We are very grateful for your detailed analysis that allowed us to improve the quality of our manuscript. We agree with your comments and we've all taken into account. Your comments are in bold text and our responses in plain italics. You had pointed out that the article was too long with too much information and oceanographic parameters presented, and that the scientific message is overlay by the description of the results and their interpretation. We fully agree with this general observation and to address them, we decided to split the article into two separate manuscripts M1 and M2 whose title and objectives are given in the general introductive response to the referees. Great benefit to the clarity of the scientific message and structure of the text resulted from this separation. Now, the description is shorter and the figures have been updated and reduced in number. The importance given to species-specific data from microscopic counting has been greatly improved by dedicating the manuscript 2 to the distribution of phytoplankton in the CHINARE 2008 area. The description of the planktonic communities by the double approach pigment and cells counting increase the robustness of the conclusions and allowed a comparison with previous campaigns in the same area. We also included additional tables to list the abbreviations (Table 1, M1) and the geographic region (Table 1, M2). English has been corrected with the help of an English speaking person. We want to thank you again for exhaustive list of specific comments which allowed us to correct many imperfections of our manuscript.

General comments: The manuscript by Coupel and co-workers builds on a large dataset of phytoplankton composition (determined by HPLC accessory pigment analysis), biomass (expressed in Chl-a and mg C m-3) and productivity across the Chukchi shelf to the central Arctic (Canadian) basin. The biological dataset is linked to the hydrographical regime and the prevailing physical-chemical (salinity, temperature, light and nutrients) conditions. Given the poor spatial-temporal data coverage of this region the large dataset by Coupel et al. is of high relevance for comparative studies and the detection of climate change induced shifts in this integral part of the marine food web. I therefore hope to see this manuscript published.

However, in its current state the manuscript does not merit publication. The manuscript is very tedious to read, overly long, not well structured, largely descriptive and suffers from poor English. I have refrained from correcting the English because it would have distracted from the scientific content. The authors use a lot of inappropriate wording, wrong grammar and sometimes mix up past and present tense. I therefore highly recommend that the manuscript is carefully checked by a native speaker prior to re-

submission.

Sincere apologies for the poor English, we have corrected all grammatical mistakes and have asked an English native speaker to edit entirely both manuscripts.

The authors use a lot of abbreviations for geographical locations, accessory pigments and water masses throughout the text which makes it difficult to read. The authors should consider an additional table with geographic/water mass abbreviations (as has been done for pigments).

The number of abbreviations has been greatly decreased reported in Table 1 (M1). In the revised manuscript, most of the abbreviations to describe geographical locations and water masses are only used in the figures to not overload it, but not in the text. The signification of these abbreviations is indicated in the figure caption.

Some of these abbreviations (e.g. SCM, AMZ, PML and SFL) were apparently coined by the authors themselves. I am for example not aware that Cota et al. 1996 and Hill and Cota 2005 explicitly used the term SCM. The authors should rather use commonly used terms or at least explain their new terms better.

AMZ (Active Melting Zone) was replaced by the MIZ (Marginal Ice Zone) as defined by Carmack and Wassmann (2006) (see Figure 3, M1). The SCM (Sub-surface Chlorophyll Maximum) introduced in section 1.4. of M1 and differs from the DCM (Deep Chlorophyll Maximum) commonly observed in the tropical ocean. The SCM (40-80m) is shallower than the DCM (>100m) and most of the time correspond with the depth of the sub-surface biomass maximum (Martin et al., 2010). The SCM is a dominant feature of the Arctic Ocean and should be considered differently to the common DCM. The Polar Mixed Layer (PML, Coachman & Aagaard 1974, McPhee, et al. 1998) is the term used to call the Surface Mixed Layer in the Polar Oceans (see section 2.1., M1). In opposition to the "normal" surface mixed layer, the PML is not formed by the winds but by thermodynamic processes linked to the formation and melting of ice. In general the PML occupies the upper 25-50m, presents low salinity (depth of the seasonal convection) and is ubiquitous in the Arctic Ocean proper (Codispoti, 2005). Concerning the SFL (Surface Freshwater Layer), we choose to coin this new abbreviation to point out the thickness of the layer affected by the freshening caused by ice melting and river runoff. We also use the LFW (Liquid Fresh Water) index in M1, which was advanced by Rabe et al., (2011) and characterizes the quantity of freshwater injected in *the surface layer (see section 1.2., M1).*

The figures and tables are difficult to read due to their small size and small font of the axis labels. For a better comparison it would be helpful if the same scale would be used in some of the figures.

