Overview:

This paper presents trace metal data from the Mauritanian shelf and places the observed distributions of redox elements in the context of O_2 control. The trace metal data look to be excellent, though the flux calculations need some further explaining so the reader can follow them through every step that was made. Overall the arguments presented for O_2 as a control in this dynamic environment are very weak and instead the data points more to the role of scavenging, aerosol deposition, resuspension and cross shelf transport. Just because it is in an oxygen minimum zone does not mean that oxygen controls the distribution of redox elements, as the data looks very similar to that from most coastal shelf regions. The paper at present is overly long and should have been edited down before submission to a more concise set of sections concentrating on the main processes as there are several sections (e.g. 3.4.2) that could summarize most of the relevant information to a simple paragraph with inclusion of a summary table. A substantially revised manuscript focusing on the key findings for which there is evidence would likely have significant impact in this field.

General Comments:

Contradictory vertical flux information: The main problem with the paper at present is with the vertical flux calculations as the repeat station data is analysed in an Eulerian framework with the concept that it is the same water mass that is being sampled at the same site several days later and horizontal advection is ignored. Yet the paper clearly states that the changes seen between repeat samplings was from an advective inflow (Thomsen *et al.*, 2018) and that this regions is known for its high current velocities in summer (Klenz *et al.*, 2018). While I understand what the authors are trying to link their data with changes in O_2, in the absence of a Lagrangian framework this makes no sense as we are left with comparing two snap shots of completely different scenes that just happened to be at the same spot.

The description of the flux calculations is also not easy to follow and it is unclear exactly what depth ranges are being considered, is it just into the mixed layer or is it over the entire depth range. The strong point is the combination of microstructure profiles and trace metal concentrations to estimate robust values for the diffusive flux. The advective fluxes however are not well constrained at all as the methodology clearly has some problems; firstly close to the coast, scatterometer winds are not reliable due to the masking that takes place close to the coast in the data analysis, so some explanation of how this was taken into account needs to be provided, secondly the

At the time this work was performed along the Mauritanian coast the area that was sampled has typically little or no upwelling present (Cropper *et al.*, 2014; Tanhua and Liu, 2015; Varela *et al.*, 2015). In the present work the authors chose to use the same wind based approach as that used earlier by Steinfeldt et al. (2015) in as study based predominantly on He isotopes. In that study they found upwelling during the summer months (M68-3) though the error bars are quite large $2.4 \pm 1.5 \times 10^{-5}$ m s^{-1}. Steinfeldt commented on the differences between their work and Tanhua and Liu, noting that the latter's data set only contains a few stations along 18 N, while their coastal stations along that line also have low vertical velocities (between 0 and 2×10^{-5} ms^{-1}), with much higher upwelling velocities to the north. It is not to say that there wasn't upwelling at this time, just that it needs to be better explained and put into context, indeed support for upwelling at this time comes from comparison to a companion paper also in BGD at present (Thomsen *et al.*, 2018) but the authors don't make mention of that work except for the low oxygen resuspension events. The present paper would clearly benefit from linking more to the Thomsen et al. (2018) work and also to Yücel *et al.*

(2015) work carried out along the same transect as both of those papers show the inherent variability in dissolved parameters in the bottom waters of this region.

The physical oceanography presented in the manuscript would also benefit from consideration of recent modelling studies on upwelling from other EBUS (Jacox and Edwards, 2011, 2012; Lentz, 1992; Lentz and Chapman, 2004; Lentz and Fewings, 2012; Messié and Chavez, 2015). The studies listed here show how the Burger number (Lentz and Chapman, 2004) with its dependence on the shelf slope and buoyancy frequency were important in understanding upwelling in such regions. Inclusion of this information would then also give the current paper more impact as it would be applicable to other EBUS as well. There are also a number of recent papers on the role of filaments and particle fluxes in this region (Bory *et al.*, 2001; Fischer *et al.*, 2009; Iversen *et al.*, 2010; Rees *et al.*, 2011) which also could be useful for interpreting the results found in the current work.

