Answer to referee comments

Dear editors, we would like to thank you and the reviewers for their comments, which we have taken into account. We believed this significantly improved our manuscript. We have added this sentence "*The three anonymous reviewers are thanked for their comments, which allowed significantly improving the manuscript.*" in the acknowledges section at lines 915-917.

Our replies point by point, in blue. The line numbers correspond to the lines in the revised version with hidden suppressions.

Referee comment 1

Summary

This study presents iron concentration and isotope data from the EUCFe cruise that extends along the equator from near Papua New Guinea to 140°W. Two earlier papers have resulted from this dataset, with this manuscript extending the dataset to some extra stations (Labatut et al., 2014; Radic et al., 2011). In their earlier papers, the authors reveal two distinct regimes: 1) a western margin zone dominated by lithogenic inputs from Papua New Guinea, including riverine, shelf and hydrothermal input, and 2) a central open ocean region where Fe is transported eastward via the Equatorial Undercurrent. Iron isotope signatures for particulate iron are close to the upper continental crust, and the offset between dissolved and particulate iron suggests equilibrium fractionation, perhaps via non-reductive dissolution. Measurements of iron isotopes in the deep chlorophyll maximum showed minimal isotopic fractionation with no clear preference for heavy or light isotopes.

Overall, the manuscript presents new iron isotope data; however, it appears that the main findings have been discussed previously in Labatut et al., 2014 and Radic et al., 2011. The new component of the manuscript presents a detailed examination of iron isotope fractionation in each water mass. I also wonder about the strength of the discussion relating to iron isotope fractionation by phytoplankton. The authors do not present any results relating to phytoplankton groups that are likely to take up iron and fractionate it. They just say that iron isotope fractionation "lies between -0.17 and +0.39 ‰ (+0.11 ± 0.28) at a 95% confidence level". There is little consideration that various phytoplankton species might have different iron acquisition strategies e.g. Sutak et al., 2020, which likely influences iron isotope fractionation. If possible, I think this needs to be explored a bit more in the manuscript.

Below are my comments on the manuscript, and below that, I address the guideline questions for peer review and interactive public discussion.

We thank the reviewer for this summary and for his recommendations.

Regarding the novelty of this article, the addition of 55 samples from 11 new stations (representing 72 % of the dataset) indeed strengthens the hypotheses and observations presented in Radic et al. (2011) and Labatut et al. (2014). We also believe that this addition does more than simply reinforce previous conclusions, and that novelty has been introduced for several reasons:

i) The observation of a particulate - dissolved exchange without redox reaction is extended from less than 100 kilometers off the coast to 1 200 kilometers, as already highlighted in our original submission in the sentence "The addition of data from five additional western stations reinforces the conclusions drawn from earlier studies and extends the geographic scope of these findings eastward beyond the

Bismarck Sea, to as far as 156°E." in lines 455–457. This observation is significant, and it was not at all obvious that such a process would be observed so far from the coast and from sedimentary discharges. In order to take into account this recommendation, we have added in the conclusion at lines 833-834 "*New data from 11 additional stations demonstrate that this process extends up to 1 200 km from the Papua New Guinea coast.*" and in the abstract at lines 32-33 "*and extends up to 1 200 km from the coast*".

ii) The preservation of an isotopic signature over such a distance (7 800 kilometers) had never been observed before. To emphasize the innovative aspect of this article, we have added the sentence "*Such a long distance of preservation of the δ^{56}DFe signature had never been observed before*." at lines 702–703 and in the conclusion, we have added '*unprecedented*' in the sentence "*The preservation of distinct Fe isotopic signatures over unprecedented long distances, 7 800 km, is a key observation of this study.*" at line 847.

iii) The addition of these new results changes the estimation of the fractionation associated to biological uptake between Radic et al. (2011) and this manuscript, due to new data and to the correction for lithogenic iron. We have added the sentence: "*Additional data and novel methodological approaches have refined these estimates.*" at lines 582-583.

iv) A second and previously unidentified source, originating from the oxygen minimum zones of the Eastern Pacific Ocean, has been identified. This was highlighted in our original submission in the conclusion, at lines 844-845, "an additional Fe source was identified bordering the lower EUC at 2°N and 2°S likely originating from the oxygen minimum zones (OMZ) of the eastern Pacific.". We have added in the conclusion "*and introduces a novel suggestion about an iron source from the eastern Pacific OMZ.*" at lines 811-812.

We have modified Section 5.2.1, which addresses isotopic fractionation during biological uptake, in order to include more information on the phytoplanktonic composition of surface waters (see the responses to the questions below).

Comments

Line 218. The presentation of the blank contributions is percentage values relative to dissolved and particulate matter iron data. Is it possible to present the amount or concentration values as well?

Yes, we added the blank quantities (ng) for DFe and PFe analysis. In our original submission, we made an error by not recalculating the percentage that the blank represented for the complete dataset, rather than only for stations 24, 28, and 30 from Labatut et al. (2014). Consequently, the percentage values were slightly different and we have modified them at lines 219-222: "*The total procedural blanks were 0.74 ± 1.17 ng (2SD) for DFe and 7.47 ± 1.09 ng (2SD) for PFe. This amounts to 0.5 % and 0.6 % of the samples average Fe content for DFe and PFe, respectively and 2.8% and 9.7 % for the sample with the smallest Fe content for DFe and PFe, respectively*".

Line 331. "Fe concentrations in the western equatorial Pacific were approximately seven times ..." It would be useful to explicitly state concentrations, perhaps in brackets, so the reader doesn't have to search for the values. Fe enrichment in the western equatorial Pacific, e.g., "Fe concentrations were approximately twice as high for DFe and seven times higher for PFe compared to central Pacific stations."

Yes, concentrations have been added at lines 348-351: "*Fe concentrations throughout the entire water column in the western equatorial Pacific were approximately twice as high for DFe (0.63 nmol.kg^{-1}*

compared to 0.30 nmol.kg^{-1}, on average) and approximately seven times higher for PFe (3.58 nmol.kg^{-1} compared to 0.49 nmol.kg^{-1}, on average) relative to the central equatorial Pacific stations.".

Lines 411 to 413. Have you considered that heavier δ^{56}DFe relative to δ^{56}PFe could be preferential complexation of dissolved iron to natural organic ligands present in seawater? Under equilibrium control, ligands should selectively bind heavier isotopes relative to lighter isotopes. If the majority of DFe is bound to strong organic ligands (Fe-L), then Fe-L should be heavier than inorganic Fe (Fe'). Persumability, Fe' is what exchanges with PFe. I guess this is what you are terming as non-reductive dissolution.

Indeed, we agree that the complexation of dissolved iron by natural organic ligands can be one possible explanation for non-reductive dissolution (our group proposed this as early as 2017 (Abadie et al. 2017)). However, desorption could also be involved. This is what we have explained in our original submission, at lines 469–471, "In all cases, at the sediment–seawater interface and within the water column, the exact processes involved remain unclear. Desorption and ligand-promoted dissolution have notably been suggested (Abadie et al., 2017; John et al., 2018; Homoky et al., 2021).". We therefore prefer to remain cautious regarding the specific process or processes involved. For clarity, we have added another sentence emphasizing this aspect at lines 470-471 in the conclusion "*Desorption and/or ligand-promoted dissolution are potential mechanisms, though the exact processes involved remain unclear (Abadie et al., 2017; Homoky et al., 2021).*".

