

Pathways and timescales of Southern Ocean hydrothermal iron and manganese transport

Corresponding Author: Dr Antony Birchill

Due to technical limitations, attachments originally included by the reviewers as part of their assessment appear at the end of this file.

Version 0:

Decision Letter:

**** Please ensure you delete the link to your author home page in this e-mail if you wish to forward it to your coauthors ****

Dear Dr Birchill,

Your manuscript titled "Pathways and timescales of Southern Ocean hydrothermal iron and manganese transport" has now been seen by 3 reviewers, and we include their comments at the end of this message. They find your work of interest, but some important points are raised. We are interested in the possibility of publishing your study in Communications Earth & Environment, but would like to consider your responses to these concerns and assess a revised manuscript before we make a final decision on publication. More specifically, your revised manuscript should 1) better discuss your findings in context of the previous literature, 2) revise figures for clarity, and 3) proofread the manuscript for unnecessary words/phrases and tense usage.

We therefore invite you to revise and resubmit your manuscript, along with a point-by-point response that takes into account the points raised. Please highlight all changes in the manuscript text file.

We are committed to providing a fair and constructive peer-review process. Please don't hesitate to contact us if you wish to discuss the revision in more detail.

Please use the following link to submit your revised manuscript, point-by-point response to the referees' comments (which should be in a separate document to any cover letter), a tracked-changes version of the manuscript (as a PDF file) and the completed checklist:

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We hope to receive your revised paper within six weeks; please let us know if you aren't able to submit it within this time so that we can discuss how best to proceed. If we don't hear from you, and the revision process takes significantly longer, we may close your file. In this event, we will still be happy to reconsider your paper at a later date, as long as nothing similar has been accepted for publication at Communications Earth & Environment or published elsewhere in the meantime.

Please do not hesitate to contact us if you have any questions or would like to discuss these revisions further. We look forward to seeing the revised manuscript and thank you for the opportunity to review your work.

Best regards,

Annie Bourbonnais, PhD
Editorial Board Member
Communications Earth & Environment
orcid.org/0000-0001-7247-5230

Alienor Lavergne, PhD
Associate Editor

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Furthermore, please align your manuscript with our format requirements, which are summarized on the following checklist: [Communications Earth & Environment formatting checklist](https://www.nature.com/documents/commsj-phys-style-formatting-checklist-article.pdf)

and also in our style and formatting guide [Communications Earth & Environment formatting guide](https://www.nature.com/documents/commsj-phys-style-formatting-guide-accept.pdf).

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- If applicable, a statement regarding data available with restrictions
- If a dataset has a Digital Object Identifier (DOI) as its unique identifier, we strongly encourage including this in the Reference list and citing the dataset in the Data Availability Statement.

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If a community resource is unavailable, data can be submitted to generalist repositories such as [figshare](https://figshare.com/) or [Dryad Digital Repository](http://datadryad.org/). Please provide a unique identifier for the data (for example a DOI or a permanent URL) in the data availability statement, if possible. If the repository does not provide identifiers, we encourage authors to supply the search terms that will return the data. For data that have been obtained from publically available sources, please provide a URL and the specific data product name in the data availability statement. Data with a DOI should be further cited in the methods reference section.

Please refer to our data policies at <http://www.nature.com/authors/policies/availability.html>.

REVIEWER COMMENTS:

Reviewer #1 (Remarks to the Author):

The manuscript "Pathways and timescales of Southern Ocean hydrothermal iron and manganese transport" by Birchill et al describes observations of Iron and Manganese in the South Pacific ocean and connect these observations to hydrothermal sources and the Southern Ocean surface through ³He and particle tracking, respectively. As such, this analysis provides observational support to previous model-derived hypothesis that hydrothermal iron supply is an important source of micronutrients to the Southern Ocean (Tagliabue et al., 2010). I have listed many technical comments/suggestions including places where I thought unnecessary words/phrases could be removed but think the manuscript overall is publishable with minor revision.

Technical comments.

34 – This statement needs work to become consistent with the literature cited. There is need to add "anthropogenic" before "CO₂" and define the denominator in the "10%" estimate where the numerator appears to be the Southern Ocean sinking particulate organic carbon flux but the denominator could be several things (global sinking flux" anthropogenic CO₂ uptake in Southern Ocean?). Whether the SO is a source or sink is debatable. The Landschutzer paper discusses the anthropogenic sink as an anomaly from preindustrial which is strong outgassing.

58 – remove “to”
 63 – “is” should be “has”
 101, 124, 126, 127 – unknown symbol before “1.0”, “0.2”, “28.1” and “880”, respectively (also more below)
 Figure 1 – It is not clear what “OOI”, “TN”, and “TS” signify other than the latitudes provided in Figure 1C – Assuming Ocean Observatories Initiative, is OOI the white dot, TN the orange “South East Pacific Rise” triangle, and TS the blue “Pacific Antarctic Ridge” triangle in Figure 1A
 128 – “influence of this” – do the authors intend “wind driven upwelling”
 137 – “It can be noted that” is not necessary
 141 – I think “Here this vertical distribution” should be “This offset”
 144 – I am not sure how “dissolved-particle trace metal exchange” differs from the reversible scavenging” listed above.
 146 – “For completeness,” is not necessary
 151 – I think “causes a clear deviation” should be “clearly deviates” and “requiring” should be “indicating”
 158 – The assertion, “The bathymetry of the southeast Pacific Ocean is dominated by mid-ocean ridge systems” doesn’t seem necessary. I am not even sure how bathymetry can be “dominated” in this way given that the most common bathymetry in this area is abyssal plain.
 163 – remove “deep”
 171, 172 – suggest replacing “Consequently,” with “Because” and “and thus” with a comma.

 177-180 – “tracked” used twice in this sentence. Remove, and tracked”
 187 – As the first usage, “Nucleus for European Modelling of the Ocean” should be spelled out and the version of NEMO cited – I noted that the citation in the methods is only “Madec, G. NEMO ocean engine. (2014).” The authors must provide a full citation.
 188 -189 – The sentence “Trajectories estimate the flow of water within the model ocean and do not represent concentrations of trace metals” is not necessary.
 208 – “. Our simulations suggest” should be “with”
 230,231 – remove “a signal that is” and “the xs3He signal”
 232 – remove “along the PAR,”
 238 – remove “exits to”
 278 – “Mesoscale processes (50-500 km)” should be “Mesoscale (10-100 km) processes”
 287 – add comma after “signal”
 324 – remove “this is not a unique occurrence and”

Reviewer #2 (Remarks to the Author):

Review of COMMSENV-23-1238-T “Pathways and timescales of Southern Ocean hydrothermal iron and manganese transport”:

This paper reports new trace metal observations in an under-sampled and potentially important region of the southeast Pacific and uses robust model trajectories to interpret the origin of the deep hydrothermal maxima in those quantities. These observations and results represent a significant step forward in our understanding of the potential supply of those critical micronutrients to the Southern Ocean (SO) surface domain and their possibly key role in regulating SO primary production. That being said, while clearly a high-quality contribution and worth of publication in more specialized journals, I am undecided as to whether this is newsworthy enough to warrant publication in a NATURE journal. Part of my reluctance comes from the desire to see a more thorough treatment of such an important subject than is permitted by the rather stringent NATURE format guidelines. I will leave that decision to the editor.

I found no major areas of concern in my review, so the following comments are more cosmetic (with some minor exceptions) than fundamental.

ABSTRACT

There is no mention of the specific phases (total dissolved, particulate, etc.) of the various trace metals

There is no mention of timescales (either specific or ranges)

INTRODUCTION

P3 L35: micronutrients don’t “curb” primary production, rather they do the opposite. I know that their paucity does limit productivity, but this wording may confuse the more general reader.

P3 L51: “continuous Fe input in contrast to short term variations” might be better

RESULTS AND DISCUSSION

Figure 1A is ridiculously small! I realize that there are strict limits to NATURE’s figure count, but this figure is rendered useless due to its miniscule size. Moreover, it is rather busy, so it warrants more size. The “white cross” can’t be made out, for instance.

Figure 1B and 1D: The validity of the contours and colors depends on the data density/locations. Could they make the dots designating sample/data locations a little larger to help interpret that?

P8 L127 (and many following): the symbol preceding “28.1” is squirrely. I suspect from context it is the “tilde” for approximately, but there is obviously an ascii-encoding issue here.

P8 L131: might be better to say “tracer” instead of “chemical fingerprint” (better representing the fact that 3He is inert: “chemical” implies rather more).

P8 L132: this is the first mention of xs3He. I suspect an outsider would appreciate a more complete and gentler introduction to the term than the one-liner in the Methods section, including how it was calculated including an equation (perhaps in Methods?)

P8: L136-139: the rather general assertion about the vertical collocation of the 3He and should be better justified (perhaps

with reference to SM). Did they map it onto neutral density and compare? What about regional patterns in xs^3He (are there sharp gradients)?

P9 L146-152: I appreciate the desire for “completeness” but I think the question of dFe vs AOU was largely settled in the literature some time ago 1, so why waste valuable real estate?

P11 L166: I don't believe that there is a lot of xs^3He in “intermediate” waters. I tend to interpret intermediate to be ~1000m and shallower.

MORE GENERALLY: I was disappointed to see that the authors ignored (or were ignorant of) a seminal paper of relevance to this work 2. Those authors were the first to use ^3He to point out this area as a key choke-point for Pacific deep-water incursion into the SO.