O_2 as a sole control on metal abundance: This work tries to suggest that changes in metal concentrations in the water column are due to changes in O_2 concentrations but the O_2 values observed are not low enough to cause significant changes in Fe, Mn or Al redox speciation so it is more likely that the observed changes are related to resuspension events in this very dynamic mixing environment (Schafstall *et al.*, 2010). Resuspension has been known to be a major control on dissolved iron and other metals in shelf regions for some time now (Croot and Hunter, 1998; de Jong *et al.*, 2012; Elrod *et al.*, 2004; Johnson *et al.*, 1999) and this would also impact other hydroxide dominated elements (e.g. Al, Th and to a lesser extent Mn). Colloidal species are also likely important here (Moran and Moore, 1988, 1989; Schlosser and Croot, 2008; Schlosser *et al.*, 2013).

Why is O_2 unlikely to be a control on trace metals in the Mauritanian shelf? Simple - the shelf region here is typically described as being well oxygenated in the sediments (Gier *et al.*, 2016) with the exception of the extremely shallow parts of the Banc d'Arguin (Duineveld *et al.*, 1993; Kock *et al.*, 2008; Schafstall *et al.*, 2010). The only real sediment work I am aware of is that from Nolting et al. (1999) and Gier et al. (2016) and these took place mostly in oxygenated sediments it appears. That O_2 plays a role in the release of such metals is well known (Homoky *et al.*, 2012; Severmann *et al.*, 2010), but iron is only released reductively when O_2 is completely depleted (Sundby *et al.*, 1986). The Severmann et a. (2010) work shows a nice relationship between Fe fluxes and O_2, with the large caveat that the landers used there likely consume all the O_2 in the sediment (though not all of the oxygen in the water in the chamber) before the Fe efflux begins, but then even at the O_2 concentrations found on the Mauritanian shelf the flux they predict is not lot large compared to what could be generated by resuspension – additionally intense mixing tends to keep oxygen levels higher lowering the flux of reduced iron but increasing that from resuspension. Interestingly similar iron fluxes to the Mauritanian are seen in the well-oxygenated waters of the Ross Sea (Marsay *et al.*, 2014) indicating the role of resuspension dominates and not oxygen.

A previous study in the Mauritanian upwelling along almost the same transect (Schlosser and Croot, 2009) indicated that iron solubility was apparently controlled by remineralization as it varied with O_2, pH and phosphate concentrations. While no measurements of Fe solubility were made in the present work it does raise the question of whether pH may also be a control on the distribution of metals seen in this study but which has not been considered here. Although the PCA analysis made in this work would suggest that the resuspended elements (Fe, Al, Mn) were not related to the remineralization indicators.

The importance of scavenging on dissolved metals is mentioned in the introduction but after page 3 it does not come back into the text. Given that there is high particle numbers across this transect , either from the high productivity in surface waters, dust deposition, or from benthic resuspension

events it is amazing that this process is not invoked as a control on dissolved metal concentrations in the discussion. The turbidity data indicates that particles are likely important, but this is not discussed in any context in the manuscript. If O_2 is the control that the authors say it is then they should at least follow through from their introduction and examine the role of scavenging on the scavenged elements.

Atmospheric flux estimates: Using dissolved Al concentrations to estimate the aerosol flux of the other elements is complicated as while there is good data on the elemental composition of the Saharan aerosols (Fomba *et al.*, 2013; Kandler *et al.*, 2011; Müller *et al.*, 2010) what is missing is reliable fractional solubility data (Baker and Croot, 2010) for all of the elements that represent dissolution in seawater and residence times for all of these elements in the surface ocean. The residence time section in the manuscript is very interesting and could be highlighted and compared more to other works from the same region (Croot *et al.*, 2004; Dammshäuser *et al.*, 2013; Jickells, 1995; Jickells, 1999) as presently this aspect of the work is under developed. In the present work the residence time for Al is most likely on the order of 3-6 months given the large inputs of Al from the Saharan dust or indeed it may be much shorter given work on iron in the same region (Croot *et al.*, 2004).

