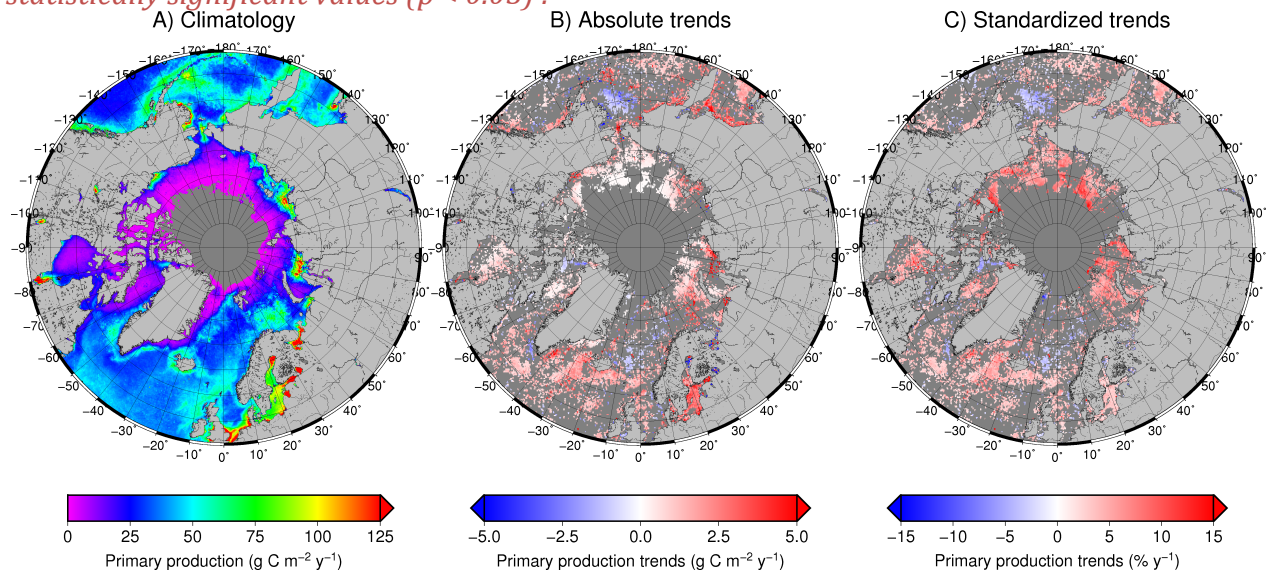
This figure shows that about half of the pixels show significant trends in PP. In the manuscript, the maps include non-significant trends to help the visual interpretation of spatial pattern in PP trends.

There is no reason to believe that our PP method is not appropriate to analyze trends in PP, CHL/KPUR or PAR. In fact the trends in PAR reported here are in agreement with several studies cited in the text (P. 13998). Finally, our method is not so different than that of Arrigo and van Dijken 2011. Both methods give similar output if they are fed with the same inputs. The most important difference between approaches resides in the choice of ocean color algorithms for both Chl-a and Kd, respectively. We chose semi-analytical approaches to minimize the variability introduced by optical constituents that vary independently from phytoplankton.

3. The statement (p. 13995, bullet 20) that the PP values determined by the authors are more accurate that the ones reported previously is not convincing in light of the comments made above. This statement is particularly astonishing because the authors haven't shown the accuracy of their PP retrievals even for some local areas/seas (although such data are available).

REPLY: There is no such statement in this manuscript and we believe that this critic is not justified. In fact, we have been actually quite clear about the limits of our method at the end of the manuscript (p. 14001). In the section 3.3 we just compare our PP estimations with previous satellite-based estimate of Arrigo and van Dijken (2011) and provide a number of reasons explaining the difference. This section will be improved by providing additional results

from our sensitivity analysis. Again our point is to say that the magnitude of PP in the Arctic based on satellite data is sensitive to the choices made regarding the ocean color algorithms and the parameterization of the P vs I relationship.