P11 L174: perhaps “series of MODEL release sights” might be better

P11 L185: It would be helpful to have a reference to some papers (e.g. 3) as to this choice, especially for the general readership

P15 L213 & 220: the timescale ranges cited seem misleadingly precise, especially in light of all the other uncertainties.

Perhaps, e.g., 10 – 12 years, or even “a decade”

P16 L226: us “Figure S4” actually Figure 4 in the manuscript?

P17-8 L266-272: The dFe: ^3He source ratio is a difficult problem, in particular because whilst ^3He is a primary volatile and portioned virtually quantitatively from the source magma, Fe is “mined” from the basalt via weathering processes, and hence the Fe: ^3He may depend on vent structure, water-rock ratio, etc. That having been said, I am disappointed that the authors did not at least try to estimate the dFe: ^3He ratio in their profile (using, of course the historical ^3He data). Clearly such an estimate is rife with uncertainty, it may be instructive to compare such an estimate with other ‘source ratios’, however uncertain.

References:

1. Boyle E, Jenkins WJ. Hydrothermal iron in the deep western South Pacific. *Geochimica et Cosmochimica Acta* 2008, 72(12S): A107.

2. Well R, Roether W, Stevens DP. An additional deep-water mass in Drake Passage as revealed by ^3He data. *Deep-Sea Research I* 2003, 50(9): 1079-1098.

3. Speer KG, Rona PA. A Model of an Atlantic and Pacific Hydrothermal Plume. *J Geophys Res-Oceans* 1989, 94(C5): 6213-6220.

Reviewer #3 (Remarks to the Author):

Please see attached PDF review.

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Author Rebuttal letter:

Nat Comms Earth & Environment Reviews

Your manuscript titled "Pathways and timescales of Southern Ocean hydrothermal iron and manganese transport" has now been seen by 3 reviewers, and we include their comments at the end of this message. They find your work of interest, but some important points are raised. We are interested in the possibility of publishing your study in Communications Earth & Environment, but would like to consider your responses to these concerns and assess a revised manuscript before we make a final decision on publication. More specifically, your revised manuscript should 1) better discuss your findings in context of the previous literature, 2) revise figures for clarity, and 3) proofread the manuscript for unnecessary words/phrases and tense usage.

We therefore invite you to revise and resubmit your manuscript, along with a point-by-point response that takes into account the points raised. Please highlight all changes in the manuscript text file.

We thank the editor and reviewers for their positive comments and support of the manuscript. We have addressed all the points below.

REVIEWER COMMENTS:

Reviewer #1 (Remarks to the Author):

The manuscript "Pathways and timescales of Southern Ocean hydrothermal iron and manganese transport" by Birchill et al describes observations of Iron and Manganese in the South Pacific ocean and connect these observations to hydrothermal sources and the Southern Ocean surface through ^3He and particle tracking, respectively. As such, this analysis provides observational support to previous model-derived hypothesis that hydrothermal iron supply is an important source of micronutrients to the Southern Ocean (Tagliabue et al., 2010). I have listed many technical comments/suggestions including places where I thought unnecessary words/phrases could be removed but think the manuscript overall is publishable with minor revision.

We thank the reviewer for their positive comments and their support for the manuscript. We value the technical comments they have provided which have improved the manuscript.

Technical comments.

34 " This statement needs work to become consistent with the literature cited. There is need to add "anthropogenic" before " CO_2 " and define the denominator in the "10%" estimate where the numerator appears to be the Southern Ocean sinking particulate organic carbon flux but the denominator could be several things (global sinking flux " anthropogenic CO_2 uptake in Southern Ocean?). Whether the SO is a source or sink is debatable. The Landschutzer paper discusses the anthropogenic sink as an anomaly from preindustrial which is strong outgassing.

We thank the reviewer for highlighting these points. We have reframed the sentence away from anthropogenic CO_2 . It now reads as "Despite being the largest high-nutrient low-chlorophyll region^{1,2}, the Southern Ocean is a globally important organic carbon sink, with the interplay of circulation and remineralisation dynamics setting the rate of carbon uptake by the biological carbon pump³ and 10% of global carbon export occurring in the Southern Ocean⁴."

58 " remove "to"
Done.

63 " "is" should be "has"
Done.

101, 124, 126, 127 " unknown symbol before "1.0", "0.2", "28.1" and "880", respectively (also more below)

The symbol was supposed to be a "~". We have replaced it with a different version of the symbol in the hope that it fixes the formatting issue.

Figure 1 " It is not clear what "OOI", "TN", and "TS" signify other than the latitudes provided in Figure 1C " Assuming Ocean Observatories Initiative, is OOI the white dot, TN the orange "South East Pacific Rise" triangle, and TS the blue "Pacific Antarctic Ridge" triangle in Figure 1A

We have amended Figure 1 and the figure caption incorporating reviewer 2 and 3 comments also in order to improve the clarity.

128 " "influence of this" " do the authors intend "wind driven upwelling"
Yes, we have added this in.

137 " "It can be noted that" is not necessary
Removed.

141 " I think "Here this vertical distribution" should be "This offset"
Amended.

144 " I am not sure how "dissolved-particle trace metal exchange" differs from the reversible scavenging" listed above.
We have changed to reversible scavenging for consistency.

146 " "For completeness," is not necessary

Removed.

151 I think causes a clear deviation should be clearly deviates and requiring should be indicating
Amended.

158 The assertion, The bathymetry of the southeast Pacific Ocean is dominated by mid-ocean ridge systems doesn't seem necessary. I am not even sure how bathymetry can be dominated in this way given that the most common bathymetry in this area is abyssal plain.

We thank the reviewer for raising this point. We agree and have amended is dominated by to has several

163 remove deep
Done.

171, 172 suggest replacing Consequently, with Because and and thus with a comma.
Done.

177-180 tracked used twice in this sentence. Remove, and tracked
We have amended the first use of tracked to initiated.

187 As the first usage, Nucleus for European Modelling of the Ocean should be spelled out and the version of NEMO cited I noted that the citation in the methods is only Madec, G. NEMO ocean engine. (2014). The authors must provide a full citation.
We have expanded the acronym and have updated the citation of the technical report.

188 -189 The sentence Trajectories estimate the flow of water within the model ocean and do not represent concentrations of trace metals is not necessary.

We appreciate the reviewers comment here but would like to keep this statement in to be absolutely clear about what we are (and are not) tracking. Our experience of presenting trajectories analysis in the context of biogeochemistry is that those less familiar with the modelling approach can misinterpret the analysis. As this is an interdisciplinary paper we think it is important to keep this statement in.

208 . Our simulations suggest should be with
We have amended the sentences to read with the SEPR region identified as

230,231 remove a signal that is and the ^{3}He signal
Done.

232 remove along the PAR,
We have left this in as the sentence is highlighting that the flow travels along the PAR, then the SEPR and towards OOI. We appreciate that it seems repetitive as the sentences starts The PAR trajectory simulations but we are trying to explicitly describe the pathways later on in the sentence which extends beyond the initial PAR release site.

238 remove exists to
We have removed exists to.

278 Mesoscale processes (50-500 km) should be Mesoscale (10-100 km) processes
Done.

287 add comma after signal
Done.

324 remove this is not a unique occurrence and
Done.

Reviewer #2 (Remarks to the Author):

Review of COMSENV-23-1238-T Pathways and timescales of Southern Ocean hydrothermal iron and manganese transport":

This paper reports new trace metal observations in an under-sampled and potentially important region of the southeast Pacific and uses robust model trajectories to interpret the origin of the deep hydrothermal maxima in those quantities. These observations and results represent a significant step forward in our understanding of the potential supply of those critical micronutrients to the Southern

Ocean (SO) surface domain and their possibly key role in regulating SO primary production. That being said, while clearly a high-quality contribution and worth of publication in more specialized journals, I am undecided as to whether this is newsworthy enough to warrant publication in a NATURE journal. Part of my reluctance comes from the desire to see a more thorough treatment of such an important subject than is permitted by the rather stringent NATURE format guidelines. I will leave that decision to the editor.

I found no major areas of concern in my review, so the following comments are more cosmetic (with some minor exceptions) than fundamental.

We thank the reviewer for their comments and their overall support for the research and the approach we took. We understand the desire for a more thorough treatment of this important subject, but we would argue that our unique observational dataset, combined with our trajectory modelling results, represent a significant advance in the ongoing debate on the role of hydrothermal trace metal supply to the Southern Ocean. We have elaborated on the areas where the reviewer has requested further information and would be happy to do this for any further specific requests from the reviewer. We believe that our research is worthy of publication in a Nature journal and are confident that it satisfies the requirements for publication and that it will stimulate subsequent research into the subject.

ABSTRACT

There is no mention of the specific phases (total dissolved, particulate, etc.) of the various trace metals
We have changed “Here we present the first full depth trace metal profiles” to “Here we present the first full depth profiles of dissolved and dissolvable trace metals”.

There is no mention of timescales (either specific or ranges)

The last sentence of the abstract mentions that the trace metal signal could reach the mixed layer within two decades. We have copied the sentence in which this is mentioned below for ease of reference.
“Additionally, the trajectories demonstrate key pathways for these SO hydrothermal ridge-derived trace metals to reach the SO surface mixed layer within two decades, providing evidence of a regular supply of micronutrients to fuel SO primary production.”

INTRODUCTION

P3 L35: micronutrients don’t curb primary production, rather they do the opposite. I know that their paucity does limit productivity, but this wording may confuse the more general reader.