Lines 456 to 459 – Figure 8. The colour scheme used in this Figure and subsequent ones makes it very hard to determine the difference between isotope values. I certainly found it difficult to see the subtle changes in blue between samples – this is highlighted in the lower panel with stations 13 and 14. Here are the values greater or less than 0‰? Perhaps change the colour palette away from Viridis to the ODV colour palette or Ferret_blue_orange.

We understand your point, but we would like to keep the "Viridis" color bar. As stated in Ocean Science's submission policy, "it is important that the color schemes used in your maps and charts allow readers with color vision deficiencies to correctly interpret your findings." The ODV color palette or Ferret_blue_orange are not suitable because they are not gradual and/or do not use colors that are distinguishable for all types of colorblindness. To make the maps more readable, we changed the color bar values in Figures 8, 9, 10, 11, and 12; the color scales are different for each figure, thus allowing better perception of the different colors. Consequently, we changed the sentence at lines 496-497 by removing "To facilitate comparison, the δ^{56}Fe color coding is the same for all density layers." and replacing it with "*To facilitate interpretation, the δ^{56}Fe color scales are different for each density layer.*". In Figure 8, we added the values in parentheses directly on the figure and included in the caption: "*Values are shown in parentheses due to the large range, which limits color scale readability. Their measurement uncertainties can be found in Section 5.2.1 and Table 2.*".

Line 465. Any ideas on what phytoplankton species occupied the deep chlorophyll maximum (DCM)? This seems important when attributing isotope fractionation to biological production. Where nutrient and associated parameters were collected on the voyage to support the interpretation of how iron might be acquired by phytoplankton, new vs recycled iron etc? Cyanobacteria and diazotrophs vs eukaryotic species.

Yes, thank you for pointing out this omission. This addition greatly improves this section of the article. First, regarding terminology, we have added at lines 514-515 "*also called the deep chlorophyll maximum (DCM)*" in order to better include the scientific community specializing in these zones and in marine biology. Second, the phytoplankton community was investigated at one station at 10 m

depth (Marchetti et al., 2010), at four stations in the DCM (Johnson et al., 2010), and at seven stations at different depths only for diazotroph species (Bonnet et al., 2009).

In the revised version, we added, at lines 516-526, the sentences: "*During the EUCFe cruise, the phytoplankton community was studied at station 2 at a depth of 10 meters (Marchetti et al., 2010), and in the chlorophyll maximum layer at station 2, station 25, and at two other stations lacking iron isotope measurements (0°, 165°E and 0°, 170°W) (Johnson et al., 2010). The cyanobacteria Prochlorococcus and Synechococcus dominate this community, followed by small pennate diatoms such as C. closterium and N. bicapitata (Johnson et al., 2010; Marchetti et al., 2010). This observation is consistent with previous studies in this equatorial upwelling region, which report a predominantly cyanobacteria-based community (Chavez et al., 1990; Landry et al., 1996). Unicellular diazotrophic cyanobacteria were also identified throughout the cruise, primarily picocyanobacteria, while the larger Trichodesmium was found only in coastal waters (Bonnet et al., 2009).*". Accordingly, we have added references for Bonnet et al., 2009; Chavez et al., 1990; Johnson et al., 1997; Landry et al., 1996; and Marchetti et al., 2010 to the References section.

Line 531. It might be worth considering the work of John et al. (2024), who tried to determine iron isotope fractionation in phytoplankton cultures.

Yes, thank you for this very useful recommendation. We added at lines 599-610 "Culture experiments examining isotopic fractionation by several diatom and a coccolithophore species (not the dominant species in our samples (Johnson et al., 2010; Marchetti et al., 2010)) revealed 'no clear relationship to species, growth rate, or Fe concentration' for biological uptake, possibly due to the sensitivity of kinetic isotope effects (John et al., 2024). During these experiments, biological uptake induced smaller fractionation (-1.3 ‰ to +0.60 ‰, mean +0.20 ± 0.38 ‰,1SD, n=62) compared to abiotic processes (approximately -4 ‰ to +5 ‰). These laboratory observations align with our in situ observations, although a comparison is not straightforward because iron acquisition processes are very different in cyanobacteria (Sutak et al., 2020). These authors suggested that seawater δ^{56}Fe may not be greatly impacted by biological uptake (John et al., 2024); a conclusion consistent with the findings of Lacan et al., 2008 and Radic et al., 2011."

We have also mentioned the work of John et al., 2024 at lines 626-627 by adding "*and culture experiments (John et al., 2024).*".

Consequently, we have added John et al., 2024 and Sutak et al., 2020 to the References section.

Lines 532 to 540. The discussion here is a little simplified and assumes that iron isotope fractionation by phytoplankton is likely to be similar across varying regions. At present, we have no real idea how cyanobacteria fractionate iron. The DCM is likely to be populated by Prochlorococcus and Synechococcus, as well as by unicellular diazotrophs, if nitrogen is limiting. Very little work has been done with these two bugs in fractionating iron under oxic conditions (Mulholland et al., 2015; Swanner et al., 2017). Perhaps this could be acknowledged. Again, it might also be worth referencing. John et al. (2024) here, who reported kinetic isotope effects during Fe(III) reduction in cultures. These findings could provide useful context for interpreting biological fractionation in this study.

In our original submission, with the sentence "The above differences in biological fractionation probably reflect regional variabilities, including differences in phytoplankton community, as well as differences in methodology such as the direct measurement of particles and the consideration of their phases" we intended to highlight the limitations of this study and the potential variability of these results depending on location, phytoplankton community, and other factors. In the revised submission, we have taken your recommendations into account. We have clarified this point by adding and

modifying some sentences at lines 611-621: "*The differences in biological fractionation probably reflect variations in phytoplankton community composition, ligand types (John et al., 2024), regional variability, and methodological approaches such as the direct measurement of particles and the consideration of their phases. Potential iron fractionation during biological uptake, if it occurs, may depend on numerous parameters, including species-specific iron acquisition processes (Sutak et al., 2020), as well as pH, ligand, and reductant types, which strongly influence kinetic isotope effects (John et al., 2024). Our study region is particularly challenging in this regard, as it is a cyanobacteria-dominated system where isotopic fractionation processes remain poorly understood (Mulholland et al., 2015; Swanner et al., 2017). Iron isotope fractionation by diazotrophic cyanobacteria has not been investigated, despite these organisms contributing disproportionately to Fe uptake relative to their numerical abundance (Lory et al., 2022).*"

To further emphasize this point, we have added the sentence "*Estimates of fractionation due to biological uptake must include consideration of phytoplankton composition, dominated here by cyanobacteria.*" to the conclusion, at lines 838-839.

Consequently, we have added Lorry et al., 2022; Mulholland et al., 2015 and Swanner et al., 2017 to the References section.

