In the following reply to our reviewers, we reproduce their comments in *italic Calibri font*, while our responses appear in normal Times New Roman font.

Response to referee 1

This work convincingly demonstrates that ice-edge bloom is nearly ubiquitous in the Arctic, a phenomenon well documented in Antarctica, but not so well in the Arctic if we exclude the Barents and Bering Seas. In fact, it has never been documented in many arctic regions (e.g. most of the canadian archipelago). It is based on a careful, and original, analysis of satellite ocean color and sea ice data collected over the Arctic Ocean. The authors are also aware of some inherent limitations of ocean color data (but not all) and discussed some of them. Although the PP model used is certainly not appropriate for the Arctic Ocean (Behrendfeld & Falkowski 1997), the results are not over-interpreted. They found that ice-edge bloom might be accounting for more than a half of the total annual primary productivity (PP) of the Arctic Ocean calculated using satellite Ocean Color data. This might be overestimate because of the sub-surface production that follows the bloom and the production prior the ice melting. Nevertheless the paper indicates (to some extent) that time resolution may be important to consider when PP may be computed from Ocean Color data. The structure of the manuscript is straightforward, concise and well written. The figures are pertinent and of very good quality.

We thank Reviewer 1 for their comments on our paper. While we believe it to be one of the first papers to address and quantify the widespread nature of the ice edge blooms in the Arctic, we have endeavoured to be cautious about over-interpretation of the results, and we appreciate the reviewer's point about further discussion of the limitations of this analysis. In particular, we have added material concerning:

- the possible contribution of pre-melt blooms (Mundy et al., 2009) …

> "Mundy et al. (2009) had reported that epontic algae growing on the underneath of the ice cover may commence a bloom before ice-melt has occurred, but not all such species will survive and prosper within the water."

- errors in quantifying the post-bloom subsurface chlorophyll layer, and its effect on productivity …

> "An important caveat is that phytoplankton mostly occur in the upper mixed layer during the initial ice–edge bloom, but may occur below the mixed layer later in the summer open–water period, at a depth which prevents remote detection by satellite. Consequently, estimates of both phytoplankton abundance and associated primary production may be systematically biased in favour of ice–edge blooms. On the other hand, phytoplankton at depth will experience much–decreased PAR availability, and consequently may be expected to have lower growth rates and be less productive. The application of VGPM here effectively assumes that, in the absence of more detailed supporting data, these two biases approximately cancel."

- that the 'adjacency effect' is likely to lead to underestimates of chlorophyll concentration within those parts of the bloom closest to the ice …

> "This could indicate that in this region the bloom onset occurs in open-water; however, the chlorophyll estimates may be biased low due to the adjacency effect, whereby reflections from nearby ice affect the brightness of an image pixel (Belanger et al., 2007). In any case, the bloom terminates at 20-100 km behind the retreating ice edge."

1. In the introduction, the authors mentioned "However, few observations of ice–edge phytoplankton blooms from satellites have been published to date (e.g. Arrigo and van Dijken, 2004), and thus a primary aim of this study is to fill this gap and investigate their existence at the large scale." It is partly true. I would like to stress that a few studies have been done ~20 years ago demonstrating the great potential of Ocean Color data (i.e. CZCS) to study Ice-Edge bloom in the Arctic. They provided insightful discussions about ice-edge bloom dynamics. One of them actually combine in situ observations acquired near-simultaneously with satellite data (Mitchell et al 1991). They discussed some inherent limitations of ocean color data (e.g. the difficulty to detect the post-bloom subsurface productivity). These studies couldn't perform large-scale studies due to lack of CZCS data in this region and limitation of the sensors. It is actually surprising that no one has addressed this issue since the SeaWiFS launch in 1997. So the present study is more than wellcome. I recommend adding these references as a recognition of their pioneer works:
a. Kögeler, J. and F. Rey (1999). "Ocean colour and the spatial and seasonal distribution of phytoplankton in the Barents Sea." International Journal of Remote Sensing 20(7): 1303-1318. b. Mitchell, B. G., E. A. Brody, et al. (1991). Meridional zonation of the Barent Sea ecosystem inferred from satellite remote sensing and in situ bio-optical observations. Pro Mare Symposium on Polar Marine Ecology, Trondheim, Polar Research. c. Maynard, N. G. and D. K. Clark (1987). "Satellite Color observations of spring blooming in the Bearing Sea shelf waters during the ice edge retreat in 1980." Journal of Geophysical Research-Oceans 92(C7): 7127-7139. d. Maynard, N. G. (1986). "Coastal Zone Color Scanner imagery in the margical ice zone " Marine Technology Society 20(2): 14-27.

We have amended the manuscript to include reference to this earlier work using CZCS imagery, and have cited two of the suggested papers …

> "Indications of ice–edge blooms had been noted in ocean colour imagery from the Coastal Zone Color Scanner (e.g. Maynard, 1986; Maynard et al., 1987; Mitchell et al., 1991; Kögeler and Rey, 1999) but detailed investigations were not possible on account of its poor sampling due to limited onboard storage, and underestimation problems close to ice due to a "ringing effect" as the scan line moved from bright to dark features (Mitchell et al., 1991)."

