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Interactive comment on "Inputs and processes affecting the distribution of particulate iron in the North Atlantic along the GEOVIDE (GEOTRACES GA01) section" *by* Arthur Gourain et al.

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Dear reviewer, We would like to thank you for your very constructive comments. All the issues you raised were carefully considered and addressed. Below are our detailed answers, including corresponding lines of text in the revised manuscript. Note that we also took in account Dr Schlosser's comments when we rewrote the manuscript. We also attach the manuscript in track changes as a supplementary material. We hope that you will find our answers satisfactory and our revised manuscript suitable for publication in this special issue of Biogeosciences. Sincerely yours, Arthur Gourain, on behalf of all the authors

This manuscript presents the vertical distribution of particulate Fe, Al, Mn and P in the North Atlantic along the Geovide section. Particulate trace elements data are still very scarce, and this dataset constitutes a major contribution to our understanding of the biogeochemical cycles of these elements. I am aware that an important work has been done to acquire such a dataset (more than 500 samples!). We thank the reviewer for this comment. However, this manuscript is too detailed and the reader can be easily lost. It is difficult to retain clear conclusions from each section. Overall, I think that the discussion section is too ambitious, and the sections about the sources (e.g. dust inputs) and processes (e.g. remineralization) affecting the PFe distribution are sometimes too speculative. We rewrote the discussion in light of this comment and are more cautious with our conclusions. We removed the remineralisation section which was too speculative. The discussion could be improved by adding additional information/parameters collecting during the cruise (Chl-a, DFe, . . .), and a link between the particulate and dissolved concentrations is missing. The link between particulate and dissolved is made and discussed thoroughly in Tonnard et al. (under review for Biogeosciences), together with Chl-a data ; this is why it is not specifically included in the manuscript. More references to Tonnard et al. are included through the discussion. The main part of this study used the PFe/PAl ratio to quantify the lithogenic PFe fraction and deduce the non-lithogenic fraction. However, it is likely that this crustal signature is not constant over the Geovide transect. The relevance and limitations of using an unique ratio need to be discussed. The use of a single PFe/PAl crustal ratio is now discussed line 309. This work deserves to be published in Biogeosciences, but only after major revisions (see my comments below). Specific comments Overall, the introduction and methods are well written. Figures and tables are not enough used in the text to discuss the results. More references to figures and tables are included in the manuscript.

The results section should be shortened – describing the particulate concentrations station by station in is probably not the easiest way to present this dataset. I think the sections 3.2 to 3.10 should be merged and synthetized. This issue was also raised by Dr Schlosser. The results have been re-arranged following your advice, with a first section regrouping all open ocean stations, then a section on margins. In addition, the authors try to describe and explain each feature of the transect. It is probably too ambitious and not so useful. Finally, the size fractionation represents an important information. This aspect is not enough discussed in the manuscript. Regarding the size fractionation, we want to discuss it in a separate paper, which will be focused on the top 100m.

L33 – near-ubiquitous . . . but only in the western part of the transect. The sentence is confusing. The sentence was indeed not clear enough, we changed it by: "Within the Iberian Abyssal Plain, ratio of PFe over particulate aluminium (PAl) is identical to the continental crust ratio (0.21), indicating the important influence of crustal particles in the water column". Line 32. L36 – I would prefer to see a flux here instead of a concentration. A flux will be indeed more interesting but we can't measure a flux over our samples. We're lacking of a spatial resolution to calculate it. L61 – The term remineralization usually refers to PFe, not DFe. Indeed the formulation of the sentence wasn't clear enough, we changed it by: "or produced by remineralisation of particles". Line 61. L209-216 –I would remove this section (ms too long), and add one or two sentences with references in the discussion if needed. If this section is conserved, type 6 and 8-haptophytes should be explained. This section has been removed. Section 3.3 and 3.4 – A figure or table should be cited to help the reader. The Figure 3 is cited at the end of the overview section 3.2 as follow: "Data are shown in Figure 3". This figure includes all the parameters discussed along the following paragraphs. We are now citing this figure throughout this section L330-340 – I would transfer this paragraph in the Methods section. Done. It is now located in the section 2.5, line 166. Section 4.1 – This is an interesting approach. I am not sure if it is possible, but it would be very interesting to do such an analysis for two depth horizons, in surface (eg 0-100 m) and below 100 m. It could enable to highlight the vertical distribution of different processes (eg formation of barite mostly in the mesopelagic?). Indeed this could be interesting to perform and we had a go at it while preparing this manuscript. The main

