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 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	82.9	104.0	78.3	100.3

sources (% g.g-1)				
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