Particular attention has been done to improve the consistency of the figures scales and axis labels. Most of the figures have been modified and reorganized to increase clarity. The main changes in the organization of the figures are listed below

<u>The Manuscript 1 (M1) includes 11 figures and 2 tables:</u>

Figure 2 has been simplified by removing the map 2d. Figures 3b and 3d were replaced by Figure 4. Figure 6 was replaced by Figure 7 in which primary production data have been added. Figure 9 has been removed. Figures 11 and 12 have been simplified and replaced by Figures 10 and 11. Data presented in figures 7 and 8 were strongly modified and displaced in the manuscript 2 (M2).

<u>The Manuscript 2 (M2) also includes 11 figures and 2 tables:</u>

Two figures present the pigments data (Figures 3, 4) and 2 figures allocate to the taxonomy data (Figures 5, 6). One figure is about the comparison between pigments and taxonomy data (Figure 7) and the figures 9, 10 and 11 compare our data with historical data.

Although mentioned in the material and methods there is hardly any mention of data from microscopic cells counts apart from general categories like diatom, dinoflagellate, nano- and picophytoplankton abundances and biomass. I was hoping to see more species-specific data as there are huge differences between species within these very generic categories.

Because of previous publication on the species counts during the same cruise (Joo et al., 2011), more importance was given to the pigments data. However, after the revision, we have decided to focus a paper (M2) on results of both microscopy and pigments. Thus, the importance of the microscopy data is greatly improved. The phytoplankton communities are described at the class level for small phytoplankton and at the species levels for diatoms (see section 2.2.3. in M2). Dominant species are also indicated for the small phytoplankton.

In the conclusions the authors state "A strong divide between the shelf and open ocean waters was noted in terms of species composition....", this is not justified given the low taxonomic resolution of the study. Instead the authors devote a lot of text to the description of the CHEMTAX analysis. The message of the manuscript gets lost in the overly long description of results and discussion.

The comparisons between CHEMTAX and microscopy have been strongly reduced from the manuscript. Instead, the description and discussion devotes to the taxonomy obtained by microscopy have been improved in the second article (M2). The pigments (HPLC) and microscopy (Taxonomy) are presented in parallel in section 2.2.2. and 2.2.3 of the manuscript 2. We only use CHEMTAX as a tool to interpret the pigments data and compare it with microscopy count. No endeavors were made to validate CHEMTAX in our manuscript. Therefore, we point out that the interpretation of phytoplankton distribution through pigments could strongly differ from microscopic observations (section 3.1. M2). A complete work on the validation of CHEMTAX is needed and will be considered in the near future thanks to the acquisition in 2009 and 2010 of pigments data in the Arctic.

The conclusion is far too long (>1 page) and should instead briefly summarize the main findings of the study.

The conclusions have been reduced and adapted to the goal of each manuscript thanks to the sharing of the findings in two manuscripts

Part of the material and methods are already included in the end of the introduction. Furthermore the introduction should not include figures.

Reference to figures has been removed from the introduction. In M1, the part of materials and methods included at the end of the introduction was removed and included in a part called "Topography description" in the Materials and methods (section 1.1., M1).

Specific comments:

Abstract:

Page 6920, line 6: 2008 was not an exceptional ice retreat but followed the general sharp decline observed since 2007 (see Stroeve et al. 2011 and Perovich et al. 2010).

The use of "exceptional ice retreat" has been replaced by "recent ice conditions" as proposed by Referee #4 or we used the expression "followed the general sharp decline observed since 2007". We have added a few very recent references about the decrease of ice in the Arctic (Perovich et al., 2010, Stroeve et al., 2011).

Page 6920, lines 13-25: How does the first part of the paragraph (productive ice free Chukchi shelf vs. oligotrophic ice covered Canadian basin) fit with the latter part (1. unproductive ice free conditions, 2. productive MIZ (or AMZ) and 3. heavy ice cover and low biomass)?

The division between a eutrophic shelf and an oligotrophic basin is clear. But over the basin, we would like to highlight the relative high productivity of the MIZ (Marginal Ice Zone) in comparison to the average conditions over the basins. To avoid any confusion, we mentioned in the text that the MIZ is not productive (like on the shelf) but more productive than the rest of the basins (section 2.2. in M1). Moreover, we made a distinction between oligotrophic conditions over the MIZ and at the SCM (Sub-surface Chlorophyll Maximum) against ultra-oligotrophic conditions in the surface waters of the ice-free and heavy ice covered areas of the basins (see section 3.1.3. in M1).

Introduction:

Page 6921, lines 11-12: Use more recent literature (e.g. Stroeve et al. 2011 and Perovich et al. 2010).

