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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.

Anonymous Referee #2

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This manuscript present 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!). 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. The discussion could be improved by adding additional information/parameters collecting during the cruise

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(Chl-a, DFe, . . .), and a link between the particulate and dissolved concentrations is missing. 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. 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. 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. 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.

L33 – near-ubiquitous . . . but only in the western part of the transect. The sentence is confusing. L36 – I would prefer to see a flux here instead of a concentration. L61 – The term remineralization usually refers to PFe, not DFe. 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. Section 3.3 – A figure or table should be cited to help the reader. Section 3.4 – Once again, Fig. 3 should be cited to help the reader. L330-340 – I would transfer this paragraph in the Methods section. 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?). L365 – A term is missing in equation 2. 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. L375 – Which feature? The dominance of lithogenic PFe discussed line 369 and 370? L375-383 – This paragraph is a bit confusing. In addition, why only atmospheric inputs are discussed here? 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. 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, . . . L489 – Replace leaded by led. 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. 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. L536 – A value / order of magnitude is needed here. Furthermore, it has to be compared with the other areas. L537-539 – This sentence is confusing. L557-564 – To reduce the length of the manuscript, I would remove this paragraph. L552-554 – What did Lam et al. (2017) precisely show? Section 4.3.2 – Here, I cannot see a clear conclusion. L586-601 – This paragraph is probably too long to conclude an absence of hydrothermal inputs. L604 – I can't see these information on Fig. 7. L604-605 – PFe/PAl is higher at station 40 than at station 38. 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%". L616-617 – See my previous comment (L416-419). Section 4.3.4 – Here, there is no clear conclusion. I would recommend to remove this section. 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. L638-641 – This sentence needs a reference. L646 – Replace pelagic by mesopelagic. L649 – How is defined the remineralization depth? It needs to be explained. 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. 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. L652-653 – The authors should explain why scavenging starts to be important only below 600 m depth. 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? 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. 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. Figures 13 and 14 – These figures are not introduced and discussed in the manuscript. I would remove them and cite the appropriate study instead.

References Fu, Y., Desboeufs, K., Vincent, J., Bon Nguyen, E., Laurent, B., Losno, R., & Dulac, F. (2017). Estimating chemical composition of atmospheric deposition fluxes from mineral insoluble particles deposition collected in the western Mediterranean region. Atmospheric Measurement Techniques, 10(11), 4389-4401. Guieu, C., LoÿeâĂŘPilot, M. D., Ridame, C., & Thomas, C. (2002). Chemical characterization of the Saharan dust endâĂŘmember: Some biogeochemical implications for the western Mediterranean Sea. Journal of Geophysical Research: Atmospheres, 107(D15),

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