We thank the reviewer for raising this and have amended the sentence to read “Iron (Fe) is an essential but often scarce micronutrient that is known to limit primary production and the efficiency of the biological carbon pump in the SO”.

P3 L51: “continuous Fe input in contrast to short term variations” might be better
Amended.

RESULTS AND DISCUSSION

Figure 1A is ridiculously small!! I realize that there are strict limits to NATURE’s figure count, but this figure is rendered useless due to its miniscule size. Moreover, it is rather busy, so it warrants more size. The “white cross” can’t be made out, for instance. Figure 1B and 1D: The validity of the contours and colors depends on the data density/locations. Could they make the dots designating sample/data locations a little larger to help interpret that?

We agree with the points raised by the reviewer and have amended Figure 1 and the figure caption, incorporating Reviewer’s 1 and 3 comments also.

P8 L127 (and many following): the symbol preceding “28.1” is squirrely. I suspect from context it is the tilde for approximately, but there is obviously an ascii-encoding issue here.
The reviewer is correct in that the symbol was meant to be tilde, we have amended the symbol to ~ in the hope that it has rectified the formatting issues.

P8 L131: might be better to say “tracer” instead of “chemical fingerprint” (better representing the fact that ^3He is inert: “chemical” implies rather more).
Amended.

P8 L132: this is the first mention of ^3He . I suspect an outsider would appreciate a more complete and gentler introduction to the term than the one-liner in the Methods section, including how it was calculated including an equation (perhaps in Methods?)

We have added the calculation to the methods section with a few sentences in the methods section to provide the general audience with more information. In the main text we have now begun the paragraph “Mantle helium (^4He), which is enriched in primordial ^3He , is also released into the ocean at mid ocean ridges. A ratio of $^3\text{He}/^4\text{He}$ in excess of atmospheric values (hereafter ^3He) in deep ocean waters is a widely used tracer of hydrothermal inputs”.

P8: L136-139: the rather general assertion about the vertical collocation of the ^3He and should be better justified (perhaps with reference to SM). Did they map it onto neutral density and compare? What about regional patterns in ^3He (are there sharp gradients)?
We have replotted Figure 2 to include ^3He on the dFe and TdFe density plot (Fig. 2D). This plot shows

the co-location of peak ^3He and peak trace metal concentration at the same neutral density. We have now referred to this in the text as Helium isotope observations, previously sampled near station OOI, revealed the presence of ^3He at the depths and neutral density of our observations of trace metal enrichment.

There are gradients in ^3He in this region that are displayed in the supporting information and described in section 2.2.

P9 L146-152: I appreciate the desire for completeness but I think the question of dFe vs AOU was largely settled in the literature some time ago¹, so why waste valuable real estate?

We have included this figure in the supporting information to minimize the amount of space used and have updated the text to refer to Fitzsimmons et al paper highlighted by the reviewer. We think including a short paragraph on dFe vs AOU is a useful addition to the manuscript as it supports our interpretation that the trace metal signal we observed requires a source of dFe in addition to remineralization of biogenic material using datasets that are not separated in time. Without this we rely on dFe and Helium observations separated by 26 years (see reviewer 3 comments).

P11 L166: I don't believe that there is a lot of ^3He in intermediate waters. I tend to interpret intermediate to be ~1000m and shallower.

We agree that there is a need to be clear on our definition of shallow, intermediate and deep as these can vary through the literature and be somewhat subjective. We have defined this in the introduction to be consistent with other recent hydrothermal iron papers (Ardyna et al. 2019; Schine et al. 2020; Bonnet et al. 2023). Shallow being <500m and deep being >2000m. We have amended our references to shallow, intermediate and deep throughout the text to be consistent.

Schine, C. et al. Massive Bloom Fed by Elevated Iron of Possible Hydrothermal Origin in the Pacific Sector of the Southern Ocean. 2020 Ocean Sci. Meet. CT23A-03 (2020) doi:10.1038/s41467-021-21339-5.

Ardyna, M. et al. Hydrothermal vents trigger massive phytoplankton blooms in the Southern Ocean. Nat. Commun. 10, 1 (2019).

Bonnet, S. et al. Natural iron fertilization by shallow hydrothermal sources fuels diazotroph blooms in the ocean. Science (80-.). 380, 812-817 (2023).

MORE GENERALLY: I was disappointed to see that the authors ignored (or were ignorant of) a seminal paper of relevance to this work². Those authors were the first to use ^3He to point out this area as a key choke-point for Pacific deep-water incursion into the SO.

Thank you for highlighting this omission from our reference list, we have included the reference and refer to the Southeast Pacific Deep Slope Water in the text.

P11 L174: perhaps a series of MODEL release sights might be better
Amended.

P11 L185: It would be helpful to have a reference to some papers (e.g. 3) as to this choice, especially for the general readership

We thank the reviewer for the suggestion and agree that references should have been included here. We have slightly amended the sentence for clarity and have added three references. The sentence now reads The mid-ocean ridge system surrounding our study region was divided into 5 regions (Fig. S4, Table S4), including an additional region PAR North (PARN) which did not have a forward-tracking release site. We quantified the backwards trajectories that passed through the likely plume depths (i.e. extending up to 500 m of water column above a ridge 59°61') in these regions (Table 1 and S2).

P15 L213 & 220: the timescale ranges cited seem misleadingly precise, especially in light of all the other uncertainties. Perhaps, e.g., 10 ± 12 years, or even a decade

We agree with the reviewer that the level of precision presented in the main text is not appropriate. We have rounded up to 10-12 years and have amended the other quoted timescale results accordingly.

P16 L226: us Figure S4 actually Figure 4 in the manuscript?

No, this refers to a specific model validation against the ECCO product that is presented in the supplementary material (now Figure S5).

P17-8 L266-272: The dFe: ^3He source ratio is a difficult problem, in particular because whilst ^3He is a primary volatile and partitioned virtually quantitatively from the source magma, Fe is mined from the basalt via weathering processes, and hence the Fe: ^3He may depend on vent structure, water-rock ratio, etc. That having been said, I am disappointed that the authors did not at least try to estimate the dFe: ^3He ratio in their profile (using, of course the historical ^3He data). Clearly such an estimate is rife with uncertainty, it may be instructive to compare such an estimate with other source ratios, however uncertain.

We agree that estimating a Fe: ^3He ratio and comparing it with source ratios in this instance would have

significant uncertainties as outlined by the reviewer. A particularly challenging issue is that the Fe:3He ratio at our location would also be the result of processes occurring during long range transport, including non-conservative dFe removal e.g. 1,2, and currently the information available on the removal rate of iron from hydrothermal neutrally buoyant plumes is limited^{1,3}. Moreover, we are not aware of any source data from the Southern Ocean ridge systems that we identify as likely source regions for the signal we observed⁴. As regional variability in Fe:3He source ratios has been reported e.g. 4,5, we would need this information to make a meaningful comparison.

1

Kipp, L. E., Sanial, V., Henderson, P. B., Van Beek, P., Reyss, J.-L., Hammond, D. E. (2018). Radium isotopes as tracers of hydrothermal inputs and neutrally buoyant plume dynamics in the deep ocean. *Mar. Chem.* 201, 51–65. doi: 10.1016/j.marchem.2017.06.011

2

Tagliabue, A., Bowie A., DeVries, T., Ellwood, M., Landing, W., Milne A., Ohnemus, A., Twining, B., Boyd, P. (2019). The interplay between regeneration and scavenging fluxes drives ocean iron cycling. *Nature Communications*. 10, 4960. <https://doi.org/10.1038/s41467-019-12775-5>.

3

Gartman, A. and Findlay, A. (2020). Impacts of hydrothermal plume processes on oceanic metal cycles and transport. *Nature Communications*. 12, 396-402. <https://doi.org/10.1038/s41561-020-0579-0>

4

Thomas M. Holmes, Zanna Chase, Pier van der Merwe, Ashley T. Townsend and Andrew R. Bowie. (2017). Detection, dispersal and biogeochemical contribution of hydrothermal iron in the ocean. *Marine and Freshwater Research* 68, 2184-2204.

5

Saito, M. A., Noble, A. E., Tagliabue, A., Goepfert, T. J., Lamborg, C. H., and Jenkins, W. J. (2013). Slow-spreading submarine ridges in the South Atlantic as a significant oceanic iron source. *Nature Geoscience*. 6, 775–779.

References:

1. Boyle E, Jenkins WJ. Hydrothermal iron in the deep western South Pacific. *Geochimica et Cosmochimica Acta* 2008, 72(12S): A107.
2. Well R, Roether W, Stevens DP. An additional deep-water mass in Drake Passage as revealed by 3He data. *Deep-Sea Research I* 2003, 50(9): 1079-1098.
3. Speer KG, Rona PA. A Model of an Atlantic and Pacific Hydrothermal Plume. *J Geophys Res-Oceans* 1989, 94(C5): 6213-6220.

Reviewer #3 (Remarks to the Author):

Pathways and timescales of Southern Ocean hydrothermal iron and manganese transport
Review for *Nature Communications Earth and Environment*

The main objectives of this paper were to:

Objective 1) Show enrichment of dissolved iron (dFe) and dissolved manganese (dMn) at depths of 2000-4000 m in the remote, hardly studied eastern Pacific section of the Southern Ocean (SO);

Objective 2) Connect these higher dFe and dMn concentrations at depth with hydrothermal sources, and more specifically to pinpoint the ocean ridge from which these elevated dissolved trace metal (dTM) concentrations are sourced;

Objective 3) Demonstrate pathways that the elevated dTM waters could follow to reach the SO surface within previously proposed residence times of hydrothermally sourced dFe and dMn.