Lined 701 to 702. A supporting reference for AAIW circulation in this region and the South Pacific is Bostock et al. (2013). It may be worth noting that this study region lies near the northern extent of AAIW influence, as discussed by Bostock et al. (2013), who reviewed AAIW circulation and mixing using geochemical tracers and Argo float data.

We have already cited several sources on AAIW circulation in Section 2, including Bostock et al., 2010. Therefore, we have not added Bostock et al., 2013 but have re-cited Tomczak and Godfrey, 2003 and Bostock et al., 2010 at line 790.

Review guidelines

1. Does the paper address relevant scientific questions within the scope of OS?

Yes

1. Does the paper present novel concepts, ideas, tools, or data?

Somewhat – as mentioned, the manuscript presents some new iron isotope data; however, it appears that the main findings have been discussed previously in (Labatut et al., 2014; Radic et al., 2011)

1. Are substantial conclusions reached?

More work is needed on how phytoplankton fractionate iron isotopes./

1. Are the scientific methods and assumptions valid and clearly outlined?

Yes the scientific method and measurements are sound.

1. Are the results sufficient to support the interpretations and conclusions?

Generally, see comments about iron isotope fractionation by phytoplankton

1. Is the description of experiments and calculations sufficiently complete and precise to allow their reproduction by fellow scientists (traceability of results)?

This is fine

1. Do the authors give proper credit to related work and clearly indicate their own new/original contribution?

Yes they credit previous work

1. Does the title clearly reflect the contents of the paper?

I think a better title would be “Iron isotopes provide insights into the biogeochemical cycling of iron in the equatorial Pacific”

1. Does the abstract provide a concise and complete summary? Yes

1. Is the overall presentation well structured and clear?

Generally, figure colours could be improved to allow the reader to distinguish between iron isotope values.

1. Is the language fluent and precise? yes

1. Are mathematical formulae, symbols, abbreviations, and units correctly defined and used? yes

1. Should any parts of the paper (text, formulae, figures, tables) be clarified, reduced, combined, or eliminated? no

1. Are the number and quality of references appropriate? yes

1. Is the amount and quality of supplementary material appropriate? yes

references

Bostock, H.C., Sutton, P.J., Williams, M.J.M., Opdyke, B.N., 2013. Reviewing the circulation and mixing of Antarctic Intermediate Water in the South Pacific using evidence from geochemical tracers and Argo float trajectories. Deep-Sea Research Part I: Oceanographic Research Papers, 73: 84-98.

John, S.G. et al., 2024. Kinetic Isotope Effects During Reduction of Fe(III) to Fe(II): Large Normal and Inverse Isotope Effects for Abiotic Reduction and Smaller Fractionations by Phytoplankton in Culture. Geochemistry, Geophysics, Geosystems, 25(6): e2023GC010952.

Labatut, M. et al., 2014. Iron sources and dissolved-particulate interactions in the seawater of the Western Equatorial Pacific, iron isotope perspectives. Global Biogeochemical Cycles, 28(10): 1044-1065.

Mulholland, D.S. et al., 2015. Iron isotope fractionation during Fe(II) and Fe(III) adsorption on cyanobacteria. Chemical Geology, 400: 24-33.

Radic, A., Lacan, F., Murray, J.W., 2011. Iron isotopes in the seawater of the equatorial Pacific Ocean: New constraints for the oceanic iron cycle. Earth and Planetary Science Letters, 306(1-2): 1-10.

Sutak, R., Camadro, J.-M., Lesuisse, E., 2020. Iron Uptake Mechanisms in Marine Phytoplankton. Frontiers in Microbiology, 11(2831).

Swanner, E.D. et al., 2017. Iron Isotope Fractionation during Fe(II) Oxidation Mediated by the Oxygen-Producing Marine Cyanobacterium Synechococcus PCC 7002. Environmental Science & Technology, 51(9): 4897-4906.

Citation: https://doi.org/10.5194/egusphere-2025-4525-RC1

Referee comment 2

General comments: “Iron isotope insights into equatorial Pacific biogeochemistry” by Camin et al. presents an expanded dataset of dissolved and particulate iron concentrations and stable isotope measurements from the western and central equatorial Pacific. The manuscript builds upon work previously published from this project that focused on a subset of the stations along with aerosol iron measurements from the same cruise. The authors combine these datasets to construct a box model for iron transport in the region and also use the data to discuss iron transport in the various water masses of the region. Overall I think that the manuscript is a helpful addition to the literature on iron biogeochemistry in the Pacific but I think the authors could strengthen parts of the discussion – for example, discussing uncertainties and limitations associated with the box model approach and how consideration of separate authigenic and biogenic contributions to non-lithogenic particulate iron (highlighted by recent work in the North Atlantic) could influence their conclusions about isotope fractionation during biological uptake. There is also some geographical overlap between this work and a recently published paper by Sarthou et al. (“Dissolved iron distribution and budget in the Solomon Sea”; accepted by Marine Chemistry shortly after this manuscript was submitted –full details below) with some of the same coauthors. There is sufficient overlap that some references to that paper could be incorporated into the discussion of this work during revision.

Thank you for these suggestions. We have strengthened the discussion on the box model and authigenic/non-biological iron processes, and added Sarthou et al., which focuses on a nearby region (see responses below).

EUCFe values agree with the DFe concentrations reported by Sarthou et al., 2025. We have therefore added this reference at lines 296-298 “*In addition, EUCFe DFe and PFe concentration data are in good agreement with data published in the same area (John et al., 2018; Marsay et al., 2018; Zheng and Sohrin, 2019; Cohen et al., 2021; Sarthou et al., 2025)*.” and in the References section.

Specific comments:

I think the calculations for the box model need some more discussion, as outlined below:

- The calculation of Flux PFeSW out appears to have been done by averaging all of the PFe concentrations from all depths from stations 21-30 to get a value of 3.6 nmol/kg, which was then multiplied by water transport. Is this the best approach? That dataset includes the one very high value of 29 nmol/kg from 39m at station 28. Excluding this one value would drop the average significantly from 3.6 to 2.9 nmol/kg. It also includes a handful of values calculated from different bottles at the same depth at certain stations (e.g. three samples at 218m depth at station 23), which potentially gives those depths more weight in the calculated average. And only 5 of the 40 values are from depths in the 400-1000m range, thus the average would be skewed towards <400m data. I think the uncertainty and the caveats associated with this approach should be acknowledged and explained more. For example, if the average of 3.6 nmol/kg is calculated as stated above, the associated standard deviation is 4.8 nmol/kg by my calculations.

