

Interactive comment on “Atmospheric Fe supply has a negligible role in promoting marine productivity in the Glacial North Pacific Ocean” by Francois Burgay et al.

Anonymous Referee #2

Received and published: 2 September 2020

In this manuscript, Burgay et al. present a new Fe record from the NEEM ice core in Greenland, and compare the observed Fe fluxes to ocean sediment records of productivity to examine potential links between Fe deposition and marine primary production in the Northern Pacific. The main takeaways from this work are that Arctic Fe fluxes are comparable-though with a few differences-to previously published ice core dust records (nssCa from Greenland as well as other Fe records from Antarctica), and that there is not a strong link between NEEM Fe flux and marine primary production in the North Pacific HNLC region, except for in the transition zone.

Major comments: Overall, the short format of this manuscript is appropriate for the

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scope of the interpretation. I think that the introduction would benefit from including a paragraph better describing “leachable iron” (compared to other types of Fe measured in ice) and implications for bioavailability. This is mentioned later in the text, but is a crucial aspect of the interpretation that I do not think is sufficiently addressed and should be mentioned earlier in the manuscript- have other studies of ice estimated what fraction of leachable Fe is bioavailable or if soluble and total leachable Fe covary?

I also think the comparison to Antarctic Fe records could be improved. Does the measurement resolution and methods for each record permit a valid comparison? The authors note that fluxes are lower at Law Dome and Dome C- it seems that the samples from Dome C (Wolff et al., 2006) may have only been acidified for 24 hrs prior to analysis- would that lead to under-recovery of Fe? Furthermore, can the comparison between Arctic and Antarctic records be used to say anything about dust source regions? Through the fluxes may be comparable between the two poles, dust source regions are very different.

The comparison to the marine sediment cores could be strengthened with some additional context and information. What is the rationale for choosing the specific marine sediment cores to compare to in this study? What is meant by the “transition zone”? Can the HNLC and Transition zones be included on one of the maps? I would also hesitate to make statements as strong as the ones on lines 243-244 saying that “the transition zone of the N Pacific was sensitive to atmospheric Fe supply” and that “a direct link between Fe transport and ocean productivity holds only in the transition zone of the North Pacific” (lines 254-255) solely based on comparisons to one marine sediment core record and without a better understanding of how much of the ice core Fe is bioavailable.

Additionally, a literature search returns a recently accepted manuscript at another journal that also presents Glacial-Interglacial Fe measurements from the NEEM ice core. I think it would be necessary to acknowledge this and include relevant discussion comparing to this record/interpretations during revisions: Cunde Xiao, Zhiheng Du, Mike

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J Handley, Paul A Mayewski, Junji Cao, Simon Schüpbach, Tong Zhang, Jean-Robert Petit, Chuanjin Li, Yeongcheol Han, Yuefang Li, Jiawen Ren, Iron in the NEEM ice core relative to Asian loess records over the last glacial-interglacial cycle, National Science Review, nwa144, <https://doi.org/10.1093/nsr/nwa144>

Lastly, the English of this manuscript could be improved to allow the reader to better interpret/assimilate findings- some of the sentences and phrasing are complex and difficult to understand throughout.

Other comments:

Lines 42-44: Sentence is confusing- consider rephrasing.

Lines 77-78: What is meant by a “low-resolution sampling apparatus”? Is this just manual collection?

Lines 79: I would replace “not contaminated” with “least likely to be contaminated” unless there is evidence for zero contamination.

Line 85: Two “%” symbols

Lines 88-89: Is there a citation to support dissolution of particles after 30 days?

Lines 105-106: Were any replicate samples run to assess reproducibility?

Line 107: Change header to “Results and Discussion”

Line 114: Include dates for Holocene to be consistent with other discussion

Line 144: What is a “dust deflation area”?

Lines 169-170: Provide some more context for these statements. Is this during modern times or from paleo studies?

Line 173: “Rather” is colloquial

Line 178: Define “high-resolution”

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Lines 195-204: This paragraph needs to be rephrased to say that nitrate is d15N is used to interpret stratification in the first sentence. This is not clear until the last sentence, making the entire paragraph seem out of place

Line 213: What is the “transition zone”?

Line 228: “Figure 2” seems to be the wrong figure for this statement.

Interactive comment on Clim. Past Discuss., <https://doi.org/10.5194/cp-2020-77>, 2020.

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