As the authors point out, the Southern Ocean is a dTM “data desert,” and this paper provides key additional dFe and dMn for a data-scarce region. Further, the authors address a major debate in trace metal oceanographical research—whether deep SO waters are enriched by hydrothermal TMs and whether these TM enrichments can possibly reach the surface SO. Their transect also shows higher concentrations of dFe and dMn at depths between 2000-4000m compared to other water depths. Due to scarce additional data for context, they correlate these enrichments with 3He data from a nearby location that also shows excess He-3 at depths between 2000-4000m. While the 3He calculations are 26 years older than the dTM data, this does lead one to believe that these waters have passed through hydrothermal plumes.

Preferably, there would be a concurrence of more data (outside of the AOU correlation) that further supports this point, although I am not aware of any data in this region that would strengthen this argument. However, a detailed identification of which water masses were enriched in trace metals and comparison to consensus background levels of dFe and dMn in

these water masses from other studies would strengthen their argument for objective 1.

We thank the reviewer for their comments and agree that further information regarding the water masses would be useful. To address this we have added in a supplementary material figure (Fig. S1; see below) of a water mass analysis from the CTD data collected along the cruise transect. This analysis was undertaken by a colleague, Sophy Oliver, who was not initially a co-author on the paper and so we request to add them to the author list.

The observed trace metal signal that has been attributed to hydrothermal origin falls within Circumpolar Deep Water (CDW). Fig. S1 shows that CDW spans a broad swath of the water column, whilst the observed trace metal signal is constrained between the 28-28.1 kg m⁻³ neutral density bounds, suggesting this is not a water mass signal. As stated by the reviewer, and to our knowledge, there is no trace metal data deeper than 1000m near our sampling site. From our searching the closest data is from the GEOTRACES GP19 transect, which is located along 170°W. The dissolved Mn data has been published for GP19 and it shows background concentrations for dMn of ~ 0.1-0.2 nM in CDW (much lower than the 0.40 nM we observed). Thus, we have amended the manuscript to read: In the southwest Pacific (170°W), CDW has background dMn concentrations of ~0.1-0.2 nM away from hydrothermal and sediment inputs. The Fe and Mn enrichment we observed was centered on a neutral density surface of $\sigma_t = 28.0-28.1$ kg m⁻³ (Fig. 2B), within CDW (Fig. S1). Comparison with background deep ocean concentrations is typically more informative for dMn than dFe, as away from external sources scavenging depletes dMn to relatively uniform low background concentrations, compared with dFe which can exhibit more variability.

Figure S1. Salinity measured by 7 CTD casts within 54-60°S, 90°W between 13-31st December 2019. Overlain are contours of neutral density [kg m⁻³]. Black solid contours mark the boundaries of Subantarctic Mode Water (SAMW), Antarctic Intermediate Water (AAIW), Circumpolar Deep Water (CDW) and Antarctic Bottom Water (AABW), as determined by Tamsitt et al. (2020)¹ and Naveira-Garabato et al. (2016)². Red dashed contours demonstrate the density bounds of the observed trace metal signal that is attributed to hydrothermal activity.

Further, as the authors point out, there is a lack of observational evidence of key transport pathways in this region of the SO, as most previous modelling efforts have taken place on a coarse resolution scale. The authors of this paper complete extensive particle trajectory modelling (both forward and back trajectory) that shows that the mid-depth waters that have higher dTM concentrations have travelled over spreading ridges containing hydrothermal systems. The authors summarize their findings in Section 2.2 by saying that the waters in question were likely enriched by the South East Pacific Rise (SEPR). However, I am unconvinced that their data rules out possible enrichment by the Pacific Antarctic Ridge (PAR and PARS) systems and would like to see this further discussed in the paper.

We agree with the reviewer and it was not our intention to rule out that some enrichment may have originated from the PAR, which we highlighted in the paragraph below. The reason that we suggest the SEPR as the most likely source is due to a combination of it appearing to be a more direct pathway (i.e. shorter and quicker) and because the depth signal of the observations and the modelling agree very well for the SEPR whereas the modelled PAR and PARS backtracked signals originate from deeper in the water column than the observations.

Therefore, the strong ³He signal to the north in the MAP region (Fig. S3A) should not be interpreted as traversing due southwards towards OOI but instead is most likely traversing eastwards from the SEPR and/or from along the PAR (Fig. 3D). The PARS/N trajectory simulations highlighted a pathway for hydrothermal transport along the PAR, that tracks northwards up to the SEPR and then eastwards towards the OOI site (Fig. S4E, G). Thus, it is possible we observed the amalgamation of trace metal inputs from multiple vent sites along the PAR and SEPR at the OOI site and along the transect, rather than a discrete vent signal.

We have also added a statement about the PAR as a possible source region into the abstract and into the conclusion.

Abstract

Modelled water trajectories revealed the understudied South East Pacific Rise and the Pacific Antarctic Ridge as the most likely source regions.

Conclusion

The observed trace metal enrichment most likely originated from the South East Pacific Rise (south of 30°S). An origin further westward, such as from the Pacific Antarctic Ridge, was also possible.

I also have a question about the particle-tracking modelling. The observed ³He dataset shown in

Figure 3D and also published in the Jenkins database and in Well et al. 2003 clearly shows a ^3He enrichment within Southeast Pacific Deep Slope Water (SPDSW) that abuts the South American continent. However, your particle tracking does not represent that flow, and so it looks like a significant difference between Figure 3 panels C and D. Can you explain this difference and what it means for accuracy of your particle tracking model for SEPR trajectories?

The NEMO model does represent this flow which tends to track close to the continental slope, and due to the eastwards nature of the ACC it does not interact with the OOI site. Below are plots of all trajectories released from two sites in the northern Mornington Abyssal Plain (MAP). These plots include all trajectories released, without any filtering for depth or passage near OOI (note the log scale). Generally, water parcels originating from the northern MAP (where the ^3He enrichment has been observed, Fig. S3) are swept landwards and track along the continental slope near Chile consistent with observations of the Southeast Pacific Deep Slope Water current.

There were no trajectories released from the northern MAP sites that passed near OOI within the observed trace metal signal depth range (see Table S3 for values). These results agree with the conclusions of Well et al. (2003) and Reid (1997), who report that the SPDSW tracks close to the continental slope, evidenced by an increase in d^3He between continental slope and the P19 section at 88°W , the latter of which exhibited relatively low d^3He waters compared to the observations along Drake Passage.

Our transect ran from $54\text{--}60^\circ\text{S}$ along 89.1°W , and the xs^3He data we presented was from 54°S and 88°W . Our Lagrangian trajectory experiments are consistent with previous literature in suggesting that the ^3He is not originating from the SPDSW. We acknowledge that the ODV interpolation in the previous version of Figure 3D made the signal appear further from the continental slope than it actually extends and we have rectified this plotting artefact in an updated version of the figure. The authors also make it clear that comparing this high-resolution study directly with coarse resolution global studies is challenging, but I feel that the comparison of their work with previously speculated SO ventilation times and hydrothermal dFe residence times was a shortcoming of this paper. The authors obviously completed a great deal of work by calculating forward trajectories from the MAP region and sharing time scales and depths from these trajectories. If this data was more fully discussed within the historical context they summarize in the text, it would help the oceanographic community better understand how trajectories can be better used to diagnose source regions and ventilation time scales in the future.

We thank the reviewer for highlighting this point. The nuance around the points we intended to make were lost during the editing of the manuscript. We have expanded the text here to explain that we meant it is hard to isolate the drivers behind different ventilation timescales (see below). We have also reorganised the text in Section 2.3 to improve the flow and reasoning. Upon re-reading the section we realised we referred to results we were yet to present which may have led to some of the points raised by the reviewers. We have rectified this and have worked to improve the clarity of the section.

We intended to highlight that it is challenging to isolate what drivers the differences between the ventilation timescales in different studies. One example that demonstrates the complexities in comparing different hydrothermal Fe modelling studies and approaches was the need to re-run an ocean circulation inverse modelling experiment from the Roshan et al. (2020) study using a higher Fe: ^3He ratio to achieve an almost identical ^3He inventory (Tagliabue et al. 2022) to the PISCES global biochemical model. This was required to undertake a robust model intercomparison with a global biogeochemical model that supplied ^3He based on spreading rates (Tagliabue et al. 2022) rather than ocean circulation inverse modelling based on observations of ^3He distributions. Re-running model simulations is often not feasible or beyond the scope of most studies and hence our assertion that it is challenging to isolate what the key drivers are that lead to the different ventilation timescales. Furthermore, when comparing several different studies identifying the key drivers is further obscured as several factors change in tandem such as different modelling approaches (e.g. Lagrangian simulations using global ocean model velocity components vs biogeochemical data-driven inverse modelling) and resolution (eddy-resolving vs large scale circulation pathways). We decided not to include the points made in this paragraph in Section 2.3 as it disrupted the flow of the text and didn't add much to the narrative of Section 2.3. We amended the original sentence to read:

It remains challenging to isolate what drives the difference between Fe supply from different modelling approaches^{39,40} and ventilation timescales from high-resolution studies (<20 years)^{44,45}, compared to coarse-resolution or global inventory studies (~100 years)^{43,46} discussed above, as several factors change in tandem.

Further, while well-written overall, this manuscript should be proofread for run-on sentences and correct/constant tense usage.

