EGUsphere Comments:

Reviewer 1:

This manuscript submitted by Gavin Piccione and co-authors presents a hydrochemical reconstruction of subglacial meltwater conditions under the East Antarctic Ice Sheet during Termination III. This study is largely of (isotope) geochemical nature but also involves a minor modelling component. I enjoyed reading this manuscript with all the various research methods involved, and was particularly intrigued by the discussed trace metal budgets as a function of prevailing subglacial water conditions and oxygen availability. It is well written and fits the journal well. I have several points which I would like to see addressed yet would suggest major revisions to the current version.

> *We thank the reviewer for the positive comments, and for the careful consideration of the chemical analyses and modelling component. These helpful suggestions will surely strengthen the manuscript. Below we have added comment-by-comment replies.*

Major points

The description dealing with the generation of U-series data in the manuscript is a bit vague. The authors describe the chemical purification and analytical measurement. In lines 99-101 the authors write:

"Accuracy of 234U-230Th dates were tested using MIS 5e coral and compared to dates from (Hamelin et al., 1991), as well as a previously dated carbonate precipitate (Frisia et al., 2017)."

Matching previous results is good practice. It suggests the mass spectrometric and chemical approach is robust. However, are we dealing with calcitic or aragonitic material here? In the first case we could encounter the problem that the oxidation state of U in aqueous solutions is 6+ and the dominant aqueous speciation involves the uranyl moiety (UO2+). The authors discuss the oxygen deficit in the parent solution, but if U was present as a uranyl tricarbonate ion, it would not fit into a calcite matrix (Reeder et al., 2000 Environ. Science Techn.), raising concerns about the long-term retention of U in these special carbonates. Did the authors also obtain U concentrations? It would be very helpful for comparison with other carbonate matrixes, marine or non-marine.

> *Carbonate in sample PRR50504 is calcite, and we measured uranium concentrations between 1.3 and 3.1 ppm (see supplementary data table 3). Although open-system behavior in the U-series system is always a concern, we note that the ages we*

collected fall in stratigraphic order, which lends confidence to closed system U-series behavior.

From XANES data in similar subglacial calcite, even under suboxic conditions uranium is incorporated into the calcite lattice as uranyl in the Ca2+ site. Unfortunately, we do not have U XANES confirm this on PRR50504.

In the revised manuscript, we have been more specific about sample mineralogy, referring to the samples as "calcite" instead of "carbonate". Starting on line 222, we added the U and Th concentrations and mentioned the possibility of open system behavior. We point to the fact that ages fall in stratigraphic order as evidence that the U-series isotopes are behaving as a closed system.

How significant is the uncertainty in initial 234U/238U in resulting U/Th ages? But the most important point – as far as I can tell – is whether these carbonates are calcite or aragonitic? Personally I made the painful experience that for example Antarctic cold water corals, which are calcitic, display pronounced U open system behaviour and even had quite variable 230Th/232Th (Gutjahr et al. 2013 Chem Geol).

We added mention of the uncertainties on the initial 234U/238U and the 230Th/232Th.

The elemental analyses are very interesting. One point that I stumbled over, however, is the question how these major, minor or trace elements are incorporated into the sample. Are these assumed to be structurally incorporated, or rather scavenged into Fe oxides or other mineralogical phases in the precipitate?

We added added text describing element incorporation.

I am asking since I wondered about the partition coefficients of the presented elements into opal or carbonate material. In other words, could some of the variability in elemental concentrations shown throughout this manuscript just be controlled by mineralogy as opposed to subglacial water chemistry and elemental concentration?

We've added language on describing the potential effect of mineralogy. We are wary of the potential for error in using Kd to try and quantify subglacial water metal concentrations. Therefore, we avoid trying to use the precipitate record to quantify subglacial metal concentrations, and do the exercise described in figure 5 as only a qualitative investigation of the sensitivity of increased metal mobilization on Fe discharge.

