

Interactive comment on
**“Photosynthesis–irradiance responses in the
Ross Sea, Antarctica: a meta-analysis” by W. O.
Smith Jr. and K. Donaldson**

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NOTE THAT A REVISED MS AND A FULL LISTING OF THE RESPONSES TO RE-
VIEWS OF ALL THREE REVIEWERS IS PROVIDED SEPARATELY (I HOPE, AS I AM
UNSURE OF THE BEST WAY TO PROVIDE THAT MATERIAL AT THIS POINT).

Responses to Review bgd-11-C8014-2015

The reviewer offered a number of suggestions that prompted us to alter the manuscript substantially to improve its clarity. 1. The reviewer pointed out an apparent contradiction listed in Tables 3-5 and the statistics, in that we found a strong seasonal comparison yet none within the data sets selected for analyses for temperature, iron and

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nitrate impacts. We have altered our discussion of the results to clarify and explain why we believe the results are consistent. In essence, the seasonal changes appeared to be robust, whereas many of the other factors had little impact when assessed within one season or between seasons. Clearly many of the oceanographic factors do indeed vary with season (mixed layers become shallower, nutrients are reduced through growth, irradiance increases seasonally), but these effects are manifested collectively to generate the seasonal affect. We have altered the discussion of these effects to clarify the differences (and consistencies) (see lines 292, 303, and 311). 2. The reviewer was correct in that the iron data were inadvertently omitted from Table 3, and they now are included (as is a more thorough discussion of these data). We have also altered our discussion of the CORSACS data to clarify the difference between the field observations (Table 5) and the controlled experimental data (Fig. 2) (lines 321) 3. The reviewer is correct in stating that type I errors may be associated with t tests. We indeed did investigate the normality of distribution of the parameters, and tried various transformations to improve the normality of the distribution, but none of our attempts enhanced the normality. After numerous attempts at this, we concluded that the large ranges of parameter values, especially for α and E_k , reduced the statistical power of the tests and resulted in a lack of significance in our comparisons of α and E_k . 4. The reviewer was concerned with the lack of difference we found between *P. antarctica* and diatoms (Table 5). As with the previous reviewer, there was confusion in the manner we stated the number of stations used in the analysis. We used 20 stations for each functional group, and so number of degrees of freedom that the reviewer assumed is an underestimate. We did not understand the exercise in artificially reducing the sample size, nor do we understand where he got the number of replicates of $N=61$. We apologize for the misunderstanding of our description of the data used in the functional group comparison, but are confident that the analysis is correct. It also is consistent with results of Robinson et al. (2003) and van Hilst and Smith (2002). We also note that the selection of data using pigments involves selecting stations that were largely dominated by one form or the other, and that both data sets had contributions from

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different taxa. Expanding the numbers of stations included would result in increasing the taxonomic variability in the selected data and, in our view, decreasing the power of the comparison. 5. We were again a bit confused by the reviewers comment that “The algorithms that use integrated chl, irradiance, and P-E response as a function of temperature actually do a reasonable job in the Ross Sea because in fact there are not big differences between spring and summer in the P-E response.” We are unaware of any publications other than those from Kevin Arrigo’s group that have modeled Ross Sea productivity. We agree that Arrigo’s algorithms do a reasonable job in estimating productivity, but suggest that if a better seasonal analysis of P-E responses were included, that estimate would be improved. Most of Arrigo’s models are bio-optical and depend on satellite estimates of chlorophyll. Since very few regional values are available (they are largely composites over one month), that variability will mask any generated by the P-E response. Again, the data we present are a synthesis of a large number of measurements, and we strongly believe the seasonal difference is real and a major feature of the temporal variations of photosynthetic parameters in the region. 6. We agree with the reviewer that emphasizing nitrate concentrations relative to P-E responses (and growth limitation) was unlikely to reveal a significant difference. However, other studies have included this variable, and we would have been remiss to include it. It certainly is not a major point of the paper, and we have left it in to be consistent with other studies and to be complete. We in no way suggested that nitrate was expected to have a significant impact. 7. The reviewer was correct in stating that Table 2 included values that were unclear, and those have all been clarified.

Interactive comment on Biogeosciences Discuss., 11, 18045, 2014.

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