

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

Anonymous Referee #2

Received and published: 24 November 2020

It is very timely to begin to analyse and synthesize interpretations of the emerging data sets from the GEOTRACES program. This study uses GEOTRACE data from the Atlantic basin to address the hypothesis that "All bioutilised elements are present at higher concentration in high latitude than in low latitude surface waters".

Discussions of elemental vertical profiles have often noted "nutrient-like" profiles for some elements, the concentrations of which increase with depth (e.g. Nozaki, 2001; Froelich, 2014; numerous subsequent references to the "periodic table of elemental profiles". Since the ocean's vertical density structure is largely controlled by temperature, so denser (deeper) water-masses outcrop at colder temperatures (higher lat-

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itudes) bringing with them higher concentrations of nutrient-like tracers mixed along isopycnals. So the basic hypothesis is uncontroversial. I found it odd that this simple mechanistic interpretation wasn't discussed until the very end of the manuscript however. The focus on latitude as the correlate was, for me, a concern. Latitude is a proxy for a more physically relevant quantity (e.g. temperature, density, buoyancy loss, atmospheric deposition,...).

Reading through the manuscript in its current form I kept asking myself why would I care about this hypothesis unless the mechanistic interpretation is front and centre? Why not try and seek relationships with the actual drivers and mechanisms of deep water formation/ventilation or atmospheric deposition, for example? In my view focusing the study on the relationship to latitude misses the chance to make a more mechanistic and physical interpretation and could lead uninitiated readers into a misleading view of broader oceanographic understanding.

The contrast between Fe, Al and the other nutrient-like elements is stark and interesting to see in this context. Presumably the role of atmospheric sources at lower latitudes is important (?) but (I felt surprisingly) this is not discussed in Section 4.4 ("Input of deep water is not the only process").

Detailed Comments:

Introduction: Lines 15-25.

What about temperature, salinity, density as contextual discussion? How does surface density vary with latitude? This seems intimately tied to the hypothesis posed here.

Line 20: Citing Levitus et al (1993) for the major basin scale pattern of surface nutrients is not wrong, but surely these patterns were known long before the 1990's and a primary source from early nutrient surveys could be cited (and would be interesting).

Line 25: The discussion about the accuracy of phosphate measurements seems spurious. The major basin-scale gradient moves from ~0 to > 1 or 2 micromolar. The

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latitudinal pattern which is the focus of this discussion doesn't depend on the difference between nano- and pico-molar accuracy.

Line 30: The citation to Moore (2016) again seems relevant but not primary. The understanding that upwelling waters are depleted in Fe relative to macronutrients (and why) can be attributed to earlier, or primary sources. (Martin? Archer and Johnson?)

Line 35-40: Again timescale and citations: "the last 20 years" - what about GEOSECS, now 40 years ago? Probably should cite Key et al (2004) for the original GLODAP paper.

Line 55-60: Follows and Williams (2011) should be Williams and Follows (2011).

Line 65: The introduction of a one sentence discussion of Nickel at this point seems very random and odd. What about the data of Nozaki (2001) from the Pacific, which provides a great deal of information about a number of elements? Why isn't it brought into this discussion/introduction?

Line 75-80: Would be useful to quote Broecker and Peng's definitions since you are discussing them.

Line 80-85: It seems that you are classifying elements that are known to be biologically essential as "unutilized" because their vertical profile is not nutrient-like (i.e. doesn't increase with depth). Why use the word "utilized" at all? Why not define a different category for your hypothesis-test (e.g. "nutrient-like", i.e. increasing concentration with depth) because that's what you are actually examining. As it stands, it appears that you are redefining the meaning of "bioutilized" to support your hypothesis - because it's almost certain that tracers that increase with depth will have higher concentrations at surface high-latitude outcrops. Overall, it seems to me that the classification scheme employed here is misleading and needs to be changed.

Summary:

This study addresses newly available, large scale tracer data sets and is thus timely.

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However, the focus on latitude-tracer correlations, without clear reference to the underlying mechanistic links, did not provide a useful concept or tool for me. The oceanographic context that appears as the discussion should have, in my opinion, been part of the introduction and motivation for seeking relationships with more meaningful correlates. Citation of relevant earlier work could be more thorough. The contrast between Fe, Al and other nutrient like tracers is brought forth clearly in the data analysis, but the underlying reasons are not really discussed. Overall, in my opinion, this manuscript and study, in its current form, represents a good start but needs quite a bit of work to reach its potential value.

Interactive comment on Biogeosciences Discuss., <https://doi.org/10.5194/bg-2020-371>, 2020.

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