This manuscript by Jones et al. presents a nice incubation study from a research voyage to the Western Antarctic Peninsula. The results seem to be of high quality, the interpretations are supported by the results, and this publication will be a valuable addition to the literature on oceanic iron sources. There are a few comments I have that aim to improve the manuscript, alongside a few editorial comments and sentences that were not very clear to me. Overall, I think the authors will be able to address these in a single revision round.

Kind regards

Sebastiaan van de Velde

General comments:

In the results, I would start with the solid-phase and porewater, as these factors are going to help us understand the experimental results (sandy vs muddy, high/low Fe PW, etc.). The current order makes us think about the experiment, but a lot of the factors that are important only get disclosed later.

We have taken this suggestion on board.

Somewhere in the results you mention you did not get core data from BB because of issues during sampling. Why was it difficult sampling BB? You got material for your experiment, so was it the nature of the sediment or other issues? The fact that you had issues might also indicate something about the nature of the sediment.

BB was difficult to sample for a number of reasons. We had issues with the multicorer and it failed quite regularly to bring up several cores due to technical issues. Across the WAP, the sediment very close to the glacier is quite hard to sample successfully, so this was a compounding problem. In Borgen Bay, at the original coring site, there was not a lot of soft substrate at this particular spot so we were not recovering very nice cores. However, we were able to sample nearby for the incubation material (BBD, on map) but were not able to retrieve a core appropriate for pore water sampling. Seb is right that this might indicate something about the nature of the sediment however unfortunately we don't have any specific evidence that this sediment was particularly different to the other sites. Anecdotally, grain size distribution was pretty similar to Marian Cove and Sheldon Cove.

I also could not find the data for porosity (at least not easily), sediment grain size, etc. It might be helpful to start the results with an overview table of all these parameters (oxygen penetration depth, nitrate, grain size, etc.) – this will help the reader to get an idea of the different sites from the experiment, and it will be easy to refer back to when they are reading the discussion.

Thank you for your suggestion. The manuscript should have had a Table 2, which includes bottom water O2, OPD, Nitrate penetration depth. Reviewer 2 also noted this and so I suspect this Table was potentially missed out of the pdf during the submission conversion process, and I will check this is included in the submission. We have now included porosity as a column in this table.

For sediment grain size, we haven't actually included this within the manuscript. If the reviewer / editor thinks this would significantly add value to the article we are happy to include the available data, although the coring strategy was slightly different for grain size and so the sediments are not always representative of the sediment used for incubation/extraction. The grain size was determined from grab samples across the bay.

In the discussion, you focus on reductive dissolution to explain the measurable iron concentrations in the oxic zone in the sediment. But Will Homoky has published a few (2 that I am aware of?) that suggest that non-reductive dissolution of iron minerals could be a source of iron as well. This could also explain your measurable iron in the surface layers, so probably should be considered as well.

We totally agree and we now include a concise discussion of this, as both you and the second reviewer suggest it.

I do not follow the final estimate is not too clear to me – it seems to me you are estimating solid iron fluxes in a similar fashion as Burdige did before, but the real novelty here is that you relate dissolved iron (sFe+cFe) to resuspended solid-phase iron. This is an important addition, since it is the dissolved fraction that will be transported away with the currents and fuel production.

Why do you not use your experimental results to link a dissolved fraction that comes off resuspended solids (you can use your end of experiment concentration, even with the porewater contribution, since this would also contribute in reality). This could then be compared to diffusive/irrigational fluxes (either the Dale estimate or other measurements) and would be very informative.

To the above two paragraphs. We thank Seb for his suggestions here and have implemented the end experiment concentration as a flux of dFe – comparing with Dale et al. 2015, and de Jong et al. 2015. We have removed the solid sediment flux estimates as we agree that these are less informative, and also retain more uncertainty (what actually dissolves, versus scavenging and other removal processes, and also general uncertainty associated with these operationally defined Fe sediment phases). Seb's suggestion of using dissolved Fe fluxes that are actually measured by the incubations is much more

powerful as these values relate to more bioavailable material, demonstrate that the sediments are a contributor of dissolved Fe (beyond the pore water alone), and are directly measured and are more robust than the fluxes we previously calculated.

Editorial comments

L25: would those fluxes not need a time-unit? E.g., d-1 ?

We have changed the flux estimates now to look at the dissolved material and will now include a time unit of per day. Originally we stated 'per suspension event' but we agree that this may be confusing to the reader particularly without context of the full manuscript.

Figure 1: 'ADI, ANV, KGI' are not readable on the map. SACC is not defined

We have now removed these labels as they are not particularly informative, and increased the size of the maps and defined SACC.

Table 1: would need S/N and E/W ?

We used the negative sign to reflect S and W but have changed this in case it makes it clearer.