The authors should still properly discuss this potentially obscuring factor. Can some of the variation in elemental concentrations be exclusively ascribed to mineralogy? The implications laid out in section 3.4 for example depend heavily on this detail.

> *We have added text addressing the potential opal versus calcite incorporation of metals the effect of mineralogy on metal incorporation.*

The authors should also spend a bit more time on discussing key parameters on oxygen availability in meltwater below the ice sheet. I have the impression that this is still not well enough laid out. I emphasize this point since it is an essential component on observed and inferred elemental subglacially dissolved metal concentrations. What is the most efficient process to provide oxygen to these subglacial water bodies?

> *We've added a paragraph to more clearly spell out the parameters controlling oxygen.*

Lines 263-264 and in general: Before quoting U isotopic ratios, please define these! I assume these are activity ratios, not elemental. And you have to introduce whether these are present-day or initial 234U/238U. In fact, if these are present-day 234U/238U, then you should also calculate initial 234U/238U since these will be substantially higher. If these ratios are modern 234U/238U it would provide evidence for substantial enrichment of 234U in these TIII meltwaters, which is worth a dedicated discussion section given apparently strong incongruent release of 234U during subglacial weathering.

> *We have added this clarification in the text and added the subscript "i" to denote that we refer to the initial 234U/238U throughout the manuscript.*

Minor comments

Line 213 and the following reads: "calcite accumulation is i) minimal during times when subglacial flushing rates are slow because CO2 delivery is low". I may be a bit slow here but as far as as I am aware higher CO2 delivery should lead to higher DIC that will lead to calcite undersaturation, not trending toward supersaturation, unless alkalinity equally increases alongside. This statement here hence seems quite speculative and not always necessarily the case. Could you clarify this point?

> *We've clarified this point in the text*

210-211: If opal precipitation is slow, is this growth substantially slower than the carbonate layers? If yes, what would this imply for the determined age model (and involved uncertainties) given the alternation of calcite and opal layers?

> *Based on previous work (Piccione, et al., 2022) we assume that opal precipitation is slower than calcite precipitates based on the fact that opal precipitates as a colloid*

> *from solution and settles out of solution. The difference in formation timescales between opal and calcite formation timescales does represent an uncertainty in our age model. While an exact quantification of this uncertainty is impossible, based on the centennial-scale precipitation of fine opal-calcite laminations below the unconformity in PRR50504 (Fig. 2b), slow opal formation contributes to uncertainty in our age model on the decade to centennial scale. Given the millennial-scale uncertainty in our age depth model (Fig. S1), we deem this the uncertainty contributed to slow opal growth to be of secondary importance.*

Fig. 4c: Why not simply show sample height in mm instead of micrometres?

> *We've added the revised figure.*

Line 416 and elsewhere in the text: I thought a glacial termination is followed by an interglacial. It hence reads a little awkward if the authors discuss climatic cooling directly following TIII. Such phrasing is used at various places in the manuscript.

> *We agree that it is unconventional to refer to cooling after terminations, given that interglacial periods are usually thought of as the warm climate state. However, terminations are the warmest periods of the glacial-interglacial cycle, and interglacials are periods of slow cooling over ~90kyr.*

Lines 462-464: Here the authors state: "Total Fe flux increases by about 0.2 Gmol yr-1 for every 0.001% increase in the fraction of basal water made up of Fe-rich brine, and increasing by an order of magnitude when subglacial waters contain 0.005% brine (Fig. 6c)." This is quite a statement. The authors may be right but I cannot entirely follow the argument. Could the authors corroborate their suggestion in a little more detail? While they may be right, I remain a bit sceptical how realistic this may be, also with regard to the potential effect of differential elemental partitioning into the sample (see my major Kd comment above). And it would obviously first of all apply to the subglacial setting here.

