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Interactive comment on "Aluminium in the North Atlantic Ocean and the Labrador Sea (GEOTRACES GA01 section): roles of continental inputs and biogenic particle removal" *by* Jan-Lukas Menzel Barraqueta et al.

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General comments

This manuscripts presents a high resolution dataset for aluminium of good quality from a region that thus far was not studied in such detail. The observed dissolved distribution is discussed with respect to the particulate fraction and potential sources, sinks and internal cycling via scavenging and remineralisation. The main finding is that the distribution of Al in this region is governed by the complex interaction between nu-

merous sources and sinks. Sources are atmospheric deposition (mainly in the south), continental run-off (incl. glaciers), sediments and Mediterranean water. Hydrothermal activity did not seem to make significant contribution in this region. The main sink is scavenging on particles, notably in the surface and near surface ocean, where biogenic silica seems to be the main carrier, but scavenging can also occur on resuspended sediments, making the latter both a source and a sink. The cycle of scavenging of Al in the (near) surface ocean and remineralisation at depth is nicely complemented by particulate Al and phytoplankton community composition data for the surface ocean.

While the manuscript is generally well written, there is definite scope for increased clarity as indicated for specific sections below. Most importantly, while the individual subsections are detailed and the division in the different sections is a logical choice at first glance, the subsections are insufficiently linked. For example section 3.3.1 deals with remineralization and scavenging in the subsurface ocean, but is presented stand alone from section 3.2.2 that deals with the equilibrium between dissolved and particulate Al in the euphotic zone. Obviously there is a connection between these two sections, but currently this connection is not discussed in the manuscript. Additionally, in section 3.3.1, increasing concentrations are discussed with respect to remineralisition, but water mass circulation (e.g. section 3.4) or sediment resuspension (section 3.4.2) as sources are not discussed here whereas these processes could play an (important) role in explaining the increase with depth and potentially the observed correlation.

As is, the manuscript is divided between the surface and deep ocean, but this is not always clear or appropriate. Probably improved headings combined with specific mention of the depth regions in the text and, more importantly, discussing similar processes in the deep and surface ocean together will remedy this. A stronger focus on linked processes rather than ocean regions or depth intervals would, in my opinion, improve this manuscript and allow for a more in depth discussion.

Overall, I would recommend this manuscript for publication in Biogeosciences after some moderate revisions. The data is of good quality and the interpretations are of interest to oceanographic community, but the discussion should be improved as detailed above and in the specific comments below.

Specific comments

Methods

Page 3 line 28 why were two different filters and pore sizes used and is there a difference between them and the Al fraction analysed?

Page4 line 22 Brown and Bruland used a 4 M buffer. Is there a reason for changing the concentration?

Page 5 line 5 What is the effect on the sample pH when adding 3 times the required amount of buffer and the subsequent pre-concentration? I find the blank determination slightly odd. If I understand it correctly, 'regular' samples are buffered online, but for the blank determination a sample was buffered offline with varying amounts of buffer. Additionally an acidified MQ sample was analysed without buffer (so I assume the system had to be modified, i.e. the buffer line was removed?). Wouldn't it be far easier and more representative to analyse an acidified MQ sample as a regular sample? With the current approach, any blank resulting from the online buffering (if any) is not accounted for.

Line 8 the average blank and standard deviation should be reported

Line 17 I would say similar analytical techniques in different labs and cite the paper (Rolison et al., 2015) for the other dataset. Here it should probably also be pointed out that one dataset was analysed shipboard, the other after storage for some time in a shore based lab and that there appears to be no difference between the approaches (At the GEOTRACES website there is a cautionary note accompanying the SAFe reference sample results warning there could be Al contamination from bottle caps during storage).

Page 7 line 25 room for clarification here; in the previous paragraph decreasing con-

centrations from eastern to western basins were described, so I guess the point here is that there is no significant difference? Also the decrease would not only be expected in the east-west direction, but also in the south north direction (away from Saharan dust source).

Section 3.2.2 What stations are used in the calculations? Probably the coastal stations should not be used here as particle concentrations and compositions here are influenced by continental and sediment sources as discussed in subsequent sections. Additionally, this section warrants some further discussion on the use of Al as a dust tracer, as this study implies the dissolved Al concentration is not only dependent on atmospheric deposition, but also the presence of diatoms.

Section 3.2.3 I'm not 100% convinced based on the current discussion the elevated Al is associated with river outflow; how can one be sure e.g. it is not all wet deposition or that sediment resuspension on the shelf doesn't play a role too (after all, a sediment resuspension source is argued as a significant source in section 3.4.2). Was there a correlation between dAl and salinity or other tracers of fluvial input (as for example observed in the Drake Passage for Al input associated with land run off (Middag et al., 2012))? The authors have the data to discuss this in more depth and to discuss the importance of fluvial input vs deposition and resuspension (the Al could be effectively removed but later re-suspended as suggested for the Californian shelf for Fe by Bruland and co workers).