L121: that is a bit odd phrasing 'equivalent sites' – did you not take cores from the same Multicorer, or from repeat casts at the same site?

This has been made clearer. Cores were taken from the same multicorer cast.

L130 that is not entirely clear, did you profile in predrilled holes?

Yes, I think you mean the 'intact' line? This has been made clearer. Generally, oxygen cores were taken from tubes that did not have predrilled holes (and the firesting probe enters the sediment from the top). All pore water cores were collected from core tubes that had predrilled holes that were taped.

L137: you already say this above, where any other measurements done on these cores prior to incubations?

This has been edited to avoid repetition

L145: so the sediment was only resuspended prior to sampling, so that is in uneven intervals (after 1h, after 3h, after 8h, …)?

Yes to make sure things stay mixed ahead of sampling. Carboys will also experience some gentle mixing with ship movement. If repeating these experiments, perhaps a more consistent shaking would have been better (e.g. every 4 or 8 h). We suggest that the shaking of the carboys did not have a significant impact on the variability in our results i.e. did not encourage continuous redissolution by mixing, as the triplicate results were highly comparable and the pattern in Fe concentration over time was pretty consistent across replicates and sites.

L199: did you keep samples anoxic during sampling and extractions?

No we did not. For this reason we suspect that the reducible Fe(II) component could be an underestimate but we are unable to verify this with the material we have.

L417: 'by 72.3 and 69.9 uM' – a bit redundant and not very useful, would either say x times higher, or just say it exceeds and let the reader work it out.

Done, removed.

L421: for the two stations you have the data you get similar fractions – so perhaps that indicates that your estimate for BB is not very robust?

True. The value of 170 uM is however calculated from the directly measured initial Fe release during the sediment incubation, using an environmentally representative porosity to calculate the required pore water Fe to account for the initial increase to ~180 nM at 1 hour (a porosity of ~0.75, similar to the other core in the bay, and the other two sites). The reason this value is so much higher is because the dFe in the incubation, released after 1 hour, is so much higher than at the two other sites. Why this is exactly, is unclear really, since all experimental conditions were kept the same.

The incubation sediment is taken close to the glacier. Because the porewater dFe profile used in this section as an example surface pore water concentration is from a core nearer the mouth of the fjord for Borgen Bay, we do not further implement this in our estimates of pore water dFe released into the incubations. Coincidentally, if we had, the subsequent estimates for the estimated fraction of dFe released, accounted for by measured pore water dFe, sits within the range of the other two sites. We have therefore not made any changes (except a small edit to improve clarity).

L426: going from FeIII to FeII is not liberating electrons but removing them (also not sure that is the correct terminology)

Thank you for noticing this mistake we have now reworded this:

'Typically, enriched dissolved Fe concentrations in pore waters are indicative of reducing (i.e. anoxic) conditions, where microbial activity uses Fe(III) as an electron acceptor, and generates reduced and soluble Fe(II).'

L431: is it that, or is it the non-reductive dissolution (as shown by Homoky) ? You would not need reduced iron diffusing upwards

We have addressed this as stated above.

Reviewer 2:

This manuscript by Jones et al. presents a valuable study of benthic Fe cycling and sediment incubation experiments in Antarctic sediments. Overall, the manuscript is well written, presents high-quality data, and includes a thorough and detailed discussion. The dataset is particularly valuable given the crucial role of Fe in the Southern Ocean and the logistical challenges associated with sampling in this region. I recommend publication after minor revisions addressing the comments below.

General comments

Flux estimates uncertainties (L524-532): I am not convinced by the value of this extrapolation. The flux estimates are based on multiple assumptions, but the associated uncertainties are not quantified. Given the large uncertainties reported for Fe fluxes in previous studies, these calculations should either include a proper uncertainty analysis or be presented more cautiously.

We thank the reviewer for this comment and agree with their concern. We have revised this approach and used the sustained dFe contribution to the incubations to determine a flux of dFe from sediment resuspension, as suggested by reviewer 1. We think this is a more robust and informative approach as a) we measured this directly within the incubation and b) as such, the estimates are more robust, and c) the dissolved Fe will be more bioavailable and can therefore have a greater direct impact upon primary productivity.

The manuscript states that the bioavailability of glaciomarine sediment Fe has not yet been determined, yet later refers to the investigated Fe as "potentially bioavailable". Since no bioavailability experiments were conducted in this study, this appears to be an overstatement. Please consider using a more cautious term (e.g., "potentially reactive Fe", "labile Fe", or "potentially bioaccessible Fe", or "easily reducible Fe oxides", ...) or clearly explain the basis for inferring potential bioavailability.