> *We add text to describe how we arrived at these estimates and to stress that these are only schematic representations of the potential impact of metal accumulation beneath the ice sheet.*

<u>Reviewer 2:</u>

The role of ice sheets in global biogeochemical cycles has received increased attention in the last decade, in part due to the sensitivity of these and their connected environments to climatic warming. There is large uncertainty in the role of the Antarctic Ice Sheet in regional biogeochemical cycles, in part because of the difficult-to-access subglacial drainage system where the majority of chemical weathering (and release of biologically important elements) is expected to occur. Associated with this is an almost complete lack of data on how meltwater and elemental fluxes from the Antarctic Ice Sheet might change under different climate scenarios. Data elucidating these temporal subglacial processes is extremely rare. Piccione et al. use the geochemical composition of accurately dated subglacial precipitates collected from an ice marginal moraine to glean information about chemical weathering conditions at the ice sheet bed, and explore how these conditions might have changed during a period of climate transition. The study uses multiple complementary and leading-edge analysis to reinforce the findings, including isotopic dating, elemental mapping and elemental speciation. The authors hypothesize that opal and calcite precipitation record centennial subglacial flushing events and that meltwater trace element export (in this case Fe, Mn, Co and Mo) is related to reconfigurations in subglacial drainage associated with ice thickness (and therefore climate). The highest flux of trace elements is assumed to be following TIII, where a thinner ice sheet and cooling climate reduces basal melting, and therefore delivery of oxygen, instead promoting mobilization of redox sensitive trace metals in a hypothesized subglacial brine.

General comments

I really enjoyed reading this manuscript – it's well written, has a really novel and high-quality dataset, and uses innovative approaches to add evidence to the main hypothesis. This is only one precipitate sample, but I appreciate the attempt to upscale processes to the ice sheet at large, given the paucity of data available. There's not much that I would recommend changing beside some suggestions, clarifications and typos in my specific comments below. My general comments below are more to stimulate some additional discussion, rather than requests for any substantial changes to the text.

> *We thank this referee for the thorough analysis and thought-provoking ideas for our paper. We feel these will greatly strengthen the manuscript. Below we have added comment-by-comment replies.*

My main general comment is more of a question. In my mind, I would have expected that these trace elements (Fe and Mn anyway) would likely originate from silicate bedrock weathering, and should therefore be enriched in opal bands, which presumably indicate elevated silicate bedrock weathering (and supersaturation of amorphous silica) and therefore longer residence waters. The argument here is that opals precipitate from freeze concentration (cryoconcentration), but it would also require a non-trivial amount of dissolved silicon. Within this context, it might also be useful to have a little additional discussion on how these precipitates might/might not incorporate these metals (i.e. are the metals retained in an opal precipitating environment). I think I just need some more clarity in my mind.

> *We agree that additional text describing how opal and calcite may/may not incorporate dissolved metals would strengthen the manuscript. The reviewer is correct that trace elements likely originate from silicate weathering, and opal precipitation would require high amounts of dissolved silicon, likely from cryoconcentration. Both opal and calcite layers below the unconformity in the sample are low in trace metals, suggesting that these elements were insoluble in an oxygen-rich solution. Both opal and calcite above the unconformity are high in Fe, Mn, and Cu, though not consistently. Combined we interpret these data to suggest that the main driver of metal incorporation in both opal and calcite is redox state. We will add more discussion of this point in the revised manuscript, and will also discuss how element partitioning into opal/calcite might affect this interpretation.*
>
> *We've added a paragraph detailing the potential incorporation mechanisms in calcite.*

Similarly, calcite accumulation is assumed minimal during periods of low CO2 delivery, presumably because there is less CO2 supplied from melting basal ice? The assumption here is that there are no other proton sources in the subglacial environment, which may/may not be correct (e.g. mineralization of organic matter, sulfide weathering etc…), and that there are no alternative mechanisms of calcite precipitation, but this probably needs a little discussion, even if it is mostly signposting.

> *We have added text clarifying carbon sourcing is likely through organic carbon mineralization.*

Another general clarification needed is that precipitation is slow and likely only occurs in low physical weathering intensity environment, as the authors comment. How do we know this is representative of subglacial environments across the EAIS? What kind of flow rates are required to sustain long term calcite/opal precipitation? Does this tell us anything about the efficiency of the subglacial hydrological system in the location that these precipitates formed? Although some of these questions are outside the scope of the paper, others are quite important for interpretations of e.g. iron export to the Southern Ocean.

