We thank reviewer #1 for their careful review. You will find our detailed answers to the reviewer's comments below marked in bold.

Overview This paper describes the photosynthetic parameters measured during a cruise in the Beaufort Sea. It provides a unique data set to add to the current un- derstanding of photosynthesis in the Arctic and was compared to the MEL data set. The paper is missing a discussion on the data in regards to a changing Arctic (less ice, more light etc), which the authors refer to in the introduction.

The introduction provided the context for the study. This paper sets the stage for an in depth study of the effects of future climate change (ice, light, photobleaching of CDOM, clouds, etc.) through modeling efforts. However, we feel that it is beyond the scope of our paper to elaborate on this topic due to the many interacting effects that must be taken into account and the data available in the context of our study.

I would also liked to have understood where the large celled communities were in the water column, the 50m split of the data seems arbitrary and is not explained.

We detail this below.

This data set is an important addition to our understanding, and this paper represents a good description of the data.

Abstract Line 4: Add "s" to photosynthetic parameter, so it reads parameters. Line C332

Fixed, thank you.

6 to 8. This sentence is a bit awkward to read, I suggest "Such measurements and their relationship to environmental variables will be required to improve the accuracy of remotely sensed estimates of phytoplankton primary production and our ability to predict future changes"

We replaced our sentence with the sentence proposed by the reviewer.

Introduction Page 2, paragraph 1. "ice free summer in the Arctic Ocean is predicted to occur sooner" – sooner than what? This sentence needs a reference to when we initially thought this would happen 2050? 2100?

The new sentence now reads:

As models are improved to match current observations, the first ice-free summer in the Arctic Ocean are not expected in the 22nd century (Walsh et al., 2005) anymore but instead in a matter of decades (Stroeve et al., 2012; Wang and Overland, 2012).

Page 2, paragraph 2. Increased light will increase primary production, however in Hill et al 2012 Progress in Oceanography they also state that the Arctic is nutrient limited so light alone will not dramatically increase PP.

We had not found the Hill and colleagues contributions before submission. We have

modified this section to take their finding into account.

A few researchers have already undertaken such efforts using remote sensing data (e.g., Brown and Arrigo, 2012; Pabi et al., 2008) or modeling approaches (e.g., Lavoie et al., 2010). Generally, these studies find that the increased light due to decreasing sea-ice cover leads to higher primary production, though these increases may be dampened by nutrient limitation (Hill et al., 2013; Codispoti et al., 2013).

Page 2, paragraph 2. In situ should be italized, throughout the paper.

Biogeosciences style explicitly states that « in situ » should not be italicized.

Page 2, paragraph 3. Sentence fragment, time of incubation is repeated in the second sentence of this paragraph.

We removed the repetitive fragment. Thank you.

Methods PvsE curves. Why did the authors use a Sf factor of 0.5, when their absorption spectra whould indicate using 0.35? Community size and taxonomy. I would like more detail on which pigments were used to identify size fractions.

These are two different numbers. The $S_{\langle f \rangle}$ represents a weighting factor when summing two extreme phytoplankton absorption spectra provided in Ciotti et al. (2002, cited in the text) to best represent in situ phytoplankton absorption. Here, we used $S_{\langle f \rangle}$=0.5. The value of 0.35 represents a scaling factor (the ratio of PUR/PAR in the incubation chambers) that allows PAR-based measurements to be tranformed to PUR-based measurements. This scaling factor is dependent on the absorption spectrum of phytoplankton and the lamp spectrum used during incubation. We find that using the absorption spectrum measured in situ, we find exactly the same value of 0.35 for the scaling factor as when using an $S_{\langle f \rangle}$ of 0.5 , this means that using an $S_{\langle f \rangle}$ of 0.5 provides a good approximation of the phytoplancton spectrum or at least one that weights the light spectrum in a very similar fashion. No pigments are used in this section to identify size fraction. When we used pigments to identify size fractions, as in Figure 2, we use the pigments as defined in Uitz et al. (2006) (cited underneath Figure 2).

We have not changed this section as we feel it is correct.

Results Fig 1C. There is no subK in the axis titles.

We have rerendered the figure which now contains the subscripted letter. Thank you for noticing. As for the original subscripted "k" we are still looking for it.

Page 6, paragraph 6. Earlier in the text it was mentioned that a subsurface Chl a max was found along the cruise track. In reference to Fig 2, at what depth did that occur?

We now mention in the methods that the chlorophyll maximum was generally located between 60 and 70 m during the cruise.

Was there a difference in community? This paragraph confused me, are shallower waters referring to the total water depth? Or the surface mixed layer?

We agree that, as written, the sentence was confusing. We modified it as follows, which should prevent any confusion : "Waters in the upper 50 meters were typically dominated by picoplankton (similar to what was found by Lovejoy et al., 2007), or occasionally by microplankton, while deeper waters were typically dominated by nanoplankton."

How did you choose the 50m split seen in figure 2.

The 50 m depth was chosen based on the observation that there was hardly any communities with a significant percentage of large cells (microphytoplankton) below that depth. That can be observed on Figure 4A. It corresponds also roughly to the region above the deep chlorophyll maximum.

Page 10. I do not see a reference to Fig 8B or 8C.

Our mistake, we have added appropriate references in the paragraph that discusses Figure 8.

In figure 8 C it does not appear that the predicted A_{Chl} had the same dynamic range as the measured values.

That is true. We have not been able to find any variables, which allowed us to explain more of the variability. We suspect that at least some of it arises from measurements and or fitting procedures. Fig. 8C clearly illustrates the limitation in predicting α^{chl} using our statistics.