Overall, I find this manuscript to be a very important oceanographic contribution to a hotly debated topic, and so Nature Communications Earth & Environment may be a suitable target. However, it needs significant revision before it can be considered for publication in this journal.

We thank the reviewer for their constructive comments and overall support of the manuscript.

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Figures and Table Feedback

Figure 1- This figure is key to explaining your first objective of the paper and I believe you can make changes to make this argument more convincing.

- In panel A, some of the symbols are difficult to see, especially the white cross that indicates helium sampling and the black line that shows the transect. Can you make this panel larger? In the least, please make all fonts larger. I think it could also benefit from a summary of major currents in the 2000-4000m depth range for this region, including the fronts of the ACC.

- For panel C, Please make the lines connecting markers straight instead of curved. Also, perhaps move this panel to Figure 2? (see Figure 2 comments)

- For panels B and D, Red-Blue color bars are best used for showing positive/negative anomalies from a "magic value" shown in white. I recommend changing the color bar to another color bar that does not lead the reader to think that there is a magical value around 0.6nM (for dFe) or 1.2nM (for TdFe) to which all concentrations should be compared. Corresponding to a comment below, I would also like to see water mass contours on these panels. It would more clearly show if there was TM enrichment in a specific water mass.

We have amended Figure 1 and the figure caption incorporating reviewer 1 and 2 comments.

We have added a figure into the supplementary material indicating the water mass structure along the transect in relation to the neutral density isopycnals that bound the trace metal signal (Fig. S1). The trace metal enrichment falls in the center of the much broader CDW water mass. We haven't added the water mass contours to the figure as we are conscious of crowding it but we do refer to how the enrichment relates to the water mass structure in the manuscript.

• The Fe and Mn enrichment are centered on a neutral density surface of $\sigma_\theta = 28.0-28.1 \text{ kg m}^{-3}$ (Fig. 2D), within Circumpolar Deep Water (CDW) (Fig. S1).

Figure 2- This figure is also part of explaining the first objective of the paper as outlined above.

- Panel A- It should be indicated clearly that the helium data is decoupled in space and time so as not to mislead the reader. This should be done at least by indicating the distance the ^3He and Fe/Mn sites were located. I would also have lines connect the data markers in all the profiles or none of them, as having marker lines in only some of the elements is confusing.

We do quantify the distance in time and space between the ^3He and Fe/Mn data in caption of Figure 1, refer to this again in the caption for Figure 2 and in the main body of the text. Hopefully now we have made Figure 1 clearer this will be more apparent.

We have removed the lines connecting the data markers in Figure 2 as suggested.

- Panel B- I appreciate seeing this panel but I believe that instead you could include a sentence about this in the manuscript and move it to the supplement for reference. Also is there any reason you would expect the dFe elevation to be capped at a neutral density of 27.00 kg/m^3 ? Consider replacing this panel with Panel C from Figure 1.

We have amended Figure 2 to include xs^3He and dFe, TdFe on a plot against neutral density, which addresses the additional comments around co-location of He and Fe enrichment. The reason for the uniformly low dFe in waters with a neutral density $< 27.00 \text{ kg/m}^3$ is biological drawdown in surface waters. Surface waters will be annually homogenized by winter mixing which may give the appearance of a cap, immediately below remineralization increases dFe, before we see the observed hydrothermal signal at depth.

We have also added two plots to show the profiles of dFe and dMn (originally in Figure 1C) to Figure 2.

Table 1- This table addresses objective 2 and shows a summary of the original work done for both forward and back trajectories

- Why isn't PARN in the top section of this table? Even if it is zero, it should be mentioned.

We did not have a release location from within the PARN box for the forward tracking experiments. But when undertaking the backwards tracking analysis, we added the PARN "box" to cover the ridge systems comprehensively. We state this in the methods section of the manuscript: "For PARN which had no specific vent release site for the forward tracking simulation, a vent depth of 2500 m was chosen by

examining the depths of the topography within the PARN area as a histogram to identify the ridge depths.

We have also added a sentence into the main body of the manuscript:

The mid-ocean ridge system surrounding our study region was divided into 5 regions (Fig. S4, Table S4), including an additional region PAR North (PARN) which did not have a forward-tracking release site.

- I am not sure that the raw number of trajectories passing near observed signal in each test case should be listed here, since the number of possible trajectories changed with each simulation (according to Table S1). Thus, you should only include one column currently labeled "Proportion of release (%)" that should be renamed "Proportion of total possible trajectories that passed observed signal (%)".

We have moved the full table to the supplementary material and have removed the raw trajectories number from the main text. We have renamed the "Proportion of release (%)" column "Proportion of trajectories that passed near observed signal (%)" and "Proportion of trajectories that passed near potential source (%)" for the backward tracking simulations.

- The table caption should quantify what threshold marked "passing near" the observed signal, instead of using those qualitative words.

We have added a sentence into the Table caption that reiterates the information presented in the methods: "The proportion of trajectories that passed within a 0.5° area around the OOI site are presented relative to the number of particles that were successfully released during each simulation (Table S2)".

Figure 3- This figure also addresses objective 2, and some small changes can make this figure more convincing

- Panel C- Right now it is difficult to see the green, red, and blue color bars that you have added to this figure

- Panels C and D- Your conclusions rely on the reader to be able to easily see the similarities between panels C and D. I would start by making the shown depth range the same (i.e. zoom into 1900m to 4000m in panel D).

We have amended Figure 3 as suggested, except that rather than zoom in on panel D we have zoomed out on panel C instead. This is because zooming in would have obscured the difference in concentration between the xs3He signal and surrounding waters, which is a key takeaway from the figure. The amended Figure and caption as presented below.

Figure 3. Trajectories of the forward-tracking simulations from the SEPR (A, B, C) and coincident xs3He

anomaly (D). Trajectories were released between the ridge and 500 m above to mimic a neutrally-

buoyant hydrothermal plume. A Trajectory density, which indicates the number of instances of trajectories passing into a grid cell (log scale), of all SEPR trajectories. B Trajectory density map of SEPR

trajectories that pass within a 0.5° area around the OOI station (red marker). C Depth section of

trajectory density for the SEPR trajectories that pass within a 0.5° area of the OOI station (location

indicated by pink line). Additionally, the grey line superimposed on the pink line highlights the depth

range where dFe concentrations were >0.75 nM (Fig. 2). The cyan line marks the trajectory seeding

depth which mimics a neutrally-buoyant plume. Bathymetry along 54°S (to match station OOI) is shown.

Note the colorbar scale is different for C compared to A and B. D An east-west transect of excess Helium

(xs3He) at 54°S as an indicator of hydrothermal influence from the Jenkins et al. (2019) dataset⁵².

Further, is there a better way to display this comparison? Could you show a scatter plot of the SEPR trajectory density compared to the helium data? Perhaps it could then be colored by a third variable of "median first passage time" which is a major part of putting the data in context to address objective 3.

We have plotted a scatter plot of the trajectory density of the SEPR simulation against xsHe concentration as seen below. The "plume" signals overlap rather well meaning that only elevated xsHe concentrations (>0.3 fM) appear on the plot. You can observe that the highest xsHe concentrations from the SPDSW (>0.6 fM; black circle) are not associated with elevated trajectory densities, in line with our conclusions that our observed signal is from a different source. We want to highlight here that the

trajectory density broadly represents the major pathways but because it does not account for diffusion or other biogeochemical processes we would not expect a definitive relationship (for example a linear relationship) with the historical xsHe concentrations. Furthermore, the trajectory density will naturally be higher at the release site due to the nature of trajectory releases (it takes time for them to spread out) and around the OOI site as that is the criterion we are using to filter the trajectories in the figure. Also, the xsHe concentration is relatively uniform throughout the plume (between 0.3 and 0.5 fM) whereas the trajectory density has much greater variability due to the 10,000s of modelled trajectories timesteps that feed into the trajectory density calculation. We therefore feel that the plot does not add much more information than what is gleaned from visual inspection of panel A and B in Figure 3. Based on this we did not continue to add in the median passage time into the plot. We were also unsure about the reviewers comments referring to median first passage time in relation to objective 3, as the median first passage time is indicating time taken to reach the OOI site rather than to give any indication of the downstream impacts of the observed signal. If the reviewer was referring to objective 2 then we would not expect trajectory density and median passage time per grid cell to exhibit a statistical relationship. The passage time will increase from the release site to OOI whereas trajectory density is higher where the pathways are more coherent. The coherence of a pathway is not time dependent and hence we would not expect a relationship.

Figure 4- This figure addresses where future trajectories show that waters potentially enriched in dTMs could end and at what depths these trajectories end. This is a key way to illustrate their thoughts for objective 3.

- Could this figure be expanded (a panel added or something of the like) to focus on the trajectories that end >600m and >1000m? These depths were explicitly called out in the manuscript as the potential maximums for the winter water mixing depth and thus are key to surface ventilation. This might be a good opportunity to use the red/blue color bar, with one of those 'magic depths' outlined in white.

We thank the reviewer for this suggestion, as it has led to a much more interesting presentation of the model results. Please see the updated figure and caption below.

Figure 4. Indication of trajectory locations at the end of the 20 years simulation from the source regions.

A Trajectory depth and location at 20 years and B the depths presented as above (red) and below (blue) the maximum winter MLD in the SO (1000m) and C Trajectory density at 20 years. OOI is shown by the

red marker. Note that only trajectories that passed within the cruise transect (OOI to 60°S) and the observed hydrothermal signal depth range (2000-4000 m) are shown.