First, we changed the value used for water transport through Vitiaz Strait from 18.7 Sv to 19.4 Sv, which is the exact value from Germineaud et al. (2016) for the Austral winter. The revised sentence at lines 364-366 is therefore: “*The transport of water masses in this area, from the surface to a depth of 1,000 m, was estimated at 19.4 ± 0.4 Sv (1SD), based on the flow in Vitiaz Strait during the Austral winter*

(i.e., the same period as the EUCFe cruise)". We also made another mistake: Tilliette et al. (2022) did not publish PFe data, and therefore we have removed this reference from the manuscript. We changed the typical open ocean PFe concentration, from 0.5 nmol.kg^{-1} to 0.15 ± 0.08 nmol.kg^{-1} (1SD) (Marsay et al., 2018). The new sentence is: "*The incoming water is assumed to carry a typical open ocean PFe concentration (average value for the upper 1000 m of the water column, station 36, GP16 cruise) of 0.15 ± 0.08 nmol.kg^{-1} (1SD) (Marsay et al., 2018), prior to enrichment within the study area.*" at lines 366-369. Consequently, the flux of PFe transported by water masses into this area, Flux PFe$_{SW\ in}$, changed from 45 tons (PFe).day^{-1} to 14.5 x 10^6 g (PFe).day^{-1}, we have modified this value at line 370. The values presented below will differ slightly from our original submission due to these corrections.

Second, indeed, excluding the sample with very high PFe concentrations would lower the mean PFe concentration, and considering only one average value for the Go-Flo bottle replicates would modify the mean PFe concentration. The table below presents the results according to the different methods. This table constitutes a sensitivity analysis examining the model's response to variation in one input parameter, i.e., the mean PFe concentration.

In this table, the Flux PFe$_{aerosol}$ considered is 268 x 10^6 g (PFe).day^{-1} which is significantly different from our original submission. This value has been changed to take into account total atmospheric deposits (dry + wet) in response to the reviewer's comment. Details are therefore provided after the following comment from the reviewer.

We have added this table as Table A1 in the Appendice Section.

	Go-Flo bottle replicates considered separately and with the highest value (29.45 nmol.kg^{-1})	Go-Flo bottle replicates considered separately and without the highest value (29.45 nmol.kg^{-1})	Go-Flo bottle replicates averaged and with the highest value (29.45 nmol.kg^{-1})	Go-Flo bottle replicates averaged and without the highest value (29.45 nmol.kg^{-1})
Mean PFe concentration (nmol.kg^{-1})	3.6 ± 4.8 (1SD)	2.9 ± 2.3 (1SD)	3.8 ± 5.0 (1SD)	3.0 ± 2.5 (1SD)
Flux PFe$_{SWout}$ (10^6 g.day^{-1})	337.9	272.2	356.7	281.6
Required external sources flux: Flux PFe$_{SWout}$ - Flux PFe$_{SWin}$ (10^6 g.day^{-1})	323.5	257.8	342.2	267.1
Aerosol PFe flux contribution to required external sources (% g.g-1)	82.9	104.0	78.3	100.3
Riverine PFe flux contribution to required external sources (% g.g-1)	37,099.0	46,556.2	35,064.0	44,920.3

Table. Mean PFe concentration, Flux PFe$_{SWout}$, the required external sources flux, Flux PFe$_{aerosol}$ and Flux PFe$_{riverine}$ contribution to required external sources (% g.g^{-1}) according to different calculation methods. Go-Flo replicates are duplicate samples taken at sea, from the same cast.

The purpose of this model is to provide a rough estimate of the contribution of PFe sources to the PFe content in the western equatorial Pacific region. This model has several limitations, including the choice of average concentration for the study area, which is primarily based on sample data from the surface to 400 m depth.

Therefore, we revised the manuscript to provide a range of values according to the 4 scenarios tested in Table A1. We have modified the sentence: “The average PFe concentration in this area (stations 21 to 30) ranged between 2.9 and 3.8 $nmol.kg^{-1}$ depending on the calculation method chosen (Table A1).” at lines 371-372.

We have added the following clarification at lines 372-373: “The fluxes derived from these concentrations will be given as a range.” and “Thus, the riverine PFe flux is 448-fold larger than the atmospheric PFe flux. It is therefore much more likely that the PFe in this region is predominantly of riverine origins. The isotopic compositions support this hypothesis, as the δ^{56}PFe values are close to crustal values in this region (Table 2 and Figure 4), whereas aerosols, exhibit a heavier isotopic signature, around +0.3 ‰ (Camin et al., 2025).”

With this revised scenario, we have updated the flux, concentration, and water transport values at lines 374, 376 and 377 accordingly.

We have modified Figure 6 with updated fluxes and added to the caption “Note that all of the external sources end up in seawater. Some are removed to the sediments.” at line 410.

For clarity purpose, we have added “Regarding riverine particles, the question of their transport mechanism over such a distance from the coast (~1 200 km) arises.” at lines 411-412.

- For the atmospheric deposition, the deposition velocity of 1000 m/d used is commonly applied where this cannot be measured more directly, but it is usually applied to calculate dry deposition only, rather than bulk (dry + wet) deposition, and wet deposition dominates atmospheric input over much of the ocean. An alternative approach to calculate total atmospheric input would be to apply the relationship between bulk deposition velocity and precipitation rate that has been derived from beryllium-7 data by Kadko et al (2020) and recently updated by He et al (2025). This would involve deriving an estimate of the precipitation rate over the area covered by the box model from satellite imagery.

Indeed, this methodology could be very insightful. We agree with the reviewer that we must incorporate PFe input via wet deposition.

We added the following to the manuscript at lines 383-393: "*The bulk aerosol deposition velocity, denoted Vb ($m \cdot day^{-1}$), includes both wet and dry deposition. It can be calculated from the precipitation rate ($mm \cdot day^{-1}$) according to the formula from Kadko et al., 2020, recently updated by He et al., 2025, who used ^{7}Be as a proxy for atmospheric deposition:*

$Vb = 413 \pm 22 \times$ Precipitation Rate $+ 1069 \pm 71$ (Equation 2)

To estimate the precipitation rate, we used the NASA Giovanni data product TRMM to determine the mean daily precipitation rate over the entire region considered in the box model from September 20 to 30, 2006. The mean value was 11.4 ± 6.6 $mm \cdot day^{-1}$ (2SD) (Table A2). Applying Equation 2, the bulk aerosol deposition velocity is therefore 5,777 $m \cdot day^{-1}$. The resulting atmospheric PFe deposition flux

was Flux PFeaerosol = 268 x 10^6 g(PFe).day^{-1}. This estimate accounts for between 78 and 104 % of the required external sources (Table A1).".

However, this does not preclude a potentially significant riverine source. We have therefore replaced the sentence at lines 394-395: "Another external source to consider is sedimentary PFe flux delivered by rivers." has been changed to "*However as discussed below, this PFe atmospheric deposition flux is orders of magnitudes lower than that delivered by rivers.*". We have added at lines 401-402: "*This estimate accounts for between 35 064 and 46 556 % of the required external sources.*".

Given that atmospheric deposition can account for ~100% of the PFe input required for the region, we have removed this part of our original submission: "Assuming steady state, mass conservation for this model implies: Flux PFeSW in + Flux PFeaerosol + Flux PFesediment = Flux PFeSW out (Equation 2) Taking into account the above estimates for Flux PFeSW in, Flux PFeSW out, and Flux PFeaerosol, we obtain PFesediments = 232tons(PFe).day 1, for the sedimentary PFe flux delivered by rivers and reaching the western equatorial Pacific area.".