The rational of using semi-analytical and quasi-analytical approaches for CHL and Kd is based on the fact that river runoff is wide spread over Arctic Shelves. Our recent work (Fichot et al., Sc. Rep., 2013) clearly indicates this reality, which has been overlooked by previous studies. Arrigo et al. (2011) assessed the impact of CDOM on PP, but they based their analysis on a single data set collected (Matsuoka et al., 2007) in a region where the river influence is not comparable to the Siberian Shelf for instance (See Fig below).
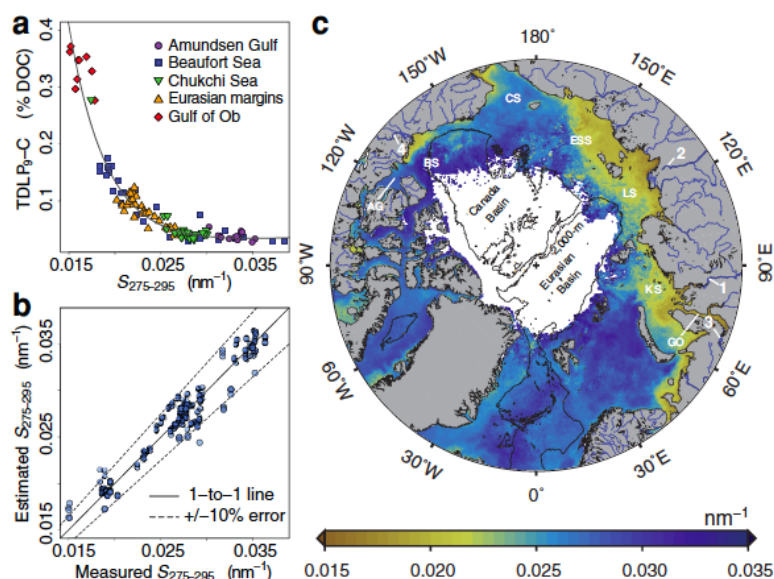

Figure 1 | The CDOM spectral slope coefficient, $S_{275-295}$, as a tracer of riverine inputs in the Arctic Ocean. (a) The relationship between $S_{275-295}$ and dissolved lignin carbon yield (TDLP$_9$-C) across various Arctic river-influenced ocean margins. (b) A performance evaluation of the $S_{275-295}$ algorithm. On average, $S_{275-295}$ can be estimated from ocean color within 4% of $S_{275-295}$ values measured *in situ*. (c) Implementation of the algorithm using MODIS Aqua ocean color providing a pan-Arctic view of an August climatology (2002–2009) of $S_{275-295}$. Increasing $S_{275-295}$ values are indicative of a decreasing fraction of tDOM. The four largest Arctic rivers are labeled and ranked in order of decreasing discharge: Yenisei (1), Lena (2), Ob (3), and Mackenzie (4). River-influenced margins of the Arctic are labeled: Gulf of Ob (GO), Kara Sea (KS), Laptev Sea (LS), East Siberian Sea (ESS), Chukchi Sea (CS), Beaufort Sea (BS) and Amundsen Gulf (AG). The contour lines represent the 2000-m isobath.

(From Fichot et al. 2013 with permission)

4. P. 13996 The statement that the largest increase in PP occurred in May is too generalized: in pelagic and shelf seas it is different. Your data should allow you documenting this issue in more detail.

REPLY: Yes. The figure below shows that the PP trend in May is important mainly in the Barents Sea. The regional monthly trends in PP will be presented in a new table (see below). This information will be added to the text.