Line Comments

Abstract

- Line 27- primordial helium enrichment == 3Hexs, further this moniker should be corrected throughout the paper to coincide with literature

Amended. With the exception of one sentence in the results which we use to give the general readership an introduction to 3He and one sentence in the methods where we introduce the calculations (see reviewer 2 comments).

- Line 31- take caution with the word 'regular,' as you only provide dTM data from one snapshot in time. Instead, replace 'providing evidence of a' with 'potentially

Supporting

Amended.

1.0 Introduction

- Line 37- indicate that Fe and Mn are not the only elements that can be limiting or colimiting in the SO

The sentence has been expanded to read:

'However, other elements can potentially be (co-)limiting alongside Fe in the Southern Ocean (SO), with recent evidence for manganese (Mn) (co-)limitation in particular'.

- Line 39 'spatially coupled'

Amended.

- Line 39- Add a sentence here introducing dust and sediments as the 'typical' main

sources of Fe and Mn to the ocean? The aerosol discussion came unexpectedly in this hydrothermal manuscript and needs a gentler introduction

Amended the text to include the below sentences to introduce the main Fe and Mn sources and to give an introduction of sediment supply, before introducing dust and hydrothermal venting.

The main external sources of Fe and Mn to the ocean are margin sediments, atmospheric dust, and hydrothermal venting^{6,12}. Input from margin sediments enhances primary production downstream of islands in the SO^{13,14}, and sustains ecosystems in Antarctic shelf regions^{12,15}, where melting glacial ice can be an additional source of Fe and Mn^{16,17}.

- Lines 39-40- can vary by >3 orders of magnitude, causing
Amended.

- Line 43- avoid the use of the word major here as this is what is currently under debate
Deleted major.

- Line 57- drive not driving
Amended.

- Line 61- Includes the depth range for shallow vs deep vents
We have added shallow (<500m) and deep (>2000m) as the Bonnet and Schine papers focus on shallow vents <500m whilst the Ardyna paper focuses on a ridge system with the shallowest bathymetry ~2000m.

- Line 62- primary production
Amended.

- Lines 67-69- Include a discussion of proposed mechanisms for physicochemical stabilization of dFe and dMn as this is a large assumption for whether waters enriched in dFe and dMn will be able to reach the surface SO

We have amended the text in the preceding paragraph to read:

Our understanding of the hydrothermal plume processes responsible for the physico-chemical stabilisation of Fe is developing, though not yet comprehensive. Within hydrothermal plumes dFe has been shown to form inorganic nanoparticles^{27,28}, larger inorganic colloids²⁹ and organic complexes^{30,31}. The co-location of carbon with Fe in plume particles suggests that organic carbon may alter the chemical behaviour of Fe oxyhydroxides²⁹ and create localised regions of Fe(II) enrichment³¹.

2.0 Results and Discussion

2.1 Observations of hydrothermal trace metals in the southeast Pacific sector of the Southern Ocean

- Line 96- Define dFe again here as <0.2 µm, as it's the first time it is used to represent your data. It is also in contrast to the unfiltered TdFe that you mention next.
Amended.

- Lines 99-101- Describing this feature is a key takeaway of the paper and so I would go into more detail. What are the ranges of dFe and dMn specifically? Do these enriched waters shoal above 2000m? What is the neutral density they are located on (discussed later)? Do you see this enrichment only within a specific water mass? If so, can you contextualize the background concentration of dTMs in this water mass? Also, can you discuss the secondary dFe maximum that seems to be present at 4000m?

We have adjusted the text to specify the range of dFe concentrations found between 2000-4000 m at our northernmost station.

We have removed and Mn from this sentence as this was the only reference to Mn in this paragraph, with the subsequent paragraph describing the distribution of Mn along the transect, which includes a description of the enrichment and also now includes comparative background values for Mn in circumpolar deep water (see comment above). The water mass figure (Fig. S1) we have now included also includes a dashed red lines denoting the neutral density contours of 28.0 and 28.1 kg m⁻³, the dFe and TdFe transect plots, which have been made larger, also have neutral density contours overlain. So, we hope this now makes the location of this density surface clear. In the text we state that this surface shoals by 880 m over the length of the transect.

The increase below 4000m is found in TdFe (unfiltered) and not dFe (<0.02 µm), which is discussed at the end of the paragraph. We attribute this to sediment resuspension coupled with removal of dFe.

- Line 103- Remove much, later you show TdFe ranges that are orders of magnitude
Higher
Removed.

- Line 107- were observed in benthic nepheloid layers at open ocean stations
Amended.

- Line 121- due to scavenging removal
Added.

- Line 122, 133- basin-scale
Amended.

- Lines 123-125- "Along our transect"; What depths do these maximum concentrations correspond to? Are they the same depths you see for dFe?
We have referred to the below table which has been added to the supporting information to provide this information in a clear format. We have included a more detailed response to the reviewer around the depths of dFe and dMn to a comment below.

Table S1- A summary of maximum dFe and dMn concentrations observed at stations OOI, TN and TS for hydrothermal plume influenced waters. At station TN and TS, the maximum concentrations of dFe and dMn were observed at the same depth. At station OOI, the maximum concentration of dFe was shallower than the maximum concentration of dMn. The southwards shoaling of depth of maximum trace metal concentration is consistent with a shoaling density surface (Fig. S1). Note that the profile maximum dFe concentration (0.94 ± 0.03 nM) for station TS was observed in near bottom waters (4889 m). Maximum ^{3}He concentration and associated depth at 88°W near OOI (see Figure 1).

Station	Max dFe (nM)	Depth (m)	Max dMn (nM)	Depth (m)	Max ^{3}He (fM)	Depth (m)
OOI (-54.4)	0.96 ± 0.00	2506	0.34 ± 0.02	2808	0.49	2291
TN (-57.0)	0.70 ± 0.01	2503	0.39 ± 0.00	2503		
TS (-60.0)	0.62 ± 0.04	1806	0.24 ± 0.01	1806		
	0.60 ± 0.04	2005	0.24 ± 0.01	2005		

- Line 128- comma splice. Split into two sentences or replace with a semicolon.
Split into two sentences.

- Lines 131-145- This paragraph could be reworked to be placed with the later helium

We appreciate the reviewers comment here but we would like to keep the current structure as the Helium data is presented for separate purposes. In Section 2.1 we present the available observational data to determine that the signal is of hydrothermal origin. In Section 2.2 we then delve into identifying the likely source region of the observed signal.

Discussion

- Line 133- replace "constrained" with "predicted"
Amended.

- Lines 137-141- Quantify the offset between dFe and helium more specifically: is it 10m? 100m? 1000m? Do you see similar evidence in the dMn data or is it decoupled from dFe (as has been shown to happen in other marine systems)?

The ^{3}He peak is at 2291m and dFe peak is at 2506m, which equates to an offset of 215m at OOI (Table S1). We have amended the main text to specify the offset at OOI and have included Table S1 to detail the depths of maximum dFe, dMn and ^{3}He concentrations at stations OOI, TN and TS.

The depth for peak concentration for dMn and dFe was the same at station TS and TN, whereas the peak dFe concentration was shallower at OOI (see comment above), though broadly the depth profiles through the plume region are similar in shape with the exception of this one data point. So, broadly our dataset would indicate that dFe and dMn are co-located rather than decoupled, which would contrast with previous findings by Fitzsimmons et al. 2017. A major motivation for their field campaign was to study the transport of elements zonally from the East Pacific Rise at 15°S , therefore they present high resolution data through the neutrally buoyant plume and map the plume evolution over 40° of latitude. This enables them to be able to see the decoupling develop. In contrast, the major motivation for our field campaign was to study upper and intermediate ocean carbon dynamics, thus we have comparatively coarser resolution data through the plume, and over 5° of latitude (when the dominant transport is zonal). Consequently, this restricts our ability to report with confidence whether dFe and dMn are coupled or decoupled, and hence we have steered clear of entering this debate. Fitzsimmons et al. 2017. Iron persistence in a distal hydrothermal plume supported by dissolved particulate exchange. DOI: 10.1038/NGEO2900

- Lines 141-143- I believe more time should be spent to justify the use of a dataset that is decoupled from the TM data by 26 years.

We have added the following text to the paragraph:

“The use of a helium dataset decoupled from our trace metal observations by 26 years, and 83 km, requires consideration. The comparable distribution of dFe, TdFe and xs3He with neutral density (Fig. 2D), provides confidence that we observed a consistent signal in both datasets. However, we cannot account for temporal variability of end member metal and helium concentrations during the 26-year gap between metal and helium sample collection.”

2.2 Identifying the source region of the observed hydrothermal signal

- Line 159- replace “shoaling to” with “with depths of”
Amended.

- Line 161- Mornington Abyssal Plain (MAP; our study region)
Amended.

- Lines 162-163- Can you add these mid-depth flows to Figure 1, panel A?
We have added general flow patterns to Figure 1A in the manuscript.

- Lines 164-166- Well et al. 2003 (Deep-Sea Research I, 50: 1079-1098) should also be cited here. It was surprising that this paper was not cited in this manuscript.
We thank the reviewer for highlighting this paper, we acknowledge that it was an oversight on our part to have not cited it. We have added it in to the paper along with the name of the current as suggested.

- Line 166- replace “latitudinal” with “meridional”
Amended.

- Line 167- “southward”
Amended.

- Line 169- replace “longitudinal” with “zonal”
Amended.

- Line 170- “eastward”
Amended.

- Line 175- add comma after “systems”
Added.

