The Cryosphere

Discussions

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Interactive comment on "Impact of Icebergs on Net Primary Productivity in the Southern Ocean" *by* S.-Y. Wu and S. Hou

Anonymous Referee #1

The authors address the interesting idea that icebergs may play a role in stimulating primary production in the Southern Ocean, through supply of bioavailable iron to nutrient limited areas. Their approach is to use two remote sensing datasets (one for ocean productivity and another for iceberg probability), and through multiple linear regression analysis to examine the effect of temperature and iceberg concentration on net primary productivity. While I agree that this issue is one that deserves attention, I have significant concerns with many of the assumptions that underpin the analysis, and in the end feel that the authors conclusions are not supported by the work presented here. Below I detail my points for the authors to consider:

1) There seems to be a rather large disconnect in scale here that I'm having trouble understanding. It appears that most of the previous work on this subject has looked at iceberg properties and associated ocean/productivity effects at small scales – i.e.,

individual icebergs or icebergs within small defined areas. How do the authors justify scaling up to a hemispheric-scale analysis based on these limited findings? One could imagine that icebergs from different regions would have radically different properties – volume, area, number, sediment concentration, sediment composition, etc. In particular, the bioavailable iron concentration would almost surely be different across various regions – how does one then compensate for that when scaling to the entire Southern Ocean?

2) The most recent work on the subject (Duprat et al., 2016) focused on large icebergs and their potential ocean impact. The authors make the assumption (lines 30-35) that the majority of small icebergs are associated with large ones, and therefore that total iceberg concentration should reveal similar impacts on ocean productivity. It's not clear to me what the basis is for this assumption. What is all or most of the sediment and hence available iron is limited to the large bergs? Smaller ones may simply be fragments that were never in contact with the bed surface. I'll readily admit here that I'm not an expert on iceberg processes, but this assumption is fundamental to the authors approach and it just seems unfounded to me.

3) The remote sensing product used to estimate iceberg probability is described briefly, but again I am struggling to understand the connection between iceberg probability (presumably a derived product and not a direct measurement) and the relevant information in this case – namely the amount of iron going into the water. Moreover, a broad-scale gridded remote sensing product seems rather coarse to evaluate small-scale processes. Again, I am not a remote sensing expert, and perhaps the Tournadre et al. 2012 product is a gold standard in the field, but its application here may be somewhat misleading.

4) My concerns about scaling and data products aside, the results of the statistical analyses are just not that compelling. There is a positive correlation between NPP and iceberg probability (not presence or concentration, as the authors write), but in all the cases the variance explained by iceberg probability is so small it's hard to imagine

any physical significance. And in particular, the variance explained by temperature vs. icebergs makes the iceberg effect quite small. What of other variables? Ocean productivity could be equally linked to any number of other factors (e.g., wind stress, upwelling, circulation, etc.) – why did the authors choose just temperature (and icebergs) for the MLR analysis? And of course, old adage that correlation does not prove causation applies here – even with a significant correlation between icebergs and NPP (albeit with ~2% of variance explained), the authors provide no argument for the linkage and a SO scale.

5) The discussion section provides a useful summary of all the reasons why there may be such a small statistical effect of icebergs on productivity, and indeed the authors cite studies suggesting that the presence of icebergs may in fact decrease local productivity. In light of all of this, I find the final 5 lines of the conclusion section to be unsupportable by the existing analysis and results. Overall I think the authors need to either a) include a significant number of additional analyses to bolster their arguments, or b) carefully consider what conclusions can actually be drawn from the existing analysis, and re-write the paper and title accordingly.