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Climate of the Past Discussions

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.

Francois Burgay et al.

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Dear Reviewer,

First, on behalf of all the authors, I would like to thank you for your precious suggestions that contributed for the overall improvement of the manuscript. Please find attached the new version. We would like to point out that in the first version there was a mistake in the calculation of the Fe and nssCa fluxes for the NEEM ice core. We repeated the calculations and the mistake is now fixed. However, this correction does not affect the interpretation of the dataset. We also included in Table 1, the nssCa concentration and fluxes since we referred to them several times in the main text.

We also introduced in the method sections more details about the analytical performances for Ca and Na and more indications on how nssCa was calculated. This info was missing in the first version of the manuscript (L122-131). Best regards,

Andrea Spolaor Corresponding author

Reviewer #1 We introduced a new Figure 1 and we edited ex Figure 2 as you suggested.

L23 - The phrase "marine productivity" how is this being defined? What proxies were used? Do you mean marine primary productivity? Yes, we meant "marine primary productivity". We introduced a new paragraph where the proxies used to evaluate marine primary productivity are described used at L298-L304.

L25 - are you referring to upwelling of major nutrients? Yes

L25 - "Fe-fertilization" when, during the LGM? During LGM and during MIS4. It is now specified at L27

L26 - "Transition zone of the North Pacific" We changed "transition zone" with "mid-latitude North Pacific" throughout the entire manuscript to be consistent with what reported in Amo and Minagawa, 2003.

L39-40 - Can you expand on the implications of the effect you describe on the Earth radiative budget We integrated this suggestion at L41-L45.

L51 - What do the authors mean by "leachable Fe"... Did the studies cited define leachable Fe in the same way? If not, how they are comparable? We address this critical aspect from L97 to L106 and from L167 to L171. In particular, we changed the terminology from "leachable Fe" to "Total Dissolvable Fe" as expressed in Edwards et al., 2006. Our procedure was consistent with what suggested by Koffman et al., 2014 for trace element analysis in ice cores. It differs from the procedure used for Fe analysis in TD and EDC„ meaning that absolute concentrations (and fluxes) are not directly comparable due to the different analytical procedures. However, the general

trends and features are still comparable. More details in the text. L59 - What are possible sources for a homogenous load over the entire continent during the LGM, can you expand? We now expanded this part from L179 to L185.

L65 - it would be good define earlier on the paper what do you mean by “leachable Fe” We introduced a sentence in the introduction (L75-78) that better defines what TDFe (Total Dissolvable Fe) means. We also discussed the analytical procedure at L97-106.

L87-88 - Were the samples filtered prior to analysis? Are you using the 30 day acidification as your definition of leachable Fe? How representative is this of bioavailable fraction? The samples were not filtered. Unfortunately, we cannot quantify the bioavailable fraction from TDFe, thus we assumed that it represents an “upper limit of the Aeolian Fe potentially available for the phytoplankton”, accordingly with what reported by Edwards et al., 2006. However, previous studies showed a significant correlation between TDFe and DFe (i.e. likely more available for the phytoplankton), indicating that when TDFe increases, DFe increases as well. DU, Zhiheng, et al. Relationship between the 2014–2015 Holuhraun eruption and the iron record in the East GRIP snow pit. Arctic, Antarctic, and Alpine Research, 2019, 51.1: 290-298. Xiao, Cunde, et al. "Iron in the NEEM ice core relative to Asian loess records over the last glacial-interglacial cycle." National Science Review (2020). We specify the reason beyond our choice of acidifying the samples for 30 days at L95-104.

L137 - I think it’s problematic that the term “leachable Fe” has not be explicitly defined, described or justified yet Now we have described and discussed “leachable Fe” at L75-78 and L97-106, we also introduced a disclaimer at L167-L171 where we underlined that, because of the different acidification times, the NEEM record cannot be directly comparable to the TD and EDC ones, even though the general trends and feature remains comparable.

L161-163 - Can you expand on why this is an upper limit, why this choice was made, and what other studies have used a similar leach? This is now reported at L97-106

and L167-L171.

L164-170, 184-186 - Is there a lag in time between when the E. Asian dust source influences the N. Pacific HNLC vs Greenland that needs to be accounted for when considering primary productivity patterns? What about the influence of dust from Sahara on Greenland? The time that dust particles spend from the E. Asian dust source to Greenland is about 10-13 days (Schupbach et al., 2018). However, some atmospheric processes during transport might occur, resulting into a different amplitude between the dust deposited over Greenland and over the HNLC North Pacific. Nevertheless, the dust fluxes between Greenland and the North Pacific sediment cores changed coherently and simultaneously during abrupt climate changes. We discussed it deeply at L265-L274. We also added a discussion about the different dust sources that can influence Greenland at L275-L285.

L190 - Can you expand on what types of marine production these proxies estimate? This is now reported at L298-L304.

L233 - Expand on reasons for enhanced water stratification during the coldest periods A deepened discussion is now reported from L297 to L306

Please also note the supplement to this comment:
https://cp.copernicus.org/preprints/cp-2020-77/cp-2020-77-AC1-supplement.pdf

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