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Comment

***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 IS SUBMITTED SEPARATELY, AS IS A PDF OF THESE  
RESPONSES

Responses to Review bgd-11-C8361-2015

We appreciate the constructive comments of the reviewer, and have changed the ms. throughout as suggested. Specifically, 1. The reviewer pointed out the difference between  $\alpha$  and  $\alpha_B$ , and is correct in his notation of the difference. We have converted from using  $\alpha$  and explained in the methods why this was used. We have also used the suggested notation for  $\alpha$  ( $\alpha_B$ ) throughout. 2. The reviewer noted the ambiguity in the

C8952

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Discussion Paper



methods with regard to the treatment of data with either a 2- or 4-parameter model. We have clarified this (lines 150 and 159-160), and have noted that no data were removed in the analyses, as that would be an arbitrary removal that potentially might introduce a bias. However, the removal of points generally resulted in only a minor change in the estimated value, and considering the variability inherent in the entire data set, was not considered significant. 3. Depth as a variable was not explicitly treated, but the range of depths sampled is now included in the methods (lines 137-8). Mixed layers in most (ca. 80%) of the stations exceeded euphotic depths. 4. The reviewer asked why irradiance was not included in our analyses, and how mixed layer depths were analyzed. We considered irradiance to be a critical feature in the phytoplankton response; however, irradiance also varies substantially on a number of scales: seasonal, diel, and “randomly” (weather/cloud impacts and vertical mixing). Hence we believe the irradiance at the time of sampling has little bearing on the photosynthetic responses measured. We do not disagree that irradiance on some scale does indeed modulate the P-E responses. That is why we did the seasonal comparison, since mean seasonal irradiance can be broadly defined as “low” and “high” in a relative sense, and why we emphasized the large difference. We initially did not want to confuse the issue by looking at irradiance on shorter scales, simply because we were not comfortable in defining the scale which is important to the phytoplankton response from our results. However, to address this issue, we have compared all samples from the 50% and the 1% isolumes. The cruises that had the most samples from the 50 and 1% isolumes were the JGOFS and PRISM cruises. JGOFS was conducted in a more restricted spatial domain, but had far fewer stations. PRISM had more samples, but had much greater spatial variability. Both, however, were in January so that the comparison was within summer when we would expect any difference to be greatest. JGOFS data did indeed show a significant difference in values, but not  $\alpha$  or Ek values. PRISM data showed no significant difference in any parameter, and when we merged the two data sets, no significant difference remained. To be honest, the analysis is far from perfect, but based on these results we cannot conclude that the irradiance environment sampled resulted

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11, C8952–C8954, 2015

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Interactive Discussion

Discussion Paper



in a significantly adapted assemblage. We have added a short comment to this effect in the results (Line 230), but did not emphasize this to a great extent. We hope our analysis satisfies the reviewer. The reviewer also noted that mixed layer depths were not included anyway, and we have done so now in Table 2. We also included data on the euphotic zone depths to provide some insight into the relative depths of the two. 5. The reviewer is correct in catching this difference, and it was our error in describing the results. What we did is select 40 independent stations, or 20 of each functional group. We have clarified this confusion. As the reviewer no doubt realizes, nearly all stations had a mixture of the two groups; we endeavored to select those that were substantially dominated by one or the other. Our means of doing so was pigment-based, and we have enhanced the description of our station selection in the methods (line 184). Specific Comments 1. We added material in the methods (line 149) to clarify this statement. 2. We have tempered this statement (line 278), but still believe that the results do indeed suggest a broad support for their conclusions. 3. We added a statement in the caption of Table 5 to clarify the number of stations used. 4. Changed. 5. Corrected. 6. Corrected to 1.13. 7. This was a serious error, and we have now included the statistical summary in Table 3. The figure has also been corrected. 8. Correct. The range was not provided because the table became too complex in our view.

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

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11, C8952–C8954, 2015

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