Page 6921, line 16: Use more recent literature (e.g. Wang&Overland 2009 and Boe et al. 2009)

This has been taken into consideration (see section "Introduction" of M1).

Page 6922, line 12: Be more specific! I assume large cells > 5 _m are mainly diatoms.

In the second manuscript, we defined the different size classes of phytoplankton (section 1.3, M2) and add the information that the large cells >5µm are largely dominated by diatoms.

Page 6922, lines 21-23: Why is the reduction in ice algae associated with reduced carbon export? It is well known that phytoplankton sink and they usually account for the much larger fraction of annual primary production and biomass build-up.

The change from Arctic to subarctic conditions associated to the earlier ice retreat could alter the marine ecosystem structure and the degree of pelagic-benthic coupling. The ice-related production, favouring benthic and bottom-feeding communities (bearded seal, walrus, gray whales), is being replaced by one dominated by water column primary production (Piepenburg 2005). In water column-based system, zooplankton grazing and the microbial loop prevail, thus limiting carbon export to the sediments with dramatic impacts on the benthic communities (see section "Introduction" of M2).

Page 6922, lines 23-24: Species composition cannot be delayed. Why is the timing of the spring bloom delayed by early ice retreat? I thought the opposite is true!

This hypothesis comes from the work of Hunt et al. (2002). This work suggests than when the ice retreats in late winter, there is insufficient light to support the bloom, and the bloom is delayed until late spring when solar heating has stratified the water column sufficiently to prevent algal cells from sinking. When the ice retreat comes later in the spring, then there is sufficient light to support an ice-associated bloom. This bloom can start under the ice, or at the ice edge in ice-melt-stabilized water. Thus, early blooms occur in cold water and are related to ice-edge blooms, whereas late blooms occur in relatively warm water and are not related to the ice-edge (see section "Introduction" of M2). Of course "Species composition cannot be delayed", this mistake has been corrected.

M&M:

Page 6924, paragraph 2.2: At which depths were nutrients sampled? What is the vertical resolution? This is important to know for the nutricline determination.

Information about the sampling depths of the nutrients has been added in the Materials and method section: "Nutrients concentrations were acquired for the 85 CTD stations. Depending on the water column depth, 3 to 10 depths were sampled from surface to bottom. At least 4 depths are sampled in the upper 100m (section 1.3., M1)."

Page 6924 and 6925, line 22 and 3: orthosilicic acid instead of acid orthosilicic

This has been corrected (section 1.3., M1).

Page 6925, paragraph 2.3: The vertical resolution of pigments is very low (2-3 samples per profile). Was Chl-a sampled at a higher vertical resolution? This is important to

know for the determination of the SCM.

The resolution of the chla profile was the same than the one from pigments (2-3 depths only). The SCM (Sub-surface Chlorophyll Maximum) was determined onboard, before sampling, by the depth of the maximum in situ fluorescence. In this study it is assumed that the maximum of chlorophyll is well connected to the maximum of fluorescence. Unfortunately the relatively low vertical resolution of pigments samples limits our interpretation of the DCM thickness although the shape of the fluorescence profile can give us an approximation.

Page 6926, line 9: chrysophytes and prymnesiophytes should be written in lower case as the other groups

This has been corrected.

Page 6927, lines 2-3: I assume the screens were not made of steel!

The method to estimate the hourly carbon and nitrogen uptake rates was fully described in (Lee et al., 2011). Dr Lee's group has performed the carbon and nitrogen uptake measurements. In materials and method section of Lee paper, they explained: "To measure the phytoplankton uptake rates of carbon and nitrogen, seawater samples from each light depth were transferred from the Niskin bottles to 1-L polycarbonate incubation bottles, which were covered with stainless steel screens to simulate the light intensity at each depth. (Lee et al., 2011)"

Page 6927, lines 3-4: How much 13-C was added?

Precise information about the method to estimate carbon uptake is described in Lee et al., (2011). Heavy isotope-enriched (98–99%) solutions of $H^{13}CO_3$ *were added to the samples at concentrations of ~0.2 µM (*$^{13}CO_2$*, Lee et al., 2010). Bottles were incubated in a deck incubator cooled with running surface seawater. The* ^{13}C *enrichment was about 5–10% of the total inorganic carbon in the ambient water, as determined by titration with 0.01N HCl (Anderson et al., 1999) during the cruise (see section 1.4., M1).*

Results:

Page 6929, line 16: What exactly is a polar mixed layer and what distinguishes it from a "normal" surface mixed layer?