> *We add language that describes the residence times of the metal-rich waters from our sample, and acknowledges that discharge may lag metal-accumulation by thousands of years, which cannot be accounted for by our model.*

My final general comment is based on the assumption that the parent waters are Fe-poor, based on Whillans Subglacial Lake data from Vick-Majors et al. (2020). Although I don't think is a bad assumption, our other data (Hawkings et al., 2020), suggests that the filterable Fe concentrations could be much higher than Whillans (even if you consider different smaller pore size filtered – i.e. 0.22 um vs 0.45 um) and Mercer Subglacial Lake has higher measured O_2 concentrations than Whillans Subglacial Lake (see cited Priscu paper). Perhaps the argument here is that Mercer might contain more of the brine that is being postulated to occur in some subglacial environments here for some reason? A related argument could be that the Mercer data is more representative of conditions beneath the East Antarctic Ice Sheet, given that waters entering Mercer include those sourced from East Antarctica. Either way, it would be an interesting experiment to include this data at the other endmember for the modelling experiment, given it's our only other datapoint for these environments.

> *We added that Whillans waters were selected because they contain dissolved oxygen and low redox sensitive elements. We view Whillans as a more representative estimate of subglacial meltwater. As we assume that the majority of water flushing from active regions in the ice sheet periphery would be closer to a fresh glacier meltwater endmember.*

Specific comments:

L293: Is the suggestion that these are part of the carbonate structure, rather than as e.g. oxides within the carbonate precipitate? Is siderite present – doesn't look like from the XANES?

> *We've added language explaining how Fe, Mn, Mo, Cu, and S are likely incorporated into the carbonate lattice or through incorporation metal nanoparticles.*

L439: I would probably use the term "subglacial meltwater" rather than "basal meltwater" as its more of a catch all term

> *We've deleted instances of "basal meltwater" and replaced with "subglacial meltwater.*

L449: Could you quote some values here for comparative purposes

> *We've made this change in the revised manuscript.*

L449: I think this needs more thought. Typically, you would assume Mo (and Cu to a degree) to be more soluble and more mobile in oxygenated environments, and less mobile in hypoxic environments, which is a reason it's used as a redox palaeo-proxy (e.g. Boothman and Coiro, 2009; Boothman et al 2022). In this way it is expected to behave differently to Fe and Mn, which would be less mobile in oxic waters. So there's a little bit of conundrum here is that you really need a redox boundary for Mo to accumulate onto Fe and Mn oxyhydroxides.

We've added clarification of this point in the text.

L474: typo "may have occurred"

We have removed the section with this typo in the revised manuscript

L488-491: I think this should probably focus on Fe, as that's the primary limiting micronutrient in the Southern Ocean.

We have made this change in the revised manuscript.

Figure 4: Can you include Si on panel c? Is it possible to include a scale bar on A?

We added a scalebar to A. Unfortunately, we are not able to measure Si concentration by LA ICP-MS due to isobaric interferences in an Ar plasma.

Figure 5: needs labelling of panels (A-C)

We've made this edit.

Figure 1 or 4: Is it possible to indicate where XANES were collected on the precipitate?

We will add a supplemental figure with XRD maps showing where the XANES data were taken.

Reviewer 3:

This is an excellent paper that provocatively provides an intriguing line of evidence for the Southern Ocean Fe fertilisation hypothesis as an explanation for the positive feedbacks in the glacial period carbon cycle. I have two substantive pieces of criticism.

We thank the reviewer for their thorough and insightful comments. They will greatly benefit the manuscript. Below, we have added comment-by-comment replies.

