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.

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 The references in the into seem out of date, which much iron isotope data being published since 2015, and this introduction missing coverage of many key studies.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, $\Delta 56Fe_{DFe-PFe}$ = +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.