The Polar Mixed Layer (PML, Coachman & Aagaard 1974, McPhee et al., 1998) is the term used to call the Surface Mixed Layer in the Polar Regions. In opposition to the "normal" surface mixed layer, the PML is not formed by the winds but by thermodynamic processes linked to the formation and melting of ice. In general the PML occupies the upper 25-50m, presents low salinity (depth of the seasonal convection) and is ubiquitous in the Arctic Ocean proper (Codispoti, 2005).

Page 6929, lines 18-19: Why is the PML thinner over ice free waters? Wind mixing should increase with open water conditions.

Over the ice free waters, the melt water inputs strongly stratify the surface layer by adding fresh water. Moreover, warming by solar irradiance reinforces the stratification. This results in a stabilization of the water column. In ice covered waters, the brine rejection and the movement of ice maintain a thicker mixed layer (section 2.1., M1).

Page 6929, lines 28-30: How do SFL and PML relate to each other? For example in the Beaufort Gyre the PML is shallow whereas the SFL is deep. Isn't the SFL determining the surface mixed layer depth?

The SFL (Surface Fresh Layer) and the PML (Polar Mixed Layer) are not correlated. The SFL is not a homogenous layer like the PML and does not give any information about mixing. SFL is the layer where salinity is lower than 31. The thickness of the SFL informs about the intensity of the freshening due to melt waters and rivers input which are the only sources able to decrease the salinity under 31. The SFL is important to understand the position of the nutricline and the DCM (section 1.2., M1).

Page 6931, lines 16-19: Are the high nitrate and silicate concentrations associated with upwelling along the shelf break?

Yes, ice edge upwelling has been already described for the Greenland Sea MIZ (Buckley et al., 1979, Mundy et al., 2009), although the effects on the biological regime are not known. A second hypothesis is that the high nutrients concentrations near the surface can be related to the presence of the ice cover which could have prevented the establishment of the stratification and the nutrients consumption until now (section 3.2., M1).

Discussion:

Page 6935 and 6936, section 4.1.1: This section could be used for a more methodological paper and not needed in this detail here.

The comparisons between CHEMTAX and microscopy have been strongly reduced from the manuscript. Instead, we present results of microscopy and pigments and discuss them in the second article (M2). CHEMTAX is used in order to interpret the pigments data in term of dominant phytoplankton taxa (Figure 7, section 3.1., M2) and compare the results with the taxonomy. Complementarity and differences of both approaches are discussed (section 3.1, M2). No endeavors were made to validate CHEMTAX in our manuscript. Therefore, we point out that the interpretation of phytoplankton distribution through pigments could strongly differ from microscopic observations (section 3.1. M2). A complete work on the validation of CHEMTAX is needed and will be considered in the near future thanks to the acquisition in 2009 and 2010 of pigments data in the Arctic.

Page 6935, line 20: no need to show figures of non-significant regressions (fig. 9b and c).

Figure 9 has been removed. The absence of regression will be simply mentioned in the text as proposed by Referees #3 and #4 (section 3.2., M2). We point out the phytoplankton distribution inferred from pigments markers should be logically contrasted with microscopy because some carotenoids and chlorophyll shared among different algal classes (Jeffrey et al., 1999).

Page 6937, line 8: Is the productivity ratio a commonly used term? Shouldn't it be chlorophyll specific productivity (productivity normalized to chlorophyll)?

Finally in the new manuscript, the term "productivity ratio" was not used. Alternatively, the term "productivity" was used to express the idea of a ratio between the chla production and the carbon production. Although this ratio is not quantitatively estimated in the manuscript, we use the expression "low productivity" to express a low carbon production despite relatively high in-situ chla concentrations.

Page 6937, lines 23-26: The high Chl-a/C ratio is an adaptation to the low light levels experienced in the SCM (shade adaptation).

Yes, this information has been added in the manuscript to explain the high chlorophyll at SCM over the basin. " Although biomass is relatively high at SCM (0.41 ± 0.26 mg chla $m^{-3,}$ *Fig. 7a), the primary productions rates are very low (0.009±0.001 mgC* m^{-3} h^{-1}*, Table 1)* *reflecting slow growing population with high chla/C ratio adapted to growth in low light environment." (see section 3.3., M1).*

Page 6942, line 21: Write species name full: Melosira arctica

This has been corrected (see M2).

Page 6942, lines 20-23: What about the microscopic phytoplankton data from this study? Do they corroborate the presence of diatoms near the surface and which species were present?