First, the manuscript infers quite a lot from a single sample. An alternative hypothesis could be that the glacial period transition causes a reconfiguration of the local glacial hydraulic system, so that the sample became cut off from regular input of subglacial water. There is plenty of offshore evidence for shifts in the sources of glacial material between glacial and interglacial cycles (i.e. Licht and Hemming, 2017). Such reconfigurations of ice streams during glacial-interglacial transitions would presumably create all sorts of local changes to ice sheet basal hydrology, with geochemical consequences. But I do think publication should go ahead, just perhaps with a bit more acknowledgement of the limitations and backing away from the claim that all of Antarctica is represented by this single sample.

> *Throughout the manuscript, we have softened language to acknowledge the limitations of the dataset, describe the results as a local subglacial signal, and only describe the potential for larger scale metal mobilization based on these local observation.*

Second, the authors cannot propagate a surface temperature record instantaneously to the bed as they do in equation 3 of the appendix. The authors write: “By using a steady-state approximation in Equation (3), we are neglecting the time lags with which a climate signal affects the thermal energy balance of an ice sheet base. MacAyeal (1993) provides an insightful discussion of these lags.” MacAyeal’s “insightful discussion” presents a mathematical argument of why any perturbation in surface temperature would never make it to the bed in anything nearly as thick as an ice sheet (specifically he postulates no temperature perturbation could make it more than 314 m through ice over the timescale of Heinrich events). Indeed, the latest temperature perturbation caused by our present interglacial period has had absolutely no effect on the temperature gradients at ice sheet beds anywhere they’ve been measured in Greenland and Antarctica. If a change to the Q term has any appreciable effect on the meltwater generation model in the appendix, then Figure 1F is invalidated.

> *While we concede (here and in the original manuscript) that the reviewer's general point is valid, this specific statement is clearly an exaggeration. At the top of the second column on page 777, MacAyeal (1993) states: "The e-fold decay scale for the stationary ice column, equation (3), is ... 314 m." The e-fold scale is simply the depth at which the amplitude of the surface temperature fluctuations is reduced by 1/e (~1/2.71 or to about 37% of the surface amplitude). So it is not correct to say that MacAyeal's analysis is used to argue that "no temperature perturbation could make it more than 314 m through the ice on the timescale of the Heinrich events". The reviewer also focuses on MacAyeal's lower diffusion-only estimate of the e-folding depth scale (314 m). However, MacAyeal clearly recognizes just below in the same column of his paper that including vertical advection at the rate of -0.25 m/yr (not too dissimilar to the vertical advection used in our simple model) increases the e-folding depth scale by about a factor of three (to ~970 m). The other fundamental difference between our simple model and MacAyeal's is that in our model the "ice sheet", simplified to a geometry of constant area and a single, variable thickness, has a thickness of about 2 km, while MacAyeal applies his analysis to the central part of the Laurentide ice sheet, which is 3-4 km thick. Thus, if we are dealing with an e-folding scale of 1 km (about 970 m), ~14% of the surface temperature amplitude reaches the depth of 2 km, but this decreases to ~5% at 3 km.*

> *Similarly, when the reviewer makes the statement that glacial-interglacial ice temperature fluctuations are still visible in measured ice temperature profiles, they probably mean the measurements made at ice core sites in thick ice near ice divides. Hermann Engelhardt has published a few papers with modeling results aimed at explaining several vertical ice temperature profiles that he measured in the thinner parts of the West Antarctic Ice Sheet, and his model fits do not focus on invoking past ice temperature changes. These more marginal, more dynamic parts of the ice sheet (compared to the locations of the ice divide where most deep ice cores are collected) are not as good as the ice core locations at "remembering" signatures of geologically recent climatic changes (e.g., LGM to Holocene).*

The authors need to revise their model of basal melt so that T_s and a, and H are held constant at long-term averages for equation 3. There are no temperature, accumulation, or thickness perturbations of anywhere near sufficient duration to effect a change in the temperature gradient at the bed across the termination III period. The approach in the supplement is otherwise correct. But this teleportation of heat from the surface to the bed is physically impossible. The analysis needs to be redone, Figure 1F replotted, and possibly some of the data reinterpreted accordingly.