We added the following conclusion at lines 403-407: "*Thus, the riverine PFe flux is 448-fold larger than the atmospheric PFe flux. This flux comparison argues in favor of a contribution dominated by input from rivers. Isotopic signatures provide an additional constraint and also support a riverine source, as the δ^{56}PFe values are close to crustal values in this region (Table 2 and Figure 4), whereas aerosols exhibit a heavier isotopic signature, around +0.3 ‰ (Camin et al., 2025).*".

We have added a new table in the Appendice section:

Date	Precipitation Rate (TRMM 3B42 Daily v7) access in December 2025 from Nasa Giovanni data product TRMM (mm.day^{-1})
September 20, 2006	7.5
September 21, 2006	7.7
September 22, 2006	10.6
September 23, 2006	8.3
September 24, 2006	10.2
September 25, 2006	9.0
September 26, 2006	11.4
September 27, 2006	13.9
September 28, 2006	14.4
September 29, 2006	16.5
September 30, 2006	16.0
Average value	**11.4 ± 6.6 (2SD)**

Table A2. Daily precipitation rates (mm.day-1) over the entire region considered in the box model (133°E–177°W, 9°S–15°N) from September 20 to 30, 2006. The data was obtained from Nasa Giovanni data product TRMM (3B42 Daily v7) (https://giovanni.gsfc.nasa.gov/giovanni/).

We have added Kadko et al., 2020 and He et al., 2025 in the References section. We therefore modified the numbering of the appendix tables. We have modified Figure 6 with the new fluxes' values.

- A related point is that the box model area to which atmospheric deposition is applied includes much of New Guinea. Any deposition over this area would technically be included in runoff (river input) to the study area rather than direct atmospheric input and so should not be included in the

atmospheric input. I suspect that this detail would become more important when calculating bulk deposition due to enhanced rainfall over high elevations of the island.

Indeed, not excluding the PNG surface area from the atmospheric deposition calculation overestimates the atmospheric contribution in our original submission.

If we remove the land area and conservatively subtract only the surface area of the island of New Guinea, 785 753 km² (Baldacchino et al., 2024), we obtain a deposition area of approximately 1.54×10^7 km² instead of 1.6×10^7 km².

We have modified it at lines 381-383: "*covers approximatively 1.54 x 10^7 km² (the box model surface area of 1.62 x 10^7 km² minus the New Guinea land area of 0.08 x 10^7 km²; Baldacchino et al., 2024)*". Consequently, we have added the reference Baldacchino et al., 2024 to the References section.

I have a couple of questions about the calculation of phytoplankton PFe and d56PFe in section 5.2.2.

- It is assumed that all non-lithogenic PFe consists entirely of organic (and specifically phytoplankton) PFe. However, recent work from the Atlantic Ocean stresses the importance of non-biogenic, non-lithogenic phases in controlling Fe dynamics in the upper ocean, with authigenic Fe often representing a greater fraction of PFe than biogenic PFe (Tagliabue et al, 2023; Sofen et al, 2023). The modeling output in Figure 4 from Tagliabue et al 2023 does suggest that biology may be the dominant control on the Fe cycle in the region of this study, but the authors should discuss how contributions from authigenic (as opposed to biogenic) PFe would affect their calculations.

For clarity purpose, we have removed all 'authigenic' and 'allogenic' mentions, we only mentioned the different PFe phases.

Indeed, part of the non-lithogenic PFe is chemogenic (i.e., particles formed by chemical reactions such as Fe oxyhydroxide precipitation). We assumed that, and only that, at the depth of the chlorophyll maximum layer, the non-lithogenic fraction was entirely of biological origin as mentioned in our original version. With this in mind, we have added few sentences to highlight the potential limitations of our approach and possible improvements at lines 544-555: "*In the chlorophyll maximum layer, we found an average of 42 % mol.mol^{-1} of particulate iron (PFe) is lithogenic (Table A3). The remaining 58 % mol.mol^{-1} can be attributed to biogenic and chemogenic sources. The chemogenic fraction cannot be distinguished and quantified in the present study (this would have required, for example, direct measurements of phytoplankton stoichiometry and phosphorus), and there is no known isotopic signature specific to this chemogenic fraction in the literature (which is essential for Equation 4)., Because we are looking at samples taken in the chlorophyll maximum, we will assume in the following that the chemogenic Fe is negligible. This assumption is supported by observations reporting minimum chemogenic contribution in the chlorophyll maximum layer (Sofen et al., 2013) and modelling work estimating that the biogenic fraction dominates in the central equatorial Pacific (Tagliabue et al, 2023). We also assume that biogenic Fe is entirely phytoplanktonic Fe (PFe_{Phyto}) and assuming mass conservation, $\delta^{56}PFe_{Phyto}$ can be estimated from:*"

Consequently, we have added the references Sofen et al., 2023 and Tagliabue et al., 2023 to the References section.

Regarding the impact of the chemogenic fraction on our calculations, this is very difficult to estimate because different processes with distinct fractionation patterns may be involved. If Fe oxyhydroxides originate from aqueous Fe(II), oxidation followed by precipitation occurs, yielding Fe(III) that is generally heavier than the initial Fe(II). If the source is aqueous Fe(III), only precipitation occurs, which tends to produce lighter Fe(III). Therefore, it is difficult to evaluate how a significant chemogenic Fe

component would affect Equation 4 calculations, as the effect could shift isotopic compositions in either direction (lighter or heavier) depending on the original Fe source of the oxyhydroxides.

- In calculating d56PFePhyto, the authors use a lithogenic d56PFe value equivalent to the crustal signature (+0.07 ‰). However, in a separate paper from the same study, the authors described atmospheric input to the region having a heavier than crustal signature of +0.31 ‰. Is the assumption that this atmospheric contribution is insignificant relative to lithogenic Fe advected from the western Pacific?

Yes, atmospheric inputs are considered negligible compared to riverine sources, as emphasized at lines 403-404 in the revised manuscript: "Thus, the riverine PFe flux is 448-fold larger than the atmospheric PFe flux. This flux comparison argues in favor of a contribution dominated by input from rivers.".

I was confused by the use of "tons" when describing the amount of Fe transported into and out of the study region throughout section 5.1. Presumably the amount referred to here is a metric ton(tonne), but ton can also refer to an imperial weight unit. I suggest expressing all values in grams instead (i.e. 45 tons would be 45 x 106 g).

We agree with the reviewer and have changed all tons to 10^6 g at lines 370, 374, 376, 377, 392, 397, 398 and 401 and in Figure 6.

Line-specific comments:

Lines 108-109: How deep does the influence of these westward currents extend?

We have added this information in the revised manuscript at lines 108-109 "extending from the surface to approximately 400 m depth (Cravatte et al., 2017)".

Lines 117-118: Some of the references cited in the figure caption are not in the reference list (Delcroix et al, 1992; Kashino et al, 1996; Kashino et al, 2007; Johnson et al, 2002). Done at lines 1004-1006, 1091-1096.

Lines 201-212: Does "leachate" refer to the digested material? I suggest using "digest" instead, as leachate can often refer to the solution resulting from a treatment that gives a partial release of elements, rather than total solubilization.