These questions will be tackled in Manuscript 2 (section 3.1., M2). Microscopy does not corroborate the drastic dominance of diatoms in the entire surface waters of basins highlighted by the pigments. Nevertheless, microscopy revealed that the diatoms Niztchia spp. and Fragilariopsis spp. account for 50% of the biomass in the relatively rich waters of the MIZ (Marginal Ice zone, Figure 7g, M2). In the rest of the basins, surface waters are ultra-oligotophic (chla<0.1mg m^{-3}) and diatoms represent less than 15% of the phytoplankton biomass. We suggest that the dominance of species could not be well distinguished in such waters because most of the accessory pigments concentrations are under the limit of detection. Moreover, we suspected that a large part of the fucoxanthin considered to belong to diatoms could be associated to other species such as haptophytes or dinoflagellates (Rodriguez et al., 2002).

Page 6945, line 15: Abundances of 4970 and 504 cells l-1 are actually very low. I assume you are referring to abundances per ml?

Yes, there is a mistake, the units is cells ml^{-1}

Page 6946, lines 10-12: How do you justify this conclusion (haptophytes dominating during ice retreat while diatoms dominate at high irradiance)?

This part of the discussion has been corrected at the light of the comparison between taxonomy and pigments results. We observe the diatoms to be well adapted to growth fast when the ice break and irradiance increase in the surface layer of the basins. Just after ice shrinking, there is sufficient light and nutrients to promote large cells as diatoms. Haptophytes seems to be increase in the whole Arctic basin of the Pacific sector of Arctic Ocean and especially at SCM (sub-surface chlorophyll maximum). The deepening of the

SCM, due to large freshwater input in surface during recent summer of extensive ice melting, could explain the prevalence of haptophytes at SCM (see section 3.2. in M2).

Page 6948, lines 8-12: To what extend is your conclusion (that your observation in 2008 does not fit into the general trend of increased productivity with increased ice melting) biased by the late summer season encountered during the cruise?

We note in the revised manuscript (1), than the conditions described in 2008 constitute only a snapshot of the Canadian Arctic. In the light of our work, conclusions about the impact of global change on productivity are now presented as a hypothesis of what future trends could be (see section 3.1.3 and conclusion in M1). In Manuscript N°2 about phytoplankton taxonomy, we will point out the differences of phytoplankton distribution between the 2008 campaign and two previous oceanographic cruises in 1994 and 2002 over the Pacific Arctic deep basins and will advance some physical hypothesis, principally change in irradiance and freshwater budget, that could explain the observed differences (see section 3.2. in M2).

References:

Page 6952, line 18: Piepenburg D instead of Piepenburg P

This has been corrected.

Figures and tables:

Page 6955, Table 1: Use either haptophytes or prymnesiophytes

All these comments were taken into account and corrected.

Page 6958: Fig. 2 c and d should be the same scale

The map 2d has been deleted from Figure 2 because of no use in the new version of the manuscript.

Page 6961: Fig. 5 a, b and e should be the same scale.

The maps in Figure 5 are now in figures 6. The scales are now consistent.

Page 6963, Fig. 7: I do not see where Chl-a is depicted in this figure.

Chla has been removed from Figure 7 (Figure 7 is now presented in Manuscript #2.)

Page 6964, Fig. 8: This figure is way too small.

CHEMTAX data in figure 8 are replaced by a histogram where CHEMTAX results are averaged over 4 regions in surface and at DCM (Figure 7k and 7l).

Page 6966, Fig. 10: Why do the dinoflagellates show up in panel f and not in panel c?

The figure 10 no longer exists in the new version of the manuscript. Nevertheless, dinoflagellates groups is presented in the figures of manuscript 2 (Figure, 5-10).

Page 6967, Fig. 11: Panel labels (a, b, c ...) and y-axis (depth in m) are missing.

Figures 11 and 12 have been simplified and replaced by Figures 10 and 11 in Manuscript#1. In spite of separate the shelf and the basins in two different figures, the figures 10 and 11 of the manuscript 1 described the parameters in the upper 300m and in the upper 100m, respectively. All the missing labels have been added.

Suggested references:

Stroeve JC, Maslanik J, Serreze MC, Rigor I, Meier W and Fowler C (2011) Sea ice response to an extreme negative phase of the Arctic Oscillation during winter 2009/2010. Geophysical Research Letters 38.

Perovich DK, MeierW, Maslanik J and Richter-Menge R (2010) Sea Ice Cover [in Arctic Report Card 2010], http://www.arctic.noaa.gov/reportcard.

Wang MY and Overland JE (2009) A sea ice free summer Arctic within 30 years? Geophysical Research Letters 36.

Boe JL, Hall A and Qu X (2009) September sea-ice cover in the Arctic Ocean projected to vanish by 2100. Nature Geoscience 2(5): 341-343.

All these references have been added in the manuscript.

We would like to sincerely thank you for your advices and constructive comments.

Sincerely,

Pierre Coupel on behalf of all the authors

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