May   June   July   August

Monthly primary production trends (g C m^{-3} y^{-1})
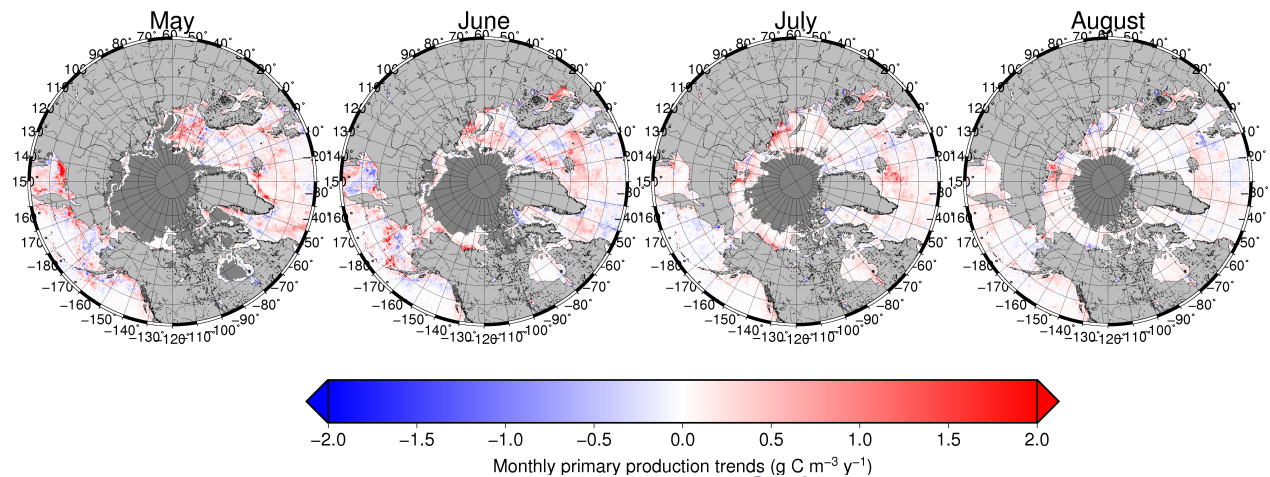
Table 3. Regionally averaged daily flux of PP (in mg C $m^{-2}d^{-1}$) and its relative trends computed for the 1998 to 2010 period (in parenthesis, in % y^{-1}). Significant trends are in bold text with superscript indicating the level of significance: (a) $0.05 < p < 0.1$, (b) $0.01 < p < 0.05$, and (c) $p < 0.01$

Region	May	June	July	August	September
Greenland Sea	263 (2.36)	352 (0.63)	193 (-0.56)	111 (0.14)	86 (0.62)
Norwegian Sea	273 (1.82)	314 (0.42)	224 (-0.05)	167 (0.76)	111 (-0.07)
Barents Sea	467 (3.13)a	233 (2.08)	168 (**1.44**)^b	123 (**1.43**)^b	93 (0.56)
Kara Sea	49 (**4.61**)^c	221 (**4.26**)^b	358 (**2.61**)^c	369 (-0.14)	311 (0.35)
Laptev Sea	122 (2.20)	220 (1.99)	505 (1.85)	527 (0.88)	410 (0.08)
East Siberian Sea	42 (5.00)	147 (4.14)	351 (2.06)	316 (**2.21**)^a	222 (**2.25**)^a
Chukchi Sea	128 (0.99)	224 (4.70)	184 (1.74)	151 (2.67)	112 (**0.86**)^b
Beaufort Sea	47 (0.39)	140 (**15.3**)^b	211 (**3.38**)^b	160 (-0.41)	96 (0.98)
Arctic Ocean	105 (5.19)	92 (**8.77**)^a	82 (**4.57**)^c	80 (**4.11**)^b	75 (**3.74**)^b
Northwestern Passages	53 (3.41)	95 (**4.10**)^b	111 (0.62)	95 (1.78)	64 (3.08)
Baffin Bay	163 (**5.06**)^b	172 (0.35)	148 (-1.04)	98 (**4.88**)^b	65 (2.42)
Hudson Bay	93 (-2.08)	154 (0.37)	194 (**2.15**)^b	164 (**2.05**)^b	128 (**1.06**)^b
Hudson Strait	60 (1.75)	198 (0.36)	240 (-0.46)	150 (-0.07)	103 (0.41)
Davis Strait	309 (0.78)	168 (0.94)	178 (0.91)	121 (0.15)	86 (1.04)
Labrador Sea	231 (1.99)	287 (0.71)	211 (0.98)	130 (-0.34)	98 (**1.93**)^b
Sea of Okhotsk	383 (**2.86**)^b	444 (1.52)	240 (0.67)	171 (3.07)	135 (**2.04**)^b
Bering Sea	343 (1.14)	345 (**2.94**)^b	225 (-0.66)	196 (0.96)	154 (**1.18**)^a
Gulf of Alaska	384 (1.60)	335 (-0.38)	254 (0.68)	221 (**2.76**)^b	171 (0.75)
Arctic + sub-Arctic Seas	302 (**1.49**)^b	280 (1.32)	222 (**0.74**)^a	177 (**1.21**)^c	135 (**0.96**)^c
Circum Arctic	276 (**2.38**)^c	237 (**1.60**)^a	214 (0.92)	175 (**1.08**)^b	133 (**0.83**)^a

