Review of Smith Jr. and Donaldson.

General Comments:

The study presents data from an impressive number of P-E experiments in the Ross Sea. Its aim is to statistically identify the principal factors governing the seasonal changes in the P-E parameters in the Ross Sea. Although the statistical analyses did not yield a clear picture of what is driving the seasonal variability, the paper presents a nice summary of a valuable dataset for understanding productivity in polar seas.

The paper would benefit from some additional text in the methods section outlining clearly how the parameters were derived (see below) and how the classification of samples based on taxonomic composition was made. Also the effect of irradiance was only mentioned in the section describing the experimental manipulation experiments, rather than on the P-E dataset as a whole. The authors do not explain why irradiance was not included alongside nutrient availability and temperature as an environmental predictor of the field data.

General Comments:

The use of P_s^B instead of the more conventional notation for photosynthetic rates at saturation light P_m^B, which is used widely in the papers cited, could be potentially confusing to readers. P_s^B is a parameter derived to fitting P-E curves in the presence of photoinhibition and is related, but not equivalent, to P_m^B (to retrieve P_m^B from P_s^B requires information on both β^B and α^B (Platt et al. 1980)). Given that some of data have been fit to a two-parameter function (i.e. assuming β=0), P_m^B should be used to avoid possible misunderstanding. Also in the introduction it does not say explicitly that α is also normalized to biomass. I wonder whether a superscript *B* on α would also provide some additional clarity.

It was not clear from reading through the methods sections, whether all the data was fitted using the 2 parameter function, or whether some were fitted using the 4 parameter function of Platt et al (1980), and P_m^B values were calculated from P_s^B, β and α to compare to those fitted by using equation 1, or if all experimental data were fitted using equation 1? For samples fitted to equation 1, was there any evidence of photoinhibition present in these samples? If so, were points removed so that photoinhibition at high light (e.g. in deeper samples that are low-light acclimated) to prevent the magnitude of P_m^B to be influenced by the degree of photoinhibition?

There is no mention in the methods section on the depth intervals that were sampled during the various oceanographic campaigns. Knowing this may explain how light conditions may be influencing the overall variability in the P-E parameters.

Pg 18054 Section 3.3 The authors examined the effect of nutrients and temperature at the depth of sampling on the P-E parameters. Why was light not included in this analysis? As pointed out by the authors, gradients in temperature are modest in this polar region, whereas I would expect light conditions could vary significantly over a seasonal cycle due changes in surface irradiance, light attenuation and mixed layer depth. Although mixed layer depths were mentioned in the methods section, I could not find any values reported in the results section or mention of how they were used?

Information on the taxonomic composition of natural samples was used to determine whether it the seasonal variability in the PE parameters could be explained by the dominance of diatoms versus Phaeocystis. In the text it states that 40 stations were identified as being identified as being dominated by one of these two groups, whereas in Table 5 it states 20 stations were selected “for inclusion in this comparison”. This is quite confusing. Moreover, in the methods section there is no mention as what sort of chemical analyses was used (I assume HPLC pigment markers) nor what threshold was used to determine dominance of a particular functional group. I guess that many stations may have been classified as being a mixed assemblage, and that this may explain the reason for a rather low number of stations/samples included in Table 5. It would be very helpful to the reader if the authors could outline how this classification was conducted using chemical and microscopic analysis, whether this classification was done on a much larger number of samples, but that dominance was only found for a subset, and what were the number of observations for each value reported in Table 5 (it is not clear if number of stations is equivalent to number of samples).

Specific Comments:

P 18050 The authors make the case that differences in the experimental protocols were not important: “While details of the methods of each study varied somewhat, we did not find that the methods introduced error to obscure the overall patterns.” How was this comparison between methods made to reach this conclusion?

Pg 18055 line 20 I would argue that one cannot base the findings of this regional study to justify the use of the temperature-dependent equation of Behrenfeld and Falkowski (1997) for the entire Southern Ocean.

Table 5. I would include values of *N*, as in the other tables.

Technical corrections:

Pg 18048 line 20 – “growth” replace with “grow”.

Pg 18052 line 16 – 0.53 should be 0.053.

Pg 18053 line 11 – A value of P_m^B of 1.14 is reported in the text, whereas 1.13 is reported in the Table 2.

Pg 18053 line 25. Table 3 does not report average PE parameters based on the concentration of Fe. However, Fig 2 does, although it is not immediately clear that there is a significant increase. Also, are the units on the y-axis correct (mg C (μg chl)$^{-1}$ h^{-1})? Not μg C (μg chl)$^{-1}$ h^{-1}? Subscript and superscript on *P* should be reversed? X axis - second Hi Fe should be Lo Fe?

Table 2 Legend mentions range but this is not reported, whereas number of observations appears in brackets but not mentioned in the legend.