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Interactive comment on "Latitudinal patterns in the concentrations of biologically utilised elements in the surface ocean" *by* Daisy Pickup and Toby Tyrrell

Anonymous Referee #1

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This manuscript about latitudinal patterns of trace elements is generally well written. It is based on existing data from the GEOTRACES program and aims to test the hypothesis that nutrient type elements occur at higher concentrations at higher latitude, notably the Southern Ocean. The fact that they were able to proof this hypothesis is not at all surprising to me given that nutrient type elements are also referred to as 'accumulated' type elements as they accumulate in older (deep) water. Besides the nutrient type profile (low in surface waters and concentrations that increase with increasing depth) this also leads to a well-known and strong interbasin fractionation where concentrations are higher in the old deep North Pacific or deep Southern Ocean compared to the relatively young deep North Atlantic. As acknowledged in the introduction and discussion of this ms, upwelling of old deep water in the Southern Ocean thus leads to supply of macronutrients. However, this inherently also supplies other nutrient type (trace) elements to surface waters (but not Fe that is subject to scavenging, hence has a hybrid type distribution (Bruland et al., 2014)), and Fe limitation results in 'left-over' nutrients. In the North Atlantic, deep mixing also leads to supply of nutrient type elements to surface waters, albeit lower than compared to the Southern Ocean due to lower deep water concentration in the Atlantic, and seasonal Fe limitation (e.g. Achterberg et al., 2018) results in some 'left-over' nutrients. So while the authors did prove their hypothesis using statistical tests, this hypothesis is actually a well-established concept, not only for the macro nutrients, but also the 'nutrient-type' trace metals (hence their classification as nutrient-type aka as recycled or accumulated type). As far as I can tell, the conclusions of this manuscript are also a main message of any chemical oceanography text book, except for the lines on the Arctic where the authors seemingly missed that the position in the global conveyor (with related absence of old deep water that is strongly enriched in nutrient type elements) is important. Moreover, established concepts regarding the importance of sources, sinks and chemistry of different elements are ignored and I disagree with the notion that recent work did not focus on latitudinal patterns (see specific comments).

Overall, I'm afraid I do not see any novel contribution of this manuscript and therefore cannot recommend it for publication in its current form.

Specific comments

Line 16 distributions of elements in the oceans (there are many distributions that were understood much earlier) Intro Jumps straight into macro nutrient distributions followed by alkalinity without any context or connection between the subsections Line 29 iron and light limited Line 52 awkward sentence. Line 64/65 what is the point of this standalone sentence? Similar observations for Cd and Zn by the way Line 69-70 for Ni is was attributed to upwelling of deep water (direct citation: 'The higher concentrations in the Southern Ocean are most likely due to upwelling of older deep water in this region whereas in contrast, the Arctic is largely supplied by nutrient poor surface water transported north with the Gulf stream' and also depicted in figure 7 of this paper. Similar arguments for Cd and Zn in Middag et al., 2019, 2020) Line 75-80 I find it extremely odd to call the bio-essential element Mn 'biounutilised' whereas it has been shown to limit productivity in the Southern Ocean. Actually, in the Southern Ocean, Mn would be classified as bioutilised (probably bio-utilised is more readable) as concentrations are depleted in the surface and increase with depth (e.g. Middag et al., 2011), whereas Fe in parts of the equatorial Atlantic would be biounutilised (probably bio-unutilised is more readable) as concentrations are elevated in the surface and decrease with depth (e.g. Rijkenberg et al., 2014). Line 89 to what salinity is the data normalized? Line 110 table 1 why is Mn data from GA02 and GIPY 05 ignored? Line 190 why was this based on one individual station (see also previous comment) Line 310 this distribution of Mn and Fe is well known and related to the chemistry of the elements (both subject to oxidative scavenging), biological utilization and notably the presence of strong sources at low latitude (mainly Saharan dust deposition at low latitude, but also fluvial input and reducing sediments) whereas these sources are lacking or much reduced at higher latitudes. The fact that Mn and Fe are low in the HNLC Southern Ocean is something that can be found in any text book or review paper on chemical oceanography addressing these elements. Line 312 For Al and Pb this distribution (e.g. Bridgestock et al., 2016; Middag et al., 2015) is well known and again related to sources and sinks in its biogeochemical cycling. Section 4.3. This is basically a brief summary of a text book on chemical oceanography (As a matter of fact, one of the re-occurring questions I ask in the exam about my chemical oceanography lectures is to explain the higher concentrations of nutrient type elements in the higher latitude regions, notably the Southern Ocean) Line 355 excess of evaporation over precipitation should be accounted for in the salinity normalization. Line 356-357 similar for Mn; and presence and absence of sources such as atmospheric dust, (reducing) sediment, fluvial input, anthropogenic sources etc. 4.5 Deep waters in Arctic Ocean are also not particularly enriched in nutrient type elements like the Southern Ocean as deep waters here are much younger, i.e. Arctic Ocean sits mainly at the beginning of the ocean conveyor with inflow of nutrient poor Atlantic surface water and only modest amounts of old pacific deep water (see large body of GEOTRACES work in Arctic from both during IPY as well as recent expeditions) Line 375-3-77 I have to disagree here, a main point of those recent papers was the importance of high nutrient (incl nutrient type trace metals) high latitude waters and their influence on both the horizontal (meridional) and vertical distributions (and coupling between elements) at lower latitudes. Line 389 this was a main conclusion of many recent papers (e.g. Middag et al., 2019; Middag et al., 2020; Middag et al., 2018; Roshan et al., 2018; Roshan and Wu, 2015a; Roshan and Wu, 2015b; Vance et al., 2017; Weber et al., 2018) and the lack of Fe supply relative to macro nutrients in upwelling regions is about as old as the term 'HNLC'. 4.7 point 2; given the absence of a strong dust source over large parts of the Pacific, there will be differences for some elements (e.g. the high concentrations of Al, Fe and Mn at low latitude are not found). Moreover, part of the equatorial Pacific is an HNLC region with elevated concentrations of nutrient type elements point 3; this is well known, hence the high-latitude North Pacific is a HNLC region whereas the high latitude North Atlantic only has minor inventories of 'left-over' macro nutrients at the end of the phytoplankton growth season and only experiences seasonal Fe limitation (end of season). point 4: except those with a strong fluvial influence, see recent work on metals in the Arctic trans polar drift. Also noted in recent work on global or Atlantic distribution of Cd, Zn an Ni. Conclusions The statement 'presumably because of its role as the limiting nutrient for primary production in upwelling regions' does not explain anything; the limiting nutrient is the one that is in shortest supply relative to demand. Assuming uptake ratios of the different nutrients don't vary dramatically between regions, basically the authors state Fe is not high in the SO because there never was much to begin with, whereas the other nutrients are high because they are abundantly supplied. Stating the exchange of surface and deep water is prevented in the Arctic is inaccurate, it is an important region of deep water formation.

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