5. The choice of the ratio Chl/KPUR as an indicator of ocean optical properties seems inappropriate: both of them are, firstly, interdependent, and secondly, the concentration of Chl is a function of other factors that are not related to hydro-optical properties. The conclusion drawn by the authors are at least debatable. The statement on p. 13999 bullet 15 is unsubstantiated. At least, the authors should clarify why in conditions of decreased PAR(0-) and increased KPUR the situation can only be explained in terms of a change in

ocean optical properties (whereas there are plenty of other, non-optical, factors that could be important players)

REPLY: We should not try to interpret this quantity as physiological index of phytoplankton. The rational for the use of the ratio CHL/K_{PUR} are two-fold:

  1. *Platt and Sathyendranath (JGR 1993) demonstrated, using a dimensional analysis, that any depth-integrated PP model can be generalized by the following canonical equation (see also Cullen et al. GBC 2012):*

$$Pz = \frac{P_m^B \cdot B}{K_{PAR}} \cdot f(E_{PAR}^*)$$

where: Pz (mg C m^-2 h^-1) is the instantaneous, depth integrated rate of primary production; P_m^B (mg C mg Chl^-1 h^-1) is the maximum rate of photosynthesis, normalized to chlorophyll; B is the concentration of chlorophyll at the surface (mg Chl m^-3); and K_{PAR} (m^-1) is the attenuation coefficient for photosynthetically available radiation (PAR; the total irradiance between 400 nm and 700 nm), evaluated from the surface to the depth of 1% surface PAR. The dependence on surface irradiance is modeled as a function of E^*_{PAR} (dimensionless), which is scalar PAR quantum irradiance just below the surface ($\overset{o}{E}_{PAR}$ (0-), µmol m^-2 s^-1) normalized to the PAR saturation irradiance for photosynthesis, $\overset{o}{E}_{k,PAR}$ (µmol m^-2 s^-1). For daily depth-integrated fluxes ($P_{z,T}$), the generic equation above can be modify to account for the day length (D), such that:

$$P_{z,T} = \frac{P_m^B \cdot B \cdot D}{K_{PAR}} \cdot f(E_{m,PAR}^*).$$

A further examination of our model reveals the same dependency to $\dfrac{P_m^B \cdot B \cdot D}{K_{PAR}}$. Since we used PUR rather than PAR, our model follows the same formulation but with K_{PUR} in place of K_{PAR}. This dependency can also be demonstrated mathematically by integrating eq 1 with some assumptions. Here daily PP is calculated using

$$PP = CHL \cdot P_m^B \int_{z=0}^{\infty} (1 - e^{-\frac{PUR(0-)\exp(-K_{PUR} \cdot z)}{E_k}}) dz.$$

The factor $(1 - e^{-\frac{PUR(0-)\exp(-K_{PUR} \cdot z)}{E_k}})$ tends to decrease exponentially with depth following the exponential attenuation of the downwelling irradiance. The depth integration of $(1 - e^{-\frac{PUR(0-)\exp(-K_{PUR} \cdot z)}{E_k}})$ is also proportional to surface irradiance. The nonlinearity of the P vs I curves can be replaced by the factor f(PUR) for simplicity. Thus, the expression can be rewritten as

$$PP \approx CHL \cdot P_m^B \cdot f(PUR) \int_{z=0}^{\infty} PUR(0-)\exp(-K_{PUR} \cdot z) dz$$

or

$$PP \approx CHL \cdot P_m^B \cdot PUR(0-) \cdot f(PUR) \int_{z=0}^{\infty} \exp(-K_{PUR} \cdot z) dz$$

Integrating the equation yield:

$$PP \approx CHL \cdot P_m^B \cdot PUR(0-) f(PUR) \Big|_0^{\infty} \frac{\exp(-K_{PUR} \cdot z)}{-K_{PUR}} dz$$

$$PP \approx CHL \cdot P_m^B \cdot PUR(0-) f(PUR) (\frac{1}{K_{PUR}})$$

So if PP is normalized to PUR(0-), we obtain the

$$\frac{PP}{PUR(0-)} \approx P_m^B \cdot f(PUR) (\frac{CHL}{K_{PUR}})$$

2. *In our approach, CHL and K_{PUR} are not fully dependent. It is true that KPUR will tend to increase when CHL increases, but not systematically. This is because Kd is not a single function of CHL as in previous models, but is estimated using the QAA. Therefore, the ratio CHL/KPUR is more strongly correlated to PP than CHL or KPUR taken separately. In other words, CHL/KPUR gives a measure of the biomass relative to all the optical constituents that contribute to light attenuation (CHL, CDOM, NAP, BBP, water). It is not a phytoplankton physiological index.*

We agree with the reviewer that the use of this index was not clearly explained. We will provide a new figure (see below) with 4 panels illustrating the following relationships:
   - *A) CHL versus KPUR,*
   - *B) CHL versus PP/PAR(0-)*
   - *C) KPUR versus PP/PAR(0-)*
   - *D) CHL/KPUR versus PP/PAR(0-)*

These relationships help to understand the point we wanted to make in the paper.