In the present work the atmospheric deposition is not well presented as it does not discuss the role of aerosol solubility in the flux calculations and does not distinguish categorically between the total aerosol flux and what it terms is the dissolved flux – which as the deposition is predominantly dry means that it is metals that are solubilized post deposition. It would help to include at the very least satellite data on the atmospheric aerosol loading to pinpoint if there was dust deposition immediately before or during the occupation of these stations.

Sections 3.5 and 3.6: These sections are long and don't add that much to the paper at present, as they read like an earlier draft of a PhD thesis and they should be shortened and tightly focused on the main points that are supported by the data. The central problem with these sections is that two repeat stations are interpreted mostly in the context of O_2 and not simply resuspension.

Geochemical data from the NW African sediments: I was a little surprised that there was little discussion of the geochemistry of the sediments and then the relationship between the dust and water column compositions. A further paper that could be included is that of Itambi et al. (2010) who examined the magnetic mineral inventory of the sediments off Senegal.

Other particle data for the North Atlantic: I was surprised not to see any comparison to the particle data of Kuss and coworkers (Kuss and Kremling, 1999a, b; Kuss *et al.*, 2010; Scholten *et al.*, 2001; Waniek *et al.*, 2005) as while it is only near surface data it does cover a similar transect and include time series data from sediment traps. There is also other recent trap data from close to the Canaries which gives more seasonal data (Brust *et al.*, 2011; Brust and Waniek, 2010).

Specific Comments:

P1 Line 20: What evidence is provided here that dust deposition did not play a role?

P1 Line 23: How were the DFe atmospheric fluxes estimated? As it is most likely dry deposition so how was the dissolution assessed?

P5 Line 19: What was the purpose of the 2nd Mn cartridge if it was not analysed? (P6, Line 24)

P5 Line 20: Please include information on the flow rate through the pumps.

P6 Line 19: The reagent is Lumogallion not Lumogallium.

P6 Line 20: Lumogallion

P6 Line 24: Please include information about the methodology that was used for these measurements and the basic operating conditions.

P6 Line 30: The iodide data is not presented so the reader can not assess what the relevance of this parameter is. For instance, was the iodide concentration ever more than what might be expected for total iodine at the salinity of the seawater as this would be a good indicator of excess iodine from a benthic source.

P7 Line 4: Were the Turbidity and Chlorophyll sensors calibrated during this expedition? Was the Turbidity sensor zeroed between CTD casts?

P7 line 9: So the decay was assumed linear for all stations, no matter the depth?

P7 Line 17: How was the tap-water assessed to be Ra free? Was it provided from the ship's system as they can be notorious for Ra contamination. More details need to be provided here.

P7 Line 24: How was the ^{228}Th assessed? Was it only from the background counts after 3 weeks when the initial ^{224}Ra had decayed?

P8 Line 2: Concentration difference between which levels? At times it seems it is between 8-29 m (P 8, Line 8) and at other times it seems it is over multiple levels (P9, Line 15)? This section of the methodology needs to be better explained.

P8 Line 4: How valid is it to ignore lateral fluxes in a region where there are strong filaments and cross shelf transport (Fischer *et al.*, 2009; Gabric *et al.*, 1993; Klenz *et al.*, 2018; Rees *et al.*, 2011; Schafstall *et al.*, 2010)? Indeed the recent paper by Klenz et al. (2018) shows that at this time there are very high current velocities along the 18 ° N line.

P8 Line 14: How close to the bottom did the microstructure profiles get to? Typically they do not go into the benthic boundary layer for fear of doing damage to the probes if they hit the sediment. Information on how close to the bottom the sensors got would be extremely helpful in assessing whether or not the fluxes from the sediment were well constrained. Similar information for the trace metal sampling should also be supplied.