> *In response to this suggestion, we have:*
>
> *(1) smoothed the **Ts** and **a** records using a 10,000-year moving-average filter*
>
> *(2) smoothed the **H** record using a 2,000 year moving-average filter*
>
> *In the initial paper comments (post review) we attached a figure describing the outputs of these excercises and refer to it here:*
>
> *-- Justification for choosing 10kyr filtering for **Ts** and **a** -- There is an a-priori argument that a smoothing filter with this period retains the impact of orbitally-forced climate fluctuations on the **Ts** and **a** records. The a-posteriori argument is that this filtering results in a very slow change in the calculated contribution of heat loss to basal melting (see the magenta line in the attached figure with no labels - the vertical axis is in units of cubic km and the horizontal axis is time in ka). The magenta line is just a gentle wave with amplitude of ca. 25 cubic kilometers as opposed to the black line (showing the summation of the two sources of basal heat: shear-heating and geothermal flux) which is varying on shorter timescales and has amplitude of ca. 120 cubic km. So, the resulting total basal melt rate (the red line in the attached figure) is obtained by subtracting the magenta line from the black line. Since geothermal flux is assumed constant, and the basal heat loss varies slowly and gently, the bulk of the variability in the calculated basal melt rate is due to changes in shear heating. So, this*

is evidence that a 10kyr filter is sufficient to mute the effect of ***Ts*** *and* ***a*** *variations on changes in basal melt rates.*

-- Justification for choosing a 2kyr moving average filter for ***H*** *-- First of all, it is important to reiterate that variability in* ***H*** *has a much greater effect on variability of shear heating than on changes in the basal heat loss. In our model, variations in* ***H*** *represent changes in ice sheet volume and thickness that are due to the dynamic response of the ice sheet to climate forcing (particularly ocean forcing) not a response to variability in* ***Ts*** *and* ***a****. Therefore, the appropriate timescale for smoothing of* ***H*** *is the timescale of dynamic response of the ice sheet. (e.g. See Supplementary Note 3 in Piccione et al. (2022, Nature Communications).)*

I have some line-by-line comments below.

Line 13: Consider adding a ka range, as not all readers will know when glacial termination III was.

We've made this revision.

Line 17-18: I might remove the phrase "that diminished subglacial meltwater flow" from this sentence. Restricted influx of oxygen is directly indicated by the data. Diminished subglacial meltwater flow is one possible explanation.

We've made this suggested revision.

Line 19: I would say "glacial-interglacial cycles", the degree to which these cycles are orbitally caused or orbitally regulated is a matter beyond the scope of the paper.

We've made the suggested edit.

Line 25: Maybe state the positive feedback more explicitly here.

We have decided to back off on the idea of feedback, as it is beyond the capability of this dataset to describe.

Line 27: Please don't use SO for Southern Ocean. This is also sulfur oxidation (i.e. SO-CD) and unnecessarily adds confusion and uncertainty, particular for casual readers. Best practice is to only use abbreviations that are highly standard within the scientific literature.

We've removed this abbreviation.

Line 41: You define an abbreviation "MDV" that you never subsequently use.

We've removed this abbreviation.

Line 46: Remove the word "fresh"

We've removed the word fresh.

Line 47: Maybe replace "lone" with "primary". There could be chemical sources of oxygen.

We've made this correction.

Line 65: Remove "unique".

We've made this edit.

Line 72: Presumably you could have quite a lot of Fe precipitation without reaching Blood Falls levels of Fe concentration.

We've avoid drawing parallels between precipitate parent waters and blood falls brine.

Line 162-168: While I appreciate that the authors cannot precisely locate the formation location of their sample, some degree of local parameterisation would be appropriate here. They could certainly average the geothermal heat flux over the catchment of Elephant Moraine (though it actually seems like Elephant Moraine is fairly close to the continental average). For the other variables, the authors actually used the EPICA record, which is an interior East Antarctic record, appropriately close to their sampling location. The authors should rewrite this paragraph to be less apologetic and say more of what they actually did (i.e. model the change in accumulation and slope based on the EPICA core). The implicit assumption is that EPICA represents interior Victoria Land to first approximation (which is reasonable enough given the options available), not that the Elephant Moraine catchment can be captured by modelling Antarctica as a whole (which is a careless approach, at best).