The panel A) shows the dependence of K_{PUR} on CHL and compares it to the relationship predicted by the Morel and Maritorena (2001) model built for the clear oceanic waters. In July 2007, for example, 82% of the variance in K_{PUR} in the circum-Arctic was explained by CHL. The remaining variance (18%) was due to other optically significant constituents, or phytoplankton pigments characteristics. It also shows that the K_{PUR} for a given value of CHL is much higher than the value predicted the case-1 water model published by Morel and Maritorena (2001). The differences are more pronounced in the low chlorophyll-a concentration range and tend to diminish as CHL increases. When PP is normalized to incident irradiance (PAR(0-)), PP*, a strong positive relationship is obtained with CHL ($r^2=0.92$; panel B). Note that PAR(0-) alone explained 18% of the variance in PP (not shown). Similarly, PP* was also positively correlated to K_{PUR} due to its dependence on CHL ($r^2=0.58$; panel C). Panel D shows the strong relationship existing between PP* and the ratio of CHL/ K_{PUR} ($r^2=0.98$). The remaining variability may be attributed to the non-linearity of the P vs I relationship (i.e. f(PUR)).
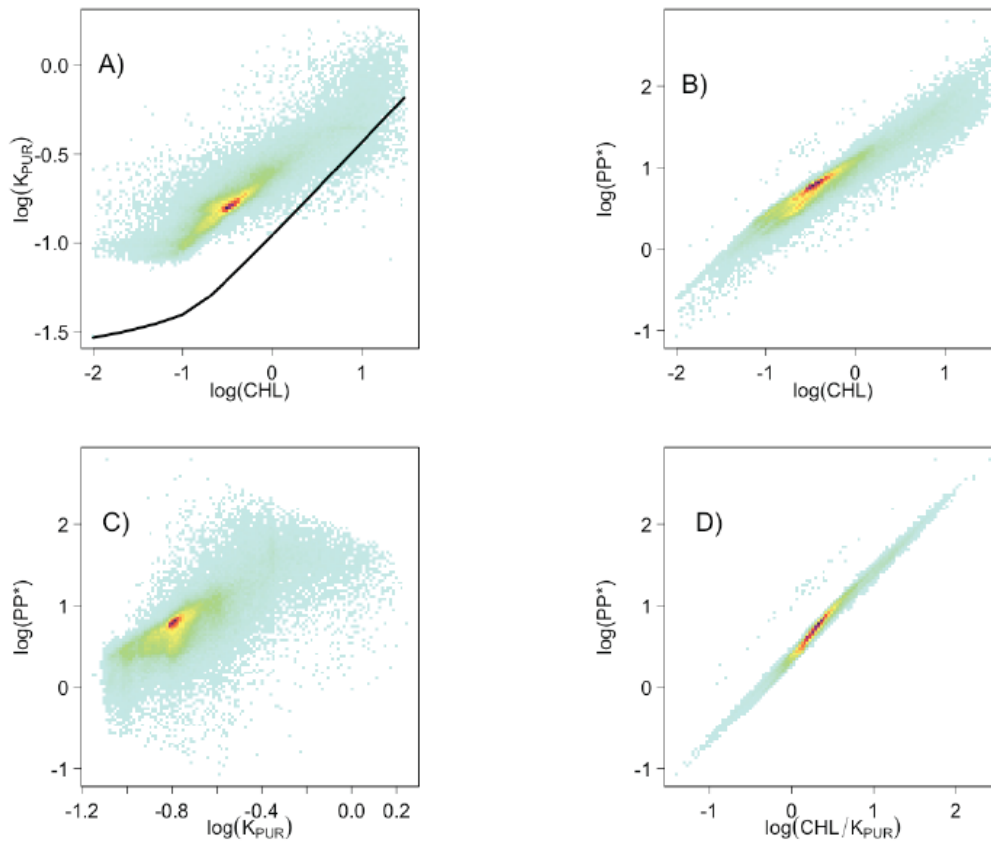
Fig. 4. Examples of relationships between (**A**) CHL and K_{PUR}, (**B**) CHL and PP normalized by surface PAR (PP*), (**C**) K_{PUR} and PP*, and (**D**) CHL/K_{PUR} and PP* for the month of July 2007. The black line on panel (**A**) is K_{PUR} predicted from CHL using the empirical model of Morel and Maritorena (2001) for K_d and Matsuoka et al. (2011) for a_{ph}.