Yes, indeed, leachate referred to the digested material (we agree that was not the right term). We removed "leachates" at lines 211 and 213 and replaced it with 'digested material' at line 213.

Line 220: Does repeatability here refer to repeat analysis of a sample or analysis of two samples collected at the same depth and processed individually?

We have modified our original submission to explain how repeatability was assessed by adding: "*For DFe, the filtered seawater from a sample was divided into several aliquots, then each was processed individually (including different double spiking, preconcentration and purification). For PFe, each sample, i.e., membrane, was first digested (no division of the membrane before digestion), then the digested sample was divided into several aliquots, and each was processed individually (including different double spiking and purification). In some instances, duplicate samples are taken at sea, from the same cast. These replicates are termed "Go-Flo replicates" in the Table A1.*" at lines 222-228.

Lines 331-333: Does the comparison of concentrations between western and central equatorial stations ("twice as high for DFe…seven times higher for PFe") refer to a specific depth range? Or averaged over all samples? I think this could be more specific.

It refers to all depths, we modified the sentence to include more clarity "*Fe concentrations throughout the entire water column in the western equatorial Pacific were approximately twice as high for DFe (0.63 nmol.kg-1 compared to 0.30 nmol.kg-1, on average) and approximately seven times higher for PFe (3.58 nmol.kg-1 compared to 0.49 nmol.kg-1, on average) relative to the central equatorial Pacific stations*" at lines 348-351.

Line 410: I'm confused as to how the exchange is both permanent and reversible. (also in the Conclusions at line 760 and the abstract at line 30).

We agree with the reviewer that this introduces confusion. We have removed the sentence: "This is interpreted as the result of equilibrium isotopic fractionation, resulting from a permanent and reversible exchange between dissolved and particulate Fe phases." in our original submission and replaced it by: "*This is interpreted as the result of equilibrium isotopic fractionation, in other words the co-occurrence of chemical fluxes from both phases toward the other*." at lines 825-827 in the revised version.

We have replaced "permanent and reversible exchange" by "equilibrium fractionation" at line 853 and a permanent and reversible dissolved-particulate exchange" by "*an equilibrium fractionation and the co-occurrence of chemical fluxes from both phases toward the other*" at lines 30-31.

Lines 696-704: Suggest not numbering statements as "1" and "2" twice in one paragraph. I don't think they are necessary for the first usage – that sentence would work well enough without numbered points – "Its signatures fall within the range observed for those water masses in previous studies, but they fall in the heavy part of those ranges and again have a smaller variability…"

We agree, we have removed "1)" and "2)".

Lines 761-763: This sentence didn't read clearly to me. It may help to use "occurs" instead of "occurring" and add parentheses for the "i.e." parts of the sentence.

Yes, we have modified the sentences at lines 854-856 "*This non-reductive dissolution (NRD), occurs either at the sediment/seawater interface (i.e., external sources), or within the water column (i.e., internal processes*)."

Figure 4: Suggest including something in the caption to the effect that error bars are not included for clarity but a scale showing relative size of typical error is included.

We added in the caption "*Individual error bars are not shown for clarity, but typical uncertainty (± 0.07 ‰) is indicated by the scale bar*." at lines 277-278.

Figures 8-12: I found the choice of colour scheme or distribution of colours involved to be unhelpful in picking out some of the trends discussed in the text. For example, in section 5.2.3, it is noted that d56DFe at stations 1 and 3 differ significantly from the other samples (line 615), but the difference in the blue and green dots is quite subtle and difficult to pick out.

Reviewer 1 also made this comment, which we have taken into account and to make the maps more readable, we changed the color bar values in Figures 8, 9, 10, 11, and 12. The color scale limits are different for each figure, thus allowing better perception of the different colors. Consequently, we changed the sentence by removing "To facilitate comparison, the δ56Fe color coding is the same for all density layers." and replacing it with "*To facilitate interpretation, the δ56Fe color scales are different for each density layer.*" at lines 496-497.

Technical corrections: We thank the reviewer for these corrections (in addition to the others).

Line 34: Should be HNLC rather than HNCL. Also at Line 750. Done at lines 35 and 843.

Line 36: Should be "…that iron was originating…" rather than "…that was iron originating…". Done at line 37.

Line 39: Sentence needs some rearrangement. Maybe "…that any fractionation must be small, no larger than…". We have modified this sentence by "that any fractionation, if present, must be small, no larger than a few tenths of a per mil." at lines 39-40.

Line 52: "be" is not necessary here – "where Fe is believed to have a main source…". Done at line 53.

Line 54: Should be "…is an eastward-flowing…". Done at line 55.

Line 135: No need for semicolon after the reference. Done at line 136.

Line 138: Degree symbol should be in front of "C". Also at Lines 460, 549, 588, 639, 672. Done.

Line 303: No need to type out "upper continental crust" again – it was defined as UCC on line 293. (also Line 370). Done at lines 314, 399 and 821.

Line 479: No need for "the" before New Guinea. Done at line 505.

Line 481: Insert "are" before "…equal to…". Done at line 541.

Line 584: Density units are given as g.m-3 rather than kg.m-3. Done at line 673.

Line 596: No need for "the" before "Fe transport". Done at line 685.

Line 625: First comma of the line should be after "above" rather than "suggests". Done at line 714.

Line 645: "Figure" rather than "Figures". Done at line 733.

Line 727: No need for "coast" in "…all located coast within…". Done at line 816.

Line 755: Suggest using "averaging" rather than "average of". Done at line 849.

Line 777: Suggest changing to "Note that the fluorescence profiles were measured…". Done at lines 879-880.

Line 781: Add "Fe" to "dissolved isotopic composition". Done at line 883.

Table 1: For origin description of NPEW, it is described as a "mixture between SPEW and NPWC". I think the latter should be "NPCW". Done in Table 1.

Figure 6: Superscript needed for two of the "day-1" labels. Done in Figure 6.

References cited in this review:

Sarthou et al (2025), Dissolved iron distribution and budget in the Solomon Sea. Marine Chemistry, https://doi.org/10.1016/j.marchem.2025.104567

Kadko et al (2020), Quantifying atmospheric trace element deposition over the ocean on a global scale with satellite rainfall products. Geophysical Research Letters, https://doi.org/10.1029/2019GL086357

He et al (2025), Constraining aerosol deposition over the global ocean. Nature Geoscience, https://doi.org/10.1038/s41561-025-01785-2

Sofen et al (2023),Authigenic iron is a significant component of oceanic labile particulate iron inventories. Global Biogeochemical Cycles, https://doi.org/10.1029/2023GB007837

Tagliabue et al (2023), Authigenic mineral phases as a driver of the upper-ocean iron cycle. Nature, https://doi.org/10.1038/s41586-023-06210-5

Reviewer 3

This paper presents a valuable dataset of Fe isotopes in the Equatorial Pacific, with an exciting new coverage of Fe sources to the EUC, and adds to a growing body of literature that Fe isotope signatures can be useful for tracing sources over large distances. This paper builds on previous efforts by the same group (Radic et al., 2011 and Labututat et al (2014), with some of the stations already published, by presenting 15 more stations between PNG 140W. The data is clearly high quality, evidenced by comparison with a crossover station. I think the paper is very well written, valuable and worthy of publication, but my main comment would be that this paper doesn't capture enough of the recent Fe isotope literature in the discussion, and this requires attention prior to publication.