We feel that the supplementary text adequately describes the modelling approach. And would like to opt to keep the details of the parameterization of the model in the supplement. So as to not add too much text to the main manuscript that would distract from the main message regarding geochemistry.

Line 172: Why yearly? In general, there must be homeostasis over a sufficiently long timescale, but either seasonal or centennial changes do perturb this. I would just remove the statement between the two commas.

We have made the suggested edit.

Line 175: It sounds like you could test the sensitivity of your model to this assumption. Presumably there's a whole range of below saturation conditions possible prior to reaching saturation. I don't know why you need to pick Subglacial Lake Whillans, specifically.

> *We added that Whillans waters were selected because they contain dissolved oxygen and low redox sensitive elements. We view Whillans as a more representative estimate of subglacial meltwater. As we assume that the majority of water flushing from active regions in the ice sheet periphery would be closer to a fresh glacier meltwater endmember.*

Line 180: I find this sentence confusing. What is the force of “plus” grammatically? Perhaps it will make more sense below.

> *We’ve changed this sentence for clarity.*

Line 214: I might say CO_2 production, rather than delivery. CO_2 in air bubbles is slight and we expect CO_2 to mostly source from the oxidation of organic matter (as the -20 ‰ $\delta^{13}C$ amply demonstrates).

> *We’ve added context regarding the carbon source and included production and delivery.*

Line 217: I think you mean figure 1D.

> *We’ve made the correction to refer here to Fig. 2c.*

Line 256: I am not convinced by the mixing curve presented in figure 3A. Assuming there is two component mixing, why does the other endmember have to be all the way at zero $\delta^{13}C$? Why can’t you have a (-17,-50) end member? It seems odd to project out to hypothetical end member for which you have no observations. Also, a rise in $\delta^{18}O$ doesn’t necessarily mean a more peripheral source for the ice. It could mean a source formed during a warmer period. Or it could indicate ice that has been enriched through processes such as regelation.

> *We decided that the mixing models were not necessary to include, and may cause some confusion to the central narrative of the paper, so we’ve excluded them from the revised manuscript.*

Line 355: I might avoid the direct comparison to Blood Falls here. Yes, you have evidence of sufficiently high Fe concentrations to precipitate siderite, but Blood Falls has several other pertinent features, like mirabilite saturation and chloride concentrations 4x that of seawater, which you have no evidence for here.

> *We’ve removed instances drawing parallels between precipitate parent waters and blood falls brine.*

Line 358: Without evidence of saturation for either sulphate or chloride minerals precipitating, it is doubtful whether this could truly be a brine.

> *We’ve removed instances drawing parallels between precipitate parent waters and blood falls brine.*

Line 418: If the thermodynamic modelling is done correctly (per my comments above), renewed surface cooling cannot suppress meltwater production, only changes to strain heating are likely to cause these sort of changes.

> *We have revised this sentence.*

Line 425: Remove n.d.

> *We’ve made this change.*

Line 440: Maybe replace “orbital-scale” with “global”

> *We made this change.*

Line 495: Again, I don’t particularly like the use of “orbital” to describe the timescale of glacial interglacial cycles.

> *We’ve made this change.*

Lines 495-499: I would explicitly state the potential for a positive feedback here. I.e. that less hydrological active ice sheet could release reduced Fe, which in turn fertilises the biological carbon pump in the Southern Ocean, resulting in even colder temperatures.

> *We have decided to back off on the idea of feedback, as it is beyond the capability of this dataset to describe.*

Line 507: I would, again, pull back the comparison to Blood Falls.

> *We’ve removed instances drawing parallels between precipitate parent waters and blood falls brine.*