We agree with the reviewer that "there are plenty of other, non-optical, factors that could be important players" *to explain the fact that could PP increase where PAR(0-) decrease. But because of the way the model is constructed, only changes in the inputs (i.e., PAR, CHL or/and Kd) of the model can explain this result. Our model does not account for physiology or changes in phytoplankton community. So if we observe a change in PP, it can only be explain by a change in one of the input parameter. That being said, any changes in optical properties, however, may be resulting from changes in phytoplankton biomass or in its concentration relative to non-chlorophyll optical component such as CDOM or non-algal particulate matter. Such changes can be driven by changes in the physical environment: nutrients supply, mixed layer, river runoff, etc.*

The phrase "In several Arctic sectors: : :" is not concrete and lacks references

REPLY : We will be more precise here, indicating the sectors we were referring to, i.e. the Canada Basin and the Hudson Bay.

On page 14000 the authors declare: CDOM production from microbial activity is 'delayed' Right, but what is the delay? Please, specify. Because it is my impression that offering the data in Fig.5a, the authors imply the delay of about 1 month. Please, provide quantitative data or references.

REPLY: We will remove this hypothesis, which was not well sustained with data. See below

6. P. 13999: the authors explain low values of the ratio Chl/KPUR during June-August exclusively by the microbial production of CDOM. Given a very intensive water transport of Atlantic waters into the Barents Sea, and a further transfer of Barents waters to the East and in the White Sea, it appears unrealistic that the effect of CDOM on Chl/KPUR should last so long. It seems to me that this case explicitly shows inappropriateness of the chosen ratio as an analytic tool. Other factors can come in to play. For instance for August, the authors do not consider the impact of E. huxleyi blooms in the Barents Sea.

REPLY: We agree with the reviewer about this point. It is true that other factors than CDOM play a role in CHL/KPUR. In fact, it is true that E. huxleyi blooms would probably decrease CHL/Kd considering the strong backscattering of coccolithophores. In such bloom conditions, however, we don't expect our method to perform well. In any case, if CHL/Kd is low, our model will yield low PP. We would need adapted algorithms to deal with such cases, which are not implemented in the current version of our model (nor in any other models used in the Arctic). We will acknowledge this fact in the revised version of the paper. In addition a closer examination of KPUR in the Barents Sea in later summer does not support our previous interpretation.

7. P. 14001, bullet 5: The authors' speculations are indeed sheer speculations. Without reliable data, it is better to abstain from such suppositions.

REPLY: We can remove this sentence, but we have evidences that the North Water is undergoing important physical changes (Kwok et al 2010; Munchow et al. 2011) and this not a speculation. There are evidences from passive microwave and optical remote sensing that the ice bridge between Greenland and Ellesmere did not form in the recent year and the amount of ice volume transported into Nares strait has increased significantly (Kwok et al. 2010). We also have evidences, from benthic bivalves, that the export of organic matter have change dramatically in the last decade (Gaillard et al., to be submitted). Our assessment also point out a significant change in phytoplankton bloom timing in the North Water. That being said more work, at fine spatial and temporal scales, is needed to elucidate which factors is driven the change in the timing of the spring bloom (SST, Wind, Sea ice). We said : « Our results indicate that the timing of this bloom may have changed over time ». We will the "may have" by

*"have". We will also rephrase "*We speculate that bloom dynamics are linked to changes in the quantity or properties (e.g. salinity, nutrients, CDOM) of the in-flow of cold, nutrient-depleted waters coming from the Arctic Ocean *" to make sure we don't give the impression that we are speculating. Finally, it is important to point out that PP is not increasing everywhere in the high Arctic.*

8. P. 14001, bullet 10: the subsurface maximum in Chl and the respective PP additions in the water column are not small. PP is underestimated by 10-11%. It is strange that the authors for their argumentation cite the work by Arrigo and Dijken: the latter, contrarily, state that Chl subsurface maxima are characteristic of the Arctic Basin and their contribution to PP should be accounted for. This is supported by our data as well.