P9 Line 5: I am missing a step here as this website seems to provide only wind speed data, so how was the alongshore wind stress calculated? The number seems reasonable given global compilations (Varela *et al.*, 2015) but what are the uncertainties? There is a NOAA site that calculates everything https://www.pfeg.noaa.gov/products/PFEL/modeled/indices/upwelling/NA/how_computed.html

P9 line 5: How was the wind close to the coast assessed? For ASCAT wind velocities closer than ~70 km (25-km products) or ~35 km (12.5-km products) from the coast are flagged because of land contamination. This is due to the fact that - in the case of the 12.5-km product - backscatter measurements (σ0) of up to 35 km away from each WVC centre are used in the spatial averaging. It would appear that station 4 would be too close to the coast to use an ASCAT wind product, so some explanation needs to be provided here.

P9 Line 6: These estimates of the upwelling velocity contradict the findings of Tanhua and Liu (2015) and others (Cropper *et al.*, 2014), who reported that there was no upwelling over the summer, the same season when this expedition took place. The upwelling flux estimates

listed here are also close to the maximum rates estimated using tracers by those authors for periods when the upwelling is active so some explanation is required here – see the general comment above regarding this.

P9 Line 12-17: The explanation for the flux estimate being made here needs to be better described, as for most readers usually the vertical advective flux would be the velocity times the concentration and would not involve the gradient in the concentration as shown on line 17 here. It needs to be explained then why you use the gradient approach and which way around the gradient is (e.g. based on the equation as it stands you could have upwelling but still have a net negative flux from the surface waters if the metal concentration gradient is negative, this would happen when there was a higher metal concentration in the surface waters than below).

P10 Line 8: So horizontal advection is at its greatest in summer, but horizontal fluxes are not considered in interpreting the trace metal and O_2 data?

P11 Line 2: Though it should be noted they are an order of magnitude or more lower than the Celtic Sea and the St Lawrence seaway.

P11 Line 21: Other studies in the same region have pointed to the role of CDOM and reactive oxygen species in potentially controlling metal distributions (Heller *et al.*, 2016; Wuttig *et al.*, 2013). Indeed Heller et al. (2016) observed changes in FDOM consistent that could be consistent with microbial respiration of resuspended sediment material, a process also invoked by Thomsen et al. (2018) to explain low oxygen patches in these waters.

P11 Line 17: If Thomsen et al. (2018) indicate this was from horizontal advection then this would invalidate the approach for estimating the vertical fluxes – see the general comment on this above.

P12 Line 4: The Hawco *et al.* (2016) paper is for the Pacific and a much more O_2 depleted water column with sulfidic sediments so a very different environment.

P12 Line 8: The values are also similar to values found by Wuttig *et al.* (2013) for the Tropical North Atlantic and near to coastal West Africa.

P12 Line 14: Of direct relevance to this work is the result that oxygen has also been shown to have a strong impact on Co speciation in the Tropical North Atlantic (Baars and Croot, 2015). This work should therefore be included in the discussion of the present data set.

P12 Line 26: Mn data is also found in the work of Wuttig *et al.* (2013) and this work is again of direct relevance to the current paper with regard to the relevance of reactions with oxygen.

P12 Line 35: Please define LOQ here and what its value is.

P13 Line 24: Is there any evidence for sulfide release from the sediments int his region (see the general comment on this above also)?

P13 Line 36: Did you encounter MOW in this work?

P14 Line 27: It is worth noting here that dilution does not impact the ratio of the two radium isotopes. Though the assumption is that there is only a single uniform benthic source.

P14 Line 29: Radium is conservative the other elements are not – resuspension and scavenging are the most likely controls as the O_2 concentration is not low enough to impact iron or manganese.

P15 Line 7: This location is not on the 18° N transect so is it a source region to the south which is then advected north as you suggest so again the horizontal fluxes seem to dominant here.

P15 Line 15: How was the PCA performed? Were the data all normalized to have a mean of 0 and a standard deviation of 1 before analysis?