Specifically, there has been work on NRD and dissolved-particulate exchange since work from this region, which it would be helpful to include. For example, work by Homoky et al (2013; 2021) has built on the idea of NRD to suggest that this process is probably mainly happening as colloidal release in sediments, while Conway and John (2014) showed this signature to be linked to oxic sediment release in the water column. Further, studies in the Atlantic, and at hydrothermal vents, have shown that dissolved-particulate exchange with ligand can result in an isotopically heavy dissolved pool (e.g. John and Adkins 2012; Conway and John, 2014; Fitzsimmons et al., 2016). So I would encourage the authors to more closely consider the distinction between NRD in sediments/sediment plumes with dissolved-particulate exchange in the water column. Also, I am not convinced that simply comparing pFe and dFe is directly relevant for isotopic signatures, because only some portion of the particle pool actually exchanges with the dFe pool

Line specific comments:

Line 64-77. The references in the into seem out of date, which much iron isotope data being published since 2015, and this introduction missing coverage of many key studies. This should be rectified – since I notice more are cited in the discussion. Also, Resing did not provide any iron isotope data.

Indeed, the purpose of the citations at lines 64–65 is to give credit to the earliest studies on iron isotopes, and then, at lines 70-74; 83–84, to cite more recent references by topic. We have removed Resing et al., 2015 from line 73 and from the References section because it does not, in fact, present any iron isotope data.

Line 83. This is missing important studies on biouptake fractionation including Ellwood 2020, Sieber 2021, Kurisu 2024, John 2024, Tian et al. 2023, which are cited in the discussion so should be included here.

We agree and have added these references at line 83: "Sieber et al., 2021; Tian et al., 2023; John et al., 2024".

Line 227 Accuracy and Precision are different, so this should be re-worded.

Yes, we agree with the reviewer and did not intend to imply that accuracy and precision were the same thing in our original submission. The concept of accuracy refers to the two concepts of precision and trueness as defined by ISO 5725 (https://www.iso.org/obp/ui/#iso:std:iso:5725:-1:ed-2:v1:en). We have added: "*referring to the two concepts of*" at line 236.

Line 307 This effort to intercalibrate is really nice to see, and even nicer to see agreement. I would like to confirm that the authors have followed the fair use agreement in the IDP however, since the GP19 data is unpublished. Have you reached out to the respective group to either offer co-authorship or acknowledgement by name in your paper? At present the authors who generated the data are not properly cited. https://www.geotraces.org/wp-content/uploads/2025/11/IDP2025_Fair_Data_Use_Statement.pdf This is especially true if you plan to interpet data in a paper.

Thank you for pointing this out. We have corrected this error by contacting Tim Conway (University of South Florida), who produced the data. He requested that him, M. Sieber, and D. Vance be acknowledged in the figure caption for GP19 and M. Sieber and him for GP15. We have added the following to the caption of Figure 5 (lines 340-342): "*The GP15 cruise data were produced by M. Sieber and T. Conway. The GP19 cruise data were produced by T. Conway, M. Sieber, and D. Vance The GP15 cruise data were produced by M. Sieber and T. Conway, and the GP19 cruise data were produced by T. Conway, M. Sieber, and D. Vance; both datasets are available in the GEOTRACES Data product (GEOTRACES Intermediate Data Product Group, 2025).*" We have also acknowledged them in the Acknowledgments section (lines 915-917): "*Tim Conway and Matthias Sieber are acknowledged for their contributions to the GP15 and GP19 cruise data, and Derek Vance for his contributions to the GP19 cruise data; all were made available through the GEOTRACES Intermediate Data Product*.". We added GP15 data during revision after the release of the Intermediate Data Product 2025.

Line 397 I am not convinced it is relevant to simply compare pFe and dFe isotopic signatures, as has been done in previous papers from this group. The exchangeable pool of pFe is unlikely to be represented by the total pFe isotope signature, and so then I would question how relevant this comparison actually is. You've also not considered or included any discussion of studies who have suggested that particulate-dissolved exchange led to heavy ligand bound iron, a process that could also be happening here. There is a reasonably large body of data from the Atlantic showing values as high as +0.8 permil which have been attributed to dust-ligand exchange. This could be playing a role in your area and should be considered, even if you ultimately decide it is not relevant. We thank the reviewer for asking these two very interesting questions:

- Regarding the process that achieves the dissolution, indeed, we agree that ligand-promoted complexation is a potential process, but desorption is also one, as mentioned in our original submission: "Desorption and ligand-promoted dissolution have notably been suggested (Abadie et al., 2017; John et al., 2018; Homoky et al., 2021).". In the revised submission, we have added in the conclusion "*Desorption and/or ligand-promoted dissolution are potential mechanisms, though the exact processes involved remain unclear (Abadie et al., 2017; Homoky et al., 2021).*" at lines 830-831.
- Regarding the exchangeable pool and the phase involved in the particulate - dissolved exchange, this question is more complex. A first aspect of this issue is that, in order to carry out a scientific study of an object, it must be measurable. The choice made by our research team is primarily driven by the objective of producing reliable data. Partial leaching techniques (e.g., Revels et al., 2015) do not allow for this absolute certainty (as mentioned by the authors

themselves). Total digestion ensures data reliability despite potential complications in the interpretation (due to the presence of refractory phases). The second issue is identifying the phase involved in the particulate-dissolved exchange, without partial leaching technique. Because, we consider that we cannot access it, what follows falls within the scope of scientific interpretation, rather than observation. To address this, we consider the context in which these samples were collected. This systematic difference in isotopic signature, Δ56FeDFe-PFe = +0.27 ± 0.32 ‰ (2SD, n=27), is observed in a region where the excess of iron is attributed to the lithogenic fraction and not to a biogenic fraction or the formation of iron oxides (Section 5.1.). The excess DFe can only be explained by this excess lithogenic PFe. Although lithogenic iron is often considered refractory, neodymium isotope studies have shown that the lithogenic phases of the particles also dissolve (Rousseau et al., 2015). The dissolution of only 0.04 % of the PFe supplied by rivers would explain the excess DFe in the western region.

In order to incorporate these considerations into the revised version, and once again thanking the reviewer for encouraging us to clarify these very important points, we have added the following text to the revised version: "*Additionally, direct comparison of DFe and total PFe isotopic compositions has inherent limitations, as it does not allow us to study the distinct processes related to different PFe phases (lithogenic, biogenic, Fe oxyhydroxides). Those PFe phases are likely not equally involved in particulate - dissolved exchanges. Nevertheless, our analytical approach employs total digestion to guarantee the absence of artifactual isotopic fractionation. The regional geochemical characteristics establish that excess PFe is predominantly lithogenic (Figure 6), with excess DFe correspondingly derived from this source. Although lithogenic iron is conventionally considered refractory, neodymium isotope studies have shown that lithogenic phases do dissolve, even if in small proportions (Lacan and Jeandel, 2005; Rousseau et al., 2015). The excess DFe in the area, from 0.30 to 0.63 nmol.kg-1, affects 19.4 Sv (Germineaud et al., 2016). This leads to a required flux of ~540 g(DFe).s-1. The riverine PFe discharge to this area is about 1.2 x 1011 g(PFe).day-1. Therefore, the dissolution of only 0.04 % of the PFe discharge would be sufficient to account for the observed DFe increase. This very low dissolution rate makes it entirely plausible that the excess DFe originates from the dissolution of lithogenic PFe*." at lines 472-485.

