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Comment on cp-2021-85

Louisa Bradtmiller (Referee)

Referee comment on "Variations in export production, lithogenic sediment transport and iron fertilization in the Pacific sector of the Drake Passage over the past 400 ka" by María H. Toyos et al., *Clim. Past Discuss.*, <https://doi.org/10.5194/cp-2021-85-RC2>, 2021

The manuscript by Toyos and coauthors presents a new, high-quality, high-resolution record of various biogenic and lithogenic proxies in the Drake Passage over several glacial cycles. The high resolution of the data and the relatively long (for the Southern Ocean) span of the record are especially useful in this fairly under-studied region. Finally, the fact that their records have been Th-normalized avoids many pitfalls of working in the SO, where sediment focusing and winnowing can be intense.

I have relatively brief comments due to 1) the overall high quality of the study and presentation and 2) the incredibly thorough comments already provided by another reviewer. First, I agree with the previous comments that it would be helpful to hear more about how much/little the age model changed as a result of tuning to high/low Fe contents. This could come in the text, or could easily be illustrated with a supplemental figure. I am also intrigued by the consistent difference between % opal measured in the different labs in samples with high terrigenous content. The methods section says sample sizes at UdeC were between 50-70mg. Were they varied intentionally due to changes in terr. content? The alkaline extractin method is sensitive to sample size; for example, for a sample with >75% terr. content, a sample size of 100mg might be warranted. The methods don't say if the same sample sizes were used at AWI. This could account for some of the difference. I am not worried about this in terms of the overall conclusions of the study, but additional details about AWI sample sizes could be included in methods. Lastly, I appreciate the care taken by the authors to consider the possibility that not all (or, as they point out, a varying amount) of the added Fe was bioavailable. Too many studies in this region assume that any Fe input is an automatic link to productivity. Even if we don't have the tools to reconstruct bioavailable Fe content, including this nuance in the discussion is a welcome change.

In summary, this is a high-quality study and a well-written manuscript. I concur with nearly all of the extensive specific comments by an earlier reviewer, and commend the authors on a very strong contribution.