- Lines 209-212- Can you provide context what these percentages (9%, 21%, etc.) mean in an oceanographic context? Are these the results you were expecting?
We have added a sentence into the paragraph to contextualise the trajectory results in an oceanographic context. In terms of results, we were expecting that the source region that had the highest % of trajectories crossing near to OOI would be the dominant source region as it would indicate the least dilution between the source region and OOI.

“In an oceanographic context, these percentages can be thought of as demonstrating the strength and coherency of transport pathways within the wider ocean circulation between the source regions and OOI i.e. the greater the percentage the more dominant the pathway”

- Lines 207-221- I am convinced that the trajectories indicate that the SEPR is a likely source region for these hydrothermal metals based on these trajectories, but I am not sure why more time is not spent discussing the PARS and PARN regions? The back trajectory table and supplemental material seem to indicate that they are potential source regions. This becomes especially important when you return to this idea in your summary paragraph in Lines 234-235.

We have addressed this comment above and have added text into the manuscript to highlight the PARN/PARS contribution further.

- Lines 230-231- “that is traversing southwards towards OOI, but instead the”
Amended.

- Lines 236-247 and Lines 248-255- These two paragraphs are a strong summary of previous work, but I would like to see this information discussed in the context of this

study's trajectory findings. If we did find PAR vents, how would this change your results?

In response to comments above we have hopefully clarified that the PAR may contribute to our observed signal, but that the SEPR was likely to be the dominant signal due to the shorter distance and close alignment between the observed signal depth and the trajectory modelling results. Therefore, finding PAR vents would not change our findings that the SEPR and PAR could contribute to the observed signal.

2.3 Could hydrothermal iron and manganese from the southeast Pacific vents fuel Southern Ocean primary production?

- Line 260- âplume-influencedâ
Amended.

- Lines 261-262- This transit time of 99+/- 18 years is for waters shoaling in the SO from the SEPR. Ensure sure this distinction is clear.

The 99+/- 18 years transit time estimated by Jenkins 2020 is not specific to waters originating from the SEPR source region in this study but rather it uses a global ^3He flux inventory and assumes that 90% exits through the Southern Ocean.

We have expanded the sentence to read âA transit time of 99 ± 18 years for the shoaling of deep waters to the SO surface has been estimated using a global ^3He mass balance model, in which the ^3He predominantly enters the SO via deep South Eastern Pacific watersâ

- Line 273: What kind of âimpactâ? Be more specific on whether you are talking about concentration impacts or primary production impacts.

We have added âimpact on dFe inputsâ into the sentence to make it clearer.

- Lines 272-275- I disagree that it is âchallengingâ to compare your high-resolution study results with the coarse-resolution studies. How do the model output data of published Fe GCM models match your results?

We have addressed this comment above.

- Lines 281-283- Please be explicit in the time scales you are talking about for both ventilation rates and dTM residence times in this section. Right now it is not clear. We have added timescales into the sentence as below and have reworded the text in section 2.3 to improve the flow and clarity:

âAs discussed above, resolving mesoscale features in high-resolution models, as in our study (0.083° horizontal resolution), results in SO ventilation timescale estimates (within 20 years) that allow for hydrothermal Fe and Mn from SO vents to reach SO mixed layer depths within Fe and Mn residence times (5-50 years)â

- Line 287- âforward-trackâ
Amended.

- Line 296- âreach depths shallower thanâ
Amended.

- Lines 287-304- I appreciate how this paragraph places your work into the context of the literature discussed.

We thank the reviewer for their positive comment.

- Lines 301-304- These numbers are poorly constrained in the literature. Can you give recommendations on how your study ADDS knowledge or boundaries to these timescales explicitly?

Our paper is part of a growing body of evidence that indicates that rapid pathways to the Southern Ocean mixed layer exist and that mesoscale processes play an important role in ventilation timescales, with high resolution models leading to shorter ventilation timescales in the Southern Ocean. Our study targets one observed hydrothermal signal and uses modelling as a tool to attempt to attribute a source region and to further understand the possible pathways and timescales of ventilation from our study site. Our study therefore provides evidence that there are pathways to the Southern Ocean winter mixed layer within Fe and Mn residence times. A different study and/or approach would be needed to provide boundaries on ventilation timescales (especially as they are model and resolution dependent) but we feel that our paper may provide stimulus for future targeted research to address such questions. Furthermore, as our trajectory modelling is purely based on physics we cannot aid in refining the chemical residence times as we have not observed the evolution of the plume from the source. As stated above we have rearranged Section 2.3 which will hopefully make it clearer what our modelling adds to the previous literature and how the trajectory modelling can be utilised for this topic.

3.0 Conclusion

- Lines 315-325- Why is there no mention of the SEPR or PARN/PARS specifically? It seems like a major conclusion of your study is discussing potential source regions for hydrothermal enrichment to the MAP area and these are not addressed in the summary. Maybe line 317? Also line 321?

We agree with the reviewer that this was an omission. As mentioned above we have added a sentence into the conclusion and added PAR into the abstract to address these comments.

Abstract

Modelled water trajectories revealed the understudied South East Pacific Rise and the Pacific Antarctic Ridge as likely source regions.

Conclusion

The observed trace metal enrichment most likely originated from the South East Pacific Rise (south of 30°S). An origin further westward, such as from the Pacific Antarctic Ridge, was also possible.

- Line 322: provide the potential for regular input of
Amended.

- I would also argue that your manuscript draws attention to the importance of SEPR vents south of 30°S, which are not those that were so well studied by GEOTRACES GP16 but travel westward. This should be emphasized if your results suggest their importance.

We agree with the reviewer and have specified this in the additional conclusion sentence above and have amended the sentence below.

We identify pathways for rapid (< 20 years) transport of water parcels enriched in hydrothermal Fe and Mn, from vents located within the understudied Pacific sector of the SO, to SO winter mixed layer depths.

Version 1:

Decision Letter:

** Please ensure you delete the link to your author home page in this e-mail if you wish to forward it to your coauthors **

Dear Dr Birchill,

We have now carefully considered your revised manuscript titled "Pathways and timescales of Southern Ocean hydrothermal iron and manganese transport" together with your responses to the reviewers. We are now delighted to say that we are happy, in principle, to publish a suitably revised version in Communications Earth & Environment under the open access CC BY license (Creative Commons Attribution v4.0 International License).

We therefore invite you to edit your manuscript to comply with our format requirements and to maximise the accessibility and therefore the impact of your work.

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Best regards,

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Pathways and timescales of Southern Ocean hydrothermal iron and manganese transport

Review for Nature Communications Earth and Environment

The main objectives of this paper were to:

Objective 1) Show enrichment of dissolved iron (dFe) and dissolved manganese (dMn) at depths of 2000-4000 m in the remote, hardly studied eastern Pacific section of the Southern Ocean (SO);

Objective 2) Connect these higher dFe and dMn concentrations at depth with hydrothermal sources, and more specifically to pinpoint the ocean ridge from which these elevated dissolved trace metal (dTM) concentrations are sourced;

Objective 3) Demonstrate pathways that the elevated dTM waters could follow to reach the SO surface within previously proposed residence times of hydrothermally sourced dFe and dMn.

As the authors point out, the Southern Ocean is a dTM “data desert,” and this paper provides key additional dFe and dMn for a data-scarce region. Further, the authors address a major debate in trace metal oceanographical research—whether deep SO waters are enriched by hydrothermal TMs and whether these TM enrichments can possibly reach the surface SO. Their transect also shows higher concentrations of dFe and dMn at depths between 2000-4000m compared to other water depths. Due to scarce additional data for context, they correlate these enrichments with $^3\delta\text{He}_{\text{xs}}$ data from a nearby location that also shows excess He-3 at depths between 2000-4000m. While the $^3\delta\text{He}_{\text{xs}}$ calculations are 26 years older than the dTM data, this does lead one to believe that these waters have passed through hydrothermal plumes. Preferably, there would be a concurrence of more data (outside of the AOU correlation) that further supports this point, although I am not aware of any data in this region that would strengthen this argument. However, a detailed identification of which water masses were enriched in trace metals and comparison to consensus background levels of dFe and dMn in these water masses from other studies would strengthen their argument for objective 1.

Further, as the authors point out, there is a lack of observational evidence of key transport pathways in this region of the SO, as most previous modelling efforts have taken place on a coarse resolution scale. The authors of this paper complete extensive particle trajectory modelling (both forward and back trajectory) that shows that the mid-depth waters that have higher dTM concentrations have travelled over spreading ridges containing hydrothermal systems. The authors summarize their findings in Section 2.2 by saying that the waters in question were likely enriched by the South East Pacific Rise (SEPR). However, I am unconvinced that their data rules out possible enrichment by the Pacific Antarctic Ridge (PARN and PARS) systems and would like to see this further discussed in the paper.

I also have a question about the particle-tracking modelling. The observed ^3He dataset shown in Figure 3D and also published in the Jenkins database and in Well et al. 2003 clearly shows a ^3He enrichment within Southeast Pacific Deep Slope Water (SPDSW) that abuts the South American continent. However, your particle tracking does not represent that flow, and so it looks like a

significant difference between Figure 3 panels C and D. Can you explain this difference and what it means for accuracy of your particle tracking model for SEPR trajectories?

The authors also make it clear that comparing this high-resolution study directly with coarse-resolution global studies is challenging, but I feel that the comparison of their work with previously speculated SO ventilation times and hydrothermal dFe residence times was a short-coming of this paper. The authors obviously completed a great deal of work by calculating forward trajectories from the MAP region and sharing time scales and depths from these trajectories. If this data was more fully discussed within the historical context they summarize in the text, it would help the oceanographic community better understand how trajectories can be better used to diagnose source regions and ventilation time scales in the future.