P15 Line 18: O_2 and AOU are linearly related so they would be expected to be orthogonal to each other in the PCA – this is ok though as only the PCA components need to be independent.

P16 Line 7: What evidence is there that the sediments are anoxic? Sediment work performed along the same transect line shows no evidence for this (Gier *et al.*, 2016). See the general comment on this above.

P16 Line 24: This assumes that all of the dissolved iron is from remineralization and not from dust dissolution (Baker *et al.*, 2013; Baker and Croot, 2010; Baker *et al.*, 2006a; Baker and Jickells, 2006; Baker and Jickells, 2017; Baker *et al.*, 2006b), given the proximity to the Sahara and the impact it is on Aluminum concentrations this seems a contradiction. There is also the potential for direct remineralization of Saharan dust by zooplankton (Barbeau *et al.*, 2001; Barbeau and Moffett, 2000; Barbeau *et al.*, 1996; Laglera *et al.*, 2017; Schmidt *et al.*, 2016).

P17 Section 3.4.2 – See the general comment about the atmospheric fluxes.

P19 Line 6: See the general comment above regarding residence times.

P19 Line 32: This would indicate it is being scavenged. There is no discussion of the role of scavenging in this manuscript after the introduction. Surely this deserves some attention given it is likely a major control on dissolved metal concentrations.

P20 Line 11: Closest to the shelf is also likely to be the most impacted by the ASCAT limitation near the coast so it will have to be explained if this value is far enough away from the coast. The station 4 advective flux is also an order of magnitude more than any of the other stations so some indication of why this is needs to be supplied as the reader does not see the distribution at station 4.

P20 Line 16: Has anyone checked the sea surface height data? Were there any mesoscale eddies around at this time (Karstensen *et al.*, 2015)?

P20 Line 32: Though most of the sediment is derived from Saharan dust and biogenic material, so you can't escape the dust entirely as a source.

P21 Line 15: What does an NTU represent here? It has not been defined in the paper anywhere. This could simply be background noise of an uncalibrated instrument as figure 7 seems to show 0.2 NTU most of the time.

P21 Line 19: Reductive dissolution is occurring in the oxygenated waters here?

P22 Line 17: This is assuming that it is the same water mass being sampled, but early we are told that this is water that came from the south, so it clearly isn't and thus trying to argue that this is a temporal trend in the same water is flawed. At the very least you could at least show the data on the same density surfaces. See the general comment on this above.

P25 Line 14: You have shown the variability in a shelf system but no evidence that this is controlled by O_2 at this location. The recent paper by Schlosser *et al.* (2018) did however show the impact of a sulfidic event on trace metal concentrations in the water column so that is an example already.

P26 Line 4: FW are not the initials of one of the authors, so we don't know who this is until we read later in the acknowledgements.

Figure 3: There is no iodide data shown, yet it was used in the PCA and Radium was not.

Figure 3: The radium data shows considerable 'young waters' in the near surface offshore, some more explanation is needed for where this might have contacted the sediments last. Or is it an artefact of the contouring in R as it is hard to see the data points.

Figure 6: suggest you use a different symbol for the leachable as when they overlap it is hard to tell which is which when they are all the same symbol.

Figure 7: As above it would be easier to read if different symbols were used.

Figure 7: The oxygen and turbidity anomalies mentioned seem to be related as mentioned in the text but the turbidity is more gaussian shaped while the oxygen has a bimodal distribution. It would have been more useful to have plotted all of this in density space so that we could see if the watermasses were similar as the T/S properties look quite different but the symbols are so large that this information is lost.

Figure 8: Different symbols between deployments would help. For station 8 and the dissolved Cd it is impossible to see if the data in the deep is the same or there was only 1 time that you sampled the deep waters.

Figure 8: Why the phosphate depletion at 150 m at station 8? It seems there is also data missing from above and below that point – any explanation why? Is this data point anomalous then as it is does not appear to be oceanographically consistent?

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