Section 3.2.4 I find the argumentation in this section a bit shaky as detailed below. I do not disagree with the point reached, but the argumentation needs to be improved

Line 7-10 it is probably worthwhile to mention these endmember estimates are conservative for dAl as they do not incorporate any Al scavenging or precipitation. Is that also the reason for pooling the dAl and pAl later in the paragraph when discussing endmembers?

Page 10 Line 10 I do not see how samples from a fjord and an iceberg are representative for glacial runoff and sea ice melt. Sea ice melt will be completely different (see also next comment) and the concentrations in the fjord will not only depend on run-off, but also the interaction with sediments and (biogenic) particles in the water column

Line 12-13 why would sea ice Al concentrations be more similar to ice berg than glacial run off? Ice bergs were once part of the glaciers, so wouldn't one expect that ice bergs and glaciers are quite similar and sea ice very different from those two?

Line 14-15 the comparison between total dissolvable and dAl+pAl is not valid in my opinion, the total dissolvable is a 'gentle leach' at pH 1.8 whereas the pAl analysis is a complete destruction. section 3.3 title of this section could be improved, section 3.2 has an informative title and deals with surface water, what is this section about and what distinguishes it from section 3.2 (as that section also deals with spatial distributions)

Section 3.3.1 I think this section should be better linked to section 3.2.2. Is there anything that can be learned from (changes in) the particulate phase in deeper waters in this region? And an increase with depth is not only related to local (vertical) processes, but also processes during advection of deep water masses and mixing of water masses with different pre-formed concentrations. The latter should also be explored/discussed as a potential driving factor behind the observed correlations. Notably in the next section, sediment re-suspension is discussed which also lead to increased concentrations at depth so a better linking of section 3.3.1 with 3.4.2 is also warranted.

Section 3.4.2 Page 13 Line 4-5 how does a positive correlation between pAl and salinity indicate the salty MOW is depleted in pAl?

Line 9 Do we need another tracer for MOW? Conventional tracers such as salinity that are far easier to measure than Al seem to work well, so what is the benefit of the relatively expensive and challenging parameter Al?

Line 28 were there more stations on the shelf without elevated concentrations? Currently not clear. Page 14 line 3-8 not directly clear station 78 is on the shelf and how

deep the seafloor is in relation to the mentioned 140 m. And how does one explain the enhanced pAl levels in the absence of enhanced dAl? (discussed later I noticed, maybe move this discussion forward or mention it will be discussed later)

Section 3.4.3 this section is very brief. Perhaps some discussion as to why enhanced dAl has been observed at two hydrothermal locations (one very close by) but not at this location or numerous other active hydrothermal vent sites?

Technical corrections

abstract last sentence of abstract is confusing, had to read it several times, please rephrase (possibly two sentences) for increased clarity

Page 2 line 6 from nanomolar to

12 into the ocean

26 want is meant by active scavenging

Page 5 Line 19 rephrase as readers are not all familiar with this test, e.g. 'A Fisher based test to compare vertical profiles as described by Middag et al. (2015) was use to. . .. . . This test calculates an integrated p-value as an objective metric to determine how far two profiles are consistent between each other within a given depth interval. This test determined. . ...

Page 5 Line 28 mention were this will be explored

Page 8 line 3-5 citations needed

Line 12 how would scavenging of Al onto other particles explain the increased pAl/dAl ratio in regions were diatoms were dominant?

Line 14 what is meant by a 'high correlation'?

Page 9 line 9 here and throughout the ms, refer to transect (GA04N) rather than cruise 64PE370 to be consistent with references to other transects

Page 10 line 1 rephase e.g. 'had coefficients of determination above R2=0.89'

line 28 'concentrations was' singular plural mismatch

Page 11 line 10-11 the ranges in dAl indeed overlap, but the medians are quite different so quoting the medians here is confusing

Page 12 line 27 elsewhere in the ms salinity is used rather than S (I prefer salinity)

Page 13 line 1 core between 1000 and 1200 does not correspond to depth of Al maxima mentioned just previously

Line 23 why z (not defined) rather than depth?

Fig1a what is the red square on Greenland?

Fig3 name the land masses on the map for those not familiar with this region

Fig 4 caption is not clear, what is fig3 in the manuscript?

Fig 5 caption, wouldn't 'around the southern tip of Greenland' be more appropriate than 'around SE and SW Greenland'?

Table 1 number of significant figures is inconsistent and seems inappropriate. Uncertainties on the slopes and intercepts should be reported (and hence for the endmember estimation)

Fig 6 b Was this plot was made in ODV? Caption implies this

Fig 7 number of significant figures does not seem appropriate and why now GA01 rather than GEOVIDE? And why is the neutral density window reported in the legend for GA01 data and not the others, was it different? This figure could be made to look better if made in another programme (same for fig 8).

Fig 8, why not station 78 as shelf station?

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