2. In section 4, the statement "Low chlorophyll values are visible between the main patch of the bloom and the sea–ice. The bloom is clearly propagating in a 20–100 km belt behind the retreating ice edge." should be revised. The issue of sea-ice contamination must be stressed here as a potential explanation for the low chlorophyll values observed between the main patch and the sea-ice. Bélanger et al 2007 showed that adjacency effect (i.e. photon reflected by ice and then scattered above the water toward the sensor) might results in severe underestimation of chlorophyll concentration within the first 15 km of the ice edge (their Fig. 5). The underestimation is more important when the actual concentration is high, which may be the case in Fig 3. In addition, it was recently found that ice-edge blooms may *be initiated below sea ice, when upon melting more light transmitted through sea ice (Mundy et al., GRL 2008). So it is actually possible that high chlorophyll values are present right at the ice edge.*

Underestimation due to the adjacency effect is referred to as a possible cause of low chlorophyll values near the ice edge (see earlier comment and quotation of new text).

3. Section 5. The lack of bloom in the central Arctic must be discussed more deeply. Several reasons may explain this observation: a. The late melting of sea ice. This reduced the chance to observe the bloom, if any, because Ocean Color data become scarce after mid-august (of are of poor quality) b. Central Arctic is strongly stratify and poor in nutrients and is likely that blooms cannot develop even if light is available. Nutrients may have been consumed during the summer because the pack ice is partly broken and let light penetrate the ocean.

Given the lack of observations, one can only speculate on the apparent lack of an ice-edge bloom in the central Arctic. Nevertheless, as suggested by the reviewer, new text has been added to briefly outline the potential mechanisms that prevent blooms in this region …

> "In the high Arctic, the strongly stratified environment and associated low nutrient concentrations may explain the absence of a bloom. Alternatively, since the pack ice partly breaks during summer, light should reach the ocean surface and potentially allow phytoplankton growth and nutrient exhaustion before the first data are collected, even though there is still too much ice for bloom detection by remote sensing. Nonetheless, given the absence of reliable observations in these regions it is not clear whether there is really minimal growth (partly engendered by lack of nutrients) or that there is a rapid and short–lived bloom that is hidden by clouds, fog or sea–ice."

4. Section 5. We could read: "For ~30% of points with recorded blooms the first observation is the highest, indicating that the bloom probably peaked before the ice concentration durably reduced to below 10%, while a further 52% show the chlorophyll peaking in the MIZ period." Here you could refer to the work of Mundy et al GRL 2008 (see above).

As noted above, reference to Mundy et al. (2009) observations of a sub-ice bloom has been added to the manuscript.

5. Section 5. The authors recognized that the VGPM have considerable uncertainty. It is probably the worst model for the Arctic since it uses an empirical relationship for Pbopt that is certainly very far from the Arctic reality. Another problem with the PP modeling is the use of OC4v4 algorithm for the chlorophyll. This algorithm is inappropriate for the Arctic waters, which are CDOM rich relative to the global ocean (Amon et al JGR 2003; Siegel et al JGR 2002; 2005; Bélanger et al JGR 2008). So the magnitude of PP must be taken with a lot of care. In addition, the ratio TPP (6b) is most likely overestimated because of the well-known sub-surface bloom development after the spring bloom (see Mitchell et al 1991). The productivity of those sub-surface chlorophyll maxima are still to be investigate but may be relatively more important than in other ocean (see also Martin et al MEPS 2010). I *recommend a deeper discussion about the validity of their results presented here (in particular Fig 6b).*

As we note in our manuscript, ***all*** primary productivity algorithms need to be treated with caution in the Arctic because of both difficulties with data availability and quality, and because of limited validation in this region. Because of this, we chose one of the simplest and most commonly used primary production models, the VGPM, and concentrated our discussion on the ratios of productivity in different periods of the year rather than absolute rates. However, following the referee's comments, we have now added a further few sentences discussing the problems of estimating productivity for sub-surface blooms (see earlier text).

Additionally, we have repeated our primary production calculations using two further algorithms: Carr (2002) and Marra, Ho & Trees (2003). As these two models produce broadly similar results to those of the VGPM algorithm, we present their estimates in an appendix section.

> "Because of the considerable uncertainties involved in estimating Arctic productivity in this way, which stem from both the input data and the VGPM's biological assumptions, we use this algorithm for illustrative purposes only. Appendix A describes estimates made using alternative algorithms that are broadly in agreement with the VGPM results."

6. Conclusion section. While most of their analysis is not so much affected by the absolute values of CHL, the authors must recognize again the large uncertainties of Ocean Color products for this peculiar ocean. The Arctic is affected by sea ice (see Bélanger et al 2007 and also Wang and Shi 2009), receives large river inputs and sub-surface production may contribute more than expected. Theses limitations make current quantitative assessment of PP from satellite very uncertain, including the work of Arrigo (e.g. OC4v4 in not appropiate at all but is still widely used).

We agree with the referee concerning the uncertainties associated with ocean colour data and productivity in the Arctic, and have added the following text to stress these points in the manuscript (the references provided by the reviewer have been added to the text):

> "The globally–calibrated, empirical algorithm OC4v4 may therefore include large errors in chlorophyll retrieval (Cota et al., 2004; Gregg and Casey, 2004), although its performance remains comparable to the regionally-tuned algorithm OC4L (Wang and Cota, 2003; Matsuoka et al., 2005)."

> "This value carries some uncertainty due to limitations of remote–sensing in the Arctic (in particular sub–pixel contamination of sea–ice; e.g. Belanger et al., 2007; Wang and Shi, 2009), but another sensor with a different chlorophyll retrieval algorithm and less data coverage also yielded a very high occurrence frequency (77%; see section 5)."