issue we encountered with clustering our dataset by depth range is the loss of positive statistical results. The PMF model needs a lot of data to work properly and by using a small subset of samples, the model is unstable. L365 – A term is missing in equation 2. We modified it. L367-373 – I recommend here to indicate that a biogenic pool is likely present but is masked by the huge proportion of lithogenic PFe. Overall, PFe/PAl is a proxy and the interpretation should be done with care. An additional sentence has been added to explain how the proxies need to be used with care and a comment on biogenic influence has been added from line 309 to 315. L375 – Which feature? The dominance of lithogenic PFe discussed line 369 and 370? The feature described is the dramatic change of regime from station 26. We rearrange this paragraph in light of Dr. Schlosser review. Line 321. L375-383 – This paragraph is a bit confusing. In addition, why only atmospheric inputs are discussed here? We have reworded this paragraph, and added a discussion on the dispersal of Iberian margin rich particles. A similar comment was raised by Dr Schlosser. From line 316 to line 319. L414-416 – This sentence, and the whole paragraph seems to say that the Fe/Al ratio from the UCC used to calculate the lithogenic component is not accurate. I am aware that there is no perfect method to discriminate biogenic and lithogenic Fe and PFe/PAl is only a proxy, however this paragraph clearly contradicts the calculation made before. As it is one of the main objective of the paper, this limitation/bias should be discussed. Regarding this paragraph concerning the benthic inputs of particles, we discussed the different composition of sediments along the section. It is important to not consider sediments as a purely lithogenic source. Benthic sediments are the results of sinking of particles from the above water column. And represent in a certain term, a record of the oceanic particles flux. They are a mix of the overall bulk of particles lithogenic, biogenic and autogenic. Differences of ratio in these sediments are not implying in any way a change of ratio in the crust (continental or oceanic). L416-419 – I may be wrong, but I think that the PFe/PAl signature of the desert dust coming from the Sahara significantly differs from the UCC ratio. See Guieu et al. 2002, Fu et al. 2017, . . . The sentence referring to the aerosol inputs have been removed. L489 – Replace leaded by led. Done. L502-507 – Other data collected during the cruise could be used here to illustrate the intensity of the bloom. For example, what was the surface chlorophyll a concentration? I recommend to add this kind of information all along the text, it should help making the manuscript less speculative. We added the Chl-a concentrations corresponding to the bloom and refer to Tonnard et al. (2018) as the Chl-a data are discussed in this paper. e. L533-535 – What does an important phytoplanktonic community mean? It needs to be more precise. Furthermore, a low PFe concentration is not in contradiction with high Chl-a concentrations as usually most of the PFe concentration is from lithogenic or detrital origin and the biogenic pool is usually minor, and driven by intense cycling in surface. The sentence has been modified in light of this comment "The important phytoplanktonic community present (maximum Chl-a= 4.91 mg m-3, Tonnard et al., in prep), is linked to low PFe of 0.79 nmol L-1 at 10 m, but, with a high PFe/PAl ratio, up to 0.4, and PP concentration of 97 nmol L-1, confirming the biologic influence". Line 472. L536 – A value / order of magnitude is needed here. Furthermore, it has to be compared with the other areas. The sentence has been modified to: "Concerning this latter process, intense remineralization at station 77 (7 mmol C m-2 d-1 compared to 4 mmol C m-2 d-1 in the Western European Basin) has been reported by Lemaitre et al. (2018a),". Line 475. L537-539 – This sentence is confusing. We removed this sentence for clarity purposes. L557-564 – To reduce the length of the manuscript, I would remove this paragraph. We consider that it is important to briefly provide a definition of the benthic nepheloid layers so, to take the reviewer's point on board, we reduced the length of the paragraph as follows: "Benthic nepheloid layers (BNLs) are important layers where local resuspension of sedimentary particles (Bishop and Biscaye, 1982; Eittreim et al., 1976; Rutgers Van Der Loeff et al., 2002) occur due to strong hydrographic stresses (i.e. boundary currents, benthic storms and deep eddies) interacting with the ocean floor ((Biscaye and Eittreim, 1977; Eittreim et al., 1976; Gardner et al., 2017, 2018). Along the GA01 section, BNLs were observable in each province with different strengths (Figures 3 and 12).". Line 494. L552-554 – What did Lam et al. (2017) precisely show? Lam was describing the role