*REPLY: Arrigo et al (2011) stated: "*Over an annual cycle, the error is approximately 8% ». *They found that the error is larger (-20%) in later summer in the Arctic Basin (Beaufort Sea). 8% underestimation over the annual cycle is actually very small relative to other uncertainties. Another detailed study (Ardyna et al BGD 2013) of the impact of SCM on the annual and seasonal PP corroborates the findings of Arrigo et al. They even showed that annual PP may be slightly overestimated. Briefly, Ardyna et al (2013) divided the annual cycle into three distinct periods: 1) bloom and pre-bloom; 2) post-bloom and 3) winter. They showed that assuming vertically homogenous water column in term CHL leads to an overestimation of the PP during the pre-bloom condition. During post-bloom conditions, when SCM is well developed, the assumption leads to an underestimation of PP. When assuming a uniform chl a profile, annual PP overestimates vary between 3.7 to 10.9% of the total annual PP estimates across the different regions of the Arctic Ocean. Given the lower contribution of the post-bloom period (<30%) to annual PP estimates, the annual PP underestimates (i.e. 0.1 to 6.9 %) remain lower compared to annual PP overestimates except for the Beaufort Sea.*

It is important to mention that most in situ observations are performed in late summer when SCM is well developed. Much less data of the spring bloom are available from field observation, when most of the annual PP occurs. Satellites provide the opportunity to document the whole seasonal cycle (see Perrette et al. 2011), except for the under-ice bloom conditions (Arrigo et al 2012; Mundy et al 2009), which may be more important in the future. This limitation is mentioned on page 14001.

9. It is correct that under-ice phytoplankton blooms can possibly represent a significant portion of the Arctic annual PP. But it should be taken into account that the length of the ice-edge in the Arctic decreases progressively, and the importance of this factor is expected to rapidly drop in the years come. Nothing of this is mentioned in the paper.

REPLY: If the reviewer means the geometrical length, this seems irrelevant. The <u>area, which is what counts,</u> of the seasonal ice zone is becoming larger over time because the winter extent of the ice-pack has remained relatively constant while the summer extent has decreased. That result suggests that the ice edge bloom conditions may be more important than before. But at the same time, the reduction of the thickness of sea ice can allow light to penetrate in the water column and trigger the bloom under the ice. At this stage it would be premature to discuss these implications.

10. In the conclusions, the authors enumerate the problems to be solved/ the knowledge that needs to be furthered. The list is incomplete and lacks such factors as nutrients availability, stratification conditions/water freshening, change of the phytoplankton composition in conditions of climate warming, the effect of increasing occurrence of deep cyclones capable of increasing PP, the impacts of NAO, PDO &IPO, and AO on the system of currents, and some others.

REPLY: *In the conclusions, we listed what we think was the most important problems <u>from a</u> <u>remote sensing monitoring perspective</u> (first three points). The last point is "4) to examine in* more details the environmental variability (e.g. SST, wind speed, storms frequency, etc.) to better understand the most important drivers of changes in the ocean optical properties and PP ». *We will provide more details in the revised version.*

11. Summing up, I have to underline that for me as a reader, the paper's results will appear persuading only on condition that the authors revise substantially their paper at least answering the questions and meeting the recommendations posed above.

REPLY: We thank the reviewer for the throughout evaluation of our manuscript. The major outcome of this work in term of trend in PAR(0+), PAR(0-) and PP. We showed that the cloudiness should not be ignored when considering the trends in PP from space. The magnitude of the decrease in PAR(0+) due to increasing cloudiness at latitude >68N compensated for a large fraction the increase in PAR(0-) resulting from the lost of sea ice. We also have made the first attempt to consider the optical complexity character of the Arctic waters in a PP model. There is now several evidences that the Arctic, and, in particular the Arctic Shelves, does not belong to the classical case 1 waters (Fichot et al. 2013; Stedmon et al., 2011; Matsuoka et al., 2011; Ben Mustapha et al. 2012; Bélanger et al. 2013; Antoine et al 2013, etc). This is another step of the development of a monitoring tool of the Arctic marine ecosystems. We hope that Reviewer 1 will recognize these significant contributions.

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