Further, while well-written overall, this manuscript should be proofread for run-on sentences and correct/constant tense usage.

Overall, I find this manuscript to be a very important oceanographic contribution to a hotly debated topic, and so Nature Communications Earth & Environment may be a suitable target. However, it needs significant revision before it can be considered for publication in this journal.

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Figures and Table Feedback

Figure 1- This figure is key to explaining your first objective of the paper and I believe you can make changes to make this argument more convincing.

- In panel A, some of the symbols are difficult to see, especially the white cross that indicates helium sampling and the black line that shows the transect. Can you make this panel larger? In the least, please make all fonts larger. I think it could also benefit from a summary of major currents in the 2000-4000m depth range for this region, including the fronts of the ACC.
- For panel C, Please make the lines connecting markers straight instead of curved. Also, perhaps move this panel to Figure 2? (see Figure 2 comments)
- For panels B and D, Red-Blue color bars are best used for showing positive/negative anomalies from a “magic value” shown in white. I recommend changing the color bar to another color bar that does not lead the reader to think that there is a magical value around 0.6nM (for dFe) or 1.2nM (for TdFe) to which all concentrations should be compared. Corresponding to a comment below, I would also like to see water mass contours on these panels. It would more clearly show if there was TM enrichment in a specific water mass.

Figure 2- This figure is also part of explaining the first objective of the paper as outlined above.

- Panel A- It should be indicated clearly that the helium data is decoupled in space and time so as not to mislead the reader. This should be done at least by indicating the distance the ^3He and Fe/Mn sites were located. I would also have lines connect the data

markers in all the profiles or none of them, as having marker lines in only some of the elements is confusing.

- Panel B- I appreciate seeing this panel but I believe that instead you could include a sentence about this in the manuscript and move it to the supplement for reference. Also is there are reason you would expect the dFe elevation to be capped at a neutral density of 27.00 kg/m³? Consider replacing this panel with Panel C from Figure 1.

Table 1- This table addresses objective 2 and shows a summary of the original work done for both forward and back trajectories

- Why isn't PARN in the top section of this table? Even if it is zero, it should be mentioned.
- I am not sure that the raw number of trajectories passing near observed signal in each test case should be listed here, since the number of possible trajectories changed with each simulation (according to Table S1). Thus, you should only include one column currently labeled "Proportion of release (%)" that should be renamed "Proportion of total possible trajectories that passed observed signal (%)."
- The table caption should quantify what threshold marked "passing near" the observed signal, instead of using those qualitative words.

Figure 3- This figure also addresses objective 2, and some small changes can make this figure more convincing

- Panel C- Right now it is difficult to see the green, red, and blue color bars that you have added to this figure
- Panels C and D- Your conclusions rely on the reader to be able to easily see the similarities between panels C and D. I would start by making the shown depth range the same (i.e. zoom into 1900m to 4000m in panel D). Further, is there a better way to display this comparison? Could you show a scatter plot of the SEPR trajectory density compared to the helium data? Perhaps it could then be colored by a third variable of "median first passage time" which is a major part of putting the data in context to address objective 3.

Figure 4- This figure addresses where future trajectories show that waters potentially enriched in dTMs could end and at what depths these trajectories end. This is a key way to illustrate their thoughts for objective 3.

- Could this figure be expanded (a panel added or something of the like) to focus on the trajectories that end >600m and >1000m? These depths were explicitly called out in the manuscript as the potential maximums for the winter water mixing depth and thus are key to surface ventilation. This might be a good opportunity to use the red/blue color bar, with one of those "magic depths" outlined in white.

--

Line Comments

Abstract

- Line 27- primordial helium enrichment == $^3\delta\text{He}_{\text{xs}}$, further this moniker should be corrected throughout the paper to coincide with literature
- Line 31- take caution with the word “regular,” as you only provide dTM data from one snapshot in time. Instead, replace “providing evidence of a” with “potentially supporting”

1.0 Introduction

- Line 37- indicate that Fe and Mn are not the only elements that can be limiting or co-limiting in the SO
- Line 39 – “spatially coupled”
- Line 39- Add a sentence here introducing dust and sediments as the “typical” main sources of Fe and Mn to the ocean? The aerosol discussion came unexpectedly in this hydrothermal manuscript and needs a gentler introduction
- Lines 39-40- “...can vary by >3 orders of magnitude, causing...”
- Line 43- avoid the use of the word “major” here as this is what is currently under debate
- Line 57- “drive” not “driving”
- Line 61- Includes the depth range for “shallow” vs “deep” vents
- Line 62- “primary production”
- Lines 67-69- **Include a discussion of proposed mechanisms for physicochemical stabilization** of dFe and dMn as this is a large assumption for whether waters enriched in dFe and dMn will be able to reach the surface SO

2.0 Results and Discussion

2.1 Observations of hydrothermal trace metals in the southeast Pacific sector of the Southern Ocean

- Line 96- Define “dFe” again here as $<0.2 \mu\text{m}$, as it’s the first time it is used to represent your data. It is also in contrast to the unfiltered TdFe that you mention next.
- Lines 99-101- Describing this feature is a key takeaway of the paper and so I would go into more detail. What are the ranges of dFe and dMn specifically? Do these ‘enriched’ waters shoal above 2000m? **What is the neutral density they are located on (discussed later)? Do you see this enrichment only within a specific water mass? If so, can you contextualize the “background” concentration of dTMs in this water mass?** Also, can you discuss the secondary dFe maximum that seems to be present at 4000m?
- Line 103- Remove “much”, later you show TdFe ranges that are orders of magnitude higher
- Line 107- were observed in benthic nepheloid layers at open ocean stations
- Line 121- due to scavenging removal
- Line 122, 133- basin-scale
- Lines 123-125- “Along our transect...”; What depths do these maximum concentrations correspond to? Are they the same depths you see for dFe?
- Line 128- comma splice. Split into two sentences or replace with a semicolon.
- Lines 131-145- This paragraph could be reworked to be placed with the later helium discussion
- Line 133- replace “constrained” with “predicted”

- Lines 137-141- Quantify the offset between dFe and helium more specifically—is it 10m? 100m? 1000m? Do you see similar evidence in the dMn data or is it decoupled from dFe (as has been shown to happen in other marine systems)?
- Lines 141-143- I believe more time should be spent **to justify the use of a dataset that is decoupled from the TM data by 26 years.**

2.2 Identifying the source region of the observed hydrothermal signal

- Line 159- replace “shoaling to” with “with depths of”
- Line 161- Mornington Abyssal Plain (MAP; our study region)
- Lines 162-163- Can you add these mid-depth flows to Figure 1, panel A?
- Lines 164-166- Well et al. 2003 (Deep-Sea Research I, 50: 1079-1098) should also be cited here. It was surprising that this paper was not cited in this manuscript.
- Line 166- replace “latitudinal” with “meridional”
- Line 167- “southward”
- Line 169- replace “longitudinal” with “zonal”
- Line 170- “eastward”
- Line 175- add comma after “systems”
- Lines 209-212- Can you provide context what these percentages (9%, 21%, etc.) mean in an oceanographic context? Are these the results you were expecting?
- Lines 207-221- I am convinced that the trajectories indicate that the SEPR is a likely source region for these hydrothermal metals based on these trajectories, but **I am not sure why more time is not spent discussing the PARS and PARN regions?** The back trajectory table and supplemental material seem to indicate that they are potential source regions. This becomes especially important when you return to this idea in your summary paragraph in Lines 234-235.
- Lines 230-231- “...that is traversing southwards towards OOI, but instead the...”
- Lines 236-247 and Lines 248-255- **These two paragraphs are a strong summary of previous work, but I would like to see this information discussed in the context of this study’s trajectory findings. If we did find PAR vents, how would this change your results?**

2.3 Could hydrothermal iron and manganese from the southeast Pacific vents fuel Southern Ocean primary production?

- Line 260- “plume-influenced”
- Lines 261-262- This transit time of 99+/- 18 years is for waters shoaling in the SO from the SEPR. Ensure sure this distinction is clear.
- Line 273: What kind of “impact”? Be more specific on whether you are talking about concentration impacts or primary production impacts.
- Lines 272-275- I disagree that it is “challenging” to compare your high-resolution study results with the coarse-resolution studies. How do the model output data of published Fe GCM models match your results?
- Lines 281-283- Please be explicit in the time scales you are talking about for both ventilation rates and dTM residence times in this section. Right now it is not clear.
- Line 287- “forward-track”

- Line 296- "...reach depths shallower than..."
- Lines 287-304- I appreciate how this paragraph places your work into the context of the literature discussed.
- Lines 301-304- These numbers are poorly constrained in the literature. **Can you give recommendations on how your study ADDS knowledge or boundaries to these timescales explicitly?**

3.0 Conclusion

- Lines 315-325- Why is there no mention of the SEPR or PARN/PARS specifically? It seems like a major conclusion of your study is discussing potential source regions for hydrothermal enrichment to the MAP area and these are not addressed in the summary. Maybe line 317? Also line 321?
- Line 322: "...provide the potential for regular input of..."
- I would also argue that your manuscript draws attention to the importance of SEPR vents south of 30°S, which are not those that were so well studied by GEOTRACES GP16 but travel westward. This should be emphasized if your results suggest their importance.