of physical characteristic on margin resuspension event. The use wasn't completely appropriate, we removed the sentence. Section 4.3.2 – Here, I cannot see a clear conclusion. We added the following sentence: "Along the GEOVIDE section, BNLs are providing high concentrations of particulate trace element in the deep open ocean that can contribute substantially to the pool of particulate trace elements such as as iron.", Line 516. L586-601 – This paragraph is probably too long to conclude an absence of hydrothermal inputs. The first part of the paragraph has been removed to shorten the paragraph. L604 – I can't see these information on Fig. 7. This paragraph has been removed as explained in the answer of L604-605 comment. L604-605 – PFe/PAl is higher at station 40 than at station 38. Indeed, in light of it, we decided to remove this paragraph due to the lack of significate proof to support this part of the discussion. L605 – This a general comment for the whole text: "PMn had a 19% sedimentary origin". The authors refer to a proxy, and should say "about 20%". This paragraph has been deleted as explained previously. Moreover we've been more careful on the use of proxy over the entire manuscript. L616-617 – See my previous comment (L416-419). We removed the sentence in question. Section 4.3.4 – Here, there is no clear conclusion. I would recommend to remove this section. We want to keep this paragraph about atmospheric inputs. Even if the fact we do not observe any atmospheric deposition is not as interesting as huge deposition events. We think it is important to discuss it, even if the conclusion is not as clear as the other sources. L643 – A range of Fe/P cell quotas has been reported for the North Atlantic (see Twining et al.). It would be interesting here to compare this ratio (assuming 100% of P is from biogenic origin) which gives an estimation of the biogenic PFe in surface with the 100% lithogenic PFe obtained at stations 1-26 using equation 1. This comparison could help to discuss the limitations of such approach. This paragraph has been deleted. In light of the reviewers' comments, we decided it was too speculative using the current dataset at our disposition. L638-641 – This sentence needs a reference. This paragraph has been deleted. In light of the reviewers' comments, we decided it was too speculative using the current dataset at our disposition. L646 – Replace pelagic by mesopelagic. This paragraph has been deleted. In light of the reviewers' comments, we decided it was too speculative using the current dataset at our disposition. L649 – How is defined the remineralization depth? It needs to be explained. This paragraph has been deleted. In light of the reviewers' comments, we decided it was too speculative using the current dataset at our disposition. L648-650 – PFe/PAl is probably not the best parameter to discuss remineralization since both elements are mostly lithogenic and the variation of this ratio due to remineralization is likely negligible. This paragraph has been deleted. In light of the reviewers' comments, we decided it was too speculative using the current dataset at our disposition. L650-651 – I am not convinced by this explanation. PP is much more labile than PFe, whatever the remineralization rate. In addition, Fe scavenging could also contributes to this increase in PFe/PP. This paragraph has been deleted. In light of the reviewers' comments, we decided it was too speculative using the current dataset at our disposition. L652-653 – The authors should explain why scavenging starts to be important only below 600 m depth. This paragraph has been deleted. In light of the reviewers' comments, we decided it was too speculative using the current dataset at our disposition. L654-659 – This paragraph is confusing. Figure 13 is not introduced and explained. In addition, how the authors conclude to a stronger scavenging of DFe? This paragraph has been deleted. In light of the reviewers' comments, we decided it was too speculative using the current dataset at our disposition. L661-664 – It is surprising to see a lower remineralization rate for P compared to Fe. This finding should be discussed. In addition, PFe/PP is not presented in a figure and it is hard for the reader to follow the discussion. This paragraph has been deleted. In light of the reviewers' comments, we decided it was too speculative using the current dataset at our disposition. Section 4.3.5 – Overall, this section is too speculative. The potential impact of the scavenging process is not really discussed, and I think that the use of the PFe/PAl ratio to discuss the different remineralization patterns is not relevant (eg the evolution of DFe would be more appropriate). Finally, it is not easy to draw any clear conclusions form this section. This paragraph has been deleted. In light of the reviewers' comments, we

decided it was too speculative using the current dataset at our disposition. Figures 13 and 14 – These figures are not introduced and discussed in the manuscript. I would remove them and cite the appropriate study instead. This paragraph has been deleted. In light of the reviewers' comments, we decided it was too speculative using the current dataset at our disposition.

Please also note the supplement to this comment:
https://www.biogeosciences-discuss.net/bg-2018-234/bg-2018-234-AC2-supplement.pdf

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