Line 625 While I do not necessarily rule out such a long distance source of Fe, there is not a lot of evidence provided to support this linkage to the west coast of the Americas, given the shallow nature of the currents and complexity of the Fe cycle. For example, the d56Fe section from GP16 does not show any light iron reaching the equator. There could be other light source of Fe on route, one obvious example would be anthropogenic aerosols. I would suggest considering these ideas further. This idea might also benefit from comparison with GP15, which while unpublished is in the new IDP and you could reach out to the data generators – this has equatorial samples which on the face of it look like they might agree and help with interpretation.

Indeed, GP16 is a cruise that remains generally between latitudes 10°S and 15°S, so there are no values directly at the equator. At these latitudes, John et al., 2018 still present negative isotopic compositions near the Peruvian coast and at greater depth (approximately 500 to 1000 m) in the ESSW (Equatorial Subsurface Water). Although this signature does not appear to propagate westward at these latitudes, this can maybe be explained by the intensity of westward currents, with a maximum of 4.5 cm/s at these latitudes and a maximum of 14.5 cm/s at the latitudes of stations 1 and 3 of the EUCFe cruise (Cravatte et al., 2017). This may explain the long-distance transport of the light isotopic composition from the eastern Pacific OMZs, differentiated at latitudes 1°S and 13°S.

We consider it unlikely that atmospheric inputs are responsible for these light isotopic signatures at stations 1 and 3. Station 2 would likely have also exhibited a lighter isotopic composition in that case.

Moreover, the isotopic compositions of bulk aerosols in the Equatorial Pacific are primarily considered crustal, with positive isotopic signatures around +0.3 ‰ (Bunnell et al., 2025; Camin et al., 2025). For the reasons discussed in the paragraph above, we do not consider the "soluble aerosol" isotopic composition data from Bunnell et al., 2025. However, it is true that a negative value, -0.16‰, was measured in bulk aerosols at station 3 (Camin et al., 2025). This value is attributed to a potential point source of anthropogenic aerosols. Nevertheless, if the input were atmospheric, this light isotopic signature should be observed in the upper layers (Fig. 8, 9, Table 2), which is not the case at all. This light signature appears to be more closely linked to currents in this density layer.

Additionally and more importantly, the O_2 data are low, characteristic of OMZs. The light isotopic signatures are consistent with low-oxygen waters. We conducted a thorough study of ocean circulation and hydrological characteristics of water masses to understand the origin of these signals.

For all these reasons, we do not consider atmospheric sources as a potential explanation for these light signatures, but we have introduced more caution in our conclusions. We have added 'may' at line 709: "These three observations support the conclusion that their Fe content may originate at least partially from the Californian and/or Peruvian oxygen minimum zones (OMZ)," 'likely' at line 718: "and also a likely additional eastern source from the eastern Pacific oxygen minimum zones," and 'appears to be' at line 846: "This OMZ Fe source appears to be traced deeper within central waters (200–500 m depth).".

In our original submission, we already included some moderation in the abstract at lines 35-37: "At the same depth, bordering the EUC at 2°N and 2°S at 140°W, light isotopic signatures suggested that iron was originating from the eastern Pacific oxygen minimum zones," and in the conclusion, at lines 844-845: "However, an additional Fe source was identified bordering the lower EUC at 2°N and 2°S likely originating from the oxygen minimum zones (OMZ) of the eastern Pacific."

Regarding the integration of GP15 cruise data, indeed the data appear to be in very good agreement! We have modified Figures 1 and 5 to include the GP15 cruise. We have therefore modified the reference to the GEOTRACES Intermediate Data Product 2023 to that of 2025 at lines 326-327, 343.

Consequently, we have changed the sentence: "EUCFe data can also be compared with three nearby cruises: […], GEOTRACES GP15 (2018), a meridional section along 150°W with one station located at the equator and […]" at lines 318-320. We have added "GP15 and" at lines 324, 325, 327, 334, 336. We have also cited the authors of these data, namely M. Sieber and T. Conway, in the caption of Figure 5 and in the Acknowledge section at lines 912-915. We have changed 'two' to 'three' cruises at lines 318 and 332. To include GP15 data, we have modified the sentence: "This prevents the use of GP16 data and limits that of GP15 and GP19 to its their equatorial stations (stations 29 and 21, respectively).” at lines 324-325.

Citation: https://doi.org/10.5194/egusphere-2025-4525-RC3

In addition to the reviewer's comments, we noticed a missing “+” sign at line 783 (+0.3 ‰, in the PNG area).

We have also updated the data availability statement for the GEOTRACES Data Product. When we submitted this manuscript in September 2025, the GEOTRACES Intermediate Data Product 2025 was

not yet available; it was released in November 2025. Consequently, we have revised 'submitted to' to 'included in' at lines 244-246: "*All Fe concentrations, Fe isotopic compositions, temperature, salinity, oxygen data, and an intercalibration report have been included in the GEOTRACES Data Product,*" and changed 'will also be' to 'are' at lines 890-892: "*All data used in this article are reported in Table 2. Fe concentration and isotope data [...] are included in the GEOTRACES Data Product*".

We also realized that a clarification was missing for the percentage values in Figure A1 and Table A1, namely that these represent molar percentages. We have therefore added *"% mol·mol^{-1}"*at lines 291, 292, 353, 341, 342, 344, 345, 820, 875.

We also forgot to mention that Radic et al. produced data for station 28. We have therefore modified the caption of Figure 3 to include this information at lines 271-272: "*Stations 14 and 28 were was previously published by Radic et al. (2011) and stations 24, 28 and 30 by Labatut et al. (2014)*".

We forgot to mention SEC and NEC currents in the caption of Figure 10. We have added "*the North and South Equatorial Current (NEC and SEC)*" at lines 675-676.

We have standardized the notation to "*particulate–dissolved*" by removing "particle/dissolved" (line 781).

For clarity, we have added the following note to the caption of Figure 5: "*Please note that at 1000 m, values from stations 2, 21 and 29 are close, making station 21 less visible*" (lines 378-339). We have also added the acronyms *"TSW"* and *"STSW"* at line 159, added *"(Figure 4)"* at line 302 and *"(Figure 2)"* at line 682. Finally, we have added "*from an ocean margin*" in the conclusion at line 860.

We have modified the reference at line 419 from "Slemons et al., 2010" to "Slemons et al., 2012" and we added a reference to Slemons et al., 2012 in the conclusion.

In addition, the English has been edited by James W. Murray, a native English speaker and co-author of this manuscript. These modifications are visible in blue in the manuscript and do not alter the meaning of the sentences.