We thank the reviewer for this comment and agree that there is lacking evidence that the material we sampled is potentially bioavailable. It has been demonstrated that glaciogenic material is potentially bioavailable, somewhat as a function of mineralogy and Fe(II)/Fe(III). We did not intentionally overstate however since we didn't measure it we

agree with the reviewer to be more moderate with the language used. However, we have now altered this section of the discussion to discuss the flux of dissolved Fe measured in the incubations. Since dissolved Fe is better understood as potentially bioavailable we have retained this language when discussing dissolved Fe. We have altered any discussion of solid phase Fe to use the term 'reactive', where appropriate. We have also included reference to the evidence that glaciogenic silicate-rich primary minerals have been shown to be bioavailable to phytoplankton (e.g. Wyatt et al. 2023; Shoenfelt et al. 2017).

Technical errors

L51: Please delete one "may".

done

L73: Replace "were conducted" with "conducted".

done

Figure 1: The legend in the small satellite pictures is not readable on the map. Please increase font size.

Done.

L153: Change capital letter in "All".

done

L155: Define the abbreviation "HR-ICP-MS" at its first occurrence and specify the manufacturer (company).

done

L175: Add the measured value of the reference material SLRS-4 (target values are given).

We have now edited and improved reference material measured values (and included missed information on reference material BIR).

L197: Add "operationally defined" prior to Fe oxide. As there is no XRD, no Mössbauer, no EXFAS the extracted Fe pools are operationally defined. The interpretation is entirely indirect.

done

L198: The notation for iron species is currently inconsistent (e.g., Fe(II), Fe2+, Fe3+, Fe(III), Fe(III)s and Fe(II)aq). Revise the manuscript to use a consistent notation throughout.

We have removed use of Fe2+ and Fe(II)aq and mentioned that the Fe(II) is either in dissolved form or soluble, where appropriate

L213: Use consistent SI units for concentration (e.g., molarity (M)), unless there is a specific reason to report N.

done

215: Additional references for this statement: Hepburn et al. (2020), Slotznick et al. (2020).

done

L247: Replace “agaim” with “again.

done

Chapter 5.2: It would be helpful to show a Table (either in the main text or supplements) with OPD estimate and BW oxygen, nutrient concentrations. This would provide a clearer picture of Fe redox cycling and make it easier for readers to follow and evaluate the interpretations.

Thank you for your suggestion. The manuscript should have had a Table 2, which includes bottom water O2, OPD, Nitrate penetration depth. Reviewer 1 also noted this and so I suspect this Table was potentially missed out of the pdf and I will check this is included in the submission.

L417: Use consistent notation for “pore-water” (sometimes pore water L420). Check throughout the manuscript.

done

L427: Are you referring to bottom water oxygen concentrations? It is not entirely clear.

Rephrased to make clearer

L432-438: Reductive dissolution is not the only mechanism responsible for dissolved Fe release from sediments. Given that the investigated sediments reveal an oxygen penetration depth >= 3cm, non-reductive dissolution pathways (e.g., proton- or ligand-promoted dissolution) may also contribute to dissolved Fe release, particularly in the oxygenated zone. Please consider acknowledging these processes

or clarify that the discussion specifically refers to reducing sediments below the oxic zone.

See response to next point

L455: This would also be an appropriate place to address Homoky's suggestion that NRD in oxic sediments are likely associated with colloidal lithogenics.

We agree with the reviewer that this is important to discuss. A previous version of the paper was not well received based on this discussion, however we think it is important to mention. Following your suggestion, we have edited the text now from lines 430 to 451.

L462: Please add “operationally defined” prior to “highly reactive”.

done

L488: Add “,” after (Gerringa et al., 2012).

done

L519: Please remove bracket prior to 2022.

done

L550: Please format “ox1” as a subscript in “Feox1”.

done

References:

Hepburn, L. E., Butler, I. B., Boyce, A., Schröder, C. (2020), The use of operationally-defined sequential Fe extraction methods for mineralogical applications: A cautionary tale from Mössbauer spectroscopy, Chemical Geology, 543, 119584. https://doi.org/10.1016/j.chemgeo.2020.119584.

Homoky, W.B., Conway, T.M., John, S.G., König, D., Deng, F., Tagliabue, A., Mills, R.A. (2021), Iron colloids dominate sedimentary supply to the ocean interior, Proc. Natl. Acad. Sci., 118, e2016078118. https://doi.org/10.1073/pnas.2016078118.

Slotznick, S. P., Sperling, E. A., Tosca, N. J., Miller, A. J., Clayton, K. E., Van Helmond, N. A. G. M., Slomp, C. P., Swanson-Hysel,l N. L. (2020), Unraveling the Mineralogical Complexity of Sediment Iron Speciation Using Sequential Extractions, Geochem Geophys Geosyst, 21, e2019GC008666. https://doi.org/10.1029/2019GC008666.