Dear reviewers,

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.

Reviewer #2

We introduced a more detailed definition for "leachable Fe". On this purpose, we decided to re-name it as "Total Dissolvable Fe", following the terminology used by Edwards et al., 2006. More details are now reported at L75-78, L97-106 and L167-L171. 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).

At L167-171 we state that a direct comparison between the NEEM record and TD and EDC cannot be done because of the different acidification times. However, the main features and general trends can be comparable. We also introduced a more detailed description of dust sources both regarding the NEEM ice core (L275-L285) and the Antarctic cores (L172-199).

Long-term productivity records in the North Pacific are sparse. We focused on two regions (the eastern and western side of the North Pacific) from where we retrieved both high-resolution productivity records for the last 27kyrs (Meheust et al., 2018) and long-term productivity records (McDonald et al., 1999; Haug et al., 1995).

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

L243-244: I would also hesitate to make statements as strong as the ones on lines 243-244 saying that "the transition zone of the North 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 (L254-255) solely based on comparisons to one marine sediment record and without a better understanding of how much of the ice core Fe is bioavailable.

We totally agree with your suggestions. We changed the sentence accordingly at L378("MPP in the mid-latitude North Pacific might have been more sensitive to the atmospheric Fe supply") and at L391 ("Merging our record with marine productivity data, we found that a link between Fe transport and ocean productivity holds in the mid-latitude North Pacific, suggesting that this area is sensitive to the atmospheric Fe supply"). For these reasons, at L397-399 we concluded that future investigations *that aim to better quantify the more labile and bioavailable Fe fractions are needed to constrain realistic Fe supply and response of the marine ecosystem.*

Regarding the Xiao et al., 2020 paper, we added a section in the manuscript at L229-240 where we disucssed the differences between our record and their findings. We found that the different analytical procedures used might have led to different results. This highlights the need for a standardization of the trace element analysis among different laboratories to achieve reproducible and more comparable records.

L77-78: “what is meant by a “low-resolution sampling apparatus”? Is this just manual collection?

Yes. We changed at L88-89.

L88-89: is there a citation to support dissolution of particles after 30 days?

Yes, we reported the most relevant citation about acidification experiments: Koffman et al., 2014.

L105-L106: were any replicate samples run to assess reproducibility?

Reproducibility was tested both using the TM-RAIN04 reference material reading it every 50 samples (as well as for accuracy determination). We also read six selected samples (3 from the interglacial and 3 from the glacial period) 5 times during the analytical run and we found an average RSD% of 5% (7% for samples from the interglacial periods, 4% for samples from the glacial period). Details are reported from L122-129.

L169-170: provide some more context for these statements. Is this during modern times or from paleo studies?

It is referred to modern times (L253)

L213: what is the “transition zone”

As stated above, we changed the term to “mid-latitude Pacific Ocean” accordingly with the terminology used by Amo and Minagawa in their paper.

Reviewer #3

We modified the figures accordingly to what you suggested. Your stylistic suggestions are implemented in the main text as well.

The authors should comment on changes in water stratification that may occur during glacial periods in the North Pacific, is productivity more limited by nitrate rather than Fe? What are the upwelling conditions like near the sediment core records you are comparing the ice core dust flux to? Need more information that suggests atmospherically transported Fe (versus upwelling) is the primary source of Fe to these sites.

We discussed more in details the reason behind the stronger water stratification during the Last Glacial Period. During that time, the nitrate consumption efficiency was high, despite the low marine primary productivity suggesting an iron limited primary production. More details at L309-327.

Considering the negligible role that aeolian Fe fertilization had in these regions during glacials, we assume that other Fe sources played a more relevant role in regulating MPP in the subarctic Pacific Ocean. More details at L347-L356.

Can you comment about oxygen content in the Glacial North Pacific during this time period?

We briefly commented about the oxygen content in the glacial North Pacific when we discussed the causes that enhanced water stratification during the Last Glacial Period (L314-319).

Why don't you compare your NEEM record to other Greenland ice core records? Or more information is needed as to why you chose TD, LD and EDC specifically to compare your NEEM Fe flux to.

We added a new section where we discussed differences and similarities between our record and a lower-temporal resolution Fe record from the same location. More details at L229-L247. There are not other Fe long-term records for the Arctic region.

To our knowledge, iron records from TD, LD and EDC are the only one that cover a time period which is comparable to the NEEM record.

L16-17: one sentence is attributing Aeolian dust as one of the main Fe sources to the ocean and the second sentence is stating that ice cores provide a sensitive and continuous archive for reconstructing Fe fluxes over last millennia. I suggest the authors add a sentence or portion of a sentence stating how Aeolian dust transported over past climate periods is preserved in the ice core record.

We modified accordingly (L15-L18)

L137 - What makes this fraction the leachable Fe concentration? Are you assuming that the 1 month leaching at a pH 1 in HNO3 is the labile portion?

We changed the terminology from "leachable Fe" to "Total Dissolvable Fe", accordingly with Edwards et al., 2006. This fraction represents the amount of Fe that can be effectively dissolved from mineral particles at pH 1 for one month. Our acidic digestion procedure was made following well established protocols as described in Koffman et al., 2014. More details, references etc… are reported at L75-78, L97-106 and L167-L171.

L160-163: this is an important point. To know the truly labile portion of Fe present in the ice core dust, it would be necessary to leach the dust in conditions similar to what is observed in the modern ocean. Could be useful to discuss that a bit more here.

As for the previous answer, we clearly stated that this represents an upper limit to the amount of Fe that can actually be available for the phytoplankton. We underlinde that previous studies have reported a correlation between DFe (Dissolved Fe) and TDFe (Total Dissolved Fe), meaning that periods with higher TDFe were also periods with higher DFe:

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).

L164 - What time periods? Glacial and interglacial? What about Lupker et al, 2010 who suggested Sahara as an additional potential source?

We discussed the possibility of other dust sources at L275-L285 where we also reported findings from Han et al., 2018 which refer directly to the NEEM ice core.

L190 - Need more information about what Brassicasterol concentration is informing on

We added a paragraph that discusses more in details the proxies used for the determination of past marine productivity (L298-L304)

L205-207 - okay but what about when the sea ice eventually melts? How long is this sea ice though to have persisted for? If atmospheric dust was deposited on sea ice surfaces presumably when the sea ice melted there would be a pulse of Fe to the surface ocean? It would be interesting to expand on this here.

Unfortunately, we do not know for how long sea-ice persisted in the investigated regions. However, we know that during seasonal and marginal sea-ice conditions, productivity was higher than during perennial sea-ice periods, which suggests that when sea-ice melted it might have provided micronutrients (as well as sunlight) to the ocean system (L334-336).

Conclusions & future perspectives - I think this section can be expanded upon, right now it just reads like a quick summary of the main points brought up in the manuscript without expanding on why we see the largest differences in the Fe record during MIS 4, what this means in terms of dust supplied Fe to subarctic Pacific in previous climate regimes (e.g. Mid Pleistocene Transition_)

L252 - What is the underlying mechanism for this large difference during MIS 4? Or hypothesis?

We added a more comprehensive and detailed explanation on the possible reasons behind the enhancement of Fe transport during MIS 4 in Greenland at L210-228. For this reason, we did not discuss it further in the conclusions keeping them short and essential.